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# Impacts of Climate Policy and Natural Disasters: Evidence from China

Ruijie Tian

UNIVERSITY OF
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# INTRODUCTION

This dissertation is comprised of three chapters studying the impact of climate policy on firm behaviour in China and the economic consequences of adverse shocks in the form of natural disasters. China is the largest contributor to global greenhouse gas (GHG) emissions responsible for anthropogenic climate change and currently accounts for over a quarter of global carbon emissions (Le Quéré et al., 2017). Over the past decade, China has aimed to reduce its GHG emissions using an ambitious Emissions Trading Scheme (ETS) as the main policy instrument. The ETS was piloted at a regional level over the period since 2013 before the launch of a national ETS in 2017. The first two chapters of the dissertation assess the impact of the pilot ETS on technical change and emissions reduction at the firm-level. The third chapter studies the international propagation of adverse shocks.

In the first chapter, I study the effect of the Chinese pilot ETS on 'green' technological change as measured by the number of green patents. The empirical identification of the ETS's effect on innovation is based on a differences-in-differences estimation using a count data model. The sources of variation are the years of implementation of the pilot ETS in different pilot regions with both regulated (i.e., treated) firms and non-regulated (i.e., control) firms in each region. Ideally, one would either compare firms that are identical in all aspects except for treatment status (being regulated or not), or exploit a random assignment of the treatment to firms. However, in the Chinese pilot ETS, only firms with yearly carbon emissions above a certain threshold are regulated. Hence, estimates from simply comparing the patent counts between treatment and control firms before and after the implementation of the regulation would be biased. I address this challenge by matching regulated firms with non-regulated firms on a vector of pre-treatment variables, such that firms in the two groups are balanced on the observable variables.

I estimate the impact of the pilot ETS in China both at the extensive (i.e., entry into green innovation) and intensive (i.e., amount of green innovation) margins.

I find that the pilot ETS increased the average number of green patents among firms by 0.16 per year. This increase amounts to 2.8% of the yearly average green patents in the post-treatment period (2013-2017). In addition, I estimate the carbon price elasticity and find that a 10% increase in carbon prices will increase the number of green patents produced by 2.3%. The effects are heterogeneous across both pilot regions and firms but more pronounced in Beijing and Shanghai–two regions that have some of the highest carbon prices. At the intensive margin, the effects are strongest for the relatively larger firms at the higher end of worker productivity.

These findings provide insight into the impact of the pilot ETS in China and the potential effectiveness of the national ETS. Overall, the regulation seems to be effective in inducing green innovations during the pilot phase. However, effectiveness differs across the pilot regions. A potential explanation for this is the regional differences in the policy design, such as the allowance allocation, coverage threshold, regulated sectors and the cost of non-compliance. I show that, on average, the higher the resulting carbon price, the higher the number of green innovations induced by the pilot ETS. This increase in green innovation is primarily driven by intensive margin decisions by regulated firms that already have high output per worker (and therefore higher productivity and/or more capital) and are more competitive initially. Likelihood of starting doing green innovation is lowered for the firms at the higher end of output per worker. The heterogeneity findings indicate that an important policy challenge is to encourage the regulated firms to start innovation in green technologies and this is especially important for firms that are larger and more productive.

In the second chapter of the dissertation (coauthored with Da Zhang, Xiliang Zhang and Thomas Sterner), we focus our attention on the Beijing pilot ETS, and assess whether and how the pilot ETS induces firms to reduce emissions. The Beijing pilot ETS is of particular interest because it is one of the pilots with the highest carbon prices. We therefore expect stronger firm-level responses due to more salient energy price increases in Beijing compared to other pilot regions. For this purpose, we exploit a unique firm-level dataset of regulated firms in Beijing from 2009 to 2017. Our identification strategy relies on the specific coverage threshold that determines whether a firm would be regulated. The existence of a coverage threshold allows us to use a fuzzy regression discontinuity design (RDD) to identify the causal effect of the pilot ETS on firm-level emissions. We rely on a fuzzy RDD, instead of a sharp RDD, as there are some other random determinants of regulatory status, such as administrative errors. We find that on average, the pilot ETS reduces carbon emissions by 39%. Firm responses vary: emissions are reduced by about 45% (mainly by reducing coal use) in the industrial sector but hardly change in the service sector.

Next, we investigate whether the initial allowance allocation had no impact on emissions in subsequent years in the Beijing pilot ETS–a necessary condition for a cap-and-trade market to be effective. We find that, overall, such an independence property holds. However, it likely fails for some firms in the service sector and is more likely to fail for smaller-sized firms. This suggests that free allowances could dampen emissions abatement for these firms.

In the final chapter (coauthored with Katharina Längle and Ankai Xu), we use Chinese firm-level data to understand the international propagation of adverse shocks triggered by the 2005 hurricane season in the United States. We provide evidence that Chinese processing manufacturers with tight trade linkages to the United States reduced their intermediate imports from the United States between July and October 2005. We further show that the direct exposure to the United States supply shocks led to a temporary decline of firm exports between September and November 2005. In addition, we show that firms with more diversified suppliers tend to be less affected by the United States hurricane disaster. We however, do not find consistent evidence for an international propagation of supply shocks along global value chains.

In summary, the results in this thesis provide insights on the role of carbon pricing in tackling climate change. This thesis also provides insights in a broader context for the analysis of supply chain effects of adverse shocks.

# Chapter 1

# EMISSIONS TRADING SCHEMES AND DIRECTED TECHNOLOGICAL CHANGE: EVIDENCE FROM CHINA

**Abstract**

This paper examines the impact of carbon emissions trading schemes (ETS) on technical change proxied by the number of green patents in the context of the pilot ETS in China. I find a small increase of 0.16 patents per firm and year. A 10 percent increase in carbon prices increases green patents by 2 percent. The strongest effects are for the two regions in the upper range of carbon prices and for more productive firms. However, there are contrasting patterns at the extensive and intensive margins of green innovation: the pilot ETS reduces entry into green innovative activities but increases levels of innovating for firms that were innovative before they were regulated by ETS, especially for the more productive firms. This indicates that an important policy challenge is to encourage the firms covered by ETS to start innovation in green technologies; this applies particularly to the larger and more productive firms.

## 1.1 Introduction

The past decade witnessed a take-off of large-scale $CO_2$ emissions reduction policies, including emissions trading schemes (ETS) that started to play a promising role in combating climate change.[1] One of the most notable ETS developments in recent years has been the implementation of pilot schemes in China. These schemes currently cover 11 percent of Chinese $CO_2$ emissions. It is expected that the Chinese pilot schemes will be integrated into a nation-wide emissions trading scheme in the future. An integrated scheme would cover more than a third of Chinese emissions (about 10 percent of global carbon emissions), making it the largest ETS globally. The effect of an ETS is to put a price on carbon emissions, with the purpose of achieving environmental goals in an efficient manner. The introduction of an emission price provides a continuous incentive for adoption and innovation of emission-reducing technologies (Baranzini et al., 2017).[2] In this paper, I empirically identify the causal effect of emission pricing on innovation in the context of the Chinese emissions trading pilots. I construct a unique Chinese firm-level panel dataset, using yearly patent counts as a measure of innovation. The dataset contains detailed information on firm characteristics, including patent activity and regulatory status (whether the firm is covered by ETS).

The empirical identification of the ETS effect on innovation is based on a differences-in-differences estimation, using a zero-inflated Poisson model. The sources of variation are the years of implementation of the pilot ETS in different pilot regions with both regulated firms and non-regulated firms in each region. Ideally, one would either compare firms that are identical in all aspects except for treatment status (being regulated or not), or exploit a random assignment of the treatment to firms. However, in the Chinese pilot ETS, only firms with yearly carbon emissions above a certain threshold are regulated. Hence, estimates from simply comparing the patent counts between treated and control firms before and after the implementation of the regulation would be biased. I address this issue by matching regulated firms with non-regulated firms on a vector of pre-treatment variables, such that firms in the two groups are balanced on the observable variables.

[1]The European Union ETS (EU ETS), set up in 2005, is the world's first carbon emissions trading system and currently operates in 28 EU member states, plus Iceland, Liechtenstein and Norway. Subsequently, ETS have been established in California and 10 states in the US (RGGI), with further implementation scheduled in Japan and more states in the US, among others.

[2]For the literature on the the role of environmental regulation in firm innovation, see e.g., Fischer et al. (2003), Biglaiser and Horowitz (1994) Requate and Unold (2003), Di Maria and Smulders (2017) and Requate (2005).

Applying my estimation strategy to the data, I find a statistically significant effect of the pilot ETS on *green patenting*. I show that the pilot ETS increased the firm average annual number of green patents by 0.16. This increase amounts to 11.7 percent of the yearly average green patents in the pre-treatment period (2007-2012) and 2.8 percent in the post-treatment period (2013-2016). In addition, I estimate the carbon price elasticity: a 10 percent increase in carbon price increases green patents by 2.3 percent. I find no evidence that this increase leads to crowding out of non-green patents. I then show that the effects are heterogeneous across both pilot regions and firms, with the strongest effects for the two regions that have some of the highest carbon prices (Beijing and Shanghai) and, at the intensive margin, for the firms that are at the higher end of worker productivity and thus are initially more competitive.

This paper contributes to the literature that analyzes the impact of environmental policies on innovation. The three papers most closely related to this study are Calel and Dechezleprêtre (2016), Zhu et al. (2019) and Cui et al. (2018).[3] Calel and Dechezleprêtre (2016) evaluate the causal effect of the EU ETS on low-carbon innovation, proxied by the number of patents filed by firms. They use a matched differences-in-differences estimator, and find a small but positive effect of the EU ETS on firms' innovation. Further, Zhu et al. (2019) and Cui et al. (2018) study the impact of the pilot ETS on innovation in China. They both find increases in green patenting induced by the pilot ETS.

This paper extends the literature in four principal ways. The first is the focus on heterogeneity across firms and pilot regions, unlike previous studies, which have estimated the average treatment effects of carbon pricing on green innovation. My analysis of heterogeneity provides new evidence on what might be driving the significant effects found in previous studies. I show that the effectiveness of the pilot ETS differs across the pilot regions. A possible explanation is the regional differences in the policy design, such as allowance allocation, coverage threshold, sectors regulated, and costs of non-compliance; these lead to substantially different emission prices across the regions. I also find that the increase in green innovation is primarily driven by intensive margin decisions by regulated firms that already have high output per worker (and therefore higher productivity and/or more capital). This provides evidence on characteristics of firms that may make them more likely to respond to ETS with green innovation.

[3] Other related empirical studies evaluate impacts of ETS on firms' investment strategy and carbon leakage (aus dem Moore et al., 2019; Fell and Maniloff, 2018), productivity and competitiveness (Chan et al., 2013; Bushnell et al., 2013) and emission abatement (Anderson and Di Maria, 2011; Petrick and Wagner, 2014)

Second, I estimate carbon price elasticity for green patents as an indicator of the continuous incentives for innovation. The pilot ETS in China is an ideal setting to estimate this because of the substantial variation in carbon prices. The various pilot schemes provide considerable heterogeneity across regions because of the decentralized manner in which they were introduced: each local government designs its own rules. (See Section 1.2.)

The third contribution is a more precise measure of the outcome variable - the number of green patents - which has the advantage of reducing potential measurement error. The policy effect is more precisely estimated in this study, compared to the two earlier studies on the Chinese pilot ETS effect on green innovation, because I only focus on the type of patents that are more valuable (invention patents)[4] and the patents that are directly impacted by the regulation (low-carbon patents). The patents in the invention category need to pass through a thorough examination for novelty, and therefore are more likely to be radical innovations.[5] I also exclude from the sample all patents that are either carbon-intensive, such as technologies for gas-turbine plants and cremation furnaces, or not directly related to low-carbon innovation, such as innovation in agricultural technologies.

Lastly, this paper separately identifies the effects of the ETS on green innovation at the extensive and intensive margins–that is, both the likelihood of entry into green innovation and the amount of such innovation. I find contrasting patterns at the two margins: the pilot ETS reduces entry but increases levels of green patents for innovating firms, especially at the upper range of output per worker distribution.

The Chinese ETS pilots are of particular interest for three reasons. First, China contributes over a quarter of global carbon emissions (Le Quéré et al., 2017). Even though this paper focuses on regional pilot implementation of ETS, even a partial policy response can have large cumulative effects on global emission trajectories. Second, China is moving towards integrating the separate emissions trading pilots; as a first step, they launched a national trading scheme in December 2017. Even though the national scheme covers only the electricity sector at

[4]There are three categories of patents in the Chinese patenting system, namely invention, utility and design. Utility and design patents require no substantive examination and reflect only incremental innovation (Hu et al., 2017). Applications for invention patents need to pass through an examination for novelty and non-obviousness. Because the other two types of patents are not subject to examination, they are particularly vulnerable to the abuses of the patenting system to preempt competition from foreign firms (Hu and Jefferson, 2009).

[5]This is a common practice in the existing literature related to studies on Chinese patenting. To list a few, depending on the type of questions answered, the literature either categorizes the patenting variables by the type of innovation ( Liu and Qiu 2016 and Hu et al. 2017), or only focuses on the invention patent category (Bombardini et al. 2017, Li 2012, and Dang and Motohashi 2015).

present, it already comprises the world's largest carbon market by covering over 30% of Chinese emissions (ICAP, 2018). A greater understanding of the industry responses to the pilot schemes will allow policymakers to better anticipate the impacts of the national ETS. Third, the Chinese context distinctly differs from the developed country context of most existing ETS: China is a transitional economy with a number of institutional and historical differences from the European and US economies. Hence, it is not obvious whether one can simply extrapolate results from the latter context to the Chinese ETS. By considering the Chinese case specifically, this paper assesses whether past research on European and North American environmental regulation generalizes to the Chinese context.

I find positive and significant effects of the pilot ETS on firms' innovation, which is in line with the existing literature on the effect of environmental regulation on innovation and technology adoption. For instance, Gray and Shadbegian (1998) find that new plants in states in the US with more stringent environmental regulation are less likely to adopt dirtier production technologies. Popp (2003) explores the effect of the US Clean Air Act (CAA) of 1990 on innovations in pollution control for power plants, and finds that innovation occurring after passage of the CAA was more environmentally friendly. Brunnermeier and Cohen (2003) find that increases in pollution abatement expenditures are associated with a small but statistically significant increase in environmental innovation. Tang (2015) studies the impact of Cleaner Production Audit (CPA) programs on innovation in Chinese listed companies, and confirms a positive effect. In summary, findings from these studies conclude that there is a positive link between environmental regulation and innovation. Beyond these substantive findings, this paper points the way forward in learning the effect of carbon pricing on green innovation.

The remainder of the paper proceeds as follows. Section 1.2 provides some additional institutional background by reviewing the main characteristics of the Chinese ETS pilot schemes. The data used in the empirical analysis are described in Section 1.3, while Section 1.4 lays out the empirical strategy. Results are presented in Section 1.5, and Section 1.6 concludes.

## 1.2 Pilot Emissions Trading Schemes in China

In recent decades, China has adopted several market mechanisms to combat climate change. With the target of efficiently reducing greenhouse gas emissions by 2020, the Chinese National Development and Reform Commission (NDRC) approved the implementation of pilot emissions trading schemes (ETS) in 2011.

Seven provinces, municipalities and regions were selected as "*pilot regions*".[6] The aim of these pilot regions is to reduce $CO_2$ emissions, learn about the effects of the program, and ease the transition towards country-wide, market-based environmental regulation. Beijing, Shanghai, Tianjin and Guangdong released individual plans and implemented pilot ETS at the end of 2013, while Shenzhen implemented its pilot ETS in June 2013. Hubei and Chongqing initiated pilot ETS in April and June 2014, respectively. Lastly, on 22 September 2016, Fujian Province voluntarily opted in and released a conditional announcement of the introduction of China's eighth pilot scheme.

The China pilot ETS are designed as trading systems based on either an absolute cap or an intensity target. In all pilots, the large majority of firms receive grandfathered emission allowances. Firms that emit less than their allowances can sell excess allowances at the market price. Conversely, if emissions exceed the initial allowance, additional allowances have to be purchased to ensure compliance. Below, I discuss several additional key aspects of the Chinese ETS, including the regulated sectors and the coverage threshold that determines which firms are regulated. Further details about these are presented in Appendix 1.A.

### 1.2.1 Allowances Allocation

There are two approaches to the allocation of emissions allowances: they are either freely allocated or sold by auction. In China, the allowances are freely allocated in all the pilot regions except for Guangdong, where at most 5% of the total amount of allowances are auctioned. Two ways of allocating allowances freely are grandfathering and benchmarking, which are commonly used in China.[7]

All eight pilot regions determined the total allowances based on the emissions mitigation targets in the 13th Five Year Plan (period 2015-2020). For instance, the target for Beijing is to rigorously control total carbon emissions and meanwhile reduce carbon emissions intensity, while Hubei aims to reduce the emissions intensity annually, without controlling for total carbon emissions. These intensity reduction targets differ slightly in a majority of the pilot regions, ranging from a 19 percent to 22 percent reduction by 2020 compared to the intensity in 2015.

[6] These are four municipalities (Beijing, Tianjin, Shanghai and Chongqing), one special economic zone (Shenzhen), and two provinces (Hubei and Guangdong).

[7] With grandfathering, regulated firms receive free allowances initially according to their historical emissions in a base period; with benchmarking, the firms receive allowances according to performance indicators, such as firms' annual production and emissions relative to an industry or a sector.

### 1.2.2 Coverage Thresholds

Unlike the thresholds in the EU ETS, which are determined at the plant level, the thresholds in the pilot ETS in China are determined at the firm level and differ across the pilot regions. The threshold is highest in Hubei at over 100,000 tons of annual $CO_2$ emissions over the period 2013-2015, and lowest in Shenzhen at 3,000 tons of annual $CO_2$ emissions. Since 2016, the thresholds dropped in Beijing, Shanghai and Hubei by over 50 percent on average. In contrast, Shenzhen, Chongqing, Tianjin and Guangdong have not reduced the thresholds.

### 1.2.3 Regulated Sectors

Apart from the thresholds, a firm's sector might determine whether a firm is regulated or not. In Tianjin, for instance, firms in the transportation sector are exempted from the regulation, regardless of emissions, while in Beijing, the threshold is the sole determinant of whether a firm is part of the ETS. In Guangdong, more sectors–that is, the paper and aviation industries, are included in the ETS. Over time, the coverage of the regulation has become broader and more sectors and firms are being regulated.

Due to differences in total allowable emissions, coverage thresholds and the sectors subject to the ETS, equilibrium prices for the emission allowances differ across the eight regions. The monthly average allowance price ranges from 87 Yuan (13 US dollars) in the Beijing pilot to 1.61 Yuan (0.24 US dollars) in the Chongqing pilot. This heterogeneity in allowance prices implies that firms' costs of compliance, and thereby the incentive to innovate, in $CO_2$-reducing technologies differ across regions.

## 1.3 Data

In this section I describe the data used for the analysis. The data originate from three different sources: the regulatory status from local Development and Reform Commissions, patent application data from the State Intellectual Property Office, and firm characteristics from the Annual Survey of Manufacturing Enterprises (ASME).

### 1.3.1 Regulatory Status

Information on the regulatory status of firms is obtained through municipal and provincial development and reform commissions (DRCs). As the Chongqing

Table 1.1: Number of entities regulated in China pilot ETS

| Pilot | Year | | | |
|---|---|---|---|---|
| | 2013 | 2014 | 2015 | 2016 |
| Beijing | 450 | 543 | 551 | 947 |
| Shanghai | 197 | 197 | 197 | 310 |
| Shenzhen | 639 | 636 | 635 | 824 |
| Tianjin | 114 | 112 | 109 | 109 |
| Hubei | NA | 138 | 167 | 236 |
| Guangdong | 184 | 194 | 186 | 244 |
| Fujian | NA | NA | NA | 277 |

DRC does not publish the list of regulated firms, it is excluded from this study. The number of regulated firms is summarized in Table 1.1.[8] Specifically, it lists the number of regulated firms in each pilot region and each year from 2013 to 2016. Most notable from Table 1.1 is the rapid increase in the number of regulated firms in Beijing, Shanghai and Shenzhen in 2016, caused by the downward adjustments in coverage thresholds.

### 1.3.2 Patent Data

The annual number of patent applications is used as a proxy for firms' innovation activities.[9] Patent data come from the system of Patent Search and Analysis, which is hosted by the State Intellectual Property Office (SIPO) of China.[10]

All patents in China are categorized based on the International Patent Classification (IPC). The IPC provides a universal language for the classification of patents according to the different technology areas to which they pertain. Because the interest of this study is to explore the effect of $CO_2$ regulation on the firms'

[8]Regulated firms in this paper refer to those that are part of the pilot ETS regulation and hence are in the treatment group. Non-regulated firms are those that are not regulated by the pilot ETS and hence are in the control group.

[9]An alternative measure of innovation in the literature is RD expense. Though patent data is broadly accessible in China, RD expenses of firms for consecutive years is limited, making it infeasible in the current context. Using patent data to proxy for innovation is a common approach in empirical studies, such as Hu and Jefferson (2009), Dang and Motohashi (2015), Bombardini et al. (2017) and Liu and Qiu (2016).

[10]SIPO was renamed the China National Intellectual Property Administration (CNIPA), on 28 August 2018. The data are accessible through the URL `http://www.pss-system.gov.cn/sipopublicsearch/portal/uiIndex.shtml` (first accessed December 2017 with subsequent access in July, 2018). I collected the data using web-scraping. There is a time lag between publication date and application date. Some patents applied before 2018 might not be published by the date of access. The average time lag between 2007 and 2012 is 400 days, and the median is 230 days. More than 75 percents of the filed patent are published after 540 days (around one and half years) of the application date. Therefore, by the date of access, the patent data could well represent the population of patent applications, at least for patents filed before 2017.

green innovation activity, I consider a subset called the "IPC Green Inventory" between 2007 and 2016. These are the patents related to so-called Environmentally Sound Technologies (EST, henceforth green patents) (IPC Committee, 2017), as listed by the United Nations Framework Convention on Climate Change. I use the patent classification codes for technologies on alternative energy production, transportation, energy conservation, waste management, nuclear power generation and administrative, regulatory or design aspects to select the green patents, with technologies on agriculture excluded from the category because these technologies are not directly related to low-carbon technology. In addition, following Dechezleprêtre et al. (2020), I exclude from the IPC green inventory patents in carbon-intensive technologies such as gas-turbine plants, cremation furnaces, and steam-engine plants.

In order to estimate the ETS effect on the direction of the technological change, and whether the ETS increases the green patents at the cost of dirty patents, I rely on Dechezleprêtre et al. (2020) to identify the patent classification codes on the dirty technologies. These mainly include patents on electricity generation technologies and technologies in the automobile industry.

For each individual patent, the dataset contains information on the IPCs, the name of the invention, application number and date, publication number and date, applicants, address of applicants, and whether an application is approved.[11]

I use this dataset to construct the number of patent applications at the firm-year level.[12] Figures 1.1 and 1.2 show the numbers and shares of green and dirty patent applications for regulated and non-regulated firms from 2007 to 2016. Figure 1.1 presents both the total and weighted number of green patents, where in the latter case a $1/n$ share of the patent is assigned to each applicant firm, with $n$ the number of applying firms. As such, the weighted patents avoid double-counting when the patent is filed by several co-applicants.

The vertical dashed lines in the figures indicate the years that ETS pilots were announced (2011) and implemented (2013). As shown in Figure 1.1, the total number of green patent applications by regulated firms did not grow as fast as those by non-regulated firms. Meanwhile, the shares of green patent applications for regulated and non-regulated firms increased nearly parallel to each other before 2011 (Figure 1.2). Since 2011, the share for regulated firms has increased rapidly, while the share for non-regulated firms has been rather flat. The trends in the unweighted green patents are similar to the weighted ones both for regulated and non-regulated firms, indicating that the average number of applicants

[11]Contrary to patent data hosted by the European Patent Office, SIPO does not include information on citation, which is commonly used as a measure on patent quality.

[12]Details about merging and constructing the dataset are in Appendix 1.B.

Figure 1.1: Number of green patents 2007-2016, weighted and unweighted

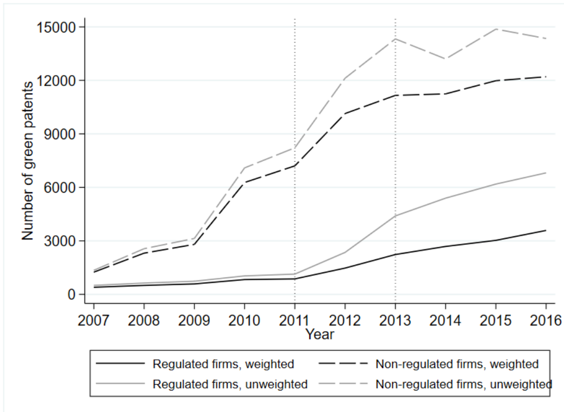
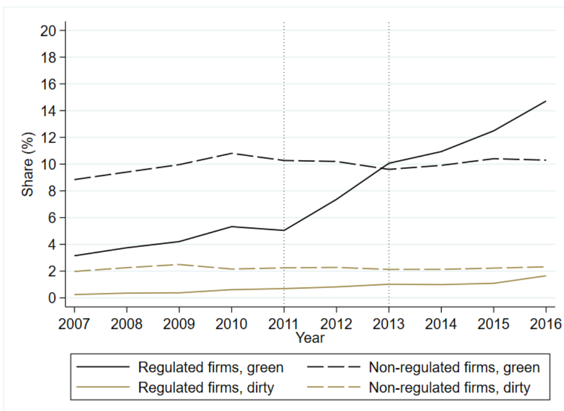



Figure 1.2: Share of green and dirty patents 2007-2016, weighted




per patent does not noticeably vary across firm types and over time. The shares of dirty patents have been flat both for regulated and non-regulated firms.[13] The figures suggest that following the implementation of the ETS pilots, regulated firms have shifted towards "greener" innovation. Such a shift is not apparent for non-regulated firms.

### 1.3.3 Firm-level production data

The firm-level production data, Annual Survey of Manufacturing Enterprises, are collected on an annual basis by China's National Bureau of Statistics (NBS). All industrial firms above a given size of annual sales are surveyed. This includes

[13]The shares of green and dirty patents are calculated as the weighted patent counts in each respective category divided by the sum of all the weighted patent counts in one year.

all state-owned firms, as well as non-state owned firms with sales exceeding 5 million Yuan.[14] In 2011, the designated size increased from 5 million to 20 million Yuan for all surveyed firms.[15]

The manufacturing data used in this study spans 2007 until 2013. I do not use the 2010 data due to data quality concerns,[16] and no data is available after 2013. The dataset includes basic information such as firm name, location and the number of employees. Almost all of the entries in a balance sheet and an income statement are included in most of the census years, such as sales revenue, total assets, output and costs.

Table 1.2 presents the summary statistics. In the table, the "pilot regions" refer to the provinces or municipalities that implemented the pilot ETS, as introduced in Section 1.2. The "non-pilot regions" include all other regions in mainland China. Table 1.2 shows that, compared to those in non-pilot regions (column 2), firms in pilot regions (column 3) are slightly larger: on average, they have higher employment, greater sales, produce more output and hold more assets and capital. In pilot regions (columns 4-5), employment in regulated firms is on average six times the employment in non-regulated firms; sales, output and assets are more than ten times larger.

Table 1.3 presents the summary statistics for patent applications. On average, firms in pilot regions file more patents, and especially more green patents, both before and after 2013 (columns 3-6). It is noteworthy that from year to year, for regulated firms, the average number of green patent applications more than quadrupled from 1.37 to 5.76 (weighted counts, columns 9 and 10), while the increase for non-regulated firms in the pilot regions is rather modest (columns 7 and 8). The number of dirty patents has also tripled, both for regulated and non-regulated firms.

The dataset presented above is constructed by first of all merging the two sources of the data, regulatory status and patent data, which gives a sample with 370,267 non-regulated firms and 1,495 regulated firms. Then I exclude all the firms in the service sector–that is, all the universities, government agencies, and restaurants and hotels, because these entities are not likely incentivized to

[14]This is equivalent to about 740,000 US dollars.

[15]For further characteristics and caveats of this dataset, see Brandt et al. (2014).

[16]Concerns have been raised about the quality of this data after 2008. For instance, Chen et al. (2019) find that investments, net exports and value-added of sectors are largely discrepant between local and national statistics. In another study, Chen (2018) discusses several issues to which the user should pay attention when using these data and suggests a method for validating the authenticity of the main variables in the survey data. Using their method, I find that the 2010 is likely problematic, while the data quality is good in other years. For this reason, I do not use the 2010 data. Cai and Liu (2009) and Feenstra et al. (2014) additionally point out potential misreporting due to administrative errors. To address this, I follow their suggested approach to clean the data and drop firms with fewer than 8 employees.

Table 1.2: Summary Statistics 2007-2012

| | (1)<br>All | (2)<br>Non-pilot regions | (3)<br>Pilot regions | (4)<br>Pilot regions<br>Non-regulated firms | (5)<br>Pilot regions<br>Regulated firms |
|---|---|---|---|---|---|
| Employment | 638.35 | 635.33 | 647.22 | 483.14 | 2,994.96 |
| | (2,976.84) | (2,935.17) | (3,096.12) | (1,768.13) | (9,805.52) |
| Total assets | 660.43 | 619.14 | 781.80 | 407.63 | 6,135.72 |
| | (6,155.51) | (4,165.89) | (9,911.92) | (4,232.05) | (34,892.08) |
| Current assets | 300.41 | 286.93 | 340.04 | 199.61 | 2,349.43 |
| | (1,998.00) | (1,722.86) | (2,645.71) | (1,149.76) | (9,162.34) |
| Sales | 630.51 | 613.92 | 679.27 | 387.03 | 4,861.03 |
| | (4,600.52) | (4,251.69) | (5,499.13) | (3,386.16) | (16,740.77) |
| Cost of sales | 526.63 | 511.50 | 571.12 | 321.66 | 4,140.62 |
| | (3,929.21) | (3,603.69) | (4,758.75) | (3,072.66) | (14,071.74) |
| Output | 607.77 | 589.86 | 660.41 | 382.03 | 4,643.73 |
| | (4,149.04) | (3,728.56) | (5,191.30) | (3,244.61) | (15,653.41) |
| Capital | 134.21 | 114.98 | 190.76 | 114.17 | 1,286.73 |
| | (3,231.56) | (2,780.51) | (4,290.85) | (3,723.94) | (9,064.54) |
| Observations | 191143 | 142629 | 48514 | 45345 | 3169 |

**Note:** This table presents means and standard errors for each variable. Standard errors are in parentheses. All variables except for employment are in million Yuan. All the statistics are based on data between 2007-2012, with the data in 2010 excluded because it is not validated, as discussed in this section.

Table 1.3: Summary statistics: number of patents, full sample

| | (1) 2007-2012 | (2) 2013-2016 | (3) 2007-2012 | (4) 2013-2016 | (5) 2007-2012 | (6) 2013-2016 | (7) 2007-2012 | (8) 2013-2016 | (9) 2007-2012 | (10) 2013-2016 |
|---|---|---|---|---|---|---|---|---|---|---|
| All patents | 2.13 | 6.78 | 1.46 | 5.26 | 4.02 | 11.02 | 2.13 | 6.52 | 31.53 | 62.71 |
| | (38.53) | (87.57) | (7.43) | (21.43) | (74.19) | (166.63) | (19.15) | (65.27) | (281.91) | (543.24) |
| Green patents | 0.20 | 0.86 | 0.15 | 0.58 | 0.35 | 1.64 | 0.24 | 0.77 | 1.88 | 11.63 |
| | (3.68) | (29.91) | (1.56) | (6.29) | (6.69) | (57.28) | (4.19) | (10.44) | (20.94) | (199.11) |
| Dirty patents | 0.04 | 0.14 | 0.04 | 0.13 | 0.04 | 0.16 | 0.03 | 0.10 | 0.18 | 0.86 |
| | (0.46) | (1.78) | (0.42) | (0.97) | (0.55) | (3.07) | (0.39) | (0.70) | (1.56) | (10.58) |
| All patents, weighted | 1.92 | 5.83 | 1.36 | 4.81 | 3.48 | 8.67 | 1.85 | 5.55 | 27.23 | 44.62 |
| | (35.28) | (53.13) | (6.90) | (16.49) | (67.92) | (99.66) | (12.71) | (48.15) | (262.29) | (309.94) |
| Green patents, weighted | 0.17 | 0.62 | 0.14 | 0.48 | 0.27 | 1.02 | 0.20 | 0.61 | 1.37 | 5.76 |
| | (2.21) | (12.02) | (1.38) | (3.44) | (3.64) | (22.68) | (2.41) | (6.22) | (10.95) | (77.21) |
| Dirty patents, weighted | 0.04 | 0.12 | 0.03 | 0.12 | 0.04 | 0.12 | 0.03 | 0.08 | 0.15 | 0.55 |
| | (0.42) | (1.13) | (0.41) | (0.89) | (0.46) | (1.62) | (0.37) | (0.59) | (1.15) | (5.33) |
| Observations | 202086 | 114237 | 149126 | 84105 | 52960 | 30132 | 49554 | 27720 | 3406 | 2412 |
| Sample | All | All | Non-pilot regions | Non-pilot regions | Pilot regions | Pilot regions | Non-Rregulated firms | Non-Rregulated firms | Regulated firms | Regulated firms |

**Note:** This table presents means and standard errors for each variable on the full sample. Standard errors are in parentheses.

innovate on their own, but rather adopt abatement technologies to reduce the marginal cost of abatement. Next, I merge the data with the firm-level production data[17], the Annual Survey of Manufacturing Enterprises, which further reduces the sample size and gives a sample with 61,358 non-regulated firms and 1,081 regulated firms. Then I drop the firms that do not contain information on industry classification, sales and labor, which leads to 56,335 non-regulated firms and 784 firms respectively. This is less than the actual number of regulated firms (2,621) for the following two reasons.

First of all, there are 1,495 regulated firms that filed at least one patent between 2007 and 2016 (regardless of being 'green' innovation or not), while there are 1,126 that never filed a patent in this period, which are excluded from the sample. These excluded firms filed no patents either before or after the implementation and hence do not respond to the policy by innovating more. Secondly, in ASME, only manufacturing firms with annual sales above a certain threshold are surveyed, as introduced in Section 1.3.3. Therefore, regulated firms that do not reach this threshold, or reach this threshold but are not manufacturing firms, such as firms in the transportation sector, would not be surveyed. In other words, the further reduction of the number of regulated firms when merging three sources of data is because those firms were not surveyed, because they did not achieve high enough annual sales.

## 1.4 Empirical Strategy

Section 1.3 documented that regulated firms and non-regulated firms are different in observable characteristics. This section introduces the empirical framework, which relies on a count data model with a matched dataset. The motivation for matching is also discussed in this section.

### 1.4.1 Empirical Model

The empirical identification of the effect of the pilot ETS on green innovation by regulated firms is based on the variations in regulatory status across firms, as well as differences in the regulation of the pilot ETS across pilot regions. I adopt a differences-in-differences design to estimate the effect of the ETS pilots on firm-level innovation.

A main challenge of empirically identifying the causal effect of the pilot ETS on innovation is the non-random assignment of the treatment due to the regula-

[17]See Appendix 1.B for the steps of the data construction.

tion threshold introduced in Section 1.2.2. If I know carbon emissions intensity (emissions per unit of output) of the population of firms, I could compare the green patenting of regulated firms with that of the non-regulated firms that have exactly the same emission intensity as the regulated firms before and after the implementation of the regulation. An alternative would be to include a vector of control variables that correlate with firms' emissions and therefore the treatment status, if I had data on full sets of control variables in both pre- and post-treatment periods – in other words, all the data on ASME between 2007 and 2016. Then I could obtain an unbiased estimation on the effect of the regulation on the number of patent applications. However, due to the lack of data availability after 2013, as discussed in Section 1.3.3, this is not feasible. To address the issue, I first pre-process the dataset using matching methods. Then I estimate the regression equations on the matched dataset. Matching is favourable as it requires only the data in the pre-treatment period and hence the matched data have better balance between the treatment group and the control group. The related matching methods are described in detail in Section 1.4.2 and Appendix 1.C.

Because the dependent variable of interest, the number of green patents, is a numerical count, I use a count data model to estimate the effect of pilot ETS. Specifically, I adopt a zero-inflated Poisson (ZIP) regression model, as proposed by Lambert (1992).[18] This model allows me to deal with the zero patent applications observed for a substantial number of firms, and allows for greater flexibility in the distributions of zeros and strictly positive applications. The firms that file a positive number of green patents likely have a different data generating process of patent counts than those with zero counts. Hence it is intuitive to use two-part models to allow for flexible specification of the distributions of zeros and positives, as proposed by Mullahy (1986).[19] Such a two-step process allows for an analysis of multiple margins of decision-making: an extensive margin decision of whether green patenting is worthwhile to the firm, followed by an intensive margin decision of how many green patents to file.

The basic idea behind ZIP is as follows. The firms are categorized as two types: firms that invest in R&D to innovate green technology (henceforth innovators), and firms that do not make any investments in green technology (henceforth non-innovators). The probabilities of being an innovator and a non-innovator

[18]This model is commonly applied in patenting studies. To give a few examples, Hu and Jefferson (2009) use ZIP regression to analyse the factors that led to a patenting surge in China; Noailly and Smeets (2015) study the driving forces of innovation on renewable and fossil-fuel energy in the electricity generation sector in Europe.

[19]This is important for the following reasons. First, there is a significant proportion of zeros in the number of filed patent applications. Second, there are very large counts of filed patents that contribute substantially to overdispersion. See also Figure 1.D.1 in Appendix 1.D.

are $1 - \pi$ and $\pi$ respectively. In turn, for an innovating firm $i$, the distribution of patent counts in year $t$ is Poisson with mean $\lambda_{it}$. This then gives the baseline regression specification:

$$f(y_{it}) = e^{-\lambda_{it}} \lambda_{it}^{y_{it}} / y_{it}!, \tag{1.1}$$

where

$$\lambda_{it} = [y_{it}] = exp(\beta_1 regulated_i \times post_t + \beta_2 regulated_i + \gamma_{i,o} + \delta_{i,size} + \alpha_t + \eta_l). \tag{1.2}$$

In the above equation, $y_{it}$ denotes the count of green patents that innovator firm $i$ filed in year $t$. The primary variable of interest, the interaction term $regulated_i \times post_t$, is an indicator equal to one if, in year $t$, firm $i$ is regulated in the carbon market. That is, the treatment indicator, $regulated_i \times post_t$, turns on for firms included in the pilot trading scheme; for control group firms, this interaction term does not change over time and equals zero. I control for year fixed effects ($\alpha_t$), which account for the time-variant changes that affect all firms similarly. I include the region dummy $\eta_l$ to account for time-invariant green patenting difference across regions. This dummy controls for region-level institutional differences, such as province-level patent subsidy programs.[20] In addition, the specification also includes a vector of ownership dummies $\gamma_{i,o}$ to account for differences in patenting behavior between state-owned and non-state-owned firms,[21] and size dummies $\delta_{i,size}$ to take into consideration different patenting ability for firms with different size.[22]

The ZIP model therefore specifies

$$Pr(greenpat_{it} = y_{it}) = \begin{cases} \pi_{it} + (1 - \pi_{it}) f(0; \lambda_{it}) & \text{if } y_{it} = 0, \\ (1 - \pi_{it}) f(y_{it}; \lambda_{it}) & \text{if } y_{it} = 1, 2, 3, 4, ... \end{cases} \tag{1.3}$$

Here, $greenpat_{it}$ is the number of green patents filed by firm $i$ in year $t$. Note that the large number of zero counts of patents may occur for two different reasons. The first reason is that firms do not find it profitable to innovate regardless of the regulation or fail to innovate and therefore file no patents (non-innovator). The second reason for zeros is that firms do innovate but do not use patents as

[20] However, the effects of the pilot ETS on green patenting are not biased by these regional policy initiatives, because 29 out of 31 provinces and municipalities in mainland China had a patent subsidy program in place by the end of 2007 (Li, 2012).

[21] The results by Hu and Jefferson (2009) indicate that non-state-owned firms may be more keen to seek patent protection.

[22] I categorize firms as large, medium, small and miniature firms based on sales and labor according to the firm size measure by the National Bureau of Statistics. For details see Appendix 1.C.

a way of protecting their intellectual property, or are incapable of filing a patent (potential innovator). These two different sources of zeros in patenting data are characterized by $\pi_{it}$ and $(1-\pi_{it})f(0;\lambda_{it})$ respectively. As noted above, $\pi_{it}$ is the probability of being a non-innovator for firm $i$ in year $t$; $(1-\pi_{it})f(0;\lambda_{it})$ is the probability of being a potential innovator with zero patents filed. At the extensive margin, the firm decides whether to be an actual innovator with positive applications, which is captured by the following logit regression, as in Lambert (1992),

$$logit(\pi_{it}) = log(\pi_{it}/(1-\pi_{it})) = X'_{it}\beta. \tag{1.4}$$

Hence the likelihood of not being an innovator is estimated via logistic regression

$$\pi_{it} = \frac{e^{\mu_{it}}}{1+e^{\mu_{it}}}, \tag{1.5}$$

where $\mu_{it} = log(\lambda_{it})$ in equation (1.2) influences the extensive margin of patenting–that is, whether the firm files patents. In summary, in the first regression, a logit model estimates the probability of filing green patents with an outcome of zero or one (extensive margin). In the second regression, a count data model estimates the patent count using a Poisson model for firms with at least one green patent filed (intensive margin).

A large variation in the carbon prices across different pilot regions in China provides a chance for me to look directly at the continuous treatment effect of the pilot ETS on firms' green innovation. Fell and Maniloff (2018) and Calel and Dechezleprêtre (2016) study the effect of the U.S. Regional Greenhouse Gas Initiative (RGGI) and the effect of the EU ETS. In these two studies, they estimate the discrete treatment effects instead of the continuous effects that would be captured by the carbon prices, which is due to little variation in the carbon prices in the RGGI states and EU ETS countries during the period studied. Complementary to their studies, I study the effect of carbon pricing on the number of green patents using the following regression specification

$$y_{it} = exp(\beta_3 price_{t+g,l} \times regulated_i \times post_t + \beta_4 regulated_i + \gamma_{i,o} + \delta_{i,size} + \alpha_t + \eta_l) + \epsilon_{it}. \tag{1.6}$$

Here $price_{t,l}$ is the logarithm of the yearly average carbon price in region $l$ in year $t$. Carbon prices are strictly positive for regulated firms after the implementation of the pilot ETS, and are zero for all non-regulated firms and regulated firms before the implementation of the pilot ETS. The coefficient $\beta_3$ is the parameter of interest that captures the average change of green patents as carbon price increases by one percent. Assuming that on average current carbon prices are the best predictor of future carbon prices, I use the current carbon prices in the baseline regression.[23]

One complexity arises from the possible firm heterogeneity that influences firms' patenting ability, which is not accounted for by matching. There is a rich literature on the econometric techniques to account for firm-level fixed effects in Poisson models, primarily Blundell et al. (1995), Blundell et al. (1999), Blundell et al. (2002) and Hausman et al. (1984). The first three papers by Blundell et al. propose that time-invariant firm heterogeneity could be accounted for using pre-sample mean of patent count, and a dummy equal to one if the firm innovated in the pre-sample period.[24] However this would require a long pre-sample history of the dependent variable to proxy the firm fixed effects, which is not feasible in this study due to lack of data in the pre-sample period. Hausman et al. (1984) developed a conditional maximum likelihood estimator which can be applied to count data of a panel nature to capture the persistent firm fixed effects. They suggest an estimator conditioning on the total sum of outcomes over the observed years to proxy the fixed effects.

The proxied firm-fixed effects in Hausman et al. (1984) require strict exogeneity–that is, that the firm-specific effect is uncorrelated with the explanatory variables. This would be violated if firms have strong innovation ability in the pre-treatment period, and hence are able to reduce the carbon emissions below the regulatory threshold. The firm-specific effect might therefore be negatively correlated with the treatment dummy. Therefore, the proxies of firm fixed effects using data in either pre-sample or in-sample period are infeasible. An alternative is to assume that the zero counts and non-zero counts have the same data-generating process without explicitly considering the probability of a regulated firm switching from a non-innovator to an innovator. Under such an assumption, I can then estimate a fixed effects Poisson model. I discuss the potential issue with this model in Section 1.5.4.4.

The remaining issue relates to the estimation of standard errors. Across specifications, I cluster the standard errors at the four-digit sector level, because the regulations differ in different sectors. For instance, different sectors might be

[23]The additional results on the estimations with different leads of carbon prices ranging from 1 to 3 are presented in Appendix 1.D.1 to take into consideration that firms decide whether to innovate based on their expectation of carbon prices in the future. Here I assume that firms are informed and are able to fully anticipate the carbon price level in the future.

[24]Building on Blundell et al., Aghion et al. (2016) derive a similar approach using the post-sample mean and dummy to capture such firm heterogeneity.

subject to different coverage threshold and rules of allowances allocation, as introduced in Section 1.2.[25]

### 1.4.2 Matching

One complexity of this study arises from the lack of data on the Annual Survey of Manufacturing Enterprises (ASME) in the post-treatment period. Matching could address this by only using the data in the pre-treatment period, so that treatment and control groups are better balanced on a vector of control variables. To control for the confounding influence of pre-treatment control variables, I match regulated and non-regulated firms in the same 2-digit sector, region, as well as on labor and sales revenue, and whether filing at least one patent in the pre-treatment period, number of green patent applications and number of all patent applications. That is, I first of all implement exact matching for firms on a 2-digit sector and province or municipality and a dummy equal to one if a firm filed at least one patent before 2013. The firms in the non-pilot region are thus dropped from the baseline sample. I then match firms on labor, sales revenue and number of patents with measures of tolerable distance between regulated and non-regulated firms, which I discuss below. The first two are selected to capture firms' size and profitability.[26] The last two variables control for firms' pre-treatment innovation ability.

The key goal of matching is to prune observations from the data so that the remaining data have better balance between the treated and control groups, meaning that the empirical distributions of the covariates in the groups are more similar (Iacus et al., 2012).[27] I use coarsened exact matching (CEM), as proposed by Iacus et al. (2012), in combination with genetic matching (GM), proposed by Diamond and Sekhon (2013). The intuition and the technical details of matching are presented in Appendix 1.C.

Figure 1.3 shows the quantile-quantile plots for the matched variables, average employment, average sales, and the numbers of all patents and green patents between 2007 and 2012. The points on the plots fall reasonably on the 45 degree straight line. Of course, matching only on the selective subset of the variables

[25]See Appendix 1.A for a detailed review on the difference of the regulation in different pilot regions. Ideally, I would adjust standard errors for clustering at region level to allow for serial correlation within a region across years. However, with six clustering units, standard errors would be underestimated, which leads to an inference problem. (Bertrand et al., 2004)

[26]The other reason for choosing these variables is that the information on these two variables is always reported across years.

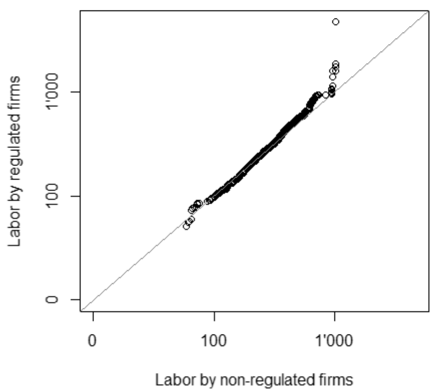
[27]Due to the large size of the control group compared to the size of the treatment group, I could identify a sub-group of non-regulated firms which are comparable with regulated firms with matching. For a useful review and practical guidance on matching methods, see Stuart (2010).

might not capture all these dimensions. I thus show in Figure 1.4 the quantile-quantile plots for the matched sample on variables that are not used for matching, including current assets, output, operating cost and total assets. As Figure 1.4 shows, the empirical distribution of the non-matching variables of the regulated and non-regulated firms are very similar.

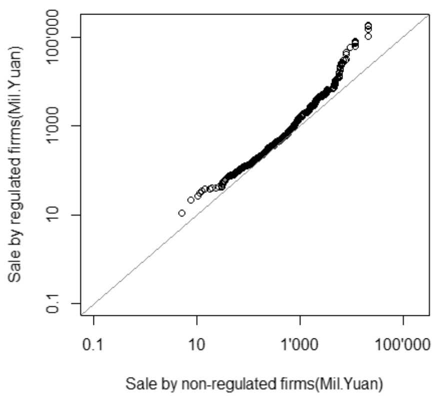
Ideally, I would have a group of unregulated firms that is exactly the same as the group of regulated firms in every aspect, especially those influencing their green innovation ability, except for the regulatory status. A related concern is that, even though the empirical distributions of the matched regulated and non-regulated firms are very similar in the variables shown in Figures 1.3 and 1.4, they might have very distinct emissions intensity of production, and therefore might not be comparable with each other. However, due to the general lack of availability of firm-level carbon emissions data in the pilot regions, it is not feasible to directly compare firms with the same emission intensity. Imagine that the matched regulated firms have far higher emissions intensity than the matched non-regulated firms. This case could be due to, for instance, the regulated firms using more carbon-intensive energy or dirtier technology for their output. However, as Figure 1.5 shows, the number of green patents of the regulated and non-regulated firms before 2013 is very similar. This provides some confidence that the regulated firms' emissions intensity is not substantially higher than the non-regulated firms' emissions intensity.[28] Figure 1.5 is also suggestive of parallel pre-regulation trends. Table 1.4 presents summary statistics for the number of patents on the matched (columns 1–4) and non-matched firms (columns 5–8) in the pilot regions before and after the implementation of the pilot ETS regulation. Comparing columns 7 and 3, the regulated firms that are relatively more innovative are not matched with any of the unregulated firms.

[28] Additionally, as Figure 1.D.3 in Appendix 1.D shows, the means of the number of green patents on the matched sample are similar in the pilot regions. This provides reassuring evidence that the production techniques should not be largely different and therefore the emissions intensity of matched regulated and non-regulated firms should be similar.

