LUND UNIVERSITY
School of Economics and Management

Master's Programme in Economic Development and Growth

The Strange Case of Climate Policy and Economic Development

An Analysis of the Porter Hypothesis in the Context of Emission Trading Pilots in China

by

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Due to the climate crisis, more developing and emerging economies are considering to implement climate policies. However, since economic development remains high on their respective agendas, understanding economic consequences of such policies is crucial. The Porter hypothesis suggests in this respect that climate policy could benefit growth under certain circumstances by encouraging innovation, technological upgrading and structural change towards high-tech sectors. The paper analyses this hypothesis in the context of emission trading pilots in China, in particular investigating how the policy affected overall economic outcomes, innovation and structural change. To this end, a difference-in-difference analysis comparing pilot and non-pilot provinces as well as an in-depth case study of Beijing are used. The latter employs a proxy for strictness and coverage as an explanatory variable in first-differenced OLS regressions. The results confirm the effectiveness of the policy in reducing energy consumption and CO_2 emissions, and both analyses show a significant positive effect on economic outcomes and innovation. The analyses additionally indicate different effects on low- and high-tech sectors in Beijing. High-tech sectors reduce their energy consumption, increase investment in research and improve both profits and output. In contrast, low-tech sectors neither reduce energy consumption nor increase research investment and, likely as a consequence of compliance costs, their output and profits suffer from the policy. In conclusion, the paper confirms the Porter hypothesis and a positive economic impact of the policy, most likely due to increased innovation and structural change.

EKHS42
Master's Thesis (15 credits ECTS)
June 2020
Supervisor: Prof. Astrid Kander
Examiner: Prof. Kristin Ranestad
Word Count: 16 251

Acknowledgements

This thesis is in many ways much more than a scientific work. It represents, for now, the end of an exciting, enriching and enlightening academic journey. A journey that allowed me to meet and learn from many inspiring people across four countries and that has helped me to become the person I am today. I will be forever thankful for this time and therefore want to take a moment to acknowledge the people that supported me on this journey.

First of all, I am thankful for the valuable support and guidance my supervisor Professor Astrid Kander provided. I also want to thank the lecturers who made the last two years so enlightening, in particular our program directors Professor Andrés Palacio in Lund and Professor Jordi Domènech in Madrid, as well as Professor Dácil Tania Juif, who supervised my first thesis.

I also want to acknowledge the team of the project “Capacity Building for the Establishment for Emission Trading Schemes in China” of the German Society for International Cooperation that I worked with during my time in Beijing, which gave me the inspiration for this thesis.

I additionally want to extend my thanks to the people who, despite having more than enough to do themselves, took the time to provide feedback, namely my good friends Victoria Becker and Anastasia Quindt, my brother Julian Sieler and my father Dr. Reinhard Sieler.

Furthermore, I want to thank my family, in particular my parents Monika Glemser and Dr. Reinhard Sieler, my step-parents Wolf Eisenmann and Dr. Ursula Sieler, my brothers Julian, Martin and Michael as well as my grandmother Gerlinde Sieler. Your love, support and the faith you had in me despite all my bold decisions and extensive travels, sometimes taking me too far away from you, are the greatest gift I will ever receive. Without you, none of this would have been possible.

Finally, I want to thank my grandmother Gerda Glemser, who sadly cannot celebrate this moment with us but who’s last “safe travels” wishes will accompany me and keep me safe. Forever.

Table of contents

List of tables

List of figures

1 Introduction

"We can avoid climate and ecological catastrophe. Humans are very adaptable: We can still fix this. But the opportunity to do so will not last for long. We must start today. We have no more excuses."

- Greta Thunberg, April 2019 (Thunberg, 2019, p. 67)

Well before Greta Thunberg and millions of young people alongside her raised international attention on the issue, we knew about the dangers of the climate crisis. A crisis that threatens not only biodiversity and ecosystems, but also food security, safety and by extension human well-being around the world (IPCC, 2014). Developed Western nations, in particular the United States and the European Union, have historically been the largest emitters of CO_2. However, China's fast economic development, especially after the country acceded to the WTO in 2001, has allowed the country to become the world's largest CO_2 emitter around 2004 (Olivier et al. 2015). As of 2015, it is responsible for around 30% of the world's overall CO_2 emissions (Olivier et al. 2015). Over recent years, however, the awareness of environmental problems and climate change within China has risen (Kuhn & Zhang, 2014). Public concerns about pollution and a changing economic structure therefore led to a change in government priorities (Averchenkova et al. 2016), motivating President Xi to announce that China would take "a driving seat in international cooperation to respond to climate change" and become a "torchbearer in the global endeavour for ecological civilization" (Xi, 2017, p. 4). Policy efforts towards a reduction of carbon emissions were increased, following the goal to peak around 2030 or before (PRC, 2015).

An essential step on this path was the establishment of several pilot emission trading schemes (ETS) on city and provincial level starting in 2013. The fact that these systems were introduced in a range of first-tier[1] cities across China and had a high coverage (Liu et al. 2015) shows that they were not just experiments but rather actual instruments of climate policy. Following the saying "crossing the river by feeling the stones" by Deng Xiaoping, which has since become a Chinese government principle, they were furthermore meant to prepare the establishment of a national emission trading scheme (Goulder et al. 2017) to be implemented by the end of 2017. Although the implementation of this national ETS was delayed due to concerns about transparency and political pressure, the Chinese government expects a breakthrough by the end of 2020 (Reuters, 2020). Therefore, the emission trading pilots are currently in the spotlight and understanding their effects on environment and economy has become crucially important with respect to the question whether China can "still fix this" (Thunberg, 2019, p. 67), especially since relaxed environmental policies are seen as contributing to China's comparative advantage (Song & Wang, 2018).

[1] The Chinese tier system is a hierarchy of cities. Even though not official, it is often referred to in the media and well known within China. Cities of the first tier are the most important economic and cultural centers of China.

Climate policy is also gaining traction in other emerging economies, exemplified by the recent introduction of carbon taxes in Argentina and South Africa as well as the launch of Mexico's national pilot ETS in January of 2020 (ICAP, 2020). Just as in the case of China, economic development is, due to remaining poverty, still high on the agendas of many emerging economies, which makes understanding economic consequences crucial for the question whether or to engage in climate policy. Since the Chinese ETS pilots were the first implementation of carbon pricing in an emerging country apart from the relatively unsuccessful implementation of emission trading in Kazakhstan around the same time (ICAP, 2020), understanding the effects of the policy in China therefore offers insights beyond China itself. It additionally represents an ideal case in order to analyse the effects of climate policy instruments in a broader emerging economy context.

Econometric research on economic impacts of the Chinese emission trading pilots is however relatively scarce and existing studies are mostly ex-ante simulations (Shi et al. 2018), in large part due to a delay in data availability. Therefore, first extended econometric analyses have only been published very recently in spring of 2020, namely papers by Zhang Haijun et al. (2020) and Zhang Wei et al. (2020). These studies, however, contradict each other, which points to the need for additional research. Furthermore, research into the so-called Porter hypothesis (Porter & van der Linde, 1995), which suggests potentially positive impacts of environmental policy, is very limited (Shi et al. 2018), in particular concerning first-mover advantages and competitiveness (Dechezleprêtre & Sato 2017). While traditional views emphasize that environmental policy is harmful for economic development (see e.g. McGuire 1982), this hypothesis offers a way to reconcile the two by proposing that such policy could, similar to Mr. Hyde in Stevenson's novel, have a hidden positive side with regards to growth. It suggests that environmental policy, in this case climate policy, could foster growth by forcing economically beneficial innovation and effectiveness-enhancing upgrading of facilities, which are otherwise not implemented due to market failures or imperfect decision-making (Porter & van der Linde, 1995). This positive effect seems plausible in the case of China, given the country's record with inefficient and outdated production facilities (Zhu et al. 2017). The hypothesis also suggests that such policy, if aligned with global trends, could push innovation in the right direction and strengthen new sectors, allowing them to gain an early-mover advantage. Such an effect is likely given the dynamic nature of the Chinese economy and fits the Chinese goal to transform its economy and strengthen high-tech sectors as fist mentioned in the 12th five-year plan (National People's Congress of China, 2011).

1.1 Research problem

Therefore, the Porter hypothesis raises the question whether the pilot ETS in China had a positive impact on the economy, in particular on overall economic outcomes, innovation, and structural change. Answering that could also allow to understand whether climate policy, in particular carbon pricing, could be a way for an emerging economy close to the technology frontier to boost innovation as well as structural change and therefore a remedy against the middle-income-trap. This question, however, cannot stand alone, as other aspects of the policy have also to be considered, in particular, whether the policy reduced carbon emissions.

Given the limited and contradictory literature on the economic effects of emission trading in China, in particular with respect to the Porter hypothesis, this paper contributes to the literature by providing an in-depth and holistic analysis of the economic effects of the policy. Following the Porter hypothesis, which suggests an impact on overall outcomes, innovation and structural change, it therefore analyses three different research hypotheses:

H1: *The introduction of emission trading in China had a positive impact on overall economic outcomes in the pilot provinces.*

H2: *Emission trading positively impacted research and innovation in the pilot provinces.*

H3: *High-tech sectors benefitted more from the introduction of the policy, which thereby contributed to structural change towards these sectors.*

The fact that eight different pilot ETS were implemented, namely in Beijing, Tianjin, Shanghai, Shenzhen, Chongqing, Hubei, Guangdong and Fujian, representing 18% of the population and 30% of the GDP of China (Liu et al. 2015), supports this analysis since it effectively creates a natural experiment as the provinces were at least in theory selected to represent "regional diversity in terms of development" (Han et al. 2012, p. 22). Furthermore, the policy was adopted and slightly changed over time due to its pilot nature, which adds variation that will additionally be used to understand the underlying effects better.

1.2 Aim and scope

This work aims to contribute to the literature on economic effects of climate policy in emerging economies, specifically concerning emission trading and the Porter hypothesis. It does so by adding to the so far relatively limited literature on the Chinese emission trading pilots. It firstly employs a holistic econometric approach, as it focuses not only on overall economic effects but also on effects on innovation and structural change in order to obtain a broad picture of the channels suggested by the Porter hypothesis. Secondly, it analyses effects on provincial level as well as sectoral effects for the case of Beijing, which adds to the holistic perspective. It differs in this respect from the literature as the effects on different sectors and on structural change were so far rarely covered. Thirdly, this work extends existing studies by two years until 2018, since the most extensive analysis of the pilots so far, which to the best of the author's knowledge is the one by Zhang Haijun et al. (2020), ends in 2016.

The study is thereby highly relevant, both in light of the ongoing climate crisis and because an increasing number of emerging economies seek to implement climate policy. In particular since the Chinese government hopes to roll-out the national ETS this year (2020), there is additionally a high level of attention being paid to evaluations of the pilot ETS, which adds to the overall relevance.

1.3 Thesis outline

The thesis is structured as follows. Firstly, a background, both on emission trading, its implementation in China as well as on the Porter hypothesis is provided. Then, data used for the analysis is presented and discussed before the employed methodology is explained in section four. Following the methodology, the results of the analysis are presented in two different parts. The first part contains an analysis on the provincial level, using an extended difference-in-difference approach. Due to certain data limitations, this analysis is then extended by a case study of the Beijing pilot ETS, using characteristics of the Beijing ETS as explanatory variables for economic outcomes. The thesis is finalized with a discussion of the obtained results, including their limitations, and a conclusion. This is followed by a bibliography and the appendix, providing additional regression results and model specification test results.

2 Theory

2.1 Emission trading and its application in China

When it comes to climate policy, different options have been brought forward over the years. While command-and-control approaches were traditionally the norm for environmental policy, market-based solutions have gradually gained momentum. The underlying reasons for this development are mostly attributed to the fact that these approaches are seen as more flexible and cost-effective ways to achieve the targeted emission reductions (Aldy & Stavins, 2012). Within the family of market- and price-based approaches, there is, however, some controversy about whether a carbon tax or more quantity-focused approaches, e.g. an emission trading system, are preferable (Goulder & Schein, 2013). The general difference between the two is that a carbon tax allows the regulator to directly set the price of emitted CO_2, while the latter allows to set an overall emissions cap for covered sectors and implements trading for emission rights. While a carbon tax is both easier to implement and more predictable for covered firms, it can be argued that emission trading is more predictable for the regulator since an explicit overall level of emissions is targeted. Especially since many countries submit concrete CO_2 reduction targets as their Nationally Determined Contributions within context of the Paris agreement, this might therefore be beneficial as it facilitates linking Paris agreement pledges with national policy. It has also been argued that the innovation effect of emission trading might be stronger (Chen et al. 2020). This could be due to companies having to engage in trading, making them more aware of the policy. In contrast, emission trading, however, suffers from potential price volatility (Goulder & Schein, 2013) as it is more vulnerable to external shocks. Due to their respective advantages, both systems are currently in use around the world. The fact that the EU and China, which are among the three largest economies in the world by GDP (World Bank, 2020), have chosen to establish an ETS has, however, given this approach particular relevance for academic studies.

