THE LONDON SCHOOL OF ECONOMICS AND POLITICAL SCIENCE

Four essays on the Economic Impacts of Environmental Conditions and Policies

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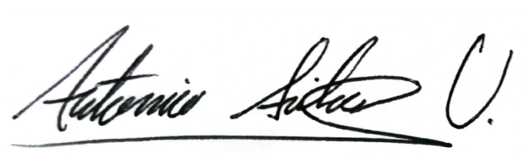
Declaration

I certify that the thesis I have presented for examination for the MPhil/PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it). The copyright of this thesis rests with the author. Quotation from it is permitted, provided that full acknowledgement is made. This thesis may not be reproduced without my prior written consent. I warrant that this authorisation does not, to the best of my belief, infringe the rights of any third party.

I declare that my thesis consists of approximately 48,935 words, excluding all appendices and the bibliography.



Statement of co-authored work

I certify that Chapter 4 of this thesis is co-authored with Sefi Roth and Brian Shields. I contributed to 75% of the work in its current form. Chapter 5 is co-authored with Shaikh M.S.U. Eskander, Kate E. Gannon, and Elena Castellano, and I contributed 25% of the work in its current form.

Statement of inclusion of previous work

I confirm that Chapter 3 was the result of a fundamental revision of a study I undertook for my master thesis on Applied Social Data Science at LSE.

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The decision to embark on this PhD started on a similar idea. I had a strong conscious motivation to use the best of my abilities to give back for all that society has given me, and how it has allowed me to grow as a thinker; and a friend to those I love. If I am any good, it is thanks to all the opportunities I have had, and the guidance and company of deeply loving people who have directly or indirectly helped me navigate my path in these twenty-eight years. In this light, this thesis is but a selected subset of various projects I have been working on these last four years to show my gratitude for the life I have and the opportunities that allowed it.

I now can see that I also started the PhD with the somewhat latent and unconscious desire of finding a place where I could effectively cooperate with society (*work*) by being myself. Looking back, I see how this long process has been my way to find my place among this group of academics, analysts, and thinkers. It feels good to be part of it, and to work and live as one. There are so many people I wish to thank for the feeling of being part of this group, while being myself.

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Abstract

This thesis examines the economic impacts of environmental conditions, the effects of policies designed to mitigate these effects, and the business adaptation strategies to environmental shocks. This work is structured into three blocks, each exploring one of these topics in environmental economics through a quantitative-empirical lens.

Block 1 delves into the impact of air pollution on the U.S. aggregate production. Using a new instrumental variable that leverages pixel-level predictions of wildfire smoke, this analysis reveals significant negative impacts of air pollution in the US rural areas. Although the effects are not as large or generally applicable as in other countries, they still justify stronger air pollution regulations.

Block 2 evaluates the impacts of Low Emission Zones (LEZ) on city-level economic growth in Germany and their impact on educational outcomes in London. Employing a difference-in-differences approach, this block estimates large and heterogeneous impacts of German LEZ on city-level GDP, having a positive effect in the period between announcement and implementation and a negative effect afterwards. Results from the London LEZ show that it significantly improved primary school test scores, highlighting a not-yet-studied benefit of such policies.

Block 3 focuses on the adaptive behaviours of small enterprises in Sub-Saharan Africa in response to environmental shocks, with an emphasis on the differences between firms with and without women in their leadership. This study reveals that the former are more likely to implement sustainable adaptation strategies, emphasising the role of leadership diversity in fostering effective environmental resilience.

Throughout, this thesis leverages detailed spatial analysis and robust empirical methodologies to offer new insights into the interactions between environmental policies and economic activity. My work highlights the importance of understanding the broader effects of environmental conditions and policies, and the need for tailored policy measures that effectively address the impacts of environmental changes. These findings contribute to ongoing policy discussions with the objective of improving environmental sustainability and economic prosperity.

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Chapter 1

Introduction

The introduction of this thesis is structured in 4 parts: First, I discuss the main topics that surround this thesis to provide the reader with a broad exposure of its context and motivation. Second, I do an overview of the thesis, which presents the common goals, themes, and methodologies studied in the forthcoming chapters. Next, I expose the various the contributions and limitations of this thesis. Finally, I present an overview of each of the chapters. All following chapters are presented, and can be read, as stand-alone academic papers. They thus include their own, slightly more developed, introduction and conclusion sections.

1.1 Motivation and Context: Air pollution, climate change, and the evaluation of environmental policies

Air pollution and climate change represent two of the most formidable environmental challenges of our time and affect the health and economic conditions of millions of people globally. The detrimental impacts of these phenomena on mortality and health care have been widely researched, with the Intergovernmental Panel on Climate Change (IPCC) estimating climate-induced heat, undernutrition, malaria and diarrheal disease to cause an excess of 250,000 deaths annually in the year 2050 (Cissé et al., 2022), and The Lancet Commission on Pollution and Health estimating more than 6.67 million deaths a year are directly attributable to air pollution, of which 4.5 are due to ambient air pollution alone (Fuller et al., 2022). However, their impact is not only confined to human health and their negative economy-wide impacts are also profound. With regards to climate change, Kotz et al. (2024) estimate that the world economy is committed to a lower bound income reduction of 19% within the next 26 years due to climate change, independent of future emission choices. On the other hand, air pollution has been found to reduce labour market performance of both manual and desk-based jobs while reducing cognitive abilities and human capital accumulation on a wide range of ages (Graff Zivin and Neidell, 2018). There are not yet global estimates of the effects of air pollution on GDP, but estimates from Europe and China suggest that the costs can be substantial (Dechezleprêtre, Rivers and Stadler, 2019; Fu et al., 2021).

Public awareness and concern about these issues has grown considerably over the last decades. On the issue of air pollution, regulations to reduce its negative impacts on human health have been progressively enforced around the world. First in high-income countries after the events of the 1952 Greater Smog of London and the 1948 Donora Smog in the US, and more recently in some developing countries (with China's "war on pollution" in 2013 being the prime example). These measures have become increasingly stringent over time, causing significant reductions in pollution in the US (US EPA, 2024), Europe (Sicard et al., 2021), and China (Greenstone et al., 2022). As an illustrative example, Dechezleprêtre, Rivers and Stadler (2019) estimates imply that the reduction of particulate concentration across the EU between 2000 and 2015 (20%) boosted EU GDP by 2.4%, and thus explains 15% of GDP growth in Europe over this period.

With regards to climate change, nearly two-thirds of people in 50 countries (covering 56% of the world's population over the age of 14) believe that "climate change is a global emergency." and, among those, 59% think that the response should be to "do everything necessary, urgently" (Flynn et al., 2021). In the European context, especially relevant for the two chapters on Low Emission Zones (3 and 4), climate change and air pollution are consistently the 1st and 2nd most important environmental issues (European Commission, 2020), with 64% of Europeans thinking that EU air quality standards are not adequate and should be strengthened (European Commission, 2022). This pressure from civil society continues to prompt ever stricter governmental and business responses, aiming to mitigate and adapt to these environmental challenges through various policies and strategies.

The search for effective policies described above is both a driver and a consequence of a booming field of empirical economics dedicated to evaluating a large set of environmental policies such as carbon markets, air pollution regulations, and Low Emission Zones. Businesses, too, are adapting, with different types of firms responding uniquely based on their characteristics and the degree of training or assistance they receive in the adoption of sustainable strategies.

Adding to this, the urbanisation trend across the globe accentuates the urgency to understand and improve urban environmental policies. Between 1950 and 2018, the urban population of the world grew more than four-fold, with now more than half of the world population living in cities (UN, 2019). As more people migrate to cities, the environmental issues mentioned above intensify with higher congestion, air pollution, and heat islands. These trends further motivate the study of the impacts of environmental conditions and policies in the urban context.

Finally, the role of technology in environmental monitoring has become pivotal; advancements in remote sensing and computing power have enhanced our capability to assess environmental impacts with better identification strategies and data. This has enabled the field of empirical research to further advance our understanding of the benefits and costs of policies.

Through rigorous analysis of the empirical evidence, this thesis aims to contribute valuable insights into the economic and social ramifications of environmental changes and the policies implemented to counteract them.

1.2 Thesis Overview

This thesis is structured into three thematic blocks, each exploring distinct aspects of environmental economics:

1. The first block delves into the economic effects of the improvement or deterioration in environmental conditions, with Chapter 2 focusing specifically on the impacts of ambient air pollution on U.S. aggregate production. This analysis highlights the tangible economic costs associated with environmental degradation and the economic feasibility of more stringent air pollution regulation in the US.
2. The second block evaluates the efficacy of environmental policies through case studies of Low Emission Zones in Germany and London in chapters 3 and 4, respectively. Chapter 3 investigates the economic consequences of Low Emission Zones (LEZ) on local production within German cities, shedding light on the trade-offs between environmental benefits and economic costs. Meanwhile, Chapter 4 examines the impact of London's Low Emission Zone on educational outcomes, revealing significant improvements in standardised exam scores among elementary school students and a reduction in educational disparities, offering crucial insights for policymakers about the broader social benefits of environmental policies.
3. The third block explores the adaptation patterns of firms to extreme weather events, focusing on Small and Medium-sized Enterprises (SMEs) in Sub-Saharan Africa. Chapter 5 investigates how female representation in business leadership influences firm-level adaptation behaviour to extreme weather shocks though the adoption of sustainable and unsustainable adaptation strategies.

Common empirical strategies:

A common trait of all chapters of this thesis is the rigorous application of quantitative empirical methods to study economic and social phenomena. Chapters 2 to 4 use causal identification strategies focused on capturing quasi-random variations in environmental shocks or policy implementations to estimate their causal impacts. Chapter 2 utilises advanced spatial econometric models to assess the effects of air pollution on U.S. aggregate production, leveraging exogenous variations provided by natural events and random changes in wind currents. Chapters 3 and 4 (examining the socio-economic and educational impacts of Low Emission Zones in Germany and London, respectively) also apply this approach by using a difference-in-differences framework under the assumption that the policy implementation was exogenous to the potential outcomes of treated and control regions. Conversely, the final chapter shifts towards a more exploratory analysis, examining the adaptive behaviours of SMEs in Sub-Saharan Africa to environmental shocks, focusing on how gender dynamics might influence these adaptation strategies.

Moreover, special attention to the spatial dimensions of economic phenomena is a central feature of the methodology and conceptualisation of all the chapters. This spatial perspective is crucial for the first three chapters as it allows to better understand and model the environmental and economic variables used. Utilising spatial data sources such as satellite measurements of weather and air pollution rasters enriches the analysis and facilitates the robust identification of causal relationships. This approach is particularly evident in the construction of the instruments, controls and variable creations in Chapter 2, the classification of treated and control regions in Chapters 3 and 4, and the construction of clusters in Chapter 5.

1.3 Contribution and Limitations

This section summarises the general and block-specific contributions and limitations of the thesis. A more detailed version is provided on each individual chapter.

General Contributions and Limitations:

The overarching contributions of this thesis lie in its application of quasi-experimental designs that exploit exogenous variation in environmental policies and environmental conditions. This allows the thesis to provide robust causal estimates that advance our understanding and help improve current and future public policies. The thesis includes the first estimates of the impacts of ambient air pollution on US GDP, and the first estimates of the effects of Low Emission Zones on local GDP and school test scores.

A notable general contribution of this thesis is revealing the nuanced effects of environmental conditions and policies across different geographic or economic contexts. For instance, the analysis of U.S. aggregate production shows distinct urban-rural differences in the economic impact of air pollution, illustrating how rural counties suffer more acutely from poor air quality compared to urban counties. In the case of the German Low Emission Zones, the study explores scenarios before and after the 2008 financial crisis, demonstrating how policy effects might change through time. Similarly, the research on London's LEZ assesses not only the direct effects on educational outcomes within the city, which are positive and strong, but contrasts them with the null spillover effects on surrounding areas. This approach not only confirms the varied impacts of such policies and environmental conditions on local economies and social outcomes but also enhances the precision of policy evaluations, making a significant contribution to the field.

Furthermore, this thesis contributes to the literature that links environmental changes and socio-economic outcomes. Through its detailed examination of air pollution and Low Emission Zones, the work demonstrates how environmental conditions and policies can inadvertently influence economic production and educational outcomes even if these are not their intended objectives. The discovery and quantification of these unintended economic consequences (positive or negative) provides valuable information to improve current and future environmental policies.

Finally, it is important to recognise the various limitations of this work. Chapter 2 to 4 are limited by the use of aggregate data, which restricts the possibility to explore the mechanisms at play. The use of GDP as an outcome also comes with the limitations of this measure, which are made clear in Chapters 3 and 4. Another limitation is the potential for unobserved confounders that could not be controlled by the empirical specification. These can be, for example, contemporaneous shocks that deferentially affect treated and control regions in Block 2. This is a common limitation of quasi-experimental designs where true randomisation is not possible and a great effort was put to control, avoid, or report potential cases of these shocks.

Moreover, the application of these findings to other contexts is constrained by the geographic and temporal scopes of the included studies and the data available. For example, Chapter 2 focuses on the impact of particulate matter ($PM_{2.5}$) instead of providing a complete picture of all air pollution impacts. In the case of Chapter 5, the context of SMEs in semi-arid regions of Sub-Saharan Africa strongly limits data gathering on adaptation practices. This motivates the use of secondary data which explores the necessary outcomes in sufficient depth, but with a limited number of firms in Kenya and Senegal. Finally, this thesis also primarily addresses the short-to-medium-term effects of environmental conditions and policies instead of long-term outcomes, which are crucial for understanding sustained impacts on economic growth and public health.

1.3.1 Contributions by block

Block 1: Impact of Environmental Conditions: Air pollution

The first block of the thesis makes an important contribution by quantifying the economic impact of air pollution on U.S. aggregate production. This analysis introduces a new instrumental variable that leverages highly detailed wildfire smoke predictions as an exogenous source of air pollution, studying its strength, geographical distribution, and its plausible chemical composition. The results provide new insights into how air quality fluctuations can affect economic outputs. This block advances the literature by demonstrating that the negative effects of air pollution in the US are not as large as those found by previous literature for Europe and China and are localised in rural counties. It also highlights the contexts where air pollution reductions could be most beneficial and gives an approximate calculation of the monetary benefits of the Clean Air Act amendments of 1990.

Block 2: Impact of Environmental Policies: Low Emission Zones

The second block contributes through a dual analysis of Low Emission Zones, providing the first evidence of their city-wide economic impact in Germany and their effect on educational outcomes in London. The first study finds that LEZ can create large and permanent reductions in local GDP, while the second focuses on the benefits they can have on educational achievements, especially among vulnerable populations. By employing robust difference-in-differences methodologies, these chapters offer a detailed evaluation of LEZs, providing policymakers with evidence to better evaluate and understand the consequences of the current implementation and expansion of such zones.

Block 3: Adaptation Patterns of Firms: Gender participation and firm-level adaptation.
The final block explores the adaptive behaviours of SMEs in response to environmental stresses in Sub-Saharan Africa, particularly in the context of Senegal and Kenya, with a focus on gender representation within firm leadership. By highlighting how these adaptations vary according to firm characteristics, such as the female representation in leadership roles, the chapter adds a significant layer of understanding to the business economics of climate adaptation in these contexts. The findings underscore the importance of policy measures that consider the diverse business realities and capacities while studying the role of women leadership in applying climate adaptation strategies.

1.4 Chapter Overview

Chapter 2

Air pollution is known to have adverse effects on individuals' health, labour market performance, and human capital accumulation, all determinants of a country's overall economic activity. So what are the effects of air pollution on aggregate economic production? To answer this, this chapter examines the effects of $PM_{2.5}$ on county-level GDP, GDP per capita, and GDP per employee in the United States (2006-2018) by exploiting a detailed dataset of yearly ambient air pollution exposure by county and a set of instrumental variables. My main specification uses exogenous year-to-year variation in predicted wildfire-induced ambient $PM_{2.5}$ exposure. Contrary to a recent study for the European Union, which found large negative effects in all regions, my results show no such effect for the US. However, these headline results mask spatial and temporal heterogeneity. Economically relevant negative effects appear in rural areas, during working days, where base levels of air pollution are above the median, and in the trade sector and educational services. The results are robust to various alternative specifications and instruments previously used in the literature. Simple back-of-the-envelope calculations show that nationwide costs of air pollution abatement under the 1990 Clean Air Act amendments were *77%* lower than the economic benefits in rural areas' GDP trough reduced air pollution. My results suggest further air pollution reduction policies would be beneficial even if we only consider the lower bound of short-term market costs, greatly increasing their political feasibility.

Chapter 3

More than 400 Low Emission Zones (LEZ) have been adopted in Europe. While the restrictions imposed by LEZ are sometimes criticised for "hurting the economy", especially local businesses, recent literature suggests LEZ could improve economic performance through their reduction of air pollution. As this policy can both harm and boost economic growth, this paper provides the first estimates of the aggregate impact of Low Emission Zones on the growth and structure of the local economy by using their staggered introduction in German cities. The results show that Low Emission Zones had heterogeneous effects on local GDP, having a positive effect for early-adopters (2.45%) and a negative and permanent effect on the late-adopters (-4.1%). Regarding its effects on sector-level output, LEZ created no changes in the early-adopters; nevertheless, they slightly reduced the relative share in overall GDP of the Industry Sector, and increased the share of the 'Public and Entertainment Services' Sector for the late-adopters. These results suggest that the application of LEZ might produce large and previously unaccounted costs (and changes) to the local economy.

Chapter 4

Focusing on London's Low Emission Zone, this chapter explores its impact on standardised test scores among elementary school students in England, addressing a critical gap in understanding the relationship between traffic-related air quality improvement policies and educational outcomes. Utilising the National Pupil Database, covering all state school students in England from 2005-2015, we employ a difference-in-differences approach to assess the LEZ's effect on the standardised Key Stage 2 results (age 11). Our analysis reveals a statistically and economically significant improvement of 0.09 standard deviations in test scores for students within the LEZ compared to those in urban control areas. Importantly, we also find that the LEZ policy demonstrates pronounced positive effects in schools with higher eligibility for free school meals and those with lower initial test scores, demonstrating that the LEZ policy can be an impactful approach to addressing educational disparities and reducing environmental inequalities. These findings are pivotal for policymakers, highlighting the importance of including educational outcomes in the evaluation of environmental policies and the potential of LEZs to mitigate long-term educational and labour market disparities driven by environmental factors.

Chapter 5

The literature on gender and climate change adaptation tends to highlight the double reality of women's position, as both especially vulnerable to climate change and especially valuable to climate change adaptation, but these ideas have been little considered in the context of adaptation within small businesses. This final chapter contributes to this gap within existing literature and explores how female representation in the ownership or management structures of small businesses shapes firm-level adaptive capacity and resulting adaptation behaviour. Using a firm-level survey from Senegal and Kenya, we adopt a Poisson regression model to identify gendered differences in the adoption of sustainable and unsustainable adaptation strategies by small businesses when exposed to climate extremes. Our results show that female-led businesses that faced a large number of extreme events adopt more sustainable and less unsustainable strategies than those with only male leadership. Consistent with the literature, we then identify that assistance can mitigate some of the harmful effects of climate shocks and additionally enable female-led SMEs to adopt more sustainable and unsustainable adaptation strategies than male-led SMEs. We interpret this result recognising that unsustainable adaptation strategies, such as selling business assets, require access to business assets and resources and thus linked to a business' coping capacity. Results highlight the value of developing an enabling environment for adaptation that targets women entrepreneurs, not just for delivering on climate justice agendas, but also for strategic resilience building.

Chapter 2

The Effect of Air Pollution on US Aggregate Production

2.1 Introduction

Air pollution is globally recognised as a major threat to human health. More than 99% of the world's population live in areas where air pollution levels exceed the World Health Organisation guidelines (WHO, 2022) and 4.5 million deaths a year worldwide are attributed to ambient air pollution alone, of which 4.14 are from particulate matter (PM)[1] (Fuller et al., 2022).

These large health costs of air pollution have led to increasingly restrictive regulations in multiple countries, with the Clean Air Act in the United States of America (US), being a worldwide role model for environmental legislation (Holman et al., 2015; Kuklinska et al., 2015). But how much air pollution control is enough? The answer depends on the costs of reducing it, and the size of its negative effects. As Graff Zivin and Neidell (2018) pointed out, the effects on hospitalisations and deaths are only the tip of the iceberg, with more common (but less lethal) negative effects on labour productivity and human capital accumulation that "can add up to considerable, society-wide impacts across the globe". Estimates of these aggregated effects on health and economic production are then a valuable source of information to guide policymakers on the optimal strength of

[1] Particulate matter (PM) consists of small suspended particles. The most conventional measurements of PM are PM_{10} and $PM_{2.5}$. Their concentration in the air is measured in micrograms per cubic meter ($\mu g/m^3$) and the number 10 or 2.5 refers to the particle's maximum diameter in micrometres.

clean air policies, especially when concerned with their impact on economic growth and job creation (Morgenstern et al., 2002).

The impact of air pollution on individuals' economic outcomes is strong and wide-ranging. Air pollution has been documented to reduce labour supply and productivity in various settings and locations (Aguilar-Gomez et al., 2022; Graff Zivin and Neidell, 2018). There is also evidence of its contemporaneous effects on human capital formation, such as lower performance in high school (Ebenstein et al., 2016; Lavy and S. Roth, 2014) and university examinations (S. Roth, 2016). Based on these results, recent research has tried to uncover the aggregate macroeconomic costs of these negative effects. Fu et al. (2021) and Dechezleprêtre, Rivers and Stadler (2019) found substantial negative macroeconomic consequences, with a 10% increase in ambient $PM_{2.5}$ (a common air pollutant) causally reducing the GDP of China and the EU by 0.4% and 0.8%, respectively. These estimates suggest large co-benefits from using less carbon-intensive fuels, reducing the overall cost of the efforts set in the Paris Agreement to decarbonise the global economy.

On the other hand, the external validity of these studies for the US economy is nonetheless questionable as the US differs strongly from the EU and China in various important characteristics that could potentially affect the relationship between pollution and aggregate economic production. First, US households are less vulnerable to ambient air pollution as 90% of their households have an air conditioner, compared to 10% in Europe (IEA, 2018a). Additionally, sick leaves in the US are less common than in Europe, making less relevant a proven mechanism in which air pollution can reduce overall GDP (Holub et al., 2006; Leroutier and Ollivier, 2022). Furthermore, air pollution-induced illnesses like asthma might even positively affect GDP due to the US' costly and highly private healthcare system. Finally, secondary results from Williams and Phaneuf (2019) conclude that air pollutants had no effect on manufacturing establishments, employment, or wages in the US, suggesting its effects on overall GDP might differ from other studied regions.

The main objective of this article is to contribute to this literature and estimate the causal effect of air pollution on US macroeconomic outcomes such as local GDP, GDP per capita, and industry-specific GDP. To recover this effect, I use panel data on local economic outcomes and ambient exposure to $PM_{2.5}$ from 2001-2018 at the US county level. The main obstacles in estimating the effect of air pollution on economic output with ordinary least squares regression are reverse causality and measurement error. Reverse causality results from air pollution being a by-product of economic activities. Additionally, 'classical' measurement error[2] (thereafter just "measurement error") is prevalent feature of studies on air pollution (Graff Zivin and Neidell, 2013). To overcome these problems, I use various instruments to create conditionally exogenous variation in air pollution levels together with a set of fixed effects to control for constant and time-varying confounders. This is a standard estimation strategy of various previous studies on the effect of air pollution on outcomes with large geographic extent such as Dechezleprêtre, Rivers and Stadler (2019), Borgschulte et al. (2022), Arceo et al. (2016), Fu et al. (2021), Chen et al. (2017) and Sager (2019). For this, I use year-on-year changes of two instruments: ambient exposure to wildfire-induced air pollution (also referred below as 'wildfire smoke') the prevalence of thermal inversions. Exposure to wildfire smoke proves to be a stronger and more consistent instrument across the US geography and is therefore chosen for the main results described below.

As anticipated, my results for the US deviate from previous analyses from Dechezleprêtre, Rivers and Stadler (2019) for the European Union and Fu et al. (2021) for China, finding precise and insignificant effects of $PM_{2.5}$ on overall GDP, GDP per capita, GDP per employee and population in urban regions. On the other hand, air pollution has a significant negative impact on US rural areas' GDP and GDP per capita of 0.47% (SE: 0.21%) and 0.42% (SE: 0.21%) per $\mu g/m^3$ of average ambient exposure to $PM_{2.5}$, respectively. This effect in rural areas seems to be only present during working days and with air pollution levels above the median concentration (8.2 $\mu g/m^3$). Concerning individual sectors, only "Trade", "Educational Services", and

[2] A random noise in the data with mean 0 and positive variance. This is different to the "endogenous" measurement error driven by strategic action explained in Section 2.4 and of the fact that that air pollution studies struggle to measure the exact "exposure" to humans to air pollution from their changing local concentrations.

“Other Services” experience a significant negative effect of 0.52% (SE: 0.21%), 0.81% (SE: 0.31%), and 0.43% (SE: 0.22%) per μg/m^3, respectively. These translate to a yearly aggregate loss of 15.3 billion 2012-equivalent dollars of GDP (185 per capita) per year. Finally, a large set of robustness tests with alternative samples, instruments, regions, pollution measures, and model specifications are performed, with no significant changes from the main results.

Focusing on the significant results found for rural areas, I explore their long-term effects and perform a simple back-of-the-envelope calculation comparing the costs of the Clean Air Act amendments of 1990 with its potential benefits (in terms of rural GDP). Regarding the long-term effects, the shock only decreases the GDP growth in that year, but its effect on the GDP level seems to be permanent as no rebound effect is visible in the following years. On the other hand, the back-of-the-envelope calculations show that nation-wise air pollution reduction abatement costs from the Clean Air Act Amendments were less than half as large as the benefits in rural areas' GDP of its air pollution reduction and thus further air pollution reduction policies could be beneficial even if we only consider the lower bound of short-term market costs, greatly increasing their political feasibility.

The rest of this paper is structured as follows: Section 2.2 discusses the current literature and how it guides my research; Section 2.3 details the research strategy; Section 2.4 describes the data sources, their transformation, and descriptive statistics; Section 2.5 describes the results and performs a back-of-the-envelope calculation with the Clean Air Act costs and benefits; Section 2.6 concludes.

2.2 Background and Literature Review

Air pollution more generally, and $PM_{2.5}$ specifically, have been consistently found to increase the risks of death and hospitalisation for cardiovascular and respiratory diseases both in the short and long term (US EPA, 2009). As mentioned before, these strong effects have recently been shown to be only the tip of the iceberg, with less life-threatening but more common effects of $PM_{2.5}$ having deep societal consequences.

After pollution particles are inhaled, they can pass from the lungs to the bloodstream, finally affecting multiple organs such as the heart and the brain (Calderón-Garcidueñas et al., 2014; Du et al., 2016; Ranft et al., 2009). Even when it does not cause hospitalisations, short-term air pollution exposure can reduce working hours and increase sick leaves of workers (Fan and Grainger, 2023; Hoffmann et al., 2022; Holub et al., 2006; Leroutier and Ollivier, 2022; Ron Chan et al., 2022) and caregiving activities when vulnerable population, such as kids, get sick (Aragón et al., 2017; Hanna and Oliva, 2015).

On top of changes in the number of hours worked, air pollution has been shown to reduce productivity at work in a wide range of occupations including outdoor and indoor, physical and desk-based. Some examples include pear packers in California (Graff-Zivin and Neidell, 2012), garment factories in India (Adhvaryu et al., 2022), call centres in China (Graff Zivin and Neidell, 2013), investors in New York (Heyes, Neidell and Saberian, 2016) and Canadian members of Parliament (Heyes, Rivers and Schaufele, 2019). More generally, Fu et al. (2021) looked at changes in productivity due to air pollution for a representative sample of Chinese manufacturing firms and found an elasticity of -0.44, with large effects in both high- and low-technology industries (elasticities of -0.73 and -0.33, respectively). Even more subtly, air pollution can reduce productivity while at work (Hanna and Oliva, 2015) by increasing fatigue, impairing cognition, or increasing stress (Sager, 2019) and sleeplessness (Heyes and Zhu, 2019). Finally, in a more recent study for the US, Cook and Heyes (2022) show that psychological exposure, i.e. 'the thought of pollution' can also reduce willingness to work (labour supply) and work performance (labour productivity) in an experimental setting.

The literature has also studied the adverse effects of air pollution on cognitive performance and human capital formation by looking at high school and university test results in the US, Israel, and the UK (Gilraine and Zheng, 2022; Lavy and S. Roth, 2014; S. Roth, 2016). More generally, Zhang et al. (2018) find similar results in nationally representative cognitive tests of Chinese families, finding larger negative effects for men and low-income families. All of them coincide that air pollution can cause a decrease in cognitive performance in the studied populations.

In summary, short-term exposure to air pollution can reduce both the number of hours worked and the productivity (per hour worked) of an individual. This has the potential to reduce overall production and change the determinants of local employment, wages, and income. Various recent papers have looked at this question too, by focusing on the regional effects of higher average pollution levels over some months or quarters. This is especially relevant when we are interested in how it affects aggregate economic outcomes in equilibrium, as regional or aggregate effects are not necessarily equivalent to the sum of short-term local effects due to spatial spillovers of long-run consequences. In a recent publication, Borgschulte et al. (2022) use the geographic extension of wildfire plumes to estimate the causal effect of air pollution changes on quarterly labour earnings, employment, and labour force participation in the US. They find a $1\mu g/m^3$ increase in $PM_{2.5}$ reduced earnings by 1.8%, employment by 0.12% and TFP by 0.27%. Focusing on the firm side, Leroutier and Ollivier (2022) find that a monthly $1\mu g/m^3$ increase in $PM_{2.5}$ decreased monthly sales of French firms by 0.26% in the two months after. On the other hand, Williams and Phaneuf (2019) found no effect of SO_2 and NO_x on manufacturing establishments, employment, and wages during the 1999-2003 period in the US.

The literature on how changes in average air pollution exposure can determine aggregate regional production and productivity is relatively recent. The two papers published on the topic are Fu et al. (2021) and Dechezleprêtre, Rivers and Stadler (2019) which study the case of China and the EU, respectively.

Fu et al. (2021) use data from a nationally representative sample of China's manufacturing firms from 1998 to 2007 and estimate the causal effects of air pollution on productivity and hiring. They use thermal inversions as an instrument to find that a $1\mu g/m^3$ increase in $PM_{2.5}$ decreases productivity by 0.82%. Using the differential effect between coastal and inner regions of China's access to the WTO, they estimate the effect of output on $PM_{2.5}$. Finally, they estimate the general equilibrium effects of $PM_{2.5}$ on GDP, concluding that an increase of 10% in $PM_{2.5}$ ($\approx 5\mu g/m^3$) is expected to cause a decrease of 0.4% in China's GDP.

Dechezleprêtre, Rivers and Stadler (2019) use the same methodology with thermal inversions as an instrument as Fu et al. (2021) but directly estimate the effects of air pollution on local GDP, GDP per capita and each sector's GDP. With 2000-2015 NUTS-3 data for the EU, they estimate that a 10% increase ($1\mu g/m^3$) in $PM_{2.5}$ causally decreases GDP by 0.8%, roughly the size of a small EU Member Country (such as Slovakia or Hungary) or 200€ per inhabitant per year. They conclude that 95% of this impact is due to reductions of output per worker (absenteeism or productivity) and that the largest effect is in the agricultural sector (5 times the overall effect). This is the closest work to this one, and their formalisation of the effect on air pollution on GDP can be applied here. Lastly, recent work by Behrer et al. (2023) uses the same strategy to get the estimate for India, finding a reduction of GDP by 0.7% for each μg increase in $PM_{2.5}$ per square meter.

