Carbon Pricing, Gender, and Occupations: Firm-Level Evidence from Kazakhstan

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Abstract: This paper presents the first evidence of gendered impacts of carbon pricing on labor market opportunities within energy-intensive firms. We explore the effects of an emissions trading system (ETS) that develops amid historical gender occupational discrimination, prevalent among resource-rich countries whose policies are essential for limiting future emissions growth. Our study uses the restoration of Kazakhstan's emissions trading system (ETS) after a two-year suspension as a natural experiment for difference-in-differences analysis. We assess the impact of carbon pricing on employment and wages across gender and occupation. The firms most constrained by the restored ETS experienced an 8% larger reduction in female employment, primarily among industrial workers. Employment decreases were driven by increased job destruction rather than reduced job creation. Concurrently, relative wages for female industrial workers decreased by 7%, while those for female administrative workers increased by 5%. Our results suggest a fall in relative demand for female industrial workers, consistent with skill bias of higher cost pressures under historical gender discrimination and the failure of the evolving ETS to promote green technology adoption that would leverage women's skills. Our findings underscore the necessity of accelerating social reforms to reduce negative impacts on vulnerable populations while developing carbon pricing policies.

Keywords: carbon pricing; ETS; just transition; gender; occupations; gross job flows

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1. Introduction

Understanding carbon pricing's impact on women's work opportunities in resource-rich countries is essential for a just transition, yet this topic suffers from a thin evidence base. The environmental economics literature recognizes the urgency of decarbonization strategies for low- and middle-income countries (LMICs) with the highest growth in business-as-usual emissions (Caucheteux et al., 2025). It also highlights the crucial role of women's extractive sector jobs for promoting gender equality in resource-rich countries (Baum & Benshaul-Tolonen, 2021) and the importance of carbon pricing's distributional implications for labor income (Shang, 2023). Despite these insights, no study has examined the gender-specific effects of carbon pricing on labor market opportunities within energy-intensive firms. This omission is striking given the considerable policy attention devoted to the gender-environment nexus by business leaders (Gloor et al., 2022) and international organizations (Brixi et al., 2023; Deininger et al., 2023; OECD, 2023; UNDP & GEF, 2023) in their pursuit of the United Nations (UN) Sustainable Development Goals. In the context of challenges specific to LMICs, there is need “for research on employment and skills in relation to a just transition” (Caucheteux et al., 2025, p. 81).

Theory suggests that the impact of carbon pricing on women’s labor market outcomes will vary by occupation and institutional context, with the most adverse consequences for female industrial workers in countries where regulations limit firm price flexibility. Drawing on prior literature, we focus on two competing mechanisms that influence labor demand for female workers. Carbon pricing can increase demand for female workers with skills suited to green tasks, especially if there is success in promoting green technology adoption. Alternatively, carbon pricing can create cost pressures that reduce demand for women's labor in occupations where they have historically faced discrimination, especially if firms continue to use legacy technologies. Which force dominates depends on whether policy effectively allows carbon costs to pass through into prices, thereby creating appropriate incentives for consumer purchases and firm production plans. Energy security regulations that hinder carbon cost pass-through are common in LMICs (de Gouvello et al., 2020), which broadly face challenges in tailoring carbon policies to their specific institutional and socioeconomic contexts (Caucheteux et al., 2025), so disadvantaged groups like women may be especially vulnerable to unintended consequences from carbon pricing policies. Adverse outcomes are likely to be strongest for industrial occupations, where women often encounter explicit prohibitions in LMICs (World Bank, 2024). Identifying adverse employment effects of carbon pricing on women would highlight the need for concurrent targeted reforms that address barriers for vulnerable groups, ensuring a just green transition.

We provide new causal evidence on the gendered labor market impacts of carbon pricing in energy-intensive firms of a resource-rich middle-income country. Our study uses rich firm-level microdata and a natural experiment: the 2018 reintroduction of an emissions trading system (ETS) in Kazakhstan (KazETS), a carbon-pricing pioneer among resource-rich economies. The reintroduction, coupled with low ETS liquidity and low firm pricing flexibility, generated quasi-random variation in firm-level pressures to reduce emissions. We leverage this variation in a difference-in-differences (DiD) framework to identify the causal impact of ETS constraints on women's and men's employment and wages. Our dataset enables gender-disaggregated analysis of labor demand and supply across occupations in 113 energy-intensive firms covered by Kazakhstan's ETS. Using this data, we can overcome a gender data gap elsewhere that limits the ability to examine women's progress (UN Women, 2018; Gloor et al., 2022). We use a causal research design in a context with several features that the literature has identified as creating challenges for LMIC green policy implementation: high-carbon industries, limited state capacity, underdeveloped financial markets, reliance on coal energy, political constraints, and labor market distortions (Harrison et al., 2017; Caucheteux et al., 2025).

To capture firm-level variation in ETS treatment intensity, we construct a proxy for emissions constraints resulting from the ETS's 2018 reintroduction. Our measure focuses on the *grandparenting* free quota allocation method for the 2018-2020 KazETS third phase, under which firms could set quotas based on their cumulative 2013-2015 CO_2 emissions. Using firm-level balance sheet data, we identify firms whose energy intensity in 2018 exceeded their 2013-2015 average. These firms' emissions likely surpassed the historical baseline used to allocate free quotas, triggering a need to either reduce emissions or acquire more allowances. We use this firm-level indicator as a proxy for binding ETS constraints, capturing variation in firms' exposure to carbon pricing pressures. Firms meeting this criterion are classified as ETS-binding (treatment group), and the remaining firms are classified as ETS-nonbinding (control group). The two groups are balanced across key observables, including gender- and occupation-disaggregated employment shares and wage levels. Event study plots for gender-disaggregated hiring differences between treatment and control groups are consistent with the parallel trends assumption. We address non-random assignment concerns through trimming by firm size, propensity score matching, and placebo tests.

Importantly for our identification, weak enforcement of the KazETS's first two phases reduced firms' incentive to alter operations in advance of the third phase in 2018. Following the first two phases, firms negotiated penalty waivers due to Kazakhstan's macroeconomic instability caused by exchange rate crises (Howie & Atakhanova, 2022). The subsequent 21-month suspension of the ETS (2016-2017) allowed industries the opportunity to renegotiate lower quotas based on international benchmarks, further reducing any urgency for emission reductions. We provide evidence to support that firms were unresponsive to the ETS until 2018 or 2019, once third-phase quotas were finalized and Kazakhstan's macroeconomy had stabilized. The broader context is that Kazakhstan experienced its highest carbon emissions ever in 2018, followed by its largest recorded annual fall in emissions at the time in 2019 (Bureau of National Statistics [BNS], 2025a). Comparing 2016–2017 (the ETS suspension period) with 2018–2019 (the ETS reintroduction period) isolates the ETS's impact and avoids the confounding effects of the COVID-19 pandemic in 2020.

Our first main result is that the ETS treatment reduces women's relative employment by 8% (95% confidence interval: 3% to 14%) based on employment-weighted regressions for the post-treatment period of 2018-2019 compared to the pre-treatment period of 2016-2017. We find no statistically significant change in male employment. A triple-differences estimation confirms that the decrease in women's employment relative to male employment is statistically significant. These results are robust to propensity score matching (PSM) and trimming the largest firms. Using standard approaches to analyzing gross job flows in firm-level data, we find that these employment falls are driven by higher job destruction rather than reduced job creation.

Our analysis of disaggregated data supports that gender- and occupation-specific shifts in labor demand are key mechanisms driving our results. We find that the ETS negatively impacts female employment and relative wages among industrial workers, indicating reduced demand. Consistent with our aggregated results, the lower employment for female industrial workers is consistent with increased job destruction. An observed increase in industrial worker vacancies is consistent with a skill mismatch. The combined wage and vacancy evidence confirms that the decline in female industrial worker employment, which drives the aggregate employment effect, stems from a labor demand shift, not a reduction in female labor supply. Simultaneously, we observe rising relative wages and job reallocation for female administrative workers, suggesting both increased demand and falling supply. The inferred negative demand shifts are consistent with the anticipated adverse consequences for women when regulations prevent carbon cost pass-through and impair green technology adoption. We confirm these effects of regulations through secondary sources that interviewed affected firms. Our analysis suggests that women bear higher costs of this policy failure due to their history of occupational discrimination in industrial occupations, because we do not observe the same adverse outcomes for female administrative workers, who were less exposed to occupational prohibitions. Our results underscore the importance of reducing discriminatory barriers while developing an ETS in a distinct institutional context.

Our study contributes to knowledge of gendered labor market effects of carbon pricing, through its unique analysis of firm-level labor data by occupation, while examining these effects in an institutional context of occupational gender discrimination that is prevalent among LMICs. The most closely related studies include Curuk et al. (2025), who focus on EU labor market outcomes by gender and occupation in response to regional energy price shocks; Dutordoir et al. (2024), who find that the EU ETS increases board gender diversity; Marin and Vona (2019), who analyze labor market effects of EU climate policies by occupation but not by gender; Yip (2018), who uses worker-level data and DiD estimation to assess how British Columbia's carbon tax affects labor market status by gender and education rather than occupation; and Walker (2013), who applies triple differences to employee data to assess various distributional outcomes of the U.S. Clean Air Act, including gender effects. Distinctively, our firm-level DiD analysis identifies relative effects on job churn and wages by both gender and occupation that are specific to variation in firm-level constraints resulting from carbon pricing policy. Our findings on occupational churn within energy-intensive firms are important for a just transition, given the established importance of these firms for gender equity in resource-rich countries.

Moreover, prior literature on gendered labor market effects of carbon pricing has focused primarily on developed countries with gender equity institutions distinct from those of the LMICs, who are essential for stemming future carbon emissions growth.

