LUND UNIVERSITY
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Master's Programme in Economic Development and Growth

Do Environmental Policies Hurt Jobs?

Evidence from China's Regional Carbon Market Pilots

by

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Abstract Little is known about the labor market impacts of climate policies in emerging economies. This paper therefore analyzes the labor market impacts of eight regional Emission Trading Scheme (ETS) pilots that were implemented in different parts of China between 2013 to 2016. Applying a difference-in-differences strategy, results indicate that the policy hurt jobs by reducing working hours and employment. Additionally, I find some evidence for the notion that the policy was implemented at the expense of low- and medium-educated workers although this result is dependent on the labor market outcome under observation. These findings highlight the need for targeted support measures for displaced workers.

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1 Introduction

"A sound environment is the cornerstone of people's lives and health. Green development must be implemented and the strictest system of environmental protection should be adopted."

Xi Jinping in Beijing, China, August 2016

By signing the Paris Agreement in 2015, 197 parties agreed to combat the impending climate catastrophe. To reduce carbon emissions, an array of national climate policies has been adopted all over the world (LSE, 2019). Nevertheless, the fear of detrimental economic effects often undermines ambitious regulation efforts (Deschenes, 2011). Considering that labor is not only the most important source of income in the world but also an important constituent of people's identity, especially the labor market consequences of climate policies have been the subject of a heated controversy (Curtis, 2018). Critics of stringent climate policies caution against the consequences of an alleged "job-versus-the environment" trade-off (Morgenstern, Pizer, and Shih, 2002), while advocates retort that climate policies create new jobs in environmental-friendly sectors (Vona, Marin, Consoli and Popp, 2018). Both groups find arguments in the theoretical literature to support their reasoning: On the one hand, additional costs for pollution abatement can reduce output and employment (Berman and Bui, 2001). On the other hand, climate policies can trigger an increase in the labor intensity in the production process or spur innovation in environmental-friendly technologies generating new jobs (Deschenes, 2011). Thus, the question on the labor market impact of stricter climate policies remains an empirical issue which in the light of the rapidly increasing number of climate policies gains in importance.[1]

Developing and emerging economies face a particular delicate balancing act between climate and economic objectives (Hafstead and Williams, 2018). Led by China and India, they account for two thirds of global greenhouse gas emissions (Blackman, Li, and Liu, 2018). As they also bear the brunt of the adverse consequences of climate change - demonstrated for the Chinese context in the opening quote - in a number of emerging countries climate policies are gaining traction on the political agenda (Blackman, Li, and Liu, 2018). While weak policy designs and a lack of monitoring and enforcement render many of these attempts futile, this must not always be the case. As summarized in a review by Blackman, Li, and Liu (2018), several policies in India and China have led to notable emission reductions. As a large part of the workforce in these countries is highly vulnerable to economic shocks, the impact of these policies on labor market outcomes merits special attention (Reidt, 2020).

Hitherto, the empirical literature is almost entirely focused on developed countries with the overwhelming majority of studies exploring the effects of environmental programs in

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[1]Climate policies and laws have increased from a number of 72 globally in 1997 to 1,500 in 2018 (LSE, 2019).

the United States (US). These studies paint a mixed picture as the type of policy under observation and the particular context seem to play a role (e.g. Berman and Bui, 2001; Walker, 2011). The impact might differ for developing and emerging countries where labor markets operate in a different way (Hafstead and Williams, 2018). The only notable exception focusing on an emerging country is Liu, Shadbegian, and Zhang (2017). They conclude that stricter waste water regulations had a negative impact on employment in textile printing and dyeing enterprises in the Lake Tai Jiangsu region in China. However, the narrow geographical and sectoral focus of this study casts doubt on whether these results apply outside of this particular context. Thus, despite the considerable research effort made in recent years to better understand the multifaceted impact of climate policies on labor markets, little is known about these effects in emerging economies.

The purpose of this study is to bridge this gap by exploiting a unique policy experiment. In 2011, the Chinese government promulgated the implementation of several Emission Trading System (ETS) pilots. An ETS is a climate policy instrument which sets a limit on the total amount of emissions and creates a market where emission allowances can be traded. In China, the ETS pilots form separate markets and were established between 2013 and 2016 in eight provinces and cities. The pilot areas encompass a vast area of the country being home to 18% of the Chinese population and representing 30% of its GDP (Liu, Chen, Zhao and Zhao 2015). They were selected to represent the economic and geographical heterogeneity of the country (Han, Olsson and Lunsford, 2012). The fact that the rest of the country was virtually unaffected by the policy as well as the found emission reduction make the program an ideal case study to explore the labor market consequences of a well-functioning environmental policy in an emerging economy. More specifically, this paper aims to answer the following research questions:

What was the impact of the implementation of the ETS pilots in China on wages, hours worked, employment and unemployment in affected provinces until 2018?

To what extent are these effects heterogeneous across workers with different educational levels?

To answer these questions, I use individual level, panel data from the China Family Panel Studies (CFPS) and carry out a Difference-in-Differences (DiD) analysis. My findings show that the policy hurt jobs by reducing working hours and employment. Wages of people that continued working seem to be unaffected. Interestingly, while the reduction in hours worked disproportionately affected medium-educated workers, I find some evidence that the rise in unemployment is concentrated among low-educated workers.

The contribution this paper makes is threefold. First, to the best of my knowledge, it is the first thorough analysis of the labor market effects of a market-based climate policy in an emerging economy. This is particularly relevant as more and more emerging economies are experiencing with this type of policy in recent years. The implementation of a carbon tax in Chile in 2017 and the establishment of an ETS in Mexico in 2020

only provide two recent examples (ICAP, 2021b). Second, the usage of individual level data allows me to provide a more complete picture of the labor market consequences by estimating the effects for workers with different educational levels. This can shed light on the distributional effects of the policy as low-educated workers are on average poorer than their high-educated counterparts. Despite the importance of the distributional effects for the political acceptance of climate policies, to this date, the empirical evidence on this issue is scarce and again limited to the developed world (Yip, 2018). Ultimately, this paper contributes to the vast body of evidence discussing the impact of the ETS pilots on the Chinese economy. The plans of the implementation of a unified national ETS brought this issue to the forefront of the political debate (Liu, 2016). Notwithstanding the importance of the labor market concern in the debate on China's environmental policies (Liu, Shadbegian, and Zhang, 2017), to this date, no thorough analysis of the labor market effects of the ETS pilots has been carried out. Three recent papers include the *employment* effects of the ETS pilots in their analysis of general economic effects of the policy (Yang, Jiang, and Pan, 2020; Yu and Li, 2021; Zhang and Duan, 2020). Besides being limited to a single labor market outcome, these empirical investigations are fraught with difficulty (e.g. the usage of endogenous controls) or cover a very limited time period. Thus, to assist the design of policy measures accompanying and facilitating the implementation of a national ETS, further analysis of the labor market effects of the policy is needed.

The rest of this paper unfolds as follows. In Section 2, I will briefly review the empirical literature on the labor market effects of environmental policies and on studies exploring the consequences of the Chinese ETS pilots. I will also highlight some features of the Chinese labor market. This will be followed by the presentation of a conceptual framework in section 3 from which I will derive hypotheses in section 4. Section 5 and 6 are devoted to the data and the methodology, respectively and section 7 includes a presentation of the empirical results. Robustness checks are shown in section 8. The findings will be discussed in section 9 prior to a short conclusion in section 10.

2 Background

2.1 The Labor Market Effects of Environmental Policies

The labor market effects of environmental policies have received considerable attention from both policymakers and academics. Early empirical investigations in this field were limited to the US context. They explore changes in the stringency of air quality standards enacted in the US from 1970 to 1990. These policies forced polluting firms in counties that did not comply with national ambient air quality standards (non-attainment counties henceforth) to install costly pollution abatement equipment (Deschenes, 2011). Exploiting the geographical variation in this regulatory intensity, a widely cited study by Berman and Bui (2001) concludes that stricter air pollution regulations led to small employment gains

in regulated firms. They attribute this to higher labor requirements for pollution control. Importantly, their study is limited to the capital-intensive petroleum industry in Los Angeles and therefore lacks external validity. When looking at four affected industries at the national level, Morgenstern, Pizer, and Shih (2002), who proxy environmental regulations with firm expenditure on environmental compliance activities, arrive to a similar conclusion. However, their study relies on comparisons of firms within the same industry and cannot capture contractions of whole industries.

That stricter local air quality regulations actually led to jobs losses among regulated firms is suggested by Kahn (1997) and Greenstone (2002). Applying a DiD approach, they find that employment growth in regulated manufacturing firms in non-attainment counties lagged behind that of their non-regulated counterparts in attainment counties. Covering a period from 1972 - 1987, Greenstone (2002), for instance, ascertains that an amendment increasing the stringency of air quality regulation in 1977 was responsible for 590,000 job losses in the manufacturing sector. This represent 3.4% of total US manufacturing employment. While these estimates might sound alarming, they are likely to overestimate the true effects of the policy. If economic activity is relocated to unregulated firms or unregulated counties that form part of the control group, the employment impact is double counted (Hafstead and Williams, 2020). Evidence for geographical relocation of pollution-intensive firm activity to regions with less regulatory pressures has been brought forward by List et al. (2003) and Kahn and Mansur (2013) although results in the latter study are not robust to different functional form assumptions. Perhaps the most compelling evidence for the detrimental job effects of environmental policies is provided by Walker (2011) who is able to link detailed longitudinal establishment-level data to plant-level regulatory data. This allows him to consider labor market outcomes which cannot be double counted such as job destruction rates. His findings reveal that stricter regulations on US air pollution caused significant employment losses which are driven by lay-offs rather than by a reduction in recruitments. In a related study he estimates that these job losses incur considerable costs for workers whose foregone earnings amount to $5.4 billion (Walker, 2013). That environmental command-and-control policies - the direct regulation of an industry by regulation - do not necessarily trigger job losses is suggested by Cole and Elliott (2007). This study does not find a significant effect of environmental regulations - proxied by environmental protection expenditure - on industry employment in the United Kingdom (UK). Yet, the author caution that data limitations might inhibit them from detecting significant effects.

With market-based climate policies gaining momentum across the globe, recent studies expand the geographical scope and focus their attention on the employment effects of carbon taxes and ETS. This new literature is still in its infancy but, with few exceptions, primarily illustrates that these environmental policies have no or only modestly negative employment effects. Exploiting exogenous variations in the eligibility to a carbon tax reductions, Martin, De Preux, and Wagner (2014), for instance, find that a carbon tax in the UK did not impact overall employment. In a similar vein, Yamazaki (2017) posits that

a revenue-neutral carbon tax in British Colombia, Canada, did not reduce employment but rather let to small employment gains on an aggregate level. Interestingly, her study provides evidence for heterogeneous effects across industries: Jobs are lost in energy-intensive and trade-exposed industries which, facing severe import competition, are unable to pass on the incurred costs to consumers. This effect is more than offset by higher employment levels in the service sector which mainly benefits from increased consumer spending due to the redistribution of the tax revenue. Using individual instead of industry level data, Yip (2018) calls this result into question. Rather than pointing to overall employment gains, her results show that the same tax incremented the unemployment rate by around 1.3 percentage points. A possible reason for this diverging result might be the failure of the common trend assumption in the DiD analysis of the former study.

Similar to carbon taxes, generally, the labor market effects of cap-and-trade programs seem to be small. Ferris, Shadbegian, and Wolverton (2014) analyze the effects of a sulfur dioxide (SO_2) trading program on electric utility employment and cannot find any significant effect on employment. A budget trading program in nitrogen oxides (NO_x) in the US, in contrast, has been found to negatively affect manufacturing employment (Curtis, 2018) leading to employment losses of 1.3%. The only studies so far dealing with the labor market consequences of carbon emission trading programs are devoted to the European Union (EU) ETS program. This program was established in 2005 and, until the establishment of the Chinese national ETS in 2021, was the largest ETS in the world (Anderson and Di Maria, 2011). Marin, Marino, and Pellegrin (2018) find weak negative employment effects in the first phase (2005-2007) of the program which vanish in the second phase (2008-2012). They tentatively attribute the absence of strong employment effects to the low price of emission allowances and the possibility to pass on increased production costs to consumers. Moreover, wages remained unchanged across both phases which might reflect the high wage rigidity characterizing European labor markets. This result resonates with earlier studies which do not find any significant employment effects of the ETS on regulated manufacturing firms in Germany - the country with the largest market share (Anger and Oberndorfer, 2008; Wagner and Petrick, 2014). Wagner, Muuls, Martin and Colmer (2014), in contrast, show that this result does not hold for the French labor market. There, regulated manufacturing plants reduced employment by 7% in the second phase. The fact that the authors analyze the effect of the same policy with the same methodology (DiD with matching) as Wagner and Petrick (2014) suggests that besides the policy design, the economic structure and the labor market institutions of a country play an important role in the way labor markets are affected by the policy change.

A related strand of literature dealing with green innovation delivers a possible explanation for the absence of strong employment effects in certain contexts: Job creation in sectors that supply environmental-friendly technology and services might compensate for losses in polluting industries (Marin and Vona, 2019). There is not only strong evidence that environmental policies induce innovations in climate-friendly technologies (see Crespi, Ghisetti, and Quatraro, 2015, for a review) but recent evidence offers broad support for the

existence of a strong positive impact of this type of innovation on job creation (Horbach and Rennings, 2013; Licht and Peters, 2013; Gagliardi, Marin, and Miriello, 2016).

Akin to other secular trends such as the skill-upgrading induced by automation (Acemoglu and Autor, 2011; Giuntella and Wang, 2019), this shift in employment from polluting to green firms and sectors might change the skill demand creating winners and losers. Workers possessing the skills needed to perform green jobs benefit from an expansion in these sectors. Conversely, workers whose competencies are specific to the task in polluting industries loose from environmental policies. Despite the importance of these consequences for the general acceptance of climate policies, hitherto, the evidence on this issue is scarce. The first study aiming to shed light on the way environmental regulations affect the demand for different skill sets by Vona et al. (2018) use a US dataset which details the tasks and skills needed in environmental-friendly occupation. Following a similar approach to the aforementioned studies on the US, they then exploit regional variations in environmental regulations in the US with a DiD strategy to assess the effect of these policies on the demand for green skills from 2006-2014. Although they find that stricter environmental regulations led to an increase in demand for technical and engineering skills, their study yields little evidence for an increase in the skill demand of environmental policies when measured with standard human capital measures such as schooling. Marin and Vona (2019) arrive to a similar conclusion when analyzing the effects of climate policy - proxied by energy prices - on workforce skills for 15 industrial sectors in 14 European countries from 1995-2011. They find that stringent climate policies shifted employment from manual workers to technicians but do not generally favour workers with a particular educational level. This finding stands in contrast to the aforementioned study by Yip (2018): His study illustrates that layoffs following a carbon tax in British Colombia were more prevalent among low- and medium-educated workers. The study explains this by the the relatively high share of these workers in energy-intensive manufacturing, a sector that is disproportionately affected by the carbon tax. In the light of these conflicting views, the potential skill-biased impacts of climate policies remain a largely unresolved empirical issue and require further investigation.

The only labor market analysis of climate policies in an emerging country has been carried out by Liu, Shadbegian, and Zhang (2017). Their analysis of the labor market effects of more stringent wastewater discharge regulations on textile and dyeing firms in the Jiangsu region in China shows a 7% employment reduction in these sectors which is driven by domestically-owned private firms (as compared to state-owned and foreign firms). The here proposed study differs in three important ways. First, I will expand the breadth and generality of their work by focusing on a more extensive policy that affected several sectors and regions across the whole country. Second, by estimating employment effects at the industry level, the study by Liu, Shadbegian, and Zhang (2017) is silent on the distributional consequences of the policy. Lastly, rather than focusing on a regulation, I will analyze the effect of a market-based instrument. With the Chinese government making increasing use of market-based environmental instruments, a detailed

exploration of the labor market effects of this type of policy gains in importance (Goulder, Morgenstern and Munnings, 2017).

