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Greening of the tax code: Promoting the move of firms towards sustainability?

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Preface

Even though only my own name is standing on the title page, there are many other people I owe the success of this dissertation to and whom I want to thank for all their support, advice and motivation throughout the past years. First and foremost, I am incredibly grateful to my supervisors, Prof. Dr. Martin Jacob and Prof. Dr. Ralf Fendel for their invaluable advice, continuous support, and patience during my doctoral journey. Both already have been part of my academic journey since Day 1 when I started my Bachelor studies at WHU – Otto Beisheim School in Fall 2014 and soon became student assistant at the chair of Prof. Jacob as well as chose Prof. Fendel to supervise my Bachelor thesis. It is an incredible honor for me to again have both of them support me also in this step of my academic career. Second, I want to thank all those that have provided valuable feedback for my papers formally on conferences, during seminars or informally by discussion my results and ideas during the lunch break. In particular, I want to thank Prof. Dr. Lisa Hillmann, Prof. Dr. Barbara Stage and my fellow doctoral students Constance Kehne, Nicola Schumacher and Thorben Wulff who always made time for me despite their own busy schedule and motivated me when it was difficult to motivate myself. Third, I want to thank my employer “The Boston Consulting Group” for supporting me as part of their Educational Leave Program. Lastly, I want to thank my parents Martin and Antje Zerwer, my boyfriend Elias Kurta and my friends. Not only during my doctoral studies all of them have supported me without any restrictions and with all of their means, but also long before this journey started. I am incredible grateful to always be able to count on them to celebrate my victories and help me get up quickly after losing one battle. This is why I devote this dissertation to you.

Abstract

This thesis examines the impact of green fiscal policies, specifically environmental taxes, on firms' decision-making and competitiveness. It aims to contribute to the taxation and financial policy literature by analyzing the influence of environmental taxes on corporate investment decisions and the distribution of the economic burden, the effect on corporate emission levels, and the shift in competitive market dynamics due to a green VAT. The findings suggest that standalone environmental taxes may not be the first best option, but their effectiveness can be improved through combining them with additional policy measures to foster firms' innovativeness. The thesis also shows that the adjustment of traditional forms of taxation, such as the VAT, can promote sector growth. The thesis thereby provides a more nuanced view on the economic consequences of environmental taxes.

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List of Abbreviations and Units

Abbreviation/Unit	Full Term/Explanation
μmol	micromole (10^{-6} moles)
CO ₂	carbon dioxide
COVID-19	Coronavirus Disease 2019
DDD	triple difference
DiD	difference-in-difference
e.g.	exempli gratia (for example)
et al.	et alia
EU	European Union
EUR/€	Euro (currency)
i.e.	id est (that is to say)
m ²	square meter (1m x 1m)
MNCs	multinational companies
NACE	nomenclature statistique des activités économiques dans la Communauté européenne
NO ₂	nitrogen dioxide
NO _x	nitrogen oxides
OMI	Ozone Monitoring Instrument
R&D	research and development
SO _x	sulfur oxides
UK	United Kingdom
VAT	value-added tax

1. Executive Summary

“Green fiscal policies are a critical part of efforts to address global challenges and transition to an inclusive green economy. By reflecting externalities in prices, aligning government expenditures with environmental goals, raising revenues, creating fiscal space for green investment and broader fiscal reform, such policies can support several Sustainable Development Goals (SDGs) and the Paris Climate Agreement.” (UN Environment Programme, 2022).

While policy makers as shown in the above quote agree that green fiscal policies are a critical part of the move towards a more sustainable economy, the exact design, extend and economic effect of such policies is still highly disputed. Especially environmental taxes as a green fiscal policy tool to reach international sustainability target are becoming more popular in their use by governments and policy makers. However, their effectiveness in promoting the move of firms towards sustainability is far from fully understood. Existing studies examine the tax effect on reducing environmentally harmful products (Lin and Li, 2011; Davis and Kilian, 2011), incentivizing the adoption of environmentally friendly technology and increases in R&D spending (Krass et al. 2013; Brown et al. 2022), and their aggregated effect on a country’s welfare (Bovenberg and de Mooij 1997; Fullerton and Heutel 2007; Yip 2018; Metcalf and Stock 2022). What the existing literature lacks is a detailed analysis of the impact of green taxes on individual firms’ decision-making as well as the competitive environment firms operate in.

To fill this gap in the literature, this dissertation project comprises four independent academic papers in the intersection of greening of the tax code and the impact on firms. I thereby aim to contribute to the taxation and financial policy literature in the following three areas: I examine how environmental taxes influence corporate investment decisions and who is bearing the economic burden (paper 1, paper 2), how emission taxes influence corporate emission levels (paper 3) and whether a ‘green’ VAT leads to a shift in competitive market

dynamics and promotes a whole sector even though primarily targeting consumers (paper 4). The first and second paper is coauthored work together with Prof. Dr. Martin Jacob (WHU), the third paper is coauthored work together with Thilo Ebertseder (Deutsches Zentrum für Luft- und Raumfahrt), Prof. Dr. Martin Jacob (WHU) and Prof. Dr. Hannes Taubenböck (Deutsches Zentrum für Luft- und Raumfahrt) while the fourth and last paper is single authored work.

The thesis contributes to the literature by providing a more nuanced view on economic consequences of environmental taxes. First, it suggests that newly introduced environmental taxes such as emission taxes by itself might not be the first best option. While the first and second paper on investment effects shows that those firms that cannot adjust as easily bear most of the economic burden rather than firms polluting the most, the third paper finds only minor real reductions of emissions following the introduction of a respective tax. Thus, while economic costs seem to be relatively high and not targeting firms at the center of creating the negative externality, ecological benefits are rather low. Second, this thesis shows that, while newly introduced standalone environmental taxes most likely are not the first best option, combining it with additional policy measures may make them still effective after all. The third paper on emission levels and emission taxes finds a particularly strong reduction in emissions for firms that are highly innovative, i.e., have high R&D spendings or are larger in size with more internal capacity to innovate. It seems that emission taxes, after all, can be effective, but only under the right circumstances. Thus, combining emission taxes with other policy measures to foster firms' innovativeness might be a way forward. Third, this thesis shows that also adjustment to more traditional forms of taxation can be beneficial. In more detail, the fourth paper shows that an adjustment of the VAT rate for organic products promotes the organic sector via increased competition. Greening of the tax code therefore not only punishes firms by taxing them in case of a negative externality but can also help to promote those that have a positive impact on the environment.

This thesis consists of six chapters: this introduction, the four research papers and a conclusion. An overall list of references as well as appendices to chapter two, three, four and five are provided at the end of the thesis. The first paper in chapter two begins by showing who bears the economic burden of environmental taxes by studying investment responses. Using private firms from Spain and the introduction of an emission tax in 2013 in the Autonomous Community Valenciana, we show that the investment response does not depend on the level of pollution, but on economic factors. Investments of firms operating in highly competitive markets, firms with low pricing power, and firms with low capital supply elasticity are most affected by environmental taxes. We also show that employees bear part of the emission tax burden through decreased wages and that firms with highly skilled employees suffer more. Overall, our results show that emission taxes affect not only the polluters but also other firms and their stakeholders. This paper benefitted from comments from Ulf Brüggemann, Juan Garcia Lara, Joachim Gassen, Nathan Goldman, Lisa Hillmann, Panagiotis Karavitis (discussant), Saskia Kohlhase, Rebecca Lester, Thorsten Martin (discussant), Maximilian A. Müller, Marcel Olbert, Gaizka Ormazabal, Cinthia Valle Ruiz, Dirk Schindler, Barbara Stage, Mary Vernon (discussant), the IÉSEG tax reading group members, and seminar participants at the EAA 2022 Bergen, UNC Tax Symposium, WHU – Otto Beisheim School of Management, Humboldt University of Berlin, IE University ESG Symposium 2022, and the Egyptian Online Seminars in Business, Accounting and Economics. We thank Constance Kehne for excellent research assistance and Elena Corrales for helpful background discussions and interviews on the Spanish institutional setting and the emission tax.

The second paper generalizes the findings of the first paper in a European setting. In a stacked DiD analysis of a European firm sample leveraging two specific reforms in France and Ireland, we provide empirical evidence on the impact of the introduction of CO₂ taxes on corporate investment. On average, following the introduction, corporate investment in total assets decreases by 1%. More importantly, the decrease is independent of firms' emission levels

but rather depends on their ability to flexibly adjust. This confirms the findings of our local Spanish setting (paper 1) in a broader European sample and helps us to show that the analysis might generalize. The short nature of the paper is driven by the intent to submit it for publication to *Economic Letters*. Feedback received for the first paper has also been considered for the writing of the second paper.

The third paper examines the role of environmental taxes in reducing emission output. Using unique satellite data to observe levels of nitrogen dioxide (NO₂), we leverage an emission tax introduction in 2013 in the Comunidad Valenciana. We find that this environmental tax reduced NO₂ levels only modestly by 1.2%. While the effect does not depend on prevalence of dirty versus clean firms in an area, we find that the NO₂ burden decreased more substantially in areas with a higher density of firms and in areas with innovative and large firms. Overall, our results imply that emission taxes are not very effective in reducing air pollution. We thank Joachim Gassen (discussant), Lisa Hillmann, Barbara Stage, and seminar participants at the IESE Business School of the University of Navarra, ESADE, OMG Transatlantic Tax Talks, Goethe University of Frankfurt, the 2022 TRR 266 Annual of Conference, and the WHU – Otto Beisheim School of Management for helpful comments and suggestions

Lastly, the fourth paper examines the role of a ‘green’ VAT in the competitive environment of firms. Using data on firms in Romania and leveraging the introduction of a tax reduction on organic products in 2019, I show that while the overall market for organic goods grows, the market share of individual firms decreases due to intensified competition post-reform. The effect depends on relative elasticity and market entry barriers. When relative elasticity is high, the decrease is stronger due to additional firms being attracted. When market entry barriers are high, the decrease is weaker, as it is harder for new competitors to enter. This paper greatly benefited from feedback and suggestions from Martin Jacob, Lisa Hillmann, Elias Kurta, Barbara Stage, Thorben Wulff and seminar participants at the Young Scholar Session of the TRR Annual Meeting 2022, WHU – Otto Beisheim School of Management and Humboldt

University Berlin. I also want to thank Catalin Albu (Professor of Accounting, The Bucharest University of Economic Studies), Irina Băncescu (Research Assistant, National Institute for Economic Research) and Simona Stanculescu (Tax Advisor, SM Accounting & Consultancy) for helpful insights into the Romanian organic market, VAT setting and other country specifics.

All of the four papers of this dissertation have benefitted from funding by the German Research Foundation (Deutsche Forschungsgemeinschaft), Project-ID 403041268 – TRR 266 Accounting for Transparency.

Table 1: Overview of Papers.

Overview of papers the dissertation consists of.

	Paper 1	Paper 2	Paper 3	Paper 4
Title	Who Bears the Burden? Evidence from Capital Investments	Carbon Taxation and Corporate Investment	How Effective are Emission Taxes in Reducing Air Pollution?	VAT do you eat? Green consumption taxes and firms' market share
Research question	Who carries the economic costs of environmental taxation?	Who carries the economic costs of environmental taxation in Europe?	What type of firms reduce emissions post emission-tax introduction?	What is the impact on firms of a reduced consumption tax for a certain product?
Methodology	Empirical study (DiD/DDD)	Empirical study (stacked DiD)	Empirical study (DiD/DDD)	Empirical study (DiD/DDD)
Sample	17,233 observations	1,369,244 observations	15,374 observations	71,174 observations
Data	Amadeus database	Amadeus database	Amadeus database/satellite data	Amadeus database
Main findings	(i) Firms in more competitive markets reduce investment more post-reform (ii) Firms with lower relative elasticity reduce investment more post-reform (iii) Part of the burden is carried by employees via lower wages (iv) Emission levels do not determine a firm's response post-reform	(i) Corporate investment in total assets decreases by 1% on average (ii) Firms with lower relative elasticity reduce investment more post-reform (iii) Emission levels do not determine a firm's response post-reform	(i) Environmental tax reduced NO2 levels only modestly by 1.2% (ii) Effect does not depend on prevalence of dirty versus clean firms (iii) NO2 burden decreases more in areas with higher density of firms (iv) NO2 burden decreases more in areas with innovative and large firms	(i) Market share of firms decreases by 1% following the reduction of a VAT (ii) Market share decreases more for firms with less market power (iii) Market share decreases more for firms with low barriers to entry (iv) Overall competition in the market intensifies via many new entrants
Main contributions	(i) Extending literature on incidence of environmental taxes by market structure & firm-level rather than consumer view (ii) Extending literature on real effects of environmental responsibilities by additional mechanism of taxation (iii) Contributing to policy debate of who is carrying the cost	(i) Generalization of Paper 1	(i) Extending literature on emission taxes by using highly granular spatial NO2 emission data from satellites (ii) Contributing to policy debate about the effectiveness of emission taxes by showing only a modest decrease in emissions post-reform that varies based on industrial activity and innovation	(i) Extending literature on how consumption taxes can influence corporations (ii) Extending literature of market effects of policy instruments in the agricultural sector (iii) Contributing to policy debate about VAT decreases to promote markets
Co-authors	M. Jacob	M. Jacob	T. Erbertseder, M. Jacob, H. Taubenboeck	-
Publication status	Unpublished working paper. Presented at EAA 2022 Bergen, UNC Tax Symposium, WHU – Otto Beisheim School of Management, Humboldt University of Berlin, IE University ESG Symposium 2022, and the Egyptian Online Seminars in Business, Accounting and Economics	Unpublished working paper.	Unpublished working paper. Presented at IESE Business School of the University of Navarra, ESADE, OMG Transatlantic Tax Talks, Goethe University of Frankfurt, the 2022 TRR 266 Annual of Conference, and the WHU – Otto Beisheim School of Management	Unpublished working paper. Presented at Young Scholar Session of the TRR Annual Meeting 2022, WHU – Otto Beisheim School of Management and Humboldt University Berlin

2. Who Bears the Burden? Evidence from Capital Investments¹

2.1. Introduction

In recent years, there has been increased public focus on climate change and on environmentally harmful activities, especially those by corporations. In response, policy makers have implemented environmental policies to combat environmental damage and climate change. In addition to environmental protection regulations and disclosure regimes, policy makers introduced environmental taxes. Although usually the objective of such a tax is the reduction of emissions or environmentally harmful behavior, it is also important to understand the economic consequences and to assess who bears the economic costs of such a policy measure. We thus explore which firms effectively pay for emission taxes by examining capital investment responses to the introduction of an emission tax, that is, a tax on a physical unit of an element—for example, tons of carbon or nitrogen dioxide—that has a proven specific negative environmental impact (OECD 2005). Recently, European Union (EU) member states collected over €330 billion in environmental taxes, or about 5.4% of total tax revenues (European Commission 2021). Hence, relative to corporate taxes, which amount to 12% of tax revenues in OECD countries, environmental taxes are relevant in terms of tax revenues.

While environmental taxes are becoming more important, their economic effects are far from fully understood. Studies examine the effectiveness of taxes in reducing environmentally harmful products (Davis and Kilian 2011; Lin and Li 2011; Pretis 2022), incentivizing research and development (R&D) spending or spending on new technologies (Krass et al. 2013; Brown et al. 2022), and their aggregated welfare effects (Bovenberg and de Mooij 1994; 1997; Fullerton and Heutel 2007; Yip 2018; Metcalf and Stock 2022). However, little is known about

¹Joint work together with Martin Jacob with the title: “Who Bears the Emission Tax Burden? Evidence from Capital Investments“. The paper has been presented at the EAA 2022 Bergen, UNC Tax Symposium, WHU – Otto Beisheim School of Management, Humboldt University of Berlin, IE University ESG Symposium 2022, and the Egyptian Online Seminars in Business, Accounting and Economics as well as several Brown Bag Seminars.

the impact of environmental taxes on individual firms and, in particular, about the distribution of economic costs across firms. This is surprising, given that one of the main concerns about environmental taxes is that they are regressive, that is, that they fall onto poorer households and those firms that cannot adjust (Hassett et al. 2009). Further, as industrial activity is the main driver of climate action, understanding who bears the economic costs is important for academics and policy makers alike.² We aim to contribute to this debate by exploring the incidence of an emission tax. Since price data are not available to us, we tackle this question by examining differences in firms' investment responses to an emission tax.

In theory, policy makers design environmental (in particular, emission) policies to make the polluter pay; the environmental tax is paid by the firms responsible for the negative externality, that is, the environmental damage (see, e.g., statements by the United Nations or the European Commission).³ However, this does not reflect the notion of tax incidence because tax costs are not necessarily borne by those who pay the tax (e.g., Jenkin 1872; Gruber 2022). Theory suggests that the investment response of firms to (environmental) taxes depends on the market structure, for example, whether the market is more versus less competitive (e.g., Fudenberg and Tirole 1984; Weyl and Fabinger 2013), and on the relative elasticity of firms versus their stakeholders (e.g., Weyl and Fabinger 2013; Fuest et al 2018; Ganapati et al. 2020; Dyreng et al. 2022). Using a simple analytical approach of a market for polluting machines as well as existing theoretical research (Weyl and Fabinger 2013), we derive two simple predictions. First, following the introduction of a tax, the demand for polluting machines in a perfectly competitive market is reduced more than in a monopoly. This is because firms are price takers (setters) in the former (latter) case. This simple model implies that in more (less) competitive

²For instance, the Boston Consulting Group study of Burchardt et al. (2021) shows that more than half of investments and efforts to reach a climate-neutral Germany need to come from industry via changes in production processes and, even more indirectly, via the adaptation of consumer products such as in the automotive sector.

³For instance, the European Commission claims that a “fundamental aim is to hold operators whose activities have caused environmental damage financially liable for remedying this damage.” (2004, p.2). Similarly, as part of the Rio Principle 16, the United Nations states that “the polluter should, in principle, bear the cost of pollution, with due regard to the public interest and without distorting international trade and investment” (1992, p. 6).

markets, firms cut investments more (less).⁴ Second, we build on the existing literature (e.g., Weyl and Fabinger 2013) and obtain predictions on the role of demand and supply elasticity driving the tax incidence. The general notion is that the pass-through of the tax—the (for us unobservable) measure of tax incidence—depends on the relative elasticity of supply and demand (Weyl and Fabinger 2013): Firms can pass on more (less) of the taxes onto customers when consumers are more inelastic (elastic) or when the firm’s supply is more elastic (inelastic). With lower (greater) pass-through, taxes reduce after-tax profits to a greater (smaller) extent, thereby resulting in greater (smaller) investment cuts. This is consistent with the notion that the economic burden of a tax is “borne by those who cannot easily adjust” (Kotlikoff and Summers 1987, p. 1047). Thus, we expect a stronger investment response by firms that are relatively inelastic vis-à-vis their stakeholders (e.g., customer or suppliers).

We test our predictions around the introduction of a local emission tax in the Spanish region of the Valencian Community in 2013. We use the neighboring provinces of Alicante (treatment group) and Murcia (control group) to explore the effect of a tax on sulfur oxides (SO_x) and nitrogen oxides (NO_x) in Alicante. This setting is advantageous for four reasons. First, the local emission tax reform had a short timeline between the policy announcement (Sep. 28, 2012) and implementation (Jan. 1, 2013), and it was triggered by an EU directive. Second, the reform substantially increased corporations’ tax bills—on average, by 7%—and caused substantial compliance costs. Third, the two provinces have similar socioeconomic characteristics and rank among the economically most significant provinces in Spain. Fourth, the 2013 tax introduction is not confounded by other major local policy changes in either region. Most importantly, we validate the setting by documenting a statistically and economically significant cut in investments of firms in Alicante due to the emission tax.

⁴Note that in the case of a perfectly competitive markets versus a monopoly, the quantity response might not directly reflect the incidence (e.g., Weyl and Fabinger 2013).

In our main analysis, we examine who bears the cost of the emission tax by exploring differences in investment responses across firms. If firms cut investments because of the reform, then this is an indication that the reform induced costs for them, resulting in lower investments. We test our predictions in several steps. We first use differences in market concentration across industries to assess the role of the competitiveness in a market. We find that the decline in investment due to the emission tax is greater in markets that are more competitive than in less competitive markets, consistent with the simple model.

Next, we examine within-market variation in the relative elasticity of firms vis-à-vis their stakeholders. These tests follow the general notion that the pass-through in consumer markets of taxes is a function of the relative elasticity of demand versus supply. If firms are less (more) elastic, the pass-through is smaller (greater) and, hence, there is a greater (smaller) investment response. We use two measures of firm's pricing power relative to their stakeholders to test for relative elasticity. First, we use the rather aggregate measure of profit margins in the spirit of the Lerner (1934) index. The idea of this measure is that a firm with a higher profit margin than another firm in the same industry is expected to have higher pricing power over its stakeholders such as customers, suppliers, or employees. Second, we use a firm's location, that is, whether the firm is located at the coast versus the hinterland. This measure captures consumer demand elasticity because in certain industries, for example, accommodation or real estate, location is a key factor determining demand elasticity. Using these two measures, we show that firms with less pricing power decrease investment more than those with more pricing power. This suggests that firms facing more elastic consumer demand bear more of the emission tax burden than firms facing less elastic consumer demand, which results in their greater investment response.

To corroborate the notion of relative elasticities, we use two different measures of firms' capital demand elasticity in the factor market based on the notion that a lack of financial flexibility results in lower capital demand elasticity (Graham 2022). If firms are more inelastic because they lack the financial flexibility, theory suggests that the pass-through is reduced (e.g.,

Weyl and Fabinger 2013). This results in reduced profitability of their investments; hence, they cut investments more. To test this empirically, we use two measures of financial flexibility: 1) availability of internal funds and 2) the Hadlock and Pierce (2010) index based on firm size and age. We find that when a firm's capital demand is rather inelastic (i.e., financial flexibility is low), the emission tax leads to a stronger reduction in investments than for firms that are rather elastic. This is consistent with the idea that the pass-through rate is lower for firms with more inelastic capital demand (Weyl and Fabinger 2013), resulting in greater investment cuts.

We supplement these findings in three ways. First, we combine the measure of market structure (more versus less competitive markets) and our measures of relative elasticity. We find evidence consistent with both characteristics being important for the investment response. Second, we examine supply chain linkages, using data in industry-level input–output tables and the level of emissions in a supply chain. We find that firm-specific pricing power only drives the investment response if the firm operates in a dirty supply chain, that is, if there is a tax burden that potentially can be shifted within the supply chain. In contrast, in cleaner supply chains, there is no response by either high-margin or low-margin firms, suggesting that our main findings are related to taxes potentially being passed on within the supply chain. Third, we explore the role of employees, as taxes can also be passed on to workers in the form of lower wages, again depending on relative elasticity in the labor market (Fuest et al. 2018). We first show that average wages slightly declined in response to the emission tax, indicating that emission tax burden falls partly on employees in the form of reduced wage growth. We then provide corroborating evidence for this notion by showing that the firm-level investment response to the emission tax is greater (smaller) when firms face more (in)elastic labor supply.

Finally, we examine the role of emission levels in shaping the investment response. Consistent with our argument that economic factors determine the incidence of the tax and contrasting the “polluter pays” principle as envisioned by policy makers, we find no relation between the average emissions of SO_x and NO_x and the investment response in the respective

industries: The investment decline is similar across high- versus low-emitting industries. This finding can be explained by the fact that investment effects depend on how much of the cost induced by the environmental tax is borne by a firm and not on who pays the tax. Firms do not respond based on their level of pollution but rather based on the relative elasticity of supply and demand as predicted by theory (Weyl and Fabinger 2013).

Our findings contribute to the literature in two ways. First, they add to the literature on the incidence of environmental taxes by taking a more nuanced look at firms' responses. Weyl and Fabinger (2013) and Delipalla and O'Donnell (2001) show that the incidence of a tax depends on the competitive structure of markets and might differ when assuming imperfect competition or a monopoly. We add to this theoretical literature by providing empirical evidence on investment responses to an emission tax, which differ with respect to relative elasticity. Further, our findings contribute to the literature on the incidence of environmental taxes that mainly considered the impact on consumers via higher prices without exploring firm-level responses (e.g., Bovenberg and Goulder 2002; Hassett et al. 2009). These rather aggregate approaches often led to an analysis without a dedicated focus on firms, even though they are seen as the most important actor towards a more sustainable future. We address this research gap by analyzing in more detail how the relative elasticity of supply and demand in consumer or factor markets influences the distribution of the economic burden of environmental taxes across firms.

Second, we contribute to the literature on the real effects of environmental and social responsibility. Previous research shows that investors react to positive and negative CSR events (Krüger 2015) and that dirty firms are punished through divestment (Oehmke and Opp 2020). In addition, a firm's value, measured as either market value, labor productivity, or sales growth, can depend on CSR standards and their realization (Flammer 2015, Dowell et al. 2000). Matsumura et al. (2014) find that firms that do not properly disclose their emissions under the existing environmental, social, and governance regime face a higher capital market discount. While the existing literature on the real effects of environmental responsibility mostly observes

the real effects of existing corporate standards as well as disclosure requirements, we contribute to this stream of literature observing the real effects of environmental policies by exploring which firms' investments are most affected (e.g., not necessarily the most polluting firms).

Finally, we add to the recent political debate around who bears the burden of climate protection measures to reach commonly set green targets, such as those outlined under the Paris Agreement.⁵ Identifying mechanisms through which costs are triggered by green policies (in this paper, environmental taxes) and the extent to which they are borne by those responsible for the environmental damage contributes to an overall understanding of what policy makers can expect in terms of distributional consequences for corporations and, thereby, for society at large when designing policies to attenuate climate change. Our results show that emission taxes do not hit the investments of firms in industries with high emissions but that the taxes are passed on to suppliers, customers, or employees. Hence, tax policy might not have the desired impact, suggesting that policy makers may utilize other tools from their regulatory toolbox to tackle polluting firms and make them more efficient actors in climate policy.

However, we acknowledge three main limitations of our approach. First, we cannot explore long-term effects; instead, we focus on the response within three years around one reform in Spain. Second, we consider only capital investments and not actual emissions or other outcomes, precluding us from making any statements about welfare effects. Third, due to data limitations, we observe only fixed asset investments, not investments in innovation and R&D.

⁵For example, an article in *The Economist* (July 2017) discusses how climate change can increase inequality, with most of the economic costs falling on the poorest. An article in *The New York Times* (Alderman, November 2021) summarizes the sentiment of recent protests in Europe due to soaring energy prices, seen as a consequence of expensive climate reforms whose costs are mostly borne by consumers.

2.2. Environmental Taxes and Corporate Investment: Predictions and Our Setting

2.2.1. Who Bears the Economic Burden of an Emission Tax?

At first glance, it seems reasonable to expect a stronger investment effect for emitting firms, as emission taxes are levied on the actual quantity emitted. The greater the emissions, the larger the firm's tax-induced cost increase and the greater the decline in profits, which leads to reduced investments. Hence, the polluter not only pays the tax but also experiences a larger negative effect on its investment. The polluter pays principle follows the notion of policy makers that "when a polluter is identified, he does have to bear certain costs and compensate the victims" (OECD 1992, p. 9).

However, this view is at odds with the tax incidence literature: The economic burden of a tax does not necessarily fall on the firms paying the tax (e.g., Jenkin 1872; Gruber 2022). It is thus very plausible that the cost of an emission tax, that is, the tax incidence, is passed on to those stakeholders "who cannot easily adjust" (Kotlikoff and Summers 1987, p. 1047). In particular, the response to the emission tax can depend on the market structure and, more importantly, on the relative elasticity of supply versus demand (Fudenberg and Tirole 1984; Delipalla and O'Donnell 2001; Weyl and Fabinger 2013; Mace, Patel, and Seegert 2020). To motivate our empirical analyses, we first use a simple model for polluting machines and then derive predictions based on Weyl and Fabinger's (2013) model on the pass-through of taxes.

2.2.1.1. The Role of the Market Structure

The role of the market structure in the investment response can be illustrated in a very simple model of a market for polluting machines that is either perfectly competitive or a monopoly. We use these two cases for illustrative purposes, even though cases of perfect competition and monopoly are very unlikely. Keeping this in mind, the inverse demand function is given by $p = 100 - q$ (demand for polluting machines) and the supply function by $p = q$ (supply of polluting machines). The quantity q is the number of polluting machines supplied within the market.

Before tax, in the competitive market, the equilibrium price is determined by setting demand equal to supply. This results in an equilibrium quantity of $q_{comp} = 50$ as well as an equilibrium price of $p_{comp} = 50$. In the monopoly, the price is determined by setting marginal costs equal to marginal revenue resulting in an equilibrium quantity of $q_{mon} = 33 \frac{1}{3}$ and an equilibrium price of $p_{mon} = 66 \frac{2}{3}$. Now, we introduce an emission tax t in both markets. After the introduction of the emission tax, the equilibrium quantity and price under perfect competition are $q_{t,comp} = 50 - \frac{1}{2}dt$ and $p_{t,comp} = 50 - \frac{1}{2}dt$. In the monopoly, the resulting quantity is $q_{t,mon} = 33 \frac{1}{3} - \frac{1}{3}dt$ with a price of $p_{t,mon} = 66 \frac{2}{3} - \frac{2}{3}dt$. Appendix A presents more detailed calculations.

By comparing the pre-tax and after-tax quantities for polluting machines ($\Delta q = q - q_t$), one can clearly see that in both market structures, the produced and supplied quantity (i.e., capital investments) decreases. Importantly, the decline in the produced quantity is greater in the case of perfect competition ($\Delta q_{comp} = -\frac{1}{2}dt$) than in a monopoly ($\Delta q_{mon} = -\frac{1}{3}dt$). Thus, the investment response is expected to be stronger in more competitive markets than in less competitive markets ($|\Delta q_{comp}| > |\Delta q_{mon}|$). Note, however, that the quantity response is not directly indicative of tax incidence, because this requires the comparison of pre-tax profits and after-tax profits (which may be ambiguous, depending on the model, as there could be overpassing; e.g., Weyl and Fabinger 2013). Important for our purposes is the unambiguous quantity effect that is larger under perfect competition than in a monopoly in response to an emission tax. We test this prediction in our empirical analysis.

2.2.1.2. *Exploring the Role of Relative Elasticity*

In the next step, we derive predictions based on the role of differences in relative elasticity between firms and their stakeholders. Tax incidence is usually described using models of perfect competition but, as shown by Weyl and Fabinger (2013), the role of relative elasticity of demand versus that of supply generalizes to other markets such as monopolies or those with imperfect competition. The key proxy of tax incidence in models of perfect competition is the

price response (e.g., Gruber 2022). Specifically, the pass-through of taxes translates into higher prices, whereby consumers bear part of the tax burden. Weyl and Fabinger (2013) describe this pass-through rate ρ as $\rho = 1/(1 + \frac{\epsilon_D}{\epsilon_S})$, with ϵ_D being the demand elasticity and ϵ_S describing the supply elasticity. The formula for ρ reflects one key principle of incidence: The pass-through rate ρ increases with the ratio of the elasticity of supply relative to that of demand. That is, a less elastic demand or a more elastic supply results in a higher level of pass-through of taxes to consumers.⁶ Similar predictions can be found in the existing literature on environmental taxes, although with a focus on input taxes (Ganapati et al. 2020).

The difference in the pass-through rate ρ that stems from differences in the relative elasticity of supply versus that of demand is not only informative about tax incidence but also about the investment response. A reduction in the pass-through of taxes leads to lower after-tax profits after the introduction of the emission tax. These reduced after-tax profits then result in a decline in a firm's investments (see Jacob et al. 2019, regarding the VAT). In contrast, if firms can pass on a substantial part of taxes because of inelastic demand (low ϵ_D), after-tax profits will not decrease as much due to the emission tax and firms will not cut their investments as much.

One can also consider the perspective of a factor market, where firms demand either capital K or labor L from suppliers. If the firm is relatively elastic in its factor market (e.g., for capital or labor), it can pass more of the tax to its factor supplier. In this case, the same consequences apply. That is, if there is a higher pass-through to the suppliers, the investment change in response to an emission tax is muted. For example, if the firm has high capital demand elasticity in the market for capital (high ϵ_D^K), the pass-through rate to its supplier is higher. This implies that a firm demanding capital with a high ϵ_D exhibits a lower profit reduction, thereby muting its investment response to taxes. Likewise, in the labor market, if labor supply is relatively

⁶There are several examples where there are differences in prices within the market for the same homogenous good. Such examples include gasoline (Jacob et al., 2022) or the market for food and other everyday products (GB Competition Commission, 2000). Some firms can sell their products at higher prices because their consumers are rather inelastic (e.g., when buying water at an airport or eating at a restaurant with a perfect sea view).

inelastic (elastic), that is, ϵ_S^L is low (high), the firm will bear less (more) of the tax burden, resulting in a lower (greater) investment response to the tax. Hence, from the factor market perspective, it is again about relative elasticity: For a firm with low capital demand elasticity and/or facing high (capital or labor) supply elasticity, we expect a larger investment decline due to a tax than for a firm with high capital demand elasticity or facing low supply elasticity.⁷

In summary, according to the tax incidence-based explanation, the investment effect depends on the relative elasticity of demand versus that of supply. If firms can pass on part of the costs to suppliers or consumers (high ρ), we do not expect to see a strong effect of emission taxes on investment. If, however, a firm has limited market power and cannot pass the costs on, that is, ρ is low, we expect it to cut investment more strongly because it partly bears the tax burden. In our analysis, we assess the role of relative elasticity through different proxies for either the demand elasticity (e.g., ϵ_D or ϵ_D^K) or the supply elasticity (e.g., ϵ_S^L).

2.2.2. *Exploiting Regional Environmental Taxes in Spain*

To investigate the effect of environmental taxes on investment across firms, we exploit the introduction of a local emission tax in 2013 to the Spanish Comunidad Valenciana.⁸ Following a short and unanticipated legislative process triggered by the non-binding EU Directive 2012/27/EU that proposes the use of energy and emission taxes by member states to reduce end-use consumption,⁹ in September 2012, the regional government of the Valencian Community announced the introduction of a new local tax law package that pursues the compensation of society for the cost it bears for environmental harm from industrial pollution and to stop the deterioration of the natural environment (see Figure 1). During the plenary

⁷While using profitability in our tests may look appealing at first glance, there are important downsides of using after-tax profitability, for example, return on assets, as a dependent variable. When firms cut investments as predicted, they likely cut the least profitable projects, thereby increasing the overall profitability of a firm.

⁸For the legal text, see Law 10/2012, December 21, of Fiscal Measures of the Administrative and Financial Management and of the Organization of the Generalitat, Official State Newsletter (BOE), No. 21, January 24, 2013, pp. 3691–3812, Article 154. The text of the law and its English translation is available upon request.

⁹Indeed, according to our background discussions with a Spanish sustainability expert, sustainability taxes are commonly established shortly after or in parallel with EU directives.

session announcing the new policy, the Minister of Finance and Public Administration, José Manuel Vela, stated that the new tax is designed in a manner “so that those that affect or create more risks for the environment in the community compensate for these effects” (Europa Press 2012). With this purpose, new consumption-based environmental tax rates were introduced as of January 1, 2013, targeting the emission of gases by corporations located in Valenciana to improve local air quality and foster positive local environmental development.



Figure 1: Timeline of the Introduction of the Valencian Emission Tax.

This figure illustrates the overall sequence of the introduction of the new emission tax in the Spanish Valencian Community, beginning with the policy announcement on September 28, 2012.

Specifically, SO_x and NO_x emissions are taxed between €9 and €50 per ton, depending on firm-specific consumption levels. The tax base for the emission taxes is the sum of the NO_x quantities (in tons) emitted multiplied by 1.5 and the SO_x quantities emitted. On average, our sample firms’ total tax burden including corporate taxes increased by 7% of the existing tax bill,¹⁰ suggesting a sizable additional tax payment. The tax base is determined via direct estimation (through automatic measurement systems or dedicated sampling within a certain timeframe) or via objective estimation (based on data from the last measurement or expert opinion).¹¹ In addition to the actual tax burden, the emission tax triggered sizable compliance costs for firms and increased the demand for tax advisory services.¹²

¹⁰We use the SO_x and NO_x emission data (in tons per year) of 251 sites in the Valencian Community, provided by the Spanish Ministry for the Ecological Transition and the Demographic Challenge (2022b) as a tax base (with the NO_x amount multiplied by a factor of 1.5, as stated in the law), as well as the available tax rates (with the amount depending on the quantity emitted) to calculate the average absolute tax burden borne by the sample of sites. We then compare this amount to the average overall tax burden of the sample firms in our setting. The tax payments of our sample firms are based on the tax expenses disclosed in their financial statements.

¹¹The allowed measurement and estimation techniques per substance are outlined by the Spanish Ministry for the Ecological Transition and the Demographic Challenge (2022a).

¹²This is anecdotally evidenced by the existence of local environmental and emission tax partners of large accounting and consulting companies in Spain.

The regional Spanish setting is advantageous for our purpose for several reasons. First, the introduction of emission taxes in the Valencian Community was unanticipated by most businesses. While the neighboring communities of Murcia, Castilla–La Mancha, and Aragón have been familiar with such taxes since the early 2000s, it took the Valencian Community almost 10 years to introduce its own emission tax. The timeline of the introduction of this emission tax indicates that there was relatively little time for businesses to prepare and adapt their behavior before the tax came into force. There were only three months between the announcement (September 28, 2012) and the effective introduction date (January 1, 2013).¹³

Second, local environmental taxes are significant in Spain (with €137.3 million in revenue raised in Catalonia in 2020 alone), as well as in the Valencian Community (The Local 2021). The Valencian Community has four regional taxes in total, namely, a tax on empty homes for those with more than 10 properties, a tax on waste processes, a tax on activities that have an impact on the environment (i.e., the emission taxes we use in our setting), and a tax for water treatment (The Local 2021). The Valencian environmental tax reform in 2013 alone added an additional €21 million in annual revenue to the local budget (Generalitat Valenciana 2022).