Beyond the studies just mentioned, our paper is distinct within the environmental economics literature because of our focus on gender, while our study is distinct within the labor and gender economics literatures because of our focus on carbon pricing. Other studies discussed in surveys of ETS distributional effects (Shang, 2023) and employment policies and just transition in LIMCs (Caucheteux et al., 2025) do not focus on gender. The same is true for studies of employment effects of environmental regulation (Berman & Bui, 2001), employment decompositions involving a factor demand shift channel (Morgenstern et al., 2002; Hille & Möbius, 2019; Amjadi et al., 2025), studies of general equilibrium effects of emissions taxes on employment and unemployment (Hafstead & Williams, 2018), and other studies using DiD analysis and firm-level data to explore outcomes of carbon pricing (Yamazaki, 2017; Wu & Wang, 2022; Ren et al., 2022; Xiao et al., 2023). Our paper shares methodological similarities with research on women's labor outcomes in the context of trade liberalization (Berik et al., 2004; Menon & Rodgers, 2009, 2021; Juhn et al., 2013, 2014; Banerjee et al., 2022) and topical overlaps with studies on gender and extractive industries (Aragón et al., 2018; Baum & Benshaul-Tolonen, 2021; Atakhanova & Howie, 2022; Guimbeau et al., 2023; Aguilar-Gomez & Benshaul-Tolonen, 2023), though our work is distinct because of its focus on carbon pricing.

The paper proceeds as follows. Section 2 summarizes relevant economic theory for carbon pricing's labor market impacts. Section 3 provides background on Kazakhstan's ETS relevant for applying theory and for our identification strategy. Section 4 describes our data and the construction of our key treatment indicator. Section 5 details our empirical methods. Section 6 presents our main results. Section 7 summarizes results of robustness checks and placebo specifications. Section 8 concludes by discussing the broader policy implications of our results.

2. Theoretical foundations

The primary theoretical mechanism relevant to our study is skill bias from carbon pricing, causing shifts in labor demand. Environmental regulation affects employment through shifts in firms' market demand and cost pressures, as well as through factor shift effects due to input substitution (Morgenstern et al., 2002). Changes in factor demands result from the skill bias of technological change (Acemoglu & Autor, 2011), such as green technology transitions. For shifts in factor demand caused by carbon pricing, the empirical literature emphasizes that as firms undertake activities that are less carbon-intensive, factor prices increase for workers with relevant skills (Shang, 2023). For EU climate policies, skilled specialists are the occupational category that benefits (Marin & Vona, 2019).

The question remains whose skills, by gender, benefit from the skill bias of carbon pricing, and in which occupations. We draw on prior literature to hypothesize how women's skills are distributed across occupations. We then discuss the implications of this skills distribution for the effects of carbon pricing and additional regulation that may moderate these effects. We de-emphasize endogenous skill acquisition and shifts in labor supply, which are likely limited over the two-year period for which we measure effects, although we recognize that these responses matter more over longer horizons.

One possibility is that women possess a *green skills advantage*, implying carbon pricing would be skill-biased in favor of women, as firms' factor demands shift as they adopt green technology. Empirical papers that find a female green skills advantage suggest that it could arise through women's upbringing (following socialization theory) or women's broader views of organizational success (following stakeholder theory). Specific evidence from finance studies and strategy studies includes female Fortune 500 leaders pursuing greener strategies (Glass et al. 2016) and female managers achieving lower carbon emissions (Altunbas et al., 2022), while board gender diversity increases carbon reduction (Barroso et al., 2024; Muktadir-Al-Mukit & Bhaiyat, 2024) and firm environmental scores (Ginglinger & Gentet-Raskopf, 2024; Schoonjans, 2024). Notably, these studies are limited to skill advantages in managerial and governance roles, although this may be due to data availability for publicly-traded firms.

The second competing possibility is that women face a skills disadvantage due to *occupational discrimination*, implying carbon pricing operates as a conventional regulation that intensifies cost pressure. Globally, in the early 2020s, 90 countries maintained some form of occupational restrictions for women, which impeded their skill acquisition (Kamidola & Chernobil, 2022), and these restrictions remain widespread

(World Bank, 2024). The possibility that women have a skills disadvantage is consistent with findings of prior environmental economics literature that women bear more earning losses from job displacement of environmental regulation (Walker, 2013), higher unemployment from carbon pricing (Yip, 2018), and greater job losses from regional energy price shocks (Curuk et al., 2025).

A key context moderating the two pathways just discussed is whether other state policies relax or generate frictions for green adjustments to carbon pricing for firms and households. The environmental economics literature acknowledges that carbon pricing alone is insufficient to address market failures that impede green innovation and diffusion, and other policies can either support or obstruct these adjustments (Jaffe et al., 2002, 2005; Stiglitz, 2019). While there is evidence of carbon pricing successes in fostering technological innovation and diffusion (Tietenberg, 2013; van den Bergh & Savin, 2021), including in China (Ding et al., 2019), widespread challenges remain in adapting climate policies to the development context of LMICs (Caucheteux et al., 2025). Specific challenges emphasized by the World Bank's Partnership for Market Readiness (PMR) include financial market underdevelopment and energy security regulations that impede carbon-cost pass-through to electricity retail prices (de Gouvello et al., 2020). In many LMICs, there are strong political incentives against allowing carbon-cost pass-through for salient goods like energy, because leaders often derive legitimacy from narratives of sustained economic performance and public service provision (Guriev & Teisman, 2019).

Reduced carbon-cost pass-through in an LMIC context has important implications, including amplifying the effects of occupational discrimination and group-specific employment effects. When price signals fail to incentivize green technology adoption, this reduces potential gains from women accumulating green skills. Conversely, such failure amplifies the effects of occupational discrimination because firms would remain locked into legacy technologies, where women have historically faced barriers to skill development. In the EU, successful ETS implementation has been associated with minimal employment effects (Martin et al., 2016; Marin et al., 2018; Dechezlepretre et al., 2023; Colmer et al., 2024), mainly because firms and consumers can substitute in response to accurate price signals under carbon pricing (Dorsey et al., 2025). In contrast, for LMICs with carbon pricing, greater frictions in price-setting leave employment as a more important margin for adjustment.

The adverse effects of carbon pricing on women in LMICs can vary by occupation, depending on the extent of gender discrimination. We expect negative impacts for female industrial workers, who are most directly affected by gender restrictions (EBRD & KAZENERGY, 2020; World Bank, 2024). Fewer adverse effects are expected for administrative roles, where women have not faced the same restrictions. However, there can be indirect effects of restrictions. Because industrial experience is often necessary for promotion, bans on industrial jobs can limit women's advancement into managerial roles (EBRD & KAZENERGY, 2020). Documenting these known direct and indirect effects of gender discrimination by occupation guides our empirical analysis.

3. Background: Kazakhstan's Emissions Trading System

This section provides KazETS background relevant to our analysis. We draw on legal documents, administrative data, and secondary sources, including studies that interviewed KazETS participants during the key third phase. First, we discuss the coverage and quotas of KazETS Phase Three. Second, we detail how Kazakhstan, like many LMICs, exhibits financial market underdevelopment and frictions to carbon-cost pass-through, which amplify the importance of labor market adjustments in our analysis. Third, we describe the context of the ETS suspension and renegotiation, key to our identification strategy. Lastly, we discuss the gendered labor market context of the ETS-covered firms, drawing on descriptive survey evidence that highlights how historical occupational discrimination and limited technological progress have constrained women's progress.

3.1 KazETS Phase Three coverage

Our analysis focuses on energy-intensive firms participating in KazETS during its 2018-2020 Phase Three. The system covers roughly half of Kazakhstan's CO_2 emissions (excluding other emissions), limited to specific sectors (heat and electric power, oil and gas, other mining, various manufacturing activities) and installations achieving a specific emissions threshold (20,000 tons of CO_2, while 10,000 tons triggers reporting requirements), and this remains true through 2025 (International Carbon Action Partnership [ICAP], 2024). Installations have been subject to emissions reporting requirements since Kazakhstan's Environmental Code of 2007. In the KazETS pilot year of 2013, 178 firms were allocated a cap based on 2010 emissions (Resolution No. 1588, 2012).

Following Phase Two (2014-2015) and the 2016-2017 suspension, the Phase Three national allocation plan covered 225 installations operated by 130 distinct firms (Resolution No. 873, 2017). For Phase Three, installations could choose between a quota based on 2013-2015 emissions levels (selected by 34%) or product-based benchmarks (selected by 66%), which were developed for 52 categories with guidance from PMR (PMR, 2017). Penalties for CO_2 emissions exceeding allowances were about 35 USD per ton in 2018 and 33 USD per ton in 2019, based on five units of Kazakhstan's monthly calculation index (Egov, 2025; National Bank of Kazakhstan, 2025), which is less than one-third of the EU ETS penalty of 100 EUR (ICAP, 2024). Heat and power firms comprise the majority of the 130 firms covered in the ETS, and these firms cumulatively hold a majority of the quota allowances (see Table 1).

[Table 1 about here]

The broader context for the KazETS is that Kazakhstan is among the world's most emissions-intensive economies and a 2016 signatory to the Paris Agreement. Kazakhstan in 2013 became the first Asian country and the first middle-income, resource-rich economy to establish an ETS, drawing inspiration from the EU ETS (Howie & Atakhanova, 2022). In 2018, the year of KazETS reintroduction, Kazakhstan's global rankings in CO_2 emissions were 21st in total emissions, 14th in per capita emissions, and 9th in emissions per unit of GDP. These statistics confirm that Kazakhstan is a significant emitter among LMICs. For KazETS Phase Three, the cap was set at 485.9 million tons of CO2 for the total three-year period, with no annual cap (ICAP, 2024). This cap aimed to achieve a 5%

reduction from 1990 emissions levels by 2020, in pursuit of Kazakhstan's unconditional Paris Agreement commitment to reduce total greenhouse gas emissions by 15%.