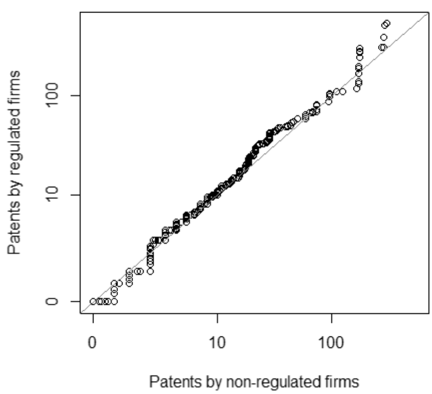
Figure 1.3: Quantile-quantile plots on matched sample, matching variables



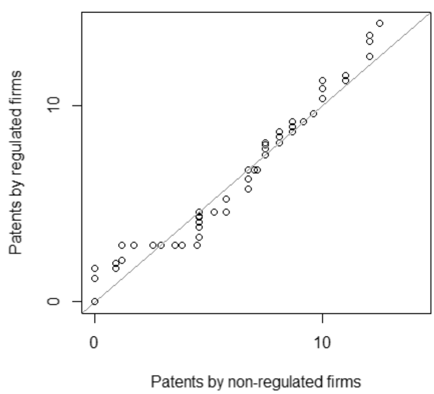

(a) Employment




(b) Sales



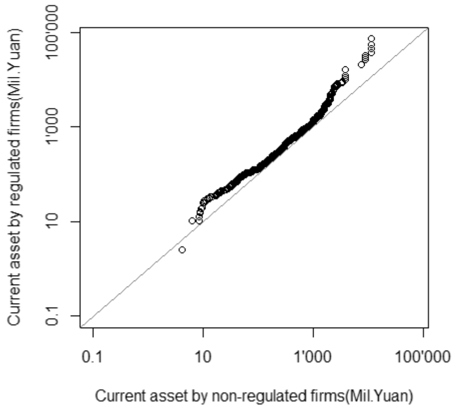

(c) Number of all patents



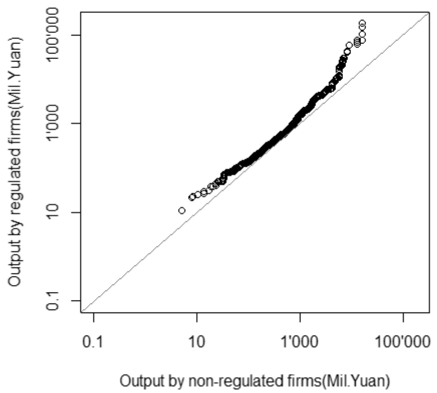

(d) Number of green patents

Figure 1.4: Quantile-quantile plots on matched sample, non-matching variables

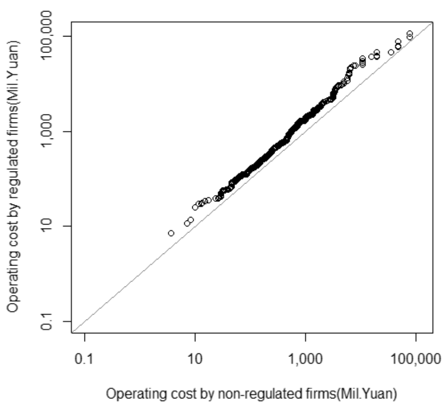
(a) Current asset




(b) Output




(c) Operating cost




(d) Total assets

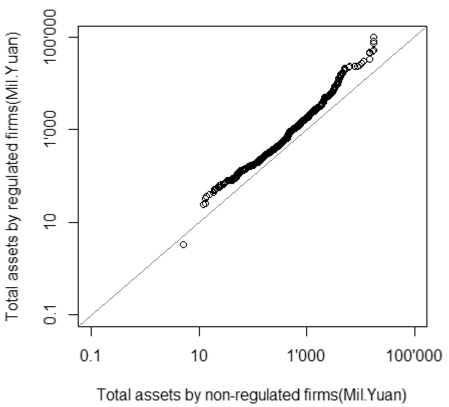
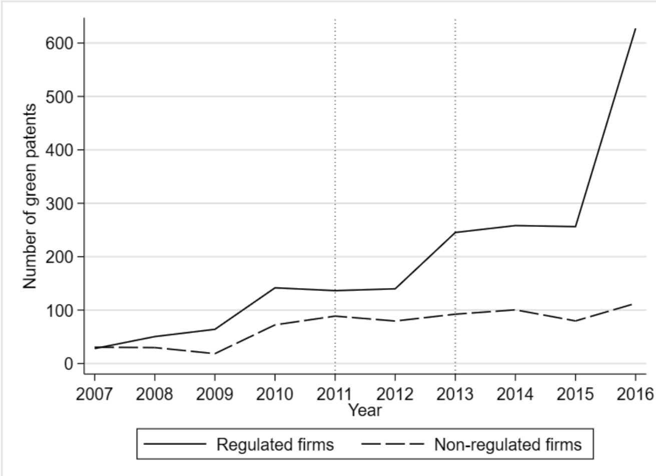


Figure 1.5: Number of green patents 2007-2016, matched sample




## 1.5 Results

### 1.5.1 The Impact of the Pilot ETS: Main Results

The first column in Table 1.5 present the Poisson estimations while the rest of the columns are estimations from the zero-inflated Poisson (ZIP) regression. Columns 2–5 compare results from estimations of equation (1.2) with ownership, pilot region, and firm size dummies added. Column 6 presents results from estimations of equation (1.6).[29] Column 7 shows the estimations of equation (1.2) using the weighted approved green patent counts as an outcome variable. All models include a full set of year dummies (not reported). ZIP is more flexible than the Poisson regression, because it relaxes the assumption that data are equi-dispersed–that is, the variance of count data conditional on a vector of regressors $x$ equals the conditional mean. Meanwhile, ZIP enables me to model zero green patenting by innovator and non-innovator differently, which better captures the data generating process. Therefore, I use the ZIP regression model as my baseline specification.

For columns 2–7, the top part of the table presents the estimations from the Poisson regression for the number of green patents, whereas the bottom part of the table presents the estimations of the logit model in the inflation equation discussed in Section 1.4.1. The coefficient estimations in the inflation equation assess

[29]The results using unmatched data are shown in appendix 1.D.4. Generally speaking, the signs of the estimations are the same as the estimations from the matched sample, but with higher magnitude.

Table 1.4: Summary statistics: number of patents, matched and non-matched samples

| | (1) Pre | (2) Post | (3) Pre | (4) Post | (5) Pre | (6) Post | (7) Pre | (8) Post |
|---|---|---|---|---|---|---|---|---|
| All patents | 3.15 | 7.26 | 4.24 | 14.00 | 2.07 | 6.80 | 187.37 | 304.88 |
| | (9.27) | (15.43) | (15.89) | (52.16) | (19.13) | (68.53) | (708.34) | (1,293.47) |
| Green patents | 0.19 | 0.59 | 0.23 | 1.05 | 0.24 | 0.82 | 11.28 | 64.08 |
| | (0.75) | (1.99) | (0.96) | (7.22) | (4.21) | (10.96) | (53.14) | (482.13) |
| Dirty patents | 0.05 | 0.10 | 0.06 | 0.28 | 0.03 | 0.10 | 0.91 | 3.76 |
| | (0.29) | (0.58) | (0.43) | (3.75) | (0.39) | (0.72) | (3.81) | (24.31) |
| All patents, weighted | 2.77 | 6.05 | 3.44 | 11.18 | 1.80 | 5.78 | 162.99 | 211.11 |
| | (8.18) | (12.65) | (11.19) | (39.41) | (12.67) | (50.53) | (661.71) | (728.72) |
| Green patents, weighted | 0.17 | 0.46 | 0.19 | 0.90 | 0.20 | 0.65 | 8.07 | 29.91 |
| | (0.68) | (1.35) | (0.81) | (7.03) | (2.42) | (6.53) | (27.29) | (185.88) |
| Dirty patents, weighted | 0.04 | 0.08 | 0.05 | 0.25 | 0.03 | 0.09 | 0.73 | 2.05 |
| | (0.28) | (0.55) | (0.42) | (3.74) | (0.37) | (0.61) | (2.72) | (10.04) |
| Observations | 1864 | 1076 | 3005 | 1897 | 49249 | 25077 | 510 | 406 |
| Sample | Non-regulated firms | Non-regulated firms | Regulated firms | Regulated firms | Non-regulated firms | Non-regulated firms | Regulated firms | Regulated firms |
| Matched | Yes | Yes | Yes | Yes | No | No | No | No |

**Note:** This table presents means and standard errors for each variable of firms in the pilot regions. Standard errors are in parentheses.

the likelihood of inflated zeros–that is, the likelihood of being a non-innovator. Therefore, a negative (positive) coefficient is interpreted as a positive (negative) effect on the likelihood of being an innovator. The estimates in columns 2–5 compare the effects of adding pilot region dummies, the ownership dummies, and the firm size dummies. The estimates reveal significant effects for green patenting, while the size of the regulation effect differs. Also, the Akaike information criteria (AIC), shown as AIC divided by the number of observations at the bottom of the table, is decreased by adding the three sets of dummies. This reveals the importance of including these dummies in the regressions.[30] Therefore, I add the ownership dummies, pilot region dummies and firm size dummies in all the following regressions (not reported).

The estimations in column 5 suggest that, compared to the non-regulated firms, the regulated firms respond to ETS by increasing the number of green patents. The average marginal effect of ETS is 0.16–that is, the number of green patents for regulated firms increased on average by 0.16 (standard error = 0.08, $p = 0.051$).[31] This is equivalent to 11.68 percent and 2.78 percent of the average number of green patents in the pre-treatment period (2007-2012) and post-treatment period (2013-2016), respectively. For large firms, the average marginal effect is 0.20 (standard error = 0.09, $p = 0.03$). The magnitude of the effects decreases as the firm size becomes smaller. For small and medium-size firms, the average marginal effects are 0.15 (standard error = 0.09, $p = 0.08$) and 0.06 (standard error = 0.03, $p = 0.06$) respectively. In the extensive margin, the effects for the regulated firms are all positive, suggesting that the pilot ETS decreases the probability of being an innovator, at least for some regulated firms.[32] However, no significant effects of the pilot ETS in the extensive margin are observed in the data. Therefore, firms respond to the pilot ETS significantly only in the intensive margin.

The estimation in column 6 yields the elasticity of carbon prices on the number of green patents. I assume that on average current carbon prices are the best predictor of future carbon prices. Qualitatively, a higher carbon price leads to more green patents for innovators (at the intensive margin) on average. The elasticity of patents with respect to the carbon price is 0.23. This means that a 10 percent increase in the carbon price will increase green patents produced by 2.3

[30] A joint hypothesis test also rejects the null hypothesis that the coefficients on the pilot region dummies, the ownership dummies, and the firm size dummies are zero, with a p-value equal to zero.

[31] Because the magnitudes for the estimations using ZIP regression are not directly interpretable, I use the Stata built-in command `margins` to get the marginal effect of the regulation on green innovation.

[32] Recall that the coefficient in the logit regression captures the probability of inflated zeros, and a positive coefficient is interpreted as a negative effect on the likelihood of being an innovator.

percent. There could also be forward-looking effects, since innovation requires a stream of investment for a period and will potentially generate returns in the future. Assuming that the firms can perfectly anticipate the future carbon price, I use the carbon price with leads up to three years to take into account the firms' expectation on carbon prices. The results are shown in Appendix 1.D.1. The one-year lead effects of the carbon prices are significant with a magnitude similar to the estimations based on the current price. No significant effects with two- and three-year leads can be observed in the data. This could be because the firms are able to anticipate the carbon price one year ahead and respond to it accordingly, but not beyond that.

It is essential to mention one characteristic of patent application data from SIPO: SIPO does not record citations, which is typically used as a measure of patent quality in the literature. This is a common issue in studies of the development of innovation in China using data from SIPO. Thus, granting rate is usually used as an alternative measure for patent quality (Dang and Motohashi, 2015). However, patent granting takes on average 3.87 years after filing a patent with SIPO. Therefore, using the patent granting rate of firms to account for patent quality would not be sufficiently informative in this study, as the policy was implemented in 2013.[33] Still, I report the estimation of the effects using the number of granted patent counts as an outcome variable in column 7 to compare whether the policy has similar effects on the number of approved patents and the number of filed patents.[34] There is a measurement error in this outcome variable in that many of the patents might not yet have been granted at the time of accessing the data. Though the positive sign remains, the effect is underestimated.[35]

The quantitative result on the carbon price elasticity of 0.23 should be interpreted cautiously for two reasons. First, this is an average effect of an increase in carbon prices on the number of green patents. However, if the carbon price is not above a certain level, as in Tianjin (TJ), the pilot regulation would not be effective in terms of inducing green innovation despite the increase in carbon

[33]One might be concerned that the estimation also captures the anticipation effect, as the policy was announced two years before 2013. However, this is not likely because the list of regulated firms and crucial rules–that is, the coverage threshold and the allowances allocation, were not released in 2011. Therefore, firms could not predict their regulatory status precisely.

[34]The trends in the means of the number of granted green patents for regulated and non-regulated firms are presented in Figure 1.D.2 in Appendix 1.D, which suggests the parallel pre-regulation trends. The approval year is usually not the same as the filing year. The data is compiled based on years that patents are filed.

[35]In another regression, I use the patent grant rate (the number of granted green patents divided by the number of filed green patents) as an outcome variable and estimate the ETS effect on this grant rate using an OLS regression with and without firm fixed effects included. I restrict my sample to a subsample of firms that have at least one green patent filed in each year. The estimations are 0.03 (without firm fixed effects) and 0.05 (with firm fixed effects) respectively. However they are not precisely estimated. The results are not reported.

Table 1.5: Emissions trading scheme and innovation

| | (1) Poisson | (2) ZIP | (3) ZIP | (4) ZIP | (5) ZIP | (6) ZIP | (7) ZIP |
|---|---|---|---|---|---|---|---|
| main | | | | | | | |
| regulated*post | 0.49** | 0.67** | 0.63** | 0.65** | 0.75** | | 0.67 |
| | (0.21) | (0.31) | (0.32) | (0.31) | (0.32) | | (0.50) |
| regulated | 0.20 | 0.10 | 0.21 | 0.34 | 0.27 | 0.23 | 0.24 |
| | (0.13) | (0.26) | (0.23) | (0.25) | (0.24) | (0.23) | (0.28) |
| Logarithm carbon price | | | | | | 0.23** | |
| | | | | | | (0.10) | |
| inflate | | | | | | | |
| regulated*post | | 0.21 | 0.28 | 0.36 | 0.49 | | 0.58 |
| | | (0.25) | (0.27) | (0.29) | (0.30) | | (0.48) |
| regulated | | 0.01 | 0.09 | 0.13 | 0.13 | 0.07 | 0.21 |
| | | (0.18) | (0.18) | (0.20) | (0.20) | (0.19) | (0.25) |
| Logarithm carbon price | | | | | | 0.16* | |
| | | | | | | (0.09) | |
| Observations | 7829 | 7842 | 7842 | 7829 | 7829 | 7829 | 7829 |
| Mean dependent var. | 0.39 | 0.40 | 0.40 | 0.39 | 0.39 | 0.39 | 0.14 |
| Sd. of dependent var. | 3.56 | 3.56 | 3.56 | 3.56 | 3.56 | 3.56 | 0.81 |
| Pilot dummy | Yes | No | Yes | Yes | Yes | Yes | Yes |
| Ownership dummy | Yes | No | No | Yes | Yes | Yes | Yes |
| Size dummy | Yes | No | No | No | Yes | Yes | Yes |
| R-squared | 0.17 | | | | | | |
| log likelihood | -8240.01 | -6964.46 | -6784.96 | -6537.58 | -6431.86 | -6424.40 | -2896.48 |
| AIC/N | 2.11 | 1.78 | 1.74 | 1.68 | 1.66 | 1.65 | 0.75 |

**Note:** This table reports OLS and maximum likelihood estimators using a count data model for the sample processed using matching. Column 1 shows the results from the Poisson regression; columns 2–7 show the results from the zero-inflated Poisson regression. Columns 2–5 show the results for estimating the overall effect of the pilot ETS on innovation. Column 6 shows the estimations on the carbon price elasticity on number of green patents. Column 7 shows the estimations using the number of approved green patents as an outcome variable. Standard errors are clustered at 4-digit sector level, with 268 clusters. Specifications in all the columns include year fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

price.[36] Second, the carbon price in Beijing is the highest among all the pilot regions. Hence, for regulated firms in Beijing, a one percent increase in the carbon price will influence the firms more significantly than those located in Tianjin. So, the 0.23 estimated elasticity of carbon prices on the number of green patents implies the average value across all six pilot regions, and applies exclusively to the regulation within the period studied. Therefore, I next present the estimation of the pilot heterogeneity effects using sub-samples of each pilot region.

### 1.5.2 The Impact of the Pilot ETS: Heterogeneity and the Direction of Technical Change

**Pilot heterogeneity.** As described in Section 1.2, the ETS regulation differs across pilot regions: each of the local Development and Reform Commissions (DRC) decides on its own allowances allocation, the coverage threshold, and which sectors are part of the pilot system. For this reason, effects of the pilots are likely heterogeneous across regions. To assess whether this is the case, I estimate the average treatment effect (ATE) with the baseline regression in equation (1.2) for each subsample corresponding to each of the six pilot regions. The Tianjin and Guangdong pilots, however, have relatively few firms, which limits statistical power. For this reason, I additionally estimate specification 1.7 below, using the full sample. This specification adds a vector of pilot region dummies interacted with the treatment interaction term to 1.2, which capture any heterogeneity in the effect of the pilot on firm innovation across regions.

$$
\begin{aligned}
y_{it} = exp(\sum_{l=1}^{6} \beta_{1l} \times pilot_l \times regulated_i \times post_t + \sum_{l=1}^{6} \beta_{2l} \times pilot_l \times regulated_i + \\
\sum_{l=1}^{6} \beta_{3l} \times pilot_l \times post_t + \gamma_{i,o} + \delta_{i,size} + \alpha_t + \eta_l) + \epsilon_{it}.
\end{aligned} \tag{1.7}
$$

In the above specification, $pilot_l$ is the pilot region dummy that equals 1 if a firm $i$ is located in pilot region $l$.[37] In this regression, $\beta_{1l}$ is the parameter of interest, representing the regulation effects in region $l$ after the pilot ETS is implemented; $\beta_{2l}$ captures the average differences among pilot regions of green patent counts

[36]The carbon price in Tianjin is the lowest among all the six pilot regions explored in this study. See Figure 1.D.5 in Appendix 1.D.

[37]Recall that I exclude two pilot regions from this study. This is due to lack of data availability on firms' regulatory status in Chongqing, and late implementation of the regulation in Fujian. Thus the pilot region in this study includes Beijing (BJ), Tianjin (TJ), Shanghai (SH), Hubei (HB), Guangdong (GD), and Shenzhen (SZ).

between regulated and non-regulated firms; $\beta_{3l}$ captures the average differences of green patent counts before and after the regulation implementation among pilot regions.

Column 1 in Table 1.6 reports the estimations of equation (1.7) for the heterogeneity effects and columns 2–5 report the estimations of the baseline regression (1.2) using different sub-samples of pilot regions Beijing, Shanghai, Hubei and Shenzhen. Estimating the effects using the pilot subsamples of Tianjin and Guangdong results in lack of statistical power and low numbers of clusters (390 and 411 observations, and 29 and 26 clusters in the subsamples of Tianjin and Guangdong respectively), I therefore estimate the pilot heterogeneity effects in these two regions using equation (1.7) on the full sample.[38] The estimates in column 1 reveal significant effects for green patenting in only one pilot region, Beijing. The estimations in columns 2–5 are qualitatively similar to the estimations in column 1 on each respective pilot region, with differing magnitudes.

To better understand the implications of the econometric results for pilot heterogeneity effects in Table 1.6, I present the marginal effects of the regulation in each of the regions in Figure 1.6.[39] The marginal effects are positive and significant at the 5% significance level in one region, Beijing, equal to 0.21 more green patents (standard error = 0.1), and marginally significant in Shanghai, equal to 0.23 (standard error = 0.12). One of the reasons for the significant effects is the carbon price: Beijing and Shanghai have the highest and the third highest average carbon prices among all the regions. Although Shenzhen has the second highest average carbon prices, the effect in Shenzhen is not significant.

Next, I estimate continuous treatment effects by the subsamples of pilot regions. Table 1.7 shows the results. Consistent with the results in Table 1.6, the increase of carbon prices increases the number of green patents significantly only in Beijing and Shanghai. On average, a 10 percent increase in carbon price is associated with about 4 percent more green innovation both in Beijing and Shanghai. Again, the insignificant estimations of the carbon price elasticity in the extensive margin suggest that only firms in the intensive margin respond to the variation of carbon prices. The effect of carbon pricing on the rest of the pilot regions (Hubei and Shenzhen) is less precisely estimated. The coefficients are positive but not statistically significant; thus it is possible that some regulated firms in these two regions were induced to file more green patents.

[38] The results using the pilot subsamples of Tianjin and Guangdong are not significant and not reported.

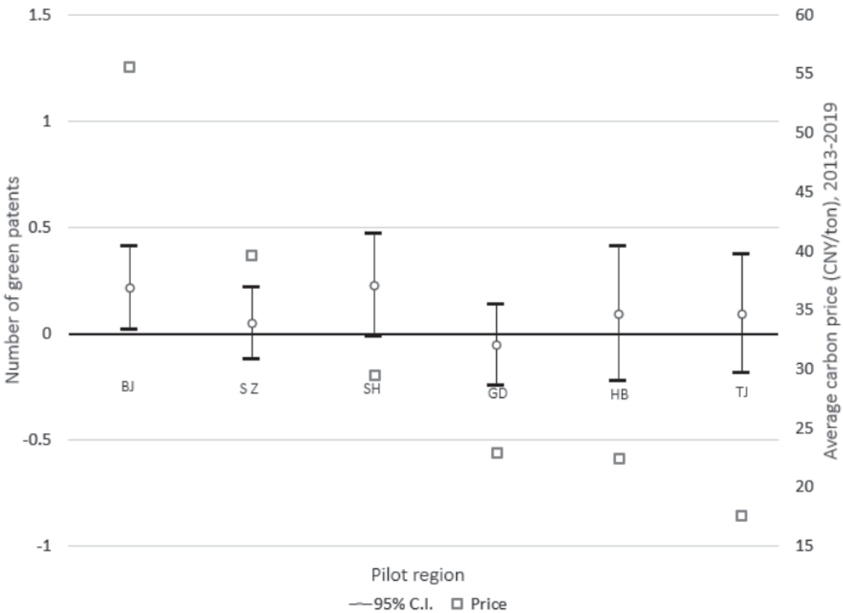
[39] Again, the marginal effects of the regulation on green innovation are calculated by the Stata built-in command `margins`. The marginal effects in Tianjin and Guangdong are obtained using the estimations in column 1.

Table 1.6: Effect of pilot ETS on green patenting using matched sample, by pilot regions

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Green patents, weighted | | | | | |
| regulated*post in BJ | 1.72** | | | | |
| | (0.79) | | | | |
| regulated*post in TJ | 2.29* | | | | |
| | (1.22) | | | | |
| regulated*post in SH | 1.05 | | | | |
| | (0.76) | | | | |
| regulated*post in HB | 0.42 | | | | |
| | (0.60) | | | | |
| regulated*post in GD | -0.46 | | | | |
| | (1.55) | | | | |
| regulated*post in SZ | 0.30 | | | | |
| | (0.41) | | | | |
| regulated*post | | 1.60** | 1.34** | 0.47 | 0.37 |
| | | (0.66) | (0.66) | (0.74) | (0.45) |
| regulated | | 0.44 | -0.82** | -0.35 | 0.35 |
| | | (0.38) | (0.38) | (0.43) | (0.27) |
| inflate | | | | | |
| regulated*post in BJ | 0.78 | | | | |
| | (0.64) | | | | |
| regulated*post in TJ | 2.19 | | | | |
| | (4.11) | | | | |
| regulated*post in SH | 0.21 | | | | |
| | (0.64) | | | | |
| regulated*post in HB | 1.07 | | | | |
| | (0.90) | | | | |
| regulated*post in GD | 0.16 | | | | |
| | (1.61) | | | | |
| regulated*post in SZ | 0.26 | | | | |
| | (0.36) | | | | |
| regulated*post | | 1.09 | 0.60 | 1.94 | 0.28 |
| | | (0.75) | (0.55) | (1.19) | (0.39) |
| regulated | | 0.52 | -0.78* | -2.22** | 0.29 |
| | | (0.52) | (0.46) | (1.04) | (0.32) |
| Observations | 7829 | 1203 | 1638 | 1066 | 3121 |
| Mean dependent var. | 0.39 | 0.56 | 0.35 | 0.20 | 0.48 |
| Sd. of dependent var. | 3.56 | 7.17 | 1.94 | 0.81 | 3.10 |
| Pilot | Full sample | Beijing | Shanghai | Hubei | Shenzhen |
| log likelihood | -6425.57 | -1087.74 | -1176.92 | -474.60 | -2873.91 |
| AIC/N | 1.66 | 1.88 | 1.49 | 0.97 | 1.87 |

**Note:** This table reports maximum likelihood estimators using a zero-inflated Poisson model for the sample processed using matching. Column 1 shows the results for estimating equation (1.7). Columns 2–5 show the results for estimating the pilot heterogeneity effects using the sub-samples by regions. Standard errors are clustered at 4-digit sector level, with 268, 93, 111, 88, and 143 clusters respectively in columns 1–5. Specifications in all the columns include year fixed effects, ownership dummies and firm size dummies. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure 1.6: The ETS heterogeneity effects in pilot regions




**Note:** The primary vertical axis stands for the effect of ETS on the number of green patents, and the secondary vertical axis is the average carbon price in each pilot region in 2013-2019 with units of Chinese Yuan (CNY)/ton. Along the horizontal axis, from left to right, each point represents one pilot region, with the order of the regions from the highest to the lowest average carbon price in 2013-2019–that is, BJ for Beijing, SZ for Shenzhen, SH for Shanghai, GD for Guangdong, HB for Hubei, TJ for Tianjin. The lines vertical to the horizontal axis at each of the pilot regions present the regulation marginal effects in different regions respectively, from the estimations in Table 1.6 with 95% confidence intervals of the marginal effects presented simultaneously. The square markers show the average carbon prices in each of the pilot regions.

**Firm heterogeneity.** Another source of heterogeneity comes from firms that potentially respond differently to the regulation because they have different quantities of inputs available with which to produce innovation. For instance, firms with more capital are able to produce more output and therefore generate more revenue, which leads to more investment, including the R&D investment that is likely to produce more innovation. To capture such a potential indirect effect of the regulation, I use output per worker as a proxy for firms' available inputs on R&D. Output per worker correlates with the capital-labor ratio, which is used as an input in R&D. The output per worker also correlates with firms' productivity, which is largely influenced by technological development. Firms that were already productive before the treatment might continue to have a stronger ability to innovate and be more likely to respond to the regulation.

Table 1.7: Effect of pilot ETS on green patenting using matched sample, carbon price elasticity by pilot regions

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Green patents, weighted | | | | |
| Logarithm carbon price | 0.40** | 0.45** | 0.17 | 0.09 |
| | (0.17) | (0.18) | (0.24) | (0.11) |
| regulated | 0.46 | -0.91** | -0.36 | 0.37 |
| | (0.38) | (0.37) | (0.41) | (0.26) |
| inflate | | | | |
| Logarithm carbon price | 0.28 | 0.23 | 0.66 | 0.08 |
| | (0.19) | (0.16) | (0.43) | (0.10) |
| regulated | 0.53 | -0.90** | -2.15** | 0.29 |
| | (0.52) | (0.46) | (1.00) | (0.31) |
| Observations | 1203 | 1638 | 1066 | 3121 |
| Mean dependent var. | 0.56 | 0.35 | 0.20 | 0.48 |
| Sd. of dependent var. | 7.17 | 1.94 | 0.81 | 3.10 |
| Pilot | Beijing | Shanghai | Hubei | Shenzhen |
| log likelihood | -1088.34 | -1173.84 | -474.77 | -2874.39 |
| AIC/N | 1.88 | 1.48 | 0.97 | 1.87 |

**Note:** This table reports maximum likelihood estimators using a zero-inflated Poisson model for the sample processed using matching. Columns 1–4 report the estimations on the carbon price elasticity on number of green patents by pilot regions using the carbon price in the same year. Standard errors are clustered at 4-digit sector level, with 93, 111, 88, and 143 clusters respectively in columns 1–4. Specifications in all the columns include year fixed effects, ownership dummies and firm size dummies. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

To test this hypothesis, I add a vector of interaction terms between the firms' output per worker and the regulation dummy in equation (1.8). The interaction captures the different patenting ability of firms with different output per worker. I use the data on output and labor in 2012, the year before the implementation of the ETS regulation, to generate the output per worker measure. For firms with missing data in 2012, I use the data from the year between 2007 and 2011 that is closest to 2012. Because output per worker varies greatly by sectors[40], it is more reasonable to compare firms in the same sector. I therefore assign an index from 1 to 4 to all firms based on the output per worker relative to the 4-digit sector average. I then run a ZIP regression with the following specification at the intensive margin:

[40]For instance, in 2012, the mean of output per worker in the water supply industry was 986 thousand Yuan, while the means in heating supply and electricity supply industries are 5230 and 252,668 thousand Yuan respectively.

$$y_{it} = exp(\sum_{q=1}^{4} \beta_{1q} \times Q_{ij}^{q} \times regulated_i \times post_t + \sum_{l=2}^{4} \beta_{2q} \times Q_{ij}^{q} \times regulated_i$$
$$+ \sum_{l=1}^{4} \beta_{3l} \times Q_{ij}^{q} \times post_t + \sum_{2}^{4} Q_{ij}^{q} + \beta_5 regulated_i + \gamma_{i,o} + \delta_{i,size} + \alpha_t + \eta_l) + \epsilon_{it}. \tag{1.8}$$

In the above specification, $q$ indexes each of the four quartiles of output per worker distribution and $Q_{ij}^{q}$ equals one if firm $i$ in 4-digit industry $j$ belongs to quartile $q$. The coefficient $\beta_{1q}$ measures the effect of different quartiles of output per worker on regulated firms.

Estimation of equation (1.8) is reported in the first columns of Table 1.8. The coefficients in column 1 estimated from the ZIP regression imply the following quantitative response in the number of green patents to the pilot ETS: the pilot ETS induces a statistically significant increase in green innovation only in the fourth quartile of the output per worker distribution.[41] Figure 1.7 presents the average marginal effects of the pilot ETS regulation evaluated for large, medium and small firms[42] and different quartiles of the output per worker distribution. The average marginal effects have higher magnitudes for firms with larger size and yet the effects are significant at the 10 percent significance level only for large firms at the fourth quartile. For a regulated large firm at the fourth quartile of output per worker, the regulation on average increases the number of green patents by 0.34 (standard error = 0.20). However, for a regulated firm at the top quartile of output per worker distribution that files no patents, the pilot ETS is associated with a reduction in the likelihood of entry into green technology innovation.[43]

**The indirect effect of carbon prices on heterogeneous firms.** To capture the indirect effect of carbon prices on firms at different output per worker quartiles,

[41] As a robustness test, I assign a quintile index instead and find that the effects are significant only in the top quintile of the output per worker distribution, with the coefficient equal to 1.74 and standard error of 0.49. The estimations are not reported.

[42] The firms with miniature size are not considered because there are no regulated miniature firms in the sample.

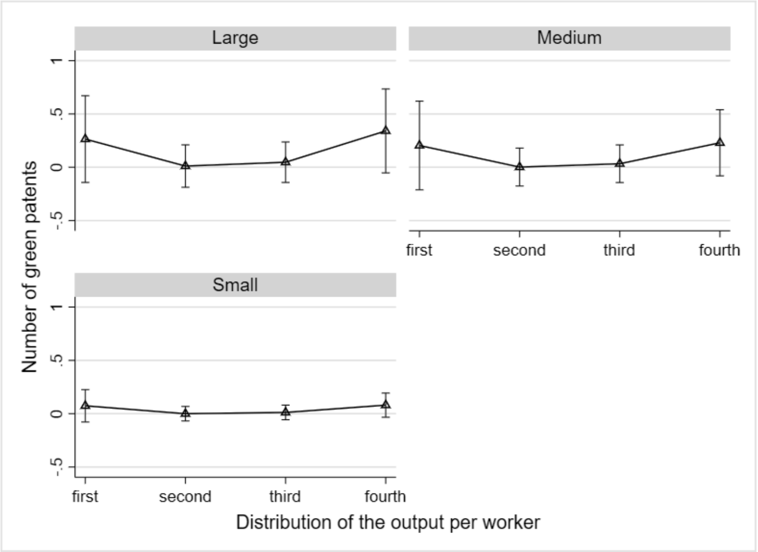
[43] A related concern is that, for firms in the top quartile of output per worker, the significant increase in the number of green patents in the intensive margin is because the pilot ETS forces these firms that have relatively small amount of green innovation stop innovating. Accordingly, the regulation appears to, on average, increase the number of green patents for firms in the top quartile, and meanwhile decrease the likelihood of entering into green innovation. To address this issue, I drop from the sample the firms in the top quartile that exited green innovation after the implementation of the pilot ETS (about 4 percent of the sample). Then, I estimate the heterogeneity effects using the same regression (equation (1.8)) on this sample. The results are robust to such an exercise. (not reported) Therefore, the increase of green innovation in the intensive margin is not due to the decrease in the likelihood of entering into green innovation.

Table 1.8: Effect of pilot ETS on green patenting and dirty patenting using matched sample

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| main | | | | | | | | | |
| first quartile × regulated*post=1 | 0.73 | | | | | | | | |
| | (0.52) | | | | | | | | |
| second quartile × regulated*post=1 | 0.12 | | | | | | | | |
| | (0.35) | | | | | | | | |
| third quartile × regulated*post=1 | 0.26 | | | | | | | | |
| | (0.38) | | | | | | | | |
| fourth quartile × regulated*post=1 | 1.47*** | | | | | | | | |
| | (0.50) | | | | | | | | |
| regulated*post | | | -0.01 | -0.04 | -0.01 | -0.02* | -0.02 | -0.17 | 0.33* |
| | | | (0.02) | (0.04) | (0.01) | (0.01) | (0.01) | (0.56) | (0.17) |
| regulated | -0.17 | -0.63 | | | 0.00 | | | 0.74 | 0.16 |
| | (0.31) | (0.45) | | | (0.01) | | | (0.57) | (0.16) |
| first quartile × Logarithm carbon price | | 0.09 | | | | | | | |
| | | (0.13) | | | | | | | |
| second quartile × Logarithm carbon price | | 0.06 | | | | | | | |
| | | (0.09) | | | | | | | |
| third quartile × Logarithm carbon price | | 0.02 | | | | | | | |
| | | (0.14) | | | | | | | |
| fourth quartile × Logarithm carbon price | | 0.58*** | | | | | | | |
| | | (0.21) | | | | | | | |
| inflate | | | | | | | | | |
| first quartile × regulated*post=1 | 0.51 | | | | | | | | |
| | (0.71) | | | | | | | | |
| second quartile × regulated*post=1 | 0.16 | | | | | | | | |
| | (0.34) | | | | | | | | |
| third quartile × regulated*post=1 | 0.18 | | | | | | | | |
| | (0.46) | | | | | | | | |
| fourth quartile × regulated*post=1 | 1.24** | | | | | | | | |
| | (0.53) | | | | | | | | |
| regulated*post | | | | | | | | -0.49 | -0.11 |
| | | | | | | | | (0.55) | (0.14) |
| regulated | 0.23 | -0.54 | | | | | | 0.56 | 0.19** |
| | (0.30) | (0.45) | | | | | | (0.57) | (0.09) |
| first quartile × Logarithm carbon price | | -0.02 | | | | | | | |
| | | (0.19) | | | | | | | |
| second quartile × Logarithm carbon price | | 0.07 | | | | | | | |
| | | (0.09) | | | | | | | |
| third quartile × Logarithm carbon price | | 0.14 | | | | | | | |
| | | (0.15) | | | | | | | |
| fourth quartile × Logarithm carbon price | | 0.45** | | | | | | | |
| | | (0.18) | | | | | | | |
| Observations | 7829 | 7829 | 7828 | 1249 | 7829 | 7828 | 4922 | 7829 | 7829 |
| Mean dependent var. | 0.39 | 0.39 | 0.15 | 0.81 | 0.06 | 0.06 | 0.09 | 0.10 | 5.10 |
| Sd. of dependent var. | 3.56 | 3.56 | 0.35 | 0.36 | 0.19 | 0.19 | 0.22 | 1.88 | 19.83 |
| R-squared | | | 0.30 | 0.51 | 0.03 | 0.25 | 0.35 | | |
| log likelihood | -6323.67 | -6291.49 | | | | | | -2087.56 | -52978.94 |
| AIC/N | 1.64 | 1.63 | | | | | | 0.55 | 13.55 |

**Note:** This table reports maximum likelihood estimators using a zero-inflated Poisson model (columns 1, 2, 8 and 9), and OLS estimations (columns 3–7) for the sample processed using matching. The columns 1 and 2 show the results for estimating the pilot ETS effects by quartile of firms' output per worker distribution. Columns 3–7 show the results from OLS with firm fixed effects (columns 3–4, and 6–7) and without (column 5). The outcome variables are the ratio between the number of green patents and the sum of the numbers of green and dirty patents (columns 3 and 4), and the ratio between the number of green patents and the number of all the patents (columns 5–7), with (columns 3, 5 and 6) and without $10^{-6}$ added (columns 4 and 7) in the denominator. Column 8 presents the effect of the pilot ETS on dirty patenting. Column 9 presents the effect on the number of patents excluding the green patents. Standard errors are clustered at 4-digit sector level, with 266, 266, 268, 131, 268, 268, 241, 268 and 268 clusters in the eight columns respectively. Specifications in all the columns include year fixed effects; specifications in columns 1, 2, 8 and 9 include pilot fixed effects, firm size dummies, and the ownership dummies. * p < 0.1, ** p < 0.05, *** p < 0.01.

Figure 1.7: Marginal effects of pilot ETS on green patenting, by firm size




I add an interaction between carbon prices and the quartiles. The intuition is that, for regulated firms in the same pilot region facing identical carbon prices, the firms with distinct output per worker might respond to the regulation differently. To assess this relationship, I replace the discrete treatment dummy with the logarithm carbon prices in year $t$ in the above specification (equation (1.8)) to allow for heterogeneous effects of carbon price changes on firms at different quartiles. Column 2 presents the indirect effect of output per worker on carbon prices. The estimations address the following response of regulated firms by the number of green patents: for firms located in the same pilot region and thus facing the same carbon price level, only firms in the fourth quartile of the output per worker distribution respond to the carbon price increase, which is consistent with what the estimations in column 1 imply. The elasticity of green patents to the carbon price for firms in the fourth quartiles of the output per worker distribution is 0.58. This means that a 10 percent increase in the carbon price will increase the green patents by 5.8 percent for firms in the top quartile. However, in the extensive margin, the increase in carbon prices reduces the likelihood of technological entry into green innovation, especially for firms in the upper range of the output-per-worker distribution.

**The direction of technical change.** One related question is about the direction of the technological change. Carbon pricing imposes a cost to pollute on the regulated firms, which in turn increases the value of innovation in clean technology. Firms might shift their innovation activities from dirty fossil fuel technology to clean low-carbon technology. To test whether the regulated firms file more green patents at a cost of reducing dirty innovation, I use the share of green patents as an outcome variable, calculated as the ratio between the number of green patents and the sum of the numbers of green and dirty patents, and estimate the ETS effect using the following regression specification:

$$share_{it} = \beta_5 regulated_i \times post_t + \alpha_t + \alpha_i + \epsilon_{it}. \tag{1.9}$$

In the above specification, $share_{it}$ is the share of green patents. I control for the firm fixed effects $\alpha_i$ and year fixed effects $\alpha_t$. Around 85 percent of the observations in the sample file neither green nor dirty patents; these need be dropped from the sample, which might potentially leads to a sample selection problem. I therefore add a small number $10^{(-6)}$ to the sum of the green patent counts and dirty patent counts to keep all the observations. Columns 3 and 4 in Table 1.8 compare whether adding this small number affects the results in a significant way. The insignificant estimations in the two columns suggest that the pilot ETS does not significantly induce the development of technology to a "greener" direction.

Because the pilot ETS increases green innovation without shifting technology in a greener direction, one of the immediate concerns is that the regulation might meanwhile increase the number of dirty patents. Therefore, I estimate the effect of the pilot ETS on the number of dirty patent applications. Column 8 reports the estimations from the ZIP regression. No significant effects of the pilot ETS on dirty innovation are observed in the data. Then, a related concern is that the discrepancy between the insignificant effects on the number of dirty patents and the share of green patents, and the significant effects on the number of green patents, might be driven by time-invariant unobservable firm heterogeneity, which is not accounted for in the ZIP regression. I address this concern by showing in Section 1.5.4.4 that the estimations on the policy effects are robust to different model specifications including Poisson and OLS regressions with firm fixed effects.

**Assessing the crowding-out effect.** Another question is whether the regulation might increase green innovation and meanwhile crowd out patents which do not belong to the classification of green patents (non-green patents). To test whether the regulated firms increase green innovation at a cost of other types of innovation, I use the ratio between the number of green patents and the number of all patents filed by a firm in a year as an outcome variable, and estimate the effect on this ratio using regression (1.9). Columns 6 and 7 show the results. Similarly a small number ($10^{-6}$) is added to the number of all patents in the ratio in column 6 to avoid dropping observations with zero patents filed in certain years. The estimations are not affected by adding the number and both are negative.[44] To further address the concern about firm specific effects, I compare the estimations on the effects of this share with (column 6) and without firm fixed effects (column 5). The estimation with firm fixed effects is slightly lower; however it is not statistically different from the one without firm fixed effects ($p = 0.58$). Column 9 presents the estimation on the policy impact on the number of patents excluding the green patents. The estimation is positive and significant at the 10 percent significance level. This could be because, for instance, some patents are somewhat related to low-carbon innovation but not counted in the outcome.

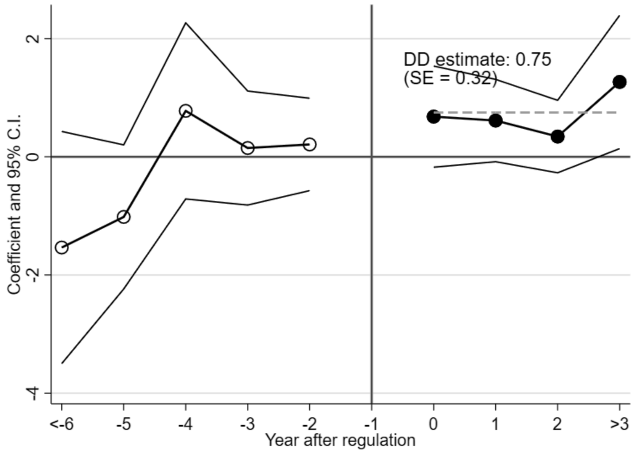
### 1.5.3 Event-Study Test of Parallel Trends Assumption

The key identifying assumption for the above estimates is that there are parallel pre-regulation trends in the number of green patents for regulated and non-regulated firms. I test this assumption in an event-study specification. That is, I interact the treatment indicator with year dummies leading up to and following the pilot ETS regulation, going from six years before to four years after the regulation. The omitted year is 2012, the year before the first implementation of the regulation. As Figure 1.8 shows, I do not find any differential green-patenting behavior for regulated versus non-regulated firms in the years leading up to the regulation. The estimations on the leads in the event-study specification are never significantly different from zero. This supports the assumption regarding parallel pre-regulation trends. In addition, this supports the assumption of no anticipatory effects. That is, even though the regulation was announced in 2011, firms did not respond to the regulation as of the year of implementation of the regulation. I discuss this issue in Section 1.5.4.1. It is worth noting that there is a delay in the policy effect: the effect is only significant in 2016, three years after the implementation of the regulation. This pattern is reasonable because innovation is an ongoing process which requires continuous inputs and has some possibility of failure. One cannot expect an immediate reaction from the regulated firms to the pilot ETS regulation.

As discussed in Section 1.4.2, one of the caveats of this study is that firms are matched at firm level, not at installation level, because the regulatory threshold is determined at the firm level, as introduced in Section 1.2.2. A parallel pre-trend reassures to some extent that the matched regulated and non-regulated firms are

[44]Because of rounding, both estimations seem to be significant. The more precise rounding estimations are $-0.016$ (standard error=0.009) and $-0.017$ (standard error=0.011) for specifications 6 and 7, respectively.

Figure 1.8: Event study of the implementation of the pilot ETS




not systematically different from each other, including their innovating ability and the firms' emissions intensity.

### 1.5.4 Robustness Analysis

The baseline results suggest that the regulated firms overall respond to the ETS by innovating slightly more. In addition, I show that the effects are heterogeneous across both pilot regions and firms. The main findings are robust to various specifications. In this section, I report a number of robustness tests. I consider mainly whether the results are driven by self-selection into non-treatment, and whether they are driven by the measurement of the outcome variable. I also consider whether there are spillover effects of the regulation, and whether time-invariant firm heterogeneity drives the estimations in a significant way.

#### 1.5.4.1 Are the Results Driven by Self-Selection?

My identifying assumption relies on the fact that the firms cannot self select their regulatory status. As discussed before, one of the main concerns is whether firms are able to influence whether they are regulated. For example, if the cost of abating by reducing productivity is lower than the cost of investing in abatement technology, firms would reduce productivity to comply rather than innovating. Hence, regulated and non-regulated firms would be systematically different from each other. In this case, the estimates would be biased. However, there is little evidence that firms have this power. Since the pilot ETS regulation was announced in 2011 but the coverage threshold was not announced at that time, firms could not obtain information in advance on how the threshold would be set. In other words, firms could not take regulation into consideration when they made decisions on productivity and hence emissions before 2013. Therefore, they could not adopt precautionary measures to strategically avoid being regulated. Moreover, the regulation came into effect in 2013 and remained unchanged until 2016. In 2015, local DRC, except for Tianjin, lowered the coverage threshold significantly for the following years. If regulated, firms just above this threshold before 2016 would behave strategically in order to not be regulated in the following years. They would have to greatly reduce their production, at a cost of losing market share and annual sales. However, purchasing carbon emissions permits from the local carbon market would by no means become a large cost share for regulated firms compared to the cost of reducing productivity, because carbon price in these pilot regions are currently not high. In addition, Figure 1.8 in Section 1.5.1 provides the evidence that there is no pre-regulation trend. Therefore, the evidence of having such a self-selection issue is weak.

#### 1.5.4.2 Are the Results Driven by the Measurement of the Outcome Variable?

All the specifications I show in Section 1.5.1 use the patent counts weighted by the number of co-applicants on each of the filed patents. My results could be driven by the re-weighting of the patent counts. If the regulated firms co-apply more (less) compared to the non-regulated firms after the implementation of the regulation, my estimation using the weighted patent counts would be lower (higher) than the estimations using the unweighted patent counts. Table 1.9 presents all the related estimations using the unweighted patent counts as an outcome variable. Column 1 presents the estimation of the overall policy impact. The average marginal effect of ETS on the unweighted number of green patents is 0.17 (standard error $= 0.09, p = 0.068$), which is close to the estimation of the effect on the weighted green patent counts. Columns 2–5 show the carbon price elasticity on the number of green patents using different carbon price leads. The elasticity of the green patents to the current carbon price is 0.26, which is comparable to the main estimation on the carbon price elasticity of 0.23. The elasticities to carbon prices with leads one to three are less precisely estimated and are all qualitatively comparable to the estimations using the weighted patent counts as an outcome. In summary, the magnitudes of the estimations using the unweighted patent counts are generally slightly higher than the estimations using the weighted counts, but they do not differ significantly. Therefore, the results discussed above are robust to the re-weighting.

Column 6 presents the indirect policy effect through the output per worker. The effects are significant for regulated firms in the first quartile of output per worker, but not for the firms with higher output per worker. The average marginal effects for the firms in the first quartile is 0.38 (standard error $= 0.18$, $p = 0.04$). One potential explanation on the difference of the effects on the weighted and unweighted green patent counts is that the regulated firms in the first quartile co-apply more after the implementation of the regulation, and there is no such an effect for firms with higher output per worker. Columns 7–9 show the estimations on the effects of the direction of the technical change. I again use the ratio of the number of green patents and the sum of green and dirty patents as an outcome variable and estimate a fixed-effects OLS model. Columns 7 and 8 present the results. Similarly, in column 7, I add a small number ($10^{-6}$) to the sum of the counts of the green and dirty patents to avoid dropping the observations that file neither green nor dirty patents. The estimations in columns 7 and 8 are not significantly different and therefore the results are not driven by dropping the observations that filed neither green nor dirty patents. I then estimate the effect on dirty patents with a ZIP and column 9 shows the result. There is no significant effect on dirty patents, though the sign becomes positive. However, the average marginal effects on the weighted and unweighted dirty patent counts are similar at 0.019 (standard error = 0.023) and 0.015 (standard error = 0.026) respectively.

Again, the key identifying assumption is the parallel pre-regulation trends in the unweighted number of green patents for the regulated and non-regulated firms. Figure 1.D.4 in Appendix 1.D shows the means of the unweighted number of green patents in 2007-2016 by the pilot regions on the matched sample. There is little to no difference in the means between the regulated and non-regulated firms before 2013.