Finally, neither this paper nor the ones just mentioned are exercises to calculate the sum of all economic costs of air pollution. GDP aggregates all market production and ignores non-market products and externalities. Economic research has used other methods, such as Integrated Assessment Models, to calculate the gross annual damages (GAD) of air pollution emissions (Muller and Mendelsohn, 2007), a relevant outcome and complement of this work. Interestingly, Tschofen et al. (2019) compare sector-specific GAD with their value added, finding that GAD is concentrated in 3 sub-sectors: Farms, Utilities, and Truck Transportation, with Farms having a higher GAD than their value added.

Three major facts motivate a study on these effects for the US. First, only a small set of studies have been published on the topic, in regions outside the US, and with no replications (that I know of). Second, their estimated effects are both statistically significant and of great economic relevance. For example, Dechezleprêtre, Rivers and Stadler (2019) estimate that the reduction in GDP due to air pollution is two orders of magnitude larger than their respective abatement costs, as estimated by the European Commission, and that "significant reductions in air pollution would easily pass a cost-benefit test, even ignoring their large benefits in terms of avoided mortality" (p. 34). It is a highly relevant question if this also applies to the US economy.

Third, it is not straightforward that these estimates from Europe or China would apply to the US, as the US household infrastructure, health system, and labour markets legislation differ strongly from the EU and China, potentially affecting the effect of air pollution exposure on overall GDP. Here I list what I consider to be the most relevant differences:

A) First and foremost is household infrastructure and filtration. This is important given the population of developed countries spends approximately 90% of their time indoors (Klepeis et al., 2001), and household characteristics can be a crucial determinant of how ambient air pollution affects indoor air pollution. The main variable to understand the relationship between ambient and indoor air pollution is the infiltration rate. This is how much the pollution inside a house is determined by changes outside of it. Building age is one of its main determinants as older houses tend to allow more of the outside air inside.[3] Active air filtration becomes relevant after the outside air enters the house. This filtration can be done by purposely built filters, but the most prevalent mechanism of air filtration is Air Conditioning (AC)[4]. An attentive reader would have noticed that the US has a better position in both, with a significantly newer housing stock and a much higher installation and use of air conditioning than Europe[5], and thus we could expect changes in ambient air pollution to have a larger effect in the European Union than in the US. Estimates of infiltration rates support this, with Europe having an average estimated rate of 55% while the US is closer to 15% (Burke, Heft-Neal et al., 2022; Hänninen et al., 2011). Interestingly, Burke, Heft-Neal et al. (2022) finds larger infiltration rates during smoke events, lower infiltration rates at higher levels of $PM_{2.5}$ (probably due to adaptation behaviour) and large heterogeneity in rates between houses in the US. On the other hand, Hänninen et al. (2011) finds infiltration rates in Europe to be larger in

[3]According to D. Zhao et al. (2015), a typical old home can have an infiltration rate of 30%, an average home would be closer to 17% and new energy efficient homes would have values below 5%.

[4]The lowest quality AC filters (MERV5, used by 25% of households in the US in 2012 (D. Zhao et al., 2015)), filter approximately 17% of ultrafine particles ($PM_{0.1}$). Common AC systems do much better work, filtering from 40 to 80% of these particles, with the newer ones filtering from 90 up to 100% (Azimi et al., 2014).

[5]According to the IEA (2018b) in 2018 90% of US households and 60% of Chinese households had air conditioning installed. This compares to only 20% in Europe for the year 2020 (Quefelec, 2022). Furthermore, cars also filter the outside air and commuting by car is much more prevalent in the US than in Europe.

summer (69%) than in winter (45%). Finally, this is highly dependent on the actual time people spend indoors in each area. If a higher proportion of work and leisure time is spent outside, such as in rural areas, people's exposure would be less protected by the household characteristics and air filtering systems.[6]

B) All else equal, previous work has shown that increases in pollution are more harmful if the average level is already high (Dechezleprêtre, Rivers and Stadler, 2019). The US has a lower average ambient exposure to $PM_{2.5}$, and thus could be less affected by its changes.[7] C) Sick leaves are a usual mechanism by which air pollution can affect economic outcomes. With sick leaves being much less prevalent in the US (3.6 days per year) than in Europe (≈11) (OECD, 2023), the effect of air pollution on GDP can be further diminished. D) The health system in the US is especially expensive and thus it could transform negative shocks of air pollution-induced illnesses into increases in GDP. For instance, air pollution-induced illnesses like asthma can cause medical expenses to surge. The impact of these conditions could be substantial, as approximately 15.4 million people in the US are diagnosed with asthma, with their average annual per-person medical cost being around $3,266 for the insured and $2,145 for the uninsured (Nurmagambetov et al., 2018).

2.3 Research Strategy

2.3.1 Econometric Specification

I start with a simple linear regression of the relation between a measure of economic output Y and average ambient air pollution exposure P in county c, state s, and year t

$$ln(Y_{cst}) = \beta_0 + \beta_1 P_{cst} + \boldsymbol{\beta_2} f(\boldsymbol{W}_{cst}) + \gamma_c + \phi_c t + \eta_{st} + \varepsilon_{cst} \tag{2.1}$$

Where $ln(\cdot)$ represents the natural logarithm, $f(\boldsymbol{W}_{cst})$ is a flexible function that captures any surface-level weather shocks that might affect both a county's pollution

[6] This can be the case for rural areas in the US, China, or Europe. The results, below, point in that direction.

[7] The average ambient exposure in the US for my sample is 8.35 $\mu g/m^3$ (see Table 2.2) while in Europe is 15.96 $\mu g/m^3$ (Dechezleprêtre, Rivers and Stadler, 2019) and 53.52 $\mu g/m^3$ in China (Fu et al., 2021).

and economic activity[8], γ_c are county fixed effects which capture any time-invariant differences between counties (such as geography), ϕ_c are county-specific slopes, η_{st} are state-year fixed effects which account for unobserved time-varying regional or policy shocks which might be correlated with both economic activity and pollution across states, and ε_{cst} is a random disturbance term.

To control for permanent county characteristics (γ_c) and to address the non-stationarity of the left-hand-side, I model the variables of Equation (2.1) in differences:

$$\Delta ln(Y_{cst}) = \beta_1 \Delta PM2.5_{cst} + \boldsymbol{\beta_2 \Delta} f(\boldsymbol{W}_{cst}) + \phi_c t + \eta_{st} + \Delta\varepsilon_{cst} \tag{2.2}$$

With $\Delta X_t \equiv X_t - X_{t-1}$

In this case, the specification continues to model the *levels* of Y_{it} (not its yearly growth) and β_1 can be interpreted as the expected change in the *contemporaneous* growth rate given an increase in the level of $PM_{2.5}$.[9]

My objective is to capture the causal effect of changes in $PM_{2.5}$ in the aggregate output level from possible changes in employment, productivity, and population, but various obstacles arise. First, changes in economic output can also affect the $PM_{2.5}$ concentrations as air pollution is a by-product of economic activity. This reverse causality would create a positive bias in our coefficient of interest, β_1. Furthermore, most air pollution estimates are prone to 'classical' measurement errors which would bias β_1 towards 0.

To overcome the issues of reverse causality and measurement error, I need a mechanism that affects exposure to ambient air pollution exogenously. In other words, one which is only related to the outcome of interest (Y) through its effect on ambient air pollution.

[8] As suggested by Deschênes and Greenstone (2011), this is modelled in bins of various weather variables. It includes second-degree polynomials for atmospheric pressure and humidity and 20 bins of surface temperature, 10 bins of rain, 10 bins of snow, 12 bins of wind speeds and interactions between the temperature bins and both humidity and humidity squared .

[9] So $\beta_1 \approx \frac{E\left(Y_{cst}|PM2.5_{cst+1}\right) - E(Y_{cst}|PM2.5_{cst})}{E(Y_{cst}|PM2.5_{cst})}$.

Based on the literature, I use the dissemination of wildfire smoke by wind currents and the presence of thermal inversions as two mechanisms that serve as a natural experiment after controlling for confounders. To do this, I adopt a two-stage estimation method with instrumental variables. The first stage predicts exogenous changes in air pollution exposure with changes in wildfire smoke or thermal inversions. The second stage estimates the effect of the predicted exogenous changes in air pollution on economic output.

The first stage that estimates an exogenous variation in air pollution can be written as

$$\Delta PM2.5_{cst} = \boldsymbol{\alpha_1} \Delta \mathbf{I_{cst}} + \boldsymbol{\alpha_2} \boldsymbol{\Delta} f(\boldsymbol{W}_{cst}) + \boldsymbol{\alpha_3} \boldsymbol{\Delta C}_{cst} + \rho_c t + \theta_{st} + \Delta \pi_{cst} \tag{2.3}$$

where $\mathbf{I_{cst}}$ is a set of one or more instruments constructed with the presence or strength of wildfire-induced $PM_{2.5}$ or thermal inversions in county c and year t and $\boldsymbol{C}_{cst}$ are instrument-specific controls that help satisfy the conditional exogeneity between the instruments and Y_{cst}. θ_{st} are state-year fixed effects and π_{it} is the error term. The second step that estimates the effect of exogenous variation in $PM_{2.5}$ exposure in percentage changes of economic outcome Y can be written as

$$\Delta ln(Y_{cst}) = \beta_1 \Delta \widehat{PM2.5}_{cst} + \boldsymbol{\beta_2} \boldsymbol{\Delta} f(\boldsymbol{W}_{cst}) + \boldsymbol{\beta_3} \boldsymbol{\Delta C}_{cst} + \phi_c t + \eta_{st} + \Delta \varepsilon_{cst} \tag{2.4}$$

where $\widehat{PM2.5}_{cst}$ is ambient air pollution exposure predicted by the first stage, all other variables are defined as in Equation (2.1), and the error term ε_{cst} is clustered at the county level. Table 2.1 describes the variables included in $\mathbf{I_{cst}}$ and $\boldsymbol{C}_{cst}$.

It is important to note that as both steps are estimated in differences, the effects are estimated by *within-county* changes in the instrument. This also recentres the estimate, avoiding biases arising from non-random exposure to exogenous shocks as described by Borusyak and Hull (2023).[10] Furthermore, including state-year fixed effects (θ_{st} and η_{st}) controls for any state-level time-specific shock such as federal legislation, differentiated

[10]This is relevant for this study as some areas in the US have a higher variation in both Thermal Inversions and wildfire-smoke $PM_{2.5}$ as shown in Figure 2.A.2 and Figure 2.B.2.

Table 2.1: Combinations of instruments and instrument-specific controls

$\mathbf{I_{cst}}$	$\boldsymbol{C_{cst}}$
Average exposure to wildfire-induced $PM_{2.5}$ *(main specification)*	• Presence of any wildfire in the county on that year • log(share of county area burned) on that year
Share of days with the whole county covered by wildfire smoke *(robustness)*	• Presence of any wildfire in the county on that year • log(share of county area burned) on that year
Share of days with thermal inversions *(robustness)*	• *None*

impacts of the 2008 financial crisis, or the number and intensity of wildfires on that state-year pair. Finally, adding county-specific slopes helps to control for any general relationship between the *growth* of the instrument and the outcome. For example, if urban areas within a state face a higher increase in both their GDP growth rate and wildfire smoke exposure. Research by Kestelman (2023) shows an example of this potential concern, where housing development in California could increase the risk of wildfires, increasing both the growth of Y_{cst} and wildfire-induced $PM_{2.5}$.

For my coefficients (β_1) to be representative of the whole contiguous US[11], I weigh individual counties i by their population or aggregate economic production. This is common in the literature (Dechezleprêtre, Rivers and Stadler, 2019; Kalkuhl and Wenz, 2020) and avoids giving disproportional weights to a large number of counties with relative low population and economic production.

The following directed acyclic graph (DAG) summarises the implied relationships between the variables in the two-step procedure:

[11] Continental US, excluding Alaska and Hawaii.

$$
\begin{tikzcd}
& \Delta\text{Weather}_{cst} \textit{ and } \Delta\text{Controls}_{cst} \arrow[dl] \arrow[d] \arrow[dr] & \\
\Delta\text{Instrument(s)}_{cst} \arrow[r, "\alpha_1"] & \Delta\text{Air Pollution}_{cst} \arrow[r, "\beta_1"] & \Delta\text{Economy}_{cst} \arrow[l, bend left] \\
& \text{State}\times\text{Year FE}_{st} \textit{ and } \text{County slopes}_{ct} \arrow[ul] \arrow[ur] &
\end{tikzcd}
$$

Using an instrumental variable addresses the reverse causality issue and changes the nature of the other two primary sources of bias: omitted variable bias and measurement error in air pollution. For omitted variable bias to be a concern before using an instrument (Equation 2.2), the non-controlled correlation had to be between changes in economic output and air pollution, two closely related variables. The use of an instrument corrects for the measurement error bias, while the omitted-variable bias now can only emerge from non-controlled correlations between changes in economic output and thermal inversions or wildfire-induced $PM_{2.5}$, both of which are clearly less plausible and relevant. This is especially the case after difference out all state-level variation and county-specific trends and controlling for weather conditions and instrument-specific controls. Nonetheless, the use of instrumental variables requires additional assumptions to hold to avoid new types of biases. These are explained in the following subsection.

2.3.2 Instrumental Variables

The two-stage method requires one (or more) instrumental variables that (1) affect air pollution exposure (i.e., are relevant), and (2) that they are not caused by pollution or economic activity and only affect the dependent variable through their effect on air pollution concentrations (i.e., that they are exogenous). The following paragraphs explain how exposure to wildfire-induced $PM_{2.5}$, my preferred instrument, can satisfy both conditions in the two-stage method shown above. To evaluate the robustness of my estimates, I also use exogenous changes in the frequency of thermal inversions, which are explained in detail in Appendix A.

Like other sources of air pollution, the combustion of vegetation creates particulate matter and other contaminants such as ozone, carbon monoxide, atmospheric mercury, and a variety of volatile organic compounds (VOCs). This pollution is then ejected into the atmosphere and dispersed by wind currents. Thus, it is the location and exact timing of wildfires, the amount of air pollution emitted, and the hour-specific pattern of weather conditions and wind currents that determine the actual locations that are affected by shocks of wildfire-induced pollution of various magnitudes. This is the source of the identifying variation used in the instrumental variable specification.[12]

While exposure to wildfire smoke is understood to operate similarly to other sources of air pollution, its chemical composition may differ. Thus, it can affect humans differently per unit of measured particulate matter. I explore this in depth and conclude that daily changes in wildfire-induced $PM_{2.5}$ concentrations in my sample have an almost-zero correlation with other pollutants such as SO_2, CO and NO_2 concentrations and have a higher correlation with O_3 as overall $PM_{2.5}$ ($\hat{\rho} = 0.15$). These results coincide with previous atmospheric science literature (Langmann et al., 2009) on the composition of wildfire smokeand allow to interpret my results as the effects of particulate matter ($PM_{2.5}$) isolated from other usual co-pollutants in the literature such as SO_2, CO, and NO_2.[13] This "chemical disconnection" also signals that wildfire-induced pollution is usually generated at a faraway source. The comparison population-weighted pairwise correlations between $PM_{2.5}$, wildfire-induced $PM_{2.5}$, and other pollutants is available in Figure 2.B.7 in the appendix.

In addition to the literature mentioned above on the effects of air pollution, large increases in wildfire smoke have been shown to produce various behavioural responses, including spending more time indoors, running air conditioners for longer times, and missing work (Langmann et al., 2009). Burke, Heft-Neal et al. (2022) record a wide range of awareness and behaviour changes in response to a substantial increase in wildfire smoke, including mobility, sentiment, and health-protective behaviours. This contrasts with smaller (and more common) changes in urban air pollution created by other factors.

[12] The work by Wen and Burke (2022) is an example of using both the presence and magnitude of wildfire-induced $PM_{2.5}$ as an independent variable.

[13] Additionally, if one subtracts the estimated wildfire-induced $PM_{2.5}$ from the total $PM_{2.5}$ to create a non-wildfire-induced equivalent (arguably, this would mostly be composed of pollution that comes from local sources and thus is influenced by the local economy), the correlation between wildfire- and non-wildfire-induced $PM_{2.5}$ is virtually zero.

In the data, wildfire smoke creates both small and large increases in daily $PM_{2.5}$, with minor changes being much more common.[14]

Wildfires account, on average, for around 17% of the $PM_{2.5}$ emitted in the United States in the last 20 years.[15] Wind currents then carry these emissions for thousands of kilometres (Langmann et al., 2009), a process that depends on current weather conditions such as moisture, rain, and heat. Thanks to satellite-based plume identification and other information on the trajectory of pollution, it is possible to predict the average exposure to $PM_{2.5}$ that originated from wildfires in each county and day. That makes the relevance of this instrument fully dependent on the prediction quality as it is on the same units as the endogenous variable ($PM_{2.5}$ exposure). On the other hand, it is convenient to think about its exogeneity to local economic activity in detail. For that, it simplifies things to think about wildfire smoke as originating from two different sources:

First, let us assume that the source of the smoke is sufficiently far away that its only effect on local economic outcomes is through the change in air pollution it generates. In that case, various concerns might arise. Unobserved local characteristics might be correlated with being down or upwind of wildfire-prone areas, but as my specification is in differences, I consider only the *changes* in local characteristics, such as deviations from prevailing wind patterns. Also, various large-scale weather shocks can affect regional economic activity while influencing the dispersion of wildfire drift from far away locations, such as a dry or windy season (Langmann et al., 2009). State-year fixed effects would account for any such regional deviations from the national average.

Second, we can focus on the case where the source of the smoke is close enough to affect local economic outcomes through other channels. These could be direct fire damages to property and amenities, emergency responses, or visible dust exposure of other pollutants which, as pointed out before, can cause their own behavioural responses. For

[14] I test if small or large *changes* in pollution have different effects by unit of $PM_{2.5}$ both by interacting the increase in $PM_{2.5}$ with the number of days that a given county experienced large changes in wildfire-induced pollution. I find no significant indication that this is not the case, although a specification with yearly data has a relatively weak power to test these results.

[15] Own calculation with data from the EPA (2022).

these local effects, I include variables that control for the presence and scale of a wildfire within a given county.[16] Lastly, even if we consider the occurrence, strength, and impact of wildfire events in local air pollution levels to be determined by the random variation of weather conditions (wind, rain, dryness, or heat)[17], these might affect economic activity by themselves. For that, I control for local weather conditions non-parametrically with large set of flexible weather controls as in Deschênes and Greenstone (2011).[18]

Finally, the variation in wildfire-induced air pollution is not necessarily homogeneous across US. This implies that some regions will have a higher variation in the instrument and thus a higher influence on the results than others, with the final results representing the "average effects for subpopulations that are induced by the instrument to change the value of the endogenous regressors", or local average treatment effect (LATE) (Imbens and Wooldrige, July 20, 2007). To visually understand the degree of this heterogeneity, Figure 2.B.2 illustrates the geographical distribution of the instrument variation (the average absolute change in the prevalence of wildfire-induced $\mathrm{PM}_{2.5}$ exposure, $avg(|\Delta WildfirePM_{2.5}|)_c$. From it, it is possible to conclude three relevant points: First, most counties share a similar variation of the instrument and thus the results will be representative of a large majority of the US geography; Second, only a very small minority of counties have almost no variation in the instrument and thus very little influence in the results[19]. Third, only a very small set of rural counties located in the northwest — which represent less than 0.01% of the population — has an exceptionally large variance in the instrument (an average yearly change above $1.5\mu\mathrm{g/m}^3$). These are excluded from all regressions to avoid them from having a disproportional effect in the results.[20]

[16] The main results are also robust to excluding county-year pairs that had a wildfire.

[17] Although it is hard to know the causes of wildfires, less than 1% of fires are considered due to arson in the combined wildfire dataset provided by the Forest and Rangeland Ecosystem service (Welty and Jeffries, 2019).

[18] Weather controls include atmospheric pressure and humidity squared, 20 bins of temperature, 10 bins of rain, 12 bins of wind speeds and interactions between the temperature bins and humidity.

[19] Of these the only one to have a significant economic weight is Phoenix, Arizona, but there is no reason to believe that the effect of air pollution there would be significantly different than the rest of the country

[20] Figure 2.B.11 in the Appendix shows the results with various thresholds, concluding that the selection of this threshold does not significantly changes the results, and if anything gives conservative estimates for some of them.

2.4 Data

To study the causal effect of $PM_{2.5}$ exposure on aggregate economic variables with the methods described in the Research Strategy Section (2.3), I aggregate multiple sources of $PM_{2.5}$ concentrations and emissions, economic variables, weather variables, and population density rasters on a county-by-year panel data. In the following paragraphs, I explain in detail the data sources, modifications, aggregations, and cleaning procedures used to construct the final panel data structure.

2.4.1 Data Sources

I start with county-level economic outcomes and demographics such as real GDP and GDP by sector[21], employment, and population. All of these are from the US Bureau of Economic Analysis (BEA)[22] and are yearly (2001-2018) estimates by county.[23] All other estimates of control values are spatially aggregated to these county-by-year dimensions. The BEA estimates total and sector-level official GDP figures using the income approach as in national GDP. This is done by summing up labour income and proprietors' income (both are readily available at a county-year level and represent the majority of GDP), and estimates of consumption of fixed capital, corporate profits, and taxes on production, less subsidies and net transfers.[24]

To look at each county's average exposure to air pollution, I use the pollution estimates constructed by Hammer et al. (2020). They consist of yearly (2000-2018) estimates

[21]The sectors available in the data with their 2017 NAICS codes are: Agriculture (11), 'Mining, Oil and Gas' (21), Utilities (22), Construction (23), Manufacturing (31-33), Professional Services (54-56), Trade (Wholesale and retail) (42-45), Transportation (48-49), Information (51), Finance (52-53), Educational Services (61), Health Services (62), 'Accommodation, food and arts' (71-72), Other Services (81), and Government (92). For a complete description and examples of firms on each sector, refer to Table 2.B.1.

[22]Source: CAGDP9 and CAINC4 datasets from https://www.bea.gov/data/employment/employment-county-metro-and-other-areas

[23]The total number of counties in my sample is around 1% less than the official number as some groups of counties ('combination areas') in Virginia are taken as one unit in the data. These consist of "one or two independent cities with 1980 populations of less than 100,000 combined with an adjacent county."

[24]These are jointly estimated and called "other income payments and costs" (OIC). The key assumptions of the BEA to estimate OIC are the following: (1) within a state, the OIC share by county is proportional to its share of value-added, and (2) within a county, the growth of OIC is proportional to the growth in sales. For a more detailed explanation, see Aysheshim et al. (2020).

of surface $PM_{2.5}$ mean concentration in a 0.01º x 0.01º grid (approx. 1km^2 at the equator). These estimates were constructed using both satellite and monitor data from the WHO Global Ambient Air Quality Database, are used extensively in the literature, and cover the contiguous United States.[25] I use this data source that combines satellite and ground measurements for my main specification for two main reasons: first, it provides me with complete spatial coverage at a fine resolution, allowing to measure regional changes in air pollution that are not due to individual sources. Furthermore, it is possible to combine these precise estimates with population density maps and create population-weighted exposure to ambient air pollution[26]. Secondly, Mu et al. (2022) documented how local air pollution monitoring stations of 14 metro areas in the US had tended to skip monitoring when they expected air quality to deteriorate. As wildfire-induced air pollution surges can be easily predicted, satellite measurements would avoid the resulting bias from this endogenous measurement error. For an alternative measure of local air pollution, I also use the county-level pollution estimates from Borgschulte et al. (2022), taken directly from monitor stations. Although they are only available for around half of the counties, they still cover more than 85% of the U.S. population. For the emissions of $PM_{2.5}$ I use the estimates of Global Air Pollutant Emissions from the EDGAR database (v5.0) from Crippa et al. (2020).[27]

To have information on the exposure to wildfire-induced $PM_{2.5}$, my main instrumental variable, I use the methodology and source data provided in the replication package of Childs et al. (2022)[28] and perform a slight change of their model (described in the next subsection) to predict ambient wildfire-smoke-attributable $PM_{2.5}$ on a daily 10km^2 grid for the contagious US (2006-2018). This prediction of wildfire-induced air pollution is a much more detailed proxy to the one used by Borgschulte et al. (2022), mentioned above, as it not only gives information of the geographic extent of wildfire smoke, but

[25]Source: https://sites.wustl.edu/acag/datasets/surface-pm2-5/#V4.NA.03

[26]Naturally, these estimates are not perfect proxies of individual exposure to air pollution as individuals continuously move in space, and enter and exit indoor spaces which might have air conditioning or other types of air filtration. While imperfect and only a measure of ambient (not indoor) air pollution exposure, these are substantially better than estimates from a relatively small set of monitors or satellite averages that do not consider the approximate location of individuals over an area as large as a US' county.

[27]Source: https://edgar.jrc.ec.europa.eu/gallery?release=v50_AP&substance=PM2.5&sector=TOTALS

[28]Source: https://www.stanfordecholab.com/wildfire_smoke

its quantity measured in $PM_{2.5}$.[29] This source code in its original version has been used by Burke, Childs et al. (2023) to describe the impact of wildfire-induced air pollution on total pollution trends. Finally, to get additional information on the location and extent of wildfires, I use a combination of the wildfire datasets for the United States done by the Forest and Rangeland Ecosystem Science Center (Welty and Jeffries, 2019)[30], which provides polygons of wildfires by day together with additional metadata on their documented causes.

Weather measurements are used to create the thermal inversion instrument and construct the surface weather controls. For the weather controls I use data on temperature, pressure, wind speed and humidity from NASA's MERRA 2 reanalysis (Randles et al., 2017)[31] at an hourly rate for a 0.50º x 0.625º grid, and daily precipitation data (interpolated from monitors) from the NOAA CPC on a 0.25ºx0.25º definition (Xie et al., 2007)[32]. Thermal inversions are also constructed based on data from NASA's MERRA 2[33] which provides estimates of air temperature a in a 0.50º x 0.625º grid at multiple heights (pressure levels). The construction of that instrument is detailed in Appendix 2.A. Finally, I use 1km^2 estimates of population density from the 2010 US census by CIESIN (2017) to create population-weighted averages for each county of surface weather conditions, $PM_{2.5}$ emissions and concentrations and instruments[34]. An illustrative example of this procedure is available in Figure 2.B.9.

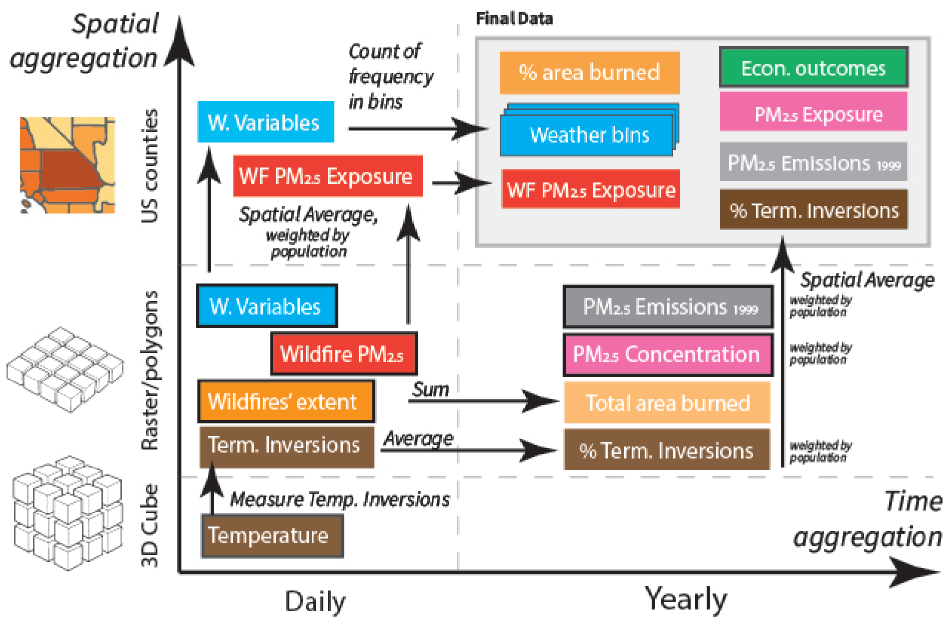
2.4.2 Data Transformation

The construction of the final panel data consists in the transformation of the 7 data sources just described into the county-year format of aggregate economic data: Economic outcomes, the share of days with temperature inversions, weather control variables at surface, temperature at various pressure levels, average historical emissions of $PM_{2.5}$, the proportion of area burnt by wildfires and average wildfire-induced $PM_{2.5}$

[29] I use Borgschulte et al. (2022) classification as an alternative instrument for a robustness test.
[30] Source: https://www.sciencebase.gov/catalog/item/5ee13de982ce3bd58d7be7e7
[31] Specifically the M2T1NXSLV_5.12.4 files (GMAO, 2015b): disc.gsfc.nasa.gov/datasets/M2T1NXSLV_5.12.4
[32] Source: psl.noaa.gov/data/gridded/data.unified.daily.conus.html
[33] Specifically the M2I3NPASM_5.12.4 files (GMAO, 2015a): disc.gsfc.nasa.gov/datasets/M2I3NPASM_5.12.4
[34] For a variable X and county c, the population-weighted average is defined as: $\bar{X}_c = (\sum_{g \in G} X_g \times Population_g)/population_c$, with g being a raster grid and G being the set of grids inside that county.

exposure. Each of these transformations is explained in the following paragraphs, with the entire process being illustrated in Figure 2.1.

Figure 2.1: Summary of the construction of the final panel data.



Notes: Different data sets (marked with black rectangles) begin with different spatial and temporal dimensions and are joined in the 'Final Data' format (county-year pairs) after various transformations (arrows).

First, the economic outcomes are not changed as they already start as county-year pairs. Estimates for the average ambient exposure and emissions of $PM_{2.5}$ in each county are constructed from a population-weighted average of 1km^2 or 10km^2 grids of yearly estimates, thus reducing the measurement error. The data to construct the weather controls starts with a set of daily weather rasters of surface measurements (such as precipitation, temperature, humidity, and atmospheric pressure) which are aggregated by county using population-weighted averages. To flexibly control for weather variables, I follow Deschênes, Greenstone and Shapiro (2017) and create counts of the number of days an average measurement falls inside a given bin (as explained above in the Econometric Specification subsection).

I then go to construct the two instruments used to generate quasi-random variation in the exposure to pollution. First, I use the replication package from Childs et al. (2022) and perform a slight change in their model to predict wildfire-smoke attributable $PM_{2.5}$ on a daily $10km^2$ grid. Their original model uses information from satellite-based smoke plume identification, simulations of air trajectories from fire locations and satellite-inferred Aerosol Optical Depth (AOD) to predict wildfire pollution peaks detected by air pollution monitors using a gradient-boosted decision trees. Once the model is trained and evaluated, they use it to predict wildfire pollution peaks in the whole contiguous US. My version of this model uses all the same methods and data sources but excludes Aerosol Optical Depth (AOD) as an input of the model. This is because AOD is a measure of local air pollution and thus could introduce a reverse causality loop with local economic activity and bias the IV estimates.[35] After having the gridded predictions, I use population weights to get the daily ambient exposure to wildfire smoke for each county and day, from which I create yearly averages. As I want to distinguish between the effects of air pollution from wildfires and other effects they might have on neighbouring economic activity, I create indicators of the proportion of area burned by year in each county. This comes from daily polygons of wildfire extent that are aggregated by year and then averaged by county.

