The Labor Market Effects of Carbon Pricing*

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Abstract

We study how carbon prices affect labor-market outcomes by exploiting a policy change in the EU Emissions Trading System that led to a sharp rise in permit prices. Using population-wide matched employer-employee data from the Netherlands and a matched difference-in-differences design, we find no adverse average effects on employment or wages. However, the distributional effects are sizable: workers initially employed by ETS firms with larger pre-reform permit shortfalls experience lower wage growth relative to their matched controls, whereas STEM-educated workers experience relative wage gains—especially those with stronger outside options. Plants with higher pre-reform shares of STEM workers subsequently reduce emissions and energy costs by more, consistent with skills facilitating the transition to low-carbon technologies. Our results illustrate that the distributional effects of carbon pricing depend on market design and worker skills.

Keywords: Carbon pricing; Cap-and-trade; Labor markets; Wages; Skills; European Union Emissions Trading System

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1 Introduction

Cap-and-trade systems are now the cornerstone of many countries' climate strategies, covering more than 20 percent of global emissions as of 2025, with several additional systems planned (World Bank, 2025).[1] By setting an aggregate emissions cap and allowing firms to trade permits, these systems are designed to achieve a target level of carbon reduction at the lowest possible cost. In practice, however, carbon prices in most existing markets have remained below the levels consistent with net-zero targets, implying that prices will need to rise substantially in the coming years. As these price adjustments unfold, concerns have emerged about how cap-and-trade systems may affect labor markets.[2]

Higher carbon prices increase production costs for participating firms, potentially influencing employment, wages, and skill demand across sectors. The magnitude and direction of these effects are likely to depend on the design of the carbon market, such as the allocation of free permits, and on the availability of worker skills that facilitate emission reductions. Yet, despite the growing global scale of cap-and-trade systems, we know little about how market design and worker skills jointly shape the labor-market consequences of higher carbon prices. This paper provides new evidence on how an exogenous increase in carbon prices affects workers' employment and earnings, and illustrates the importance of market design and worker skills.

We identify the labor-market consequences of an increase in carbon prices by exploiting a policy reform in the European Union Emissions Trading System (EU ETS). This reform, announced in February 2017 and ultimately adopted in November 2017, substantially reduced the supply of emission permits, triggering an unanticipated and large increase in carbon prices from €5 to €30 per tonne of emissions. We combine this arguably

[1] Brazil, India, Turkey, Indonesia, and Canada have announced or are developing cap-and-trade systems. The European Union will also introduce a new emissions trading system (ETS 2) in 2027, covering fuel combustion in buildings, road transport, and other sectors.

[2] Recognizing such concerns, The UN's Environment Programme (UNEP) and the ILO emphasize that carbon pricing should minimize negative distributional effects (UNEP, 2024; ILO, 2015). In line with this, the European Commission has established the Just Transition Mechanism in 2021, which aims to facilitate employment opportunities for those in transition and offer re-skilling opportunities (link).

exogenous increase in carbon prices with confidential matched employer-employee data from the Netherlands. Our dataset allows us to observe wages, hours, earnings, as well as a rich set of individual characteristics and plant-level production inputs. We use the richness of the data to implement a matched difference-in-differences design that compares workers employed in ETS-regulated firms to observationally similar workers in non-ETS firms, enabling us to isolate the causal effects of an exogenous carbon price shock on labor outcomes.

Our findings show that the labor-market incidence of carbon pricing depends on firms' net permit positions and workers' skills. On average, we find no evidence of adverse employment or wage effects for workers initially employed by ETS firms. This evidence suggests that, in this setting, the transition to higher carbon prices did not impose broad labor-market costs.[3] These average effects, however, mask economically meaningful heterogeneity associated with firms' pre-reform permit positions. A higher carbon price generates a negative cash-flow shock for firms whose emissions exceed their free permit allocations and a positive shock for firms holding surplus permits. Consistent with firms passing part of these shocks through to labor, a one-standard-deviation larger pre-reform permit shortfall reduces the post-reform wage differential between ETS workers and their matched non-ETS controls by 1.8 log points for hourly wages and 2.8 log points for total wages. We find no corresponding employment response, indicating that the adjustment occurs primarily through wages.[4]

In addition, workers' skills critically influence the effects of carbon pricing. Workers with a university degree in the "STEM" fields (science, technology, engineering, or mathematics), who possess technical expertise relevant for energy management, process

[3]The absence of adverse average labor-market effects is consistent with existing evidence on the European carbon market (e.g., Dechezlepretre et al., 2023; Colmer et al., 2025) and on carbon taxation in emerging economies (Gallé et al., 2025).

[4]This result illustrates the distributional dimension of the standard independence result in cap-and-trade markets: under ideal conditions, the initial allocation of permits need not affect the equilibrium permit price or aggregate abatement, but it determines the distribution of allowance rents (Coase, 1960; Montgomery, 1972).

optimization, and low-carbon innovation (e.g., Vona et al., 2018; Saussay et al., 2022; Alexander et al., 2024), enjoy sizable wage rises after the increase in carbon prices. Specifically, their hourly wages differentially increase by 5 percent, wages by 4.2 percent, and annual earnings by 2.4 thousand euros following the carbon price increase. We find no discernible effects on employment probabilities, which remain close to full employment for this group and likely exhibit limited scope for adjustment.

We further find that the higher STEM skill premium is especially pronounced among workers with stronger outside options—which we proxy via their past or subsequent job mobility—and does *not* reflect a more general increase in skill premiums over time. To establish the latter point, we separately study non-STEM workers across five categories: business, non-STEM non-business, applied university, vocational, and other (high school or below). The results reveal little evidence of a broader skill premium.[5] Finally, we address a potential concern that the observed wage gains for STEM workers might simply reflect their disproportionate concentration in more energy-efficient firms with permit surpluses. We show that these differences in labor market outcomes persist, and actually become larger and more precisely estimated, when we control for firm-year fixed effects in our specifications, thus effectively comparing STEM and non-STEM workers employed at the same firms.

Importantly, we provide evidence consistent with STEM workers helping firms improve their carbon efficiency, which helps explain why their wages rise. Combining unique plant-level production input data with plant-level emissions data, we find that plants with a higher pre-shock share of STEM workers achieve larger reductions in carbon emissions after the carbon price increase. These plants also experience larger reductions in energy costs, suggesting that STEM skills facilitate production-side adjustment when the return to abatement rises. By contrast, the pre-shock shares of graduates with business or

[5]Business graduates experience wage increases, concentrated among those above the 75th percentile of the within-plant wage distribution, our proxy for managerial roles. Small employment declines leave their overall earnings unchanged. For non-STEM, non-business and applied-university graduates, neither wages nor earnings change significantly.

other university degrees do not predict subsequent emissions reductions. Further supporting this interpretation, we document that STEM graduates are employed in occupations with a higher green-skill share, measured at the four-digit ISCO level as the fraction of occupation-specific skills classified as green in the European Commission's ESCO taxonomy. At the same time, ETS firms appear unable to increase the rate at which they hire STEM workers, consistent with the supply of such workers being rigid, at least over a relatively short horizon.

To illustrate the possible channels through which an increase in carbon price may affect wages, we sketch a simple theoretical framework. In our framework, the workers and the firm split the surplus produced by the worker-firm match via Nash bargaining. The match surplus can be decomposed into a firm-level component, common to all the workers of the firm, and a worker-level component, which can differ across workers within the same firm. Our framework illustrates how the effect of the carbon price increase on wages depends on both components. First, the increase in carbon prices affects the firm-level component, depending on the firm's permit shortfall—the carbon price increase implies a cost for firms whose emissions normally exceed the number of free permits they receive, and vice versa. Therefore, firms with permit shortfalls may pass the cost on to their workers by lowering their wages. Second, the increase in carbon prices affects the worker-level component, as it makes workers who can contribute to emission reductions more valuable to the firm. Our framework also reveals that the effect of the worker-level component on wages depends on workers' outside options: those with better outside options capture a larger share of the worker-level component. These predictions are borne out in the data.

Our analysis draws on confidential administrative data from Statistics Netherlands (CBS) that link the universe of Dutch workers to their employers and include detailed information on wages, hours, and job characteristics. These data are merged with additional worker-level characteristics, such as education, and firm-level balance sheets and production accounts covering all limited liability companies. Two empirical challenges arise in using these data to estimate the effects of the increase in carbon prices on workers. First, firms covered by the EU ETS are concentrated in energy-intensive sectors such as power generation and manufacturing of metals or chemical products. Second, ETS firms differ systematically from other firms, as they are larger, more profitable, and pay higher wages. To address these compositional differences, we implement a simple matching strategy that pairs workers employed by ETS firms in the year prior to the carbon price reform ("treated" workers) with observationally similar workers employed by non-ETS firms ("control" workers) within the same industry. Matching is based on pre-treatment characteristics, including age, gender, tenure, wage level and firm size and profitability, ensuring that treated and control workers are comparable along key observable dimensions. This procedure yields a balanced sample of approximately 32 thousand workers, evenly split between the treated and control groups. Our difference-in-differences design uses 2014-2016 as the pre-treatment period, and 2017-2020 as the treatment period. Supporting our matching design, we show that there are not differential pre-trends.

Notably, our key results remain robust when we implement a series of variations over our preferred matching algorithm. In our baseline matching approach, we restrict the analysis to matched units with a difference in the propensity scores, or caliper, of at most 0.025; however, we find very similar results when we use more or less conservative calipers. Similarly, matching on a single lag of hourly earnings produces similar estimates. Finally, we address the concern that our results are driven by the relatively low match rate, as we are able to match about 18% of the sample of ETS workers. As ETS firms are typically much larger than those in the control group, removing size as a matching variable substantially increases the match rate to 70%. We find largely similar heterogeneity in the results (across STEM and non-STEM workers and across firms with different levels of emission surpluses).

Overall, our findings indicate that the labor-market consequences of the increase in carbon prices depend critically on the institutional design of the carbon market and on workers' skill composition—dimensions whose distributional effects are masked when examining average outcomes alone. These insights should be informative when considering the effects that the expansion of carbon pricing to more sectors and countries may have on different parts of the workforce, which may in turn also affect political support for such policies.

Related literature Our paper relates to an ample literature on the interaction between climate policies and the real economy. The first strand of literature focuses on the effects of these policies on labor markets. One of the major concerns about climate policies is their potential adverse effects on employees in regulated sectors, which can influence the design and strictness of these policies.[6] In line with this concern, earlier studies that examine the employment effects of different types of climate policies document sizable labor costs (e.g., Greenstone, 2002; Kahn and Mansur, 2013; Gray et al., 2014; Liu et al., 2017; Curtis, 2018; Yip, 2018; Sheriff et al., 2019; Azevedo et al., 2023). Most closely related to our study, Walker (2013) uses matched employee-employer data to show that the 1990 Clean Air Act reduces the earnings of workers in the regulated sectors, primarily due to increased unemployment and lower future earnings. We contribute to this literature by examining the labor market effects of an exogenous carbon price increase in a cap-and-trade system. In line with the flexibility that cap-and-trade systems provide (Carlson et al., 2000; Fowlie et al., 2012), we do not find any negative overall labor market effects, either on wages or on employment. Moreover, we document that the design of the carbon market and the workers' skill composition are central determinants of the distributional consequences of carbon pricing.

Our paper also contributes to the debate about how climate policies influence firms. In addition to abatement costs (Anderson and Sallee, 2011; Cullen and Mansur, 2017; Meng, 2017), climate policies may affect firms by influencing valuation (Acharya et al.,

[6]President Trump justified his decision to leave the Paris Agreement by stating that it would cost 6.5 million jobs in the US (see "Paris climate deal: Trump pulls US out of 2015 accord," *BBC World*, June 2, 2017).

2025), the rate of return that investors or lenders demand (Seltzer et al., 2022; Bolton and Kacperczyk, 2023; Hsu et al., 2023; Ivanov et al., 2024; Zhang, 2025), and interact with financial constraints (Bartram et al., 2022; Martinsson et al., 2024; Biais et al., 2025; Döttling and Rola-Janicka, 2025; Oehmke and Opp, 2025), affecting production (Shapiro and Walker, 2018), investment (Biais and Landier, 2022; Brown et al., 2025; Jacob and Zerwer, 2024), and innovation (Gans, 2012; Calel and Dechezlepretre, 2016; Brown et al., 2022).[7] We focus on effects on the labor side of the production function, which is of course complementary with the capital side.

In a narrower sense, we contribute to the literature evaluating the real economic effects of the EU ETS, one of the world's earliest and largest cap-and-trade systems. Earlier work focusing on the first two phases of this market largely concludes that the system reduces emissions without triggering broad adverse effects on economic performance or inducing carbon leakage (Dechezlepretre et al., 2023; Verde, 2020; Martin et al., 2016; Marin et al., 2018; Löschel et al., 2019; Klemetsen et al., 2020; Akey et al., 2024; Biais et al., 2026). For instance, using firm-level data from France with a matched difference-in-differences design, Colmer et al. (2025) confirm a significant decline in carbon emissions without a corresponding contraction in economic activity. However, examining the 2017 reform, Boeckx et al. (2025) find that emission-intensive, financially constrained private firms significantly increase employment downsizing and divest production assets. By exploiting employee-employer matched data, we complement this literature by studying the role of the design of the EU ETS market and labor market determinants that are masked in firm-level data, such as worker characteristics, outside options, and wage bargaining. Indeed, we document that the labor market effects of carbon pricing depend on workers' abatement skills, bargaining power, and the participating firms' permit shortfall. We further show that plants with higher pre-shock STEM shares reduce emissions and energy costs more

[7] An important tradeoff this discussion considers is that while climate policies may reduce emissions, they can hurt firms' competitiveness and induce firms to move their production to jurisdictions with laxer policies (e.g., Becker and Henderson, 2000; Ryan, 2012; Greenstone et al., 2012; Fowlie et al., 2016; He et al., 2020; Känzig, 2023; Kapfhammer, 2023; Gallé et al., 2025).

after the carbon-price increase, highlighting the role of worker skills in shaping firms' adjustment to carbon pricing.

The rest of the paper proceeds as follows. Section 2 describes the EU ETS market and the 2017 carbon price hike. Section 3 presents a simple theoretical framework that guides our empirical analysis. Section 4 explains our data sources and empirical strategy. Section 5 reports our results, and Section 6 concludes.

2 The European Union Emission Trading System

The EU ETS is a cap-and-trade system, covering approximately 40 percent of greenhouse gas emissions of all EU member states, as well as Norway, Iceland, and Liechtenstein.[8] In this system, the European Commission determines a cap for the total amount of emissions that the participants may emit and issues emission allowances (EUA) accordingly. A positive carbon price emerges in this system as the cap is lower than the emissions of the participants, entailing allowance trading. One allowance permits the participants to emit one tonne of CO_2 equivalent of greenhouse gases. Importantly, any unused allowances can be deposited and used in later years.

The participation is at the installation level, defined as a "stationary technical unit" that carries out the activities listed in Directive 2003/87/EC, for instance combustion of fuels, refining of mineral oil, or production of steel, glass, or cement. Certain activities are subject to capacity thresholds, including a total rated thermal input exceeding 20 megawatts for fuel combustion and 2.5 tonnes of production per hour for iron and steel manufacturing.[9] Once an installation falls within the scope, its operator must monitor, report, and surrender emission allowances corresponding to its verified scope 1 greenhouse-gas emissions each compliance year. Per Articles 15 and 16 of Directive 2003/87/EC, the

[8]Switzerland had a separate ETS between 2013 and 2020, which has been linked to the EU ETS since 2020 (Document 22017A1207(01)).

