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

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Abstract: We study how carbon pricing impacts labor market outcomes in energy firms by gender and occupation. The impact of environmental policy on gender equality attracts attention in development studies of resource-rich economies, but causal evidence for carbon pricing impacts is scarce to date. Kazakhstan offers a unique setting for identifying these relationships due to plausible exogeneity in how the 2018 restoration of an emissions trading system (ETS) constrained individual firms, following nearly two years of system suspension and renegotiation. Based on a difference-in-differences approach, we find an 8% decrease in women's employment for firms most constrained by the ETS in 2018 and 2019, driven by a fall in female industrial workers. At the same time, wages decrease for women's industrial workers and wages increase for women's administrative workers. The main mechanisms consistent with our findings are that energy firms sought efficiency in incumbent industrial processes, for which women face long-standing discrimination and skill mismatch, while firm demand for environmental knowledge (abundant among women) increased only for administrative occupations. Since women's occupational discrimination coincides globally with the social importance of energy employment, our empirical findings support the broader relevance of economic theory that policy should address environmental distortions and social distortions in tandem.

Keywords: carbon pricing, ETS, gender, female employment, occupations

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1. Introduction

Empirical knowledge of how carbon pricing impacts women's labor market opportunities in the energy sector is important for development policy and business strategy. Decarbonization and gender equity are relevant pursuits for United Nations Sustainable Development Goals and Environmental, Social, and Governance (ESG) measures, but too often the intersection of environmental and social concerns is neglected even though the two are closely intertwined (Gloor et al., 2022). Economic theory supports the need for carbon pricing to consider distributional considerations given "the second-best policy world in which we live" (Stiglitz, 2019). The implications of environmental policy specifically for women's energy-sector employment is a prominent social concern in resource-rich states, both for industry (Ergon, 2020) and development agencies (UNDP & GEF, 2023). In practice, policymakers include social policy alongside environmental policy in policy packages aimed at both social objectives and decarbonization (Jotzo & Azhgaliyeva, 2022), so nuanced knowledge of carbon pricing's impact on women's energy employment is crucial for the careful design of the interconnected social policies.

Theory of carbon pricing's distributional impacts applied to women's energy employment implies that outcomes could vary by occupation and local context. The environmental economics literature argues that carbon pricing's impact on factor markets is skill-biased (Shang, 2023). The question remains, biased to whose skills? Evidence that female managers have superior performance in environmental measures (Glass et al., 2016) and specifically decarbonization performance (Altunbas et al., 2022) suggests that carbon pricing could increase firm demand for women's mix of skills, absent other distortions. However, distortions are not absent. Development agencies emphasize that the historic underrepresentation of women in the energy sectors extends to green energy jobs, which exhibit greater demand for skill in Science, Technology, Engineering, and Mathematics (STEM) fields, for which women have historically faced barriers to entry (Gloor et al., 2022; Brixi et al. 2023). Additional barriers to green employment include "restrictive policies, discriminatory legal frameworks, unconscious biases, and unfavorable hiring practices" (Deininger & Gren, 2022). Thus, there are a variety of social and economic forces that could cause carbon pricing to help or hinder women's labor market outcomes. The conflicting gender implications are reminiscent of the literature on liberal trade policies, for which there was mainstream optimism that international competition would mechanically reduce gender discrimination à la Becker (1957), but feminist scholars revealed special challenges for women varying by education and occupation (Menon & Rodgers, 2021).

Evaluating the effects of carbon pricing on women's labor market opportunities naturally involves challenges of data and identification, and we are not aware of any study of the subject using a causal research design. There is a well-documented gender data gap that limits the ability to monitor the progress of women (UN Women, 2018; Gloor et al., 2022). When gender employment and wage data is available in the energy sector, studies tend to be descriptive and aggregated. Though there are studies of gender and climate change (Eastin, 2018) and distributional impacts of carbon pricing (Dorband et al, 2019), a recent prominent survey of carbon pricing's distributional impacts in environmental economics (Shang, 2023) includes no mention of gender.

We study how implementing a carbon price affects gendered labor market outcomes. We overcome challenges in data and identification by focusing on the 2018 re-introduction of a carbon price in Kazakhstan, where we can access confidential firm-level data on employment and wages disaggregated by gender and occupation. Our identification strategy exploits unique circumstances in the evolution of Kazakhstan's Emissions Trading System (ETS) suitable for a difference-in-differences (DiD) analysis. We use DiD to assess employment effects of the ETS for women and men. Analysis of wages and employment disaggregated by occupation allows us to untangle mechanisms underlying the aggregate effects. Kazakhstan offers a suitable research setting to explore the short-run impact of a carbon price, in a specific context where energy is the dominant sector of the economy, the price incentives are too weak to encourage substantial technological upgrading (Howie & Atakhanova, 2022), and where women have been recently targeted for support but historically disadvantaged (Ergon, 2020; Atakhanova & Howie, 2022; UNDP & GEF, 2023).

Our identification relies on heightened impact of the ETS for selected firms in 2018-2019 relative to an ETS suspension period of 2016-2017. Following a pilot phase in 2013 and a second 2014-2015 phase that coincided with the collapse of Kazakhstan's fixed exchange rate regime, the ETS was suspended for redesign and renegotiation of quotas for 21 months and then re-implemented for Phase 3 from 2018-2020. Phase 3 allowed firms to select one of 52 sectoral benchmarks or a historical quota based on their average annual emissions from 2013-2015 (Howie & Atakhanova, 2022). There are multiple arguments for why firms who ultimately found their emissions bound by Phase 3 rules may not have acted in anticipation in 2017. One reason is that the benchmarks were set at the industry level and negotiated with the state throughout 2017 (PMR, 2017). Another reason is the weakened credibility of the system's penalties after non-compliance penalties for Phase 1 and 2 were waived (Howie et al., 2020), so Phase 3 was at risk of likewise being rendered toothless due to an adverse external event. Ultimately, 2020 was defined by the COVID-19 pandemic, and the 27% of firms who violated their emission allowances in phase III were not penalized (KazEnergy, 2021; Howie & Akhmetov, 2024). However, the years of 2018 and 2019 concluded a stretch of relative macroeconomic stability in Kazakhstan, so there were heightened prospects that the ETS would ultimately yield a cap with noncompliance penalties, since actual trading on the system was scarce. We reconstruct whether the ETS imposed a binding constraint on emissions leading into Phase 3, to the extent our data permits. In assessing the impact of the carbon price on women's labor market outcomes, we take these constrained firms to be treated by the ETS. The two groups are balanced across key observables, though there is inevitably some imbalance among firm size given the presence in data of exceptionally large firms, which motivates robustness checks that trim or match. So we argue that the evolution of other firms between the suspension and implementation periods provides a reasonable counterfactual.