Third, the setting enables us to use the variation within Spain, thereby holding general economic conditions and regulations constant. Valenciana does not share a border with France or Portugal, eliminating cross-country effects that can exist in communities such as Asturias or Catalonia. Furthermore, while the cities of Madrid and Barcelona are home to many Spanish firms, the choice of a community setting with a lower urban–rural gradient such as the Valencian Community reduces potentially unobserved effects due to differences in infrastructure and other local conditions. Still, the Valencian Community is home to more than five million Spaniards and thus relevant in its size and economic significance for the country.

¹³In the Valencian Community and the Region of Murcia, the Spanish right-wing party Partido Popular has been in power for the last two decades. Thus, no political change could have led to the anticipation of the introduction of a new emission tax, as could have been possible with a change in power from a right-wing to a green party.

Fourth, the Valencian policy change is not confounded by other major (tax) changes or major political events in either the Valencian Community or the control community of Murcia.¹⁴ Local policy changes that were an additional part of the overall policy package in the Valencian Community agreed upon in September 2012 relate to, for example, changes in the administration fees of the regional government or changes in fees in the field of culture. Therefore, we do not expect these to have any influence on our setting. In addition, while some amendments were made to the local environmental tax laws, these changes occurred at least five years after the initial introduction or are only minor (see Figure 1). Other local environmental taxes in Valenciana or Murcia, such as the tax on discharges into coastal waters (Murcia) or the tax on the deposit and storage of waste (Murcia), as well as non-environment-related taxes, such as the gambling tax (Murcia) or the tax on empty dwellings (Valenciana), did not change during our period.¹⁵ Hence, we do not expect them to have an influence on our setting. While the broader Valencian Community was affected by the tax change, Murcia and Alicante serve as an optimal treatment–control combination, since political conditions in both communities remained fairly stable during the observed timeline. The other neighboring communities—Castilla–La Mancha, Aragón, and Catalunya—are less suitable as control groups because we observe at least one swing in the governing party over the period 2010–2015. In contrast, the distribution of power of political parties remained stable in Murcia and Valenciana during that time. In addition, due to a change in the personal income tax rate in the Valencian Community in 2015 as well as a shift in the political balance of power in Valenciana after 20 years of predominance of the People’s Party (*Partido Popular*) with the election of the Socialist Party of the Valencian Country into office, in coalition with the left-wing *Coalició*

¹⁴Although the introduction of a local emission tax in Catalonia in 2014 could also be an interesting setting, the attempted Catalanian independence referendum in 2014 is a potentially confounding event.

¹⁵A detailed list of all regional taxes and their associated changes in all Spanish communities (particularly in the treatment Community of Valenciana and the control Community of Murcia) can be retrieved from the homepage of the Spanish Ministry of Finance and Public Administration at <https://www.hacienda.gob.es/es-ES/Areas%20Tematicas/Financiacion%20Autonomica/Paginas/tributospropiosautonomicos.aspx>

Compromís,¹⁶ we end our sample period in 2014 to avoid political or economic reactions to the historic regional election result confounding our findings. Moreover, ending the sample in 2014 ensures that the 2015 personal income tax change in Murcia did not affect our results (see, e.g., Jacob and Vossebürger 2022, on investment effect of this and other personal tax changes).

2.3. Empirical Setting and Data

2.3.1. Estimation Strategy



Figure 2: Graphic Illustration of the Local Spanish Setting. This figure illustrates the settings used to test for a potential effect. We use Alicante as the treatment group (solid fill) and Murcia as the control group (dotted fill).

¹⁶For example, an article in *El Diario* (May 2015), a leading Spanish online newspaper, discusses the political and historical significance of the election loss and the expected shift in policy making due to the coalition of the two new parties in power after two decades, without any participation of the People’s Party.

We exploit the introduction of the emission tax in the Valencian Community in a triple difference (DDD) setting. Figure 2 shows a map of Spain that highlights all two-digit postal code areas, autonomous communities, and our treatment and control groups. Since the validity of the DDD approach hinges on the quality of the control group, we focus on a very local setting, comparing firms located in the province of Alicante (treatment group) to firms located in the bordering province of Murcia (control group). We chose a narrow local setting of only two provinces (rather than the entire Valencian Community, comprising three provinces) as our main setting because both provinces had similar socioeconomic characteristics and remained politically stable during the sample period. For instance, Alicante (Murcia) had a population of about 2 (1.5) million in 2013. Both provinces contribute approximately 3% to the overall Spanish national GDP (Instituto Nacional de Estadística 2020). This comparability can also be seen in our sample, with observations splitting almost equally between Alicante (55%) and Murcia (45%). We thus use the following DDD approach:

$$\begin{aligned}
Inv_{i,t} = & \alpha_0 + \beta_1 Alicante_i \times Post_t + \beta_2 Split Variable_j \times Alicante_i \times Post_t \quad (1) \\
& + \beta_3 Split Variable_j \times Post_t + \gamma X_{i,j,t-1} + \alpha_i + \alpha_{ind,t} + \varepsilon_{i,t}
\end{aligned}$$

where the dependent variable, $Inv_{i,t}$, is the gross investment of firm i in year t , defined as the change in fixed assets from year $t-1$ to t plus depreciation, scaled by total assets in year $t-1$ (see also Badertscher et al. 2013; Asker et al. 2015; Jacob et al. 2019). Hence, in contrast to prior literature on corporate tax using wage data (Suarez-Serrato and Zidar 2017; Fuest et al. 2018) or consumer prices (e.g., Baker et al. 2021; Jacob et al. 2022) to estimate if the corporate tax incidence falls on workers or consumers, we assess whether a firm bears the economic burden of the emission tax by examining the investment response as discussed in Section 2.

To arrive at the DDD analysis, we first sort firms into treatment and control groups based on the postal code. We define the treatment group as firms located in the Valencian Community province with the postal code 03 ($Alicante = 1$) in which the new emission tax was introduced

in 2013. The control group comprises firms located in Murcia, that is, firms with the postal code 30 (*Alicante* = 0), where an emission tax was implemented in 2005. The variable *Post* is equal to 1 for 2013 and 2014 and 0 otherwise. Our regression sample thus includes the two years prior to the reform announcement (2010 and 2011), the announcement year (2012), and two post-reform years (2013 and 2014). The *Split Variable* is a dummy variable we use for the different tests that is equal to 1 if it falls in the high category of the respective split variable (e.g., more competitive markets) and 0 otherwise (e.g., less competitive markets). Thus, the interaction $Alicante \times Post$ captures the effect of firms for which *Split Variable* equals 0. The DDD coefficient $Split Variable \times Alicante \times Post$ captures the difference in investments between high- and low-category firms, whereas the sum of both ($\beta_1 + \beta_2$) captures the effect of high-category firms. We always measure the split variable in 2011, that is, prior to the reform, to ensure that the reform does not affect the respective split variable. All other interactions or main effects of the DDD model ($Split Variable \times Alicante$, *Split Variable*, *Alicante*, and *Post*) are absorbed by the fixed effects. Hence, they are not shown in Equation (1).

The baseline regression includes a vector of lagged control variables ($X_{i,t-1}$), building on prior literature on private firms' investments (e.g., Badertscher et al. 2013; Shroff et al. 2014; Shroff 2017; Fox et al. 2022). Specifically, we control for debt financing (*Leverage*, defined as long-term debt over total assets), profitability (*Return on Assets*, defined as net income over total assets), the logarithmic growth in sales (*Sales Growth*), firm size (*Firm Size*, defined as the natural logarithm of sales), and cash holdings (*Cash*, defined as cash holdings over total assets). Further, we include firm fixed effects (α_i) to account for time-invariant firm and local characteristics. We also include industry–year fixed effects ($\alpha_{ind,t}$) to ensure that we compare treated and control firms in the same industry. Industry–year fixed effects also account for differences in the industry composition between Alicante and Murcia. These fixed effects ensure that we compare firms in the treatment and control groups within the same industry and

hold. They also hold the market structure constant when exploring within-industry differences in relative elasticity of demand vis-à-vis supply. We cluster standard errors at the firm-level.

2.3.2. *Data and Descriptive Statistics*

We use all available data on Spanish firms from Bureau van Dijk's Amadeus database for the period 2009–2014. Our analysis is based on the unconsolidated financial statements of private, unlisted corporations we extract from the baseline data of Spanish firms. In contrast to consolidated balance sheet information, as provided, for example, in Compustat Global, unconsolidated data allow us to locate the activity of a single firm, since the information is not consolidated per group. We focus on standalone firms, that is, we use a sample of firms that do not belong to a multinational or domestic group. These standalone firms in our sample cannot easily relocate their business activity and likely source their inputs locally. For the analysis, we require firms to exist for more than two years to have sufficient information available before and after the environmental tax reform. We exclude companies with total assets below €50,000 and fixed assets below €5,000 as well as those that do not report earnings before interest and taxes. We also exclude observations with negative sales, total assets, or cash. These sample requirements result in 3,360 firms and 17,233 observations.

Table 2 reports statistics for the variables of our sample of 17,233 observations used for the baseline analysis. The variable definitions can be found in Appendix B, Table B.1. All firm control variables are winsorized at the first and 99th percentiles. Firms, on average, have a gross investment of 3.5% of their prior year's total assets. The average (median) firm has a leverage of 16.1% (8.7%), a return on assets of 1.3% (1.2%), sales of €2.4 million (€1.2 million), and cash holdings of 10.4% (5.4%) of total assets. Further, Table 3 illustrates the industry composition base on the NACE code definition used in the EU. Most sample firms are from the wholesale and retail trade sector (Sector G, 35.2%), the manufacturing sector (Sector C, 28.1%), and the construction sector (Sector F, 11.5%).

Panel C of Table 2 presents descriptive evidence on how the firms in our treatment group compare to those in our control group. Overall, it shows that the firms in both groups are fairly similar in terms of descriptive statistics. The difference between the two groups is mostly very small and close to 0 for several variables. For instance, firms in Alicante, on average, have a gross investment of 3.5% of their prior year's total assets, whereas firms in Murcia have a gross investment of 3.6%, making them very comparable. We also find that firms in Alicante have, on average, more leverage, a smaller return on assets, lower sales, and larger cash holdings and are, on average, smaller than firms in Murcia. These differences do not drive our findings (see the entropy balanced sample analysis below).

Table 2: Descriptive Statistics.

This table presents descriptive statistics of our main variables for 17,233 observations from 2009 to 2014. The variables are defined in the Appendix.

Panel A: Dependent Variables					
Variable	Mean	St. Dev.	25th Perc.	Median	75th Perc.
Gross Investment	0.0352	0.0915	0.0000	0.0100	0.0422
Net Investment	0.0011	0.0891	-0.0302	-0.0096	0.0110
Change in Fixed Assets	-0.0074	0.2939	-0.1142	-0.0352	0.0398
Change in Leverage	-0.0028	0.0824	-0.0291	-0.0014	0.0040
Panel B: Other Firm Variables					
Alicante	0.5451	0.4980	0.0000	1.0000	1.0000
Post	0.3438	0.4750	0.0000	0.0000	1.0000
Leverage	0.1609	0.1969	0.0020	0.0870	0.2510
Return on Assets	0.0132	0.0705	0.0009	0.0116	0.0339
Sales Growth	-0.0579	0.3968	-0.1650	-0.0195	0.0988
Firm Size	13.8965	1.4046	13.1637	14.0554	14.8357
Sales (1,000 EUR)	2,398.6730	4,854.5700	521.0915	1,271.1590	2,773.8390
Cash	0.1039	0.1273	0.0177	0.0538	0.1419
Panel C: Difference Between Alicante and Murcia					
	Alicante = 1	Alicante = 0		Difference	
Gross Investment	0.0349	0.0357		0.0008	
Net Investment	-0.0000	0.0025		0.0025*	
Change in Fixed Assets	-0.0149	0.0014		0.0163***	
Change in Leverage	-0.0038	-0.0017		0.0020	
Leverage	0.1648	0.1561		-0.0087***	
Return on Assets	0.0107	0.0162		0.0055***	
Sales Growth	-0.0555	-0.0608		0.0061	
Firm Size	13.7497	14.0724		0.3227***	
Sales (1,000 EUR)	2,125.7650	2,725.6160		599.8506***	
Cash	0.1068	0.1005		-0.0063***	

Table 3: Industry Sample Composition.

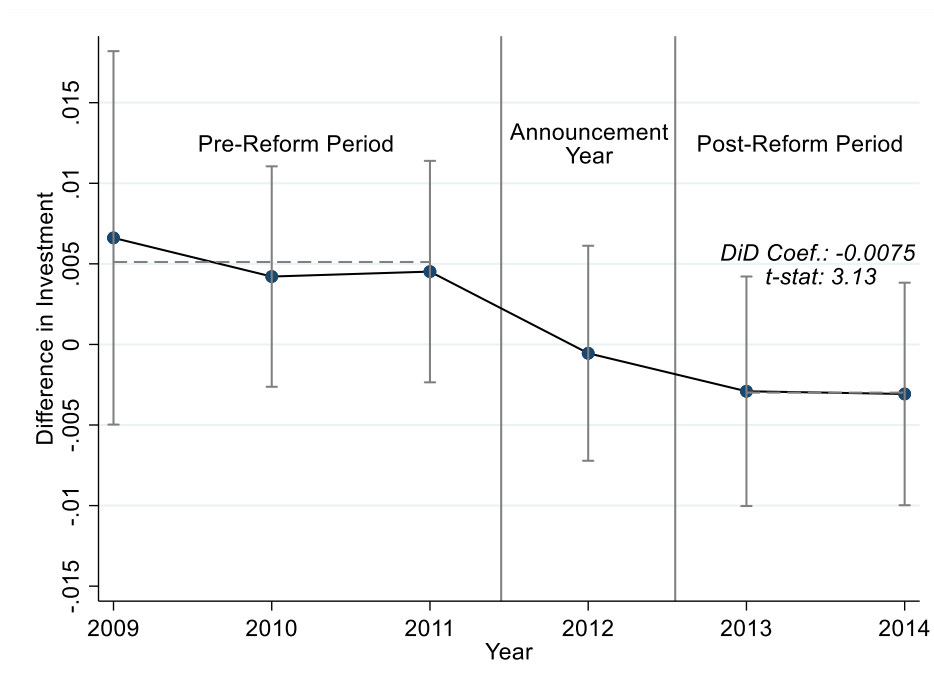
This table shows the number of observations per NACE industry code and their percentage in the overall sample.

Code	Description	Obs.	%
A	Agriculture, forestry, and fishing	672	3.90
B	Mining and quarrying	130	0.75
C	Manufacturing	4,833	28.05
D	Electricity, gas, steam, and air conditioning supply	129	0.75
E	Water supply, sewerage, waste management, remediation activities	84	0.49
F	Construction	1,989	11.54
G	Wholesale and retail trade, repair of motor vehicles	6,063	35.18
H	Transportation and storage	810	4.70
I	Accommodation and food service activities	486	2.82
J	Information and communications	181	1.05
K	Financial and insurance activities	75	0.44
L	Real estate activities	594	3.45
M	Professional, scientific, and technical activities	372	2.16
N	Administrative and support service activities	352	2.04
P	Education	53	0.31
Q	Human health and social work activities	150	0.87
R	Arts, entertainment, and recreation	180	1.04
S	Other services' activities	80	0.46
Total		17,233	100
	<i>of which treatment</i>	9,394	54.91
	<i>of which control</i>	7,839	45.49

2.3.3. Validation of Empirical Setting: Average Investment Responses

Before examining the question of who bears the emission tax burden, we first validate that the emission tax introduction is a suitable setting to examine investment responses. To this end, we first assess the dynamics of investment of firms in Alicante (treatment group) relative to the investment response of firms located in Murcia (control group). The underlying assumptions of our approach are that 1) absent the tax reform in 2013, corporate investments in our control group of Murcia and our treatment group of Alicante would have evolved similarly and that 2) on average, there is a decline in investments in Alicante in response to the emission tax. We thus regress investments on the treatment dummy (*Alicante*), interacted with year indicator variables. We also account for industry–year fixed effects.

Panel A: Without Controls



Panel B: With Controls and Firm Fixed Effects

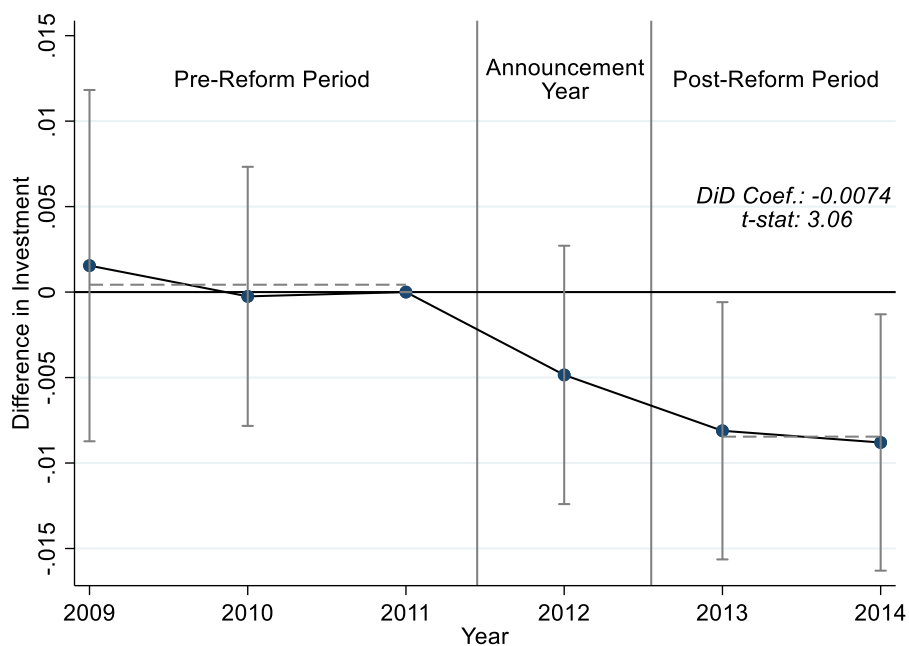


Figure 3: Trends in the Investment Differences Between the Treated and Control Groups.

This figure illustrates the difference in investment over the period 2009–2014 between the treated group (firms located in Alicante) and the control group (firms located in Murcia). Panel A estimates the difference without controls and fixed effects. In Panel B, we include firm fixed effects as well as control variables. In Panel B, the year before the announcement serves as the benchmark year. The 95% confidence bounds are based on standard errors clustered at the two-digit industry code level. The dashed line indicates the pre-reform average difference.

Panel A of Figure 3 shows the difference in investment between firms in Alicante and those in Murcia over the period 2009–2011 (*pre-reform period*). We find that investments in Murcia and Alicante follow parallel trends prior to the emission tax announcement. We find that after the reform, the investment in Alicante decreases relative to Murcia, consistent with the notion that the emission tax leads to lower investments. Panel B shows continued support for the parallel trends assumption prior to the announcement year when firm fixed effects are included along with control variables. Panel B also shows that the investment effect becomes negative and significant in 2013, when the emission tax was implemented, and remains stable in 2014.

In the next step, we run Equation (1) as a simple difference-in-differences (DiD) model to assess the average investment response to the tax reform in Alicante in the following form:

$$Inv_{i,t} = \alpha_0 + \beta_1 Alicante_i \times Post_t + \gamma X_{i,j,t-1} + \alpha_i + \alpha_{ind,t} + \varepsilon_{i,t} \quad (2)$$

where all variables are defined as above. Table 4 presents the results for the DiD analysis of corporations in Alicante as the treatment group versus corporations in Murcia as the control group. Column 1 includes only the DiD terms, without any controls or fixed effects. We then gradually add the controls and industry–year fixed effects (column 2) and, finally, firm fixed effects, to arrive at Equation (2) (column 3). The results in column 1 indicate that firms in Alicante decrease their investment after the emission tax reform relative to firms in Murcia. The results remain similar, also in terms of the economic magnitude, when firm control variables and industry–year fixed effects (column 2) or firm fixed effects (column 3) are added. The results indicate that firms cut investments by approximately 0.95% of their total assets. This effect is sizable, considering that, using a comparable set of non-listed domestic firms, a corporate tax cut of 25% increases investments by approximately 6% of total assets (Dobbins and Jacob 2016). The magnitude of 0.95% in our setting thus appears plausible, given that the emission tax accounts for, on average, approximately 7% of firms’ total income tax payments.

In Table 4, we also address concerns about differences in observable firm characteristics. This deals with concerns that differences in the availability of internal funds, in firm size, or in

the industry composition of Alicante versus Murcia would affect the results. To this end, we employ entropy balancing on firm controls (leverage, return on assets, sales growth, firm size, and cash holdings) and on the broad industry codes shown in Table 3. We then use these weights and estimate Equation (2) as weighted least squares. As shown in Table 4, the coefficient on the interaction between *Alicante* and *Post* remains statistically significant. This result holds without any controls or fixed effects (column 4), when controls and industry–year fixed effects are added (column 5), and when firm fixed effects are also included (column 6). Hence, the baseline DiD results support the prediction that emission taxes reduce investments.

The main finding of Table 4 is also robust to many additional tests (all untabulated). We find similar results when using alternative dependent variables, alternative model specifications, alternative definitions of treatment and control group, different placebo tests, and when excluding single industry tests. Importantly, we also use the approach by Berg et al. (2021) to address the concern that investments from the Valencian Community spread to unaffected firms, which would violate the stable unit treatment value assumption. The results indicate that there are no spillover effects to other regions, supporting our empirical approach.

Table 4: Validating the Setting: Emission Taxes and Investment.

This table presents the results of the validation of the empirical setting using an unbalanced panel as well as an entropy balanced approach. The primary dependent variable is gross investment, defined as the change in fixed assets from year $t - 1$ to t plus depreciation, scaled by total assets in year $t - 1$. The primary independent variable is the interaction between *Alicante* and *Post*. All control variables are lagged by one year. The table shows robust standard errors clustered at the two-digit industry code level in parentheses. The entropy balancing approach balances on all control variables and one-digit NACE codes (letter level). We include firm fixed effects in columns (3) and (6) and industry–year fixed effects in columns (2), (3), (5), and (6). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline Approach			Entropy Balanced Approach		
<i>Alicante</i>	0.0017 (0.0028)	0.0045 (0.0028)	-	0.0051* (0.0027)	0.0055* (0.0028)	-
<i>Post</i>	0.0025 (0.0019)	-	-	0.0040 (0.0029)	-	-
<i>Alicante</i> × <i>Post</i>	-0.0075*** (0.0024)	0.0088*** (0.0027)	-0.0095*** (0.0031)	-0.0088*** (0.0032)	0.0096*** (0.0034)	0.0109*** (0.0038)
<i>Leverage</i> _{<i>t-1</i>}		-0.0013 (0.0044)	-0.1228*** (0.0172)		0.0030 (0.0045)	0.1157*** (0.0176)
<i>Return on Assets</i> _{<i>t-1</i>}		0.1113*** (0.0153)	0.0337* (0.0185)		0.0943*** (0.0164)	0.0158 (0.0211)
<i>Sales Growth</i> _{<i>t-1</i>}		0.0114*** (0.0032)	0.0048* (0.0025)		0.0116*** (0.0032)	0.0058*** (0.0020)
<i>Firm Size</i> _{<i>t-1</i>}		0.0014* (0.0007)	-0.0040 (0.0029)		0.0017* (0.0009)	-0.0051* (0.0026)
<i>Cash</i> _{<i>t-1</i>}		0.0249*** (0.0064)	0.1401*** (0.0199)		0.0260*** (0.0062)	0.1319*** (0.0175)
Firm FE	No	No	Yes	No	No	Yes
Industry–Year FE	No	Yes	Yes	No	Yes	Yes
Observations	17,278	17,233	17,233	17,173	17,128	17,127
Adj. R ²	0.0003	0.0452	0.1424	0.0005	0.0434	0.1356

2.3.4. Parallel Trends of the DDD Analyses

Before turning to our main analysis, we assess the parallel trends assumption for the DDD setting outlined in Equation (1). To test this, we replace the *Post* dummy variable with year indicator variables and rerun Equation (1), without controls and with only year fixed effects. We then plot these interactions for the eight split variables used in Section 4. The results are presented in Figure 4. In none of the cases are striking pre-trends in investments of the DD (= *Alicante*) and the DDD coefficients (*Alicante* × *Split Variable*; with the *Skill* split being somewhat of an exception). Of the 24 DD coefficients, only two are significant, but in no systematic pattern. Likewise, of the 24 DDD coefficients, four are significant (two of them are

in the case of *Skill*). Again, there is no systematic pattern, suggesting that there are parallel pre-trends in the underlying trend of Alicante versus Murcia and in that of all split variables.

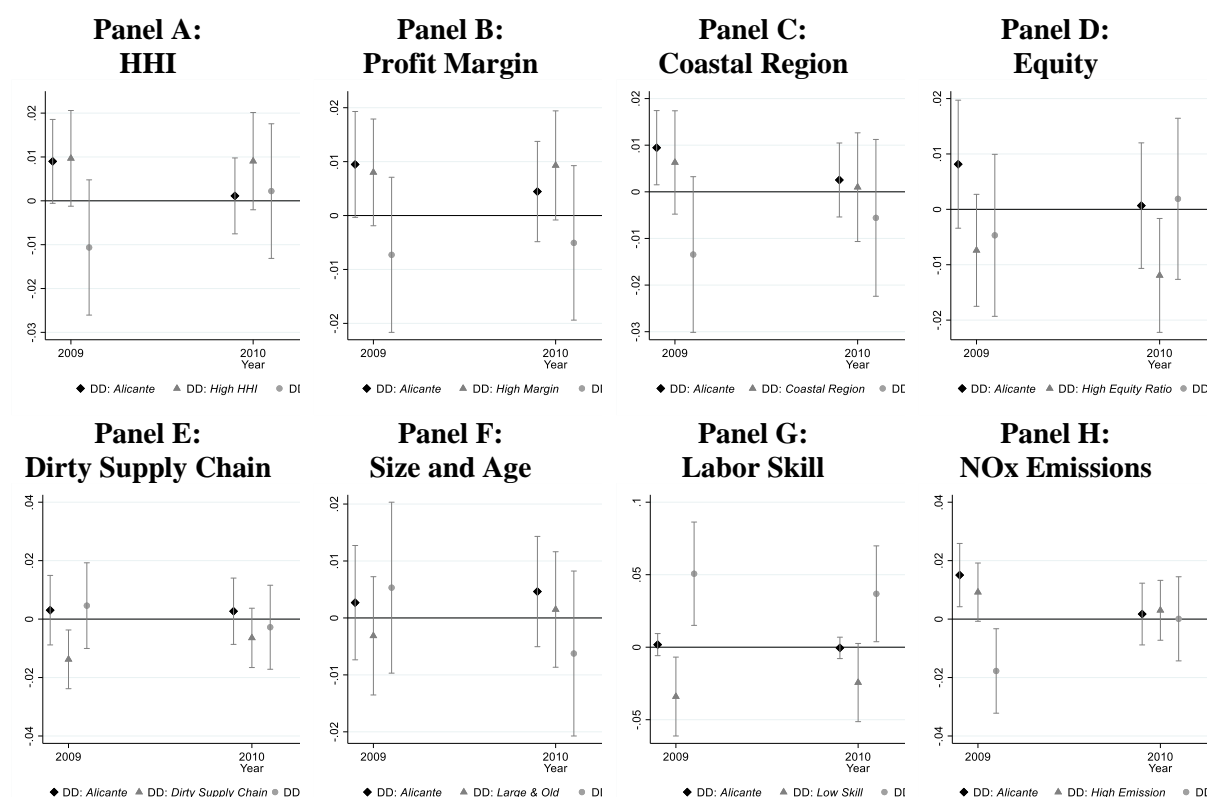


Figure 4: Parallel Trends of the DDD Approach.

This figure shows additional parallel trends tests for the DDD estimations. We use the following variables as split variables: HHI, profit margin, non-coastal versus coastal, high versus low equity ratio, small and young versus large and old, clean versus dirty supply chain, high- versus low-skilled labor, and high versus low NOx. We build on Equation (1) and replace Post with year dummy variables. We use the sample between 2009 and 2011. The 95% confidence bounds are based on standard errors clustered at the two-digit industry level.

2.4. Results on the Incidence of the Emission Tax

Next, we present the results of our main analysis of the economic burden of the emission tax and try to answer the question whether the costs of the tax are distributed in a manner that depends on the relative elasticity of supply and demand as suggested in Section 2. Specifically, we start by exploring the role of the market structure in investment responses (Section 4.1) and then explore within-market variation in relative elasticity (Sections 4.2). We supplement these analyses in two ways: first, by specifically considering supply chain linkages of industries (Section 4.3) and second, by considering the role of other stakeholders, in particular, employees, in bearing the emission tax burden (Section 4.4).

2.4.1. Role of Market Structure

First, we look at how differences in market structure—more versus less competitive markets—can lead to a difference in response to the emission tax in terms of investment. Given the simple model in Section 2.1.1, we expected the investments of price takers (i.e., firms operating in more competitive markets) to be more affected by the introduction of the emission tax than the investments of price setters (i.e., firms operating in less competitive markets). We test this notion by estimating Equation (1) and by using the Herfindahl-Hirschman Index (HHI) to split our sample into firms facing more (*High HHI* = 0) versus less competitive markets (*High HHI* = 1). We measure the HHI based on sales, using the full sample of Spanish firms including multinational and non-standalone firms, to reflect the competitive environment and avoid any misrepresentation due to our focus on standalone firms. We perform the split in 2011 to avoid the reform affecting competition. We define HHI by using either the four-digit NACE code or the two-digit NACE code. Consistent with our prediction that firms in more competitive markets bear more of the emission tax burden, we expected $\beta_2 > 0$.

The results are presented in Table 5. We present the overall effect for firms with a low HHI (more competitive market; β_1), the overall effect for firms with a high HHI (less competitive market; $\beta_1 + \beta_2$), and the difference between these two effects (β_2). Table 5 shows a significant negative coefficient for firms in more competitive markets ($\beta_1 < 0$), consistent with our prediction. Moreover, it shows a negative but non-significant coefficient for high HHI firms for both HHI definitions (full four-digit NACE code in columns 1 and 2 and two-digit NACE code in columns 3 and 4). Importantly, in both cases, the two coefficients are significantly different ($\beta_2 > 0$). This implies that while quantities decrease in both types of markets, as indicated by the negative coefficient in all columns, firms operating in more competitive markets exhibit a greater reduction in their investment activity.

Table 5: Incidence-Based Explanation, Role of Market Structure.

This table presents the results of estimating Equation (1). Panel A shows the interaction of Alicante, Post, and Alicante \times Post with the dummy variable High HHI, which equals 1 if the firm has an HHI above the median (monopolistic market) and 0 otherwise (competitive market). We execute this split in 2011 to avoid the emission tax reform already affecting market competitiveness. We use lagged controls and firm and industry-year fixed effects for all the regressions. Robust standard errors clustered at the firm level are shown in parentheses. Standard errors (in parentheses) are clustered at the firm level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	HHI, Full NACE Code, Full Sample		HHI, Two-Digit NACE Code, Full Sample	
	(1) Low (Competitive)	(2) High (Monopolistic)	(3) Low (Competitive)	(4) High (Monopolistic)
<i>Alicante</i> \times <i>Post</i>	-0.0171*** (0.0044)	-0.0040 (0.0044)	-0.0155*** (0.0038)	-0.0028 (0.0057)
Difference [<i>t-stat.</i>]		0.0131** [2.11]		0.0127* [1.86]
Controls	Yes		Yes	
Firm FE	Yes		Yes	
Ind.-Year FE	Yes		Yes	
Observations	15,821		15,821	
Adj. R ²	0.1377		0.1376	

Since firms in more versus less competitive markets may differ from each other (see Table D.1), we use an entropy balancing approach that ensures that treated and control firms are similar in terms of observable characteristics. The results in Table D.2, Panel A in Appendix D indicate that once we account for these differences, the results are still very similar.

2.4.2. Exploring Variation in Relative Elasticity

2.4.2.1. Variation of Pricing Power and Consumer Demand Elasticity

The following tests explore if differences in firm-specific relative elasticity can explain why some firms respond more to the emission tax than others. These tests build on the notion that taxes are “borne by those who cannot easily adjust” (Kotlikoff and Summers 1987, p. 1047) and follow the logic of the pass-through rate ρ , which can differ depending on the relative elasticity of demand versus supply and which indicates if more incidence falls on consumers. For example, a dirty firm with low pricing power is expected to have a hard time passing on the tax-induced costs to their stakeholders (lower ρ). Put differently, this firm bears much of the tax burden; thus, it will reduce its investments to a greater extent than a firm with a high ρ .

We start to test this idea using two different pricing power measures. First, we use a broad measure of profit margins. According to Weyl and Fabinger (2013), low-margin firms are likely to face more elastic consumers (see also Jacob et al. 2019) and therefore have a lower pass-through rate of the emission tax. The notion that the profit margin relative to that of industry peers captures pricing power is related to the Lerner index (Lerner 1934). The existing literature further shows that the Lerner index is directly related to the path-through rate of taxes (Ganapati et al. 2020). We estimate Equation (1) using the dummy *High Margin* as our split variable, which equals 1 if the firm’s profit margin from 2011 is above the median profit margin *within* an industry and 0 otherwise.¹⁷ Since the split into high- versus low-margin firms is done separately for each industry, and we include industry–year fixed effects, our DDD approach explores variation in pricing power within a certain sector. This is particularly important as existing literature shows that pricing power is market-specific (Mace, Patel, and Seegert 2020). Consistent with the idea that firms with a lower (higher) profit margin face more (less) elastic demand, they should be less (more) able to pass on the tax-induced costs, resulting in a lower (higher) ρ . In our test, we include a dummy for *High Margin*. Hence, we expect $\beta_2 > 0$.

The results of this test are presented in Table 6, columns 1 and 2. We find a negative and significant coefficient for low-margin firms ($\beta_1 < 0$) and a negative but non-significant coefficient for high-margin firms ($\beta_1 + \beta_2$). Importantly, these two effects are significantly different from each other ($\beta_2 > 0$). This indicates that those firms with lower pricing power (i.e., those with a lower profit margin) indeed seem to bear more of the tax burden relative to firms with more pricing power, as evidenced by greater investment reductions due to the tax.

To address the concern that the within-industry comparison of high- versus low-profit margin firms may capture many other things, we explore an alternative measure of consumer demand elasticity based on firm location. As Spain in general and the autonomous Community

¹⁷The untabulated results show that our results are similar when using the gross margin as the split variable.

of Valenciana in particular¹⁸ is a region driven by tourism, we expect coastal regions to face less elastic demand than non-coastal regions. This is because in popular areas, firms can demand higher prices for the same activity or good (e.g., buying water is more expensive at an airport than in a supermarket, as consumers are more inelastic at airports), and tourists can increase the local purchasing power (see, for instance, Balaguer and Cantavella-Jorda 2002, on the relation between local prices and tourism in Spain). Thus, we expect that firms in tourist or tourist-related sectors along the coast face less elastic demand (ϵ_D). A lower ϵ_D increases the pass-through of taxes (higher ρ). We therefore introduce *Coastal Region* as the split variable that is equal to 1 for regions (defined by the very granular five-digit postal code) located directly at the sea and 0 otherwise. As firms at the coast arguably face less elastic consumer demand, we expect $\beta_2 > 0$. For firms with more elastic consumers (*Coastal Region* = 0), we would expect a decline in investment according to the predictions in Section 2 ($\beta_1 < 0$).

Columns 3 and 4 of Table 6 present the results for coastal versus hinterland regions. For this test, we focus on the accommodation and food service sector, real estate activities, administrative and support service activities (e.g., car rentals), and the education sector because the location near the coast is expected to matter in these sectors. Table 6 shows a negative and significant coefficient for firms located in non-coastal regions ($\beta_1 < 0$) and a negative but non-significant coefficient for firms located in coastal regions ($\beta_1 + \beta_2$ is insignificant). Both coefficients significantly differ from each other, as indicated by the positive β_2 coefficient. This supports the notion that firms with less pricing power, that is, firms facing more elastic consumer demand, bear more of the emission tax burden compared to firms with less elastic consumers. Again, our results are similar when using entropy balancing approach (Table A.2).

¹⁸According to the official Spanish statistical office, Valenciana counts the second most visitors of all Spanish Autonomous Communities, after Andalucía and before the Balearic Islands (INE, 2022).

Table 6: Incidence-Based Explanation, Within-Market Variation of Pricing Power.