[9]While the information about the threshold values is publicly available, the installation-level capacity information is not.

amount of emissions must be verified by third-party auditors, and installations that fail to surrender the allowances must still surrender the missing allowances and pay a €100 penalty for each tonne of missed emissions.

Allowances in the EU ETS are distributed either through auctions or via free allocation. As shown in Figure 1, during the first two phases (2005–2012), the vast majority of allowances were granted for free. Beginning with Phase III in 2013, however, auctioning became the principal allocation method, accounting for around 57 percent of the total distribution. The EU uses free allocation to mitigate carbon leakage, defined as the transfer of production to other countries with more lenient regulations, and to reward carbon-efficient installations. Aiming for these goals, the free allocation amount is calculated using the equation below, which includes industry-, production-, and installation-specific parameters:

$$Free\ allocation = HAL \times Benchmark \times Carbon\ leakage \times Linear\ reduction \tag{1}$$

In this equation, *HAL* is the median of the historical activity levels of each installation in the earlier phases, ensuring that the amount of free allocation reflects the scale of the installation's activity. *Benchmark* is a product-level parameter that rewards efficient installations. During Phase III, this parameter is the average of the most carbon efficient installations for each product between 2007 and 2008. Therefore, an installation may receive more free allowances than its emissions by improving its carbon efficiency.[10] *Carbon leakage* is set at 100 percent for sectors included on the carbon leakage list, while for other industries, it was initiated at 80 percent in 2013 and declined linearly to 30 percent by 2020.[11] The *linear reduction factor* reduces the total cap of allowances by a fixed annual percentage each year, which is set at 1.74 percent during Phase III; see Table OA1 for details. While the information on the industries on the carbon leakage list and the linear

[10] This is illustrated in Figure OA1, which depicts the percentiles of installations' carbon efficiency and the benchmark value for colorless glass production.

[11] The industries on the carbon leakage list are reported in Document 32014D0746.

reduction factor is publicly available, HAL and the distance between the installation carbon efficiency and benchmark values are not disclosed at the installation level. Therefore, we cannot directly observe all determinants of free allocation and permit shortfall at the installation level.

The 2017 reform in EU ETS The EU ETS experienced a crucial problem during the first years of Phase III: the carbon prices were too low due to a structural supply-demand imbalance, weakening the incentives to reduce emissions. The European Commission cites high allowance supply and low economic activity in the aftermath of the Global Financial Crisis as the reasons (Document 52012DC0652). Motivated by the EU's commitment to reducing emissions by at least 40 percent below 1990 levels by 2030, the Environment Council of the EU announced its negotiating position for the review of the EU ETS to address this structural imbalance on February, 28 2017, as indicated in the timeline in Figure 2 (link). To enhance the effectiveness of the EU ETS, the council made several proposals, which ended up being accepted on November 9, 2017, and added to the EU ETS Directive on March 14, 2018 (De Jonghe et al., 2020; Apicella and Fabiani, 2023; Boeckx et al., 2025, also see Document 32018L0410).

First, the capacity of the "Market Stability Reserve" (MSR) was increased. The MSR rebalances the allowance supply in accordance with predefined thresholds of the Total Number of Allowances in Circulation (TNAC); the 2017 proposal increased the withdrawal rate of the MSR.[12] In addition, the reform gives the MSR the right to invalidate allowances permanently if the allowances held in the MSR exceed the total number of allowances auctioned during the previous year starting from 2023. Moreover, 900 million allowances that were supposed to be auctioned were instead added to the MSR. Lastly, the linear reduction factor would be increased to 2.2 percent from 1.74 percent, starting in 2021.

Two features of this reform make it an exemplary shock to carbon prices in the EU

[12]Specifically, when TNAC exceeds 833 million, the withdrawal rate would be increased from 12 percent to 24 percent. If TNAC is below 400 million, the MSR releases 100 million allowances.

ETS. First, the reform was unanticipated, preventing participants from taking measures beforehand. Confirming the surprising nature of the reform, the carbon prices in the EU ETS did not increase prior to the reform, as depicted in Figure 3. In addition, the firms that participate in the EU ETS began lobbying against the changes in the reform after the announcement, suggesting that the reform had taken them by surprise.[13] Second, the reform substantially reduced the supply of allowances, placing upward pressure on permit prices. The magnitude of the resulting shock was economically meaningful: ETS installations in our data surrender an average of 155 allowances per worker annually, so the roughly €20 increase in the allowance price raised the market value of their annual compliance obligations by approximately €3,100 per worker. Emphasizing the importance of this reform, the size of MSR already exceeded that of TNAC one year after its initiation in 2020 (see Figure OA2). In line with the reform, the MSR invalidated more than 2 billion allowances in 2023, while the TNAC was approximately 1.1 billion. Confirming the reform's importance, the carbon price started to increase in the second half of 2017 from € 5, and reached € 30 by 2020 (Figure 3).

3 Theoretical Framework

To guide the empirical analysis, we present a simple framework that combines rent sharing with the intuition of on-the-job search models (Postel-Vinay and Robin, 2002; Cahuc et al., 2006). Consider a worker i employed by firm j. The match surplus is given by $S_{ij}(p_c) \equiv_j (p_c) + V_i(p_c)$, where p_c denotes the carbon price. The first component, $V_j(p_c)$, is a firm-level component that is common to all workers in firm j. This component captures the effect of the carbon price on the value of the firm-worker match through the firm's permit position. A higher carbon price increases the value of unused permits for firms with permit surpluses, but increases compliance costs for firms with permit shortfalls. Hence,

[13] Many participants issued public statements, emphasizing that the reform is too strict and may hurt the competitiveness of the participants. See the statements from the steel industry (link), ceramic, glass, and cement industries (link).

the effect of the carbon price on $V_j(p_c)$ is expected to be decreasing in the firm's permit shortfall.

The second component, $V_i(p_c)$, is worker-specific. It captures the value that worker i contributes to the match. The carbon price increase may raise this component for workers whose skills help firms reduce emissions, lower energy costs, or reorganize production toward less carbon-intensive technologies. We expect this channel to be especially relevant for workers with STEM education, whose technical skills may be useful for energy management, process optimization, and low-carbon innovation.

Absent a credible outside offer, wages are determined through bilateral Nash bargaining between the worker and the firm. The worker's bargaining weight is denoted by $\beta \in (0, 1)$, while the shareholders' bargaining weight is $1 - \beta$. We normalize the worker's nonemployment value and the shareholders' outside option to zero. In the absence of an outside offer, the wage solves

$$\max_{w_i} w_i^{\beta} \left(V_j(p_c) + V_i(p_c) - w_i\right)^{1-\beta}, \tag{2}$$

which yields

$$w_{0i}(p_c) = \beta \left[V_j(p_c) + V_i(p_c)\right]. \tag{3}$$

This wage captures the standard rent-sharing benchmark: the worker receives a fraction β of the total match surplus.

We now introduce on-the-job search in a reduced-form way. Let $\lambda_i \in [0, 1]$ denote the probability that worker i receives a credible outside offer from another employer. This parameter captures the relevance of outside options for the worker. Workers with more portable skills, greater labor-market mobility, or stronger demand for their skills in the external labor market have higher λ_i. Being distinct from the bargaining power parameter, λ_i captures the probability that interfirm competition for the worker becomes relevant.

If an outside offer arrives, the incumbent firm must compete with the outside employer to retain the worker. Following the intuition of on-the-job search models, this competition increases the worker's ability to appropriate the worker-specific component of surplus. We capture this in a parsimonious way by assuming that, conditional on receiving a credible outside offer, the worker receives a share θ of the worker-specific component $V_i(p_c)$, where $\theta \in (\beta, 1]$. The parameter θ summarizes the strength of wage pressure generated by outside offers. Since an outside offer improves the worker's alternative to remaining with the current employer, it is natural that $\theta > \beta$: the worker captures a larger share of the worker-specific surplus when competing employers value the worker's skills.[14]

We apply the outside-offer premium to the worker-specific component rather than to the firm-level component. This distinction is important in our setting. The firm-level component $V_j(p_c)$ reflects permit-related rents or costs that are specific to the incumbent firm. In contrast, the worker-specific component $V_i(p_c)$ reflects skills that may be portable across employers. A competing employer can bid for the worker's transition-relevant skills, but it cannot bid for the incumbent firm's permit position, meaning the poaching firm's valuation of the worker is independent of the incumbent's idiosyncratic carbon rents. Accordingly, when an outside offer arrives, the wage is

$$w_{1i}(p_c) = \beta V_j(p_c) + \theta V_i(p_c). \tag{4}$$

Before the realization of outside offers, the expected wage is therefore

$$\begin{aligned} E[w_i(p_c)] &= (1-\lambda_i)\beta\left[V_j(p_c) + V_i(p_c)\right] + \lambda_i\left[\beta V_j(p_c) + \theta V_i(p_c)\right] \\ &= \beta V_j(p_c) + \left[\beta + \lambda_i(\theta - \beta)\right] V_i(p_c). \end{aligned} \tag{5}$$

Equation (5) shows that the firm-level component and the worker-specific component

[14]The upper bound $\theta \leq 1$ imposes surplus feasibility: the worker cannot capture more than the full worker-specific surplus. The case $\theta = 1$ corresponds to the limiting case in which competition for the worker fully dissipates firm rents from the worker-specific component. More generally, $\theta < 1$ allows the incumbent firm to retain part of this surplus even when an outside offer arrives.

enter wages differently. The firm-level component is shared according to the standard rent-sharing parameter β. The worker-specific component is also shared according to β when the worker has no outside offer, but the expected share rises with λ_i because outside offers create competition for the worker's skills.

Differentiating expected wages with respect to the carbon price gives

$$\frac{\partial E[w_i(p_c)]}{\partial p_c} = \beta \frac{\partial V_j(p_c)}{\partial p_c} + [\beta + \lambda_i(\theta - \beta)] \frac{\partial V_i(p_c)}{\partial p_c}. \tag{6}$$

Equation (6) can be used to derive the key predictions that will inform our empirical analysis. First, ceteris paribus, the increase in carbon price will lead to a wage increase if $\frac{\partial V_j(p_c)}{\partial p_c} > 0$. The effect of a carbon price increase is, however, ambiguous, and it depends on whether a firm is a net seller or a net buyer of permits. A net seller will enjoy the ability to sell the outstanding permits at a higher price, resulting in a cash windfall. We identify net sellers as firms with an outstanding surplus of permits: these are the firms that have been able to use fewer permits than those granted. By a similar reasoning, firms with a permit shortfall experience a cash outflow, as they will be forced to buy permits at a higher price. These considerations lead us to the first prediction.

Prediction 1. The effect of the carbon price increase on wages in an ETS firm is negatively related to the firm's permit shortfall.

The wage effect of the carbon price increase is also related to how it affects the worker-specific component of the match surplus, namely V_i. We expect the carbon price increase to affect the importance of workers who are essential in achieving a reduction in emissions, and thus obtain significant savings. For these workers, we expect $\frac{\partial V_i(p_c)}{\partial p_c} > 0$, implying a positive effect on wages as well. We anticipate that STEM workers are particularly useful to help firms reduce their emissions. Hence, our second prediction is the following.

Prediction 2. The carbon price increase leads to a wage increase for STEM workers.

Lastly, to understand whether the wage effect of a carbon price increase is larger for workers with stronger outside options, we differentiate equation (6) with respect to λ_i, yielding

$$\frac{\partial^2 E[w_i(p_c)]}{\partial p_c \partial \lambda_i} = (\theta - \beta)\frac{\partial V_i(p_c)}{\partial p_c}. \tag{7}$$

The sign of this expression depends on two terms. First, $\theta - \beta > 0$ because a credible outside offer increases the worker's ability to appropriate the worker-specific component of surplus relative to bilateral bargaining with the incumbent firm. Second, $\partial V_i(p_c)/\partial p_c$ captures how the carbon price affects the worker-specific value of the match. For workers whose skills are useful for low-carbon adjustment, such as STEM workers, we expect $\partial V_i(p_c)/\partial p_c > 0$. Hence, for these workers,

$$\frac{\partial^2 E[w_i(p_c)]}{\partial p_c \partial \lambda_i} > 0. \tag{8}$$

Intuitively, when carbon prices rise, transition-relevant skills become more valuable. If these skills are also valued by outside employers, the worker can use the outside option to obtain a larger share of the additional worker-specific surplus. Outside options therefore amplify the wage effect of the carbon price increase for workers with transition-relevant skills.

Prediction 3. The carbon price increase leads to a more pronounced wage increase for STEM workers who are more likely to have stronger outside options.

4 Data and Econometric Strategy

In this section, we describe how we analyze the effect on labor market outcomes of the sudden increase in carbon prices occurred after 2017 in a matched difference-in-differences setting.

4.1 Data Sources and Filters

For our analysis, we make use of a confidential employer-employee matched dataset made available by the *Centraal Bureau voor de Statistiek* (CBS), the Dutch statistical agency. The dataset includes detailed information on labor market variables, such as employment status, wages, and hours worked, for the universe of the Dutch workforce starting from 2010. We also have access to demographic information, such as gender and age. Moreover, for the majority of our sample, we have access to detailed information regarding workers' education, namely their highest degree, the type of institution (i.e., school of applied sciences, vocational school, university, etc.) and the major.

Every worker is associated with a plant identifier (*BE_ID*), which can, in turn, be linked to a firm identifier (*Rog_Identificatie*). On the firm side, CBS reports basic items from the firms' financial reports, such as total assets, liabilities, and profits.

We start from an initial sample of 8.2 million individuals recorded by CBS as having at least one employment contract in 2016, the year prior to the event. We restrict the analysis to workers between 25 and 60 years of age. We further employ a "tenure restriction," requiring workers to have at least two years of tenure in the same company (meaning that they have the same employer in 2015 and 2016). When we observe two or more employers for the worker in a given year, we assign the worker to the employer that earned them the highest wage. Hence, by "wage" we refer to the worker's compensation in a year from their "dominant" employer. However, we also record the variable "earnings," which instead includes all the labor income earned by the worker in a given year, possibly from multiple employers. 4.9 million workers satisfy these filters. We further require a successful match with the firm-level data, in order to observe profitability, which reduces the sample to 3 million workers. In addition, we drop 237,000 workers earning less than €10,000 and 1 million workers for which we do not observe education (a further 37% reduction in our sample size).

We then merge the publicly available ETS data (which includes free permit allocation, permit trades, and emissions) with CBS data.[15] In our final sample, we identify 95,441 workers that were employed in 212 plants subject to the ETS scheme in 2016, belonging to 181 unique firms. We refer to these as our "treated" workers. One important feature of our setting to keep in mind is that the treatment status is defined at the plant level, not at the firm level. Hence, the same firm can simultaneously employ treated and control workers, i.e., workers who are not in ETS plants but are employed in firms that do have ETS plants. To the extent that there are meaningful spillovers across plants belonging to the same firm, including these workers in the set of potential control units is likely to lead to a violation of the "Stable Unit Treatment Value Assumption" (SUTVA). Hence, in our analysis, we exclude the 77 thousand untreated workers employed in firms that have at least one ETS plant from the pool of potential control workers. After adopting this final filter, we are left with a set of 1,670,567 untreated workers, resulting in our final sample of 1,766,008 workers. Panel A of Table 1 shows the distribution of these workers across industries and size bins. For simplicity, we display statistics for the ten industries with the highest number of treated workers, and pool all the others in the "Others" category. Industries are defined according the two-digit Standard Industry Classification (*Standaard Bedrijfsindeling*, or "SBI") provided by CBS. Size is defined as firm's employment, which we categorize using the following bins: [1, 49], [50, 249], and 250 or more employees. The manufacturing sector dominates the list, and includes six of these industries. However, the list also includes firms in the service sector (Education and Human Health Activities), energy production, and air transport. About half of the workforce in the sample are employed in large (250 employees or more) firms.