2.2 The Functioning and Impact of the Chinese ETS Pilots

This paper also adds to the active discussion of the environmental and the economic impacts of the Chinese ETS pilots. Prior to presenting insights from this literature, I will briefly elucidate the rationale behind and the functioning of the program.

The establishment of the Chinese ETS pilots reflects the effort of the Chinese government to carefully balance its ambitious economic growth objective with its internationally promulgated climate targets (Zhao, Wang, and Luo, 2019). Since the opening of its economy in 1978, its spectacular growth trajectory was accompanied by a rapid rise in energy consumption (Dong, Dai, Zhang, Zhang and Long, 2019). Although the US and the EU are historically the largest emitter of carbon dioxide (CO_2) emissions, in 2006, China surpassed the amount of CO_2 emissions of the US, becoming the largest emitter in the world (Jotzo and Löschel, 2014). A deteriorating environment and concerns about long-term energy security stimulated policy efforts to curb emissions (Schreurs, 2016). This commitment culminated in the announcement of the first quantifiable target at the 2009 Copenhagen summit where China announced to reduce its CO_2 intensity - measured by carbon emissions per unit of GDP - by 40-45% compared with 2005 levels by the year 2020.[2] To achieve its climate targets, the country adopted a wide range of climate regulations. These proved to be effective but came at high economic costs (Goulder et al., 2017).[3] Negative experiences with regulatory interventions combined with the need for innovations to sustain high levels of economic growth ushered in a paradigm shift: Hoping that market-based instruments would target the climate challenges in a more cost-effective way and at the same time promote innovations, these are playing an increasingly important role in the Chinese climate policies repertoire (Goulder et al., 2017). This policy shift was vividly reflected in the announcement to establish the seven ETS pilots in 2011.

With the aim of informing the design of a national unified carbon market, five cities (Beijing, Shanghai, Tianjin, Chongqing and Shenzhen) and two provinces (Hubei, Guangdong) were designated to establish separate carbon trading markets. Note that the pilot ETS in the city Shenzhen is located within the pilot province Guangdong but still forms an independent market. Together the pilot areas cover 260 million people (Zhang, Karplus, Cassisa and Zhang, 2014). After the initial implementation of these seven pilots in 2013 and 2014, an eighth ETS pilot was established in Fujian in 2016. The pilots are not only located in different parts of the country (see Figure 1) but were chosen to represent the heterogeneous economic circumstances in China (Han et al., 2012). These range from the

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[2]In the Paris agreement in 2015, China set a more ambitious pledge of a 60-65% reduction target of its CO_2 intensity by 2030. This is also the year in which it wants to achieve the peak of its emissions (Goulder et al., 2017).

[3]For example, in order to achieve the energy conservation targets of the 11th Five-Year Plan period (2006-2010), some provinces were cut-off from electricity in 2010 (Duan, Pang, and Zhang, 2014).

dependence on heavy industry in less-developed Hubei to China's post-industrial economic powerhouses Beijing and Shanghai whose economies are dominated by the tertiary sector (Jotzo and Löschel, 2014). The combined experience of these separate emission schemes informed and paved the way for the establishment of a national unified carbon market. This trading scheme came into force in 2021. Although it currently only encompasses the electricity sector, it will gradually be expanded to other sectors (Goulder et al., 2017).
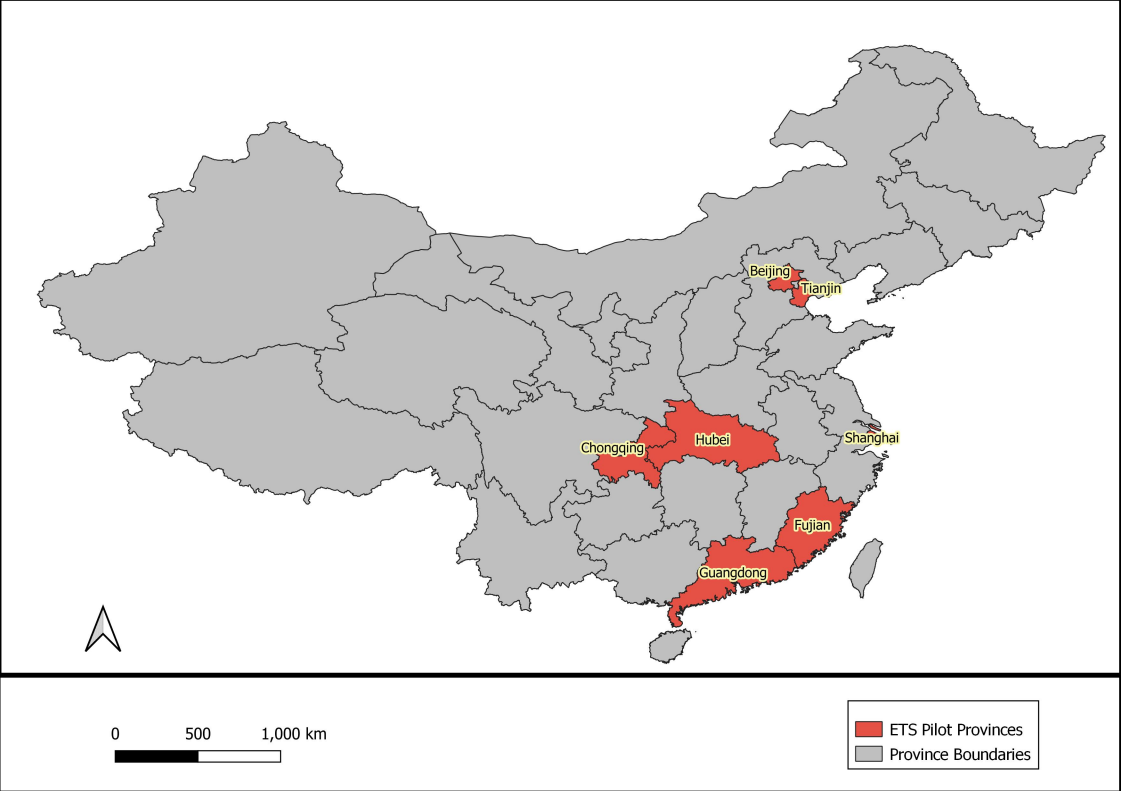
Figure 1: Location of the Chinese ETS pilots

Note: Author's elaboration with data from OCHA (2021).

The idea to use emission trading schemes in order to internalize pollution costs goes back to the Canadian economist Dales (1968) and had already been put forward in the Kyoto protocol in 1997 (Dong et al., 2019). Becoming an increasingly popular climate policy instrument, there are currently 24 ETS operating globally which cover 16% of all emissions (ICAP, 2021a). In general, these schemes work as follows: A central authority decides on the maximum amount of pollutant that can be emitted in a certain period. To ensure compliance with this cap, tradable allowances for each unit of emission are given out (via auctioning or for free) to all installations or firms covered by the system. When the compliance period ends, firms must surrender allowances for their emissions. By limiting the availability of permits for emissions, an effectively functioning ETS puts a price on pollution. The ability to trade allowances in a market gives firms, whose emissions surpass the allowed amount, the choice to either adapt their production or to purchase new allowances in the market (Carmona and Fehr, 2010). The latter decision is taken when the marginal cost of reducing one unit of emissions is higher than the market

price for this unit (Smith, 2011). In a competitive, well-functioning market, this should achieve emission reductions in a cost-effective way (Böhringer and Rosendahl, 2009).

Although all eight Chinese ETS pilots broadly follow this set-up, they differ in one important aspect: The exact caps for each sector are set up in a bottom-up approach (Jotzo and Löschel, 2014). More precisely, allowances are issued based on a standardized amount of emissions for each unit of production (benchmarks) and sector-level growth rates. The issued allowances for each sector are then added to a total cap for each ETS pilot.[4] Besides the cap determination, the precise design of important elements of the scheme such as allocation, coverage and inclusion threshold are deliberately left in the hands of local authorities. The resulting mixture of different policies, summarized in Table 1, is supposed to further foster the learning experience for the national scheme (Zhang et al., 2014). A common element to all pilots is the free allocation of the majority or of all allowances. Even in pilot areas were auctioning is possible, only a small number of permits have been given out in this way (Hu, Ren, Wang and Chen, 2020). Furthermore, in contrast to the EU ETS, all pilots cover indirect emissions attributable to energy including imported electricity from outside the pilot area (Zhang et al., 2014). Thus, emission coverage is generally high ranging form 45 to 70%. Important differences between the pilots emerge in terms of inclusion threshold, sectoral coverage and carbon prices: For instance, whereas in Beijing companies with annual emissions of at least 5,000 tonne of CO_2 in covered sectors are included in the scheme, in Guangdong only firms emitting four times this amount are covered. Thus, despite the ambition behind the program reflected in high coverage levels, it is meant to allow for enough flexibility to not interfere with regional growth objectives (Jotzo and Löschel, 2014).

This is further underpinned by the introduction of a supplementary project-based market aiming at lowering the cost burden for firms (Li et al., 2019). Specifically, firms can obtain Certified Emission Reductions (CER) by engaging in mitigation projects and thereby offset 5 to 10% of their total emissions (Liu, 2016).[5] Mitigation projects generally focus on the promotion of renewable energies or on the improvement of energy efficiency. The price for CER is generally lower than the carbon price (Lo and Cong, 2017).

Given that the pilots are a precursor to the establishment of the worldwide largest carbon market, their economic and environmental consequences have become a subject of abundant research. Most of these studies place the environmental and growth consequences at the heart of their empirical investigation. They provide compelling evidence for the notion that the program has been effective in generating sizeable CO_2 emissions reduction of between 15 and 25% (Hu et al., 2020; Zhang, Li, Li and Guo 2020) and in curbing haze pollution concentration in the pilot areas (Yan, Zhang, Zhang and Li, 2020). However, the studies also point to considerable heterogeneity in the environmental performance of the ETS between provinces. After Beijing (Tang, Zhou, Liang and Zhou,

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[4]The number of permits allocated to each firm is either based on its historical emissions in a base period or year (grandparenting) or on its performance (benchmarking).

[5]This mechanism is similar to the UN Clean Development Mechanism (CDM).

2021; Zhou, Liang, Zhou and Tang 2020), the greatest environmental improvements have been found in Guangdong (Yan, Zhang, Zhang and Li, 2020) and in Hubei (Qi, Cheng, and Cui, 2021). As Hubei is the least developed region included in the scheme, the latter result is astonishing. It suggests that the quality and efficiency of each ETS pilot is, at least partly, orthogonal to the level of economic development of the area where it has been implemented (Sieler, 2020).

With regard to the effect on GDP, a more ambiguous picture emerges: The findings of Zhang, Zhang, Li, Li and Choi (2020) and Zhang and Zhang (2020b) point to a negative effect of the pilots on regional GDP of around 4%. They stand in contrast to the absence of significant growth effects in Dong et al. (2019) and Qi, Cheng, and Cui (2021). As all four studies use official regional GDP statistics and the same methodology (DiD analysis), these conflicting views seem strange. At the root of these diverging results might be differences in the inclusion of control variables. Especially controlling for variables such as the level of industrial pollution abatement investments as in Qi, Cheng, and Cui (2021), which are likely to be endogenous to the policy and to growth, might be problematic. If for instance, the policy depresses growth by pressuring firms to purchase costly abatement technology, then the policy might not have a statistically significant association with growth conditioning on these expenses. The fact that the inclusion of potentially endogenous confounders is a common problem to all four studies calls for a revaluation of the growth effects of the policy. Closely related to this are the studies that focus on the impact of the program on innovations. Results from these studies indicate that the ETS pilot spurred innovation in low-carbon technologies (measured by patent applications) (Cui, Zhang, and Zheng, 2018; Zhou, Liang, Zhou and Tang, 2020) and increased Research and Development (R&D) spending (Liu, Ma, and Xie, 2020).

Only a few recent studies estimate the employment effect of the policy. Employing a DiD strategy, Yu and Li (2021) find that the policy led to employment *increases* of around 11.5% in the pilot areas and 10% in the neighboring provinces. Without providing further empirical evidence, their explanation that the ETS pilots induced "neighboring provinces to try and explore carbon trading" (p.7) is highly questionable. Yang, Jiang, and Pan (2020) conduct their analysis at the province level and similarly ascertain an immediate overall employment surge of 10% as a consequence of the policy. The fact that positive employment effects of climate policies found in the previous literature tend to be small and only materialize after some time casts doubt on the plausibility of these results. A severe shortcoming in both studies is the inclusion of potentially endogenous controls such as business income growth in Yu and Li (2021) and wages in Yang, Jiang, and Pan (2020). More robust evidence is brought forward by Zhang and Duan (2020) who combine a DiD method with a Propensity Score Matching (PSM) method. They find that industrial sub-sectors, that are included in the trading scheme, reduce employment by 17% in comparison to their non-affected counterparts. Although their study is methodologically sound, it ends in 2015 and can therefore only make conclusions about the immediate ef-

fects of the policy.[6] A related study by Zhang and Zhang (2020a) similarly employs a DiD strategy and examines the impact of the ETS pilots on poverty alleviation. The authors expect the policy to reduce poverty by inducing firms to engage in mitigation projects. These are typically carried out in rural, disadvantaged communities (Liu, 2016). Their findings indicate that the ETS improved the position of rural households by increasing the annual rural residential income (2.4% annually) and by incrementing the ratio of the rural employed population to the total population (1.8%). Nevertheless, it is unclear whether the employment effect found in the study is driven by additional jobs in rural areas or by a relatively stronger reduction in overall employment numbers in the pilot areas.

In a nutshell, the literature clearly identifies a positive environmental and innovation impact of the policy whereas the overall economic effects as measured by GDP are more contentious. Furthermore, the evidence on the employment effects of the policy is thin and requires further empirical investigation.

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[6]Note that in the province Fujian, the policy was not even implemented at that stage.

Table 1: Overview Chinese ETS Pilots

Pilot	Launch	Allocation	Inclusion Threshold	Emissions Covered	Average Allowance Price 2020 (per tCO_2)
Beijing	11/2013	Free allocation Auctioning possible	10,000 tCO_2/year 5,000 tCO_2/year since 2016	45%	CNY 87.06 (USD 12.62)
Chongqing	6/2014	Free allocation	20,000 tCO_2/year or energy consumption of 10,000 tce/year	62%	CNY 26.38 (USD 3.82)
Fujian	9/2016	Free allocation Auctioning possible	energy consumption 10,000 tce/year	60%	CNY 17.24 (USD 2.50)
Guangdong	12/2013	Free allocation	20,000 tCO_2/year or energy consumption of 10,000 tce/year	70%	CNY 28.21 (USD 4.09)
Hubei	4/2014	Free allocation	energy consumption 60,000 tce/year energy consumption 10,000 tce/year since 2016	42%	CNY 27.21 (USD 3.94)
Shanghai	11/2013	Free allocation	20,000 tCO_2/year or energy consumption of 10,000 tce/year stricter thresholds for transports & buildings	57%	CNY 40.11 (USD 5.81)
Shenzhen	6/2013	Free allocation Auctioning possible	3,000 tCO_2e/year for enterprises 10,000 m^2 for large public buildings and government buildings	40%	CNY 23.91 (USD 3.46)
Tianjin	12/13	Free allocation Auctioning possible	20,000 tCO_2/year	55%	CNY 22.64 (USD 3.28)

Source: ICAP (2021b) and Wnag, Liu, Tan and Liu, (2019).
Note: tce stands for tonnes of coal equivalent, CNY for Chinese Yuan and USD for US Dollar. Yuan are converted to dollars using the official exchange rate for 2020.