The mechanism of an ETS is relatively simple: The policymaker sets a cap on the total amount of emissions for selected sectors and a specified timeframe. Then, a number of tradable emissions allowances equal to the cap are either distributed for free, sold or auctioned to covered firms. At the end of the compliance period, firms have to surrender a number of allowances equal to their emissions during the period. If a company has too few allowances, it needs to purchase more from the market, while it can sell allowances if it has a surplus. This trading aspect of the approach does not only create a price for emissions allowing firms to either innovate and reduce emissions or to buy allowances, it also forces firms to engage and, therefore, fosters awareness of this price. ETS are, however, highly complex to implement, and a policymaker has to fine-tune several design elements, including cap setting, sectoral coverage, firm-size thresholds, and the form of allocating allowances, including sector-specific allocation quotas (German Federal Ministry for the Environment, 2016).

The US possessed historic dominance in terms of establishing emission trading schemes due to its early implementation of such schemes for SO_2 and NOx (Burtraw & Szambelan, 2010). However, the EU challenged this dominant stance by introducing the EU ETS in 2005, which became the most extensive emission trading system in the world (Borghesi et al. 2016) and will probably hold that title until the Chinese National ETS is fully operational. Partially based on the experiences made in the EU and on its own experience with the Clean Development Mechanism, the National Development and Reform Commission of China announced in April of 2011 that it would roll out ETS Pilots in seven selected cities and provinces (Han et al. 2012). The cities Beijing, Shanghai, Tianjin, Chongqing, Shenzhen as well as the provinces Guangdong and Hubei (see Figure 1) were selected to represent "regional diversity in terms of development" (Han et al. 2012, p. 22). Fujian joined as the 8th pilot in 2016 (ICAP, 2020). They were then tasked to set their own carbon reduction targets and to design the characteristics of their ETS, which means that considerable variation between the technical details and the level of ambition of the different pilots can be observed. These differences in ambition are crucial to acknowledge as Zhang Kangkang et al. (2019) show that the ETS did only lead to reductions in the carbon intensity of output in some provinces, most notably in Beijing and Guangdong. This is underlined by Hu et al. (2017) who analyse the Beijing pilot in comparison to the other pilots and show that capacity, as well as awareness, differ fundamentally between the systems, while they furthermore argue that the ETS of Hubei and Beijing are the ones with the highest capacity. This is interesting as the economic structures of the capital Beijing and the industrial province Hubei are fundamentally different. Hubei is more representative of China as a whole (Qi et al. 2014), while Beijing is more modern and marked by high technology sectors, which indicates that the implementation quality might not be dependent on economic development and partly exogenous to it. All pilots, however, have a generally high coverage of between 33 and 60% of national emissions (Liu et al. 2015), which shows that the effort being made is significant. This is supported by the findings of Fan and Todorova (2017), who show that emitters in different regions have factored in the carbon price in production decisions. This is confirmed by several rounds of the China Carbon Pricing Survey, in which the majority of stakeholders have consistently answered that the ETS would affect their investment decisions (Jotzo et al. 2013; De Boer et al. 2015; De Boer et al. 2017; Slater et al. 2017; Slater et al. 2019).

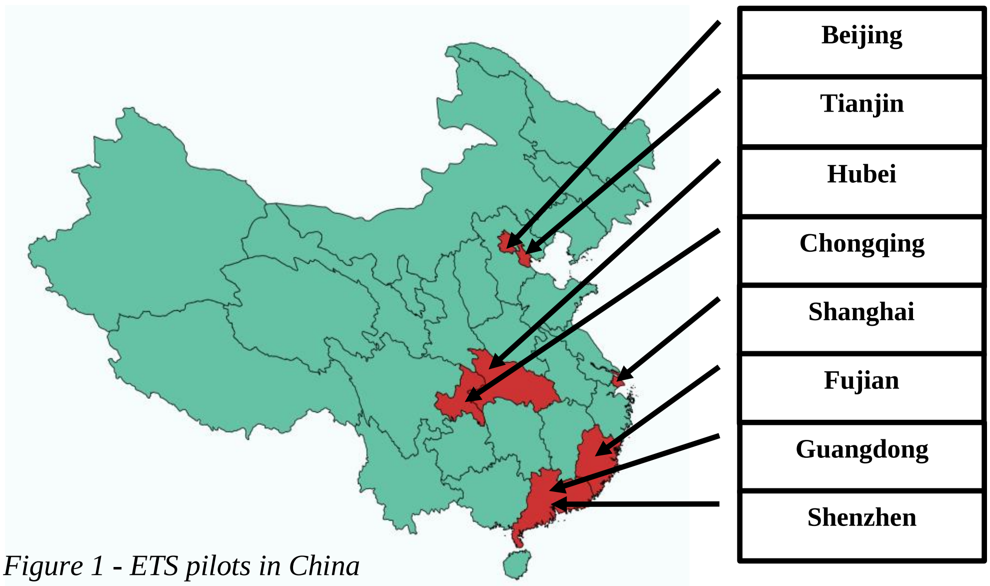

Figure 1 - ETS pilots in China
(own work based on https://mapchart.net/)

Regarding the technical details of the ETS pilots, however, considerable differences between the pilots have to be noted. Inclusion thresholds range from 5.000 tons of CO_2 in the second phase of the Beijing ETS to 20.000 tons in Tianjin and Guangdong, while the sectoral coverage also differs, with Beijing sticking out by including almost all industrial and service sectors (ICAP, 2020). Together, these differences imply that the actual percentage of covered emissions ranges from 40-60% and that both the overall cap and the number of covered companies differ widely (Xiong et al. 2015). Prices also fundamentally differ and even though trading gradually evolved, markets in many pilots, e.g. Beijing (Hu et al. 2017), are still not fully developed, with trading remaining limited in scale and peaking around commitment deadlines. Furthermore, the way of allocating allowances differs slightly. Most pilots allocate the majority of their allowances for free based either on grandparenting, meaning based on a given entity's historical emissions, like during the first two phases of the EU ETS, or industry benchmarking like during latter phases of the EU ETS (Ling et al. 2015). Some pilots also auction off a share of the allowances (Hu et al. 2017). Reporting and verification are however similar, as emission reports in all pilots have to be verified by a third party (ICAP, 2020). A detailed overview of the different pilots is provided in Table 1.

Table 1 - Overview over technical details of the ETS pilots. Source: ICAP 2020

Pilot	**Launch**	**Industry coverage**	**Inclusion threshold**	**Share of emissions covered**	**Covered entities (2018)**	**Allowance allocation**
Beijing	Nov 2013	All industrial and non-industrial companies and entities, the service sector, and public transport.	5,000 t CO_2/year	40%	903	Free allocation using grandparenting for existing facilities and benchmarking for new entrants, expanded capacity and power. Auctions are possible.
Chongqing	June 2014	Power, electrolytic aluminium, iron, ferroalloys, calcium carbide, cement, caustic soda, steel.	20,000 t CO_2/year or 10,000 tce/year	50%	195	Free allocation using grandparenting.
Fujian	Sept 2016	Electricity, petrochemical, chemical, building materials, iron and steel, nonferrous metals, paper, aviation, and ceramics.	10,000 tce/year	60%	255	Free allocation using benchmarking or grandparenting based on historical emission intensity, depending on the sector. Auctions are possible.
Guangdong	Dec 2013	Power, iron and steel, cement, papermaking, aviation, and petrochemicals	20,000 t CO_2/year or 10,000 tce/year.	60%	242	Free allocation using grandparenting, historical intensity, or benchmarking, depending on the sector. A share of the allowances is auctioned.
Hubei	April 2014	Most industrial sectors, including power and heat, iron and steel, chemicals, textile, cement and automobile manufacturing.	10.000 tce/year	45%	338	Free allocation using grandparenting or benchmarks, depending on the sector. Auctions are possible.

Shanghai	Nov 2013	Most industrial sectors, including chemicals, power and heat, iron and steel and petrochemicals. Hotels and financial, aviation and shipping.	20,000t C CO_2/year or 10,000 tce/year, lower for transport, buildings, firms covered earlier.	57%	298	Free allocation using benchmarks, historical emission intensity or historical emissions, depending on the sector. Auctions are possible.
Shenzhen	June 2013	Power, water, gas, manufacturing sectors, buildings, port and subway sectors, public buses, and other non-transport sectors.	3,000 t CO_2e/year, 10.000m2 for government buildings.	40%	794 (2017)	Free allocation using benchmarking and grandparenting, depending on the sector.
Tianjin	Dec 2013	Heat and electricity production, iron and steel, petrochemicals, chemicals, and oil and gas exploration.	20,000t CO_2/year	55%	107	Free allocation using grandparenting and benchmarking for new entrants. No auctions before 2019.

The Beijing ETS deserves a closer look as this work features a case study specifically targeting the city. This is particularly important with respect to the allocation method, as the approach used in Beijing is slightly different from other provinces in the sense that although Beijing uses grandparenting and benchmarking like the others, an emission reduction factor is also used (Ling et al. 2015). Besides Beijing, only Guangdong uses this approach. This factor, differing between sectors, is multiplied with historical emissions in order to create a decline in the number of allowances, which makes the ETS stricter over time. The factor will therefore be used for the analysis of the Beijing case study as this adds variation, especially since the factor does not develop linearly. Apart from this particularity, Beijing also sticks out due to comparatively higher fines for non-compliance as companies can be fined op to 50.000 CNY for failing to submit reports and up to five times the average market price for not surrendering a sufficient amount of allowances (ICAP, 2020).

In a nutshell, the pilots implemented in China starting in 2013 share a relatively high coverage and a comparatively high level of ambition, while there are also significant regional differences. Trading and market activity are however limited, which is most likely attributable to the minimal experience companies have with the instrument, as the fact that trading tends to rise sharply in close proximity to deadlines points towards the necessity of trading for compliance. Even though prices are relatively low and the markets not yet fully mature, this could strengthen the policy effect as it can be assumed that companies inexperienced with trading will reduce their emissions instead, even if it most likely lowers the economic efficiency of the instrument. And even though the prices are lower than in other markets (ICAP, 2020), many companies in China still benefit from very low production costs and weak environmental policy (Song & Wang, 2018). Therefore, even a lower carbon price might already impact them. This is also confirmed by the high share of companies reporting that the ETS would impact their investment decisions, as explained above.

2.2 The Porter hypothesis: What is the economic impact of environmental regulation?

In the mind of the public and political decision-makers, the question of the economic impact of environmental regulation seemed to have a clear answer in the 1990s (Meyer, 1995). Although it has long been acknowledged in economics that such regulation can solve social inefficiencies and contribute to overall welfare, conventional wisdom held that environmental regulation would increase costs for firms and thereby hurt industrial output alongside with competitiveness (McGuire, 1982; Kalt, 1985; Jorgenson & Wilcoxen, 1990). However, this traditional notion was fundamentally questioned when Porter and van der Linde suggested in 1995 that there could be cases in which "properly designed environmental standards can trigger innovation that may partially or more than fully offset the costs of complying with them" (Porter & van der Linde, 1995, p. 98). This argument has since led to a considerable stream of literature about the growth effects of environmental regulation, discussing and further developing what has come to be known as the Porter hypothesis. In the traditional sense, it follows from the assumption of an imperfect decision-making process within companies due to either market failures, such as market power, asymmetric information or spill over of research and development (R&D), or of organizational failures (Ambec et al. 2013). Such imperfect decision-making could lead to underinvestment in environmentally friendly technology, even if such investments would benefit firms, for example by increasing the efficiency of resource use (Porter & van der Linde, 1995). Some other channels were additionally suggested, including better market access and the possibility to sell pollution-control technology (Mohr & Saha, 2008).