Panel B shows the fraction of treated employees in each industry-firm size combination. While in the air transport sector 98% of the workers are treated, in the other sectors the fraction of workers in ETS firms drops to less than 50%. The fraction of treated workers in

[15]To do so, we collect the KVK numbers of the ETS firms—the Chamber of Commerce number that uniquely identifies business units in the Netherlands. CBS then matched the KVK numbers to the encrypted identifiers, allowing us to identify the plants that operate under the ETS framework.

the manufacturing and energy production industries varies between one quarter and one third, and falls to less than 20% for the service sectors. When looking at the distribution across firm size classes, we find that almost 10% of the employees of large firms belong to ETS firms; the percentage drops to 1% for medium-size firms (between 50 and 249 employees), and becomes negligible for smaller firms.

4.2 The Matching Approach

In the first step of our analysis, we restrict the analysis to a subsample of workers that is sufficiently balanced in terms of observable covariates, such as wages and firm size. To achieve this, for every "treated" worker, that is, a worker employed in an ETS plant in 2016, we find a suitable control worker, comparable in terms of observable characteristics. We require control workers to be employed in the same industry, as polluting firms tend to be concentrated in specific sectors, such as air transport and extraction of oil and natural gas.

At the worker level, we also match exactly on gender and on a "high tenure" dummy, that is, a dummy equal to one if the worker has tenure above the sample median.[16] Within each combination of gender, part-time status, tenure, and industry, we then estimate a standard logit model to derive the probability of being employed in an ETS firm, using as predictors the following continuous variables: age, the logarithm of wages and lagged wages, the logarithm of firm size (measured as the firm headcount), and a proxy for productivity, given by profits scaled by the number of employees. All the variables are measured in 2016, prior to the carbon price increase. For every treated worker, we select the worker with the closest propensity score as our matched control worker, with a maximum absolute difference in the propensity scores (caliper) of 0.025.

[16]As our data start in 2010, our measure of tenure is severely censored, which is why we do not use it as a continuous variable in our matching algorithm.

4.3 The Final Sample

Table 2 presents statistics on the full sample and on the matched sample obtained after employing our matching algorithm. All the variables are winsorized at the 1% level. After employing the filters described in Section 4.1, our starting sample includes about 1.8 million workers, of which the fraction of treated workers is 5.4%. The first three columns display remarkable differences between workers employed in ETS firms vs. those in non-ETS firms. Treated workers earn, on average, 34 log-points more, and are almost one year older. Their employers are almost three times as big and more than twice as profitable as the non-ETS firms. These differences underscore the need for the matching strategy described in Section 4.2.

Given the large imbalances, our conservative matching approach results in a meaningful reduction in the sample size, which however still includes 35,082 workers, equally split between the treatment and control groups, employed in 152 and 1,139 unique firms, respectively. Panel C of Table 1 shows the distribution across industries and firm size bins. As before, we present detailed data for the largest ten industries in our sample, and pool the others in the "Other" category. Of the top ten industries displayed in the full sample statistics of Table 1, six are among the top ten of our matched sample as well, again with a preponderance of the manufacturing sector. We lose, however, the air transport industry, given the difficulty in finding a suitable match. More than two thirds of the workers belong to large firms, a fraction that is somewhat lower than the one in the full sample.

As shown in Table 2, our matching approach is indeed successful in reducing the differences in key observable characteristics. In the matched sample, the differences in wages between treated and control workers is now just one log-point, and the firms of the treated workers are very similar in terms of size and profitability to the employers of control workers. Finally, the difference in average age is very close to zero.

4.4 Econometric Strategy

The purpose of our econometric approach is to analyze how the increase in carbon prices affects labor market outcomes in a matched difference-in-differences setting. We estimate the following difference-in-differences regression:

$$y_{it} = \beta ETS_j \times Post_t + \gamma_i + \delta_t + \epsilon_{it}, \tag{9}$$

where the subscripts i, j, and t index workers, plants, and years, respectively. ETS_j is a dummy equal to one if worker's i 2016 plant is in the treatment group, that is, if they are employed in an ETS plant as of 2016. $Post_t$ is a dummy equal to one after the carbon price increase has taken places, that is, from 2018 onward. γ_i and δ_t represent worker and year fixed effects, respectively. Finally, y_{it} represents an outcome of interest; typically the logarithm of hourly wages, but also hours worked, total earnings, and an employment dummy (i.e., a dummy equal to one if the worker is employed for at least one hour at any firm). Our analysis focuses on a symmetric 7-year window around the event year, and thus includes the years 2014–2020.

For most of our outcomes, we will also present an event-study version of equation (9), which reads as follows:

$$y_{it} = \sum_{\tau=-3}^{3} \beta_\tau ETS_j \times \mathbb{1}(t = t^* + \tau) + \gamma_i + \delta_t + \epsilon_{it}. \tag{10}$$

In this specification, $\mathbb{1}(t = t^* + \tau)$ is an indicator function equal to one if the year t is equal to $t^* + \tau$, where t^* is the event year (2017) and $\tau \in [-3, +3]$. The coefficients of interest are represented by the vector $\beta_{-3}, ..., \beta_{+3}$. Following standard practice, we normalize the coefficient β_{-1} to zero. In all the tests, standard errors are clustered at the 2016 employer level.

5 Results

5.1 Baseline Results

Table 3 shows how several labor market outcomes are affected by the increase in carbon prices in ETS firms. We focus on four outcomes. Our first outcome of interest, in column 1, is the hourly wage; we take the logarithm of this variable so that the coefficient β in equation (9) has a "semielasticity" interpretation. In column 2 our dependent variable is total earnings. The are two key differences relative to the hourly wage. First, while in the latter we only consider the income earned from the dominant employer, in the variable earnings we sum all the labor incomes earned by the worker, potentially from different employers, in a given year. Second, as we do not take the logarithm, we can also include unemployed workers, who are assigned earnings equal to zero. In column 3 we focus on the logarithm of wage, without scaling by hours. This allows us to gauge how much of the effect we detect on workers' income is due to changes in the intensive margin of the labor supply. Finally, in column 4 we study the effect of carbon price increases on the probability of being employed, with the dependent variable being a dummy equal to one if the worker earns nonzero labor income.

As shown in Table 3, outcomes of workers improve somewhat after the carbon price increase; however, the effects are small and insignificant. Specifically, treated workers experience a 1.8 percent increase in wages and hourly wages, and a 2-thousand euro increase in earnings. However, none of these coefficients is statistically significant. We find a statistically significant, but modest, increase in the employment probability for treated workers, equal to 0.6 percentage points.

These conclusions are confirmed by the plots of the event-study coefficients presented in Figure 4. Except for employment, there are no apparent differences in the post-carbon price increase evolution of the outcome variables. Importantly, we do not see different trends prior to the event year, which lends credibility to the parallel trends assumption.[17]

The lack of negative effects on labor market outcomes is not only consistent with much of the existing literature (Colmer et al., 2025), but is also in line with Dutch firms being somewhat better equipped to face the green transition than the average EU firm. In Appendix-Figure OA3, we plot the fraction of free permits granted to Dutch firms over the EU total throughout the period 2013–2020, together with the fraction of carbon emissions (and thus of used permits). After the carbon price hike, Dutch firms become, on average, net sellers of permits. As we show in the next Section, even within the Netherlands, workers benefit from being employed in firms that are net permit sellers.

5.2 Heterogeneity in the Permit Shortfall

Our simple theoretical framework sketched in Section 3 suggests that, *unconditionally*, the rise in carbon price has ambiguous effects on workers' wages; hence, the null results discussed in the previous section are not surprising. However, based on simple rent-sharing considerations, cash flow shocks due to the carbon price increase should be partially passed on to workers' wages. Whether the carbon price increase represents a positive or negative shock for the plant depends on its ability to reduce emissions. Firms that did not fully use their permits prior to the carbon price increase could accumulate them and, after the carbon price increase, sell them at a higher price. Thus, firms with a surplus of outstanding permits benefit from a *positive* cash flow shock. Conversely, firms that were systematically in need to buy more permits than they were granted, i.e., firms with a shortfall of permits, must now pay a much higher price to keep buying them.

Motivated by these considerations, we take the pre-event permit shortfall, normalized by the number of employees in the plant, as a measure of inefficiency, and estimate the

[17]Panel D of the figure shows a treatment effect of exactly zero, with no standard error, for the employment outcome in period $t^* - 2$. This is because we require all workers in our sample to be employed in periods $t^* - 2$ and $t^* - 1$, as explained earlier. This requirement also explains why the other outcomes are also much more tightly estimated for period $t^* - 2$ than for other periods.

following triple difference-in-differences regression:

$$
\begin{aligned}
y_{it} = \alpha ETS_j \times Post_t + \beta Shortfall_j \times Post_t + \theta Shortfall_j \times Post_t \times ETS_j \\
+\gamma_i + \delta_t + \epsilon_{it},
\end{aligned}
\tag{11}
$$

where $Shortfall_j$ is firm j's permit shortfall scaled by its number of employees. For every plant, we compute the 2010-2016 average to ensure that we are not capturing idiosyncratic fluctuations in emissions. As all the other continuous variables, it is winsorized at the 1% level. In the regression, we standardize this variable by subtracting the sample mean and divide it by the sample standard deviation, for ease of interpretation.[18] In this model the coefficient of interest is the one on the triple interaction term $Shortfall_j \times Post_t \times ETS_i$, namely θ. Figure 5 illustrates the distribution of the permit shortfall across firms in our sample, and also shows that shortfalls are quite persistent within firm over time.

We furthermore estimate an event-study version of equation (11), that is:

$$
\begin{aligned}
y_{it} = \sum_{\tau=-3}^{3} \alpha_\tau ETS_j \times \mathbb{1}(t = t^* + \tau) + \sum_{\tau=-3}^{3} \beta_\tau Shortfall_j \times \mathbb{1}(t = t^* + \tau) \\
+ \sum_{\tau=-3}^{3} \theta_\tau Shortfall_j \times ETS_i \times \mathbb{1}(t = t^* + \tau) + \gamma_i + \delta_t + \epsilon_{it}.
\end{aligned}
\tag{12}
$$

In this specification, we are of course interested in the vector of coefficients $\theta_{-3}, ..., \theta_{+3}$.

Table 4 shows that the level of shortfall prior to the carbon price hike is a strong predictor of the evolution of income and earnings. Workers employed in firms with a one-standard deviation lower shortfall experience a 1.8 percent rise in hourly wages and a 2.8 percent rise in wages, relative to workers employed at firms with average efficiency. In terms of earnings levels, these workers experience a differential 1,373-euro increase. Column 4 shows that employment prospects for workers in inefficient firms do not worsen; hence, the decline in earnings is solely driven by the wage effect. Finally, in Figure 6 we

[18] Note that the permit shortfall is of course not defined for the workers in the control firms. In order to keep using the full sample, we assign them the shortfall of the matched worker's employer.

plot the vector of coefficients $\theta_{-3}, ..., \theta_{+3}$ obtained after estimating equation (12). We find coefficients very close to zero for $\tau < 0$, and negative and typically significant for $\tau \geq 0$ for the wage and earnings outcomes.

5.3 Education and Labor Market Outcomes

An important advantage of our data is that we can observe the level of schooling of the workforce and that, for college graduates, we can precisely identify the subject of study. As discussed earlier, we expect STEM graduates to become especially valuable after the carbon price increase, as they can drive the transition to a cleaner production process.

In Table 5 we therefore present estimates of equation (9) where we distinguish between STEM university graduates and the remaining sample of workers.[19] The STEM graduates represent 7.6% of the sample, namely 2,653 workers, distributed approximately equally between ETS and control firms.

By comparing the coefficients in columns 1 and 2, we find that STEM graduates in ETS firms experience a significant increase in hourly wages, equal to 5 log points. Conversely, the effect remains small and insignificant in the remaining sample.

The effects are qualitatively similar but statistically weaker when we look at earnings (columns 3 and 4) and wage (columns 5 and 6), with STEM workers exhibiting improvements in both outcome measures. This lack of statistical power appears motivated by the fact that the labor market improvements occur gradually. Indeed, when we plot the event-study coefficients, we find that three years after the carbon price increase, there are meaningful gains in hourly wages (+6.9 percent), earnings (+5,813 euros), and wages (+6.6 percent), all significant at the 5 percent level (see Figure 8). Interestingly, we find a small positive effect for non-STEM workers on the employment probability (0.6 percentage points) and an insignificant one for STEM workers; however, the difference is neither significant nor economically large.

[19] To reduce the risk of endogeneity in career choices, we hold the education variable fixed at the end of 2016.

For completeness, at the bottom of the table, we also report differences between the coefficients β estimated in the two subsample, with corresponding standard errors. Given that the number of STEM graduates in our matched sample is relatively small, statistical power to reject the null hypothesis that the coefficients are identical is limited.

One concern, motivated by the evidence in the previous section, is that this evidence is merely capturing firm-level differences, as STEM graduates may be disproportionately concentrated in efficient firms, i.e., those with a low permit shortfall. However, this is unlikely to be the case, as the correlation between the STEM dummy and the shortfall measure is a mere -0.3%. More importantly, as the skill-level heterogeneity is independent of firm characteristics, we can run a triple difference-in-differences model by controlling for firm-year fixed effects, and comparing labor market trajectories of workers employed at the same firms but characterized by different skill levels. The last row of Table Table 5 shows the coefficient on the triple interaction coefficient ETS$\times$Post$\times$STEM; we find that it is consistently positive and significant, at the 1 percent level for the wage and hourly wage regressions, and at the 10 percent level for the earnings regressions. We also display the corresponding event-study coefficients in Figure 9.

5.4 STEM or College Premium?

While the increase in carbon price leads to an increase in wages for STEM workers, one may wonder whether it leads to broader changes in the "college premium." We investigate this question by studying the labor market outcomes of individuals with college degrees in different fields, or without college degrees. In Table 6, we see that the only group of workers who experience a comparable increase in both wages and hourly wages is represented by the college graduates in business, the most popular major in our sample. However, due to an (insignificant) drop in the employment probability, the effect on earnings is small and insignificant. For workers with other university degree, we find small and insignificant effects on wages; while the effect on earnings is relatively large, the large standard errors prevent strong inferences. Overall, while the evidence on STEM workers appears solid, it is unclear whether workers characterized by other university degrees experience marked improvements in their labor market outcomes relative to workers with lower levels of schooling.

We then turn to workers with lower educational achievements. We find no effects for workers with an applied university degree, whereas workers with a vocational degree experience a significant increase in earnings. While not especially large, this appears to be driven mainly by a small increase in the employment probability (+0.6 percentage points). Finally, other workers (those with at most a high school degree) see a meaningful increase in the employment probability (+1.4 pp) but no comparable effect on wages.

Motivated by prior evidence that managers can facilitate firms' adjustment to environmental regulation, in Table OA4 we explore whether the effects for business graduates and other workers are stronger among workers with relatively high pre-reform wage ranks, who may be more likely to perform managerial or supervisory roles. The wage effects for business graduates and the earnings and employment effects for other workers are larger among workers above the 75th percentile of the within-plant wage-rank distribution, although positive earnings and employment effects for other workers remain below this threshold. Because we do not directly observe workers' positions in the job hierarchy, we interpret these patterns as suggestive evidence that managerial or supervisory responsibilities may contribute to the observed effects.