2.3 The Chinese Labor Market

To obtain a better perspective on potential labor market impacts of the ETS pilots in China, it is worth considering in more detail some salient features of the Chinese labor market. These can best be understood by taking a glance at its labor market history.

The existence of a labor *market* in China is a relatively recent phenomenon. Until 1978, wages and employment were in the firm hands of the Chinese government (Song, 2013). Urban workers were almost exclusively hired in state-owned enterprises (SOE) or collective-owned enterprises. A system famously coined "the iron rice bowl" forbid firms to lay off redundant workers guaranteeing lifelong employment (Feng, Hu, and Moffitt, 2017). Furthermore, a household registration system (*hukou*) anchored agricultural workers to their rural homelands: People with an agricultural registration status (agricultural *hukou*) were not allowed to move to the cities (Zhang and Wu, 2018). As a consequence of these policies, immobility, a lack of incentives and excess labor prevailed in the Chinese labor system (Song, 2013).

With the privatization of many SOEs and the emergence of non-state enterprises, this highly centralized system has been dismantled gradually (Zhang and Wu, 2018). Especially a law allowing firms to freely hire and dismiss workers in the mid-1990s marked a watershed in the transition towards a market-based allocation of workers (Feng, Hu, and Moffitt, 2017). The *hukou* policy is still in place but has been relaxed gradually. It no longer prohibits rural-to-urban migration (Song, 2013). Despite these important reforms, the Chinese labor market still displays some unique features partly originating from this time.

First, it is characterized by a high degree of labor market segmentation. Fields (2011) defines this as a situation in which qualitatively distinct labor market sectors pay different wages to workers with similar characteristics. In the sector with higher prevailing wages, jobs are rationalized and only accessible to a part of the population (Fields, 2011). While in many developing countries this divide arises along a formal and an informal sector, in China the segmentation between SOEs, the private sector and the agricultural sector plays a bigger role (Fields and Song, 2013).[7] By operating in strategic industries such as petroleum, electric power or finance, SOEs still form an essential pillar of the Chinese economy (Zhang and Wu, 2018). Coveted jobs in SOEs are scarce. Less than a fifth of the workforce is employed in these firms (Zhang and Wu, 2018). The private sector, in contrast, hires most workers (83% in 2014) (Li et al., 2017). In this sector, wages cannot compete with the high wages paid in the SOEs but exceed those in the agricultural sector (Fields and Song, 2013).

A second idiosyncrasy of the Chinese labor market is the *hukou* policy which imposes certain barriers to labor mobility (Song, 2013). The place a household is registered at still defines whether it obtains access to particular services (e.g. education, housing subsidies)

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[7]There is neither an official definition of informal employment nor official figures. Survey estimates of the informal employment share in urban areas range between 20 and 40% (Albert Park and Du, 2012).

in a specific locality (Song, 2014).[8] The details of the policy are defined by local governments and vary between locations (Song, 2013). Especially in big cities, people with a local registration status enjoy a range of welfare benefits which migrants cannot access. This limits the expected returns from moving to these places (Song, 2013). Although rural migrants make up a substantial part of the workforce in the vibrant Chinese coastal cities (Fields and Song, 2013), it has been argued that without the *hukou* policy, the influx of people from the countryside would be much higher (Bosker, Brakman, Garretsen, and Schramm, 2012). Furthermore, discriminatory labor market policies against non-local *hukou* holders have only been abolished in 2004 (Song, 2013). In practice, migrants still have to cope with different forms of labor market discrimination (Fields and Song, 2013). Several studies document huge wage and hiring differentials between migrants and urban residents which cannot be explained by productivity-related characteristics (e.g. Zhang, 2010; Song, 2014). In the light of these difficulties, migrants are mostly employed in low-paying occupations in the construction and manufacturing industries (Song, 2013).

Ultimately, in contrast to many Western countries, China lacks a plural union landscape (Liu et al., 2015). Workers do not enjoy the right to freely associate and all unions fall under the government controlled All-China Federation of Trade Unions (ACFTU) (Song, 2013). This is the only legal entity that can engage in wage negotiations with employers (Meng, 2012). Despite the lack of a genuine representation of their rights, Chinese workers are not completely unprotected. Since the mid-2000s, the state has passed a series of labor laws to guarantee a better protection of employment and labor rights (Cooke, 2011). A prominent example are the "Minimum Wage Regulations" passed in 2004 which heightened the frequency of minimum wage rises and increased the penalty in cases of non-compliance (Kang and Peng, 2017). Despite some difficulties in enforcement, at least in the Eastern part of the country, where enforcement is stricter, minimum wages seem to exert some downward wage rigidity in the labor market (Fang and Lin, 2015; Mayneris, Poncet, and Zhang, 2018).

To recapitulate, the Chinese labor system has undergone an important transformation towards a largely market-oriented system. Yet, the labor market is still segmented along sectors with different ownership. In addition, the existing *hukou* system as well as labor market discrimination against migrants drive a wedge in the wage and employment opportunities between the local and migrant labor force in the cities and hamper labor mobility.

3   Conceptual Framework

The last section illustrated that the labor market effects of environmental policies vary greatly depending on the type of policy and the characteristics of the labor market. This

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[8]Although several provinces no longer classify workers in agricultural and non-agricultural types, a local hukou registration is still in place.

motivated an overview of these features for the Chinese context under observation. In order to derive hypotheses based on these insights, in this section, I will set out a simple framework on potential labor market impacts of environmental policies.[9]

With regard to the behavioral responses of firms facing pressure from stringent environmental policies, two conflicting hypothesis have been brought forward: The *pollution haven hypothesis* envisages the possibility that firms facing high compliance costs relocate economic activity towards regions with more lax environmental regulations. These areas form so-called pollution havens (e.g. Copeland and Taylor, 1994). This gravitates employment towards regions with more lenient environmental policies and leads to employment losses in regulated regions (Dechezleprêtre and Sato, 2017). One might argue that due to the presence of fixed production factors (e.g. in the short and medium run), it is not always possible to reallocate firm activity. If firms stay in the regulated area, they face compliance costs which raise market prices. Following simple microeconomic theory, this leads to a fall in demand, subsequent output contractions and reduces employment (Dechezleprêtre and Sato, 2017).

Two factors can alleviate this drop in labor demand. First, if affected firms operate in a less competitive environment and face an inelastic demand, they can pass on cost increases to consumers without fearing a substantial reduction in demand (Morgenstern, Pizer, and Shih, 2002). Second, abatement activities that involve monitoring (e.g. price monitoring in an ETS) or maintenance activities of abatement technologies require additional workers. This would counteract the fall in the labor demand arising from a reduction in output. Apart from the direct cost effect, the cost increase for polluting inputs might induce a shift in production technologies. This factor shift has an ambiguous effect on employment (Morgenstern, Pizer, and Shih, 2002). Depending on whether the environmental policy triggers a substitution towards or away from labor, firms might increase or decrease the labor intensity of their production (Morgenstern, Pizer, and Shih, 2002). Thus, the cost effect depends on the ability to reallocate economic activity, the elasticity of demand, the labor intensity of abatement activities and the induced factor shift in production technology (Daitoh, 2003).

The second hypothesis, the *Porter Hypothesis* casts doubt on whether stricter environmental policies incur costs for firms at all. In their seminal work, Porter and Linde (1995) postulate that "properly designed environmental standards can trigger innovation that may partially or more than fully offset the costs of complying with them" (p.98). The underlying reasoning is that in the presence of market imperfections, bounded rationality or information deficits, an environmental policy can induce firms to take beneficial investments or to correct inefficient production processes (Brännlund and Lundgren, 2009).[10] Given the special characteristics of the "green" sector, the increase in innovation

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[9]The here presented analysis only includes the direct effects of climate policies leaving out any indirect labor market effects caused by a mitigation of environmental damage. For instance, a reduction in global warming might increase labor productivity (Dell, Jones, and Olken, 2012).

[10]For instance, knowledge spillovers might lead to an underinvestment in environmental-friendly R&D

might be substantial: Environmental-friendly technologies are characterized by relatively large knowledge spillovers which allow for a range of complementary innovations (Dechezleprêtre, Martin, and Mohnen, 2013). Additionally, in the light of the urgent need to reduce environmental degradation, climate policies often form part of a clear, long-term policy agenda. As this limits the uncertainty about the future return to green innovations, firms might be more prone to incur the high switching costs towards climate-friendly technologies (Gagliardi, Marin, and Miriello, 2016). The resulting employment effects of this increase in innovation are dependent on the type of innovation. Product innovations are expected to heighten the exploitation of new market opportunities and create more employment opportunities in innovating firms (Gagliardi, Marin, and Miriello, 2016). In a more indirect way, additional jobs might be created through the call for labor in the adaption of these new technologies. Process innovations, on the contrary, are assumed to enhance productivity by reducing the amount of labor in the production process (displacement effect)(Gagliardi, Marin, and Miriello, 2016). However, in the long run, if higher productivity translates into lower prices, output might expand generating a positive impact on employment.

Labor market changes might not only arise in sectors where production processes are directly affected by an environmental policy. Spillover effects can alter the number of available jobs even in non-regulated sectors: On the one hand, labor-enhancing effects in green sectors emerge when consumers substitute consumption from goods produced in the regulated industry to goods produced in non-regulated industries (Hafstead and Williams, 2020). In a similar vein, reductions in the labor demand in regulated industries decreases competition in the hiring market and might induce firms in the unregulated industries to hire more workers for lower wages (Hafstead and Williams, 2020). On the other hand, job losses might be aggrevated if supply chain linkages result in spillovers to unaffected industries (Hafstead, Williams, and Chen, 2018). For instance, an ETS encompassing the power sector can trigger a rise in electricity prices. This potentially depresses employment by increasing production costs and reducing output in industries with energy-intensive inputs (Oral, Santos, and Zhang, 2012).

These considerations highlight that environmental policies affect labor markets in a complex way. As my analysis is limited to the short and medium run (up to 5 years after the implementation of the policy), it is useful to distinguish when these different effects are expected to materialize. According to a framework developed by Fankhaeser, Sehlleier, and Stern (2008), job losses in directly affected carbon-intensive sectors as well as certain replacement opportunities in low-carbon sectors arise in the short run.[11] In the medium run, the ripple effects of the policy become pronounced affecting whole supply chains. Job-creating forces arising from innovations in climate-friendly technologies unfold

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as firms do not reap the full return of their investment. Stringer environmental policies can counteract this effect by incentivising firms to undertake the investment (Lanoie, Laurent-Lucchetti, Johnstone, and Ambec, 2011).

[11]Unfortunately, Fankhaeser, Sehlleier, and Stern (2008) do not define which time span the short, medium and long run encompass in their framework.

in the long run. Thus, in the long run, the overall labor market effects, whether positive or negative, are expected to be small. Innovations and an increase in labor demand in less-polluting sectors should offset potential job losses arising from output contractions in polluting sectors (Deschenes, 2011). In the short and medium run, this picture changes. Potentially facing adjustment costs, workers are less mobile and cannot always directly transit towards less-polluting sectors. Therefore, stringent environmental policies can cause severe temporary disruptions and lead to frictional unemployment (Hafstead and Williams, 2018).

4   Expected Results

When applying the aforementioned theories to the Chinese context, two features stand out. Especially the Chinese coastal regions, where most pilots have been implemented, display a high degree of trade openness (Fang, Huang, and Yang, 2020). In an environment of fierce international competition, demand is highly elastic. This severely limits the opportunity for firms to pass on costs arising from the policy to consumers. Indeed, apart from relatively low labor costs, lax environmental regulations have often been said to be one of China's comparative advantages. This allowed it to produce pollution-intensive products at relatively low prices (Qi and Xiao, 2007). In the light of this, stringent environmental policies should depress exports of pollution-intensive industries and - ceteris paribus - reduce the job demand in exporting sectors. Since a large part of the workforce is employed in these sectors (Fu and Balasubramanyam, 2005), I expect this effect to overtrump potential employment gains arising from factor shifts or monitoring activities.

Furthermore, the Chinese labor market is characterized by a high level of labor market segmentation. This should exacerbate short run disruptions from environmental policies (Hafstead and Williams, 2018). The fact that certain laid off workers, in particular migrants, face institutional obstacles in finding new employment should slow down the adjustment process to environmental policies and lead to at least temporary negative effects for workers in industries that fell under the policy. Taking into account that wages, working hours and employment are no longer in the hands of the central government, I expect to find negative effects across all these indicators.

Notably, the here described temporary labor market disruptions might not affect all worker groups equally. As could be seen in Section 2.1, the empirical evidence on this question is scant and inconclusive. Economic theory also offers little guidance on the differential impacts of climate policies by educational levels. A simple calculation by the Organization for Economic Cooperation and Development (OECD) shows that on a global scale, job destruction and creation is largest among low-educated workers. If job creations or the transition of these workers is not immediate, they might experience stronger temporary labor market repercussions. Furthermore, since many migrants workers, which are expected to be hardest hit, display lower educational levels (low- or medium-educated)

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(Meng, 2012), I anticipate to find stronger effects in these groups. Recapitulating the above, the following hypothesis of potential labor market effects of the Chinese ETS pilots can be derived:

H1: The introduction of the Chinese ETS pilots depressed wages, hours worked and employment and incremented unemployment in the pilot provinces.

H2: The introduction of the Chinese ETS pilots led to more pronounced negative labor market effects for low- and medium-educated workers than for high-educated workers.

5 Data

5.1 Data Sources

For the empirical test of the plausibility of these hypotheses, I compile a unique dataset based on two different data sources. First, I draw on rich biennial individual-level data from the China Family Panel Studies (CFPS, 2010-2018). The CFPS is a longitudinal survey conducted by the China Social Sciences Research Centre (ISSS) at Peking University. Each survey round includes around 30,000 individuals. To obtain nationally representative data, the survey applies a multi-stage proportional probability sampling. It covers a wide range of topics including individuals' employment details and demographic information and is therefore suited for my analysis. Modeled on the Panel Study of Income Dynamics (PSID) of the US, the data is generally considered to be of high quality and has found wide use in many empirical studies including publications on the Chinese labor market (Xie, Zhang, Xu, and Zhang, 2015; Feng, Hu, and Moffitt, 2017).