#### 1.5.4.3 Are There Any Spillover Effects?

In the main analysis, I match the regulated firms and non-regulated firms in the same pilot region. The effects might be under- or over-estimated if the non-regulated firms in the pilot regions also respond to the regulation – for example, to avoid being regulated in the future. To test whether there are such spillover effects of the regulation, I match regulated firms with non-regulated firms outside pilot regions on variables introduced above. If there is no significant difference between the estimations using this sample and the ones in my baseline estimations, I could conclude that non-regulated firms in the pilot regions are not responding to the regulation and the estimations are not biased by spillover effects. Otherwise, if the new estimation results in a higher point estimator, I could

Table 1.9: Effect of pilot ETS on unweighted green patenting using matched sample, count data model

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| main | | | | | | | | | |
| regulated*post | 0.86*** | | | | | | -0.02 | -0.06 | 0.24 |
| | (0.33) | | | | | | (0.02) | (0.05) | (0.56) |
| regulated | 0.09 | 0.03 | 0.17 | 0.36 | 0.22 | -0.11 | | | 0.63 |
| | (0.29) | (0.31) | (0.34) | (0.46) | (0.50) | (0.50) | | | (0.61) |
| Logarithm carbon price | | 0.26** | | | | | | | |
| | | (0.12) | | | | | | | |
| Logarithm carbon price T+1 | | | 0.21* | | | | | | |
| | | | (0.11) | | | | | | |
| Logarithm carbon price T+2 | | | | 0.15 | | | | | |
| | | | | (0.12) | | | | | |
| Logarithm carbon price T+3 | | | | | 0.20 | | | | |
| | | | | | (0.17) | | | | |
| first × regulated*post=1 | | | | | | 0.74** | | | |
| | | | | | | (0.29) | | | |
| second × regulated*post=1 | | | | | | 1.11* | | | |
| | | | | | | (0.58) | | | |
| third × regulated*post=1 | | | | | | 0.98 | | | |
| | | | | | | (0.60) | | | |
| fourth × regulated*post=1 | | | | | | 1.29 | | | |
| | | | | | | (0.85) | | | |
| inflate | | | | | | | | | |
| regulated*post | 0.52** | | | | | | | | 0.07 |
| | (0.26) | | | | | | | | (0.50) |
| regulated | -0.01 | -0.12 | -0.02 | 0.15 | 0.06 | -0.17 | | | 0.14 |
| | (0.18) | (0.19) | (0.22) | (0.26) | (0.29) | (0.41) | | | (0.51) |
| Logarithm carbon price | | 0.19** | | | | | | | |
| | | (0.08) | | | | | | | |
| Logarithm carbon price T+1 | | | 0.14 | | | | | | |
| | | | (0.08) | | | | | | |
| Logarithm carbon price T+2 | | | | 0.10 | | | | | |
| | | | | (0.09) | | | | | |
| Logarithm carbon price T+3 | | | | | 0.13 | | | | |
| | | | | | (0.12) | | | | |
| first × regulated*post=1 | | | | | | 0.07 | | | |
| | | | | | | (0.31) | | | |
| second × regulated*post=1 | | | | | | 1.18** | | | |
| | | | | | | (0.55) | | | |
| third × regulated*post=1 | | | | | | 1.06* | | | |
| | | | | | | (0.60) | | | |
| fourth × regulated*post=1 | | | | | | 0.66 | | | |
| | | | | | | (0.69) | | | |
| Observations | 7129 | 7129 | 7129 | 7129 | 7129 | 7129 | 7129 | 899 | 7129 |
| Mean dependent var. | 0.36 | 0.36 | 0.36 | 0.36 | 0.36 | 0.36 | 0.12 | 0.79 | 0.11 |
| Sd. of dependent var. | 3.74 | 3.74 | 3.74 | 3.74 | 3.74 | 3.74 | 0.32 | 0.38 | 1.96 |
| R-squared | | | | | | | 0.25 | 0.51 | |
| log likelihood | -5367.54 | -5360.02 | -5450.49 | -5379.60 | -5370.85 | -5193.10 | | | -1803.54 |
| AIC/N | 1.52 | 1.52 | 1.54 | 1.52 | 1.52 | 1.49 | | | 0.52 |

**Note:** This table reports the effect of the pilot ETS on green patenting using the patent counts which are not weighted by the number of co-applicants on each patent. Columns 1–6 and 9 show the results from the zero-inflated Poisson regression, with the outcome variables as the green patent counts in columns 1–6 and the dirty patent counts in column 9; columns 7 and 8 show the results from OLS regression with the outcome variable as the share of the green patent counts. Column 1 shows the overall effect of the regulation on green patenting; columns 2–5 show the the estimations on the carbon price elasticity on number of green patents, with different price leads; column 6 shows the results for estimating the pilot ETS effects by quartile of firms' output per worker distribution; columns 7 and 8 present the estimations of the ETS effects on the share of green patenting; column 9 shows the estimations on the ETS effects on dirty patenting. Standard errors are clustered at 4-digit sector level, with 118 clusters in column 8 and 270 clusters in all the other columns. Specifications in all the columns include year fixed effects; specifications in columns 1–6 and 9 include pilot fixed effects, firm size dummies and the ownership dummies. (not reported) * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

conclude that non-regulated firms located in a pilot region innovate more than non-regulated firms outside of pilot regions and, therefore, the effects in my baseline estimation are underestimated. By contrast, if the new estimation is lower, I could conclude that non-regulated firms in pilot regions innovate less than firms outside of the pilot regions and thus the effects in my baseline estimation are overestimated.

Columns 1–5 in Table 1.10 report the estimations on the effects of the pilot ETS and the carbon price elasticities with different price leads. The estimations become more precisely estimated in these columns compared to the estimations in Section 1.5.1, with the magnitudes higher in the estimations in the first three columns. Because I include the region fixed effects to control for the regional unobserved heterogeneity that influence firms' green patenting, I rule out the possibility that the firms in the pilot region systematically file more green patents than the non-pilot regions. The estimations with higher magnitude therefore suggest that the non-regulated firms within the pilot regions also respond somewhat positively to the ETS regulation. A possible explanation is that, to avoid being regulated in the future, given the full information on the regulatory threshold after 2013, the non-regulated firms that have carbon emissions close to the threshold and therefore are more likely to be regulated also increase their green innovation, which potentially helps mitigating carbon emissions.[45] This suggests an underestimation of the policy effects discussed in Section 1.5.1. I can therefore interpret my estimations as a lower bound of the policy effects. Column 6 presents the estimations on the effect of the pilot ETS on each quartile of output per worker distribution. The pilot ETS induces a statistically significant increase in the number of filed green patents only in the third quartile of the output per worker distribution. The effect on the rest of the quartiles is positive but not statistically significant. This does not necessarily suggest that the result is inconsistent with the baseline estimation, because the matched non-regulated firms which are outside the pilot regions do not belong to exactly the same industries as the matched non-regulated firms which locate in the pilot regions, and the technology development might differ across industries.[46] Columns 7–9 present the estimations on the effects on the share of green patenting and on the number of dirty patents, which are statistically indistinguishable from the respective estimations in columns 3–4 and 7 in Table 1.8.

[45]However this is not empirically testable because of lack of availability on firm-level carbon emissions data.

[46]43 percent of the matched non-regulated firms are in Jiangsu and Zhejiang, and 57 percent in the other 18 provinces. The matched sectors are different across provinces. For instance, 24 percent and 36 percent of the matched non-regulated firms in the chemistry industry and the computer and telecommunications industry are located in Jiangsu.

Table 1.10: Effect of pilot ETS on green patenting, regulated firms matched with firms outside the pilot regions

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| main | | | | | | | | | |
| regulated*post | 1.31$^{***}$ | | | | | | -0.00 | -0.03 | -0.45 |
| | (0.44) | | | | | | (0.02) | (0.05) | (0.48) |
| regulated | -0.87 | -1.14$^{**}$ | -0.86$^{*}$ | -0.43 | -0.68 | -0.68 | | | 2.37$^{**}$ |
| | (0.55) | (0.52) | (0.47) | (0.56) | (0.59) | (1.16) | | | (1.20) |
| Logarithm carbon price | | 0.41$^{***}$ | | | | | | | |
| | | (0.11) | | | | | | | |
| Logarithm carbon price T+1 | | | 0.33$^{***}$ | | | | | | |
| | | | (0.10) | | | | | | |
| Logarithm carbon price T+2 | | | | 0.22$^{*}$ | | | | | |
| | | | | (0.12) | | | | | |
| Logarithm carbon price T+3 | | | | | 0.27$^{**}$ | | | | |
| | | | | | (0.13) | | | | |
| first quartile × regulated*post=1 | | | | | | 0.92 | | | |
| | | | | | | (0.69) | | | |
| second quartile × regulated*post=1 | | | | | | 0.28 | | | |
| | | | | | | (0.57) | | | |
| third quartile × regulated*post=1 | | | | | | 2.17$^{***}$ | | | |
| | | | | | | (0.60) | | | |
| fourth quartile × regulated*post=1 | | | | | | 1.31 | | | |
| | | | | | | (1.16) | | | |
| inflate | | | | | | | | | |
| regulated*post | 1.89$^{**}$ | | | | | | | | 0.29 |
| | (0.87) | | | | | | | | (0.47) |
| regulated | -0.18 | -0.57 | -0.16 | 0.40 | 0.05 | 0.60 | | | 5.61 |
| | (0.98) | (0.95) | (0.88) | (1.04) | (1.08) | (1.58) | | | (15.50) |
| Logarithm carbon price | | 0.58$^{***}$ | | | | | | | |
| | | (0.21) | | | | | | | |
| Logarithm carbon price T+1 | | | 0.48$^{***}$ | | | | | | |
| | | | (0.17) | | | | | | |
| Logarithm carbon price T+2 | | | | 0.35$^{*}$ | | | | | |
| | | | | (0.20) | | | | | |
| Logarithm carbon price T+3 | | | | | 0.42$^{*}$ | | | | |
| | | | | | (0.22) | | | | |
| first quartile × regulated*post=1 | | | | | | 1.74 | | | |
| | | | | | | (1.46) | | | |
| second quartile × regulated*post=1 | | | | | | 1.46$^{**}$ | | | |
| | | | | | | (0.72) | | | |
| third quartile × regulated*post=1 | | | | | | 3.96$^{*}$ | | | |
| | | | | | | (2.31) | | | |
| fourth quartile × regulated*post=1 | | | | | | 1.33 | | | |
| | | | | | | (1.48) | | | |
| Observations | 11985 | 11985 | 11985 | 11985 | 11985 | 11980 | 11966 | 1223 | 11985 |
| Mean dependent var. | 0.17 | 0.17 | 0.17 | 0.17 | 0.17 | 0.17 | 0.09 | 0.72 | 0.09 |
| Sd. of dependent var. | 0.95 | 0.95 | 0.95 | 0.95 | 0.95 | 0.95 | 0.29 | 0.42 | 0.76 |
| R-squared | | | | | | | 0.22 | 0.47 | |
| log likelihood | -5012.93 | -5004.32 | -5014.87 | -5025.36 | -5020.48 | -4935.47 | | | -2607.79 |
| AIC/N | 0.85 | 0.85 | 0.85 | 0.85 | 0.85 | 0.85 | | | 0.45 |

**Note:** This table reports the effects of the pilot ETS on green patenting using the sample that the regulated firms matched with the non-regulated firms outside the pilot regions. Columns 1–6 and 9 show the results from the zero-inflated Poisson regression, with the outcome variables as the green patent counts in columns 1–6 and the dirty patent counts in column 9; columns 7 and 8 show the OLS estimators with firm fixed effects, and the outcome variable is the share of the green patent counts. Column 1 shows the overall effect of the regulation on green patenting. Columns 2–5 show the the estimations on the carbon price elasticity on number of green patents, with different price leads. Column 6 shows the results for estimating the pilot ETS effects by quartile of firms' output per worker distribution. Columns 7 and 8 present the estimations on the direction of the technological change; column 9 shows the estimations on the ETS effects on dirty patenting. Standard errors are clustered at 4-digit sector level, with 147 clusters in column 8 and 342 clusters in all the other columns. Specifications in all the columns include year fixed effects; specifications in columns 1–6 and 9 include pilot fixed effects, firm size dummies and the ownership dummies. (not reported) * p < 0.1, ** p < 0.05, *** p < 0.01.

#### 1.5.4.4 Are the Results Robust to Controls for Firm-Fixed Effects?

Because there is no standard routine available for estimating the ZIP with fixed effects, as discussed in Section 1.4.1, the most common practice is to include the pre-sample, post-sample, or in-sample sum of the patent counts as a proxy for the unobserved firm heterogeneity which correlates with firms' innovation ability. This type of method requires either a long pre-sample or post-sample period, or an assumption on the strict exogeneity of the firm-specific effect, which are ruled out because of lack of data or unfulfilled assumptions. To compare whether the unobserved firm heterogeneity drives the results, I use an OLS with firm-fixed effects (FE) as a baseline reference and compare it without firm-fixed effects:

$$y_{it} = \beta_6 regulated_i \times post_t + \alpha_t + \alpha_i + \epsilon_{it}. \tag{1.10}$$

If, for instance, the estimations with and without FE differ significantly, then the unobserved firm heterogeneity might drive the results upward or downward depending on the difference between the two estimations. Table 1.D.4 in Appendix 1.D.2 presents the results. Columns 9 and 10 compare the effects on the number of dirty patents with and without firm fixed effects, and they are not statistically significantly different ($p = 0.30$). However, compared to the estimations on the effects on the number of green patents with firm fixed effects (column 1), the magnitude of the estimations without such effects (column 7) is inflated moderately and they are significantly different ($p = 0.05$). This seemingly suggests that the unobserved firm heterogeneity correlates with the ETS effects positively and not accounting for it might lead to an overestimation of the marginal effects of the pilot ETS on green innovation. I therefore estimate a fixed-effects Poisson regression; Table 1.D.5 in Appendix 1.D.3 shows the results. Column 2 shows the estimations of the ETS effect from a fixed-effects Poisson model. This estimation suggests that, on average, the pilot ETS increases the number of filed green patents by 0.28, which is higher than the baseline estimation of the average marginal effect of 0.16 from the ZIP model. One caveat of this model is that all the firms that have a constant amount of innovation between 2007-2016 are dropped because they are not informative in estimating the model. This is not ideal because half of the matched firms are dropped, which might introduce selection bias. This can potentially lead to an overestimation of the policy effect if, for instance, the comparable treatment firms that file no green patents over time are dropped. Moreover, in Table 1.D.4, I present the OLS estimations with firm fixed effects using the same subsample of firms used in the fixed effects Poisson model (column 8). If the unobserved firm heterogeneity accounted for in the fixed-effects Poisson model indeed drives the estimation, I expect that this estimation that uses partial information (column 8) differs from the estimation that does not account for the unobserved firm heterogeneity but uses full information (column 7). Because the two estimations do not differ significantly, I have some confidence that accounting for this unobserved heterogeneity but using partial information is at least not superior than not accounting for the firm heterogeneity but using full information.

## 1.6 Conclusion

In this paper, I study the impact of an environmental regulation on technological change in the context of a transitional economy. Specifically, I estimate the effect of China's pilot ETS on firms' green innovation, measured by the number of green patent applications. My main contribution is to study the heterogeneity across regions and firms in the factors that induce technological change. I take into consideration that innovator firms may or may not file patents and therefore distinguish between zero patent counts from innovators and non-innovators. Additionally, I consider innovation decisions at both the intensive margin–that is, the level of green innovation, and the extensive margin–that is, whether firms enter into green innovation. Using a zero-inflated Poisson estimation on a uniquely constructed dataset, I find that the ETS regulation induces a small but positive effect on green innovation in those two pilot regions with sufficiently high carbon price, with an upward trend, but no significant effects in the other regions. The effect is most pronounced for large firms and firms in the top quartile of the output per worker distribution. I also estimate a carbon emission price elasticity, showing that a 10 percent increase in the carbon price is associated with a 2.3 percent increase in the number of filed green patents.

These estimation results lead to two main implications. First, this finding adds to the debate on the effectiveness of the pilot ETS in China. Overall, the regulation works effectively in terms of inducing technological change through green innovation. However, the effects are not significant in all pilot regions. One possible explanation is the varying carbon emission prices. Varying prices between different pilot regions reflect regional differences in policy designs, such as allowances allocation, coverage threshold, the sectors being regulated, and the cost of non-compliance (i.e., enforcement and penalties). I show that, on average, the higher the carbon prices, the more green innovation is induced by the pilot ETS. Second, the pilot ETS is advantageous in the intensive margin to the regulated firms that already have high output per worker (and therefore higher productivity and/or more capital) and are likely to be more competitive initially. However, the firms in the top quartile of output per labor are less likely to enter into green innovation if they previously had zero knowledge stock of green innovation. The policy challenge thus is to encourage the regulated firms to start innovation in green technologies, and this is especially important for firms that are larger and more productive. Once they actually start and continue with conducting green innovation, they can potentially be the firms that are the most promising in green technologies.

A major objective of environmental regulation is to reduce pollution at a reasonable cost. The goal can be achieved in several ways, such as fuel-switching, technology diffusion and adoption, or innovation. Further research could explore the policy effects of the spread and adoption of new technology. Also, future research should explore more directly the short-term effectiveness of the pilot ETS, using firm-level carbon emissions data as an outcome variable.

# Appendix 1

## 1.A Additional Institutional Detail

### 1.A.1 Allowances Allocation

Grandfathering refers to a practice whereby future free emission allowances are dependent on past emissions or emission intensity. Specifically, grandfathering emission intensity determines the allowances in such a way that future allowances are in proportion to the emission intensity of an entity, while grandfathering emission requires that future allowances are in proportion to average yearly emission of an entity in a certain period. Benchmarking determines the allowances based on an emission benchmark of an industry, as well as firms' annual production. The difference of allowances allocation matters because allocating allowances overly generously dampen firms' incentives to adjust production plans to adapt to the regulation, and thus offers little incentive to innovate.

- Beijing: For heating companies and thermal power companies, allowances are allocated based on grandfathering emission intensity; for firms in industries other than heating and thermal power, allowances are allocated based on grandfathering yearly average emission between 2009 and 2012.

- Shanghai: Allowances are allocated based on benchmarking for the power and heating industries. For industries such as aviation, ports and waterway transportation, grandfathering is based on emission intensity. For those in commercial industries, hotels and airports, and firms for which it is hard to measure production, it is difficult to use industry benchmarking or emission intensity grandfathering, and therefore grandfathering based on historical emissions is adopted.

- Shenzhen: Government can repurchase allowances, at most 10% of total allowances, to stabilize the market price. Taking into consideration the annual decrease rate of carbon intensity, allowances are allocated based on grandfathering emission intensity for all firms regardless of industry. This annual decrease rate is formulated by Shenzhen DRC.

- Chongqing: Annual allowances are the same as reported emissions (RE) if total RE is smaller than an upper limit of total allowances. The upper limit of allowances is determined by the maximum yearly carbon emissions (YCE) between 2008 and 2012. Before 2015, this is decreased by 4.13%

yearly; after 2015, this is determined by the central government's mitigation goal. If total RE is larger than the upper limit of allowances, allowances are allocated based on both reported emissions and historical maximum emissions between 2008 and 2012.[47]

- Tianjin: Allowances are allocated mainly for free through grandfathering based on emissions from 2009 to 2012 or emission intensity. Benchmarking is adopted for new entrants and expanding capacity. Auction or purchasing at fixed prices may be implemented to stabilize the allowance price in case of acute fluctuations in market prices. Tianjin DRC did not publish clear guideline for how each industry's allowances would be allocated.

- Hubei: For firms in the power-generation industry, allowances are allocated using benchmarking; for firms in industries other than power generation, allowances are allocated using grandfathering based on average emissions of the last three years. The allowances allocation method in 2017 has been changed. Allowances for firms in the cement, power generation, and heating industries are allocated using benchmarking, while for firms in the paper, glass and ceramic industries, allowances are allocated using grandfathering based on emission intensity. Allowances for all the other regulated firms are allocated using grandfathering based on emission intensity.

- Guangdong: 95% of the allowances are allocated for free to the power-generation industry, while 97% of the allowances are allocated for free to the other industries. Benchmarking is adopted for firms in the coal-fired and gas-fired generation, cement, steel, paper and aviation industries. For other industries, the allowances are based on grandfathering and reduction of historical emission intensity.

- Fujian: All allowances are allocated for free in the first year. The threshold of being regulated is decreasing to 5,000 tons yearly carbon emissions (YCE) gradually. Meanwhile, aiming to introduce an allowances auction over time, the share of free allowances will be reduced. Similar to the allowances allocation method in Shanghai, benchmarking, grandfathering based on emissions, or emissions intensity are adopted for different sectors and industries.

[47]See Chongqing DRC for more details.`http://www.cqdpc.gov.cn/c/2014-05-29/521437.shtml`

### 1.A.2 Coverage Threshold

- Beijing: On November, 20th, 2013, Beijing Municipal DRC announced that entities with YCE higher than 10,000 tons (including both direct and indirect emissions[48]) are regulated in the scheme. On December, 16, 2015, this threshold was adjusted to target those with YCE higher than or equal to 5,000 tons. Emission-reporting entities are those who have consumed more than 2,000 tons coal equivalent (tce) of energy.

- Shanghai: For the first period (2013-2015), firms in industrial sectors such as iron and steel, petrochemicals, chemicals, non-ferrous metals, power generation, building materials, and paper, textiles, rubber and chemical fiber with YCE higher than or equal to 20,000 tons in either 2010 or 2011 are regulated in the pilot scheme. In contrast, firms in non-industrial sectors such as aviation, ports, airports, railways, commercial sectors, hotels and finance with YCE higher than or equal to 10,000 tons in either 2010 or 2011 are regulated in the pilot scheme.[49]

  As to the second period (2016-now), firms in industrial sectors that were not regulated in the first period, with YCE higher than or equal to 20,000 tons, are added to list of regulated firms. In addition, firms regulated from 2013 to 2015 with YCE higher than 10,000 tons are covered in the second period. Transportation sectors such as ports and aviation with YCE higher than 10,000 tons and waterway transportation with YCE higher than 100,000 tons are regulated in the pilot ETS.

- Shenzhen: Firms with YCE higher than 3,000 tons in any year are regulated. Firms with YCE higher than 1,000 tons and lower than 3,000 tons in any year are responsible for reporting carbon emissions annually. Moreover, Shenzhen Municipality also requires that owners of buildings for public affairs and national authority offices with area exceeding 10,000 square meters should be regulated in the pilot ETS as well.

- Chongqing: Before 2015, industrial firms with $CO_2$ equivalent higher than 20,000 tons in years between 2008 and 2012 are regulated. It is noteworthy that the Chongqing ETS is the only pilot that covers six greenhouse gases

[48] Direct emissions refer to the emissions generated during the production process by burning fossil fuels. Indirect emissions refer to emissions related to the use of purchased electricity and heating. Direct and indirect emissions are counted in all eight pilot regions.

[49] See Municipal Government's Opinions on Pilot ETS in Shanghai published by Shanghai Municipal People's Government in July, 3, 2012. URL `http://www.shanghai.gov.cn/nw2/nw2314/nw2319/nw10800/nw11407/nw29273/u26aw32789.html` (in Chinese).

(GHGs) including $CO_2$, $CH_4$, $N_2O$, $HFCs$, $PFCs$ and $SF_4$. All other seven pilot regions only regulate firms on $CO_2$ emissions.

- Tianjin: Firms in steel, power generation, heating, petrochemical, oil and gas exploration and construction industries with YCE higher than 20,000 tons in years between 2009 and 2012 are regulated.

- Hubei: Industrial firms with energy consumption exceeding 60,000 tons coal equivalent (tce) in either 2010 or 2011 are regulated in 2014, the starting year of the pilot ETS in Hubei. In contrast, in 2015, this time horizon changed to any year between 2009 and 2014. In 2016, the coverage became broader. Firms in the "seven industries"[50] with yearly energy consumption higher than 10,000 tce, as well as industrial firms in an industry other than the "seven industries" with yearly energy consumption exceeding 60,000 tce, in any year between 2013 and 2015, are regulated in the Hubei ETS. The coverage threshold is even stricter in 2017. All industrial firms with energy consumption exceeding 10,000 tce in any year between 2014 and 2016 are regulated.

- Guangdong: At the beginning of the pilot ETS, firms or entities in the industries of power generation, cement and petrochemical, with YCE higher than 20,000 tons, are regulated in the pilot; firms in the above industries with YCE higher than 10,000 tons are defined as emission-reporting entities. Starting from 2016, firms or entities in the paper and aviation industries satisfying the above coverage criteria are regulated as well.[51]

- Fujian: Firms or entities with total energy consumption higher than 10,000 tce in any year between 2013 and 2015, in the seven industries and industries of aviation and ceramics, are regulated in the Fujian pilot ETS.

  Fujian is famous for both the productivity and quality of ceramics and this industry contributes a large share of $CO_2$ emissions. Therefore, the Fujian DRC regulates the ceramics industry. There are 119 firms in ceramics among the 277 regulated firms.

[50] These are the iron and steel, petrochemicals, chemicals, non-ferrous metals, power generation, building materials, and paper industries.

[51] There were 4 firms in the aviation industry and 51 firms in paper industries newly added as regulated firms. See Summary of Allowances Allocation Method for Aviation and Paper Industries in Guangdong ETS `http://www.gddrc.gov.cn/zwgk/zcwj/zcjd/201712/t20171229_458124.shtml` for more details (in Chinese).

### 1.A.3 Punishment

If the cost of non-compliance is lower than the cost of technology development, emission reduction and purchasing allowances, firms tend to disregard the mitigation responsibility and to not take carbon emission into consideration in production planning. In this case, it is necessary to increase the cost of non-compliance. Pilot firms are punished if they emit more than the verified allocated allowances. Specifically,

- Beijing: Firms are fined for excess emissions at three to five times the average allowance price for the past year.
- Shanghai: Firms are fined at between 50,000 Yuan and at the highest 100,000 Yuan.
- Shenzhen: The amount of excess emissions is deducted from next year's allowance; firms are fined for excess emissions at three times the average allowance price for the last six months.
- Chongqing: Firms are not allowed to receive subsidies for energy-saving and climate-change related projects for three years. For state-owned companies, the irregularities are recorded in the Performance Appraisal System for State-owned Enterprise Leaders.
- Tianjin: Firms can not be financially supported for the next three years.
- Hubei: Two times the excess emissions are deducted from next year's allowances; firms are fined for excess emissions up to three times the average allowance price for the last year (however, no more than 150,000 Yuan).
- Guangdong: Two times the excess emissions are deducted from next year's allowances; firms are fined 50,000 Yuan.
- Fujian: Two times the excess emissions are deducted from next year's allowances; firms are fined for excess emissions up to three times the average allowance price for the last year (however, no more than 30,000 Yuan).

The stringency of punishment varies to a certain degree among different pilot regions. For instance, there is an upper limit of fines in Fujian, Guangdong, Shanghai and Hubei, which makes the punishment less harsh. Meanwhile, there is an allowance deduction for the next years in different degrees in Fujian, Guangdong, Hubei and Shenzhen if firms fail to stay within their allowances. In contrast, in

Table 1.A.1: Government plans and interim measures in eight pilot ETS

| Pilot | Document | Time |
|---|---|---|
| Beijing | Implementation Plan of Beijing ETS (Beijing DRC) | 20 November 2013 |
| | Threshold Adjustment on Beijing ETS (Beijing Municipal People's Government) | 28 December 2015 |
| | 2017 Beijing ETS Plan (Beijing DRC) | 15 December 2016 |
| Shanghai | Implementation Plan of Shanghai ETS (Shanghai Municipal People's Government) | 3 July 2012 |
| | Interim Management Measures of Shanghai ETS (Shanghai Municipal People's Government) | 18 November 2013 |
| | Allowances Allocation Plans (Shanghai DRC, 2016 and 2017) | 10 November 2016 and 20 December 2017 |
| Shenzhen | Interim Management Measures of Shenzhen ETS (Shenzhen Municipal People's Government) | 19 March 2014 |
| Chongqing | Interim Management Measures of Choingqing ETS (Chongqing Municipal People's Government) | 26 April 2014 |
| | Allowance Allocation Plans (Chongqing DRC) | 29 May 2014 |
| | Interim Rules of Carbon Emission Verification (Chongqing DRC) | 29 May 2014 |
| Tianjin | Implementation Plan of Tianjin ETS (Tianjin Municipal People's Government) | 5 February 2013 |
| | Interim Management Measures of Tianjin ETS (Tianjin Municipal People's Government, 2013 and 2016) | 20 December 2013 and 3 March 2016 |
| | Tianjin ETS China Certified Emission Reduction (CCER) (Tianjin DRC) | 9 July 2015 |
| Hubei | Implementation Plan of Hubei ETS (Hubei Provincial People's Government) | 18 February 2013 |
| | Allowances Allocation Plans (Hubei DRC, 2014, 2015, 2016 and 2017) | 14 April 2014, 25 November 2015, 3 Jaunuary 2017 and 10 January 2018 |
| | Interim Rules for Allowances Launch and Repurchase (Hubei DRC) | 29 September 2015 |
| | Hubei ETS CCER (Hubei DRC, 2016 and 2017) | 8 July 2016 and 13 June 2017 |
| Guangdong | Interim Management Measures of Guangdong ETS (Guangdong Provincial People's Government) | 1 March 2014 |
| | Allowance Allocation Plans (Guangdong DRC, 2014, 2015, 2016 and 2017) | 8 August 2014, 10 July 2015, 8 July 2016 and 25 August 2017 |
| | Allowances Verification and Compliance (Guangdong DRC, 2015, 2016 and 2017) | 18 February 2016, 22 February 2017 and 12 February 2018 |
| Fujian | Interim Management Measures of Fujian ETS (Fujian Provincial People's Government) | 22 September 2016 |
| | Interim Management Measures of Fujian GHGs Reporting (Fujian DRC) | 30 November 2016 |
| | Interim Implementation Measures of Fujian ETS (Fujian DRC) | 2 December 2016 |

Tianjin and Chongqing, firms are punished only by not being able to get subsidies or financial support for their projects. Notably, there is no upper limit of the fine in Shenzhen and Beijing, signalling that punishment is harsher than in cities that determine a specific upper limit on the fine.[52]

### 1.A.4 Measures and Plans

Principles of determining coverage threshold and punishment measures are from government official plans and plans. I will summarize the measures and plans in this section.[53] As listed in Table 1.A.1, government and DRC in some pilot regions, such as Hubei and Guangdong, release Allowances Allocation Plans annually, while DRC in some pilot regions, such as Chongqing and Shenzhen, merely released the plans once, when they were about to implement pilot ETS.

## 1.B Steps of Merging Datasets

He et al. (2018) discuss technical details on merging each firms' data in ASME with patent data in SIPO. I follow the main steps discussed in that paper but the principles used for merging the two datasets are more comprehensive; for

[52]For example, two firms in Shenzhen failed to achieve their 2016 mitigation liability; therefore they were fined 1,540,000 Yuan and 1,220,000 Yuan respectively. See `http://www.szpb.gov.cn/xxgk/qt/tzgg/201709/t20170914_8689504.htm`and `http://www.szpb.gov.cn/xxgk/qt/tzgg/201709/t20170914_8689503.htm` for details of penalty decisions (in Chinese).

[53]Reference URLs for all the plans and measures in Table 1.A.1 can be given upon request.

example, I use a broader list to define a company as a patent applicant. In order to construct the dataset, I first of all merge firms' information in ASME with each of the patent applications in SIPO using the entity name in ASME and the applicant in SIPO, and then merge the dataset with regulatory status.

### 1.B.1 Preparation of SIPO and ASME

First of all, I drop those patents applied for by individuals. There are two related caveats about SIPO. As this dataset is accessed via web-scraping, and there is often at least one co-applicant for each patent application, the information about applicants is scraped as one column regardless of the number of applicants for each application. Moreover, patent applications in SIPO are not specifically categorized as different types of applicants, for example, individual, educational institution and company. Therefore, I first of all need to drop those applied for by individuals and only keep applications for which there is at least one applicant which is non-individual.

I determine whether the applicant is an individual or a company based on three criteria. First, if the length of the name of the applicant is no longer than two Chinese characters, the applicant is an individual, as it is not likely that a firm's name only has two Chinese characters. This criteria rules out applications with a single individual applicant if the applicants' name is at most two Chinese characters. Next, patent applicants contain at least one non-individual applicant if the applicant ends with characters, such as station, plant, bureau, department, or school.[54] Moreover, if applicants contain the characters, for instance, university, academy, laboratory, hospital, headquarter, park, supermarket, trading, organization, committee, or group, they are considered as non-individual.[55]

After dropping all patents applied for by individuals, I duplicate each patent such that one applicant takes one row. For example, if there are five applicants for a certain patent, there are five observations for the same patent application.

The only identifier available for merging the two datasets is the firm's name. I use stem name in two datasets to do the matching. First of all, I remove all the punctuation in firms' name for both datasets.[56] Then, words specifying firms' type and ownership are removed, for example, group, board, branch, limited company.

[54]Full list contains 19 different characters. This can be given upon request.

[55]The full list contains more than 200 key words.

[56]Punctuation includes parentheses, brackets, slash, comma, and space, as well as some other marks.

Because of administrative error which leads to misreporting by firms, there are potential measurement errors in the variables in this dataset. Following Cai and Liu (2009) and Feenstra et al. (2014), I clean the data using the following criteria to obtain a clean sample. First, the total assets must be higher than the current assets and fixed assets, as well as the net value of the fixed assets; second, the year of incorporation must be earlier than the year that the data were surveyed, and the opening month must be between 1 and 12; third, the interest expenses must be non-negative; fourth, the firms that have fewer than 8 employees are dropped.

### 1.B.2 Merging and Post-Merging Validation

In order to not to lose information, I apply ever-matching, which only requires the ASME firm name and the patent applicants' name to be matched, irrespective of the year in which the firm appears in the ASME database or is filed with the SIPO. Moreover, firm name and patent applicants are matched as long as the ASME firm name is a left-aligned strict substring of the patent applicant's name.

Thereafter, I conducted a post-merging check to validate whether matched pairs are true matches.[57] Post-matching validation was checked in Python. There were 1,628,058 true matches after running the checking algorithm, while 383,058 matches required manual checking. I compiled them into 20,444 unique pairs of firm name and patent applicant's name. Then I checked these pairs manually. If the applicant is a subsidiary of the firm in ASME, it is considered as a true match. The match is also considered as a true match if there is no obvious reason indicating that they are not. By manually checking the matches, I find that 129,225 matches are not true matches, while 253,833 pairs are true matches.

## 1.C Coarsened Exact Matching and Genetic Matching

Rather than adopting the more commonly used and more conventional propensity score matching (PSM) techniques, I use coarsened exact matching (CEM), in combination with genetic matching (GM). One of the main caveats of PSM is that by projecting a number of covariates to a scalar propensity score and trying many models before choosing one to present, the data generation process is rarely known. Hence PSM potentially increases model dependence and imbalance on matching variables (King and Nielsen, 2015). In contrast, CEM operates on the same metric as the original data and thus obeys the congruence princi-

[57] The method is discussed in He et al. (2018).

ple.[58] Methods violating this principle lead to less robust inferences (Mielke and Berry, 2007).

The intuition of CEM is that, by choosing a certain value for each matching variable (defined as the coarsening in CEM, which describes how rough the matching is), observations are assigned with the same numerical value of strata if they are in the same coarsened strata. In other words, for a certain variable, the coarsening splits the variable into several intervals. Then observations in the same interval are assigned with the same numerical value of strata. Hence, CEM defines a number of strata based on the coarsening, and the observations in the same strata are grouped together. Then, matches are determined by exact matching on the numerical value of the strata. Using CEM, I can match based on the distribution of matching variables rather than the absolute distance defined by a certain caliper. Moreover, CEM is particularly appropriate in this study because the distributions of the matching variables, such as the number of patent applications, are highly right-skewed. Also, CEM prunes few unmatched treated units if there is a large number of control units in the dataset (Iacus et al., 2012). In addition, with CEM it is guaranteed that all variables are balanced on all higher order moments and interactions. Therefore, unlike propensity score matching, this method requires no checks on the balance of interactions on matching variables.[59]

CEM has the many advantages discussed above only if the coarsening is chosen based on substantive criteria. However, concerns can arise if the coarsening is set more arbitrarily. A reasonable argument with meaningful economic sense is thus important for the choice of coarsening. I address this potential threat in two aspects. First of all, I define the coarsening according to the statistical size of firms in China announced in *the Measures for Classification of Large, Medium, Small and Miniature Enterprises* by the National Bureau of Statistics.[60] With the defined coarsening, firms in the same pilot region and sector with the same statistical size are assigned to the same CEM stratum. Next, I complement the CEM

[58] Methods that violate the congruence principle include, for instance, propensity score matching and Mahalanobis distance matching. Both methods project the covariates from the k-dimensional space in the original data to one space defined by propensity score or Mahalanobis distance metrics (Iacus et al., 2012).

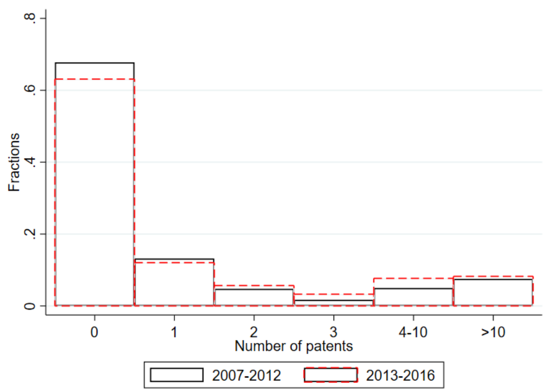
[59] CEM also reduces model dependence and is computationally efficient. For a detailed discussion, see Iacus et al. (2012).

[60] Large firms have annual sales higher than 400,000 thousand yuan and more than 1,000 employees; firms with sales between 20,000 thousand yuan and 400,000 thousand yuan and between 300 and 1,000 employees are medium-size; small firms have between 20 and 300 employees and sales between 3,000 and 20,000 thousand yuan; miniature firms have fewer than 20 employees and 3,000 thousand yuan. All the large, medium and small firms must fulfill both criteria for sales and employees; otherwise, the firms would be classified as one level lower. See `http://www.stats.gov.cn/tjsj/tjbz/201109/t20110909_8669.html` for a full list of classifications.

with genetic matching (GM) to improve the balance between regulated and non-regulated firms in the pre-treatment period and to reduce the model dependence. That is, I run GM within each CEM stratum to assure that both the congruence principle and the monotonic imbalance bounding are satisfied. Firms within the same CEM strata are matched on the number of filed green patents and the number of all filed patents between 2007 and 2012, the dummy for whether a firm filed at least one patent before 2013, and average sales and the employment between 2007 and 2012.[61] The main advantage of GM is that it directly optimizes covariate balance and avoids iterative manual checking on the estimated propensity score.

## 1.D Additional Empirical Results

Figure 1.D.1: Number of green patents: fraction distribution




[61] Again, for sales and employment, data in 2010 are excluded from the study due to poor quality of the data in this year, as discussed in Section 1.3.

Figure 1.D.2: Averages of weighted granted green patents 2007-2016, matched sample

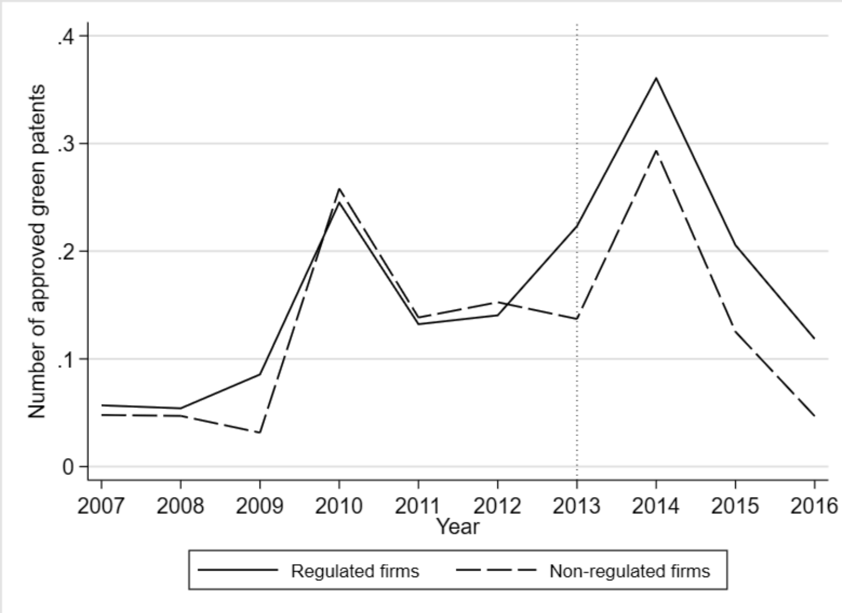



Figure 1.D.3: Averages of weighted green patents 2007-2016 by pilot region, matched sample

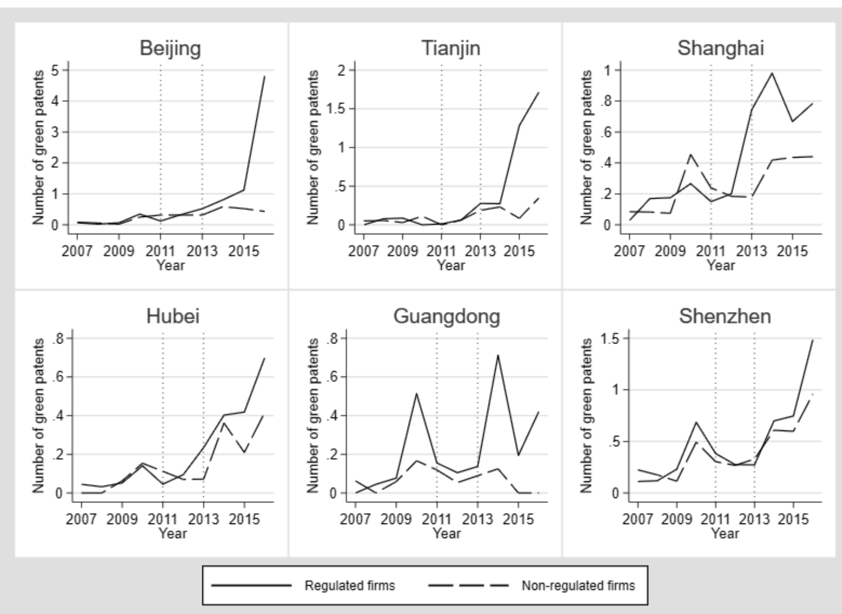


Figure 1.D.4: Averages of unweighted green patents 2007-2016 by pilot region, matched sample

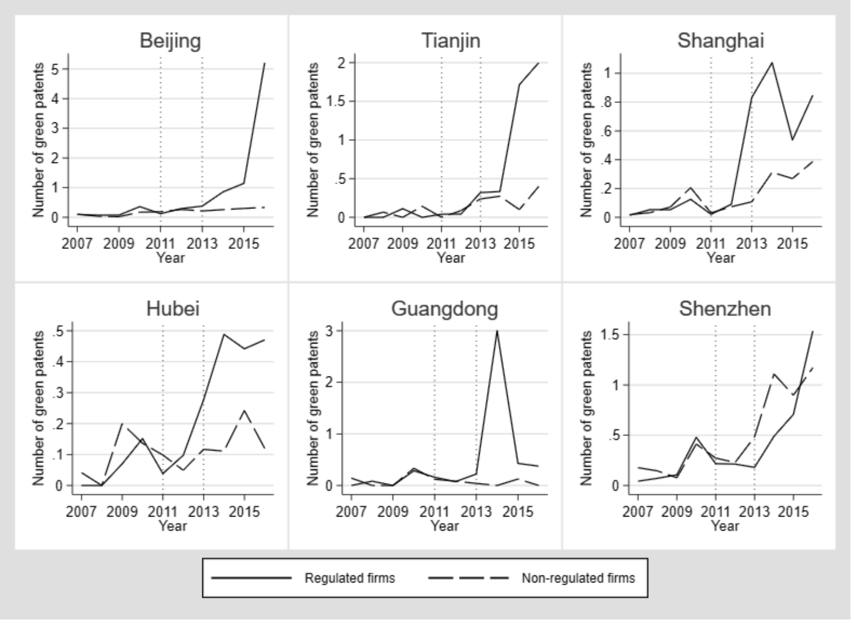
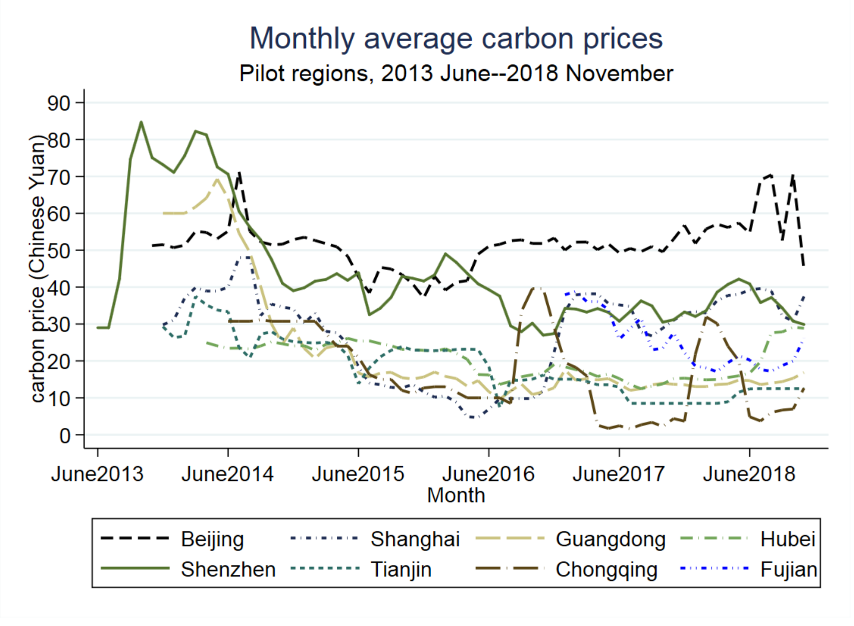



Figure 1.D.5: Monthly average carbon price in pilot regions



## 1.D.1 The Carbon Price Elasticity Using Different Leads

Table 1.D.1: Effect of pilot ETS on green patenting using matched sample, carbon price elasticity by pilot regions

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Green patents, weighted | | | | | |
| Logarithm carbon price T+1 | 0.19** | 0.40** | 0.41** | 0.11 | 0.09 |
| | (0.10) | (0.17) | (0.18) | (0.28) | (0.12) |
| regulated | 0.31 | 0.44 | -0.83** | -0.23 | 0.37 |
| | (0.25) | (0.38) | (0.37) | (0.49) | (0.26) |
| inflate | | | | | |
| Logarithm carbon price T+1 | 0.13 | 0.28 | 0.18 | 0.58 | 0.08 |
| | (0.09) | (0.19) | (0.15) | (0.45) | (0.11) |
| regulated | 0.14 | 0.52 | -0.79* | -2.04* | 0.29 |
| | (0.19) | (0.52) | (0.44) | (1.14) | (0.31) |
| Observations | 7829 | 1203 | 1638 | 1066 | 3121 |
| Mean dependent var. | 0.39 | 0.56 | 0.35 | 0.20 | 0.48 |
| Sd. of dependent var. | 3.56 | 7.17 | 1.94 | 0.81 | 3.10 |
| Pilot | Full sample | Beijing | Shanghai | Hubei | Shenzhen |
| log likelihood | -6433.02 | -1087.85 | -1176.05 | -474.91 | -2874.39 |
| AIC/N | 1.66 | 1.88 | 1.49 | 0.97 | 1.87 |

**Note:** This table reports maximum likelihood estimators using a zero-inflated Poisson model for the sample processed using matching. Columns 1–6 report the estimations on the carbon price elasticity on number of green patents by pilot regions using the carbon price with one-year ahead. Standard errors are clustered at 4-digit sector level, with 93, 29, 111, 88, 26, and 143 clusters respectively in columns 1–6. Specifications in all the columns include year fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.D.2: Effect of pilot ETS on green patenting using matched sample, carbon price elasticity by pilot regions

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Green patents, weighted | | | | | |
| Logarithm carbon price T+2 | 0.26 | 0.39** | 0.35* | 0.12 | 0.10 |
| | (0.16) | (0.17) | (0.20) | (0.25) | (0.12) |
| regulated | 0.15 | 0.46 | -0.65* | -0.30 | 0.37 |
| | (0.37) | (0.38) | (0.38) | (0.48) | (0.25) |
| inflate | | | | | |
| Logarithm carbon price T+2 | 0.14 | 0.26 | 0.13 | 0.61 | 0.08 |
| | (0.12) | (0.19) | (0.17) | (0.39) | (0.11) |
| regulated | 0.03 | 0.54 | -0.65 | -2.23** | 0.29 |
| | (0.24) | (0.52) | (0.46) | (1.12) | (0.30) |
| Observations | 7829 | 1203 | 1638 | 1066 | 3121 |
| Mean dependent var. | 0.39 | 0.56 | 0.35 | 0.20 | 0.48 |
| Sd. of dependent var. | 3.56 | 7.17 | 1.94 | 0.81 | 3.10 |
| Pilot | Full sample | Beijing | Shanghai | Hubei | Shenzhen |
| log likelihood | -6547.22 | -1088.12 | -1180.14 | -474.51 | -2874.44 |
| AIC/N | 1.68 | 1.88 | 1.49 | 0.97 | 1.87 |

**Note:** This table reports maximum likelihood estimators using a zero-inflated Poisson model for the sample processed using matching. Columns 1–4 report the estimations on the carbon price elasticity on number of green patents by pilot regions using the carbon price with two years ahead. Standard errors are clustered at 4-digit sector level, with 93, 111, 88, and 143 clusters respectively in columns 1–4. Specifications in all the columns include year fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.D.3: Effect of pilot ETS on green patenting using matched sample, carbon price elasticity by pilot regions

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Green patents, weighted | | | | | |
| Logarithm carbon price T+3 | 0.32 | 0.40** | 0.35* | 0.17 | 0.10 |
| | (0.23) | (0.17) | (0.19) | (0.21) | (0.12) |
| regulated | 0.02 | 0.38 | -0.70* | -0.44 | 0.39 |
| | (0.49) | (0.39) | (0.40) | (0.42) | (0.26) |
| inflate | | | | | |
| Logarithm carbon price T+3 | 0.20 | 0.27 | 0.18 | 0.64* | 0.09 |
| | (0.17) | (0.19) | (0.17) | (0.33) | (0.12) |
| regulated | -0.07 | 0.48 | -0.74 | -2.35** | 0.30 |
| | (0.31) | (0.53) | (0.48) | (0.99) | (0.30) |
| Observations | 7829 | 1203 | 1638 | 1066 | 3121 |
| Mean dependent var. | 0.39 | 0.56 | 0.35 | 0.20 | 0.48 |
| Sd. of dependent var. | 3.56 | 7.17 | 1.94 | 0.81 | 3.10 |
| Pilot | Full sample | Beijing | Shanghai | Hubei | Shenzhen |
| log likelihood | -6523.06 | -1085.94 | -1179.44 | -474.22 | -2874.81 |
| AIC/N | 1.68 | 1.88 | 1.49 | 0.97 | 1.87 |

**Note:** This table reports maximum likelihood estimators using a zero-inflated Poisson model for the sample processed using matching. Columns 1–6 report the estimations on the carbon price elasticity on number of green patents by pilot regions using the carbon price with three years ahead. Standard errors are clustered at 4-digit sector level, with 93, 29, 111, 88, 26, and 143 clusters respectively in columns 1–6. Specifications in all the columns include year fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