5.5 STEM Workers, Energy Costs, and Carbon Emissions

The evidence above points toward a "STEM premium" emerging after the increase in carbon prices. Our hypothesis, as discussed in Section 3, is that workers with a STEM education are valuable in this setting because their skills help firms adjust production toward less carbon-intensive technologies. Such adjustment may operate through process optimization, energy management, input substitution, or the adoption and operation of cleaner technologies. Ideally, we would like to estimate the causal effect of increasing the STEM share of a plant's workforce on subsequent abatement. However, we do not have exogenous variation in the educational composition of the plant workforce. We therefore take a more indirect approach. To the extent that STEM workers facilitate such adjustment, plants with a higher pre-reform share of STEM workers should exhibit larger reductions in emissions and energy costs after the carbon price increase.

We exploit additional plant-level data from CBS to study this mechanism. These data contain detailed information on production at the plant level, including energy expenditures. We combine these data with plant-level carbon emissions from the EU ETS and estimate the following difference-in-differences specification:

$$y_{jt} = \beta Post_t \times Frac.\,STEM\,Workers_j + \alpha_j + \delta_{ts} + \varepsilon_{jt}, \tag{13}$$

where the subscripts j, t, and s index plants, years, and sectors, respectively. The dependent variable y_{jt} is either the logarithm of plant-level carbon emissions or the logarithm of energy costs. $Post_t$ is a dummy variable equal to one from 2018 onward, and $Frac.\,STEM\,Workers_j$ is the fraction of workers in plant j with a STEM university degree, measured in 2016, before the carbon price increase. We keep the STEM share fixed at its pre-reform value to avoid mechanically capturing endogenous workforce adjustments after the shock. We include plant fixed effects and industry-by-year fixed effects, thereby comparing plants within the same sector over time while absorbing time-invariant plant characteristics and sector-specific shocks. This design mirrors the worker-level analysis, where we explicitly account for industry differences in technology and exposure to the carbon transition.

As discussed in Section 4, information on education is not available for the universe of workers. Thus, we measure the STEM share using the subset of each plant's workforce for which educational attainment is observed. To reduce measurement error, we restrict the sample to plants for which education is observed for at least 50% of the workforce.

Standard errors are clustered at the firm level, allowing for common shocks across plants belonging to the same firm.

The results are reported in Table 7. We begin with carbon emissions, which provide the most direct test of the mechanism. In panel a of column 1, the coefficient on $Post \times Frac.STEM\,Workers$ is negative and statistically significant. The estimate implies that plants with a higher pre-reform share of STEM workers reduce emissions more after the carbon price increase. Quantitatively, using the pre-reform standard deviation of the STEM share, 0.054, the coefficient of -1.34 implies that a one-standard-deviation higher STEM share is associated with approximately a 7% larger reduction in emissions after the reform. The estimate remains very similar after controlling for pre-reform plant size and profitability interacted with the post dummy in column 2. Next, we ask whether the STEM share is simply proxying for a more educated workforce. To do so, we replace the fraction of STEM graduates with the fraction of workers with a business degree and the fraction with other university degrees in the next four columns. Suggesting that the post-reform reduction in emissions is consistently related to STEM skills rather than with university education per se, we do not find any significant effects for other education variables.

We then examine energy costs. This outcome is complementary to emissions because it captures one important margin through which plants may abate: reducing energy use or reorganizing production toward less energy-intensive processes. In panel b of column 3, we find that plants with a higher pre-reform STEM share also experience a larger decline in energy costs after the carbon price increase. The coefficient is economically meaningful: a one-standard-deviation higher STEM share is associated with roughly a 14% larger reduction in energy costs. The estimate remains negative, though somewhat smaller and less precisely estimated, after controlling for pre-reform size and profitability interacted with the post dummy. Similar to emissions, the alternative education shares are not significantly related to the post-reform decline in energy costs.

Lastly, we present the corresponding event-study evidence in Figure 7. We estimate

$$y_{jt} = \sum_{\tau=-3}^{+3} \beta_\tau \left[1(t = t^* + \tau) \times Frac.\, STEM\, Workers_j\right] + \alpha_j + \delta_{ts} + \varepsilon_{jt}, \tag{14}$$

where t^* denotes the event year and the coefficient for $\tau = -1$ is normalized to zero. The event-study coefficients show no evidence of differential pre-trends before the carbon price increase. The negative coefficients emerge only after the reform, consistent with the interpretation that STEM-intensive plants adjust their emissions and energy costs when the return to abatement rises. Overall, these results support the mechanism underlying the STEM wage premium. When carbon prices rise, plants with more STEM workers appear better able to reduce emissions and energy costs. This makes STEM workers more valuable to regulated firms, consistent with the worker-level evidence showing that STEM workers experience wage gains after the carbon price increase.

5.6 Green Jobs

Based on this evidence, the presence of STEM workers appears to be beneficial with regard to emissions reduction. To further investigate this link, we conduct a descriptive analysis, drawing on two surveys conducted by the CBS, namely the National Survey on Working Conditions (*Nationale Enquête Arbeidsomstandigheden*, or NEA) and the Labor Force Survey (*Enquête beroepsbevolking*, or EBB). Both are conducted every year across repeated cross-sections of workers and cover about 40,000–60,000 and 120,000–150,000 individuals, respectively. Crucially, while these surveys do not cover the entire workforce, they have information on workers' occupation, measured using the ISCO classification, which we link to a taxonomy of "green skills" released by the European Commission. For every occupation (classified using the 4-digit ISCO definition), we have a "green share" variable, a green skill intensity indicator at the occupation level measuring the fraction of green skills required for that occupation. A skill is classified as green if it is needed to reduce the impact of human activity on the environment.[20] Green skills include, for example, promoting sustainable energy and developing energy-saving concepts. Examples of occupations with a high green share are environmental engineers (0.49), insulation workers (0.20), and roofers (0.19). Occupation with a zero green share include, among others, telecommunications engineers, health services managers, and statistical, mathematical and related associate professionals.

We build an unbalanced panel of workers surveyed in the two surveys for the period 2014–2020, comprising 981,115 observations, and estimate the following regression:

$$Green\,Share_{it} = \beta STEM_i + \gamma_{tsang} + \varepsilon_{it}, \tag{15}$$

where *Green Share* is the fraction of green skills pertaining to an occupation, STEM is a dummy equal to one for workers with a STEM college degree, and γ is a year $\times$ industry $\times$ age category $\times$ size category $\times$ gender vector of fixed effects.[21] Standard errors are double-clustered at the occupation and worker level.[22]

In Table 8 we report estimates of the coefficient β. We start from a simpler specification, where we only control for year fixed effects. In column 1, we find that STEM workers are characterized by a higher level of green skill share, with a coefficient equal to 0.03, and significant at the 1% level. The coefficient drops somewhat, to 0.019, when we further saturate the specification with controls for industry, age, size, and gender, but remains precisely estimated and significant at the 1% level. This is an economically large effect, considering that *Green Share* has a sample mean of 0.028 and a sample standard

[20]The classification has been developed by the Public Employment Services (PES) Network Working Group on Taxonomies. The process follows three steps. First, every skill in the European classification of skills and occupations (ESCO) is classified as green or not using a machine learning algorithm and human validation. Second, every occupation is associated to a set of skills required to perform that particular job. Third, the green share variable is computed as the ratio of green skills to the total number of skills pertaining to each occupation. See European Commission (2025) for additional details.

[21]The NEA and EBB surveys classify firms' size in nine bins (1-4; 5-9; 10-19; 20-49; 50-99; 100-249; 250-499; 500-999; 1,000 or more employees) and workers' age in six classes (15-25; 25-35; 35-45; 45-55; 55-65; and 65-75 years old).

[22]While the vast majority of the workers appear in the sample only once, some workers are surveyed multiple times, which is why we cluster not only at the occupation but also at the individual level.

deviation of 0.053. To test whether these results simply reflect a higher propensity of college graduates to engage in green jobs, we replicate the same specification by replacing the STEM dummy with a "business degree" dummy or with an "other degree" dummy. In either case, we do not detect a significant effect. Finally, in Table OA5, we examine whether, prior to the carbon-price increase, workers in ETS and non-ETS firms sorted into occupations with different green-skill content. Within each education category and conditional on industry-by-year fixed effects, we find no statistically detectable association between ETS employment and occupational green share. This evidence alleviates the concern that our estimates reflect pre-existing differences in occupational greenness between workers in ETS and non-ETS firms and provides complementary support for the comparability of the treatment and control groups.

5.7 Outside Options

As discussed in the theoretical framework above, the carbon price increase should lead to an improvement in the outside options available to STEM workers. This would lead to in increase in compensation only to the extent that the worker is actually willing to take advantage of them, and thus can credibly threaten to leave the current employer to seek more remunerative opportunities.

We investigate this hypothesis in two ways. First, we define "movers" as workers who, prior to the carbon price increase, had engaged in at least one job-to-job transition since the first year of our data (2010), and non-movers as those that had not. We label this as an *ex ante* classification. As shown in Table 9, we find that the positive effect on wages and earnings among STEM college graduates is driven by movers; conversely, we find smaller and insignificant effects among workers without prior job-to-job transitions.

As an alternative, *ex post* classification, we define movers as those workers who leave their original employer *after* the carbon price increase. Non-movers are instead those who retain their 2016 employer. These are assumed to be employees who would have remained with their current employer regardless of the carbon price increase, net of selection on the retention margin. In the language of the randomized control trial literature, these workers are "never takers": to the extent that the carbon price increase leads to a boost in outside option, these workers are unable or unwilling to take advantage of these opportunities. Again, we find much larger effects for movers as opposed to non-movers. In these cases, the differences are also statistically significant at the 5% or 10% levels.[23]

5.8 Workforce Composition

Given the evidence on STEM workers' pay, and the fact that they appear to be valuable to effectively counteract the higher costs of emissions, our final analysis tests whether the increased demand of STEM workers by ETS firms leads to more of these workers getting hired. Alternatively, their supply may be fixed in the short run, which may explain their steep wage increases. To test this hypothesis, we move to a firm-level design and estimate:

$$Fraction\,STEM_{ft} = \beta ETS_f \times Post_t + \gamma_f + \delta_t + \epsilon_{ft}, \tag{16}$$

where f and t index firms and years, respectively. If the supply of high-skill workers is not fully inelastic, we expect a positive coefficient β; conversely, there will be no effect in short run, resulting in $\beta \approx 0$.

While this design is at the firm level, we can also move to a *worker-level* design, by asking whether ETS firms are more likely to hire and less likely to separate from STEM workers. That is, we estimate the following model:

$$STEM_{ift} = \beta ETS_f \times Post_t + \gamma_f + \delta_t + \epsilon_{it}, \tag{17}$$

where i index workers, either on the sample of new hires or on the sample of separated workers. We identify new hires by restricting the attention to workers at the firs job

[23]We do not display regressions with the employment dummy as the outcome, as this is mechanically set to one throughout the postevent period for the Stayers sample, making the comparison uninformative.

spell with firm f. Similarly, we identify separated workers by restricting the attention to workers at the last job spell with firm f.

As shown in Table 10, whether we look at the fraction of workers with a STEM degree (column 1) or at the propensity to hire or separate from a STEM worker (columns 2 and 3, respectively) we do not find effects, suggesting that an inelastic supply of STEM workers prevents firms from increasing their number.

6 Conclusion

In this paper, we examine the labor-market consequences of a policy-induced increase in carbon prices in the EU ETS. Leveraging confidential employer–employee matched data for the universe of Dutch workers, together with detailed plant-level production information, we isolate the causal effect of the 2017 EU ETS permit-supply reform through a matched difference-in-differences design. We document no adverse average wage or employment effects for workers in regulated firms, but we uncover pronounced heterogeneity shaped by both market design and worker skills. Workers in firms with large permit shortfalls experience wage losses, while those with STEM education, whose skills facilitate emission reductions, see sizable wage gains, particularly when they hold strong outside options. Plants with a higher share of STEM workers also achieve larger post-reform reductions in energy costs. Taken together, these findings reveal meaningful distributional consequences of carbon pricing: the incidence of a carbon-price shock depends critically on the allocation of free permits and on the skill composition of the workforce.

These results suggest important implications for climate-policy design. As additional countries are developing cap-and-trade systems and the European Union prepares to launch a second ETS, understanding the labor-market effects of carbon pricing becomes increasingly central. Moreover, carbon prices in existing systems remain well below levels consistent with net-zero pathways, suggesting that substantial increases are likely in the near future. Our results indicate that such increases need not impose broad labor-market costs, but they will generate significant distributional effects driven by both policy rules and skill scarcity. Designing carbon markets that account for these distributional patterns and ensuring that workers can acquire the skills necessary to contribute to low-carbon production, will be essential for achieving a fair and efficient transition to a decarbonized economy.

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7 Figures and Tables

Table 1

Workers' Distribution by Industry and Size: Full Sample

Panel A displays the number of workers satisfying the filters described in Section 4.1 for each size class-industry combination. Size is defined as firm employment as of 2016, and the three size classes are: [1, 49], [50, 249], and 250 or more employees. We present data for the 10 industries with the highest presence of workers in ETS firms, and pool the other workers in the "Other" category. Panel B presents, for each size class-industry combination, the percentage of workers in ETS firms. Panel C displays the number of workers in the matched sample, obtained using the procedure described in Section 4.2, for each size class-industry combination. As before, we present detailed data only for the 10 industries with the highest presence of workers in this sample.

Panel A. Number of Workers by Industry and Size Class

Industry \ Size	[1, 49]	[50, 249]	250+	Total
Air transport	103	133	12,414	12,650
Education	5,647	3,694	60,517	69,858
Electricity, gas, steam and air conditioning supply	488	1,229	9,948	11,665
Human health activities	16,686	6,458	109,364	132,508
Manufacture of basic metals	808	2,004	5,164	7,976
Manufacture of chemicals and chemical products	2,145	5,574	10,913	18,632
Manufacture of food products	4,934	13,366	21,367	39,667
Manufacture of motor vehicles, trailers and semi-trailers	1,107	1,945	6,032	9,084
Manufacture of other non-metallic mineral products	1,733	2,184	2,200	6,117
Manufacture of paper and paper products	804	2,829	1,875	5,508
Other	408,802	333,289	710,946	1,453,037
Total	443,257	372,705	950,740	1,766,702

Panel B. Percentage of Workers in ETS Firms by Industry and Size Class

Industry \ Size	[1, 49]	[50, 249]	250+	Total
Air transport	0	0	98.52	96.68
Education	0	0	20.30	17.59
Electricity, gas, steam and air conditioning supply	0	25.63	33.50	31.27
Human health activities	0	0	21.51	17.75
Manufacture of basic metals	2.48	0.60	81.04	52.87
Manufacture of chemicals and chemical products	0.09	13.83	51.70	34.43
Manufacture of food products	1.32	9.12	46.28	28.16
Manufacture of motor vehicles, trailers and semi-trailers	0	0	51.74	34.36
Manufacture of other non-metallic mineral products	3.29	12.18	56.95	25.76
Manufacture of paper and paper products	3.86	16.51	56.85	28.40
Other	0.03	0.41	2.00	1.08
Total	0.07	1.18	9.54	5.40

Workers' Distribution by Industry and Size: Matched Sample

This table displays the number of workers in the matched sample, obtained using the procedure described in Section 4.2, for each size class-industry combination. Size is defined as firm employment as of 2016, and the four size classes are: [0, 49], [50, 249], and 250 or more employees. We present data for the 10 industries with the highest presence of workers in ETS firms, and pool the other workers in the "Other" category.