3.2 Frictions in firm responses to the KazETS

KazETS's most apparent deficiency is its illiquid trading market, caused by tight restrictions on intermediation and frequent appeals for free allowances that reduce market demand. Annual transaction volume as a share of allowances ranged from 0 to 1.3% during the KazETS first three phases (2013-2015, 2018-2020). There were only nine transactions total in Phase Three (Astana International Financial Centre [AIFC], 2025), and trade prices during the first two phases were low and highly volatile (Howie & Atakhanova, 2022). The low liquidity stems from poor choices at the outset. Kazakhstan rushed the implementation in advance of the 2013 pilot, failed to seek PMR expert guidance until deep into the second phase, and assigned enforcement to bureaucrats lacking market experience (Sammut et al., 2017). Further design flaws have persisted. Energy firm interviews confirm that the government has been too permissive in accommodating appeals for new free allowances, leading to carbon market price crashes (Howie & Akhmetov, 2024). Excessive trading restrictions limit financial intermediary participation, reducing price transparency (AIFC, 2025).

A second important set of frictions involves output price adjustment. The KazETS prohibits carbon cost pass-through, and energy security policies heavily regulate heat and power prices. These policies are in place because low energy prices are widely seen as essential for social stability in Kazakhstan (Howie & Atakhanova, 2022). This inability to modify output prices reduces incentives for investment in carbon mitigation or green technology adoption. Interviews with managers and administrators of heat and power firms confirm that they have limited flexibility in output price or quantity, and they broadly agree that energy security policy limits ETS effectiveness and that energy prices are socially sensitive (Howie & Akhmetov, 2024).

3.3 The ETS suspension and Phase Three reintroduction

Our DiD approach relies on the validity of the assumption that firms did not respond to their Phase Three ETS incentives during the suspension period of 2016 and 2017. During the suspension, there were refinements affecting market oversight, the distribution of quotas, and the rules for trading (PMR, 2017), so a restoration of the ETS was anticipated. However, firms plausibly avoided reacting to the ETS in 2016 and 2017 due to success in obtaining waivers for all 2013-2015 penalties after Kazakhstan experienced macroeconomic instability due to falling oil prices (Howie & Atakhanova, 2022). KazETS faced strong industry resistance with sufficient political power to preserve toothless policy implementation. All these points are confirmed by firm interviews conducted during Phase Three (Akhmetov & Howie, 2024; Howie & Akhmetov, 2024). Moreover, Kazakhstan's CO_2 emissions reached a record-high in 2018, the year after the ETS suspension (BNS, 2025a).

Our empirical strategy relies on firms adjusting employment in response to the ETS reintroduction in 2018 and 2019, despite the system's earlier ineffectiveness. Although some arguments for a weak 2016-2017 anticipation response plausibly extend to a weak 2018-2019 reintroduction response, we find several points of evidence to the contrary.

First, the macroeconomic conditions that led to penalty waivers in 2015 stabilized by 2018 and 2019, which were the two lowest inflation years in Kazakhstan's history as an independent state (BNS, 2025b). Consequently, the 2018-2019 period was a short window when ETS penalty enforcement seemed possible. Ex-post, ETS penalties were waived for 27% of firms that were non-compliant under Phase Three in 2020, but this was due to the unanticipated COVID-19 pandemic (Howie & Akhmetov, 2024).

Second, Kazakhstan's CO_2 emissions experienced a record 10% fall in 2019 after the record high in 2018. The 2019 emissions level was the lowest since 2013 and reversed an upward trend that persisted through the ETS first two phases and the suspension. The data is consistent with a one-year lagged reaction to the suspension and reintroduction.

Third, public disclosures of key firms in Kazakhstan suggest a reaction to the KazETS. Samruk-Kazyna, the Kazakh sovereign wealth fund invested in heat and power through Samruk Energy (Howie & Atakhanova, 2022), mentions "obligations to reduce GHG emissions in the national GHG emissions trading system" in its 2019 sustainability report (Samruk-Kazyna, 2019, p. 38) but not in reports from the prior three years (Samruk-Kazyna, 2016, 2017, 2018). KazAtomProm, a key firm with a core business of uranium mining that is majority-owned by Samruk-Kazyna and partially privatized since 2018, sold off its assets related to coal power in 2019 (KazAtomProm, 2020).

Lastly, 2020 interviews with ETS participants confirm that firms paid attention to carbon fines and estimated them (Akhmetov & Howie, 2024). The interviewees, all representing coal-powered firms, viewed carbon trading as a compliance procedure handled by environmental managers and mostly did not develop carbon reduction goals, but they engaged in some energy-saving projects. Therefore, we interpret that even if some firms negotiated lax benchmarks for their Phase Three quotas (Howie et al., 2020; Atakhanova & Howie, 2022), firms were still ex-post constrained by the ETS. A distinct set of interviews in 2019 noted that several ETS participants struggled to find workers with the necessary skills for procedures common in environmental markets (Howie & Akhmetov, 2024). We interpret the interview findings as confirming that firms responded to the ETS during Phase Three by adjusting their employment.

3.4 The gender context of Kazakhstan under the ETS

Kazakhstan during our period of study exhibits the gender-occupation discrimination that is prevalent among LIMCs; specifically, legal barriers for industrial workers, which also hinder women's advancement to more skilled positions. *De jure* prohibitions of women's work in specific occupations were recognized as a major barrier for women until activists succeeded in lobbying Kazakhstan to remove them fully in 2021 (Kamidola & Chernobil, 2022; World Bank, 2023). An earlier report by EBRD and KAZENERGY (2020), based on industry surveys that overlap with ETS-covered firms, listed occupational prohibitions first among challenges for women's representation in the energy sector. Relevant prohibitions include underground oil production and repair roles.

The report acknowledged progress in 2018, when the state relaxed laws on 75 of the 287 prohibited occupations, but the remaining prohibitions were still a "significant impediment." (p. 77). The report clarifies that women's underrepresentation in energy is not caused by skill gaps from formal education, where women's attainment exceeds men's. It supports that occupational prohibitions limited women's advancement into skilled roles: "training and experience in technical and operational fields are frequently considered prerequisites for promotion to management and senior leadership roles" (p. 77). Formal analysis of Kazakhstan's labor force survey data in the energy sector likewise confirms the presence of such vertical discrimination (Atakhanova & Howie, 2022).

Importantly, a key mechanism linking low ETS carbon cost pass-through to low women's energy-sector employment is supported by our secondary sources in Kazakhstan. Energy firm managers confirm that state prohibitions on price adjustments led them to make insufficient technology updates (Howie & Akhmetova, 2024). Among barriers to women in the energy sector, "insufficient updates to production technologies" (p. 76) is emphasized by EBRD and KAZENERGY (2020). These two sources together suggest that regulatory impediments for the ETS led to missed opportunities to improve women's representation in the energy sector.

4. Data and Key Measures

4.1. Source data

Our data sources include the public list of firms covered under KazETS in the third National Allocation Plan (NAP) for 2018-2020 (Resolution No. 873, 2017), the public database matching Business Identification Number (BIN) to firm names (BNS, 2024a), the public Business Register Database (BNS, 2024a), and confidential deidentified administrative data covering firm characteristics, costs, and labor reports (BNS, 2024b).

The first main step of analysis was to subset the deidentified data to include a balanced panel of firms covered by the KazETS for 2016 to 2019. First, we used the firm names (installation operators) in the NAP to obtain a BIN for each firm in the KazETS. Second, we used each BIN to obtain, for each firm in the KazETS, the employment size bin, industry (5-digit NACE Rev. 2), location, and ownership from the Business Register Database. Using the resulting dataset of KazETS firm characteristics, we then filtered the de-identified firm-level data to retain only KazETS firms.

We successfully matched 113 of 130 KazETS firms, with matching detailed by sector in Table 1. There are several reasons why we could not match all firms. First, BINs were not available for some firms. Second, firms with employment under 100 were not included in the confidential data. Lastly, constructing a balanced panel required excluding some firms that changed legal status between 2016 and 2019.

Our key confidential dataset is derived from annual labor reports and includes information on employee numbers (actual number of workers per year) and total salary funds by occupation for each firm, categorized by industries and regions. Following the International Standard Classification of Occupations (ISCO), there are nine occupational groups in the raw data (Ministry of Investment and Development, 2017). These groups include managers; senior specialists; mid-level specialists; administrative workers;

service and sales workers; agricultural workers; industry, construction, and related workers; production equipment operators and assemblers; and unskilled workers. To streamline our analysis and enhance the clarity of our findings, the nine occupational groups have been aggregated into five comprehensive groups: managers; specialists (senior and mid-level); administrative and sales workers; industry workers (including industry, construction, production operators and assemblers); and unskilled workers. Like Marin and Vona (2019) and Curuk et al. (2025), grouping similar occupations helps to identify patterns and relationships in employment dynamics and wage structures.

The other BNS administrative datasets we use are based on annual firm production reports and vacancy reports. The production reports provide data on firm input costs, including energy costs, fuel costs, and material costs, as well as firm sales revenue. Firm vacancy data is based on the National Classification of Occupations, and we aggregate these to match our aggregation of the labor report data using similar groupings.

For our main analysis, we restrict the study period to 2016-2019 to mitigate statistical noise from macroeconomic shocks, such as the end of Kazakhstan's fixed exchange rate regime in 2015 and the COVID-19 pandemic in 2020. Each of these shocks directly impacted the proper functioning of the ETS, since penalties for violating allowances were waived for Phase 2, ending in 2015, and Phase 3, ending in 2020 (Howie & Akhmetov, 2024). Our study period includes four years of relatively stable macroeconomic conditions for Kazakhstan, including two years of ETS suspension (2016-2017) and two years of ETS reintroduction under Phase Three (2018-2019).