The second instrument used is the prevalence of thermal inversions. As a dataset of this phenomenon is not readily available, I construct my own measure from NASA's MERRA-2 database. Air temperature measurements are available in a 3-hourly 3D raster (with layers of altitude 200-1000m wide). Given that a thermal inversion happens when temperature increases with altitude, the instrument is constructed by comparing the temperature of overlapping layers in a unique coordinate grid. This can be done in multiple ways by looking at the presence or the 'strength' of thermal inversions. I focus on the definitions presented by Dechezleprêtre, Rivers and Stadler (2019) and Chen et al. (2017), which look at differences in temperature in the closest layers to the surface. First, I average all measurements to the day, and then I count a day-grid as

[35] The diagnostics of the model used for this paper can be found in Figure 2.B.12, which closely mirrors Childs et al. (2022)' Figure 2 and shows the model overall and geographic precision, most relevant variables, and predictors of precision performance.

having a thermal inversion if the measurement at the lowest altitude level is *colder* than the one just above it. An extensive explanation of how exactly this is constructed is available in Appendix 2.A. After having a daily indicator of thermal inversions, I count the proportion of days in a year a given grid had a thermal inversion. Finally, these grids are averaged for each county using population weights[36].

After having a unified dataset with all the necessary variables, I check the data for outliers. The distribution of year-on-year changes in average county $PM_{2.5}$ exposure and growth levels of GDP, GDP per capita, GDP per employee and population have a small number of observations showing extreme changes, most probably due to extreme local events in the case of economic variables, and measurement error or large wildfires in the case of $PM_{2.5}$ estimates. I exclude all observations of the top and bottom 0.5% of these 5 variables, ensuring that the remaining data is not driven by extreme values[37], this excludes 3% of all observations. For sector regressions, all county-year pairs that exhibit an extreme growth in the top or bottom 0.5% on any sector are also discarded ($\approx$ 7% of the sample).

2.4.3 Descriptive Statistics

Here I present the descriptive statistics and maps to describe the variation (Table 2.2) and geographical distribution (Figure 2.2) of the variables used.

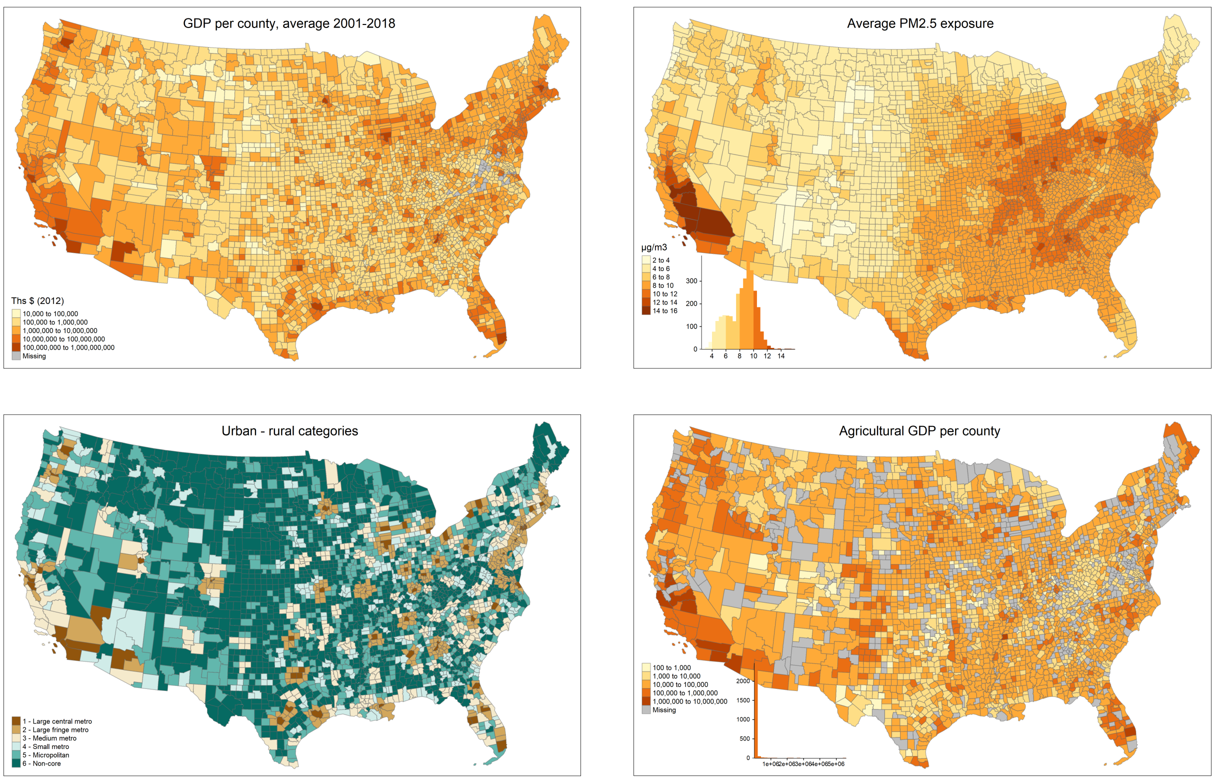
From the first map of average GDP per county, it is evident that economic production is heavily concentrated in a few counties (see that the legend is in a logarithmic scale of thousands of US$). This highlights the motivation to weight the regressions by county production or population, as not doing so would assign equal weights to counties with vastly different economic relevance. The second map shows the average exposure to ambient $PM_{2.5}$ over the sample period (2001-2018). This map also shows wide heterogeneity between regions, with the east side of the contiguous US having higher

[36] In the case there are no raster centroids in a county, the closest raster centroid to the county centroid is selected.

[37] The counties of St Bernard Parish, St. John the Baptist Parish, and Kemper County are completely excluded from the sample due to their substantial relative changes in population and GDP due to extreme weather events and very large infrastructure constructions (hurricane Katrina and the Kemper Project power plant)

levels of ambient air pollution exposure, with the area of Los Angeles on the top. The third map shows the urban and rural categories used, which are a measure of urbanity. The fourth map shows the average Agricultural GDP of each county in the US. As the other indicators of sectorial GDP, it is highly concentrated in some counties and has a fair proportion of counties with no data.

Figure 2.2: Regional variation of overall GDP, $PM_{2.5}$ exposure, Urban-Rural categories, and Agricultural GDP by US counties



After excluding observations with extreme changes in average air pollution or economic output, the resulting panel data consists of 3077 counties over 18 years (2001-2018). Nevertheless, the measure of average wildfire exposure to $PM_{2.5}$ is available for only 12 years (starting from 2006), and one time period is lost when analysing the outcomes in differences.

For this final data, Table 2.2 describes the main variables' overall (x_{it}), between ($\bar{x}_i$)

Table 2.2: Panel Descriptive Statistics

Variable	Panel	Mean	Sd	Min	Max	Observations
GDP (M$, 2012)	Overall	5,169	21,900	9.95	710,893	N = 53614
	Between		21,497	15.37	588,194	n = 3065
	Within		2,650	-106,130	132,731	$\bar{T}$ = 17.49
Δ ln(GDP)	Overall	0.02	0.07	-0.29	0.39	N = 50549
	Between		0.02	-0.07	0.30	n = 3065
	Within		0.07	-0.34	0.38	$\bar{T}$ = 16.49
Δ ln(GDP/capita)	Overall	0.01	0.07	-0.29	0.38	N = 50216
	Between		0.02	-0.08	0.25	n = 3042
	Within		0.07	-0.36	0.39	$\bar{T}$ = 16.51
Δ ln(GDP/employee)	Overall	0.01	0.07	-0.29	0.37	N = 50216
	Between		0.02	-0.08	0.26	n = 3042
	Within		0.07	-0.35	0.39	$\bar{T}$ = 16.51
Δ ln(Population)	Overall	0.00	0.01	-0.04	0.05	N = 50216
	Between		0.01	-0.02	0.05	n = 3042
	Within		0.01	-0.05	0.06	$\bar{T}$ = 16.51
$PM_{2.5}$ Exposure	Overall	8.35	2.70	2.07	42.48	N = 53614
	Between		2.31	2.73	33.01	n = 3065
	Within		1.51	-2.41	18.66	$\bar{T}$ = 17.49
Δ Avg. $PM_{2.5}$	Overall	-0.19	0.91	-3.01	3.14	N = 50549
	Between		0.14	-0.74	0.38	n = 3065
	Within		0.91	-3.44	3.32	$\bar{T}$ = 16.49
Δ Avg. Wildfire $PM_{2.5}$	Overall	0.03	0.39	-3.26	3.59	N = 35827
	Between		0.05	-0.48	0.51	n = 3065
	Within		0.39	-3.02	3.69	$\bar{T}$ = 11.69
% Area Burned	Overall	0.00	0.01	0.00	0.64	N = 53614
	Between		0.01	0.00	0.19	n = 3065
	Within		0.01	-0.19	0.60	$\bar{T}$ = 17.49
Area Burned = 0	Overall	0.88	0.32	0.00	1.00	N = 53614
	Between		0.22	0.00	1.00	n = 3065
	Within		0.24	-0.06	1.83	$\bar{T}$ = 17.49
Prop. Inversion Days	Overall	0.23	0.09	0.00	0.64	N = 53596
	Between		0.09	0.00	0.55	n = 3064
	Within		0.02	0.14	0.47	$\bar{T}$ = 17.49
Δ Prop. Inversion Days	Overall	0.00	0.03	-0.14	0.24	N = 50532
	Between		0.00	-0.01	0.02	n = 3064
	Within		0.03	-0.14	0.22	$\bar{T}$ = 16.49
Avg. $PM_{2.5}$ Emis. (1999)	Overall	48.69	95.8	0.81	1749	N = 53614
	Between		95.11	0.81	1749	n = 3065
	Within		0.00	48.69	48.69	$\bar{T}$ = 17.49

and within ($x_{it} - \bar{x}_i + \bar{x}$) variation. We can first see the extreme differences in GDP levels between counties, also visible in Figure 2.2. The growth of GDP, GDP per capita and GDP per employee show much less heterogeneity. Although some regions grew at an average of 30% a year and others shrank at an average of 7%, the variation is strong both within and between counties. Average air pollution exposure shows significant variation both in levels and changes, with much of the heterogeneity on the changes in $PM_{2.5}$ exposure happening within counties. The variation in the instruments is also large, especially within counties and not between them. This shows that the instrument is strong within counties while being homogeneous in space, which is crucial for interpreting of the final coefficients as causal effects representative of the whole sample. Figures 2.B.2 and 2.A.2 in the Appendix show the spatial distribution of the changes in wildfire smoke and thermal inversions, respectively.

2.5 Results

I start with a simple linear model looking at the conditional correlation between yearly changes in average exposure to $PM_{2.5}$ and GDP growth, as described in Equation 2.4 above. The results, available in Table 2.3, show that there is no significant correlation between these two variables.

Table 2.3: Linear model

	All counties Δ ln(GDP)	Urban Δ ln(GDP)	Rural Δ ln(GDP)
Δ PM2.5 exposure	-0.00062	-0.0010	-0.00027
	(0.00057)	(0.00078)	(0.00070)
Nº obs	50531	13101	37396
Nº of counties	3063	789	2272
R^2	0.41	0.55	0.29

Standard errors in parentheses and clustered by county (BEA). Weighted by county population. Weather controls include atmospheric pressure and humidity squared, 20 bins of temperature, 10 bins of rain, 12 bins of wind speeds and interactions between the temperature bins and humidity and humidity squared.
* $p < 0.1$, ** $p < .05$, *** $p < .01$

But as explained above in detail, air pollution emissions are a by-product of economic activity and thus these estimates are expected to have a positive bias. To correct for the intrinsic reverse causality between changes in air pollution exposure and economic growth and move to causal estimates, I use the two-stage method described in Section 2.3. In the first stage, I use the exposure of $PM_{2.5}$ from wildfires to predict average $PM_{2.5}$ exposure while controlling for various possible confounders as in Equation 2.3.[38] The results of this first stage are shown in column (1) of Table 2.4, where, as expected, a $1\mu g/m^3$ increase in $PM_{2.5}$ exposure from wildfire smoke is almost exactly equivalent to a $1\mu g/m^3$ increase in overall exposure.[39] This is then a especial case in which it would be possible to use the created prediction of $PM_{2.5}$ exposure from wildfires in a simple reduced-form linear regression specification, and get remarkably similar results. Nevertheless, I keep the IV setup to be consistent with the literature and continuously show the strength and validity of this instrument.

The second stage estimates the aggregate causal effect of $PM_{2.5}$ in economic output as in Equation 2.4, these are presented in columns 2-5 of Table 2.4 with their respective Kleibergen-Paap F statistic (larger than 700). Second-stage coefficients for GDP, GDP per capita, GDP per employee, and Population are almost identical to zero and not statistically significant, even if the standard errors are equivalent or smaller to previous results from the literature.

On the other hand, these null effects on economic outcomes for the whole US can hide heterogeneous effects across sectors, geography, or time. I perform various alternative specifications and tests to explore this heterogeneity and give a notion of the possible mechanisms behind any effect.

[38] The residual variance in wildfire $PM_{2.5}$ exposure and thermal inversions after including their respective controls can be seen in Figure 2.B.8 in the appendix.

[39] Other alternative instruments are also explored in the Appendix on Table 2.B.3, showing the first stage for each one of them and including the ones used by Borgschulte et al. (2022), Dechezleprêtre, Rivers and Stadler (2019) and Fu et al. (2021).

Table 2.4: Effect of $PM_{2.5}$ on economic output

	First Stage	Second Stage			
	(1)	(2)	(3)	(4)	(5)
	Δ $PM_{2.5}$ exposure	Δ ln(GDP)	Δ ln(GDP/capita)	Δ ln(GDP/employee)	Δ ln(Population)
Δ $PM_{2.5}$ exposure		-0.0017	-0.0014	-0.00021	-0.00032
		(0.0021)	(0.0022)	(0.0022)	(0.00026)
Δ Wildfire $PM_{2.5}$ exp.	1.04***				
	(0.037)				
Nº obs	35814	35814	35582	35582	35582
Nº of counties	3063	3063	3040	3040	3040
R^2	0.76	0.44	0.43	0.31	0.83
Kleibergen-Paap F		789.1	790.4	790.4	790.4

Standard errors in parentheses and clustered by county (BEA). Weighted by county population. Weather controls include atmospheric pressure and humidity squared, 20 bins of temperature, 10 bins of rain, 12 bins of wind speeds and interactions between the temperature bins and humidity and humidity squared. Includes state-year fixed effects and county-specific slopes.
Stock-Yogo weak ID test for a 10% critical value: maximal IV size of F = 16.38
* $p < 0.1$, ** $p < .05$, *** $p < .01$

Sector-specific GDP: The results on how increases in $PM_{2.5}$ affect different sectors are shown in Figure 2.B.4 and Table 2.B.2. As the geographical distribution of these industries varies widely, counties are weighted by the average level of each sector's GDP over the whole sample period. Contrary to previous research by Dechezleprêtre, Rivers and Stadler (2019) for Europe, I find no significant effects on the GDP of most sectors. Only *Trade*, *Educational Services*, and *Other Services* return significant results, with a 1μg increase in ambient exposure to $PM_{2.5}$ decreasing Trade's GDP by 0.5%, Educational Service's GDP by 0.8% and Other Services by 0.4%. For some of these sectors (such as Mining, Agriculture, Information, or Manufacturing), it could well be those large standard errors, possibly arising from missing data on these sectors (see Table 2.B.2), make plausible relevant effects statistically insignificant, but for others, such as Health Services, Government Expenditure, or Transportation, the results suggest no causal effect. Of the sector-level results that have relatively low margin of error (Educational Services, Professional Services, Trade, Other Services, Health Services, and Government), it is hard to draw direct comparisons with other research on the topic due to changes in the classification of sectors; nevertheless, some parallels can be drawn. First, the effect in Professional Services found by Dechezleprêtre, Rivers and Stadler (2019) for Europe is much larger (2.2%) and outside of the 95% confidence interval of the estimate found here for the US. On the other hand, the negative effect on *Other Services* and *Trade* could be compared with the *Others* category from Dechezleprêtre, Rivers and Stadler (2019)' results, who find an 1.6% reduction per additional μg of average yearly concentration.[40] Finally, it is advisable to treat the statistical significance of these estimates with caution for various reasons. First, as only 20% (3 out of 15) of the coefficients are statistically significant at the 5% level and after adjusting for multiple hypothesis testing, all become insignificant.

Urban vs. rural counties: To look at heterogeneities between geographies, Table 2.5 divides the main results between urban and rural counties, using the NCHS Urban-Rural Classification Scheme for Counties (see Figure 2.2) and defining “large and medium metros” as urban and “small metros, micropolitan and non-core-areas” as rural. The first stage is strong in both samples, although the results differ. As the main results, urban counties do not show significant effects on any of the outcomes. On the other hand, for rural counties, air pollution levels negatively affected both GDP and GDP per capita, but not GDP per employee or population numbers. The coefficients suggest a 1μg increase in average ambient exposure to $PM_{2.5}$ decreased GDP and GDP per capita levels by 0.47% and 0.43%, respectively. These effects, overall, translate to a yearly aggregate loss of 15.3 billion dollars of GDP and 185 dollars of GDP per capita.[41] The results coincide with previous research suggesting the effect of pollution to be larger in rural areas in Europe. On the other hand, the results are half the size of those brought forward by Dechezleprêtre, Rivers and Stadler (2019), with no significant reduction in GDP per employee.

In summary, air pollution does not have a significant effect on urban counties, which represent 73% of the population according to the 2010 census and an 80% of GDP in 2001, while it has negative impacts on rural counties. Given this is a prevalent feature of the results, all other tests of heterogeneity are performed separately for both samples of urban and rural regions.

[40] The comparison with the work of Leroutier and Ollivier (2022) is even harder as a large share of sectors in their data are aggregated based on "business-to-business" or "business-to-consumer" sectors.
[41] 2012 US$-equivalent, base estimates multiplied by the sum of average GDP of rural counties over the study period.

Table 2.5: IV results for urban and rural counties

	First Stage	Second Stage			
	(1)	(2)	(3)	(4)	(5)
	Δ $PM_{2.5}$ exposure	Δ ln(GDP)	Δ ln(GDP/capita)	Δ ln(GDP/employee)	Δ ln(Population)
		Large and medium metros			
Δ PM2.5 exposure		-0.00162	-0.00122	-0.000351	-0.000395
		(0.00274)	(0.00281)	(0.00292)	(0.000318)
Δ Wildfire PM2.5 exp.	1.067***				
	(0.0550)				
Nº obs	9336	9336	9238	9238	9238
Nº of counties	789	789	779	779	779
R^2	0.782	0.572	0.560	0.403	0.873
Kleibergen-Paap F		375.7	372.7	372.7	372.7
		Small metros, micropolitan and non-core areas			
Δ PM2.5 exposure		-0.00472**	-0.00428**	-0.00206	-0.000528
		(0.00210)	(0.00215)	(0.00219)	(0.000356)
Δ Wildfire PM2.5 exp.	0.925***				
	(0.0466)				
Nº obs	25828	25828	25712	25712	25712
Nº of counties	2272	2272	2259	2259	2259
R^2	0.776	0.333	0.323	0.278	0.720
Kleibergen-Paap F		393.5	393.3	393.3	393.3

Standard errors in parentheses and clustered by county (BEA). Weather controls include atmospheric pressure and humidity squared, 20 bins of temperature, 10 bins of rain, 12 bins of wind speeds and interactions between the temperature bins and humidity and humidity squared. Includes state-year fixed effects and county-specific slopes.

* $p < 0.1$, ** $p < .05$, *** $p < .01$

Working days vs. weekends: Another way to understand the mechanisms by which air pollution can affect economic outcomes is to examine weekends and working days separately, as the economic activities performed differ significantly between these periods. Working days are characterised by active realisation and coordination of the productive process, while weekends are more focused on consumption and leisure activities. Additionally, average air pollution levels are generally lower on weekends.

To explore whether the effects of air pollution on the economy differ between the two periods, I construct yearly averages of exposure to wildfire $PM_{2.5}$ during weekends or working days, and use these two new instruments separately. The results, available in Table 2.6, show that the adverse effects of air pollution are concentrated in rural areas as previously observed, however, their effect is only significant during working days and not on weekends.

Table 2.6: IV results for weekends and weekdays at urban and rural counties

	First Stage	Second Stage			
	(1)	(2)	(3)	(4)	(5)
	Δ PM$_{2.5}$ exposure	Δ ln(GDP)	Δ ln(GDP/capita)	Δ ln(GDP/employee)	Δ ln(Population)
Large and medium metros					
Δ PM$_{2.5}$ exposure		-0.000198	0.000130	0.00136	-0.000373
		(0.00321)	(0.00324)	(0.00329)	(0.000306)
Δ Wildfire PM$_{2.5}$ exp. Workday	1.463***				
	(0.103)				
Δ PM$_{2.5}$ exposure		-0.00197	-0.00174	-0.00139	-0.000246
		(0.00232)	(0.00244)	(0.00249)	(0.000345)
Δ Wildfire PM$_{2.5}$ exp. Weekend	2.997***				
	(0.205)				
Nº obs	9206	9206	9124	9124	9124
Nº of counties	788	788	778	778	778
R^2 *(min)*	0.780	0.579	0.566	0.412	0.876
Kleibergen-Paap F *(min)*		200.4	201.4	201.4	201.4
Small metros, micropolitan and non-core areas					
Δ PM$_{2.5}$ exposure		-0.00570**	-0.00520**	-0.00275	-0.000600
		(0.00227)	(0.00230)	(0.00232)	(0.000382)
Δ Wildfire PM$_{2.5}$ exp. Workday	1.155***				
	(0.0655)				
Δ PM$_{2.5}$ exposure		-0.00237	-0.00207	-0.000375	-0.000353
		(0.00245)	(0.00254)	(0.00258)	(0.000371)
Δ Wildfire PM$_{2.5}$ exp. Weekend	2.452***				
	(0.132)				
Nº obs	25828	25828	25712	25712	25712
Nº of counties	2272	2272	2259	2259	2259
R^2 *(min)*	0.770	0.330	0.323	0.278	0.720
Kleibergen-Paap F *(min)*		311.5	313.0	313.0	313.0

Standard errors in parentheses and clustered by county (BEA). Weather controls include atmospheric pressure and humidity squared, 20 bins of temperature, 10 bins of rain, 12 bins of wind speeds and interactions between the temperature bins and humidity and humidity squared. Includes state-year fixed effects and county-specific slopes.

* $p < 0.1$, ** $p < .05$, *** $p < .01$

Non-linearities with respect to background concentrations: A prevalent finding of previous work is that the effect of air pollution can be non-linear with respect to background concentrations, with higher impacts as the baseline pollution concentration grows (Aragón et al., 2017; Dechezleprêtre, Rivers and Stadler, 2019). To assess if this is also the case for US aggregate output, I divide the urban and rural counties into two equal samples by their average ambient pollution levels by county and compare their results, displayed below in Table 2.7.

The results coincide very strongly with the ones in Table 2.5, with average PM$_{2.5}$ exposure decreasing GDP and GDP per capita only on rural areas. On top of this, we

Table 2.7: IV results by levels of background $PM_{2.5}$ at urban and rural counties

[Min Mean Max]	*Stats*	(1) Δ ln(GDP)	(2) Δ ln(GDP/capita)	(3) Δ ln(GDP/employee)	(4) Δ ln(Population)
		Large and medium metros			
Δ Avg. $PM_{2.5}$ exposure	Nº obs: 4581	0.0000	-0.0001	0.0025	0.0001
[3.6 8.2 9.5]	F = 324.1	(0.0045)	(0.0045)	(0.0049)	(0.0004)
Δ Avg. $PM_{2.5}$ exposure	Nº obs: 4579	-0.0041	-0.0038	-0.0024	-0.0004
[9.5 10.9 33.0]	F = 77.2	(0.0030)	(0.0030)	(0.0029)	(0.0005)
		Small metros, micropolitan and non-core areas			
Δ Avg. $PM_{2.5}$ exposure	Nº obs: 12706	-0.0032	-0.0029	-0.0025	-0.0002
[2.7 6.3 8.2]	F = 322.9	(0.0027)	(0.0028)	(0.0029)	(0.0005)
Δ Avg. $PM_{2.5}$ exposure	Nº obs: 13051	-0.0086***	-0.0085***	-0.0047	-0.0004
[8.2 9.6 27.2]	F = 311.2	(0.0032)	(0.0033)	(0.0032)	(0.0005)

Standard errors in parentheses and clustered by county (BEA). Kleibergen-Paap F reported.
Sample divided by the median of the average exposure to ambient pollution by county, over the whole sample period. Weather controls include atmospheric pressure and humidity squared, 20 bins of temperature, 10 bins of rain, 10 bins of snow, 12 bins of wind speeds and interactions between the temperature bins and both humidity and humidity squared. Includes state-year fixed effects and county-specific slopes. First stages are not displayed for simplicity but are all highly significant.
* $p < 0.1$, ** $p < .05$, *** $p < .01$

can see that the previous average effect of 0.47% in rural areas (Table 2.5) is concentrated on county-years with average air pollution levels above the median [8.2μg/m^3], doubling its effect to ≈0.85% of GDP and GDP per capita. These effects are large in scale but coincide with previous findings such as Dechezleprêtre, Rivers and Stadler's 1.0% negative effect for European regions with high pollution. Additionally, these effects are for concentrations with an average of 9.6μg/m^3, showing that increases of air pollution can have large negative effects even when levels are way below the EPA yearly standard of 12μg/m^3. For urban areas, there is no statistically significant effect.

2.5.1 Robustness Tests

To verify the robustness and heterogeneity of my main results, I conducted a series of robustness checks and alternative specifications. These checks are organised into groups of changes in instruments, regions, sample, and others, and are summarised in Table 2.8. The results for rural counties are also examined with a series of alternative specifications, samples and alternative variables, from which the conclusion is that none of them deviates significantly from the results provided above. This is presented in Figure 2.B.10.

Table 2.8: Robustness tests and alternative specifications

Changes in...	Δln(GDP)	Δln(GDP/capita)	Δln(GDP/employee)	Δln(Population)	#Counties / (F))
Instruments					
Share of days with TI interacted with 1990	0.0029	0.0040	0.0008	−0.0007	3061
county $PM_{2.5}$ emissions	(0.0027)	(0.0028)	(0.0027)	(0.0006)	(17)
Winter and summer TI as Fu et al. (2021)	0.0021	0.0024	0.0008	0.0001	3061
and Dechezleprêtre, Rivers and Stadler (2019)	(0.0030)	(0.0031)	(0.0030)	(0.0006)	(19)
Share of days county covered by smoke plume	0.0000	−0.0001	0.0026	−0.0001	3059
polygons as Borgschulte et al. (2022)	(0.0055)	(0.0055)	(0.0055)	(0.0004)	(99)
Saturday-Sunday Wildfire-induced $PM_{2.5}$	−0.0013	−0.0010	−0.0002	−0.0003	3062
	(0.0019)	(0.0020)	(0.0020)	(0.0003)	(387)
Monday-Friday Wildfire-induced $PM_{2.5}$	−0.0009	−0.0006	0.0008	−0.0004	3062
	(0.0025)	(0.0026)	(0.0026)	(0.0002)	(451)
Geography					
North of population centroid	−0.0022	−0.0013	−0.0017	−0.0007	1501
	(0.0022)	(0.0022)	(0.0021)	(0.0004)	(288)
South of population centroid	−0.0004	0.0000	0.0018	−0.0003	1561
	(0.0030)	(0.0031)	(0.0031)	(0.0003)	(183)
East of population centroid	−0.0008	−0.0004	−0.0016	−0.0005	1242
	(0.0024)	(0.0025)	(0.0024)	(0.0005)	(272)
West of population centroid	−0.0013	−0.0009	0.0007	−0.0004	1820
	(0.0026)	(0.0026)	(0.0027)	(0.0003)	(304)
Not weighted	−0.0082 ***	−0.0077 ***	−0.0051 *	−0.0005	3062
	(0.0026)	(0.0027)	(0.0027)	(0.0003)	(861)
Weighted by average GDP	−0.0004	−0.0001	0.0013	−0.0003	3062
	(0.0027)	(0.0028)	(0.0028)	(0.0002)	(395)
Sample					
Including economic outliers	−0.0018	−0.0018	−0.0007	−0.0001	3064
	(0.0014)	(0.0015)	(0.0015)	(0.0002)	(805)
Excluding county-year pairs with active	−0.0022	−0.0017	−0.0006	−0.0005	2978
wildfires	(0.0035)	(0.0035)	(0.0034)	(0.0004)	(310)
Excluding the Great Recession (2008-2011)	−0.0010	−0.0007	0.0005	−0.0003	3058
	(0.0015)	(0.0016)	(0.0015)	(0.0004)	(214)
Excluding the San Francisco	−0.0015	−0.0013	0.0001	−0.0003	3058
	(0.0024)	(0.0025)	(0.0025)	(0.0002)	(462)
Specification					
With monitored $PM_{2.5}$ measurements from	−0.0009	−0.0006	0.0006	−0.0003	1606
Borgschulte et al. (2022)	(0.0029)	(0.0030)	(0.0031)	(0.0003)	(58)
Without county-specific slopes	0.0010	0.0009	0.0022	0.0000	3063
	(0.0019)	(0.0019)	(0.0019)	(0.0004)	(497)
With only year FE	0.0070 ***	0.0064 ***	0.0051 **	0.0007 **	3064
	(0.0024)	(0.0025)	(0.0024)	(0.0003)	(380)
Without weather controls	−0.0008	−0.0006	0.0003	−0.0002	3062
	(0.0022)	(0.0023)	(0.0022)	(0.0002)	(362)

Standard errors in parentheses and clustered by county (BEA). Kleibergen-Paap F reported.

Weather controls include atmospheric pressure and humidity squared, 20 bins of temperature, 10 bins of rain, 10 bins of snow, 12 bins of wind speeds and interactions between the temperature bins and both humidity and humidity squared. Includes state-year fixed effects and county-specific slopes. First stages are not displayed for simplicity but are all highly significant.

* $p < 0.1$, ** $p < .05$, *** $p < .01$

Changes in instruments: To ensure that my main results are not sensitive to the selection of the instrument, I use various alternative instruments: (1) yearly thermal inversions interacted with historical emissions, (2) summer and winter thermal inversions[42] from Fu et al. (2021) and Dechezleprêtre, Rivers and Stadler (2019), and (3) the share of days a county is covered by wildfire smoke plumes used by Borgschulte et al. (2022). These alternative instruments consistently yield the same null results. Additionally, I examine differentiated impacts on weekends and working days for all counties, but the results remain insignificant.