Number of Workers by Industry and Firm Size Class After Matching

Industry	**[1, 49]**	**[50, 249]**	**250+**	**Total**
Crop and animal production, hunting and related service activities	134	221	271	626
Electricity, gas, steam and air conditioning supply	176	662	3,674	4,512
Manufacture of basic metals	50	130	472	652
Manufacture of basic pharmaceutical products and pharmaceutical preparations	33	242	1,431	1,706
Manufacture of chemicals and chemical products	239	1,733	6,778	8,750
Manufacture of food products	194	2,012	6,662	8,868
Manufacture of other non-metallic mineral products	231	635	990	1,856
Manufacture of paper and paper products	77	797	460	1,334
Warehousing, storage and support activities for transportation	64	425	1,791	2,280
Wholesale trade	44	156	1,590	1,790
Other (pooling non-top-10 industries)	196	814	1,698	2,708
Total	1,438	7,827	25,817	35,082

Table 2
Balance Tests

This table presents descriptive statistics for the full sample and for the matched sample of workers included in our dataset. Treated workers are those employed in an ETS plant; control workers are those not employed in an ETS plant. We only include workers for which we know the schooling level and that satisfy the filters described in Section 4.1. We present sample averages of age, two lags of the logarithm of hourly wages, the logarithm of firm size (given by the number of employees), and the ratio of profits (in thousand euros) over the number of workers. All the variables are measured in 2016 (with hourly wages measured in 2016 and 2015). We present sample averages separately for treated and control workers, as well as the sample difference, for both the full sample and the matched sample. Standard errors clustered at the firm level are displayed in parentheses. The matched sample is obtained after running a propensity score model, matching (exactly) on industry, gender, and a high-tenure (i.e., above the sample median) dummy, and approximately on the variables presented in the table, with a caliper for the difference in the propensity scores of 0.01.

	Full Sample			*Matched Sample*		
Variable	**Control**	**Treated**	**Difference**	**Control**	**Treated**	**Difference**
Age_{t-1}	40.261	41.196	0.934	42.476	42.495	0.019
	(0.105)	(0.317)	(0.333)	(0.353)	(0.268)	(0.443)
$\log(\text{Hourly Wage}_{t-1})$	3.019	3.354	0.335	3.358	3.371	0.013
	(0.008)	(0.016)	(0.018)	(0.046)	(0.030)	(0.055)
$\log(\text{Hourly Wage}_{t-2})$	2.965	3.293	0.328	3.310	3.323	0.013
	(0.009)	(0.015)	(0.018)	(0.046)	(0.029)	(0.055)
$\log(\text{Size}_{t-1})$	5.494	8.375	2.881	6.426	6.443	0.017
	(0.137)	(0.274)	(0.306)	(0.178)	(0.125)	(0.218)
$\text{Profits}_{t-1}/\text{Workers}_{t-1}$	20.170	45.403	25.233	90.652	86.894	-3.758
	(1.285)	(13.294)	(13.319)	(14.575)	(13.700)	(19.974)
Observations	1,670,567	95,441	1,766,008	17,541	17,541	35,082

Table 3
Baseline Results

This table presents coefficients obtained after estimating equation (9) on the sample of workers obtained with the matching procedure described in Section 4.2. ETS is a dummy equal to one if the worker is employed in an ETS firm in 2016. Post is a dummy equal to one from 2018 onward. The dependent variables are the logarithm of the hourly wage (column 1), total earnings (column 2), the logarithm of the wage (column 3), and a dummy equal to one if the worker receives nonzero labor income during the year (column 4). Standard errors clustered at the 2016 employer level are reported in parentheses. ***, **, and * indicate the 1%, 5%, and 10% level of significance, respectively.

Dep. Var.:	Log(Hr. Wage)	Earnings	Log(Wage)	Employed
	(1)	(2)	(3)	(4)
ETS × Post	0.018	2,105.471	0.019	0.006**
	(0.016)	(1,482.941)	(0.017)	(0.003)
Observations	240,047	245,574	240,047	245,574
Worker FE	X	X	X	X
Year FE	X	X	X	X

Table 4

Outstanding Permit Deficit and Workers' Outcomes

This table presents coefficients obtained after estimating equation (11) on the sample of workers obtained with the matching procedure described in Section 4.2. ETS is a dummy equal to one if the worker is employed in an ETS firm in 2016. Post is a dummy equal to one from 2018 onward. Emissions is defined as the outstanding deficit of permits as of 2016, divided by the number of employees in the ETS plant. For non ETS workers, the variable is defined as the value of the deficit for the matched treated worker. The variable is demeaned and standardized for ease of interpretation. The dependent variables are the logarithm of the hourly wage (column 1), total earnings (column 2), the logarithm of the wage (column 3), and a dummy equal to one if the worker receives nonzero labor income during the year (column 4). Standard errors clustered at the 2016 employer level are reported in parentheses. ***, **, and * indicate the 1%, 5%, and 10% level of significance, respectively.

Dep. Var.:	Log(Hr. Wage)	Earnings	Log(Wage)	Employed
	(1)	(2)	(3)	(4)
ETS × Post	0.018	2,105.471	0.019	0.006**
	(0.016)	(1,472.398)	(0.017)	(0.003)
Post × Shortfall (Standardized)	0.005	284.042	0.003	0.000
	(0.005)	(480.042)	(0.005)	(0.002)
ETS × Post × Shortfall	-0.018***	-1,372.780**	-0.028***	-0.003
	(0.006)	(560.164)	(0.009)	(0.002)
Observations	240,047	245,574	240,047	245,574
Worker FE	X	X	X	X
Year FE	X	X	X	X

Table 5
STEM and Non-STEM Workers

This table presents coefficients obtained after estimating equation (9) on the sample of workers obtained with the matching procedure described in Section 4.2. ETS is a dummy equal to one if the worker is employed in an ETS firm in 2016. Post is a dummy equal to one from 2018 onward. Columns 2, 4, 6, and 8 presents results obtained in the subsample of workers with a university STEM degree; the remaining columns present results obtained when running equation (9) on all the remaining workers. The dependent variables are the logarithm of the hourly wage (columns 1 and 2), total earnings (columns 3 and 4), the logarithm of the wage (columns 5 and 6), and a dummy equal to one if the worker receives nonzero labor income during the year (columns 7 and 8). Standard errors clustered at the 2016 employer level are reported in parentheses. ***, **, and * indicate the 1%, 5%, and 10% level of significance, respectively.

Dep. Var.:	Log(Hr. Wage)		Earnings		Log(Wage)		Employed	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ETS × Post	0.016	0.050**	1,981.220	3,533.814*	0.018	0.038	0.006**	0.005
	(0.017)	(0.024)	(1,492.998)	1,821.845	(0.017)	(0.024)	(0.003)	(0.006)
Observations	221,913	18,134	227,003	18,571	221,913	18,134	227,003	18,571
$\beta_{STEM} - \beta_{NO\,STEM}$	0.035		1,552.594		0.021		-0.001	
	(0.022)		(1,449.177)		(0.019)		(0.005)	
$\beta_{STEM} - \beta_{NO\,STEM}$	0.050**		2,420.570*		0.042***		-0.001	
(with Firm-Year FE)	(0.016)		(1,397.933)		(0.016)		(0.006)	
STEM	No	Yes	No	Yes	No	Yes	No	Yes

Table 6
Other Degrees

This table presents the coefficients β obtained after estimating equation (9) on the sample of workers obtained with the matching procedure described in Section 4.2. Each row refers to a different dependent variable. Going from top to bottom, they are: the logarithm of the hourly wage, total earnings, the logarithm of the wage, and a dummy equal to one if the worker receives nonzero labor income during the year. Every column refers to a different subsample of workers. Going from left to right, they are: workers with a university degree in business, with a university degree neither in business nor in STEM subjects, with an applied university degree, with a vocational degree, and others (i.e., with a high school degree or lower). Standard errors clustered at the 2016 employer level are reported in parentheses. ***, **, and * indicate the 1%, 5%, and 10% level of significance, respectively.

Sample / Dep. Var.	Business	Other Uni.	Applied Uni.	Vocational	Other
Log(Hr. Wage)	0.066*	0.020	0.017	0.012	0.009
	(0.034)	(0.023)	(0.018)	(0.014)	(0.012)
Earnings	3,098.606	3,430.825	1,376.268	1,811.948	2,147.397**
	(3,636.719)	(2,773.616)	(1,769.106)	(1,122.603)	(1,076.613)
Log(Wage)	0.061*	0.022	0.012	0.015	0.017
	(0.035)	(0.029)	(0.020)	(0.016)	(0.014)
Employed	-0.014	0.009	0.005	0.002	0.015***
	(0.009)	(0.009)	(0.005)	(0.003)	(0.005)
Observations	12,775	14,595	62,097	86,324	43,428

Table 7
STEM Workers, Emissions, and Energy Costs

This table presents coefficients obtained after estimating equation (13) on the sample of ETS plants for which total energy costs and carbon emissions are disclosed, and the schooling level of more than 50% of the plant's workforce is known. Panel A presents the results for plant-level carbon emissions, while Panel B presents the results for total energy costs. Post is a dummy equal to one from 2018 onward. Fraction STEM, Fraction Business and Fraction Other are the fractions of the plant's workforce with a STEM, business, or other university degree. Size is the number of employees of the firm. Profit/Worker is the ratio of a firm's profits to the number of the firm's workers. All the variables interacted with Post are measured in 2016. Standard errors clustered at the firm level are reported in parentheses. ***, **, and * indicate the 1%, 5%, and 10% level of significance, respectively.

Panel A

Dep. Var.:	Log(Emissions)					
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Frac. STEM	-1.447**	-1.669*				
	(0.731)	(0.886)				
Post × Frac. Bus.			-0.841	-0.676		
			(0.822)	(0.852)		
Post × Frac. Other					-1.353	-1.211
					(1.081)	(1.107)
Post × Log(Size)		-0.004		-0.017		-0.014
		(0.026)		(0.027)		(0.025)
Post × Profits/Workers		0.000		0.000		0.000
		(0.000)		(0.000)		(0.001)
R^2	0.985	0.985	0.985	0.985	0.985	0.985
Observations	721	721	721	721	721	721
Plant FE	X	X	X	X	X	X
Year-Ind. FE	X	X	X	X	X	X

Panel B

Dep. Var.:	Log(Energy Costs)					
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Frac. STEM	-2.566**	-1.725*				
	(1.131)	(0.982)				
Post × Frac. Bus.			-1.671	-0.637		
			(1.204)	(1.111)		
Post × Frac. Other					0.587	1.608
					(1.734)	(1.829)
Post × Log(Size)		-0.086*		-0.098*		-0.117**
		(0.049)		(0.054)		(0.055)
Post × Profits/Workers		0.000		0.000		0.000
		(0.001)		(0.001)		(0.001)
R^2	0.938	0.938	0.937	0.938	0.937	0.938
Observations	794	794	794	794	794	794
Plant FE	X	X	X	X	X	X
Year-Ind. FE	X	X	X	X	X	X

Table 8
STEM Workers and Green Jobs

This table presents coefficients obtained after estimating equation (15) on the sample of workers in either the EBB or NEA surveys run by the CBS. The dependent variable is the fraction of skills pertaining to an occupation that are considered as "green" by the European Commission. The independent variables are dummies equal to one if the worker has a STEM (columns 1 and 2), business (columns 3 and 4), or other college degree (columns 5 and 6). The regressions in columns 1, 3, and 5 control for year fixed effects. The regressions in columns 2, 4, and 6 control for year × industry × age category × size category × gender fixed effects. Standard errors double-clustered at the worker and 4-digit ISCO code are reported in parentheses. ***, **, and * indicate the 1%, 5%, and 10% level of significance, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
STEM	0.030***	0.019***				
	(0.010)	(0.006)				
Business			0.004	0.002		
			(0.005)	(0.003)		
Other					-0.009	-0.002
					(0.004)	(0.002)
R^2	0.009	0.274	0.000	0.271	0.003	0.271
Observations	981,115	981,115	981,115	981,115	981,115	981,115
Year FE	X		X		X	
Year × Industry × Gender × Size × Age FE		X		X		X

Table 9
Movers and Non-Movers

This table presents coefficients obtained after estimating equation (9) on the sample of workers obtained with the matching procedure described in Section 4.2 who have a STEM university degree. ETS is a dummy equal to one if the worker is employed in an ETS firm in 2016. Post is a dummy equal to one from 2018 onward. Columns 2, 4, 6, and 8 presents results obtained in the subsample of workers that have had at least one job-to-job transition between 2010 and 2016; the other columns include all the other workers. The dependent variables are the logarithm of the hourly wage (columns 1 and 2), total earnings (columns 3 and 4), the logarithm of the wage (columns 5 and 6), and a dummy equal to one if the worker receives nonzero labor income during the year (columns 7 and 8). Standard errors clustered at the 2016 employer level are reported in parentheses. ***, **, and * indicate the 1%, 5%, and 10% level of significance, respectively.

Panel A. Ex Ante Definition

Dep. Var.:	Log(Hr. Wage)		Earnings		Log(Wage)		Employed	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ETS × Post	0.026	0.077**	2,339.055	4,923.031***	0.026	0.062**	-0.005	0.010
	(0.021)	(0.035)	(2,577.594)	(1,867.189)	(0.026)	(0.028)	(0.009)	(0.008)
Observations	7,611	10,523	7,763	10,808	7,611	10,523	7,763	10,808
$\beta_{Mover} - \beta_{Non\,Mover}$	0.051		2,583.976		0.036		0.015	
	(0.034)		(2,633.434)		(0.030)		(0.013)	
Mover	No	Yes	No	Yes	No	Yes	No	Yes

Panel B. Ex Post Definition

Dep. Var.:	Log(Hr. Wage)		Earnings		Log(Wage)	
	(1)	(2)	(3)	(4)	(5)	(6)
ETS × Post	0.033	0.089**	542.463	10,176.575***	0.007	0.114***
	(0.023)	(0.036)	(1,850.971)	(4,501.059)	(0.022)	(0.048)
Observations	11,880	6,254	11,921	6,650	11,880	6,250
$\beta_{Mover} - \beta_{Non\,Mover}$	0.057**		9,634.213*		0.107**	
	(0.026)		(4,974.257)		(0.049)	
Mover	No	Yes	No	Yes	No	Yes

Table 10
Workforce Composition

This table presents coefficients obtained after estimating equations (16) (column 1) and (17) (columns 2 and 3) on the sample of firms obtained with the matching procedure described in Section 5.8. ETS is a dummy equal to one if the worker is employed in an ETS firm in 2016. Post is a dummy equal to one from 2018 onward. The dependent variables are the fraction of workers with a university degree in a STEM subject (column 1), and dummies equal to one if the worker has a STEM university degree. Columns 2 and 3 include only workers just hired and just separated, respectively. Standard errors clustered at the firm level are reported in parentheses. ***, **, and * indicate the 1%, 5%, and 10% level of significance, respectively.

Dep. Var.:	Fraction STEM	STEM Hire	STEM Separated
	(1)	(2)	(3)
ETS × Post	-0.002	0.008	0.002
	(0.002)	(0.007)	(0.006)
R^2	0.962	0.214	0.133
Obs.	1,288	236,083	247,456
Firm FE	X	X	X
Year FE	X	X	X

Figure 1
Emissions and Free Allowances

This figure illustrates the amount of emissions and free allowances across the years in the EU ETS.