Especially since the original paper of Porter and van der Linde (1995) is not empirical and instead based on anecdotal evidence, it has been a matter of extended scientific debate ever since (Ambec et al. 2013). In the course of this debate, a system of different versions of the Porter hypothesis has emerged, in particular differentiating between a weak, strong and narrow form. Following Ambec et al. (2013), the weak form refers to environmental regulation having a positive effect on innovation, which is essentially the main channel for a potential overall effect suggested by the original paper. The narrow form, sometimes referred to as the narrow weak form, focuses that even more by referring to a positive effect on environmentally friendly or green innovation. Closest to the original argument is, however, the strong form, which relates to overall economic outcomes, stating that the positive Porter effect on growth, productivity or profits stemming from increased innovation and efficiency outweighs the negative impact of additional costs, leading to a net gain from the regulation. Most of the existing literature follows this system with the majority of studies focusing on the weak or the narrow weak form, namely on the innovation effects of environmental regulation. Even though there remains some controversy, this part of the Porter hypothesis is generally supported by literature (Ambec et al. 2013). Lanoie et al. (2011) for example confirm the weak Porter hypothesis analysing survey data from OECD countries, while Kneller and Manderson (2012) confirm the narrow form, finding evidence for an effect on environmental R&D, but not on its overall level.

Of particular importance in this context are however findings related to emission trading systems, where the EU ETS has been the focus of researchers for years, even if econometric evidence is still scarce (Teixidó et al. 2019). While an early study by Löfgren et al. (2014) for the case of Sweden did not find an effect of the EU ETS, Borghesi et al. (2015), Rogge (2016) as well as Calel and Dechezlepretre (2016) confirm a positive innovation effect. So do the majority of non-econometric studies analysed by Teixidó et al. (2019). A paper by Bel and Joseph (2018) could solve the controversy between Löfgren et al. (2014) and the other studies in this regard, as it shows that stringency of the regulation is what crucially drives innovation in the case of the EU ETS. Keeping the systematic problems of the EU ETS in the beginning in mind, in particular the low allowance prices, therefore serves as a good explanation for why Löfgren et al. did not find innovation effects. This is further underlined by generally high production costs and a history of strong environmental regulation in Sweden, which makes it even less likely that a low allowance price leads to innovation. Teixidó et al. (2019) support this line of reasoning as they argue that a more robust setup of the EU ETS can be expected to lead to a stronger innovation effect in following years, even though econometric evidence in that regard was not yet available at the time of their study.

Since the pilot ETS in China only commenced in 2013, econometric evidence on their innovation effects is still limited. The few very recent studies that exist paint a mixed picture concerning the narrow Porter hypothesis. Zhang Lu et al. (2019), for example, find an effect on green innovation, while Lyu et al. (2020) find no significant effect, however using only a simple difference-in-difference (DiD) approach. An earlier simulation by Lin et al. (2018) also predicts a positive effect on green innovation, however alongside with crowding-out, causing an overall negative effect.

Generally speaking, it is, therefore, safe to say that both the weak and the narrow forms of the Porter hypothesis experience academic support amidst an ongoing debate. What matters for the larger question of economic impacts of environmental regulation is, however, the strong version of the Porter hypothesis, i.e. whether the overall effect is positive. In this respect, the results in the literature are substantially more controversial, even though the strongly adverse effects suggested by traditional literature, in particular that regulation drives industries out of the market as suggested by McGuire (1982), are rarely found. However, many studies concerned with the Porter hypothesis also find no clear support of a positive effect, meaning the strong form of the hypothesis, for instance Rubashkina et al. (2015), Anger and Oberndorfer (2008) as well as Rexhäuser and Rammer (2014), which find neither a positive nor a negative effect of environmental regulation on firm performance and profitability, respectively. It can, however, be argued that the absence of strong adverse effects by itself already indicates that there is some Porter mechanism at play since it can reasonably be assumed that environmental regulation leads at least to some costs for covered firms. Furthermore, despite empirical results being generally mixed, more recent studies tend to support the strong Porter hypothesis (Lanoie et al. 2008), while results seem to depend on the research design and the context of analysis. Regarding the former, Lankoski (2010) for example argues that a central issue of the literature might be that the time dimension is not taken into account since Lanoie et al. (2008) find a positive effect both on innovation and productivity using lags in their analysis. The latter, namely the importance of context, is shown by Albrizio et al. (2017) who find that environmental regulation leads to an increase in productivity growth only in the most advanced economies close to the technology frontier.

Despite these mixed results obtained for developed economies, it can be argued that the Porter mechanism might be stronger in emerging economies, in particular in China, which is, at least in some regions, relatively close to the technology frontier (Wang et al. 2013) and therefore, following the reasoning of Albrizio et al. (2017), comparatively well positioned. The main reason behind this potentially stronger effect is the challenge of the middle-income trap. Even though China has been able to modernize its economy relatively well so far, at least in some regions, it is still at risk to fall into this trap (Glawe & Wagner, 2016), meaning that failing to consistently climb the technological ladder could lead to a situation of decreasing economic growth in the medium run. In order to overcome that, sustained industrial upgrading, diversification, innovation and the development of a comparative advantage in new products are essential (Lin & Treichel, 2012; Kharas & Kohli, 2011; Engel & Taglioni, 2017).

The Chinese government is aware of this challenge and therefore seeks to fundamentally transform its economy, as first mentioned in the 12th five-year plan (Xielin Liu, 2017). Crucial aspects of this transformation are innovation and the development of "new strategic industries … while continuing to strengthen and enlarge high-tech industries" (National People's Congress of China, 2011, p. 9). While the former relates directly to the original notion behind the Porter hypothesis, the latter opens up another way in which environmental regulation could increase economic growth and provide a remedy against the middle-income trap. Regulation could in particular benefit high-tech sectors by "aligning profits with low-emission investment and innovation [and by] channel[ing] private capital flows" (Partnership for Market Readiness, 2016, p. 2) towards these sectors. It could thereby work similar to a modern version of the industrial policy applied in several East Asian economies in the past. This is supported by a main argument presented in the original Porter and van der Linde (1995) paper, which has however only received limited attention in the literature. In addition to the argument about effectiveness-enhancing innovation, the authors talk about a potential positive economic effect through a competitive first-mover advantage in new sectors. They argue that if "national environmental standards anticipate and are consistent with international trends in environmental protection" (Porter & van der Linde, 1995, p. 105), environmental regulation could pressure domestic companies to innovate in the right direction and thereby give them a competitive advantage. Even in the 90s, they furthermore argued for the importance of this channel since "world demand is moving rapidly in the direction of valuing low-pollution and energy-efficient products" (Porter & van der Linde, 1995, p. 104), something that has arguably accelerated over recent years as the world is at least somewhat collectively moving towards a low-carbon future. This channel could even be more important than innovation alone, since, as Porter and van der Linde themselves admit, there is a form of pollution-reducing innovation that produces no positive economic side effects. The argument is also in line with traditional views on first-mover advantage, as Lieberman and Montgomery (1988) for instance argue that even though there are broad first-mover advantages, some disadvantages can stop firms from innovating, for instance free-rider problems, uncertainty and incumbent inertia. Environmental regulation could therefore push firms to innovate, allowing them to reap first-mover advantages and thereby strengthen new and innovative sectors, supporting structural change towards these sectors. However, according to Dechezleprêtre and Sato (2017), there is no empirical study that has analysed this as of now, although Costantini et al. (2012) find a general positive effect of environmental regulation on green sectors and exports for the EU.

Albeit, some limited research on first-mover advantage due to environmental regulation in combination with subsidies has been conducted in a particular context, namely regarding innovation in the solar industry in Europe and its interaction with the emergence of the same industry in China. Groba (2014) confirms the strong Porter hypothesis for this case and shows that European policy supported strong European export performance. He furthermore shows that the earlier the policy was introduced, the stronger its effect was, which is in line with the first-mover advantage argument. The most prominent case in this respect is Germany, which became the world's second-largest exporter of solar energy technology components around 2003. However, China became the world's largest exporter soon afterwards, which is why critics claimed that the German technological leadership was quickly lost and the policy merely supported the solar industry in China. Quitzow (2015), however, shows this to be an overly simplified argument as German companies were facing constraints to scale-up production and therefore entered in cooperation with Chinese firms to do so. Chinese entrepreneurs, in turn, were more dynamic and could quickly scale-up production. They also increased their research, especially when the Chinese government started to push for an extension of domestic solar capacity (Quitzow, 2015). Groba and Cao (2015) expand on this by showing that China gained a comparative advantage relatively fast and that provincial R&D spending in China had a positive impact on exports of solar components. This very particular case of the solar industry therefore offers three main insights. Firstly, it provides support for the theory on first-mover advantage and a strengthening of high-tech sectors for the case of environmental regulation in Europe. Secondly, it shows that Chinese companies were quick to exploit new developments and enter new sectors, implying a high potential for fast adaptation to environmental regulations within China. And thirdly, it shows that domestic R&D spending as well as an increasing market for green technology within China, which are also both expected consequences of the Chinese pilot ETS, led to a strengthening of a particular high-tech sector in China.

In addition to this structural change channel, some characteristics of the Chinese economy could also lead to a faster and more committed reaction to implemented policy, causing effects to be observable despite prices and regulations that appear relatively weak from a European standpoint. These include the more dynamic nature of the Chinese economy, a history of government intervention and industrial policy as well as a high share of state-owned companies. Furthermore, environmental policy could force state-owned companies struggling with a lack of efficiency (Zhu et al. 2017) to innovate and upgrade their equipment, which could additionally imply that the traditional channel of efficiency-enhancing innovation could be more pronounced in China.

However, there is little empirical evidence on the overall economic effect of the ETS in China so far, and the existing literature is contradictory. Most papers concerned with the ETS Pilots are ex-ante simulations (Shi et al. 2018), for example, Liu et al. (2017), Zhang et al. (2016) and Dong et al. (2019), the latter of which at least includes a small empirical part using data from the first two years after implementation. These papers find either no or only very weak adverse effects on the economy, which at least gives some support to a Porter-style mechanism if it is assumed that the policy leads to some costs for the firms, which seems likely given the extent of the pilots. That the ETS in China is a highly important issue, in particular in light of the expected rollout of the national ETS, is, however, reflected in the fact that new research on the topic has been published very recently, most notably an article by

Zhang Haijun et al. (2020), which was released in the April 2020 volume of *Energy* and looks into the effects of the ETS on carbon reduction and industrial output. Although the authors only use data up to 2016, therefore taking two years less into account than this analysis, they manage to confirm not only an impact on carbon emissions, but also a positive effect on industrial output in line with the strong Porter hypothesis. In contrast to that stands another study by Zhang Wei et al. (2020) from mid-March, using a sectoral approach and finding a negative effect on output, however using only data up to 2015, virtually one year after implementation. More comprehensive empirical studies in this context exist for earlier environmental policy programs, namely the trading scheme for SO_2 (Ren et al. 2020) and the China Low Carbon Pilot (CLCP) program which started in five Chinese provinces in 2010 (Yang et al. 2019). These two studies found that the respective policy programs both encouraged innovation and industrial growth, in line with the Porter hypothesis, while Ren et al. (2020) additionally found that the observed effect was stronger with stricter enforcement. It can, therefore, be said that the literature provides some support for the strong Porter hypothesis in the context of China. This support furthermore appears to be more robust than in the case of the broader literature, which might be related to the arguments presented before.

In a nutshell, the literature generally tends to confirm both the weak and the narrow Porter hypothesis. While findings concerning the strong Porter hypothesis are generally relatively mixed, they are less so for the case of China and tend to support the strong Porter hypothesis. However, empirical research so far has been very scarce, in particular regarding emission trading and the effects on structural change towards high-tech sectors. The same applies to an emerging country context, in particular to China, where the effects might be even stronger. In light of the climate crisis, it is, however, crucial to better understand the entirety of the Porter hypothesis system in the context of emerging economies, especially China, as they are responsible for a substantial and growing share of global CO_2 emissions. Since economic development is however still imperative for these countries due to remaining poverty, understanding whether climate policy leads to economic damages or might even support their transition is therefore essential for the ongoing policy debate.