Despite the popularity, there are several limitations of the CFPS data. First, industry categories are coarse and do not allow for a precise separation of individuals directly working in industries that fall under the ETS regulations and those that are not. While this still allows me to estimate the overall labor market impact of the program, I cannot estimate the effect of directly affected sectors. Second, changes in the survey design regarding the measurement of employment and unemployment status from the 2010 to the 2012 round can lead to significant measurement errors. For example, the question "Do you currently have a job" in the 2010 wave directly asks respondents about their employment status. Ji and Yan (2020) point out that this subjective assessment is problematic as "Gongzuo", the word used for job in the Chinese version of the survey, refers to formal jobs. If respondents only subsume full-time formal employment under this category, informal and part-time employment would not be captured and overall employment levels would be biased downwards. Addressing this shortcoming, in all subsequent rounds, a combination of different questions was used to identify respondent's employment status (e.g. "have you worked for at least one hour last week"). With regard to the measurement of the unemployment rate, similar improvements have been undertaken from the first to the

subsequent survey rounds.
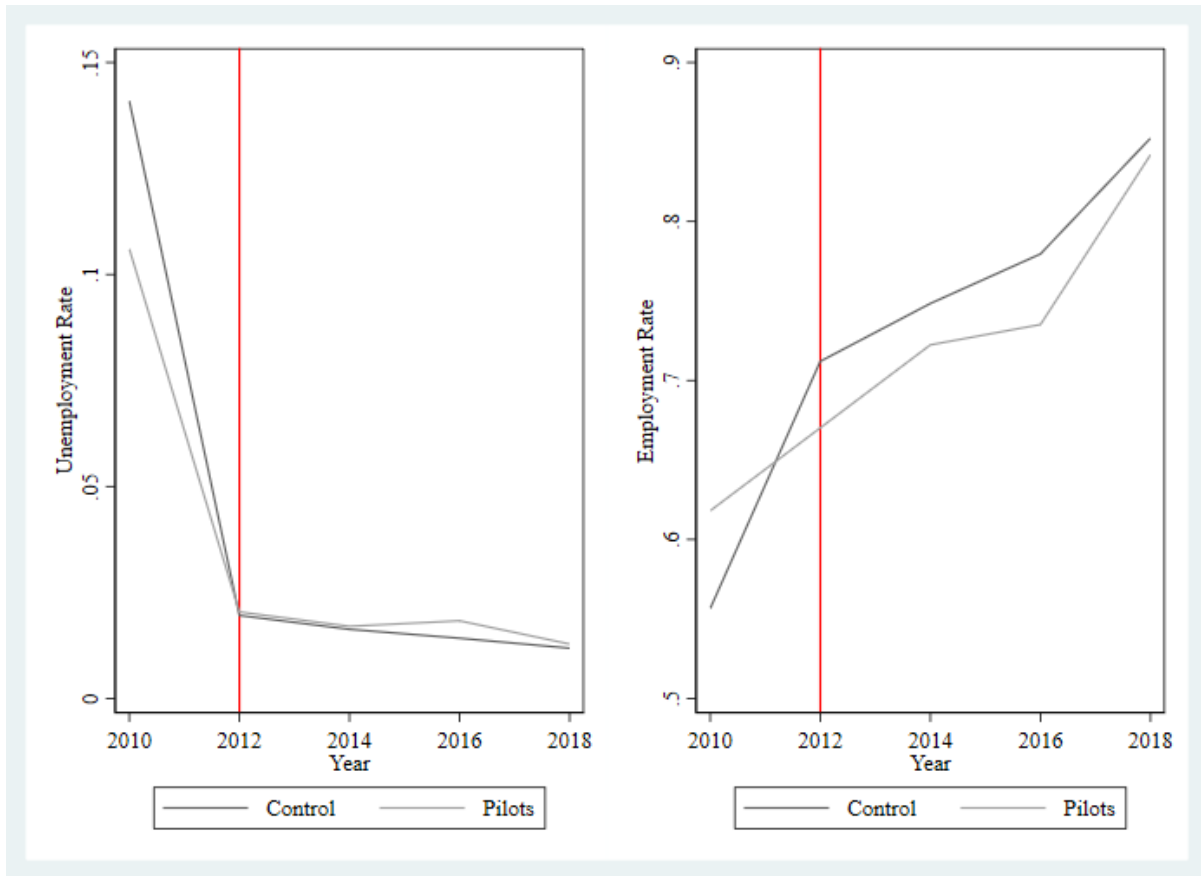
Figure 2: Mean values for employment and unemployment before and after change in the survey design in 2012

Note: Author's elaboration with data from ISSS (2020).

Figure 2 which plots the average unemployment and employment rates over time gives strong support to the notion that the questionnaire in the first round of the survey (over-)understated (un-)employment. This is further corroborated by a comparison with official statistics. The observed slump in unemployment in the survey from around 13% to 2% is nowhere to be seen in the official statistics (NBS, 2010-2018). To ensure that a possible measurement error arising from the survey design in the first round does not bias my result, I will only use the last four survey rounds when estimating the effect of the ETS pilots on employment and unemployment.

Despite these pitfalls, to the best of my knowledge, the CFPS is the only publicly available nationally-representative panel survey which covers the time period under observation and provides detailed labor market information.[12]

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[12]Recognizing the dearth of individual-level panel labor market surveys, the Sun Yat-Sen University launched the China Labor-force Dynamics Survey (CLDS). Unfortunately, the survey has been taken down from the website and is no longer available to the public.

In order to control for other development at the province level that might affect labor market outcomes, I additionally collected aggregated data on economic characteristics (e.g. exports, share of secondary sector) at the province level from the Chinese Statistical Yearbooks from 2010 to 2018 (NBS, 2010-2018). On the one hand, Chinese official statistics collected by the National Bureau of Statistics in China (NBS) have invoked some criticism from the academic community. Researchers point to a lack of transparency and certain data inconsistencies (Plekhanov, 2017). On the other hand, no egregious anomalies could be detected for the official GDP statistics (Holz, 2014). This, however, could also be due to the lack of alternative estimates which limits researchers in their evaluation of the reliability of the data (Plekhanov, 2017). As I only retrieve additional controls from this data source and perform all regressions with and without these controls, these concerns should not pose a challenge to my estimations. Furthermore, I do not expect the data to by systematically biased in favour or against economic developments in the pilot areas.

5.2 Data Cleaning and Variables

The raw data from the CFPS consisting of all five survey rounds (2010, 2012, 2014, 2016 and 2018) includes 176,797 observations. I exclude all individuals that cannot be uniquely identified across survey rounds. Additionally, I drop individuals with missing information on the province of residence from the survey. This is done to make sure that individuals can be matched to either the treatment or the control group. As I am interested in labor market outcomes, I only keep individuals of working age (16-59) in my sample.[13]

For my empirical strategy, it is further desirable that individuals living in the pilot areas are similar to those in the control group. In the light of the great regional diversity in China, individuals living in areas that are very different from the pilot areas most likely do not fulfil this criteria. Figure 3 shows a map presenting regional heterogeneity in per capita GDP data prior to the implementation of the ETS pilots in 2013.

This simple visualization calls the alleged representatives of the pilot areas with respect to China's economic development into question: None of the pilot areas belongs to the lowest GDP per capita group. Furthermore, the pilots are primarily located in the Eastern part of the country which has a different demographic composition (e.g. less minorities) and generally follows a different economic trajectory than the Chinese West (Han and Paik, 2017). In order to obtain a control group that more closely resembles the treatment group, I exclude individuals living in the poorest Western provinces (Xinjiang, Tibet, Qinghai, Sichuan, Ganus, Yunnan and Guizou) from my sample. This leaves me with a final sample of 99,828 individuals (see Table 2).

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[13]In China, men generally retire at the age of 60 and women at the age of 55 (Zhang and Wu, 2018).
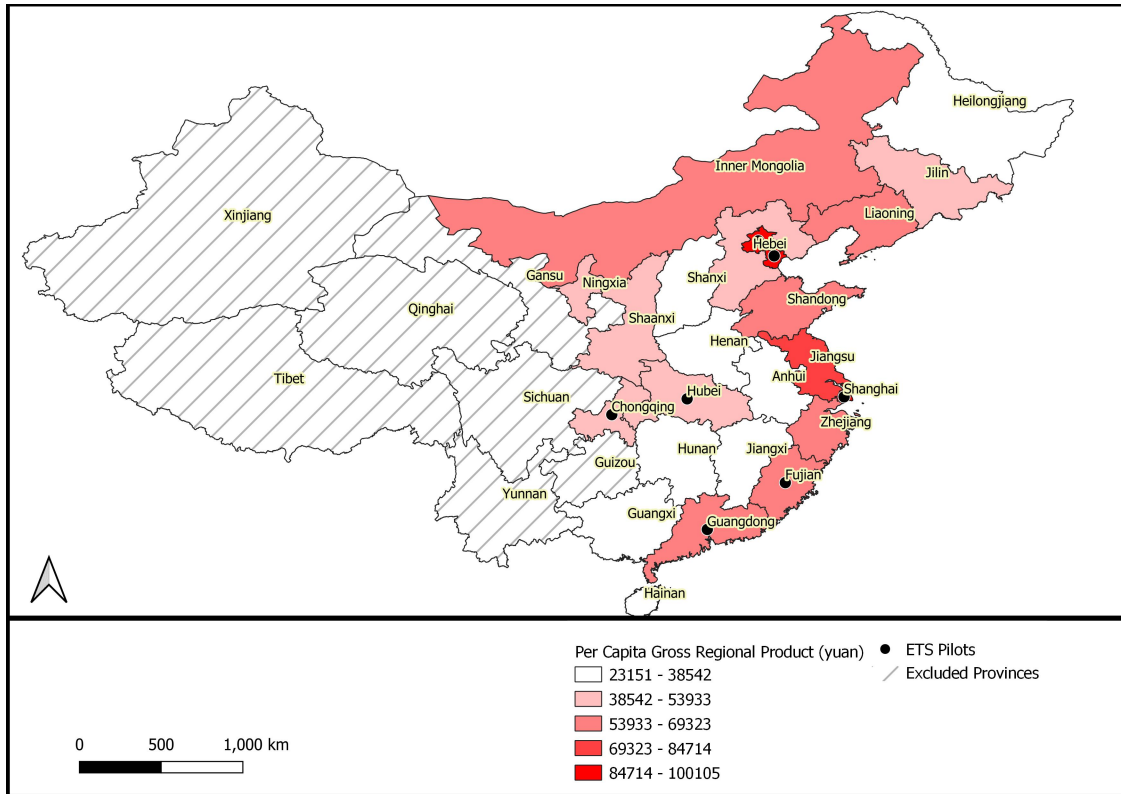
Figure 3: Definition of the control group

Author's elaboration with data from OCHA (2021).

Table 2: Sample Restrictions for the CFPS 2010-2018

Sample restriction	Observations dropped	Share of original sample (176,025)
Missing Personal ID	772	0.44%
Missing provincial information	479	0.27%
Younger than 16 and older than 59	43,598	24.66%
Poorest Western provinces	42,268	23.91%
Total dropped	76,197	43.29%

Note: As some categories overlap, the total amount of observations dropped is lower than the sum of the observations that do not fulfil the sample restrictions in each category.

To obtain a more complete picture on how the policy affected labor markets, I estimate the effect for four different labor market variables (employment, unemployment, hours worked and wages). Employment (a dummy variable taking a value of 1 when an individual is employed and 0 otherwise) measures whether an individual *of working age* is currently working. The variable unemployment (a dummy variable taking a value of 1 when an individual is unemployed and 0 when it is employed), in contrast, captures whether an individual *that forms part of the labor force* does not have a job. Thus, it

is important to distinguish between both outcomes. For example, if after the policy laid off workers became discouraged and left the labor force, employment would drop while unemployment remained constant. I also estimate the effect on the log of monthly hours worked and on the log of real monthly wages. For these two variables, I limit the sample to respondents with a positive amount of hours worked and positive wages, respectively. This ensures that I do not confound the effects on wages and hours worked with changes in the employment status. More precisely, these variables allow me to investigate whether people that have stayed employed reduced their working hours (e.g. by engaging in part-time employment) or experienced a change in their wages. The collection of data on working hours and wages was subject to several changes in the survey design. To avoid any bias arising from this, I exercised great caution in creating variables that are comparable across all five survey rounds. For example, along the lines of Lei (2020), I created a wage variables that included the same items across all five waves. The resulting variable captures the monthly labor income from an individual's main job after taxes including the regular salary, bonuses and cash benefits and excluding insurances and public housing allowances. A brief overview of the creation of the four outcome variables is given in Table A.1 in Appendix A.

5.3 Descriptive Statistics

Descriptive statistics for some variables relevant for the analysis for the year 2012 are presented in Table 3. Individuals living in the pilot provinces form part of the treatment group while all other individuals (that were kept in the sample) make up the control group.[14] Two patterns bear mention:

First, with around 2% in both groups, the unemployment rate is strikingly low. It falls behind the official unemployment rate in urban areas (the only one available) of around 4% (NBS, 2010-2018) and is substantially lower than the unemployment rate estimated from survey data (5-7%) for urban areas in Feng, Hu, and Moffitt (2017). Importantly, my sample also includes rural areas which, assuming that unemployment in these areas is lower, can account for some of this discrepancy. However, in the light of the extremely low value, it seems likely that the unemployment variable does not adequately capture all unemployed people. This highlights the importance of additionally looking at employment numbers which in this case might be a better indicator of direct job losses.

Second, pilot and control provinces seem to be balanced in terms of their demographic composition. No significant differences with regard to gender, the percentage of married individuals, ethnicity, age and education can be detected. Yet, individuals in the pilot areas are less likely to be employed but those who are work longer hours. Accounting for differences in pre-treatment levels should mitigate potential biases arising from this. A more detailed description of the estimation will be given in the following section.

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[14]As employment and unemployment estimates might be flawed in the first survey round, I chose to present these statistics for the year 2012 instead of the baseline 2010.

Table 3: Descriptive statistics for the year 2012 separated by treatment and control group

Variable	N	Control Mean	St.Dev.	N	Treatment Mean	St.Dev.	Diff.
Employed	15999	0.72	0.45	5695	0.65	0.48	-0.068*
Unemployed	11765	0.02	0.15	3796	0.02	0.15	0.001
Monthly hours worked	7887	161.37	110.40	1834	202.56	116.45	41.188***
Monthly labor income	6256	1465.74	1315.25	2412	2236.31	2744.60	770.576
Gender	15999	0.49	0.50	5695	0.50	0.50	0.010
Married	15999	0.80	0.40	5695	0.75	0.44	-0.054
Han Ethnicity	15999	0.27	0.44	5695	0.31	0.46	0.040
Age	15999	38.81	12.35	5695	38.32	12.84	-0.491
No or Primary Education	15999	0.40	0.49	5695	0.36	0.48	-0.040
Secondary Education	15999	0.52	0.50	5695	0.51	0.50	-0.005
Tertiary Education	15999	0.08	0.28	5695	0.13	0.34	0.045

Note: Author's elaboration with data from ISSS (2020). The table presents averages and standard deviations for selected variables for the control and treatment group. All individuals residing in a pilot province belong to the latter group. The Diff. column presents the coefficients of a simple regression of treatment status on the variable, with clustered standard errors at the province level. The proportion of employed people are calculated from the full sample while the proportion of unemployed are calculated from the economically active population (employed+unemployed). Summary statistics fro monthly hours worked and monthly income only correspond to employed people.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

6 Methodology

6.1 Identification Strategy

In this section, I will outline the details of my identification strategy. It aims at estimating the *causal* effect of the ETS pilots on the labor market. A common problem in the estimation of the impact of environmental policies is the non-random nature of their assignment. A vast body of political economy literature demonstrates that the lobby efforts of different interest groups shape regulatory designs and can lead to exemptions for firms with certain characteristics (e.g. Stigler, 1971; Grossman and Helpman, 1994). What is more, regulations are often imposed in more polluted areas which differ in their industrial structure and might therefore follow different labor market trajectories (Deschenes, 2011). Thus, the designation of these policies is typically not orthogonal to industrial and labor market outcomes which complicates the search for an adequate counterfactual. The top-down approach in the designation of the Chinese ETS pilot areas mitigates some of these concerns: The goal of the pilot program was to obtain a representative picture of

potential effects of a national ETS rather than to curb pollution in worst-affected areas.