This table presents the results of estimating Equation (1). Panel A, columns 1 and 2, shows the interaction of Alicante, Post, and $\text{Alicante} \times \text{Post}$ with the dummy variable High Profit Margin, which equals 1 if the firm has a profit margin above the median and 0 otherwise. Panel A, columns 3 and 4, shows the interaction of Alicante, Post, and $\text{Alicante} \times \text{Post}$ with the dummy variable Coastal Region, which is equal to 1 if the firm is located in a coastal region of Spain (or in very close proximity) and 0 otherwise. We perform both splits in 2011 to avoid the emission tax reform already affecting both variables. We use lagged controls and firm and industry–year fixed effects for all the regressions. The figure shows robust standard errors clustered at the firm level in parentheses. Standard errors (in parentheses) are clustered at the firm level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Breakdown by	Profit Margin		Consumer Purchasing Power	
	(1) Low Margin	(2) High Margin	(3) Non-Coastal Region	(4) Coastal Region
<i>Alicante</i> \times <i>Post</i>	-0.0186*** (0.0042)	-0.0016 (0.0043)	-0.0411** (0.0182)	-0.0038 (0.0142)
Difference [<i>t-stat.</i>]	0.0170*** [2.85]		0.0373* [1.65]	
Controls	Yes		Yes	
Firm FE	Yes		Yes	
Industry–Year FE	Yes		Yes	
Observations	17,116		1,485	
Adj. R ²	0.1425		0.1416	

2.4.2.2. Variation in Financial Flexibility (Relative Flexibility in Factor Markets)

In addition to differences in pricing power, we explore differences in capital demand elasticity as another test for relative elasticity. Conceptually, we based our line of reasoning on the factor market for capital. We use differences in financial flexibility as a proxy for relative elasticity of a firm vis-à-vis its capital suppliers. Intuitively, if firms (demanding capital) are financially flexible, they have more elastic capital demand as they can easily adjust investment plans (see, e.g., Graham 2022, on the role of financial flexibility in investment decisions). Our model suggests that firms with a high level of demand elasticity ϵ_D^K also have a high pass-through rate ρ . Hence, according to Weyl and Fabinger (2013), these firms bear less of the tax burden and cut investments less than financially inflexible firms that have a lower ϵ_D^K .

Since measuring capital demand elasticity in a sample of private firms such as ours is inherently hard, we use two measures capturing financial flexibility. We first use the equity ratio of firms. We argue that firms with high equity ratios have greater financial flexibility than firms with low equity ratios for two reasons. First, they have greater debt capacity, which

enables these firms to increase their debt if necessary. Second, these firms likely have more internally generated funds from retained earnings, resulting in higher equity ratios. This implies that firms with low equity have less financial flexibility and thus also less capital demand elasticity (ϵ_D^K in the factor market is low). In this case, our simple model suggests a larger drop in investments because the lower pass-through results in a greater drop in after-tax profitability.

We then define the split variable from Equation (1) as *High Equity Ratio*, which equals 1 if the firm's equity-to-assets ratio is above the median equity-to-assets ratio within an industry and 0 otherwise. We expect $\beta_2 > 0$, as firms with a higher equity ratio can more flexibly adjust to the emission tax, that is, they have higher values of ϵ_D in the factor market. As an alternative measure, we follow Hadlock and Pierce's (2010) approach by using a firm's size and age to measure financial flexibility. With increasing firm size as well as maturity, firms have increased internal resources as well as better and less costly access to external financing sources, increasing their financial flexibility (Berger and Udell 1998; Hennessy and Whited 2007). Since these firms are more elastic in their capital demand (higher ϵ_D^K), the investment decrease can be expected to be smaller for more mature firms than for newer firms. Thus, we define the split variable *Large & Old* as equal to 1 if a firm is above the bottom quartile in terms of firm size as well as above the median firm age and 0 otherwise. We expect $\beta_2 > 0$ because firms that are larger and older have flexibility to adjust, and we expect small and young firms to bear more of the burden. Hence, we predict that $\beta_1 < 0$.

The results are presented in Table 7. Columns 1 and 2 present the results for the split according to the equity ratio of firms. The coefficient for firms with a low equity ratio (column 1) is negative and significant ($\beta_1 < 0$). For firms with a high equity ratio (column 2), the effect is negative but non-significant. Importantly, both coefficients significantly differ from each other ($\beta_2 > 0$). This indicates that firms with less financial flexibility and that rely more on external resources bear more of the economic burden of the emission tax due to their low capital demand elasticity in the factor market. Columns 3 and 4 show the results for the split according

to firm size and age. The coefficient for small and young firms (column 3) is negative ($\beta_1 < 0$) and significant and, consistent with our prediction, we find a statistically different effect for large and old firms ($\beta_2 > 0$). Consistent with the predictions based on the pass-through rate ρ (Weyl and Fabinger 2013), the findings in Tables 6 and 7 suggest that firms with less financial flexibility seem to bear more of the economic burden of the emission tax than financially flexible firms, as indicated by their greater drop in investments. Again, the results are similar when using an entropy balancing approach (see Table A.2).

Table 7: Within-Market Variation of Capital Demand Elasticity.

This table presents the results of estimating Equation (1). Panel A, columns 1 and 2, shows the interaction of Alicante, Post, and Alicante \times Post with the dummy variable High Equity Ratio, which equals 1 if the firm has an equity ratio above the median and 0 otherwise. Panel A, columns 3 and 4, shows the interaction of Alicante, Post, and Alicante \times Post with the dummy variable Large & Old, which is equal to 1 if the firm has a combined firm size and age above the median and 0 otherwise. We perform both splits in 2011 to avoid the emission tax reform already affecting both variables. We use lagged controls and firm and industry-year fixed effects for all the regressions. Robust standard errors clustered at the firm level are shown in parentheses. Standard errors (in parentheses) are clustered at the firm level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Equity Ratio		Firm Size and Age	
	(1) Low Ratio	(2) High Ratio	(3) Small & Young Firm	(4) Large & Old Firm
<i>Alicante</i> \times <i>Post</i>	-0.0177*** (0.0045)	-0.0032 (0.0039)	-0.0156*** (0.0043)	-0.0044 (0.0045)
Difference [<i>t-stat.</i>]	0.0145** [2.50]		0.0111* [1.83]	
Controls	Yes		Yes	
Firm FE	Yes		Yes	
Industry-Year FE	Yes		Yes	
Observations	17,116		15,821	
Adj. R ²	0.1424		0.1376	

2.4.2.3. Combining Market Structure and Relative Elasticities

In the next step, we combine the test exploring the market structure from Section 4.1 and the previous tests considering within-industry differences in relative elasticities. Specifically, we combine *High HHI* with the respective pricing power (*High Margin*) or financial flexibility split variable (*High Equity Ratio* or *Large & Old*) and used this interaction (for example, *High HHI & High Margin*) as our split variable in our DDD model.¹⁹ The results are shown in Table

¹⁹Since the test using *Coastal Region* uses only few industries, we cannot combine *HHI* and *Coastal Region*.

8. Column 1 shows the results for the interaction between *High HHI* and *High Profit Margin*. Column 2 (3) shows *High HHI & High Equity (Old & Large)* as the split variable. Consistent with the earlier results, we found non-significant main effects in all columns. This suggests that when the market is less concentrated or firms have low demand elasticity and/or high supply elasticity, the emission tax has no identifiable effect on a firm's investment. If, however, the market is concentrated (*High HHI* = 1) and the firm has high (consumer or capital) demand elasticity (i.e., *High Margin*, *High Equity Ratio*, or *Old & Large* = 0), then the emission tax introduction has a significant negative effect on corporate investment. This is indicated by the positive triple interaction terms in all three columns. This resonates again with the earlier results and supports the notion that when firms are relatively inelastic, they are more likely to bear the emission tax, as suggested by reduced investments.

Table 8: Combining Market Structure and Relative Elasticities.

This table presents the extension of Equation (1) by interacting HHI with the measures for pricing power and capital demand elasticity. Column 1 shows the interaction of *Alicante*, *Post*, and *Alicante × Post* with *High HHI* and *High Margin*. Column 2 shows the interaction of *High HHI* with *High Equity Ratio* and column 3 shows *High HHI* with *Large & Old*. The definitions of all dummy variables are the same as in the above analysis. We perform all splits in 2011 to avoid the emission tax reform already affecting both variables. We use lagged controls and firm and industry-year fixed effects for all the regressions. Robust standard errors clustered at the firm level are shown in parentheses. Standard errors (in parentheses) are clustered at the firm level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1) Profit Margin	(2) Equity Ratio	(3) Firm Size and Age
<i>Alicante × Post</i>	-0.0149*** (0.0034)	-0.0162*** (0.0038)	-0.0127*** (0.0036)
<i>Alicante × Post × High HHI & High Margin</i>	0.0226*** (0.0086)		
<i>Alicante × Post × High HHI & High Equity Ratio</i>		0.0197*** (0.0064)	
<i>Alicante × Post × High HHI & Large & Old</i>			0.0076 (0.0069)
<i>Joint Significance</i> [<i>t-stat.</i>]	0.0077 [0.96]	0.0035 [0.66]	-0.0050 [0.60]
Controls & FE	Yes	Yes	Yes
Observations	15,821	15,821	15,821
Adj. R ²	0.1379	0.1379	0.1376

2.4.3. *Role of Supply Chain Linkages*

One limitation of this analysis of relative elasticity via profit margins is that it lacks information on supply chain linkages between firms. The notion that firms with higher pricing power bear less of the burden of the emission tax implies that dirty, high pricing power firms can pass the cost on within their supply chain, either to their consumers or to their suppliers. Unfortunately, data on firm-level supply chain linkages are not available for our sample firms. However, this information is available at the industry level. Thus, in an additional step, we combine our analysis of high- versus low-margin firms with information on supply chain linkages and industry-specific emissions. The general idea of this test is straightforward. In a clean supply chain, investments of high versus low profit margin firms should not differ because there is little to no tax to be passed on along the supply chain. In contrast, in a dirty supply chain, where it is more likely that one or more of the firms pay the emission tax, investments of low-margin firms should respond to the reform, whereas high profit margin firms should show a lower investment response, consistent with our predictions.

To test this notion, we merge two additional datasets with our data. First, we use input–output tables on industrial linkages for Spain (Eurostat 2022). For each of the 62 industries, we define the top supplier and the top customer. Next, we define industry-level emissions using satellite data from Ebertseder et al. (2022). Specifically, we define an industry as having *High Emissions* if the individual industry’s emissions are above the median of total emissions.²⁰ We then define suppliers and customers based on *High Emissions* as a clean or dirty supplier and customer, respectively. Likewise, we use *High Emissions* to define the firm as clean versus dirty. Using these three characteristics (firm, supplier, and customer), we then create a dummy variable, *Dirty Supply Chain*, that is equal to 1 for firms if two or more of the three supply chain links (customer, firm, and supplier) are dirty and 0 otherwise. Thus, a supply chain is clean if

²⁰In more detail, we define firms with the NACE codes F, G45, G46, G47, I, L and M69 to be “dirty”.

one or none of the three involved industries is dirty. We then fully interact *Alicante*, *Post*, *Dirty Supply Chain*, and *High Margin* to arrive at the following estimation equation:

$$\begin{aligned}
Inv_{i,t} = & \alpha_0 + \beta_1 \times Alicante_i \times Post_t + \beta_2 \times High\ Margin_i \times Post_t + \beta_3 \times Dirty\ Supply\ Chain_i \quad (3) \\
& \times Post_t + \beta_4 \times High\ Margin_i \times Dirty\ Supply\ Chain_i \times Post_t + \beta_5 \times High\ Margin_i \\
& \times Alicante_i + \beta_6 \times Dirty\ Supply\ Chain_i \times Alicante_i + \beta_7 \times High\ Margin_i \times Dirty \\
& Supply\ Chain_i \times Alicante_i + \beta_8 \times High\ Margin_i \times Alicante_i \times Post_t + \beta_9 \times Dirty \\
& Supply\ Chain_i \times Alicante_i \times Post_t + \beta_{10} \times High\ Margin_i \times Dirty\ Supply\ Chain_i \times \\
& Alicante_i \times Post_t + \gamma X_{i,j,t-1} + \alpha_i + \alpha_t + \varepsilon_{i,t},
\end{aligned}$$

where the dependent variable, controls, and fixed effects are defined as above. The main effects of the variables are absorbed by either firm fixed effects or year fixed effects. Based on these interactions, we are then able to present a two-by-two matrix where we split the effect according to (1) dirty versus clean supply chains and (2) high versus low margins. The main coefficients of interest are the differences between high versus low profit margin firms in clean supply chain (captured by β_8) and in dirty supply chains (captured by $\beta_8 + \beta_{10}$). Since in cleaner supply chains, profit margins are not expected to matter, β_8 should be 0. In contrast, the difference between low and high profit margin firms is supposed to be significant in the case of a dirty supply chain ($\beta_8 + \beta_{10} > 0$). Moreover, we expect that when comparing low profit margin firms, only firms in a dirty supply chain should reduce their investments ($\beta_9 < 0$).

The results of estimating Equation (3) are shown in Figure 5. The results are consistent with the notion that only if there is a potential tax burden within the supply chain *and* if firms face more elastic consumers (i.e., a firm has low margins) is the emission tax a burden to the firm and results in a reduction in investments. Specifically, the sum of β_2 and β_9 , which captures the investment response of low-margin firms in a dirty supply chain, is negative. In all other cases, there is no significant investment response, either because there is no substantial tax burden to be shared in the supply chain (in the case of low- and high-margin firms in a clean supply chain) or because the firm faces sufficiently inelastic demand to pass on the tax to their stakeholders (in the case of high-margin firms in a dirty supply chain). Importantly, we find significant and non-significant differences across groups when expected. First, the difference between dirty

and clean supply chains is significant for low-margin firms ($\beta_9 < 0$) but not for high-margin firms ($\beta_9 + \beta_{10}$ is indistinguishable from zero). Second, the difference between high- and low-margin firms (β_8) is non-significant in the case of clean supply chains, but it is highly significant for dirty supply chains ($\beta_8 + \beta_{10} > 0$). Finally, the quadruple difference β_{10} —capturing the difference in the response of dirty versus clean and high- versus low-margin firms—is also positive and significant. Overall, these findings are consistent with the notion that the emission tax is being passed on within the supply chain. Arguably, the key caveat of this test is that we only use a rough proxy via industry-level input–output tables instead of (for us not observable) firm-specific information on suppliers and customers.

	Clean Supply Chain	Dirty Supply Chain	Difference Between Clean and Dirty Supply Chain
Low Margin	β_2 -0.0082 (0.0058) N = 7,839	$\beta_2 + \beta_9$ -0.0210*** (0.0035) N = 12,478	β_9 -0.0128* [1.90]
High Margin	$\beta_2 + \beta_8$ -0.0036 (0.0066) N = 7,810	$\beta_2 + \beta_8 + \beta_9 + \beta_{10}$ -0.0012 (0.0044) N = 14,401	$\beta_9 + \beta_{10}$ 0.0024 [0.31]
Difference Between High and Low Margin	β_8 0.0046 [0.49]	$\beta_9 + \beta_{10}$ 0.0198*** [3.52]	β_{10} 0.0153* [1.75]

Figure 5: Clean Versus Dirty Supply Chain & Low- Versus High-Margin Firms.

This figure presents the results of the investment response to the emission tax in Alicante based on Equation (3). Standard errors (in parentheses) are clustered at the industry level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. In addition, the number of observations in each combination are shown.

2.4.4. *Passing on the Emission Tax Burden to Employees*

While the above tests focus on what type of firm bears the economic burden of the tax, in the next step, we expand our analysis and consider other stakeholders. While taxes can also be passed on to consumers (Baker et al. 2021; Jacob et al. 2022), we focus on taxes being passed on to employees (see, e.g., Fuest et al. 2018; Dyreng et al. 2022). This is because we lack data on consumer or input prices for our sample firms to test the pass-through to consumers or

suppliers. To further link our tests to factor markets, specifically to the labor market, we use information from Amadeus on the number of employees and on wages to assess whether employees bear part of the emission tax in the form of reduced labor demand or in the form of lower wages. Empirically, we use the number of employees, *# of Employees* (defined as the natural logarithm of the number of employees), and *Employee Wages*²¹ as our dependent variable. We then estimate the DiD model from Equation (2) that compares Alicante (treatment group) to Murcia (control group). The results are presented in Table 9. We find that the number of employees is not affected by the emission tax (column 1), suggesting that no employee is fired because of the emission tax. However, we find a significant, 1.4% reduction in average wages (column 2). Thus, while the number of employees remains constant, the employees seem to bear part of the emission tax burden through lower wages.

To further tie the investment response to the notion that firms can pass on the cost of the emission tax to other stakeholders such as employees, we explore differences in labor supply elasticity following the approach by Dyreng et al. (2022) as another example of relative elasticity in the factor market. Specifically, they use differences in labor skill as a proxy for labor supply elasticity. Low- (high-) skilled employees exhibit low (high) supply elasticity ϵ_S^L . The existing literature shows that less (more) elastic workers, that is, low- (high-) skilled employees, bear more (less) of the corporate tax burden (Fuest et al. 2018; Dyreng et al. 2022). This is in line with our theoretical predictions. Firms may be able to pass on the emission tax to employees if the workers are low skilled (low ϵ_S^L), thereby muting the investment response. In contrast, if a firm has highly skilled workers (high ϵ_S^L), the firm is less able to pass on the tax to employees. Hence, the firm bears more of the tax burden and, thus, reduces investments.

Since firm-level information on labor skill is not available, we use industry-level skill data for Spain from Eurostat. For each industry, we calculate the share of workers with a low skill

²¹The average wage is defined as total staff expenses divided by the number of employees. We require at least five employees for this test.

level (defined as the percent of “low status employees” of the total workforce, using the Eurostat data and definition). We then define an indicator variable *Low Skill*, which equals 1 if the percentage of low-skill workers in an industry is above the bottom quartile in 2011 and 0 otherwise. We then use *Low Skill* as the split variable in Equation (1). The results are reported in columns 3 and 4 of Table 9. Consistent with our prediction, we find that firms with higher skilled labor are more impacted than firms in industries with more low-skilled workers. While the overall effect is negative and significant for firms in industries with more high-skilled workers ($\beta_1 < 0$), the effect is also negative and significant for firms in industries with more low-skilled labor ($\beta_1 + \beta_2 < 0$). More importantly, the effect in the case of highly skilled workers is substantially greater ($\beta_2 < 0$). These results are robust to using entropy balancing matching, as shown in Table A.2. These results suggest that workers bear part of the emission tax burden.

Table 9: Incidence-Based Explanation, Passing on the Burden to Employees.

This table presents the results of estimating Equation (2) in columns 1 and 2 and Equation (1) in columns 3 and 4. In columns 1 and 2, the number of employees and the average wage are used as the dependent variable, respectively. Columns 3 and 4 show the interaction of *Alicante*, *Post*, and *Alicante* \times *Post* with the dummy variable *Low Skill*, which is equal to 1 if the firm operates in an industry with an above-bottom-quartile ratio of low-skilled workers in an industry and 0 otherwise. We perform the split in 2011. We use lagged controls and firm and industry–year fixed effects for all the regressions. Robust standard errors clustered at the firm level are shown in parentheses. Standard errors (in parentheses) are clustered at the firm level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable	# of Employees	Wage Growth	Investment	
Breakdown by:	–	–	Labor Skills	
	(1)	(2)	(3)	(4)
			Low Skill	High Skill
<i>Treatment</i> \times <i>Post</i>	0.0080 (0.0140)	-0.0144* (0.0082)	-0.0084*** (0.0032)	-0.0440*** (0.0150)
Difference [<i>t-stat.</i>]	–	–	-0.0356** [2.32]	
Controls	Yes	Yes	Yes	
Firm FE	Yes	Yes	Yes	
Industry–Year FE	Yes	Yes	Yes	
Observations	13,179	12,962	15,821	
Adj. R ²	0.9125	0.7714	0.1379	

2.4.5. *Supplemental Test: Assessing the Polluter Pays Principle*

Finally, we test for the polluter pays principle as envisioned by policy makers. We estimate Equation (1) using *High Emissions* as our split variable for NOx. When basing the split on SOx, *High Emissions* is defined as equal to 1 if a firm operates in an industry with emissions of SOx (scaled by aggregate sales) and 0 otherwise, using data from Eurostat (2020).²² We use 2011 emissions to avoid the emission tax reform affecting the emission levels. Low-emission industries are, for example, real estate activities or the IT sector, whereas high-emission industries involve, for example, agriculture or transportation and storage activities.²³

The polluter pays principle suggests that $\beta_2 < 0$, that is, *High Emissions* firms respond more than their cleaner counterparts. The results are presented in Table 10. We find a significant negative impact for low-SOx-emitting and low-NOx-emitting firms (β_1) and a significant negative effect only for high-NOx-emitting firms ($\beta_1 + \beta_2$). For firms in high-SOx-emitting industries, the overall effect $\beta_1 + \beta_2$ is negative but borderline non-significant (t -stat. = 1.66). This contrasts with the polluter pays principle, as indicated by the statistically non-significant DDD estimate β_2 .²⁴ These findings corroborate our earlier findings that economic factors and not the actual tax payment should explain which firms respond to an emission tax.

²²These are the most granular SOx data available (emissions per industry) because alternative emission databases, such as the Emissions Database for Global Atmospheric Research of the European Commission, possess only emission data at the industry code and country levels (not at the two-digit postal code or other regional levels).

²³Descriptive statistics for low- versus high-emitting firms and number of observations by industry are untabulated. We note that, while there are differences in observable characteristics between the high- and low-emission groups (and for the other split variables), these differences do not affect our results.

²⁴We also use alternative splits (e.g., at the 66th, 75th, and 90th percentiles) or apply the formula used to calculate the tax base ($1.5 \cdot \text{NOx} + \text{SOx}$) as a basis for the split. The results remain unchanged (untabulated test).

Table 10: Polluter Pays Principle, SOx and NOx Emissions.

This table presents the results of estimating Equation (1). Panel A shows the interaction of Alicante, Post, and Alicante \times Post with the dummy variable High Emission, which equals 1 if the firm operates in an industry with SOx or NOx emissions above the industry median and 0 otherwise. For NOx, we used the satellite data provided by Ebertseder et al. (2022). For SOx, we use the aggregate data provided by Eurostat (2020). We execute this split in 2011 to avoid the emission tax reform already affecting these emission levels. We use lagged controls and firm and industry–year fixed effects for all the regressions. Robust standard errors clustered at the firm level are shown in parentheses. Standard errors (in parentheses) are clustered at the industry level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Breakdown by	Industry SOx Emission		Industry NOx Emission	
	Low (1)	High (2)	Low (3)	High (4)
<i>Average Emission (tons scaled by sales)</i>	15,732	911,125	295,563	2,052,054
<i>Coefficient from Equation (2)</i>	β_1	$\beta_1 + \beta_2$	β_1	$\beta_1 + \beta_2$
<i>Alicante \times Post</i>	-0.0113*** (0.0038)	-0.0079 (0.0051)	-0.0071 (0.0047)	-0.0126*** (0.0041)
<i>Difference [t-stat.]</i>		0.0034 [0.54]		-0.0055 [-0.90]
Controls		Yes		Yes
Firm FE		Yes		Yes
Industry–Year FE		Yes		Yes
Observations		17,116		17,116
Adj. R ²		0.1420		0.1420

To further corroborate our main finding that market characteristics determine the investment response, we follow Equation (3) but use *High Emission* instead of the supply chain linkage measure. Based on the interactions, we again split the effect according to high versus low emissions and high versus low margins. Figure 6 shows the results of this analysis for *High Emission* and *High Margin*. The coefficients for the low-margin firms—irrespective of whether they are in the low- or high-emission group—are significant and negative. The investment effect for high-margin firms is non-significant in the low- as well as in the high-emission group. The difference between low- and high-margin firms is significant, irrespective of the emission levels, whereas the difference between the high- and low-emission groups is always non-significant. These results support the predictions of the general notion of incidence. For tax incidence, it does not matter who pays the tax (Jenkin 1872). What matters is who is more versus less able to pass on taxes, which is a function of the relative elasticity of supply versus demand (e.g., Weyl and Fabinger 2013). The untabulated test shows that combinations of the other split variables with emissions yield numerically similar results.

	Low SOx Emission	High SOx Emission	Difference Between <i>High and Low Emission</i>
Low Margin	β_2 -0.0208*** (0.0049) N = 4,665	$\beta_2 + \beta_8$ -0.0155** (0.0060) N = 3,768	β_8 0.0053 [0.69]
High Margin	$\beta_2 + \beta_9$ -0.0024 (0.0042) N = 4,910	$\beta_2 + \beta_8 + \beta_9 + \beta_{10}$ -0.0001 (0.0069) N = 3,773	$\beta_8 + \beta_{10}$ 0.0023 [0.29]
Difference Between <i>High and Low Margin</i>	β_9 0.0184*** [3.11]	$\beta_9 + \beta_{10}$ 0.0154** [2.01]	β_{10} -0.0030 [-0.31]

Figure 6: High- Versus Low-Emission Firms & Low- versus High-Margin Firms.

This figure presents the results of the investment response to the emission tax in Alicante. We run Equation (3) but use High Emission instead of the supply chain measure. Standard errors (in parentheses) are clustered at the industry level in Panel B. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. This figure also shows the number of observations in each combination of high versus low emissions and high versus low margins, respectively.

Overall, these results suggest that the effect of the emission tax does not depend on which firm emits SOx or NOx but, rather, on which firm bears the economic burden of the tax (firms facing high elastic demand, firms with low capital demand elasticity, or firms facing more elastic labor supply). Thus, we show that a possible consequence of emission taxes is that those firms that are the least able to adjust and cannot shield themselves from bearing the emission tax burden are made to pay (in terms of reducing their investment).

2.5. Conclusion

This paper examines who bears the economic burden of emission taxes using the introduction of a local emission tax in Spain in 2013. We explore investment responses to examine which firms respond to an emission tax. Our results show that the investment response is stronger when firms are more likely to bear the burden of the emission tax, that is, when firms face highly elastic demand, when they have low financial flexibility (i.e., low capital demand elasticity), and when they face high labor supply elasticity. We also find some evidence that part of the emission tax is passed on to employees in the form of reduced wages. In summary,

we find that firms that are likely to bear the economic burden of the emission tax cut investment, irrespective of their actual SO_x or NO_x emissions.

Our findings have implications for the debate on who ultimately pays for climate change policies. While environmental taxes have become an increasingly important part of the policy toolkit, especially in finance climate initiatives, it is important to understand which firms' investment responds to emission taxes. Generally targeting environmentally harmful behavior, environmental taxes can have a distorting effect on corporate investment and, thereby, economic growth. Importantly, our findings suggest that it may not be the firms in dirty industries that respond to environmental taxes, particularly if they can pass on the burden of the tax to their customer and supplier networks. While environmental taxes are often designed under the polluter pays principle, in the end, it may be that not only the polluter pays. It may also be that customers, suppliers, employees, and stakeholders in industries that do not directly generate emissions and pollution pay the price of emission taxes. This may explain why emission reductions or air quality improvements due to emission taxes can be rather modest (e.g., Ebersteder et al. 2022; Metcalf and Stock 2022; Pretis 2022). Hence, policy makers may need to turn to other policy tools in their toolkit to target polluters.

We acknowledge that our analysis has several limitations. While we explore the short-term effect on fixed asset investment, we do not perform a full welfare analysis. Future research could explore possible long-term effects, potentially slow-moving technological advancements, impacts on other asset classes, as well as the effect on wages. In addition, future research could consider the impact on consumer prices to better understand the extent to which households might bear the burden of environmental taxes.

3. Carbon Taxation and Corporate Investment²⁵

3.1. Introduction

Carbon (CO₂) taxes have been on the rise in the past decades and have become an essential part of the policy toolkit to tackle climate change. While only Finland and Poland had a carbon tax in 1990, until 2022, 47 countries adopted carbon pricing initiatives such as carbon taxation (Worldbank 2022). The economic impact of carbon taxes, however, remains disputed. While part of the literature finds regressive welfare implications of carbon taxes (Wesseh Jr. et al. 2017; Poterba 1991), others observe only relatively weak effects if implemented under certain conditions (Metcalf and Stock 2022; Goulder et al. 2019; Bovenberg and de Mooij 1997). However, given the focus on aggregate effects, little is known about the impact on firms and their investments. This is surprising given that firms engage the most in environmentally harmful behavior (UNEP 2010) and are the main actor to reach states' environmental targets (Burchardt et al. 2021).

We examine the impact of carbon taxes on corporate investment. Using data on private European firms and exploring the introduction of CO₂ taxes in France in 2014 and in Ireland in 2010 in a stacked difference-in-difference (DiD) design, we find that CO₂ taxes reduce investments by about 1% of total assets. More importantly, we find that it is not the polluters who respond the most but those firms that are the weakest to adjust.

We contribute to the literature on environmental taxes and corporate investments. At the macro level, Metcalf and Stock (2022) show a zero to modest positive impact of carbon taxes on GDP growth. Brown et al. (2022) document increased R&D activity when emission taxes increase. In contrast, we provide evidence that firm-level capital investment declines, in line with the macro-level evidence of Känzig (2022) of a downsized economy following tighter

²⁵Joint work together with Martin Jacob with the title: "Carbon Taxes and Corporate Investment". This paper is a generalization of the paper "Who Bears the Emission Tax Burden? Evidence from Capital Investments" in a European setting. The paper targets a length of 2,000 words only as it is tailored for submission to the journal "Economic Letters".

carbon pricing regimes. We thus generalize firm-level findings by Jacob and Zerwer (2022) by considering CO2 taxes.

3.2. Estimation approach

We use CO2 taxes in Europe collected from the World Bank following Metcalf and Stock (2022). However, their data have two limitations. First, the rates are not comparable across countries since different or selective sectors are covered. Second, due to the dynamic approach to data handling at the World Bank, changes in rates may reflect data improvements. We thus focus on the introduction of a CO2 tax in a stacked DiD design (Goodman-Bacon 2021; Baker et al. 2022). We use the introduction of CO2 taxes in France (2014) and Ireland (2010). Both events comprise the introduction of a CO2 tax by introducing rates of 7 €/t (France) and 15 €/t (Ireland). The control group comprises firms in European countries with no CO2 tax change during our sample period. We use France and Ireland for two reasons. First, while a CO2 tax was introduced (changed) in Iceland in 2010 (in Switzerland in 2014 and 2016), our firm data lacks coverage for these two countries. Second, we exclude the CO2 taxes introductions in the United Kingdom in 2013 and in Portugal in 2015, since both countries changed corporate tax rates around the same years.

Our empirical design is a stacked DiD design leveraging the two CO2 tax introductions in France and Ireland in the following equation:

$$Inv_{i,t,e} = \alpha_0 + \beta_1 Treatment_{i,e} \times Post_{t,e} + \gamma X_{i,t-1,e} + \delta Z_{j,t,e} + \alpha_{ind,t,e} + \alpha_{i,e} + \varepsilon_{i,t} \quad (4)$$

The dependent variable, $Inv_{i,t,e}$, is the gross investment of firm i in year t around event e . Our main variable of interest is $Treatment \times Post$. $Treatment$ equals one if the firm is from France (Ireland) in the case of the French (Irish) reform, and zero otherwise. $Post$ denotes the respective post-reform period (2014–2016 for France, 2010–2012 for Ireland). We include several lagged control variables ($X_{i,t-1,e}$), namely, *Leverage*, *Return-on-Assets*, *SalesGrowth*, *Size*, and *Cash*. We also include country-level controls, namely, corporate taxes, payout taxes,

the VAT rate, two governance indicators, inflation, the unemployment rate, the natural logarithm of the GDP, and GDP growth following prior literature (e.g., Jacob et al. 2019). We include event-specific industry–year fixed effects and event-specific firm fixed effects. We cluster standard errors at the event-specific country–industry level.

We use all available data on unconsolidated financial statements of private unlisted corporations from the Amadeus database over the period 2007–2019. We focus on standalone firms that do not belong to a domestic or multinational group. We require firms to have existed for at least two years. We exclude companies with total (fixed) assets below €50,000 (€5,000) and that have negative sales, total assets, or cash (see, also Jacob and Zerwer 2022). Country-level (tax rate) data are from the World Bank (Jacob et al. 2019). Table 11 reports the statistics for the variables of our stacked regression sample.

Table 11: Descriptive Statistics.

This table presents descriptive statistics of our main variables for 1,369,244 observations from 2007 to 2017.

Variable	Mean	St. Dev.	Median
<i>Gross Investment</i>	0.0479	0.1292	0.0465
<i>Net Investment</i>	0.0088	0.1247	0.0104
$\Delta \ln(K)$	0.0000	0.4822	0.0586
<i>Leverage</i>	0.6516	0.3104	0.8763
<i>Return on Assets</i>	0.0763	0.1594	0.1066
<i>Sales Growth</i>	0.0385	0.5087	0.1535
<i>Firm Size</i>	13.2291	1.5514	14.3138
<i>Cash</i>	0.1383	0.1803	0.1993
<i>Corporate Tax</i>	0.2894	0.0646	0.3129
<i>Payout Tax</i>	0.2176	0.0651	0.2358
<i>VAT</i>	0.2107	0.0101	0.2200
<i>Regulatory Quality</i>	0.8321	0.2337	0.9605
<i>Rule of Law</i>	0.5872	0.4552	0.68356
<i>Inflation</i>	1.2093	0.7257	1.38012
<i>Unemployment</i>	10.6185	1.8968	12.1500
<i>GDP (ln)</i>	10.2981	0.4345	10.4911
<i>GDP Growth</i>	0.3009	1.8766	0.9520

3.3. Results

Figure 7 presents a time trend of the investment difference between the treated group and the control group around a CO2 tax introduction. Supporting the parallel trends assumption, we find that investments of treated versus control groups follow parallel trends prior to the CO2 tax introduction. The difference between treated and control groups decreases with the introduction of the CO2 tax and remains at this lower level.

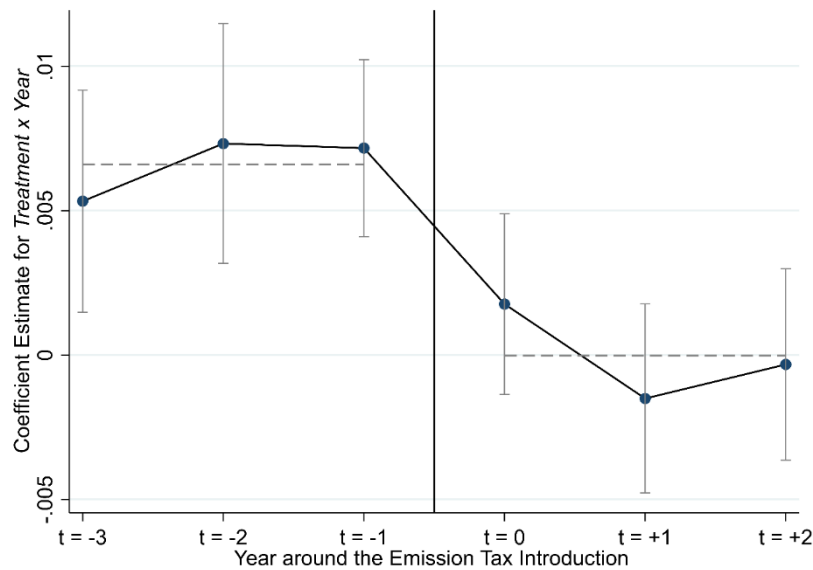


Figure 7: Investment Trends.

This figure presents the differences in investment between the treated group and the control group along with 95% confidence bounds over time.

Table 12 reports the results of the stacked DiD. In columns 1 and 2, we estimate Equation (4) with and without controls and fixed effects. We find a significant decline in corporate investment following CO2 tax introductions. In economic terms, the introduction of CO2 taxes reduces investment by about 1% of total assets (see column 2). We test the robustness using the alternative dependent variables of net investment (column 3), change in fixed assets (column 4) and WLS where we give more weight to larger firms (column 5). The coefficient of the interaction of *Treatment* and *Post* remains always negative and significant.

Table 12: Emission Taxes and Investment.

This table shows the results of the stacked DiD regression. We report clustered standard errors at the country–industry level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Main Results					
	(1)	(2)	(3)	(4)	(5)
	Baseline Approach		Robustness Tests		
Dependent Variable	<i>Gross Investment</i>	<i>Gross Investment</i>	<i>Net Investment</i>	Δ <i>Fixed Assets</i>	<i>WLS</i>
<i>Treatment</i> \times <i>Post</i>	-0.0059*** (0.0013)	-0.0103*** (0.0020)	-0.0155*** (0.0018)	-0.0523*** (0.0047)	-0.0088*** (0.0022)
Controls	No	Yes	Yes	Yes	Yes
FE	No	Yes	Yes	Yes	Yes
Observations	1,369,244	1,369,244	1,369,244	1,357,945	1,361,574
Adj. R ²	0.0052	0.0387	0.0157	0.0163	0.0598

We also test the polluter pays principle (dirty firms react more strongly) or the incidence-based explanation (firms that cannot adjust as easily react more; Jacob and Zerwer 2022). To test this, we extend Equation (4) to a DDD model. We assess the polluter pays principle through a dummy variable *High CO2* that equals one if the firm operates in an industry with high CO2 emissions based on aggregate CO2 emissions scaled by aggregate sales (Eurostat 2022), and zero otherwise. We perform the split in the year prior to the respective CO2 tax introduction. The results where *High CO2* is interacted with the DD terms are reported in Panel A of Table 13. We find that firms in low–CO2-emitting *and* in high–CO2-emitting industries reduce their investments. Both coefficients are similar in size, negative, and significant. Moreover, the DDD coefficient is nonsignificant. Thus, it appears that the investment response is not related to the CO2 emissions.