Figure 1 presents an initial exploration of how the ETS affects women's labor market outcomes. We plot the difference between the female employment share of firms with binding ETS (our treatment group) and the female employment share of firms with non-binding ETS (our control group). Prior to the ETS re-introduction in 2018, we observe the two groups having a similar female employment share, while after the ETS re-introudction, a gap opens up with the ETS binding group exhibiting a persistently lower female employment share.

Our first main result from our DiD estimation is that the ETS treatment reduces women's employment by 8% with a 95% confidence interval of [3%, 14%] based on employment-weighted regressions for the post-period of 2018-2019 relative to the pre-treatment period of 2016-2017.[1] We see no statistically significant change in male employment. A triple-differences estimation confirms that the fall in women's employment relative to male employment is statistically significant in the weighted regressions, confirming the importance of gender-specific labor market factors. These results are robust either to propensity score matching (PSM) or to trimming the largest firms.

In our analysis of labor market outcomes at the occupation level, we find that for the average firm, the ETS treatment causes a negative effect on the female employment share for industrial workers and a negative effect on their relative wages within the firm. We also observe a positive effect in the number of vacancies for the industrial worker occupation category. The female employment and wage results are consistent with carbon pricing causing a fall in demand for female industrial workers, while an increase in vacancies for industrial workers suggests a skill mismatch for women. The results corroborate the hypothesis that carbon pricing favors technical skills that women have been historically discouraged from obtaining. Other qualitative or descriptive findings from Kazakhstan support this interpretation. Women have been more successful in entering higher-skilled specialist positions than industrial occupations, some of which women were still legally prohibited from accepting (Ergon, 2020) until women's occupation restrictions in Kazakhstan were finally repealed in October

[1] We obtain results when extending the pre-treatment period back to 2013 and the post-treatment period to 2022, but we focus on the 2016-2019 sample given our greater confidence in the identification strategy over this period, whereas the larger sample spans several ETS schemes.

2021. Additionally, the ETS system has not initially succeeded in providing incentive for upgrading from Soviet-era technology (Howie & Atakhanova, 2022), with which men have greater experience due to the legacy of gender discrimination. The results illustrate well the importance of the environmental-social interaction (Gloor et al, 2002), since correcting one environmental distortion exacerbates an existing social distortion, absent a policy package addressing both distortions.

Our wage results also reveal a heterogeneity in occupational outcomes, as there is a positive increase in the relative wages for female administrative workers. For all occupations other than industrial workers, we find no statistically significant effect on employment or vacancies. Results are consistent with a fall in labor supply for women in those roles or with our initial hypothesis that carbon pricing increases labor demand for women with greater skills in environmental administration.

Our study makes a unique contribution by exploring the women's employment effects of a carbon price in the energy sector, using a DiD research design with firm-level variation in the binding of the ETS. A recent literature uses firm-level data and DiD research design based on the staggered regional rollout of China's ETS, but this literature looks at firm total factor productivity (Wu & Wang, 2022), innovation (Ren et al., 2023) and the labor income share (Xiao et al., 2023), not female labor market outcomes. The paper also relates theoretically and methodologically to a literature on how liberal trade policy affects women's labor market outcomes (Berik et al., 2004; Menon & Rodgers, 2009; Juhn et al., 2013, 2014; Banerjee et al., 2022). Existing studies of Kazakhstan and women's labor market outcomes by occupation in energy rely on more descriptive analysis and do not focus specifically on the ETS (Ergon, 2020; Atakhanova & Howie, 2022; UNDP 2023). Existing studies of ETS in Kazakhstan focus on explaining the ineffectiveness to date of Kazakhstan's ETS in reducing emissions (Howie et al., 2020; Howie & Atakhanova, 2022; Howie & Akhmetov, 2024) rather than labor market outcomes.

The paper proceeds as follows. Section 2 provides a theoretical framework and background on Kazakhstan's ETS. Section 3 discusses our data and our construction of the firm-level measure for whether the ETS is binding. Section 4 presents our empirical strategy. Section 5 presents the main results, robustness checks, and placebo specifications. Section 6 concludes by discussing the broader implications of our results for policy, in Kazakhstan and beyond.

2. Theoretical Framework and Background

2.1 Theoretical framework

The theoretical framework for how carbon pricing impacts factor outcomes builds on fundamental producer theory: a higher carbon price would cause firms to substitute from more carbon-intensive activity to less carbon-intensive activity, and higher factor prices can result for workers skilled in the less carbon-intensive activity (Shang, 2023). The question then remains, whose skills? The skill bias of available technology and distribution of skills is often left exogenous in theory and needs to be calibrated by available empirical evidence.

One set of empirical studies implies technological parameters and skill distributions such that carbon pricing would increase firm demand for women's skill mix in certain specialist occupations in energy. Empirical evidence from

Europe supports that climate policies have been biased in favor of specialist's wages (Marin & Vona, 2019). Evidence from Kazakhstan is that women have invested in education for these specialist occupations in energy relative to more industrial occupations (Ergon, 2020). Given such an occupation-gender specific demand increase, our first hypothesis is that women's employment and wages would increase for non-industrial occupations.

A second set of empirical studies implies parameters such that carbon pricing would reduce firm demand for women's skill mix in industrial occupations. If carbon pricing incentives are too weak, firms may respond to carbon constraints by seeking more efficiency in their incumbent technologies rather than adopting new green technology. These incentives have been a central concern for decades in policy design (Montero, 2002) and adoption can be heterogenous within countries such as China (Ren et al., 2022). For our specific context of Kazakhstan, prior study suggests incentives for technology upgrading were weak (Howie & Atakhanova, 2022; Howie & Akhmetov, 2024). Given historical discrimination against women in manual occupations, including legal prohibitions, ETS could then favor male skills. Women's investment in skills for specialist occupations rather than manual occupations would then reflect a skill mismatch. Given such an occupation-gender specific demand decrease, our second hypothesis is that women's employment and wages would decrease for industrial occupations.

The two hypotheses just described have conflicting effects on women's aggregate employment and wages. Aggregate effects could go in either direction depending on which of the two occupation-specific forces dominate.

In line with our empirical study that focuses on two years of an ETS policy, the theoretical argument just discussed has a short-run focus where gender skill levels are exogenous and there is no endogenous skill acquisition. Carbon pricing could ideally influence longer-run investment in green skills, but this channel is absent here in our time horizon.

The theoretical arguments also disregard any policy adjustment complementary to ETS, which is appropriate for our empirical setting of a middle-income economy. Carbon pricing can be included in policy packages that address social goals in addition to environmental goals, though this is more common for states with greater capacity than middle-income economies (Jotzo & Azhgaliyeva, 2022). Though women's employment in energy in Kazakhstan is a social goal pursued by organizations such as the industry group KazEnergy and the European Bank for Reconstruction and Development (Ergon, 2020), we are not aware of evidence that consideration of gendered outcomes of carbon pricing has influenced policy in our context of Kazakhstan. We come back to this point in the conclusion.