Although this should limit the differences between pilot and non-pilot areas, the pilot areas were not randomized and potentially experience distinct labor market outcomes even in the absence of the policy. Thus, in contrast to a randomized control trial, a simple comparisons of labor market outcomes between treatment and control group after the policy would lead to biased results. To account for this heterogeneity and to isolate the average treatment effect on the treated (ATT), I will resort to quasi-experimental techniques and pursue a DiD strategy. This widely-used method measures the difference in average outcomes between treatment and control group over time subtracting the difference that existed between the two groups prior to the treatment (e.g. Angrist and Pischke, 2008; Lechner, 2010). Thereby, it allows to control for time-invariant heterogeneity between treatment and control group as well as common time trends. This strategy is based on several identifying assumptions:

First, the method requires that in the absence of the policy, the labor market outcomes under consideration in both treatment and control group would have followed a parallel time trend. Put differently, controlling for observable characteristics, the development of the idiosyncratic error term over time does not depend on the treatment status. As an individual either belongs to the treatment or the control group, this assumption is impossible to verify. However, the fact that pre-treatment outcomes shown in the last section were largely balanced lends confidence to this assumption. Concerns might arise from the inclusion of China's biggest four cities (Beijing, Shanghai, Chongqing and Tianjin) in the treatment group. In comparison to the rest of the country, their economic structure is generally more export-oriented and service-based (He, Yu, and Zhu, 2020). The inclusion of a broad range of controls for these factors in the regression should mitigate these concerns. Thus, conditional on these controls, on average, it can be expected that labor market outcomes in both groups evolve in parallel. Further support for the assumption will be provided in the results section.

The second identifying assumption relates to what is commonly referred to as the "Ashenfelter Dip" (Ashenfelter, 1978) or "fallacy of alignment" (Heckman, LaLonde, and Smith, 1999): If firms or workers adjusted their labor demand or supply prior to the implementation of the policy in anticipation of its effect, the estimated effect would be biased. For instance, if firms - in anticipation of high future carbon prices - already reduced their output and labor demand before the policy came into place, an estimated negative employment effect would present a lower bound of the true effect. The fact that the implementation of the pilots (from June 2013 onward) followed relatively quickly after the first notice of the chosen pilot areas in October 2011 should alleviate this concern. Additionally, in the results section, I will present empirical evidence which gives support to this assumption.

Third, one must assume that the policy did not exert an influence on the labor market outside of the treatment regions. Several factors could violate this assumption: Despite the institutional barriers erected by the *hukou* system, especially Chinese cities are ex-

periencing large scale in-migration. If, for example, migrants negatively affected by the policy returned, the labor supply in their home provinces would increase. The same holds true if people that lost their job but stayed in the pilot areas commuted to firms outside of their province. Similarly, new employment opportunities in environmental-friendly sectors could have attracted people from outside to the pilot areas. As the direction of potential biases arising from these effects is unclear, I will further address this possibility in the robustness checks.

6.2 Estimation

To outline the details of the estimation strategy, I begin with a discussion of the following regression model specification:

$$D_{ict} = \beta P_t \times T_c + \gamma P_t + \kappa T_c + \delta X_{ct} + \alpha_i + \lambda_t + \epsilon_{ict}, \tag{1}$$

,

where D_{ict} is the respective labor market outcome (employment status, unemployment status, the log of monthly income and the log of monthly hours worked) for individual i residing in province c in survey round t. T_c is a dummy variable indicating whether an individual resides in a pilot area, P_t is a dummy variable which takes the value one for the post-treatment period (2014-2018). The coefficient for β is the coefficient of interest. It estimates the overall effect of belonging to a pilot area on the respective individual labor market outcome. ϵ_{ict} is an idiosyncratic error term. Following the recommendations of Abadie and Wooldridge (2017), standard errors are clustered at the treatment level (by province).

To control for common labor market development over time, I include time dummies for each survey round indicated by λ_t. α_i represents individual fixed effects which control for individual-level heterogeneity. Note that due to the inclusion of individual and time fixed effects, the coefficients γ and κ cannot be estimated. The usage of fixed effects (FE) was preferred over a random effects (RE) specification: Although a RE estimator is generally more efficient than its FE counterpart, the validity of the former relies on the assumption that individual unobserved heterogeneity is uncorrelated with all explanatory variables (Wooldridge, 2016). One might easily think of cases when this assumption is violated. For example, ability might be included in the error term and also be correlated with educational achievement which is used as a control variable in the RE specification (Winter, 2020). I still applied both random and fixed effects and conducted the Hausman test.[15] This approach allows to formally test whether there are statistically significant differences in the coefficients of the time-varying regressors. In case the null hypothesis - that there

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[15]In the random effects specification, I followed the labor market literature and included the following time-invariant individual controls: Gender, family size, marriage status, highest educational achievement, household registration status, age, age squared, a dummy indicating whether an individual belongs to the han majority, whether he or she lives in an urban area and whether he or she currently attends school.

are no significant differences in both estimators - cannot be rejected, the RE model can be used (Wooldridge, 2016). However, for all dependent variables, I obtain evidence in favour of a rejection of the null hypothesis and therefore carry out all regressions with a FE approach.

X_{ct} contains a vector of time-variant provincial level controls. These are included to isolate the effect of the pilots from other shocks that might exert an influence on labor market outcomes in different provinces. For instance, China is prone to natural disasters which cause significant human and economic losses every year. As large-scale disasters have found to have significant labor market effects, I include controls for disaster losses by region (e.g. Banerjee, 2007; Kirchberger, 2017). Other covariates include regional GDP per capita, the secondary and tertiary share of regional GDP, as well as trade exposure proxied by the share of imports and exports on regional GDP. The rationale behind the inclusion of these variables is to control for differences in the industrial structure which determines the regional exposure to other shocks potentially influencing the labor market (increase in automatisation, trade wars, etc.). Importantly, these variables might themselves be influenced by the regulation. For instance, regional GDP might shrink or expand in the aftermath of the regulation. As the inclusion of endogenous controls generates a considerable bias in the estimates, I follow the approach in Vona et al. (2018): To avoid endogeneity problems, I will fix these variables at pre-policy levels (2010) and interact them with a time trend. This allows to control for different patterns of average growth in labor market outcomes depending on specific industrial characteristics. Differences in time-invariant regional characteristics are already controlled for by the individual-level fixed effects. All income variables are transferred into real values for the base year 2010 using consumer price indices at the provincial level.

Although most studies assessing the impact of the Chinese ETS pilots define a single post-treatment period, this approach it not methodologically sound. In fact, the implementation dates of the ETS pilots varied slightly with four of the seven pilots being implemented in 2013, two in the first half of 2014 and the Fujian pilot in September 2016. To account for this, I use a single treatment dummy that varies over time:

$$D_{ict} = \beta \times T_{ct} + \delta X_{ct} + \alpha_i + \lambda_t + \epsilon_{imct}, \tag{2}$$

,

where T_{ct} is an indicator that takes the value one when an individual works in a province that fell under the ETS at survey round t. The DiD estimator β is equivalent to a weighted average of all DiD estimators that compare groups that change treatment status with those that do not (Goodman-Bacon, 2018). In an alternative specification, I borrow from Cui, Zhang, and Zheng (2018) and replace the discrete treatment variable with the logarithm of the yearly average carbon price in each province. This allows me to estimate the labor market effect of a one percentage increase in carbon prices. The latter can be thought of as a proxy for the burden of the environmental policy firms face. In contrast to the EU ETS, as shown in Figure 4, carbon prices in the Chinese ETS pilots are very volatile and

differ substantially between provinces.
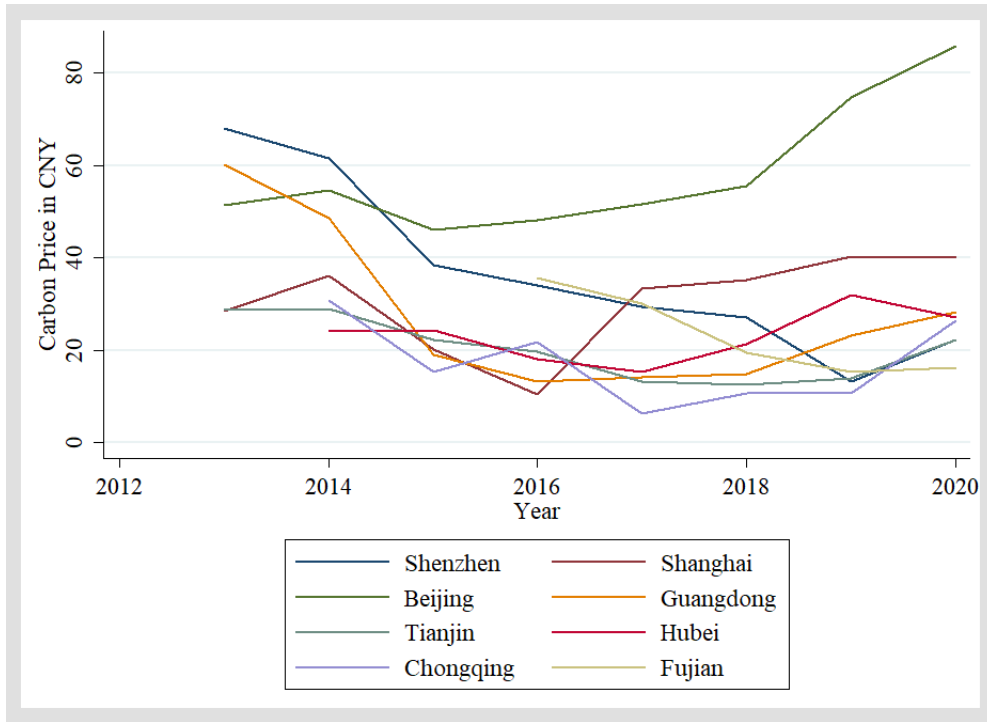
Figure 4: Development of carbon prices for each ETS pilot from 2013 until 2020

Note: Author's elaboration. The source of the date is China Carbon Trading Platform (2013-2020).

As it is therefore reasonable to account for differences in treatment intensity, this approach has found wide application in the empirical literature on the ETS pilots (Tian, 2020; Mo, Agnolucci, Jiang, and Fan, 2016). Yet, carbon prices are determined endogenously and can be driven by factors that also impact the labor market. Especially in early stages of the ETS scheme, short-term market fluctuations can affect carbon prices. Therefore, the results from this endeavor are relegated to the Appendix (see Appendix B) and should be interpreted with some caution.

The estimation of the aggregate effect of the ETS pilots can give a first indication of the labor market impacts of the policy. However, in the fledgling ETS scheme, the effects might have only materialized at a later stage when regulatory pressures intensified and carbon prices rose. To account for this possibility, I will follow Deryugina, Kawano, and Levitt (2018) and assess the dynamic effect of the policy in an event-type approach. The following equation shows an approximation of the estiamted model:

$$D_{ict} = \sum_{\theta=1, \theta \neq 3}^{5} \beta_\theta \times \mathbf{1}[t = \theta] \times T_c + \alpha_i + \lambda_t + \delta \mathbf{X}_{ct} + \epsilon_{imct}, \tag{3}$$

where θ indicates the survey round from 1 (2010) to 5 (2018). The coefficients for β are the coefficients of interest. Due to multicollinearity issues, β_3 is omitted. β_4 and β_5 estimate the effect of the policy in the years 2016 and 2018, respectively. β_1 and β_2 show the difference between individuals in the pilot and non-pilot areas in the years prior to the

policy. If there are no systematic differences in the labor market trajectory between both groups and the parallel assumption holds, these coefficients should be close to zero. The main advantage of this approach in comparison to a visual inspection of parallel trends in the pre-policy period is that this method determines whether any observed difference in the labor market developments is *statistically significant*.

In an additional specification, I evaluate the distributional effects of the policy by allowing for heterogeneity in the effect for workers with different educational backgrounds. More specifically, I classify workers with no or primary education as being low-educated, workers with some degree of secondary education as medium-educated and workers with tertiary education as high-educated. Following a Triple-Difference-Strategy, I estimate the following model:

$$D_{ict} = \eta T_c + \zeta P_t + \theta S_i + \kappa S_i T_c + \gamma S_i P_t + \beta T_c P_t + \iota S_i T_c P_t + \alpha_i + \lambda_t + \delta \mathbf{X}_{ct} + \epsilon_{ict}, \quad (4)$$

where S_i are dummy variables denoting the highest educational level of an individual at baseline (2010). The coefficient for β shows the effect of the policy on the omitted category, in this case low-educated workers. The net marginal effect on the other groups (middle- and high-educated workers) is estimated by the sum of the coefficients β and ι. Note that due to the inclusion of individual and time fixed effects, the coefficients for η, ζ, θ and κ cannot be estimated.

Ultimately, for the estimation of the effect of the policy on the dichotomous dependent variables (employment and unemployment), I use a linear probability model (LPM) for all specifications. I prefer this model over a logistic regression model with fixed effects as in the latter case individuals who do not alter their employment and unemployment status across the different waves are dropped from the estimation. This concerns more than two thirds of the sample and therefore substantially reduces sample size. Furthermore, using a LPM facilitates the interpretation of the coefficients. Yet, as a LPM has some drawbacks (e.g. constant slope, probabilities can be greater than one (Wooldridge, 2016)), as a sensitivity check, I additionally estimated all specifications using a fixed effects logistic model. Results (shown in Appendix B) do not change notably.

7 Empirical Results

7.1 Aggregate Effect

I start with a stepwise estimation of Equation (2) for each of the four labor market outcomes of interests. In what follows, columns with even numbers always include province-specific controls and are therefore my preferred specification. Results for the effect of the policy for a single post-treatment period are illustrated in Table 4. When controls are included, all coefficients show the expected sign pointing to a negative labor market impact

of the policy: The impact on monthly wages, monthly hours worked and employment is negative, whereas unemployment seems to have risen slightly. Yet, with the exception of the coefficient showing the effect on employment (Column 6), none of these coefficients are statistically significant. Even in the case of employment, where the coefficient is significant at a 10% significance level, the effect is small in magnitude: After the implementation of the ETS, individuals in the pilot areas are 1.4% less likely to be employed in comparison to people living in the control group (see Column 6). These findings suggest that the policy did not exert a strong overall impact on the labor market.

Interestingly, without the inclusion of province-level controls, coefficients tend to be less negative. In the case of employment, when province-controls are absent in Column 5, my findings even show a positive effect which is statistically significant at a 5% significance level. This suggests that other developments (e.g. export growth) related to the industrial structure of the provinces exerted a positive impact on the probability of being employed in the pilot areas (or a negative impact on the control group). Given that some of China's fastest growing cities (e.g. Shenzhen, Shanghai and Beijing) are included in the treatment group, this finding makes intuitive sense and underlines the importance of controlling for developments caused by the industrial structure of the provinces.

Table 4: Aggregate Effect of the ETS Pilots

	Log Wages		Log Hours		Employed		Unemployed	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat*Post	0.036	-0.009	-0.048	-0.050	0.037**	-0.014*	0.003	0.003
	(0.034)	(0.035)	(0.065)	(0.073)	(0.017)	(0.007)	(0.004)	(0.004)
Province-specific controls	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.088	0.089	0.035	0.035	0.013	0.015	0.001	0.002
N	33705	33705	51499	51499	79421	79421	58090	58090

Standard errors (clustered by provinces) in brackets. Each regression includes individual and time fixed effects.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

7.2 Dynamic Effect

These findings only provide evidence for the average effect of the policy over the whole post-treatment period. Labor market effects that materialized only in certain years might remain hidden with this approach. To account for this possibility, estimations based on Equation (3) show the dynamic treatment effect. Two things should be noted. First, due to the flaws in the survey design in the employment and unemployment question in the first survey round, the coefficient for the year 2010 can only be estimated for monthly wages and for monthly hours worked. Second, the ETS pilot in the Chinese province Fujian was only implemented in 2016. While Fujian is included in the treatment group

here, results - available upon request - are robust to its exclusion.