Next, we thus examine whether firms than cannot easily adjust end up bearing the burden of the tax. Specifically, we explore differences in profit margins and in the availability of internal funds in a DDD design. The indicator variables *High Margin* and *High Equity* equal one if firms have a profit margin or equity ratio above the median, respectively, and zero otherwise. We split the sample in the year prior to the respective reform. Panel B of Table 13 shows the results. In this case, we find a negative investment response for low–profit margin firms and low–equity ratio firms, as well as for high–profit margin and high–equity ratio firms. Importantly, the effect

is always significantly more negative for low-margin or low-equity firms, as indicated by the significant DDD coefficient. This indicates that firms with low margins and equity ratio respond most to CO2 taxes by cutting investments.

Table 13: Heterogeneity Analysis.

This table shows the results of the heterogeneity analysis. We report clustered standard errors at the country–industry level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A				
Breakdown by	Industry CO2 Emissions			
	Low (1)		High (2)	
<i>Treatment</i>	-0.0098***		-0.0105***	
<i>×Post</i>	(0.0022)		(0.0023)	
<i>Difference High vs. Low</i>		-0.0007		
<i>[t-stat.]</i>		[-0.35]		
Controls & FE		Yes		
Observations		1,361,967		
Adj. R ²		0.0388		
Panel B				
Breakdown by	Profit Margin		Equity Ratio	
	Low (1)	High (2)	Low (3)	High (4)
<i>Treatment</i>	-0.0117***	-0.0084***	-0.0145***	-0.0062***
<i>×Post</i>	(0.0021)	(0.0022)	(0.0025)	(0.0021)
<i>Difference High vs. Low</i>		0.0032**		0.0083***
<i>[t-stat.]</i>		[2.44]		[3.45]
Controls & FE		Yes		Yes
Observations		1,361,967		1,361,967
Adj. R ²		0.0390		0.0398

3.4. Concluding Remarks

We contribute to the discussion of CO2-tax effects by showing micro-level evidence of a negative economic impact on investment levels. We find that the introduction of carbon taxes decreases investments, particularly in firms that cannot adjust as easily. This has policy implications. While policymakers often design emission policies to make those pay that create the negative externality (the polluters), using emission taxes without complementing policy measures may not be the first best option to achieve this target.

4. How Effective are Emission Taxes in Reducing Air Pollution?²⁶

4.1. Introduction

Rising sea levels, more frequent and intense climate-induced extreme events, and environmental damage show that there is a price to be paid for the emission of greenhouse gases such as carbon dioxide (CO₂) and methane (IPCC, 2022). While the debate about CO₂ is omnipresent, other anthropogenic emissions such as nitrogen oxides (NO_x) are less discussed in public despite their potential harm. NO_x are produced by combustion, e.g. in industrial production processes. For this reason, NO_x and CO₂ emissions are strongly linked and NO_x are a robust proxy for combustion CO₂ (Reuter et al., 2019; Liu et al., 2020; Hakkarainen et al., 2021). In addition to several negative health effects due to NO_x emissions such as coughing, wheezing, asthma, and other respiratory infections (EPA, 2022a), environmental effects can be especially significant when NO_x levels are high. NO_x interact with water, oxygen, and other chemicals in the atmosphere and can lead to acid rain (EPA, 2022a). This harms sensitive ecosystems such as lakes and forests and contributes to nutrient pollution in coastal waters. As a precursor of ozone, NO_x also play a key role in greenhouse gas formation (IPCC, 2022).

While the international community was able to agree on limiting global warming and stopping environmental damage caused by greenhouse gases and air pollutants, the path to achieving this goal, especially the distribution of associated costs and efforts for necessary action measures, is unclear. One frequently discussed path toward reduced emissions is emission taxes, as “their principal rationale is that they are generally an effective tool for meeting domestic emission mitigation commitments” and they “provide a clear incentive for redirecting energy investment toward low-carbon technologies” (IMF, 2019). As NO_x emissions can be primarily attributed to combustion processes and a significant portion of these

²⁶Joint work together with Thilo Erbertseder, Martin Jacob and Hannes Taubenboeck with the title: “How Effective are Emission Taxes in Reducing Air Pollution?”. The paper has been presented at the IESE Business School of the University of Navarra, OMG Transatlantic Tax Talks and the 2022 TRR 266 Annual of Conference as well as several Brown Bag Seminars.

result from industrial production, firms are often seen as drivers of innovation in clean technology to curb emissions (e.g., Krass et al., 2013, or Brown et al., 2022). Hence, taxing polluting firms seems to be a viable option for policymakers given that emission taxes are endorsed by the European Commission and the United Nations. However, empirical evidence on the effectiveness of emission taxes in reducing emissions is scarce.

In this paper, we examine how emission taxes on industrial NO_x pollution affect levels of NO₂, the most common NO_x form. Understanding whether emission taxes are effective in achieving this goal is important since (1) environmental and health damage due to NO_x can be severe, (2) there is little empirical evidence on the role of emission taxes in reducing NO₂-related air pollution, and (3) empirical findings can inform the policy debate. In theory, the effect of emission taxes on emission levels appears straightforward. With an emission tax, firms face a new cost directly related to their emission output, which should reduce emission levels (Rafaty et al., 2020). This is a standard response that considers Pigouvian pollution pricing and an adjustment of the market failure arising from pollution (Metcalf, 2019). However, since the price of emission taxes may be passed on by “dirty” firms to “clean” firms, as evidenced by “clean” firms cutting investments as much as “dirty” firms in response to an emission tax (Jacob and Zerwer, 2022), the effectiveness of an emission tax in curbing emissions is unclear *ex ante*. That is, while a decrease in emissions following the introduction of a respective tax seems likely, it is unclear to what extent emissions are cut and when they are cut more or less.

Prior research on emission levels and their relationship with emission taxes faces several data-related limitations. Maybe for that reason, earlier studies use analytical models (e.g., Goto, 1995, Nakata and Lamont, 2001, Wissema and Dellink, 2007, Lu et al., 2010). Later empirical studies use sector-level or more aggregated country-level emission data (e.g., Davis and Kilian, 2011, Lin and Li, 2011 Bayer and Aklin, 2020, Best et al., 2020, Metcalf and Stock, 2022, Pretis, 2022). As a result, there is large variation in the estimated effects, ranging from zero aggregate effects (e.g., Pretis, 2022) to very large effects for certain sectors or plants

(Andersson, 2019, Rafaty et al., 2020). While being informative about single firms, plant-level data cannot capture other sources of overall emissions (e.g., commercial traffic) and the average emissions into the air. Both are, however, important to inform the policy debate. For this reason, we examine whether and to what extent emission taxes can curb NO₂ pollution.

In this paper, we overcome key empirical challenges and examine emission levels by using granular air pollution data and a tax reform in a Spanish autonomous community. First, we use satellite data from the Ozone Monitoring Instrument (OMI) (Levelt et al., 2006) over the period 2009–2016 that allow us to measure the area-wide NO₂ burden at the very local level. We use data on yearly average tropospheric NO₂ column densities on an equidistant grid layered on Spain of 0.125° latitude and 0.125° longitude spatial resolution (equivalent to areas of about 10.8 × 13.9 km, or about 6.7 × 8.6 miles) (Boersma et al., 2011). Such satellite-based data of tropospheric NO₂ column densities have been extensively used to infer NO_x emissions (e.g., Silvern et al., 2019 and references therein; Goldberg et al., 2021; Ding et al., 2022, Voigt et al., 2022; Lange et al., 2022). Second, we leverage the introduction of an emission tax in the Spanish region of the Comunidad Valenciana in 2013 that taxes the amount of SO_x and NO_x emitted. The Comunidad Valenciana is our treatment group, and matched regions of the rest of Spain comprise our control group. This setting has two key advantages. First, despite being local, the emission tax is economically significant, contributing an additional €21 million in annual revenue to the local budget, adding about 5% to 13% to local firms' tax bills, and triggering investment cuts (Jacob and Zerwer, 2022). Second, the within-country setting allows us to explore differences in emission levels across Spain while holding general economic conditions and regulations on a national level constant.

In our difference-in-differences (DiD) analysis, we show across several tests that the local emission tax leads to a modest reduction of NO₂ levels during the observed period. That is, NO₂ levels in the Valencian Community are cut by around 1.2% due to the emission tax. Given that similar emission initiatives that introduced taxes on input factors such as gasoline or other

fuels led to a cut in CO₂ emissions of 1% to 7.3% (Martin et al., 2014, Rafaty et al., 2020) or the fact that emission taxes reduced firm- or plant-level emissions by up to 45% (e.g., Klemetsen et al., 2016), our findings indicate that when using granular data on NO₂ air pollution as a proxy for emission output levels, the estimated effect is rather at the lower end of prior estimates. In terms of absolute magnitude, the reduction in NO_x emissions associated with the 1.2% reduction in NO₂ pollution is about 728 tons per year in Valenciana. However, this approximation is based on several assumptions as detailed in footnote 8. Given that the tax revenues of the emission tax on SO_x and NO_x are about €21 million, this translates in a “price per ton of NO_x reduction” of about €14,430 to €28,850. This price per ton is well above alternative abatement technologies (DEFRA/DTI, 2001).

In addition to the average response, we explore the heterogeneity in the response across regions. The objective of these tests is twofold. First, these tests can inform policymakers which regions benefit most and which benefit least from introducing emission taxes. Second, these tests help us in assessing some of the mechanisms through which emission taxes can reduce emission levels. First, we show that emissions are reduced more in areas with high industrial activity. When defining industrial activity by the actual number of firms in an area, in areas with many firms, emissions are reduced by about 2.5%, while there is no change in emissions in areas with only a few firms. When defining industrial activity by the degree of urbanization, emissions are reduced by about 5% in urban areas with high industrial activity, while there is no change in air pollution in rural areas where fewer firms are active. This is consistent with the expectation that areas with many firms have more potential to reduce emissions than areas with fewer firms given that NO_x emissions mostly result from industrial activity.

We also show that when splitting the sample into areas with more “clean” versus “dirty” firms, we do not see a significantly stronger reduction for areas with more “dirty” firms. We find a similar reduction in areas with “clean” firms, suggesting that emission taxes affect not only those firms that are mainly responsible for emissions but also cleaner sectors. One reason

is that “dirty” firms pass on the emission tax burden to “cleaner” firms, who respond to this local emission tax by cutting investments (Jacob and Zerwer, 2022). It thus appears as if emission taxes hit all industrial areas, irrespective of the prevalence of “dirty” firms.

Next, we test the notion that emission reduction is often stronger for firms with R&D activity and technological innovation. We do this by sorting areas into those with more versus fewer firms with high intangible assets. Consistent with this prediction, we find that those areas with more innovative firms cut emissions more than areas with fewer or less innovative firms. This indicates that innovation and R&D activity indeed seems to be key when it comes to cutting emissions and meeting emission targets. We also test for the notion that larger firms potentially have more resources and thus more potential to reduce emissions post-reform. We confirm this idea and find a negative and significant effect for areas with larger firms. This effect is statistically different from the effect for areas with smaller firms.

Finally, we test if firms that have the opportunity to exit the treated area by relocating their activity are partially responsible for our findings. We explore this opportunity through the existence of multinational companies (MNCs) vis-à-vis domestic or standalone firms in an area. A higher fraction of MNC activity would indicate that these firms find it easier to relocate their activities than standalone firms, as the latter are mostly family firms with local social and economic ties, making them less mobile than MNCs. Our results indicate that the NO₂ reduction is concentrated in areas with a higher fraction of MNCs. Emission taxes may thus partially result in a shift of emissions to other jurisdictions. While such relocation reduces local emissions, total emissions are unchanged. This result is consistent with the evidence of carbon leakage (e.g., Babiker, 2005, or Aichele and Felbermayr, 2015).

We contribute to the literature in three ways. First, using the merits of the satellite-based observations, we add to the literature on emission taxes (e.g., Strand, 2013, Sen and Vollebergh, 2018, Qia et al., 2022). To measure emissions, previous literature either modeled emissions or used proxies or sensor data that were aggregated to administrative units or sectors (see, e.g.,

Zhen et al., 2019, Omrani et al., 2020). Prior firm-level emission data focus on information on free allocation of emissions allowances (Abrell et al., 2011) or only comprise limited information at the firm or plant level (Klemetsen et al., 2016). We overcome these limitations by using highly granular spatial NO₂ column density data to proxy for emissions. Another advantage of using satellite NO₂ data is that this tracer for anthropogenic combustion processes remains rather local and close to the source, as it dissolves locally after a few hours and is hardly advected into other regions (Antweiler and Gulati, 2016). With this data, we contribute to studies measuring emissions at an aggregate level by showing that emission taxes can reduce emissions (Pretis, 2022). However, our estimate is below country-level estimates (Metcalf and Stock, 2022) and much smaller than plant-level or sector-specific estimates (e.g., Andersson, 2019, Klemetsen et al., 2016), indicating the importance of capturing effects beyond the actual plant or the specific potentially emission-intense industry. Plant- or sector-level tests may overstate the aggregate response, as emissions that stem from buildings, commercial traffic, and transport need to be considered when exploring the effectiveness of environmental policy initiatives, even though industrial processes are the main contributor to overall emission levels. With our finding that an emission tax decreases the NO₂ burden, but at rather modest levels, we contribute to an understanding of the real effects of environmental taxes on a key air pollutant (NO₂) that has many detrimental effects on human health and the environment.

With these findings, we also add to the policy debate about the effectiveness of emission taxes. By showing that emission taxes lead to only a modest decrease in emissions that varies based on intensity of industrial activity, technological innovation, and firm size, but that this effect is not fully related to the prevalence of cleaner versus dirtier industries, we provide a basis to discuss the addition of innovation stimulation policies as well as policies directly targeting specific areas (e.g., industry areas) more directly to the standard policy toolkit to reach net-zero targets. Since the price per ton NO₂ reduction in our sample appears to be much higher than the price of other abatement investments, we cautiously conclude that a careful design of

tax policy via tax incentives and a combination of other measures can help in achieving the desired outcomes. In particular, if policymakers intend to target dirty industries, emission taxes may not be the best solution. Instead, fostering abatement investments through tax incentives coupled with other regulations and policy standards as suggested by Zhao and Mattauch (2022) might be a less costly alternative to achieve the goal of emission reductions.

4.2. Institutional Background

4.2.1. NOx and their Effects

Nitrogen oxides are primarily emitted when fossil fuel is burned (EPA, 2022a). This can be by the emission from cars, trucks, or other vehicles or from buildings (e.g., heating), but it also comes from industrial emissions of industrial production processes. Indeed, data from the EEA shows that 15% can be directly attributed to industrial processes and their energy consumption, 22.5% to energy production and distribution and about 2% to agricultural processes (Figure 8; EEA, 2022). This sums up to about 40% of emissions that directly can be related to the industry with additional parts of transport emissions also being attributable to commercial traffic.

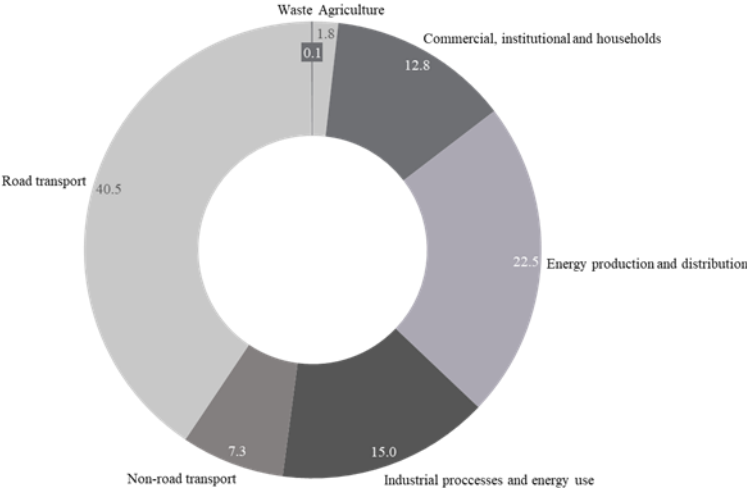


Figure 8: NOx Sources.

This figure illustrates the sector share of nitrogen oxides emissions in 2011 reported to the Convention on Long-range Transboundary Air Pollution (LRTAP Convention) by the EEA.

The quantity of NO_x emitted by firms depends largely on the firm size and industry, the availability of abatement technologies, and the explicit and implicit price of emissions. Moreover, NO_x are ground-level greenhouse gases that are not very stable and therefore cannot be transported too far by wind. Under average conditions, NO₂ lasts in the atmosphere for only a few hours up to one day. This local and temporary preciseness is supported by “the weekend effect,” which shows lower emissions on Saturdays and Sundays (Kaynak et al., 2009; Erbertseder et al., 2015). Thus, NO₂ can be attributed to a certain location and to the local economic activity with primarily small additive effects from earlier emissions or emissions generated at a different location.

In addition to several negative health effects being attributed to high NO_x emissions, environmental effects can also be significant when NO_x levels are high (EPA, 2022a). NO₂ and other NO_x interact with water, oxygen, and other chemicals, leading to acid rain (EPA, 2022a). The latter can harm ecosystems and contributes to nutrient pollution in coastal waters. Thus, in recent years, policymakers have developed several approaches to tackling high NO_x levels and reducing potential negative consequences. Policy responses range from softer forms such as air quality monitoring, modeling, and reporting to putting a hard price on pollution by introducing emission taxes or other pricing schemes. The latter—taxes on emissions—has increased in popularity in recent years. For instance, while Sweden, Italy, and Denmark were early adopters, introducing a charge on NO_x in 1992 (IEA, 2017) and 1998 (EU Commission, 2015, 2016), respectively, other countries, including Estonia, Norway, and other Eastern European countries, adopted similar taxes only in the early 2000s.

4.2.2. Exploiting Regional Environmental Taxes in Spain

To explore the effect of NO_x emission taxes on NO_x emissions, we leveraged the introduction of a local emission tax in 2013 in the Spanish Comunidad Valenciana (see also Jacob and Zerwer, 2022, for more details). This reform was triggered by European Union

initiatives and suggestions by the European Commission on environmental taxes. Following the new law introduced on January 1, 2013, SOx and NOx within the community are taxed at between €9 and €50 per ton, depending on firm-specific consumption levels. In Figure 9, we illustrate the exact timeline of the introduction of the tax (see, also, Jacob and Zerwer, 2022).



Figure 9: Introduction Timeline Valencian Emission Tax (Jacob and Zerwer, 2022). This figure illustrates the overall sequence of the introduction of the new emission tax in the Spanish Valenciana Community, beginning with the policy announcement on September 28, 2012.

To observe a potential change in emissions, this setting is advantageous for several reasons. First, it was the only tax reform on NOx emissions during our observed period in Spain. While other local emission taxes were introduced during the early 2000s, the introduction in the Valencian Community allowed us to compare it with the rest of Spain without confounding emission reforms. Other local environmental reforms during our sample period in Spain are unrelated to emissions. We controlled for these reforms in a separate test. Second, since existing studies struggle to find an appropriate control group as most emission reforms are at the federal level (Bayer and Aklin, 2022), leveraging a regional reform is advantageous. The within-country setting allows us to explore differences in NO2 levels within Spain while holding general economic conditions and federal regulations constant. Since only a few areas share borders with France or Portugal, the potential for spillover effects is limited.

Second, despite being local in nature, the Valencian emission tax is economically significant, with tax revenues of about \$21 million per year. The reform increased firms’ corporate tax bills by 5% to 13% and caused substantial compliance and consulting costs; it also reduced investments (Jacob and Zerwer, 2022). Finally, firms can reduce emissions by investing in abatement techniques, including filters or electrostatic precipitators, or by changing

production processes (e.g., storage and transportation optimization). Collectively, it appears that our setting is suitable to explore the effect of an emission tax on emission levels.

4.3. Data Preparation and Merging

4.3.1. Satellite Data

To measure area-specific NO₂ levels in Spain for the years 2009–2016, we use sensor data from the OMI on board NASA’s AURA satellite (Levelt et al., 2006). The OMI is a nadir-viewing spectrometer on a polar sun-synchronous orbit crossing the equator at 1:45pm local time. With its wide swath and daily global coverage, it has been providing observations of tropospheric NO₂ vertical column densities since 2004 at a spatial resolution of 13 km × 24 km at nadir. In this study, we apply gridded tropospheric NO₂ vertical column densities (QA4ECV version 1.1) at an equidistant spatial sampling of 0.125° × 0.125° (Boersma et al., 2011), which corresponds to 10.8 × 13.9 km at the geographic latitude of Valencia. The NO₂ values are vertically integrated throughout the troposphere with the unit μmol/m².

Tropospheric NO₂ is a representative short-lived tracer for anthropogenic emissions from transport, energy production, and industrial processes into the boundary layer (Müller et al., 2022), with small possible contributions from natural emissions from lightning (Perez-Invernon et al., 2022) and soil (Lu et al., 2021). Therefore, tropospheric NO₂ is extensively used to infer NO_x emissions (Kaynak et al., 2009, Silvern et al., 2019; Ding et al., 2022; Lange et al., 2022), quantify lockdown effects during the COVID-19 pandemic (Liu et al., 2021, Voigt et al., 2022), examine economic impacts (Montgomery and Holloway, 2018, Bichler and Bittner, 2022), and identify urban pollution islands and their long-term trends (Erbertseder et al., 2015, Georgoulias et al., 2019; Goldberg et al., 2021). As substantiated by Goldberg et al. (2021a) and Geddes et al. (2016), there is a strong correlation of tropospheric NO₂ with surface NO₂ concentrations.

As meteorological conditions can affect variation in NO₂, we follow prior literature and annualize the daily data to reduce the volatility possibly caused by weather and similar factors

(Huang et al., 2017; Song et al., 2019; Müller et al., 2022). We assess the sensitivity and robustness of our analysis by controlling for possible weather effects (5.3.1). The average uncertainty of the satellite-based NO₂ observations due to the tropospheric air mass factor over Europe is quantified by 18% to 26% per pixel (Boersma et al., 2018). However, a detailed error analysis for individual retrievals exhibits a strong variation of these estimates (Boersma et al., 2004). Parameters such as cloud fraction, surface albedo, surface pressure, and the a priori NO₂ profile shape contribute to the overall error budget. Since the yearly mean is calculated from n daily observations and the standard error decreases by $1/\sqrt{n}$, the overall error is reduced with annualized data. Regarding the systematic error of the NO₂ data, a negative bias is evident compared with ground-based measurements (Celarier et al., 2008). However, since we analyze annual NO₂ variation, this systematic error should not impact our findings.

Compared with in-situ measurements from ground-level stations, a major advantage of satellite-based observation is the area-wide coverage and consistency of high spatial resolution. NO₂ values are homogeneously integrated with the same spatial resolution. Hence, they represent the same conditions and are less prone to issues of representativity, as is the case for measurement stations (Zhu et al., 2020). This ensures the spatial comparability of the data across space. Our equidistant sample further reduces the influence of artificial and inconsistent spatial units such as administrative boundaries.

While the data have clear advantages, they come at a cost of several disadvantages. For example, uncertainties remain on the spatiotemporal variability of the resulting emission because many assumptions need to be made on the emission rates and emission factors (e.g., when and where what amount of fossil fuel is combusted, at which temperature, and at which efficiency). The satellite observations, however, enable a quantification of the resulting total NO₂ burden from all emitting sources, which cannot be disentangled in sector-level data. Moreover, optical measurements from satellites rely on backscattered solar radiation. Hence, no data are available during night or under cloudy conditions. While in-situ measurements on

the ground are direct measurements, the quantities are indirectly obtained from satellite through the retrieval of trace gas amounts from measured spectra. Despite the necessary assumptions during the retrieval of atmospheric quantities, the characterization of uncertainties and error propagation has improved significantly in the last few decades. While in-situ measurements can be performed continuously during day and night, the repetition rate of satellite observations is confined to the orbit type. However, one clear advantage of satellite observations vis-à-vis in-situ measurements is that our approach captures all overall NO₂ levels and not just one specific source (e.g., a chimney). Ultimately, it is the local overall NO₂ concentration that is relevant for health damage and air pollution. Our data can thus directly inform policy goals.

4.3.2. Firm-Level Data and Merging Databases

To measure and control for local economic activity, we append our satellite data with aggregated firm data. For our firm data, we use all available data on Spanish firms from Bureau van Dijk's Amadeus database over the period 2009–2016. Our analysis is based on all unconsolidated financial statements. In contrast to consolidated balance sheet information, as provided, for example, in Compustat Global, unconsolidated data allow us to locate the activity of a single firm to match economic activity with the spatial emission data. We start with all available firms and exclude micro-firms with total assets below €50,000, fixed assets below €5,000, and missing data on earnings before interest and taxes. We also exclude observations with negative sales, total assets, or cash, as these observations are most likely misreported. To match our firm and emission data, we use geocoding to add the geographic longitude and latitude to the postcodes of the Spanish firms. We consider five-digit postcodes since these postcode areas are fairly small. We use the OpenCage STATA code that matches the closest longitude and latitude to a given postcode (where we use the midpoint). We then locate the postcodes into the pixel resolution of our satellite data so that we can assign each postcode to one specific 10.8×13.9 km quadrant from the satellite data. Next, we weight our main

economic variables by sales to ensure that our firm-level economic control variables reflect the local economic activity. Finally, we add additional regional variables such as population and car registration based on two-digit postcodes as more granular data are not available. We obtain these data from the Instituto Nacional de Estadística (Spanish National Statistics Institute).

This gives us a full panel of emission data as well as economic and demographic control variables. We also make one final modification: We exclude all areas without any meaningful economic activity, that is, areas of 10.8×13.9 km with fewer than five firms are excluded. We choose five firms, as this is below the bottom quartile of the existence of industrial activity. After these steps, we arrive at our final sample of 15,374 observations from 1,957 areas.

4.4. Empirical Setting

4.4.1. Estimation Strategy



Figure 10: Treatment and Control Group, Map of Spain.

This figure illustrates the choice of our treatment (dark gray area) and control group (light gray area).

We exploit the local emission tax reform in a DiD analysis. As illustrated in Figure 10, the treatment comprises the Comunidad Valenciana identified per two-digit postcode (i.e., observations with the two-digit postcode 03, 46, or 12). As our control group, we use all other autonomous communities of Spain. We thus estimate the following equation:

$$Emissions_{i,t} = \alpha_0 + \beta_1 Treatment_i \times Post_t + \gamma X_{i,j,t-1} + \alpha_i + \alpha_t + \varepsilon_{i,t} \quad (5)$$

Here, the dependent variable, $Emissions_{i,t}$ is the natural logarithm of the amount of emission measured in $\mu\text{mol}/\text{m}^2$ in an area i in year t . While previous studies often use the absolute amount as their main dependent variable (for instance, see Müller et al., 2022), we use the natural logarithm of the absolute amount of NO₂ as our main dependent variable for two reasons. First, the raw emission values are skewed (skewness is above 2.3). Second, the log transformation allows us to more easily interpret the coefficient estimate as a percent change (with some simple calculations). The dummy variable *Treatment* is equal to 1 for area i in the Valencian Community where the emission tax was introduced in 2013 and 0 otherwise. The dummy variable *Post* is equal to 1 for years 2013–2016 and 0 otherwise. Since our regression sample starts in 2009, we use four pre-reform years and four post-reform years. The main variable of interest is the interaction $Treatment \times Post$. We expect that, relative to areas in other places of Spain, areas in Valenciana decrease their emissions after the reform ($\beta_1 < 0$).

The baseline regression also includes a vector of control variables ($X_{i,j,t-1}$), building on prior economic literature on investment decisions (e.g., Badertscher et al., 2013, Shroff et al., 2014, Shroff, 2017, Fox et al., 2022) and on the literature using similar satellite data (Müller et al., 2022). We further include area-fixed effects (α_i) and year-fixed effects (α_t) to account for time-invariant area characteristics and general yearly trends. The control variables are lagged by one year and comprise controls for *Firm Size* (defined as the natural logarithm of aggregate sales), *Number Firms* (defined as the natural logarithm of the number of firms within an area), *Population* (defined as the natural logarithm of the number of inhabitants), and *Past Emissions* (defined as a dummy variable equal to 1 if above median emissions in past years). We also include weighted averages of several firm characteristics: *Investment* (defined as the change in fixed assets scaled by total assets), *Sales Growth* (defined as the change in the natural logarithm of sales), *Profitability* (defined as net income scaled by total assets), and *Tangibility* (defined as fixed assets over total assets). As NO₂ emissions are partially caused by cars and road traffic

(European Environment Agency, 2022), we control for *Car Registrations* (defined as car registrations per capita). We cluster standard errors at the area level.

Another important research design choice relates to entropy balancing. Because emission-level data are noisy and because regions differ in their economic activity, we ensure that the treated areas are comparable with the control areas prior to the emission tax introduction. For this reason, we balance our sample using pre-treatment emissions of 2009, 2010, and 2011, the dummy variable for areas with above-median emission in the past, firm size, and the area size in terms of number of firms as well as the firm control variables that differ the most between the two groups, namely, car registrations, sales growth, profitability, and tangibility. The approach of using matching DiD estimators, including balancing on past outcomes, ensures that the treatment and control groups are more comparable and the resulting estimator less biased when evaluating policy measures (see, e.g., Blundell and Costa Dias, 2000, Girma and Görg, 2006, Ham and Miratrix, 2022), particularly in the case of environmental policies (see, for instance, Boampong, 2020). According to these authors, using matching DiD estimators can “improve the quality of non-experimental evaluation results significantly” (Blundell and Costa Dias, 2000, p. 438). We thus include three lags of pre-treatment NO₂ to reduce the potential bias and to increase the reliability of the estimates (Ham and Miratrix, 2022). The final weights in our tests are illustrated in Figure 11. Matched areas are mostly located in the autonomous communities of Galicia, Castilla y Leon, Castilla la Mancha, Andalucía, and northern Catalunya. In contrast, the cities of Madrid and Barcelona and the areas surrounding these major cities are assigned only very small weights. This is plausible as the areas around Madrid and Barcelona, with each having over 5 million inhabitants, differ from Valenciana and its biggest cities, Valencia and Alicante. Instead, the control group covers areas around Seville, Malaga, Murcia, Bilbao, and Oviedo, which are more comparable with Valencia and Alicante in terms of population and economic activity than Madrid or Barcelona.



Figure 11: Weights, Map of Spain.

This figure illustrates the weights used for the main analysis.

4.4.2. *Descriptive Statistics*

Table 14 reports statistics for the variables of our sample of 15,374 observations used for the baseline analysis. The variable definition can be found in the Appendix. All area-level control variables are winsorized at the first and 99th percentiles. Areas, on average, have emissions of 28.12 $\mu\text{mol}/\text{m}^2$. The average (median) area has an investment of 4.6% (3.7%), sales growth of 4.2% (3.8%), and a profitability of 2.8% (2.3%). Panel C presents evidence of how the area of our treatment and control groups compare. For most variables, we see a difference between areas located in Valenciana and those in the rest of Spain. For instance, areas in Valenciana have on average more firms within a single area, higher past emissions, and higher profitability. For this reason, we use a balanced sample in all our tests, as discussed above. Panel D of Table 14 shows descriptive statistics for the balanced panel.

Table 14: Descriptive Statistics.

This table presents descriptive statistics for our main variables for 15,374 observations from 1,957 areas from 2009 to 2016. The Appendix defines the variables. Panels A and B show a general overview of statistics. Panel C shows the difference in mean between the treatment group and the control group. Panel D shows again the difference in mean between the treatment group and the control group, this time for our balanced panel.

Panel A: Dependent Variables					
	Mean	St. Dev.	25th Perc.	Median	75th Perc.
ln(NO2)	3.2923	0.2861	3.0886	3.2808	3.4551
NO2	28.1160	9.0636	21.9468	26.5963	31.6609
Panel B: Other Firm Variables					
Firm Size	17.5777	1.8756	16.1924	17.4057	18.8663
Number Firms	3.8544	1.4038	2.7081	3.5835	4.8040
Population	7.2969	1.7633	6.0051	7.3671	8.5222
Car Registrations	1110.9580	3006.1750	105.7044	286.6482	889.9471
Past Emissions	0.1776	0.1803	0.0487	0.1165	0.2493
Investment	0.0459	0.0464	0.0211	0.0368	0.0578
Sales Growth	0.0420	0.1445	-0.0260	0.0379	0.1001
Profitability	0.0275	0.0357	0.0090	0.0232	0.0419
Tangibility	0.3810	0.1149	0.3110	0.3676	0.4370
Panel C: Difference between Treatment and Control					
	Treatment = 0	Treatment=1		Difference	
ln(NO2)	3.2796	3.4767		-0.1971***	
NO2	27.7684	33.1937		-5.4252***	
Firm Size	17.5040	18.6538		-1.1498***	
Number Firms	3.7949	4.7229		-0.9280***	
Population	7.2500	7.9828		-0.7328***	
Car Registrations	1107.888	1155.808		-47.9197	
Past Emissions	0.1768	0.1896		-0.0129**	
Investment	0.0461	0.0442		0.0019	
Sales Growth	0.0416	0.0489		-0.0073*	
Profitability	0.0270	0.0348		-0.0078***	
Tangibility	0.3825	0.3589		0.0236***	
Panel D: Difference between Treatment and Control (balanced)					
	Treatment = 0	Treatment = 1		Difference	
ln(NO2)	3.5402	3.5358		0.0044	
NO2	36.0965	35.6481		0.4485	
Firm Size	18.4323	18.4489		-0.0166	
Number Firms	4.5272	4.5481		-0.0209	
Population	7.2966	7.9573		-0.6609***	
Car Registrations	750.0697	848.8609		-98.7912	
Past Emissions	0.1855	0.1935		-0.0080	
Investment	0.0431	0.0407		0.0023	
Sales Growth	0.0405	0.0424		-0.0019	
Profitability	0.0279	0.0308		-0.0029**	
Tangibility	0.3576	0.3531		0.0045	

To provide more background on the Spanish setting, Figure 12 presents statistics on the nature of Spanish firms along two dimensions. First, Panel A presents the percentage of economic activity defined by sales of standalone firms, firms that are part of a Spanish group of firms, or firms that belong to a multinational group (i.e., they have either a foreign parent or

a foreign subsidiary). We find that approximately 55% of revenues stem from standalone firms, 22% from domestic groups, and 23% from MNCs. Second, we present a breakdown by firm age in Panel B of Figure 12. We find that the vast majority of firms (almost 75%) have existed for more than 10 years and that 45% of the firms have existed for more than 20 years. In sum, the fact that most firms are older, standalone firms reduces the likelihood that firms respond to the emission tax by relocating. Established standalone firms are mostly family-run businesses with social and economic ties to the local area, which likely prevents them from relocating.

Panel A: Percent of Sales by Firm Type



Panel B: Percent of Firms by Age

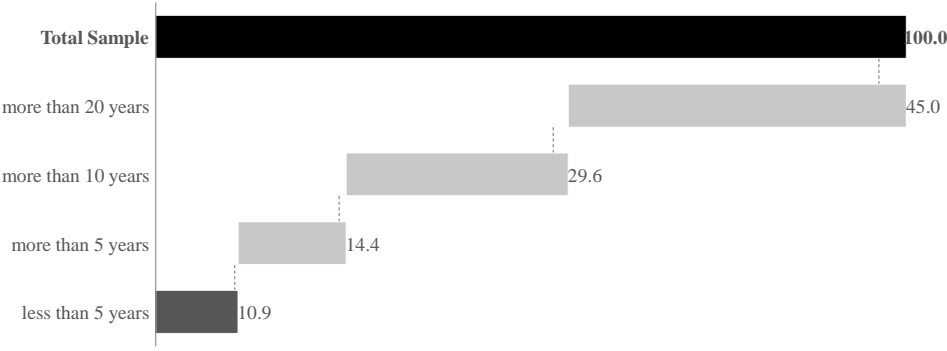


Figure 12: Firm Composition in Spain. This figure illustrates the relevance of domestic firms, MNCs, and standalone firms. We show the percentage of sales stemming from either group of firms. In Panel B, we present the numbers as percentages of firms depending on firm age cohorts.

4.4.3. Identifying Assumptions

Next, we assess the parallel trend assumption prior to the introduction of the emission tax in 2013. The underlying assumption for our chosen empirical approach is that absent the reform in 2013, emission levels in our treatment (Comunidad Valenciana) and our control group (rest of Spain) would have evolved similarly. While we cannot test this argument after the reform, we conduct a parallel trend test for the pre-reform years 2009–2012. Figure 13 shows the difference in emission levels between areas located in the Valencian Community and the other areas as well as the 95% confidence intervals. The figure suggests that prior to the reform, we see a parallel trend of our treatment and control groups, allowing us to proceed with our empirical approach (which is due to our entropy-balancing approach).

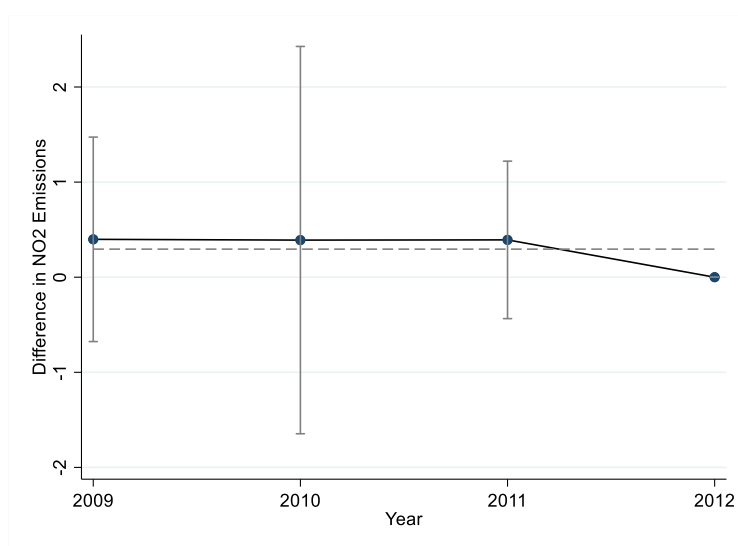


Figure 13: Parallel Pre-Trends.