2.2 Background on Kazakhstan's ETS

This section, derived in part from various reports (DEHSt, 2017; ICAP, 2022), provides details on Kazakhstan's emission trading scheme relevant for the analysis.

Kazakhstan's Emission Trading Scheme (KazETS) stands as a significant policy measure in the country's efforts to regulate and reduce greenhouse gas (GHG) emissions. The legislative groundwork for the KazETS was established in 2007 when the Kazakh government introduced the Environmental Code, mandating specific companies to prepare and report annual GHG emissions inventories starting in 2008. The formal establishment of the KazETS occurred in December 2011, following the President of Kazakhstan's signing of a law that added a chapter to the Environmental Code, thereby officially launching the scheme in January 2013 and the implementation of the first National Allocation Plan (NAP).

The National Allocation Plan (NAP) is the foundational instrument of the KazETS, covering large firms in the most energy-intensive sectors of the economy, specifically those whose emissions exceed 20,000 tons of CO2 annually. The NAP details the quotas and reduction commitments for these firms. For example, in 2013, 178 firms were allocated 147,190,092 tons of CO2 quotas with a 0% reduction commitment from the base year 2010. By 2015, the number of enterprises included decreased to 166, with a slight reduction commitment of 1.5% based on 2011-2012 levels.

The allocation of quotas during the first two phases from 2013 to 2015 was based on historical averages, specifically referencing baseline years of 2010 for the 2013 pilot phase and the average values from 2011 to 2012 for Phase 2 covering 2014 to 2015. This approach was applied uniformly to ETS sectors such as energy, coal, oil and gas, and manufacturing. Despite the early establishment of the ETS, this period highlighted significant gaps, including inaccuracies in the Environmental Code, potential for additional free quota allocations, calculation errors and a lack of verifiers (Yessekina, 2021).

The ETS was temporarily suspended in April 2016. In addition to the issues specific to ETS mechanics, there was significant opposition from industry stakeholders (Howie & Atakhanova, 2022), especially following the global decline in oil prices from 2013-2016 and the end of Kazakhstan's fixed exchange rate regime in 2015. The period of 2016 and 2017 was spent refining the approach. Refinements affected market oversight, the distribution of quotas, and the rules for trading (PMR, 2017).

The ETS resumed with a refined third phase in January 2018, concluding a 21-month suspension. Phase 3 introduced benchmarking as a key method for quota allocation, aligning Kazakhstan's practices with international standards. Firms were given the flexibility to choose between the historical method and the benchmarking method, with the latter representing a specific volume of greenhouse gas emissions per unit of production across various sectors. 52 specific benchmarks were developed, providing a more precise allocation of quotas. Notably, two-thirds of the installations included in the third NAP chose benchmarking, while the remaining one-third opted for the historical method for quota allocation. By 2021, Kazakhstan fully transitioned to the benchmarking method.

3. Data

This section details our main data sources and our measure of firm-level energy intensity constructed from confidential data. We then detail how we use our energy intensity measure to construct our indicator of whether the ETS is binding in 2018. This indicator is the key variable in forming the treatment and control groups in our DiD approach.

3.1. Source data

We retrieve a list of firms included in the KazETS from the third National Allocation Plan for 2018-2020 (Resolution No. 873, 2017). The National Allocation Plan provides the total volume of greenhouse gas emission quotas by regulated sectors (energy, oil and gas, mining & manufacturing) and a list of companies with allocated emission quotas. By tracking each firm's operator name using other public sources, we retrieved each firm's Business Identification Number (BIN). The BIN codes are then entered into the Bureau of National Statistics (BNS, 2024a) Business Register Database to obtain detailed information about the firms, including size, industry, location, and ownership.

We use this data to align the firms with confidential firm-level data (BNS, 2024b). For every industry included in the ETS, we identify the number and types of firms, their precise locations, 5-digit NACE Rev.2 codes, and firm sizes. Given that the firms included in the ETS are typically large and relatively few in each location, tracking them was relatively straightforward. Consequently, our final dataset exclusively focuses on firms included in the ETS.

Our confidential data is structured as a balanced panel, allowing us to include firms that were part of the dataset prior to the ETS binding period starting in 2018. This approach prevents the loss of firms from earlier years, ensuring continuity and stability in our analysis.

The number of matched firms, shown in Table 1, is less than the list provided in the National Allocation Plan due to several factors. Firstly, we were unable to find business identification numbers (BINs) for some firms. Secondly, the confidential data we used was restricted to firms with more than 100 employees, excluding smaller firms. Lastly, the use of a balanced panel resulted in the exclusion of certain firms that may have changed their legal status, thus they were omitted from our analysis.

The confidential datasets are provided on an annual basis by the Bureau of National Statistics of Kazakhstan. The first dataset is derived from 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. This aggregation reduces complexity and focuses on broader trends within the labor market. Additionally, grouping similar occupations together aids in identifying overarching patterns and relationships, providing a clearer understanding of employment dynamics and wage structures.

The second dataset is compiled from production reports submitted by firms and provides information on firm input costs, including energy costs, fuel costs, and material costs as well as firm revenue measured by sales. We also use vacancy data from the BNS, detailing the number of available vacant positions each year across various occupations. These occupations correspond to those in the National Classification of Occupations, and similar groupings are applied to aggregate different occupations.

3.2. Energy intensity and ETS binding measures

To identify whether ETS is binding or not, 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. $fuel\ costs_{it}$ cover the costs of fuels like gasoline, diesel, used in the production process and $total\ output_{it}$ is a measure of production in terms of sales generated.

We calculate this measure for each firm on an annual basis. We then compare the energy intensity measure of 2018 to the average energy intensity for the reference period of 2013-2015. The choice of 2018 as the comparison year is justified because it marks the first year after the suspension period of 2016-2017, allowing us to capture the initial effects of the re-implementation of the ETS. By subtracting the 2018 energy intensity from the reference period average, we can determine whether firms are bound by the ETS. 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. Firms where the difference in energy intensity equals zero are excluded from the analysis.[2] Our approach then captures whether ETS is binding based only on comparing 2018 emissions to the 2013-2015 reference period and not to benchmarks. Our binding ETS group includes some firms that could relax constraints by selecting benchmarks, and the inclusion of such firms could only attenuate our estimates of the effects of binding ETS.

For our main analysis, we restrict the sample to the period 2016-2019 to mitigate the influence of macroeconomics shocks such as the collapse of Kazakhstan's

[2] Five firms were dropped with the difference equal to 0. The exclusion does not affect our analysis, as these firms collectively account for 1 percent of total employment.

fixed exchange rate regime in 2015 and the COVID-19 pandemic in 2020. Each of these changes directly impacted the proper functioning ETS scheme, as penalties for violating allowances were ultimately not realized after either Phase 2 ended in 2015 or Phase 3 ended in 2020 (Howie & Akhmetov, 2024). By focusing on 2016-2019, we have a clean distinction between the suspension period of 2016-2017 and the first two years of Phase 3 in 2018-2019, in all covering four years of relatively stable macroeconomic conditions for Kazakhstan.