Changes in regions: To examine at regional heterogeneity within the contiguous US, I divided counties into North and South, and East and West based on their location relative to the population centroid, ensuring each sample includes about half of the population. The results are not significantly different from the main estimates. Additionally, I altered the regression weights to explore geographical heterogeneity further. First, I weighted counties by GDP instead of population, interpreting the results as "the average effect to a unit of GDP", and found no significant effects. Secondly, I removed all regression weights to interpret the results as "the average effect to a county" in the contiguous US. From this last regression, a strong negative effect of $PM_{2.5}$ on GDP, GDP per capita, and GDP per employee is clear with a $1\mu g/\mathrm{m}^3$ increase in $PM_{2.5}$ concentrations causing a 0.82%, 0.77% and 0.51% decrease in each of them, respectively. This is a clarifying result as it shows that the effect from changes in $PM_{2.5}$ are concentrated on counties with small populations and GDP.

Changes in sample: All changes in the sample yield results consistent with the main specification. Including all counties, even those with exceptionally large changes in GDP, productivity, or population, only increases the standard errors of the estimates. Excluding the years of the Great Recession (2008-2011) reduces the standard errors but does not alter the estimates. Finally, excluding the San Francisco area, given its significant GDP contribution and recent large wildfire events, yields the same robust results. Additionally, I perform a series of robustness tests by changing the threshold

[42]Summer is defined as April 15th to October 14th and winter as October 15th to April 14th.

of maximum average changes in the instrument, set at $1.5\mu g/\text{m}^3$. The results for these tests are shown in Figure 2.B.11 finding no significant differences.

Specification changes: I explore the robustness of the results with four additional specification changes. First, I use the measure of local air pollution derived exclusively from monitor stations as in Borgschulte et al. (2022). The results are consistent with my main specification[43]. Next, I run the analysis without county-specific slopes or state-year fixed effects to test if these controls were necessary to attain my main results and as a guide of possible biases for future research. Omitting county-specific slopes does not significantly alter my results. However, using only year fixed effects (instead of state-year) in addition to differenced outcomes produces biased results, as it fails to control for unobserved time-specific regional shocks, such as state-level legislation, differentiated impacts of the 2008 financial crisis, or the number and intensity of wildfires within that state-year. Finally, not including a flexible set of weather controls does not change the results or the precision of the estimates.

Placebo tests and long-term effects: Finally, I regress changes in exogenous pollution in time t on past (lags) and future (leads) changes in GDP ($GDP_{t-9}, \ldots, GDP_{t+9}$). I focus on rural counties, as the negative effects of air pollution are observed only in this sub-sample. Regressions with lags of GDP changes serve as a placebo test, as present shocks, if exogenous, should not be correlated with *past* changes in GDP. The results, shown in Figure 2.B.7 in the appendix, indicate that all estimates are statistically insignificant. Additionally, I examine whether air pollution shocks affect future GDP changes. The results, presented in figure 2.B.7 do not point to that conclusion and suggest the impact in GDP growth might be only happen on the year of the pollution shock. The fact that there is no positive coefficient in any future year suggests that this effect is permanent.

[43] The number of counties included is slightly lower and represents counties with a higher population as not all counties had enough pollution monitors to create their measure.

2.5.2 Back of the Envelope Calculation

To assess the relevance of the empirical results, I quantify the potential economic benefits of a realistic air pollution reduction policy under the framework of the Clean Air Act Amendments of 1990. This involves comparing the costs of air pollution abatement to the economic benefits derived from contemporary improvements in rural GDP, estimated using the observed semi-elasticity of GDP with respect to changes in $PM_{2.5}$ levels.[44] As for the results above, one microgram per cubic meter ($1\mu g/m^3$) increase in $PM_{2.5}$ in rural counties results in a 0.47% decrease in their local GDP, leading to a yearly aggregate loss of 15.3 billion 2012 dollars (185 dollars per capita).

Based on research from the US Environmental Protection Agency EPA (2011), the average annual compliance cost to the Clean Air Act regulations in the period from 2000 to 2020 was 8.6 billion 2012 dollars per $\mu g/m^3$ decrease in $PM_{2.5}$. My comparing the two numbers above, it is clear that the increase in rural areas' GDP attributable to this pollution reduction is approximately 77% *greater* than the associated costs. With an average reduction of $6.1\mu g/m^3$, the cumulative net benefits of this policy — excluding other pollutants and additional benefits not reflected in immediate GDP changes — amount to about 838 billion 2012 US dollars over the 2000-2020 period.

2.6 Conclusions

The primary goal of this article was to evaluate the causal influence of air pollution on macroeconomic outcomes in the United States: local GDP, GDP per capita, and industry-level GDP. To recover this impact, I employed a panel data set of local economic outcomes and $PM_{2.5}$ exposure at the county level from 2001 to 2018, as well as two instrumental variables to account for reverse causality and measurement error: year-on-year variations in exposure to wildfire smoke and the occurrence of thermal inversions.

My main findings suggest $PM_{2.5}$ had no substantial effects on overall GDP, GDP per capita, GDP per employee, and population in urban areas even when considering only

[44] This assumes that these contemporary improvements in rural GDP do not accumulate in time and only affect GDP growth on the year of the air pollution reduction.

working days or years with high baseline air pollution. This is contrasts with previous studies by Dechezleprêtre, Rivers and Stadler (2019) for the European Union and Fu et al. (2021) for China. On the other hand, air pollution has a considerable negative impact on rural regions' GDP and GDP per capita of 0.47% and 0.42% per μg/m^3 of average ambient exposure to $PM_{2.5}$, respectively. These go in line with previous research in Europe by Dechezleprêtre, Rivers and Stadler (2019) and on the effect of $PM_{2.5}$ on crop production and employment in the US by Borgschulte et al. (2022). As expected, these effects seem to be more salient when pollution increases during working days or in counties with an already high base level of air pollution. For this last case, a 1μg/m^3 increase of average ambient exposure to $PM_{2.5}$, when its level is above 8.2μg/m^3, can cause up to a 0.86% reduction in local yearly GDP and GDP per capita. Looking at individual sectors, only "Trade", "Educational Services" and "Other Services" GDP seem to exhibit a substantial decrease of 0.5%, 0.8%, and 0.4 per μg/m^3, respectively. These results are robust to a large number of alternative sample limits, instruments, geographies, pollution indicators, and model specifications.

Finally, it is important to have some limitations in mind when interpreting the results of this paper. To start, the use of aggregate data restricts the possibility to explore the mechanisms at play in depth. Additionally, county-level GDP can be considered an imperfect measure from various perspectives. First, it should not be considered as a complete measure of social cost as it does not capture the consumption of non-market assets such as leisure or clean air itself, a critical issue when talking about air pollution.[45] Second, it can result in double counting of investment; both when it is invested and as a stream of future consumption benefits. Finally, as described on the Data Section (2.4), the estimates of GDP rely on some assumptions by the Bureau of Economic Analysis given limited data at the county-year level.

The policy implications of this work can be viewed from two perspectives. First, economically, they depend on the size of air pollution costs (here measured in aggregate production) relative to its abatement costs through policy. The back-of-the-envelope

[45]The EPA's Scientific Advisory Board provides a detailed discussion on this (SAB, 2017)

calculations above show that the economic benefits from reduced air pollution seem to substantially exceed the estimated costs incurred by the Clean Air Act Amendments. This supports the view that relatively small reductions in $PM_{2.5}$ can yield substantial economic returns. This conclusion reinforces the economic rationale for present and future air quality regulations, particularly in regions where the economic impact of pollution is pronounced. This detailed assessment provides policymakers with a grounded justification for continued investment in air pollution control strategies that are both economically beneficial and supportive of public health objectives.

Second, and focusing on public policy, this study provides additional information to policymakers on the distribution of monetary costs of air pollution. The current federal legislation on air quality was last updated in 2012. It thus was conceived with a limited knowledge of the economic costs of air pollution, including the estimates brought forward by this paper. This should lead to a reconsideration of current policy and stricter limits on air pollution levels. These limits would be most effective where the negative impact of air pollution on local GDP is larger, such as in rural areas during working days, or where pollution levels are high enough to affect aggregate GDP (above $8.2\mu g/m^3$ in rural areas). While current legislation already sets a limit for yearly average concentration of $12\mu g/m^3$, it still makes no distinction between areas or days of the week.

In summary, this study contributes to the ongoing debate on the causal effect of air pollution on economic outcomes by using a panel data set from the United States and a robust econometric methodology. The results suggest that the impact of air pollution on US aggregate production is heterogeneous across regions and time. Moreover, these findings highlight the importance of further research on the effects of air pollution on the economy, as well as the need for effective and evidence-based air pollution policies.

2.A Appendix A: Thermal Inversions

2.A.1 Relevance and Exogeneity

Under normal atmospheric conditions, air temperature decreases with altitude (up to the end of the Troposphere, $\approx$ 11 km above sea level). This creates a natural convection flow called 'atmospheric ventilation' that rises and dissipates air from the surface; air that tends to be more polluted. Thermal inversions are a temporal deviation from this rule and occur when a mass of air is below a warmer mass of air. This breaks the convection cycle, eliminating a way in which pollution dissipates and thus potentially increasing air pollution levels (Trinh et al., 2019; Wallace and Kanaroglou, 2009).

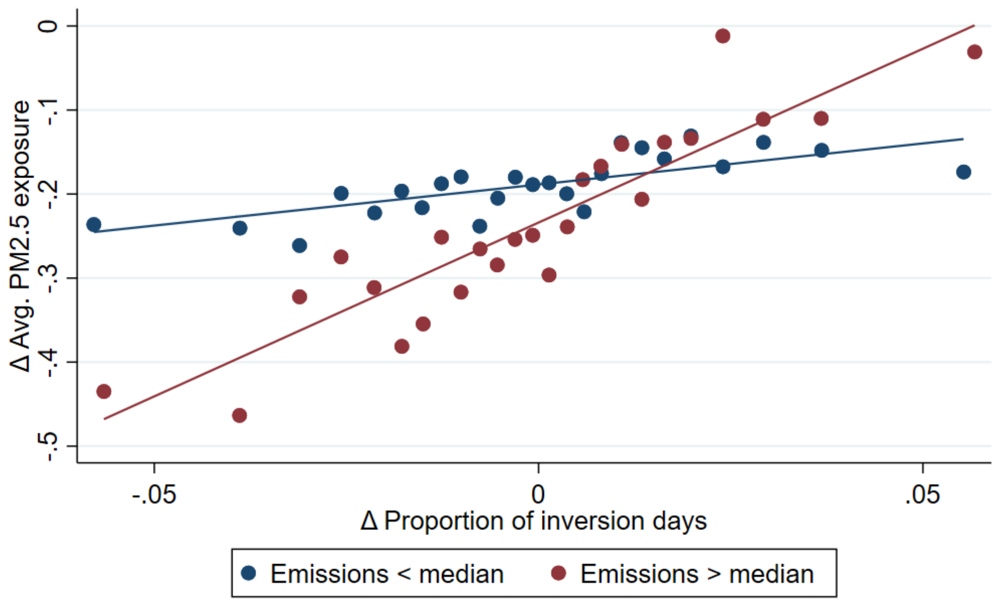
It is important to note that this effect does not create air pollution and thus the relevance if the instrument depends on local emissions. If there are no local emissions, thermal inversions do not affect the concentration of air pollutants. Figure 2.A.1 shows this empirical relationship for US counties above and below the median emissions level, with high-emission counties having a much steeper relationship between the number of inversions and air pollution exposure. This heterogeneity is the main reason why this instrument is not included in the main results, as it is not strong enough in rural areas where emissions per area are much lower than in big cities. To account for this heterogeneity, the equivalent of Equation 2.3 for Thermal Inversions includes an interaction between thermal inversions and the population-weighted emissions from 1999 to predict $\Delta\widehat{PM2.5}_{cst}$[46].

Thermal inversions occur mainly through atmospheric conditions and large movements of air masses. For example, the large-scale movement of air masses throughout the atmosphere typically forms thermal inversions at its leading edge, as warm air masses rise over cooler air masses. Thermal inversions also form in winter at higher latitudes, as the air higher in the atmosphere gets more heat from the low-angle sun than the ground-level air, or when precipitated snow cools the ground-level air. It is important

[46] As I include implicit county fixed effect by taking differences on the right-hand side, we should not worry that 1990 emissions are correlated with thermal inversions, breaking the exclusion restriction. The results of this specification are reported in Table 2.8.

to note that thermal inversions work with different mechanisms in winter and in summer and "summer inversions tend to happen during the morning, whereas winter inversions usually take place in the afternoon, having different effects on the pollution levels" (Hicks et al., 2016). This is why I differentiate between summer and winter inversions in an additional robustness test.

Figure 2.A.1: Relation between the proportion of thermal inversions and the average exposure to $PM_{2.5}$.



Notes: Binned scatterplot of the relationship between changes in the proportion of days with thermal inversions in a year and the changes in average pollution exposure. This is after controlling for weather controls, county-level and state-year FE and weighted by county population. Each dot represents a 4% of the sample.

The validity of the exogeneity assumption can only be viable if thermal inversions are not affected by air pollution concentrations or economic activity. Firstly, existing literature does not support the notion that air pollution can induce thermal inversions. Secondly, air pollution and thermal inversions should not be simultaneously affected by something else. On this, yearly changes in surface temperature are usually assumed to be exogenous in the climate economics literature (Burke, Hsiang and Miguel (2015); Dell et al. (2008); Kalkuhl and Wenz (2020) so it is fair to argue that the air temperature at higher levels that causes thermal inversions, is also exogenous.

While thermal inversions exogenously affect air pollution concentrations, they are also associated with weather conditions that might influence economic activities on the ground (Burke, Hsiang and Miguel, 2015). Thermal inversions can determine cloud formation, reduce precipitation, increase temperatures, and reduce visibility (Encyclopedia Britannica, 2020). This is why I flexibly control for on-the-ground weather conditions as to rule out these potential correlations as Dechezleprêtre, Rivers and Stadler (2019) and Fu et al. (2021). Finally, $PM_{2.5}$ is also (positively and negatively[47]) correlated with other pollutants that are also likely to be affected by thermal inversions. As other research that uses this same instrument (T. Y. Chang et al., 2018), the estimates include the effects of other air pollutants correlated with local concentrations of $PM_{2.5}$.

Finally, thermal inversions and air pollution emissions are not necessarily homogeneous across large extents of land such as the US, Europe, or China. As discussed in Section 2.3, this implies that some regions will have a higher influence in the results than others. To understand the degree of this heterogeneity, Figure 2.A.2 shows the average absolute change in the prevalence of thermal inversions ($\overline{\boldsymbol{\Delta TI}_c}$) and the log of tons of $PM_{2.5}$ emissions per 10km in 1999. Figure 2.A.2 shows that most counties are in the centre of the distribution of both variables and that, in general, there are no large regions with much larger variation in the instrument.

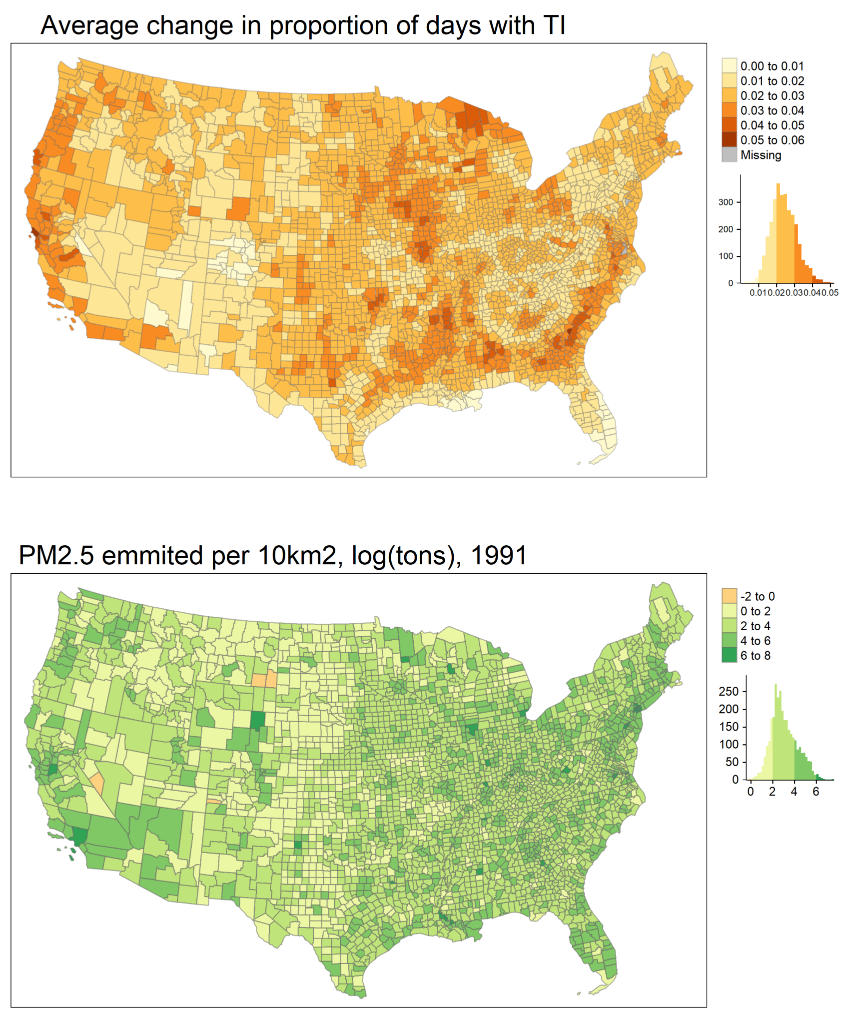
2.A.2 Construction

To construct a measure of thermal inversions I follow Chen et al. (2017) and Dechezleprêtre, Rivers and Stadler (2019). I start with data on air temperature from a 3D raster in which, for each coordinate pair, there are temperature measurements for a set of altitude layers. Layers are separated by 200m (roughly, these depend slightly on the temperature and the initial height as they are defined by pressure levels), with the lowest

[47] Figure 2.B.7 gives my results for the US while Dechezleprêtre, Rivers and Stadler (2019) finds similar results for Europe. This is not the case for wildfire-induced $PM_{2.5}$, which is very weekly correlated with other pollutants.

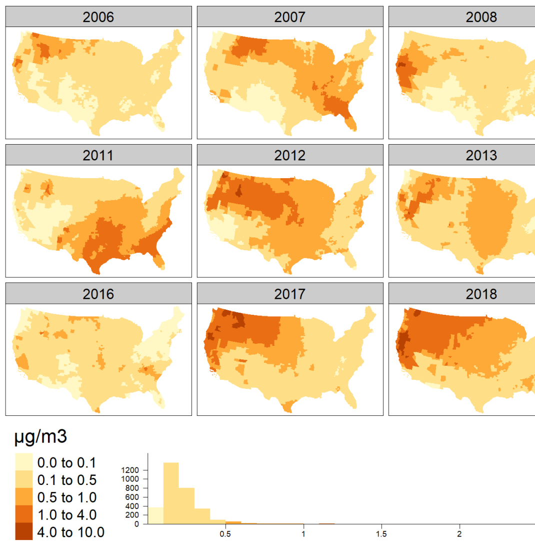
layer representing the first 200m above the surface. As thermal inversions are a deviation from the monotonic declining relationship between altitude and air temperature, and I focus on surface air pollution, I define a given day as having a thermal inversion if the temperature of the second layer is higher than that of the surface.

Figure 2.A.2: Identifying variation of the thermal inversion instrument.



2.B Appendix B: Additional Tables and Figures

Figure 2.B.1: Geographical distribution of yearly exposure to Wildfire $PM_{2.5}$



Notes: These series of maps illustrate the average ambient exposure to wildfire-induced $PM_{2.5}$ for each year and each county (all values of the main instrument). Although the northwest tends to have higher levels of exposure, there is significant variation across time and space.

Figure 2.B.2: Identifying variation of the wildfire-induced $PM_{2.5}$ instrument

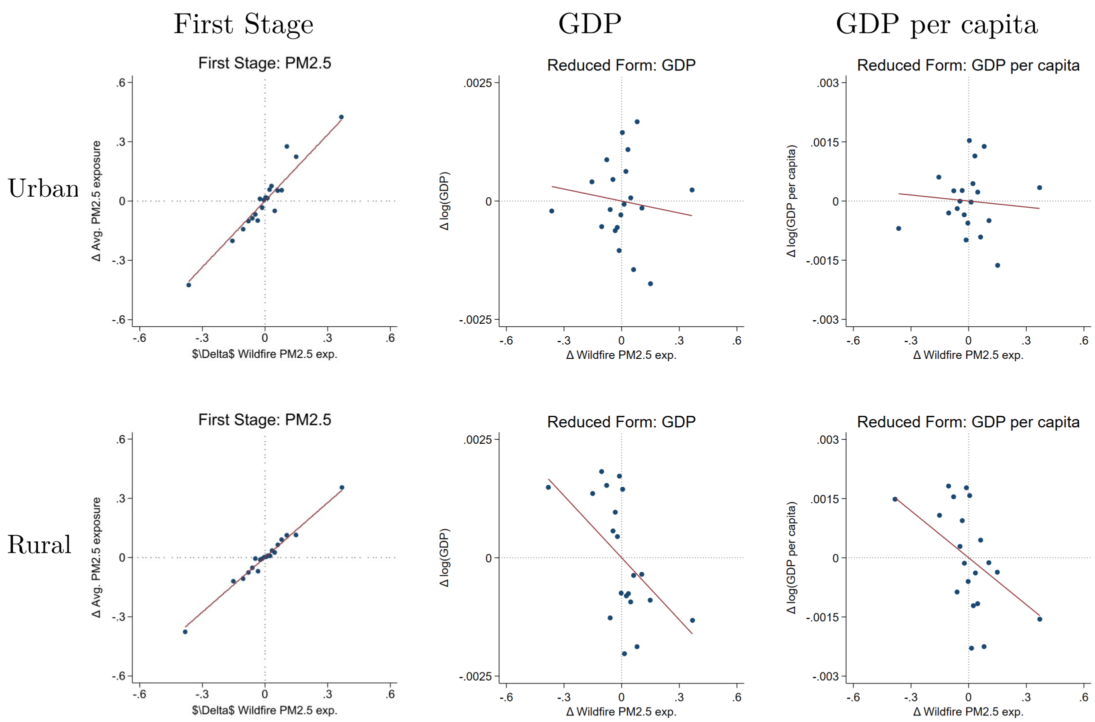
Notes: This figure is meant to help the reader have an intuition of relative weight that each county has on the final results. First, it shows the geographical distribution of the average yearly *changes* of exposure to Wildfire $PM_{2.5}$ $avg(|\Delta Wildfire\ PM_{2.5}|)_{county}$ in the colours. Second, it transforms the shapes of US counties so their area is proportional to their population. Counties in black, with low population and high variance in the instrument ($> 1.5\mu g/m^3$), are excluded from all regressions.

Table 2.B.1: Sector Description, NAICS Code, and Examples

Sector	**NAICS Code**	**Complete Sector Description**	**Firms Examples**
Agriculture	11	Agriculture, Forestry, Fishing and Hunting	Cargill, ADM, Tyson Foods, JBS USA
Mining, Oil and Gas	21	Mining, Quarrying, and Oil and Gas Extraction	ExxonMobil, Chevron, BP, Rio Tinto, BHP
Utilities	22	Utilities	Duke Energy, Pacific Gas and Electric, Exelon, NextEra Energy, Southern Company
Construction	23	Construction	Bechtel, Turner Construction, Fluor, Kiewit, Skanska
Manufacturing	31-33	Manufacturing	General Motors, Procter & Gamble, Boeing, Caterpillar, 3M
Trade	42-45	Wholesale and Retail Trade	Costco, Sysco, Grainger, Ferguson, McKesson, Walmart, Target, Home Depot, Lowe's, Walgreens
Transportation	48-49	Transportation and Warehousing	FedEx, Union Pacific, UPS, CSX
Information	51	Information	Google, AT&T, Verizon, Comcast
Finance	52-53	Finance and Insurance, Real Estate and Rental and Leasing	JPMorgan Chase, Goldman Sachs, Bank of America, Wells Fargo, Citigroup, CBRE, Realogy, AvalonBay Communities
Professional Services	54-56	Professional, Scientific, and Technical Services; Management of Companies and Enterprises; and Administrative and Support, and Waste Management and Remediation Services	Deloitte, Accenture, PwC, EY, KPMG, ManpowerGroup, Republic Services, Adecco
Educational Services	61	Educational Services	Harvard University, University of Phoenix, Kaplan
Health Services	62	Health Care and Social Assistance	Mayo Clinic, UnitedHealth Group, Kaiser Permanente, Cleveland Clinic, HCA Healthcare
Accommodation, Food and Arts	71-72	Accommodation and Food Services; and Arts, Entertainment, and Recreation	Marriott, McDonald's, Hilton, Hyatt, Starbucks, Metropolitan Museum of Art, Live Nation, Walt Disney Parks, Universal Studios, Madison Square Garden
Other Services	81	Other Services (except Public Administration)	H&R Block, ServiceMaster, Merry Maids
Government	92	Public Administration	US Department of Defense, City of New York, State of California, Police departments, IRS

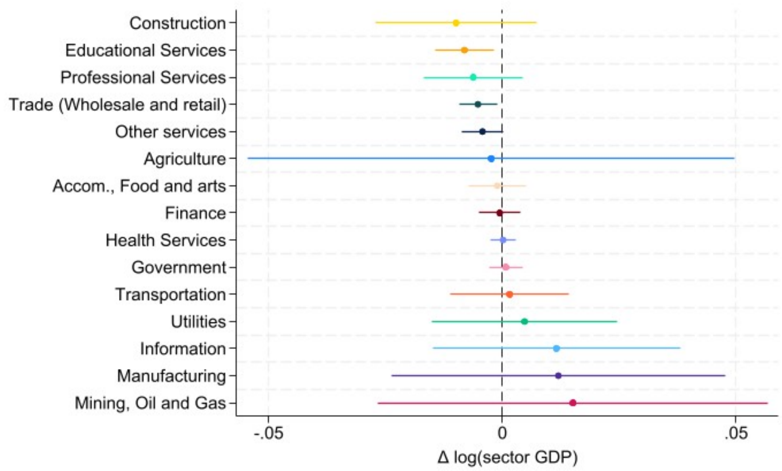
Note: Example firms are added just for illustration purposes and might have activity in multiple sectors apart from their main one.

Figure 2.B.3: Binscatters of the first stage and reduced form for urban and rural counties.



Notes: Each dot represents a 5% of the counties, population-weighted. Outcomes and $PM_{2.5}$ exposure are cleaned of all influence of fixed effects and control variables. The slope of the line of the first stage coincides with the estimates reported in Table 2.5.

Figure 2.B.4: Impact of $PM_{2.5}$ on sector-level GDP



Notes: Impact of an 1μg increase in $PM_{2.5}$ on sector-level GDP with 95% CI, clustered by county (BEA). Full results are available in Table 2.B.2, in the Appendix. Not corrected for multiple hypothesis.

Table 2.B.2: Effect of $PM_{2.5}$ on the output of economic sectors

	(1)	(2)	(3)	(4)	(5)
	Δ ln(Agriculture)	Δ ln(Mining)	Δ ln(Utilities)	Δ ln(Construction)	Δ ln(Manufacturing)
Δ PM2.5 exposure	-0.0024	0.015	0.0047	-0.0099	0.012
	(0.027)	(0.021)	(0.010)	(0.0088)	(0.018)
Nº obs	24470	26875	26532	29757	30055
Nº of counties	2714	2612	2807	2856	2836
R^2	0.23	0.51	0.32	0.50	0.46
Kleibergen-Paap F	236.0	187.1	330.3	454.9	376.5
	(6)	(7)	(8)	(9)	(10)
	Δ ln(Transportation)	Δ ln(Trade)	Δ ln(Information)	Δ ln(Finance)	Δ ln(Prof. Services)
Δ PM2.5 exposure	0.0015	-0.0052**	0.012	-0.00058	-0.0062
	(0.0065)	(0.0021)	(0.013)	(0.0022)	(0.0054)
Nº obs	20810	25146	26544	33332	26119
Nº of counties	2298	2623	2659	3055	2842
R^2	0.39	0.76	0.64	0.60	0.59
Kleibergen-Paap F	303.0	368.4	169.5	348.1	296.1
	(11)	(12)	(13)	(14)	(15)
	Δ ln(Educ. Services)	Δ ln(Health Services)	Δ ln(Acom. Food & Arts)	ln(Other Services)	Δ ln(Government)
Δ PM2.5 exposure	-0.0081**	0.00015	-0.0011	-0.0043*	0.00074
	(0.0031)	(0.0013)	(0.0031)	(0.0022)	(0.0018)
Nº obs	20730	20055	29916	29806	33376
Nº of counties	2337	2250	2966	2914	3057
R^2	0.50	0.55	0.59	0.56	0.60
Kleibergen-Paap F	244.5	334.9	417.9	371.0	317.1

Standard errors in parentheses and clustered by county (BEA). Weighted by average county sector's GDP over the whole sample. Counties with the 0.5% more extreme growth in any sector are excluded. Weather controls include atmospheric pressure and humidity squared, 20 bins of temperature, 10 bins of rain, 10 bins of snow, 12 bins of wind speeds and interactions between the temperature bins and both humidity and humidity squared. Includes state-year fixed effects and county-specific slopes.