This figure illustrates the difference in NO₂ pollution levels over the period 2009–2012 between the treated group (areas in Valenciana) and the control group (areas in the rest of Spain).

4.5. Results

4.5.1. Graphical Evidence of Potential Emission Reductions

We start our analysis by illustrating the NO₂ data in a map of Spain over time. Figure 14 shows average NO₂ levels across Spain from 2009 until 2016. Darker-gray areas indicate areas with high emission levels (with black being the maximum of 80 $\mu\text{mol}/\text{m}^2$), and light gray represents areas with fewer emissions (with white being the minimum of 0 $\mu\text{mol}/\text{m}^2$). As

expected, one can clearly see the largest cities, such as Madrid and Barcelona. While there is an overall trend of reduced emissions over time (more light gray areas and fewer dark spots), there is no striking visual evidence of lower emissions in Valenciana.

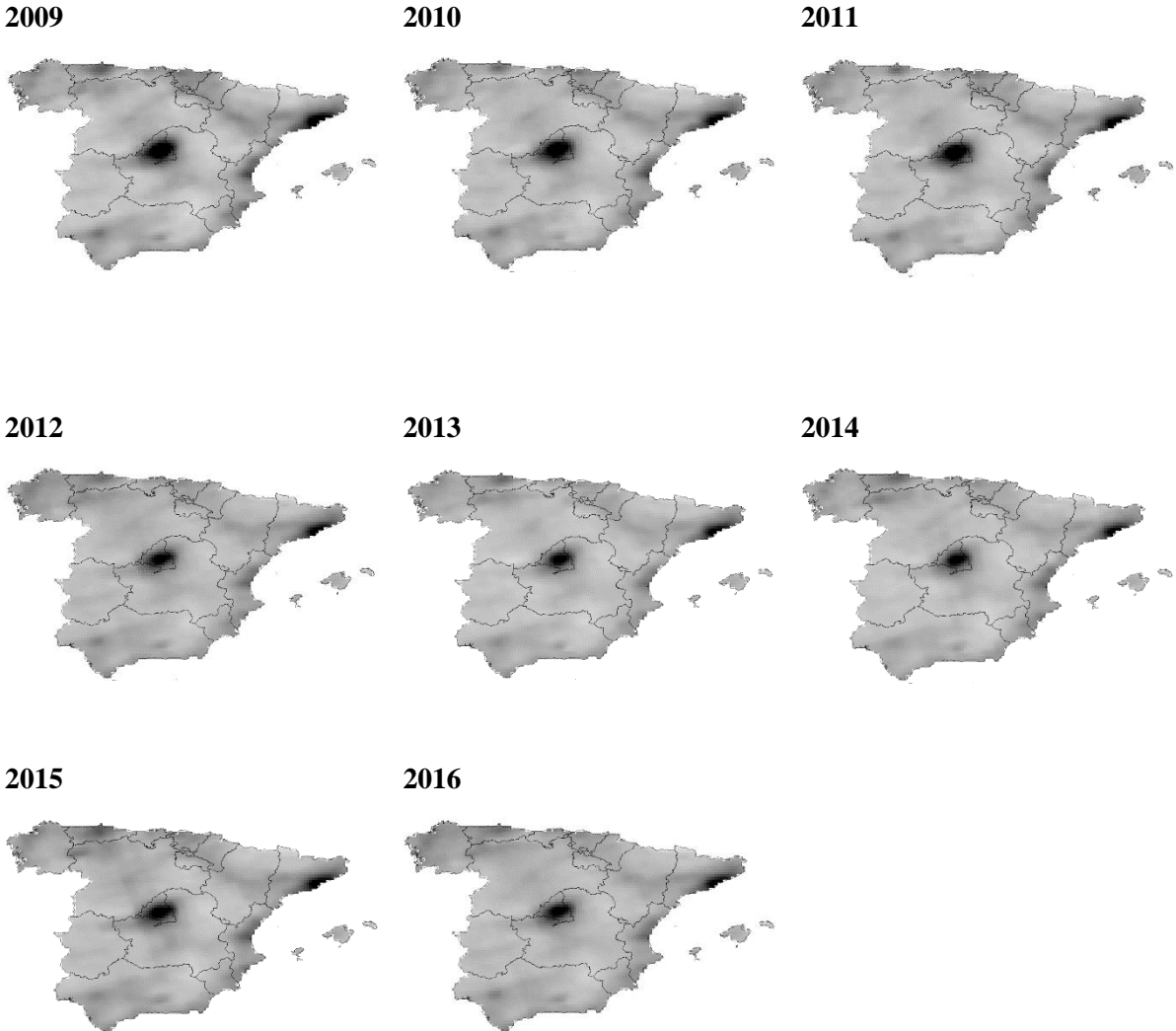


Figure 14: Emission Proxy Over Time Across Spain.
 This figure illustrates the yearly average of NO₂ levels across Spain from 2009 until 2016. Darker gray values indicate higher NO₂ contents. The maximum value shown (darkest spots) is 80 $\mu\text{mol}/\text{m}^2$ and the minimum value (lightest spots) shown is 0 $\mu\text{mol}/\text{m}^2$.

We obtain similar results when zooming into the autonomous community of Valenciana and the neighboring provinces in Figure 15. The figure shows the difference in Valencian NO₂ levels relative to the Spanish average NO₂ levels for the years before (2009, 2010, 2011), during (2012, 2013), and after (2014, 2015, 2016) the tax reform. The maximum (minimum) value of the emissions relative to the annual mean amounts to 15 $\mu\text{mol}/\text{m}^2$ (-25 $\mu\text{mol}/\text{m}^2$) in the figure.

While NO2 levels are generally quite high in the pre-reform years (darker gray areas), with a peak in 2012, the graphical evidence suggests that there is a reduction in NO2 emissions after the reform, as indicated by a lightening of the gray areas. However, this reduction is not visibly greater in the treated areas (highlighted in red) of each map. Hence, it appears as if NO2 levels in the Valencian Community did not respond substantially to the tax reform.

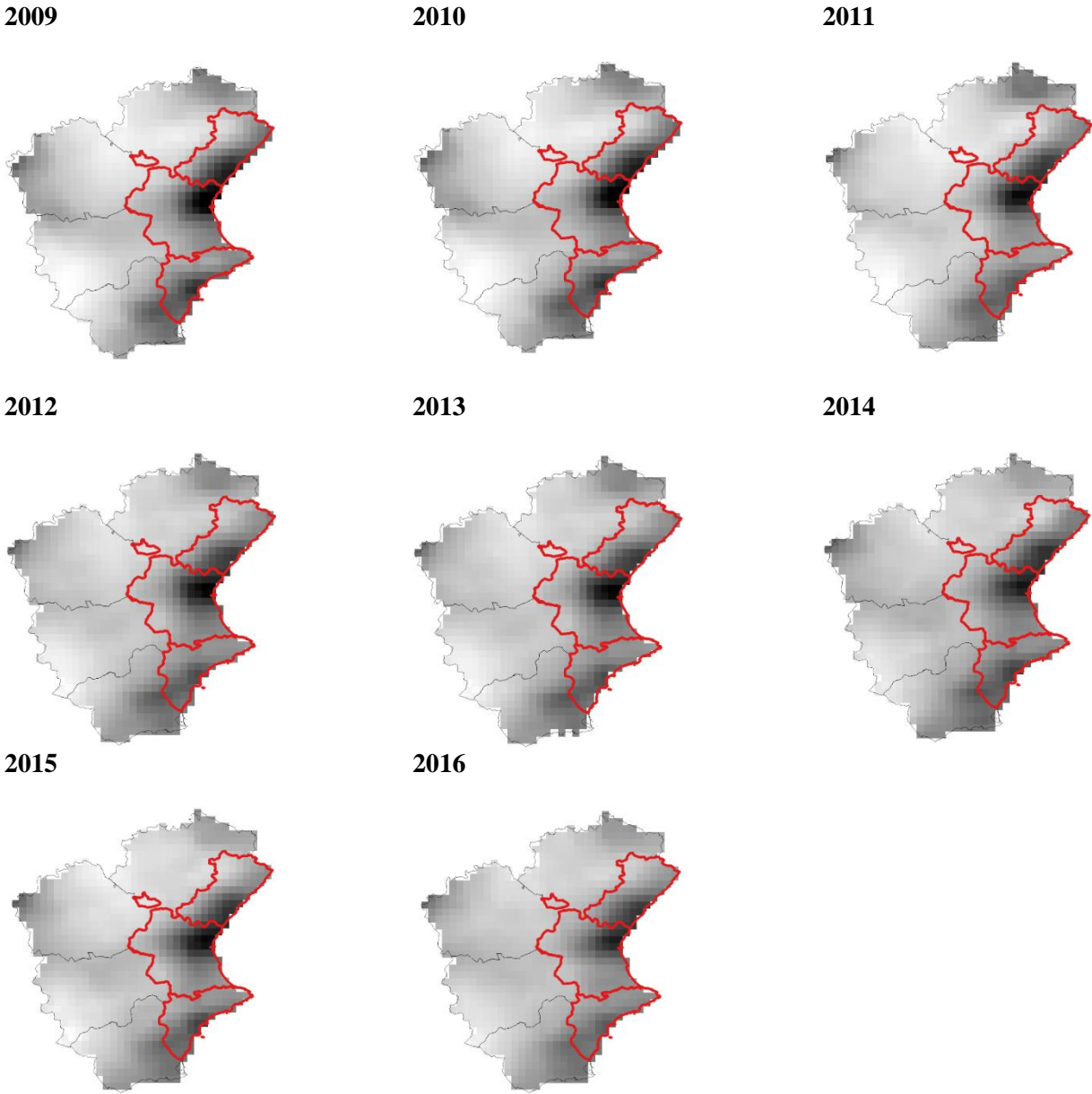


Figure 15: Emission Proxy Over Time in Valenciana and Bordering Provinces. This figure illustrates the yearly average of NO2 levels in Valenciana and the bordering provinces from 2009 until 2016. The maximum value shown (darkest spots) is $15 \mu\text{mol}/\text{m}^2$ and the minimum value (lightest spots) shown is $-25 \mu\text{mol}/\text{m}^2$.

4.5.2. *Baseline Results*

In Table 15, we test the emission-level response in a regression analysis of areas located in the Valencian Community (treatment group) versus the entropy-balanced rest of Spain (control group) from Equation (5). In column 1, we estimate the regression using year- and area-fixed effects, but without control variables. The DiD coefficient ($Treatment \times Post$) is negative and significant at the 10% level. Once we include controls, the estimate remains very similar. The results indicate that emissions in an area are cut by around 1.2% following the introduction of the emission tax in 2013. Given that other initiatives that introduced taxes on input factors such as gasoline or other fuels led to a cut in CO₂ emissions of between 1% and 7.3% (Martin et al., 2014, Rafaty et al., 2020) or the fact that emission taxes reduced firm- or plant-level emissions by up to 45% (e.g., Klemetsen et al., 2016), our finding is at the lower end of prior estimates. Put differently, our findings indicate that when actual emission output levels are examined on a granular level, the estimated effects are much smaller than plant- or sector-level data. One potential reason is that our data are able to capture all other sources of NO_x emissions, such as commercial traffic and transportation. To inform policymakers, it is, however, critical to obtain a holistic picture of emissions and to account for all potential sources of emissions.

Table 15: Emission Taxes and Emission Levels, Main Results.

This table presents the main results of our analysis using an entropy-balanced panel. The primary dependent variable is the natural logarithm of NO₂ emissions. The primary independent variable is the interaction between Treatment and Post. In column (2), all control variables are lagged by one year. We report robust standard errors clustered at the area level for both columns (1) and (2). The entropy-balanced approach balances on selected control variables as well as emissions of 2009, 2010, and 2011. We include year- and area-fixed effects. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)
<i>Treatment × Post</i>	-0.0106* (0.0062)	-0.0115* (0.0069)
<i>Firm Size</i>		-0.0033 (0.0050)
<i>Number Firms</i>		-0.0086 (0.0202)
<i>Population</i>		-0.0626** (0.0279)
<i>Car Registrations</i>		0.0000 (0.0000)
<i>Past Emissions</i>		0.0766*** (0.0239)
<i>Investment</i>		0.0375 (0.0369)
<i>Sales Growth</i>		0.0080 (0.0091)
<i>Profitability</i>		-0.0715 (0.0679)
<i>Tangibility</i>		-0.0874* (0.0519)
Area FE	Yes	Yes
Year FE	Yes	Yes
Balanced	Yes	Yes
Observations	15,374	15,374
Adj.-R ²	0.9518	0.9527

It is well known that the relationship between NO_x emissions and the NO₂ column density depends on chemical and meteorological conditions and is by no means a one-to-one relationship. However, to foster the discussion about economic impacts of environmental taxes we present some scenarios i.e., assumptions here. A reduction of the NO_x emissions in the autonomous community of Valenciana by 1.2% would result in $728,2 \pm 0.1$ tNO_x/year for 2011 according to the numbers reported by EDGAR v6.1. Given that the tax revenues of the emission tax on SO_x and NO_x are about €21 million, this translates in a “price per ton of NO_x reduction” of between about €14,430 (if we assume that the half of the emission tax revenues are due to NO_x) to €28,850 (if we assume that the entire tax revenues are due to NO_x).

To put this range into perspective, there are many other potential abatement strategies for companies to reduce NO₂ emissions. For example, technological changes such as filters or electrostatic precipitators are highly effective in reducing NO₂ but are not very costly. Using the numbers in a report of measures to reduce emissions (amongst them NO₂) in the UK by the official environmental agency (DEFRA/DTI, 2001), the average cost per ton for technological changes is about €6,380 per ton of NO₂ reduction for industrial processes and about €22,800 per ton of NO₂ reduction for transport and shipping. Changing processes such as transport or storage optimization is even cheaper per ton of NO₂ reduction (between €3,500 and about €6,150 per ton). One implication is that the emission tax is a rather costly tool to curb emissions and that investment incentives such as tax credits to fund such abatement investments could be less costly policy tools to achieve a reduction in NO₂ emissions.

4.5.3. Sensitivity and Robustness Tests

4.5.3.1. Control for Weather

In our first robustness test, we address the concern that a measurement error induced by unavailable data for cloudy days or interannual variability of meteorological parameters drives our result. While the DiD approach based on annual data coupled with area-fixed effects and year-fixed effects should help in eliminating a potential bias, we address remaining concerns about weather conditions driving our findings. Specifically, we use ERA-5 reanalysis data from the Climate Data Store (CDS) provided by the Copernicus Climate Change Service (see Muñoz Sabater, 2019) with a comparable resolution as our main data and control for wind variables (wind speed and wind direction) as well as weather variables (temperature, surface solar radiation, and precipitation). The results are reported in Table 16, Panel A. Since the resolutions of the datasets differ slightly, there is some sample attrition. Importantly, our main inferences are unchanged when controlling for these weather variables, corroborating our main inferences of a rather modest reduction in air pollution.

Table 16: Emission Taxes and NO2 Pollution Levels, Robustness Tests.

This table shows the results of our robustness tests. In Panel A, we control for different weather-related variables. In column (1), in addition to the controls of our main analysis, we include two weather controls with regard to wind, namely wind speed and wind direction. In column (2), we additionally include three other weather controls, namely solar radiation, average temperature, and precipitation. In column (3), we include all five weather controls in our analysis. Panel B presents additional robustness tests. In columns (1) and (2), we use alternative dependent variables. In column (3), we adapt the model specifications to only allow for areas with at least 10 firms. In column (4), we use standard errors clustered at the five-digit postcode level. In column (5), we exclude all areas with local reforms during the pre-reform years. All regressions include area- and year-fixed effects as well as lagged controls. We report robust standard errors clustered at the area level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Controlling for Weather Conditions					
	(1)	(2)	(3)		
<i>Treatment</i> × <i>Post</i>	-0.0120* (0.0071)	-0.0143* (0.0085)	-0.0125* (0.0070)		
<i>Wind Speed</i>	-0.0307** (0.0155)	-	-0.0287* (0.0154)		
<i>Wind Direction</i>	0.0009*** (0.0003)	-	0.0008*** (0.0003)		
<i>Solar Radiation</i>	-	0.0001* (0.0001)	0.0001** (0.0000)		
<i>Temperature</i>	-	-0.0089 (0.0087)	-0.0114 (0.0088)		
<i>Precipitation</i>	-	0.0037** (0.0017)	0.0019** (0.0010)		
Area FE	Yes	Yes	Yes		
Year FE	Yes	Yes	Yes		
Balanced	Yes	Yes	Yes		
Observations	8,656	8,656	8,656		
Adj.-R ²	0.9519	0.9499	0.9521		
Panel B: Other Robustness Tests					
	(1)	(2)	(3)	(4)	(5)
	None			With other local tax reforms	
Excl. Regions	Baseline		#Firms>10	SE Cluster	Baseline
Specification	NO2	Future NO2		ln(NO2)	
Dep. Variable	NO2	Future NO2		ln(NO2)	
<i>Treatment</i> × <i>Post</i>	-0.4512* (0.2742)	-0.4960** (0.2419)	-0.0122* (0.0071)	-0.0119* (0.0072)	-0.0156** (0.0062)
Area FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Balanced	Yes	Yes	Yes	Yes	Yes
Observations	15,374	15,374	12,780	15,374	14,160
Adj.-R ²	0.9457	0.9333	0.9508	0.9526	0.9507

4.5.3.2. Alternative Dependent Variables

We further test the robustness of our results by using two alternative dependent variables in our main specification. First, we use the absolute amount of NO2, as is common in the literature using similar data (see, for instance, Müller et al., 2022). The results are presented in column 1 of Table 16, Panel B. The coefficient remains negative and significant. While the size

of the first coefficient is not comparable due to its different nature, the statistical significance is not affected. Second, we lag the treatment status by one year (column 2) to capture adoption effects that may take time. Also, for this dependent variable, the DiD coefficient of *Treatment* \times *Post* remains negative and significant.

4.5.3.3. *Alternative Model Specifications*

Next, we test the robustness of our findings to alternative model specifications as we restrict our analysis to certain requirements. First, we further strengthen the assumption that areas are required to have at least five firms to requiring at least 10 firms (column 3 of Table 16, Panel B). The *Treatment* \times *Post* coefficient remains negative and significant, with a very similar magnitude. Second, as areas are not completely independent of each other and share, for example, administrative institutions, we use standard errors clustered at the five-digit postcode level. The main coefficient in column 4 of Table 16 remains significant.

4.5.3.4. *Exclusion of Other Spanish Regions*

As there are many local taxes in Spain, we test the robustness of our main results by excluding those regions that had any kind of tax reform between 2012 and 2014 from our control group. These local tax reforms cover, for example, the introduction of a tax on empty housing in Catalonia in 2015, a gambling/bingo tax in Asturias in 2014, and all environmental tax reforms in the given time period. These reforms can relate to water, waste, or any other environment-related product, or the reform can serve a general environmental purpose. The result of this analysis is shown in column 5 of Panel B. The DiD coefficient remains negative and significant, and even slightly increases in size, indicating a reduction of emissions reform by 1.6%. Overall, these tests indicate that the 2013 emission tax introduction in Valenciana led to a modest decline in NO₂ levels between 1.2% to 1.6%. Hence, these results continue to indicate that the overall response was rather modest in comparison with prior estimates, in particular when using plant-level data (e.g., Klemetsen et al., 2016).

4.5.3.5. *Placebo test*

Next, we perform two placebo tests to address concerns about unobservable local trends. First, we run a test for rural areas without any industrial activity (measurable by us) and compare these areas with all other areas (i.e., urban areas or rural areas with industrial activity). The advantageous feature of this approach is that areas that can be classified as rural without industry are a pseudo treatment group. There should be no response for these areas. However, we expect to find a negative overall effect for all other areas, as urban and rural industrial areas should exhibit a cut in emissions. Results are presented in columns 1 and 2 of Table 17. As expected, the coefficient for rural areas without industry is non-significant and very close to 0. Importantly, we find that for all other areas, there is a modest effect of emission taxes on emission levels. This effect is statistically different from the coefficient of rural areas with industry and non-rural areas, supporting our main inferences. Second, we run a test where we split the effect into areas with fewer versus more cars per capita. If the effect is related (unrelated) to industrial activity, we should find no difference between areas with fewer versus more cars per capita (a higher effect in areas with more cars). The results in columns 3 and 4 support our interpretation. The coefficient estimates are similar for the groups of areas with fewer and more cars. Put differently, the density of cars does not seem to be related to the reduction in emissions due to the emission tax on industrial activity.

Table 17: Placebo Test.

This table shows the results of estimating Equation (5) for a rural area without industry versus a rural area with industry and non-rural areas as well as for areas with more versus fewer cars per capita. In columns 1 and 2, we interact Treatment, Post, and Treatment \times Post with a dummy variable, which is equal to 1 for rural areas without industry and 0 otherwise. In columns 3 and 4, we interact Treatment, Post, and Treatment \times Post with a dummy variable, which is equal to 1 for areas with above-median registered cars per capita and 0 otherwise. All regressions use area- and year-fixed effects as well as lagged controls. We report robust standard errors clustered at the area id level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Breakdown by	Population and # Firms		Cars per Capita	
	Rural with Industry & Non-Rural (1)	Rural without Industry (2)	Low (3)	High (4)
<i>Treatment \times Post</i>	-0.0244*** (0.0083)	0.0014 (0.0083)	-0.0118** (0.0055)	-0.0117 (-0.0117)
<i>Difference</i>	0.0258**		0.0001	
<i>[t-stat]</i>	[2.29]		[0.01]	
Controls	Yes		Yes	
Area FE	Yes		Yes	
Year FE	Yes		Yes	
Observations	15,393		15,374	
Adj.-R ²	0.9528		0.9526	

4.6. Exploring the Heterogeneity in the NO2 Emission-Level Response

We next examine differences in the response to the emission tax. The objective of these tests is to inform policymakers and academics which regions benefit most from changing NOx taxes and in which regions emission taxes may have no, or a much smaller, effect on emissions. Moreover, these tests can help us in assessing (some of) the mechanisms through which emission taxes can reduce emission levels. Finding evidence of reduced emission levels when expected by theory can further corroborate the causal interpretation of our findings.

4.6.1. Estimation Approach

To navigate through these tests, we first present the empirical approach to test for heterogeneity. We consider industrial activity (proxied by number of firms, urbanization, or past emission levels) and firm characteristics (i.e., intangible assets and firm size). Using these characteristics, we perform a triple difference (DDD) analysis based on the following equation:

$$\begin{aligned}
Emissions_{i,t} = & \alpha_0 + \beta_1 Treatment_i \times Post_t \\
& + \beta_2 High\ Split\ Variable_j \times Treatment_i \times Post_t \\
& + \beta_3 Split\ Variable_j \times Post_t + \gamma X_{i,j,t-1} + \alpha_i + \alpha_t + \varepsilon_{i,t} \quad (6)
\end{aligned}$$

Here, $Emissions_{i,t}$ is defined as above. We again include all lagged controls as well as area- and year-fixed effects. Split Variable is a dummy variable equal to 1 if it falls in the high category of the respective split (e.g., area with high industrial activity) and 0 otherwise (e.g., area with low industrial activity). In this model, the interaction $Treatment \times Post$ is the emission effect in the low category of the split variable, and the DDD coefficient $High\ Split\ Variable \times Treatment \times Post$ captures the difference between the high and low groups of the respective split variable. All other interactions of the DDD model are either absorbed by the fixed effect structure or included in the regression but not tabulated for brevity.

4.6.2. Role of Industrial Activity and Emissions

As a first channel, we test whether the industrial activity in an area impacts the reduction of emissions post-reform. Emission taxes are designed to target “dirty” firms or larger industries, as these contribute more to overall emissions. We test this notion and the role of industrial activity by exploring the number of firms within an area (industrial activity measured by the presence of firms), the degree of urbanization (industrial activity measured by urbanization), and past emission levels (industrial activity measured by emissions).

4.6.2.1. Presence of Firms

We start by measuring the industrial activity through the presence of firms. Taking the number of firms, we use *Large Industry Area* as our split variable in Equation (6). The dummy *Large Industry Area* is equal to 1 if an area is in the top quartile in terms of number of firms in an area and 0 otherwise. We conduct the split in 2011 to prevent the reform affecting the location of firms in our sample period. Given that large industry areas are often seen as the main

contributors to pollution, we expect a larger decrease in industrial areas relative to areas with fewer firms. In other words, we expect β_2 to be negative.

The results are shown in Table 18, Panel A. In column 1, we present the overall effect of the interaction $Treatment \times Post$, which is the coefficient β_1 . This coefficient shows the reduction of emissions for areas with a low number of firms ($Large\ Industry\ Area = 0$). The coefficient is close to 0 and not significant, indicating a zero response of areas without much industry. In column 2, we show the overall effect for large industry areas ($Large\ Industry\ Area = 1$), which is calculated as $\beta_1 + \beta_2$ ($Treatment \times Post + Large\ Industry\ Area \times Treatment \times Post$). The results are consistent with the idea that we see a larger reduction in large industry areas given their previously high pollution levels, as the coefficient is negative and significant and larger than the baseline estimate (about 2.5% post-reform reduction of emissions). Importantly, the two coefficients are significantly different from each other (t-stat = -2.09). We subject this finding to similar robustness tests as our main findings. These tests are reported in Panel A of Table D.3 in the Online Appendix and obtain qualitatively similar findings.

Table 18: Emission Taxes and NO2 Pollution Levels, Role of Industrial Activity.

This table shows the results of estimating Equation (6) for number of firms in an area, degree of urbanization, and emission levels. In Panel A, we interact Treatment, Post, and Treatment \times Post with the dummy variable Large Industry Area, which is equal to 1 if an area is in the top quartile in terms of the number of firms in an area and 0 otherwise. In Panel B, we interact Treatment, Post, and Treatment \times Post with the dummy variable City, which is equal to 1 if an area is above the defined population threshold of 50,000 inhabitants and 0 otherwise. In Panel C, we interact Treatment, Post, and Treatment \times Post with the dummy variable High Emissions, representing the percentage of dirty firms in an area above the top quartile. All three splits are executed in 2011. All regressions use area- and year-fixed effects as well as lagged controls. We report robust standard errors clustered at the area id level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Breakdown by Number Firms		
Split Variable	Number of Firms in Area	
	Few (1)	Many (2)
<i>Treatment \times Post</i>	0.0000 (0.0084)	-0.0250*** (0.0091)
<i>Difference</i>		-0.0249**
<i>[t-stat]</i>		[2.09]
Controls		Yes
Area FE		Yes
Year FE		Yes
Observations		15,374
Adj.-R ²		0.9528
Panel B: Breakdown by Urbanization		
Split Variable	Degree of Urbanization	
	Rural (1)	Urban (2)
<i>Treatment \times Post</i>	-0.0096 (0.0065)	-0.0484*** (0.0132)
<i>Difference</i>		-0.0389***
<i>[t-stat]</i>		[2.72]
Controls		Yes
Area FE		Yes
Year FE		Yes
Observations		15,393
Adj.-R ²		0.9526
Panel C: Breakdown by Emission Levels		
Split Variable	% Firms in High Pollution Industry	
	Clean (1)	Dirty (2)
<i>Treatment \times Post</i>	-0.0126** (0.0063)	-0.0098 (0.0099)
<i>Difference</i>		0.0028
<i>[t-stat]</i>		[0.25]
Controls		Yes
Area FE		Yes
Year FE		Yes
Group Size FE		Yes
Observations		15,374
Adj.-R ²		0.9563

4.6.2.2. *Urbanization*

We next use the degree of urbanization to test for industrial activity within an area. Urban areas can generally be characterized by the presence of more firms than rural areas due to advantages in infrastructure, labor markets, and closeness to consumers. For instance, in our sample, in urban areas, we have an average of 612 firms per area, whereas in rural areas we only have 147 firms per area. Thus, the degree of urbanization serves also as a proxy for industrial activity. We then define an area to be a city if its population is 50,000 or above ($City = 1$), following the World Bank's approach (2020), and areas with a population below this threshold to be rural ($City = 0$). When more firms are in an area as indicated by the degree of urbanization, we again expect a stronger response in contrast to rural areas. That is, the coefficient β_2 is expected to be negative.

The results of this analysis are presented in Table 18, Panel B. While we see a negative but nonsignificant coefficient for rural areas in column 1, the overall effect for cities in column 2 is higher and statistically different from the coefficient of rural areas. This indicates that emissions are indeed cut more strongly in urban areas. The reduction in emissions in urban areas (about 2% of our sample) amounts to 4.8%. We subject this finding to several robustness tests, using the absolute amount NO₂ as an alternative dependent variable, adjusting the threshold of firms required to be 15, and using standard errors clustered at the two-digit postcode level (Table D.3, Panel B). These tests generally support the idea of a stronger effect for urban areas.

4.6.2.3. *Emissions of Firms*

Since the split by the number of firms or degree of urbanization does not account for the potential pollution caused by a firm or industry, we next sort areas into those with “dirty” versus “clean” industrial activity. To test if polluters cut emissions more strongly than relatively “clean” firms, we use *High Emissions* as our split variable. We base this measure on the industry

composition in an area in 2011. Each firm is first sorted into “dirty” and “clean” industries based on industry-level NO_x emissions scaled by aggregate sales obtained from Eurostat (2021). Industries above the median of industry-level NO_x emissions are defined as “dirty”. We then calculate the sales-weighted percentage of firms that are classified as “dirty” in an area. This measure provides us an area-specific measure of the prevalence of dirty versus clean firms. We then use this percentage and define a dummy variable, *High Emission*, of the percentage of dirty firms in an area above the median. Since the overall level of economic activity can affect the reform response (see Panel A), we perform the sorting into high- versus low-emission areas within quintiles of the number of firms per area. This ensures that the cutoff to define dirty and clean areas considers the size of the area. Moreover, we modify Equation (6) and include size-quintiles-year fixed effects. These two modifications ensure that we sort on the extent of dirty versus clean firms within similarly sized areas and we compare areas with similar numbers of firms that differ in the density of dirty versus clean firms. If dirty firms react more strongly, i.e., if the “polluter pays” applies, we expect the β_2 to be negative.

The results are presented in Table 18, Panel C. We include group size fixed effects to account for time-invariant characteristics of areas with more firms relative to areas with fewer firms. In column 1, we show the coefficient for “clean” areas (β_1 , *Treatment* \times *Post*). Column 2 shows the coefficient for “dirty” areas ($\beta_1 + \beta_2$, *Treatment* \times *Post* + *High Emissions* \times *Treatment* \times *Post*). The results show negative coefficients for “clean” and “dirty” areas. However, the estimate is only significant for “clean” areas, and the two effects are not statistically different from each other (t-stat = 0.25). This supports the notion that it may not only be the polluters that pay for the emission tax but that clean industries also respond to emission taxes (as evidenced, for example, by the cut in investment among these firms; Jacob and Zerwer, 2022). These results are similar when using absolute NO₂ emission as the dependent variable, when using our dependent variable NO₂ to split into “dirty” and “clean,” and when using standard errors clustered at the five-digit postcode level (see Panel C, Table

D.3, of the Online Appendix). Overall, these results show that emission taxes do not necessarily only target polluters but also “clean” firms.

4.6.3. *Role of Firm Characteristics*

Second, reducing emissions most likely depends on specific firm characteristics that foster and fund technological innovation and new product and process development. Existing literature considers R&D spending and innovation central to reducing emissions and meeting given targets (Acemoglu et al., 2012, 2016, Metcalf, 2019). Further, Brown et al. (2022) and Krass et al. (2013) show that emission taxes can stimulate R&D spending and incentivize the adoption of environmentally friendly technology. This environmentally friendly technology can in turn help firms reduce their emissions through, for example, innovative filter technologies. Thus, more innovative firms are expected to decrease emissions more than their non-innovative counterparts. This idea is also supported by Gerlagh and Lise (2005), who show that carbon taxes are indeed only effective if they induce technological change. We test this idea by looking at intangible assets (as a proxy for R&D activity or affinity to technological advancements) as well as firm size (as a proxy for the availability of resources).

4.6.3.1. *Intangible Assets*

Since our firm-level data do not contain information on R&D spending, we use firms’ stock of intangible assets as a proxy to test this notion. Specifically, we create the split variable *Intangibles*, which is equal to 1 if an area is in the top tercile of the ratio of intangibles over fixed assets in 2011 and 0 otherwise. As we expect firms with more innovations to be able to cut emissions more effectively, we expect β_2 to be negative. Results are presented in Table 19, Panel A. The coefficient for areas with low intangibles-to-fixed-asset ratios is slightly negative, but not significant (β_1). In contrast, the overall effect for areas with high intangibles-to-fixed-asset ratios is negative and significant ($\beta_1 + \beta_2$). This effect, however, is slightly nonsignificantly different from the coefficient for areas with a low intangible ratio (t-stat =

1.50). We again perform several robustness tests in Panel D of Table D.3, Online Appendix, using the absolute amount of NO₂ as the main dependent variable, requiring 15 firms per area, and using standard errors clustered at the five-digit postcode level. Our inferences remain robust in these tests and, more importantly, the difference between low- and high-intangible areas becomes significant, supporting the above interpretation of results. Overall, the results support the idea that firms that innovate and invest in environmentally friendly technologies are better equipped to cut emissions in response to an emission tax in contrast to their non-innovative counterparts.

Table 19: Emission Taxes and NO₂ Pollution Levels, Role of Firm Characteristics.

This table shows the results of estimating Equation (6). In Panel A, we interact Treatment, Post, and Treatment \times Post with the dummy variable Intangibles, which is equal to 1 if an area has an above-median ratio of intangibles over fixed assets and 0 otherwise. The split is executed in 2011. In Panel B, we interact Treatment, Post, and Treatment \times Post with the dummy variable Firm Size, which is equal to 1 if an area's average firm size is in the top quintile in 2011 and 0 otherwise. The split is again executed in 2011. All regressions use area- and year-fixed effects as well as lagged controls. We report robust standard errors clustered at the area id level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Breakdown by Intangibles			
Split variable	Intangible Assets		
	Low (1)		High (2)
<i>Treatment \times Post</i>	-0.0040 (0.0080)		-0.0220** (0.0094)
<i>Difference</i> <i>[t-stat]</i>		-0.0180 [1.50]	
Controls		Yes	
Area FE		Yes	
Year FE		Yes	
Observations		15,374	
Adj.-R ²		0.9528	
Panel B: Breakdown by Firm Size			
Split variable	Firm Size		
	Small (1)		Large (2)
<i>Treatment \times Post</i>	0.0033 (0.0088)		-0.0225** (0.0104)
<i>Difference</i> <i>[t-stat]</i>		-0.0258* [1.95]	
Controls		Yes	
Area FE		Yes	
Year FE		Yes	
Observations		15,377	
Adj.-R ²		0.9523	

4.6.3.2. *Firm Size*

We also use firm size to test for differences across regions. We build our split variable, *Firm Size*, on the natural logarithm of sales. *Firm Size* is equal to 1 if an area's average sales are in the top quartile in 2011 and 0 otherwise. As larger firms potentially have more resources to adapt to new technology and thereby adjust to the new price of emissions, we expect the coefficient β_2 to be negative. Results are presented in Panel B of Table 19. The coefficient for areas with small firms is small and not significant (β_1 , *Treatment* \times *Post*). In contrast, the coefficient for areas with larger firms is negative and significant ($\beta_1 + \beta_2$, *Treatment* \times *Post* + *Firm Size* \times *Treatment* \times *Post*) and significantly different from the coefficient for areas with smaller firms. The estimates suggest a post-reform emission reduction of 2.3%. This supports the idea that firms that are bigger and potentially have more resources to adapt indeed react more strongly after the introduction of the new emission tax. Again, this result is robust to similar tests as above, as shown in the Online Appendix, Table D.3, Panel E.

4.6.4. *Role of Mobile Firms*

In the final step, we examine if firm mobility could be partially responsible for the reduction in emissions. Specifically, MNCs could simply shift their activities from Valenciana to other jurisdictions, either within Spain or outside Spain. As a result, emissions in Valenciana would decrease, but at the same time, these emissions would show up elsewhere. To test this notion, we rerun Equation (6) and interact the variables of interest with MNC, which is an indicator variable equal to 1 if either the local firm is owned by a foreign firm or the local firm is the owner of a foreign subsidiary. With this definition, we capture subsidiaries of MNCs, headquarters of MNCs, and intermediary entities of MNCs. The results are reported in Table 20. We find that when more firms in an area are mobile, that is, the more local firms in an area are parts of MNCs, the effect on emissions is stronger. This could be interpreted as evidence of an NO₂ leakage. Put differently, if it is easier for firms to exit or downsize activities in

Valenciana, the emission reduction is greater, which could suggest that there are spillover effects to other areas to which emitting activities are shifted. This result is consistent with the evidence of carbon leakage (e.g., Babiker, 2005, or Aichele and Felbermayr, 2015) and suggests that emission taxes may also partially result in a shift of emissions to other jurisdictions. While this relocation reduces local emissions, total emissions may be unchanged.

Table 20: Emission Taxes and NO₂ Pollution Levels, Role of Internationally Mobile Firms.