3 Data

This analysis is mostly conducted with official economic data, stemming from a range of Chinese statistical publications. The most important of these, which was used for the provincial level analysis, is the China Statistical Yearbook for the Years 2011 to 2019. Additionally, the China Statistical Yearbook on Science and Technology, as well as the China Statistics Yearbook on High Technology Industry, were also used. For the case study of Beijing, most of the data stems from the Beijing Statistical Yearbook for the Years 2015 to 2019. Generally speaking, there is some controversy about the accuracy of Chinese statistical data, even though that was mainly an issue during the first years of China's economic opening, since the statistical offices could not keep up with the rapid economic development, and less so afterwards (Holz, 2004). Some doubts however remain, and many researchers are critical of Chinese data, especially in light of the lack of independence of China's National Bureau of Statistics (NBS), but analyses have found no explicit biases or large error margins for overall GDP statistics (Holz, 2014). It has, however, to be mentioned that it is challenging to even reliably check the quality of official statistics (Plekhanov, 2017).

Provincial data has been found to be manipulated in the past, although it was mostly the NBS itself highlighting these issues (Plekhanov, 2017). Given that the detailed provincial-level data used stems from the capital Beijing itself, there is, however, less potential for data manipulation. There is furthermore no reason to believe that there might be systematic data manipulation in favour of the pilot provinces. Given that the pilots were mostly established in larger cities, it is instead reasonable to assume that the institutional capacity and the central government's control is stronger there, which could be expected to lead to lower GDP values in comparison to other provinces with more opportunities for data manipulation. This would bias the obtained results, if they are positive, downwards and therefore poses no problem to the analysis. Additionally, there is effectively no alternative to the usage of official Chinese data and this analysis would not have been possible otherwise.

Apart from the main indicators, which stem from the statistical publications described above, the dataset was complemented using a range of other sources. The estimated CO_2 emissions, for example, stem from Shan et al. (2020, 2017, 2016). They are generally robust, but earlier years suffer from scarcity and a low quality of underlying data. The earlier years are, however, not used in this paper, and the regressions using CO_2 emissions as the dependent variable are not a central part of the analysis, which makes this less of an issue. That is furthermore the case as a similar analysis is repeated for the Beijing case study, using energy consumption from official sources. Global fossil fuel prices, namely of Oil, Gas and Coal, are obtained from the economic database of the federal reserve of St. Louis and originally sourced from the International Monetary Fund (2020a, 2020b) and the US Energy Information Administration (2020). They can, therefore, be assumed to be reliable, although they are not the actual prices in China. Since they are only used to reflect broad developments in these prices, this issue is, however, not crucial for the quality of the results.

The number of covered entities, which is used in order to create the main explanatory variable in the Beijing case study is obtained from the Annual Report of Beijing Carbon Market in the years 2014-2018, which is published by the China Beijing Environment Exchange, the authorized trading platform for carbon trading in Beijing. It is, therefore, most likely reliable. The emission reduction factor, the second variable used to create the main explanatory variable for the Beijing case study, is directly taken from the official 2018 Beijing enterprise (unit) quota verification method document (original: 北京市企业（单位）配额核定方法（2018版）, Beijing Municipal Bureau of Ecology and Environment, 2019). The latter two variables were combined in order to create a proxy for coverage and strictness of the Beijing ETS. Although this approach is not ideal, it is a sufficiently good approximation of the overall pressure put on the economy by the ETS. It is furthermore the best available measure as the trading activity in the pilots is relatively limited and prices in carbon trading furthermore closely related to overall economic developments. Therefore, the usage of prices would not be an exogenous and reliable indicator of ETS strictness, which is even more so as it has to be assumed that, given the limited trading activity in Beijing (Hu et al. 2017), some companies, in particular state-owned ones, just comply with the emission reduction factor.

4 Methods

In order to analyse the hypotheses described before, two separate sets of regressions are performed. The first of these sets focuses on provinces, comparing the provinces with ETS pilots to similar provinces that did not implement the policy, using an extended difference-in-difference approach. Since there are significant heterogeneities between the provinces concerning data availability, in particular regarding sector-level data, this first set of regressions is, however, limited to macroeconomic indicators on provincial economic activity. This only allows relatively limited insights into potential channels and drivers of the observed macroeconomic developments. It is also ignorant of the strictness of the implemented policy, which can be relevant for the effect, as demonstrated by Bel and Joseph (2018). In order to address these shortcomings, the first set of difference-in-difference regressions on provincial level is complemented by a second set of regressions focusing on Beijing in particular. According to the literature, Beijing has one of the most mature carbon markets, second only to Hubei (Hu et al. 2017) and previous studies have shown that the Beijing ETS was effective in reducing carbon intensity of output, while the one implemented in Hubei was not (Zhang Kangkang et al. 2019). In comparison to Hubei, the availability of data for Beijing is furthermore substantially better, which is why Beijing was chosen as a case study. This choice somewhat limits the conclusions that can be drawn, which will be addressed in detail in the discussion following the presentation of the main results. The Beijing case study focuses only on the period during which the ETS was in force and instead of employing a difference-in-difference approach uses weighted least squares regressions in first differences, employing measures of the strictness of the Beijing ETS as the main explanatory variable. By focusing on the sectoral level, it thereby allows a better understanding of the underlying dynamics.

Both models have in common that they use panel data with provinces in one case and sectors, following a slightly adapted Chinese Standard Industrial Classification to account for changes in the reporting over time, in the other case. This naturally raises specific issues, in particular concerns regarding unit roots and potentially spurious regressions. In order to address these issues, Harris-Tzavalis unit root tests are performed in both cases for all variables of interest. These tests show, as expected, a considerable indication of unit root presence for many of the variables. In order to remove unit roots and to avoid issues of spurious regression, both the non-matched regressions in the first as well as all regressions in the second part are performed in first differences. Unit root tests show that after this transformation, no variable used exhibits a unit root, while additional Harris-Tzavalis or Wooldridge unit root tests are employed after the regressions and sectoral cluster-robust standard errors are implemented in order to ensure that no issues remain. For the panel difference-in-difference approach with matched provinces, this is, however, not an issue as data is matched based on pre-treatment trends. Furthermore, all variables were transformed into real values before the unit-root tests. For these transformations, the GDP deflator was used for the majority of macroeconomic indicators on the provincial and sectoral level since the limited data available for the case of China did not allow a more detailed selection of deflators. For costs that are closely connected to firms' personnel expenses, however, namely management expenses and R&D expenditures, average wages were used as a deflator. Output and sales value of new products were deflated using the CPI, while exports were deflated using the average CNY USD exchange rate. Changes to the usage of deflators did however not lead to fundamentally different results. This is in particular the case as sign and significance of estimators are the main focus of this analysis because different matching strategies and the creation of a proxy for ETS implementation strictness do not allow a direct interpretation of the estimators.

After these transformations, the regression model for the first set of regressions follows the one pictured below, with changing variable choices for each regression sub-set. The variable of interest is a pilot dummy, which is one if a province *i* currently has an ETS implemented and zero otherwise. Controlling for level differences between pilot and non-pilot provinces with a time-invariant dummy is only necessary for the models that do not employ provincial fixed effects, which most models do. In cases where a random-effects estimator was used after performing a Hausman test however, such a dummy is introduced in order to show this level difference. Since time fixed effects for year *t* are also employed in every regression, it is furthermore not necessary to control for pre- and post-treatment phases.

$$Y_{i,t} = \beta_0 + \beta_1 pilot_{i,t} + \beta_2 controls_{i,t} + year_t + province_i + u_i$$

While the model shown above follows the general intuition behind difference-in-difference approaches, which are widely used in policy evaluation, the case of ETS Pilots in China is different from conventional applications. The difference thereby comes down to the fact that the pilot ETS were implemented at different points in time. This difference in implementation timing is usually solved by simply including time and unit fixed effects (Goodman-Bacon, 2018), as in the basic version of the approach employed in this analysis. This is however not without problems since knowledge regarding this approach is relatively limited, as Goodman-Bacon (2018) points out. According to Goodman-Bacon (2018), such an approach actually leads to an estimated coefficient that represents the weighted average of all difference in difference estimators comparing different timing groups. Such an estimated coefficient, however, only represents the unbiased average treatment effect if the effects are homogenous and if the treatment effect does not change over time, both of which are assumptions that are difficult to make in this case.

Apart from the issues related to differences in treatment timing, there are also two additional assumptions necessary in order to be able to interpret estimators in this case as the average treatment effect. Following Morgan and Winship (2015), these are that the hypothetical outcome under treatment is the same for both the treated and the control group and that the hypothetical outcome without treatment is also the same for both groups. And even though the Chinese government announced that the selection of the Pilot provinces was meant to represent diversity (Han et al. 2012), the fact that Beijing, Shanghai, Chongqing and Tianjin, which are the four biggest cities in China, are part of the seven pilot provinces shows that this is not the case in practice. Furthermore, Guangdong, which is home to cities number five and six, Guangzhou and Shenzhen, is also part of the pilot provinces. The only exceptions are Hubei, which resembles the average economic structure of China as a whole, and Fujian.

Therefore, it cannot reasonably be assumed that the treatment, namely the implementation of the pilot ETS, was random. In order to deal with this issue, the fixed- or random-effects regressions employ a range of controls tailored to each individual regression. As these controls account for important potential baseline differences, it can be somewhat reasonably assumed that the regressions using these controls satisfy the second assumption, namely that, conditional on the controls, the outcome without treatment would have been the same. Since Albrizio et al. (2017), however, show that the effect of environmental policy differs between countries with respect to their proximity to the technology frontier, which can be extended to Chinese provinces, it cannot be expected that the outcome in the case of treatment would be the same for both groups, implying that the first assumption does not hold. Following Morgan and Winship (2015), rejecting the first assumption while maintaining the second implies that the estimated effect can only be interpreted as the average treatment effect on the treated.

Nevertheless, even if the interpretation is limited to the average treatment effect on the treated, issues with this approach remain in addition to the problem of variation in treatment timing. Firstly, even though a test for parallel trends using the years 2010 to 2014 is performed before the regressions, doubts about the equality in pre-treatment trends remain, in particular because rejecting the null-hypothesis of differences in pre-treatment trends is not the same as proving that they are the same. Secondly, even though controlling for a range of variables should in theory allow the assumption of equal outcomes without treatment for both the treated and the untreated provinces, the inclusion of the wide range of fundamentally different provinces across China implies a larger risk of unobserved differences.

In order to overcome these issues, regressions using matched pairs of provinces are implemented in addition to the simple random- or fixed-effects difference-in-difference regressions, which are therefore mostly used as robustness tests. Matching provinces based on a range of variables has the advantage that it compares only similar provinces with each other, reducing the risk of large unobserved heterogeneities. Nevertheless, the issue of potentially different policy effects on different provinces remains, which also only allows an interpretation of the estimator as the average treatment effect on the treated.

A regression using matched data is in this case, however, challenging in two ways. On the one hand, using these models with panel data implies that matching provinces in levels is not sufficient, but that matching instead has to take both level differences and pre-treatment outcome trends into account. On the other hand, the variation in treatment timing leads to the technical difficulty that conventional approaches for matching cannot be applied. Thankfully, Dettmann et al. (2020) have developed a Stata implementation of a modified version of the Heckman (1998) matching and difference-in-difference approach to be used for panels with differences in treatment timing. This approach enables a flexible choice of the period of pre-treatment trends used for matching and allows to adjust the point in time at which the treatment effect is estimated. This comes in handy in this case as timing differences in the treatment effect realization can easily be accounted for. It furthermore solves the issue of time bias as described by Goodman-Bacon (2018), implying that changes in unobservable variables over time have implications for difference-in-difference applications with varying treatment time. Therefore, a difference-in-difference model using provinces matched by the Dettmann et al. (2020) approach is used as the main regression model in the first part of this analysis.