### 1.D.2 OLS Estimations

Table 1.D.4: Effect of pilot ETS on green patenting using matched sample, OLS estimations

| | (1) OLS | (2) OLS |
|---|---|---|
| treat | -0.12*** | -0.11*** |
| | (0.02) | (0.03) |
| outputvalue_verified | | 0.00** |
| | | (0.00) |
| *N* | 746 | 495 |
| Mean dependent var. | 0.97 | 0.94 |
| Sd. of dependent var. | 0.33 | 0.31 |
| R-squared | 0.03 | 0.05 |

**Note:** This table reports the OLS estimations for the sample processed using matching. Columns 1 and 9 show the overall effects of the regulation on green patenting; columns 2–5 show the the estimations on the carbon price elasticity on number of green patents, with different price leads; column 6 shows the results for estimating the pilot ETS effects by quartile of firms' output per worker distribution; columns 7 and 10 show the effects on dirty patenting; column 8 shows the effects on the share of green patents calculated as the ratio between the number of green patents and the sum of the numbers of green and dirty patents. Standard errors are clustered at 4-digit sector level, with 266 clusters in column 6 and 268 clusters in the rest of the columns. Specifications in columns 1–7 include year fixed effects and firm fixed effects; specifications in columns 8–10 include year fixed effects, the pilot region dummies, the ownership dummies and the firm size dummies. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

### 1.D.3 Fixed-Effect Poisson Estimations

Table 1.D.5: Effect of pilot ETS on green patenting using matched sample, fixed-effect Poisson estimations

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| main | | | | | | | | | | | |
| regulated*post | 0.49** | 0.28*** | 0.96** | 0.93 | 0.47 | -0.17 | -0.03 | 0.06 | | | 0.48 |
| | (0.23) | (0.09) | (0.44) | (0.77) | (0.35) | (0.49) | (0.65) | (0.33) | | | (0.35) |
| regulated | 0.20 | | | | | | | | | | |
| | (0.23) | | | | | | | | | | |
| Logarithm carbon price | | | | | | | | | 0.09 | | |
| | | | | | | | | | (0.06) | | |
| first quartile × regulated*post=1 | | | | | | | | | | 0.15 | |
| | | | | | | | | | | (0.40) | |
| second quartile × regulated*post=1 | | | | | | | | | | -0.06 | |
| | | | | | | | | | | (0.33) | |
| third quartile × regulated*post=1 | | | | | | | | | | -0.03 | |
| | | | | | | | | | | (0.30) | |
| fourth quartile × regulated*post=1 | | | | | | | | | | 0.60 | |
| | | | | | | | | | | (0.43) | |
| Observations | 7829 | 3882 | 586 | 157 | 827 | 551 | 174 | 1587 | 3882 | 3882 | 1584 |
| Mean dependent var. | 0.39 | 0.80 | 1.15 | 0.46 | 0.71 | 0.38 | 0.32 | 0.94 | 0.80 | 0.80 | 0.50 |
| Sd. of dependent var. | 3.56 | 5.03 | 10.25 | 1.46 | 2.68 | 1.09 | 0.75 | 4.30 | 5.03 | 5.03 | 4.15 |
| Pilot | | | Beijing | Tianjin | Shanghai | Hubei | Guangdong | Shenzhen | | | |
| Pseudo R-squared | 0.17 | | | | | | | | | | |
| log likelihood | -8240.01 | -3068.36 | -532.83 | -68.50 | -576.16 | -261.40 | -67.76 | -1384.34 | -3066.96 | -3040.07 | -842.06 |
| AIC/N | | | | | | | | | | | |

**Note:** This table presents estimations from the Poisson regression with firm fixed effects using the matched sample. Column 1 shows the results for estimating the overall ETS effects without firm fixed effects, while column 2 shows the effects with firm fixed effects included. Columns 3–8 show the results for estimating the pilot heterogeneity effects using sub-samples by pilot regions–that is, the effects of regulation in different municipalities or provinces with the implementation of emissions trading scheme (ETS). Standard errors are clustered at the 4-digit sector level in column 1, with 268 clusters. Robust standard errors are reported in columns 2–10. Specifications in all the columns include year fixed effects. Specifications in column 1 include the ownership dummies, the pilot region dummies and the firm size dummies. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

## 1.D.4 Estimations Using the Non-Matched Sample

Table 1.D.6: Effect of pilot ETS on green patenting using non-matched sample, count data model

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| main | | | | | | | | | |
| regulated*post | 0.65 | | | | | | -0.00 | -0.02 | 0.54 |
| | (0.47) | | | | | | (0.01) | (0.02) | (0.34) |
| regulated | 0.42 | 0.26 | 0.24 | 0.18 | 0.13 | 0.02 | | | 0.59* |
| | (0.27) | (0.27) | (0.28) | (0.30) | (0.32) | (0.49) | | | (0.35) |
| Logarithm carbon price | | 0.24 | | | | | | | |
| | | (0.15) | | | | | | | |
| Logarithm carbon price T+1 | | | 0.25 | | | | | | |
| | | | (0.16) | | | | | | |
| Logarithm carbon price T+2 | | | | 0.28* | | | | | |
| | | | | (0.17) | | | | | |
| Logarithm carbon price T+3 | | | | | 0.29* | | | | |
| | | | | | (0.17) | | | | |
| first quartile × regulated*post=1 | | | | | | 0.93* | | | |
| | | | | | | (0.48) | | | |
| second quartile × regulated*post=1 | | | | | | -0.03 | | | |
| | | | | | | (0.21) | | | |
| third quartile × regulated*post=1 | | | | | | 0.26 | | | |
| | | | | | | (0.19) | | | |
| fourth quartile × regulated*post=1 | | | | | | -0.03 | | | |
| | | | | | | (0.34) | | | |
| inflate | | | | | | | | | |
| regulated*post | 0.27*** | | | | | | | | 0.43** |
| | (0.09) | | | | | | | | (0.20) |
| regulated | -0.09 | -0.13 | -0.14 | -0.16 | -0.18* | -0.01 | | | -0.47* |
| | (0.13) | (0.12) | (0.11) | (0.11) | (0.11) | (0.20) | | | (0.26) |
| Logarithm carbon price | | 0.09*** | | | | | | | |
| | | (0.03) | | | | | | | |
| Logarithm carbon price T+1 | | | 0.09*** | | | | | | |
| | | | (0.03) | | | | | | |
| Logarithm carbon price T+2 | | | | 0.10*** | | | | | |
| | | | | (0.03) | | | | | |
| Logarithm carbon price T+3 | | | | | 0.11*** | | | | |
| | | | | | (0.04) | | | | |
| first quartile × regulated*post=1 | | | | | | 0.17 | | | |
| | | | | | | (0.13) | | | |
| second quartile × regulated*post=1 | | | | | | 0.40* | | | |
| | | | | | | (0.23) | | | |
| third quartile × regulated*post=1 | | | | | | 0.41** | | | |
| | | | | | | (0.20) | | | |
| fourth quartile × regulated*post=1 | | | | | | 0.17 | | | |
| | | | | | | (0.15) | | | |
| Observations | 83050 | 83050 | 83050 | 83050 | 83050 | 83046 | 82686 | 9202 | 83050 |
| Mean dependent var. | 0.55 | 0.55 | 0.55 | 0.55 | 0.55 | 0.55 | 0.12 | 0.82 | 0.07 |
| Sd. of dependent var. | 13.97 | 13.97 | 13.97 | 13.97 | 13.97 | 13.97 | 0.32 | 0.36 | 1.04 |
| R-squared | | | | | | | 0.33 | 0.65 | |
| log likelihood | -103555.85 | -103177.58 | -103106.69 | -102912.47 | -102772.60 | -98399.51 | | | -17017.89 |
| AIC/N | 2.50 | 2.49 | 2.48 | 2.48 | 2.48 | 2.37 | | | 0.41 |

**Note:** This table reports maximum likelihood estimators using a zero-inflated Poisson model for the sample of all firms locating in the six pilot regions processed without matching. Columns 1–6 and 9 show the results from the zero-inflated Poisson regression, with the outcome variables as the green patent counts in columns 1–6 and the dirty patent counts in column 9; columns 7 and 8 show the results from OLS regression with the outcome variable as the share of the green patent counts. Column 1 shows the overall effect of the regulation on green patenting; columns 2–5 show the estimations of the carbon price elasticity on number of green patents, with different price leads; column 6 shows the results for estimating the pilot ETS effects by quartile of firms' output per worker distribution; columns 7 and 8 present the estimations of the ETS effects on the share of green patenting; Column 9 shows the estimations on the ETS effects on dirty patenting. Standard errors are clustered at 4-digit sector level, with 330 clusters in column 8 and from 532 to 536 clusters in the other columns. Specifications in all the columns include year fixed effects. * p < 0.1, ** p < 0.05, *** p < 0.01.

# Chapter 2

# Heterogeneous Responses to Carbon Pricing: Firm-Level Evidence from Beijing Emissions Trading Scheme


**Abstract**

Using a fuzzy regression discontinuity design on a unique emissions and allowances dataset for firms participating in Beijing's emissions trading scheme (ETS), we study firm behaviour facing a carbon price. We find that on average, the ETS reduces carbon emissions by 39%. Firm responses vary: emissions are reduced by about 45% (mainly by reducing coal use) in the industrial sector but hardly change in the service sector. By exploring the effects of allowance allocation on emissions reduction, we further find that free allowances may dampen firms' abatement, although only for small firms or firms in the service sector. An additional ton of allowances is associated with about 0.14 to 0.4 ton of additional emissions, which could partially explain the null effects of carbon pricing on service firms.



This chapter is joint work with Da Zhang (Tsinghua University), Xiliang Zhang (Tsinghua University), and Thomas Sterner(University of Gothenburg).

## 2.1 Introduction

The last decade has seen heightened interest in pricing carbon emissions. On the one hand, many countries have carbon taxes, while on the other, large-scale cap-and-trade schemes for $CO_2$ have been implemented (Stavins, 2020). A number of countries have joined the European Union in adopting an emissions trading scheme (ETS) as a policy instrument to price carbon. As the world's largest carbon emitter, China launched ETS pilots in 2013 in seven provinces and cities, covering 10% of its carbon emissions.

While there is near unanimity among economists that some type of carbon pricing is needed (see, for instance, Weitzman 2014; Nordhaus 2015; Beccherle and Tirole 2011), there is less consensus about whether taxation or ETS should be used, and debate is ongoing about the exact details of how to implement either of these instruments. Outside of economics, many policy analysts even fail to appreciate the importance of pricing. In fact, there is much opposition to the idea of emissions pricing, and the Paris Agreement does not give pricing a central role. Therefore, accurate assessment of the effectiveness of carbon pricing practice, such as the permit trading system, is a central issue for this debate.

In this paper, we study the mitigation effects of the permit trading system in China's Beijing pilot ETS and then evaluate whether different levels of permit allocation–and thus the surplus or deficit that firms have–will affect firms' behaviour in the permit market. It is often assumed that in cap-and-trade systems, the government can establish the overall mitigation goal by setting the cap without affecting the behaviour of firms through allocation decisions (Coase, 1960; Hahn and Stavins, 2011). This 'independence criterion' means that emissions reduction is achieved in a cost-effective manner. The Beijing ETS is of particular interest because it is the pilot region with the highest carbon prices. We compile a unique entity-level dataset of the firms covered in Beijing from 2009 to 2017 and assess whether the initial allocations of allowances affect equilibrium emissions.

In a first analysis we check whether the ETS in Beijing significantly reduces firms' emissions. One must understand, however, that this is likely as the development of the aggregate cap for Beijing is falling and the number of firms increases. Firms in Beijing would be covered by the regulation if their carbon emissions in 2012, the year before the implementation of the regulation, exceeded $10,000$ tons. The ideal identification would be to compare emissions of two groups of firms that are similar in all aspects except for their treatment status. Alternatively, comparing emissions of firms that are randomly assigned to treatment and control groups also would give the causal impacts of the policy, but this is infeasible in reality. Often, treated firms are larger and more polluting. A simple comparison of emissions between treated and control firms would lead to a biased estimation of the policy impacts.

The role of the threshold emissions of the regulation (hereafter referred to as the cutoff) motivates us to use a fuzzy regression discontinuity design to identify the causal effect. We compare firms that have emissions close to the cutoff and therefore are as similar as possible in characteristics that may affect their emissions. One group of firms has carbon emissions higher than the cutoff and hence the probability of their being pilot firms is higher, while another group of firms has carbon emissions lower than the cutoff, and hence the probability of their being pilot firms is much lower. The intuition is that firms with carbon emissions close to the cutoff should not be systematically different, and therefore by comparing the emissions of firms below and above the cutoff, we can identify the regulation effect. In this study, we mainly focus on the regulation effects of the first phase. Firms that are part of the pilot in the first phase are in the treatment group, and firms that are not are in the control group. We rely on a fuzzy regression discontinuity design (RDD) instead of a sharp one, as there are some other random determinants of the regulatory status, such as administrative error in reporting and recording firms' historical emissions in the pre-regulation period. However, there is no evidence that firms can systematically self-select into nontreatment when their emissions are above the cutoff.

We find that emissions in 2015 were reduced by 39% but the effect is not statistically significantly different from zero. It turns out that effects vary strongly by sector. Firms in the industrial sector significantly reduced carbon emissions in 2015 by 45%. This result was mainly driven by the coal-using industrial firms at the extensive margin: the ETS reduces the likelihood of consuming coal by about 50% at no cost of any significant output reduction. Such a reduction in coal consumption might be due to several factors, such as fuel switching or improvement of production efficiency, but none of the factors play a significant role in explaining the coal consumption reduction.

Next, we look at allowance allocations to tackle the question of whether initial allowances affected firm emissions.[1] At least half the firms had an allowance surplus between 2013 and 2017; the rest had a shortage and had to either purchase allowances from the permit trading market or reduce emissions to comply with the regulation. Firms with a surplus could bank the permits or sell them on the carbon market.

[1] In the Beijing ETS, all emissions allowances were allocated free of charge to covered firms, according to their average emissions or historical output between 2009 and 2012.

Standard theory suggests that the market outcome is independent of the initial allocation because the latter does not affect firms' marginal abatement cost. However under some conditions there might still be an effect: see, for instance, the seminal works by Coase (1960) and Hahn and Stavins (2011). Overall, we find that allowance surplus has no significant impact on emissions, although we observe some such effects for small and medium-sized firms as well as firms in the service sector.

The main empirical challenge in this estimation is that of simultaneity: a shock affecting firms' allowance surplus also affects emissions. We address this concern using an instrumental variable (IV) estimation. Specifically, we instrument an endogenous allowance surplus variable on past emissions shocks that affect firms' future allowance surplus and are therefore highly predictive of firms' future allowance surplus but, as we document, are uncorrelated with future emissions shocks and are therefore exogenous to firms' emissions.

Our paper contributes to the literature in three primary ways. First, we add to the general debate on the effectiveness of the emissions trading scheme. To date, the causal evidence on the effectiveness of an ETS directly using firm-level carbon emissions as a measure is still limited. Anderson and Di Maria (2011) use a dynamic panel data model to construct the business-as-usual emissions at industry level and then compare the counterfactual emissions with allocated and verified emissions. They find that in the first phase of the EU ETS both abatement and overallocation occurred. Using country-sector level data, Rafaty et al. (2020) estimate the impact of carbon pricing on carbon emissions and find significant impacts on electricity and heating as well as road transportation sectors, but not on commercial and residential buildings, and the manufacturing sector. Adopting difference-in-differences on the data on German manufacturing firms, **?** study the effect of the EU ETS on carbon emissions of German manufacturing firms and conclude that the EU ETS did not reduce carbon emissions in significant ways in the first phase; however, it did reduce carbon emissions significantly in the second phase. Colmer et al. (2020) use French manufacturing data and find similar results. In this paper, we look at the regulation effect on the population of pilot firms, including firms in both the manufacturing industry and other sectors.

Second, our study complements previous studies by directly estimating the effect on emissions reduction by sector and by energy source. We exploit a special policy setting that allows us to use an RDD, which enables a stronger internal validity of estimations than with the matched difference-in-differences estimation, which has been broadly used in the existing literature to study the ETS impacts. Moreover, previous studies mostly focus on firms in the United States or the EU, whereas this study provides new evidence in a transitional economy. Like the economies of several other middle-income countries, China's growing quickly, which creates a number of special challenges and opportunities. The Chinese economy is also still in transition from a planned socialist economy to a capitalist one. Planning continues to be important in the Chinese economy, and many of China's firms are very large and have considerable market power. They are also former state-owned companies and maintain close ties to the government giving them considerable political power. In short, they are not the ideal 'atomistic' firms, and it is questionable they will respond to market signals such as a price on carbon. We know that monopolies, or more generally companies with market and political power that have an ability for strategic planning, react differently from the textbook example of a firm. This is often referred to as 'soft budgets', a term coined by Kornai (1979) when analyzing the planned economies of Eastern Europe. In a context where the state gave the companies the budget they needed, the introduction of a new instrument (in this case, the ETS) would have no effect if the regulated companies expected to be compensated through other interactions with the state (for example, getting free permits or reductions in other taxes in proportion to increased costs).

Finally, the discussion on whether and how free initial allowances affect firms' emissions broadly relates to the growing literature that discusses whether the independence property holds in a cap-and-trade market. Hahn and Stavins (2011) examine factors that may impede independence property, such as transaction cost, market power, and uncertainty. A large body of literature discusses how transaction cost prevents firms from actively participating in the pollution trading market. Stavins (1995) concludes that the cost-effectiveness of market-based environmental policies, such as tradeable pollution permits, has often been exaggerated because of an underestimation of the nontrivial transaction cost firms may incur. Notably, this issue is more salient among smaller firms. For instance, Baudry et al. (2020) provide descriptive evidence that in the EU ETS, the firms that have excess permits are generally small and have low emissions. Typically, it may not be worth their time and effort to reduce emissions because of the transaction cost. Sandoff and Schaad (2009) survey ETS firms in Sweden and suggest that formulating trading strategies is low priority, especially for small firms that do not have specialised environment departments (see also Jaraitė-Kažukauskė and Kažukauskas (2015)). Naegele (2018) concludes that fixed transaction costs make the trading of international carbon offsets unprofitable for smaller firms. To the best of our knowledge, there is only one paper estimating the causal relationship between firms' initial allowances and their subsequent emissions. Exploring an exogenous variation in the timing of firm's permit allocations in the Southern California's Regional Clean Air Incentives Market, Fowlie and Perloff (2013) study the relationship between firms' initial allowances and emissions and find that such a property likely holds in that market.

We take advantage of the past shocks to emissions which are exogenous to firms' future emissions and propose a general test of the independence property in cap-and-trade markets. Our results are consistent with the previous literature: the independence property is unlikely to hold for smaller firms or firms in the service sector, which face nontrivial transaction costs. Because we do not directly observe firms' trading behaviour or transaction costs, we cannot draw any causal link between transaction costs and firms' emissions. Instead, we identify another unintended consequence of generously allocating allowances: on average, smaller firms are more likely to increase emissions, or less likely to reduce emissions. A potential explanation might be that the transaction costs are relatively larger for them.

The remainder of the paper proceeds as follows. Section 2.2 describes our data. This section also describes the setting of the Beijing ETS, which is relevant for our empirical estimations. Section 2.3 presents the empirical strategy that estimates the mitigation effects of the ETS in Beijing and the related estimation results. Section 2.4 outlines the IV model in identifying the effects of allowance surplus on firms' emissions, discusses the validity of our instrument and presents the related results. Section 2.5 concludes.

## 2.2 Data and Background

### 2.2.1 The Pilot Emissions Trading Scheme in Beijing

**The cap.** The total cap remained about the same from 2013 to 2015, while the number of regulated entities increased by a quarter during this period and then more than doubled in Phase II. If the aggregate emissions went down by 1%, then clearly the average had to go down by at least 1% if the number of firms did not change. With the coverage expansion and the increase in the number of regulated entities increased by a quarter over Phase I, the average emissions could have gone down by as much as 25%. Either way some firms may have increased while others decreased their emissions, and the purpose of this paper is to study this heterogeneity in behaviour.

**Allowance allocation rule.** A firm's allowance allocation was decreasing over years and was determined ex ante by an emissions reduction factor. The rules of allocating allowances were different across sectors. For instance, for heating companies and thermal power companies, allowances were allocated based on individually adjusted benchmarking; for firms in industries other than heating and thermal power, allowances were allocated based on grandfathering yearly average emissions between 2009 and 2012.

**Timing and coverage threshold.** On November 20, 2013, Beijing Municipal Development and Reform Commission (DRC), the regulatory agency for $CO_2$ emissions, announced that entities with carbon emissions higher than 10,000 tons in 2012 were to be regulated in the scheme. The first compliance event would be in June 2014, meaning that firms would receive allowances for 2013 and surrender the allowances for compliance in 2014. They had almost no time to mitigate emissions in 2013, so we expect that the policy effect in 2013 was limited. On December 16, 2015, this threshold was adjusted to target those with emissions higher than or equal to 5,000 tons in 2015.[2]

**Cost of noncompliance.** If firms did not comply, with emissions above the cap, they were fined for excess emissions at three to five times the average allowance price for the previous year. The compliance rates in Beijing from 2013 to 2017 were 97%, 100%, 99%, 100%, and 100%, respectively.

**Transaction costs.** Firms purchasing or selling emissions allowances from or to other firms in the carbon market would have to pay a transaction fee proportional to their trading value. There was no account registration fee or annual fee for pilot firms. Parties that were not covered by the ETS but wished to trade in the carbon market (nonpilot firms) had to pay a registration fee of 50,000 CNY (about 7,500 USD) and an annual fee of 30,000 CNY (about 4,500 USD). The transaction fee was the same for both pilot and nonpilot firms: for every transaction at the exchange, both buyers and sellers had to pay a variable cost of 0.75% (10 CNY at minimum, about 1.5 USD); for an agreement-based private transaction, both buyers and sellers paid a variable cost of 0.5% (1,000 CNY at minimum, about 150 USD).

**Market linkages.** Pilot markets were not linked. Due to differences in total allowable emissions, coverage thresholds, and sectors subject to the ETS, equilibrium prices for the emissions allowances differed across the eight pilot regions. The average allowance price ranged from 24 CNY (3.6 USD) in the Hubei pilot to 51 CNY (7.7 USD) in the Beijing pilot between 2013 and 2015 (Beijing pilot's first

[2] This is the official announcement language. However there were nuances when it came to implementation. First of all, carbon emissions were based on coal equivalent consumption of different energies rather than actual emissions. Second, there might have been errors due to special circumstances, such as missing reports.

phase), and from 15 CNY (2.3 USD) in the Tianjin pilot to 61 CNY (9.2 USD) in the Beijing pilot to between 2016 and 2020 (the Beijing pilot's second phase).

### 2.2.2 Data

The analysis is based on the dataset that provides firm-level information on annual carbon emissions. These data come from the DRC and span from 2009 to 2017. This balanced panel dataset covers 971 firms and comprises the population of pilot firms in Phases I and II.

The dataset contains a wide range of variables on energy consumption and emissions for different types of energy, such as coal, natural gas, oil and electricity, as well as the allowances each firm received over the regulation period.

Our baseline sample consists of 741 firms, with 366 nonpilot firms and 375 pilot firms in Phase I. Pilot firms are those that were regulated from 2013. That is, we exclude those that are regulated from 2014 or 2015 in the baseline estimation for a more comparable treatment group.[3] Furthermore, to minimise the influence of outliers in the estimations of the regulation effect, we cut off the top and bottom 2.5% of the distribution of the overall growth rate of $CO_2$ between 2012 and 2015.[4]

Table 2.1 presents summary statistics for the baseline sample on variables of energy consumption as well as verified $CO_2$ emissions. Panel A reports summary statistics for 366 nonpilot firms in Phase I in our baseline sample in 2012 and 2015, and panel B for 375 pilot firms. Comparing panels A and B, we see that pilot firms on average emitted significantly larger amounts of $CO_2$ and consumed more energy in both 2012 and 2015. Then, comparing the emissions and energy consumption in 2012 and 2015 for two groups of firms, we see that the emissions and coal consumption decreased considerably more for pilot firms, while their natural gas consumption increased greatly. We do not see such a pattern with similar magnitude for nonpilot firms.

However, panels A and B do not make for a good comparison because of the different firm (and emissions) sizes. Instead, we focus our analysis on the firms that are close to the cutoff of 10 ktons and thus are more similar to each other. Figure 2.1 presents the scatter plots on emissions in 2015 of firms just above and below the 2012 emissions threshold, by sectors and ownership types, as well as whether firms in each sector are heavy coal or oil users in 2012 and therefore have

[3]There are 139 such firms. However, results are robust to including these firms in the treatment group, as we show in Table 2.A.9 in Appendix 2.A.

[4]The emissions of outliers in 2015 either dropped to thousandths of the emissions in 2012 or increase by as large as two thousand times of emissions in 2012.

Table 2.1: Summary statistics, baseline sample

| | (1) 2012 | | (2) 2015 | |
|---|---|---|---|---|
| | Mean | S.d. | Mean | S.d. |
| Panel A: Control group | | | | |
| Total emissions (kton) | 10.3 | 10.4 | 10.1 | 9.6 |
| Total energy consumption (ktce) | 6.1 | 5.2 | 6.1 | 5.2 |
| Coal consumption (ktce) | 1.3 | 4.4 | 0.8 | 3.3 |
| Oil consumption (ktce) | 0.1 | 0.2 | 0.0 | 0.2 |
| Natural gas consumption (ktce) | 0.9 | 1.4 | 1.1 | 1.8 |
| Electricity consumption (ktce) | 3.8 | 3.6 | 4.1 | 4.1 |
| Panel B: Treatment group | | | | |
| Total emissions (kton) | 130.1 | 530.1 | 112.2 | 467.8 |
| Total energy consumption (ktce) | 61.5 | 236.2 | 56.6 | 224.7 |
| Coal consumption (ktce) | 19.9 | 132.9 | 10.1 | 80.0 |
| Oil consumption (ktce) | 4.4 | 60.9 | 3.5 | 49.1 |
| Natural gas consumption (ktce) | 14.8 | 77.8 | 19.4 | 107.7 |
| Electricity consumption (ktce) | 22.3 | 122.9 | 23.7 | 134.9 |
| Observations | 741 | | 741 | |

**Note:** Means and standard errors for each variable. The first two columns present summary statistics for variables in 2012, and the other two for 2015. Panel A is for firms in the control group, and panel B for the treatment group. Firms' total emissions are calculated as the sum of emissions from coal, oil, and natural gas consumption. In addition, emissions from electricity consumption are counted according to accounting rules in China. All energy consumptions are in kilotons coal equivalent (ktce) transformed based on their standard conversion factors. The variable emissions are given in kilotons (kton) $CO_2$.

large potential for fuel switching.[5] With only 76 firms within the bandwidth in the industry sector, of which 45 were heavy coal or oil users in 2012 and 25 were state-related firms[6], we see a discontinuity of emissions in 2015 at the cutoff (panels b, d, and f). With 95 firms relying more heavily on coal or oil consumption, we see a less clear discontinuity at the cutoff (panel c). We do not see such a pattern for firms in service sector (panel a) or state-related firms in general (panel e).

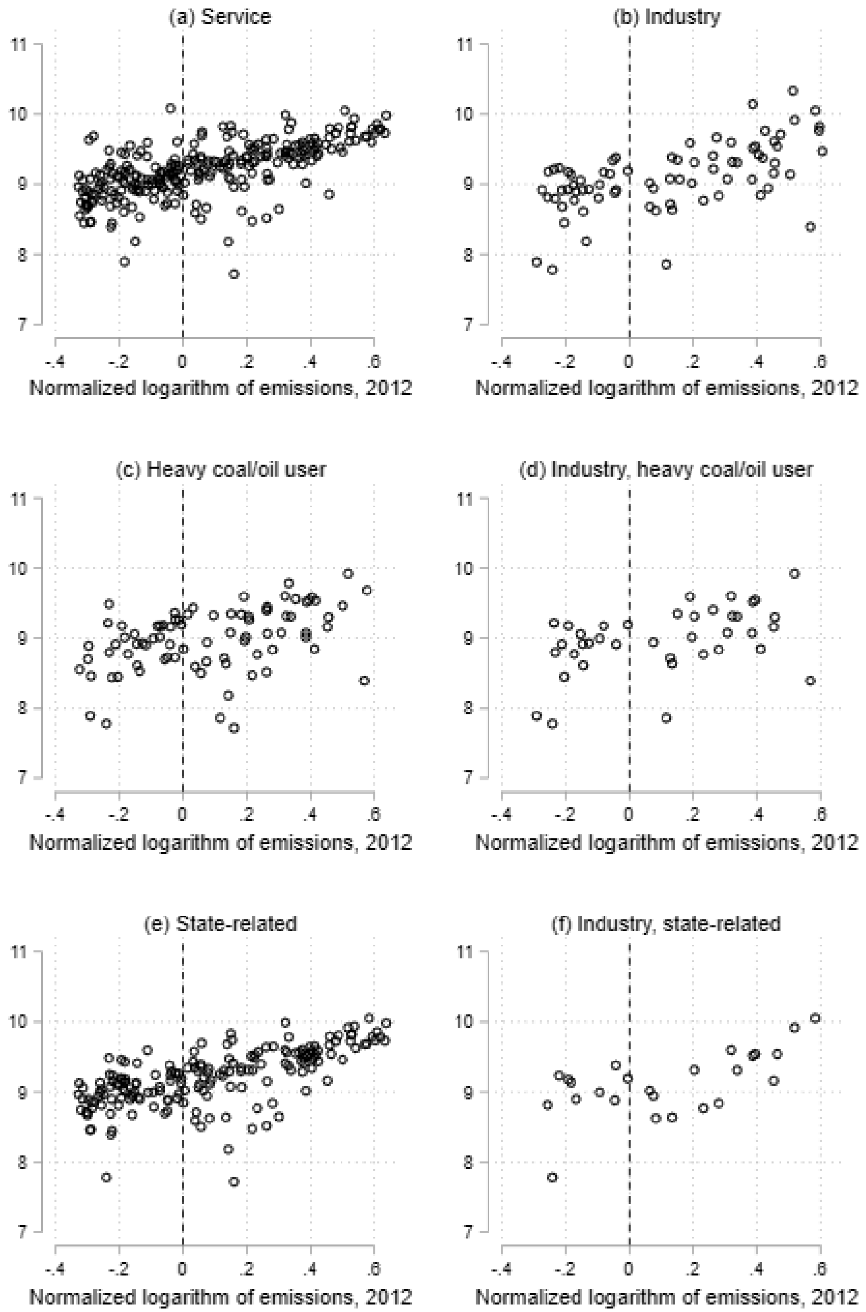
## 2.3 The Impact of the ETS on Emissions Reduction

In this section, we discuss the empirical strategy in estimating the ETS effects in Beijing and then present the estimation results. We first estimate the effect on emissions and then explore some potential explanatory factors by looking at fuel switching.

[5] Bandwidth selection around the cutoff and our definition of heavy coal or oil user are discussed in Section 2.3.

[6] State-related firms or entities include state-owned firms, government agencies, and public institutions; non-state-related firms include private firms, joint ventures and foreign firms.

Figure 2.1: Emissions in 2015 by sector and whether consuming coal in 2012, in logarithm, RD sample



### 2.3.1 Baseline Estimations

To identify the causal effect of the ETS, we rely on a fuzzy regression discontinuity design (RDD), as the allocation into the ETS scheme is not perfectly aligned with the threshold of $10,000$ tons, due, perhaps, to administrative errors in recording emissions data (Figure 2.2). Prior to the pilot, the government did not have full knowledge about firms' emissions because there were no statistics for $CO_2$ emissions at the firm level. Therefore, the government created a list of firms that had emissions higher than $10,000$ tons with a very high probability, according to their energy use or output statistics. Therefore, the government verified a firm's emissions only when it decided to include the firm in the pilot, so all the verified firms were covered by the ETS. This process obviously created some type I (a few) and type II (many) errors. In preparation for the second phase, the cutoff was decreased to $5,000$ tons $CO_2$ in 2015, and the same process was repeated, with only emissions data of firms that the government believed had emissions higher than $5,000$ tons being collected and verified. In short, the government only knew firms' exact emissions ex post.

In our setting, the running variable is the $CO_2$ emissions in 2012, and the outcome variable is the emissions in 2015 (both in logarithms). The treatment effect of the regulation is therefore estimated by a two-stage least squares (2SLS) estimator.

$$\begin{aligned} log(Y_{2015,i}) &= \gamma_0 + \gamma_1 \hat{treat}_i + g(Y_{2012,i}) \times \beta + o_i + s_i + e_i + w_i, \\ treat_i &= \alpha_0 + \alpha_1 T_i + g(Y_{2012,i}) \times b + o_i + s_i + e_i + u_i. \end{aligned} \tag{2.1}$$

In this specification, $treat_i = 1$ if firm $i$ is regulated by the ETS since 2013 and 0 otherwise. The dummy variable $T_i$ equals 1 if emissions for firm $i$ in 2012 were larger than $10,000$ tons. Following the argument in Gelman and Imbens (2019) that third- or higher-degree polynomials of the forcing variable in $g(Y_{2012,i})$ can lead to noisy estimates that are sensitive to the degree of the polynomial, we use a specification allowing for different linear slopes above and below the $10,000$ tons threshold in the baseline regression–that is, $g(Y_{2012,i}) = log(Y_{2012,i}) - c + [log(Y_{2012,i}) - c] \times T_i$, where $c$ is the cutoff, i.e., equal to $10,000$ tons $CO_2$.[7] Ownership dummies $o_i$ are intended to capture any differences in emissions depending on ownership type; sector dummies $s_i$ capture differences in emissions among sectors. Firms with different energy consumption sources before the implementation of the ETS might have different emissions. For instance, firms that consumed

[7]We provide alternative estimations using a quadratic polynomial in Table 2.A.10 in Appendix 2.A. Estimation results are qualitatively similar with less precision.

only coal and oil in 2012 may have larger mitigation potential than firms consuming only electricity. To capture this type of systematic difference, we include energy type dummies $e_i$, capturing whether firms consumed coal, oil, natural gas or electricity in 2012.[8] Following the suggestion in Imbens and Lemieux (2008), we use robust standard errors in all the regressions.

Using a weighted local linear regression, we estimate the probability of treatment in the first stage, giving higher weights to the firms closer to the cutoff; then in the second stage, we estimate the effect of the regulation on emissions in 2015. A key identifying assumption in estimating the causal effect of the ETS is that firms around the cutoff could not systematically choose their treatment status. There is little evidence that firms could anticipate whether they would be regulated. The coverage threshold was announced in 2013, stating that the regulatory status would be determined by the emissions in 2012. Therefore, it would have been difficult for firms to behave strategically in 2013 and select into the nontreated group. We use a McCrary density test to further validate this argument. The p-value of the test is 0.92, and we therefore accept the null hypothesis that the density of the running variable emissions in 2012 is continuous at the discontinuity point $10,000$ tons.[9] (See Figure 2.A.1 in Appendix 2.A.)

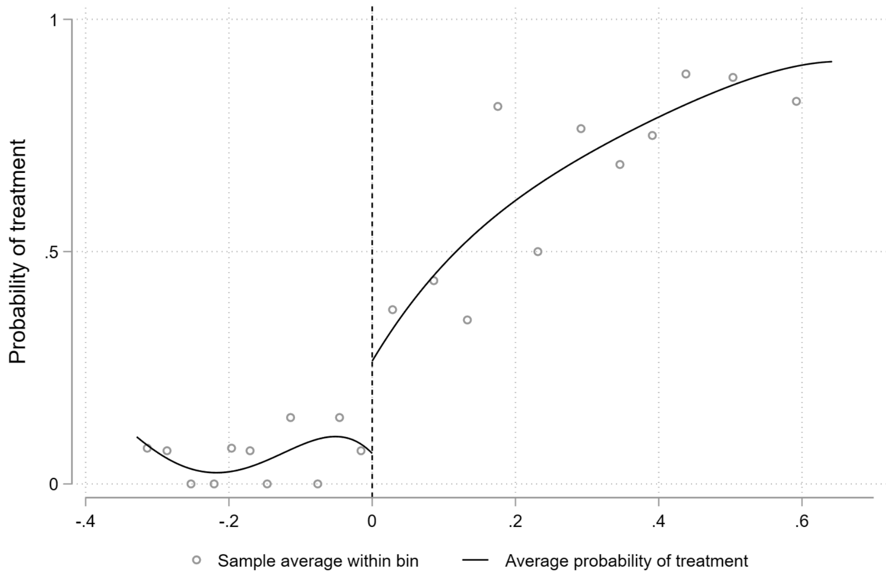
Table 2.2 presents the results from estimating equation (2.1) on the observations with emissions in 2012 between $log(10,000) - \delta_1$ and $log(10,000) + \delta_2$, where $\delta_1$ and $\delta_2$ denote the lower- and upper-bounds of the optimal bandwidth.[10] On average, the trading scheme reduced firms' emissions in 2015 by 39%. We

[8]It is worth highlighting that firms that consumed only coal were likely in different four-digit sectors than firms that consumed both coal and oil. For instance, comparing two firms in the service sector, one that consumed oil could have been a property management company, and one that had no such consumption could have been a university. While we do not claim this was a general rule among all the firms, the inclusion of the dummies reflects such a possible pattern and therefore controls for the potential differences among firms that relied on different sources of energies. While we cannot control for the difference by including four-digit sector dummies because this information is unavailable, we include dummies for firms using different energy sources in the pre-treatment period to control for their systematic differences.

[9]Zhang et al. (2019) compare firms' self-reported emissions and emissions verified by a third party in two of the pilot regions in China, Beijing and Hubei. They observe no evidence of deliberate misreporting in the two pilots. This provides reassuring evidence that firms did not manipulate around the cutoff.

[10]In all baseline regressions, we use the bandwidth that minimises the mean squared error (MSE) of the point estimator and allow for different MSE-optimal bandwidth selectors below and above the cutoff. We provide robustness tests on estimations using different bandwidths, including the MSE–optimal and coverage error rate (CER)–optimal bandwidth choices, as developed by Calonico et al. (2014) and Calonico et al. (2018) (see in Table 2.A.2 in Appendix 2.A). We show that the estimations are robust to different bandwidth selectors; however, they are less precisely estimated because of narrower alternative bandwidths. All models include the full set of sector, ownership, and energy type dummies. Table 2.A.1 in Appendix 2.A presents OLS estimation using full sample (column 1) as well as 2SLS estimations using full sample (column 2) and different subsamples (columns 3–10). Not surprisingly, treated firms had significantly larger emissions in 2015 than the non-treated firms because firms that are further away from the threshold do not make a good comparison.

Figure 2.2: Emissions in 2012 and the probability of treatment




turn our attention to different groups of entities–industrial firms versus those in the service sector and state-related versus non–state-related firms–and we also single out heavy coal or oil users, potential fuel switchers. Compared with firms that consumed relatively less carbon-intensive energy, such as natural gas and electricity, in the pre-treatment period, firms that consumed more coal and oil before 2013 should have had greater potential to reduce emissions and therefore might have experienced larger emissions reductions. To test this hypothesis, we estimate the policy impacts on firms that were more polluting and thus were potentially more likely to reduce their emissions to a relatively large degree. Specifically, a firm was a potential fuel switcher if its oil and coal consumption accounted for more than 20% of its total energy consumption.[11]

Columns 2 and 3 in Table 2.2 compare the estimations of the policy impacts on the two groups of firms whose consumption of coal and oil accounted for more than or less than 20% of firms' total energy consumption in 2012. On average,

[11]We choose the threshold of 20% based on the density distribution of the share of coal and oil in 2012. The density plot of the share in Figure 2.A.2 in Appendix 2.A shows that firms either relied heavily (more than 75%) on coal and oil consumption in 2012 or relied on these fuels to a very low extent (less than 20%). The estimations are robust to varying cutoffs of oil and coal consumption for potential fuel switchers. We estimate the policy impacts on emissions reductions using subsamples of firms whose oil and coal consumption accounted for more than or less than 5, 10, 15, 20, 25, ..., 60, 65, 70% of their total energy consumption, respectively. Results are presented in Tables 2.A.11 and 2.A.12 in Appendix 2.A.

the ETS reduced heavy coal or oil users' emissions by 54.2% (column 2), and the effect on the rest of the firms is not significantly different from zero (column 3). Intuitively, the effect on firms in industry sector was significant, and the emissions were decreased by 45.7% (column 4), but there was no such effect on firms in the service sector (column 5). This might be due to a lack of the possibility of fuel switching for firms in service sector.[12] Columns 6–9 compare the policy impacts by sector and whether firms were state-related firms. On average, the ETS reduced emissions of state-related firms in the industry sector by 53.3% (column 8) and of firms in the service sector by 16%, although not significantly different from zero (column 9). Interestingly, having reached the regulatory threshold has little explanatory power on the non-state-related firms, as indicated by weak first-stage estimations in columns 6 and 7. This is due to a relatively low share of compliers–that is, firms (not) reaching the threshold and (not) regulated.

Most importantly, among the firms reaching the threshold, there were about the same number of never-takers–that is, firms reaching the threshold but not regulated–as compliers, with most of the never-takers centering around the regulatory threshold. This drives the first-stage estimation downward and leads to weaker explanatory power of reaching the threshold on the treatment status. Panels c and d in Figure 2.A.3 in Appendix 2.A explain such a pattern.

Table 2.2: Effect of the ETS in Beijing on carbon emissions, linear, triangular kernel

| | All | Heavy coal/oil users | | Sector | | Non state-related | | State-related | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Treat | -0.50* | -0.78** | -0.26 | -0.61** | -0.36 | -0.49 | 0.41 | -0.76*** | -0.18 |
| | (0.31) | (0.39) | (0.53) | (0.27) | (0.41) | (0.54) | (0.75) | (0.28) | (0.23) |
| Observations | 328 | 83 | 245 | 76 | 252 | 51 | 100 | 25 | 152 |
| 1st stage F-stat. | 9.47 | 13.01 | 1.78 | 12.56 | 4.47 | 3.81 | 0.96 | 6.70 | 16.68 |
| Sample | Full | Yes | No | Industry | Service | Industry | Service | Industry | Service |

**Note:** 2SLS estimations on the effect of the ETS on $CO_2$ emissions in 2015. Columns 2 and 3 compare effects for firms with high oil and coal consumption (>20% in 2012). Columns 4 and 5 show the estimations on firms in the industry and service sectors, respectively. Columns 6–9 show the estimations by firms' sector and ownership type. All columns include full set of sector, ownership, and energy type dummies. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Tables 2.A.7 and 2.A.8 in the Appendix 2.A show the policy impacts in 2013 and 2014. We use the same bandwidth as for the estimations for 2015. There was no significant effect of the policy in 2013, which is intuitive, because the regulation details were announced at the end of 2013. Effects in 2014 are significantly

[12]Noticeably, the first-stage F-statistics of estimations in columns 3 and 5 are only 1.78 and 4.47, respectively, indicating that whether emissions in 2012 exceeded the coverage threshold has little to no predictive power of firms' actual treatment status on firms that had little carbon-intensive energy consumption in 2012 or firms in the service sector.

different from zero only for firms that heavily relied on carbon-intensive energy and firms in the service sector.[13]

Figure 2.3 plots 2015 emissions (in logarithm)–by sector and whether the share of coal and oil consumption was higher than 20%–just above and below the 2012 emissions threshold separately for pilot and nonpilot firms.[14] Panels b and c depict the results from our RD estimations in columns 4 and 5 of Table 2.2. They show that the ETS significantly reduced $CO_2$ emissions for pilot firms in the industry sector (but not the service sector). We find a significant effect on the firms that relied more heavily on coal or oil (panels d and e).

### 2.3.2 Abatement Mechanism

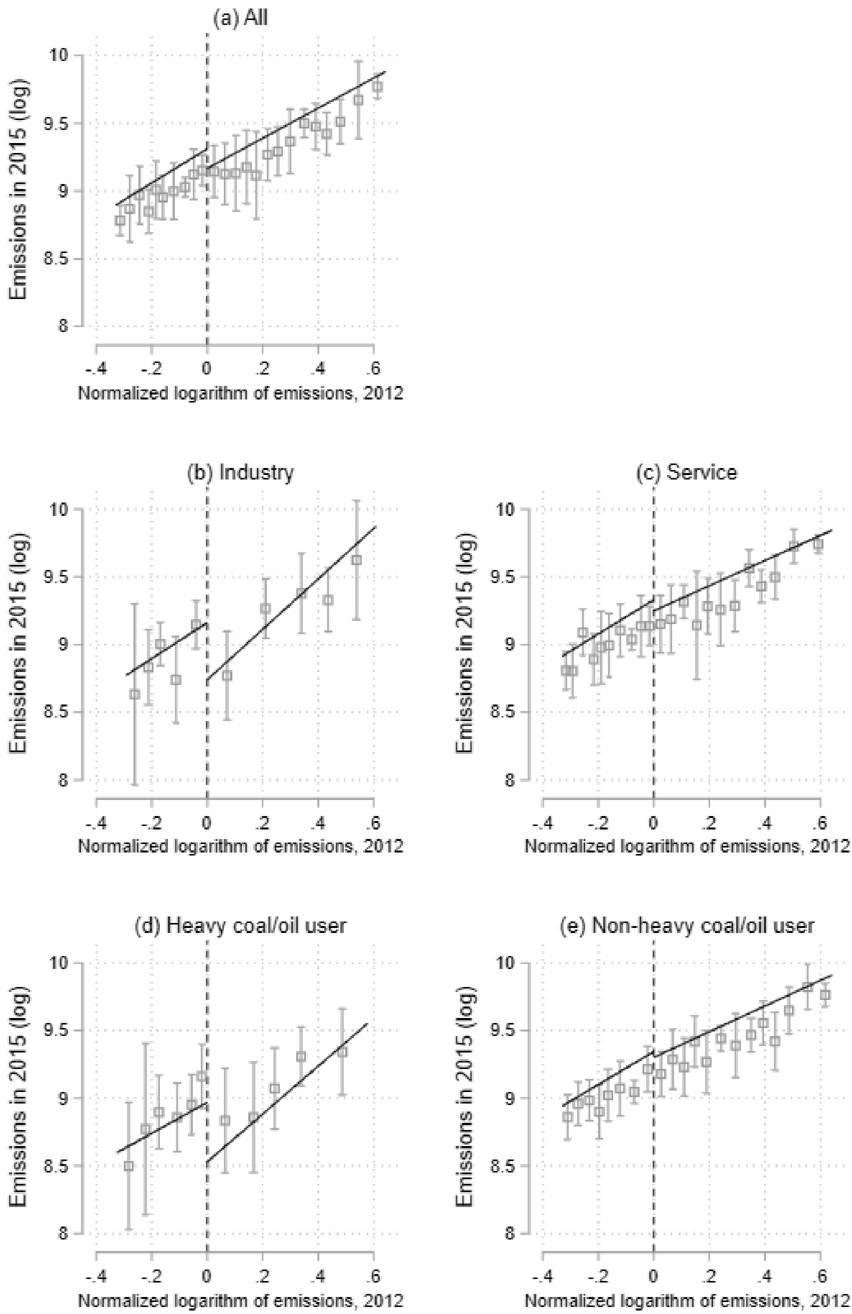
To understand industrial firms' underlying abatement mechanism, we next study the regulation impacts on several outcomes, including output, energy consumption, fuel switching, and production efficiency. We use carbon intensity, the ratio between $CO_2$ emissions and total energy consumption, to measure any fuel-switching effect. Production efficiency is measured as the energy consumption per unit of output. Figure 2.4 graphically depicts the results from our RD estimations on industrial firms' different outcome variables. Industrial firms' emissions reduction was largely driven by a decrease in total energy consumption (panel a). An insignificant decrease in carbon intensity suggests some degree of fuel switching or reduced consumption of carbon-intensive energy such as coal (panel b). Panels c–e in Figure 2.4 present the effects of the ETS on industrial firms' coal consumption, output, and energy intensity in 2015, which explains how the ETS affected the treated firms' energy consumption. Coal consumption decreased significantly (panel c), while output was slightly lower but the effect is not significantly different from zero (panel d), while production efficiency is not significantly affected (panel e).

Next, to understand the potential fuel-switching effect indicated by the estimations presented in panels a–c of Figure 2.4, we look at the distribution effect of the ETS on specific energy consumption using 2SLS of a linear probability model for the probability of falling into each interval. Specifically, we use the model in

[13] As a placebo test, we also estimate whether the policy had impacts on emissions and results are presented in 2009–11 in Tables 2.A.4–2.A.6. As expected, there was no significant difference on emissions in 2009–11 between the treated and control firms. However,the treated industrial firms on average had significantly higher emissions, but only in 2009 (column 4 in Table 2.A.4). This is reassuring and suggests that our treated and control groups are comparable with each other.

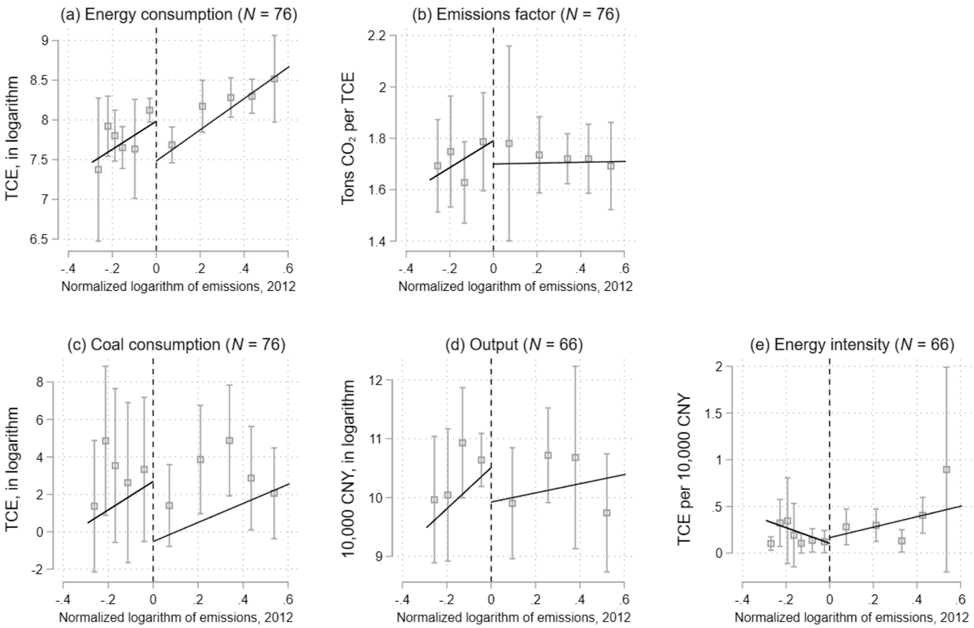
[14] Locations of bins are all constructed using the type of quantile-spaced bins such that bins contain the same number of observations. The number of bins is selected based on the mimicking variance method such that the overall variability of the binned means 'mimics' the overall variability in the raw scatter plot of the data and therefore gives a better sense of the data variability (Cattaneo et al., 2019).