This table shows the results of estimating equation (2), where we interact Treatment, Post, and Treatment \times Post with the dummy variable High MNC, which is equal to 1 if an area has an above-median ratio of sales attributed to firms that are part of MNCs. All regressions use area- and year-fixed effects as well as lagged controls. We report robust standard errors clustered at the area id level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Breakdown by	% of Sales by Multinational Companies	
	Low (1)	High (2)
<i>Treatment \times Post</i>	-0.0034 (0.0070)	-0.0317** (0.0128)
<i>Difference</i> <i>[t-stat]</i>		-0.0283** <i>[2.01]</i>
Controls & FE		Yes
Observations		15,374
Adj.-R ²		0.9529

4.7. Policy Implications and Conclusion

This paper investigates the impact of an emission tax on emission levels, leveraging a local tax on NO_x emissions in Spain in 2013 and area-wide multitemporal satellite data on levels of NO₂. Our results show that the local Spanish emission tax can reduce the actual NO₂ burden by about 1.2%. The effect depends on industrial activity and technological innovativeness. Large industry areas with many firms, highly urbanized areas, and areas with a high degree of innovative or larger firms reduce emissions more in response to the emission tax reform than, for example, rural areas or areas with smaller firms. However, we also find that areas with more dirty industries exhibit no significantly different reduction in NO₂ levels from areas with cleaner industries, suggesting that emission taxes affect not only dirty firms but also cleaner firms, explaining also the rather modest aggregate response.

Our findings thus have important implications for the debate around the optimal design of emission taxes. While the emission tax seems to be effective, leading to a net decrease in

emissions, the net effect is rather modest, even in areas with substantial industrial activity. This results in a relatively high price per ton NO₂ reduction in our sample. Since this price appears to be much higher than the price of abatement investments to reduce NO₂ emissions, we cautiously conclude that emission taxes may not be the best solution. Instead, combining policy measures such as R&D and abatement investment tax credits to support R&D investments and innovation to accelerate the reduction as well as more targeted measures to reduce emissions of NO₂ emitters appear to be more suitable policy tools to achieve net-zero targets.

We acknowledge that our analysis has several limitations. First, while the local setting of the Valencian Community has many advantages, our findings may not generalize to other countries and settings. Related to this, firms may simply move their activities to other countries or jurisdictions, which would imply that our estimate is rather an upper bound of the aggregate effect. Second, the reform in our setting was almost 10 years ago. Advancements in abatement technologies since then are highly likely. Future research can tackle these two issues by using other settings and countries or more recent reforms. Third, as we measure the integrated NO₂ amount from satellite, the contributions from different sectors are mixed, and a direct correlation to emissions from industry can only be made by several assumptions, although we can still see that industrial concentration and urbanization play an important role in the tax effect on emissions. Fourth, due to our empirical matching approach, the treatment and control groups give only limited weight to hot spots such as Barcelona and Madrid. While our findings give some indication about a potential response for more urbanized areas, we cannot make direct statements about large city hubs. Fifth, while complementary policy options in addition to an emission tax might be meaningful to specifically target polluting firms, we cannot make any statements about the effectiveness of such policies. Future research could concentrate on the combined effect of emission taxes and other environmental policy measures.

5. VAT do you eat? Green consumption taxes and firms' market share²⁷

5.1. Introduction

Greening of the tax code has become increasingly popular in recent years, allowing governments to use their fiscal structure to achieve environmental goals and to incentivise firms and consumers to act more sustainably. While newly introduced environmental taxes are a commonly used green tax instrument, more traditional forms of taxation (such as corporate tax or value-added tax (VAT)) are also tailored for sustainability purposes. In December 2021, EU finance ministers agreed on a new set of VAT rules, allowing member states to apply reduced (rates down to 5%) or even so-called 'super-reduced' consumption tax rates (less than 5%) to more categories of services and products, such as organic or local food, electricity from renewable sources and other categories that aim to 'benefit ... the final consumer and pursue objectives of general interest' (European Council 2021). In this paper, I focus on the latter form of green taxes by analysing the example of sustainability-tailored VAT rates. In the EU, revenue from taxes on products accounted for 80.9% of total revenue in 2020, and VAT accounted for 51.7% of the total taxes on products and imports (Eurostat 2021). Thus, a change of the VAT in either direction significantly impacts on tax revenue for the respective member state making a decision about a greener VAT not only in terms of sustainability but also economically.

The impact of reduced VAT rates on consumption behaviour and consumer prices has been studied previously (e.g. Fuest, Neumeier and Stöhlker 2021; Kosonen 2015). However, it remains to be seen how a tax change impacts the competitive environment that corporations operate in, that is, whether the market share of firms whose products are directly affected by the VAT change increases or decreases. It is important to understand this in order to draw a full picture of the policy implications a VAT change can have. Policy makers mostly design

²⁷Single authored paper with the title: "VAT do you eat? Green consumption taxes and firms' market share". The paper has been presented during the Young Scholar Session of the 2022 TRR 266 Annual of Conference as well as during several Brown Bag Seminars. Unpublished working paper.

consumption tax reforms (and decreases) in a manner that targets consumer behaviour and prices – typically without considering consequences for firms.²⁸ However, creating awareness that a change in VAT can promote competitiveness of a certain sector (and thereby boost it economically²⁹) can help policy makers identify additional use cases of this specific measure and use the policy instrument in an overall broader setting.

In theory, when the VAT on a good is lowered, the overall price for the good (product price plus tax) decreases, respectively. In standard models, the change in price in turn leads to an increase in quantity demanded by consumers, an overall lower consumer price on the product and firms receiving a higher price for their good. The wedge between consumer and producer price due to the consumption tax decreases, leading to a benefit for both consumers (by paying a lower price) and producers (by receiving a higher price). This now attracts firms producing other goods into the market, as it is now more attractive to sell lower-taxed goods. If market entry barriers are relatively low (e.g. low additional investment, low legal requirements, etc.) and it is generally easy to switch production from one good to the other, additional firms will then enter the market. For an individual firm, this means that even though it is generally more attractive to sell the lower-taxed good, and the quantity demanded and supplied on an individual firm and industry level increases, the market share of an individual firm can decrease if a sufficient number of new firms enter the market. However, the size of each of these effects depends on the market power/relative elasticity, in addition to barriers to the entry of the individual firms. In regard to market power, when the relative elasticity is high, the quantity response by consumers and the increase in the price received by producers are larger, and the

²⁸For example, see discussions in the *Financial Times* about the cut in VAT for British energy bills (2022, 9 August) or discussions in *The Economist* about a tax on sanitary products (2016, 14 May) that centre on the potential reaction of consumers to a tax reform rather than the impact on firms. In addition, for existing or planned cuts in VAT for food products, articles mostly focus on the impact for consumers, as shown in *Bloomberg* articles for the Lithuanian planned VAT cut for foodstuffs (2022, June 20) the Croatian planned VAT cut for some food products (2022, 30 March) or the Paraguayan proposal to reduce VAT for food (2022, 27 June).

²⁹Various studies and policy papers show the relation between increased competitiveness and economic benefits. For example, in the discussion with Barry Lynn in *The Economist* about why competition matters, the interviewee highlights the benefits of competition for innovation, safety, product prices and employees – all desirable industry aspects for policy makers (2018, 7 August).

reduced price paid by consumers due to the VAT decrease is relatively small. In this case, firms would benefit more from a VAT decrease by receiving a relatively larger share of the price wedge reduction. This would attract more firms into the market, as lower elasticity and a higher supplier rent increase are more likely to outweigh any efforts/investment costs the firm must invest to enter the market. The market share of an individual firm would therefore decrease proportionally more. In regard to market entry barriers, in the case of high entry barriers (e.g. high additional investments, special know-how, etc.), it is relatively difficult for new entrants to enter the market. Thus, the market share of an individual firm would decrease proportionally less, as industry sales are not split between as many parties as when market entry barriers are low and many additional competitors enter the market.

I used the introduction of a reduced VAT rate from 9% to 5% for organic products in Romania in 2018/2019 to test empirically how a VAT decrease influences a firm's market share. I used producers who owned the relevant organic certificate granted by the Romanian Ministry of Agriculture and Farming (the basis for applying the reduced rate) as the treatment group and the remaining comparable firms³⁰ as the control group. This setting has several advantages. First, it allowed for a precise definition of treatment versus control, as the certificates for organic production were directly mapped via addresses and company names to the firms in the Amadeus firm financials sample. Second, while the production method varied (organic versus non-organic), all firms had similar socio-economic conditions, making the treatment and control groups comparable. Third, the cut in VAT rate from 9% to 5% was substantial for organic products, making it economically relevant. Fourth, Prime Minister Viorica Dancila claimed that the VAT cut was intended to increase access to health-beneficial products by 'lowering the price of these products and thus encouraging people to eat healthy

³⁰'Comparable firm' in this case is defined as a firm operating in the same industry as defined by the NACE code as the firms in the treatment group (namely, those firms with NACE code A (Agriculture, forestry and fishing), C (Manufacturing) and G (Wholesale and retail trade)).

products'³¹ (Romania Insider 2019). Fifth, there was limited time between the official policy announcement (May 2019) and implementation (June 2019), not allowing firms to prepare in advance. Finally, the VAT tax cut in May/June 2019 was not confounded by any other major policy events affecting the setting.

In this setting of standalone Romanian firms, the reduction in VAT rate significantly reduced the market share of organic firms in comparison to their non-organic counterpart. The results indicate that the market share decreased by 1% following the reform. With the average market share being only 2.5% across the sample, a 1% decrease was significant. Importantly for the empirical approach applied, I also provide evidence of parallel pre-trends before the reform and that the decrease in market share persisted for at least two years after the reform. The finding is robust in relation to the alternative dependent variable, model specifications and placebo tests.

Results also showed that the decrease in market share depends on the market power/relative elasticity of firms, in addition to market entry barriers. I tested these two channels by using the purchasing power of consumers and the value chain position of firms (market power), in addition to the ease of adapting the business model to organic production and the capital intensity of the business (entry barriers). I found that, consistent with my theoretical predictions, firms with less market power/relative elasticity (i.e. firms located in low-purchasing-power regions and firms further away from the end consumer) experienced a stronger reduction of their market share. Further, firms with higher entry barriers (i.e. firms with a business model that was difficult to switch or with high capital intensity) experienced a weaker reduction of their market share.

³¹While there is no evidence for the VAT reform for organic products, previous similar VAT rate cuts in Romania for foodstuff have also shown that the reduced VAT rate after the reform is reflected in lower consumer prices and higher sales but only for some products such as coffee, dairy products, meat, non-alcoholic beverages, and snacks (Nielsen Romania, 2015).

This research contributes to the literature in three ways. First, it contributes to the debate on how consumption taxes can influence corporations in addition to consumers. While one stream of literature claims that consumption taxes fully fall on consumers (e.g. Besley and Rosen 1999; Marion and Muehlegger 2011), which is also used as a common claim by policy makers, others argue that consumption taxes directly affect producers and consumers (e.g. DeCicca, Kenkel and Liu 2013; Jacob, Michaely and Müller 2019; Kenkel 2005; Poterba 1996). Especially with regards to agricultural products and changes in taxation, the existing focus mostly lies on influencing consumer behavior (Fearne et al. 2021). My findings show an additional mechanism – a decrease in market share following a VAT reduction for certain firms – through which a change in consumption taxes can affect corporations in addition to being designed by policy makers to target certain consumption patterns.

Second, it adds to the literature analysing the market effects of policy instruments in the agricultural sector. Some of the existing literature shows that direct agricultural support schemes have rather limited market-distorting effects (Haß 2021) while other authors propose a decoupling, reducing or even abolishing of direct payments to the agricultural sector (Boulanger and Philippidis 2015; Uthes 2011; Binfield et al. 2003). My findings add to this by showing empirically that also indirect support via a change of taxation specific to agricultural products can have market effects via a decrease in market share of individual firms and intensified overall competition.

Third, this work contributes to the policy debate about VAT decreases – among them the recent discussion about a ‘green’ VAT. Decreasing VAT is typically discussed as a tool to guide consumption behaviour by, for example, promoting the purchase of a certain product. This can be done for health considerations, as in the example of a ‘green’ VAT, but also more broadly to promote the consumption of certain goods, as in the example of decreased VAT rates following COVID-19. The impact on firms and the competitive environment often only plays a minor role. This research therefore enriches the policy debate on the impact of decreased VAT

by directly addressing the impact on corporations in addition to the existing consumer view. I show that although the Romanian VAT reform was intended to trigger healthier eating behaviours among consumers, it also influenced the competitive environment of Romanian organic firms by decreasing the market share while boosting overall industry and firms' sales. Thus, for VAT cuts on other products outside the sustainability debate, the market environment of firms should also be considered. A VAT cut could be a useful addition to the policy toolkit if the promotion of a whole sector is desired via increased competition.

5.2. Consumption Taxes and Market Share: Hypothesis and Setting

5.2.1. Hypothesis Development

In theory, when the VAT on a good is lowered, the overall price for the good (product price plus tax) decreases, respectively. In standard models, the change in price in turn leads to an increase in quantity demanded by consumers. With a lower tax, consumers pay an overall lower price on the product, and firms receive a higher price for their good. The wedge between consumer and producer price due to the consumption tax decreases, leading to a benefit for both consumers (by paying a lower price) and producers (by receiving a higher price).

For the case at hand, imagine a market with two similar goods that are sold – organic products (good₁) versus non-organic products (good₂). The VAT for good₁ now is reduced. The consumer price for good₁ falls, whereas the producer price rises. Both consumers and producers benefit from the VAT reduction. This now attracts firms producing good₂ into the market, as it is now more attractive to sell good₁. If market entry barriers to sell good₁ now are relatively low (e.g. low additional investment, low legal requirements, etc.), and it is generally easy to switch production from good₁ to good₂, many additional firms previously producing good₂ will now enter the market of good₁. For an individual firm, this means that even though it is generally more attractive to sell good₁, and the quantity demanded and supplied on a good₁ industry level and individual firm level increases, the market share of an individual firm can decrease if a

sufficient number of good₂ firms enter the good₁ market and increased industry sales are split by more firms.

This seems to be particularly plausible for organic products. While it requires a few months to acquire the respective certificate, it is relatively easy for a food producer to switch from the production of non-organic goods to organic goods. Table 21 shows the organic standards in Romania. Most requirements to receive an organic certificate need some investment but are relatively easy in that they do not require a reorganisation of the full production process. For example, the requirement of ‘enhancement of soil fertility’ would require the firm to use specific types of fertilisers that are less aggressive and do less harm to the local soil. Thus, in theory, it seems likely that firms producing good₂ would switch to good₁ if supplier rents were higher post-reform, making the market entry attractive and outweighing any additional market entry costs. Due to the intensified competition and industry sales being split by more firms, the market share of an individual firm decreases. Therefore, I propose the following hypothesis:

H₀: Following a VAT rate reduction, the market share of an individual firm decreases.

However, the size of each of the effects, i.e. how much the price wedge really is reduced and the distribution among individual producers of the reduction gains, depends on the relative elasticity of demand and supply (i.e. the market power of the individual firm) and the difficulty of entering the respective sub-market (i.e. the market entry barriers).

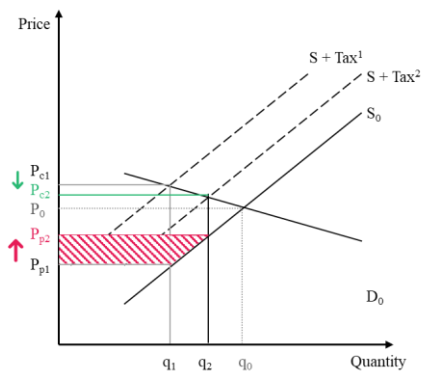
Table 21: Organic Standards Romania.

This table presents the organic standards in Romania that must be met by a firm converting from conventional to organic farming according to EU organic regulation 2018/848.

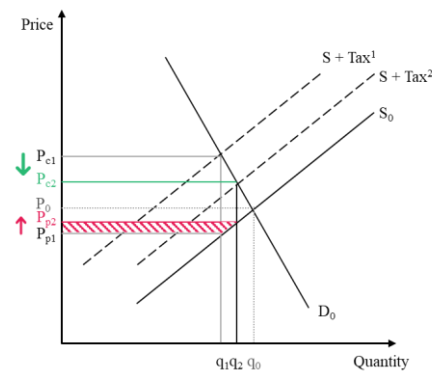
Area	Requirement
Resources	Responsible use of energy and natural resources
Biodiversity	Maintenance of biodiversity
Ecological balances	Preservation of regional ecological balances
Soil fertility	Enhancement of soil fertility
Water quality	Maintenance of water quality
Animal welfare	High standards of animal welfare and specific behavioural needs of animals

In regard to the market power/relative elasticity of firms, when demand is more elastic (first quadrant of Figure 16), the quantity response by consumers and the increase in the price received by producers are larger, and the reduced price paid by consumers due to the VAT decrease is relatively small. In this case, suppliers would benefit more from a VAT decrease by receiving a relatively larger share of the price wedge reduction. In turn, when demand is more inelastic (second quadrant of Figure 16), the quantity response by consumers is smaller. Suppliers receive only a slightly higher price than before (due to a small quantity increase). Benefitting most from a VAT decrease in this case are consumers who face a significantly smaller price paid. Thus, the wedge again decreases but this time more for the benefit of consumers. When supply is more elastic (third quadrant of Figure 16), there is no difference in quantity consumed relative to inelastic supply, but the price increase for suppliers is lower than for their more inelastic counterpart (fourth quadrant of Figure 16). In other words, the more elastic the supply is, the less the supplier benefits from a decreased VAT by receiving a higher price (see Jacob et al. 2019 for a similar argument for a VAT decrease).

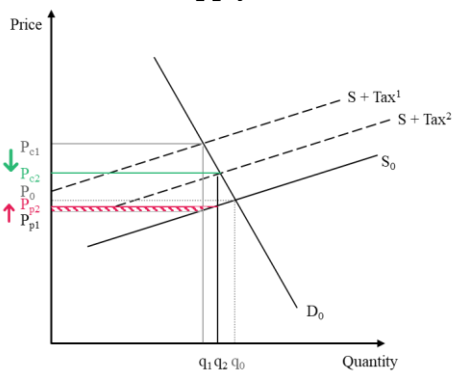
More Elastic Demand



More Inelastic Demand



More Elastic Supply



More Inelastic Supply

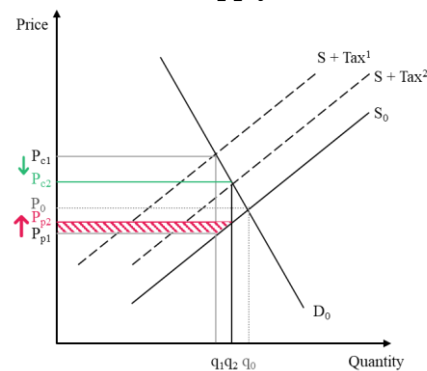


Figure 16: VAT Change and Elasticity.

This graph shows the differences in benefits for producers and consumers of a VAT decrease depending on the level of supply or demand elasticity.

Imagine once again a firm selling good₁. This firm faces highly elastic demand. In this case, the price paid by consumers stays almost constant, while the producers receive a higher price. Almost the entire benefit of the VAT decrease thereby falls onto the good₁-producing firm. This would attract relatively more firms into the market, as lower elasticity and a higher supplier rent increase are more likely to outweigh any efforts/investment costs the firm must invest to enter the market. For an individual firm facing elastic demand, this would mean that the market share decreases proportionally more than for their inelastic counterpart, as more firms are attracted to the market.

In regard to market entry barriers, higher market entry barriers can protect firms operating in the respective sub-market from a loss of market share to competitors and industry sales. That is, sub-industry sales are divided by a lower number of firms; thus, the potential to lose the

market share is lower. High market entry barriers could be, for example, a large number of additional capital investments or specific know-how required to produce the respective good.

Imagine a firm that is producing good₁, organic pork. This firm has relatively high entry barriers, as it requires specific knowledge to produce organic pork, in addition to further capital, as more space is required to farm organic pigs than non-organic pigs, i.e. the organic requirements need substantial additional investment. Thus, a non-organic pig farmer would find it relatively difficult to switch to organic production, as the additional investment required is high and potentially even higher than the gained benefits post-reform. The incumbent organic pig farmer would face fewer competitors entering the market and thus a lower loss of the market share.

5.2.2. *Exploiting value added taxes in Romania*

For my empirical analysis, I exploited the cut in VAT for organic products from 9% to 5% in Romania in 2019 in a difference-in-difference setting (DiD)³². Following the official announcement on 14th May 2019, the law was published on 23rd May 2019 and took effect within a month on 1st June 2019. The cut was introduced to increase Romanians' access to healthy products (Romania Insider 2019). This was as a response to the 'EU Health at a Glance: Europe 2018' report, which describes Romania as the country with the second lowest life expectancy and healthy life years at birth (only higher than Bulgaria), with the main cause of death being cardiovascular and cerebrovascular diseases that are often directly linked to insufficient nutrition and unhealthy consumption habits (OECD and European Commission 2019). Discussions about the new law began immediately after the report was published in early 2018. The law itself came into force on 1st June 2019 after being pre-published on 23rd May 2019. The full timeline of events is provided in Figure 17.

³²For further details on the actual legal text, please refer to Government Emergency Ordinance no. 31/2019 regarding the granting of tax incentives and the amendment of Law no. 227/2015 regarding the Fiscal Code.



Figure 17: Timeline.

This timeline shows the order of events of the announcement and implementation of the reduced VAT for organic products law in Romania 2019 and the relationship to recent EU legislation.

The reduced VAT of 5% applies to the delivery of high-quality goods approved by the Ministry of Agriculture and Rural Development, i.e. food certified as a mountain, organic or traditional product (see Table 21 for certificate requirement). In order to be granted the reduced VAT rate, food producers must have a respective certificate issued by an inspection and certification body for organic products approved by the Ministry of Agriculture and Rural Development. Product packaging must include the correct labelling, such as ‘mountain product’ or the national logo ‘ae’ for organic products (see Online Appendix Figure C.1 for available official logos used for labelling products). A list of all certified producers in each year is made available publicly on the Ministry website.

This specific Romanian setting had several advantages for analysis. First, it allowed for a precise definition of treatment versus control groups, as the certificates for organic production could be directly mapped via addresses and company names to the firms in the sample. This allowed me to identify the specific producers who would potentially benefit from the VAT decrease versus those that were still subject to standard VAT rates for food. While retailers and wholesalers can sell non-organic and organic food, most producers exclusively produce organic food when holding the certificate. In addition, organic firms in Romania only sell very locally and do not ship food products broadly within the country or export, leading to better identification of the location of the actual economic activity and consumption behaviour and minimising spillover effects. Second, while the production method varied (organic versus non-

organic), all firms had similar socio-economic conditions, making the treatment and control groups comparable. While certain regional differences in Romania exist (urban versus rural or mountain regions versus non-mountain regions), all companies in my sample were subject to the same national policies, economic developments and social trends. Third, the cut in VAT rate was substantial for organic products, from 9% to 5%, making it economically relevant. With a yearly Romanian VAT revenue of €64 billion (Eurostat 2021) and a market share of ~1% for organic food products (USDA Foreign Agricultural Service 2017), this would translate to missed revenue of ~€290 million (all else remaining constant). As VAT tax revenue is often the main source of tax income for governments, this revenue gap is substantial. Fourth, the VAT cut was intended to increase access to healthy products. Indeed, most newspaper articles reporting on the VAT cut reference high mortality rates due to cardiovascular and cerebrovascular diseases and increasing obesity in Romania linked to unhealthy consumption patterns as the main reason for the policy (e.g. Neagu 2019). Fifth, the limited time between the official announcement of the reform (May 2019) and the date it came into effect (June 2019) did not provide firms with sufficient time to adapt, making the cut appear sudden. Figure 17 shows the exact order of events. In addition, although I included the year 2020 in my analysis, when COVID-19 lockdowns began, I did not anticipate that the crisis would have an impact on findings, as the food production and retail industries were not directly affected by the events. Finally, the VAT tax cut in May/June 2019 was not confounded by any other major policy events affecting this setting. In May 2019, a national Romanian referendum was held on whether the government could pass emergency orders and additional regulations against corruption officers; I did not anticipate that it would have an impact on the setting, as it was only distantly related to the agricultural sector. Further, newly introduced tax laws in 2019 included a new asset tax for banks, a turnover tax for utility firms, a new tax incentive regime for construction firms, a turnover tax for telecommunication firms, updated health insurance benefits for software developers and a gambling tax (Florescu 2019). These events were not

directly related to the study setting, as they targeted a different industry; therefore, I anticipated no impact. Overall, this setting therefore provided a tax cut relevant to two groups: a specific group of firms subject to the cuts and another group not subject to them. Both groups shared similar local economic conditions, thereby minimising any potential unobserved effects.

5.3. Empirical Setting and Data

5.3.1. Amadeus Data

To generate the panel of Romanian firms from 2014 to 2021, I used all available data on Romanian firms from Bureau van Dijk's Amadeus database. I used unconsolidated financial statements of private, unlisted corporations (in contrast to the consolidated balance sheet information of Compustat Global) in order to locate the activity of a single firm rather than using consolidated data per group. Standalone firms, i.e. those firms not belonging to a multinational or domestic group, have the advantage that they cannot relocate their activity as easily and operate locally, thereby providing better insights about the local Romanian market structure (Drake et al. 2021; Jacob and Vossebürger 2022). The Romanian agricultural market is structured so that most firms produce and sell goods very locally without shipping them to other regions of the country or exporting. This further supports the assumption of economic activity and impact when using standalone firms.³³ For this research, firms were required to exist for more than two years to allow for sufficient information before and after the VAT reform. I excluded all firms with total assets below €50,000 and fixed assets below €5,000, in addition to firms with negative total assets, fixed assets, cash and sales and with missing information for earnings before interest and taxes (this approach is similar to that used by Jacob et al. 2019, who also used Amadeus firm data to observe the impact of a consumption tax on firms). In addition, I only used firms with the NACE codes A, C or G (as those are the industries that are theoretically able to obtain an organic certificate) to make the control group more

³³I confirmed this assumption in interviews with Romanian agriculture experts.

appropriate.³⁴ Using these codes, firms included in the sample with NACE code A were local farmers producing vegetables, fruits, cereal, crops, nuts and oil plants, in addition to stockbreeders, winemakers, forest product farmers (mushrooms and wild berries) and beekeepers. Firms included with NACE code C were organic product processors, such as fresh and frozen food processors, but also processors of organic ingredients for other purposes, such as cosmetics. Finally, firms with the NACE code G were wholesalers and retailers. A detailed listing of included types of firms is included in Table D.4 of the Online Appendix. These selection criteria resulted in a sample of 71,174 observations from 14,776 firms from 2014 to 2021.

5.3.2. *Romanian Ministry of Agriculture and Rural Development Data*

Information on whether a firm held a certificate was collected from the Romanian Ministry of Agriculture and Rural Development website by downloading all available certificates from 2014 to 2021 (>15,000 certificates per year) and matching it to the previously prepared Amadeus data manually via company name and address. As different authorities are allowed to issue such certificates, and they all differ in terms of their exact layout, each certificate was opened manually, the company name searched in the Amadeus data via the search function and quality checked against the address to avoid falsely including firms with the same or a similar name. Out of the overall sample of 71,174 observations from 14,776 firms, I identified 589 observations³⁵ from 233 firms to be organic certificate holders at the time of the reform.

³⁴While more precise NACE codes were used at a later point of the analysis, I first stayed on the broader letter NACE code level to include as many firms as possible in the original sample.

³⁵Many of the organic certificate holders listed on the website of the Romanian agriculture ministry are very small-scale farmers that, due to reporting thresholds, do not appear in the Amadeus data and therefore cannot be matched. Thus, there was a larger gap between certificates issues (>15,000 certificates) versus firms identified (233 firms).

5.3.3. Descriptive Statistics

Table 22 presents the descriptive statistics for the sample of the combined Amadeus and organic certificate data. The variable definitions are listed in the Appendix. All variables were winsorised at the 1st and 99th percentiles, with the exception of the sales variable, which was winsorised at the 10th and 90th percentile due to unusually large outliers unsuitable for analysis³⁶. On average, firms had a market share of 2.5% (1.0% for a three-digit NACE code). The average firm had gross investments of 1.7%, a leverage of 0.6%, return on assets of 6.7%, sales growth of 3.8% and cash holdings of 12.7%.

Panel C of Table 22 illustrates how the treatment group (*Organic* = 1) compared to the control group (*Organic* = 0). While changes in leverage and sales growth were statistically comparable between the two groups, there was a significant difference between the treatment and the control group for gross investment, leverage, return on assets, firm size and cash. For instance, organic firms were, on average, larger than the non-organic firms in the sample and had a higher return on assets and larger sales growth but lower cash holdings. I addressed these concerns by using an entropy balanced sample (balanced by firm size) for all analysis beyond the initial baseline regression (also see Jacob and Zerwer 2022). Descriptive statistics for the treatment and control groups using the balanced approach are shown in Panel D of Table 22.

³⁶Main results were also confirmed using the ‘standard’ winsorisation for sales at the 1st and 99th percentiles.

Table 22: Descriptive Statistics.

This table presents the descriptive statistics of the main variables for 71,174 observations from 2014 to 2021. Appendix B defines the variables.

Panel A: Dependent Variables					
Variable	Mean	St. Dev.	25th Perc.	Median	75th Perc.
Market Share (4-digit)	0.0250	0.0962	0.0003	0.0017	0.0096
Market Share (3-digit)	0.0097	0.0523	0.0002	0.0008	0.0036
Panel B: Other Firm Variables					
Organic	0.0067	0.0816	0	0	0
Post	0.3491	0.4767	0	0	1
Gross Investment	0.1724	0.3777	0	0	0
Change in Leverage	0.0001	0.0133	0	0	0
Leverage	0.0059	0.0266	0	0	0
Return on Assets	0.0674	0.2177	0	0.0416	0.1493
Sales Growth ³⁷	0.0378	0.2834	-0.1230	0.0364	0.1963
Firm Size	13.4895	1.4667	12.3726	13.3500	14.4530
Cash	0.1273	0.1710	0.0138	0.0552	0.1711
Panel C: Difference between Treatment and Control					
	Organic = 0	Organic =1	Difference		
Market Share (4-digit)	0.0246	0.0851	-0.0605***		
Market Share (3-digit)	0.0096	0.0344	-0.0248***		
Gross Investment	0.0259	0.0933	-0.0673***		
Change in Leverage	0.0001	0.0015	-0.0014		
Leverage	0.0057	0.0349	-0.0292***		
Return on Assets	0.0673	0.0894	-0.0222***		
Sales Growth ⁵	0.0377	0.0508	-0.0131		
Firm Size	13.4788	15.0715	-1.5927***		
Cash	0.1276	0.0894	0.0381***		
Panel D: Difference between Treatment and Control in Balanced Panel					
	Organic = 0	Organic =1	Difference		
Market Share (4-digit)	0.0386	0.0807	-0.0421***		
Market Share (3-digit)	0.0149	0.0325	-0.0176***		
Gross Investment	0.0699	0.1063	-0.0364***		
Change in Leverage	-0.0004	0.0015	-0.0019		
Leverage	0.0231	0.0265	-0.0034		
Return on Assets	0.0728	0.0904	-0.0176**		
Sales Growth ⁵	0.0336	0.0552	-0.0216		
Firm Size	14.9133	14.9146	-0.0013		
Cash	0.1027	0.0971	0.0056		

5.3.4. Estimation Strategy

My empirical approach exploited the reduction of the VAT rate in Romania in 2019 in a DiD setting using Romanian firms that held an organic certificate (as of June 2019) as the treatment group and comparable firms with the NACE codes A (Agriculture, Forestry and

³⁷Winsorised at 90% due to outliers

Fishing), C (Manufacturing and Processing) and G (Wholesale and Retail Trade) as the control group. This led to the following equation:

$$MS_{i,t} = \alpha_0 + \beta_1 Organic_i \times Post_t + \gamma X_{i,j,t-1} + \alpha_{ind,t} + \varepsilon_{i,t} \quad (7)$$

where the dependent variable $MS_{i,t}$ was the sales of an individual firm i at year t over industry sales (by NACE code and per year). The variable *Organic* was equal to one for Romanian firms that held an organic certificate and zero otherwise. The cut-off date to count as an organic firm (and therefore have $Organic = 1$) was the day the reform was officially introduced, i.e., 1st June 2019. Firms obtaining a certificate after this cut-off date were not included. As it is possible to obtain a certificate on any day of the year, this allowed for a precise definition of the treatment group. The variable *Post* was equal to one for 2019, 2020 and 2021 and zero otherwise. The regression sample included four years prior to the reform (2017, 2016, 2015, 2014), the years of the reform (2018, 2019) and two post-reform years (2020, 2021). The main variable of interest was the coefficient of the interaction $Organic \times Post$, which was the DiD coefficient. I expected the coefficient to be negative, given that sales on an industry level may have increased after the reform, attracting many new competitors and leading to a decrease in an individual firm's market share ($\beta_1 < 0$).

I further included a vector of control variables ($X_{i,j,t-1}$), building on prior literature (Almeida and Campello 2007; Campello 2003; Fresard 2010). All control variables were lagged by one year ($t - 1$). Specifically, I controlled for firm size (*Firm Size*, defined as the natural logarithm of sales), cash holdings (*Cash*, defined as cash holdings over total assets) and return on assets (*Return on Assets*, defined as net income over total assets). I further included industry-year fixed effects ($\alpha_{ind,t}$) to ensure that I compared treated and control firms in the same industry. I clustered standard errors at the firm level to account for potential correlations within a firm.

5.3.5. Identifying Assumptions

I used a DiD approach to test for the impact of the VAT, and the assumption of this approach was that, absent of the VAT reform, the market share of the treatment and control groups would have evolved in the same way. I therefore tested for parallel trends before the VAT reform in 2019. This was achieved by testing for five years before the reform, i.e. from 2014 until 2019 when the reform was officially introduced. Following Patel and Seegert’s approach (2017), Figure 18 shows the difference in the market share between the treatment and control groups for the five pre-reform years, in addition to the 95% confidence intervals. The figure shows that organic and non-organic firms’ market share followed a parallel trend before the introduction of the VAT reform. I further validated this by testing if the sum of the three coefficients was different to zero. I did not find a significant joint effect ($p = 0.235$), supporting parallel pre-trends.

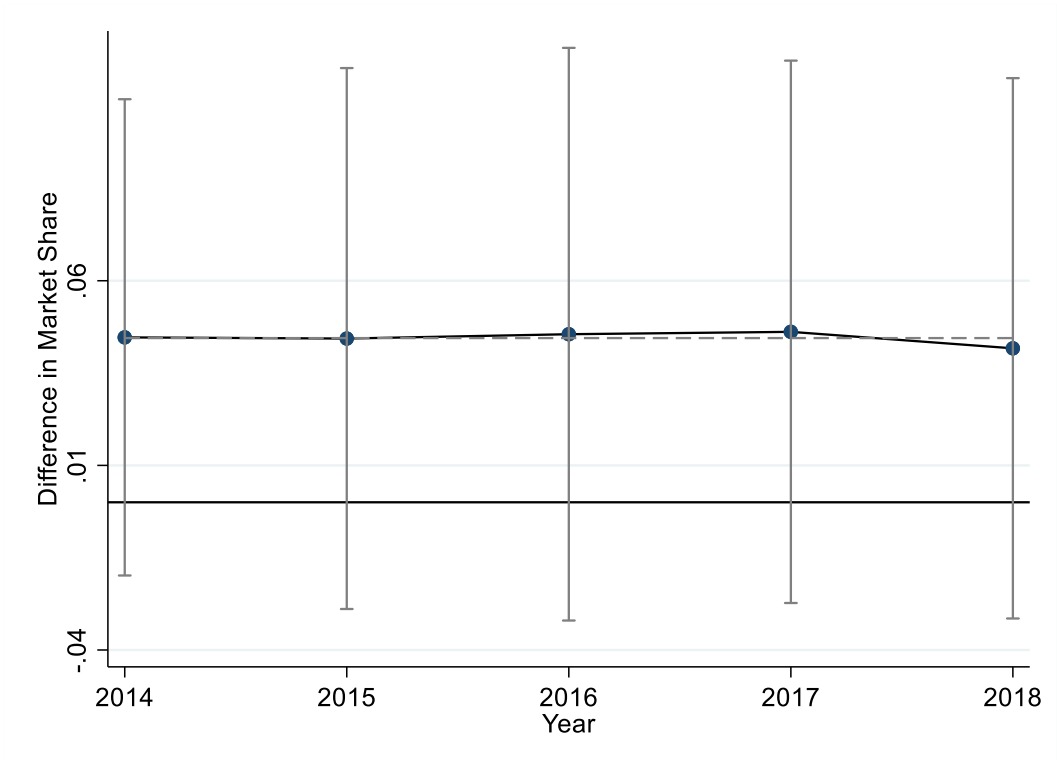


Figure 18: Parallel Trends. This figure shows the parallel trends of the treatment (organic, traditional and mountain farmers) versus the control group before the VAT cut in 2019. The 95% confidence bounds are based on standard errors clustered at the firm level. The dashed line indicates the pre-reform average difference.

5.4. Baseline Results

Table 23: VAT and Market Share, Main Results.