Table 2 provides summary statistics for the main measures used in the analysis. We see that in terms of employment shares, the treatment and control groups are relatively balanced. The control group though has an average larger firm size, a concern that we will ultimately need to address in our empirical analysis.

4. Empirical Strategy

4.1. Estimation approaches

We focus on a difference-in-differences (DiD) approach using Poisson quasi-maximum likelihood estimation (Poisson QMLE), also known as Pseudo-Poisson Maximum Likelihood (PPML). Such an exponential mean approach that restricts the conditional mean to be non-negative is natural for count data, such as employment data. Our regression functional form assumptions are thus well-justified, as DiD best practice demands (Roth et al, 2023). Furthermore, we encounter zeroes when considering firm-level occupation data, which prevents a log transformation of employment. PPML is a widely accepted solution to zeroes in the dependent variable, including in the context of DiD estimation (Chen & Roth, 2024), PPML also 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.

To evaluate whether female employment decreased in ETS-binding firms during the years 2018-2019, compared to the preceding period of 2016-2017, we focus on the following estimating equation:

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

where y_{it} is firm employment (female or male employment, either aggregate or for a specific occupation) or firm wages (female or male wages)[3] in a given year (t). The ETS_i measure at the firm level remains time-invariant and is constructed using information from the firm-level energy intensity measure described above. $POST_t$ is a binary indicator for restoration of ETS measures ($POST_t$=0 for t in 2016-2017 and $POST_t$=1 for t in 2018-2019). Lastly, α_i indicates firm fixed effect, λ_t year fixed effects, and ε_{it} the error term. The error term correctly lies outside the exponential, so the exponential term reflects the conditional mean of y_{it}. The coefficient of interest is γ, which is the average treatment effect on the treated ETS group over the post period.

We estimate Equation (1) using both unweighted and weighted regressions. The weighted regressions use employment weights based on levels from 2016, the initial period of our analysis.[4] Our descriptive statistics reveal substantial differences in firm sizes across the two groups, so the results may be distinct.

[3] Wages are adjusted for inflation using the Consumer Price Index from the BNS.
[4] Using employment data from this initial period for weighting helps to avoid potential distortions that could arise if the weights were based on periods impacted by the treatment effects.

Each approach offers distinct advantages. Weighted regressions capture aggregate effects for wages and employment. Unweighted regressions capture effects for the average firm and avoid excess influence of large-firm effects.

An additional model that we estimate is 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 reflecting female employment. Now η is the main coefficient of interest as it captures the effect of ETS for female workers relative to male workers.

For inference in all cases, we cluster standard errors at the firm level, as is standard for DiD since the treatment variable ETS_i varies at firm level (Roth et al, 2023).

4.2. Identification Strategy

Validity of the DiD approach requires satisfaction of the assumptions of no anticipation and of parallel trends. The paper introduction justifies no anticipation at length, so we focus here on parallel trends. As is standard, we run event study regressions to assess the existence of pre-trends, the absence of which are necessary but not sufficient to satisfy the parallel trends assumption. Ultimately, the question is whether the control firms' evolution over the post period is a relevant counterfactual for the treated firms. The results in Table 2 suggest overall balance in the employment shares. Control firms are distinctly larger, and this motivates robustness checks to address this concern. One approach we take is to trim the largest firms, while a second approach is propensity score matching. We defer discussion of method specifics to Section 5.2.

A main threat to our identification strategy would be a firm economic variable correlated with our binding ETS indicator, which then causes the firm to be more subject to certain shocks specific to 2018 and 2019 and not 2016 and 2017. One such concern may be that our ETS indicator is correlated with energy intensity, and our treatment effect estimate is driven by the greater impact of rising oil prices in 2018 and 2019 on the treatment group relative to the control group. To address such threats, we conduct placebo specifications for the treatment and control groups, to corroborate that our ETS indicator captures economic consequences of binding ETS rather than spurious relationships with other economic factors.

5. Results

5.1. Main results

We present in Table 3 our first main results on how the binding ETS impacts employment by gender for the 2016-19 period. Column A estimates the DiD specification of Equation (1) for female employment, and column B does so for male employment. For female employment, the effect of the binding ETS is statistically significant at 1% level. For male employment, we fail to reject the null hypothesis of no effect.

We conduct an event study specification to assess the absence of pre-trends, essential to justifying the parallel trends assumption in our DiD approach. Figure 2 presents the pre-trends analogous to the results presented in columns A and B of Table 3, for female and male employment. We estimate the interactions between

ETS and each year 2016, 2018, and 2019, relative to 2017, while incorporating firm and year fixed effects. Our analysis shows no 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 observed in ETS binding firms.

Though the female employment effect of ETS is statistically distinct from zero, 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 C of Table 3. Here the main coefficient of interest is the triple interaction. We find lower female employment than male employment to be statistically significant at the 5% level.

So among our competing hypotheses for whether the ETS effect on women's employment is positive or negative, our results support the conclusion that ETS in Kazakhstan further amplified forces discouraging women's employment in energy, more than favoring the environmental skill sets of women. Thanks to our data on gender and occupation, we can further explore more specific forces.

Table 4 reports the results for employment broken down by our 5 occupation categories. We find a negative and statistically significant effect for the female share of industrial workers, and we fail to reject a null effect on employment for the other occupations. Industrial employment is an occupation where women have been historically underrepresented in energy (Ergon, 2020). Our result is consistent with the binding ETS exacerbating this underrepresentation.

To more fully interpret what drives the fall in employment in industrial workers, we explore wage results by occupation and in the aggregate in Table 5. Here we find that binding ETS leads to no statistically significant change in wages overall, but there is a decline in relative wages of female industrial workers within the firm. The decrease in employment and in wages for female industrial workers is then theoretically consistent with a decline in relative labor demand for female industrial workers, as anticipated by the theory of skill mismatch detailed in Section 2.1.

To further interpret the decline in labor demand, we consider the effects of the ETS on vacancies by occupation in Table 6. We find an increase in vacancies for the industrial worker occupation overall, even though our female employment and wage results are consistent with a fall in relative labor demand for female industrial workers. The results of Table 6 then further corroborate that the binding ETS amplifies a skill mismatch for female industrial workers.