Figure 2.3: Local linear regression of emissions in 2015 conditional on X, by sectors and the share of coal and oil consumption, optimal bin and RD sample




**Note:** 95% confidence interval in figure

Figure 2.4: Local linear regression, abatement mechanism




**Note:** Panels a–c are constructed using the full sample of firms in the industry sector. Because the output value data are not carefully verified by the third-party verifier, we exclude firms with a dramatic change of output in 2015 compared with 2012 and almost invariant emissions and energy consumption in panels d and e.

equation (2.1) with the outcome variable $I_{if}$ equal to 1 if firm $i$'s consumption of fuel $f$ is larger than a specified value $a$. Table 2.3 presents the effects on the distribution of coal consumption. The ETS reduced the probability of having positive coal consumption by about 50 percentage points (column 1). Firms with lower coal consumption saw a larger decrease in relative probability: the probabilities of having coal consumption higher than 500 and 1500 tce were both reduced by about 80 percentage points (columns 2 and 3), and for the threshold of 2000 tce (column 4), about 70 percentage point. The estimations are less significant with a lower magnitude as the threshold increases (columns 5 and 6). This suggests that the ETS mainly affected firms with low or marginal coal consumption in 2015.

Table 2.3: Distribution effects of the ETS on industrial firms' coal consumption

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Treat | -0.50* | -0.79** | -0.80** | -0.72** | -0.46* | -0.43* |
| | (0.30) | (0.37) | (0.38) | (0.34) | (0.24) | (0.24) |
| Observations | 76 | 76 | 76 | 76 | 76 | 76 |
| Mean | 0.43 | 0.36 | 0.29 | 0.21 | 0.15 | 0.11 |
| S.d. | 0.50 | 0.48 | 0.46 | 0.41 | 0.36 | 0.32 |
| 1st stage F stat. | 12.56 | 12.56 | 12.56 | 12.56 | 12.56 | 12.56 |
| Coal consumption | >0 | >500 | >1500 | >2000 | >2500 | >3000 |

**Note:** This table presents 2SLS estimations of linear probability models for the probability of having coal consumption (in tce) falling into each interval in 2015, with intervals specified in the last row. All columns include full set of sector, ownership, and energy type dummies. Robust standard errors in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 2.4 presents the ETS effects on distribution of natural gas consumption. The positive insignificant estimation suggests that, on average, the ETS increases the likelihood of having natural gas consumption only for some of the industrial firms. The magnitude of the estimation is lower at the higher end of the distribution, suggesting that the ETS affects firms with higher natural gas consumption to a lesser degree.

## 2.4 Allowances and Emissions Reduction

Lastly, we study whether the generosity of allowance allocation affects emissions. This is challenging because the initial allocations might have been endogenous to firms' emissions. Estimations from simply regressing emissions on allowances would be biased. We propose an instrumental variable to address this issue, and look at the effects by firm size, ownership, and sector.

Table 2.4: Distribution effects of the ETS on industrial firms' natural gas consumption

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Treat | 0.15 | 0.47 | 0.40 | 0.19 | 0.08 | 0.22 | 0.01 |
| | (0.24) | (0.32) | (0.37) | (0.33) | (0.33) | (0.15) | (0.11) |
| Observations | 76 | 76 | 76 | 76 | 76 | 76 | 76 |
| Mean | 0.60 | 0.51 | 0.35 | 0.28 | 0.23 | 0.10 | 0.05 |
| S.d. | 0.49 | 0.50 | 0.48 | 0.45 | 0.42 | 0.30 | 0.22 |
| 1st stage F stat. | 12.56 | 12.56 | 12.56 | 12.56 | 12.56 | 12.56 | 12.56 |
| Natural gas consumption | >0 | >250 | >500 | >1000 | >2000 | >3000 | >4000 |

**Note:** 2SLS estimations of linear probability models for the probability of having natural gas consumption (in tce) falling into each interval in 2015, with intervals specified in the bottom row. All columns include full set of sector, ownership, and energy type dummies. Robust standard errors in parentheses.

### 2.4.1 Empirical Strategy

In the ETS in Beijing, the allowances were allocated for free and generally grandfathered. Regulated firms received free allowances in proportion to emissions in a base period between 2009 and 2012.[15] This allocation did not account for firms' abatement potential or growth rate. Firms that had a large potential to reduce their emissions but initially had high emissions were likely to have allowance surplus. Firms that grew fast or had high costs of abatement were likely to have a shortage. The question is whether such shortages or surplus would affect firm behaviour. We quantify the relationship between firms' allowances and emissions using the following regression specification:

$$Y_{i,t} = \beta A_{i,t} + m_{i,t}, \tag{2.2}$$

where $Y_{i,t}$ is the carbon emissions of firm $i$ in year $t$, and $A_{i,t}$ is the allocated allowances firm $i$ received in year $t$. This regression does not take into consideration that more productive firms or more polluting firms had higher emissions and got more allowances. We could address the issue by including a proxy for firm size, such as productivity. However, this is not feasible due to a lack of availability of production data. Therefore, we could use the difference of emissions between two years as the outcome variable to difference out the time-invariant factors that may have affected emissions,

$$R_{i,t} = \alpha E_i + u_{i,t}. \tag{2.3}$$

[15]For a detailed discussion on the advantages and disadvantages of grandfathering, see Damon et al. (2019).

In equation (2.3), the relative emissions $R_{i,t}$ is the difference between real emissions $Y_{i,t}$ for firm $i$ in year $t$ and firm $i$'s emissions in 2013, $R_{i,t} = Y_{i,t} - Y_{i,13}$; the allowance surplus $E_i$ is the difference between the emissions quota firm $i$ received in 2013, $A_{i,13}$, and its relative emissions in 2013–that is, $E_{i,13} = A_{i,13} - Y_{i,13}$. The variable of interest, $E_{i,13}$, measures the amount of allowance surplus in 2013. The coefficient estimate $\alpha$ therefore captures the cumulative effects of surplus in the years after 2013. Because this variable is time-invariant, we drop the time index to be parsimonious.

Note that we use the allowance surplus in 2013, $E_i$, as the variable of interest. This variable precisely measures whether a firm had an allowance surplus or deficit in 2013. Instead of using the allowance surplus in each year between 2013 and 2017, we use the amount in 2013, because firms could save the surplus from 2013 for later years, and this could have a cumulative impact on the allowances and emissions in subsequent years. Note also that the key determinant of the number of allowances $A_{i,t}$ a firm receives is the average of carbon emissions between 2009 and 2012.[16] Therefore, on one hand, a historically more polluting firm is likely to receive more allowances in the future; on the other, any shocks that had an impact on emissions in 2013 would affect both relative emissions in year $t$ and allowance surplus in 2013. The allowance surplus $E_i$ correlates with the unobserved shocks that affected emissions in 2013 and is thus endogenous.

To address this issue, we propose to use the past shocks on firms' carbon emissions as instruments for allowance surplus. More specifically, we instrument allowance surplus on emissions shocks in 2011 and 2012. The shocks are constructed as the difference between real emissions and the predicted counterfactual emissions if there had been no factors influencing firms' emissions, such as technology adoption or production efficiency improvement.

In our context, exclusion restriction means that past shocks on emissions before 2013 affected firms' emissions reductions in 2014–17 relative to emissions in 2013 only through the channel of allowance surplus–that is, $cov(e_{i,l}, u_{i,t}) = 0$ and $cov(e_{i,}, E_i) \neq 0$ with $l$ denoting the years shocks appeared and $l = 11, 12$. We use estimated emissions residuals to proxy shocks in past years on allowance surplus. To construct such shocks, we estimate firms' counterfactual emissions if there was no shock on firms' emissions and subtract the predicted emissions from actual emissions. We use a dynamic panel model with an AR(1) process using a system generalised method of moments (GMM) estimator to estimate the counterfactual emissions. Here, we need to assume that there is no serial correlation

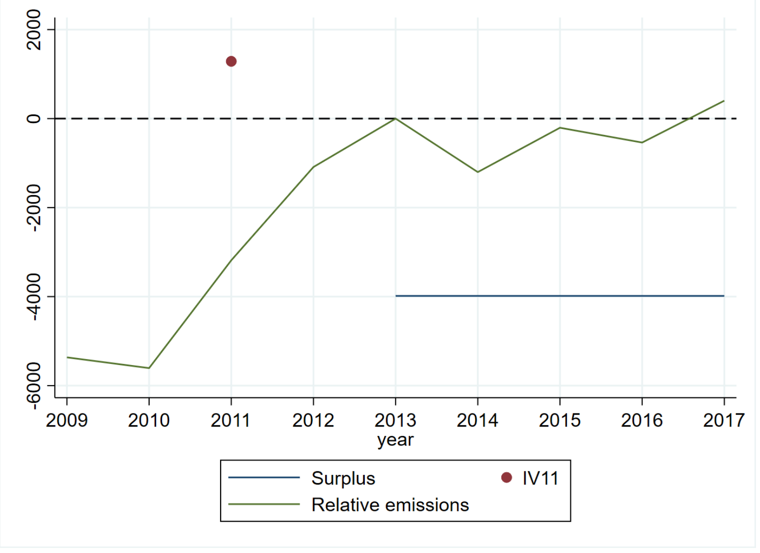
[16] Furthermore, firms may have gotten additional allowances if they had new construction areas or higher productivity compared with 2009–12. Firms needed to file an application to request additional allowances.

in the error term. Our system GMM estimator consists of two equations: equilibrium equations and first difference equations. If there is no serial correlation in the equilibrium equation, then there is a first-order serial correlation error term in the first difference model but no higher-order correlation. These assumptions are testable, and we have found them satisfied. Details are discussed in Section 2.4.2. We use such error terms as instrumental variables to proxy random emissions shocks. Such shocks only appear in each specific year and thus are not correlated with emissions in later years. Accordingly, they should affect emissions in later years only by affecting our endogenous variable, the allowance surplus in 2013.

The motivation for the relevance of such an instrument is that the allocated allowances since 2013 were a function of emissions between years 2009 and 2012, and hence any past shocks that affected carbon emissions between 2009 and 2012 would have had an impact on future allowances–that is, $cov(e_{i,l}, E_i) \neq 0$ with $l = 11, 12$. However, the unobservable determinants for past years' emissions $e_{i,l}$ need not correlate with unobservable variables that influenced firms' emissions reduction, conditional on the assumption of no serial correlation in the past shock discussed earlier–that is, $E(e_{it}e_{is}) = 0, t \neq s$.

To illustrate the intuition of the instrument relevance, we present relative emissions by year (the outcome variable), allowance surplus (the endogenous variable), and shock in 2011 (the instrument variable) of one representative firm in Figure 2.5. This firm was hit by a positive shock in 2011, which had two opposing effects on firms' allowance surplus (or deficit). On one hand, a positive shock increased firms' average emissions between 2009 and 2012 and therefore potentially increased their emissions allowances. On the other, firms' emissions remained around the same level in subsequent years after 2011. In other words, an emissions increase induced by this positive shock partially increased allowance deficits. On aggregate, a shock in 2011 negatively correlates with the amount of allowance surplus (or deficit, if negative). Firms with a deficit could decide to reduce emissions below allocated allowances, which would render a real cost of abatement. Alternatively, they would have to purchase emissions reductions from other firms in the carbon market, rendering a transaction cost that consisted of a fixed cost and a variable cost increasing with the amount of traded allowances. Firms with a surplus could sell the excess amount on the market, again with a payment of the transaction cost. As allowances banking is allowed in Beijing ETS, firms may decide to save the surplus after surrendering allowances for their emissions. There might be several reasons why firms would choose banking, instead of selling. For instance, firms might prefer to not trade actively to avoid the nontrivial transaction cost, in case the return to trading does not exceed this transaction cost. Or they may expect the carbon price to increase in the future and therefore actively choose not to sell. Theory suggests that all covered firms should have the same incentive facing the same permit price. We try to determine whether firms with different allowance statuses are likely to respond differently.

Figure 2.5: Emissions, allowance surplus, and shocks in 2011, an example (all in tons $CO_2$)




Using the emissions shocks in 2011 and 2012 as instrumental variables, we thereby compare the changes in emissions in 2014–17 relative to emissions in 2013 among firms that experienced different shocks on emissions in 2011.

$$\begin{aligned} R_i &= \alpha_0 + \alpha \hat{E}_i + W_i\theta + u_i; \text{ (Second stage)} \\ E_i &= \beta_0 + \beta_1 e_{i,11} - \beta_2 e_{i,12} + W_i\lambda + v_i. \text{ (First stage)} \end{aligned} \tag{2.4}$$

In this model, $e_{i,11}$ and $e_{i,12}$ are emission shocks for a firm $i$ in 2011 and 2012, respectively; $E_i$ is the allowance surplus (or deficit, if negative) for firm $i$ in 2013; $W_i$ represents a set of control variables, including sector dummy, ownership dummy, and a firm's average emissions before 2013.

### 2.4.2 Data Generating Process of Emissions

The data generating process of firm $i$'s counterfactual carbon emissions in the absence of the ETS is captured by the following AR(1) model with unobserved individual-specific effects:

$$Y_{i,t} = \lambda_0 + \lambda_1 Y_{i,t-1} + \sum_{s=0}^{k} \gamma_s X_{i,t-s} + \eta_t + \eta_i + e_{i,t} \tag{2.5}$$

In this dynamic panel data model, $X_{i,t}$ is the output of firm $i$ in year $t$, with $s$ representing time lags. The year fixed effects $\eta_t$ capture time-varying shocks common to all the firms; the firm fixed effects $\eta_i$ capture time-invariant firm-specific impacts on their emissions. Both carbon emissions $Y_{i,t-1}$ and $X_{i,t}$ are endogenous in that the output is correlated with factors that influence both carbon emissions and productivity, such as technology development and labor, that we lack data on. The error term $e_{i,t}$ captures all these unobserved characteristics that may affect firm $i$'s emissions in the pre-treatment period.

To estimate the AR(1) model presented in equation (2.5), we use a system GMM estimator as proposed in Blundell and Bond (1998). The estimator imposes further restrictions on the initial condition process in addition to those imposed in the standard first-difference GMM estimator as in Anderson and Hsiao (1981) and Arellano and Bond (1991), which have been found to be biased when the time period is short, as in this paper. Taking a first-difference transformation of the model, we get

$$\Delta Y_{it} = \lambda_1 \Delta Y_{i,t-1} + \sum_{s=0}^{k} \gamma_s \Delta X_{i,t-s} + \Delta\eta_t + \Delta e_{it}. \tag{2.6}$$

We use $Y_{i,t-2}$ as an instrument for $\Delta Y_{i,t-1} = Y_{i,t-1} - Y_{i,t-2}$ in the first difference equation (2.6), and $\Delta Y_{i,t-1}$ as an instrument for $Y_{i,t-1}$ in the equilibrium equation (2.5). In order for the instruments to be valid, we assume that $Y_{i,t-2}$ is uncorrelated with $\Delta e_{i,t}$ but correlated with $\Delta Y_{i,t-1}$. These are satisfied as long as there is no serial correlation in the error term–that is, $E(e_{it}e_{is}) = 0, t \neq s$. Therefore, the moment conditions for the estimator are

$$\begin{aligned} E[Y_{i,t-s}, \Delta e_{i,t}] &= 0, s = 2, 3, ..., t; \\ E[X_{i,t-s}, \Delta e_{i,t}] &= 0, s = 2, 3, ..., t; \\ E[\Delta Y_{i,t-1}(\eta_i + e_{i,t})] &= 0, t = 2, 3, ..., T; \\ E[\Delta X_{i,t-1}(\eta_i + e_{i,t})] &= 0, t = 2, 3, ..., T. \end{aligned} \tag{2.7}$$

We estimate the coefficient $\lambda_1$ using emissions data for nonpilot firms between 2009 and 2015 and for pilot firms between 2009 and 2012, as the ETS was implemented in 2013. This gives us an unbalanced panel for 7 years. If $e_{it}$ is serial uncorrelated, then the instruments are valid. With an Arellano-Bond test proposed in Arellano and Bond (1991), we test for autocorrelation of the first-differenced residuals. The null hypothesis is that there is no autocorrelation of the first-differenced residuals–that is, $corr(\Delta e_{it}, \Delta e_{i,t-j}) = 0, j > 0$. For the instruments to be valid, higher-order serial correlation must be absent: $corr(\Delta e_{it}, \Delta e_{i,t-2}) = 0$ and $corr(\Delta e_{it}, \Delta e_{i,t-3}) = 0$; $corr(\Delta e_{it}, \Delta e_{i,t-1}) \neq 0$ by construction. Statistical tests of p-values 0.48, 0.43, and 0 do not reject the first two null hypotheses with orders of 2 and 3, but they do reject the last null hypothesis with the order of 1.

Using the empirical strategy described above, we obtain a coefficient estimate of $\lambda_1$ in equation (2.5) equal to 0.99. The estimation close to 1 suggests that firms' carbon emissions are mainly dependent on a permanent component–that is, a serially uncorrelated, permanent emissions shock.

### 2.4.3 Results

In this section, we present the estimations on the effects of allowances on emissions, using the empirical strategy discussed in Section 2.4.1. We use a balanced panel with 226 firms, with 111 firms having an allowance deficit in 2013 and 115 having a surplus.[17] Table 2.5 presents the results.

We begin by documenting the correlation between emissions and allowance surplus in years 2014–17 in Panel A of Table 2.5. The results point to a negative correlation between allowance surplus and emissions between 2014 and 2016, although the coefficients are not statistically significant. As discussed in Section 2.4.1, these relationships are likely subject to a substantial omitted variable bias; for example, a firm might obtain an allowance surplus in 2013 because it adopted more advanced abatement or production technologies, which led to decreasing emissions over time.

**Overall effects.** To address concerns about the endogeneity of allowances and emissions in 2013, we instrument for allowance surplus using past emissions shocks, as introduced in Section 2.4.1.

Panel B in Table 2.5 presents the estimation results on the relationship between allowance surplus and emissions in 2014–17 relative to emissions in 2013.[18] On

[17] We exclude from the sample the six outlier firms with surplus (deficit) in the top and bottom 1.5 percentile. Figure 2.B.1 in Appendix 2.B presents the first-stage of the 2SLS graph on the relationship between emissions shocks in 2011 and allowance surplus in 2013 with the outliers excluded.

[18] The coefficient estimates using the full sample with six outliers included is presented in Table 2.B.1 in Appendix 2.B. The estimations are qualitatively similar with a weaker first stage.

Table 2.5: Do surpluses explain emissions?

| | (1) 2014 | (2) 2015 | (3) 2016 | (4) 2017 |
|---|---|---|---|---|
| ***Panel A: OLS** ($N = 220$)* | | | | |
| Surplus | -0.07 | -0.07 | -0.02 | 0.01 |
| | (0.09) | (0.11) | (0.12) | (0.13) |
| R-squared | 0.15 | 0.21 | 0.22 | 0.23 |
| ***Panel B: IV, all** ($N = 220$)* | | | | |
| Surplus | 0.07 | 0.10 | 0.08 | 0.13 |
| | (0.09) | (0.13) | (0.15) | (0.17) |
| 1st stage F stat. | 62.77 | 62.77 | 62.77 | 62.77 |
| p-value of Hansen J | 0.62 | 0.18 | 0.15 | 0.18 |
| ***Panel C1: IV, by sector: service** ($N = 131$)* | | | | |
| Surplus | 0.14** | 0.18 | 0.20 | 0.22 |
| | (0.07) | (0.15) | (0.14) | (0.18) |
| 1st stage F stat. | 68.94 | 68.94 | 68.94 | 68.94 |
| p-value of Hansen J | 0.28 | 0.23 | 0.27 | 0.47 |
| ***Panel C2: IV, by sector: industry** ($N = 89$)* | | | | |
| Surplus | -0.06 | -0.11 | -0.16 | -0.06 |
| | (0.18) | (0.21) | (0.25) | (0.26) |
| 1st stage F stat. | 18.45 | 18.45 | 18.45 | 18.45 |
| p-value of Hansen J | 0.38 | 0.92 | 0.78 | 0.47 |
| ***Panel D1: IV, by size: below** ($N = 111$)* | | | | |
| Surplus | 0.14 | 0.30* | 0.31* | 0.40* |
| | (0.12) | (0.16) | (0.17) | (0.21) |
| 1st stage F stat. | 37.00 | 37.00 | 37.00 | 37.00 |
| p-value of Hansen J | 0.35 | 0.44 | 0.41 | 0.43 |
| ***Panel D2: IV, by size: above** ($N = 109$)* | | | | |
| Surplus | 0.12 | 0.02 | 0.01 | 0.06 |
| | (0.16) | (0.19) | (0.22) | (0.21) |
| 1st stage F stat. | 23.14 | 23.14 | 23.14 | 23.14 |
| p-value of Hansen J | 0.16 | 0.66 | 0.56 | 0.69 |
| ***Panel E1: IV, by ownership: non state-related** ($N = 107$)* | | | | |
| Surplus | 0.01 | -0.08 | -0.21 | -0.12 |
| | (0.13) | (0.13) | (0.15) | (0.19) |
| 1st stage F stat. | 21.74 | 21.74 | 21.74 | 21.74 |
| p-value of Hansen J | 0.94 | 0.06 | 0.05 | 0.10 |
| ***Panel E2: IV, by ownership: state-related** ($N = 113$)* | | | | |
| Surplus | 0.13 | 0.19 | 0.27* | 0.28* |
| | (0.12) | (0.16) | (0.16) | (0.17) |
| 1st stage F stat. | 56.46 | 56.46 | 56.46 | 56.46 |
| p-value of Hansen J | 0.60 | 0.52 | 0.58 | 0.74 |

**Note:** Specifications in all columns include sector dummies, ownership dummies, and average emissions between 2009 and 2012. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

average, one additional unit of allowance surplus in 2013 is associated with an increase in emissions, an increase that furthermore grew from 2014 to 2017, although the coefficients are not statistically significantly different from zero. The p-values of over-identifying tests show nonrejection of null hypotheses that our instruments are exogenous.[19]

To understand the heterogeneity of how allowance surplus affects firms' emissions, we estimate equation (2.4) by sector, firm size, and ownership. With heterogeneous effects, monotonicity must also be assumed. In our setting, the monotonicity assumption requires that firms that were hit by larger negative (positive) shocks in 2011 likely have more allowance surplus (deficit) in 2013. One testable implication of this assumption is that the first-stage estimation must be nonpositive for any subsample. We estimate the first stage on the specified subsample, and Table 2.B.2 in Appendix 2.B reports the results. Following Bhuller et al. (2020), in columns 1–3 of Table 2.B.2, we estimate the propensity score of having an allowance surplus for each firm, and then estimate three separate first-stage estimates for the three tertiles of predicted probability of having allowance surplus.[20] Columns 4–5 divide the data by sector. Columns 6–7 divide the data by firm size, proxied by firms' average output value in the pre-treatment period 2009–12. Columns 8–9 divide the data by ownership. For most of these subsamples, the first-stage estimates are negative and statistically different from zero (columns 4–9); for the rest, the estimates are at least nonpositive (columns 3 and 7). This indicates that the monotonicity assumption is likely to hold.

**Did the effect differ by sector?** Panels C1 and C2 in Table 2.5 present estimates from equation (2.4) for firms in the service and industry sectors from 2014 to 2017. On average, an additional ton of allowance surplus in 2013 increased service firms' relative emissions in subsequent years by about 0.1 to 0.2 ton, although they were significantly different from zero only in 2014. The results suggest that some firms did increase their relative emissions, or decrease their emissions abatement, in response to the increase in allowance surplus, or the reduction in allowance deficit. With a weaker first stage, there is no such effect on firms

[19] In Table 2.B.4 in Appendix 2.B, we estimate the effects using the 2011 shock as an instrument. Estimations are qualitatively similar with a weaker first stage. We therefore prefer to use both 2011 and 2012 shocks as instrumental variables. We also estimate whether surplus status affects firms' emissions using dummy variables of having positive shocks in 2011 and 2012 as instrumental variables. Table 2.B.3 in Appendix 2.B shows the results. We do not find any significant evidence that the status affects firms' emissions.

[20] Specifically, we estimate the propensity score using the following equation:

$$propensity_i = \gamma_0 + \gamma_1 e_i + \gamma_2 avgemi_i + s_i + o_i + \epsilon_i, \tag{2.8}$$

where $s_i$ and $o_i$ are sector and ownership dummies of firm $i$; $avgemi_i$ is average emissions of firm $i$ in the pre-treatment period 2009–12; $propensity_i$ is a dummy variable equal to one if a firm $i$ had allowance surplus in 2013, and zero otherwise.

in the industry sector. However, Wald tests of the estimation equality between service and industrial sectors on how allowance surplus in 2013 affected emissions in subsequent years show a nonrejection of the null hypothesis,[21] which suggests that although an increase in allowance surplus had an impact on emissions of some firms in the service sector in 2014, it was not large enough to be statistically distinguishable from the impact for industrial firms.

**Did the effect differ by firm size?** When firms trade emissions allowances in the carbon market, they need to pay a transaction cost, such as for clerical work to open a trading account, file a transaction application, and time spent on communicating and processing the trading rules. This transaction cost is not negligible and has been widely discussed in the existing literature.

Firms trade if and only if the benefit of selling (in case of having allowance surplus) exceeds the trading costs. Smaller firms might have less decision capacity, and in such cases this fixed decision cost is not trivial. In contrast, larger firms have cumulative knowledge about their abatement capacity, emissions, whether and how to trade allowances in the market, and whether to save the allowance surplus for subsequent years. When faced with a new regulation such as the carbon emissions trading system, small- or medium-sized firms and firms in the service sector may have to incur relatively higher communication costs, because managers who are in charge of permit trading may have little knowledge about their energy usage and how to reduce emissions.[22]

To answer the question of whether allowance surplus affected emissions of larger firms differently, we use firms' average output value in 2009–12 as a proxy for firm size. Because firms' outputs vary by sector, it is more reasonable to compare firms in the same sector. We therefore assign a dummy equal to one if a firm's output is below its median in each respective two-digit sector, and zero otherwise.[23]

Panels D1 and D2 in Table 2.5 present the estimation results of how allowance surplus affected firms' emissions for firms below and above the median of output value in 2012, respectively. On average, for smaller firms, one ton increase in allowance surplus in 2013 was transmitted to an increase in emissions in 2015–

[21] The p-values of the Wald tests are 0.28, 0.25, 0.20 and 0.38 for 2014–17, respectively.

[22] Bolton and Dewatripont (1994) analyze the trade-off between specialisation and communication in an organisation, and the determinant of the form of efficient networks. They argue that the more specialised workers are, the more communication is necessary for coordination of their activities, and therefore the larger and more sophisticated the organisations are. Although this does not necessarily imply that larger firms must have more specialised workers and better ways to communicate, it is still reasonable to assume that firms of greater size likely have more specialised workers.

[23] We also define the firm size using output tertile or quartile. However, the results are less precisely estimated because of a small sample size. We therefore prefer using the median as a way to define firms' relative size.

17, by about 0.3–0.4 ton (columns 2–4 in panel D1); the effects are not precisely estimated for 2014, but a positive estimation suggests that at least some of the firms increased emissions because of an increase in allowance surplus (column 1 in panel D1). For larger firms, the estimations are never significantly different from zero (panel D2). Equality tests of whether the effects on firms in different sectors differed suggest a nonrejection of null hypotheses, and thus there is no significant difference in responses among firms of different sizes.[24]

**Did the effect differ by ownership?** Panels E1 and E2 compare the effects on firms that are state-related, such as state-owned firms and government agencies, and firms that are non–state-related, such as private and foreign firms and joint ventures. None of the estimations for non–state-related firms are statistically significantly different from zero, and the effects are significant at the 10% significance level for state-related firms, with estimation in 2016 significantly larger than for the non–state-related ones.[25]

Because we do not observe firms' trading behaviour, we cannot conclude that large firms must trade more. But our empirical finding suggests that with more generous emissions allowances, smaller firms do increase their emissions and therefore likely trade less. Intuitively, this pattern is slightly more pronounced for firms in the service sector, as firms in this sector have fewer mitigation strategies when faced with environmental regulations such as the permit trading system. For instance, manufacturing and industrial firms can often mitigate by switching fuel sources from carbon-intensive coal to less carbon-intensive natural gas. However, fuel switching is not available to firms in the service sector. The results provide a potential explanation for why the ETS in Beijing failed to induce any significant emissions reduction for firms in the service sector. Including firms in the service sector induces an unintended consequence: a more generous allocation of allowances counteracts emissions reductions of firms in the service sector to some degree.

## 2.5 Conclusion and Discussion

In this paper, we have studied the effects of the permit trading system in the context of the pilot Emissions Trading Scheme in Beijing. We identify which firms were induced to mitigate and how much of an impact allocated allowances may have had on firms' emissions. We find that the policy reduced emissions of firms in the industry sector, but not in the service sector. We then explore the potential

[24] The p-values of the tests are 0.91, 0.25, 0.27, and 0.26 in 2014–17, respectively.
[25] The p-values of Wald tests on the equality are 0.47, 0.17, 0.02 and 0.12 in 2014–17, respectively.

abatement mechanisms of industrial firms and find that their emissions reduction was realised mainly by reducing coal consumption. We find that the ETS shifted their distribution of coal consumption to the left, without significantly reducing output.

Next, we study whether the initial allowance allocation had no impact on emissions in subsequent years in the Beijing ETS, which is a necessary condition for a cap-and-trade market to be effective. We find that, overall, such an independence property holds. However, it likely fails for some firms in the service sector and is more likely to fail for those of smaller size. This suggests that free allowances could dampen these firms' abatement.

# Appendix 2

## 2.A The Mitigation Effects of the Pilot ETS in Beijing

Figure 2.A.1: McCrary density test

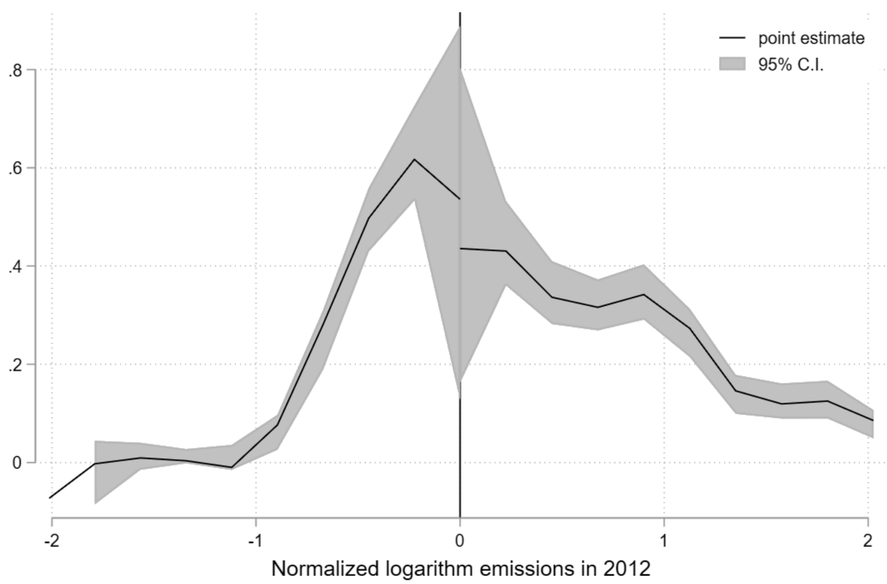


Figure 2.A.2: Density plot of share of oil and coal consumption in 2012, RD sample

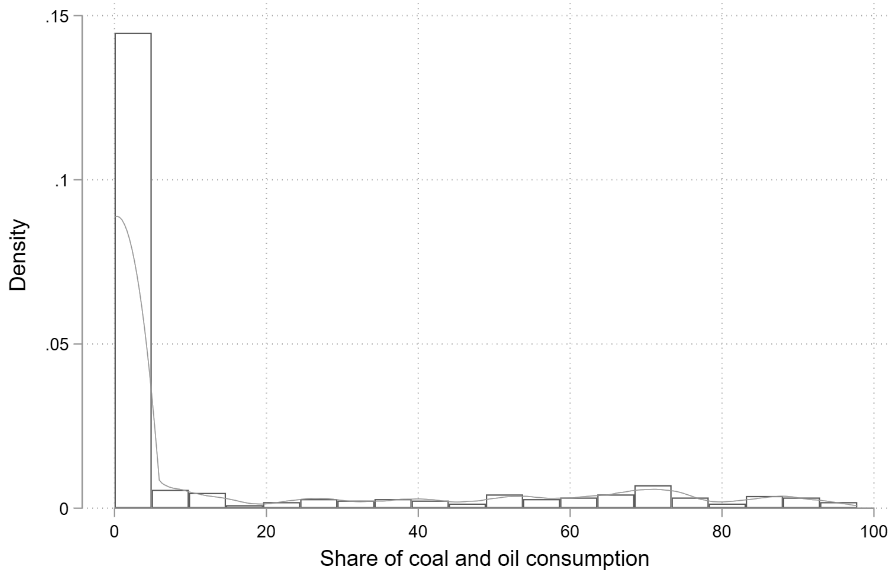



Figure 2.A.3: The probability of treatment

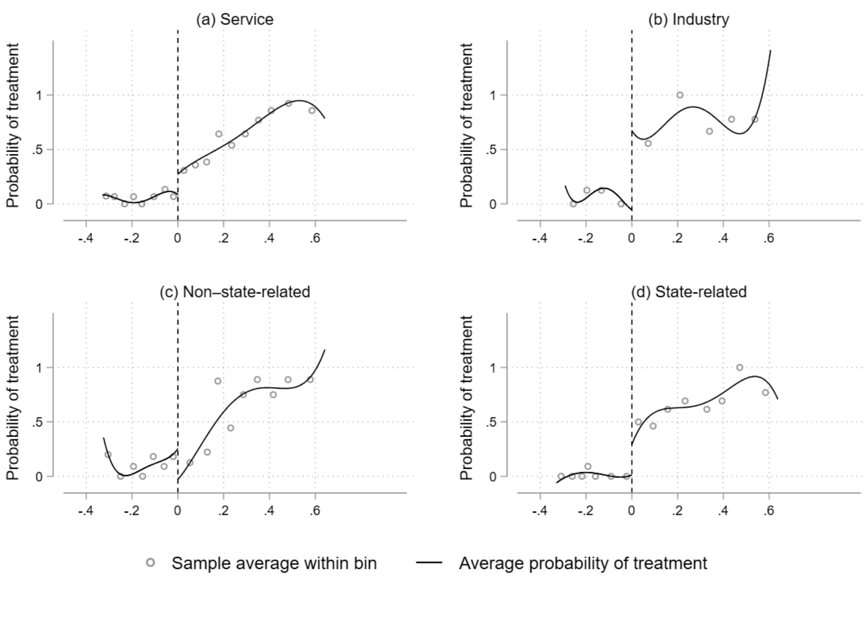


Table 2.A.1: Effect of pilot ETS in Beijing on carbon emissions in 2015, OLS and 2SLS on full sample

| | All | | Heavy coal/oil users | | Sector | | Non state-related | | State-related | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| | OLS | IV | IV | IV | IV | IV | IV | IV | IV | IV |
| Treat | 0.98*** | -0.01 | -0.42*** | 0.11* | -0.09 | 0.03 | -0.13 | 0.10 | -0.20 | 0.04 |
| | (0.05) | (0.06) | (0.15) | (0.06) | (0.12) | (0.08) | (0.16) | (0.22) | (0.25) | (0.07) |
| Observations | 741 | 741 | 209 | 532 | 245 | 496 | 138 | 177 | 107 | 319 |
| 1st stage F stat. | | 209.24 | 71.30 | 128.73 | 85.62 | 114.34 | 43.74 | 13.00 | 17.73 | 135.30 |
| Sample | Full | Full | Yes | No | Industry | Service | Industry | Service | Industry | Service |

**Note:** OLS (column 1) and 2SLS (columns 2–10) estimations of the effect of Beijing ETS on firms' $CO_2$ emissions in 2015 using the full sample. Specifications in all columns include sector dummies, ownership dummies, and energy type dummies in 2012. Robust standard errors in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 2.A.2: Effect of pilot ETS in Beijing on carbon emissions, by different bandwidth selectors, linear, triangular kernel

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | msetwo | msesum | mserd/msecomb1 | msecomb2 | cerrd/cercomb1 | certwo | cersum | cercomb2 |
| Treat | -0.50* | -0.65 | -0.69 | -0.66 | -0.53 | -0.52 | -0.50 | -0.53 |
| | (0.31) | (0.46) | (0.51) | (0.47) | (0.56) | (0.40) | (0.54) | (0.56) |
| Observations | 328 | 272 | 267 | 268 | 193 | 258 | 197 | 193 |
| Mean dependent var. | 9.13 | 9.08 | 9.08 | 9.08 | 9.08 | 9.14 | 9.08 | 9.08 |
| Sd. of dependent var. | 0.39 | 0.37 | 0.37 | 0.37 | 0.36 | 0.37 | 0.36 | 0.36 |
| 1st stage F stat. | 9.47 | 4.94 | 4.30 | 4.89 | 2.71 | 5.76 | 2.76 | 2.71 |
| Bandwidth-left | .336 | .359 | .354 | .354 | .254 | .242 | .258 | .254 |
| Bandwidth-right | .641 | .359 | .354 | .359 | .254 | .461 | .258 | .258 |

**Note:** 2SLS estimations on the effect of the ETS on $CO_2$ emissions in 2015 using different bandwidth selectors. Columns 1 and 3 are mean squared error-optimal bandwidth selectors that are different (column 1) or the same (column 3) below and above the cutoff for the RD treatment-effect estimator: the former is used in the main analysis. Column 2 uses one common MSE-optimal bandwidth selector for the sum of regression estimates. Column 4 uses the median of bandwidth selectors in 1–3 for the left and right bandwidths separately. Columns 5–8 specify coverage of error-rate-optimal bandwidth selector either for the RD treatment-effect estimator or for the sum of regression estimates. Bandwidth length are given in the bottom two rows. Specifications in all columns include sector dummies, ownership dummies and energy type dummies in 2012. Robust standard errors in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 2.A.3: Effect of pilot ETS in Beijing on carbon emissions, linear, uniform kernel

| | All | Heavy coal/oil users | | Sector | | Non state-related | | State-related | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Treat | -0.45* | -0.68** | -0.25 | -0.54* | -0.37 | -0.31 | 0.91 | -0.88** | -0.24 |
| | (0.25) | (0.34) | (0.39) | (0.29) | (0.32) | (0.49) | (2.49) | (0.38) | (0.22) |
| Observations | 328 | 83 | 245 | 76 | 252 | 51 | 100 | 25 | 152 |
| 1st stage F stat. | 13.11 | 15.47 | 3.37 | 11.58 | 6.99 | 4.36 | 0.19 | 4.25 | 19.57 |
| Sample | Full | Yes | No | Industry | Service | Industry | Service | Industry | Service |

**Note:** 2SLS estimations on the effect of the ETS on $CO_2$ emissions in 2015 using uniform kernel. Columns 2 and 3 compare effects for firms with high oil and coal consumption (>20% in 2012). Columns 4 and 5 show the estimations on firms in the industry and service sectors, respectively. Columns 6–9 show the estimations by firms' sector and ownership type. All columns include full set of sector, ownership, and energy type dummies. Robust standard errors in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 2.A.4: Placebo tests, effect of pilot ETS in Beijing on carbon emissions, linear, triangular kernel, 2009

| | All | Heavy coal/oil users | | Sector | | Non state-related | | State-related | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Treat | 1.69 | 4.15 | -1.45 | 4.14** | 0.05 | 2.12 | 3.20 | 7.75** | 1.82 |
| | (2.13) | (2.63) | (3.20) | (2.03) | (2.98) | (1.44) | (5.45) | (3.57) | (2.16) |
| Observations | 328 | 83 | 245 | 76 | 252 | 51 | 100 | 25 | 152 |
| 1st stage F stat. | 9.47 | 13.01 | 1.78 | 12.56 | 4.47 | 3.81 | 0.96 | 6.70 | 16.68 |
| Sample | Full | Yes | No | Industry | Service | Industry | Service | Industry | Service |

**Note:** 2SLS estimations on the effect of the ETS on $CO_2$ emissions in 2009. Columns 2 and 3 compare effects for firms with high oil and coal consumption (>20% in 2012). Columns 4 and 5 show the estimations on firms in the industry and service sectors, respectively. Columns 6–9 show the estimations by firms' sector and ownership type. All columns include full set of sector, ownership, and energy type dummies. Robust standard errors in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 2.A.5: Placebo tests, effect of pilot ETS in Beijing on carbon emissions, linear, triangular kernel, 2010

| | All | Heavy coal/oil users | | Sector | | Non state-related | | State-related | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Treat | 0.27 | 2.16 | -1.87 | 3.22 | -1.74 | 0.66 | 4.33 | 6.39** | -0.00 |
| | (1.86) | (2.43) | (3.13) | (1.98) | (2.57) | (0.44) | (5.69) | (3.01) | (1.61) |
| Observations | 328 | 83 | 245 | 76 | 252 | 51 | 100 | 25 | 152 |
| 1st stage F stat. | 9.47 | 13.01 | 1.78 | 12.56 | 4.47 | 3.81 | 0.96 | 6.70 | 16.68 |
| Sample | Full | Yes | No | Industry | Service | Industry | Service | Industry | Service |

**Note:** 2SLS estimations on the effect of the ETS on $CO_2$ emissions in 2010. Columns 2 and 3 compare effects for firms with high oil and coal consumption (>20% in 2012). Columns 4 and 5 show the estimations on firms in the industry and service sectors, respectively. Columns 6–9 show the estimations by firms' sector and ownership type. All columns include full set of sector, ownership, and energy type dummies. Robust standard errors in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 2.A.6: Placebo tests, effect of pilot ETS in Beijing on carbon emissions, linear, triangular kernel, 2011

| | All | Heavy coal/oil users | | Sector | | Non state-related | | State-related | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Treat | -0.27 | 0.57 | -0.79 | 0.14 | -0.61 | 0.29 | 1.87 | 0.27 | -0.09 |
| | (0.87) | (0.37) | (2.43) | (0.30) | (1.50) | (0.21) | (4.70) | (0.42) | (0.44) |
| Observations | 328 | 83 | 245 | 76 | 252 | 51 | 100 | 25 | 152 |
| 1st stage F stat. | 9.47 | 13.01 | 1.78 | 12.56 | 4.47 | 3.81 | 0.96 | 6.70 | 16.68 |
| Sample | Full | Yes | No | Industry | Service | Industry | Service | Industry | Service |

**Note:** 2SLS estimations on the effect of the ETS on $CO_2$ emissions in 2011. Columns 2 and 3 compare effects for firms with high oil and coal consumption (>20% in 2012). Columns 4 and 5 show the estimations on firms in the industry and service sectors, respectively. Columns 6–9 show the estimations by firms' sector and ownership type. All columns include full set of sector, ownership, and energy type dummies. Robust standard errors in parentheses.

Table 2.A.7: Effect of pilot ETS in Beijing on carbon emissions, linear, triangular kernel, 2013

| | All | Heavy coal/oil users | | Sector | | Non state-related | | State-related | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Treat | -0.10 | -0.74 | 1.23 | -0.02 | -0.07 | -0.36 | 2.55 | 0.25 | 0.24 |
| | (1.13) | (1.64) | (1.78) | (0.16) | (1.85) | (0.28) | (5.33) | (0.22) | (1.06) |
| Observations | 328 | 83 | 245 | 76 | 252 | 51 | 100 | 25 | 152 |
| 1st stage F stat. | 9.47 | 13.01 | 1.78 | 12.56 | 4.47 | 3.81 | 0.96 | 6.70 | 16.68 |
| Sample | Full | Yes | No | Industry | Service | Industry | Service | Industry | Service |

**Note:** 2SLS estimations on the effect of the ETS on $CO_2$ emissions in 2013. Columns 2 and 3 compare effects for firms with high oil and coal consumption (>20% in 2012). Columns 4 and 5 show the estimations on firms in the industry and service sectors, respectively. Columns 6–9 show the estimations by firms' sector and ownership type. All columns include full set of sector, ownership, and energy type dummies. Robust standard errors in parentheses.

Table 2.A.8: Effect of pilot ETS in Beijing on carbon emissions, linear, triangular kernel, 2014

| | All | Heavy coal/oil users | | Sector | | Non state-related | | State-related | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Treat | -0.57** | -0.66** | -0.53 | -0.32 | -0.68 | -0.36 | 0.64 | -0.20 | -0.42** |
| | (0.27) | (0.31) | (0.54) | (0.20) | (0.44) | (0.39) | (0.83) | (0.22) | (0.20) |
| Observations | 328 | 83 | 245 | 76 | 252 | 51 | 100 | 25 | 152 |
| 1st stage F stat. | 9.47 | 13.01 | 1.78 | 12.56 | 4.47 | 3.81 | 0.96 | 6.70 | 16.68 |
| Sample | Full | Yes | No | Industry | Service | Industry | Service | Industry | Service |

**Note:** 2SLS estimations on the effect of the ETS on $CO_2$ emissions in 2014. Columns 2 and 3 compare effects for firms with high oil and coal consumption (>20% in 2012). Columns 4 and 5 show the estimations on firms in the industry and service sectors, respectively. Columns 6–9 show the estimations by firms' sector and ownership type. All columns include full set of sector, ownership, and energy type dummies. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.A.9: Effect of pilot ETS in Beijing on carbon emissions, with firms involved since 2014 and 2015 included

| | All | Heavy coal/oil users | | Sector | | Non state-related | | State-related | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Treat_p | -0.42* | -0.49** | -0.34 | -0.91 | -0.19 | -0.64 | 1.89 | -0.99 | -0.03 |
| | (0.23) | (0.24) | (0.37) | (0.60) | (0.21) | (0.61) | (5.02) | (1.02) | (0.14) |
| Observations | 414 | 94 | 320 | 111 | 303 | 74 | 121 | 37 | 182 |
| 1st stage F stat. | 15.59 | 24.06 | 4.89 | 5.12 | 10.15 | 3.31 | 0.14 | 1.65 | 35.07 |
| Sample | Full | Yes | No | Industry | Service | Industry | Service | Industry | Service |

**Note:** 2SLS estimations on the effect of the ETS on $CO_2$ emissions in 2015 with firms covered since 2014 and 2015 included. Columns 2 and 3 compare effects for firms with high oil and coal consumption (>20% in 2012). Columns 4 and 5 show the estimations on firms in the industry and service sectors, respectively. Columns 6–9 show the estimations by firms' sector and ownership type. All columns include full set of sector, ownership, and energy type dummies. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.A.10: Effect of pilot ETS in Beijing on carbon emissions, quadratic polynomial

| | All | Heavy coal/oil users | | Sector | | Non state-related | | State-related | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Treat | -0.42 | -0.64 | -0.08 | -0.37 | -0.39 | -0.11 | 0.77 | -1.05* | -0.10 |
| | (0.34) | (0.39) | (0.57) | (0.33) | (0.52) | (0.54) | (1.26) | (0.57) | (0.24) |
| Observations | 536 | 133 | 403 | 137 | 399 | 91 | 149 | 46 | 250 |
| 1st stage F stat. | 7.20 | 13.76 | 1.40 | 7.80 | 3.21 | 3.36 | 0.61 | 2.95 | 16.08 |
| Sample | Full | Yes | No | Industry | Service | Industry | Service | Industry | Service |

**Note:** 2SLS estimations on the effect of the ETS on $CO_2$ emissions in 2015 using a quadratic polynomial as a functional form for $g(.)$ in equation (2.1). Columns 2 and 3 compare effects for firms with high oil and coal consumption (>20% in 2012). Columns 4 and 5 show the estimations on firms in the industry and service sectors, respectively. Columns 6–9 show the estimations by firms' sector and ownership type. All columns include full set of sector, ownership, and energy type dummies. Robust standard errors in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 2.A.11: Effect of pilot ETS in Beijing on carbon emissions, fossil users

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Treat | -0.77** | -0.73** | -0.77** | -0.78** | -0.97** | -1.02** | -1.03** | -1.25** | -0.97* | -0.97* | -1.61 | -1.53 | -0.98 | -1.26 |
| | (0.36) | (0.36) | (0.37) | (0.39) | (0.47) | (0.49) | (0.49) | (0.60) | (0.51) | (0.54) | (1.29) | (1.15) | (0.85) | (1.66) |
| Observations | 103 | 95 | 86 | 83 | 79 | 73 | 72 | 67 | 62 | 60 | 50 | 47 | 39 | 32 |
| Mean dependent var. | 9.01 | 8.99 | 8.96 | 8.96 | 8.95 | 8.93 | 8.92 | 8.90 | 8.88 | 8.87 | 8.83 | 8.82 | 8.78 | 8.74 |
| Sd. of dependent var. | 0.42 | 0.43 | 0.42 | 0.43 | 0.43 | 0.43 | 0.43 | 0.44 | 0.43 | 0.43 | 0.44 | 0.45 | 0.44 | 0.44 |
| 1st stage F stat. | 13.34 | 14.63 | 14.01 | 13.01 | 10.83 | 10.31 | 10.36 | 8.15 | 9.42 | 8.08 | 1.98 | 2.16 | 2.78 | 0.92 |
| Sample | >5% | >10% | >15% | >20% | >25% | >30% | >35% | >40% | >45% | >50% | >55% | >60% | >65% | >70% |

**Note:** 2SLS estimations on the effect of the ETS on $CO_2$ emissions in 2015 for heavy fossil users using varying share of coal and natural gas consumption. Respective share indicated in the last row. All columns include full set of sector, ownership, and energy type dummies. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.A.12: Effect of pilot ETS in Beijing on carbon emissions, non–fossil users

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| treat | 0.12 | -0.04 | -0.27 | -0.26 | -0.21 | -0.17 | -0.17 | -0.10 | -0.20 | -0.18 | -0.14 | -0.10 | -0.16 | -0.22 |
| | (0.85) | (0.77) | (0.59) | (0.53) | (0.47) | (0.44) | (0.43) | (0.33) | (0.36) | (0.34) | (0.28) | (0.27) | (0.28) | (0.29) |
| Observations | 225 | 233 | 242 | 245 | 249 | 255 | 256 | 261 | 266 | 268 | 278 | 281 | 289 | 296 |
| Mean dependent var. | 9.19 | 9.19 | 9.19 | 9.19 | 9.19 | 9.19 | 9.19 | 9.19 | 9.19 | 9.19 | 9.19 | 9.18 | 9.18 | 9.17 |
| Sd. of dependent var. | 0.35 | 0.35 | 0.35 | 0.35 | 0.35 | 0.35 | 0.35 | 0.35 | 0.35 | 0.35 | 0.35 | 0.35 | 0.35 | 0.35 |
| 1st stage F stat. | 0.56 | 0.69 | 1.49 | 1.78 | 2.15 | 2.34 | 2.44 | 3.75 | 3.47 | 3.70 | 5.21 | 5.46 | 5.65 | 5.97 |
| Sample | ≤5% | ≤10% | ≤15% | ≤20% | ≤25% | ≤30% | ≤35% | ≤40% | ≤45% | ≤50% | ≤55% | ≤60% | ≤65% | ≤70% |

**Note:** 2SLS estimations on the effect of the ETS on $CO_2$ emissions in 2015 for non–heavy-fossil users using varying share of coal and natural gas consumption. Respective share indicated in the last row. All columns include full set of sector, ownership, and energy type dummies. Robust standard errors in parentheses.