The coefficients for the pre-policy period (2010 and 2012) allow to assess whether the assumptions of parallel trends and of no anticipatory effects can be reasonably expected to hold. If this was the case, no difference in the development of labor market outcomes between treatment and control group should be noted prior to the policy and coefficients for the pre-policy period should be close to zero. Table 5 presents the results for the log of monthly wages and log of monthly hours worked. Coefficients in the upper part of the table give strong support to these assumptions. They show that no significant differences in the development of the respective labor market outcomes between the pilot and non-pilot areas existed for the years 2010 and 2012.

With regard to the effect of the policy in the post-treatment period - illustrated in the lower part of Table 5 - the findings confirm that the policy did not lead to a significant deterioration of real wage incomes for people with positive incomes. As shown in Column (1) and (2), none of the coefficients are significantly different from zero. In contrast, the policy led to a sizable reduction in monthly hours worked. According to the point estimate in Column (4), people living in the pilot areas reduced their working hours by 12.19% relative to the control group in the year 2016.[16] This effect dissipates over time. When including the province-specific controls, the effect is no longer statistically significant in 2018.

Table 5: Dynamic Effect of the ETS Pilots on Log of Wages and Log of Hours Worked

	Log of Monthly Wages		Log of Monthly Hours Worked	
	(1)	(2)	(3)	(4)
Treat*2010	-0.045	-0.039	-0.005	0.007
	(0.045)	(0.031)	(0.095)	(0.097)
Treat*2012	-0.010	-0.005	0.030	0.032
	(0.044)	(0.034)	(0.086)	(0.083)
Treat*2016	0.018	0.010	-0.116***	-0.130**
	(0.044)	(0.053)	(0.031)	(0.047)
Treat*2018	0.010	-0.003	-0.073**	-0.103
	(0.030)	(0.048)	(0.031)	(0.074)
Province-specific controls	No	Yes	No	Yes
R^2	0.089	0.090	0.038	0.038
N	33678	33678	51456	51456

Standard errors (clustered by province) in brackets. Each regression includes individual and time fixed effects.

The 2014 dummy is omitted. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6 shows the dynamic treatment effect for the binary outcomes employment and unemployment. Again, as shown in the upper part of the Table, no differential shocks in

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[16]$e^{-0.130} - 1 = -0.1219$

the probability of being employed and in the probability of being unemployed between treatment and control group can be found prior to the implementation of the policy. Yet, when the policy was implemented, individuals in the pilot areas experienced a non-negligible negative impact on the likelihood of being employed. My preferred specification in Column (2) shows that in the year 2016, on average, the pilot schemes reduced the probability of being employed by 3.7%. As opposed to the effect for hours worked, this effect amplifies over time and rises to 5.4% in 2018. Despite this negative employment effect, unemployment remains unaffected (see Column 4). This divergence should not necessarily be taken as evidence that laid off workers no longer form part of the labor force. It might simply arise because not all unemployed workers are captured by the unemployment variable in the survey.

Table 6: Dynamic Effect of the ETS Pilots on Employment and Unemployment

	Employed		Unemployed	
	(1)	(2)	(3)	(4)
Treat*2012	-0.013	0.013	-0.004	-0.005
	(0.013)	(0.008)	(0.004)	(0.005)
Treat*2016	-0.013	-0.037***	0.002	0.002
	(0.011)	(0.010)	(0.002)	(0.004)
Treat*2018	-0.007	-0.054***	0.005	0.006
	(0.021)	(0.015)	(0.003)	(0.007)
Province-specific controls	No	Yes	No	Yes
R^2	0.017	0.018	0.002	0.002
N	79368	79368	58048	58048

Standard errors (clustered by province) in brackets. Each regression includes individual and time fixed effects.

The 2014 dummy is omitted. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

7.3 Effect by Skill Level

The labor market effect of the policy might differ according to the skill level of workers. To check for this mechanism, I estimate the heterogeneous effect of the policy by skill level following the approximation in Equation (4).

Results for monthly wages and hours worked are shown in Table 7. Confirming the pattern found above, no significant effect on wages can be found. Furthermore, the observed negative effect on hours worked (Column 3 and 4) appears to be driven by medium-educated workers. Recall that according to the classification used here, these are workers that have obtained some secondary education but no tertiary education. The effect is not only statistically significant at a 1% level but also large in magnitude: According to the specification in Column (4), on average, the policy led to a 23.43% drop

in monthly hours worked for a medium-educated person in the pilot area.[17]

Table 7: Effect of the ETS Pilots on Log of Wages and Log of Hours Worked by Educational Level

	Log of Monthly Wages		Log of Monthly Hours Worked	
	(1)	(2)	(3)	(4)
Treat*Post	0.029	0.008	0.066	0.091
	(0.061)	(0.050)	(0.094)	(0.099)
Treat*Post*Middle	-0.001	-0.001	-0.263***	-0.267***
	(0.039)	(0.040)	(0.056)	(0.057)
Treat*Post*High	-0.008	-0.006	-0.213	-0.221
	(0.057)	(0.057)	(0.213)	(0.211)
Province-specific controls	No	Yes	No	Yes
R^2	0.090	0.091	0.040	0.041
N	33491	33491	51110	51110

Standard errors (clustered by province) in brackets. Each regression includes individual and time fixed effects.

Low-educated workers constitute the omitted category. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8 illustrates results for the probability of being employed and unemployed. In my preferred specification in Column (4), I find a small heterogeneous effect of the policy on the probability of being unemployed. At a 10% significance level, low-educated workers which form the omitted category, seem to experience a small increase of 1.3% in the probability of being unemployed (Column 4 line 1). In contrast, the marginal effect of the policy on medium-educated workers, increases by only 0.2%.[18] Taken together, these results point to different adjustment costs depending on the educational level. While medium-educated workers experience the negative shock along the intensive margin (reduction in hours worked), low-educated workers are rather affected along the extensive margin (increase in unemployment). The coefficient for the effect on the probability of being unemployment for high-educated workers shows the expected sign (negative) but is not statistically significant. This means that I cannot reject the null hypothesis that high-educated workers experienced a different effect on the probability of being unemployed than their low-educated counterparts. Additionally, I cannot find any effect on employment in Column 1 and 2. However, as shown above, this might be due to the fact that the negative employment effect materialized only at a later stage.

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[17] $e^{-0.267} - 1 = 0.2343$

[18] $0.013 - 0.011 = 0.002$

Table 8: Effect of the ETS Pilots on Employment and Unemployment by Educational Level

	Employed		Unemployed	
	(1)	(2)	(3)	(4)
Treat*Post	0.014	-0.011	0.013***	0.013***
	(0.022)	(0.012)	(0.004)	(0.004)
Treat*Post*Middle	-0.008	-0.004	-0.010**	-0.011**
	(0.012)	(0.012)	(0.004)	(0.004)
Treat*Post*High	-0.046	-0.037	-0.016	-0.020
	(0.029)	(0.026)	(0.013)	(0.012)
Province-specific controls	No	Yes	No	Yes
R^2	0.019	0.020	0.002	0.002
N	78875	78875	57770	57770

Standard errors (clustered by province) in brackets. Each regression includes individual and time fixed effects.

Low-educated workers constitute the omitted category. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

To account for this and to assess whether the effects on hours worked and unemployment amplify over time, I estimate the dynamic effect of the policy by educational level for each of these three outcomes. This is done by replacing the post-treatment dummy with three different dummy variables denoting the respective survey round in the post-policy period (2014, 2016, 2018). Results in Table 9 confirm the pattern outlined above: Medium-educated workers experience a reduction in monthly hours worked which slightly grows bigger over time. This is in line with the dynamic effect of the policy on employment and unemployment. As already shown above, the negative employment effect only came into place from 2016 onward and grew from a 5.5% decrease in 2016 to a 5.8% decrease in 2018. Similarly, the effect on the probability of being unemployed rises from an increase in 1.2% and 1.3% (for low-educated workers) in 2014 and 2016, respectively to a 2.5% increase in 2018. Yet, whereas in the case of employment, no significant differences between workers of different educational backgrounds can be found, the coefficients on unemployment suggest that the policy had the largest repercussions for low-educated workers. With only a 0.2% increase in unemployment in 2014[19] which rose to 0.4% in 2018[20], medium-educated workers experienced a smaller increase in unemployment. This difference, however, is not significant for the year 2016. For high-educated workers, the effect even turns positive in the year 2018 when they experience a drop in the probability of being unemployed of 0.6%.[21]

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[19] $0.012 - 0.010 = 0.002$
[20] $0.025 - 0.019 = 0.004$
[21] $0.025 - 0.031 = -0.006$

Table 9: Dynamic Effect of the ETS Pilots by Educational Level

	Log Hours		Employed		Unemployed	
	(1)	(2)	(3)	(4)	(5)	(6)
T*2014	0.109	0.101	0.026	-0.003	0.010***	0.012***
	(0.109)	(0.110)	(0.020)	(0.012)	(0.003)	(0.004)
T*2014*Middle	-0.258***	-0.258***	-0.019	-0.016	-0.009*	-0.010**
	(0.072)	(0.073)	(0.016)	(0.016)	(0.004)	(0.005)
T*2014*High	-0.229	-0.229	-0.040	-0.031	-0.010	-0.013
	(0.214)	(0.211)	(0.029)	(0.027)	(0.011)	(0.010)
T*2016	-0.005	-0.021	-0.002	-0.055***	0.010**	0.013*
	(0.130)	(0.122)	(0.022)	(0.013)	(0.004)	(0.007)
T*2016*Middle	-0.261**	-0.267**	0.011	0.014	-0.005	-0.006
	(0.126)	(0.129)	(0.016)	(0.015)	(0.007)	(0.007)
T*2016*High	-0.167	-0.185	-0.056	-0.050	-0.015	-0.018
	(0.243)	(0.237)	(0.037)	(0.035)	(0.016)	(0.016)
T*2018	0.039	0.010	0.018	-0.058***	0.021***	0.025*
	(0.087)	(0.103)	(0.032)	(0.014)	(0.007)	(0.012)
T*2018*Middle	-0.270***	-0.281***	-0.015	-0.011	-0.017**	-0.019***
	(0.065)	(0.066)	(0.016)	(0.015)	(0.007)	(0.006)
T*2018*High	-0.212	-0.239	-0.048	-0.036	-0.027*	-0.031**
	(0.200)	(0.197)	(0.036)	(0.035)	(0.013)	(0.013)
Province-specific controls	No	Yes	No	Yes	No	Yes
R^2	0.038	0.039	0.020	0.021	0.002	0.002
N	51153	51153	78928	78928	57812	57812

Standard errors (clustered by province) in brackets. Each regression includes individual and time fixed effects.
The low-educated worker dummy is omitted. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In sum, my results broadly seem to confirm my first hypothesis: They provide evidence for a negative effect of the policy at the intensive and the extensive margin. In the case of employment, the effect is not immediate and only materializes when the policy encompasses more establishments in the pilot areas in 2016 and 2018. Yet, contrary to what was expected, no significant changes on wages can be found. Furthermore, in accordance with the second hypothesis, I find some evidence for differences in the effect by educational level. Only the medium-educated workers seem to experience a reduction in their working hours. With regard to job losses along the extensive margin, results are less clear-cut. While I cannot find any evidence for different effects on the probability of being employed, the rise in unemployment - at least after some time - seems to be strongest for low-educated workers. Before discussing these results, I will test their robustness in the next section.

8 Robustness Checks

8.1 Spillover Effects

One important concern relates to the existence of spillover effects. As the ETS pilots were only implemented in certain parts of the country, they might have triggered the relocation of economic activity or of workers to nearby provinces. Although these effects are less likely to occur in the short-run (when workers and capital are partially immobile), this possibility should still be addressed. The evaluation of spillover effect is somewhat limited by the fact that the data only reveals information on the *province* a worker *resides* at instead of the location of its unit of work. With this caveat in mind, I start by evaluating whether the implementation of the ETS pilots has altered migration patterns. For example, if people previously living in the pilot areas lost their job but moved and found new employment in the control areas, the effect of the policy would be understated. To account for this, I re-estimate the effect of the policy by excluding people that have changed their residence from the treatment to the control area and vice versa after the implementation of the policy. Reassuringly, this only concerns a very small portion of the sample.[22] Perhaps not surprisingly, results - available upon request - do not change when these movers are excluded from the sample. Yet, even people that have stayed might have found work outside of their province. In the absence of information on the place of work, I cannot directly assess this possibility. However, if this concerns a substantial part of the sample, labor market effects in nearby provinces should become visible after the implementation of the policy. This can be tested by estimating the effect of the policy on provinces that share a border with the pilot areas following the same method outlined above. To obtain a large enough control group, I use all remaining Chinese provinces (that neither belong to the pilot nor to the adjacent provinces) as a control group. Results are displayed in Table 10. Statistically speaking, none of the estimates is significantly different from zero. This lends certain confidence to the notion that the policy did not trigger any large-scale spillover effects. Nevertheless, the evaluation of spillover effect at province level provides only a rough indication as Chinese provinces cover huge areas (see Figure X in Appendix B). Spillover effects that occur only in the closer border regions cannot be detected with this approach.

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[22]0.24% of the sample left the pilot areas after the introduction of the policy and 1.42% moved from the control to the pilot areas.

Table 10: Dynamic Effect of the ETS Pilots on Adjacent Provinces

	Log Wages		Log Hours		Employed		Unemployed	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Adjacent*2010	-0.03	-0.03	0.02	0.05				
	(0.03)	(0.03)	(0.09)	(0.09)				
Adjacent*2012	-0.02	-0.02	-0.01	0.01	0.02	0.02	-0.00	-0.00
	(0.05)	(0.04)	(0.08)	(0.08)	(0.01)	(0.02)	(0.00)	(0.00)
Adjacent*2016	-0.04	-0.04	0.03	0.02	-0.01	-0.02	0.00	0.00
	(0.06)	(0.07)	(0.04)	(0.05)	(0.01)	(0.02)	(0.00)	(0.00)
Adjacent*2018	0.04	0.05	0.01	-0.03	0.00	0.00	0.00	0.00
	(0.02)	(0.04)	(0.04)	(0.07)	(0.02)	(0.02)	(0.00)	(0.00)
R^2	0.088	0.089	0.035	0.035	0.013	0.015	0.001	0.002
N	33705	33705	51499	51499	79421	79421	58090	58090

Standard errors (clustered by provinces) in brackets. Each regression includes individual and time fixed effects.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

8.2 Concurrent Events

Another source of bias could arise from concurrent events that affected treatment and control group in a different way. Although the usage of time fixed effect accounts for common external shocks, it does not control for heterogeneous shocks with varying labor market impacts on the different provinces. Especially the announcement of a pilot trading scheme in energy use quotas in four provinces in the year 2016 and its implementation in the subsequent year could bias the results (Zhang and Zhang, 2020a). By putting a cap on the energy consumption in certain industries, similar to the ETS pilots, the policy potentially exerted an effect on production patterns and altered labor market outcomes in affected provinces. Since three of the affected provinces are in the control group and only Fujian belongs to the treatment group, these effects might differ between treatment and control group and bias the results. To account for this possibility, along the lines in Zhang and Zhang (2020a), I exclude all provinces that fell under this policy from the sample (Zhejiang, Fujian, Henan and Sichuan). Results in Table 11 do not change substantially both in significance and in magnitude. The coefficients for hours worked and employment are only slightly smaller than in my main specification. Since this change can already be noted in the year 2016, before the energy trading scheme was implemented, this policy is unlikely to be the driver of this change. Further analyses shown in Appendix B reveal that the slightly lower effect in employment arises entirely from the exclusion of Zhejjiang from the control group whose labor market experienced a relatively strong upswing since 2014. Thus, one can conclude that results are not biased by the establishment of this concurrent policy.