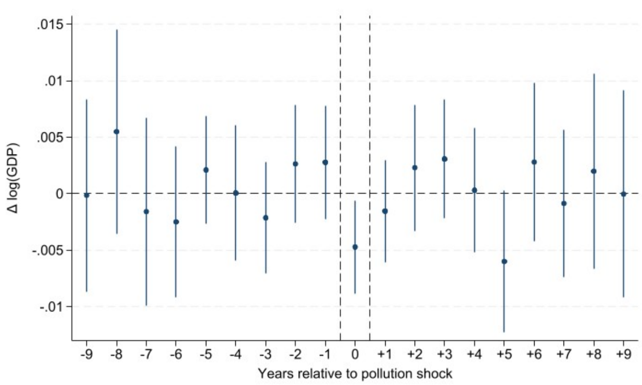
* $p < 0.1$, ** $p < .05$, *** $p < .01$

Table 2.B.3: Alternative first stage specifications

Instruments	Δ PM2.5 Exposure								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
ΔWildfire $PM_{2.5}$ exposure (main instrument)	1.091***				1.071***				
	(0.0502)				(0.0480)				
ΔWildfire $PM_{2.5}$ exposure with AOD as predictor		1.076***							
		(0.0506)							
Δ Prop. days with TI			3.388***	-2.812**	4.052***	-2.449***	4.360***		
			(0.645)	(1.150)	(0.689)	(0.887)	(0.753)		
Δ Prop. days with TI^2				11.68***					
				(2.450)					
ΔProp. days with TI × log(avg. emissions)						1.289***			
						(0.269)			
ΔProp. days with TI × Urban (1990) = 0							-3.175***		
							(0.536)		
ΔProp. days with TI (Summer)								1.550***	
								(0.414)	
ΔProp. days with TI (Winter)								1.804***	
								(0.298)	
ΔProp. days smoke as Borgschulte et al. (2022)									0.0217***
									(0.00218)
Nº obs	35058	35058	49479	49479	35046	49479	49479	49479	32180
Nº of counties	3062	3062	3061	3061	3061	3061	3061	3061	3059
R^2	0.759	0.760	0.741	0.744	0.767	0.743	0.743	0.741	0.729
Kleibergen-Paap F	471	452	28	23	250	17	18	19	99

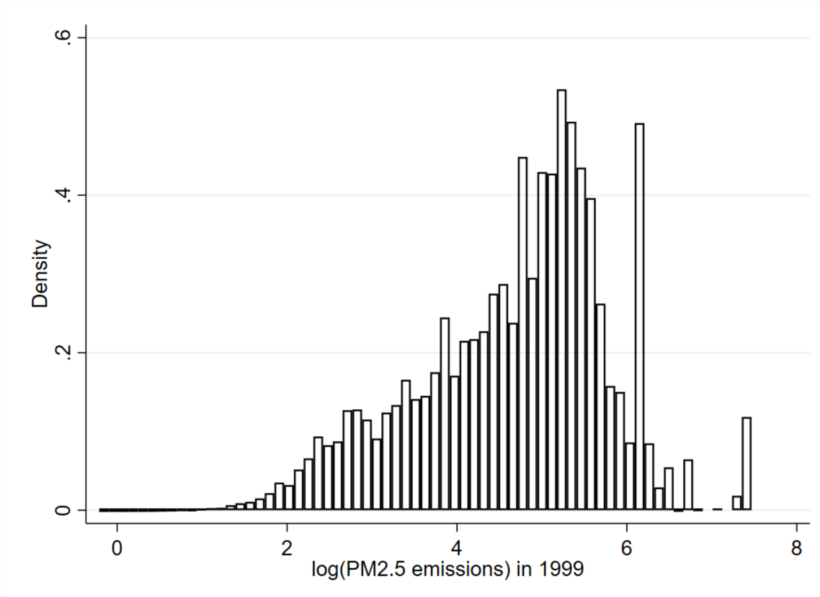
Standard errors in parentheses and clustered by county (BEA). Weighted by county population. Includes state-year fixed effects and county-specific slopes. Weather controls include atmospheric pressure and himidity squared, 20 bins of temperature, 10 bins of rain, 10 bins of snow, 12 bins of wind speeds and interactions between the temperature bins and both humidity and humidity squared.
* $p < 0.1$, ** $p < .05$, *** $p < .01$

Figure 2.B.5: Placebo and long-term effects — Rural counties



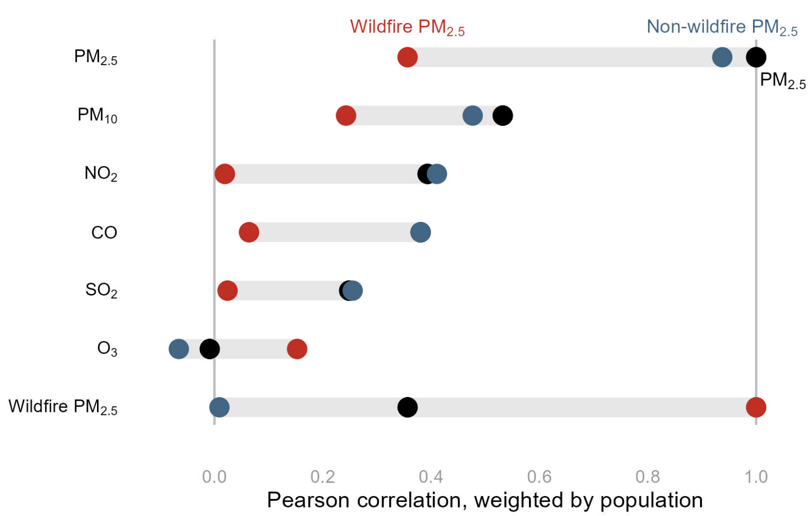
Notes: Estimated coefficients of β_1 for the effect of $\Delta PM_{2.5,t=0}$ on $\Delta log(GDP_t)$ for 9 years before and after in rural areas with 95% C.I. clustered by county (BEA). $t = 0$ corresponds with the effect in rural areas' GDP from Table 2.5.

Figure 2.B.6: Histogram of log(Emissions) in 1999



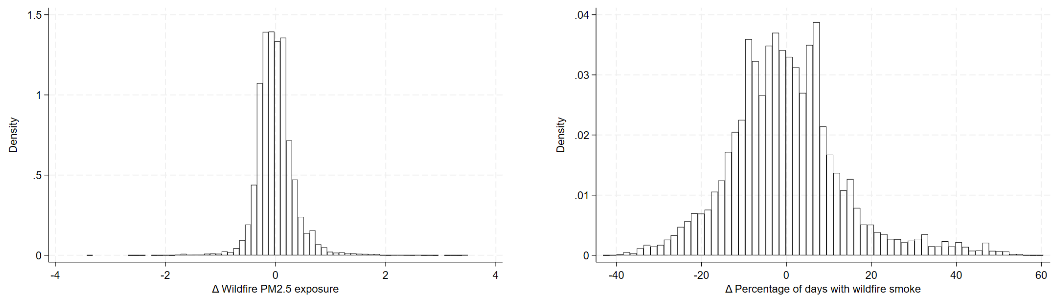
Notes: Density weighted by county population

Figure 2.B.7: Correlation of daily *changes* in wildfire-, non-wildfire-induced, and total $PM_{2.5}$ concentrations and other common pollutants.



Notes: These are average Pearson correlations between various pollutants and particulate matter concentrations, calculated over 1000 population-weighted bootstrap samples of counties. Daily concentrations of $PM_{2.5}$, PM_{10}, SO_2, NO_2, CO and O_3 are from monitoring station data form Borgschulte et al. (2022). "Wildfire $PM_{2.5}$" corresponds to my measure of daily exposure to $PM_{2.5}$ from wildfire smoke and non-wildfire $PM_{2.5}$ is equal to $PM_{2.5}$ − Wildfire $PM_{2.5}$. Correlation estimates are very precise, with the highest standard error (clustered by county) being 0.006.

Figure 2.B.8: Histogram of residual variation in the instruments after including controls

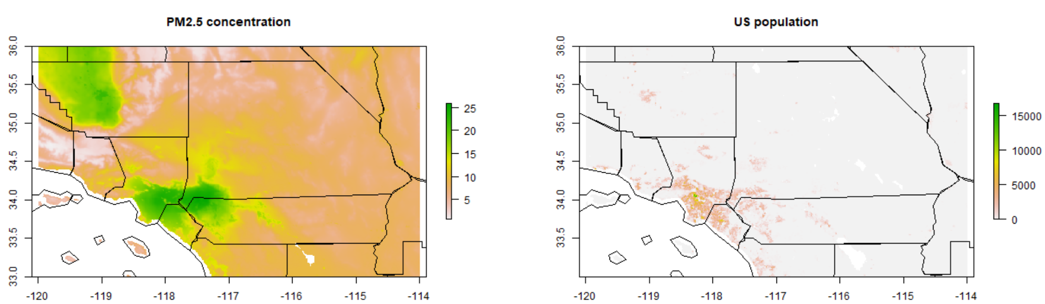


Yearly wildfire-induced PM$_{2.5}$ exposure

Wildfire smoke from Borgschulte et al. (2022)

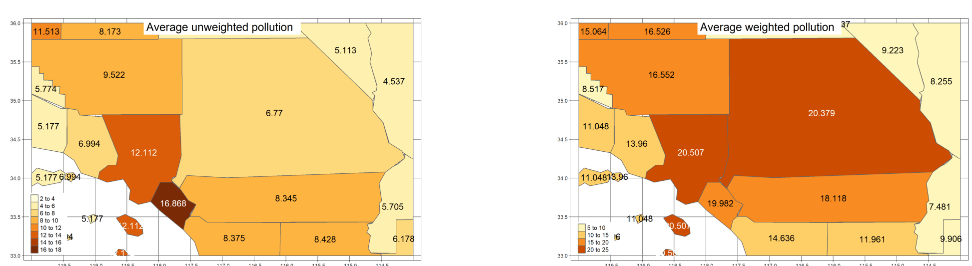
Notes: Densities weighted by county population

Figure 2.B.9: Illustrative example of population weighting within counties.



(a) Outcome to weight (X)

(b) Weights (W)

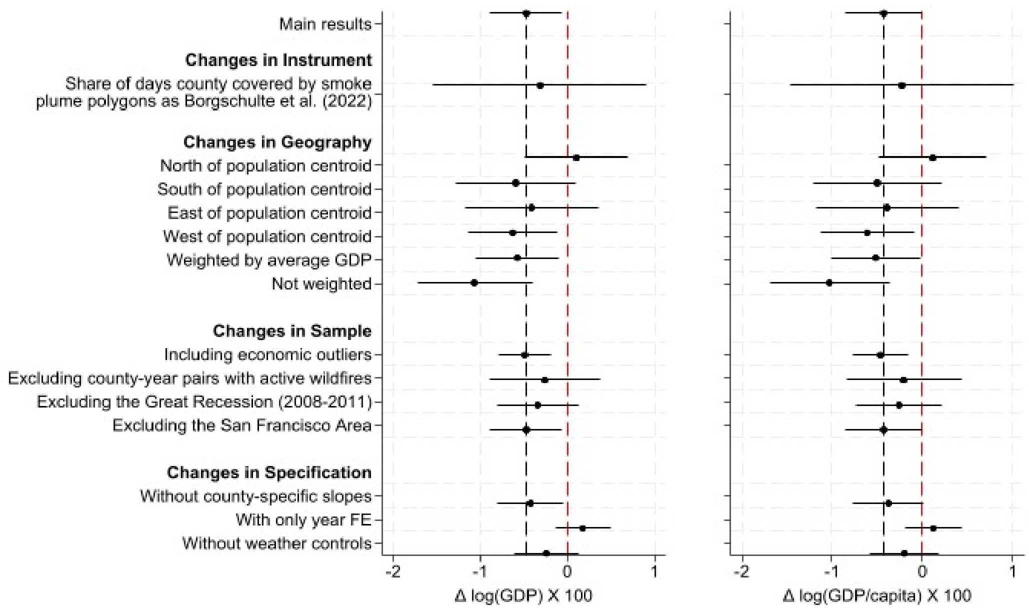


(c) Average with no population weights ($\bar{X}$)

(d) Average with population weights ($\bar{XW}$)

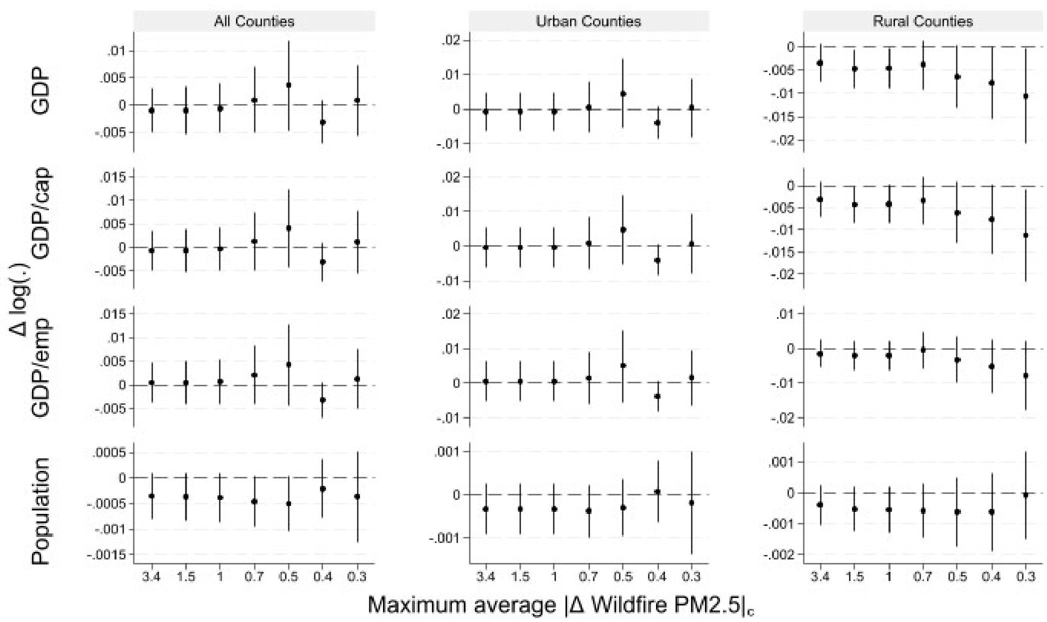
Notes: As shown in the figures, this is especially relevant in areas such as Las Vegas where population, emissions, and air pollution are spatially clustered.

Figure 2.B.10: Robustness tests and alternative specifications — Rural counties



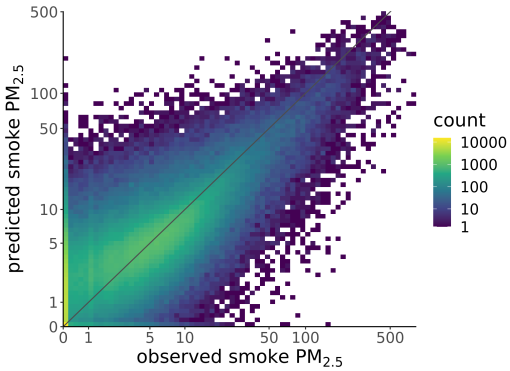
Notes: The Figure represents the effect of an increase in 1μg of $PM_{2.5}$ on GDP and GDP per capita (β_1) in rural counties for various regressions. Coefficients are multiplied by 100 so they can be interpreted as percentage points. The main results (as in Table 2.5) are compared with results with various changes in Instruments, Geography, Sample or Specification (as in Table 2.8. The comparisons show that all changes but one result in coefficients which are not statistically different from the main specification. The only one with significantly different results is the specification where only year FE are included, which fails to control for unobserved time-specific regional shocks such as state legislation, differentiated impacts from the 2008 financial crisis, or the number and intensity of wildfires in that stat-year pair. Although not significantly different, not including weather controls reduces the precision and creates some bias in the estimate. The two first instruments (from Thermal Inversions) are not included as the instrument was not strong ($F < 1$). All other results have an Kleinbergen-Paap F statistic above 190.

Figure 2.B.11: Robustness to different thresholds of maximum instrument variance

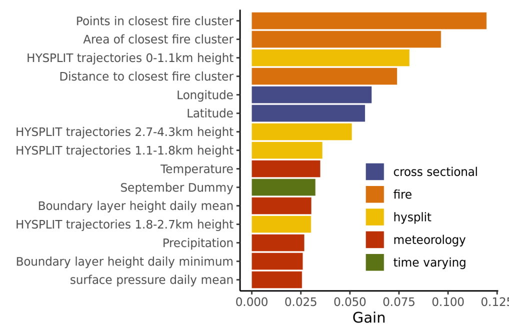


Notes: This figure shows the effect of an increase in 1μg of $PM_{2.5}$ on county-level GDP and GDP per capita (β_1) with various threshold of maximum average changes in the instrument ($|\Delta WildfirePM_{2.5}|_c$), for various outcomes (GDP, GDP per capita, GDP per employee, and population) and samples (all counties, Urban counties and Rural counties). The results reported in the paper (as explained and motivated in Section 2.3), are those with a maximum value of 1.5 (and thus represent 99.9% of the total population). Limits of 3.4, 1, 0.7, 0.5, 0.4 and 0.3 include 100, 99.5, 97.9, 95.3, 91.9, and 83.8 of the population for the whole sample, respectively. The results show that the main results from urban and rural counties are robust to a range of values of this threshold. In the case of rural counties they become stronger as the sample is restricted to regions with less changes in the exposure to wildfire smoke, although not explored in depth in this paper, this might signal a degree of "adaptive capacity" on areas that are used to receive pollution shocks.

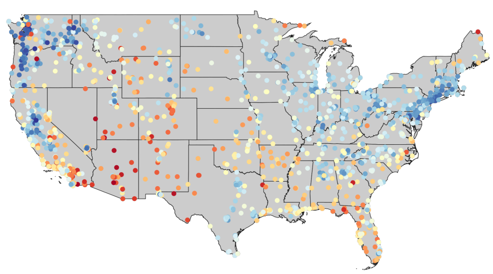
Figure 2.B.12: Diagnostic plots for the model that predicts wildfire-induced $PM_{2.5}$ concentrations.



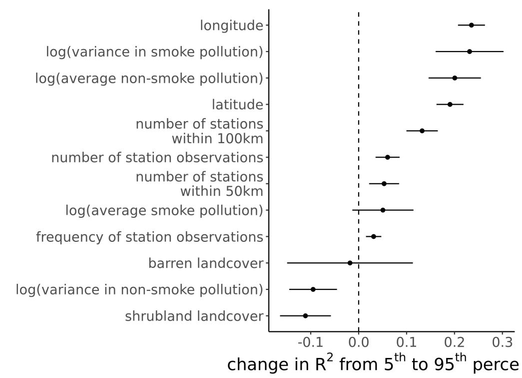
(a) Predicted and observed smoke $PM_{2.5}$



(b) Feature importance



(c) Out-of-sample R^2 by monitor



(d) Predictors of model performance

Notes: These images are constructed by drawing from the code and presentation of Childs et al. (2022) to ease comparison with their 'Figure 2'. The following description closely follows theirs: **(a)** The colour represents the frequency of monitored smoke days within categories of observed (horizontal axis) and predicted (vertical axis) smoke $PM_{2.5}$ concentrations. Both axes employ a pseudolog transformation, and the colour scale is log-transformed. A black line marks the 45º line, indicating where the model's predictions perfectly align with actual observations. **(b)** The importance of the top 15 features in the final model is depicted on the vertical axis, quantified by gain on the horizontal axis, and differentiated by colour according to broad feature categories, including elevation and land cover cross-sections, fire variables, HYSPLIT, and meteorological data. Areosols are not included as the model used for this paper explicitly excludes them. **(c)** For each monitoring station with at least 50 observations (represented as points on the map), R^2 is computed for all smoke days with available smoke $PM_{2.5}$ data, based on predictions from the model where the station was not included in the sample. **(d)** To elucidate the differences in model performance across monitors as observed in (c), a cross-sectional linear model is used to correlate specific monitor performance (R^2) as shown in (c) with characteristics of the monitor/location. The predictive influence of each characteristic is determined by the estimated change in monitor R^2 when that characteristic is altered from the 5th to the 95th percentile of its range. Displayed points indicate central estimates, while line segments illustrate 95% confidence intervals.

Chapter 3

The Effect of Low Emission Zones on Local Production: The case of German Cities

3.1 Introduction

The effects of air pollution on human health and wellbeing are a major global concern. Over 6.7 million deaths a year worldwide and 498.000 in Europe are attributed to air pollution alone (European Environmental Agency, 2019; Fuller et al., 2022). Air pollution is the second biggest environmental concern for Europeans, after climate change (European Commission, 2017), and is considered "the most important environmental risk to human health" by the European Environmental Agency (2020). Substantial and diverse policy initiatives to reduce urban air pollution have been implemented around the world, most of them being restrictions to the use of vehicles.[1] As one of these alternatives, Low Emission Zones (LEZ) — geographical areas with restricted access for highly-polluting cars — have been widely adopted across Europe with at least 404 LEZ either implemented or planned for the near future, and most of Europe's largest cities having applied one (Sadle Consultants Europe, 2022).

[1] Some examples include pedestrian areas, parking schemes, Low Emission Zones, congestion pricing schemes, limitations for certain vehicles at certain times of the day, lanes exclusive for massive transport vehicles, retrofitting of taxis and public buses or subsidies for electric or hybrid cars.

Their implementation has delivered on the promise of reducing air pollution (Gehrsitz, 2017) and thus might have positive impacts on health, worker productivity, and human capital formation (Graff Zivin and Neidell, 2018). Nevertheless, they create restrictions and cause substantial capital losses to owners and sectors that own highly-polluting vehicles when these are either prohibited or need to be retrofitted to attain the new emissions standards. In Germany, the opposition to this measure has been strong and widespread, with local retail businesses being particularly afraid of a reduction in sales from a lower demand for goods from vehicle-dependent clients. Another possible impact could come from supply and transport costs, as a majority of commercial vehicles were not compliant with LEZ standards (Wolff, 2014).

In this paper, I study the causal effect of the announcement and application of Low Emission Zones in the local production, productivity, employment, population, and economic structure of a large group of German cities. To estimate this, I use the staggered announcement and adoption of LEZ across German cities from 2006 to 2017 together with unique data on city-level GDP, population, employment, and Gross Value Added (GVA) for 5 sectors of the local economy. Germany is an optimal case study for this policy as the application of Low Emission Zones is triggered by local violations of the European Union air quality standards, and thus it is forced upon cities by state governments responsible for compliance with EU legislation. Moreover, German LEZ were standardised by federal law in 2006, so they apply the same fines and restrict the same classes of vehicles on each of their stages, which is not the case in most European countries.

I use a panel data of local macroeconomic variables of German districts (*Kreise*) from 1991 to 2017 together with a new dataset of the announcement dates of German Low Emission Zones. After carefully selecting the treatment and control cities, I apply the staggered differences-in-differences method from Callaway and Sant'Anna (2021) to estimate the effect of the announcement and application of German LEZ on their cities' aggregate economic outcomes, and the relative shares of GVA of the major economic sectors: Trade and Personal Services (TPS), Professional Services, 'Public Services, Education, Health, Entertainment, Art, and other services' (from now on 'Public and entertainment services' or PES), Construction, and Industry.

To avoid confounding the effects of the application of Low Emission Zones and two major and possibly confounding events: the 2008 financial crisis and the 2009 German scrappage program, I carefully divide the treated cities into those that announced the LEZ before the start of the crisis (2008) and those that announced it after its end (2010) (Harari, 2014; Rinne and Zimmermann, 2012). For each of these two groups, I select the most comparable set of regions where the assumption of parallel trends would likely hold. The large metropolitan areas that form the first group use a control set of comparable not-yet-treated regions, while the cities from the second group are compared with a restricted sample of non-treated cities that have a similar demographic and economic structure but have not applied a LEZ. I further allow for a 60km buffer between control and treated cities to avoid spatial spillovers shown by Wolff (2014) and Sarmiento et al. (2021). As shown in upcoming tables and figures, relevant covariates are balanced in both pairs of treated and control cities, and outcomes exhibit parallel trends of more than 15 years before the announcement of their respective Low Emission Zones.

It is important to note that the treated cities of these two groups differed on various important characteristics. In general, cities of Group 2 have on average lower aggregate population, GDP per capita, employment rate, and population density than those of Group 1. Differences in the timing of the policy could also be relevant, with cities from Group 1 being pioneers in announcing and applying their LEZ before the 2008 financial crisis while cities from Group 2 announced their LEZ after the start of the financial crisis and thus their application was in a context of less aggregate economic growth.

The results suggest that the announcement and application of Low Emission Zones had a large and heterogeneous effect on GDP. First, the announcement of Low Emission Zones in Group 1 had temporarily positive economic impacts before the 2008 financial crisis, and the application of the policy's restrictions. On the other hand, its application after the financial crisis (2011) in cities from Group 2 had negative effects on overall GDP, GDP per capita, and employment.

More specifically, the cities from Group 1 experienced and average increase in their GDP of 2.45% with respect to their control, with most of this increase being attributable to increases in productivity (GDP per employee) and focused on those cities where the impact is visible for at least two years before the start of the 2008 financial crisis. With regards to impacts by sector, the results suggest that the growth was evenly distributed.

A very different picture emerges for treated cities in Group 2. These cities, which differed in characteristics, timing of application, and length of the announcement period, the application of the restrictions from LEZ reduced GDP by 4.1% on average , roughly equivalent to 1370€ per person per year and a total of ≈6.8€ billion per year for this subset of cities. The results suggest that the driving factor of this negative effect is a large reduction in GDP per capita driven by reductions in employment, and not changes in population. The results also signal for a slight decrease in productivity, albeit not statistically significant.

With regard to the changes in the composition of the local economy, the implementation of LEZs in Group 2 cities promoted a significant decrease in the share of GVA from the industrial sector, coupled with an increase in the 'Public and entertainment services' sector. The Trade and Personal Services sector, which includes commerce and local services, such as restaurants and hotels, exhibited positive effects, but these were only statistically significant when focusing on treated and control cities from historic West Germany.

This pattern of economic shifts aligns with what is expected from this type of policy. The benefits of reduced air pollution in the city centre, such as increased productivity and sales, are to affect the economic activity located there. Furthermore, from the supply side, the costs of upgrading old commercial vehicles might be higher for the local industry. The relative increase in the 'Public and entertainment services' sector is probably attributed to the inherent resilience of Public Services to the overall decrease in GDP, and the potential of the entertainment sector to benefit from the policy in a similar manner than the trade and personal services sectors due to its similar location and demand. The changes in the economic structure described above can have long-run effects on economic development, employment, and income as they could increase the relative production of non-tradable over tradable goods and services.

Overall, this paper contributes to the literature on the economic effects of Low Emission Zones with the estimation of their impacts in previously unstudied outcomes: aggregate local production, productivity, and economic structure. Furthermore, it contributes to the growing literature on the effect of Low Emission Zones on economic outcomes such as the costs of substitution of old vehicles (Wolff, 2014) and its effect on local commerce and trade, a contentious topic central to the opposition to LEZ. While previous literature has found neutral effects on consumer revenue in retail for Madrid LEZ (Galdon-Sanchez et al., 2021), I complement these findings by studying the effects on a sector level for a large number of cities. Finally, this paper contributes to the literature on the macroeconomic effects of negative capital shocks in an open economy (Alvarez-Cuadrado, 2008) by providing a case study of local economies and decomposing the effect on GDP into relevant macroeconomic indicators.

In summary, although Low Emission Zones have been shown to reduce air pollution (Wolff, 2014) and improved outcomes in health (Gehrsitz, 2017), the results presented here show that they can have important consequences in the local economy. For some regions, the results indicate that LEZ might change the structure of the economy away from industry and towards local commerce and personal services and create large costs in the form of reductions in GDP per capita.

The remainder of this paper is structured as follows: Section 3.2 describes the institutional background, situates this work in the broader literature and describes the plausible effects of LEZ on the local economy. Section 3.3 details the data collection and analysis strategy. Section 3.4 describes and interprets the results. Section 3.5 concludes.

3.2 Institutional Background and Plausible Effects of LEZ

3.2.1 German Low Emission Zones

Low Emission Zones aim to improve local air quality, and their design rely on two main facts: First, a significant proportion of cities' ambient air pollution comes from traffic (35-55% of PM_{10} particles in average for Europe according to Viana et al., 2008)[2], an especially harmful source to humans given they are emitted close to where we breathe. Second, old vehicles can have emissions orders of magnitude larger than newer ones.[3] By restricting the use of the most polluting vehicles, LEZ are designed to reduce air pollution emissions from an especially dangerous source while affecting the minimum number of vehicles.

The large-scale application of LEZ around Europe has been heavily shaped by regulations of the European Union on air pollution levels[4], with regional and national governments being either threatened by fines or strongly incentivised to pursue decisive policy action if found in non-attainment[5]. As a result of this European legislation, infringement procedures have been opened against 16 Member States[6] and the EU Court of Justice has already handed down judgements in Bulgaria and Poland (European Commission, 2018). For comparison, in the US, regions are categorised into "attainment" and "non-attainment" areas based on their compliance with the National Ambient Air

[2]This is also the case for German cities. In Berlin, for example, traffic produces 38% and 23% of pollution gathered in close-to-traffic and background stations, respectively (Lenschow et al., 2001).

[3]An old petrol car with a Euro 1 emission standard generates 10 times more NO emissions on average than a newer vehicle with a standard of Euro 4. For diesel cars is 2.8 times, and for light-goods diesel vehicles, it is around 2 (Rhys-Tyler et al., 2011).

[4]These are a complex series of maximum "limit values" which are defined by pollutant and compared with a yearly, hourly, or 35-day average of multiple monitoring stations inside each region. Gehrsitz (2017) provides further detail on the limits and their temporal evolution.

[5]Directive 2008/50/EC required a higher level of reporting and action and implemented the new (more restrictive) limits with a "margin of tolerance" (which would decrease yearly until disappearing at the attainment date) for which if a region was above the limit value plus the margin of tolerance, it had to report and apply a series of action plans to reduce air pollution to the limit value by the attainment date (EC, 2020). As in the web page of the city of Hanover "The air pollution control action plan for Hanover was adopted (...) due to the level of the measured values for nitrogen dioxide (NO_2) and particulate matter (PM_{10}) at the Göttinger Straße traffic station, the threshold (limit value + tolerance margin) that requires the preparation of an air pollution control plan was exceeded." (Niedersachsen Government, 2019).

[6]Belgium, Bulgaria, the Czech Republic, Germany, Greece, Spain, France, Hungary, Italy, Latvia, Portugal, Poland, Romania, Sweden, Slovakia and Slovenia.

Quality Standards (NAAQS), and The EPA requires states to implement plans to achieve compliance.

From 2005 to 2007, two-thirds of German cities with a population higher than 100.000 were violating European limits. Similar to the EPA requirements in the US, cities that were violating these limits were forced to develop Clean Action Plans (*Luftreinhaltepläne* in German), defining a set of measures to attain compliance with EU standards. LEZs in Germany were applied as the main measure of most of these plans in large cities(Gehrsitz, 2017)[7]. Between 2008 and 2015, 46 LEZ were implemented, including most German cities with more than 10.000 inhabitants. The support and standardisation from the federal government was a key factor to this: all vehicles are required to have a unified "emissions windshield sticker" to access, with the colour of the sicker signalling the vehicle's emission standard. Stickers, in increasing exhaust emissions, go from green to yellow to red. The strictest LEZ only allow cars with green stickers, and the most polluting cars have no sticker and are forbidden from all LEZ.[8] The restrictions of LEZ apply 24 hours, every day. No German lez has been withdrawn and, with time, German LEZ have become increasingly restrictive. From March 2018, all German LEZ but one only allow vehicles with green stickers (see Figure 3.A.1 in the Appendix). Entering a LEZ with a banned vehicle is fined with 80€, is considered a serious misdemeanour, and implies a 1 demerit point in the central traffic registry (Gehrsitz, 2017)[9]. The largest German LEZ was implemented in the Ruhr area, covering up to 13 cities with a population of over 5 million people.

Here are some illustrative figures on the level of disruption the application of a LEZ can represent: First, LEZs almost always include the city centre, where most of the commercial and cultural activities take place and include about 44.5% of the city

[7]The individual plans tend to be similar and to include other small policies such as optimising traffic lights to increase trafic fluidity, reduce pollution emissions from construction, change for greener public buses, reduce wood burning, and do public awareness campaigns

[8]Specific exceptions apply to special vehicles such as construction vehicles (mobile engines and equipment and working machinery), tractors, ambulances, and vintage cars, among others. (Federal Ministry of Justice, 2006).

[9]Drivers with 8 points are banned from driving.

population, making them hard to avoid Gehrsitz (2017)[10]. Second, it is important to consider the proportion of vehicles banned. In 2008, when 7 LEZ were already introduced in major German cities including Berlin, Stuttgart, and Köln (and had been announced for years), 1 of every 4 commercial vehicles in circulation (and 4% of private vehicles) were completely banned from entering these major cities. In 2010, more than half (62.1%) of commercial (and 13% of private) vehicles in Germany were completely banned from entering the Berlin and Hannover LEZ, and plans had already gone through to ban them on an additional 25 large cities in the next 3 years (the number of announced LEZ grew to more than 33 cities by 2013)[11]. This becomes even more relevant if we consider that the value of commercial vehicles' is to be more dependent on access to the city as they do not have public transport alternatives. In summary, a large share of vehicles, especially commercial ones, were being banned an increasing number of major German cities. But, what macroeconomic consequences can we expect from such a policy?