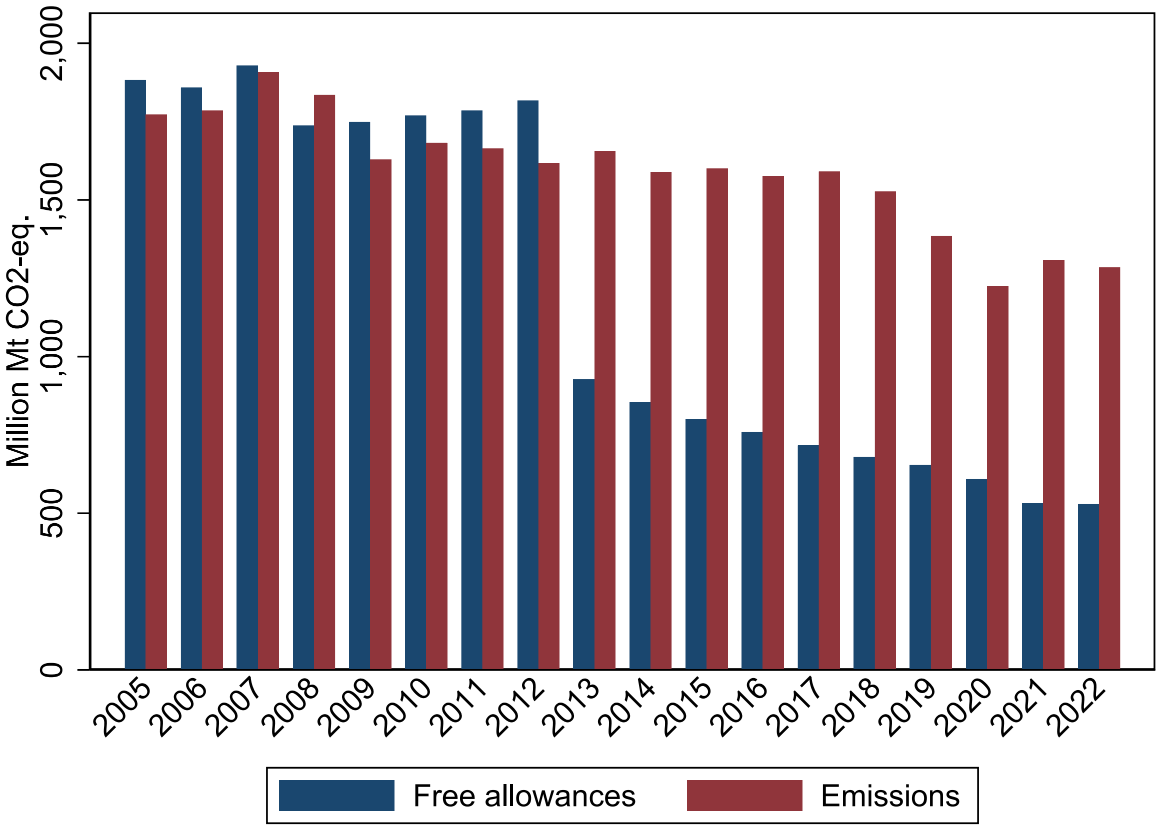


Figure 2
Timeline of key EU ETS milestones during Phase III

This figure illustrates the timeline of the 2017 reform in the EU ETS.

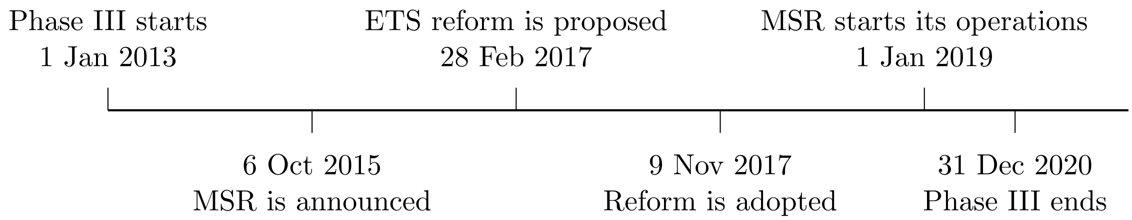


Figure 3
Allowance Prices in EU ETS

This figure shows the evolution of the allowance price (in euros) in the EU ETS. The two vertical lines denote February 28, 2017 (when the reform was proposed) and November 9, 2017 (when it was adopted). Source: European Energy Exchange (EEX), Emission Spot Primary Market Auction Reports.

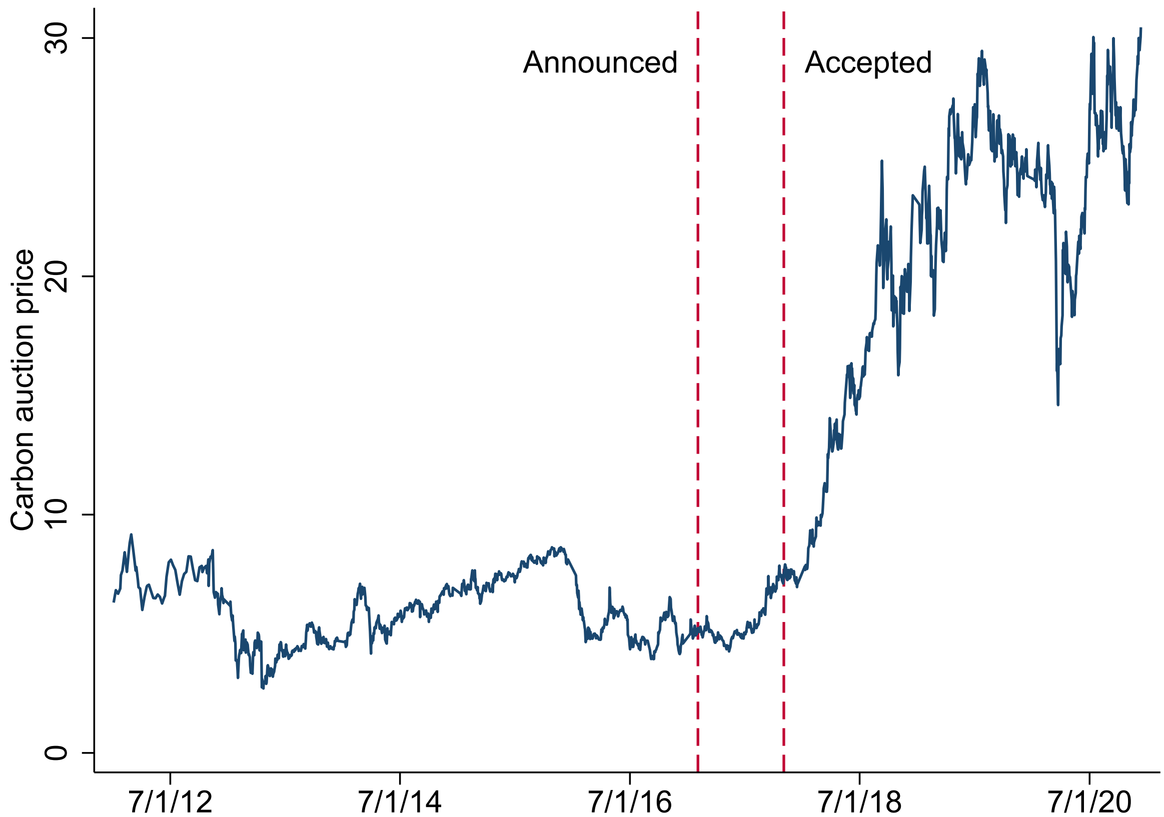


Figure 4
Baseline: Event-Study Evidence

This table presents the vector $\beta_{-3}, ..., \beta_{+3}$ of event-study coefficients obtained after estimating equation (10), together with 95% confidence intervals. In Panel A, the dependent variable is the logarithm of the hourly wage. In Panel B, it is total earnings. In Panel C it is the logarithm of the wage. In Panel D it is a dummy equal to one if the worker earns nonzero labor income. Standard errors are clustered at the 2016 employer level.

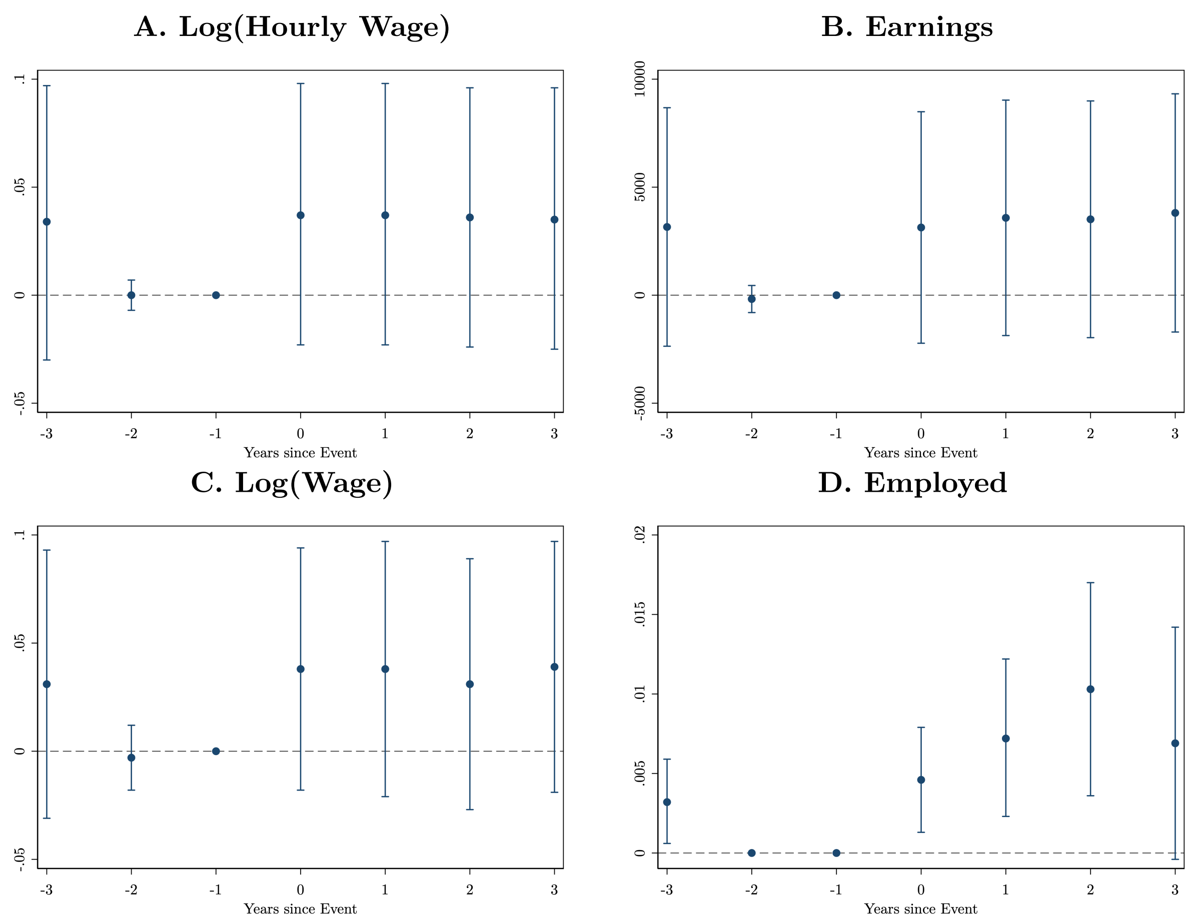
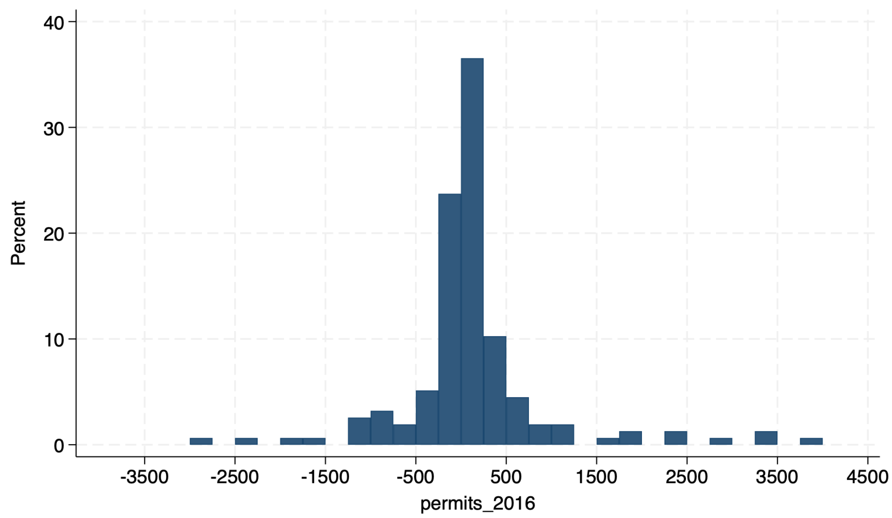


Figure 5
Permit Shortfall: 2016 and 2020

Panel A of this figure shows the distribution of plant-level permit shortfalls (with negative numbers denoting surpluses) in 2016. Panel B presents a binned scatter plot showing that within plant, shortfalls/surpluses are quite persistent across years. The $x-$axis displays the permit shortfall as of 2016, whereas the $y-$axis displays the shortfall accumulated in the four following years, between 2017 and 2020. The permit shortfall is scaled by the number of workers employed in the plant. The distributions are censored at -5,000 and +5,000 for ease of interpretation.

A. Distribution of Shortfall in 2016



B. Binscatter of 2020–2017 Shortfall against 2016 Shortfall

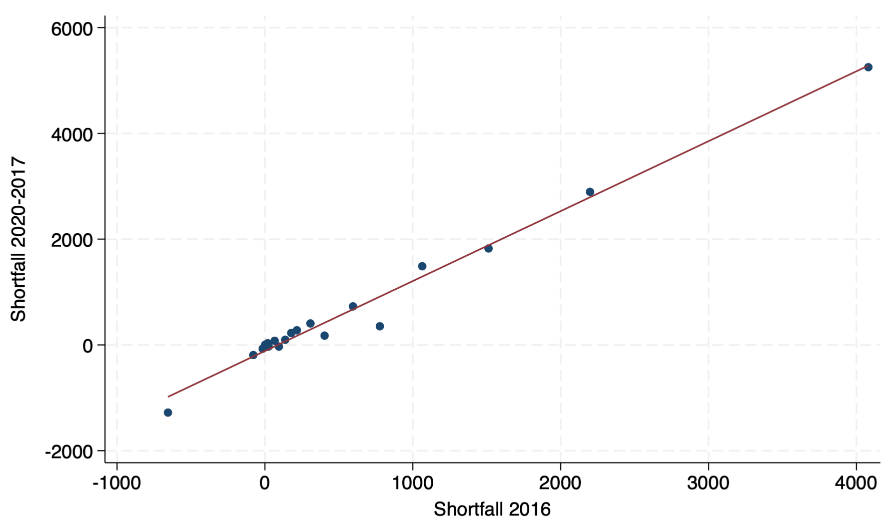


Figure 6

Permit Shortfall and Labor Market Outcomes: Event-Study Evidence

This figure presents the vector $\theta_{-3}, ..., \theta_{+3}$ of event-study coefficients obtained after estimating equation (12), together with 95% confidence intervals. In Panel A, the dependent variable is the logarithm of the hourly wage. In Panel B, it is total earnings. In Panel C it is the logarithm of the wage. In Panel D it is a dummy equal to one if the worker earns nonzero labor income. Standard errors are clustered at the 2016 employer level.