4.2. Energy intensity and ETS binding measures

To progress toward a measure of firm constraints under the ETS, we first define a firm's *energy intensity* as the amount of energy consumed per unit of output:

$$Energy\ intensity_{it} = \frac{(energy\ costs_{it} + fuel\ costs_{it})}{total\ output_{it}},$$

where $energy\ costs_{it}$ include all expenses related to the consumption of electricity, natural gas, and other forms of energy for firm i at time t. The variable $fuel\ costs_{it}$ includes the costs of fuels like gasoline and diesel in production, and $total\ output_{it}$ is a measure of production based on sales generated. We calculate the energy intensity for each firm on an annual basis.

To construct our proxy for whether a firm faces a binding constraint under the reintroduction of the ETS in 2018, we compare each firm's 2018 energy intensity to its average energy intensity from 2013-2015. The motivation for this measure is that CO_2 emission volume for the 2013-2015 reference period was one option that firms could choose as their CO_2 emissions quotas for 2018-2020. Firms with higher energy intensity in 2018 compared to the reference period are classified as ETS-binding, while those experiencing a decline in energy intensity are classified as ETS non-binding. Five small firms with energy intensity of zero, which account for 1% of total employment for ETS-covered firms, are excluded from the analysis.

There are several further reasons to motivate our proxy over alternatives. We avoid a comparison between actual emissions and actual quotas, because, first, we lack access to the firm-level CO_2 emissions data, and second, using actual quotas could introduce non-random selection based on how successful firms were in influencing the process for determining benchmarks in 2017. Firms in the treatment group successfully lobbying for lax benchmarks, thereby relaxing ETS constraints, would attenuate our estimates for the effects of the binding ETS. In Section 7, we consider placebo specifications and robustness to alternative methods of energy intensity calculation to ensure that we capture the effects of ETS constraints rather than generic energy intensity growth.

4.3 Gross job flows

To examine firm adjustments, we measure gross job flows using methods from firm-level data analysis standard in labor economics. We define *job creation* as expansions in employment at the firm level or within narrower employment categories, and *job destruction* analogously as contractions. These measures are sensitive to the level of employment disaggregation used in the analysis, as higher aggregation can obscure underlying job churn. Accordingly, we compute job creation and destruction at three levels of disaggregation: for all firm employment, for firm employment disaggregated by gender, and for firm employment disaggregated by both gender and occupation. Unlike labor force surveys such as the EU Labor Force Survey, which provide worker-level data and often lack sufficient granularity on firm-level dynamics, our dataset captures within-firm adjustments and job reallocation patterns that are critical for understanding the mechanisms of employment change. Our approach builds on Davis, Haltiwanger, and Schuh (1996), who measure job creation and destruction based on employment changes in expanding and contracting establishments within U.S. firms, and the U.S. Bureau of Labor

Statistics (BLS) applies their method to construct the Business Employment Dynamics series (BLS, 2015). Subsequent research using firm-level data on employment by occupation has adapted the original measures of job creation and job destruction by studying how employment expands and contracts across occupational categories within firms (Böckerman & Maliranta, 2013; Forsythe, 2023; Bauer & Forsythe, 2025).

4.4 Summary statistics

Table 2 provides summary statistics for the main measures used in the analysis, disaggregated by treatment and control groups and by pre- and post-treatment periods. For pre-treatment employment shares, the treatment and control groups are relatively balanced. There is a difference in firm size that motivates a trimming robustness check.

Comparing the pre- and post-treatment descriptive statistics by group, notable differences for the treated group relative to the control include a fall in female industrial employment, a rise in female administrative employment, a fall in female industrial wages, and a rise in female job destruction. Our main DiD analysis proceeds to confirm the statistical significance of these differences observed in the descriptive statistics.

5. Methodology

5.1. Main estimation approaches

We focus on a difference-in-differences approach using Poisson quasi-maximum likelihood estimation (Poisson QMLE), also known as Pseudo-Poisson Maximum Likelihood (PPML). We justify our assumptions about the regression functional form, consistent with DiD best practice (Roth et al., 2023). A PPML exponential mean approach that restricts the conditional mean to be non-negative is natural for count data, such as employment data or gross job flows. For zeroes in the dependent variable, which prevent log transformations, PPML is a widely accepted solution, including for the context of DiD estimation (Chen & Roth, 2024). PPML allows one dimension of fixed effects to be partialled out, avoiding the incidental parameters problem in our large-N, small-T panel context, and estimation under PPML is consistent even with departures from a Poisson error assumption (Wooldridge, 1999; Cameron & Trivedi, 2013).

To evaluate the effects of binding ETS on firms' employment outcomes during the years 2018-2019, compared to the preceding period of 2016-2017, we focus on the following estimating equation:

$$y_{it} = exp(\gamma ETS_i \times POST_t + \alpha_i + \lambda_t) + \varepsilon_{it}, \quad (1)$$

where y_{it} is a firm (i) employment outcome variable in a given year (t). We estimate this equation for various outcomes of disaggregated employment by gender and gender-occupation, as well as gross job flow outcomes. ETS_i is constructed at the firm level using the approach detailed in Section 4.2, and it is time-invariant. $POST_t$ is a binary indicator for ETS restoration ($POST_t$=0 for t in 2016-2017 and $POST_t$=1 for t in 2018-2019). Lastly, α_i indicates firm fixed effects, λ_t year fixed effects, and ε_{it} the error term. The error term lies outside the exponential, so the exponential term reflects the conditional mean of y_{it}. The coefficient of interest γ is the average treatment effect on the treated ETS group over the post period. Throughout, we estimate Equation (1) using weighted regressions to capture aggregate effects, with weights fixed at 2016 employment levels to ensure exogeneity concerning the treatment. For inference in all cases, we cluster standard errors at the firm level, consistent with standard DiD practice, because the treatment variable ETS_i varies at the firm level (Roth et al., 2023).

We also use the following triple-differences specification, which pools the male and female employment data.

$$y_{igt} = exp(\zeta ETS_i \times POST_t + \eta ETS_i \times POST_t \times GENDER_g + \kappa_{ig} + \varphi_{tg}) + \varepsilon_{igt} \qquad (2)$$

Here $GENDER_g$ is an indicator defined as 1 for observations of female employment. Now η is the main coefficient of interest, as it captures the effect of the ETS for female workers relative to male workers.

For wage regressions, we estimate using ordinary least squares (OLS) with two-way fixed effects. Our dependent variable in these regressions, which model relative female wages by occupation, is the ratio of the firm's average annual wage for women in an occupation to the firm's average annual wage for all workers in the occupation. This measure captures within-occupation gender wage gaps at the firm level.

5.2. Identification strategy

The validity of our DiD analysis relies on the assumptions of no anticipation and parallel trends. Section 3.3 discusses evidence that firms in 2016-2017 anticipated weak ETS enforcement but then changed their views in 2018-2019 during strengthening macroeconomic conditions. To validate the parallel trends assumption, we employ a range of methods to assess potential violations. First, we estimate event study regressions to confirm the absence of observable pre-treatment trends. Second, to address potential confounders, we conduct several robustness checks, including estimation with trimmed data, propensity score matching, and estimation with additional controls. Third, we estimate multiple placebo specifications. To ensure our results are driven by the KazETS introduction, not pre-existing trends, our first placebo specification compares 2013-2015 emissions to 2016 emissions. To verify that our findings reflect KazETS incentives rather than general energy intensity growth, our second placebo specification constructs a treatment group composed of the top 48 firms in 2018 energy intensity growth.

6. Main results

6.1. Employment effects by gender

Table 3a presents our first set of main results on how the binding ETS impacts employment by gender for the 2016-19 period. Columns (A), (B), and (C) estimate the DiD specification of equation (1) for total employment, female employment, and male employment, respectively. For total employment, the effect of binding ETS is statistically significant at the 5% level. For female employment, the effect of the binding ETS is statistically significant at the 1% level, while for male employment, we fail to reject the null hypothesis of no effect. The estimated coefficient implies that treatment reduces the conditional mean of female employment for each treated firm by 8%.

A question raised by the results is whether the female effect is statistically different from the male effect, whose point estimate is also negative. So we run the triple differences specification of equation (2), which we report in Column D of Table 3. Here the main coefficient of interest is the triple interaction, and it is statistically significant at the 5%

level. The estimated coefficient implies that for treated firms, the conditional mean of female employment is reduced by 7% relative to the conditional mean of male employment.

[Table 3a about here]

We conduct an event study specification to assess pre-trends. Figure 1 presents the pre-trends for female and male employment, corresponding to the results presented in columns B and C of Table 3a, respectively. We estimate the interactions between ETS and each year 2016, 2018, and 2019, relative to a baseline year of 2017, while incorporating firm and year fixed effects. Our analysis shows no statistically significant effect for 2016. Thus, we can conclude that the decline in female employment in the post period does not reflect a continuation of prior trends observable in ETS binding firms.[1] Our results also indicate a stronger reaction in 2019 than in 2018, consistent with our argument from Section 3.3 that firms weighed ETS penalties more carefully as the duration of Kazakhstan's macroeconomic stability lengthened.

[Figure 1 about here]

Our results for gross job flows in Table 3b show that net female employment declines are driven by higher job destruction for women in ETS-binding firms rather than reduced job creation. For gross flows measured through changes in total firm employment, in the table's first two columns, we see no statistically significant effect for

[1] This conclusion also holds when we extend the analysis back to 2013. See section 7.2.

job creation or job destruction. The results reveal the importance of our disaggregated analysis by gender in capturing the job churn caused by the ETS.

[Table 3b about here]

Revisiting our discussion in Section 2 about conflicting potential ETS effects on women's employment, our findings support the conclusion that the ETS reinforces occupational discrimination against women more than it promotes women's green skills.