3.2.2 Plausible Effects of LEZ

Low Emission Zones are a restriction to the use of highly polluting vehicles in a delimited area of the city. Old vehicles are prohibited, some vehicles have to be retrofitted to attain the new emissions standards, and all vehicles have to be labelled by their level of emissions, creating sizeable costs to some of their owners.

The implementation of LEZ can be especially damaging to individuals and businesses that work (or depend) on the transport of products and materials. Empirical evidence shows that environmental regulations can lead to adverse effects on trade, employment, plant location, and productivity in the short run, in particular in the subset of sectors that bear a higher load of the regulatory stringency (Dechezleprêtre and Sato, 2017). In this case, businesses that work or depend on the transport of commercial goods by large and highly polluting vehicles. These costs can end up reducing savings and investment in specific sectors, having an impact on their medium- or long-run economic growth and

[10] The boundaries of various LEZ relative to the boundaries of the regions studied can be seen in Figure 3.A.12.

[11] The source of these figures is vehicle registration data used by (Wolff, 2014) and my own calculation from his figures.

changing the structure of the economy.

Additionally, some voices from the retail sector have criticised the policy for the potential effects it might have on their businesses. Local commerce and personal services could be negatively affected if a proportion of their clients is excluded from entering a city's commercial areas (such as the historic centre). In an online survey from 2009, over 91% of Germans rejected LEZ as being "too bureaucratic and likely having little effect" while store owners complained that LEZs lead to declining sales (Wolff and Perry, 2010). Furthermore, the German pro-business "Institute for Retail Research" estimated a 7% decrease in customers for stores located within a city's centre after the introduction of a LEZ (Lindstaedt, 2009). An example is the city of Freiburg, often visited by neighbouring French and Swiss tourists, being estimated to lose close to 100M€ per year in revenue by their local retail association (Badische Zeitung, 2009). The shocks mentioned above can also affect the demand for real estate (housing and retail) within or around commercial areas. Most of the research looking at the impact of traffic regulations on real state prices find negative effects both in housing and retail (S. Agarwal et al., 2015; Percoco, 2014).

As mentioned above, the effects on freight transport are expected to be negative, although unequal, for the different industries dependent on it, based on their specific costs and capability to bear them. These costs can have multiple forms such as the retrofitting of old vehicles, the cost of new ones, or the reduction in the residual value of vehicles that become non-compliant with the new LEZ.[12] The forced upgrade, replacement, or even disposal of their fleet can have ripple effects on other areas of the affected firms and industries. These costs might increase as more LEZ are applied and polluting vehicles are excluded from more and more cities, reducing the vehicles resale value. It is hard to hypothesise on the magnitude of these effects, but with the figures mentioned above of 1 of every 4 commercial vehicles being banned in 7 major German cities in 2008 and more than half of the German fleet in 2010 being

[12] As noted by Wolff (2014), these losses can be substantial as "conversion to the next higher sticker costs 800–2,500 US Dollar (USD) for passenger cars and 7,000-22,000 USD for larger vehicles and trucks, although conversion is technologically infeasible for some vehicles."

banned from entering most (25) major German cities by 2013 can help the reader to consider it. On the other hand, those firms that had already changed their fleet before the policy can benefit from a higher degree of industry consolidation or a reduction of congestion. On this subject, the study from Browne et al. (2005) of London's LEZ explains that its effect could be strongly dependent on the restrictions imposed and the rate of vehicle replacement of companies. Their study concludes that companies with specialised vehicles, small companies, and self-employed owner-drivers end up bearing most of the negative consequences. Finally, and in line of the hypothesis of sector consolidation, Dablanc and Montenon (2015) complement these findings by saying that the application of LEZ reduces the number of firms that perform deliveries by expelling less profitable and smaller firms from the market.

From these costs, a narrative of "Jobs versus the Environment" (Morgenstern et al., 2002) can emerge in which regulators have to balance both objectives and make compromises. Before concluding that this is the case, it is fitting to first look at the positive economic effects that LEZ might have though their reduction of air pollution.

This is based on recent research by Wolff (2014), Gehrsitz (2017), Pestel and Wozny (2021), and Margaryan (2021) showing the significant effect of German LEZ on the reduction of pollution and health costs. Specifically, the application of German LEZ reduced average pollution (from -0.67 to -1.3μg/m^3) (Gehrsitz, 2017) and cardiovascular and respiratory diseases in nearby hospitals (Pestel and Wozny, 2021).

Additionally, recent studies suggest that air pollution reduces aggregate economic output though the effect it has on humans. After pollution particles are inhaled, they can enter the lungs and pass to the bloodstream, finally affecting multiple organs such as the heart and the brain (Calderón-Garcidueñas et al., 2014; Du et al., 2016; Ranft et al., 2009). Dechezleprêtre, Rivers and Stadler (2019) show how air pollution causes economy-wide reductions in productivity and economic activity in Europe. They conclude that a 1 μg/m^3 ($\approx$ 10%) increase in $PM_{2.5}$'s yearly average concentration causes a 0.8% decrease in GDP and, strikingly, that air pollution abatement costs would be two orders of magnitude smaller than the associated economy-wide increases in aggregate production. The negative impact of air pollution in aggregate production has also been found in other regions and economies such as the US (Avila-Uribe, 2023), China (Fu et al., 2021) and India (Behrer et al., 2023). Using the estimates of literature cited above of the effect of German LEZ in air pollution and the effect of air pollution on aggregate GDP in Europe, back of the envelope calculations suggest that German LEZ could foster an increase of city GDP between 0.2 and 1.6%.

Related to these macroeconomic effects, air pollution has also been found to decrease sales (Leroutier and Ollivier, 2022), increase absenteeism (Aragón et al., 2017; Hanna and Oliva, 2015; Holub et al., 2006; Ransom and Pope, 1992), reduce the productivity of physical and intellectual work (Adhvaryu et al., 2022; T. Chang et al., 2016; Graff-Zivin and Neidell, 2012) and reduce cognitive performance (Ebenstein et al., 2016; Zhang et al., 2018). Finally, the reduction of air pollution levels from the LEZ is expected to be larger in the city centre (Pestel and Wozny, 2021; Wolff, 2014) and thus to especially benefit the economic activity and sectors located there. Additionally, a reduction in air pollution is expected to have a larger positive effect on populations that are more vulnerable to air pollution such as young children, the elderly, and persons with certain underlying diseases (Makri and Stilianakis, 2008).

In summary, Low Emission Zones can have both negative and positive effects on local aggregate production though various channels and have the potential to change the structure of the local economy away from sectors relatively penalised and towards sectors relatively benefited by it. To shed light on the resulting effects of these two plausible channels, this paper looks at the effect of LEZ in local aggregate production (GDP) — decomposed into productivity, average employment, and population — and the structure of the local economy, measured as changes in the relative shares of different sectors over total GVA.

3.3 Data Collection and Analysis Strategy

To explore the causal effect of Low Emission Zones on the various outcomes mentioned above I use a staggered differences-in-differences (DiD) procedure. As with any DiD estimation procedure, this will consist in comparing the outcome paths of a set of treated and control cities before and after treatment. In the next paragraphs, I explain the sources, characteristics, and context of the data, and how I select the treated and control cities to satisfy the necessary assumptions to infer causality from my results.

The initial sample includes all German districts (*Kreise*, also equivalent to NUTS3 regions) from 1991 to 2017. Data on outcomes and controls was gathered from the European ARDECO database, which aggregates data from Eurostat, and National sources and provides yearly statistics on GDP, GVA per sector[13], employment, and population. Although the spatial boundaries of German districts are usually a good representation of a city extent (from large cities such as Berlin or Hamburg to very small cities of less than 50.000 inhabitants such as Amberg), they do not represent Functional Urban Areas or travel-to-work areas. Furthermore, a small number of LEZ have been implemented in small rural towns where "rural districts" (*Landkreise*) do not follow their town boundaries. An example of both representative and unrepresentative boundaries is presented in Figure 3.A.2 in the Appendix. All LEZ applied in small rural towns without a representative district are excluded from the analysis. An illustration of the boundaries of all German districts can be found in Figure 3.A.12 in the Appendix.

Additionally, a detailed calendar of the announcement and application of LEZ in Germany was constructed from two sources: First, the implementation of LEZ in Germany is well documented by the German Environment Agency (UBA, *Umweltbundesamt* in German) with dates for the application of each "stage" of each LEZ. Second, the announcement dates of each LEZ are alas not documented on a structured database and were searched individually on official documents such as local news and each city's "Environmental Plan" as published by their respective local governments. This is crucial to assign the announcement period as the start of the treatment. Figure 3.A.1 in the

[13] GVA per sector is divided into 6 aggregate sectors of economic activity according to the NACE Rev.2 codes, in parenthesis: **(A)**: Agriculture, forestry and fishing, **(B-E)**: Industry, **(F)**: Construction, **(G-J)**: Trade and Personal Services or "Wholesale and retail trade; transport; accommodation and food service activities; information and communication", **(K-N)**: Financial; real estate; professional, scientific; technical; administrative and support service activities, and **(O-U)**: Public administration and defence; compulsory social security; education; human health and social work activities; arts, entertainment, repair of household goods and other services.

Appendix summarises the treatment status of all LEZ in Germany included in the analysis from 2006 to 2017.

3.3.1 Identifying Requirements and Sample Selection

Various requirements have to be met to interpret the estimates from the differences-in-differences specification as causal:

First, the application of the policy has to be exogenous to the potential outcome paths of treated and control units, conditional on covariates. This would imply that treatment and control units would exhibit parallel outcome trajectories for all periods in the absence of treatment. Although this is an untestable assumption for any situation, two key points make it plausible in the case of German Low Emission Zones. First, as explained above, the policy was exogenously forced upon cities by state governments or court rulings based on of EU air quality legislation (Pestel and Wozny, 2021). Second, having parallel trends of the outcomes before the policy is implemented for a sufficient number of years would increase the confidence in this assumption. The data allows for visual inspection of parallel trends for more than 15 years before the policy, a period larger than most empirical applications. Finally, and to further increase confidence in this crucial assumption, I carefully choose a control pool that ensures the balance of key economic and demographic variables between treatment and control cities.

Second, the policy should not have any unaccounted anticipation effects. This can be an issue given LEZ are usually publicised before being enacted to incentivise the public to upgrade their vehicles before their introduction. This preemptive adaptation has been documented by Wolff (2014), which shows how the German city of Regensburg had a very strong increase in green-labelled cars after their LEZ announcement but before its application. This is accounted for by setting the “announcement” year as the start of treatment as cities differ in the number of years they give the local population to prepare for the policy. Although ignoring this point can be a potential source of bias, previous research on LEZ tends not to control for it, most likely due to the previous lack of data on LEZ announcement dates.[14]

Third, there must be no interference between units. In other words, control cities should not be influenced by the treatment of nearby treated cities (or vice versa). This can happen through the spillover of air pollution reductions through wind currents or the upgrade of the vehicle fleet around neighbouring treated cities. With respect to air pollution, Sarmiento et al. (2021) find air pollution spillover effects in German LEZ of up to 25km.[15] With regards to upgrades in the vehicle fleet, Wolff (2014) shows that the application of a LEZ correlates with a change towards cleaner vehicles in the cities affected and in their surrounding cities. For these two reasons, and taking the most conservative estimate by Wolff (2014), only cities that are *at least* 60km away from any LEZ implemented in the studied period are included as controls.[16]

Fourth, there should be no significant events that differently affect treated (or control) units in the post-intervention period as their effect would be wrongly added (or subtracted) from the real causal effect of the treatment. Two main events of this sort could pose a threat to this analysis: the shock of the great financial crisis, which lasted between 2008 and 2010 for Germany (Harari, 2014; Rinne and Zimmermann, 2012), and the 2009 German scrappage ("cash for clunkers") program.

Relative to normal circumstances, it would be harder to assume that the local growth and the economic structure of treated and control cities were similarly affected by the financial crisis. Most importantly, the 2009 German scrappage program — a policy specifically thought to stimulate the economy after the financial crisis — poses a potential source of bias for cities that had a LEZ either implemented or announced before the end of this program. As the largest policy of its type in the world, it offered a lump-sum subsidy of 2.500€ for buying a new car when the buyer scrapped their

[14] Wolff (2014), Gehrsitz (2017), Pestel and Wozny (2021), Morfeld et al. (2014), and Browne et al. (2005) are some examples.

[15] These are positive (increases in pollution) for the first 500m in the case of O_3, PM_{10} and NO_2, and negative from 500m up to 10-25km for CO, PM_{10} and NO_2

[16] This is taken as the distance from the centre of a treated city to the closest point of a "control" NUTS 3 zone (and thus it is a conservative measure).

old one. From the 14^{th} of January to the 2^{nd} of September 2009, two million car sales were subsidised, implying the substitution of two million cars older than nine years old for new cars with better emissions standards (at least "Euro 4" compliant and thus worthy of a "green" sticker) (Kaul et al., 2012). The population of cities that had a LEZ implemented or announced during this period had stronger incentives to change their old cars for new ones, given they were (or would) not be able to use them inside the LEZ. This implies that these cities would potentially receive larger sums of fiscal stimulus from the central government relative to control cities.

To satisfy this last and crucial condition, I carefully divide the *treated* units into 3 groups such that the financial crisis and the scrappage program do not influence the final results: First, those districts that announced their LEZ between the start and the end of the financial crisis (2008-2010), which includes the period when the scrappage program was active, are discarded from the sample. Even if it is possible to check for parallel trends before their announcement, it would be hard to know if the financial crisis, and specially the scrappage program, affected them differently than control districts, and thus the estimates from these cities would not reliable. Second, I create a group of *early treated* units that announced their LEZ *before* 2008 and discard all outcomes after the start of the crisis, as those outcomes could be affected both by the financial crisis and the scrappage program (this will be referenced throughout the paper as Group 1). Third, I create a group of *late-treated* units that announced their LEZ *after* the end of the financial crisis (2010) (Group 2). For this group, it is possible to test the pre-treatment parallel trends before, during, and after the financial crisis and the scrappage program, giving confidence that it responded on a very similar matter to these shocks and thus giving validity to the empirical strategy.

This gives two final groups of treated units: *Group 1* districts studied until 2008 and where only the effects of LEZ announcement are visible, and *Group 2* districts where it is possible to observe the effects of the application of LEZ.[17] The Control

[17] As shown in Figure 3.A.1, the treated cities of Group 2 had, in general, much shorter periods of announcement. Almost all of them below a year. Thus, their results almost completely represent the effects of the application stage.

cities for each group are selected with the objective to get the most comparable stable set of cities that are not treated on those years. As Groups 1 and 2 differ in their characteristics and treatment timings their control groups also differ to improve the balance on relevant economic determinants and the validity of comparison groups. We can expect different results from these two groups as they show different stages of the policy (announcement and implementation), vary on their cities characteristics, and face different macroeconomic conditions and vehicle fleets when the effects are studied. To improve the balance on relevant economic determinants and the validity of comparison groups, treated cities from Group 1 are compared with not-yet-treated cities that apply a LEZ after the start of the financial crisis. Treated cities from Group 2 are compared with cities that share similar population density and GDP per capita but did not applied a LEZ before the end of the sample period (2018).

Tables 3.3.1 and 3.3.2 compare the pre-treatment levels of various relevant covariates between treatment and control regions for each group. Although DID does not rely on a balance in the levels of pre-treatment covariates, this increases the confidence that both groups have similar treated and control units and would behave similarly had the policy not been implemented. For Group 1, treatment and control are balanced for all relevant variables, such as GDP per capita, population density, and the share of the major sectors in the economy. Sample means are similar, and the extreme values of the treated units are closely mirrored by the controls. For Group 2, I select a suitable control set from not-treated districts: those that are bounded between the minimum and maximum GDP per capita and population density of treated cities. This improves the balance substantially, with only economically small differences remaining between them. I control for some of these pre-treatment differences in some of the regressions as described in the next sub-section. Finally, and to discard the presence of relevant pre-treatment trends in the outcomes or covariates of interest, Figure 3.A.5 in the Appendix shows the distribution of yearly values for the treated and control groups, with a boxplot overlying them.

To fully clarify the time, space, and treatment dimensions of Group 1 and 2, in the Appendix show, the staggered adoption of LEZ for the complete sample (3.A.1), a step-by-step flow chart of the data selection procedure (3.A.3), and the geographic location of treated and control units for Groups 1 and 2 (3.A.4).

Table 3.3.1: Summary Statistics for Group 1

	Treated					Not-yet-treated				
Variable	N	Mean	SD	Min	Max	N	Mean	SD	Min	Max
GDP per capita (€)	12	44797	14258	25687	76618	17	38121	19895	18863	95452
Log(Population)	12	13	1	12	15	17	13	0.7	12	14
GDP per employee (€)	12	71001	10624	57524	95883	17	65260	16083	48458	103224
Employment rate (%)	12	62	12	45	80	17	56	15	36	92
Population per km2	12	1634	1046	258	3803	17	1580	1097	201	4041
Share of GVA (%) from:										
Industry	12	22	7	13	34	17	19	9	9	38
Construction	12	4	1	3	6	17	5	2	3	8
Trade and personal services	12	22	5	13	31	17	21	4	12	27
Agriculture, forestry and fishing	12	0	0	0	0	17	0	0	0	1
Public sector and entertainment services	12	24	7	15	34	17	26	7	12	40
Financial, real state, professional, scientific and technical activities	12	28	4	21	34	17	29	7	20	47

Values for the year 2005.

Table 3.3.2: Summary Statistics for Group 2

	Treated					Not treated				
Variable	N	Mean	SD	Min	Max	N	Mean	SD	Min	Max
GDP per capita (€)	19	31534	9804	19611	66794	47	36853	10770	18154	64241
Log(Population)	19	12	0.5	12	13	47	12	0.8	10	14
GDP per employee (€)	19	63014	8517	50870	78829	47	61752	8054	45176	92686
Employment rate (%)	19	50	11	36	86	47	60	17	28	92
Population per km2	19	1213	914	201	3334	47	845	594	191	2668
Share of GVA (%) from:										
Industry	19	26	11	9	42	47	24	10	5	56
Construction	19	5	2	3	8	47	4	2	2	11
Trade and personal services	19	19	4	12	26	47	20	6	9	35
Agriculture, forestry and fishing	19	0	0	0	1	47	0	0	0	1
Public sector and entertainment services	19	24	7	13	40	47	27	8	13	50
Financial, real state, professional, scientific and technical activities	19	25	4	19	35	47	24	5	13	36

Values for the year 2005.

Never-treated units were restricted to have a 2005 GDP per capita between 7,000€ and 17,000€ and a population density above 180 per km2.

3.3.2 Regression Model

The objective of this paper is to explore the dynamic effects of the announcement and application of Low Emission Zones in the economic output and the structure of the economy of the cities that applied them. The binary nature of the treatment and the panel structure of the data motivates the use of difference-in-difference methods, and more specifically, the use of a classic dynamic two-way fixed effect (TWFE) or 'event-study' model as follows:

$$y_{it} = \alpha + \sum_{\substack{k=-15 \\ k \neq -1}}^{K} \beta_k LEZ_{ik} + \gamma_i + \delta_t + \varepsilon_{it} \tag{3.1}$$

where y_{it} indicates the outcome of interest for city i at time t, LEZ_{ik} indicates dummy variables of yearly leads and lags up to 15 years before the announcement of a Low Emission Zone. Each β_k for $k < -1$ gives a test of parallel pre-treatment trends, and each β_k for $k \geq 0$ represents the causal effect of the announcement or application of a LEZ relative to the year immediately before its announcement ($k = -1$). Finally, city fixed effects γ_i capture any time-invariant characteristics of cities while time fixed effects δ_t control for any trends that are common for all cities. The standard errors are calculated such that the error term ε_{it} is allowed to be heteroskedastic and correlated within metropolitan areas.[18]

Although this classic TWFE model has been previously used to estimate the causal effects of German LEZ (Pestel and Wozny, 2021; Wolff, 2014), recent literature has shown that this recovers the unbiased causal parameters and correct pretreatment trends only if the average treatment effect for each period since treatment is equal across time of treatment (J. Roth et al., 2022). There are various reasons why this is not expected to be the case. These include the fact that cities have different treatment calendars (i.e. take different time spans to go from announcement to the application of their various stages), and that that the stages are to become less restrictive with time, given the number of high-polluting cars in circulation gets reduced as the fleet gets naturally

[18] This accounts for the plausible correlation of exogenous shocks to y_{it} for cities inside the same metropolitan area.

upgraded over time. The reasons of such biases are described in depth by Sun and Abraham (2021) and Goodman-Bacon (2021) and are clearly summarised by J. Roth et al. (2022).

Based on this recent literature, I use the more recent procedure by Callaway and Sant'Anna (2021), which groups treated units by their announcement year g and estimates Equation (3.1) separately for each of them, creating estimates that change by period and year of announcement ($\beta_{k,g}$). This avoids comparing units that are treated at different times and gives unbiased average treatment effects even with heterogeneity of effects between cities of different treatment times. In the case that the parallel trends assumption holds only conditional on observable covariates (that is, a vector of time-invariant covariates measured before treatment $\mathbf{X_i'}$ which may have a time-varying impact on the outcome), it is possible to use doubly-robust estimators that control for them. In short, this allows to control for pre-treatment covariates either (a) parametrically by using the regression adjustment procedure proposed by Heckman et al. (1997) and Heckman et al. (1998) or (b) non-parametrically by constructing a group-specific propensity score to balance on covariates using Inverse Probability Weighting (IPW). The doubly-robust estimator is valid if either the regression adjustment model or the propensity score model is correctly specified (Sant'Anna and J. Zhao, 2020) and there is a strong overlap. This procedure is more extensively explained on J. Roth et al. (2022) and is similar to the strategy implemented by Wolff (2014), which uses pre-LEZ levels of PM2.5 to match units before doing its differences-in-difference analysis.

An additional challenge for this research is to construct correct statistical inference for the low number of units by group (12 regions in Group 1 and 19 in Group 2 (with 17 and 47 controls, respectively). To address this, I estimate bootstrap standard errors as the number of units is too small for the usual inference procedures, but not enough to be concerned by the issues raised by Canay et al. (2021).[19] Specifically, I use cluster wild bootstrap with 1000 iterations as recommended by the literature (Cameron et al., 2008; J. Roth et al., 2022). This involves resampling the data using entire clusters (rather

[19] As Canay et al. (2021) recommends, groups should not to be smaller than four units. For this reason, some of the results below join all cities treated between 2014 and 2017 in one unified group (n=7).

than individual observations) to mimic the dependence structure within clusters, and obtain empirical distributions for our estimators that take into account both within-cluster correlation and heteroskedasticity.

Finally, aggregate estimates are constructed from weighted averages of period-group effects $\beta_{k,g}$. These consist of the average treatment effect of the treated (ATT) β, or more specific results such as the average treatment effect 2 periods after the announcement $\beta_{k=2}$ or the average effect of the set of cities treated in 2011 $\beta_{g=2011}$. Further details are given by Callaway and Sant'Anna (2021) and J. Roth et al. (2022). With this method, I construct the ATT, the average dynamic treatment effects β_k, and the average treatment effect for each set of units treated on a given year (with sufficient units) β_g, all for both Group 1 and Group 2.

3.4 Results

The results are divided into 2 sections: The effect of LEZ on aggregate production, decomposing it on GDP per capita, GDP per employee, average employment, and population (3.4.1) and the effect on the structure of the economy, looking at the shares of individual sectors over total GVA (3.4.2). In each of them the results are given for both Group 1 (large cities where only the effects of the announcement are visible), and Group 2 (large and medium cities where the results mostly reflect the effects of its implementation) While cities in Group 1 announced the implementation of their LEZ in up to 4 years in advance, all cities of Group 2 but one gave less than 10 months' notice before restricting the use of LEZ-excluded vehicles. This makes it impossible to accurately estimate the effects of *announcement* in Group 2 with yearly GDP data, and directly compare them with those found in Group 1. Relatedly, the relative short period of preparation allowed between announcement and application in Group 2 cities might be plausible explanation of the negative effects described below.

Various robustness tests have been conducted, such as restricting the sample to cities from historic West Germany, excluding any period affected by the 2013 European floods, or using the specification from Sun and Abraham (2021), finding almost equivalent results for both groups in all of them (see Appendix Figure 3.A.11).

3.4.1 The Effect of LEZ on Aggregate Production

In this section I detail the effects of Low Emission Zones on local aggregate production (GDP). In order to better understand the drivers of changes in aggregate GDP, I decompose it into relevant economic components by the following identity:

$$GDP \equiv \underbrace{\overbrace{\frac{GDP}{Employees}}^{\text{Productivity}} * \overbrace{\frac{Employees}{Population}}^{\text{Avg. employment}}}_{GDP\ per\ capita} * Population$$

and study the effect of each component in separate analyses. The results of the preferred specifications for each outcome are shown in Table 3.4.3:

Table 3.4.3: Effects in Aggregate Production

	Group 1 *Announcement — Before 2008*		Group 2 *Application — From 2011*	
	ATT	ATT by ann. year β_g [min, max]	ATT	ATT by ann. year β_g [min, max]
GDP	2.45%** (0.91)	[0.47%, 2.94%**]	-4.09%** (1.56)	[-0.10%**, -7.02%]
GDP/population	1.78% (0.93)	[0.26%, 2.16%*]	-5.24%** (1.25)	[-0.32%**, -8.60%]
GDP/employees	2.42%* (0.98)	[0.19%, 2.98%**]	-0.97% (1.13)	[-0.07%, 1.79%]
Employees/pop	-0.65% (0.39)	[-0.81%, 0.03%]	-2.18%* (0.91)	[-0.66%**, -3.05%]
Population	0.14% (0.34)	[0.13%, 0.14%]	0.05% (0.48)	[-0.23%, 0.24%]

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

All values are expressed as % changes relative to 2005. Standard errors are constructed with wild bootstrap and clustered by metropolitan area. Cities with announcement years 2014-2017 are merged in one "announcement year" group g. In some specifications, parallel trends are improved with the inclusion of pre-treatment covariates, these are detailed in Table 3.A.1 in the Appendix. All estimates have at least 15 years of parallel trends before the announcement.

For Group 1, there was a significant increase in overall GDP (2.45%) and productivity (2.42%) following LEZ announcement. The increase in GDP per capita is not as large as the one in productivity and there is no significant effect on either average employment of the population or population size.

For Group 2, I find a negative and significant effect of LEZ application of a permanent 4.1% decrease in GDP with respect to 2005 values. This result is after controlling for pretreatment values of population (2005), and GDP (2000) as the unconditional DiD exhibited a small pre-trend, but is robust to not controlling for them (Figure 3.A.6c) shows the event-study results for both specifications). The strong negative effect on local GDP seems to be mainly driven by reductions in GDP per capita (−5.24%) due to a reduction in employment (−2.18%) and maybe productivity, but not a reduction in population. With regards to the heterogeneity between groups, this reduction in overall GDP is driven by a large negative effect on the later years of the set of cities that announced the LEZ in 2011 (Figure 3.A.6d), including the cities of Halle, Hagen, Heidenheim, Magdeburg, and various municipalities from Ruhr metropolitan area. There are 3 possible reasons for this: I) We can conclude from the positive effects of the announcement and the main post-treatment event-study plot (Figure 3.A.6c), that the most negative effects might be delayed in time and thus would be more visible in the set of cities with most post-treatment periods. II) This group includes the Ruhr area, where the LEZ adopted is both the largest in Germany (in km2) and also covers a relatively large share of the urban area in its cities (visible in Figure 3.A.12). These two characteristics make it harder to avoid than other LEZs (such as the one in Berlin), plausibly increasing its effects. III) Finally, the Ruhr area was already in industrial decline, and thus, the costs from the LEZ adoption might have exacerbated an already difficult economic situation. As discussed below, the results on the relative weight of the industrial sector in the local production (Figure 3.A.10c) indicate that the industrial sector was especially affected in this subset of cities.

All event study plots of the effects on GDP, GDP per employee and average employment rate for both Group 1 (announcement) and Group 2 units (announcement and implementation) can be seen in the Appendix (Figures 3.A.6, 3.A.7, and 3.A.8). These show the pre-treatment parallel trends and effects relative to the announcement year, with and without controlling for selected pre-treatment covariates.

3.4.2 The Effect of LEZ on the Structure of the Local Economy

The results on the composition of the local economy, that is changes in the relative share of production from the 5 major sectors (Industry, Construction, 'Trade and Personal Services', Professional Services, and finally the 'Public and entertainment services'), also strongly differed between the Group 1 and Group 2, as reported in Table 3.4.4:

Table 3.4.4: Effects in Sectorial Composition

	Group 1 *Announcement — Before 2008*		Group 2 *Application — From 2011*	
	ATT	ATT by ann. year β_g [min, max]	ATT	ATT by ann. year β_g [min, max]
GVA_{TPS}/GVA	0.21% (0.24)	[-0.30%, 0.87%**†]	0.51% (0.42)	[0.09%, 0.61%]
$GVA_{Industry}/GVA$	0.31% (0.49)	[-0.60%, 0.95%*†]	-1.48%* (0.60)	[-2.90%**, 0.36%]
$GVA_{Construction}/GVA$	-0.22% (0.19)	[-0.29%, 0.01%]	-0.21% (0.14)	[-0.15%, 0.11%]
$GVA_{Prof.\ Serv.}/GVA$	-0.93%† (0.65)	[-0.79%**†, 0.52%*†]	0.05% (0.45)	[-0.51%, 0.56%]
GVA_{PES}/GVA	-0.10% (0.26)	[-0.01%, 0.49%**†]	0.77%* (0.33)	[-0.08%, 1.50%**]

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

All values are expressed as % changes relative to 2005. Standard errors are constructed with wild bootstrap and clustered by metropolitan area. Cities with announcement years 2014-2017 are merged in one "announcement year" group g. In some specifications, parallel trends are improved with the inclusion of pre-treatment covariates, these are detailed in Table 3.A.1 in the Appendix. All estimates but those marked with † have at least 15 years of parallel trends before the announcement.

Regarding cities from Group 1, the positive aggregate economic effect seems to have been equally balanced in all sectors, with no economically or statistically significant relative increase of any of them. The estimates for Professional Services are not very reliable as they don't have the 15-year parallel trends that other sectors have, even after controlling for relevant covariates.[20]

With regards to cities from Group 2, in contrast, there were sectors that were more affected by the policy than others. The results show both an average decrease in the share of GVA coming from industry (-1.48% over a pre-treatment average of 24%) and an increase in the share from the 'Public and Entertainment Services' sector ($+0.77\%$ over a pre-treatment average of 27%). The estimates for Trade and Personal Services

[20] This is also the case for some subsets of cities that announced the LEZ on a specific year, as reported in the table.

(commerce, restaurants, hotels, and other local services) are positive and relevant, but statistically insignificant for the whole Group 2 sample. This uncertainty is reduced if we focus on a sample of only the west of Germany (see appendix Figure 3.A.10), where a positive effect in in Trade and Personal Services of around 1% is clear and statistically significant. All other sector-level effects are kept the same as in the full sample. Regarding other sectors, no economic or statistically significant effect emerges for the share of production from Construction or Professional Services neither for the whole country or in its western side.