## 2.B Allowance Surplus and Emissions

Figure 2.B.1: First stage, the effect of emissions shocks in 2011 on allowance surplus in 2013

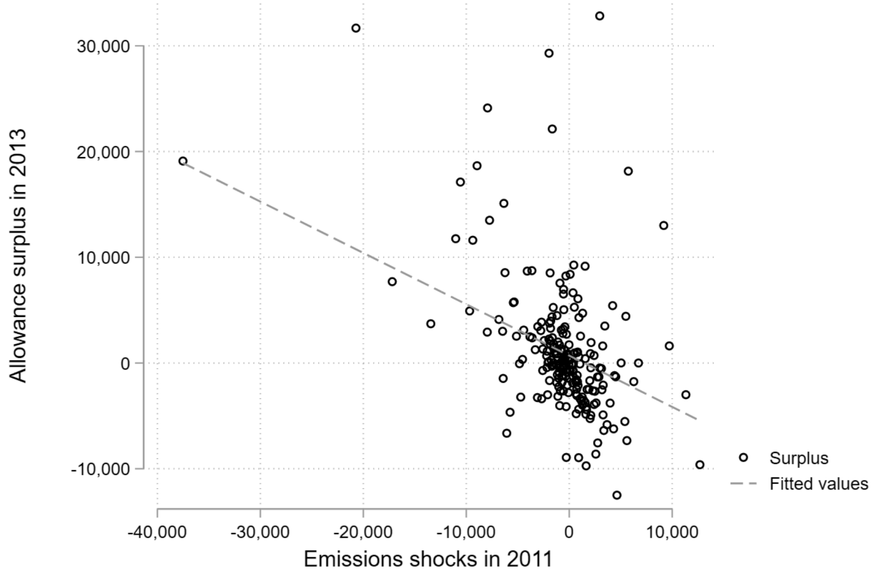



Table 2.B.1: IV estimations, full sample, Phase I

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | 2014 | 2015 | 2016 | 2017 |
| Surplus | 0.04 | 0.01 | -0.03 | -0.09 |
| | (0.10) | (0.14) | (0.16) | (0.24) |
| Observations | 226 | 226 | 226 | 226 |
| 1st stage F stat. | 21.86 | 21.86 | 21.86 | 21.86 |
| p-value of Hansen J | 0.29 | 0.14 | 0.41 | 0.30 |

**Note:** Specifications in all columns include sector dummies, ownership dummies, and average emissions between 2009 and 2012. Standard errors in parentheses.

Table 2.B.2: Tests for the monotonicity assumption

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| iv11 | -0.43*** | -0.61*** | -0.20 | -0.39*** | -0.47*** | -0.41*** | -0.33 | -0.43*** | -0.40** |
| | (0.11) | (0.15) | (0.24) | (0.13) | (0.14) | (0.09) | (0.22) | (0.11) | (0.17) |
| iv12 | -0.49*** | -0.60*** | -0.74*** | -0.53*** | -0.76*** | -0.46*** | -0.79*** | -0.59*** | -0.73*** |
| | (0.16) | (0.17) | (0.15) | (0.12) | (0.17) | (0.10) | (0.18) | (0.11) | (0.15) |
| Observations | 74 | 73 | 73 | 131 | 89 | 111 | 109 | 113 | 107 |
| Mean dependent var. | -1.42 | 451.94 | 3310.10 | 602.69 | 2197.42 | 1432.49 | 1059.78 | 1530.90 | 948.89 |
| Sd. of dependent var. | 5813.16 | 4235.48 | 7956.44 | 5219.54 | 7621.89 | 5273.34 | 7278.91 | 6534.16 | 6133.86 |
| Sample | 1st tertile | 2nd tertile | 3rd tertile | Service | Industry | Below median | Above median | State related | Non-state related |
| R-squared | 0.59 | 0.49 | 0.55 | 0.58 | 0.50 | 0.66 | 0.63 | 0.63 | 0.46 |

**Note:** Specifications in all the columns include sector dummies, ownership dummies, and average emissions between 2009 and 2012. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.B.3: Does surplus status explain emissions?

| | (1) 2014 | (2) 2015 | (3) 2016 | (4) 2017 |
|---|---|---|---|---|
| ***Panel A: OLS*** ***(**$N = 220$**)*** | | | | |
| $\mathbf{1}(surplus > 0)$ | -240.54 | 439.96 | 1529.05 | 730.54 |
| | (662.41) | (900.57) | (1064.59) | (1115.32) |
| R-squared | 0.14 | 0.21 | 0.22 | 0.24 |
| Mean | -2311.48 | -4049.84 | -4865.23 | -5842.60 |
| ***Panel B: IV, all*** ***(**$N = 220$**)*** | | | | |
| $\mathbf{1}(surplus > 0)$ | 32.15 | 136.43 | 376.40 | -1265.29 |
| | (1428.43) | (1967.23) | (2263.02) | (2409.86) |
| 1st stage F stat. | 35.99 | 35.99 | 35.99 | 35.99 |
| p-value of Hansen J | 0.39 | 0.18 | 0.19 | 0.24 |
| Mean | -2311.48 | -4049.84 | -4865.23 | -5842.60 |
| ***Panel C: IV, by sector: service*** ***(**$N = 131$**)*** | | | | |
| $\mathbf{1}(surplus > 0)$ | 894.80 | -1375.66 | -124.85 | -1575.76 |
| | (1349.97) | (2650.48) | (2806.73) | (3087.53) |
| 1st stage F stat. | 15.99 | 15.99 | 15.99 | 15.99 |
| p-value of Hansen J | 0.14 | 0.14 | 0.09 | 0.15 |
| Mean | -2356.96 | -3400.92 | -3540.38 | -4439.74 |
| ***Panel D: IV, by sector: industry*** ***(**$N = 89$**)*** | | | | |
| $\mathbf{1}(surplus > 0)$ | -1891.65 | 257.06 | 840.68 | -172.42 |
| | (2314.16) | (3058.62) | (3560.33) | (3744.29) |
| 1st stage F stat. | 21.44 | 21.44 | 21.44 | 21.44 |
| p-value of Hansen J | 0.50 | 0.83 | 0.67 | 0.72 |
| Mean | -2244.53 | -5005.01 | -6815.29 | -7907.48 |
| ***Panel E: IV, by size: below*** ***(**$N = 111$**)*** | | | | |
| $\mathbf{1}(surplus > 0)$ | -195.92 | 281.04 | 876.29 | -305.17 |
| | (1833.16) | (2682.44) | (3143.17) | (3329.59) |
| 1st stage F stat. | 14.53 | 14.53 | 14.53 | 14.53 |
| p-value of Hansen J | 0.21 | 0.31 | 0.31 | 0.32 |
| Mean | -2549.40 | -4451.11 | -5474.38 | -6896.75 |
| ***Panel F: IV, by size: above*** ***(**$N = 107$**)*** | | | | |
| $\mathbf{1}(surplus > 0)$ | 655.07 | 1196.39 | 1384.04 | -583.03 |
| | (2108.13) | (2686.26) | (3079.24) | (3284.87) |
| 1st stage F stat. | 20.09 | 20.09 | 20.09 | 20.09 |
| p-value of Hansen J | 0.10 | 0.87 | 0.90 | 0.90 |
| Mean | -2069.19 | -3641.21 | -4244.90 | -4769.10 |
| ***Panel G: IV, by ownership: non state-related*** ***(**$N = 107$**)*** | | | | |
| $\mathbf{1}(surplus > 0)$ | 158.91 | 1193.89 | 447.81 | -29.80 |
| | (1709.85) | (2196.04) | (2643.56) | (2755.70) |
| 1st stage F stat. | 25.04 | 25.04 | 25.04 | 25.04 |
| p-value of Hansen J | 0.64 | 0.16 | 0.10 | 0.10 |
| Mean | -2634.98 | -4773.40 | -5751.29 | -6473.15 |
| ***Panel H: IV, by ownership: state-related*** ***(**$N = 113$**)*** | | | | |
| $\mathbf{1}(surplus > 0)$ | 492.30 | -881.56 | 2016.68 | -635.83 |
| | (2458.44) | (3426.47) | (3696.67) | (3950.70) |
| 1st stage F stat. | 14.53 | 14.53 | 14.53 | 14.53 |
| p-value of Hansen J | 0.40 | 0.53 | 0.59 | 0.83 |
| Mean | -2005.14 | -3364.70 | -4026.21 | -5245.53 |

**Note:** Specifications in all columns include sector dummies, ownership dummies, and average emissions between 2009 and 2012. Standard errors in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 2.B.4: Does surplus explain emissions–shock 2011 as the instrument?

| | (1) 2014 | (2) 2015 | (3) 2016 | (4) 2017 |
|---|---|---|---|---|
| ***Panel A: IV, all ($N = 220$)*** | | | | |
| Surplus | 0.13 | 0.34 | 0.36 | 0.39 |
| | (0.16) | (0.22) | (0.23) | (0.25) |
| 1st stage F stat. | 23.51 | 23.51 | 23.51 | 23.51 |
| ***Panel B: IV, by sector: service ($N = 131$)*** | | | | |
| Surplus | 0.30* | 0.44* | 0.45 | 0.38 |
| | (0.18) | (0.25) | (0.27) | (0.27) |
| 1st stage F stat. | 15.27 | 15.27 | 15.27 | 15.27 |
| ***Panel C: IV, by sector: industry ($N = 89$)*** | | | | |
| Surplus | -0.31 | -0.14 | -0.08 | 0.20 |
| | (0.36) | (0.36) | (0.40) | (0.49) |
| 1st stage F stat. | 5.40 | 5.40 | 5.40 | 5.40 |
| ***Panel D: IV, by size: below ($N = 111$)*** | | | | |
| Surplus | 0.28 | 0.45* | 0.48* | 0.54** |
| | (0.20) | (0.27) | (0.27) | (0.25) |
| 1st stage F stat. | 16.89 | 16.89 | 16.89 | 16.89 |
| ***Panel E: IV, by size: above ($N = 109$)*** | | | | |
| Surplus | -0.14 | 0.11 | 0.15 | 0.19 |
| | (0.27) | (0.30) | (0.35) | (0.43) |
| 1st stage F stat. | 8.12 | 8.12 | 8.12 | 8.12 |
| ***Panel F: IV, by ownership: non state-related ($N = 107$)*** | | | | |
| Surplus | 0.03 | 0.36 | 0.37 | 0.56 |
| | (0.29) | (0.30) | (0.37) | (0.51) |
| 1st stage F stat. | 11.57 | 11.57 | 11.57 | 11.57 |
| ***Panel G: IV, by ownership: state-related ($N = 113$)*** | | | | |
| Surplus | 0.20 | 0.31 | 0.38 | 0.34 |
| | (0.18) | (0.24) | (0.26) | (0.25) |
| 1st stage F stat. | 21.34 | 21.34 | 21.34 | 21.34 |

**Note:** Specifications in all the columns include sector dummies, ownership dummies, and average emissions between 2009 and 2012. Standard errors in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

# Chapter 3

# Assessing the Supply Chain Effect of Natural Disasters: Evidence from Chinese Manufacturers

**Abstract**

This paper uses Chinese firm level data to detect the international propagation of adverse shocks triggered by the US hurricane season in 2005. We provide evidence that Chinese processing manufacturers with tight trade linkages to the United States reduced their intermediate imports from the United States between July and October 2005. We further show that the direct exposure to US supply shocks led to a temporary decline of firm exports between September and November 2005, although we do not find consistent evidence of international propagation of supply shocks along global value chains. Moreover, the paper finds that firms with more diversified suppliers tend to be less affected by the US hurricane disaster, pointing to firm sourcing diversification as a way to increase resilience to adverse shocks.

This chapter is joint work with Katharina Längle (WTO) and Ankai Xu (WTO).

## 3.1 Introduction

In 2020, the outbreak of the COVID-19 pandemic drastically demonstrated how an adverse shock can abruptly halt social lives and economic activity around the globe. The pandemic has also heightened an emerging debate on the role of global value chains as amplifiers or absorbers of economic shocks provoked by pandemics and natural disasters. On the one hand, trade openness and integration in global production networks trigger a higher risk for disruptions of production processes as adverse shocks abroad can propagate along trading routes and value chains (Baldwin and Freeman, 2020; Carvalho et al., 2021; Acemoglu et al., 2012). On the other hand, there exists empirical evidence that diversified production networks support firms in coping with adverse shocks thereby enabling firms to quickly resume business operations (Caselli et al., 2020; Todo et al., 2015; Miroudot, 2020).

As the frequency and intensity of natural shocks such as epidemics, flood, and storms are projected to be on the rise, partly as a result of climate change, it is crucial to understand how shocks are transmitted through global value chains and under which circumstances global value chains contribute to economic resilience and recovery. The present paper fills this research gap by studying the effect of the US hurricane season in 2005 on the export performance of Chinese manufacturers. The 2005 hurricane season represented a substantial negative economic shock for an advanced country like the United States, so there is a high probability that its economic consequences spilled over to other industries across different countries. We focus on this hurricane season because two of the hurricanes that occurred during the season were among the three costliest and most devastating in US history (NHC, 2014, 2011). The United States was the fourth-largest intermediate input source for Chinese processing firms in terms of trade value in 2006, despite the geographical distance between the two countries, and the trade relationship between them has become increasingly important over the past decade.

In this context, it is particularly interesting to study economic consequences for firms in China, as the country has rapidly integrated into the world economy over the past few decades and continues to cover a growing number of production steps along global value chains (Kee and Tang, 2015; Wang et al., 2017; Criscuolo and Timmis, 2018). As a result of the strong integration in global value chains, Chinese manufacturers are especially exposed to adverse shocks provoked by natural disasters, so both the propagation of shocks and benefits from diversification are likely to be detected.

In this paper, we first investigate whether supply shocks tend to propagate directly and indirectly via import-export linkages. Second, we focus on identifying determinants that make firms more resilient to supply chain interruptions.

To investigate the propagation of the natural disaster shock, we measure the extent to which a US supply shock affects Chinese firms with direct imports from affected US states. The theoretical background of the paper builds on the multi-country sourcing model first developed by Antràs et al. (2017) and extended by Huang (2017) to show that firms more diversified in sourcing are more resilient to supply chain disruptions.

There are two major identification challenges in our empirical analysis. First, we study the 3 hurricanes that are the most devastating during the 2005 hurricane season. Given that these hurricanes affected only 7 out of 50 states, not all trade flows between the United States and China were affected by these natural disasters. Second, the Chinese firm-level data from custom authorities used in this paper contain no information about firm-level domestic production linkages. In view of the growing part of global value chains covered by Chinese manufacturers, this lack of information is problematic (Wang et al., 2017). To overcome the first identification issue, we leverage trade data of individual states and focus on sectors that are highly concentrated in affected US regions. To overcome the second issue, we focus our analysis on processing firms, as this allows us to minimise the 'black box' of domestic production linkages among firms.[1]

The key assumption in identifying the impact of US hurricane season on Chinese firms is that the unexplained factors that may affect Chinese firms' imports and exports with the United States are not correlated with the occurrence of the hurricanes. This is likely because even though natural disasters are likely to occur in certain locations, the exact location and magnitude of natural disasters are difficult to predict and therefore exogenous to any firm-specific trade pattern. In addition, to account for the fact that hurricanes are recurrent disasters in specific time periods, in most of the empirical specifications we consider firm performance indicators relative to the same months in the previous year.

Based on this data, we provide evidence that Chinese processing manufacturers with tight trade linkages to the United States saw a temporary decline in US intermediate imports between July and October 2005. More specifically, we find that such a decline occurred for Chinese processing manufacturers that, prior to the disaster, sourced more than 90% of their intermediate imports from US industries that are more concentrated in the hurricane-affected states. Moreover,

[1] In China, processing firms are characterised by the ability to use imported raw materials and intermediates without tariff charges in local production or assembling of export products (Yu, 2015).

we detect a statistically significant link between firms' *direct* exposure to supply shocks and their export performance. We also try to detect the *indirect* propagation of the shock through global value chains, although we did not find consistent evidence for a propagation of the supply shock via the international production network.[2]

The paper further investigates the heterogeneous effects of the 2005 US hurricane season on firms' resilience depending on their sourcing diversification. Defining resilience as the pass-through of a trade cost shock to a firm's marginal cost and imports as well as exports, we find that more diversified firms are more resilient to adverse shocks and are overall less volatile in exports. Furthermore, we find that Chinese processing firms heavily exposed to US supply shocks increased their diversification of suppliers in the aftermath of the US hurricanes.

The remainder of the paper is organised as follows. The subsequent section provides background information on the 2005 US hurricane season and reviews the related literature. Section 3.2 gives details on the data and descriptive statistics. Section 3.3 presents the empirical strategy and results for the *direct* effect of the US hurricane season on Chinese processing manufacturers. Section 3.4 provides evidence on the resilience and diversification of Chinese processing firms building on a theoretical framework. Section 3.5 concludes.

### 3.1.1 US Hurricanes

In 2005, the US southeast coast was hit by a devastating series of hurricanes. Between July and October, a total of 27 tropical storms formed, of which 3 storms developed into category 5 hurricanes, the maximum on the existing scale (National Aeronautics and Space Administration (NASA), 2006). According to the US National Hurricane Center (NHC), Hurricanes Katrina, Rita, and Wilma hit the United States in late August, September, and October, respectively.[3] With estimated damages of around $108 billion and 1,300 deaths, Hurricane Katrina ranks among the "most devastating natural disasters in US history" (NHC, 2011; National Aeronautics and Space Administration (NASA), 2006). Katrina mainly hit Louisiana, Mississippi, Florida, Georgia, and Alabama, where it left wide swaths of the landscape, homes, and businesses devastated. It caused power outages affecting around three million people, which in some cases lasted for several weeks

[2]In previous versions of the paper, our identification strategy focused on the quantification of the propagation of the supply shock via the international production network. Estimation results are not consistently significant. Thus, so as not to blur the analysis of the present paper, results on the network propagation of the supply shock are provided in Appendix 3.D.

[3]There were also other storms categorised as hurricanes in 2005, but we do not consider them in this paper. For a full list and more details of storms affecting the United States in 2005, see NHC (2005).

(NHC, 2011). Only about three weeks later, in late September, parts of Louisiana, Texas, Mississippi, Alabama, and Arkansas, as well as the Florida Keys, were hit by tornadoes and floodings caused by Hurricane Rita, with total damages of around $12.037 billion (National Hurricane Center (NHC), 2011). Economic damage was not only caused by direct destruction from the storm but also resulted from a halt in business as a consequence of large-scale evacuations of up to two million people, such as in Texas (National Hurricane Center (NHC), 2011). Southern Florida was subsequently hit by Hurricane Wilma in October 2005, causing damages of roughly $20.6 billion. Wilma ranks as the third-costliest hurricane in US history (behind Katrina, 2005, and Andrew, 1992) and accounted for the largest disruption of electrical service ever recorded in Florida (NHC, 2014).

Although the 2005 hurricane season resulted in significant damages, most of the effects were concentrated in seven states in the southeast region directly hit by the hurricanes: Alabama, Arkansas, Louisiana, Mississippi, Florida, Georgia and Texas. This is illustrated in Figure 3.1, where we plot the year-on-year export growth rate of the seven affected states compared with the other states. The seven states affected by the hurricane season experienced a significant drop in exports around the time the hurricanes hit, while the exports from other states remained relatively stable.

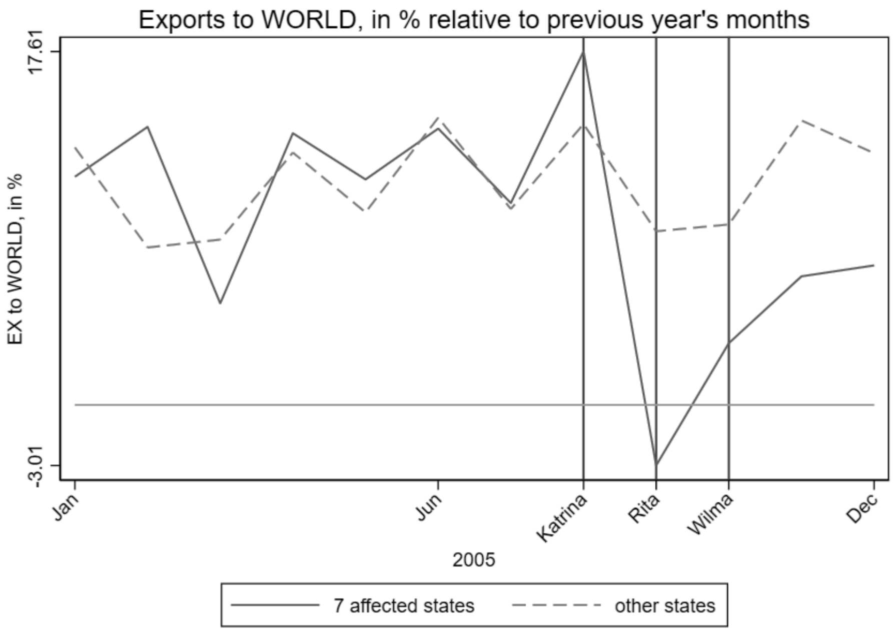
### 3.1.2 Literature Review

This paper can be placed in three threads of economic literature. First, it links to a well-established literature pointing to the fact that complementarities and multi-stage processing can lead to the amplification of shocks (Kremer, 1993). Problems at any point in a production chain can reduce output substantially if inputs enter production in a complementary fashion (Jones, 2011). A growing body of literature also focuses on the role of input-output networks as a mechanism to propagate and amplify shocks (Long and Plosser, 1983; Acemoglu et al., 2012). In particular, Acemoglu et al. (2012) posit that, if intersectoral input-output linkages exhibit asymmetries, a sectoral shock propagates strongly to the rest of the economy and affects aggregate outcomes.[4]

A related empirical literature documents the propagation of shocks over production networks. This includes a study by Acemoglu et al. (2016) that looks at

[4]More recent studies in this literature focus on the endogenous formation of production networks. Among them, Carvalho and Voigtländer (2014) and Acemoglu and Azar (2020) study the formation of production networks at the industry level, while Oberfield (2018), Baqaee (2018) and Lim (2018) look at the firm-level formation of production network. This literature can also be placed in a larger body of works studying the microeconomic origins of macroeconomic fluctuations, such as Gabaix (2011), di Giovanni et al. (2017) and Kramarz et al. (2020), who emphasise the role of firm-size distribution in translating micro shocks into macro fluctuations.

Figure 3.1: US exports growth to the world by hurricane-affected and unaffected states




**Note:** The figure plots the year-on-year growth rate of the value of exports from US states, calculated as $(EX_t - EX_{t-12})/EX_{t-12} \times 100\%$, where $EX_t$ indicates the export value of relevant states in a particular month. The solid line indicates the export growth of the seven states affected by the 2005 US hurricane season, while the dotted line indicates the export growth of other states.

the transmission of shocks at the industry level and a number of other studies that look at the propagation of shocks at the firm level (Barrot and Sauvagnat, 2016; Carvalho et al., 2021; Baldwin and Freeman, 2020; Dhyne et al., 2021; Huneeus, 2018; Demir et al., 2018). Among them, several studies exploit natural disasters as exogenous shocks to examine the propagation of such shocks in supply networks. For example, Barrot and Sauvagnat (2016) show that input specificity is a key determinant of the propagation of firm-level shocks. Firms' sales growth and stock prices drop significantly only when a major disaster hits one of their specific suppliers. Studying the 2011 Japanese earthquake, Carvalho et al. (2021) document that the disruption caused by the disaster propagated upstream and downstream along supply chains, affecting the direct and indirect suppliers and customers of disaster-stricken firms. The authors estimate that the earthquake and its aftermaths resulted in a 0.47 percentage point decline in Japan's real GDP growth in the year following the disaster.

While these studies look at the propagation of shocks within a country, there is limited evidence on the international transmission of shocks. Boehm et al. (2019) focus on the *direct* impact of the 2011 Japanese earthquake on imports of US-based Japanese multinationals in the months following the disaster. The authors find that the output of Japanese multinationals fell by a magnitude comparable to the drop in imports, indicating a very rigid supply chain relationship. Our paper fits into this literature by documenting both the *direct* and *indirect* propagation of a major natural disaster shock in the United States on the performance of Chinese processing firms. Closest to our paper is the study by Kashiwagi et al. (2018), who investigate the indirect effects of shocks by Hurricane Sandy, which hit the United States in 2012, and show that the effect on their trade partners outside the United States is insignificant.

Second, this paper contributes to understanding the role of trade and diversification in mitigating the negative consequences of shocks. In this regard, it is related to the 'technological diversification' mechanism used by Koren and Tenreyro (2013), who explain the country-level output volatility in a model with endogenous growth. Caselli et al. (2020) show that openness to international trade can lower income volatility by reducing exposure to domestic shocks and allowing countries to diversify the sources of demand and supply across countries, as long as country-wide shocks are important (as opposed to sector-specific shocks).

Our paper relates to empirical works on diversification, resilience, and volatility. Among them, Todo et al. (2015) examine how supply chain networks affected the resilience of firms (defined as the amount of time required to recover production) after the 2011 Great East Japan Earthquake and find that the positive effect of supply chain diversification exceeds the negative effect of higher exposure to disruptions. Hamano and Vermeulen (2019) study the effect of natural disasters on port-level exports after the 2011 Great East Japan Earthquake. They find that at least 40% of exports was substituted to other ports following the disaster, and the substitution effect is the strongest in technology-intensive industries. Huang (2017) looks at the diversification in global sourcing and the resilience of Chinese firms after the 2003 SARS epidemic and finds that firms with more diversified sourcing strategies are associated with higher resilience and lower volatility. Other papers link diversification with aggregate volatility. For instance, Burgess and Donaldson (2010) consider the specific case of railway expansion in India and demonstrate that the decline in transportation costs in India lowered the impact of productivity shocks on real income, implying a reduction in volatility. In comparison, our paper focuses on supplier diversification as a means to mitigate the impact of shocks from upstream suppliers on downstream firms. We also document that firms with more diversified sourcing strategies tend to have lower export volatility and that firms exposed to supply disruptions increased their level of diversification after a natural disaster.

Third, this paper also connects to a literature quantifying the economic consequences of natural disasters. Among them, some studies quantify the average effect of natural disasters on trade and economic output (Gassebner et al., 2010; Andrade da Silva et al., 2012; Cavallo et al., 2013; Felbermayr and Gröschl, 2014; Xu and Kouwoaye, 2019). Most find that exports seem to be affected negatively by the occurrence and severity of disasters, while the effects on imports are ambiguous (Osberghaus, 2019). A number of recent studies have investigated the effects of individual natural disaster events, such as the 2011 Great East Japan Earthquake (Boehm et al., 2019; Carvalho et al., 2021; Todo et al., 2015), the 2003 outbreak of SARS in China (Huang, 2017; Fernandes and Tang, 2020), and the Thai Flood in 2011 (Haraguchi and Lall, 2015). Pelli and Tschopp (2017) find that firms shift resources toward industries with a higher comparative advantage within the three years following a hurricane shock. Zhu et al. (2016) show that the 2011 Japanese earthquake had a positive effect on firms' offshoring in manufacturing activities, possibly because the damaged transport network in the Tohoku area forced some manufacturing firms to replace domestic contractors with foreign contractors. Todo and Inoue (2021) document that Japanese firms increased their level of supplier diversification between 2006 and 2016. Our paper adds to the literature by studying the impact of the 2005 US hurricane season, with a focus on the transmission of negative supply shock to the performance of downstream firms through global value chains.

## 3.2 Data Source and Descriptive Statistics

In this section, we describe the source of the data and provide several empirical facts that motivate our analysis.

### 3.2.1 Data Source

The data used in the paper are taken from three sources. The firm-level data are primarily from China Customs Statistics, which contain administrative customs data on product-level trade transactions by HS8 product and respective trade partners on a monthly frequency for individual Chinese firms between 2001 and 2006. Besides information on a unique time and firm identifier, a firm's name, the product code, trade partners, and values of transactions, this data set also contains information on quantities traded, a firm's address, its phone number, and its zip code, as well as identifiers for processing trade. A detailed explanation of the raw data set is provided in Appendix 3.B.

To control for any reporting irregularities at the disaggregated HS-8 product level, we aggregate flows by firm at the HS-6 product level and convert all HS-6 product codes to HS Rev. 2007. Based on these unified product codes, we classify goods as intermediates using the Broad Economic Categories (BEC) classification (Rev. 4) and assign them to different two-digit ISIC (Rev. 3) manufacturing industries.[5] Moreover, transactions with a value of less than \$500 are dropped, as well as observations without information on the firm identifier, the date, the transaction value, or the import-export identifier.

We perform the following steps to process the data for our analysis. First, we identify and exclude all intermediaries that act as a link between manufacturers and customers, since these firms do not perform manufacturing activities themselves and thus respond differently to supply chain disruptions (Bernard et al., 2011). We rely on the data cleaning procedure proposed by Ahn et al. (2011) and drop all firms whose names contain the Chinese equivalents of 'exports', 'imports', 'imports and exports' or 'trade'. Second, we further remove observations that indicate trading partner as 'China', since according to China customs, destination or origin of 'China' is often assigned to goods consignments that have not been traded internationally, such as the movements of goods in and out of a special economic zone. Third, we focus our analysis on processing firms to capture the effect of import disruptions on exports and minimise the 'black box' of

[5]Given that data are merged with information from the OECD ICIO database, which aggregates two-digit ISIC industries to different subcategories, our industry classification follows the OECD ICIO aggregation. A list of industries is provided in Table 3.B.1 in Appendix 3.B.

domestic production linkages. Processing firms are defined as those that have any processing transactions for a given year. Table 3.1 presents an overview of the number of firms as well as firm-product observations for the cleaned sample and the subsample of processing firms considered in this paper.

Table 3.1: Number of firms & observations in Chinese customs statistics, 2001–2006

| Year | Raw data | Cleaned sample | | Processing firms | |
|---|---|---|---|---|---|
| | # firms | # firms | # firm-prod. | # firms | # firm-prod. |
| 2001 | 89,403 | 74,824 | 1,058,433 | 30,781 | 669,828 |
| 2002 | 103,017 | 86,680 | 1,174,884 | 31,800 | 661,589 |
| 2003 | 122,336 | 101,423 | 1,340,037 | 36,210 | 741,592 |
| 2004 | 151,327 | 123,558 | 1,598,692 | 38,089 | 797,150 |
| 2005 | 179,407 | 153,395 | 1,987,158 | 47,357 | 932,608 |
| 2006 | 207,872 | 162,811 | 2,078,356 | 48,912 | 995,388 |

As a second data source, the paper relies on trade data from the US Census Bureau accessed via *USA Trade Online* (US Census Bureau, 2020). This data set provides bilateral trade data at the HS-6 product level by US state at a monthly frequency. Thus, data are available defined by the state of origin.

As a third source, information on input-output linkages among industries is taken from the OECD ICIO database, 2016 edition (Organisation for Economic Co-operation and Development (OECD), 2016). This data set contains information on the intermediate use, final demand, value added, and output of industries in 63 different countries plus an aggregate rest-of-the-world region between 1995 and 2011. Importantly, the OECD ICIO database also provides specific information on processing industries in Mexico and China. Such information allows us to precisely determine international production linkages for processing firms, which is crucial because of the focus of this paper on this specific subgroup of firms.

### 3.2.2 Descriptive Evidence

Firms source multiple inputs from multiple countries. We provide evidence on the number of intermediate inputs imported and the number of products exported by Chinese processing firms in Table 3.2. On average, importers sourced 40 inputs from three foreign countries in 2006. However, this result was largely driven by a small number of firms that sourced a large variety of inputs. A median Chinese processing firm sourced 13 intermediate inputs from one country. Regarding exports, the Chinese processing firms exported to a higher number of destination markets with a lower number of varieties: the median firm exported 5 HS-6 products to four destinations on average in 2006.

Table 3.2: Firm-level statistics on the number of sourcing and exporting countries and HS-6 products

| | # source & destination countries per HS6 product | | | | # HS6 products per source & destination country | | | |
|---|---|---|---|---|---|---|---|---|
| | Median | Mean | Std. dev. | Max | Median | Mean | Std. dev. | Max |
| **Intermediate imports** | | | | | | | | |
| 2004 | 1 | 2.76 | 3.26 | 43 | 13 | 46.82 | 133.49 | 1,402 |
| 2005 | 1 | 2.67 | 3.17 | 39 | 12 | 39.47 | 113.45 | 1,342 |
| 2006 | 2 | 2.86 | 3.21 | 43 | 13 | 41.54 | 124.65 | 1,477 |
| **Total exports** | | | | | | | | |
| 2004 | 4 | 10.71 | 14.15 | 129 | 5 | 28.47 | 91.35 | 1,105 |
| 2005 | 3 | 10.41 | 14.12 | 138 | 6 | 23.37 | 70.80 | 1,002 |
| 2006 | 4 | 11.68 | 15.05 | 145 | 5 | 23.07 | 79.85 | 1,187 |

**Source:** Compiled from the Chinese customs data.
**Note:** The first four columns report statistics on the number of countries from which a firm imported HS-6 intermediate inputs and to which a firm exported HS-6 products. The last four columns report statistics on the number of HS6 products that a firm imported from a source country or exported to a destination country.

Second, we provide information on the countries and economies Chinese processing firms sourced from. Table 3.3 reports the top 10 source economies for Chinese processing firms in 2006. Chinese Taipei was the largest source of intermediate inputs in terms of number of importers, followed by Japan, Hong Kong, China, and South Korea. As firms sourced from multiple locations, the percentages sum up to more than 100%. Japan was the largest source of inputs in terms of value of imports, followed by Chinese Taipei and South Korea. The United States was the fifth-largest source of intermediate inputs in terms of the number of importers, with about 27% of Chinese processing firms sourced from the United States in 2006; it was the fourth-largest source of inputs in terms of value, with about 10% of the value of intermediate inputs sourced from the United States. The sourcing pattern suggests that firms' sourcing decisions tended to be inversely correlated with distance: nearby sources were more likely to be the top providers of intermediate inputs to Chinese processing firms.

Third, we document the pattern of firm-level sourcing diversification using the Herfindahl-Hirschman Index (HHI). The HHI sums over the squares of input expenditure share from all sources, for each firm and each imported intermediate HS6 product, and the input expenditure share is measured by the share of source-specific inputs in total inputs. This can be expressed as $HHI_{fp} \equiv \sum_s \chi_{fps}^2$, where $f$ stands for firm, $p$ product, and $s$ source, and $\chi_{fps}$ represents the input expenditure share from each source per firm per imported intermediate product. The HHI measures the sourcing concentration level: a value of 1 indicates full concentration (i.e., only one supplier), and a value close to zero indicates full di-

Table 3.3: Top 10 source economies for Chinese processing firms, 2006

| Source | Rank by Firms | Rank by Value | Number of importers: Firms | Number of importers: Percentage of total | Value of imports: Imports (million USD) | Value of imports: Percentage of total |
|---|---|---|---|---|---|---|
| Chinese Taipei | 1 | 2 | 20,757 | 47% | 49,658 | 28% |
| Japan | 2 | 1 | 19,193 | 44% | 53,960 | 31% |
| Hong Kong, China | 3 | 5 | 19,189 | 44% | 16,007 | 9% |
| South Korea | 4 | 3 | 18,008 | 41% | 46,718 | 27% |
| United States | 5 | 4 | 11,845 | 27% | 18,341 | 10% |
| Germany | 6 | 8 | 8,064 | 18% | 7,441 | 4% |
| Thailand | 7 | 9 | 6,619 | 15% | 7,039 | 4% |
| Singapore | 8 | 7 | 5,983 | 14% | 8,430 | 5% |
| Malaysia | 9 | 6 | 5,621 | 13% | 11,297 | 6% |
| Italy | 10 | 11 | 5,305 | 12% | 2,336 | 1% |

**Note:** The table reports the top 10 economies from which Chinese processing firms imported in 2006. The sample is the universe of Chinese processing firms after data-cleaning.

versification (i.e., intermediate imports spread over many suppliers). While the number of economies from which a firm sources represents the extensive margin of sourcing, the HHI captures both the intensive and extensive margins of sourcing.[6]

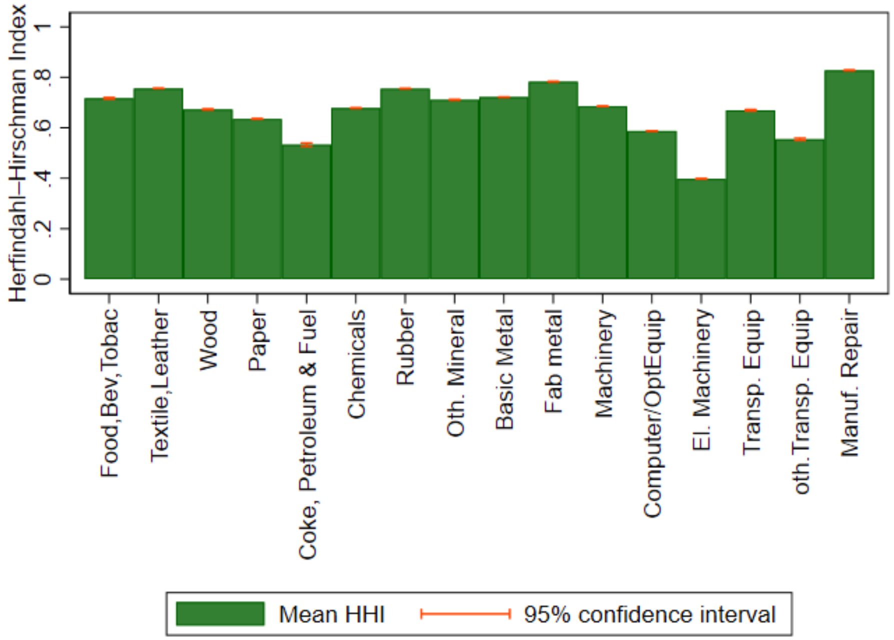
Figure 3.2 plots the HHI by industry, where we aggregate the HHI at HS6 level to industry level weighted by trade value. The average HHI ranges between 0.4 and 0.8, with the *manufacturing & repairing* industry having the highest concentration and *electrical machinery* the lowest concentration.

## 3.3 The *Direct* Effect of the US Trade Shock

In this section, we evaluate to what extent Chinese processing manufacturers were *directly* affected by the US hurricanes. In Section 3.3.1, we investigate whether exports and intermediate imports of Chinese processing firms are sensitive to negative shocks triggered by the US hurricane season. Second, based on these results, in Section 3.3.2, we assess whether firms' *direct* exposure to US hurricane supply shocks is associated with a decline in output. A complementary analysis of *indirect* effects of the US trade shock propagating via the international production network is provided in Appendix 3.D.

[6]To understand this, consider two firms, $A$ and $B$. Firm $A$ sources from two economies, with each contributing $\frac{1}{2}$ of total inputs; firm $B$ sources from three economies, with one contributing $\frac{3}{4}$ and the other two contributing $\frac{1}{8}$. The concentration of firm $A$'s sourcing strategy measured by HHI is $\frac{1}{2}^2 + \frac{1}{2}^2 = \frac{1}{2}$ and the HHI for firm $B$ is $\frac{3}{4}^2 + \frac{1}{8}^2 + \frac{1}{8}^2 = \frac{19}{32} > \frac{1}{2}$. So $B$ looks more diversified by the extensive margin, but less diversified after taking the intensive margin into account.

Figure 3.2: Herfindahl-Hirschman index (HHI) at sector level




**Note:** The Herfindahl–Hirschman index (HHI) is calculated as the sum over the squares of input expenditure share from all sources, while the input expenditure share is measured by the share of source-specific inputs in total inputs for each firm at an HS-6 product level at a quarterly interval for years 2004 to 2006. The HHI is then aggregated to sector level using the trade value as weights.

### 3.3.1 Chinese Firm-Level Trade Flows during the US Hurricane Season

We begin by presenting the reduced-form evidence of the impact of the 2005 US hurricane season on firm-level US trade. We rely on a dynamic treatment effect specification. Accordingly, US-specific trade flows and respective extensive and intensive margins are regressed on time dummies for the calendar months around a disaster as well as interactions of these time dummies with an indicator for the treated group.

#### 3.3.1.1 Empirical Strategy

We estimate the following model that captures the dynamic treatment effects of negative shocks on trade.

$$V_{fpt} = \alpha_f + \sum_{t=-3}^{2} \beta_t M_t + \sum_{t=-3}^{2} \gamma_t M_t \cdot TREATMENT_{fp}^{V} + \zeta_t XRATE_t + \epsilon_{fit}, \quad (3.1)$$

where $V_{fpt}$ refers to US exports ($EX$) and intermediate imports ($IMI$) measured in levels of firm $f$ for product $p$ in month $t$.[7] $M_t$ indicates the six months from June to November 2005, with the hurricanes hitting the United States in August–November. To control for any time-specific shocks on firm $f$'s exports or imports,[8] we include firm fixed effects, $\alpha_f$, to control for time-invariant, unobserved firm characteristics. The dummy variable $TREATMENT_{fp}^{V}$ equals one if the trade flow of a Chinese processing firm $f$ in product $p$ is assigned to the treatment group. Moreover, China reformed its exchange rate regime in July 2005, which may have systematically affected Chinese firms' imports and exports. To control for the effect of such a reform on Chinese processing firms' imports and exports, we include a dummy variable $XRATE_t$ equal to 1 for months from July 2005 onward and 0 otherwise to take into account the revaluation of the Chinese yuan against the USD (Reuters, 2012).[9] The interaction term equals 1 if a firm had a trade flow with the states that were heavily hit by the hurricane season. The coefficients of interest are captured by $\gamma_t$, which estimate the differences of imports or exports of affected firms before and after the natural disasters took place.

One challenge in defining the treatment group is the fact that the hurricanes affected only 7 out of 50 states. Therefore, *not all* trade flows from and to the United States were affected by the hurricane. We use the following two criteria to define the treatment group. First, a firm's trade value with the United States must account for more than 90% of a firm's import and export of a given product prior to the disaster. We choose the threshold of 90% based on the density

[7] Following Boehm et al. (2019), we prefer to capture the trade flow $V_{fpt}$ in levels for two reasons. First, measuring the dependent variable in levels allows us to include missing values as zeros. This is particularly important when firms' trade is interrupted for a certain time period by adverse shocks, such as natural disasters. Accordingly, we maintain zero trade flows in the sample by replacing missing values with 0 when a firm is 'active'. A firm is defined as 'active' if it first appeared in the full sample from 2001 to 2006 until its definite exit. Second, the specification of dependent variables in levels implicitly weights firms by their relative size.

[8] We remove firm-industry-specific trends from dependent variables, thus controlling for different development patterns of companies over the considered time span. Further controls for common seasonal patterns across firms are not necessary, as the treatment and control groups should follow the same seasonal fluctuations.

[9] The results of the dynamic treatment estimation are consistent including or excluding this dummy variable.

distribution of pre-disaster US trade shares. Density plots of US export and intermediate import shares are provided in Figure 3.C.1 in Appendix 3.C. Second, we distinguish manufacturing industries that are relatively more concentrated in affected states based on state-specific trade flows.[10] Therefore, Chinese processing manufacturers are assigned to the treatment group if their US trade share for a given product exceeds 90% and if they are importing from or exporting to a manufacturing industry that is relatively more concentrated in affected states than other industries.[11] Firms that do not meet the above two criteria at the same time are in the control group. Accordingly, around 4.5% and 5% of firms are assigned to the treatment group when intermediate imports and exports are considered, respectively.

It is worth highlighting further technical details about the estimations of equation (3.1) as well as the considered product scope. One concern is that firms might self-select into the treatment group based on their size or their industries. To address this concern, we weight firms in the control group by the propensity scores of individual firms assigned to the treatment group. Thus, firms in the control group that share similar characteristics with firms in the treatment group are assigned a higher weight.[12] Accordingly, we estimate the likelihood of being assigned to the treatment group using a probit model, where we include dummies containing information on whether a firm exports or imports in a certain sector as well as the export and intermediate import values prior to the disaster.[13]

Moreover, it is important to highlight that we focus on processing firms' imports of *intermediate* goods rather than all kinds of goods. With respect to exports, however, we consider the whole range of products exported by processing manufacturers–namely, intermediate and final goods. We do this for the following two reasons. First, in the context of global value chains, it is of particular

[10]Details on export and import shares of affected US states by manufacturing industry are presented in Table 3.C.1 in Appendix 3.C.

[11]According to this criterion, Chinese processing manufacturers that operate in the following industries are assigned to the treatment group if their export (intermediate import) share with the United States exceeded 90% prior to the disaster: For Chinese *importers*: textile; pulp, paper; coke; chemicals; machinery; electrical and optical equipment. For Chinese *exporters*: coke; machinery; electrical and optical equipment; wood; other non-metals; basic metals.

[12]We calculate firm-industry-specific weights as $weight_{fi} = \frac{p_{fi}}{1-p_{fi}}$, where $p_{fi}$ stands for the propensity score of being assigned to the treatment group. Firms with propensity scores of more than 50% are weighted by a number greater than 1, while firms with propensity scores smaller than 50% are weighted by a number smaller than 1. Weights for firms in the treatment group are 1.

[13]More specifically, we estimate the following model where $I_\omega$ refers to a dummy equal one if a firm $f$ exports in industry $i \in [1, N]$; $avgEX_f$ and $avgIMI_f$ measure a firm $f$'s average exports and intermediate imports, respectively, prior to the disaster between August 2004 and July 2005:

$$TREATMENT^{V}_{fi} = \sum_{i=1}^{N} \alpha_i I_i + \beta avgEX_f + \gamma avgIMI_f + u_{fi} \tag{3.2}$$

interest to investigate to what extent imported inputs are further processed to be eventually embodied in final or intermediate goods' exports. Imports on final goods tend to reflect consumers' consumption habits in an economy rather than firms' involvement in global production sharing. Therefore, we exclude imports of final goods from our analysis and focus on intermediate imports instead. Second, we consider the whole range of exports with regard to the relative downstream position of Chinese firms in global value chains and their role as a global assembling hub especially during the early 2000s.[14]

#### 3.3.1.2 Results

Figure 3.3 plots the estimation results for the reduced-form evidence of equation (3.1) on Chinese exports to the United States (the upper three graphs) and intermediate imports from the US (the lower three graphs). Individual graphs show the coefficient plots for estimations of parameter $\gamma_\tau$ along with their 90% and 95% confidence intervals, indicated by the capped spikes and spikes, respectively. Accordingly, estimates indicate how the imports and exports of the affected Chinese processing firms changed before and after the US hurricane season, with the hurricanes hitting during July and October 2005.[15] The dependent variable is measured as normal trade flows and trade margins for both exports and intermediate imports. Therefore, the extensive trade margin captures the number of goods exported to the United States, and the intensive margin captures the average value exported to (imported from) the United States.

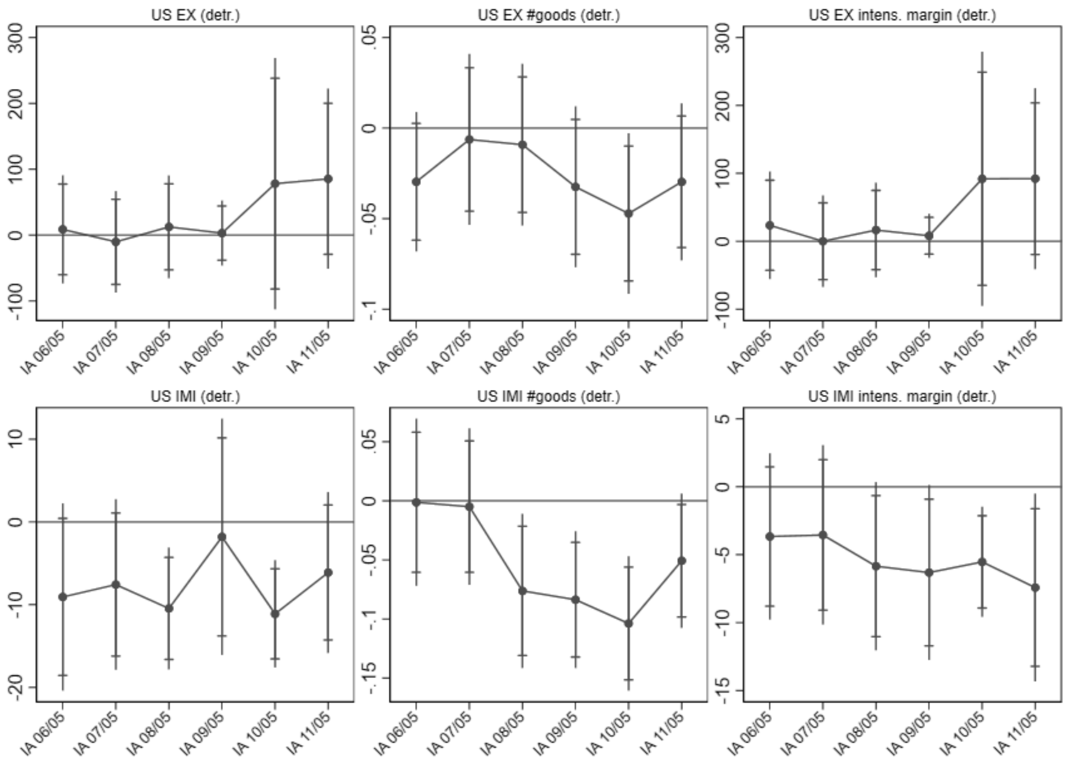
As shown in the upper three plots of Figure 3.3, exports of Chinese processing manufacturers with tight trade linkages to the United States did not significantly deviate from common exporting patterns of firms in the control group. However, with respect to the extensive margin, there was a decline in the number of goods exported to the United States starting in August 2005. We can therefore conclude that Chinese processing firms with a pre-disaster US trade share of more than 90% temporarily reduced the number of exported goods in industries that were highly concentrated in hurricane-affected states.