6.2. Mechanisms: gendered occupational labor demand and supply shifts

Our employment and wage data by gender and occupation and vacancy data by occupation allow us to explore specific forces driving our gendered employment results. By exploring effects on employment in Tables 4a and 4b and relative wages in Table 5, we can infer relative labor demand and labor supply shifts. Additionally, results on vacancies in Table 6 reveal skill mismatches by occupation.

[Tables 4a, 4b, 5, and 6 about here]

Our results are consistent with a fall in relative demand for female industrial workers driving the overall fall in female employment. We report a negative and statistically significant effect for both the female employment of industrial workers in Panel A of Table 4a and the relative wages of female industrial workers in Table 5, while Panel C of Table 4a shows that this fall in female industrial employment is driven by increased job destruction. We see no similar effects for male industrial workers in Table 4b, and in fact, we find increased job creation for male industrial workers in panel B. The increased vacancies for industrial workers in Table 6 are consistent with a skill mismatch.

Our results are consistent with our Section 2 discussion, which anticipated that ETS cost pressures would be biased against skills of female industrial workers who have historically faced occupational discrimination.

For female administrative workers, our results indicate a labor demand increase coupled with a labor supply decrease. Table 5 reports a statistically significant increase in relative female wages for administrative roles, while Panel A of Table 4 shows no corresponding female employment effect, reflecting offsetting positive labor demand and negative labor supply effects for women. Panels B and C of Table 4a and 4b show substantial job reallocation (both job creation and destruction) for both female and male administrative workers, consistent with changing composition for these positions in ETS-binding firms. Our results align with interview evidence that firms reacted to the compliance burden of the ETS (Akhmetov & Howie, 2024; Howie & Akhmetov, 2024). The inferred demand shift and increased job creation are consistent with a green skills advantage for female administrative workers, who did not face the same occupational discrimination as female industrial workers. A potential explanation for the decline in labor supply is that firms managed the ETS as a routine compliance concern (Akhmetov & Howie, 2024); such red tape has been linked to lower job satisfaction in management research (Steijn & van der Voet, 2017).

We observe no statistically significant effects on employment or gross job flows for female managers, nor for specialists for whom effects are the most precisely estimated. For female specialists, the increased vacancies in Table 6 also suggest a skill mismatch.

The results do not offer any evidence that the ETS increases demand for women's labor due to a green skills advantage in these occupations. An explanation for the lack of effects is that the ETS operates in a context where energy security regulations limit incentives for adopting green technologies (as detailed in Section 3), thereby hindering the ETS's ability to promote any green skills advantage for female managers or specialists.

7. Robustness checks and placebo specifications

7.1 Sample Balancing via Trimming and PSM-DiD

Our first set of robustness checks addresses sample imbalance by trimming the largest firms. Employment shares are balanced (Table 2), but the firm size disparity is notable. We repeat our main employment analysis excluding the top 5 percent of firms by employment size. Our trimming excludes six firms: five in the control group and one in the treatment group. Excluding just these six firms addresses the firm size imbalance issue between the two groups, as seen in the modified sample summary statistics for total and female employment (Table A1a).

Our second set of robustness checks uses propensity score matching. We follow Xiao et al. (2023) in including a set of matching variables that influence the labor share of income, the natural log value of total assets, a dummy variable of state-owned enterprises, the return on assets, and the ratio of assets to laborers. We also include the share of female managers. Including variables related to firm size and female leadership as potential factors influencing women's labor market outcomes is supported by the labor economics literature (Azmat & Boring, 2020; Theodoropoulos et al., 2022). We use a standard K=1 nearest-neighbor matching with replacement, and following Xiao et al. (2023), a PSM match threshold of 0.05. In total, 100 of 108 firms were matched, while 8 were unmatched due to a lack of common support or failure to find a match within the suitable threshold. Table A1b shows null t-tests confirming match suitability.

Using either the trimmed sample or PSM-DiD estimation, our results by gender remain largely consistent. We continue to observe a negative and statistically significant effect on female employment in ETS-binding firms following ETS restoration (Table A2). For industrial occupations, there remains a negative impact on female employment and no impact on male employment (Table A3). We continue to find a negative relative wage effect for females in industrial occupations and a positive wage effect for females in administrative occupations (Tables A4).

7.2 Alternative analysis periods

While we already found no evidence of pre-trends during the 2016-2017 pre-treatment suspension period, we can also confirm the absence of pre-trends in earlier years. In Figure A1, we present the results of the event study extended back to 2013. We continue to find no evidence of pre-trends. Notably, the standard error bands are wider for all estimated effects from 2013-2015 for both males and females. These results support our decision to exclude the earlier years from our main analysis, as we anticipated those years would introduce statistical noise from earlier ETS phases and macroeconomic volatility caused by Kazakhstan's currency devaluations in 2014 and 2015.

We reproduce our main results for two alternative analysis periods of 2013-2019 and 2016-2022. For the relative declines in female employment (Table A5) and in female employment in industrial occupations (Table A6), results for the alternative analysis periods remain consistent with our main findings. The wage decreases for industrial occupations also continue to hold (Table A7). The estimated wage increase for administrative workers is statistically insignificant for the period 2016-2022, whereas the estimate for 2013-2019 is consistent with our main results.

The persistence of these gender-differentiated effects across both alternative analysis windows reinforces the robustness of our results. However, we interpret the longer-term effects estimated for 2016-2022 with caution. Importantly, penalties under the Phase 3 ETS were waived again in 2020 (Howie & Akhmetov, 2024), potentially weakening the overall policy effect of our treatment. Alternatively, the longer-term estimates could attribute effects in 2021 and 2022 to our Phase 3 treatment that are caused by Phase 4 and Phase 5 policies correlated with our treatment. Nonetheless, the overall results from the alternative analysis periods strengthen our core conclusions.

7.3 Placebo tests

To confirm that our ETS measure is capturing meaningful variation related to the binding ETS and not other economic factors, we conduct two placebo specifications. Our first placebo specification defines the binding ETS based on the difference in energy intensity between 2016 (rather than 2018) and the 2013-2015 reference period. For the second placebo test, we construct a treatment group comprising the 48 firms with the highest 2018 energy intensity growth, thereby confirming that our results are not driven by generic energy intensity trends. Each of these alternative specifications yields no statistically significant results (Table A8). These null effects corroborate that our main effects are attributable to ETS policy rather than external factors.

7.4 Other robustness checks:

We assess the robustness of our results to the definition of energy intensity by redefining our treatment and control groups based on energy costs divided by total costs. The estimates presented in Table A9 are consistent with our main results in Table 3a.

Lastly, to ensure our results are not driven by non-ETS group differences, we add two sets of controls: first, oil price-sector interactions address concerns about sector-specific oil price sensitivity; and second, firm characteristics (from our PSM exercise), interacted with year fixed effects, to control for time-varying effects based on initial group characteristics. In Table A10, adding these controls individually and jointly maintains the stability and consistency of our main results in Table 3a.

8. Conclusion

Our findings illustrate the unintended gender consequences for an evolving ETS in a resource-rich middle-income country. Our main results demonstrate that KazETS reduces women's employment in constrained energy-intensive firms. Utilizing rich, disaggregated data, we identify key mechanisms: a decline in relative demand for female industrial workers, evidenced by both increased job destruction and falling wages, and an increase in demand coupled with a reduction in supply for female administrative workers.

Drawing on economic theory and secondary sources that include interviews with affected firm managers, we attribute our results to state-imposed restrictions on output price adjustments, an underdeveloped emissions trading market, and occupational discrimination against women. These systemic rigidities appear to limit green technology adoption and shift firm adjustment costs to disadvantaged women, while failing to leverage their green skills. Although the issues leading to negative outcomes for women under KazETS are pronounced, they are not unique. The legacy of de facto and de jure discrimination against women in industrial occupations is widespread (World Bank, 2024), and many LMICs face challenges reconciling their energy policies with carbon pricing (de Gouvello et al., 2020; Caucheteux et al., 2025).

Our results support accelerating reforms against discriminatory policies affecting women while LIMCs are refining their carbon pricing policies. Given the institutional heterogeneity of carbon-intensive LIMCs (Caucheteux et al., 2025), Kazakhstan's iterative experience in ETS design is likely to be common. Although a better initial KazETS design could have incentivized green investment, disadvantaged groups cannot expect LIMCs to immediately determine optimal policies within their institutional contexts. Therefore, accelerating reforms that reduce labor market discrimination can mitigate costs of ETS design iteration, which we have shown disproportionately affect women.

Kazakhstan's case illustrates both the advantages of directly targeting social and environmental goals concurrently and disadvantages of placing poorly targeted social conditions on environmental policy. We have argued that restrictions on carbon price pass-through, intended to maintain social stability through low energy prices, inadvertently weakened the KazETS system and hindered women's economic participation. So importantly, our analysis critiques adding poorly-targeted social conditions to environmental policies (like the critique of Klein & Thompson, 2025), such as the 'buy-American' procurement rules in the U.S. Inflation Reduction Act aimed at decarbonization (Klein, 2023). Optimal policy, following the targeting principle (Bhagwati & Ramaswami, 1963), requires appropriate instruments to address all relevant social distortions. This principle likewise applies to the environmental economics literature on policy portfolios when there are relevant distortions left uncorrected by carbon pricing (Jaffe at al., 2005; Stiglitz, 2019), including social distortions (Jotzo & Azhgaliyeva, 2022). Evidence matters for addressing a complex problem like gender equality (Eden & Wagstaff, 2021). Our paper provides evidence to support well-targeted gender and environmental reforms in tandem.