Among other results in Tables 4-6, the remaining finding of interest is a statistically significant increase in the share of female administrative workers, who have historically been well-represented among women (Ergon, 2020; Atakhanova & Howie, 2022). We observe no statistically significant effect of employment or vacancies for administrative workers. The wage increase could then be consistent with greater labor demand for female administrative workers or lower labor supply, or a combination of the two. A fall in labor supply could result if administrative jobs in ETS are relatively less appealing, though we have no evidence corroborating this channel. The greater demand for women's administrative labor is consistent with our Section 2.1 theory (and other evidence) that ETS promotes some green firm activity for which women have greater skills. Our results imply though that these skill demands are heterogenous by occupation and isolated to administrative work.

5.2 Robustness

5.2.1 Trimmed Sample and PSM-DiD

Our first set of robustness checks focus on two approaches to address conern about imbalances in the sample: trimming the sample and propensity score matching. Though our descriptive statistics (Table 2) reflect balance between the treatment and control groups in terms of various employment shares by gender and occupation, the average firm size by employment is twice as high in the control group compared to the treatment group. This imbalance raises questions about whether the control group could be a reasonable counterfactual to the treatment group, imperiling our parallel trends assumption. We take two approaches to address concern about balance.

Our approach to trimming is to repeat analysis excluding the top 5 percent of firms by employment size. As a result, 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 (Table A1a).

For a second set of robustness checks using propensity score matching, our approach builds on the PSM-DiD of Xiao et al. (2023) in evaluating how China's ETS affects labor's share of income. We follow these authors 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. Inclusion of variables related to firm size and female leadership as potential influences on women's labor market outcomes has a basis in 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 all, 100 of 108 firms match, while 8 are unmatched due to lack of common support or failure to find a match with a suitable threshold. Table A1b shows results of matching and null t-tests confirming the suitability of the match.

Estimating using either trimmed sample or PSM-DiD, our results remain largely consistent. Specifically, we continue to observe a negative and statistically significant effect on female employment in ETS-binding firms following the restoration of ETS (Table A2). This adverse impact on female employment is still particularly concentrated in industry occupations (Tables A3a, A3b). We continue to find a negative wage effect for females in industry occupations and a positive wage effect for females in administrative occupations (Tables A4a, A4b) for the treated firms.

5.2.2 Expanded post-treatment period

For additional perspective, we extend our sample to the latest available data, 2016-2022, while expanding the post period to 2018-2022. We anticipate there could be fewer long-run and persistent effects of the binding ETS in 2018, given that the ETS ultimately did not result in penalties in 2020. There is also additional noise from the COVID-19 pandemic and then the Russia-Ukraine war, and additional ETS schemes both before and after the ETS phase 3 that is our focus. Our aggregate results for employment by gender during the expanded period are in Table A5, while the occupation employment share results are in Table A6a, and the relative wage results are in Table A6b. We find that our main results for total employment and industrial workers still hold, though the positive effect on employment for administrative workers is not persistent. We interpret these longer-run results with some more caution, however, considering the potential complications related to the prior and subsequent ETS schemes and how they relate to our treatment.

5.2.3 Placebo tests

To confirm that our ETS measure is capturing meaningful variation related to the binding ETS and not other economic factors that would threaten our identification (as discussed in Section 4.2), 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 specification, we define the treatment group by selecting the top 48 firms based on top energy intensity in 2018, rather than the 48 firms with binding ETS. This second placebo specification is designed to confirm that our results are not just driven by selecting firms with higher energy intensity. Each of these alternative specifications yields no statistically significant results (Table A7). The absence of results in the placebo specifications strengthens the validity of our results, as they corroborate that our main effects are attributable to ETS policy rather than spuriously driven by external factors.

5.2.4 Atlernative definition of energy intensity

To ensure that our results are not sensitive to our definition of the ETS binding and ETS non-binding groups, we consider the defining the groups using an alternative measure of energy intensity, energy costs divided by total costs. Results in Table A8 remain similar to our original results from Table 2.

5.2.5 Additional controls

Lastly, to ensure that our results are not driven by differences between groups other than ETS, we consider a richer set of controls. The first set of controls that we consider is interactions between the oil price and major sector (3 categories), which addresses a concern that group differences may be driven by group sector compensation and oil price sensitivity. The second set of controls that we consider is various firm characteristics (the same set that we consider for the PSM) interacted with year fixed effects. This approach addresses the concern that differences in the evolution over time of effects based on the initial set of characteristics of each group could be driving results. In Table A9, we estimate our main employment specifications while adding each set of controls and then both sets of controls. All results are fully stable and consistent with Table 2.

6. Conclusion

We conclude by discussing the broader relevance of our results for the economics of gender, energy, and related policy. Our main findings are that carbon pricing reduces women's employment in the energy sector, and that these effects are driven by a decline in employment for female industrial workers. Given that female industrial workers exhibited a decline in wages even though industrial worker vacancies increased, the results are consistent with the ETS exacerbating a skill mismatch between the labor demand of the energy firms and the labor supply of female industrial workers. Our finding is in some respects specific to the context of our research setting of Kazakhstan. Specific features of Kazakhstan plausibly relevant to our findings include the legacy of discrimination against female industrial workers in the energy sector in Kazakhstan, and the weak incentives of Kazakhstan's ETS to encourage upgrading for Soviet-era technologies that could better attract women who have invested in education as skilled specialists.

Though results are inevitably specific in some respects to our research setting of Kazakhstan, the implications of our results extend far beyond. The legacy of de facto and de jure discrimination against women in industrial occupations is not unique to Kazakhstan, and legal barriers to women's occupations are so widespread globally that the World Bank releases an annual report on the subject (World Bank, 2024). Even if de jure discrimination ends for women entering certain specific occupations, as was the case for Kazakhstan in October 2021, the gender consequences can be persistent long after the legal barriers end, for reasons such as lasting advantages in male experience. Likewise, the challenges of an ETS to encourage more technological upgrading is hardly unique to Kazakhstan (ICAP, 2022).

Our empirical results are specifically relevant for policy packages that include carbon pricing with both environmental and social goals (Jotzo & Azhgaliyeva, 2022). Our results highlight the empirical relevance of a social distortion in gender that should motivate policy packages that weigh gender or even directly target gender. Our results should not be interpreted naively as arguing that environmental interests must come at the expense of women, but rather the need to avoid tunnel vision in focusing on a single policy targeting a single distortion. The idea that first-best policy includes a policy mix that addresses all relevant social distortion dates to the targeting principle of Bhagwati and Ramaswami (1963). The original motive of the targeting principle was to argue against distorting trade policy to achieve domestic social objectives. Likewise, our results are a call for policy packages that best target both environmental and social distortions, not a blanket endorsement of weaker climate policy to achieve gender goals. As Stiglitz (2019) notes in the context of carbon policy, optimal policy overall will depend on which policies are available to governments, and states in practice are incapable of achieving the first-best ideal through available policies. In a second-best world, the tradeoffs of multiple policies in addressing multiple distortions must be weighed.