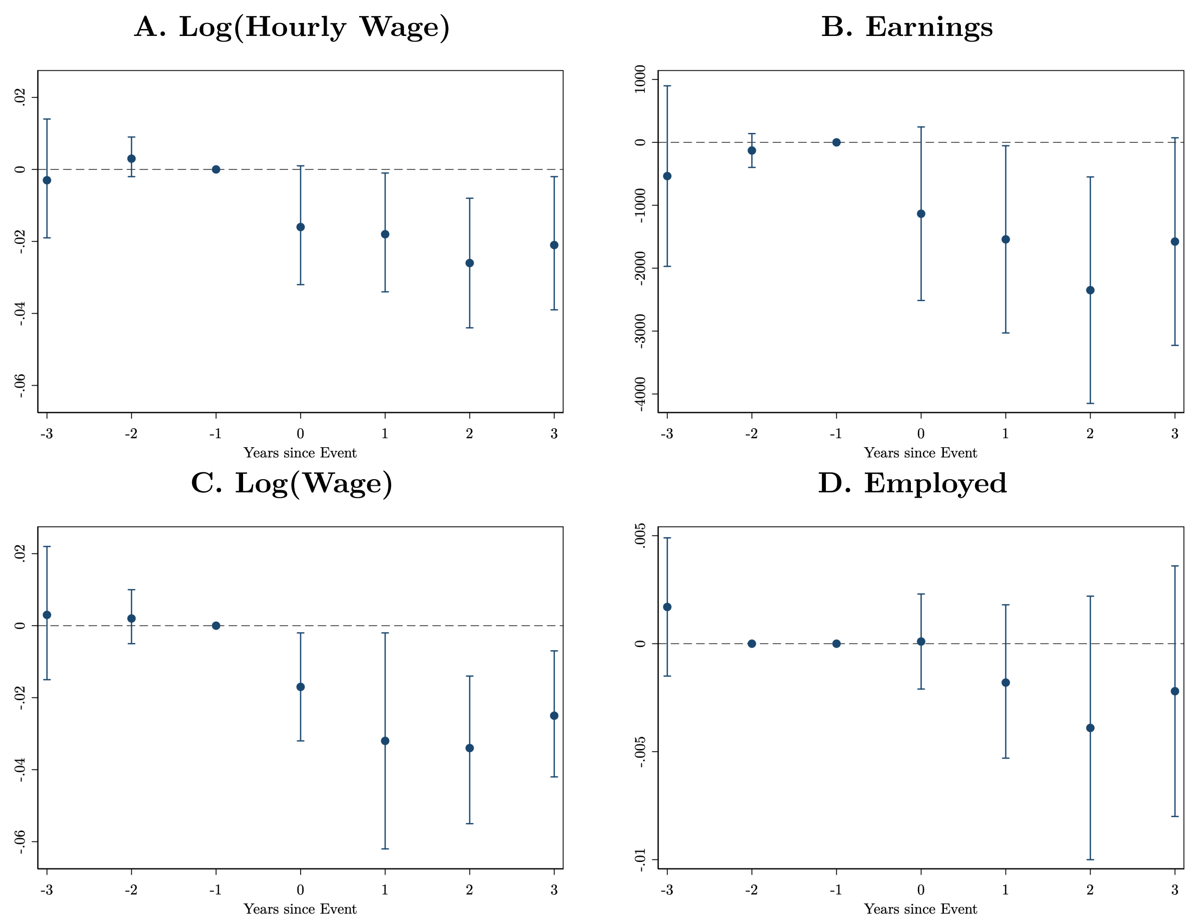
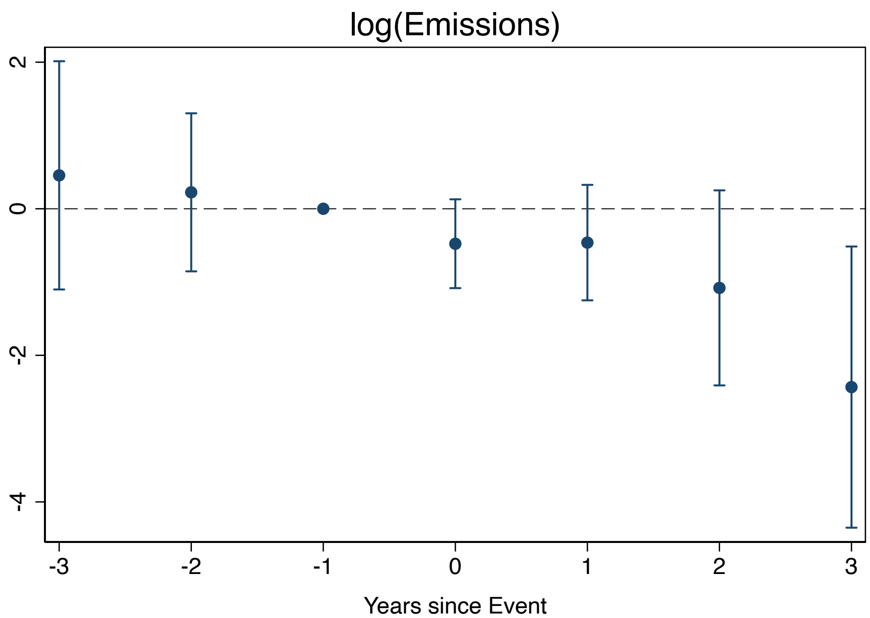


Figure 7
STEM Workers, Emissions, and Energy Costs

Panels in this figure presents the vector $\beta_{-3}, ..., \beta_{+3}$ of event-study coefficients obtained after estimating equation (14), together with 95% confidence intervals. The dependent variable in panel A is the logarithm of emissions. The dependent variable in panel A is the logarithm of energy costs. Standard errors are clustered at the firm level.

A. log(Emissions)



B. log(Energy costs)

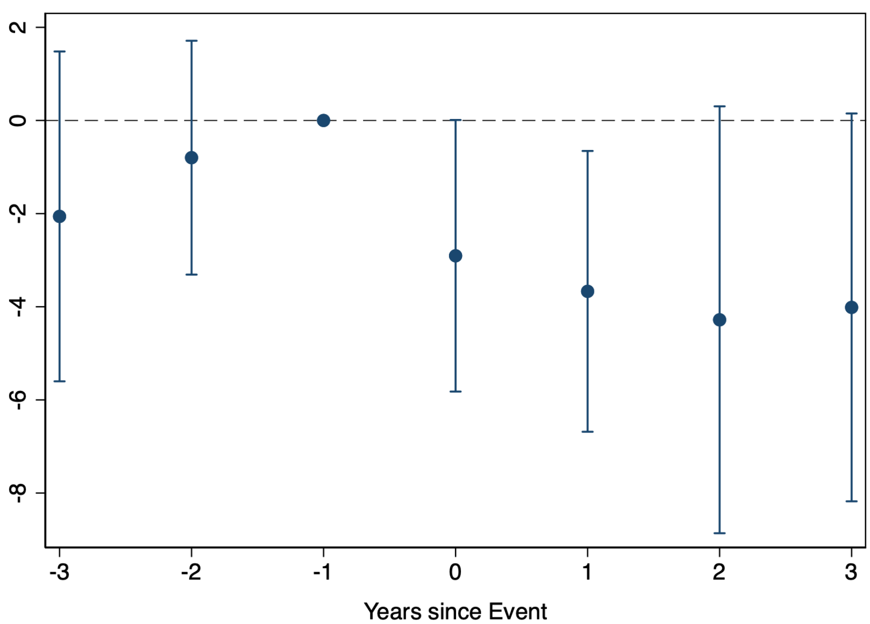


Figure 8
STEM Workers: Event-Study Evidence

This figure presents the vector $\beta_{-3}, ..., \beta_{+3}$ of event-study coefficients obtained after estimating equation (10), together with 95% confidence intervals. The sample includes only workers with a university degree in a STEM subject. In Panel A, the dependent variable is the logarithm of the hourly wage. In Panel B, it is total earnings. In Panel C it is the logarithm of the wage. In Panel D it is a dummy equal to one if the worker earns nonzero labor income. Standard errors are clustered at the 2016 employer level.

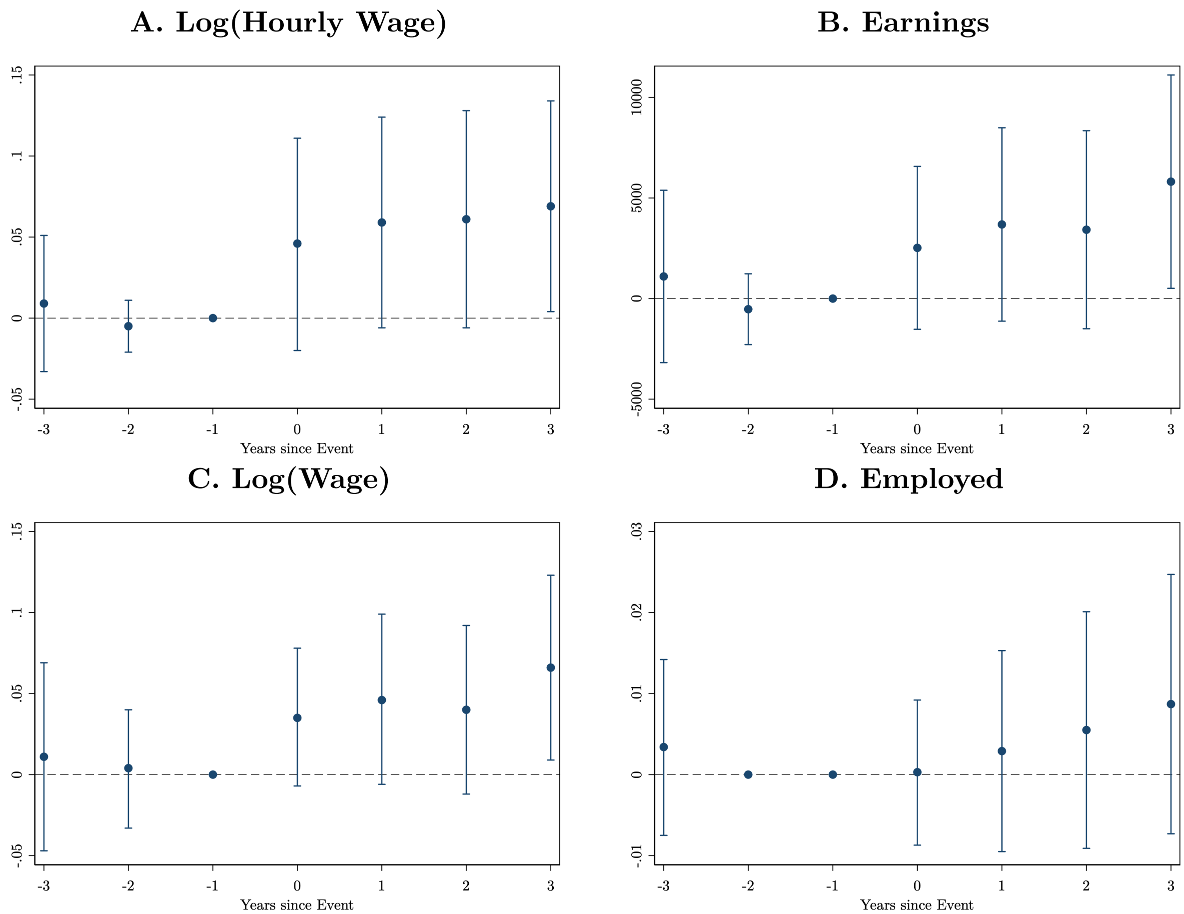
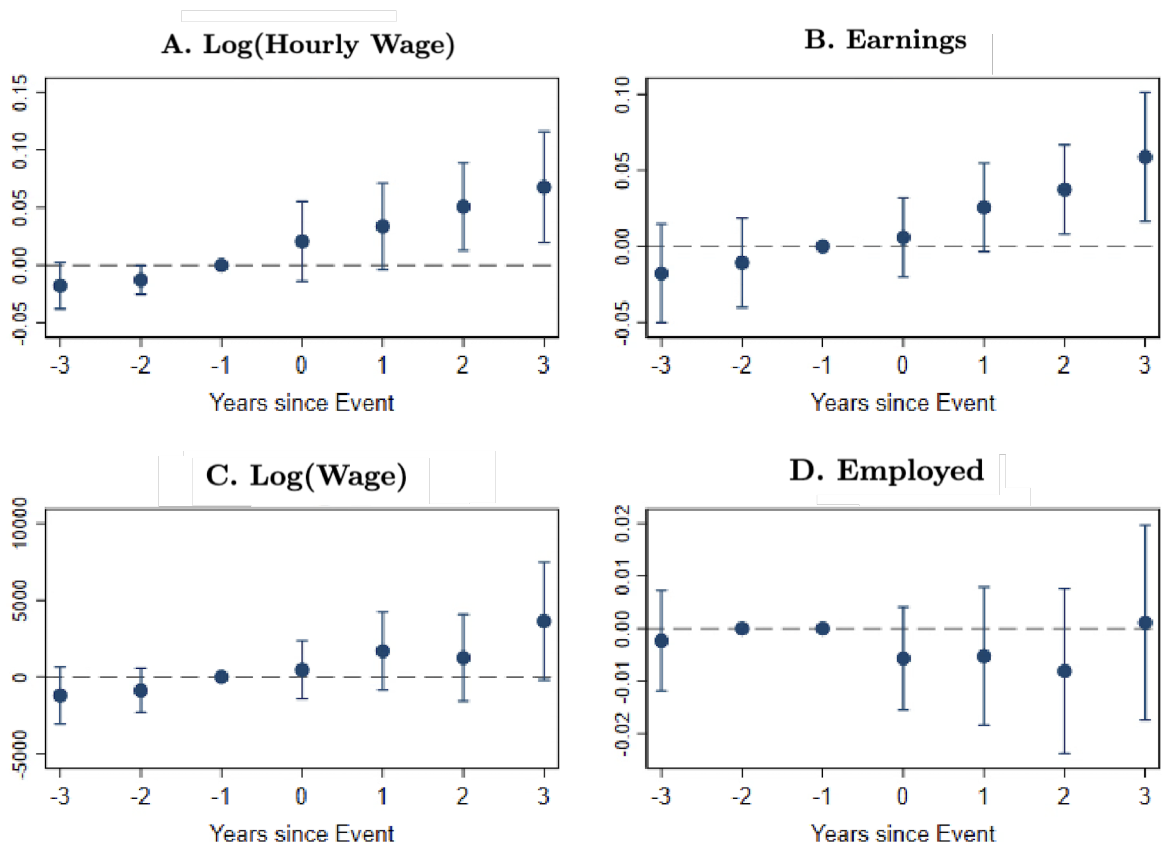


Figure 9

STEM and non-STEM Workers: Triple Difference-in-Differences Model

This figure presents event-study coefficients from a triple difference-in-differences in model. We plot the coefficients associated with the interaction between the ETS dummy, the STEM dummy, and year dummies. In Panel A, the dependent variable is the logarithm of the hourly wage. In Panel B, it is total earnings. In Panel C it is the logarithm of the wage. In Panel D it is a dummy equal to one if the worker earns nonzero labor income. All the regressions control for firm-year fixed effects, in addition to worker fixed effects. Standard errors are clustered at the 2016 employer level.



Online Appendix for
"The Labor Market Effects of Carbon Pricing"

Andreas Fuster   Gazi Kabaş   Vincenzo Pezone   Kasper Roszbach

Table OA1
Carbon Leakage and Linear Reduction Factors

This table presents the carbon leakage factor in panel (A), and the linear reduction factor in panel (B), used in the free allocation formula (Equation 1).

A: Carbon leakage factor

	2013	2014	2015	2016	2017	2018	2019	2020
Electricity producers	0%	0%	0%	0%	0%	0%	0%	0%
Sectors exposed to carbon leakage	100%	100%	100%	100%	100%	100%	100%	100%
Others	80.0%	72.9%	65.7%	58.6%	51.4%	44.2%	37.1%	30.0%

B: Linear reduction factor

	2013	2014	2015	2016	2017	2018	2019	2020
Electricity producers	100.0%	98.3%	96.5%	94.8%	93.0%	91.3%	89.6%	87.8%
Non-electricity producers	94.3%	92.5%	90.8%	89.1%	87.3%	85.6%	83.8%	82.1%

Table OA2

Outstanding Permit Deficit and Workers' Outcomes: Alternative Matching Strategies

This table presents versions of Table 4 estimated on samples obtained with different matching approaches. In Panel A and B, we use maximum calipers of 0.01 and 0.05, respectively. In Panel B we match on a single lag of the logarithm of the hourly wage.