When looking at the heterogeneity between cities with different announcement years, it is possible to see that the effects in Group 2 seem to be driven by the cities that announced the LEZ in 2011, including various cities inside the Ruhr area, as before. This goes in line with the literature of negative impacts on businesses that can be specially affected by this environmental policy and with the fact the 2011 group is the one who announced and applied it the soonest of this group and includes the Ruhr area, the largest LEZ in the country.

There are various mechanisms that would drive such changes in relative economic production. First, air pollution, and possibly traffic, is expected to have its largest reductions in the city centre, and thus relatively benefit the economic activity located there (the relevant literature is explained in depth in Subection 3.2.2). This can be though an increase the amenities and thus the demand of goods and services there, but it is also possible though a relative increase in the productivity of these sectors though larger air pollution reductions. Given that the local industry is usually located outside of the city centre, we would not expect it to benefit as much as Retail, Personal Services or Entertainment Services. Second, and from the supply side, the costs of upgrading old commercial vehicles might be higher for the local industry. This is in line with the results shown above. Finally, The relative increase in the “Public and Entertainment Services’ sector might be explained in two parts. On one hand, Public Services are not to be affected as much as market production from a policy like this, and with overall GDP decreasing it is expected that its relative weight would rise. On the other hand, the entertainment sector might have a positive effect similar to the 'Trade and Personal Services' sector as it produces similar services, and it is also usually located in the city centre. These changes in the relative weight of various sectors may lead to enduring impacts on economic growth, as they might boost the production of non-tradable goods and services over tradable ones.

3.5 Conclusions

This paper is the first to study the effect of Low Emission Zones on local production and the structure of the local economy. With a sample of 31 treated German cities over the period from 1991 to 2017, it exploits the staggered introduction of Low Emission Zones in a differences-in-differences specification. In order to apply this methodology to the case of German LEZ and avoid the influence of two major events (the 2008 financial crisis and the 2009 German scrappage program), I carefully define two experimental groups. One consisting of cities that announced the LEZ before the financial crisis where only the effects between announcement and implementation can be estimated (Group 1), and one of in which announced and applied their LEZ after 2011, where it is possible to see the effects after implementation (Group 2). These cities differed on various important characteristics, with the most relevant being the following: First, cities of Group 2 had, on average, a lower population, GDP per capita, employment rate, and population density. Second, Group 1 cities were pioneers in announcing and applying the LEZ, and the socioeconomic context of their results is before the 2008 financial crisis while treated cities from Group 2 announced their LEZ after the financial crisis and thus faced an economy with less aggregate growth. Finally, Group 2 cities a shorter period between announcement and implementation, making the necessary preparations harder.

The results suggest that the announcement and application of Low Emission Zones had significant and varied effects on GDP. Initially, the announcement of Low Emission Zones in Group 1 had positive economic impacts before the 2008 financial crisis. However, the implementation in 2011, post-crisis, in cities from Group 2 had adverse effects on overall GDP, GDP per capita, and employment.

Specifically, cities in Group 1 experienced an average GDP increase of 2.45% compared to their controls, primarily due to productivity gains (GDP per employee). This growth was notable on cities where the impact persisted for at least two years before the 2008 financial crisis. Sector-wise, the growth was evenly distributed but it is not possible to evaluate if it was permanent. For cities in Group 2, the application of Low Emission Zones permanently reduced local GDP by 4.1% on average, roughly equivalent to 1370€ per capita per year, with some cities having an even larger reduction of their local GDP. This reduction in aggregate production was mostly driven by reductions in GDP per capita and the share of the population employed, but not changes in the total population.

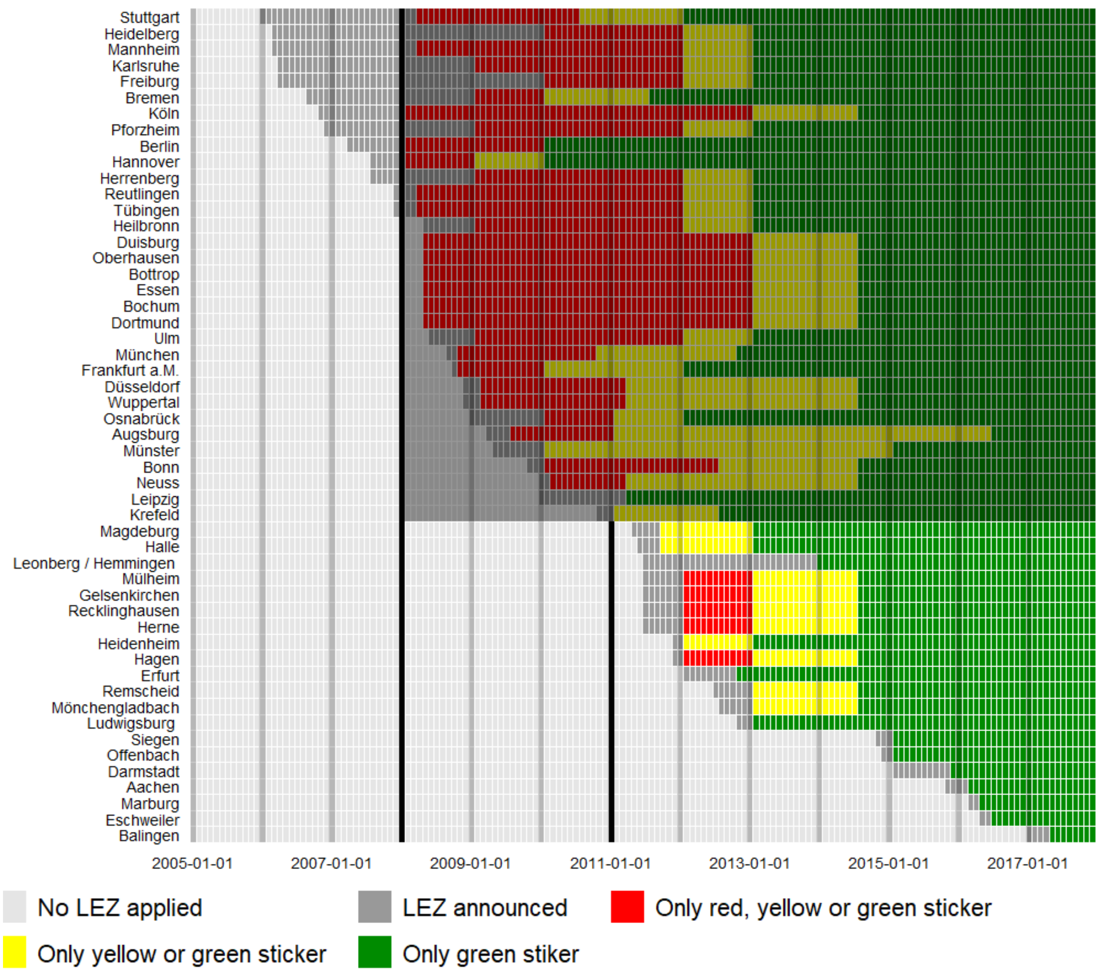
On the effect of LEZ from Group 2 on changes in the structure of the local economy, the results suggest that these were mainly driven by a selected group of cities (those that announced it in 2011). Low Emission Zones promoted a significant increase in the weight of the 'Trade and Personal Services' sector and the 'Public and Entertainment Services' sector, together with a significant decrease in the weight of Industry. This goes in line with expectations as the industry might not benefit as much as Personal Services, Retail, or entertainment from a reduction in air pollution in the city centre (and thus better amenities and probably higher productivity) and might be more affected by the costs of replacing the old (and mostly commercial) polluting vehicles now banned by the policy. These transformations in the economic structure could persistently affect economic growth, as they may promote an increase in the production of non-tradable versus tradable goods and services.

Future research will probably be able to overcome various data and context limitations of this work using the increasingly large number of regions in Europe and abroad that are applying similar policies. In particular, it might allow to test if the effects are primarily influenced by the country's macroeconomic situation, the length of the preparation period, or the city's size or income level. Additionally, the use of micro-level data, such as individual-level data on labour and productivity outcomes from businesses, together with their specific location would allow to reduce measurement error, allow to explore the heterogeneous effects of LEZ in the local economy (for example the distributional effects by income, neighbourhood, car usage, closeness to public transport or by sectors such as industry) and the potential causal mechanism of pollution reduction and improvement of health outcomes. All of this could help to explore the reasons behind the large heterogeneity of treatment effects observed.

Overall, the results of this paper have important implications for policymakers. Although the literature shows that Low Emission Zones reduce air pollution and hospital admissions, my findings indicate they can also significantly impact GDP, productivity, and employment. In some regions, LEZs may also shift the economic structure away from industry and towards local commerce and personal services.

3.A Appendix: Additional Tables and Figures

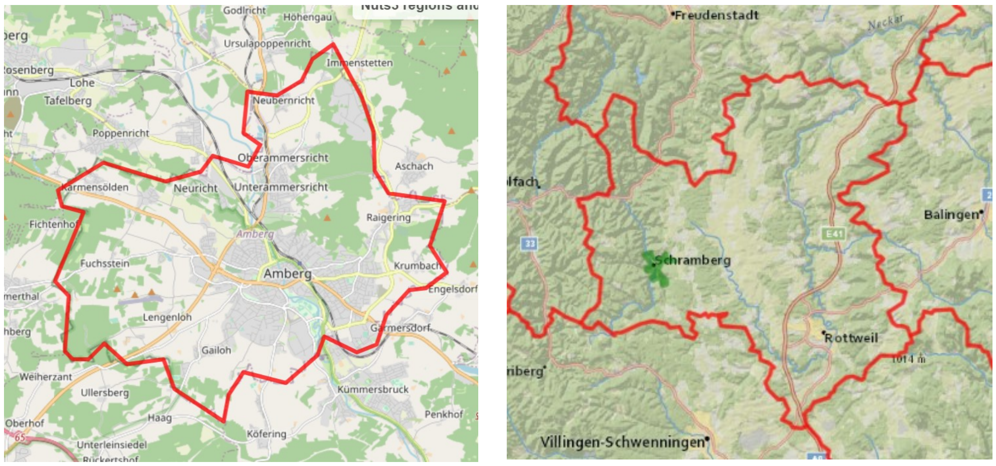
Figure 3.A.1: Announcement and Application of German LEZ Included in the Study



Notes: Colours mark the announcement date and category of environmental stickers allowed. The start and the end of the financial crisis are marked with two vertical lines. All outcomes in the shaded area are excluded from the analysis to avoid the financial crisis and the scrappage program to confound the causal estimates.
Sources: Umweltbundesamt and local 'Environmental plans'.

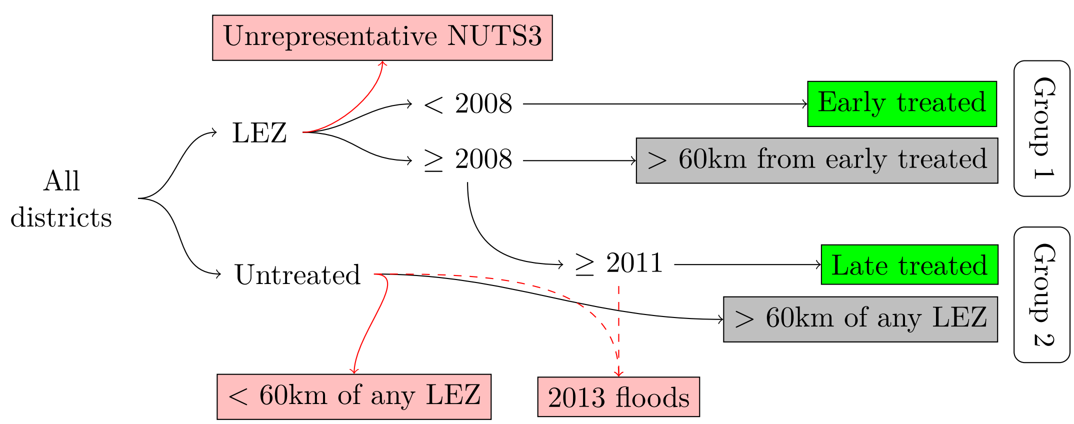
Figure 3.A.2: Examples of German Districts

(a) Well represented city extent (b) Bad measurement



Notes: Districts (NUTS3 regions) can be imperfect spatial proxies of small cities or towns. Here the city of Anberg is well represented by its regional unit while the town of Schramberg, which applied a LEZ (in green) is not well represented by its rural district and thus is excluded from the sample.

Figure 3.A.3: Diagram of the Construction of the Two Treatment and Control Groups

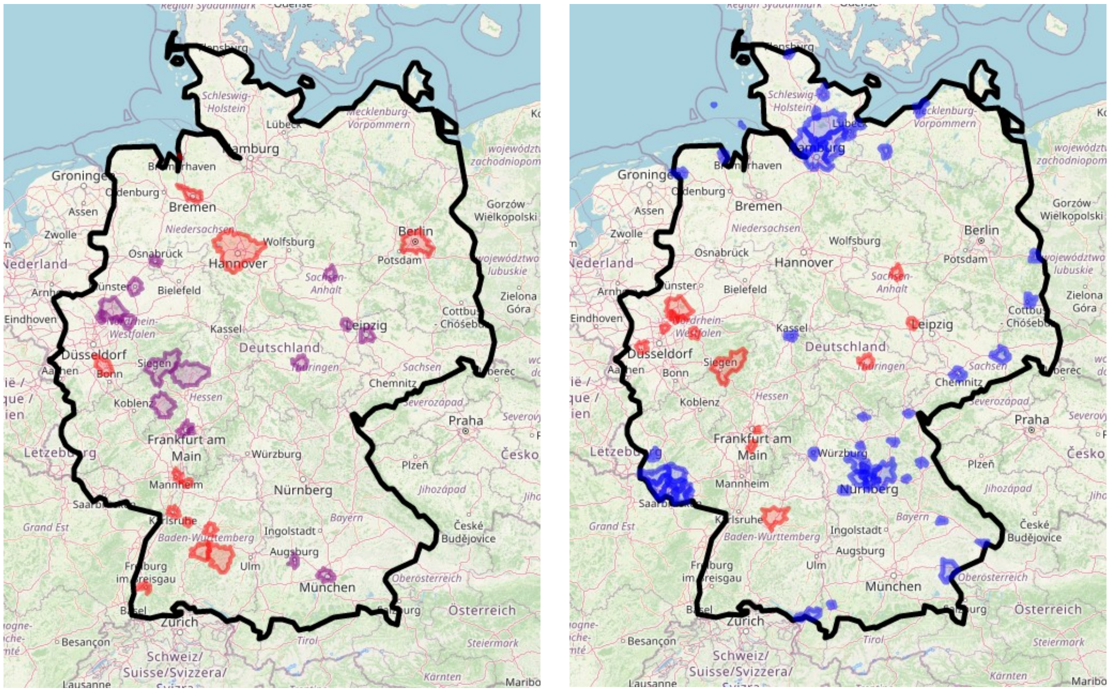


Notes: Green boxes represent treatment units, grey boxes control units and red boxes excluded regions/cities. A robustness test is done by excluding periods affected by the 2013 floods. The control cities of Group 2 are also restricted to have a similar population density and GDP per capita as the treated cities.

Figure 3.A.4: Treated and Control Units for Both Groups

(a) *Group 1*
Treated (red) and not-yet-treated cities (purple) in January 2008

(b) *Group 2*
Treated after 2010 (red) and not treated control cities (blue)



Notes: A minimum of 60 km is observed between treated and control units for each case.

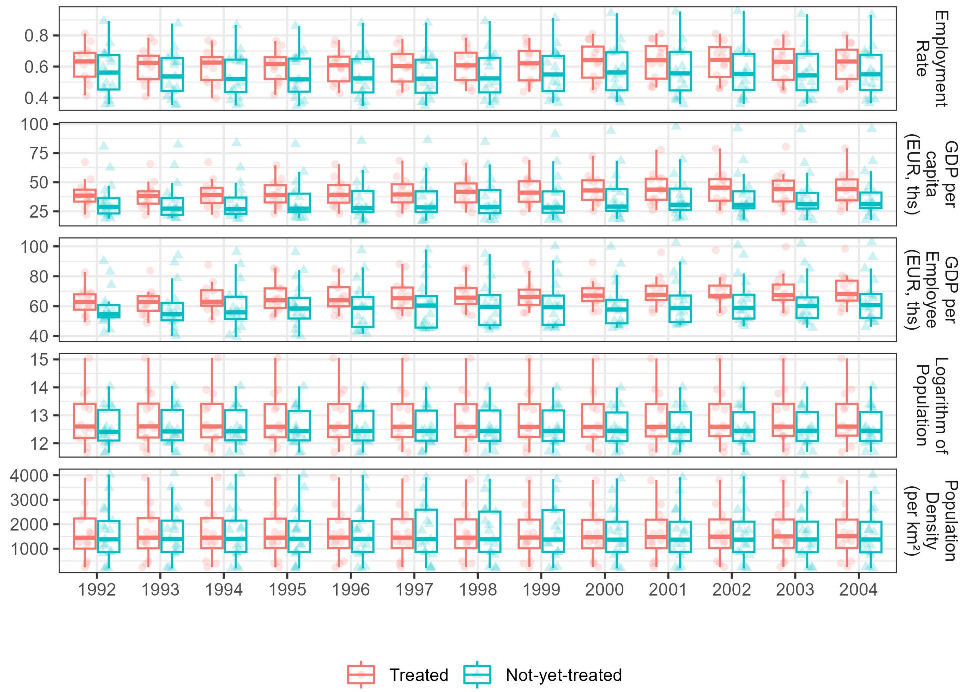
Table 3.A.1: Controls in Main Specifications

	Group 1	Group 2
Aggregate production:		
GDP	GDP (1991, 2000)	Log Pop. (2005) and GDP (2000)
GDP/population	No controls included	No controls included
GDP/employees	No controls included	No controls included
Employees/pop	No controls included	Employees/pop (1991, 2000)
Population	Population (1991, 2000)	Population (1991, 2000)
Sector shares of production:		
GVA_{TPS}/GVA	No controls included	No controls included
$GVA_{Industry}/GVA$	No controls included	Log Pop. (2005) and GDP/pop. (2005, 1995)
$GVA_{Construction}/GVA$	Log Pop. (2005) and $GVA_{Construction}/GVA$ (1991)	$GVA_{Construction}/GVA$ (1991)
$GVA_{Prof.\ Serv.}/GVA$	No controls included	No controls included
GVA_{PES}/GVA	No controls included	GVA_{PES}/GVA (1991, 1992)

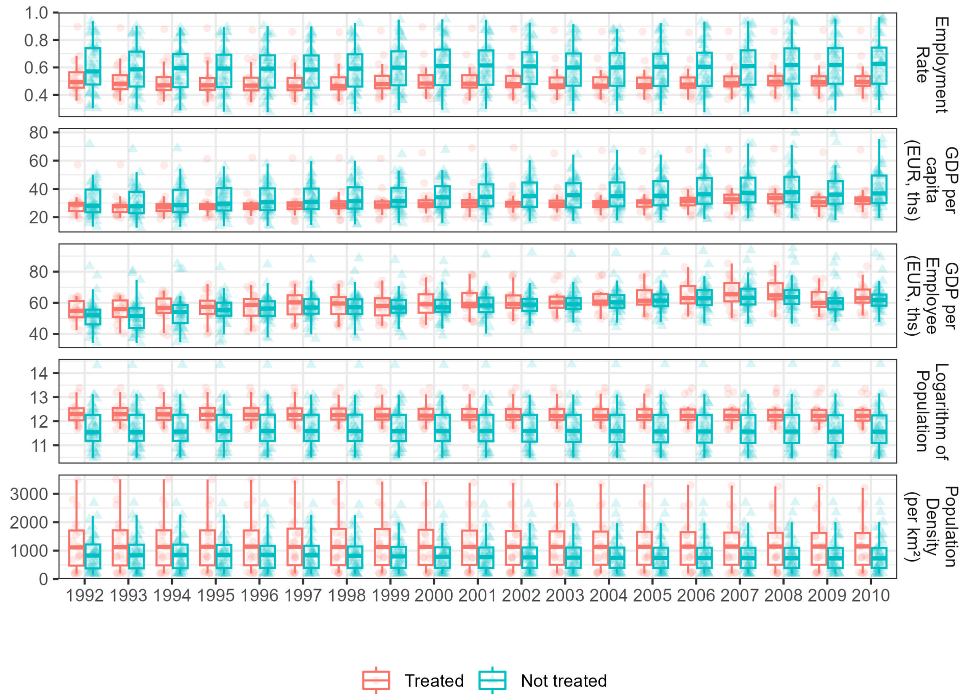
Notes: This table details the covariates included in each specification with the method explained in subsection 3.3.2. The results for the main outcome (GDP) with and without pre-treatment covariates can be seen in Figure 3.A.11.

Figure 3.A.5: Parallel Covariate Trends in the Common Pre-treatment Periods

(a) Group 1



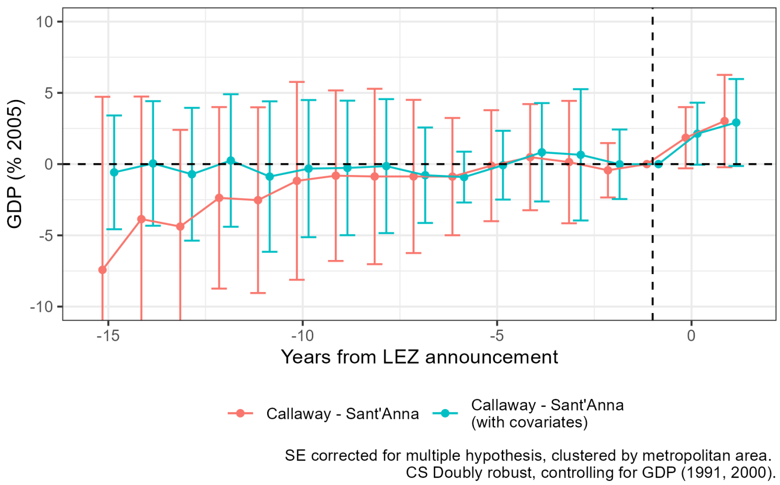
(b) Group 2



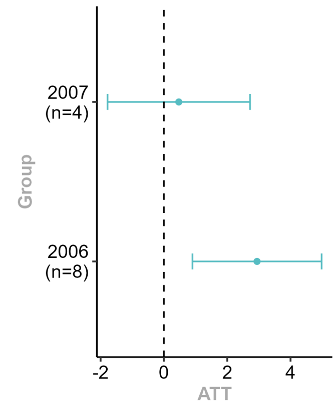
Notes: The figure shows the distribution (points and boxplot) of relevant covariates for treated and control regions in their common pre-treatment years in Group 1 and 2. By inspecting the differences between both groups across time, no diverging trends between treated and control cities in the pre-treatment period for any of these covariates of both groups are visible on the median, 25th and 75th percentiles.

Figure 3.A.6: Effect of LEZ in Total GDP

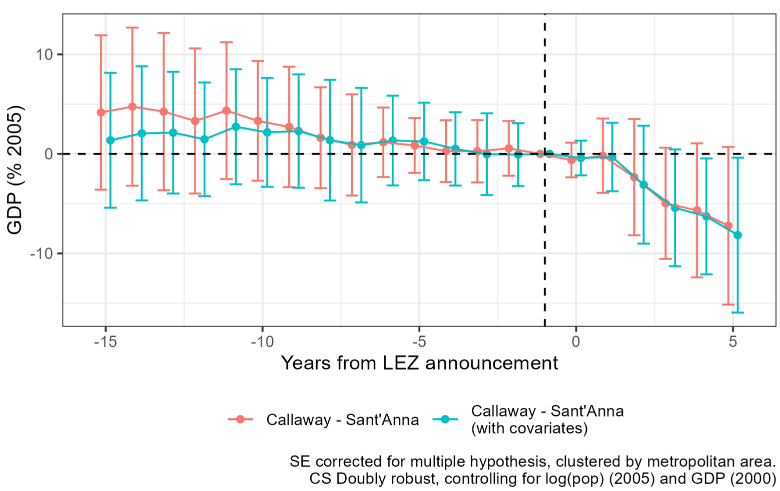
(a) Dynamic effects of LEZ announcement for Group 1 (β_k)



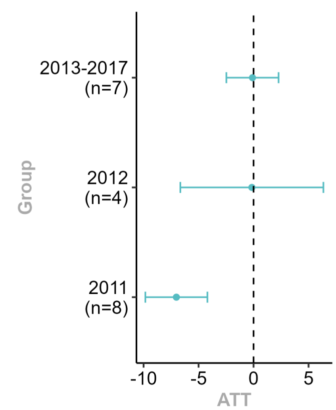
(b) Average effect of announcement by ann. year of Group 1 (β_g)



(c) Dynamic treatment effects for Group 2 (β_k)



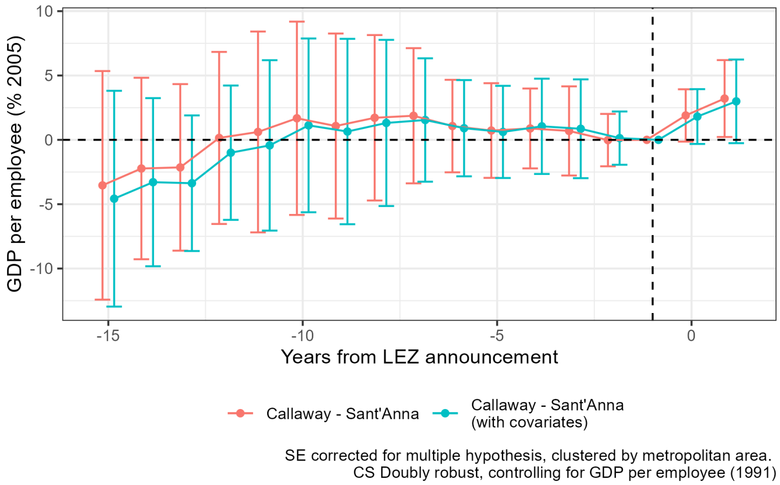
(d) Average treatment effect by ann. year of Group 2 (β_g)



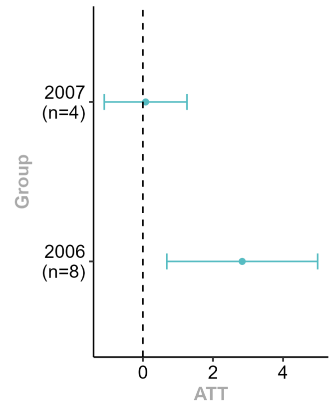
Notes: All SE are clustered by metropolitan area, and take into account multiple hypothesis testing.

Figure 3.A.7: Effect of LEZ in Total GDP per Employee (*Productivity*)

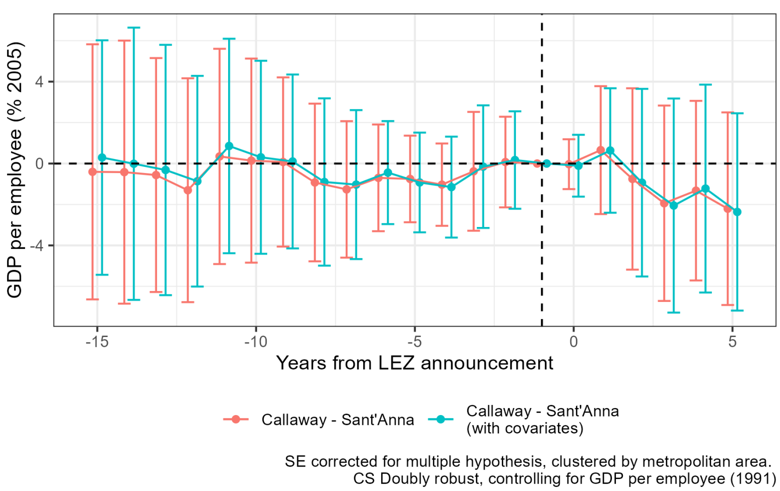
(a) Dynamic effects of LEZ announcement for Group 1 (β_k)



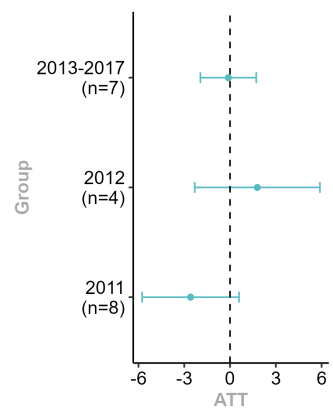
(b) Average effect of announcement for each group in Group 1 (β_g)



(c) Dynamic treatment effects for Group 2 (β_k)



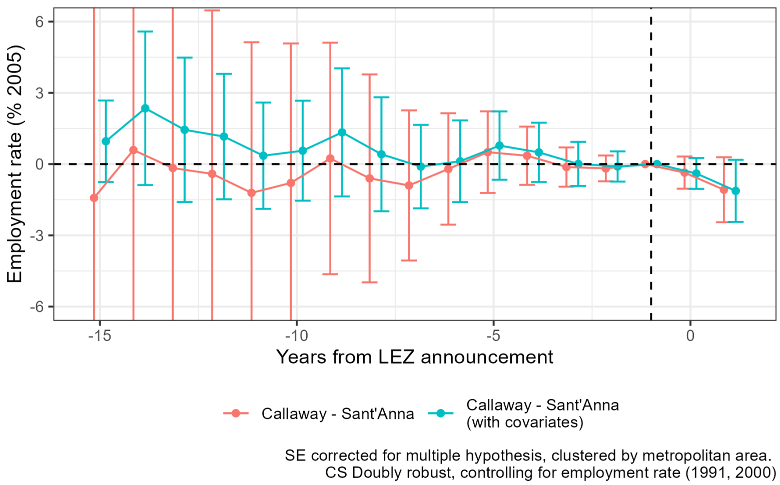
(d) Average treatment effect by ann. year of Group 2 (β_g)



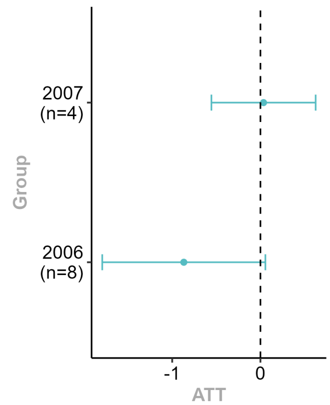
Notes: All SE are clustered by metropolitan area, and take into account multiple hypothesis testing.