So the broader lesson of our results is that social distortions exacerbated by carbon pricing are empirically relevant and need to be weighed in tradeoffs in designing policy packages. In the context of Kazakhstan, our results imply a greater social cost of the first 3 ETS phases failing to achieve more technology upgrading and in delaying the removal of occupational restrictions on women until 2021. Ex-ante knowledge of our findings could have tilted tradeoffs in favor of stronger ETS and better policy toward women in the late 2010s. Even though all states may not share the specific social distortions of Kazakhstan, other states will have their own social distortions that need to be considered in policy tradeoffs.

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Figure 1. Evolution of Female Employment Share Difference: ETS-Binding vs. ETS-Non-Binding Groups

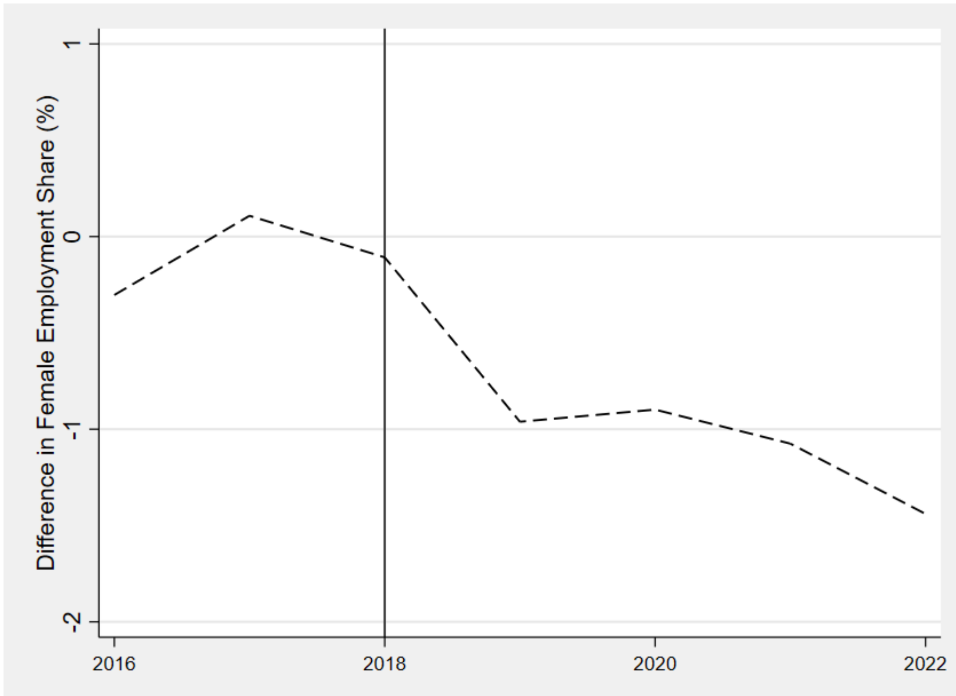

Figure 2. Event study regression analysis for the effects of binding ETS on employment

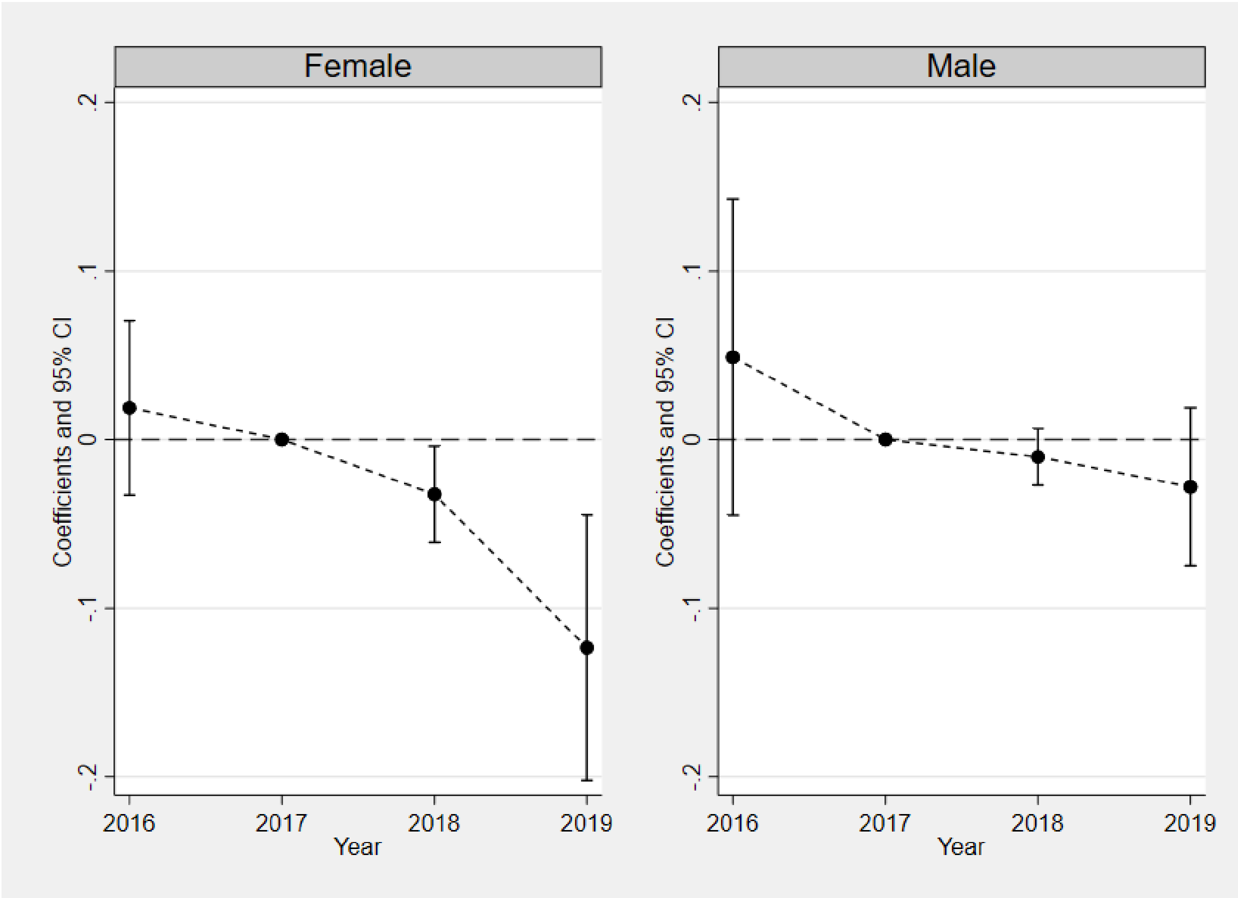

Table 1. KazETS firms included in the analysis

Industry	Number of firms in the National Allocation Plan 2018-2020	Number of firms matched with confidential firm-level data
Energy	52	45
Oil & gas	39	32
Mining and manufacturing	39	36
Total	130	113