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Figures and Tables

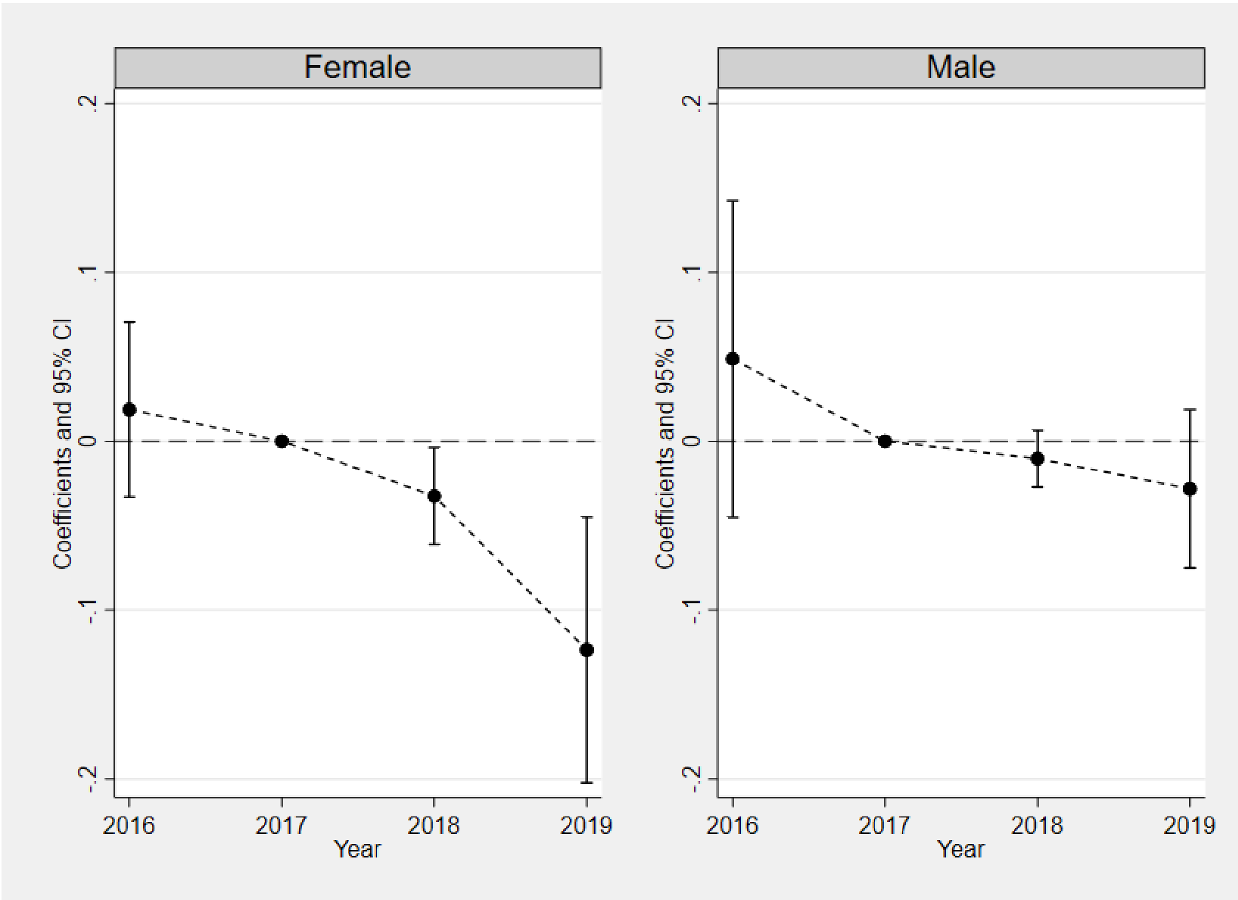

Figure 1. Event study regression analysis for the effects of binding ETS on employment by gender in Kazakhstan, 2016-2019.

Table 1. KazETS firms covered and included in the analysis

Industry	Share of ETS allowances	Number of firms in the National Allocation Plan 2018-2020	Number of firms matched with confidential firm-level data
Electric power	55.6%	52	45
Oil & gas	14.1%	39	32
Mining and manufacturing	30.3%	39	36
Total	100%	130	113

Notes: The firms are divided into industry according to the Phase Three national allocation plan (Resolution No. 873, 2017), where the categories of mining (excluding oil and gas), metallurgy, chemicals, and building materials have been aggregated into mining and manufacturing.

Table 2. Summary statistics.

	Control N=240				Treatment N=192			
	Pre		Post		Pre		Post	
	mean	s.d.	mean	s.d.	mean	s.d.	mean	s.d.
A. Employment								
Employment	2190.25	4328.87	2210.33	4412.48	1125.32	1084.99	1085.35	1049.47
Female employment	514.10	993.69	507.50	990.07	263.15	258.38	244.57	233.29
Female employment share	0.276	0.163	0.271	0.158	0.257	0.142	0.253	0.147
Female managers share	0.217	0.163	0.226	0.191	0.196	0.133	0.211	0.153
Female specialists share	0.481	0.180	0.463	0.175	0.454	0.189	0.444	0.183
Female industry workers share	0.142	0.129	0.152	0.169	0.154	0.170	0.136	0.142
Female administrative workers share	0.760	0.262	0.774	0.248	0.722	0.292	0.757	0.299
Female unskilled workers share	0.571	0.291	0.584	0.292	0.575	0.334	0.584	0.341

B. Wages								
Average wages	147.104	129.728	153.747	131.662	153.747	131.662	134.083	81.053
Average female wages	126.371	109.766	131.527	111.905	131.527	111.905	121.829	79.147
Average female manager wages	250.324	166.784	277.392	201.497	277.392	201.497	249.545	141.892
Average female specialist wages	133.051	188.047	137.225	108.930	137.225	108.930	130.539	82.838
Average female industry worker wages	93.831	81.293	106.096	91.380	106.096	91.380	98.926	67.409
Average female administrative wages	92.347	62.001	91.656	57.880	91.656	57.880	86.976	47.862
Average female unskilled worker wages	57.491	40.171	57.111	30.782	57.111	30.782	57.298	32.980
C. Other firm characteristics								
Energy intensity	0.155	0.176	0.140	0.163	0.208	0.234	0.235	0.249
Energy costs per worker	10605.74	46313.91	10874.36	48111.12	6377.35	15226.22	8891.20	19411.25
Log of tangible assets	16.192	2.283	16.353	2.289	16.203	2.226	16.329	2.205

Return on assets	0.745	1.040	1.100	2.310	1.050	3.552	8.503	72.663
Ratio of assets to workers	76641.13	203757.1	75956.37	176272	89303.26	283716.5	103372.8	318026.4
D. Gross job flows								
Total job creation as a share of total employment	0.021	0.005	0.017	0.007	0.008	0.003	0.008	0.003
Female job creation as a share of female employment	0.014	0.004	0.021	0.008	0.006	0.003	0.008	0.003
Male job creation as a share of male employment	0.026	0.006	0.019	0.007	0.010	0.003	0.013	0.004
Total job destruction as a share of total employment	0.017	0.006	0.022	0.005	0.022	0.005	0.036	0.010
Female job destruction as a share of female employment	0.027	0.007	0.026	0.006	0.030	0.005	0.057	0.011
Male job destruction as a share of male employment	0.016	0.006	0.023	0.006	0.021	0.006	0.035	0.011

Notes: Authors' calculations based on 2016-2019 firm-level data (BNS, 2024b). N reflects firm-year observations. Wages are presented on a monthly basis in thousands of tenge (KZT), deflated using the CPI (BNS, 2025b). Reported job flow shares are weighted by firm-level employment for each category (total, female, and male), using 2016 values as the base year for constructing initial employment weights.

Table 3a. Employment effects.

	Total (A)	Female (B)	Male (C)	Pooled (D)
$ETS_i \times POST_t$	-0.057**	-0.086***	-0.046	-0.014
	(0.027)	(0.027)	(0.031)	(0.019)
$ETS_i \times POST_t \times GENDER_g$				-0.069**
				(0.028)
Pseudo-R^2	0.9945	0.9939	0.9932	0.9926
Obs.	432	432	432	864

Notes: Coefficients are estimated from 2016-2019 using PPML. The ETS indicator varies at the firm level, and the POST indicator is defined as 1 for 2018-2019. The unit of analysis for columns (A) to (C) is firm-years, weighted by firm employment in the initial year of 2016. Columns (A) to (C) include firm fixed effects and year fixed effects. The unit of analysis for column (D) is firm-gender-years, weighted by gender-specific employment in the initial year of 2016. Column (D) includes firm-gender and gender-year fixed effects. The GENDER indicator is defined as 1 for female and 0 for male. Standard errors clustered by firms are in parentheses. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table 3b. Job creation and job destruction

	Total		Female		Male	
	Job creation	Job destruction	Job creation	Job destruction	Job creation	Job destruction
$ETS_i \times POST_t$	0.927	0.944	-0.383	1.728**	1.326	0.630
	(1.082)	(0.864)	(1.284)	(0.764)	(1.036)	(0.854)
Pseudo-R^2	0.7844	0.6258	0.4757	0.6159	0.8106	0.6407
Obs.	328	400	324	416	352	392

Notes: Coefficients are estimated using PPML. The unit of analysis is firm-years, run from 2016-2019. The ETS indicator varies at the firm level, and the POST indicator is defined as 1 for 2018-2019. Each column includes firm fixed effects and year fixed effects. All regressions are weighted by initial employment based on 2016 values. Standard errors clustered by firms are in parentheses. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table 4a. Female employment effects by occupations