Table 11: Dynamic Effect of the ETS Pilots Without Provinces Participating in Energy Trading

	Log Wages		Log Hours		Employed		Unemployed	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat*2010	-0.068	-0.063**	0.069	0.079				
	(0.045)	(0.028)	(0.079)	(0.097)				
Treat*2012	-0.042	-0.029	0.095	0.096	-0.016	0.004	-0.004	-0.003
	(0.039)	(0.034)	(0.076)	(0.082)	(0.013)	(0.009)	(0.004)	(0.006)
Treat*2016	-0.007	-0.025	-0.094***	-0.101**	-0.017	-0.034***	0.002	0.001
	(0.049)	(0.054)	(0.029)	(0.037)	(0.011)	(0.011)	(0.002)	(0.005)
Treat*2018	0.006	-0.029	-0.055***	-0.076	-0.005	-0.038**	0.004	0.002
	(0.032)	(0.050)	(0.018)	(0.058)	(0.023)	(0.017)	(0.003)	(0.008)
R^2	0.085	0.086	0.050	0.051	0.016	0.018	0.002	0.002
N	27511	27511	40586	40586	63143	63143	46100	46100

Standard errors (clustered by provinces) in brackets. Each regression includes individual and time fixed effects.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

8.3 Major Urban Poles

Besides potential biases, one might be worried that results are driven by the inclusion of the economically advanced coastal regions in the treatment group. Especially Beijing and Shanghai stand out for their high level of economic development. While they are home to only 3% of the total Chinese population, in 2018, their regional GDP accounted for 7% of the national GDP (NBS, 2010-2018). With a per capita GDP that is more than twice as high as the national average and a post-industrial economic structure, they share features with developed countries and are little representative of an emerging economy (NBS, 2010-2018). If the found effects only arise because of the inclusion of these two exceptional cities, contrary to my objective, the findings would contribute little to the discussion of labor market effects in *emerging economies*. To assess whether this is the case, I exclude individuals residing in Beijing and Shanghai from the analysis. Results in Table 12 illustrate that even without these cities, the overall picture remains unchanged. Only the coefficient of the employment effect of the policy in 2018 drops moderately (from -0.054 to -0.043). This suggest that Beijing and Shanghai experienced a stronger drop in employment than the average of all pilot areas potentially arising from a relatively higher level of stringency of the policy. In sum, the findings from this robustness check support the external validity of the results. On average, the policy has detrimental effects on employment and hours worked which are not limited to the most prosperous parts of the country.

Table 12: Dynamic Effect of the ETS Pilots Without Beijing and Shanghai

	Log Wages		Log Hours		Employed		Unemployed	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat*2010	-0.080	-0.047	0.034	0.068				
	(0.050)	(0.033)	(0.084)	(0.105)				
Treat*2012	-0.019	-0.001	0.062	0.073	-0.023***	0.006	-0.004	-0.005
	(0.066)	(0.054)	(0.077)	(0.086)	(0.007)	(0.009)	(0.004)	(0.005)
Treat*2016	0.036	0.023	-0.105***	-0.126**	-0.011	-0.037***	0.001	0.001
	(0.054)	(0.067)	(0.032)	(0.052)	(0.014)	(0.013)	(0.002)	(0.004)
Treat*2018	0.014	-0.011	-0.071**	-0.112	0.010	-0.043***	0.003	0.003
	(0.045)	(0.071)	(0.029)	(0.083)	(0.016)	(0.015)	(0.003)	(0.008)
R^2	0.088	0.089	0.035	0.036	0.015	0.017	0.002	0.003
N	29261	29261	48575	48575	73368	73368	53467	53467

Standard errors (clustered by provinces) in brackets. Each regression includes individual and time fixed effects.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

9   Discussion

The last two sections brought to light detrimental labor market effects of the policy which, to a certain extent, occur at the expense of the less qualified workers. Unexpectedly, wages proved to be somewhat sticky and did not experience significant drops.

When placing these results into a broader academic context, one should first notice that the way this paper aimed to grasp the complex ways, in which environmental policies affect labor markets, differs from the standard approach in the literature. More precisely, rather than estimating the effects of the policy on a particular industry or sector, my estimations show the *average* labor market impact for the whole working-age population residing in affected regions. Although one can expect that in an interconnected business world, labor market effects trickle down to other industries, the strongest effect should be found in industries that directly fall under the respective policies. Keeping this in mind, the strong negative effects found in this paper are striking. Especially when comparing the results to the estimates of other studies focusing on the labor market effects of market-based instruments, the disruption local labor markets in the pilot regions experienced on average seems relatively strong. For example, the literature of the effects of the EU ETS - with the exception of the French labor market in the second phase - points to weak or or no negative employment effects on *directly* affected establishments (e.g. Marin, Marino, and Pellegrin, 2018).

Yet, my findings resonate with the scarce empirical evidence on environmental policies in China. They are in accordance with a sharp drop in employment following stringent environmental regulations on affected firms found in Liu, Shadbegian, and Zhang (2017). Moreover, they are broadly in line with the strong negative employment effect of 17% in 2015 in Zhang and Duan (2020), the only methodologically convincing study on the employment effects of the ETS pilots. They estimate the effect on certain directly affected subsectors which were covered in all ETS pilots. According to my own calculations (see

Table C.1 and C.2 in Appendix C), employment in these sectors only makes up a small fraction of the total employment in the pilot provinces (on average around 5%). Although the magnitude of the *average* negative employment effect of 3.7% found in this paper is therefore not per se comparable to their estimations, both studies point to a strong negative employment effect.

This raises the questions as to potential reasons for the strong toll the launch of the ETS pilots took on the Chinese workforce. The answer most likely resides in the realm of the Chinese economic and labor market structure. In the light of the high trade dependency and a limited labor market mobility, which slows down the transition of laid-off employees to less polluting sectors, a negative labor market impact was anticipated (see section 4). In addition, with the secondary industry contributing to around 46% of the national GDP and nearly 40% of the regional GDP in most pilot areas (see Figure C1 in Appendix C), China has not yet entered a post-industrialization stage (Huang, 2018).[23] Energy-intensive industrial bases, which directly fell under the policy, still play an important role in the Chinese economy (Liu, 2016). They are predisposed to experience severe repercussions, rippling through the economy, when carbon reduction policies like the ETS pilots are introduced rapidly (Liu, 2016). The fact that the negative employment effect magnified over time is consistent with the idea that this effect gradually spread to other industrial sectors. However, due to data constraints, I cannot verify this channel empirically. When I estimated the labor market impact separately by the sector in which a worker is employed, no coherent picture emerged. This is likely due to the coarse sector categories and a substantial number of missing values in this variable. Thus, without additional evidence, this idea remains conjectural.

Another factor potentially exacerbating any negative labor market effect was the economic slowdown China experienced over the last few years. This might have limited the scope for firms to adapt to the new situation (Zhang and Zhang, 2020a). Furthermore, Chinese firms have little experience with market-based climate policy instruments and "have yet to grapple with this new and unfamiliar territory" (Liu, 2016, p.5). Rather than harnessing market forces to achieve their emission targets at the lowest cost possible, a number of entities only participates in the market to fulfil their obligations at the end of the compliance period. This is reflected in a sharp rise in the trading volume before the compliance period ends (Liu, 2016). Evidence for the notion that a number of Chinese firms were severely affected by the policy is brought forward by Zhang and Duan (2020). Their analysis suggests that the covered sub-sectors reduced carbon emissions by lowering their gross industrial output value rather than by bringing down carbon intensity.[24] That these output reductions have motivated a reduction in hiring rates or

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[23]In comparison, in the US and the UK, the focus of much of the existent literature, the value added of the secondary industry in the same year was below 20% (The World Bank, 2015).

[24]Please notice that this result does not necessarily contradict the large consensus in the literature pointing to innovation-enhancing effects of the ETS pilots. On average, enhanced R&D spending and environmental-friendly innovations could have not sufficed to comply with the carbon target making output and subsequent employment losses inevitable.

dismissals does not seem far-fetched. Taken together, China's limited labor market mobility, its industrial structure and the lack of experience with market-based instruments of firms can potentially explain the relatively large job losses in the aftermath of the carbon trading markets.

My estimates further suggest that the adjustment to the policy has been entirely carried out via a reduction in employment and working hours instead of wages. Unfortunately, the empirical evidence on this issue is rather thin and inconclusive. My results stand in contrast to large wage losses found following an environmental policy in the US (Walker, 2011) but align with the finding that establishments that fall under the EU ETS did not lower the wages of their employees (Marin, Marino, and Pellegrin, 2018). However, it must be noted that the EU ETS did not exert a strong effect on the labor market overall.

One possible explanation for my finding is a potential lack of wage flexibility in the Chinese labor market. This reasoning would be consistent with the divergent results of the former two studies as wages in the US are more flexible than in the EU (Babecky, Caju, Kosma, Lawless, Messina, and Rõõm, 2010). Indeed, as outlined above, some downward-wage rigidity has been introduced in the Chinese labor market by the "Minimum Wage Regulations" in 2004 (Kang and Peng, 2017). The wage floor imposed could have hindered firms to lower wages in response to the shock. One might argue that this can only account for the absence of negative wage effects for workers at the lower end of the wage spectrum. However, high-educated workers, who typically do not fall in this wage category, seem to be less affected by the policy in general (at least according to some indicators).

Another possibility for the absence of significant wage effects relates to the quality of the data. The fact that the questions to determine monthly wages are not uniform across survey round can lower the accuracy of the wage data.[25] Thus, a potential measurement error inherent in my wage variable could have introduced substantial noise in the wage estimates. In the light of this, I cannot discard the possibility that small wages declines in the aftermath of the policy remained undetected in my study. This highlights the need for a better data coverage on Chinese labor market outcomes.

Ultimately, my results provide some evidence for differential effects on workers according to their educational level. Whereas low-educated workers experienced a strong and persistent increase in unemployment, medium-educated workers experienced a reduction in working hours. Hence, they were either able to keep their employment working less hours or found new part-time positions. This broadly confirms a pattern found for Canada suggesting that low and medium-educated workers were strongest affected by a carbon tax (Yip, 2018). Yet, one should note that my results are less clear-cut than what is suggested by this study. No difference across educational levels could be found in the probability of being employed. In the light of this ambiguity, the support for my second hypothesis - that the policy affected low- and medium-educated workers disproportionately - is

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[25]I also estimated the effect on family wages as this variable has been measured in a consistent way across all rounds. This does not alter results and wages remain unaffected.

rather weak and should be treated with some caution. A potential explanation for the lack of robust results is brought forward by Marin and Vona (2019). Their study suggest that standard human capital measures might not be optimal to assess how climate policies affect workers with different skills. In particular, climate policies bias employment towards specific technical occupations which are not limited to workers of a particular educational background. An exploration of the skill bias of climate policies with different skill measures is left for future research.

10 Conclusion

The aim of this study was to examine the labor market impacts of the regional ETS pilots in China until the year 2018. Although the adoption of sound climate policies is becoming more frequent in emerging countries, hitherto, little attention has been paid to their labor market consequences (Blackman, Li, and Liu, 2018). In particular, this is the first thorough examination of the labor market impact of a market-based climate policy in a non-Western, emerging economy context. I hypothesized that the policy would lead to job losses (measured by hours worked, employment and unemployment) and depress wages and that these effects would be strongest for low- and medium-educated workers. This was based on theoretical insights suggesting that, in the long run, climate policies shift workers from high-polluting to less-polluting economic activities. Prior to this transformation, substantial temporary labor market disruptions can arise. To test these hypotheses, I used individual-level panel data from the CFPS from 2010 to 2018 and employed a DiD strategy. Empirical challenges with regard to potential spillover effects or the presence of concurrent evidence were addressed with a range of sensitivity checks corroborating the robustness of the results.

My findings suggest that while wages remained unaffected, on average, the policy led to a drop in the number of hours worked and reduced employment. With an approximately 4% reduction in employment and a 10% reduction in hours worked, the effect is substantial and generally larger than the estimated employment effects of market-based policies in developed countries. I tentatively attribute this to the Chinese industrial structure which is still highly dependent on pollution-intensive manufacturing and the high degree of labor market segmentation hindering a smooth transition of workers. My analysis further provided some evidence for heterogeneous effect of the policy by the educational background of workers: The policy led to a drop in working hours for medium-educated workers while low-educated workers were more likely to become unemployed. As this pattern is not reflected in the employment numbers, it awaits further exploration. Yet, it gives a first indication of potential distributional consequences of the policy. More specifically, if the carbon market was implemented at the expense of low- and medium-educated workers, it might accentuate inequality.

These findings do not suffer a shortage of policy implications: First and foremost, Chinese policymakers need to better align environmental, economic and social objectives.

This is not only desirable from a normative perspective but it can also increase the political viability of climate policies. Special attention needs to be paid to the detrimental effect of the carbon market on workers. Schemes should be designed to compensate and quickly reintegrate those that have lost their jobs. Policies that foster labor market mobility could also be promising in this aspect. For example, a full abolition of the Chinese *hukou* system, which still promotes discrimination against rural migrants, could facilitate job transitions for this group. In order to provide a sound empirical foundation for Chinese policymakers, further research should estimate the labor market effects for each ETS pilot separately. This would allow to tailor mitigation strategies to the specific situation of workers in different areas. Furthermore, a comparison of the effects across regions with different industrial structures could be informative on the channels behind the empirical findings in this paper.

On a more aggregate level, the international community should recognize that the transition towards a low-carbon economy in China entails high costs for its workforce, at least in the short run. A large share of its workforce is still employed in polluting sectors which are typically hit hardest by climate policies (Liu, 2016). Despite these difficulties, stricter climate policies in China are an indispensable part in the global fight against climate change. To achieve a low-carbon transition in China without severe social disruptions, further international support should be offered. This might also be important for other emerging countries. It is yet too early to say whether the adverse labor market effects documented here apply outside of the Chinese context. This should motivate further research to explore whether similar labor market effects of climate policies can be observed in other emerging and also in developing countries. To achieve the commitments undertaken in the Paris Agreement without putting an unequal burden burden on workers in the poorer countries, the social and economic consequences of climate policies need to be closely monitored.