This table presents the main results of the analysis using an unbalanced panel and an entropy balanced approach. The primary dependent variable was the market share, defined as a firm's sales over industry sales (by four-digit NACE code). The primary independent variable was the interaction between Organic and Post. All control variables were lagged by one year. Robust standard errors clustered at firm level are reported in parentheses. The entropy balanced approach balanced on all control variables and four-digit NACE codes. I included industry-year fixed effects in columns 2 and 4. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively.

	(1)	(2)	(3)	(4)
	Baseline Approach		Entropy Balanced Approach	
<i>Organic</i>	0.0654*** (0.0210)	0.0505** (0.0200)	0.0509** (0.0212)	0.0493*** (0.0166)
<i>Post</i>	-0.0015*** (0.0004)	-	-0.0026** (0.0011)	-
<i>Organic</i> × <i>Post</i>	-0.0134** (0.0058)	-0.0139*** (0.0053)	-0.0124** (0.0059)	-0.0116** (0.0047)
<i>Firm Size</i> _{<i>t-1</i>}		0.0175*** (0.0008)		0.0281*** (0.0050)
<i>Cash</i> _{<i>t-1</i>}		0.0077** (0.0039)		-0.0304 (0.0201)
<i>Return on Assets</i> _{<i>t-1</i>}		0.0018 (0.0020)		0.0479** (0.0241)
Industry-Year FE	No	Yes	No	Yes
Observations	71,174	71,174	71,174	71,174
Adj.-R ²	0.0027	0.2128	0.0249	0.2600

Table 23 shows the results of the baseline regression for the DiD coefficient of *Organic* × *Post*, with organic firms as the treatment and all other firms as the control group. The baseline approach is shown in columns 1 and 2. In column 1, Equation (7) was estimated without controls or industry-year fixed effects, whereas in column 2, both were added to the regression. The coefficient of *Organic* × *Post* for both regressions was negative and significant, suggesting that an organic firm's market share decreased after the VAT reduction. This implies that an individual firm's response to a decrease in VAT is a decrease in market share if it attracts many new firms into the respective market and leads to a change in the competitive environment. In columns 3 and 4, an entropy balanced sample was used to account for differences in firm characteristics, and Equation (7) was run again without controls and fixed effects (column 3) and with controls and fixed effects (column 4). There was a negative and significant effect for the DiD coefficient *Organic* × *Post*, indicating that the market share of an individual organic

firm decreased. Importantly, the magnitude of the coefficient remained similar in columns 1 to 4. The results indicate that a firm’s market share decreased by about 1% after the reform. Relative to the average market share of the sample (2.5%), a loss of 1% is substantial economically. Figure 19 shows the average yearly market share of an organic firm versus a non-organic firm in Romania. While the market share was fairly stable from 2014 to 2017, there was a slight decrease from 2017 to 2018 when the reform was first discussed and an even more drastic decrease from 2018 to 2019 and from 2019 to 2020 after the reform was implemented.

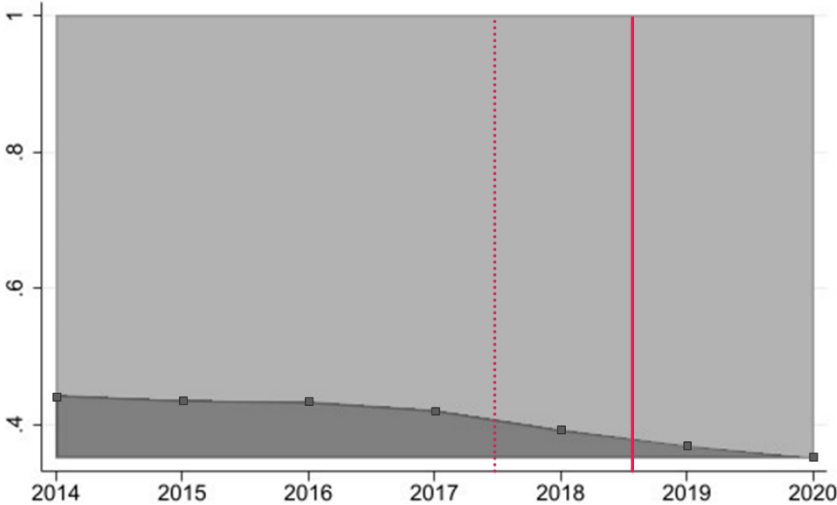


Figure 19: Market Share of Organic Firms. This figure shows the average yearly market share of an individual organic firm in Romania before the reform (2014–2017), during planning (2018) and after the reform (2019–2020) relative to non-organic firms.

5.4.1. *Timing of the Response*

Next, I explored the dynamics of this market share response by expanding Equation (7) and including an interaction of *Organic* with year indicators for 2018, 2019 and 2020, respectively. I thus separately observed any response in the year when discussions about the reform started (2018), the year of the legal implementation of the VAT reduction (2019) and one year post-reform (2020). I separately performed this analysis using the baseline approach, including controls and fixed effects (column 1, Table 24), and applied an entropy balance approach

(column 2, Table 24). The baseline approach showed a slight anticipatory effect in 2018³⁸. This may be because it can take up to 12 months for firms to obtain an organic certificate within the European Union (European Commission 2022), and some farmers may have started with the conversion earlier, as discussions started in 2018. Both columns show that the market share response was the strongest in 2019 and still strong but less significant in 2020. The coefficients for 2019 and 2020 were again fairly similar in size to the main analysis results in Table 23. This seems plausible, as most firms were likely attracted into the market in an early stage right after the law was introduced in 2019. The fact that the market share response remained significant for at least two years after the reform suggests that the associated change in the competitive environment is persistent.

Table 24: VAT and Market Share, Breakdown by Years.

This table presents the results for the timing analysis, including year indicators for 2018, 2019 and 2020 in the baseline regression. The primary dependent variable was market share, defined as a firm's sales over industry sale (by four-digit NACE code). The primary independent variable was the interaction between Organic and Post. All control variables were lagged by one year. Robust standard errors clustered at firm level are reported in parentheses. All results were generated using an entropy balanced approach and included industry-year fixed effects. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively.

	(1) Baseline Approach	(2) Entropy Balanced Approach
<i>Organic</i> × 2018	-0.0094** (0.0043)	-0.0054 (0.0037)
<i>Organic</i> × 2019	-0.0161*** (0.0058)	-0.0127*** (0.0048)
<i>Organic</i> × 2020	-0.0169** (0.0068)	-0.0135** (0.0058)
Controls	Yes	Yes
Industry-Year FE	Yes	Yes
Observations	71,174	71,174
Adj.-R ²	0.2128	0.2600

³⁸As the entropy balanced approach did not show any anticipatory effects, this again confirmed my choice to use this method for all remaining analysis instead of only using the baseline approach.

5.4.2. Robustness Tests for Baseline Findings

I assessed the robustness of the main findings by testing for alternative dependent variables, different model specifications and general industry and regional trends.

Table 25: Robustness Tests, Main Results.

This table presents the result of the robustness tests. In Panel A, I defined the baseline regression using alternative dependent variables (columns 1 to 3) and alternative model specifications (columns 4 to 7). In column 1, the main dependent variable was the market share, this time using three-digit NACE code to calculate industry sales. In column 2, I used the natural logarithm of market share as the dependent variable. In column 3, I winsorised the market share at 5% and 95% to generate the dependent variable. In columns 4 and 5, I used standard errors clustered at the two-digit and four-digit NACE code level respectively. In columns 6 and 7, I added gross investment and leverage as additional control variables to the model. In Panel B, I tested for general food industry trends by defining the treatment group to be all food producers except for organic firms. The main independent variable remained for all regressions in the interaction of Organic (Treatment) and Post. Except for columns 4 and 5, all standard errors were clustered at firm level. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively.

Panel A: Dependent Variable					
	(1)	(2)	(3)		
	3-digit	ln	Win. at		
	Market Share	(Market Share)	5% and 95%		
<i>Organic</i> ×	-0.0048	-0.1198*	-0.0080*		
<i>Post</i>	(0.0032)	(0.0631)	(0.0046)		
Panel B: Model Specifications					
	SE Cluster		Controls		Fixed Effects
	(1)	(2)	(3)	(4)	(5)
	2-digit	4-digit	Gross	Leverage	Firm Fixed
	Cluster	Cluster	Investment		Effects
<i>Organic</i> ×	-0.0116**	-0.0116**	-0.0109**	-0.0102**	-0.0056*
<i>Post</i>	(0.0055)	(0.0063)	(0.0057)	(0.0048)	(0.0032)
Controls	Yes	Yes	Yes	Yes	Yes
Ind.-Y. FE	Yes	Yes	Yes	Yes	Yes
Balanced	Yes	Yes	Yes	Yes	Yes
Obs.	71,174	71,174	71,174	71,174	56,037
Adj.-R ²	0.2600	0.2600	0.2616	0.2683	0.9769
Panel C: Testing for General Industry Trends					
Treatment	Food industry excluding organic producers/retailers by 3-digit NACE code				
Control	All other non-food industry producers/retailers by 3-digit NACE code				
<i>Treatment</i>	-0.0101				
× <i>Post</i>	(0.0079)				
Controls	Yes				
Industry-Year FE	Yes				
Balanced	Yes				
Observations	41,333				
Adj.-R ²	0.1709				

5.4.2.1. Alternative Dependent Variables

Table 25, Panel A, shows the three alternative dependent variables. First, I used the three-digit NACE code to define the market share of firms (column 1). The coefficient remained negative and close to statistical significance, supporting the overall tendency of a decrease in the market share. This is not surprising, as the definition of the food industry is granular, and

the use of three-digit NACE codes better supports this granularity. I also used the natural logarithm of the market share (four-digit)³⁹ as the dependent variable in column 2. The coefficient again remained negative and significant, supporting the main findings. As a last alternative dependent variable, I winsorised the market share at the 95th percentile to test whether the main results were driven by larger outliers, given the small size of the sample (column 3). The coefficient for the DiD interaction of *Organic* × *Post* remained negative and significant.

5.4.2.2. *Model Specifications*

Table 25, Panel B shows the alternative standard error clusters used to test for the robustness of the model specifications, additional controls and a different fixed effect structure. In regard to alternative standard error clusters, I used standard errors clustered at the two-digit NACE code level (column 1) and at the four-digit NACE code level (column 2). Both coefficients of *Organic* × *Post* remained negative and significant, supporting the main finding of the baseline regression. In addition to being negative and significant, the size of the coefficients remained stable in comparison to the baseline regression. Further, I added controls to Equation (7) to test for alternative explanations driving the decrease in the market share. First, as shown in column 3, I added gross investment, following research showing a relation between a firm's investment behaviour and its market share (e.g. Jiang et al. 2015). The coefficient of the interaction *Organic* × *Post* remained negative, significant and similar in magnitude. Additionally, as shown in column 4, I added leverage, following prior research (e.g. Almeida and Campello 2007; Campello 2003; Fresard 2010) indicating a relationship between market share and financial strength or capital structure of firms. Again, *Organic* × *Post* showed a significant negative coefficient, in line with the main finding of a decreased market share for organic firms after the

³⁹I yielded similar findings using the natural logarithm of market share based on the three-digit NACE code.

reform.⁴⁰ As a last robustness test adapting the model specifications, I included firm fixed effects. Results for this test are presented in column 5 of Panel B. The DiD coefficient again remained negative and significant. As the sample already included only a limited number of treated firms, and firm fixed effects absorbed some proportion of the sample (56,037 instead of 71,174 observations), I continued the main analysis without firm fixed effects and relied on industry-year fixed effects.

5.4.2.3. *Placebo Tests*

As a last robustness test for the main findings, I tested for two general trends – an industry trend and a regional trend – that could lead to bias in the results. First, it could be argued that the negative coefficient of *Organic* × *Post* was driven by a general downward trend in the Romanian food industry (for both organic and non-organic products). To test for this notion, I created a pseudo-treatment group including all food industry firms (producers and retailers) and excluding the organic producers and a pseudo-control group of all other non-food related firms based on the three-digit NACE code. The result of this analysis is presented in panel C of Table 25. While the coefficient of the interaction *Treatment* × *Post* was negative, it was not statistically significant, indicating that there was no general industry trend driving the main results. Additionally, it could be argued that the trend was not specific to the food industry but could also be seen in any other retail or wholesale sector. To test for this, I performed a set of placebo tests examining a pseudo-reform in 2019 (Table 26). In panel A (panel B), I defined the pseudo-treatment to include firms of any other wholesale (retail) industry, such as the sale of motor vehicles or sale of household goods. I tested the pseudo-treatment group against a control group including all other wholesalers (retailers) except for those selling foodstuffs. Most DiD coefficients were not significant, with the exception of the coefficients for the wholesale

⁴⁰In untabulated tests, I simultaneously added both additional controls to the equation, with the same finding of a negative and significant coefficient of the interaction *Organic* × *Post*.

of vehicle parts (Panel A, column 3), retail of motor vehicles (Panel B, column 1) and other special sales (Panel B, column 6). As the coefficients were either pointing in the other direction (positive) or close to zero (other special sales), I did not consider them to have any influence on the analysis.

Table 26: Placebo Tests, Wholesale and Retail.

This table presents the results of several placebo tests. In Panel A (Panel B), I defined the pseudo-treatment group to include other firms of the wholesale (retail) sector and the pseudo-control to include all other wholesalers (retailers) except for foodstuff firms. In Panel C, I defined the pseudo-treatment group to include all non-foodstuff firms and the pseudo-control to include all foodstuff firms. All regressions included lagged control variables. Standard errors (in parentheses) were clustered at firm level. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively.

Panel A: Wholesale									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Pseudo-Treatment	Sale Motor Vehicles	Repair Motor Vehicles	Sale Parts Vehicles	Sale Motor Cycles	Sale Goods House	Sale IT Equip-ment	Sale Machi- nery	Other Special. Sale	Non-Special. Sale
NACE Code	451	452	453	454	464	465	466	467	469
Pseudo-Control	All other wholesalers except foodstuff								
<i>Organic</i> × <i>Post</i>	-0.0025 (0.0026)	0.0016 (0.0011)	0.0024** (0.0011)	-0.0250 (0.0213)	0.0010 (0.0017)	-0.0085 (0.0054)	-0.0036 (0.0026)	0.00069 (0.0014)	0.0012 (0.0014)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12,593	12,593	12,593	12,593	12,593	12,593	12,593	12,593	12,593
R ²	0.1533	0.1533	0.1533	0.1534	0.1623	0.1577	0.1569	0.1611	0.1643
Panel B: Retail									
	(1)	(2)	(3)	(4)	(5)	(6)			
Pseudo-Treatment	Sale Motor Vehicles	Retail Auto Fuel	Retail Comms Equipment	Sale Goods House	Sale Culture Goods	Other Special. Sale			
NACE Code	471	473	474	475	476	477			
Pseudo-Control	All other retailers except foodstuff								
<i>Organic</i> × <i>Post</i>	0.0016*** (0.0005)	0.0005 (0.0004)	-0.0042 (0.0026)	-0.0007 (0.0005)	-0.0018 (0.0052)	-0.0008* (0.0005)			
Controls	Yes	Yes	Yes	Yes	Yes	Yes			
Observations	22,494	22,494	22,494	22,494	22,494	22,494			
R ²	0.0961	0.0490	0.0749	0.0506	0.1096	0.0525			

Second, it could be argued that it was not an industry-specific trend but a trend specific to the region affecting all food producers in Eastern Europe at this time. To test for this, I prepared a panel data set for Hungarian firms from 2014 to 2021 using Amadeus financial data, following the same steps as for the Romanian panel. I then randomly assigned firms that could potentially obtain an organic certificate to the treatment group (i.e. firms in the food producing or processing industry, retailers and wholesalers) and assigned the remaining firms to the control

group. The regression specification remained the same as for the Romanian sample using industry-year fixed effects, selected controls and a balanced panel. The results of this test are shown in Table 27. The DiD coefficient was not significant, independent of whether I used a non-balanced or balanced panel, fixed effects or controls. Particularly in the main specification using industry-year fixed effects and control variables, the DiD coefficient was very close to zero. Thus, this also did not seem to be a regional trend but specific to Romanian organic food producers.

Table 27: Placebo Tests, Neighbor Country.

This table presents the results for a placebo test using firm data from Romania’s neighbor Hungary to test for regional trends of the food sector. Robust standard errors clustered at firm level are reported in parentheses. All results were generated using an entropy balanced approach and included industry-year fixed effects. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively.

	(1)	(2)	(3)	(4)
	Baseline Approach		Entropy Balanced Approach	
<i>Treatment × Post</i>	-0.0035 (0.0027)	-0.0007 (0.0022)	-0.0038 (0.0027)	-0.0017 (0.0018)
Industry-Year FE	No	Yes	No	Yes
Observations	9,828	9,804	9,828	9,804
Adj.-R ²	0.0200	0.9182	0.0141	0.8996

5.4.3. Changing the Competitive Environment

5.4.3.1. Late Movers

My analysis generally suggests that the reduction of VAT for organic products in Romania has attracted new firms into the market, leading to an overall decrease in the market share for organic producers and retailers. However, it is useful to observe what type of firms were attracted, despite being unable to draw any causal inference from pure descriptive statistics. Within the sample, 48 so-called ‘late movers’ were attracted to the organic market post-reform in 2019, i.e. firms that obtained an organic certificate after 1st June 2019. This is a substantial number of firms given that the sample contained 233 organic firms during the reform. This is also evident in Figure C.2 of the Online Appendix, which shows (in light grey) a sharp increase in the overall number of organic firms per year from 2018 to 2019 driven by these late movers. Descriptive statistics for these late movers are shown in Table 28. Late movers differed in some

characteristics from the other firms in the sample. On average, they had a larger gross investment and were bigger in size. This seems reasonable, as a switch to the organic market requires significant investment and is mostly done by larger firms. In regard to their financial structure (leverage and change in leverage), return on assets, cash holdings and sales growth, late movers did not differ from other firms in the sample.

Table 28: Characteristics of Late Mover Firms.

This table presents descriptive statistics for the 54 Romanian firms moving into the organic market after the reform in 2019 (late movers).

	Late Mover=0	Late Mover=1	Difference
Market Share (4-digit)	0.0250	0.0377	-0.0127**
Market Share (3-digit)	0.0097	0.0148	-0.0050**
Gross Investment	0.0262	0.0868	-0.0606***
Change in Leverage	0.0001	0.0015	-0.0014
Leverage	0.0059	0.0153	-0.0094
Return on Assets	0.0674	0.0645	0.0029
Sales Growth	0.0379	0.0093	0.0285
Firm Size	13.4863	14.4288	-0.9425***
Cash	0.1273	0.1173	0.0101

5.4.3.2. *Industry and Firm Sales*

As shown above, the individual firms' market share decreased, and the number of firms in the organic market increased, indicating an increase in competition within the sector. To make a more precise statement about the economic boost due to the reform, I also wanted to observe whether industry sales and individual firms' sales changed while the individual market share decreased. For this additional test, I used Equation (7) for the regression and re-estimation, using industry sales growth and individual firms' sales growth as the dependent variables. Results are shown in Table 29. Both coefficients were positive and significant. The organic sector sales grew by approximately 6.7% post-reform, and individual firms' sales grew slightly less by about 4.8%. Therefore, while individual firms lost market share due to the intensified competition, the reform still had an economically positive impact by growing sales on both the firm and industry level.

Table 29: Industry and Firm Sales.

This table presents the results for the regression analysis of industry sales growth (column 1) and individual firms' sales growth (column 2). Robust standard errors clustered at firm level are reported in parentheses. All results were generated using an entropy balanced approach and included firm and industry-year fixed effects. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively.

	(1) Industry Sales Growth	(2) Individual Firm Sales Growth
<i>Treatment × Post</i>	0.0669*** (0.0071)	0.0480** (0.0227)
Controls	Yes	Yes
Industry–Year FE	Yes	Yes
Balanced	Yes	Yes
Observations	70,881	66,164
Adj.-R ²	0.9922	0.1450

5.4.3.3. *Measures to Compete*

While the above analysis seems to support the idea of intensified competition post-reform, it was also important to investigate how firms competed, show in Table 30. In column 1, the profit margin (defined as sales minus material and employee costs over sales) of treated firms improved significantly. While initially counterintuitive to the finding that the individual market share of firms decreased, it seems reasonable when considering that individual firms' sales increased. In addition, it is also likely that firms optimised their costs as a response to intensified competition in order to become more efficient and thereby also improve their profit margin. I tested for this notion by examining two different cost components (employee costs and material costs) that can lead to an improvement. First, I tested for the average wage of employees.⁴¹ As shown in column 2, the average wage per employee decreased for the treated firms, indicating that firms potentially cut employee costs to become more efficient and compete against the intensified competition. Particularly in Romania and the agricultural sector, this seems to be a likely measure, with seasonal worker availability being high and family members often helping extensively, making salary adjustments much more flexible than in other cases. I also tested for material costs. Results in column 3 indicate that firms cut material costs following the reform

⁴¹The average wage is defined as total staff expenses divided by the number of employees. I required at least five employees in this test.

but not significantly. Thus, while the individual firms suffered a loss of market share following the reform, the intensified competition led to firms cutting costs significantly for employees and at least partially (even though not significantly) for material inputs, thereby leading to an overall optimisation of firms' profit margins.

Table 30: Competition Measures.

This table presents the results of tests for how firms competed following the introduction of the reduced VAT. Column 1 shows the regression results for using the profit margin as the dependent variable, column 2 shows the results for using salary per employee and column 3 shows the results for using scaled material costs. Robust standard errors clustered at firm level are reported in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively.

Panel A: Competition Measures			
	(1) Profit Margin	(2) Salary	(3) Material Costs
<i>Treatment × Post</i>	0.1072** (0.5160)	-0.0427* (0.0218)	-0.0254 (0.0559)
Controls	Yes	Yes	Yes
Industry–Year FE	Yes	Yes	Yes
Balanced	Yes	Yes	Yes
Observations	61,284	4,786	4,702
Adj.-R ²	0.0012	0.9416	0.5170

5.5. Mechanism

5.5.1. Elasticities and Market Power

It seems reasonable that the overall market share of organic firms decreased following the reduction of the VAT in Romania. However, based on the anecdotal evidence and existing literature on consumption taxes (e.g. Jacob et al. 2019), I also wanted to observe whether the effect depended on relative market power/relative elasticity and market entry barriers.

A first proxy for relative elasticity can be the geographic position of a firm. Rural areas can be expected to have more elastic demand for organic products than urban areas. Consumer demand is generally more elastic for products that are regarded as ‘nice-to-haves’ by consumers, that are perceived to have many substitutes, or the consumer has no meaningful other attachment to the product (e.g. branding). In urban areas, education about differences between organic and non-organic food is likely to be higher, and consumers likely appreciate organic products more. Further, in urban areas, self-production of food is limited. As the terms ‘local’ and ‘organic’ are often seen as substitutes for food (Meas et al. 2015), consumers in

urban areas have fewer options for direct substitutes in comparison to rural consumers that can more easily self-grow products. Anecdotally, this seems to be particularly relevant in the case of Romania as a former socialist country, as many Romanians used to self-produce all food necessary for their everyday life. This holds especially true in the more rural areas outside of the capital, Bucharest, where people still grow their own food instead of buying it in the supermarket. After adaption to a more Westernised lifestyle and the opening of additional commercial supermarket chains, especially in rural areas, people still do not acknowledge the difference between organic and non-organic products, as food is always considered to be ‘organic’ in their self-grown sense, and consumers simply buy what is cheaper and/or self-grow their food (see Băncescu 2021 for an analysis of the Romanian rural/urban dynamics and the importance of subsistence agriculture outside of the capital, Bucharest). People in Bucharest in turn are more aware of the difference, acknowledging the environmental and health benefits of organic food and, given the lack of opportunity to self-grow food, are more willing to buy organic products. Thus, I expected demand to be less elastic in the capital region, Bucharest, in comparison to the rest of the country. Given the above, I expected the market share of an individual firm to decrease proportionally more in rural areas than in urban areas of Romania.

To test for this notion, I split the sample into high-purchasing-power areas (*High Purchasing Power* = 1) and low-purchasing-power areas (*High Purchasing Power* = 0). The respective areas are shown in Figure 20, with the dark grey area representing the high-purchasing-power areas (the Romanian capital, Bucharest, and nearby regions) and light grey areas representing the low-purchasing-power areas (remaining regions of Romania). I performed the split based on the GDP per inhabitant in € in 2017 (Eurostat 2022) to avoid any unwanted effects of the reform in 2019 on the purchasing power. I then combined this indicator variable *High Purchasing Power* with the coefficient *Organic* × *Post*, leading to a triple difference (DDD) model with the following equation:

$$\begin{aligned}
MS_{i,t} = & \alpha_0 + \beta_1 Organic_i \times Post_t \\
& + \beta_2 Organic_i \times Post_t \times High\ Purchasing\ Power_j \\
& + \beta_3 Post_t \times High\ Purchasing\ Power_j + \gamma X_{i,j,t-1} + \alpha_{ind,t} + \varepsilon_{i,t}
\end{aligned} \tag{8}$$

where the dependent variable, $MS_{i,t}$, is the sales of an individual firm i at year t over industry sales (by NACE code and per year). I again include lagged controls and industry-year fixed effects. In this triple difference model, the coefficient of $Organic \times Post$ captured the effect on market share in low-purchasing-power regions, and the DDD coefficient $Organic \times Post \times High\ Purchasing\ Power$ captured the difference in market share between firms operating in high-purchasing-power regions versus low-purchasing-power regions. Consistent with the idea that firms in urban areas face less elastic demand and higher entry barriers, I expected the coefficient $\beta_2 > 0$.



Figure 20: Purchasing Power in Romania.

This figure shows the Romanian two-digit zip code areas with high purchasing power (dark grey) and those with low purchasing power (light grey).

The results are presented in Table 31, Panel A, columns 1 and 2. There was a positive significant coefficient for firms operating in high-purchasing-power regions and a negative significant coefficient for firms operating in low-purchasing-power regions. Importantly, the

two coefficients were significantly different to each other. This supports the idea that firms in urban areas indeed face less intense competition post-reform, as demand is less elastic, attracting fewer additional firms into the market, and their overall market power is stronger.

Another proxy for elasticity is the position of firms along the value chain. Firms closer to the consumer are typically more powerful than firms further away from the consumer. Especially for food producers, it seems reasonable to assume less market power because of the seasonality of their production process, the associated time lag in adjustment and their smaller scale in comparison to larger supermarket brands close to the consumer. Anecdotal evidence from Romania supports this assumption, as most supermarkets selling organic products are part of a larger brand and/or collaborate with each other, whereas farmers are often small-scale, without much independent power and/or collaboration between each other. Thus, given the lower market power of producers, I expected the negative effect to be stronger for producers/processors than for retailers/wholesalers.

To test for this second proxy for market power/relative elasticity, I again split the sample into firms further away from the end consumer ($Far=1$) and firms closer to the end consumer ($Far = 0$). I defined firms far away from the end consumers to be food processors and wholesalers, whereas firms closer to the consumer were retailers. I removed firms with the NACE code A (farmers) for the main analysis, as it was not clear whether they were close to the end consumer; this is because many Romanian farmers have small shops selling their products directly to the end consumer, particularly in rural areas⁴². I again combined the indicator variable Far with the coefficient $Organic \times Post$, leading to a triple difference (DDD) model with the following equation:

$$MS_{i,t} = \alpha_0 + \beta_1 Organic_i \times Post_t + \beta_2 Organic_i \times Post_t \times Far_i \quad (9)$$

$$+ \beta_3 Post_t \times Far_i + \gamma X_{i,j,t-1} + \alpha_{ind,t} + \varepsilon_{i,t}$$

⁴²For example, see Ion (2012), who sees direct sales from farmers as one of five important distribution channels of organic food in Romania.

where the dependent variable, $MS_{i,t}$, was the sales of an individual firm i at year t over industry sales (by NACE code and per year), lagged controls and industry-year fixed effects were included and the coefficient of $Organic \times Post$ captured the effect on market share of firms operating close to consumers. The DDD coefficient $Organic \times Post \times Far$ captured the difference in market share between firms operating close to the consumer versus further away. Consistent with the idea that firms closer to the end consumer faced less intense competition post-reform, I expected the coefficient $\beta_2 < 0$.

The results are shown in Panel A of Table 31, columns 3 and 4. While the two coefficients for firms far away from and close to the consumer remained negative, the coefficient for firms operating closer to the consumer was smaller than the coefficient for firms further away from the consumer, indicating that firms indeed faced less intensified competition if they had a more elastic supply. The difference between the two coefficients is significant, supporting this notion.

Overall, the results therefore suggest that although the VAT reform decreased the market share for an individual organic firm by attracting many new competitors, firms with more market power could profit from the reform or at least lose less market share, whereas their counterparts with less market power could not and lost more.⁴³

⁴³This result can also be supported by looking at the number of recent farmers in high-purchasing-power areas versus low ones. The majority of new entrants can be seen in low-purchasing-power areas (~98%), while only a few new firms have entered the market in high-purchasing-power areas (~2%), and the overall number stays fairly constant.

Table 31: Market Power and Entry Barriers.

This table presents the results of the heterogeneity splits. Panel A shows the results of the triple difference analysis for firms' position in terms of purchasing power (columns 1 and 2) and value chain (columns 3 and 4). I combined Organic, Post and Organic×Post with the dummy variable High Purchasing Power (Far), which was equal to 1 if a firm operated in a high-purchasing-power region measured by GDP per capita (far away from the end consumer) and zero otherwise. Panel B shows the results of the triple difference analysis for firms' business model adaptability and capital intensity. I combined Organic, Post and Organic×Post with the dummy variable Difficult Switch (High Capital Intensity), which was equal to 1 if a firm had a business model that was difficult to adapt to organic standards (had a high capital intensity of its business model) and zero otherwise. I conducted all splits in 2017. I used lagged controls and industry-year fixed effects for all regressions. Robust standard errors clustered at firm level are reported in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively.

Panel A: Market Power				
Split by	Purchasing Power		Value Chain Position	
	Low (1)	High (2)	Close (3)	Far (4)
<i>GDP 2017 per Inhabitant (in €)</i>	9,540	26,600		
<i>Organic × Post</i>	-0.0123** (0.0048)	0.0271** (0.0108)	-0.0066*** (0.0018)	-0.0164*** (0.0042)
<i>Difference High vs. Low [t-stat.]</i>	0.0393*** [3.00]		-0.0099** [-2.38]	
Controls	No		Yes	
Industry–Year FE	No		Yes	
Balanced	Yes		Yes	
Observations	71,174		56,014	
Adj. R ²	0.2663		0.2723	
Panel B: Entry Barriers				
Split by	Model Adaptability		Capital Intensity	
	(1) Easy	(2) Difficult	(3) Low	(4) High
<i>Organic × Post</i>	-0.0112** (0.0052)	-0.0077 (0.0097)	-0.0251** (0.0107)	-0.0055 (0.0049)
<i>Difference [t-Stat]</i>	0.0042* [2.56]		0.0195** [2.05]	
Controls	Yes		Yes	
Industry–Year FE	Yes		Yes	
Balanced	Yes		Yes	
Observations	71,174		68,179	
Adj.-R ²	0.2625		0.2773	

5.5.2. Barriers to Entry

In addition to the market power of firms, I also tested for the role of market entry barriers as an explanation for differences in the response of firms. Firms with higher market entry barriers face less competition post-reform, and I expected them to experience a smaller decrease of the market share than their counterparts with lower market entry barriers.

As a first proxy for market entry barriers, I used the ease of switching the business model from non-organic to organic. Examples of business models that make it easy to switch are, for example, growing fruits and vegetables or growing crops that only require minor efforts (e.g. use of different fertilisers, different harvesting methods, etc.) to switch from non-organic to organic. Examples of business models that make it harder to switch, for instance, are those that would require additional space to produce organic goods (e.g. livestock farming), have complex production processes and need for specialised know-how (e.g. wine producers) and/or require larger capital investments (e.g. specialised manufacturers for organic cosmetic products).

To test for this proxy for market entry barriers, I split the sample into firms with a business model that was hard to adapt to organic standards (*Model Adaptability*=1) and firms with a business model that was easy to adapt to organic standards (*Model Adaptability* = 0). I performed the split according to an assessment of the above-mentioned criteria of each individual business model. That is, I examined whether a switch from non-organic to organic production would require an extensive change of business processes, additional knowledge and/or larger capital investments⁴⁴. I then combined the indicator variable *Model Adaptability* with the coefficient *Organic* × *Post*, leading to a triple difference (DDD) model with the following equation:

$$\begin{aligned}
 MS_{i,t} = & \alpha_0 + \beta_1 Organic_i \times Post_t & (10) \\
 & + \beta_2 Organic_i \times Post_t \times Model\ Adaptability_i \\
 & + \beta_3 Post_t \times Model\ Adaptability_i + \gamma X_{i,j,t-1} + \alpha_{ind,t} + \varepsilon_{i,t}
 \end{aligned}$$

where the dependent variable, $MS_{i,t}$, was the sales of an individual firm i at year t over industry sales (by NACE code and per year), lagged controls and industry-year fixed effects were included and the coefficient of *Organic* × *Post* captured the effect on market share of firms

⁴⁴I performed this assessment by evaluating each organic certificate individually by its activity description and ordering the respective firms on a scale from 1 to 4, where 1 was easiest to switch and 4 was hardest to switch. The easy-to-switch regression included firms ordered into 1 and 2 and hard-to-switch firms ordered into 3 and 4.

with an easy-to-switch business model. The DDD coefficient $Organic \times Post \times Model$ *Adaptability* captured the difference in market share between firms with an easy-to-switch versus a hard-to-switch business model. Consistent with the idea that firms with an easy-to-switch business model face more competition, I expected the coefficient $\beta_2 < 0$.

Results are shown in Table 31, Panel B, columns 1 and 2. Following the notion that those with a business model that is easier to switch from non-organic to organic face more intense competition after the tax reform, there was a negative and significant effect for firms with an easy-to-switch business model. In contrast, there was a negative and non-significant coefficient for those with a hard-to-switch business model. Both coefficients were statistically significantly different from each other.

As a second proxy for market entry barriers, I used the capital intensity of the business. For more capital-intense firms, it is potentially harder to switch from non-organic to organic production, as the adaptation of processes may require additional investment compared to firms with low-capital-intensity processes that can adapt more easily and quickly by simply rearranging standard labour processes. Examples of capital-intense firms are food processors, fisheries and specialised food producers such as wine makers. Examples of less capital-intense firms are those producing crops or fruits and vegetables that are often still hand-picked in Romania.

To test for this proxy for market entry barriers, I split the sample into firms with high capital intensity ($High\ Capital\ Intensity = 1$) for firms with an above median capital intensity and firms with low capital intensity ($High\ Capital\ Intensity = 0$), whereas capital intensity was defined as depreciation over a firm's total assets. I combined the indicator variable *High Capital Intensity* with the coefficient $Organic \times Post$, leading to a triple difference (DDD) model with the following equation:

$$\begin{aligned}
MS_{i,t} = & \alpha_0 + \beta_1 Organic_i \times Post_t \\
& + \beta_2 Organic_i \times Post_t \times High\ Capital\ Intensity_i \\
& + \beta_3 Post_t \times High\ Capital\ Intensity_i + \gamma X_{i,j,t-1} + \alpha_{ind,t} + \varepsilon_{i,t}
\end{aligned} \tag{11}$$

where the dependent variable, $MS_{i,t}$, was the sales of an individual firm i at year t over industry sales (by NACE code and per year), lagged controls and industry-year fixed effects were included and the coefficient of $Organic \times Post$ captured the effect on the market share of firms with low capital intensity. The DDD coefficient $Organic \times Post \times High\ Capital\ Intensity$ captured the difference in market share between firms with high and low capital intensity. Consistent with the idea that firms with lower capital intensity face more competition, I expected the coefficient $\beta_2 < 0$.

Results for this test are shown in Table 31, Panel B, columns 3 and 4. There was a negative and significant coefficient for firms with low capital intensity and a negative but smaller and non-significant coefficient for firms with high capital intensity. Both coefficients were statistically different from each other. Thus, these results support the notion that firms with higher capital intensity as an entry barrier lost less market share than their counterparts with a lower capital intensity.

5.5.3. Robustness Tests for Heterogeneity of Results

As the heterogeneity tests were based on various assumptions, I performed robustness tests for the split according to purchasing power and value chain position (market power/relative elasticity tests). I also tested business model adaptability and capital intensity (market entry barrier tests) by adapting the definition of treatment and control and model specifications. All results can be found in the Online Appendix, Table D.5 and confirm the above analysis of the two channels that explain differences in the response.

5.6. Conclusion

This paper investigated the role of a reduction in VAT in changing the competitive environment of firms by examining the impact on an individual firm's market share. Results showed that, following a reduction in the VAT on organic products in Romania, the market share of firms decreased due to additional firms entering the organic market immediately after the reform, even though industry and individual firms' sales increased. The effect depended on the market power/relative elasticity of firms and market entry barriers. Firms with less market power and firms with lower market entry barriers lost more market share to competitors than their counterparts, with respectively more market power or higher entry barriers.

The findings of this study have implications for the debate on using differentiated VAT rates to promote sustainability targets. While policy makers often consider consumer behaviour only when designing such policies, my findings show that reducing VAT rates for a certain product category can also have implications for firms producing, processing or trading such goods. For example, although a decrease in an organic VAT rate can promote healthier consumption patterns, it also leads to intensified competition for the lower-taxed good due to new entrants entering the market, as it becomes more attractive to sell the respective good. This conclusion has implications for the Romanian market. Romania is often mentioned by the EU as the country with the largest potential for a large organic agricultural market and put at the centre of discussions about how to reach long-term (food) sustainability goals (see interview with Canga Fano, Principal Adviser of the European Directorate-General for Agriculture and Rural Development 2022). While officials now could see the 2019 VAT reform as a general success by promoting consumption locally, my findings show that it also has implications for Romanian firms. The reduced VAT – at least in this local example – led to intensified competition for the respective good and additional firms entering the market. Thus, it did not only potentially promote consumption but also had a positive impact on production by making the market more competitive, larger and eventually more efficient.