	Manager	Specialists	Industry	Admin	Unskilled
Panel A: Female employment					
$ETS_i \times POST_t$	-0.109	-0.034	-0.101**	-0.218	-0.164
	(0.079)	(0.028)	(0.049)	(0.169)	(0.143)
Pseudo-R^2	0.8930	0.9668	0.9866	0.6995	0.8939
Obs.	404	428	352	368	328
Panel B: Female employment creation					
$ETS_i \times POST_t$	1.385	0.020	-0.810	5.119***	1.445
	(0.942)	(0.902)	(1.405)	(1.396)	(0.940)
Pseudo-R^2	0.6990	0.3962	0.4812	0.7611	0.5112
Obs.	344	392	268	284	252
Panel C: Female employment destruction					
$ETS_i \times POST_t$	0.578	0.677	2.511**	2.875**	1.341***
	(1.085)	(0.610)	(0.989)	(1.155)	(0.410)
Pseudo-R^2	0.5202	0.4435	0.6900	0.6680	0.7044
Obs.	360	396	344	328	308

Notes: Coefficients are estimated using PPML. The unit of analysis is firm-years, run from 2016-2019. The ETS indicator varies at the firm level, and the POST indicator is defined as 1 for 2018-2019. Each column includes firm fixed effects and year fixed effects. All regressions are weighted by initial employment based on 2016 values. Standard errors clustered by firms are in parentheses. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table 4b. Male employment effects by occupations

	Manager	Specialists	Industry	Admin	Unskilled
Panel A: Male employment					
$ETS_i \times POST_t$	-0.045	-0.031	-0.063	-0.023	-0.363**
	(0.032)	(0.034)	(0.051)	(0.400)	(0.162)
Pseudo-R^2	0.9709	0.9344	0.9899	0.5980	0.8484
Obs.	432	432	388	248	332
Panel B: Male employment creation					
$ETS_i \times POST_t$	0.600	-0.285	2.554**	4.076**	-0.399
	(1.112)	(0.584)	(1.058)	(1.739)	(0.571)
Pseudo-R^2	0.5361	0.5224	0.8098	0.6801	0.4340
Obs.	348	420	332	192	276
Panel C: Male employment destruction					
$ETS_i \times POST_t$	1.173	1.777***	0.637	3.375**	-0.786
	(1.099)	(0.660)	(0.873)	(1.604)	(0.672)
Pseudo-R^2	0.4606	0.4876	0.5789	0.6198	0.3532
Obs.	372	384	376	240	304

Notes: Coefficients are estimated using PPML. The unit of analysis is firm-years, run from 2016-2019. The ETS indicator varies at the firm level, and the POST indicator is defined as 1 for 2018-2019. Each column includes firm fixed effects and year fixed effects. All regressions are weighted by initial employment based on 2016 values. Standard errors clustered by firms are in parentheses. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table 5. Wage effects by occupations.

	Manager	Specialists	Industry	Admin	Unskilled
Female					
$ETS_i \times POST_t$	-0.038	0.017	-0.065**	0.048***	0.002
	(0.040)	(0.019)	(0.028)	(0.019)	(0.026)
R^2	0.5586	0.5116	0.6588	0.4491	0.5680
Obs.	405	428	347	359	318

Notes: Coefficients are estimated using OLS. The unit of analysis is firm-years, run from 2016-2019. The ETS indicator varies at the firm level, and the POST indicator is defined as 1 for 2018-2019. Each column includes firm fixed effects and year fixed effects. All regressions are weighted by initial employment based on 2016 values. Standard errors clustered by firms are in parentheses. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table 6. Vacancy postings.

	All	Manager	Specialists	Industry	Admin	Unskilled
$ETS_i \times POST_t$	1.000**	0.565	1.244*	1.056**	-0.018	0.320
	(0.490)	(0.728)	(0.655)	(0.453)	(0.462)	(0.401)
Pseudo-R^2	0.6823	0.4944	0.6769	0.6946	0.8312	0.6348
Obs.	113	90	110	101	62	79

Notes: Coefficients are estimated using PPML. The unit of analysis is firm-years, run from 2016-2019. The ETS indicator varies at the firm level, and the POST indicator is defined as 1 for 2018-2019. Each column includes firm fixed effects and year fixed effects. All regressions are weighted by initial employment based on 2016 values. Standard errors clustered by firms are in parentheses. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Appendix Figures and Tables

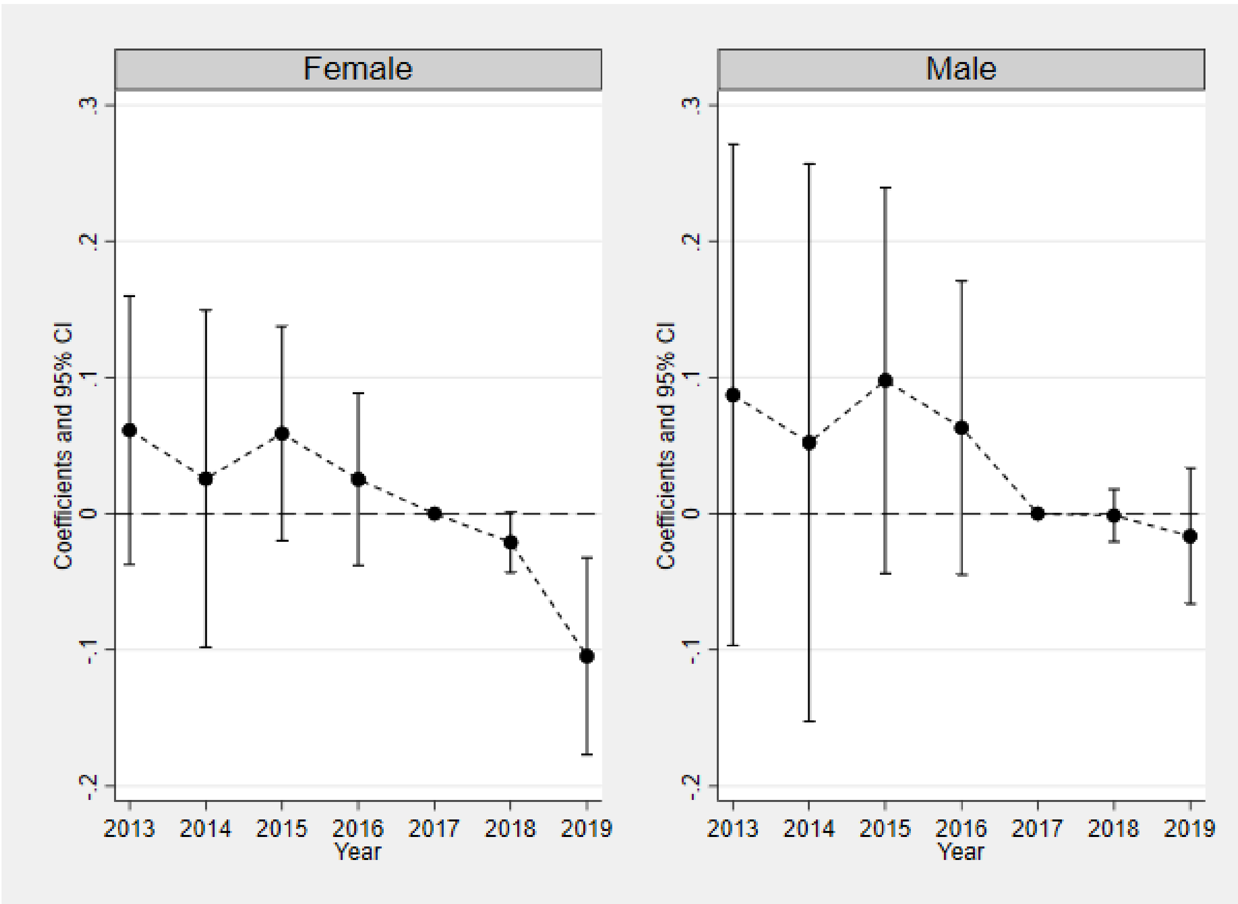

Figure A1. Event study regression analysis for the effects of binding ETS on employment by gender in Kazakhstan, 2013-2019.

Table A1a. Summary statistics for the sample excluding the 6 largest firms.

	Control N=240				Treatment N=192			
	Pre		Post		Pre		Post	
	mean	s.d.	mean	s.d.	mean	s.d.	mean	s.d.
Employment	1004.48	974.83	1009.83	991.40	1125.32	1084.99	1085.35	1049.47
Female employment	238.61	241.33	235.84	242.47	263.15	258.38	244.57	233.29

Source: Authors' calculations based on 2016-2019 firm-level data (BNS, 2024b). N reflects firm-year observations.

Table A1b. Propensity score matching: balancing test results

	Raw sample				Matched sample			
	mean		t-test		mean		t-test	
Variable	Treated	Control	t	p>|t|	Treated	Control	t	p>|t|
Log of total assets	16.282	16.258	0.06	0.955	16.383	16.223	0.39	0.700
Ownership	2.146	2.086	0.63	0.531	2.133	2.083	0.50	0.616
RoA	1.134	0.893	0.42	0.672	0.563	0.824	-1.22	0.227
Average total assets per worker	93362	77645	0.32	0.750	37554	73075	-1.10	0.275
Female manager shares	0.202	0.228	-0.74	0.462	0.209	0.215	-0.19	0.849

Table A2. Employment effect, robustness results: excluding largest firms & PSM-DiD.

	Panel A: Excluding largest firms			Panel B: PSM-DiD		
	Female (A)	Male (B)	Pooled (C)	Female (A)	Male (B)	Pooled (C)
$ETS_i \times POST_t$	-0.078***	0.005	-0.001	-0.087***	-0.050	-0.020
	(0.030)	(0.037)	(0.033)	(0.027)	(0.032)	(0.018)
$ETS_i \times POST_t \times GENDER_g$			-0.067*			-0.065**
			(0.039)			(0.028)
Pseudo-R^2	0.9525	0.9717	0.9821	0.9939	0.9928	0.9935
Obs.	408	408	816	400	400	800

Notes: The unit of analysis for columns (A) and (B) is firm-years, weighted by firm employment, run from 2016-2019, with POST indicator defined as 1 for 2018-2019. Panel A excludes the largest firms from the sample. Panel B employs a Propensity Score Matching with DiD (PSM-DiD) approach. Each ETS measure in columns (A) and (B) varies at the firm level. Columns (A) and (B) include firm fixed effects and year fixed effects. The unit of analysis for column (C) is firm-gender-year run from 2016-2019, with POST indicator defined as 1 for 2018-2019, and GENDER indicator defined as 1 for female and 0 for male. Each ETS measure in column (C) varies at the firm-gender level. Column (C) includes firm-gender and gender-year fixed effects. Coefficients are estimated using PPML. Standard errors clustered by firms are in parentheses. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A3. Employment effects in industrial occupations: excluding largest firms & PSM-DiD.