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Annex A: Construction of Variables

<p style="text-align:center">Table A.1: Variable names and labels in different survey rounds</p>

Variable name	Variable label/ Comment	Year
Employed		
qg3	*Currently has a job*	*2010*
employ	Employment status	2012
employ	Employment status	2014
employ	Employment status	2016
employ	Employment status	2018
Unemployed		
qj1	*Actively looking for a job last month*	*2010*
employ	Employment status	2012
employ	Employment status	2014
employ	Employment status	2016
employ	Employment status	2018
Monthly Hours Worked		
qg402*qg403	Average working days/month during working months *Average working hours/working day	2010
sg413*sg414	separate calculations for non-agricultural,	2012
qg512a1*qg513a1	self-employed	
((qg203*qg204)+(qg205*qg206))/2	& agricultural employment	
qg6*4.33	Weekly working time (hours)	2014
qg6*433	Weekly working time (hours)	2016
qg6*433	Weekly working time (hours)	2018
Monthly Wages		
qk101+qk102+(qk103/12)	Average monthly salary + floating wage + annual bonus	2010
qg417_a_1/12	After-tax income of 1st job last year (yuan)	2012
qk11 + qg1101/12	Salary after tax (yuan) + annual bonus	2014
qk11 + qg1101/12	Salary after tax (yuan) + annual bonus	2016
qk11 + qg1101/12	Salary after tax (yuan) + annual bonus	2018
for more details, see Lei (2020)		

Annex B: Additional Results

Table B.1: Aggregate Effect of the ETS Pilots on Log of Monthly Wages

	Log of Monthly Wages					
	(1)	(2)	(3)	(4)	(5)	(6)
Treat*Post	0.020	-0.006	-0.009			
	(0.039)	(0.034)	(0.051)			
Log of Carbon Price				0.008	0.005	0.002
				(0.012)	(0.011)	(0.015)
Province-specific controls	No	Yes	No	No	Yes	No
Province-specific time trends	No	No	Yes	No	No	Yes
R_2	0.088	0.089	0.090	0.088	0.089	0.090
N	31799	31799	31799	31799	31799	31799

Standard errors (clustered by provinces) in brackets. Each regression includes individual and time fixed effects.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.2: Aggregate Effect of the ETS Pilots on Log of Monthly Hours Worked

	Log of Monthly Hours Worked					
	(1)	(2)	(3)	(4)	(5)	(6)
Treat*Post	-0.077	-0.060	0.019			
	(0.071)	(0.078)	(0.104)			
Log of Carbon Price				-0.015	-0.007	0.004
				(0.026)	(0.026)	(0.030)
Province-specific controls	No	Yes	No	No	Yes	No
Province-specific time trends	No	No	Yes	No	No	Yes
R^2	0.036	0.037	0.042	0.036	0.037	0.042
N	48793	48793	48793	48793	48793	48793

Standard errors (clustered by provinces) in brackets. Each regression includes individual and time fixed effects.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.3: Aggregate Effect of the ETS Pilots on Employment

	Employed (Logit)				Employed (LPM)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat*Post	0.032	-0.046			0.005	0.001		
	(0.062)	(0.084)			(0.018)	(0.007)		
Log Carbon Price			0.009	-0.004			0.001	0.001
			(0.020)	(0.025)			(0.005)	(0.002)
Province-specific controls	No	Yes	No	Yes	No	Yes	No	Yes
X^2	478	508	478	508				
p	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
R^2					0.010	0.011	0.010	0.011
N	23302	23302	23302	23302	75584	75584	75584	75584

Standard errors (clustered by provinces) in brackets. Each regression includes individual and time fixed effects.

Column (1)-(4) are estimated using a logit model and Colum (5)-(8) using a linear probability model.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.4: Aggregate Effect of the ETS Pilots on Unemployment LPM and Logit

	Unemployed (Logit)				Unemployed (LPM)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat*Post	0.363*	0.094			0.004	0.001		
	(0.220)	(0.291)			(0.004)	(0.004)		
Log Carbon Price			0.106	0.039			0.001	0.001
			(0.068)	(0.083)			(0.001)	(0.001)
Province-specific controls	No	Yes	No	Yes	No	Yes	No	Yes
X^2	54	61	53	61				
p	0.000	0.000	0.000	0.000	0.002	0.000	0.002	0.000
R^2					0.002	0.002	0.002	0.002
N	1967	1967	1967	1967	55331	55331	55331	55331

Standard errors (clustered by provinces) in brackets. Each regression includes individual and time fixed effects.

Column (1)-(4) are estimated using a logit model and Colum (5)-(8) using a linear probability model.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.5: Dynamic Effect of the ETS Pilots on Employment and Unemployment Logit Model

	Employed		Unemployed	
	(1)	(2)	(3)	(4)
Treat*2012	-0.093	0.025	-0.298	-0.270
	(0.076)	(0.093)	(0.257)	(0.315)
Treat*2016	-0.123	-0.238**	0.167	0.142
	(0.079)	(0.093)	(0.259)	(0.315)
Treat*2018	-0.050	-0.280**	0.411	0.359
	(0.093)	(0.137)	(0.300)	(0.477)
Province-specific controls	No	Yes	No	Yes
X^2	480	515	56	62
p	0.000	0.000	0.000	0.000
N	23302	23302	1967	1967

Standard errors (clustered by province) in brackets. Each regression includes individual and time fixed effects.

All coefficients are estimated using a logit model.

The 2014 dummy is omitted. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.6: Effect of the ETS Pilots on Log of Monthly Wages and Log of Monthly Hours Worked by Educational Level

	Log of Wages				Log of Hours			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat*Post	0.029	0.010			0.067	0.097		
	(0.059)	(0.049)			(0.100)	(0.107)		
T*P*Middle	-0.001	-0.001			-0.264***	-0.269***		
	(0.038)	(0.039)			(0.065)	(0.066)		
T*P*High	-0.010	-0.007			-0.214	-0.223		
	(0.057)	(0.057)			(0.212)	(0.210)		
Carbon Price			0.033	0.025			0.026	0.032
			(0.023)	(0.021)			(0.029)	(0.030)
CP*Middle			-0.025	-0.026			-0.063***	-0.067***
			(0.019)	(0.019)			(0.018)	(0.017)
CP*High			-0.024	-0.025			-0.059	-0.066
			(0.019)	(0.020)			(0.043)	(0.041)
Province-specific controls	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.089	0.090	0.089	0.090	0.038	0.038	0.037	0.038
N	33518	33518	33518	33518	51153	51153	51153	51153

Standard errors (clustered by province) in brackets. Each regression includes individual and time fixed effects.

The 2014 dummy is omitted. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.7: Effect of the ETS Pilots on Employment and Unemployment by Educational Level Logit Model

	Employed				Unemployed			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat*Post	0.079	-0.047			0.993**	0.727*		
	(0.104)	(0.121)			(0.393)	(0.441)		
Treat*Post*Middle	-0.040	0.053			-0.758	-0.713		
	(0.136)	(0.138)			(0.503)	(0.515)		
Treat*Post*High	-0.375	-0.184			-1.345*	-1.368*		
	(0.282)	(0.289)			(0.790)	(0.805)		
Carbon Price			0.032	0.001			0.220*	0.141
			(0.032)	(0.035)			(0.115)	(0.124)
Carbon Price *Middle			-0.037	-0.010			-0.128	-0.116
			(0.041)	(0.042)			(0.149)	(0.154)
Carbon Price*High			-0.074	-0.016			-0.395*	-0.409*
			(0.086)	(0.089)			(0.235)	(0.242)
Province-specific controls	No	Yes	No	Yes	No	Yes	No	Yes
X^2	542.388	580.268	541.664	579.517	59.169	65.461	56.957	64.565
p	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
N	23302	23302	23302	23302	1967	1967	1967	1967

Standard errors (clustered by province) in brackets. Each regression includes individual and time fixed effects.

All coefficients are estimated using a logit model.

Low-educated workers form the omitted category. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.8: Dynamic Effect of the ETS Pilots Excluding only Zheijiang from the Control Group

	Log Wages		Log Hours		Employed		Unemployed	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat*2010	-0.049	-0.058*	-0.012	0.006				
	(0.045)	(0.031)	(0.095)	(0.104)				
Treat*2012	-0.016	-0.016	0.021	0.025	-0.013	0.007	-0.004	-0.004
	(0.044)	(0.034)	(0.088)	(0.087)	(0.014)	(0.009)	(0.004)	(0.005)
Treat*2016	0.016	0.013	-0.120***	-0.143***	-0.012	-0.030**	0.002	0.002
	(0.046)	(0.056)	(0.032)	(0.050)	(0.011)	(0.012)	(0.002)	(0.004)
Treat*2018	0.006	0.004	-0.084***	-0.132*	-0.005	-0.040**	0.004	0.004
	(0.030)	(0.051)	(0.027)	(0.076)	(0.021)	(0.016)	(0.003)	(0.008)
R^2	0.087	0.088	0.035	0.036	0.017	0.018	0.002	0.002
N	32514	32514	50148	50148	77247	77247	56342	56342

Standard errors (clustered by provinces) in brackets. Each regression includes individual and time fixed effects.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
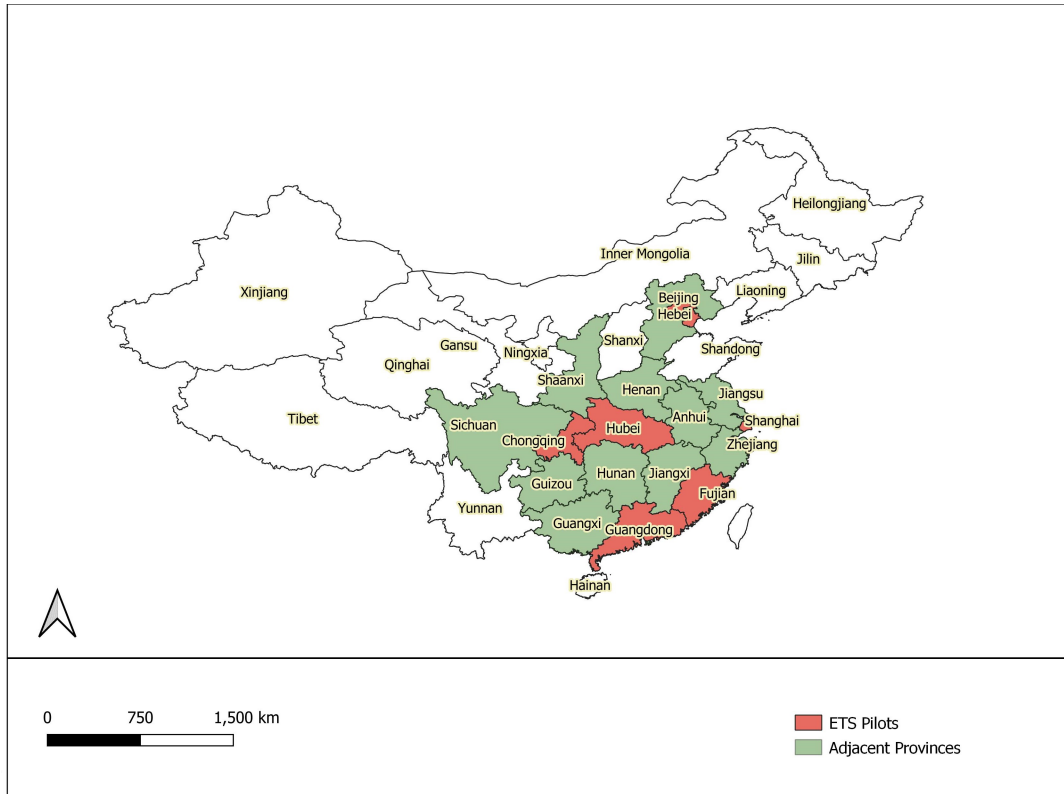
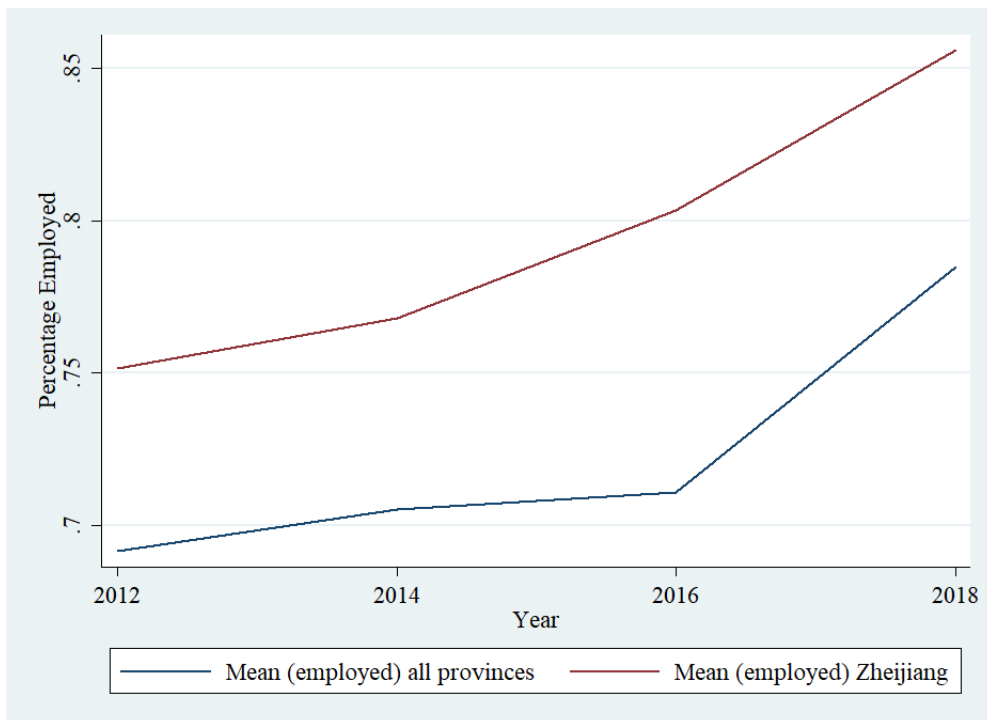Adjacent provinces of the ETS pilot areas

Figure B.1: Development of employment in Zheijiang and other provinces

Annex C: Chinese Industrial Structure and Covered Subsectors in all ETS Pilots

Table C.1: Industrial Subsectors Covered in all ETS Pilots

Sector Number	Sector Name
Sub14	Manufacture of Paper and Paper Products
Sub17	Processing of Petroleum, Coking, and Processing of Nuclear Fuel
Sub18	Manufacture of Raw Chemical Materials and Chemical Products
Sub22	Manufacture of Non-metallic Mineral Products
Sub23	Smelting and Pressing of Ferrous Metals
Sub24	Smelting and Pressing of Non-ferrous Metals
Sub35	Production and Distribution of Electric Power and Heat Power

Author's elaboration based on Zhang, Zhang, Li, Li, and Choi, 2020.

Table C.2: Employment in Covered Industrial Subsectors in 2013

ETS Pilot	Employment in Covered Subsectors	Total Urban Employment	% of Total
Beijing	194,039	11,628,000	1.7%
Tianjin	346,092	4,348,000	7.9%
Shanghai	330,886	1,0451,000	3.1%
Fujian	748,774	11,298,000	6.6%
Hubei	875,964	13,195,000	6.6%
Guangdong	1,686,581	36,371,000	4.6%
Chongqing	345,430	9,077,000	3.8%

Author's elaboration based on data from All China Data Center (2020).

The second column refers to the sum of the employed persons in the subsectors from Table C.1.
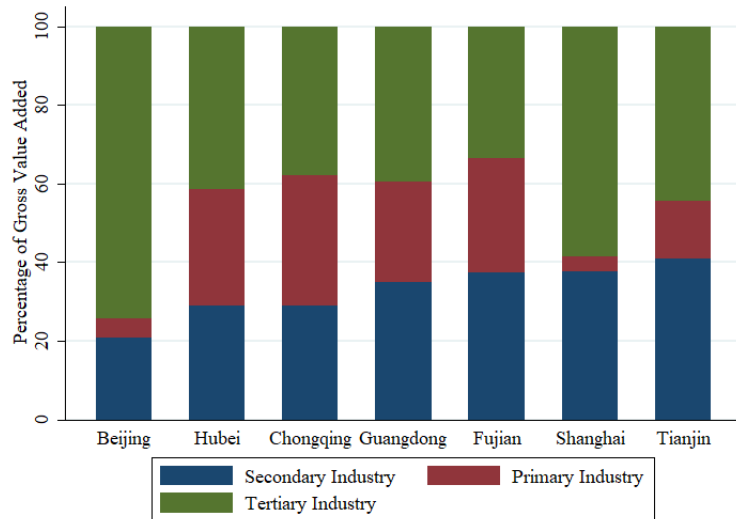
Figure C.1: Contribution of the three strata of industry to regional gross domestic product for each pilot region in 2010