Dep. Var.:	Log(Hr. Wage)	Earnings	Log(Wage)	Employed
	(1)	(2)	(3)	(4)
		Caliper = 0.01		
ETS × Post × Shortfall	-0.017***	-1,376.104**	-0.027***	-0.004*
	(0.006)	(557.376)	(0.009)	(0.002)
		Caliper = 0.05		
ETS × Post × Shortfall	-0.018***	-1,396.557**	-0.028***	-0.004*
	(0.006)	(555.575)	(0.009)	(0.002)
		Matching on One Lag of log(Wage)		
ETS × Post × Shortfall	-0.016***	-1,197.634**	-0.025***	-0.001
	(0.006)	(494.427)	(0.009)	(0.001)

Table OA3

STEM and Non-STEM Workers – Alternative Matching Approaches

This table presents versions of Table 5 estimated on samples obtained with different matching approaches. In Panel A and B, we use maximum calipers of 0.01 and 0.05, respectively. In Panel B we match on a single lag of the logarithm of the hourly wage.

Dep. Var.:	Log(Hr. Wage)		Earnings		Log(Wage)		Employed	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Caliper = 0.01								
ETS × Post	0.014	0.050**	1,928.256	3,623.466**	0.016	0.040*	0.007**	0.005
	(0.016)	(0.023)	(1,448.119)	(1,818.903)	(0.017)	(0.023)	(0.003)	(0.006)
$\beta_{STEM} - \beta_{NO\,STEM}$	0.036*		1,695.210		0.024		-0.002	
	(0.021)		(1,439.930)		(0.020)		(0.006)	
$\beta_{STEM} - \beta_{NO\,STEM}$	0.053***		2,597.749*		0.046***		-0.001	
(with Firm-Year FE)	(0.016)		(1,398.524)		(0.016)		(0.007)	
B. Caliper = 0.05								
ETS × Post	0.015	0.052**	2,095.908	3,530.771*	0.018	0.038	0.008***	0.006
	(0.016)	(0.023)	(1,481.447)	(1,792.264)	(0.017)	(0.023)	(0.003)	(0.006)
$\beta_{STEM} - \beta_{NO\,STEM}$	0.037*		1,434.862		0.021		-0.002	
	(0.021)		(1,438.717)		(0.019)		(0.006)	
$\beta_{STEM} - \beta_{NO\,STEM}$	0.053***		2,277.849*		0.043***		-0.003	
(with Firm-Year FE)	(0.016)		(1,376.475)		(0.016)		(0.007)	
C. Matching on One Lag of log(Wage)								
ETS × Post	0.013	0.049**	1,860.493	3,756.621**	0.018	0.036	0.008***	-0.002
	(0.016)	(0.024)	(1,413.176)	(1,844.689)	(0.017)	(0.023)	(0.003)	(0.006)
$\beta_{STEM} - \beta_{NO\,STEM}$	0.036		1,896.128		0.019		-0.010	
	(0.022)		(1,510.617)		(0.020)		(0.006)	
$\beta_{STEM} - \beta_{NO\,STEM}$	0.057***		2,692.992*		0.040***		-0.011	
(with Firm-Year FE)	(0.015)		(1,409.883)		(0.015)		(0.007)	
STEM	No	Yes	No	Yes	No	Yes	No	Yes

Table OA4

Other Degrees This table presents the coefficients β obtained after estimating equation (9) on the sample of workers obtained with the matching procedure described in Section 4.2. Each row refers to a different dependent variable. Going from top to bottom, they are: the logarithm of the hourly wage, total earnings, the logarithm of the wage, and a dummy equal to one if the worker receives nonzero labor income during the year. Every column refers to a different subsample of workers. Going from left to right, they are: workers with a university degree in business, with a university degree neither in business nor in STEM subjects, with an applied university degree, with a vocational degree, and others (i.e., with a high school degree or lower). Panel A reports the estimates for samples of workers whose wage ranks are above 75^{th} percentile. Panel B reports the estimates for samples of workers whose wage ranks are above 75^{th} percentile. Ranks are calculated within plants and education by using 2016 wages. For other category, plant-level ranks are used. Standard errors clustered at the 2016 employer level are reported in parentheses. ***, **, and * indicate the 1%, 5%, and 10% level of significance, respectively.

Panel A: Wage Rank above 75^{th} percentile

Dep. Var. \ Sample	Business	Other Uni.	Applied Uni.	Vocational	Other
Log(Hr. Wage)	0.087**	0.013	-0.001	-0.001	0.014
	(0.027)	(0.031)	(0.017)	(0.015)	(0.018)
Earnings	5,887.878	473.396	781.913	2,430.831	4,322.291**
	(6,224.783)	(5,893.304)	(2,416.409)	(1,496.423)	(2,010.405)
Log(Wage)	0.099**	0.001	0.012	0.019	0.032*
	(0.049)	(0.046)	(0.022)	(0.018)	(0.020)
Employed	-0.023	-0.018	0.005	0.010**	0.030***
	(0.015)	(0.021)	(0.005)	(0.004)	(0.010)
Observations	3,737	4,580	18,920	25,476	7,460

Panel B: Wage Rank below 75^{th} percentile

Dep. Var. \ Sample	Business	Other Uni.	Applied Uni.	Vocational	Other
Log(Hr. Wage)	0.041	0.023	0.014	0.016	0.011
	(0.033)	(0.025)	(0.022)	(0.014)	(0.012)
Earnings	813.319	1,974.071	1,563.022	1,563.022	1,680.375*
	(2,934.171)	(1,843.683)	(1,694.329)	(978.183)	(987.949)
Log(Wage)	0.023	0.015	0.005	0.012	0.011
	(0.035)	(0.024)	(0.022)	(0.016)	(0.015)
Employed	-0.011	0.001	0.005	-0.000	0.013**
	(0.011)	(0.010)	(0.004)	(0.004)	(0.006)
Observations	9,192	11,186	46,374	59,380	35,968

Table OA5
ETS Firms and Green Jobs by Education

This table presents coefficients obtained after estimating equation $Green\, Share_{it} = \beta ETS_j + \gamma_{jt} + \varepsilon_{it}$ on the sample of workers in either the EBB or NEA surveys run by the CBS. The dependent variable is the fraction of skills pertaining to an occupation that are considered as "green" by the European Commission. The independent variables are dummies equal to one if the worker works in an ETS firm. Column 1 uses STEM workers. Column 2 uses business workers. Column 3 uses workers with other educational backgrounds. The regressions include industry × year fixed effects. The sample period is between 2014 and 2017. Standard errors double-clustered at the worker and 4-digit ISCO code are reported in parentheses. ***, **, and * indicate the 1%, 5%, and 10% level of significance, respectively.

Sample:	STEM	Business	Other
	(1)	(2)	(3)
ETS	0.001	-0.002	-0.000
	(0.004)	(0.002)	(0.001)
R^2	0.202	0.064	0.150
Observations	26734	26553	95255
Industry × Year FE	X	X	X

Figure OA1
Benchmark parameter

This figure illustrates an example for benchmark parameter in the free allocation formula (Equation 1) by using bottles and jars of colourless glass. The x-axis shows the percentiles of installations in the EU ETS. The y-axis shows the GHG emission intensity of these installations in 2016 and 2017. The redline is the level of benchmark for colorless glass production, which is the mean of the most efficient 10 percent installations during the Phase II. Source: "Update of Benchmark Values for the Years 2021-2025 of Phase 4 of the EU ETS" document published by the European Commission (link).

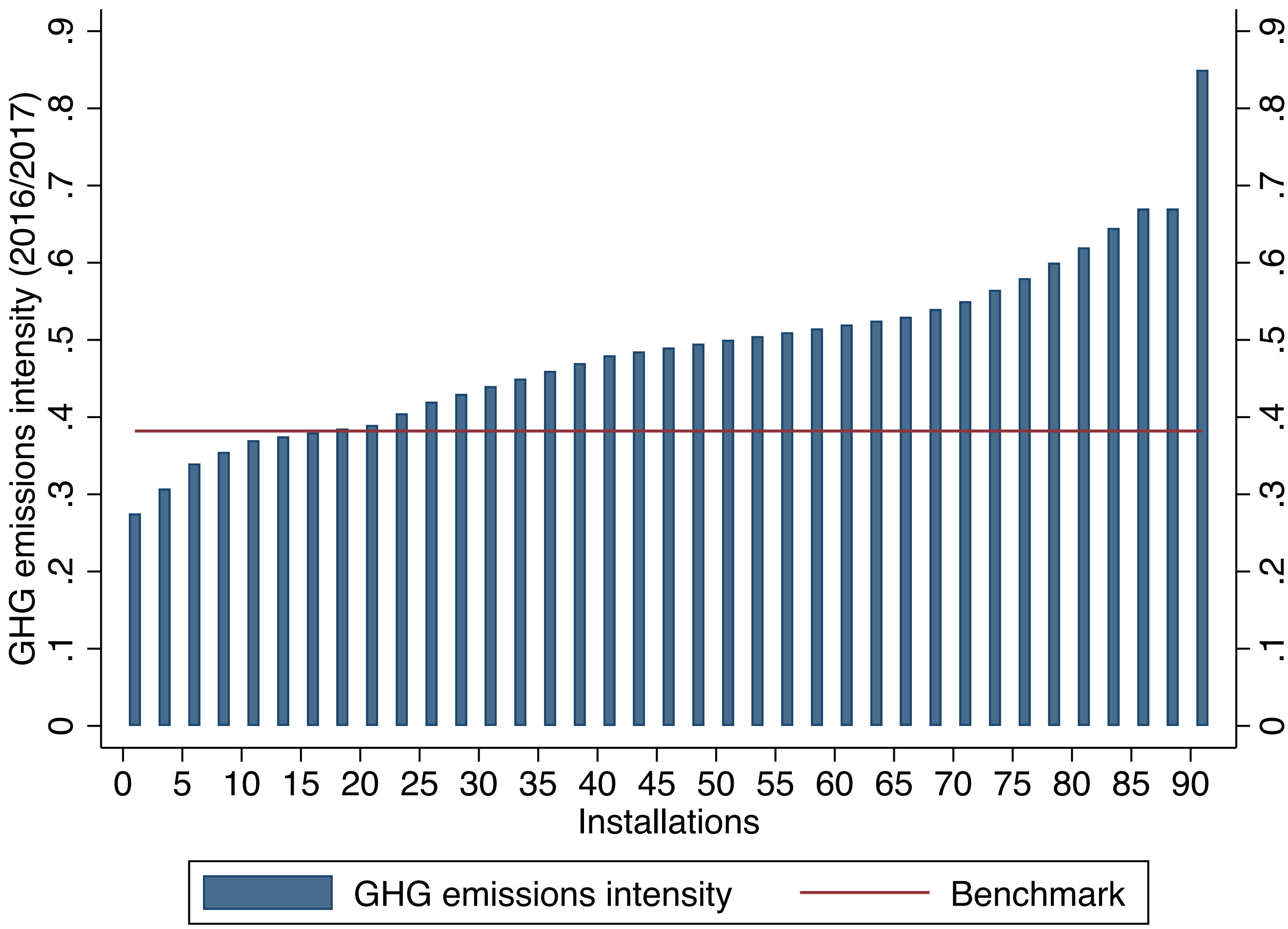


Figure OA2

Total Number of Allowances in Circulation and Market Stability Reserve

This figure illustrates the amount of total number of allowances in circulation (TNAC), the allowances in the Market Stability Reserve (MSR), and the amount of allowances invalidated by the MSR across the years in the EU ETS.

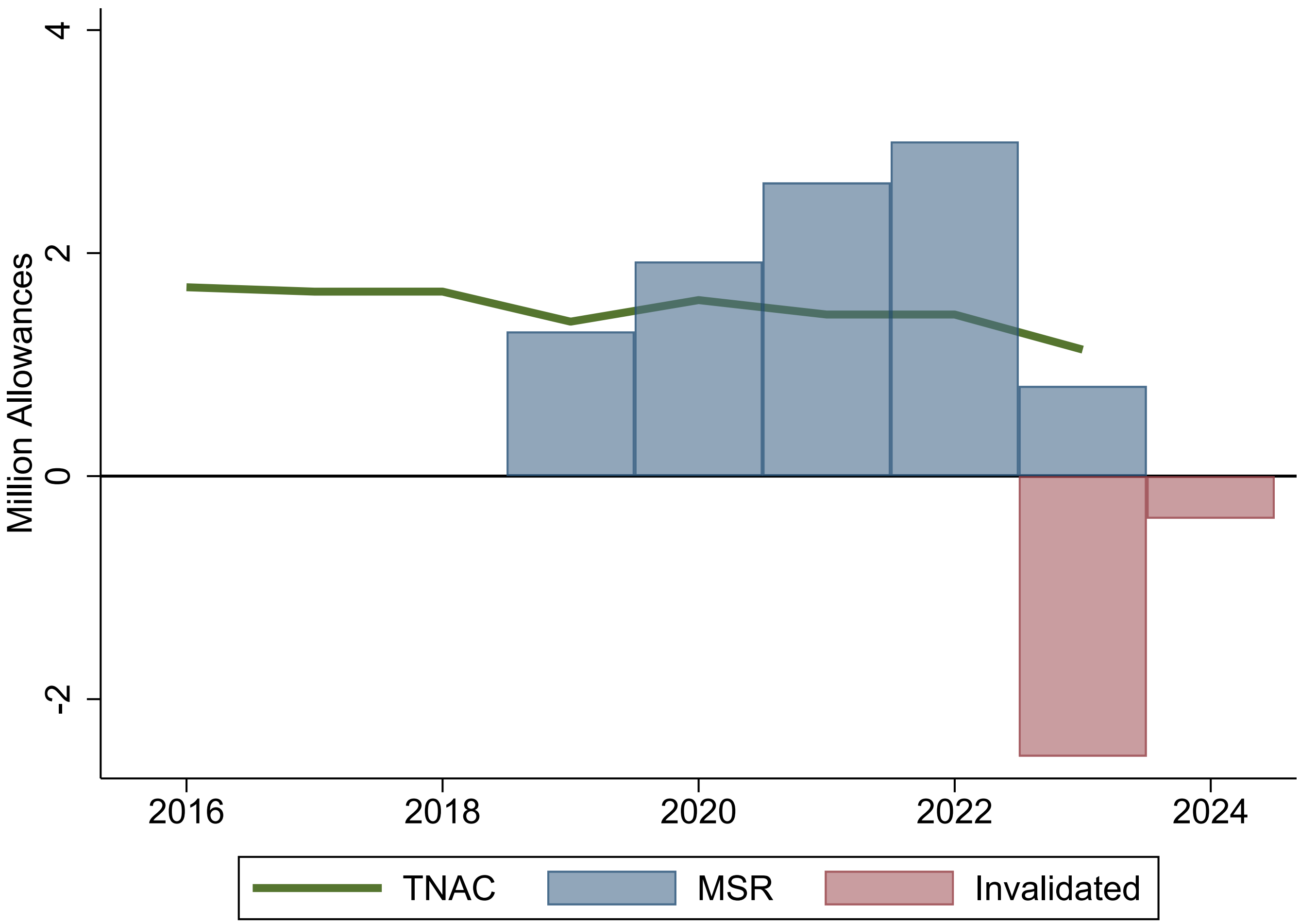


Figure OA3
Share of Free Permits and Emissions of Dutch Firms

This figure shows the percenrage of free permits allocated to Dutch firms (green solid line) and the fraction of carbon emissions, and thus actual permits used (black dashed line).