Table 2. Summary statistics

	All firms N=432			Control N=240			Treatment N=192		
	mean	median	s.d.	mean	median	s.d.	mean	median	s.d.
A. Employment									
Employment	1714	774	3369	2200	769	4363	1105	829	1065
Female employment	397	184	766	511	197	990	254	164	246
Female employment share	0.266	0.227	0.153	0.274	0.242	0.160	0.255	0.217	0.144

Female managers share	0.213	0.183	0.163	0.221	0.181	0.176	0.203	0.195	0.143
Female specialists share	0.462	0.459	0.181	0.472	0.484	0.177	0.449	0.425	0.186
Female industry workers share	0.146	0.127	0.153	0.147	0.138	0.150	0.145	0.112	0.156
Female administrative workers share	0.754	0.833	0.274	0.767	0.833	0.255	0.739	0.833	0.295
Female unskilled workers share	0.578	0.609	0.311	0.577	0.569	0.291	0.580	0.961	0.336
B. Wages									
Average wages	141.575	106.795	111.216	150.426	103.893	130.467	130.511	111.428	79.940
Average female wages	124.121	89.165	97.590	128.949	83.570	110.639	118.086	90.744	78.153
Average female manager wages	259.161	204.825	172.891	263.976	204.825	185.201	252.868	203.636	155.639
Average female specialist wages	131.487	102.215	98.0294	135.147	101.744	108.281	126.947	102.764	83.641
Average female industry worker wages	98.511	72.801	78.256	99.900	71.477	86.426	96.816	73.636	67.185
Average female administrative wages	87.797	70.758	54.722	91.993	72.201	59.773	82.766	68.893	47.666

Average female unskilled worker wages	57.075	48.877	34.905	57.302	49.211	35.739	56.769	48.245	33.881
C. Other firm characteristics									
Energy intensity	0.181	0.100	0.208	0.148	0.091	0.170	0.221	0.124	0.241
Energy costs per worker	9359.705	1778.913	36994.27	10740.05	1484.059	47122.37	7634.275	2370.141	17444.54
Log of tangible assets	16.269	16.348	2.248	16.272	16.208	2.283	16.266	16.613	2.211
Ownership	2.116	2.000	0.482	2.075	2.000	0.487	2.167	2.000	0.473
Return on assets	2.672	0.320	34.687	0.923	0.504	1.798	4.776	0.183	51.443
Ratio of assets to workers	85373.14	13519.53	246271.6	76298.75	12115.59	190098.3	96338.03	16376.04	300652.9

Source: Authors' calculations based on 2016-2019 firm-level data. N reflects firm-year observations. Wages are presented in thousand tenge (KZT), calculated on a monthly basis and deflated using CPIs.

Table 3. Employment effect

	Female (A)	Male (B)	Pooled (C)
$ETS_i \times POST_t$	-0.086*** (0.027)	-0.046 (0.031)	-0.014 (0.019)
$ETS_i \times POST_t \times GENDER_g$			-0.069** (0.028)
Pseudo-R^2	0.9939	0.9932	0.9926
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. 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 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. Standard errors clustered by firms are in parentheses. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table 4. Employment effects by occupations

	Manager	Specialists	Industry	Admin	Unskilled
$ETS_i \times POST_t$	0.035	0.008	-0.206*	0.008	0.024
	(0.059)	(0.032)	(0.112)	(0.048)	(0.073)
Pseudo-R^2	0.0798	0.0461	0.1334	0.0485	0.0793
Obs.	412	431	375	362	346

Notes: The unit of analysis is firm-years, run from 2016-2019. Each ETS measure in the table varies at the firm level and is interacted with the treatment years of 2018 and 2019, separately. Each column includes firm fixed effects and year fixed effects. Standard errors clustered by firms are in parentheses. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table 5. Female wage effects by occupations

	Manager	Specialists	Industry	Admin	Unskilled
$ETS_i \times POST_t$	-0.041	0.019	-0.073**	0.049***	0.002
	(0.043)	(0.021)	(0.031)	(0.019)	(0.027)
Pseudo-R^2	0.0161	0.0048	0.0115	0.0024	0.0059
Obs.	405	428	347	359	318

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

Appendix Tables

Table A1. Summary statistics for sample excluding 6 largest firms

	All firms N=408			Control N=220			Treatment N=188		
	mean	median	s.d.	mean	median	s.d.	mean	median	s.d.
<u>*A. Employment*</u>									
Employment	1053	691	1021	1053	650	1025	1054	763	1020
Female employment	245	167	243	240	165	251	251	168	234
Female employment share	0.267	0.227	0.157	0.269	0.230	0.171	0.264	0.226	0.140
Female managers share	0.217	0.190	0.166	0.210	0.182	0.181	0.226	0.2	0.147
Female specialists share	0.458	0.455	0.185	0.461	0.470	0.193	0.455	0.437	0.177
Female industry workers share	0.144	0.118	0.157	0.144	0.102	0.175	0.145	0.130	0.135
Female administrative workers share	0.757	0.857	0.279	0.757	0.882	0.291	0.756	0.833	0.267
Female unskilled workers share	0.572	0.581	0.317	0.565	0.569	0.313	0.579	0.644	0.322

B. Wages

Average wages	139.693	103.378	111.155	147.854	92.653	132.551	130.511	111.429	79.940
Average female wages	123.408	88.667	97.384	128.139	82.326	111.710	118.086	90.744	78.153
Average female manager wages	256.605	200.617	173.627	259.797	192.730	187.955	252.868	203.636	155.639
Average female specialist wages	130.127	101.661	97.615	132.978	100.558	108.749	126.947	102.764	83.641
Average female industry worker wages	96.977	71.255	76.687	97.127	67.646	84.816	96.816	73.636	67.185
Average female administrative wages	86.858	70.697	54.456	90.739	73.057	60.075	82.766	68.893	47.666
Average female unskilled worker wages	56.405	46.488	35.438	56.096	44.733	36.806	56.769	48.245	33.881

C. Other firm characteristics

Energy intensity	0.184	0.100	0.213	0.151	0.087	0.177	0.221	0.124	0.241
Energy costs per worker	9771.153	1646.43	38028.51	11670.6	1298.584	49594.2	7634.275	2370.141	17444.54
Log of tangible assets	16.099	16.217	2.198	15.946	15.865	2.181	16.266	16.613	2.211
Ownership	2.113	2.000	0.488	2.065	2.000	0.497	2.167	2.000	0.473
Return on assets	2.787	0.301	35.714	0.942	0.518	1.890	4.776	0.183	51.443
Ratio of assets to workers	89351.4	13149.59	253011.9	82902.2	9361.096	199747	96338.03	16376.04	300652.9

Source: Authors' calculations based on 2016-2019 firm-level data.
Notes: Six firms are excluded for the control group, and one firm is excluded from the treatment group.
Wages are presented in thousand tenge (KZT), calculated on a monthly basis and deflated using CPIs.