Considering estimation results for intermediate imports, the 2005 hurricane season appears to have played a more important role. As shown in the lower three plots of Figure 3.3, the overall intermediate imports of the treatment group significantly deviated from the sample's average estimate for October 2005. This is particularly driven by the extensive margin of intermediate imports: the num-

[14] Wang et al. (2013) consider China's trade position compared with that of the United States and show that Chinese exports of final goods include a relatively high share of foreign value added because of the use of foreign intermediates.

[15] Table 3.C.2 in Appendix 3.C provides the coefficient estimates of equation (3.1).

Figure 3.3: Coefficient plots of dynamic treatment effects




**Note:** The figure plots the coefficient estimates of $\gamma_\tau$ in equation (3.1) capturing the interaction between dummies for months around the disaster and the $TREATMENT_{fi}^{EX,IMI}$ variable. The sample is the universe of Chinese processing firms. Firm-industry observations are assigned to the treatment group if their pre-disaster trade share was greater than 90% and if they traded with industries that were highly concentrated in affected states. Plots include 90% and 95% confidence intervals. The unit of the vertical axes of the plots for trade and intensive margins is thousand USD.

ber of products exported to the United States declined significantly between August and November 2005 and reached the lowest point in October 2005. Because there is a time gap of around a month for container shipments from the US East Coast to China, it seems evident that the biggest drop in intermediate imports occurred in October 2005 after the United States had been hit by two severe hurricanes in late August and mid-September. Similarly, but to a lesser extent, there was a decline in the intensive margin of processing firms in the treated group in October and November 2005.

### 3.3.2 *Direct* Impact of Supply Shocks on Firm Output

This subsection investigates how negative US supply shocks are statistically linked to export fluctuations of Chinese firms. For this purpose, we focus on a subset of Chinese processing manufacturers that are *directly* exposed to US supply shocks because of *direct* import linkages to the United States. Unlike Subsection 3.3.1, this analysis quantifies the actual *direct* exposure of Chinese firms to US supply fluctuations during the US hurricane season and examines to what extent temporary supply shortages triggered a temporary decline in firms' exports.

#### 3.3.2.1 Empirical Strategy

We measure the firms' *direct* exposure to US supply shocks as fluctuations of direct imports from the United States during the 2005 hurricane season. To estimate how foreign supply shocks are associated with export fluctuations, we need to ensure that the explanatory *direct* supply shock variable effectively captures supply changes triggered by the US hurricane season and that it is not confounded by unobserved changes in import demand of Chinese firms. This assumption can be violated if, for instance, US import fluctuations of Chinese firms are caused by fluctuations of the firms' demand. We therefore construct the *direct* US import supply shock variable $direct\, SUPshock_{fjt}^{7\,USstates}$ using equation (3.3) to capture these import fluctuations as a supply-side shock:

$$direct\, SUPshock_{fjt}^{7\,USstates} = \sum_{p \in j} dirIMI_{fpjt}^{CHN \leftarrow US} \cdot EX_{pjt}^{7\,USstates \rightarrow RoW}. \tag{3.3}$$

Accordingly, the firm-specific dummy variables $dirIMI_{fpjt}^{CHN \leftarrow US}$ indicate whether a Chinese firm $f$ imports a product $p$ from the United States in month $t$. We match these dummies with export flows from the seven hurricane-affected states to the rest of the world, $EX_{pjt}^{7USstates \rightarrow RoW}$.[16] We then aggregate these matched US supply-side dummies at the industry level $i$ and obtain the measure for the Chinese processing firms' *direct* exposure to the US supply shocks triggered by the 2005 hurricane season.

We estimate the relationship between negative foreign supply shocks and export fluctuations using the following model:

$$\begin{aligned} \Delta\, ln\, EX_{fpit} = {} & \alpha_f + \beta_j + \gamma_{it} \\ & + \zeta\, H^{Sep-Nov,2005} + \tau_1\, \Delta\, ln\, direct\, SUPshock_{fjt}^{7\,USstates} \\ & + \eta\, H^{Sep-Nov,2005} \cdot \Delta\, ln\, direct\, SUPshock_{fjt}^{7\,USstates} \\ & + \tau_2\, \Delta\, ln\, direct\, IMI_{fjt}^{ROW} + \epsilon_{fpit}. \end{aligned} \tag{3.4}$$

Accordingly, firm $f$'s exports of product $p$ in industry $i$ are explained by import supply fluctuations in the United States and the rest of the world, $direct\ SUPshock_{fjt}^{7\,USstates}$ and $direct\ IMI_{fjt}^{ROW}$, respectively, as well as by firm- and industry-specific characteristics. The effect of the supply shock triggered by the 2005 US hurricane season is captured by the interaction term between changes in the logarithm of $direct\ SUPshock_{fjt}^{7\,USstates}$ and the dummy variable $H^{Sep-Nov,2005}$, which equals one for the months between September and November 2005, indicating the months when hurricanes hit the states.

There are three features in the regression specification worth highlighting. First, we use year-on-year differences of logarithmic variables to control for outliers as well as to rule out firm-product specific seasonality in exports. Also, the year-on-year difference enables us to control for any year-invariant pattern of hurricanes and its relative impacts on firms to take into consideration that hurricanes hit the United States almost every year.

Second, we include firm, import industry, and export industry-time fixed effects $\alpha_f$, $\beta_j$, and $\gamma_{it}$, respectively, with $j$ indicating the import industry. It is important to stress that by including export industry-time fixed effects, we control for any industry-specific demand shocks. This is essential because it allows us to disentangle the demand shocks from the impacts of the US supply-side shocks.[17]

[16] While it seem counterintuitive that exports of affected states are used to calculate the supply shock for Chinese processing firms, it is important to stress that the US supply capacity is reflected by its exports.

[17] Previous versions of this paper aimed at studying the propagation of both demand- and supply-side shocks. However, there is a trade-off between the diligent identification of shocks and an adequate inclusion of controls. Consequently, a simultaneous identification of the effect of both shocks risks being blurred because identified effects can hardly be assigned to one or the other shock exclusively.

Third, by including *direct* $IMI_{fjt}^{ROW}$ in the regression, we control for both time-specific direct supply shocks in industry $j$ from the rest of the world and import demand shocks at the firm level.

#### 3.3.2.2 Results

In this subsection, we present estimation results on the impacts of the *direct* exposure to supply shocks on firms' exports, using the model presented in equation (3.4).

We expect a *positive* relationship between supply shocks and exports in case there is a drop of both the explained and explanatory variables. However, China has had a very strong export performance, especially from the early 2000s onward. Therefore, there might be a concern that a positive coefficient estimation only reflects a growing trade volume between the United States and China in general. To attenuate this concern, we show that there is evidence for a drop of direct supply from affected states between September and November 2005.[18]

Table 3.4: Regression results of direct supply shocks

| | All | Textile | Paper | Coke | Chemicals | Machinery | El/OptEq. |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| $\Delta$ ln dir. SUPshock | 0.003* | 0.002 | 0.003** | 0.003** | 0.004*** | 0.004** | 0.003** |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) |
| IA: $\Delta$ ln dir SUPshock$^{7\,USstates}$ x Hurricane = 1 | 0.014*** | 0.015*** | 0.014*** | 0.015*** | 0.014*** | 0.012*** | 0.012*** |
| | (0.006) | (0.006) | (0.006) | (0.007) | (0.005) | (0.006) | (0.005) |
| IA: $\Delta$ ln dir. SUPshock$^{7\,USstates}$ x IMI-industry = column(2-7) | | 0.001 | -0.001 | -0.000 | -0.004*** | -0.003*** | -0.001 |
| | | (0.004) | (0.003) | (0.016) | (0.004) | (0.003) | (0.008) |
| IA: $\Delta$ ln dir. SUPshock$^{7\,USstates}$ x Hurricane = 1 x IMI-industry = column(2-7) | | -0.003*** | -0.001 | -0.008*** | -0.000 | 0.006*** | 0.010*** |
| | | (0.012) | (0.014) | (0.008) | (0.011) | (0.012) | (0.034) |
| Firm-FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| EXindustry-time-FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| IMIindustry-FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| ROW-control | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 |

**Note:** The sample is processing firms that imported intermediate inputs from the United States during the pre-disaster period. The dependent variable is standardised $\Delta \ln EX$ for all regressions. *Hurricane* refers to a dummy variable that equals 1 from September to November 2005. $IMI - industry$ dummy variables equal 1 if the importing industry corresponds to the industry indicated by the column. Individual $IMI - industry$ dummy variables are dropped due to common import industry fixed effects. Standard errors in parentheses, clustered at the firm level. *** p< 0.01, ** p< 0.05, and * p< 0.1.

Table 3.4 presents estimation results for the impact of the *direct* supply shock on firms' exports. Column 1 shows that Chinese firms' exports were positively associated with the exposure to the direct supply shocks induced by the 2005 US hurricane season. Accordingly, a drop in supply from the United States by one

[18] Summary statistics of the supply shock variable $\Delta\, ln\, direct\, SUPshock_{fjt}^{7\,USstates}$ are presented in Table 3.C.3 in Appendix 3.C.

standard deviation triggered a drop in exports by 0.017 (0.003 + 0.014) standard deviations. Columns 2 to 7 add another interaction between the variable of supply shocks triggered by the US hurricane season and a dummy equal to one if firms are in the industry indicated in each column. These are the industries that are highly concentrated in the affected states, as discussed in Section 3.3.1.1. This exercise is in line with our empirical strategy, which pays particular attention to supply fluctuations of US industries. These triple interaction terms show that the effects of the direct supply shock are lower in textile and coke industries (columns 2 and 4), while they are higher in the machinery (column 6) and electrical/ optical equipment (column 7) industries. Interestingly, triple interaction terms for other industries are not positive and significant, with one exception being the electrical machinery industry, which also shows a positive and significant link of the direct supply shock to exports.[19] Estimation results for other sectors are presented in Tables 3.C.4 and 3.C.5 in Appendix 3.C.

The results suggest that firms that are directly exposed to US supply shocks triggered by the 2005 hurricane season, slightly reduced their export production in the same period. In view of this finding, we can draw the conclusion that the *direct* exposure to supply shocks can affect manufacturing output.

## 3.4 Resilience to the US Trade Shock

So far, we have examined the direct impacts of the 2005 US hurricane season on the firms that imported from the United States. In this section, we explore firms' characteristics that affect their resilience, measured by the pass-through of adverse shocks to firm performance. The section contains three parts: Subsection 3.4.1 outlines the theoretical background of our analysis, Subsection 3.4.2 analyzes the heterogeneous effects of the US hurricanes on firms directly affected, and Subsection 3.4.3 provides some evidence on the level of supplier diversification and export volatility and on the development of supplier diversification in the aftermath of the 2005 hurricane season.

### 3.4.1 Theoretical Background

To guide our empirical analysis, we use a model built on Antràs et al. (2017) and Huang (2017), which allows us to make theoretical predictions on firms' sourcing diversification and resilience to supply chain disruption. In this section we

[19]Triple interaction terms for other industries tend to show lower effects of adverse *direct* supply shocks. Still, the sum of relevant estimation coefficients remains positive, thus pointing to a propagation of supply shocks.

briefly describe the theoretical background. Appendix 3.A provides a detailed derivation of the model.

We define a small, idiosyncratic trade cost shock that changes the iceberg trade cost $\tau_{cs}$ to $\tau'_{cs}$. A firm's resilience is measured as the pass-through of adverse shocks to firm performance (e.g., import value, export value, marginal cost). A firm is defined to be more resilient if the pass-through is smaller.

Using an 'exact hat algebra' approach (Jones, 1965; Dekle et al., 2007), we denote $\widehat{X} \equiv \frac{X'}{X}$ and have the following:[20]

$$\frac{\partial \ln \widehat{(c_c(\varphi))}}{\partial \ln \widehat{\tau_{cs}}} \approx \chi_{cs}(\varphi). \tag{3.5}$$

This result suggests that the impact of the shock is determined by the intensive margin and increases with $\chi_{cs}(\varphi)$. As indicated in Section 3.2, $\chi_{cs}$ represents the input expenditure share of intermediate inputs from each source of supply. If the firm is not diversified at all and relies solely on one supplier hit by a shock, the pass-through is 100%. On the other hand, high-productivity firms are more diversified and source from more places. Their share of inputs from any particular source is smaller, and so is the pass-through. This can be shown in the second derivative of equation (3.5):

$$\frac{\partial^2 \ln \widehat{(c_c(\varphi))}}{\partial \ln \widehat{\tau_{cs}} \partial \varphi} \leq 0. \tag{3.6}$$

Furthermore, if the adverse shocks on sources are not perfectly correlated and have the same variance $\xi^2$, we can show that opening to trade lowers the volatility of firms' source capabilities. Additionally, if we assume that sourcing decisions are complementary across sources and the adverse shocks are independent and identically distributed, the volatility of firm revenue is

$$var(\widehat{R(\varphi)}) \propto \xi^2 HHI(\varphi), \tag{3.7}$$

where HHI is the Herfindahl-Hirschman Index, which sums over the squares of input expenditure share from all sources.

Since marginal costs are not observable in our data, to generate empirically testable predictions, we study how firm-level import flows will respond to an adverse shock. The model delivers the following result: for a small trade cost shock thatincreases $\tau_{cs}$ to $\tau'_{cs}$,

[20]The result is based on the assumption that sourcing decisions are complementary, such that $\sigma - 1 > \theta$, and the adverse shocks increase trade costs $\tau'_{cs} > \tau_{cs}$

$$-\frac{\partial \ln \widehat{M_{cs'}(\varphi)}}{\partial \ln \widehat{\tau_{cs}}} = \begin{cases} \theta + (\sigma - 1 - \theta)\chi_{cs'}(\varphi), & \text{if } s' = s; \\ (\sigma - 1 - \theta)\chi_{cs'}(\varphi), & \text{otherwise,} \end{cases} \tag{3.8}$$

where $M_{cs'}(\varphi)$ denotes a firm's intermediate input purchases from a country $s'$. The pass-through of the adverse shock endogenously depends on firm productivity $\varphi$ and the usual Fréchet shape parameter $\theta$, which captures the direct impact of the shock. An additional term $(\sigma - 1 - \theta)\chi_{cs'}(\varphi)$ is positive if sourcing decisions are complementary ($(\sigma - 1)/\theta > 1$) and negative if inputs are substitutable ($(\sigma - 1)/\theta < 1$).

According to equation (3.5), the trade cost shock reduces firms' sourcing capability and increases their marginal cost. This drives down marginal demand curve for all inputs if the sourcing decisions are complementary. Such a feedback effect through interdependencies amplifies the initial cost shock and reduces imports further. In contrast, if the inputs are substitutable, the cost shock reduces firm output and drives up the marginal demand curve. Such an increase in the marginal demand for the input dampens the initial negative shock. This difference allows us to identify whether sourcing decisions are complementary or substitutable. Furthermore, the pass-through also varies by the sourcing intensity $\chi_{cs'}(\varphi)$. The feedback effect is stronger if a firm has a heavier load on inputs from a country affected by an adverse shock. Finally, the interdependency is also reflected by the result that imports also respond to shocks on other source countries in a firm's sourcing strategy.

### 3.4.2 Resilience of Firms to US Hurricanes

The theoretical model predicts that the effect of an adverse shock on imports depends on firms' pre-shock sourcing intensity. To verify such a prediction, we estimate the following equation, derived from equation (3.8):

$$\Delta \ln M_{fpst} = \alpha_f + \beta_{pt} + \nu_s + \iota_t + \gamma_1 \chi^{US}_{fp} + \gamma_2 H_t + \gamma_3 \chi^{US}_{fp} \cdot H_t + \epsilon_{fpst}, \tag{3.9}$$

in which we examine how the year-on-year change in firm $f$'s imports of a particular intermediate product $p$ sourced from country $s$ at time $t$, $\Delta \ln M_{fps,t}$, would respond after a hurricane hit. The US sourcing intensity before the shock $\chi^{US}_{fp}$ is measured as the average expenditure share of firm $f$ for inputs $p$ from the United States before a hurricane (between August 2004 and July 2005). The time dummy $H_t$ captures the duration of the US hurricane season, which equals one for months between September and November 2005. The interaction term between the hurricane shock dummy $H_t$ and the pre-hurricane US sourcing intensity $\chi_{fp}^{US}$ captures the heterogeneous pass-through of the hurricane shock of Chinese processing firms. The main coefficient of interest, $\gamma_3$, is expected to be negative if sourcing decisions are complementary.

We control for a range of fixed effects: $\beta_{pt}$ captures import-product-time fixed effects at quarterly intervals, which would absorb time varying trends specific to an imported product. Since the hurricane is defined at monthly intervals, an import-product-time fixed effects at quarterly intervals would not fully absorb the effect of the hurricane season. $\nu_s$ controls for time-invariant characteristics of the source country. Most important, we include firm fixed effects $\alpha_f$ to control for any time-invariant firm-level characteristics such as firm size and productivity, which may also affect firms' imports and performance.

The impact of the hurricane season may also differ by the intermediate products the firm imports. Specifically, as equation (3.A.4.28) in Appendix 3.A indicates, products with higher elasticity of substitution may enable firms to substitute away from a source country hit by an adverse shock, and instead import from another source country unaffected by the natural disaster. To test the heterogeneous effects of the US hurricane shock on different imported products, we estimate the following triple difference-in-differences equation:

$$\begin{aligned}\Delta \ln M_{fpst} = \alpha_f + \beta_{pt} + \nu_s + \iota_t + \gamma_1 \chi_{fp}^{US} + \gamma_2 H_t + \gamma_3 \chi_{fp}^{US} \cdot H_t + \\ \gamma_4 H_t \cdot \theta_p + \gamma_5 \chi_{fp}^{US} \cdot \theta_p + \gamma_6 \chi_{fp}^{US} \cdot H_t \cdot \theta_p + \epsilon_{fpst}, \quad (3.10)\end{aligned}$$

where $\theta_p$ is substitution elasticity for product $p$. The coefficient $\gamma_4$ captures the effect by which higher substitutability enables the firm to mitigate the impact of a disaster by substituting away from the source country hit by the shock; $\gamma_5$ captures the effect by which firms with a higher share of US imports prior to the disaster experienced a larger drop in imports as they substituted for imports from other sources; $\gamma_6$ captures the heterogeneous pass-through varying by products' substitution elasticity.

We use the monthly data on imports of Chinese processing firms between 2004 and 2006, aggregated by product to HS-3 digit level. The HS-3 import products are then matched with the product-level substitution elasticity estimated by Broda and Weinstein (2006) for China. To capture the fact that firms may drop out of importing because of an adverse shock, we use a value of zero for imports if a firm imported a product or exported in the beginning of the sample period and stopped trading in the middle of the sample period.

The estimation results are reported in Table 3.5. Along the columns, we add one more variable in each column. The effects of pre-hurricane US import intensity on imports are negative and significant in all columns, suggesting that imports were lower for firms with concentrated import sources for intermediate inputs prior to a hurricane. Column 2 suggests that, on average, the year-on-year import growth fell by roughly 19% during the hurricane season. In column 3, we add an interaction of the hurricane dummy variable and the pre-hurricane US import intensity. The result suggests that firms that imported relatively more intermediate inputs from the United States before the hurricane season could have experienced a greater decrease in their imports during the hurricane season. If a firm fully relied on imports from the United States before the hurricane seasonâ that is, with a pre-hurricane US import intensity equal oneâits year-on-year import growth could be reduced by about 33% between September and November 2005. Additionally, the effect of the hurricane shock is attenuated in column 3 compared with column 2, suggesting that the negative effect of the hurricanes was largely driven by firms that relied heavily on US intermediate imports. It is also worth noting that the negative coefficient of the interaction term between the hurricane dummy and the pre-hurricane US import intensity in column 3 corresponds to the parameter estimates of $\sigma - 1 - \theta$ in equation (3.8), implying that sourcing decisions are complementary: when imports from one source were hit by a natural disaster, year-on-year import growth from other sources also dropped in the short run.

Column 4 reports the coefficient estimates of equation (3.10), which provide evidence on heterogeneous effects of the US hurricanes varying by products' substitution elasticity. A negative coefficient on the interaction of US import intensity and the substitution elasticity $\theta_p$ in column 4 indicates that, firms with a higher US import share see a larger decrease in their imports of products with a higher substitution elasticity. For example, for firms that fully relied on US imports before the hurricane (i.e., a pre-hurricane US import intensity equal to one), the imports of stones (HS-710), with a substitution elasticity of more than 100, would fall 25% more than the imports of parts of electronic machinery (HS-854), with a substitution elasticity of close to 1.

### 3.4.3 Evidence of Sourcing Diversification

In this section, we provide evidence on the level of sourcing diversification. We demonstrate that firms with more diversified sourcing experience less volatility in exports and give some evidence on the evolution of firms' sourcing diversification around the 2005 US hurricane season.

Table 3.5: Resilience of firms to the US hurricane

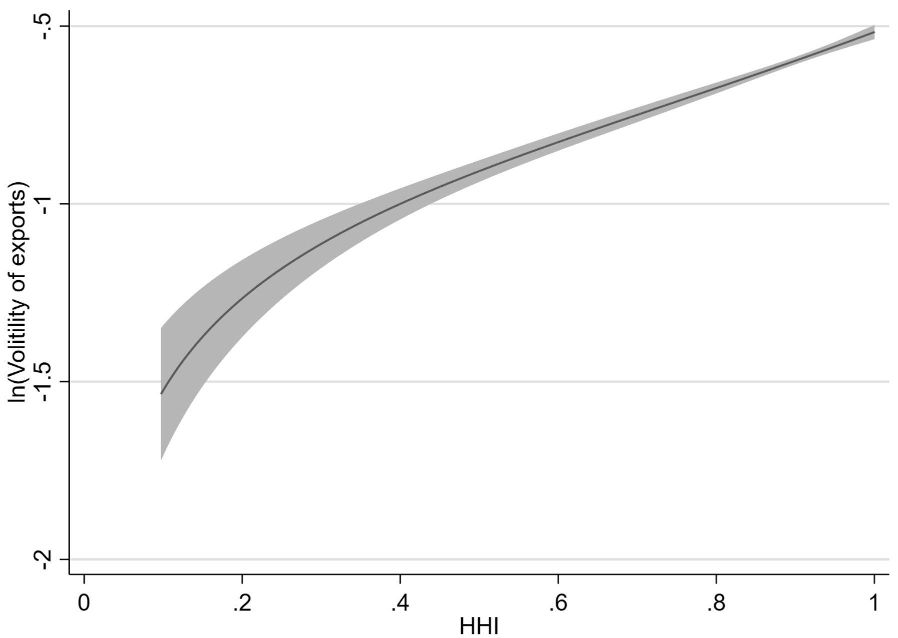
| **Panel A: Dependent variable log imports** | | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Pre-hurricane import intensity | -0.352*** | -0.352*** | -0.294*** | -0.269*** |
| | (0.026) | (0.026) | (0.029) | (0.033) |
| Hurricane = 1 | | -0.188*** | -0.160*** | -0.147*** |
| | | (0.014) | (0.015) | (0.016) |
| Hurricane = 1 $\times$ Pre-hurricane US import intensity | | | -0.253*** | -0.256*** |
| | | | (0.046) | (0.050) |
| Hurricane = 1 $\times$ $\theta_p$ | | | | -0.003** |
| | | | | (0.001) |
| US import intensity $\times$ $\theta_p$ | | | | -0.005 |
| | | | | (0.004) |
| Hurricane = 1 $\times$ Pre-hurricane US import intensity $\times$ $\theta_p$ | | | | -0.000 |
| | | | | (0.005) |
| Observations | 4,440,066 | 4,440,066 | 4,440,066 | 4,432,747 |
| R-squared | 0.132 | 0.132 | 0.133 | 0.131 |
| Firm FE | ✓ | ✓ | ✓ | ✓ |
| Import product-quarter FE | ✓ | ✓ | ✓ | ✓ |
| Source FE | ✓ | ✓ | ✓ | ✓ |

**Note:** The dependent variable is the log of monthly imports at firm-product level of Chinese processing firms between September 2005 and December 2006. Pre-hurricane US import intensity is calculated as the share of imports from the United States over total imports for a firm-product. Indicator variable Hurricane equals 1 if the month is between September and November 2005. Trade elasticity at the HS-3 digit level is from Broda and Weinstein (2006). Robust standard errors clustered at firm level are reported in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

As shown in equation (3.7), the volatility of firms' revenue is proportional to the level of supplier concentration measured by the HHI. This relationship is demonstrated in Figure 3.4, which shows the relationship between firms' export volatility and their sourcing diversification. We define volatility as the variance of the year-on-year export growth rate of firms' quarterly exports from 2000 to 2006. To mitigate fluctuations of the index originating from different sourcing patterns across months, we aggregate all the variables to a quarterly level. Figure 3.4 plots a local polynomial regression of (logarithm) firm-level export volatility on sourcing concentration measured by the HHI at quarterly intervals, while controlling for firm fixed effects. The figure displays a general upward slope: firms with more concentrated sourcing have higher export volatility, whereas firms with more diversified sourcing strategies are associated with lower export volatility.

A linear regression of logarithm of export volatility over the firm sourcing HHI, while controlling for firm fixed effects, gives a coefficient of 0.8. This suggests that if a firm decreases its sourcing concentration such that its sourcing HHI falls by 0.1, the export volatility can decrease by 0.8%.

Figure 3.4: Sourcing concentration and export volatility

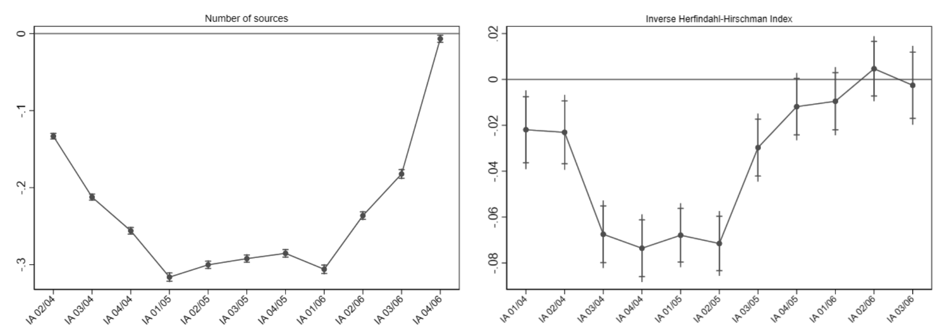



**Note:** The Herfindahl–Hirschman Index (HHI) is calculated as the sum over the squares of input expenditure share from all sources, while the input expenditure share is measured by the share of source-specific inputs in total inputs at an HS-6 product level at quarterly intervals. Volatility is measured as the export growth rate of firms' quarterly exports from 2000 to 2006.

Second, we provide some evidence on the evolution of firms' sourcing diversification. We have shown that diversification can be an important tool to mitigate the risk of supply shortages. However, it remains debatable whether firms will adjust their supply chains diversification following a temporary adverse shock. Antràs (2020), for instance, argues that the COVID-19 pandemic alone is unlikely to alter firms' supply chain organisation, as a temporary shock is unlikely to induce firms to sever international ties and incur fixed costs in identifying and establishing new suppliers.

Against this background, we test whether Chinese processing manufacturers changed their sourcing strategies after the 2005 US hurricane season. We identify both the extensive and intensive margins by considering two measures of diversification: (1) the number of suppliers per HS-6 product, capturing the extensive margin of sourcing, and (2) an inverse of the normalised HHI, capturing the degree of sourcing diversification. A higher inverse HHI indicates that firms not only source from a variety of suppliers but also spread the import share more evenly across different suppliers.

Figure 3.5: Coefficient plot for the dynamic treatment effect of supplier diversification




(a) Number of partners

(b) Inverse HHI

**Note:** Regression equations are the same as the specification outlined in Section 3.3 with different outcome variables. Accordingly, firms are assigned to the treatment group when the US import share exceeds 90% in sectors that are highly concentrated in affected states. In the left panel, extensive margin is captured by the number of suppliers per firm per HS-6 product. In the right panel, import supply diversification is captured as the inverse of the normalised HHI. To account for different firm and product characteristics, we include firm and product fixed effects. Each respective quarter is indicated on the x-axis; for instance, 02/04 represents the second quarter in 2004, while 03/05 represents the third quarter in 2005.

Following an approach similar to the estimation of the dynamic treatment effects in equation (3.1), Figure 3.5 presents the coefficient estimates of firms' diversification for the treatment group (i.e., firms that import over 90% of their intermediate goods from the United States and in the industries heavily concentrated in affected states) in comparison with the control group. The left panel shows the evolution of the number of suppliers, and the right panel shows the evolution of diversification measured by the inverse HHI, before and after the US hurricane season. It is worth noting that by definition, firms in the treatment group are less diversified, since they are assigned to the treatment group if they rely heavily on intermediate imports from the United States. What we are interested in, however, is the evolution of supplier diversification of these firms before and after the hurricane season.

Figure 3.5 shows that the import diversification of Chinese processing firms with tight trade linkages to the United States was significantly lower than the average supplier diversification of firms in the control group in 2005. Nonetheless, there was a slight increase in diversification after the third quarter of 2005. The left panel indicates that the diversification is largely driven by the extensive margin, measured by the number of countries from which firms source intermediate inputs, and the right panel indicates that the level of diversification measured by inverse HHI has also increased. This growing diversification is likely to be associated with firms' choice to expand the import supplier base of intermediates in response to a supply shortage during the US hurricane season in the third and fourth quarters of 2005.

## 3.5 Conclusions

This paper has investigated the link between natural disaster shocks and global value chains. We have used the 2005 US hurricane season as a natural experiment to study how it affected the export performance of Chinese processing manufacturing firms. We constructed a firm-level data set that links three sources of data: trade data from Chinese custom authorities, input-output tables from the OECD ICIO database, and trade data from the US Census Bureau.

Following Acemoglu et al. (2016), we investigated how an adverse natural disaster shock in the United States directly affects firms in China. We showed that Chinese processing manufacturers with tight trade linkages to the United States reduced their intermediate imports from the United States between July and October 2005. We further estimated the heterogeneous effects of the US hurricane on firms' imports. We find that firms with more diversified suppliers tend to be less affected by the US hurricane in their imports of intermediate inputs and their exports. The evidence also points to a degree of complementarity in source decisions, such that an adverse shock affecting one supplier may induce a decline in sourcing from other suppliers.

At the same time, we do not find a significant impact of the cross-border propagation of supply shocks through input-output linkages, suggesting that a temporary supply reduction induced by an adverse shock in a foreign country does not impose a substantial risk for Chinese processing firms on their production of exports. This result stands in contrast to recent research that detects an indirect propagation of natural disaster shocks *within* countries (Barrot and Sauvagnat, 2016; Carvalho et al., 2021), which may be for several reasons. First, firms often form stronger input-output links within domestic supply chains; in contrast, firms participating in international production networks can more easily substitute alternative domestic or international suppliers for disaster-affected trading partners. This result is also in line with that of Kashiwagi et al. (2018), who mapped firm-to-firm transactions following 2012 Hurricane Sandy and find no propagation of the negative shock outside the United States. Second, the 2005 US hurricane season affected only a few states and thus did not constitute a major shock in comparison with the total amount of US exports.

Although this study focuses on a single type of natural disaster, the results can provide insights in a broader context for the analysis of supply chain effects of adverse shocks. First, the COVID-19 pandemic has raised concerns that global supply chains could potentially propagate a regional shock to a global scale. Our results indicate that although Chinese processing firms that directly import from the United States experienced a drop in their imports in the months following the 2005 US hurricane season, one standard deviation change in imports translates into about 0.017 standard deviation change in exports, suggesting a limited direct propagation of the shock. Furthermore, we do not detect an indirect propagation of the shock through global input-output linkages.

Second, we analyzed firms' levels of resilience according to their sourcing strategy and find that firms with more diversified supplier sources experienced a lower pass-through of the natural disaster shock in their imports. This finding is in line with the theoretical prediction that more productive firms have a more diversified sourcing strategy and are therefore more resilient to adverse trade shocks. Our results point to a potential way for firms to mitigate impacts of unexpected adverse shocks and enhance resilience to future risks from adverse shocks.

Third, we have also provided some preliminary evidence that supply chains can adjust after natural disasters. We find that firms heavily affected by the hurricanes increased their supplier diversification in the period after the hurricane.

This could be due to firms' strategy adjustment to seek alternative suppliers and avert future shocks. The finding contributes to the debate on whether an adverse shock such as COVID-19 could lead to permanent adjustments in firms' sourcing decisions.

# Appendices 3

## 3.A Theoretical Framework

In this section, we describe a multicountry model of international sourcing adapted from Antràs et al. (2017) and extended by Huang (2017). The model allows us to establish a relationship between firm's sourcing strategies, their sourcing diversification and resilience to adverse shocks. We also summarise a model in Acemoglu et al. (2016) that serves as basis for our empirical analysis of the propagation of shocks.

### 3.A.1 Demand Side

We consider a world consisting of $W$ countries in which individuals value the consumption of differentiated varieties of manufacturing goods according to a standard symmetric CES aggregator.

$$U_{M_c} = \Big(\int_{\omega\in\Omega_c} q_c(\omega)^{(\sigma-1)/\sigma}\, d\omega\Big)^{\sigma/(\sigma-1)}, \sigma > 1, \tag{3.A.1.1}$$

where $\Omega_c$ is the set of manufacturing varieties available to consumers in country $c \in W$. The preferences are assumed to be common worldwide and give rise to the following demand for variety $\omega$ in country $c$:

$$q_c(\omega) = E_c P_c^{\sigma-1} p_c(\omega)^{-\sigma}, \tag{3.A.1.2}$$

where $p_c(\omega)$ is the price of variety $\omega$, $P_c$ is the standard price index associated with equation (3.A.1.1), and $E_c$ is aggregate spending on manufacturing goods in country $c$. For what follows it will be useful to define a market demand term for market $c$ as

$$B_c = \frac{1}{\omega}\Big(\frac{\sigma}{\sigma-1}\Big)^{1-\sigma} E_c P_c^{\sigma-1}, \tag{3.A.1.3}$$

There is a unique factor of production, labour, which commands a wage $w_c$ in country $c$.

### 3.A.2 Supply Side

There exists a measure $N_c$ of final-goods producers in each country $c \in W$, and each of these producers owns a blueprint to produce a single differentiate variety. The market structure of final-goods production is characterised by monopolistic competition, and there is free entry into the industry. Production of final-goods varieties requires the assembly of a bundle of intermediates. We index final-goods firms by their productivity, which we denote by $\varphi$, and which governs the mapping between the bundle of inputs and final-goods production.

Following Melitz (2003), Antràs et al. (2017) assume that firms learn their productivity $\varphi$ only after incurring an entry cost equal to $f_c$ units of labour in country $c$. This core productivity is drawn from a country-specific distribution $g_c(\varphi)$, with support in $[\underline{\varphi_c}, \infty)$, and with an associated continuous cumulative distribution $G_c(\varphi)$.

The bundle of intermediates contains a continuum of firm-specific inputs, assumed to be imperfectly substitutable for each other, with a constant and symmetric elasticity of substitution equal to $\rho$. Intermediates can be traded internationally, and a key feature of the equilibrium will be determining the location of production of different intermediates. All intermediates are produced with labour under constant return to scale technologies. $a_s(v, \varphi)$ denotes the unit labour requirement associated with the production of firm $\varphi$'s intermediate $v \in [0, 1]$ in country $s \in W$.

A final-goods producer based in country $c$ acquires the capability to offshore in $s$ only after incurring a fixed cost equal to $f_{cs}$ units of labour in country $c$. We denote by $\mathcal{W}_c(\varphi) \subseteq W$ the set of countries for which a firm based in $c$ with productivity $\varphi$ has paid the associated fixed cost of offshoring $w_c f_{cs}$. We will refer to $\mathcal{W}_c$ as the *global sourcing strategy* of that firm.

Intermediates are produced by a competitive fringe of suppliers who sell their products at marginal costs. Shipping intermediates from country $s$ to country $c$ entails iceberg trade cost $\tau_{cs}$. As a result, the cost at which firms from $c$ can procure input $v$ from country $s$ is given by $\tau_{cs}\, a_s(v, \varphi)\, w_s$, and the price that firm $\varphi$ based in country $c$ pays for input $v$ can be denoted by

$$z_c\big(v, \varphi; \mathcal{W}_c(\varphi)\big) = \min_{s \in \mathcal{W}_c(\varphi)} \{\tau_{cs}\, a_s(v, \varphi)\, w_s\}, \tag{3.A.2.4}$$

We can then express the marginal cost for firm $\varphi$ based in country $c$ of producing a unit of a final-goods variety as

$$c_c(\varphi) = \frac{1}{\varphi}\Big(\int_0^1 z_c\big(v, \varphi; \mathcal{W}_c(\varphi)\big)^{1-\rho}\, dv\Big)^{1/(1-\rho)}, \tag{3.A.2.5}$$

Following Eaton and Kortum (2002), Antràs et al. (2017) assumes that the firm-specific intermediate input efficiencies for supplier in country $s$, $1/a_s(v, \varphi)$, are realised by drawing from the Fréchet distribution:

$$Pr(a_s(v,\varphi) \geq a) = e^{-T_s a^\theta}, \text{ with } T_s > 0, \tag{3.A.2.6}$$

where $T_s$ governs the state of technology in country $s$, while $\theta$ determines the variability of productivity draws across inputs. A lower $\theta$ indicates more heterogeneity across inputs and thus fosters the emergence of comparative advantage *within* the range of intermediates across countries.

### 3.A.3 Firm-Level Sourcing Decision

Consider a firm based in country $c$ with productivity $\varphi$ that has incurred all fixed costs associated with a given sourcing strategy $\mathcal{W}_c$. In light of the cost function in (3.A.2.5), the firm will choose the location of production for each input $v$ that solves $\min_{s\in\mathcal{W}_c(\varphi)}\{\tau_{cs}a_s(v,\varphi)w_s\}$.

Using the properties of the Fréchet distribution in (3.A.2.6), the share of intermediate input purchases sourced from any country $s$ (including the home country $c$) is given by

$$\chi_{cs} = \begin{cases} \frac{T_s(\tau_{cs}w_s)^{-\theta}}{\Theta_c(\varphi)}, & \text{if } s \in \mathcal{W}_c(\varphi); \\ = 0, & \text{otherwise,} \end{cases} \tag{3.A.3.7}$$

where the term $\Theta_c$ summarises the *sourcing capability* of firm $\varphi$ from $c$, such that

$$\Theta_c \equiv \sum_{k\in\mathcal{W}_c(\varphi)} T_k(\tau_{ik}w_k)^{-\theta}. \tag{3.A.3.8}$$

We further denote the term $\phi_s \equiv T_s(\tau_{cs}w_s)^{-\theta}$, which represents the *sourcing potential* of country $s$ from the point of view of the firm in $c$.

After choosing the least-cost source of supply for each input $v$, WE CAN EXPRESS the overall marginal cost faced by firm $\varphi$ from $c$ as

$$c_c(\varphi) = \frac{1}{\varphi}\big(\gamma\Theta_c(\varphi)\big)^{1/(1-\theta)}, \tag{3.A.3.9}$$

where $\gamma = \left[\Gamma(\frac{\theta+1-\rho}{\theta})\right]^{\frac{\theta}{1-\rho}}$ and $\Gamma$ is the gamma function. In light of equation (3.A.3.8), the addition of a new location to the set $\mathcal{W}_c$ increases the sourcing capability of the firm and necessarily lowers its effective marginal cost.

Using the demand function in (3.A.1.2) and the derived marginal cost function in (3.A.3.9), we can express the firm's profits conditional on a sourcing strategy

$\mathcal{W}_c$ as

$$\pi_c(\varphi) = \varphi^{\sigma-1}\big(\gamma\Theta_c(\varphi)\big)^{(\sigma-1)/\theta} B_c - w_c \sum_{s\in\mathcal{W}_c(\varphi)} f_{cs}, \tag{3.A.3.10}$$

where $B_c$ is the market demand term given in (3.A.1.3).

Equation (3.A.3.10) shows a firm's trade-off in sourcing decisions: when deciding whether to add a new country $s$ to the set $\mathcal{W}_c(\varphi)$, the firm weights the reduction in costs associated with the inclusion of that countryâwhich increases the sourcing capacity $\Theta_c(\varphi)$âagainst the payment of the additional fixed cost $w_c f_{cs}$.

For a firm with productivity $\varphi$, its intermediate input purchases from any country $s \in \mathcal{W}_c(\varphi)$ are a fraction $(\sigma-1)\chi_{cs}(\varphi)$ of firm profits. Using (3.A.1.3) and (3.A.3.10), they can be expressed as

$$M_{cs}(\varphi) = \begin{cases} (\sigma-1)B_c\gamma^{(\sigma-1)/\theta}\varphi^{\sigma-1}\big(\Theta_c(\varphi)\big)^{(\sigma-1)/\theta}\chi_{cs}(\varphi), & \text{if } s \in \mathcal{J}_c(\varphi); \\ 0, & \text{otherwise.} \end{cases} \tag{3.A.3.11}$$

When $(\sigma-1)/\theta > 1$, the sourcing decisions are complementary, and $M_{cs}(\varphi)$ is thus increasing in all the terms in $\Theta_c(\varphi)$. Intuitively, when demand is sufficiently elastic (i.e., $\sigma$ is high enough) or the strength of comparative advantage in the intermediate-goods sector across countries is sufficiently high (i.e., $\theta$ is low enough), the scale effect through the demand response to lower costs dominates the direct substitution effect related to market shares, shifting toward the locations whose costs of sourcing have been reduced.

In this case, holding constant the market demand level $B_c$, whenever $(\sigma-1)/\theta > 1$, an increase in the sourcing potential $\phi_c T_s(\tau_{cs}w_s)^{-\theta}$ or a reduction in the fixed cost of sourcing $f_s$ for any country $s$ (weakly) increases the input purchases by firms in $c$ not only from $s$ but also from all other countries. The intuition behind the result is as follows: since sourcing decisions are complementary, an increase in sourcing potential of any supplier is likely to raise the marginal benefit of including a supplier in the sourcing strategy, which makes it more attractive for a firm to add a new supplier.

### 3.A.4 Diversification and Resilience

Based on the framework of firms' sourcing decisions in Antràs et al. (2017), Huang (2017) extends the model to show results on firms' resilience to shocks on supply chains. We summarise these results in this section.

If sourcing decisions are complementaryâthat is $(\sigma-1)/\theta > 1$âthe concentration of firms' sourcing strategies as measured by the Herfindahl-Hirschman Index $HHI_c \equiv \sum_s \chi_{cs}(\varphi)^2$ is nonincreasing in $\varphi$. This is because high-productivity firms have greater sourcing capability and more alternatives. Therefore, high-productivity firms are more diversified even after considering the intensive margin.

We define a small, idiosyncratic trade cost shock that changes $\tau_{cs}$ to $\tau'_{cs}$. A firm's resilience is measured as the pass-through of adverse shocks to firm performance. A firm is defined to be more resilient if the pass-through is smaller.

We can gauge the effect of adverse shocks using a 'hat algebra' approach (Jones, 1965; Dekle et al., 2007).

**Proposition 1.** *For a small, idiosyncratic shock that changes $\tau_{cs}$ to $\tau'_{cs}$ such that the firm does not abandon source s, (a) the pass through to the margin cost is given by*

$$\frac{\partial \ln \widehat{(c_c(\varphi))}}{\partial \ln \widehat{\tau_{cs}}} = \frac{\chi_{cs}(\varphi)}{1 - \sum_{s \in \mathcal{N}_s(\varphi)} \chi_{cs}(\varphi)}, \tag{3.A.4.12}$$

*where $\widehat{X} \equiv \frac{X'}{X}$ and $\mathcal{N}_s(\varphi)$ is the set of new suppliers chosen by the firm after the shock. (b) With complementarity of sourcing decisions across countriesâ$(\sigma - 1)/\theta > 1$âand an adverse shock ($\tau'_{cs} \geq \tau_{cs}$), we have*

$$\frac{\partial \ln \widehat{(c_c(\varphi))}}{\partial \ln \widehat{\tau_{cs}}} \approx \chi_{cs}(\varphi). \tag{3.A.4.13}$$

*Proof.* According to equation (3.A.3.9), in case of a shock to any supplier, the change in unit cost for the firm is given by:

$$\widehat{c_c} \equiv \frac{c'_c}{c_c} = \widehat{\Theta_c}(\varphi)^{1/\theta}, \tag{3.A.4.14}$$

which implies that $\frac{\partial \ln \widehat{c_c}}{\partial \ln \widehat{\Theta_c}} = -\frac{1}{\theta}$.

The change in sourcing capability of firm $\varphi$ in country $c$, $\widehat{\Theta_c}(\varphi)$, can be expressed as

$$\begin{aligned}
\widehat{\Theta_c}(\varphi) &= \frac{\sum_{s \in \mathcal{C}} \phi'_s + \sum_{s \in \mathcal{N}} \phi'_s}{\Theta_c(\varphi)} \\
&= \sum_{s \in \mathcal{C}} \frac{\phi'_s}{\phi_s} \frac{\phi_s}{\Theta_c(\varphi)} + \sum_{s \in \mathcal{N}} \frac{\phi'_s}{\Theta'_c(\varphi)} \frac{\Theta'_c(\varphi)}{\Theta_c(\varphi)} \\
&= \sum_{s \in \mathcal{C}} \widehat{\phi_s} \chi_{cs} + \widehat{\Theta_c}(\varphi) \sum_{s \in \mathcal{N}} \chi'_{cs} \\
&= \frac{\sum_{s \in \mathcal{C}} \chi_{cs} \widehat{\phi_{cs}}}{1 - \sum_{s \in \mathcal{N}} \chi'_{cs}},
\end{aligned} \tag{3.A.4.15}$$

where $\mathcal{C}$ is the set of sources the firm continues to use, and $\mathcal{N}$ is the set of new sources used by the firm. Equation (3.A.4.15) indicates that one unit change in sourcing potential $\phi_s$ translates into $\frac{\sum_{s\in\mathcal{C}}\chi_{cs}}{1-\sum_{s\in\mathcal{N}}\chi'_{cs}}$ unit change in sourcing capability $\widehat{\Theta_c}(\varphi)$.

For a small change in $x$, we know that $\ln(x) \approx x-1$, thus $\widehat{\Theta_c}(\varphi) = 1 + \ln(\widehat{\Theta_c}(\varphi))$ and $\widehat{\phi_s}\chi_{cs} \approx 1 + \ln(\widehat{\phi_s}\chi_{cs})$. Then we have

$$\ln\widehat{\Theta_c}(\varphi) \approx \frac{\sum_{s\in\mathcal{C}}\chi_{cs}\ln(\widehat{\phi_{cs}})}{1-\sum_{s\in\mathcal{N}}\chi'_{cs}} + \frac{\sum_{s\in\mathcal{C}}\chi_{cs}(\chi_{cs}-\chi'_{cs})}{1-\sum_{s\in\mathcal{N}}\chi'_{cs}}, \tag{3.A.4.16}$$

which implies

$$\frac{\partial\ln\widehat{\Theta_c}(\varphi)}{\partial\ln\widehat{\phi_{cs}}} \approx \frac{\chi_{cs}}{1-\sum_{s\in\mathcal{N}}\chi'_{cs}}. \tag{3.A.4.17}$$

From the definition of sourcing potential $\phi_s = T_s(\tau_{cs}w_s)^{-\theta}$, we have $\frac{\partial\ln\widehat{\Theta_c}(\varphi)}{\widehat{\phi_{cs}}} = -\theta$. The pass-through of the trade cost shock $\widehat{\tau cs}$ to marginal cost of the firm is therefore given by

$$\begin{aligned}\frac{\partial\ln\widehat{\Theta_c}(\varphi)}{\partial\ln\widehat{\tau_{cs}}} &= \frac{\partial\ln\widehat{c_c}}{\partial\ln\widehat{\Theta_c}}\cdot\frac{\partial\ln\widehat{\Theta_c}}{\partial\ln\widehat{\phi_{cs}}}\cdot\frac{\partial\ln\widehat{\phi_{cs}}}{\partial\ln\widehat{c_{cs}}}\\ &\approx \frac{\chi_{cs}}{1-\sum_{s\in\mathcal{N}}\chi'_{cs}}.\end{aligned} \tag{3.A.4.18}$$

□

Equation (3.A.4.12) indicates that the pass-through of the adverse shock has two components: the intensive margin captured by $\chi_{cs}(\varphi)$ and the extensive margin captured by $1-\sum_{s\in\mathcal{N}_s(\varphi)}\chi_{cs}(\varphi)$. Both depend on firm productivity $\varphi$. However, assuming that sourcing decisions are complementary, no firm will add new suppliers facing adverse shock, and in this case, the pass-through depends only on the intensive margin.