The approach for the second set of regressions, namely the Beijing case study, is slightly different. Instead of a difference-in-difference approach, policy variables determining the theoretical strictness of the ETS pilot are used as explanatory variables in weighted least squares regressions. This allows a better understanding of the underlying dynamics and functions as a robustness test. If the mechanism can be shown both in a comparison between provinces and by using policy developments within a given province over time, one could convincingly argue that the effect is indeed connected to the ETS. The general model follows the one shown below, namely a weighted least squares regression with analytical weights corresponding to the number of companies n in the given sector i. These weights were implemented since the regressions, in this case, are essentially regressions using group averages, which is why only weighing allows an accurate representation of the whole economy (Angrist & Pischke, 2008). Furthermore, as in the regressions before, sectoral cluster-robust standard errors are implemented in order to account for potential correlation between sectoral observations over time. This is, however, only a precaution, since residual unit root tests are also implemented after the regressions to ensure that autocorrelation is not an issue.

$$Y_{i,t} = \beta_0\sqrt{n_i} + \beta_1 EmissionTrading_{i,t}\sqrt{n_i} + \beta_2 controls_{i,t}\sqrt{n_i} + u_i\sqrt{n_i}$$

$$EmissionTrading_{i,t} = EmissionFactor_{i,t} * NumberofcoveredEntities_t$$

As shown above, the main explanatory variable in these regressions is an interaction term between the number of covered entities in year *t* and the emission reduction factor specific to sector *i* and year *t*. The difficulty is, however, that the quota for electricity is not very clear as it depends on the individual mix of coal, gas and other facilities a given electricity provider has in service. Since this furthermore changes over time, the average of all emission reduction factors was instead chosen for the electricity sector. Due to the significant coal intensity of the Chinese electricity mix and the fact that the emission reduction factor is very low for coal, this is most likely portraying the policy as too strict for the electricity sector. Since the hypothesis, however, expects a positive effect of strictness, an overestimation of strictness can only lead to an underestimation of the effect. Potential positive results would therefore still be valid. The other part of the interaction term is the number of covered entities. Due to adjustments in policy, monitoring as well as opening and closing of enterprises, this number changes from year to year and, most importantly, increases sharply in 2016 as the threshold for inclusion in the ETS was lowered. Therefore, the number of entities included in the ETS is a good proxy for its overall scope, which in connection with its strictness, meaning the emission reduction factor, measures the overall extent of the ETS reasonably well. The higher the emission trading variable therefore is, the stronger the ETS implementation is in a given year for a given sector. The actual price of allowances traded was, however, not taken into account, as the trading activity was still relatively limited. This approach could be improved by using the number of entities covered in each given sector. However, even though the government of Beijing releases a list of covered entities every year, many companies have no easily accessible information, which makes it highly challenging to assign them to a given sector and therefore exceeds the scope of this work.

Since it can reasonably be assumed that both the emission reduction factor and the coverage, which are both politically determined beforehand, are independent of economic developments in given sectors, a basic first-difference weighted least squares approach is sufficient for the analysis. In comparison to the matching strategy in the first set of regressions, there is however more room for potential issues regarding the model specification, which is why both a Link specification test and a Wooldridge unit root test are implemented after every regression. Because this analysis builds not only on one fundamental hypothesis, but on one central hypothesis and two additional ones regarding potential channels, both the provincial- and the sectoral-level regressions are implemented using a range of different variables. This also implies the usage of slightly different controls and matching strategies in each case, which is, however, clearly described in the following results.

5 Empirical analysis

5.1 Provincial-level results

As stated before, the first part of the analysis focuses on the provincial level, implementing both a panel difference-in-difference (DiD) model with matched data, which takes differences in treatment timing into account, and a simpler fixed or random-effects model. The latter is mainly implemented as a robustness check as the disregard for differences in treatment timing as well as the relatively simple test for pre-treatment trends most likely leads to biases. In order to further test the robustness of the results, the panel difference-in-difference approach is repeated using leads instead of the actual value, which should turn out to be insignificant. With respect to the analysis it has additionally to be mentioned that the Shenzhen pilot was excluded, as there is less data available for the city and it overlaps with the Guangdong pilot. Tibet was furthermore excluded due to a lack of data, but as it is only one of many non-treated provinces its exclusion should not have a significant impact on the results. It furthermore has to be noted that the matching strategy employed for the main regression did not in all cases find matching partners for all pilot provinces, which is why in some of the following regressions the outcomes of the main regression are not observed for all of them. In most cases, however, not more than one province was not matched. In combination with the fact that the observations focus more on sign and significance of estimators than on their absolute value and since the overall DiD model is additionally implemented as a robustness check, this should however not have a crucial impact on the results.

Focusing on the first research hypothesis, namely on the question whether a strong Porter hypothesis mechanism, meaning a positive effect of the implemented policy on economic outcomes, is observable, provincial per capita GDP was chosen as an outcome measure. For the main analysis, provinces were matched based on the pre-treatment trend in the outcome variable during the period between three and one years before policy implementation. This timeframe was chosen since it can be assumed that companies, knowing about the upcoming ETS implementation, changed their behaviour shortly before. Therefore, the trend could already be different during the year leading up to implementation, a problem sometimes being referred to as the Ashenfelter-dip, following Ashenfelter (1978). Additionally, overall GDP, investment level and, in order to control for the structure of the economy, secondary and tertiary value added were used for matching. Pre-regression diagnostics show no significant difference between the chosen province-pairs with regard to these measures, and in particular the difference in pre-treatment trends is minimal. The results of these variable balance tests for the entire provincial-level analysis can be found in Appendix 3. The outcome was then observed two years after implementation of the policy to account for a potential delay in the effect. For the robustness check, random effects were chosen following Hausman-test results on the 5% significance level.

The implemented explanatory variables control for drivers of economic development, economic structure, trade and, since there might be a connection between international fossil-fuel markets and the policy, the crude oil price. The results of these regressions are displayed in Table 2 below.

Table 2 - Regression results. Dependent variable: Provincial GDP per capita

	Panel DiD	**Random effects DiD**	**Fixed effects (test for trend)**	**Panel DiD lead**
Post treatment	-50.92** (0.015)			-73.78 (0.166)
Average treatment effect on the treated	58.28*** (0.002)	19.30** (0.027)		119.9 (0.113)
Treated		3.63 (0.455)		
Trend for the treated until 2014			6.73* (0.058)	
Trend until 2014			-0.85 (0.577)	
Year FE	yes	yes	no	yes
Controls	constant	constant; investment; foreign investment; secondary share; primary share; research share; imports; trade; oil price		constant
N	85	240	240	40
R^2	0.9466	0.4800	0.3188	0.8460

Cluster-robust standard errors, p-values in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

The results for the principal regression show a positive and significant average treatment effect on the treated on the 1% significance level. Even though the effect is not large, it still corresponds to approximately one quarter of the standard deviation in per capita GDP and is therefore also economically significant. The results consequently show a strong positive effect of the policy on per capita GDP, which confirms the strong version of the Porter hypothesis. The random-effects regression implemented as a robustness check supports these results and also shows a positive and significant effect on the 5% level, although there is a slight indication for a difference in pre-treatment trends. A further robustness check shows that the lead of the explanatory variable of interest does not have a significant effect, which underscores the robustness of the employed model.

The Porter hypothesis furthermore suggests an increase in research and innovation as the main channel behind such an increase in economic output. In order to study this hypothesis, the following analysis uses data from a previous study, namely Lyu et al. (2020), who created a provincial innovation index for China up until 2016. The authors of this study failed to confirm an innovation effect using this index as a dependent variable but implemented a different and simpler econometric strategy. Neither did they match similar provinces, using all other provinces as the comparison group instead, nor did they compare pre-treatment trends, only implementing two counterfactual tests shifting the main explanatory variable in time. Additionally, their study suffered from the inclusion of carbon emissions and investment in pollution control as explanatory variables, since a properly working ETS would reduce carbon emissions and increase investment in pollution control. The authors furthermore control for per capita GDP, which, as shown before, is positively influenced by the ETS.

Therefore, controlling for these three variables controls effectively for the channel through which the implementation of an ETS could have a positive impact and therefore, by construction, leads to a negatively biased estimate. It, therefore, seems reasonable to repeat the analysis presented in Lyu et al. (2020) using the general framework of this paper. As in the case of the first set of regressions, the main focus is placed on the panel difference-in-difference strategy, with provinces being matched based on three-year pre-treatment trends as well as on per capita GDP and tertiary industry value added in order to control for economic structure, in particular the importance of the service industry for a given province. Because the dataset covers a shorter period, the three years directly before the policy implementation were chosen for the pre-treatment trend, which is however not problematic as the indicator used measures the innovation outcome for which an Ashenfelter-dip is less likely. The differences between these variables are not significant, and in particular, the difference in pre-treatment trends is marginal, even being lower in the case of the treated units, which would lend additional support to the robustness of the results in case of a positive treatment effect. The outcome is observed two years after implementation, as in the first analysis. Additionally, a robustness check is implemented, in this case a fixed effects regression following Hausman-test results. The control variables used are similar to the first case, including international fossil fuel prices and industrial structure, while long-term determinants of innovation, namely the number of R&D institutions and the overall full-time R&D personnel are added. Lastly, a regression using a lead of the main explanatory variable is implemented. Results are displayed in Table 3 below.

Table 3 - Regression results. Dependent variable: Innovation index (Lyu et al. 2020)

	Panel DiD	**Fixed effects**	**Fixed effects (test for trend)**	**Panel DiD lead**
Post treatment	-0.101 (0.262)			0.082 (0.255)
Average treatment effect on the treated	0.123** (0.031)	0.0417** (0.028)		-0.0027 (0.961)
Trend for the treated until 2014			-0.0018 (0.894)	
Trend until 2014			-0.0192** (0.005)	
Year FE	yes	yes	no	yes
Controls	constant	constant; trade; tertiary value added; secondary value added; investment; foreign investment; research and development institutions; R&D personnel		constant
N	76	180	117	54
R^2	0.5325	0.1496	0.1923	0.621

Cluster-robust standard errors, p-values in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

The results for the principal panel difference-in-difference regression show a significant and positive average treatment effect on the treated on the 5% significance level. The magnitude of the effect is comparable to one standard deviation, which shows that the results are also significant in the economic sense. The fixed effects regression confirms this finding with a positive and significant result of comparable magnitude, while there is no indication for a statistically significant difference in pre-2014 trends. The panel difference-in-difference regression using leads produces non-significant results, which further supports the robustness of the findings. These results confirm the weak version of the Porter hypothesis, namely that the introduction of the ETS leads to an increase in innovation. Thereby, they stand in contrast to the results obtained by Lyu et al. (2020) mentioned earlier.

Since this contrast to the Lyu et al. (2020) results is somewhat surprising given the usage of the same index, their approach was replicated in a follow-up analysis. These replication exercises, which are reported in Appendix 1, mostly failed to reproduce any significant negative estimates, although without the inclusion of the investment in pollution control variable, which was not available. The only way in which the replication led to a negative estimator close to significance on a 10% significance level was by performing the analysis not in differences, but in levels. This is, however, highly problematic as many of the variables exhibit unit roots, which implies that the risk of a spurious regression is high. Additionally, following the reasoning regarding problems caused by the inclusion of per capita GDP described before, the analysis was repeated while excluding per capita GDP from the set of controls. This led to a positive and significant treatment effect, even in the regression in levels, suggesting that the inclusion of per capita GDP entirely drives the negative effect shown in the Lyu et al. paper. The analysis thereby supports the approach of this paper and calls the Lyu et al. (2020) results in question. It, however, also proves a connection between innovation and per capita GDP in the context of the ETS policy, which naturally raises the issue of direction of causality. Following the theoretical reasoning behind the Porter hypothesis, the implementation of the policy would cause both innovation and an efficiency-increasing reorganization of firms. It thereby already suggests both a direct and an indirect innovation-related effect on economic outcomes and thus a simultaneous impact on both. Another channel could, however, be that raising GDP increases spending on R&D and thereby innovation. Unfortunately, using lags of both variables in regressions on each other leads to insignificant results, which implies that innovation and GDP are simultaneously determined, making it impossible to determine the direction of causality in the limited scope of this work.

Before turning to the case study of Beijing, two additional questions remain to be answered, which, even though not part of the main analysis, are essential to understand the full picture of the ETS pilots. The first one is whether the ETS had any environmental effect, meaning whether the policy was effective. The second one, which matters both with regards to the first one and with regards to the economic effects, is whether inter-provincial carbon leakage, meaning the shift of carbon-heavy industry out of the ETS provinces, was an issue. In order to answer both these questions, the panel difference-in-difference approach used before is employed in a set of regressions with the estimated provincial CO_2 emissions until 2017 as the dependent variable. These CO_2 emissions are estimated values using the IPCC sectoral approach as calculated by Shan et al. (2020, 2017, 2016). As in the case of the regressions before, provinces are matched on pre-treatment trends as well as on GDP and trade.