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 share	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		
	Female	Male	Pooled	Female	Male	Pooled
$ETS_i \times POST_t$	-0.078***	0.005	-0.001	-0.087***	-0.050	-0.020
	(0.030)	(0.037)	(0.033)	(0.027)	(0.032)	(0.018)
$ETS_i \times POST_t \times GENDER$			-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. 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 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.Standard errors clustered by firms are in parentheses. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A3a. Female employment effects by occupations: excluding largest firms

	Manager	Specialists	Industry	Admin	Unskilled
$ETS_i \times POST_t$	0.030	0.005	-0.215*	0.011	0.010
	(0.061)	(0.035)	(0.119)	(0.051)	(0.079)
Pseudo-R^2	0.0811	0.0485	0.1423	0.0504	0.0831
Obs.	388	407	351	338	324

Notes: The unit of analysis is firm-years, run from 2016-2019. We exclude the largest six firms. Each ETS measure in the table varies at the firm level and is interacted with the treatment years of 2018 and 2019, separately. Each column includes firm fixed effects and year fixed effects. Standard errors clustered by firms are in parentheses. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A3b. Female employment effects by occupations: PSM-DiD

	Manager	Specialists	Industry	Admin	Unskilled
$ETS_i \times POST_t$	0.070	0.008	-0.201*	0.013	0.037
	(0.059)	(0.034)	(0.116)	(0.051)	(0.073)
Pseudo-R^2	0.0604	0.0461	0.1346	0.0501	0.0796
Obs.	384	399	359	340	327

Notes: The unit of analysis is firm-years, run from 2016-2019. The analysis employs a Propensity Score Matching with DiD (PSM-DiD) approach. Each ETS measure in the table varies at the firm level and is interacted with the treatment years of 2018 and 2019, separately. Each column includes firm fixed effects and year fixed effects. Standard errors clustered by firms are in parentheses. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A4a. Female wage effects by occupations: excluding largest firms

	Manager	Specialists	Industry	Admin	Unskilled
$ETS_i \times POST_t$	-0.058	0.015	-0.074**	0.041**	-0.001
	(0.044)	(0.022)	(0.033)	(0.017)	(0.029)
Pseudo-R^2	0.0163	0.0045	0.0113	0.0025	0.0060
Obs.	381	404	323	335	296

Notes: The unit of analysis is firm-years, run from 2016-2019. We exclude the largest six firms. Each ETS measure in the table varies at the firm level and is interacted with the treatment years of 2018 and 2019, separately. Each column includes firm fixed effects and year fixed effects. Standard errors clustered by firms are in parentheses. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A4b. Female wage effects by occupations: PSM-DiD

	Manager	Specialists	Industry	Admin	Unskilled
$ETS_i \times POST_t$	-0.046	0.029	-0.066**	0.047**	0.004
	(0.046)	(0.022)	(0.032)	(0.019)	(0.029)
Pseudo-R^2	0.0167	0.0045	0.0099	0.0024	0.0058
Obs.	377	397	331	337	301

Notes: The unit of analysis is firm-years, run from 2016-2019. The analysis employs a Propensity Score Matching with DiD (PSM-DiD) approach. Each ETS measure in the table varies at the firm level and is interacted with the treatment years of 2018 and 2019, separately. Each column includes firm fixed effects and year fixed effects. 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, 2016-2022

	Female	Male	Pooled
$ETS_i \times POST_t$	-0.087*	0.002	0.000
	(0.051)	(0.079)	(0.040)
$ETS_i \times POST_t \times GENDER_g$			-0.910**
			(0.040)
Pseudo-R^2	0.9902	0.9927	0.9941
Obs.	756	756	1512

Notes: The unit of analysis for columns (A) and (B) is firm-years, weighted by firm employment, run from 2016-2022, 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 2016-2022, with POST indicator defined as 1 for 2018-2019, and GENDER indicator defined 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. Standard errors clustered by firms are in parentheses. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A6a. Female employment effects by occupations, 2016-2022

	Manager	Specialists	Industry	Admin	Unskilled
$ETS_i \times POST_t$	-0.009	0.018	-0.184*	0.091	0.057
	(0.060)	(0.033)	(0.102)	(0.057)	(0.064)
Pseudo-R^2	0.0753	0.0482	0.1292	0.0415	0.0757
Obs.	721	755	674	647	605

Notes: The unit of analysis is firm-years, run from 2016-2022, with POST indicator defined as 1 for 2018-2022. Each ETS measure in the table varies at the firm level. Each column includes firm fixed effects and year fixed effects. Standard errors clustered by firms are in parentheses. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A6b. Female wage effects by occupations, 2016-2022

	Manager	Specialists	Industry	Admin	Unskilled
$ETS_i \times POST_t$	-0.008	-0.001	-0.077***	0.029	-0.010
	(0.035)	(0.017)	(0.029)	(0.023)	(0.021)
Pseudo-R^2	0.0159	0.0048	0.0089	0.0026	0.0057
Obs.	707	749	620	641	562

Notes: The unit of analysis is firm-years, run from 2016-2022, with POST indicator defined as 1 for 2018-2022. Each ETS measure in the table varies at the firm level. Each column includes firm fixed effects and year fixed effects. Standard errors clustered by firms are in parentheses. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A7. Placebo specifications

	Female (A)	Male (B)	Pooled (C)	Female (D)	Male (E)	Pooled (F)
	define ETS indicator using 2016 emissions rather than 2018			define ETS indicator as top 48 firms in 2018 energy intensity		
$ETS_i \times POST_t$	-0.009	-0.035	-0.039	-0.013	-0.034	-0.037
	(0.020)	(0.042)	(0.043)	(0.019)	(0.037)	(0.038)
$ETS_i \times POST_t \times GENDER_g$			0.037 (0.035)			0.029 (0.035)
Pseudo-R^2	0.9908	0.9927	0.9944	0.9908	0.9927	0.9944
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 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. 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. Standard errors clustered by firms are in parentheses. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A8. Employment effects of alternative energy intensity measure: energy costs as a share of total costs

	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 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. Standard errors clustered by firms are in parentheses. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A9. Employment effect, robustness results

	(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)
$ETS_i \times POST_t \times GENDER$			-0.066**			-0.059**
			(0.027)			(0.026)
Oil price#sector	Yes	Yes	Yes	No	No	No
Firm characteristics#year	No	No	No	Yes	Yes	Yes
Pseudo-R^2	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 characteristics#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 (https://fred.stlouisfed.org/series/POILBREUSDA), 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 includes both sets of controls from columns 1-6. The differences between the columns labeled (A), (B),and (C) are analogous to those in Table 2. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.