	Panel A: Excluding largest firms		Panel B: PSM-DiD	
	Female (A)	Male (B)	Female (A)	Male (B)
$ETS_i \times POST_t$	-0.112*	-0.021	-0.114**	-0.061
	(0.059)	(0.068)	(0.050)	(0.048)
Pseudo-R^2	0.8803	0.9425	0.9926	0.9943
Obs.	340	376	336	368

Notes: The unit of analysis is firm-years, run from 2016-2019. Panel A excludes the largest six firms. Panel B employs a Propensity Score Matching with DiD (PSM-DiD) approach. The ETS indicator varies at the firm level, and the POST indicator is defined as 1 for 2018-2019. Each column includes firm fixed effects and year fixed effects. Coefficients are estimated using PPML. Standard errors clustered by firms are in parentheses. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A4. Female wage effects by two occupations: excluding the largest firms & PSM-DiD.

	Panel A: Excluding the largest firms		Panel B: PSM-DiD	
	Industry	Admin	Industry	Admin
$ETS_i \times POST_t$	-0.066**	0.040**	-0.059**	0.046**
	(0.029)	(0.017)	(0.029)	(0.019)
R^2	0.6435	0.4785	0.6217	0.4600
Obs.	323	335	331	337

Notes: The unit of analysis is firm-years, run from 2016-2019. Panel A excludes the largest six firms. Panel B employs a Propensity Score Matching with DiD (PSM-DiD) approach. The ETS indicator varies at the firm level, and the POST indicator is defined as 1 for 2018-2019. Each column includes firm fixed effects and year fixed effects. Coefficients are estimated using OLS. Standard errors clustered by firms are in parentheses. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A5. Employment effects: alternative analysis periods

	2013-2019			2016-2022		
	Female (A)	Male (B)	Pooled (C)	Female (A)	Male (B)	Pooled (C)
$ETS_i \times POST_t$	-0.096** (0.040)	-0.069 (0.065)	-0.011 (0.039)	-0.087* (0.051)	0.002 (0.079)	0.000 (0.040)
$ETS_i \times POST_t \times GENDER_g$			-0.089** (0.038)			-0.091** (0.040)
Pseudo-R^2	0.9902	0.9854	0.9868	0.9939	0.9928	0.9941
Obs.	756	756	1512	756	756	1512

Notes: The unit of analysis for columns (A) and (B) is firm-years, weighted by firm employment. Panel A covers 2013–2019, and Panel B covers 2016–2022. Each ETS measure in columns (A) and (B) varies at the firm level. Columns (A) and (B) include firm fixed effects and year fixed effects. The unit of analysis for column (C) is firm-gender-year run from 2016-2019, with POST indicator defined as 1 for the treatment period (2018–2019 in Panel A; 2018–2022 in Panel B), and GENDER indicator defined as 1 for female and 0 for male. Each ETS measure in column (C) varies at the firm-gender level. Column (C) includes firm-gender and gender-year fixed effects. Coefficients are estimated using PPML. Standard errors clustered by firms are in parentheses. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A6. Employment effects in industrial occupations: alternative analysis periods

	2013-2019		2016-2022	
	Female	Male	Female	Male
$ETS_i \times POST_t$	-0.109**	-0.112	-0.141*	-0.004
	(0.048)	(0.078)	(0.085)	(0.060)
Pseudo-R^2	0.9861	0.9822	0.9813	0.9851
Obs.	616	679	616	679

Notes: The unit of analysis is firm-years. Panel A covers 2013–2019 and Panel B covers 2016–2022. Each ETS measure in the table varies at the firm level and is interacted with the treatment years (2018–2019 in Panel A; 2018–2022 in Panel B). Each column includes firm fixed effects and year fixed effects. Coefficients are estimated using PPML. Standard errors clustered by firms are in parentheses. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A7. Female wage effects by two occupations: alternative analysis periods.

	2013-2019		2016-2022	
	Industry	Admin	Industry	Admin
$ETS_i \times POST_t$	-0.051**	0.039**	-0.068**	0.028
	(0.022)	(0.018)	(0.026)	(0.023)
R^2	0.5409	0.3622	0.5290	0.3282
Obs.	632	646	620	641

Notes: The unit of analysis is firm-years. Panel A covers 2013–2019 and Panel B covers 2016–2022. Each ETS measure in the table varies at the firm level and is interacted with the treatment years (2018–2019 in Panel A; 2018–2022 in Panel B). Each column includes firm fixed effects and year fixed effects. Coefficients are estimated using OLS. Standard errors clustered by firms are in parentheses. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A8. Placebo specifications.

	Female (A)	Male (B)	Pooled (C)	Female (D)	Male (E)	Pooled (F)
	ETS indicator defined using 2016 emissions rather than 2018			ETS indicator defined as top 48 firms in 2018 energy intensity growth		
$ETS_i \times POST_t$	-0.009	-0.035	-0.039	0.021	0.046	0.050
	(0.020)	(0.042)	(0.043)	(0.017)	(0.046)	(0.047)
$ETS_i \times POST_t \times GENDER_g$			0.037 (0.035)			-0.029 (0.042)
Pseudo-R^2	0.9908	0.9927	0.9944	0.9908	0.9928	0.9945
Obs.	432	432	864	432	432	864

Notes (A)-(C): The unit of analysis for columns (A) and (B) is firm-years run from 2016-2019, with POST indicator defined as 1 for 2018-2019. Each ETS measure in columns (A) and (B) varies at the firm level. Columns (A) and (B) include firm fixed effects and year fixed effects. The unit of analysis for column (C) is firm-gender-year run from 2016-2019, with POST indicator defined as 1 for 2018-2019, and GENDER indicator defined as 1 for female and 0 for male. Each ETS measure in column (C) varies at the firm-gender level. Column (C) includes firm-gender and gender-year fixed effects. Coefficients are estimated using PPML. Standard errors clustered by firms are in parentheses. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Notes (D)-(F): The unit of analysis is firm-years, run from 2016-2019, with POST indicator defined as 1 for 2018-2019. Each ETS measure in the table varies at the firm level. Each column includes firm fixed effects and year fixed effects. Coefficients are estimated using PPML. Standard errors clustered by firms are in parentheses. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A9. Employment effects using energy costs as a share of total costs to measure intensity

	Female (A)	Male (B)	Pooled (C)
$ETS_i \times POST_t$	-0.046**	0.067	0.017
	(0.023)	(0.043)	(0.024)
$ETS_i \times POST_t \times GENDER_g$			-0.053*
			(0.029)
Pseudo-R^2	0.9586	0.9959	0.9889
Obs.	432	432	864

Notes: The unit of analysis for columns (A) and (B) is firm-years, weighted by firm employment, run from 2016-2019, with POST indicator defined as 1 for 2018-2019. Energy intensity measure is calculated as the ratio of energy costs to total costs, used to differentiate between binding and non-binding ETS groups. Each ETS measure in columns (A) and (B) varies at the firm level. Columns (A) and (B) include firm fixed effects and year fixed effects. The unit of analysis for column (C) is firm-gender-year run from 2016-2019, with POST indicator defined as 1 for 2018-2019, and GENDER indicator defined as 1 for female and 0 for male. Each ETS measure in column (C) varies at the firm-gender level. Column (C) includes firm-gender and gender-year fixed effects. Coefficients are estimated using PPML. Standard errors clustered by firms are in parentheses. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A10. Employment effects: alternative sets of control variables.

	(1)	(2)	(3)	(4)	(5)	(6)
	Female (A)	Male (B)	Pooled (C)	Female (A)	Male (B)	Pooled (C)
$ETS_i \times POST_t$	-0.090***	0.002	-0.000	-0.075***	-0.010	-0.015
	(0.024)	(0.027)	(0.027)	(0.026)	(0.026)	(0.023)
$ETS_i \times POST_t \times GENDER_g$			-0.066**			-0.059**
			(0.027)			(0.026)
Oil price#sector	Yes	Yes	Yes	No	No	No
Firm chars.#year	No	No	No	Yes	Yes	Yes
Pseudo-R2	0.9939	0.9929	0.9821	0.9786	0.9957	0.9690
Obs.	432	432	864	424	424	848

	(7)	(8)	(9)
	Female (A)	Male (B)	Pooled (C)
$ETS_i \times POST_t$	-0.073***	0.033	-0.000
	(0.024)	(0.024)	(0.026)
$ETS_i \times POST_t \times GENDER_g$			-0.059**
			(0.029)
Oil price#sector	Yes	Yes	Yes
Firm chars.#year	Yes	Yes	Yes
Pseudo-R^2	0.9787	0.9953	0.9690
Obs.	424	424	848

Notes: Columns 1-3 control for oil prices (FRED, 2024), interacted with year fixed effects. Columns 4-6 control for firm characteristics specified in PSM approach (excluding the ownership variable) fixed at the 2016 level, also interacted with year fixed effects. Columns 7-9 include both sets of controls from columns 1-6. Columns labelled (A) and (B) also include firm fixed effects and year fixed effects, while columns labelled (C) include firm-gender and gender-year fixed effects. Coefficients are estimated using PPML. Standard errors clustered by firms are in parentheses. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.