Since in this case, however, both the Ashenfelter-dip and a short timeframe are an issue, only the years one and two before the implementation are used, which is very short to establish a trend. Since these analyses are however only an addition to the main analysis, it should not be a crucial issue, but has to be kept in mind. In order to answer the question of whether the policy affected CO_2 emissions, a pilot dummy is used to indicate treatment, just as before. The question of carbon leakage is, however, analysed by using a dummy that is one if a given province in a given year is next to an ETS pilot province. Assuming that companies that have their centre of operations in one of the major cities would not shift production far away, one should see significant positive CO_2 emission effects of the policy in surrounding provinces in the case of carbon leakage. The results are displayed in Table 4 below.

Table 4 - Regression results. Dependent variable: Estimated CO_2 emissions

	Pilot province	**Borders with pilot province**
Post treatment	19.84 (0.120)	-43.65 (0.114)
Average treatment effect on the treated	-19.55^{*} (0.079)	15.15 (0.513)
N	88	124
R^2	0.334	0.254

Cluster-robust standard errors, *p*-values in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These results confirm that the policy did have a significant negative effect on overall CO_2 emissions in the pilot provinces themselves, with the estimator being significant on the 10% level. Regarding the second question, the estimated effect of the artificial proximity dummy turns out to be non-significant on any conventional significance level. Due to concerns regarding the fact that Hubei is surrounded by many provinces, which could weaken the estimated results, the analysis was repeated without the provinces surrounding Hubei, which also did not lead to significant results. Even though this approach is rudimentary and does not prove the absence of carbon leakage, it does, however, indicate that it is not a crucial issue and therefore does most likely not drive the emission reductions observed for pilot provinces.

Generally speaking, the provincial-level analysis has confirmed both the strong and the weak version of the Porter hypothesis as the regressions have demonstrated a positive effect of the policy on both per capita GDP and innovation. Furthermore, it could be confirmed that the policy was effective in the sense that it did reduce CO_2 emissions in pilot provinces, while the absence of a significant effect for surrounding provinces suggests only limited carbon leakage. This first part, however, did not allow an analysis of the structural change effect or of channel effects more broadly, which is why the analysis was, as mentioned before, extended to include a case study of sectoral effects of the ETS policy in Beijing, the results of which are presented in the following.

5.2 The sectoral effects of the Beijing ETS - a case study

The implementation of an ETS pilot in the Chinese capital of Beijing was a very fortunate policy decision, both from an environmental and an analytical point of view. Given considerable heterogeneities concerning data availability and institutional capacity between the provinces, the implementation of the policy in the capital, where both of these are considerably better than in other provinces, allows a detailed case study in a setting where the policy was implemented comparatively well. It thereby allows an analysis on a sectoral level which also takes a broader range of variables for the potential channels into account and allows a better control of other effects. However, because of this, the effects in Beijing have to be seen as an upper boundary, and it is to assume that other pilots had weaker effects.

In order to analyse the sectoral effects in more detail, each analysis is once implemented for all sectors before the main explanatory variable is interacted with group dummies in two additional regression sets. Firstly, sectors are divided into high- and low-tech, following the definition of high-tech sectors used in the Beijing statistical yearbook, which includes pharmaceuticals, aircraft and spacecraft, electronic and communication equipment, electronic computer and office equipment, medical equipment and meters, chemical manufacturing and transport equipment. The second group division is then made between sectors with high and low energy consumption, classifying each sector within the lowest four deciles of average total energy consumption as low and the remaining ones as high. Since it is not known how many companies are above or below the inclusion threshold, this is an approximation and will be used as a robustness test, as only sectors with a high absolute energy consumption should be included in the ETS and therefore be affected by the policy.

The first analysis for the Beijing case study is both a test of the model employed and of the effectiveness of the ETS policy. In order to achieve both these aims, the regression model described above and in the methodology section is employed using total sectoral energy consumption as the dependent variable, which the ETS policy, if effective, can be expected to significantly lower. Even though CO_2 emissions would have been the better indicator, the quality of available sectoral data in this respect is not sufficient, which is why energy consumption was used instead. This makes additional sense since most of the energy in China is produced using coal, which is why energy consumption can be assumed to be closely related to overall CO_2 emissions. As described above, the regressions are weighted least squares (WLS) regressions in first differences, using an interaction of the emission reduction factor and the total number of covered entities as the main explanatory variable. The timeframe of the analysis spans from 2014, when the policy was effectively implemented after the official launch in December of 2013, to 2018. A constant as well as a range of sector-level controls is used. The latter control for broad firm-based variables, which are expected to influence the overall energy consumption through a sector's economic activity, including the number of employees, assets, liabilities and the number of enterprises in a given sector. Furthermore, finished products and inventories are included to give a clear picture of the actual production activities within the given year. The amount of governmental and foreign funds for research is additionally used as a control variable to determine the level of foreign or government involvement in a given sector.

An index for fossil energy costs is also used as a control, effectively multiplying a sector's consumption of coal and oil- as well as gas-based fossil fuels with developments in the corresponding world-market prices. This is however only an approximation as data on price developments within China could not be obtained, while in particular coal is heavily subsidized. The results of the first set of regressions are shown in Table 5 below.

Table 5 - Regression results. Dependent variable: Total energy consumption

	Weighted least squares	High- /low-tech WLS	Robustness check WLS
Emission trading	-0.0220* (0.079)		
Emission trading * high-tech		-0.0992*** (0.000)	
Emission trading * low-tech		-0.0221 (0.396)	
Emission trading * high energy consumption			-0.0235* (0.066)
Emission trading * low energy consumption			0.00931 (0.594)
Controls	constant, average number of employees, current assets, liabilities, finished products, inventories, energy costs, foreign R&D funds, governmental R&D funds, number of enterprises		
N	131	131	131
R^2	0.320	0.409	0.321
Wooldridge test	0.306	0.293	0.304
Link test 1 (p-value)	0.000	0.000	0.000
Link test 2 (p-value)	0.430	0.862	0.437

Cluster-robust standard errors, p-values in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

The results of this first set of regressions are interesting, as the negative and on a 10% level significant estimator for the principal regression confirms that the policy was effective in reducing overall energy consumption. These results also indicate that the strategy employed is the right approach as they are in line with the theory on the relatively straightforward issue of the environmental effectiveness of the policy. The implemented specification tests support these results. The link test indicates no model specification issues as, if the model is correctly specified, the first estimator is expected to be significant and the second one is expected to be insignificant, which is the case. The Wooldridge test furthermore fails reject the null-hypothesis of no unit-root presence at all conventional significance levels for all regressions. The regression focusing on high- and low-tech sectors furthermore shows that the policy had a significant and negative impact on total energy consumption of high-tech sectors on all conventional significance levels. Low-tech sectors did, however, not significantly reduce their energy consumption as a consequence of the policy. The last regression shows, as expected, no significant impact on sectors with low energy consumption and a negative and significant impact on the 10% level for high energy-consuming sectors. It therefore confirms the robustness of the results.

Generally speaking, these results confirm that the ETS worked as intended and reduced energy consumption, which is in line with the results on CO_2 emissions obtained in the provincial level analysis before, additionally confirming the employed model as well as the robustness of the results. The regressions for high- and low-tech sectors however offer further insights. The negative effect for high-tech sectors shows that these sectors were, in fact, able to reduce their energy consumption relatively fast, reacting to policy changes within the same year. The results for low-tech sectors, on the other hand, show that they did not significantly reduce their energy consumption. This suggests a considerable difference in their respective reaction to the policy. Although this theory cannot be confirmed from this first regression alone, these results could imply that low-tech industries did not have the opportunity for improvement, either due to technical or economic reasons, and therefore opted to pay a fine or buy allowances instead. High-tech sectors, on the other hand, managed to reduce their emissions, potentially through innovation. Concerning the Porter hypothesis, this could indicate that the latter might be able to reap potential Porter-style benefits, while the former might not. In order to thoroughly discuss this theory, however, further analyses are necessary.

After confirming the effectiveness of the policy in reducing energy consumption, the overall economic effects are the next crucial issue to analyse. The sectoral level approach and better data availability allow in this case a more detailed analysis, looking at both enterprise profits and sectoral output. This division is in so far interesting as the policy might have different effects on both indicators. On the one hand, it seems plausible that the policy leads to short-term costs due to increased investment in technical upgrading as well as in research and development, which would most likely affect profits negatively, even though energy savings and other improvements might also already pay off in the short run. Total output, on the other hand, is likely to be less impacted by these short-term investments while being directly affected by technological improvements and the upgrading of facilities, which is why in this case a positive effect could be expected within a shorter timeframe, at least if the Porter hypothesis and the predicted effectiveness increases would be confirmed.

Starting with the effect on profits, the same model is implemented as in the first set of regressions. There are however some slight changes to the control variables. Fixed assets are added, since it can be assumed that the amount of fixed assets, and by extension of production facilities that might need upgrading, could be related to emission trading and its effects on profits. Furthermore, the analysis for low- and high-tech sectors is extended to include a regression using the first lag of the main explanatory variable instead to allow a better understanding of the effects over time. The results are displayed in Table 6 below.

Table 6 - Regression results. Dependent variable: Total profits

	Weighted least squares	**High- / low-tech WLS**	**Lagged high- / low-tech WLS**	**Robustness check WLS**
Emission trading	15.36** (0.029)			
Emission trading * high-tech		12.18* (0.055)	18.93* (0.059)	
Emission trading * low-tech		-17.15** (0.034)	-15.14 (0.104)	
Emission trading * high energy consumption				16.11** (0.028)
Emission trading * low energy consumption				-8.595 (0.113)
Controls	constant, average number of employees, current assets, fixed assets, liabilities, finished products, inventories, energy costs, foreign R&D funds, governmental R&D funds, number of enterprises, exports			
N	131	131	130	131
R^2	0.752	0.754	0.745	0.754
Wooldridge test	0.902	0.830		0.903
Link test 1 (p-value)	0.000	0.000		0.000
Link test 2 (p-value)	0.192	0.184		0.210

Cluster-robust standard errors, *p*-values in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

The estimator for the emission trading variable in the principal regression turns out to be positive and significant on the 5% level, while link and Wooldridge tests show no indication for model misspecification or autocorrelation. Given the theoretical deliberations above, this is all the more interesting, as it was to expect that profits would suffer most from the necessary expenses for innovation and upgrading in the short run. These results, therefore, clearly confirm the strong Porter hypothesis for the case of the Beijing ETS as the implementation of the policy raised profits even in the short run. As before, the following regressions for high- and low-tech sectors mirror these results but allow fascinating additional insights. The estimator for high-tech sectors is significant and positive on the 10% level, indicating a positive effect on profits for these sectors. The regression using the first lag shows furthermore that this estimated effect stays significant and even grows over time, which indicates both additional costs in the short run, as suggested before, and a consistent positive effect on profits. The picture for the low-tech sectors is however very different, as the estimator, in this case, is significant and negative on the 5% level. The fact that the first lag is insignificant furthermore adds to the theory that the policy leads to short term impacts for these sectors, which would be consistent with increased costs for either the purchase of allowances or due to fines. The last regression again confirms a significant effect on sectors with high energy consumption and no significant effect on sectors with low energy consumption, underlining the robustness of the results.

Following the observations made before, these regressions confirm not only the strong Porter hypothesis, but also shed additional light on the different reactions of high- and low-tech sectors. High-tech sectors, which, as shown before, reacted to the policy by reducing energy consumption, potentially reap the benefits of their investment and experience a positive Porter effect on their profits that even grows stronger over time. Low-tech sectors, on the other hand, failed not only to reduce energy consumption, but also suffer from a negative impact on their profits, likely as a consequence of increasing compliance costs.

However, one could argue that these effects could be due to an overallocation of allowances to particular sectors, mirroring the experiences with the EU ETS generating large windfall profits for some sectors in its early phase (Branger & Quirion, 2015). In this case, such an effect seems, however, less likely for two reasons. Firstly, the high-tech sectors which profited from the policy did also significantly reduce their energy usage, which would not have been the case if there was a significant oversupply of certificates. If the following analyses should also confirm a positive effect on innovation for high-tech sectors, this point would be even stronger. Secondly, the very erratic market behaviour of the Beijing carbon market, the limited trading as well as the relatively low prices (Hu et al. 2017) make a significant positive impact on profits through this channel highly unlikely.