This research has several limitations. First, while the setting allowed for a very accurate mapping of organic certificates awarded to firms, the sample was still limited in size and locality. Future research could address this by taking this approach to a larger setting once more countries use differential VAT rates for sustainability purposes, as currently, a ‘green’ VAT is the exception and not the rule in most countries, with EU legislation only very recently adapting and supporting a variation in rates. Second, while I examined the market share of firms as a measure for change in competitiveness, I did not perform a full analysis of the competitive environment of firms and cannot draw any definitive conclusions about it. Third, due to the recent nature of the reform, my findings are limited to a relatively short period after the reform (2019, 2020, 2021). Future research could explore long-term consequences of a decreased VAT once a permanent reduction in VAT is sustained over a longer period.

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6. Concluding Remarks

While policy makers claim to have found the cure to many environmental problems by using fiscal policy measures as a tool to fight climate change, the work of this dissertation provides only mixed evidence in support of this claim. In my analysis of newly introduced forms of taxation, I not only show that emission taxes come with an economic cost but also that those economic costs are not making the polluter pay but instead the weakest firms, i.e., those that do not have the capacity to adjust. Further, in addition to the sizeable and not targeted economic costs of emission taxes found in the analysis, they also only seem to have a moderate effect on emissions. Again, also the emission effect is not targeted to polluting firms but rather is the strongest for those that have the most capacity to be innovative. Thus, emission taxes as a standalone policy tool appear not to be the first best options. In my analysis of the adaptation of traditional forms of taxation (namely VAT) to green standards, I find that the 'green' adaptation of a VAT can foster a whole sector via promoting competition as well as the entrance of new firms into a 'green' market. This indeed can be something very desirable in addition to the promotion of consumption as often intended by VAT changes.

However, even though especially the results of the work on new forms of taxation (i.e., emission taxes) are somewhat disillusioning, this dissertation can provide guidance for further research and policy makers. While emission taxes are on average found to have large economic costs and low effect in reducing the targeted substance, the results also show that they might be more effective for a certain type of firm with higher capacity to innovate. That is, if emission taxes are combined with other policy measures (e.g., grants that foster innovativeness of firms), they might become a much more targeted and efficient policy tool.

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Conferences and Presentations

Date	Conference Name (Location)	Title Paper of Dissertation	Title of Presentation
January 13, 2022	TRR Brownbag Seminar (Zoom)	Who Bears the Burden? Evidence from Capital Investments	Environmental Taxes and Investment
February 4, 2022	Egyptian Online Seminars (Zoom)	Who Bears the Burden? Evidence from Capital Investments	Environmental Taxes and Investment
March 11, 2022	UNC Tax Symposium (Chapel Hill, USA)	Who Bears the Burden? Evidence from Capital Investments	Environmental Taxes and Investment
May 12, 2022	EAA (Bergen, Norway)	Who Bears the Burden? Evidence from Capital Investments	Environmental Taxes and Investment
September 13, 2022	CBT Doctoral Conference (Oxford, UK)	Who Bears the Burden? Evidence from Capital Investments	Environmental Taxes and Investment
September 20, 2022	WHU Brownbag Seminar (Zoom)	VAT do you eat? Green consumption taxes and firms' market share	VAT do you eat?
October 13, 2022	WHU Brownbag Seminar (Zoom)	How Effective are Emission Taxes in Reducing Air Pollution?	Environmental Taxes and Air Pollution
November 10, 2022	TRR Brownbag Seminar (Zoom)	How Effective are Emission Taxes in Reducing Air Pollution?	Environmental Taxes and Air Pollution
November 23, 2022	TRR Annual Conference (Munich, Germany)	How Effective are Emission Taxes in Reducing Air Pollution?	Environmental Taxes and Air Pollution
November 17, 2022	OMG Transatlantic Tax Talks (Zoom)	How Effective are Emission Taxes in Reducing Air Pollution?	Environmental Taxes and Air Pollution
December 14, 2022	ESADE (Barcelona, Spain)	How Effective are Emission Taxes in Reducing Air Pollution?	Environmental Taxes and Air Pollution
February 15, 2023	DLR (Munich, Germany)	How Effective are Emission Taxes in Reducing Air Pollution?	Environmental Taxes and Air Pollution
March 3, 2023	Ifo (Munich, Germany)	How Effective are Emission Taxes in Reducing Air Pollution?	Environmental Taxes and Air Pollution

Appendix

Appendix A: Theoretical Calculations

This table provides details for the theoretical calculations of chapter 2.

Demand before tax: $p = 100 - q$

Demand after tax: $p = 100 - q - dt$

Supply: $p = q$

$\Delta q = q - q_t$

Before Tax

Perfect Competition

Demand = Supply

$100 - q_{\text{comp}} = q_{\text{comp}}$

$q_{\text{comp}} = 50, p_{\text{comp}} = 50$

Monopoly

Marginal Revenue = Marginal Costs

$100 - 2q_{\text{mon}} = q_{\text{mon}}$

$q_{\text{mon}} = 33 \frac{1}{3}, p_{\text{mon}} = 66 \frac{2}{3}$

After Tax

Perfect Competition

Demand = Supply

$100 - q_{t,\text{comp}} - dt = q_{t,\text{comp}}$

$q_{t,\text{comp}} = 50 - (1/2)dt, p_{t,\text{comp}} = 50 - (1/2)dt$

$\Delta q_{\text{comp}} = -1/2dt$

Monopoly

Marginal Revenue = Marginal Costs

$100 - 2q_{t,\text{mon}} - dt = q_{t,\text{mon}}$

$q_{t,\text{mon}} = 33 \frac{1}{3} - (1/3) dt, p_{t,\text{mon}} = 66 \frac{2}{3} - (2/3) dt$

$\Delta q_{\text{mon}} = -1/3dt$

Appendix B: Variable Definitions
Table B.1: Variable Definitions Chapter 2

This table provides descriptions for all the regression variables.

Panel A: Firm-Level Variables (Source: Bureau van Dijk's Amadeus)	
Variables	Description
<i>Gross Investment</i>	Change in fixed assets from year $t - 1$ to t plus depreciation scaled by total assets in year $t - 1$.
<i>Net Investment</i>	Change in fixed assets from year $t - 1$ to t scaled by total assets in year $t - 1$.
<i>ΔFixed Assets</i>	Natural logarithm of fixed assets in year t minus the natural logarithm of fixed assets in year $t - 1$.
<i>Change in Leverage</i>	Change in leverage from year $t - 1$ to t scaled by lagged total assets.
<i>Intangible Assets</i>	Change in intangible assets from year $t - 1$ to t scaled by total assets in year $t - 1$.
<i>Cost Price Ratio</i>	Ratio of material costs plus tax costs to total firm sales.
<i>Alicante</i>	Dummy variable, equal to 1 for firms located in Alicante and 0 otherwise. The firm location is based on the 2-digit postal code.
<i>Post</i>	Dummy variable, equal to 1 for 2013 and 2014 and 0 otherwise.
<i>Leverage</i>	Long-term debt over total assets.
<i>Return on Assets</i>	Net income over total assets.
<i>Firm Size</i>	The natural logarithm of sales.
<i>Sales Growth</i>	Natural logarithm of sales in year t minus natural logarithm of sales in $t - 1$.
<i>Cash</i>	Cash holdings scaled by total assets.
<i>Margin</i>	Net income over sales.
<i>Equity</i>	Total equity over total assets.
<i>High HHI</i>	Dummy variable, equal to 1 for firms with above median HHI and 0 otherwise.
<i>High Margin</i>	Dummy variable, equal to 1 for firms with above median profit margins and 0 otherwise.
<i>Coastal Region</i>	Dummy variable, equal to 1 for firms located directly at the sea and 0 otherwise.
<i>High Equity Ratio</i>	Dummy variable, equal to 1 for firms with above median equity-to-asset ratios and 0 otherwise.
<i>Large & Old</i>	Dummy variable, equal to 1 for firms with firm size above the bottom quartile as well as above the median firm age and 0 otherwise.
<i>High Emissions (NOx)</i>	Dummy variable, equal to 1 for firms with individual industry's emissions above the median of total emissions (measured by Ebertseder et al. 2022).
<i>Dirty Supply Chain</i>	Dummy variable, equal to 1 for firms if two or more of the three supply chain links (customer, firm, supplier) are dirty and 0 otherwise.
<i># of Employees</i>	Natural logarithm of the total number of a firm's employees.
<i>Employee Wage</i>	Natural logarithm of staff costs over the total number of a firm's employees.
<i>Low Skill</i>	Dummy variable, equal to 1 for firms if the percentage of skilled workers in an industry is above the bottom quartile and 0 otherwise.
<i>High Emissions (SOx)</i>	Dummy variable, equal to 1 for firms with individual industry's emissions above the median of total emissions (measured by Eurostat 2020).

Table B.2: Variable Definitions Chapter 4

This table provides descriptions for all the regression variables.

Variables	Description	Level
<i>ln(NO2)</i>	Natural logarithm of NO2 column density in $\mu\text{mol}/\text{m}^2$.	Area
<i>NO2</i>	NO2 column density in $\mu\text{mol}/\text{m}^2$.	Area
<i>Treatment</i>	Dummy variable equal to 1 for areas located in Valenciana and 0 otherwise. The area location is based on longitude and latitude data.	Two-digit post code
<i>Wind Speed</i>	Wind speed.	Area
<i>Wind Direction</i>	Wind direction $[-180^\circ; 180^\circ]$.	Area
<i>Solar Radiation</i>	Amount of solar radiation reaching the Earth's surface minus the amount reflected by the Earth's surface (considering clouds).	Area
<i>Temperature</i>	Temperature of air 2 meters above the surface.	
<i>Precipitation</i>	Precipitation accumulated between 13:00 and 14:00 UTC over the corresponding year.	Area
<i>Post</i>	Dummy variable equal to 1 for 2013, 2014, 2015, and 2016 and 0 otherwise.	Area
<i>Firm Size</i>	The natural logarithm of sales.	Area
<i>Number Firms</i>	The natural logarithm of firms within an area.	Area
<i>Population</i>	The natural logarithm of absolute population count.	Two-digit post code
<i>Car Registrations</i>	Car registrations per inhabitant. If the number of cars per inhabitant is not available for an area, we use the region's average.	Two-digit post code
<i>Past Emissions</i>	Dummy variable equal to 1 for areas with above-median emissions in the past and 0 otherwise.	NACE Code
<i>Investment</i>	Change in fixed assets from year $t - 1$ to t plus depreciation scaled by total assets in year $t - 1$.	Area
<i>Sales Growth</i>	The natural logarithm of sales in year t minus the natural logarithm of sales in $t - 1$.	Area
<i>Profitability</i>	Net income in year t scaled by total assets in year $t - 1$.	Area
<i>Tangibility</i>	Fixed assets in year t over total assets in year t .	Area
<i>Large</i>	Dummy variable equal to 1 for areas with number of firms in the top 25th quartile and 0 otherwise.	Area
<i>Dirty</i>	Dummy variable equal to 1 for areas with emissions in the top 25th quartile and 0 otherwise.	Area
<i>Intangible</i>	Dummy variable equal to 1 for areas with intangible assets (scaled by fixed assets) above median and 0 otherwise.	Area
<i>Urban</i>	Dummy variable equal to 1 for areas with population above the World Bank city definition (50k inhabitants) and 0 otherwise.	Area
<i>MNC</i>	Dummy variable equal to 1 for areas with above-median share of sales from firms that are part of a multinational company and 0 otherwise.	Area

Table B.3: Variable Definitions Chapter 5

This table provides descriptions for all the regression variables.

Variables	Description
<i>Market Share (Four-digit)</i>	Firm sales over industry sales defined per four-digit NACE code
<i>Market Share (Three-digit)</i>	Firm sales over industry sales defined per three-digit NACE code
<i>Organic</i>	Dummy variable equal to 1 if firms held an organic certificate on 1st June 2019 and 0 otherwise
<i>Post</i>	Dummy variable equal to 1 for 2019, 2020, 2021 and 0 otherwise
<i>Return on Assets</i>	Net income over total assets
<i>Firm Size</i>	Defined as the natural logarithm of sales
<i>Cash</i>	Cash holdings scaled by total assets
<i>Gross Investment</i>	Change in fixed assets from year t - 1 to t plus depreciation scaled by total assets in year t - 1
<i>Change in Leverage</i>	Change in leverage from year t - 1 to t scaled by lagged total assets
<i>Leverage</i>	Long-term debt over total assets
<i>Sales Growth</i>	Natural logarithm of sales in year t minus natural logarithm of sales in t - 1
<i>Late Mover</i>	Dummy variable equal to 1 if firms held an organic certificate after 1st June 2019 and 0 otherwise
<i>Profit Margin</i>	Sales minus material and salary costs over sales
<i>Salary</i>	Salary costs over number of employees
<i>Material Costs</i>	Material costs over sales
<i>Purchasing Power</i>	Dummy variable equal to 1 if GDP per capita (in €) was above defined threshold and 0 otherwise
<i>Value Chain Position</i>	Dummy variable equal to 1 if position of firm was far from the customer and 0 otherwise
<i>Model Adaptability</i>	Dummy variable equal to 1 if business model of firm was hard to adapt and 0 otherwise
<i>Capital Intensity</i>	Dummy variable equal to 1 if business had an above median capital intensity (depreciation over total assets) and 0 otherwise
<i>Industry Sales Growth</i>	Natural logarithm of industry sales in year t minus industry sales in year t - 1 over industry sales in year t
<i>Ind. Firms Sales Growth</i>	Natural logarithm of sales in year t minus sales in year t - 1 over sales in year t

Appendix C: Additional Figures

Figure C.1: Examples for Organic Earmarking of Products

This figure shows examples of earmarks used for organic products when they are sold in Romania, such as the official ‘ae’ logo and labelling of products as ‘mountain’ or ‘traditional’ products.

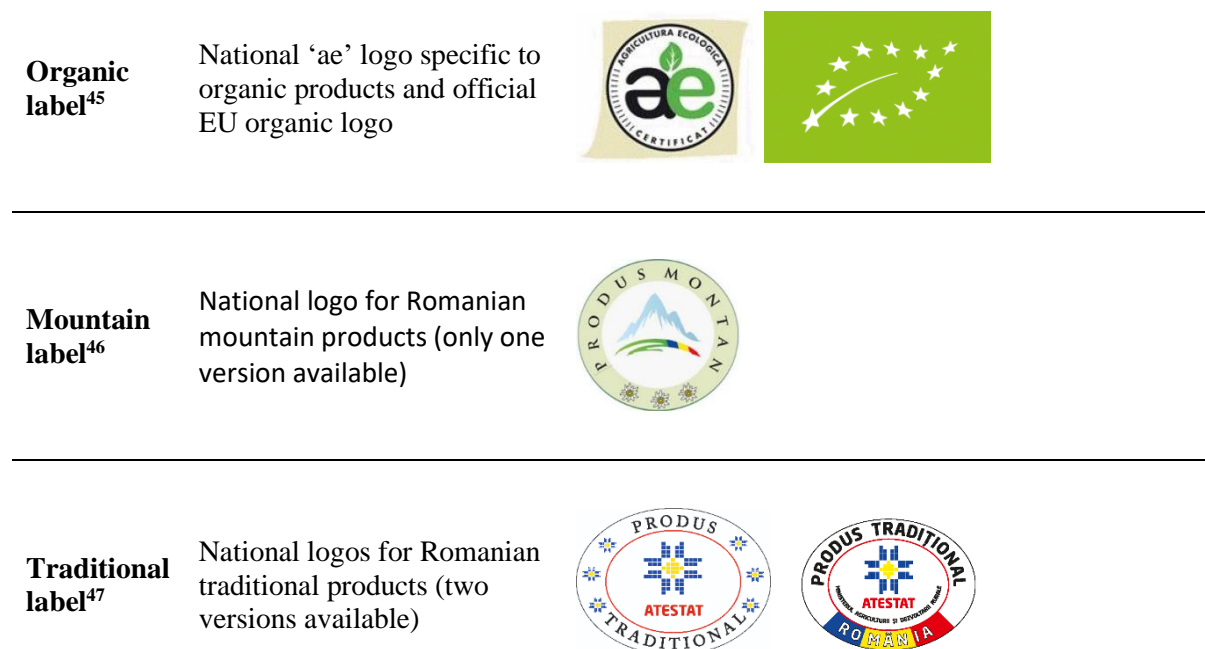
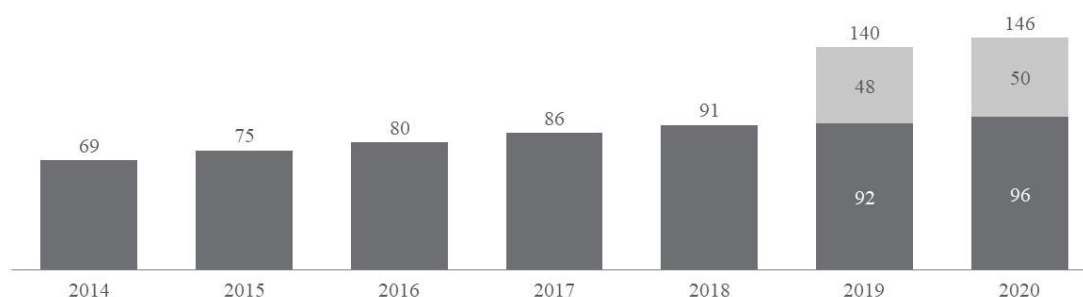


Figure C.2: Certificates over Time

This figure shows the development of several organic certificate holders in Romania over the period of 2014–2021. In dark grey are all certificate holders that already possessed a certificate before the reform introduction on 1st June 2019. In light grey are all ‘late movers’ that obtained a certificate after this cut-off date.



⁴⁵Source: <https://www.madr.ro/en/organic-farming.html>

⁴⁶Source: <https://www.madr.ro/industrie-alimentara/sisteme-de-calitate-europene-si-indicatii-geografice/produse-agricole-si-alimentare/produs-montan.html>

⁴⁷Source: <https://www.madr.ro/industrie-alimentara/produse-traditionale-romanesti/implementarea-ordinului-nr-724-2013-privind-atestarea-produselor-traditionale.html>

Appendix D: Additional Tables
Table D.1: Additional Descriptive Statistics

This table presents descriptive statistics of our main variables for 17,233 observations from 2009 to 2014. The variables are defined in Appendix B.

Breakdown by	NOx Emissions			SOx Emissions			HHI		
	Low	High	Diff.	Low	High	Diff.	Low	High	Diff.
<i>Gross Investment</i>	0.0420	0.0277	0.0142***	0.0294	0.0426	-0.0132***	0.0305	0.0410	-0.0105***
<i>Change in Leverage</i>	-0.0029	-0.0028	-0.0001	-0.0029	-0.0028	-0.0001	-0.0027	-0.0027	-0.0001
<i>Leverage</i>	0.1660	0.1561	0.0099***	0.1561	0.1678	-0.0117***	0.1586	0.1604	-0.0018
<i>Return on Assets</i>	0.0160	0.0101	0.0059***	0.0110	0.0160	-0.0050***	0.0116	0.0147	-0.0031***
<i>Sales Growth</i>	-0.0359	-0.0816	0.0458***	-0.0784	-0.0314	-0.0470	-0.0747	-0.0422	-0.0324***
<i>Firm Size</i>	13.9610	13.8256	0.1353***	13.7979	14.0215	-0.2236***	13.8076	13.9778	-0.1702***
<i>Sales (in 1,000 EUR)</i>	2552.016	2230.001	322.0151***	2200.512	2649.963	-449.4507***	2112.089	2287.215	-175.1257***
<i>Cash</i>	0.1031	0.1048	-0.0017	0.1074	0.0996	0.0078***	0.1078	0.1008	0.0070***
Breakdown by	Margin			Coastal Region			Equity		
	Low	High	Diff.	No	Yes	Diff.	Low	High	Diff.
<i>Gross Investment</i>	0.0315	0.0388	-0.0073***	0.0315	0.0388	-0.0073***	0.0373	0.0334	0.0039***
<i>Change in Leverage</i>	-0.0005	-0.0052	0.0047***	-0.0005	-0.0052	0.0047***	-0.0009	-0.0046	0.0037***
<i>Leverage</i>	0.1469	0.1752	-0.0283***	0.1469	0.1752	-0.0283***	0.2391	0.0929	0.1461***
<i>Return on Assets</i>	-0.0070	0.0328	-0.0398***	-0.0070	0.0328	-0.0398***	0.0053	0.0202	-0.0149***
<i>Sales Growth</i>	-0.0835	-0.0324	-0.0511***	-0.0835	-0.0324	-0.0511***	-0.0418	-0.0716	0.0298***
<i>Firm Size</i>	13.7953	13.9953	-0.1999***	13.7953	13.9953	-0.1999***	13.8994	13.8945	0.0049***
<i>Sales (in 1,000 EUR)</i>	2240.911	2552.94	-312.03***	2240.911	2552.94	-312.03***	2433.28	2369.162	64.1182
<i>Cash</i>	0.1047	0.1032	0.0015	0.1047	0.1032	0.0015***	0.0807	0.1243	-0.0436***
Breakdown by	Large & Old			Dirty Supply Chain			Skill		
	No	Yes	Diff.	No	Yes	Diff.	Low	High	Diff.
<i>Gross Investment</i>	0.0360	0.0326	0.0034**	0.0426	0.0306	0.0121***	0.0344	0.0373	-0.0028
<i>Change in Leverage</i>	-0.0033	-0.0018	-0.0015	-0.0023	-0.0032	0.0009	-0.0021	-0.0095	0.0074***
<i>Leverage</i>	0.1651	0.1505	0.0147***	0.1669	0.1571	0.0098***	0.1528	0.2319	-0.0791***
<i>Return on Assets</i>	0.0096	0.0176	-0.0079***	0.0154	0.0119	0.0035***	0.0120	0.0219	-0.0099***
<i>Sales Growth</i>	-0.0619	-0.0619	0.0000	-0.0387	-0.0701	0.0314***	-0.0579	-0.1074	0.0495***
<i>Firm Size</i>	13.4353	14.5351	-1.0998***	13.9496	13.863	0.0866***	13.9902	12.5622	1.4280***
<i>Sales (in 1,000 EUR)</i>	1312.433	3486.855	-2174.421***	2443.18	2370.569	72.61147	2289.718	947.5562	1342.162***
<i>Cash</i>	0.1169	0.0873	0.0296***	0.1017	0.1054	-0.0037*	0.1056	0.0992	0.0992

**Table D.2: Emission Taxes & Investment,
Heterogeneity Analysis with Entropy Balancing**

This table presents the results of a separate analysis for firms in high-emission industries of our baseline regression in Panel A. We interact *Alicante*, *Post*, and *Alicante* \times *Post* with the dummy variable *High Emission*, which equals 1 if the firm's SO_x or NO_x emissions are above the industry median and 0 otherwise. We execute this split in 2011 to avoid the emission tax reform already affecting these firms' emission levels. We use lagged controls and firm and industry-year fixed effects for all the regressions. Robust standard errors clustered at the firm level are shown in parentheses. Panel B shows the interaction of *Alicante*, *Post*, and *Alicante* \times *Post* with the dummy variable *High Margin*, which equals 1 if the firm's profit margin is above the industry median and 0 otherwise. We executed this split in 2011 to avoid the emission tax reform already affecting these firms' profit margins and capital demand. We then repeat this approach and interact the DiD coefficients with *High Equity*, which equals 1 if the firm's equity-to-assets ratio in 2011 is above the 2011 industry median and 0 otherwise. In Panel C, we combine the analyses of Panels A and B and include interactions for *High Emission* as well as *High Margin/High Equity*. We include control variables and fixed effects in all the tests. In each regression, we use entropy balancing to balance the sample with respect to the control variables on *High SO_x Emission*, *High NO_x Emission*, *High Margin*, and *High Equity*, respectively. Standard errors (in parentheses) are clustered at the industry level in Panel B and at the firm level in Panel A. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Role of Market Structure

Breakdown by	HHI			
	HHI, Full NACE Code		HHI, Two-Digit NACE Code	
	Low (1)	High (2)	Low (3)	High (4)
<i>Alicante</i> \times <i>Post</i>	-0.0178*** (0.0047)	-0.0033 (0.0045)	-0.0160*** (0.0042)	-0.0028 (0.0057)
<i>Difference</i> [<i>t-stat.</i>]	0.0145* [2.26]		0.0133* [1.88]	
Controls	Yes		Yes	
Firm FE	Yes		Yes	
Industry-Year FE	Yes		Yes	
Balancing	Yes		Yes	
Observations	15,731		15,731	
Adj. R ²	0.1354		0.1351	

Panel B: Within-Market Variation of Pricing Power

Breakdown by	Pricing Power			
	Profit Margin		Coastal Region	
	Low (1)	High (2)	Non-Coastal (3)	Coastal (4)
<i>Alicante</i> \times <i>Post</i>	-0.0172*** (0.0043)	-0.0026 (0.0045)	-0.0090** (0.0036)	-0.0103 (0.0077)
<i>Difference</i> [<i>t-stat.</i>]	0.0146** [2.38]		-0.0014 [-0.16]	
Controls	Yes		Yes	
Firm FE	Yes		Yes	
Industry-Year FE	Yes		Yes	
Balancing	Yes		Yes	
Observations	17,011		17,127	
Adj. R ²	0.1387		0.1385	

Panel C: Within-Market Variation of Capital Demand Elasticity				
Breakdown by	Capital Demand Elasticity			
	Equity Ratio		Firm Size and Age	
	Low (1)	High (2)	Small & Young (3)	Large & Old (4)
<i>Alicante</i> × <i>Post</i>	-0.0171*** (0.0047)	-0.0035 (0.0040)	-0.0154*** (0.0045)	-0.0040 (0.0045)
<i>Difference</i> [<i>t-stat.</i>]	0.0136** [2.28]		0.0114* [1.84]	
Controls	Yes		Yes	
Firm FE	Yes		Yes	
Industry–Year FE	Yes		Yes	
Balancing	Yes		Yes	
Observations	17,011		15,731	
Adj. R ²	0.1387		0.1358	

Panel D: Passing on the Burden to Employees				
Change of/ Breakdown by	Dependent Variable		Labor Skills	
	# of Employees (1)	Wage Growth (2)	Low (3)	High (4)
	<i>Alicante</i> × <i>Post</i>	0.0134 (0.0139)	-0.0133 (0.0083)	-0.0078** (0.0033)
<i>Difference</i> [<i>t-stat.</i>]	-	-	-0.0387** [-2.19]	
Controls	Yes	Yes	Yes	
Firm FE	Yes	Yes	Yes	
Industry–Year FE	Yes	Yes	Yes	
Balancing	Yes	Yes	Yes	
Observations	13,162	12,945	15,731	
Adj. R ²	0.9102	0.7710	0.1365	

Panel E: Assessing the Polluter Pays Principle				
Breakdown by	Industry Emission Level (Polluter Pays)			
	Industry SO _x Emission		Industry NO _x Emission	
	Low (1)	High (2)	Low (3)	High (4)
<i>Alicante</i> × <i>Post</i>	-0.0114*** (0.0040)	-0.0074 (0.0048)	-0.0064 (0.0044)	-0.0130*** (0.0045)
<i>Difference</i> [<i>t-stat.</i>]	0.0040 [0.65]		-0.0066 [-1.05]	
Controls	Yes		Yes	
Firm FE	Yes		Yes	
Industry–Year FE	Yes		Yes	
Balancing	Yes		Yes	
Observations	17,011		17,011	
Adj. R ²	0.1386		0.1385	

Table D.3: Robustness Checks Cross-Sections

This table shows the various robustness checks we perform for our heterogeneity analysis. Panel A shows the robustness checks for the *Large Industry Area* split by using NO2 as an alternative dependent variable (columns (1) and (2)), estimating the regression for areas with at least 15 firms (columns (3) and (4)), and using standard errors clustered at the postcode level (columns (5) and (6)). Panel B shows the robustness tests for *Cities*. Again, we use NO2 (columns (1) and (2)), estimating the regression for areas with at least 15 firms (columns (3) and (4)) and standard errors clustered at the postcode level (columns (5) and (6)). Panel C shows the robustness tests for *High Emissions*. In columns (1) and (2), we use NO2 as the dependent variable. In columns (3) and (4), we use our dependent NO2 variable to perform the split into dirty and clean. In columns (5) and (6), we use standard errors clustered at the postcode level. Panel D shows the robustness tests for *Intangibles*. In columns (1) and (2), we again use NO2 as our dependent variable. In columns (3) and (4), we require again 15 firms per area. In columns (5) and (6), we use standard errors clustered at the postcode level. Panel E shows the robustness tests for *Firm Size*. Again, we use NO2 (columns (1) and (2)), estimating the regression for areas with at least 15 firms (columns (3) and (4)) and standard errors clustered at the postcode level (columns (5) and (6)). All regressions use area- and year-fixed effects as well as lagged controls. We report robust standard errors clustered at the area id level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Robustness Checks Industrial Activity (# Firms)						
	Dependent variable		Model specification			
	NO2		# Firms >15		SE Cluster	
	Small	Large	Small	Large	Small	Large
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Treatment</i> × <i>Post</i>	0.0703 (0.3697)	-1.0270*** (0.3465)	0.0060 (0.0092)	-0.0235*** (0.0088)	-0.0000 (0.0097)	-0.0250*** (0.0089)
<i>Difference</i>	-1.0973**		-0.0296**		-0.0249**	
<i>[t-stat]</i>	[2.22]		[2.41]		[1.97]	
Controls	Yes		Yes		Yes	
Area FE	Yes		Yes		Yes	
Year FE	Yes		Yes		Yes	
Observations	15,374		11,005		15,374	
Adj.-R ²	0.9461		0.9493		0.9528	
Panel B: Robustness Checks Industrial Activity (Population)						
	Dependent variable		Model specification			
	NO2		# Firms >15		SE Cluster	
	Rural	City	Rural	City	Rural	City
	(1)	(2)	(1)	(2)	(1)	(2)
<i>Treatment</i> × <i>Post</i>	-0.3571 (0.2636)	-1.9592*** (0.6485)	-0.0085 (0.0071)	-0.0458*** (0.0131)	-0.0096 (0.0110)	-0.0484*** (0.0150)
<i>Difference</i>	-1.6021**		-0.0374**		-0.0388***	
<i>[t-stat]</i>	[2.32]		[2.53]		[2.93]	
Controls	Yes		Yes		Yes	
Area FE	Yes		Yes		Yes	
Year FE	Yes		Yes		Yes	
Group Size FE	Yes		Yes		No	
Observations	15,393		11,018		15,393	
Adj.-R ²	0.9456		0.9490		0.9526	

Panel C: Robustness Checks Industrial Activity (Emissions)						
	Dependent variable		Model specification			
	NO2		Dummy Split		SE Cluster	
	Clean (1)	Dirty (2)	Clean (3)	Dirty (4)	Clean (5)	Dirty (6)
<i>Treatment × Post</i>	-0.5485** (0.2291)	-0.3458 (0.4181)	-0.0098*** (0.0055)	-0.0143 (0.0133)	-0.0126* (0.0076)	-0.0098 (0.0093)
<i>Difference</i>	0.2027		-0.0045		0.0028	
<i>[t-stat]</i>	[0.45]		[0.23]		[0.24]	
Controls	Yes		Yes		Yes	
Area FE	Yes		Yes		Yes	
Year FE	Yes		Yes		Yes	
Group Size FE	Yes		Yes		No	
Observations	15,374		15,374		15,374	
Adj.-R ²	0.9505		0.9563		0.9566	
Panel D: Robustness Checks Firm Characteristics (Intangible Assets)						
	Dependent variable		Model specification			
	NO2		# Firms >15		SE Cluster	
	Low (1)	High (2)	Low (1)	High (2)	Low (1)	High (2)
<i>Treatment × Post</i>	-0.0749 (0.3473)	-0.9455*** (0.3650)	0.0026 (0.0081)	-0.0249* (0.0098)	-0.0040 (0.0090)	-0.0220** (0.0095)
<i>Difference</i>	-0.8705*		-0.0275**		-0.0180**	
<i>[t-stat]</i>	[1.77]		[2.24]		[1.43]	
Controls	Yes		Yes		Yes	
Area FE	Yes		Yes		Yes	
Year FE	Yes		Yes		Yes	
Observations	15,374		11,005		15,374	
Adj.-R ²	0.9459		0.9493		0.9528	
Panel E: Robustness Checks Firm Characteristics (Firm Size)						
	Dependent variable		Model specification			
	NO2		# Firms >15		SE Cluster	
	Low (1)	High (2)	Low (1)	High (2)	Low (1)	High (2)
<i>Treatment × Post</i>	0.2537 (0.4279)	-1.0218*** (0.3893)	0.0062 (0.0114)	-0.0219** (0.0101)	0.0033 (0.0092)	-0.0225** (0.0102)
<i>Difference</i>	-1.2755**		-0.0280*		-0.0208	
<i>[t-stat]</i>	[2.27]		[1.93]		[1.60]	
Controls	Yes		Yes		Yes	
Area FE	Yes		Yes		Yes	
Year FE	Yes		Yes		Yes	
Observations	15,377		11,008		15,377	
Adj.-R ²	0.9450		0.9479		0.9523	

Table D.4: List of included Types of Firms

This table presents the included types of firms with an organic certificate included in the sample by NACE code and a short description of their respective activities.

NACE Code	Sub-Type	Description of Activities
A	Vegetable/fruit producers	Cultivation of vegetables and fruits
	Cereal/crops producers	Cultivation of cereals and forage crops
	Nut/oil plants producers	Cultivation of nuts and other oil plants
	Livestock farmers	Husbandry of pigs, cows and other animals
	Winemakers	Cultivation and production of wine
	Forest product farmers	Cultivation of mushrooms and wild fruits
C	Beekeepers	Production of honey and related products
	Fresh food processors	Processing of fresh food (e.g. cheese)
	Frozen food processors	Processing of frozen food (e.g. frozen berries)
G	Ingredient processors	Processing of ingredients (e.g. cosmetics)
	Wholesaler	Selling of products B2B in larger amounts
	Retailer	Selling of products B2C in smaller amounts

Table D.5: Robustness Checks Heterogeneity Tests

This table presents the robustness tests for the heterogeneity tests. In panel A, I tested the robustness of the purchasing power split by using an alternative measure of purchasing power, namely the Purchasing Power Standard (PPS). In panel B, I tested the robustness of the value chain position split by defining firms far away from the end consumer to also include farmers in columns 1 to 2 and only include wholesalers in columns 3 to 4. In panel C, I tested the robustness of business model adaptability by running to separate regressions for firms with easy-to-adapt business models (column 1) and firms with difficult-to-adapt business models (column 2). In panel D, I ran separate regressions for firms with low capital intensity (column 1) and firms with high capital intensity (column 2). Standard errors (in parentheses) are clustered at firm level or two-digit-industry level as indicated. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively.

Panel A: Robustness Checks Purchasing Power				
Split by	Purchasing Power Standard			
	(1) Low		(2) High	
<i>Organic</i> × <i>Post</i>	-0.0134*** (0.0050)		0.0276*** (0.0117)	
<i>Difference</i> [<i>t-stat.</i>]		0.0411*** [2.91]		
Controls		Yes		
Industry-Year FE		Yes		
SE Cluster		ID		
Balanced		Yes		
Observations		66,220		
Adj. R ²		0.2697		
Panel B: Robustness Checks Value Chain Position				
Split by	Value Chain Position			
	(1) Close	(2) Far	(3) Close	(4) Far
Treatment (Far)	Wholesalers, Processors, Farmers		Wholesalers	
Control (Close)	Retailers		Retailers	
<i>Organic</i> × <i>Post</i>	-0.0028 (0.0027)	-0.0117* (0.0058)	-0.0028 (0.0027)	-0.0117* (0.0058)
<i>Difference</i> [<i>t-stat.</i>]		-0.0089** [-2.45]		-0.0219 [-4.52]
Controls		Yes		Yes
Industry-Year FE		Yes		Yes
SE Cluster		Two-Digit Industry		Two-Digit Industry
Balanced		Yes		Yes
Observations		66,029		36,314
Adj.-R ²		0.2611		0.4209

Panel C: Robustness Checks Model Adaptability		
Split by	Model Adaptability	
	(1) Easy	(2) Difficult
<i>Organic</i> × <i>Post</i>	-0.0099** (0.0051)	-0.0071 (0.0094)
Controls	Yes	Yes
Industry-Year FE	Yes	Yes
SE Cluster	ID	ID
Balanced	Yes	Yes
Observations	71,091	70,780
Adj.-R ²	0.2307	0.2855
Panel D: Robustness Checks Capital Intensity		
Split by	Capital Intensity	
	(1) Low	(2) High
<i>Organic</i> × <i>Post</i>	-0.0265** (0.0105)	-0.0055 (0.0043)
Controls	Yes	Yes
Industry-Year FE	Yes	Yes
SE Cluster	ID	ID
Balanced	Yes	Yes
Observations	70,863	71,004
Adj.-R ²	0.2460	0.2449