Equation (3.A.4.13) suggests that the impact of the shock increases with $\chi_{cs}(\varphi)$. If the firm is not diversified at all and relies solely on one supplier hit by the shock, the pass-through is 100%. On the other hand, high-productivity firms are more diversified and source from more places. Their load of inputs on any particular route is smaller, and so is the pass-through. It also tells us that the pass-through is larger for sources with higher sourcing potential. These results can be shown in the second derivative of equation (3.A.4.13):

$$\frac{\partial^2\ln\widehat{(c_c(\varphi))}}{\partial\ln\widehat{\tau_{cs}}\partial\varphi} \leq 0,\ \ \frac{\partial^2\ln\widehat{(c_c(\varphi))}}{\partial\ln\widehat{\tau_{cs}}\partial\phi_s} > 0.$$

Furthermore, it can be shown that more diversified firms are also less volatile. This can be expressed in the following proposition:

**Proposition 2**. *(a) If the shocks on trade costs are not perfectly correlated and have the same variance $\xi^2$, opening to trade lowers the volatility of firms' source capabilities. (b) If sourcing decisions are complementary across sources and the adverse shocks are independent and identically distributed, the volatility of firm revenue is*

$$var(\widehat{R(\varphi)}) \propto \xi^2 HHI(\varphi), \tag{3.A.4.19}$$

*which weakly decrease with productivity.*

*Proof.* From the proof of Proposition 1, we know that the change in sourcing capability $\widehat{\Theta} = \sum \phi_s$ (For simplicity, we omit the subscript $c$.) for a particular firm is given by

$$\widehat{\Theta} = \frac{\sum_{s\in\mathcal{C}} \chi_s \widehat{\phi}_s}{1 - \sum_{s\in\mathcal{N}} \chi'_s}. \tag{3.A.4.20}$$

Denoting $\Omega$ and $\Omega'$ as the sets of sources before and after the shock, we further simplify the notation as

$$\widehat{\Theta} = \sum_{s\in\Omega} \chi_s \Delta_s, \tag{3.A.4.21}$$

where $\Delta_s = \widehat{\phi}_s \delta_s(\varphi, \widehat{\phi})$, with $\delta_s(\varphi, \widehat{\phi}) = \frac{1}{1-\sum_{s\in\mathcal{N}} \chi'_s}$ if $s \in \mathcal{C}$, and 0 otherwise, which captures the extensive margin shock of sourcing capabilities. Under the assumption that $\delta_s$ has the same variance $\xi^2$ across source countries, we have

$$\begin{aligned} var(\widehat{\Theta(\varphi)}) &= var(\sum_{s\in\Omega} \chi_s(\varphi)\Delta_s) \\ &= \sum_{s\in\Omega} \chi_s(\varphi)^2 var(\Delta_s) + \sum_{m\neq n, m,n\in\Omega} \chi_m \chi_n cov(\Delta_m, \Delta_n) \\ &= \xi^2 (\sum_{s\in\Omega} \chi_s(\varphi)^2 + \sum_{m\neq n, m,n\in\Omega} \chi_m \chi_n \rho(\Delta_m, \Delta_n) \\ &\leq \xi^2, \end{aligned} \tag{3.A.4.22}$$

where $\rho \equiv \frac{cov(\Delta_m, \Delta_n)}{\xi^2}$ is the correlation of the shocks. The last inequality holds because $\left(\sum_{s\in\Omega} \chi_s(\varphi)\right)^2 = \sum_{s\in\Omega} \chi_s(\varphi)^2 + \sum_{m\neq n, m,n\in\Omega} \chi_m(\varphi)\chi_n(\varphi) = 1$. As long as the shocks are not perfectly correlated, we have $var(\widehat{R(\varphi)}) < \xi^2$.

If the shocks are i.i.d., such that $\rho_{mn} = 0$, we have

$$\begin{aligned} var(\widehat{\Theta(\varphi)}) &= var(\sum_{s\in\Omega} \chi_s(\varphi)\Delta_s) \\ &= \xi^2 \sum_{s\in\Omega} \chi_s(\varphi)^2 \\ &= \xi^2 HHI(\varphi). \end{aligned} \tag{3.A.4.23}$$

Since the firm's revenue is given by $R(\varphi) = \varphi^{\sigma-1}\big(\gamma\Theta(\varphi)\big)^{(\sigma-1)/\theta} B$ in equation (3.A.3.10), we have

$$\widehat{R(\varphi)} = \widehat{\Theta(\varphi)}. \tag{3.A.4.24}$$

Therefore the variance of the firm revenue is proportional to the variation in sourcing capability, thus proportional to $\xi^2 HHI(\varphi)$. □

To generate empirically testable predictions, we study how easily observed firm-level import flows will respond to an adverse shock. The model delivers the following result.

**Proposition 3.** *For a small trade cost shock that increases $\tau_{cs}$ to $\tau'_{sc}$ such that firms do not abandon source s, the import flows respond according to*

$$-\frac{\partial \ln \widehat{M_{cs'}(\varphi)}}{\partial \ln \widehat{\tau_{cs}}} = \begin{cases} \theta + (\sigma - 1 - \theta)\chi_{cs'}(\varphi), & \text{if } s' = s; \\ (\sigma - 1 - \theta)\chi_{cs'}(\varphi), & \text{otherwise.} \end{cases} \tag{3.A.4.25}$$

*Proof.* The trade flow at firm level is given by equation (3.A.3.11). Facing a supply shock, the change in trade flow is determined by

$$\begin{aligned} \widehat{M_{cs}}(\varphi) \equiv \frac{\widehat{M'_{cs}(\varphi)}}{\widehat{M_{cs}(\varphi)}} &= \widehat{\Theta(\varphi)}^{\frac{\sigma-1}{\theta}} \widehat{\chi_{cs}}(\varphi) \\ &= \widehat{\Theta(\varphi)}^{\frac{\sigma-1}{\theta}-1} \phi_{cs}. \end{aligned} \tag{3.A.4.26}$$

Taking the log on both sides of equation (3.8), we have $\ln \widehat{M_{cs}}(\varphi) = (\frac{\sigma-1}{\theta} - 1)\ln \widehat{\Theta}(\varphi) + \ln \phi_{cs}$. From the proof of Proposition 1, we know that for an adverse shock,

$$\frac{\partial \ln \widehat{\Theta_c}(\varphi)}{\partial \ln \widehat{\phi_{cs}}} \approx \chi_{cs} \text{ and } \frac{\partial \ln \widehat{\phi_{cs}}}{\partial \ln \widehat{\tau_{cs}}} = -\theta.$$

Thus Proposition 3 holds. □

Equation (3.A.4.25) indicates that the pass-through endogenously depends on firm productivity $\varphi$. Other than the usual Fréchet shape parameter $\theta$, which captures the direct impact of the shock, there is an additional term $(\sigma - 1 - \theta)\chi_{cs'}(\varphi)$, which is positive if sourcing decisions are complementary ($(\sigma - 1)/\theta > 1$) and negative if inputs are substitutable ($(\sigma - 1)/\theta < 1$).

Accordingly, the trade cost shock reduces firms' sourcing capability and increases their marginal cost. This drives down the marginal demand curve for all inputs if the sourcing decisions are complementary. Such a feedback effect through interdependencies amplifies the initial cost shock and reduces imports further. In contrast, if the inputs are substitutable, the cost shock reduces firm output and drives up the marginal demand curve. Such an increase in the marginal demand for the input dampens the initial negative shock. This difference will allow us to identify whether sourcing decisions are complementary or substitutable.

The pass-through also varies by the sourcing intensity $\chi_{ck}(\varphi)$. The feedback effect is stronger if the firm has a heavier load on inputs from a source being shocked, which tends to be the case for a less diversified firm. Finally, the interdependency is also reflected by the result that imports also respond to shocks on other routes in the firm's sourcing strategy.

So far, we have assumed that the final-goods producers use inputs from the same industry. We can also generalise the model to allow firms to use inputs from different industries.

The firm's marginal cost is given by

$$c_c(\varphi) = \frac{1}{\varphi}\Big(\sum_{i=1}^{I} c_c^i(\varphi)^{1-\eta}\Big)^{\frac{1}{1-\eta}}, \eta > 1, \tag{3.A.4.27}$$

where $\eta$ is the elasticity of substitution for inputs from different industries. The pass-through of an adverse shock is given by

$$-\frac{\partial \ln \widehat{M^i_{cs'}}(\varphi)}{\partial \ln \widehat{\tau^i_{cs}}} = \begin{cases} \theta_i + \big[(\sigma-\eta)\delta_c^i(\varphi) + (\eta - 1 - \theta_i)\big]\chi^i_{cs'}(\varphi), & \text{if } s' = s; \\ \big[(\sigma-\eta)\delta_c^i(\varphi) + (\eta - 1 - \theta_i)\big]\chi^i_{cs'}(\varphi), & \text{otherwise,} \end{cases} \tag{3.A.4.28}$$

where $\delta_c^i$ is the cost share of industry $i$'s inputs, and $\chi^i_{cs'}(\varphi)$ is the share of industry $i$'s inputs sourced from country $s$. The substitutability of varieties within each industry is captured by $\theta_i$. On the one hand, a higher substitutability enables the firm to substitute away from source countries hit by a shock, which can lead to a higher pass-through. On the other hand, since the firm can find substitutable inputs from other sources, the marginal cost does not go up as much and thus this effect tends to decrease the size of the pass-through.

### 3.A.5 Input-Output Linkages

This paper also aims to assess the impact of natural disasters on Chinese manufacturing exporters via their production network. It is therefore important to highlight how the theoretical framework of Acemoglu et al. (2016) can be linked with insights from the input-output literature as described in, for example, Koopman et al. (2014) and Wang et al. (2013). While Acemoglu et al. (2016) show how an industry's output is affected by domestic shocks within the *national* input-output structure of an economy, this paper applies the theoretical concept to individual firms and generalises the model to include shocks affecting the *international* input-output structure underlying a firm's production.

Since we do not observe firm-to-firm sales in our data, we need to assess the supply chain propagation of a natural disaster shock in the context of industry-level input-output linkages. Assuming that firms follow the same profit maximisation across industries, the firm-level shock can be aggregated to the industry level, in which $i$ and $j$ represent the downstream and upstream industries, respectively. The corresponding input-output matrix $\mathbf{A}$ for $N$ industries in the world can be represented as follows.

$$\mathbf{A} = \begin{pmatrix} a_{11} & a_{12} & ... & ... \\ a_{21} & a_{22} & ... & ... \\ ... & ... & ... & ... \\ ... & ... & ... & a_{NN} \end{pmatrix}$$

Accordingly, the individual output allocation coefficients $a_{ji}$ provide information on how much of its output an industry $j$ (indicated by the column) provides to another industry $i$ (indicated by the row) for output production.[21] This plays an essential role in pinning down the input-output structure of the world economy.

To assess the change of output in response to exogenous changes of inputs, we consider the so-called *Ghosh inverse* matrix $\mathbf{G}$ based on the input-output matrix $\mathbf{A}$.[22]

Mathematically,

$$\mathbf{G} = (I - \mathbf{A})^{-1}.$$

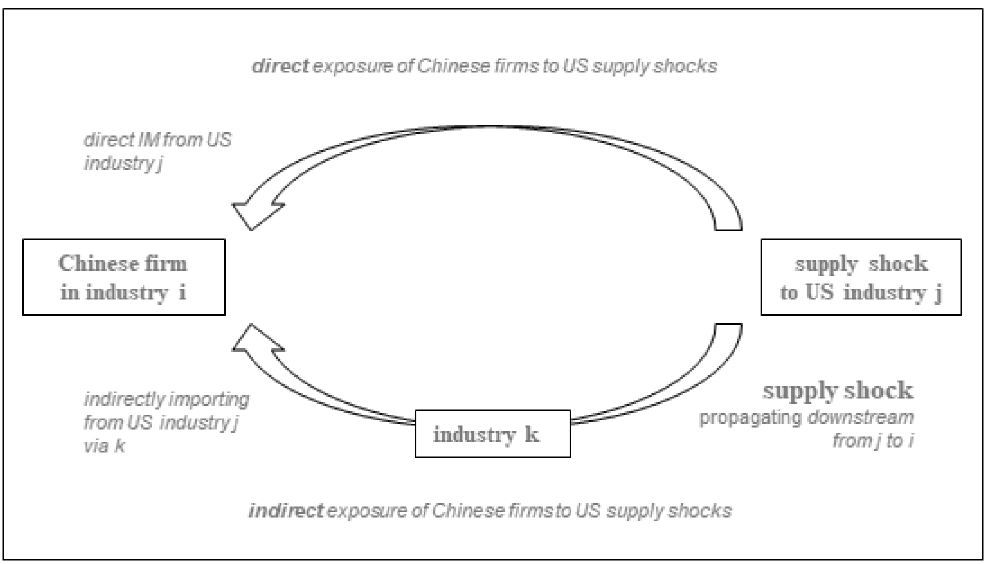
[21] With sales from $i$ to $j$ and industry output $x_i$, coefficients are calculated as $a_{ij} = z_{ij}/x_i$ (Galbusera and Giannopoulos, 2018).

[22] Alternatively, some studies use the common Leontief inverse indicating how much value added is needed to sustain the production of one more unit of output. Different from the Ghosh approach, the Leontief inverse considers the following technical coefficients to compute the inverse matrix: $e_{ij} = z_{ij}/x_j$. For a discussion on the approaches of both the Leontief and Ghosh models in studying the impacts of natural disasters, see Galbusera and Giannopoulos (2018).

The Ghosh inverse matrix is a compact representation of the ripple effects in an economy where industries are interconnected. Individual elements of the Ghosh inverse, such as $g_{ji}$ contain information on the change in output of industry $i$ in response to an exogenous change of inputs from sector $j$ (Dietzenbacher, 1997).[23]

Against this background, one might also understand that a shock to upstream industry $j$ in the form of a sudden drop in output influences the production of its downstream industry $i$. In this spirit, Acemoglu et al. (2016) use the input-output inverse matrices to show how different shocks of an industry can propagate up- and downstream through the production network. To evaluate the impact of the 2005 US hurricane season on the trade performance of Chinese processing manufacturers, we apply the idea of a shock propagation through the domestic production network to an international setting. In particular, we focus on two different channels through which a shock to industry $j$ located in the United States might affect Chinese firms operating in industry $i$. Figure 3.A.1 summarises relevant mechanisms of a shock transmission from a US industry $j$ to a Chinese firm operating in sector $i$.

Figure 3.A.1: Propagation of demand and supply shocks




The downstream industry $i$ might be affected either *directly* or *indirectly* by a shock in $j$. Regarding the former case, a firm in $i$ is assumed to be *directly* affected

[23]Similar to matrix **A**, the Ghosh inverse matrix **G** is a matrix of N × N dimension.

by a supply shock in $j$ if it directly imports from the affected industry $j$. With less supply from $j$, a firm in industry $i$ has to reduce or even halt production of output, depending on the substitutability of intermediates sourced from industry $j$.

Regarding the latter case, a firm in $i$ is assumed to be *indirectly* affected by a supply shock in $j$ if $i$ is not sourcing directly from $j$ but *indirectly* via a third industry $k$ (located anywhere in the world). With less supply from $j$, industry $k$ has to reduce or halt its production of output so that, in turn, $k$ supplies less to Chinese firms in industry $i$. Consequently, the US supply shock in industry $j$ propagates *downstream* to Chinese firms in sector $i$ via the production network involving industry $k$.

More concretely, we take the Ghosh inverse of the OECD Inter-Country Input-Output (ICIO) Tables to approximate the interlinkage of industries across countries. We then take the relevant portions of the Ghosh inverse matrix relating to the interlinkages between US and Chinese industries. The supply effect of upstream sector $j$ in the United States on downstream sector $i$ in China would be represented by the block in the lower left side of the matrix $\mathbf{G}^{USA\to CHN}$.

$$
\mathbf{B} = \begin{array}{cc}
 & \begin{array}{ccccc} \cdots & CHN & \cdots & USA & \cdots \end{array} \\
\begin{array}{c} \cdots \\ CHN \\ \cdots \\ USA \\ \cdots \end{array} &
\begin{bmatrix}
\cdots & \cdots \\
\ddots & \begin{array}{|ccc|} \hline g_{11}^{CHN\to USA} & \cdots & \\ & \ddots & \\ & \cdots & g_{NN}^{CHN\to USA} \\ \hline \end{array} \\
\cdots & \cdots \\
\begin{array}{|ccc|} \hline g_{11}^{USA\to CHN} & \cdots & \\ & \ddots & \\ & \cdots & g_{NN}^{USA\to CHN} \\ \hline \end{array} & \ddots \\
\cdots & \cdots
\end{bmatrix}
\end{array}
$$

## 3.B Preparation of the China Customs Statistics

The China Customs Statistics is at the transaction-month level, and the raw data for 2001–2006 are in 2,051 subfiles, with each file containing 60,000 transactions. Therefore, as the first step, we converted all the files to UTF-8 encoded files and unified the variables' names in all the subfiles. Then we vertically merged all the files by year.

Next, we aggregated the transaction-level data to monthly firm-level import and export data by transition country and trading partner, including more than 200 countries and regions; customs port in China where the goods are loaded; customs regime, such as ordinary trade and processing trade; transporting method; and locations of importers and exporters. The aggregated data contain the monthly volume and value of imports and exports of firms.

Table 3.B.1: OECD ICIO (2016 edition) industry aggregation of ISIC sectors

| 2-digit ISIC industry | Industry description |
|---|---|
| C15T16 | Food products, beverages & tobacco |
| C17T19 | Textiles, textile products, leather & footwear |
| C20 | Wood & products of wood & cork |
| C21T22P | Pulp, paper (products), printing & publishing |
| C23 | Coke, refined petroleum products & nuclear fuel |
| C24 | Chemicals & chemical products |
| C25 | Rubber & plastics products |
| C26 | Other non-metallic mineral products |
| C27 | Basic metals |
| C28 | Fabricated metal products |
| C29 | Machinery & equipment, nec |
| C30T33 | Computer, electronic & optical equipment |
| C31 | Electrical machinery & apparatus, nec |
| C34 | Motor vehicles, trailers & semi-trailers |
| C35 | Other transport equipment |
| C36T37 | Manufacturing nec; recycling |

## 3.C The *Direct* Effect of the US Trade Shock

Figure 3.C.1: Density plots of average US trade shares during the pre-disaster period (08/2004-07/2005)

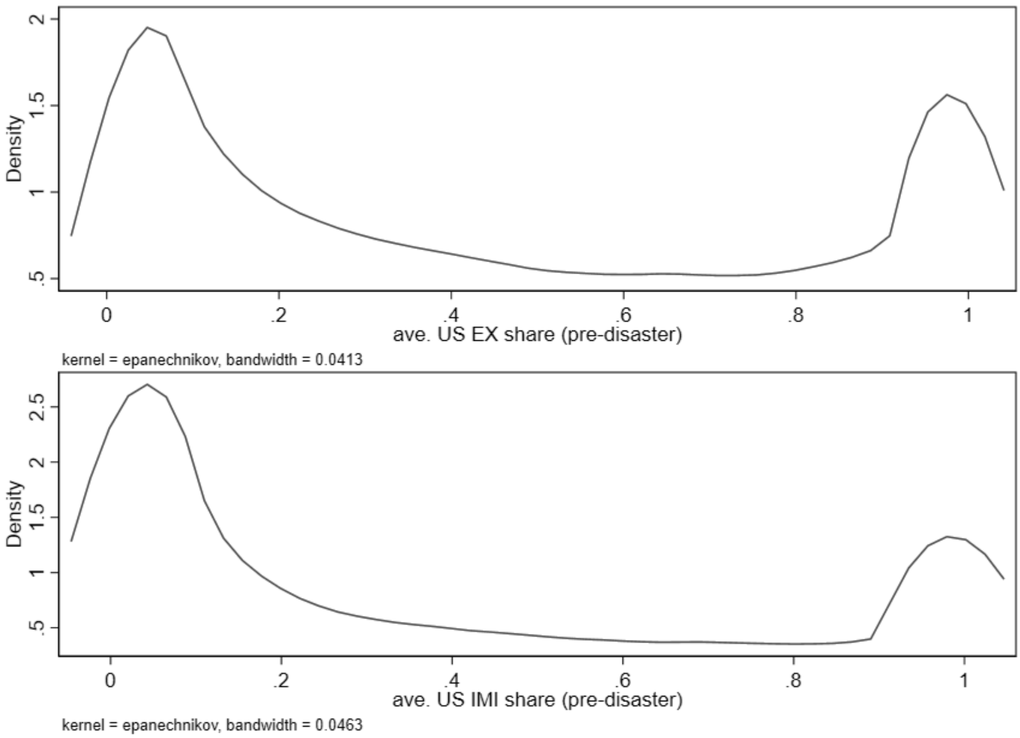


Table 3.C.1: Shares of 7 states affected by the US hurricane season 2005, by sector

| Industry description | Share of 7 states in total exports of sectors (in %) | Share of 7 US states in total imports of sectors (in %) |
|---|---|---|
| Food products, beverages & tobacco | 24 | 19 |
| Textiles, textile products | 34 | 14 |
| Leather & footwear | 24 | 11 |
| Wood & products of wood & cork | 16 | 20 |
| Pulp, paper (products), printing & publishing | 28 | 11 |
| Coke, refined petroleum products & nuclear fuel | 58 | 43 |
| Chemicals & chemical products | 32 | 18 |
| Rubber & plastics products | 22 | 17 |
| Other non-metallic mineral products | 16 | 23 |
| Basic metals | 21 | 23 |
| Machinery & equipment, nec | 26 | 24 |
| Electrical & optical equipment | 27 | 21 |
| Transport equipment | 19 | 13 |
| Manufacturing nec; recycling | 12 | 13 |

Table 3.C.2: Regression results for coefficient plots of Figure 3.3

| | US EX | US EX ext. margin | US EX int. margin | US IMI | US IMI ext. margin | US IMI int. margin |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| XRATE | -6256.0** | -0.0440*** | -2468.7 | 172.5 | -0.00232 | -114.0 |
| | (3,149.826) | (0.005) | (1,763.232) | (646.463) | (0.002) | (346.505) |
| $\text{TREATMENT}_{fi}^{EX,IMI}$ | -14156.4 | 0.0149 | -7708.4 | 2613.0 | 0.0607*** | 1716.4 |
| | (21,020.457) | (0.020) | (19,133.867) | (3,855.774) | (0.020) | (1,948.219) |
| 07/05 | 3926.3 | 0.0244*** | 1114.9 | -560.3 | -0.00324 | 99.27 |
| | (2,978.657) | (0.005) | (1,699.393) | (659.695) | (0.002) | (388.085) |
| 08/05 | 4782.4 | 0.0210*** | 1496.2 | -326.9 | 0.00269 | 56.38 |
| | (3,026.481) | (0.005) | (1,599.216) | (636.876) | (0.002) | (345.433) |
| 09/05 | 9688.5*** | 0.0179*** | 3869.3*** | -535.9 | 0.00137 | -104.1 |
| | (3,398.534) | (0.005) | (1,404.904) | (614.059) | (0.002) | (330.226) |
| 10/05 | 9210.3** | 0.00264 | 4711.7*** | -1327.8** | -0.0114*** | -326.9 |
| | (4,010.199) | (0.005) | (1,386.542) | (622.059) | (0.002) | (341.322) |
| 11/05 | 8150.0** | 0.00199 | 3699.4*** | -818.2 | -0.00108 | -7.453 |
| | (3,436.759) | (0.004) | (1,180.151) | (596.598) | (0.002) | (342.762) |
| IA 06/05 | 19903.1 | -0.000951 | 21332.3 | 1340.9 | 0.00451 | 1415.8 |
| | (20,612.198) | (0.016) | (19,418.855) | (2,976.408) | (0.027) | (1,849.500) |
| IA 07/05 | 5853.0 | 0.00700 | 3844.4 | -1132.5 | -0.0160 | -1181.8 |
| | (20,959.054) | (0.024) | (17,770.322) | (2,936.111) | (0.026) | (1,796.859) |
| IA 08/05 | 13763.8 | -0.0108 | 8191.6 | -2131.9 | -0.0622** | -1026.0 |
| | (21,224.309) | (0.023) | (18,152.225) | (2,461.755) | (0.025) | (1,725.233) |
| IA 09/05 | 5704.3 | -0.0231 | 1967.8 | 4175.4 | -0.0729*** | -657.4 |
| | (17,274.805) | (0.022) | (13,160.357) | (4,209.221) | (0.023) | (1,998.304) |
| IA 10/05 | 29805.7 | -0.0419* | 33404.6 | -6045.9** | -0.103*** | -3954.8** |
| | (39,802.648) | (0.022) | (38,158.509) | (2,531.462) | (0.022) | (1,579.730) |
| IA 11/05 | 26407.9 | -0.0174 | 28574.5 | -2886.0 | -0.0588*** | -3908.1** |
| | (29,801.822) | (0.020) | (28,048.612) | (2,725.469) | (0.022) | (1,830.252) |
| Firm-FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 587,240 | 587,240 | 587,240 | 911,188 | 911,188 | 911,188 |

**Note:** Standard errors in parentheses, clustered at the firm level. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 3.C.3: Summary statistics for std. $\Delta\, ln\, dir.\, SUP\, shock$ variable

| # observations | Mean | Std. dev. | Min. | Max. |
|---|---|---|---|---|
| Sample period Sep. 2005–Dec. 2006 | | | | |
| 4,786,158 | -0.1073438 | 0.5059222 | -4.358899 | 5.747049 |
| Hurricane season 2005 Sep.-Nov. 2005 | | | | |
| 1,081,193 | -0.0870469 | 0.4744142 | -3.872983 | 4.477215 |

Table 3.C.4: Regression results in addition to Table 3.4, part I

| | All | Food | Textile | Wood | Paper | Coke | Chem. | Rubber | o.nmMin |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| $\Delta$ ln dir.SUPshock$^{7USst.}$ | 0.003$^{*}$ | 0.002$^{*}$ | 0.002 | 0.002 | 0.003$^{**}$ | 0.003$^{**}$ | 0.004$^{***}$ | 0.003$^{*}$ | 0.003$^{**}$ |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) |
| IA: $\Delta$ ln dir.SUPshock$^{7USst.}$ x Hurricane = 1 | 0.014$^{***}$ | 0.014$^{***}$ | 0.015$^{***}$ | 0.015$^{***}$ | 0.014$^{***}$ | 0.015$^{***}$ | 0.014$^{***}$ | 0.014$^{***}$ | 0.015$^{***}$ |
| | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) | (0.007) | (0.005) | (0.006) | (0.006) |
| IA: $\Delta$ ln dir.SUPshock$^{7USst.}$ x IMI-industry = col.2–9 | | 0.002$^{***}$ | 0.001 | 0.006$^{***}$ | -0.001 | -0.000 | -0.004$^{***}$ | 0.000 | -0.002 |
| | | (0.008) | (0.004) | (0.011) | (0.003) | (0.016) | (0.004) | (0.003) | (0.014) |
| IA: $\Delta$ ln dir.SUPshock$^{7USst.}$ x Hurricane = 1 x IMI-industry = col.2–9 | | 0.000 | -0.003$^{***}$ | -0.006$^{***}$ | -0.001 | -0.008$^{***}$ | -0.000 | 0.001 | -0.005$^{***}$ |
| | | (0.034) | (0.012) | (0.020) | (0.014) | (0.008) | (0.011) | (0.012) | (0.018) |
| Firm-FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| EXindustry-Time-FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| IMIindustry-FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| ROW-Control | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 |

**Note:** (Standardised) $\Delta$ ln EX as dependent variables for all regressions. *Hurricane* refers to a dummy variable that equals 1 from September to November 2005. $IMI - industry$ dummy variables equal 1 if the importing industry corresponds to the industry indicated by the column. Individual $IMI - industry$ dummy variables are dropped due to common import industry fixed effects. Standard errors in parentheses, clustered at the firm level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.C.5: Regression results in addition to Table 3.4, part II

| | All | BasMet. | FabMet. | Mach. | El/OptEq. | ElMach. | TrEq. | o.TrEq. | M.Recyc |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| $\Delta$ ln dir. SUPshock$^{7USst.}$ | 0.003$^{*}$ | 0.002 | 0.003$^{**}$ | 0.004$^{**}$ | 0.003$^{**}$ | 0.002 | 0.002$^{*}$ | 0.003$^{**}$ | 0.001 |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) |
| IA: $\Delta$ ln IMI$^{7USst.}$ | 0.014$^{***}$ | 0.016$^{***}$ | 0.014$^{***}$ | 0.012$^{***}$ | 0.012$^{***}$ | 0.010$^{***}$ | 0.014$^{***}$ | 0.014$^{***}$ | 0.014$^{***}$ |
| x Hurricane = 1 | (0.006) | (0.007) | (0.005) | (0.006) | (0.005) | (0.005) | (0.006) | (0.006) | (0.006) |
| IA: $\Delta$ ln dir. SUPshock$^{7USst.}$ | | 0.001 | -0.002 | -0.003$^{***}$ | -0.001 | 0.002 | 0.001$^{**}$ | -0.005$^{**}$ | 0.007$^{***}$ |
| x IMI-industry = col.2–9 | | (0.011) | (0.005) | (0.003) | (0.008) | (0.004) | (0.004) | (0.042) | (0.010) |
| IA: $\Delta$ ln dir. SUPshock$^{7USst.}$ | | -0.009$^{***}$ | -0.001 | 0.006$^{***}$ | 0.010$^{***}$ | 0.009$^{***}$ | 0.001 | -0.003$^{**}$ | -0.001 |
| x Hurricane = 1 | | (0.024) | (0.013) | (0.012) | (0.034) | (0.018) | (0.033) | (0.031) | (0.023) |
| x IMI-industry = col.2–9 | | | | | | | | | |
| Firm FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| EX-industry-time FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| IMI-industry FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| ROW control | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 |

**Note:** (Standardised) $\Delta$ ln EX as dependent variables for all regressions. *Hurricane* refers to a dummy variable that equals 1 from September to November 2005. $IMI - industry$ dummy variables equal 1 if the importing industry corresponds to the industry indicated by the column. Individual $IMI - industry$ dummy variables are dropped due to common import industry fixed effects. Standard errors in parentheses, clustered at the firm level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

## 3.D The *Indirect* Effect of the US Trade Shock

This appendix evaluates the extent to which the US supply shock propagated through international production networks. Thus, the focus is on the *indirect* exposure of Chinese processing manufacturers to US supply shocks through international production network of Chinese firms. The following subsection explains the empirical strategy along with the precise calculation of the network supply shock.

### 3.D.1 Empirical Strategy

To calculate the US *network* supply shock, we combine exports of affected states to the rest of the world with information on international production linkages. Specifically, this approach uses input-output tables from the OECD to compute a Ghosh inverse matrix. Individual elements of the Ghosh inverse matrix allow us to "calculate changes in gross sectoral outputs for exogenously specified changes in the sectoral inputs" (Dietzenbacher, 1997). We therefore calculate a measure on the *indirect* exposure of Chinese processing firms to the US supply shock using equation (3.D.1.1).

$$netw.SUPshock_{fit}^{7USstates} = \sum_{j} \left[ (g_{ji}^{2004} - dirIMI_{fjt}^{CHN \leftarrow US} \cdot g_{ji}^{2004})' \cdot EX_{jt}^{7USstates \rightarrow RoW} \right], \tag{3.D.1.1}$$

where $g_{ji}^{2004}$ is the Ghosh inverse matrix element for industries $j$ and $i$ of 2004. In the spirit of equation 3.3, $EX_{jt}^{7USstates \rightarrow RoW}$ captures the supply capacity of hurricane-affected states, while $dirIMI_{fjt}^{CHN \leftarrow US}$ represents a dummy variable equal to 1 if a Chinese firm directly imports from the United States in industry $j$ at time $t$.

The analysis of this paper distinguishes *direct* effects from *network* effects. Given that a calculation of *network* effects based on the Ghosh inverse, $g_{ji}^{2004}$, and $EX_{jt}^{7USstates \rightarrow RoW}$ would technically include *direct* imports from the United States, it is important to control for double counting of direct effects. We thus eliminate elements of the Ghosh inverse for industries from which Chinese processing firms are sourcing directly. Thus the network linkages, which are already captured by the direct shock variables, are canceled out. Technically, this approach is captured by the term $(g_{ji}^{2004} - dirIMI_{fjt}^{CHN \leftarrow US} \cdot g_{ji}^{2004})$, where $dirIMI_{fjt}^{CHN \leftarrow US}$ is a dummy variable equal to one if a firm is directly importing from US industry $j$ at time $t$ so that the corresponding Ghosh inverse element is zero. The network supply shock hence captures the extent to which a Chinese processing firm

$f$ operating in industry $i$ is *indirectly* exposed to US supply fluctuations given the firm's international production networks.

### 3.D.2 Results

We begin the analysis of our results by presenting Figure 3.D.1, which illustrates the extent to which Chinese manufacturing industries were exposed to fluctuations of suppliers from the affected states. In line with equation (3.D.1.1), the *network* supply shock is calculated based on export fluctuations of the seven affected states in conjunction with information on input-output flows between the United States and Chinese manufacturing industries derived from the Ghosh inverse. For ease of interpretation, values in Figure 3.D.1 were standardised by Chinese importing industries. From the perspective of individual manufacturing industries in China, this supply shock variable captures the extent to which US supply fluctuation can propagate downstream to Chinese manufacturing industries along respective value chain linkages. The vertical lines in the charts denote the points in time when three of the most severe hurricanes made landfall in the United States, the end of August, September, and October in 2005.[24]

As shown in Figure 3.D.1, standardised network supply shock temporarily dropped in September and October 2005. This pattern suggests that Chinese processing firms were *indirectly* exposed to a drop in supply from the affected states along their international production linkages. It should be noted that, the supply shock depicted in Figure 3.D.1 does not measure the actual drop in trade on the part of Chinese firms. It measures the *potential indirect exposure* of Chinese manufacturing industries to US supply shocks via the United States' and China's international production network throughout the rest of the world.[25]

We followed a similar empirical strategy in Section 3.3.2.1 to estimate the impact of firms' *indirect* exposure to negative US supply shocks on their exports.[26]

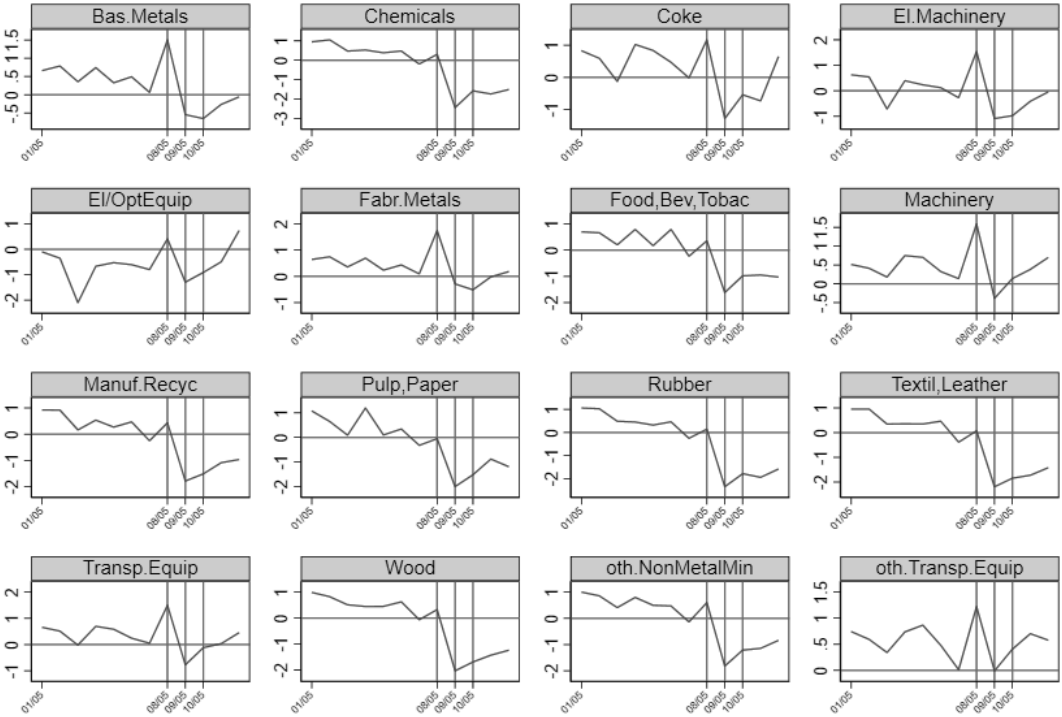
[24]Because data from Chinese customs statistics represent values by the *end* of a given month, the area between the vertical lines *de facto* represents the months of September and October 2005.

[25]To demonstrate that the drop in supply from affected states is not due to a common decline in US output, we present supply shocks from the 43 unaffected US states that were not directly hit by the hurricanes during the 2005 season. These results are plotted in Figure 3.D.2.

[26]We estimate the effects of the indirect exposure to supply shocks on firms' exports using the following equation based on equation (3.4). The results are presented in Table 3.D.1.

$$\begin{aligned}\Delta\, ln\, EX_{fpit} = {} & \alpha_f + \beta_j + \gamma_{it} + \zeta\, H^{Sep-Nov,2005} \\ & + \tau_1\, \Delta\, ln\, direct\, SUPshock^{7\,USstates}_{fjt} + \tau_2\, \Delta\, ln\, netw.\, SUPshock^{7\,USstates}_{fit-1} + \\ & + \eta_1\, H^{Sep-Nov,2005} \cdot \Delta\, ln\, direct\, SUPshock^{7\,USstates}_{fjt} \\ & + \eta_2\, H^{Sep-Nov,2005} \cdot \Delta\, ln\, netw.\, SUPshock^{7\,USstates}_{fit-1} \\ & + \tau_3\, \Delta\, ln\, direct\, IMI^{ROW}_{fjt} + \epsilon_{fpit}.\end{aligned}$$

Figure 3.D.1: Exposure of Chinese manufacturing industries to supply shocks triggered by the 2005 US hurricane season




**Note:** This figure presents network supply shocks by industry calculated using equation (3.D.1.1) and aggregated by Chinese manufacturing sectors over time. Each chart represents a different sector.

Hurricane season for the US East Coast occurs during a certain time of year, so it is important to verify that trade fluctuations triggered by the 2005 US hurricane season exceeded common seasonal fluctuations. To address this concern, we use the standardised year-on-year differences between export supply flows of 2004 and 2005. This causes the seasonal fluctuations to be differenced, out and a decline of respective shock variables implies that trade flow substantially deviated from the mean values in September and October 2005.

Table 3.D.1 presents the result estimations of the link between US supply shocks and exports of Chinese firms. Column 1 shows that the relationship of a positive link between the *direct* supply shock and exports is robust against the inclusion of the *network* supply shock variable. Still, regarding the latter, network supply is negatively associated with Chinese processing firms' exports during the 2005 US hurricane season. More precisely, a drop in the network supply

Table 3.D.1: Regression results of *direct* and *indirect* supply shocks

| | All | Textile | Paper | Coke | Chemicals | Machinery | El./Opt. Eq. |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| $\Delta$ ln dir. SUPshock$^{7\,USstates}$ | 0.003* | 0.002 | 0.003** | 0.003** | 0.004*** | 0.003** | 0.003** |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) |
| IA: $\Delta$ ln dir. SUPshock$^{7\,USstates}$ x Hurricane = 1 | 0.014*** | 0.015*** | 0.014*** | 0.016*** | 0.014*** | 0.012*** | 0.012*** |
| | (0.006) | (0.006) | (0.006) | (0.007) | (0.005) | (0.006) | (0.005) |
| IA: $\Delta$ ln dir. SUPshock$^{7\,USstates}$ x IMI-industry = col. 2–7 | | 0.001 | -0.001 | -0.001 | -0.004** | -0.003*** | -0.001 |
| | | (0.004) | (0.003) | (0.015) | (0.004) | (0.003) | (0.008) |
| IA: $\Delta$ ln dir. SUPshock$^{7\,USstates}$ x Hurricane = 1 x IMI-industry = col. 2–7 | | -0.003** | -0.001 | -0.008*** | -0.000 | 0.006*** | 0.010*** |
| | | (0.012) | (0.014) | (0.008) | (0.010) | (0.012) | (0.034) |
| netw. SUPshock$_{t-1}$ | -0.001 | -0.001 | -0.002 | -0.001 | -0.000 | -0.002 | 0.002 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| IA: netw. SUPshock$_{t-1}$ x Hurricane = 1 | -0.014*** | -0.014*** | -0.014*** | -0.014*** | -0.016*** | -0.014*** | -0.014*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| IA: netw. SUPshock$_{t-1}$ x IMI-industry = col. 2–7 | | 0.003* | 0.004*** | 0.001 | -0.001 | 0.004*** | -0.007*** |
| | | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| IA: netw. SUPshock$_{t-1}$ x Hurricane = 1 x IMI-industry = col. 2–7 | | 0.001 | -0.000 | -0.000 | 0.004*** | -0.001 | -0.001 |
| | | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Firm FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| EX-industry-time FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| IMI-industry FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| ROW control | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 |

**Note:** (Standardised) $\Delta$ ln EX as dependent variables for all regressions. *Hurricane* refers to a dummy variable that equals 1 from September to November 2005. $IMI - industry$ dummy variables equal 1 if the importing industry corresponds to the industry indicated by the column. Individual $IMI - industry$ dummy variables are dropped due to common import industry fixed effects. Standard errors in parentheses, clustered at the firm level. * p < 0.1, ** p < 0.05, *** p < 0.01.

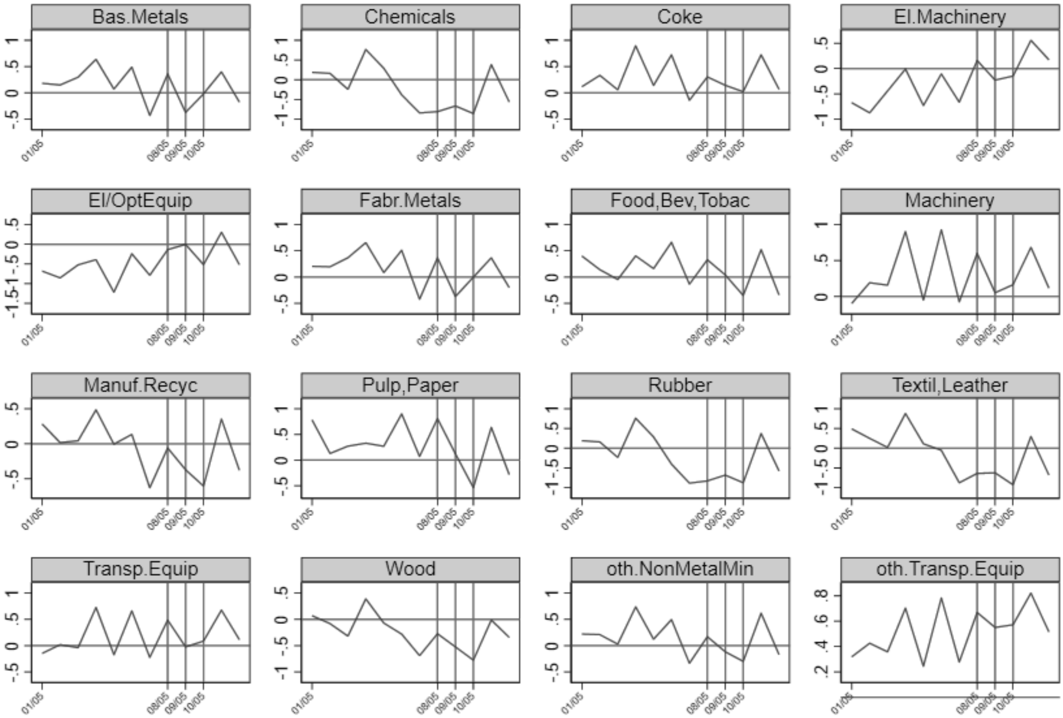
shock by one standard deviation in $t-1$ triggers an increase in exports by 0.015 $(-0.014 - 0.001)$ standard deviation at time $t$, thereby almost offsetting the impact of the *direct* supply shock on exports. This result is at odds with an expectation of a positive estimation coefficient, as should be the case when there is a propagation of adverse shocks to firm-level output.[27] It indicates that the US hurricane shock does not propagate along international supply chains.

Columns 2–7 present estimations of the impacts of adverse supply shocks on firms' exports in China by industry of the US suppliers. These are the industries that are highly concentrated in the states that were heavily hit by the hurricane. The impacts are statistically significantly different from zero only in the chemical industry (column 5). Specifically, the negative effect of the indirect exposure of Chinese firms to US supply shocks is weaker (by 0.004 standard deviations) when intermediates are sourced from the chemical industry. However, the overall impact is still negative (-0.016 + 0.004), which is at odds with an expected positive

[27] Similar to our findings in Section 3.3, the theoretical reasoning suggests finding a positive relation between shock variables and exports in case there is a drop in both the explained and explanatory variables.

sign of a shock propagation. Therefore, the result should be interpreted with caution. A potential explanation for this is that the US supply shock triggered by the 2005 hurricane season did not propagate to Chinese processing manufacturers through global value chains.

Figure 3.D.2: Exposure of Chinese manufacturing industries to supply shocks of remaining 43 states in 2005




**Note:** Individual charts plot results of network supply shocks computed according to equation (3.D.1.1) and aggregated by Chinese manufacturing sectors over time.

Table 3.D.2: Regression results in addition to Table 3.D.1, part I

| | all | Food | Textile | Wood | Paper | Coke | Chemi. | Rubber | o.nmMin |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Δ ln dir. SUPshock(usa7) | 0.003* | 0.002* | 0.002 | 0.001 | 0.003** | 0.003** | 0.004*** | 0.003* | 0.003** |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) |
| IA: Δ ln dir. SUPshock x Hurricane = 1 | 0.014*** | 0.014*** | 0.015*** | 0.015*** | 0.014*** | 0.016*** | 0.014*** | 0.014*** | 0.015*** |
| | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) | (0.007) | (0.005) | (0.006) | (0.006) |
| IA: Δ ln dir. SUPshock x IMI-industry = col.(2–9) | | 0.002*** | 0.001 | 0.006*** | -0.001 | -0.001 | -0.004** | 0.000 | -0.002 |
| | | (0.008) | (0.004) | (0.011) | (0.003) | (0.015) | (0.004) | (0.003) | (0.014) |
| IA: Δ ln dir. SUPshock(usa7) x Hurricane = 1 x IMI-industry = col.(2–9) | | 0.000 | -0.003** | -0.006*** | -0.001 | -0.008*** | -0.000 | 0.001 | -0.005*** |
| | | (0.035) | (0.012) | (0.020) | (0.014) | (0.008) | (0.010) | (0.012) | (0.017) |
| netw. SUPshock_t-1 | -0.001 | -0.001 | -0.001 | -0.001 | -0.002 | -0.001 | -0.000 | -0.001 | -0.001 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| IA: netw. SUPshock_t-1 x Hurricane = 1 | -0.014*** | -0.014*** | -0.014*** | -0.014*** | -0.014*** | -0.014*** | -0.016*** | -0.014*** | -0.014*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| IA: netw. SUPshock_t-1 x IMI-industry = col.(2–9) | | 0.002** | 0.003* | 0.003*** | 0.004*** | 0.001 | -0.001 | 0.001 | 0.003*** |
| | | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| IA: netw. SUPshock_t-1 x Hurricane = 1 x IMI-industry = col.(2–9) | | -0.002** | 0.001 | 0.000 | -0.000 | -0.000 | 0.004*** | -0.002** | -0.002*** |
| | | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Firm-FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| EXindustry-Time-FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| IMIindustry-FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| ROW-Control | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 |

**Note:** (Standardised) Δ ln EX as dependent variables for all regressions. *Hurricane* refers to a dummy variable that equals 1 from September to November 2005. $IMI - industry$ dummy variables equal 1 if the importing industry corresponds to the industry indicated by the column. Individual $IMI - industry$ dummy variables are dropped due to common import industry fixed effects. Standard errors in parentheses, clustered at the firm level. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 3.D.3: Regression results in addition to Table 3.D.1, part II

| | all | BasMet. | FabMet. | Mach. | El/OptEq. | ElMach. | TrEq. | o.TrEq. | M.Recyc |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Δ ln dir. $\text{SUPshock}^{7USst.}$ | 0.003* | 0.002 | 0.003** | 0.003** | 0.003** | 0.002 | 0.002* | 0.003* | 0.001 |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) |
| IA: Δ ln dir. $\text{SUPshock}^{7USst.}$ x Hurricane = 1 | 0.014*** | 0.016*** | 0.015*** | 0.012*** | 0.012*** | 0.010*** | 0.014*** | 0.015*** | 0.015*** |
| | (0.006) | (0.007) | (0.005) | (0.006) | (0.005) | (0.005) | (0.006) | (0.006) | (0.006) |
| IA: Δ ln dir. $\text{SUPshock}^{7USst.}$ x IMI-industry = col.(2–9) | | 0.001 | -0.002 | -0.003*** | -0.001 | 0.002 | 0.001* | -0.005** | 0.007*** |
| | | (0.011) | (0.005) | (0.003) | (0.008) | (0.004) | (0.004) | (0.042) | (0.010) |
| IA: Δ ln dir. $\text{SUPshock}^{7USst.}$ x Hurricane = 1 x IMI-industry = col.(2–9) | | -0.009*** | -0.001 | 0.006*** | 0.010*** | 0.009*** | 0.001 | -0.003** | -0.001 |
| | | (0.023) | (0.013) | (0.012) | (0.034) | (0.017) | (0.033) | (0.031) | (0.023) |
| netw. SUPshock_t-1 | -0.001 | -0.001 | -0.002 | -0.002 | 0.002 | 0.003 | -0.001 | -0.001 | -0.001 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| IA: netw. SUPshock_t-1 x Hurricane = 1 | -0.014*** | -0.014*** | -0.014*** | -0.014*** | -0.014*** | -0.014*** | -0.014*** | -0.014*** | -0.014*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| IA: netw. SUPshock_t-1 x IMI-industry = col.(2–9) | | 0.002 | 0.004*** | 0.004*** | -0.007*** | -0.008*** | 0.001 | 0.002*** | 0.003** |
| | | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| IA: netw. SUPshock_t-1 x Hurricane = 1 x IMI-industry = col.(2–9) | | -0.000 | -0.001 | -0.001 | -0.001 | 0.001 | -0.001 | -0.001** | -0.002** |
| | | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Firm-FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| EXindustry-Time-FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| IMIindustry-FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| ROW-Control | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 | 4,786,123 |

**Note:** (Standardised) Δ ln EX as dependent variables for all regressions. *Hurricane* refers to a dummy variable that equals 1 from September to November 2005. $IMI - industry$ dummy variables equal 1 if the importing industry corresponds to the industry indicated by the column. Individual $IMI - industry$ dummy variables are dropped due to common import industry fixed effects. Standard errors in parentheses, clustered at the firm level. * p < 0.1, ** p < 0.05, *** p < 0.01.

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