Almost more interesting than the effect on profits is, however, the effect on value added. While increasing costs do affect profits in the short run, it is to expect that efficiency increases as a reaction to the policy would have a more direct impact on sectoral value added. In order to look into this, a similar strategy is employed with sectoral value added as the dependent variable. In these regressions, less control variables are employed as it is to assume that output is less sensitive to developments in assets and liabilities of a sector. Sales income from new products is, however, added as a control in order to limit the analysis to efficiency increases in the existing production. The results are displayed in Table 7 below.

Table 7 - Regression results. Dependent variable: Total value added

	Weighted least squares	**High- / low-tech WLS**	**Lagged high- / low-tech WLS**	**Robustness check WLS**
Emission trading	23.64** (0.012)			
Emission trading * high-tech		17.06 (0.222)	46.72* (0.067)	
Emission trading * low-tech		-26.70*** (0.001)	-1.670 (0.967)	
Emission trading * high energy consumption				25.87*** (0.008)
Emission trading * low energy consumption				-0.154 (0.988)
Controls	constant, output of new products, average number of employees, energy costs, foreign R&D funds, governmental R&D funds, number of enterprises			
N	131	131	130	131
R^2	0.781	0.781	0.808	0.782
Wooldridge test	0.145	0.137		0.143
Link test 1 (p-value)	0.000	0.000		0.000
Link test 2 (p-value)	0.123	0.122		0.119

Cluster-robust standard errors, p-values in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

With regards to sectoral value added, the estimator of interest in the main regression is positive and significant on a 5% significance level, mirroring the results obtained before. The Wooldridge and link tests give furthermore no reason for concern. The fact that the regressions found overall positive effects for both profits and output furthermore shows the robustness of the approach and underlines that the implementation of the policy led to efficiency increases, confirming the strong Porter hypothesis. The regressions for high- and low-tech sectors are also similar to the results obtained before, although they show that the output effect for high-tech sectors is only significant after the first period.

This is puzzling since the effect on profits was directly observable for these sectors, but is likely related to the inclusion of the output value of new products as a control. New products might have raised the profits in a shorter amount of time while output increases in the existing production took longer to materialize. This observation, alongside with the fact that the effect raises and gains significance over time, however, supports the theoretical argument that companies innovate as a reaction to the policy and therefore increase the efficiency of production over time. The estimator for low-tech companies, however, is negative and only the main estimator is significant on the 1% level, while the lag is not. This also supports the argument made before. Following the theory that low-tech companies did not innovate, but instead paid a fine or bought allowances, it would be reasonable to assume that their value added also suffered mostly in the short run because these costs or a potential reduction of production to lower emissions would have occurred in the same year. The increase of output for high-tech sectors in combination with the decrease for low-tech ones also shows a structural change towards high-tech sectors, which confirms the structural change channel. The third set of regressions shows that the effect on low energy consuming sectors is insignificant while it is positive and significant on the 1% level for high energy consuming ones, supporting the robustness of the results.

In order to fully understand the mechanics, however, it is crucial to also look at the effect on research and development once again. Following the Porter hypothesis, firms should increase investment in R&D as a reaction to the policy, which would then explain the increases in output and profits observed before. In order to get a clear picture on the reaction of enterprises, sectoral R&D expenditures were used as the dependent variable, which offers additional insights after analysing the effects on innovation outcomes previously. The controls are similar to the regressions before, although assets and liabilities were added again. Exports were also controlled for as it is to expect that exporting firms have a different innovation dynamic. The results of the regressions are displayed in Table 8 below.

Table 8 - Regression results. Dependent variable: R&D expenditure

	Weighted least squares	**High- / low-tech WLS**	**Lagged high- / low-tech WLS**	**Robustness check WLS**
Emission trading	0.00121* (0.084)			
Emission trading * high-tech		0.00153^{**} (0.041)	0.000828 (0.336)	
Emission trading * low-tech		-0.00107 (0.187)	-0.000722 (0.420)	
Emission trading * high energy consumption				0.00128^{*} (0.076)
Emission trading * low energy consumption				0.000443 (0.699)
Controls	constant, average number of employees, assets, liabilities, foreign R&D funds, governmental R&D funds, number of enterprises, exports			
N	131	131	130	131
R^2	0.709	0.709	0.489	0.702
Wooldridge test	0.195	0.989		0.199
Link test 1 (p-value)	0.000	0.000		0.000
Link test 2 (p-value)	0.000	0.000		0.000

Cluster-robust standard errors, p-values in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

For the case of R&D expenditures, the estimator of the main explanatory variable is positive and significant on the 10% significance level. After the two previous regressions confirmed the strong Porter hypothesis, this additionally confirms the weak Porter hypothesis and shows that the main channel suggested by the Porter hypothesis, namely increased spending on R&D and innovation, is indeed a plausible explanation for the results obtained before, even though internal improvements and an upgrading of facilities are potentially additional channels. The regressions for high- and low-tech sectors continue to improve the picture of the differing dynamics for both groups. The estimator for high-tech sectors is positive and significant on the 5% level and therefore indicates that companies in these sectors increased their investment in R&D, which most likely is at least one of the causes for the previously observed results. The effect is, however, only observable for the first period, which was to expect. While it is intuitive that investment in innovation pays off over longer periods, explaining significant results for lags in previous regressions, such an effect is less likely for expenditure on innovation as it is the direct response to the policy. Thereby, the insignificance of the lag adds robustness to the results and indicates that the increase in R&D expenditure was not an effect of increasing profits or output but its cause, as it should otherwise also rise over time. The estimator for low-tech sectors is insignificant in all regressions, which supports previous findings and shows that these sectors failed to innovate as a reaction to the policy, explaining negative outcomes for profits and output. The last regression again confirms the robustness of the results as the estimator for high-energy-consuming sectors is significant, while the one for low-energy-consuming ones is not.

However, although the Wooldridge test is no reason for concern, the link test, as well as the distribution of the residuals for this set of regressions, indicate slight issues with the model specification. These could however not be fully solved by modifications of the model itself. In order to test whether the results are nevertheless valid, the presented set of regressions was repeated using the number of R&D personnel instead of R&D expenditures as the dependent variable. The specification tests do not raise similar concerns in this case, while the results, which are reported in Appendix 2, are similar to the ones presented above. The only difference is that the results are also consistent using the first lag, indicating a different dynamic of personnel development, and that the effect on low-tech sectors is negative in this case, potentially suggesting that companies in these sectors had to lay people off as a consequence of negative profit and output consequences.

To summarise, the case study of Beijing has on one hand confirmed the effectiveness of the policy as well as both the strong and the weak version of the Porter hypothesis, in line with the results obtained for the provincial level before. This confirms both the first and the second research hypothesis, namely that the policy positively affected overall economic outcomes and innovation. On the other hand, it has furthermore shown that the policy effects on energy consumption, value added, profits and R&D expenditure differ fundamentally between high- and low-tech sectors. The results thereby indicate that the Porter effect is observable for high-tech sectors, which benefit from the policy. It is however not detectable for low-tech sectors, which experience negative effects in line with traditional theory. As a consequence, the results also show that the policy had structural change effects, confirming the third research hypothesis.

5.3 Discussion

The results obtained from both the provincial level analysis as well as the Beijing case study are fascinating for a multitude of reasons. Firstly, the fact that both analyses confirm the same results on output and innovation, using different strategies and analytic levels, shows that the results are highly robust, which is also supported by the implemented robustness tests. Secondly, the results are entirely in line with the original Porter hypothesis as they confirm both its weak and strong version. Regarding the latter, both analyses of output, looking at per capita GDP and sectoral value added, respectively confirm a significant positive effect and therefore the first research hypothesis of this paper. Regarding the former, both the provincial level analysis using an index of innovation and the sectoral level analysis looking at enterprise expenditures on research and development confirm that the implementation of the policy had a significant positive effect on innovation. The analyses thereby confirm the second research hypothesis. Thirdly, the analyses of CO_2 emissions and energy consumption show that the policy did have the intended effect and reduced both.

More interesting than these general insights are, however, the different impacts found for high- and low-tech sectors. All regressions for the Beijing case study suggest fundamentally different policy effects on the sectors in each group. The results show that high-tech sectors reduced their emissions as a reaction to the policy and, at the same time, increased expenditure on research and development. The policy furthermore increased profits and output in these sectors, which is, following the Porter hypothesis, likely due to the increased investment in research and development, even though the channel itself could not be clearly verified empirically. Additional channels, for example improvements in internal management and an upgrade of technical facilities, are therefore plausible additional options. Low-tech sectors, however, were affected by the policy in a fundamentally different way. The results show that they failed to reduce emissions and did not increase investment in research and development following the implementation of the policy. At the same time, their profits and output were negatively affected. Following the Porter hypothesis, this could be attributed to a lack of both innovation and efficiency improvement as a response to the policy, which in turn did not only prohibit the reaping of Porter-style benefits, but furthermore led to a stronger exposure to negative consequences of the policy. The observed difference in output for high- and low-tech sectors therefore also confirms the third and last research hypothesis of this paper, as it shows a structural change effect in favour of high-tech sectors. This suggests that the implementation of the ETS supported China in its attempt to transform its economy to high-tech sectors and might, therefore, be a way to reduce its dependency on outdated and energy-intensive low-tech manufacturing, offering a way to escape the middle-income trap.

These results, however, also have their limitations. Firstly, the sectoral effects were only observed for Beijing, which already enjoys a high presence of high-tech and service sectors. Furthermore, exemplified by the presence of some of the most prestigious universities in China, Beijing showcases a high potential for innovation given its economic structure and high human capital. It, therefore, has to be assumed that the overall effects on other provinces with a higher share of low-tech industry are less positive than for the case of Beijing.

This also extends to the first part of the analysis. As most of the pilots were established in first-tier cities and since the estimated effect can only be interpreted as the average treatment effect on the treated, the results only apply to these settings. Additionally, given the strong presence of high-tech sectors in these cities, this could imply that the positive effect of the policy on these sectors could be the driver of the overall positive impacts. Provinces that are less innovative and more reliant on low-tech manufacturing might instead see an overall negative impact as the policy would potentially impact a large part of their economy negatively. The same is true for other emerging economies. Since China is relatively close to the technology frontier, at least in comparison to other emerging economies, the results obtained here are even less generalizable for other less innovative emerging economies than they are for other provinces within China. They show, albeit, that environmental or climate policies, in particular emission trading, can be tools to push innovation and sectoral transformation in leading cities and provinces of an emerging economy and might help them to transform their economy away from low-tech manufacturing. This also applies to industrialized economies with remaining low efficiency and low-tech manufacturing.

Additionally, it might also be possible that the unique character of the Chinese economy with its high share of state-owned companies and the existence of many low-efficiency facilities supported the strong positive effect, which might further limit the generalizability of the obtained results, at least for countries not sharing these characteristics. In comparison to other emerging economies, the institutional capacity of the Chinese state is also high (Popov, 2011), which furthermore has to be kept in mind. The fact that Beijing is one of the innovation centres of China might additionally lead to more substantial effects on research and innovation, as some companies might channel their research efforts there, which could imply an upward bias for the effect on expenditures in research and development.

Other limitations of this work include the usage of non-optimal deflators as well as the limited time frame of the analysis, which did not allow a detailed look at potential effects over longer periods of time. Even though the literature suggests increasingly positive effects over more extended periods of time, this could therefore not be analysed within the scope of this work. Additionally, the suggestion of an efficiency increase could not be further verified and is only derived from the positive output effect. The analysis of potential carbon leakage was furthermore relatively rudimentary, which is why carbon leakage cannot be entirely ruled out. Lastly, the lack of maturity of the carbon markets themselves did not allow for an analysis of the effect of prices or offsets, which might be an interesting topic for future research.

6 Conclusion

The economic effects of environmental regulation have been subject to intense discussion, in particular since Porter and van der Linde (1995) suggested that environmental policy could, contrary to traditional belief, have positive economic implications under certain conditions. Their suggestion, known as the Porter hypothesis, predicts that environmental policy could, by encouraging innovation, efficiency increases and potential first-mover advantages if policies are in line with global trends, not only counterbalance its costs but lead to increases in overall economic outcomes.

This paper has analysed this theory in the context of the implementation of emission trading pilots in China. Using both a provincial-level approach and a case study of Beijing, the analysis has confirmed the strong version of the Porter hypothesis, namely a positive effect on the overall economy. The analysis furthermore revealed that the implementation had a positive effect on innovation, confirming the weak version of the Porter hypothesis, and fostered structural change towards high-tech sectors. These results are highly robust since the effects on output and innovation could be shown for both the national level and the Beijing case study. The analysis has, therefore, achieved its objective to analyse and confirm the aforementioned hypotheses and to provide a holistic view of the Porter hypothesis system in the context of the emission trading pilots in China, including not only the overall effect, but also effects on innovation and structural change.

It has additionally shown that the policy was, in fact, successful at reducing CO_2 emissions and energy consumption and allowed some additional insights based on findings for high- and low-tech sectors. In this regard, the results suggest that the policy had a positive impact on profits and output of high-tech sectors, most likely due to increased spending on innovation and efficiency increases. Low-tech sectors, in contrast, suffered from the implementation of the policy, since they did not reduce emissions or increase spending on research and development and therefore did not benefit from the Porter mechanism. Instead, they experienced decreasing output and profits, likely due to compliance costs. In the long run, it can be expected that the policy will even have stronger negative impacts on their output while maintaining positive effects on high-tech sectors, causing structural change towards the latter.

These findings add to the existing literature on both the Porter hypothesis and the economic consequences of the Chinese ETS pilots, the latter of which is still limited and engaged in an ongoing debate. This paper managed to add in particular the holistic approach, investigating both the overall effects as well as the two main channels suggested by the literature on provincial and sectoral level. It furthermore stands out in comparison to other literature by differentiating the effects for high- and low-tech sectors, allowing a better understanding of the Porter hypothesis and the mechanisms behind it.

There are, nonetheless, some limitations to this work, constraining its practical implications. Since this study necessarily focuses on the effects of the policy on treated provinces, which in this case happen to be mostly large first-tier cities in China, and since the results indicate positive results for high-tech sectors only, the overall positive effects that were observed are most likely limited to such settings. Therefore, the findings can neither be extended to less-developed provinces within China nor to less developed emerging economies. They do, however, suggest that climate policy can be a tool for economic transformation in an emerging economy situated closely to the technology frontier, which possesses sufficiently strong high-tech sectors able to benefit from the policy. Even though that might not be the case for a country as a whole, an implementation of such a policy in the most advanced regions of an emerging economy might be an interesting option, following the successful example of China.

The fact that the findings of this paper are mostly limited to the case of leading Chinese cities and provinces also points towards promising avenues for future research. Given the establishment of emission trading schemes and other climate policies in an increasing number of emerging economies, the number of potential case studies, in particular concerning policy effects in areas with a more traditional or low-tech economy, is set to increase in the coming years. This would allow further research to overcome the main limitation of this paper and to derive conclusions that are more broadly applicable to emerging economies. Furthermore, the author was also provided with data from the China Carbon Pricing Survey by the China Carbon Forum (Slater et al. 2019), which could not be integrated into this paper but would offer interesting material to study the effects of companies' awareness and perception of the ETS on their reaction as well as on economic outcomes. Continuously improving provincial statistics in China will also allow a more detailed analysis of other provinces in the future. In this regard, in particular a case study of Hubei would be interesting as the province has both implemented a very capable ETS and has a fundamentally different economic structure than Beijing.

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Appendix 1 - Replication Lyu et al. 2020

Table Appendix 1 - Replication results. Dependent variable: Innovation index (Lyu et al. 2020)

	Replication in first differences	**Test for trend**	**Replication in levels**	**Replication in levels w/out GDP per capita**
Average treatment effect on the treated	-0.00592 (0.886)		-0.0580 (0.102)	0.0504^{*} (0.070)
Post treatment	-0.0214 (0.440)		0.00296 (0.907)	0.119^{***} (0.000)
Trend treated until 2014		-0.0000193 (0.493)		
Trend until 2014		0.0000123 (0.440)		
Controls	constant, per capita GDP, foreign investment, investment, exports			constant, foreign investment, investment, exports
N	180	180	210	210
R^2	0.035	0.034	0.480	0.312

Cluster-robust standard errors. p-values in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix 2 - Regression R&D Personnel

Table Appendix 2 - Regression Results. Dependent variable: R&D Personnel

	Weighted least squares	**High- / low-tech WLS**	**Lagged high- / low-tech WLS**	**Robustness check WLS**
Emission trading	4.974* (0.071)			
Emission trading * high-tech		2.951* (0.089)	3.024* (0.065)	
Emission trading * low-tech		-5.884* (0.052)	-7.088** (0.049)	
Emission trading * high energy consumption				5.175* (0.070)
Emission trading * low energy consumption				-2.343 (0.176)
Controls	constant, average number of employees, assets, liabilities, foreign R&D funds, governmental R&D funds, number of enterprises, exports			
N	131	131	130	131
R^2	0.469	0.479	0.206	0.470
Wooldridge test	0.991	0.989		0.996
Link test 1 (p-value)	0.000	0.000		0.000
Link test 2 (p-value)	0.064	0.053		0.055

Cluster-robust standard errors, *p*-values in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix 3 - Variable balance tests

Table Appendix 3 - Tests for variable balance for Panel DiD in Table 2 (GDP pc)
Variables depicted are the ones used for matching. The tests compare matched pairs.

Variable	Mean Treated	Mean Control	%bias	t-test t	t-test p>|t|	V(T)/V(C)
RGDP	269.72	244.91	15.5	0.29	0.777	1.20
RInvestment	167.83	200.26	-33.4	-0.62	0.544	0.46
RTertiaryIndustryValueAdded	136.91	101.27	49.1	0.92	0.376	1.40
RSecondaryIndustryValueAdd	118.33	118.03	0.4	0.01	0.994	1.30
outcome_dev	68.766	67.869	2.9	0.05	0.957	2.39

```
* if variance ratio outside [0.17; 5.82]
```

Table Appendix 4 - Tests for equality of distribution for Panel DiD in Table 2 (GDP pc)
Variables depicted are the ones used for matching. The tests compare matched pairs.

```
Two-sample Kolmogorov-Smirnov test for equality of distribution functions

 Smaller group       D       P-value  Corrected
 ----------------------------------------------
 0:                  0.2857    0.565
 1:                 -0.1429    0.867
 Combined K-S:       0.2857    0.938      0.896
ksmirnov RInvestment , by(treated)

Two-sample Kolmogorov-Smirnov test for equality of distribution functions

 Smaller group       D       P-value  Corrected
 ----------------------------------------------
 0:                  0.1429    0.867
 1:                 -0.2857    0.565
 Combined K-S:       0.2857    0.938      0.896
ksmirnov RTertiaryIndustryValueAdded , by(treated)

Two-sample Kolmogorov-Smirnov test for equality of distribution functions

 Smaller group       D       P-value  Corrected
 ----------------------------------------------
 0:                  0.5714    0.102
 1:                  0.0000    1.000
 Combined K-S:       0.5714    0.203      0.135
ksmirnov RSecondaryIndustryValueAdd , by(treated)

Two-sample Kolmogorov-Smirnov test for equality of distribution functions

 Smaller group       D       P-value  Corrected
 ----------------------------------------------
 0:                  0.1429    0.867
 1:                 -0.1429    0.867
 Combined K-S:       0.1429    1.000      1.000
ksmirnov outcome_dev , by(treated)

Two-sample Kolmogorov-Smirnov test for equality of distribution functions

 Smaller group       D       P-value  Corrected
 ----------------------------------------------
 0:                  0.2857    0.565
 1:                 -0.1429    0.867
 Combined K-S:       0.2857    0.938      0.896
```

Table Appendix 5 - Tests for variable balance for Panel DiD in Table 3 (Innovation) Variables depicted are the ones used for matching. The tests compare matched pairs.

Variable	Mean Treated	Mean Control	%bias	t-test t	t-test p>|t|	V(T)/ V(C)
RPerCapitaGDP	732.35	560.7	87.1	1.51	0.162	3.45
RTertiaryIndustryValueAdded	139.36	116.34	28.1	0.49	0.636	1.21
outcome_dev	.03667	.075	-33.3	-0.58	0.577	1.22

* if variance ratio outside [0.14; 7.15]

Table Appendix 6 - Tests for equality of distribution for Panel DiD in Table 3 (Innovation) Variables depicted are the ones used for matching. The tests compare matched pairs.

Two-sample Kolmogorov-Smirnov test for equality of distribution functions

Smaller group	D	P-value	Corrected
0:	0.5000	0.223	
1:	0.0000	1.000	
Combined K-S:	0.5000	0.441	0.343

ksmirnov RTertiaryIndustryValueAdded , by(treated)

Two-sample Kolmogorov-Smirnov test for equality of distribution functions

Smaller group	D	P-value	Corrected
0:	0.3333	0.513	
1:	-0.1667	0.846	
Combined K-S:	0.3333	0.893	0.837

ksmirnov outcome_dev , by(treated)

Two-sample Kolmogorov-Smirnov test for equality of distribution functions

Smaller group	D	P-value	Corrected
0:	0.0000	1.000	
1:	-0.5000	0.223	
Combined K-S:	0.5000	0.441	0.343

Table Appendix 7 - Tests for variable balance for Panel DiD in Table 4 (CO2 pilots) Variables depicted are the ones used for matching. The tests compare matched pairs.

Variable	Mean Treated	Mean Control	%bias	t-test t	t-test p>|t|	V(T)/ V(C)
RGDP	267.17	303.69	-20.8	-0.36	0.726	1.19
RTradeRegionDestination	56676	31960	40.9	0.71	0.495	4.21
outcome_dev	-15.848	.0528	-90.1	-1.56	0.150	3.33

* if variance ratio outside [0.14; 7.15]

Table Appendix 8 - Tests for equality of distribution for Panel DiD in Table 4 (CO2 pilots)
Variables depicted are the ones used for matching. The tests compare matched pairs.

```
Two-sample Kolmogorov-Smirnov test for equality of distribution functions

 Smaller group       D       P-value  Corrected
 ----------------------------------------------
 0:                  0.1667    0.846
 1:                 -0.3333    0.513
 Combined K-S:       0.3333    0.893      0.837
ksmirnov RTradeRegionDestination , by(treated)

Two-sample Kolmogorov-Smirnov test for equality of distribution functions

 Smaller group       D       P-value  Corrected
 ----------------------------------------------
 0:                  0.3333    0.513
 1:                  0.0000    1.000
 Combined K-S:       0.3333    0.893      0.837
ksmirnov outcome_dev , by(treated)

Two-sample Kolmogorov-Smirnov test for equality of distribution functions

 Smaller group       D       P-value  Corrected
 ----------------------------------------------
 0:                  0.0000    1.000
 1:                 -0.5000    0.223
 Combined K-S:       0.5000    0.441      0.343
```

Table Appendix 9 - Tests for variable balance for Panel DiD in Table 4 (CO2 surrounding)
Variables depicted are the ones used for matching. The tests compare matched pairs.

Variable	Mean Treated	Mean Control	%bias	t-test t	t-test p>|t|	V(T)/ V(C)
RGDP	267.57	236.4	23.2	0.52	0.611	1.32
RTradeRegionDestination	22112	17731	16.0	0.36	0.724	1.69
outcome_dev	18.012	1.9673	68.4	1.53	0.143	2.96

* if variance ratio outside [0.25; 4.03]

Table Appendix 10 - Tests for equality of distribution for Panel DiD in Table 4 (CO2 surrounding)
Variables depicted are the ones used for matching. The tests compare matched pairs.

```
Two-sample Kolmogorov-Smirnov test for equality of distribution functions

 Smaller group       D       P-value  Corrected
 ----------------------------------------------
 0:                  0.2000    0.670
 1:                 -0.1000    0.905
 Combined K-S:       0.2000    0.988      0.976

Note: Ties exist in combined dataset;
      there are 19 unique values out of 20 observations.
ksmirnov RTradeRegionDestination , by(treated)

Two-sample Kolmogorov-Smirnov test for equality of distribution functions

 Smaller group       D       P-value  Corrected
 ----------------------------------------------
 0:                  0.4000    0.202
 1:                 -0.1000    0.905
 Combined K-S:       0.4000    0.400      0.294

Note: Ties exist in combined dataset;
      there are 19 unique values out of 20 observations.
ksmirnov outcome_dev , by(treated)

Two-sample Kolmogorov-Smirnov test for equality of distribution functions

 Smaller group       D       P-value  Corrected
 ----------------------------------------------
 0:                  0.4000    0.202
 1:                 -0.1000    0.905
 Combined K-S:       0.4000    0.400      0.294
```