Figure 3.A.8: Effect of LEZ in the total Employees per Capita (*employment rate*)

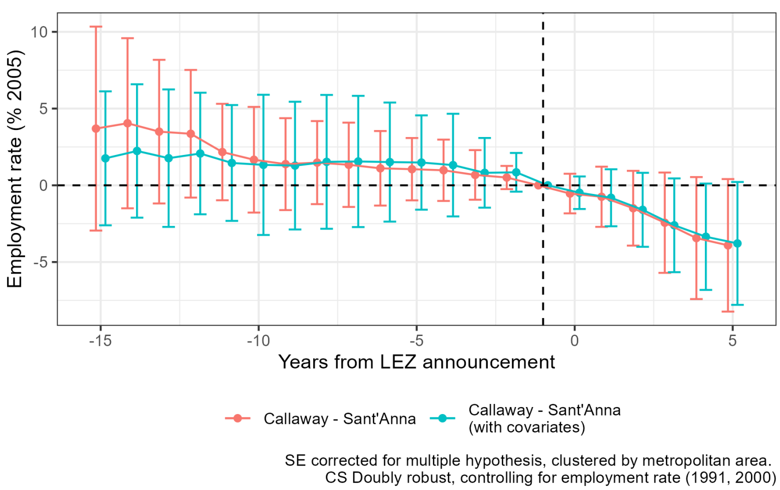
(a) Dynamic effects of LEZ announcement for Group 1 (β_k)



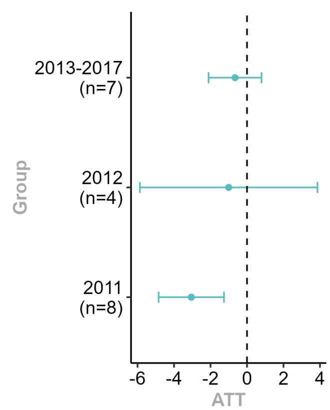
(b) Average effects of announcement for each group in Group 1 (β_g)



(c) Dynamic treatment effects for Group 2 (β_k)



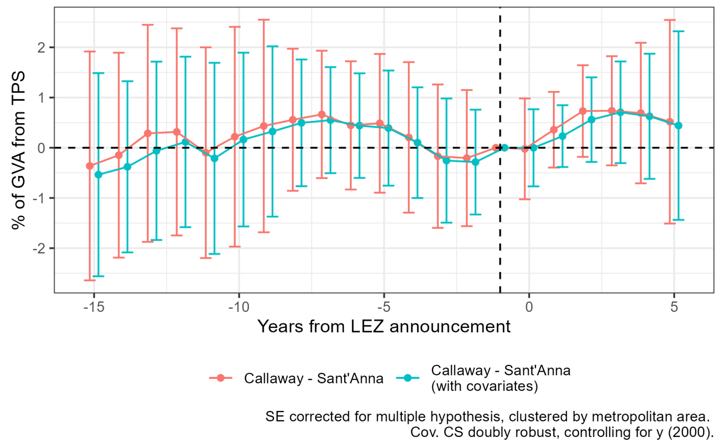
(d) Average treatment effect by ann. year of Group 2 (β_g)



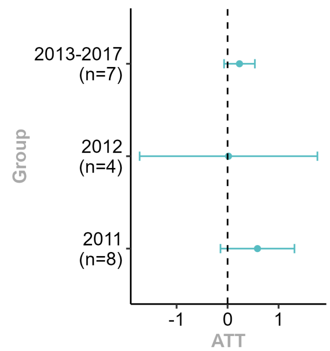
Notes: All SE are clustered by metropolitan area, and take into account multiple hypothesis testing.

Figure 3.A.9: Effect of LEZ in the Share of GVA for Selected Sectors — Group 2

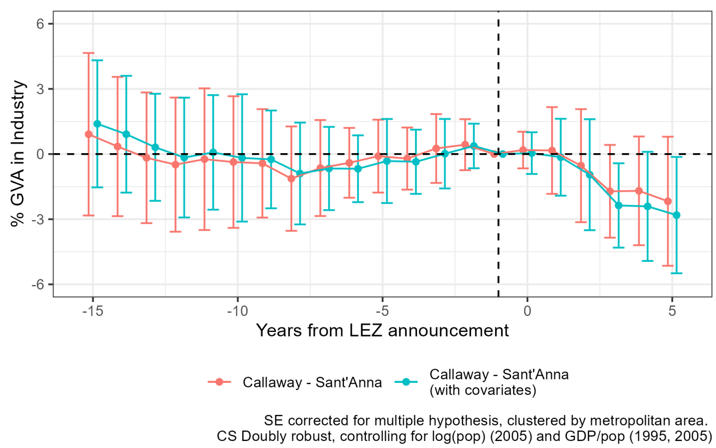
(a) Dynamic effects of LEZ for the share of TPS in GVA (β_k)



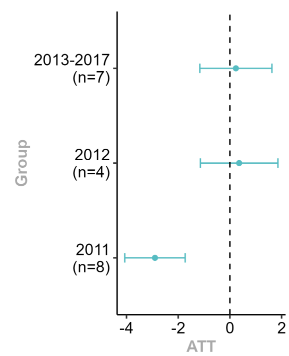
(b) Average treatment effect by ann. year (β_g)



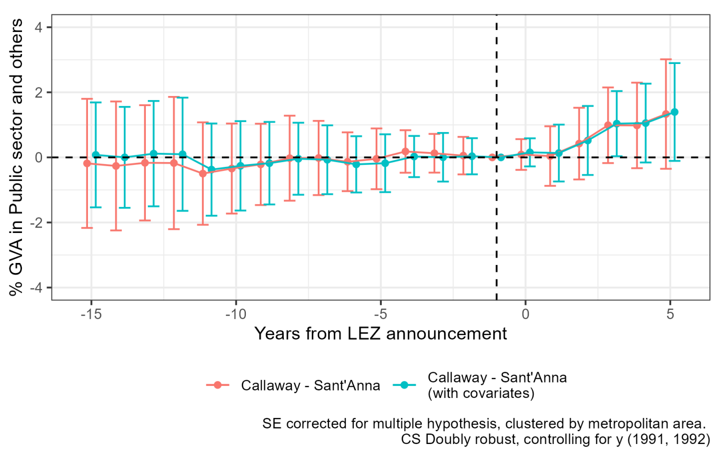
(c) Dynamic effects of LEZ for the share of Industry in GVA (β_k)



(d) Average treatment effect by ann. year (β_g)



(e) Dynamic effects of LEZ for the share of 'Public and entertainment services' in GVA (β_k)



(f) Average treatment effect by ann. year (β_g)

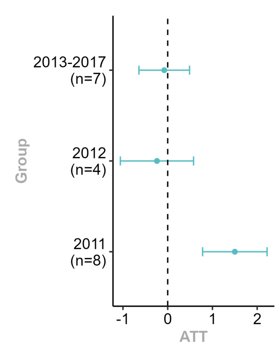
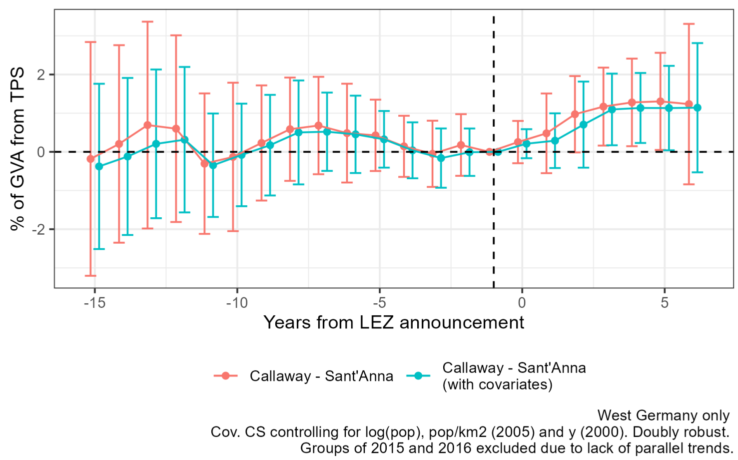
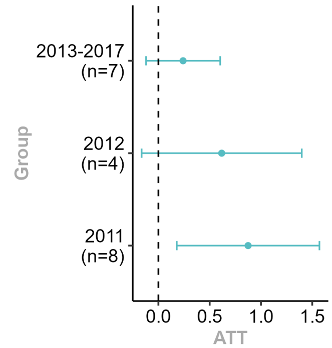


Figure 3.A.10: Effect of LEZ in the Share of GVA for Selected Sectors — Group 2, Only historic West Germany

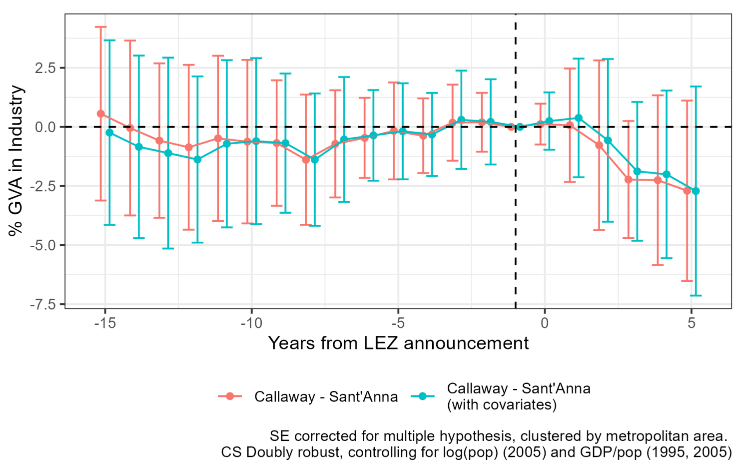
(a) Dynamic effects of LEZ for the share of TPS in GVA (β_k)



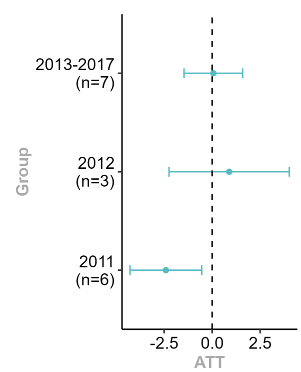
(b) Average treatment effect by ann. year (β_g)



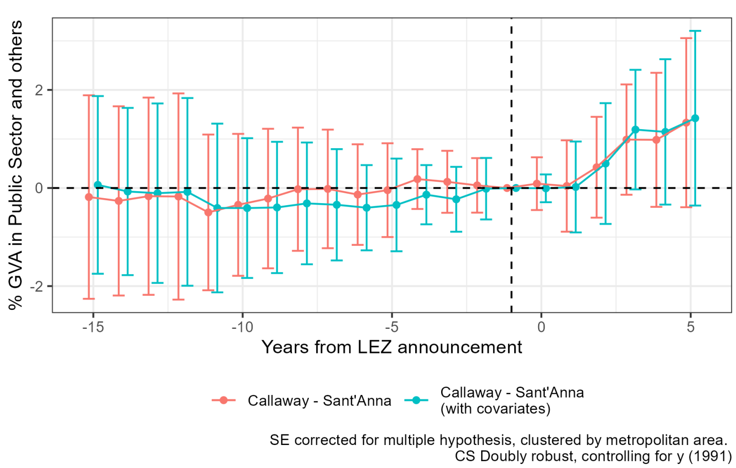
(c) Dynamic effects of LEZ for the share of Industry in GVA (β_k)



(d) Average treatment effect by ann. year (β_g)



(e) Dynamic effects of LEZ for the share of 'Public and entertainment services' in GVA (β_k)



(f) Average treatment effect by ann. year (β_g)

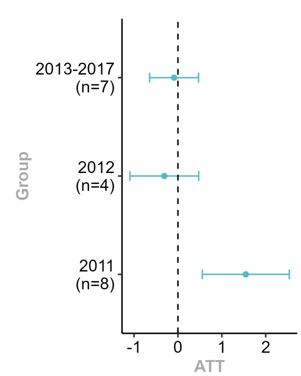
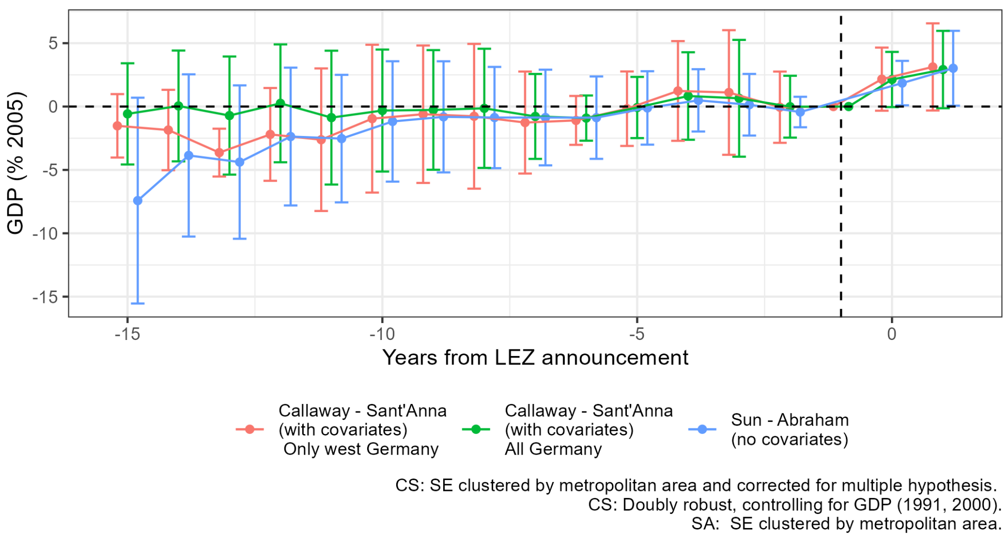


Figure 3.A.11: Effect of LEZ in Total GDP - Robustness

(a) Dynamic Effects of LEZ Announcement for the Group 1 (β_k)



(b) Dynamic treatment effects for Group 2 (β_k)

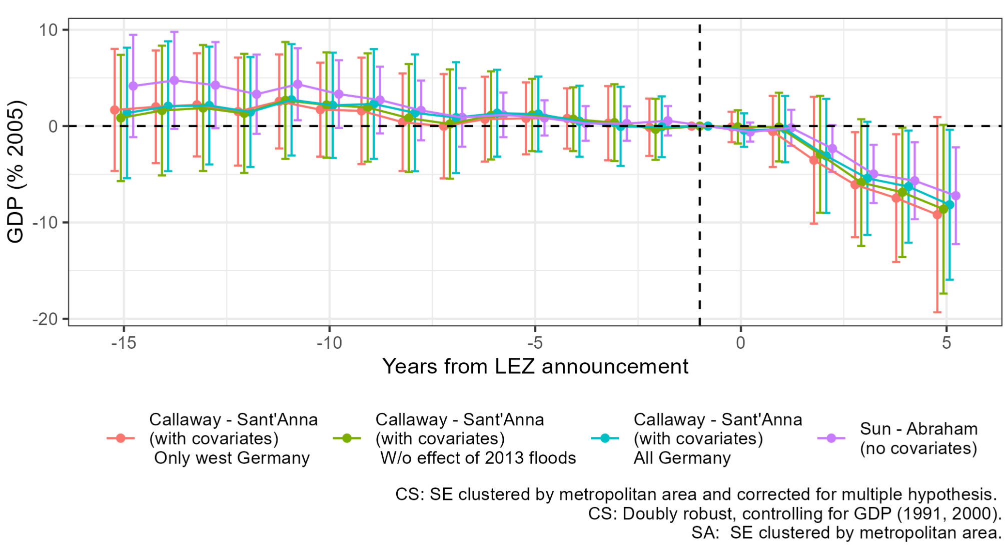
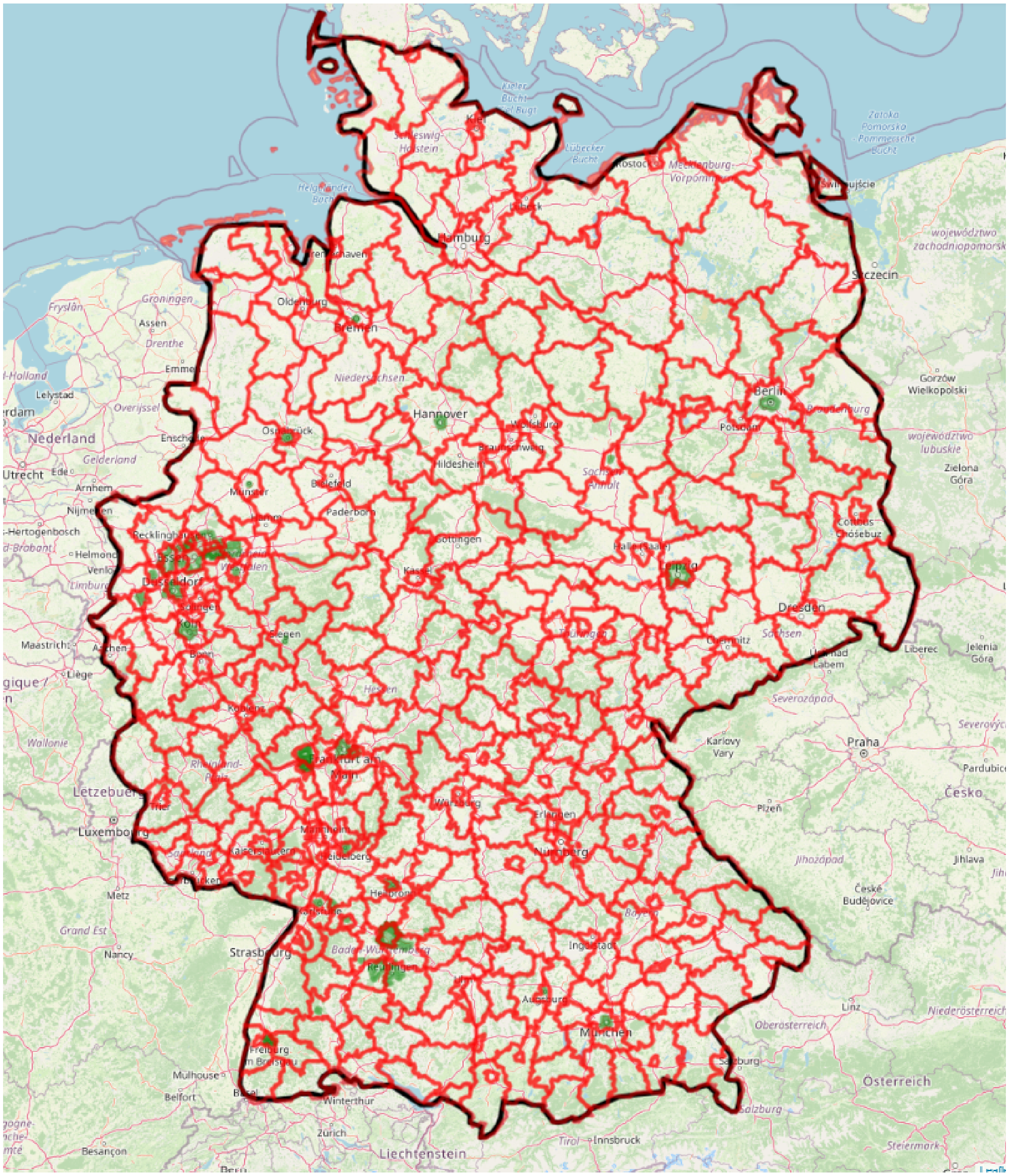


Figure 3.A.12: Main coverage of NUTS3 regions (districts)



Notes: Districts vary widely in size across Germany, with urban areas having smaller districts and small and large cities being mostly well represented by their respective boundary. Some Low Emission Zones are shown in green.

Chapter 4

Putting Low Emission Zones (LEZs) to the Test: The Effect of London's LEZ on Education

4.1 Introduction

Air pollution is considered one of the leading global environmental risks mainly due to its profound implications for public health. In particular, the World Health Organization (WHO) estimates that ambient air pollution led to 4.2 million premature deaths worldwide in 2019 and that in the same year, a staggering 99 percent of the global population resided in areas where the air quality fell short of the WHO's guidelines (*WHO* 2021). These shocking figures underscore the magnitude of this public health emergency and the widespread nature of this challenge. The adverse impacts of ambient air pollution and its associated costs have received substantial academic attention in economics and other disciplines, showing how exposure to air pollution affects not only mortality and morbidity (Currie and R. Walker, 2011; Schlenker and W. R. Walker, 2016) but also documenting its effects on other aspects of human life including worker productivity, crime and education (Bondy et al., 2020; Ebenstein et al., 2016; Zivin and Neidell, 2012).

Various sources contribute to ambient pollution, but vehicle traffic stands as a principal contributor to air pollution concentrations in many urban areas worldwide, significantly impacting the quality of the air we breathe in cities. As such, various policies have been devised to address this critical environmental issue. One notable example is the implementation of Low-Emission Zones (LEZs) in many cities around the world, which restrict the entry of high-emission vehicles into specific areas, aiming to curb the release of pollutants from vehicular exhaust. Previous literature has evaluated such policies in terms of their effects on pollution reduction and health outcomes (Gehrsitz, 2017; Margaryan, 2021; Pestel and Wozny, 2021; Wolff, 2014). However, despite the well-documented link between air pollution and scholastic achievements, there is no well-identified evaluation of the effect of LEZs on standardised exam scores to the best of our knowledge.

Our paper sought to address this gap in knowledge by evaluating the impact of London's LEZ on standardised exam scores among elementary school students in England. To do so, we utilise administrative data from the National Pupil Database (NPD), which provides information on exam results and various characteristics of pupils for the years 2005-2015. The NPD covers all students who study in state schools in England and is considered one of the richest education datasets in the world (DFE, 2015). Our focus in this study is on Key Stage 2 results, which are evaluated at year 6 (age 11). For identification, we employ a standard difference-in-differences approach in conjunction with fixed effects to overcome potential confounders. In particular, our control group consists of urban schools in the 20 largest cities in the England which are also outside of a 100km buffer from the LEZ border to ensure that our control group is unaffected by the policy.

We show that students within London's LEZ exhibited significant improvements in test results. More specifically, we find that on average, primary schools within the LEZ experienced a 0.09 standard deviation improvement in test scores relative to primary school students in urban areas in our control group. This estimated effect is highly statistically and economically significant. To put the latter point in perspective, the magnitude of our results is slightly smaller than the estimated effects of increasing average teacher quality by one standard deviation and similar to the estimated effect of reducing class size by 10 students or paying teachers large financial bonuses (Lavy, 2009;

Rivkin et al., 2005). Importantly, we also show that London's LEZ provides more substantial positive effects in schools with lower initial test scores at baseline, suggesting that the policy might also reduce environmental and educational inequalities that can have long-term labour market impacts. Furthermore, we show that the positive effect of this long-term intervention appears to increase over time, suggesting that prolonged reductions in pollution concentrations yield increasing improvements in educational outcomes. Finally, we conducted a comprehensive series of robustness tests to validate our findings. These tests include changing our main difference-in-differences specification in various ways, using matching techniques, and varying the size of our buffer zone, consistently show that the results are robust across various model specifications and sets of control schools.

Overall, this paper provides important contributions to the academic literature and policymakers more broadly. First, to the best of our knowledge, this is the first paper to link LEZs with standardised exam scores. Other papers in the economic literature that examined the impacts of LEZs focused solely on the pollution and health implications except for the paper by Brehm et al. (2022) which examines the effect of LEZs on education outcomes in Germany. In particular, they study the impact of LEZs on the transition rates of elementary school students to academic tracks in the German State of North Rhine-Westphalia. Utilising a staggered difference-in-differences framework and school-level data they show that LEZs have led to an increase in the transition rates to the academic track by 0.9-1.6 percentage points. This is the closest paper to our work given the mutual interest in the educational impacts of LEZs and we see both studies as complementary. Nevertheless, there are several important differences between the studies. Most notably, our paper utilises data on standardised test scores, a broadly applicable measure of educational achievement, facilitating direct comparisons with other interventions aimed at improving educational performance. Furthermore, our research not only examines a different geographical area (England) but also delves into the nuanced effects of LEZs across different income groups and initial levels of academic achievement, offering a further detailed exploration of the policy's educational implications.

Second, our results suggest that previous studies which focused solely on the pollution and direct health impacts of LEZs underestimated the true benefits of LEZ policies. This shows the potential for environmental policies to improve educational outcomes and highlights the importance of incorporating educational outcomes in the evaluation of environmental policies. As such, we propose that environmental policies should be considered as a viable alternative or complement to traditional education interventions (such as smaller class sizes and providing incentives for teachers) as the former can provide similar, if not higher, improvement in test scores.

Third, the existing economic literature on the link between education and environmental factors has certain limitations. First, it primarily explores this relationship in terms of short-length exposure, predominantly examining contemporaneous effects. Moreover, most prior research overlooks the critical early education phases, which play a foundational role in a child's educational trajectory. Our paper addresses these two gaps in the empirical literature and examines longer-term changes to the environment in primary education[1].

Finally, our analysis indicates that LEZs policies can effectively address both environmental and educational inequalities, offering potential long-term benefits for labor market outcomes as well. In particular, the more substantial positive impact of the policy on schools with lower initial test scores at baseline showed in this study is likely to extend into greater economic opportunities, highlighting the importance of incorporating such environmental policies into broader socio-economic strategies.

[1]It is important to mention that a limited number of studies have examined the effects of longer-term exposure to environmental factors during early development stages. For instance, a paper by Gazze et al. (2024) explores how lead exposure in children affects the long-run outcomes of their peers in terms of high school graduation, SAT-taking rates, and increased suspensions and absences. Additionally, a paper by Wen and Burke (2022) studies the negative impact of an increase in the average 9-month air pollution on US test scores, finding more pronounced effects on primary school students.

4.2 Background

4.2.1 Key Stages in England's Education System

England's education system is structured into distinct phases known as "Key Stages" that span from early childhood through to secondary education. These stages are part of the National Curriculum, which sets out the content to be taught and the standards children should reach in each subject at each stage of their education journey. Overall, there are four key stages, starting at KS1, which covers the first two years of school education (pupils aged 5 to 7), up to Key Stage 4, which encompasses the final stage of compulsory education, for students aged 14 to 16. Each stage serves a specific developmental and educational purpose, ensuring that students acquire the necessary knowledge, skills, and competencies to succeed in their subsequent educational and life endeavours.[2]

In this study, we focus on Key Stage 2 (KS2) which covers Years 3 to 6 (pupils aged 7 to 11). The curriculum at this stage is fairly broad and includes English, Maths, Science, History, Geography, Art and Design, Music, Physical Education, Foreign Language, and Computing. KS2 ends with national standardised tests that cover the core subjects of English (reading and grammar, punctuation, and spelling) and mathematics, with exams externally marked.[3] The national tests take place in May every year and last less than 4 hours overall. The KS2 assessments play a pivotal role in England's educational system, serving as a crucial benchmark for evaluating students' academic progress at the end of their primary education.

From an economic perspective, the analysis of KS2 test scores offers valuable insights into the human capital formation process at a relatively early stage of education. Human capital theory posits that investments in education are crucial for the development of

[2] More information on the national curriculum can be found here: https://www.gov.uk/national-curriculum

[3] This standardisation is critical for policymakers and researchers alike, as it provides a reliable dataset for analysing trends in educational achievement, evaluating the efficacy of policies, and identifying areas requiring intervention.

skills and abilities that enhance productivity and contribute to economic growth (Barro et al., 2013; Becker, 1964; Hanushek and Woessmann, 2008). The analysis of KS2 test scores also extends beyond educational achievement to encompass broader discussions on inequality, social mobility, and the efficacy of policy interventions. As such, KS2 assessments are not only a measure of educational attainment but also a lens through which the dynamics of human capital development and social inequality can be examined, offering a rich area for economic and policy analysis. Finally, recent economic research has increasingly focused on the role of environmental factors in shaping educational outcomes, with studies examining the impact of air quality and school infrastructure on test scores (Ebenstein et al., 2016; Gilraine, 2023; Stafford, 2015). In this context, the implementation of Low Emission Zones and their impacts on KS2 outcomes can provide important empirical evidence on the intersection of environmental policy and educational performance.

4.2.2 Policy Background

Low Emission Zones (LEZs) have been introduced around the world to tackle the issue of air pollution from transport, and in particular, high-polluting vehicles (Wolff and Perry, 2010). Through banning their use or requiring them to undergo expensive retrofitting procedures before being permitted to enter designated LEZ spaces, LEZ implementation is considered an impactful way to limit the air pollution produced on inner-city roads (Holman et al., 2015). The first generation of LEZs, Sweden's Environmental Zones (*Miljözon*), were implemented towards the end of the 1990s, laying the groundwork for future LEZs across many European countries including France, Germany, Italy, and the UK. As of 2022, there were 320 LEZs in Europe alone and this number is project to reach 507 by 2025[4]. Other countries around the world such as China, Indonesia and Japan have also introduced LEZs in urban areas to tackle vehicle emissions.

In May 2007, following increasing dialogue surrounding urban air pollution in the UK

[4]for more information on LEZs in Europe see: https://cleancitiescampaign.org/wp-content/uploads/2022/07/The-development-trends-of-low-emission-and-zero-emission-zones-in-Europe-1.pdf

and London most specifically, policymakers announced the upcoming implementation of the Greater London LEZ which, since its inception, has evolved into one of the largest and most rigorous traffic regulation policies globally (Zhai and Wolff, 2021). The policy itself was launched on 4th February 2008, and enforces a very large and rigid pricing scheme aimed at disincentivising the use of highly polluting motor vehicles across almost all of Greater London, an area of roughly 1,600 square kilometres with a population of 9 million people. The policy remains in force 24 hours per day, every day of the year with no exemptions during national and public holidays. It is enforced through monitoring a wide network of cameras installed across major and minor roads within the LEZ boundaries where Automatic Number Plate Recognition technologies are utilised in conjunction with Transport for London (TfL) databases to monitor and flag vehicle compliance (Zhai and Wolff, 2021).

From its inception, the Greater London LEZ has evolved according to several phases. The initial phase specifically targeted Heavy Goods Vehicles (HGVs) weighing over 12 tonnes and truck vehicles that were in violation of the Euro III standard of vehicle emissions. Drivers of such vehicles are expected to make their vehicle compliant with LEZ regulations (through engine replacement and/or retrofitting with particulate filters), to replace their non-compliant vehicle, or to pay a daily charge of £200 (Transport for London, 2006). Driving within the LEZ across midnight leaves drivers susceptible to a double charge, though charging does not apply to parked vehicles. In July 2008, shortly following the LEZ's launch, a second phase was implemented subjecting non-Euro III compliant Light Goods Vehicles (LGVs) weighing over 3.5 tonnes to LEZ vehicle standards or a £100 daily charge. These grounds were later extended to cover minibuses and large vans through a third phase in October 2010, and in January 2012 the policy was tightened, requiring HGVs, buses, and coaches to now meet Euro IV (rather than III) standards. Failure to both comply with the guidelines and to pay the daily entrance charge would result in a fine ranging between £250 and £1000 depending on vehicle type and speed of payment.

It is estimated that the costs associated with retrofitting non-compliant vehicles into

LEZ compliance would result in an additional cost of 2-4% on top of standard HGV and LGV operation costs (Transport for London, 2008). As a result, a Vehicle Operator Survey conducted by TfL in 2006 concluded that up to 5% of vehicle owners would deliberately opt for non-compliance, choosing instead to either pay the daily charge to enter the zone, or to risk the consequences of evasion of the rules (Transport for London, 2008). This figure is reflected in the compliance rates reported by TfL in the summer of 2021 where compliance stood at 95.5%. This is almost double the 48% compliance reported upon the LEZ's announcement in 2007 (Mayor of London, 2021).

Analysis of the impacts of the Greater London LEZ by Broaddus et al. (2015) found that the LEZ effectively stimulated drivers' replacement of non-compliant vehicles with more efficient, less polluting, and often smaller vehicles. Through this, alongside those choosing to retrofit their existing vehicles, TfL claimed the LEZ to have affected a 20% reduction in London coarse Particulate Matter (PM_{10}) within 5 years of operation (Mayor of London, 2021). This is significant given that vehicular emissions comprised approximately 60% of all PM_{10} emissions in London and 80% of those within central London prior to the LEZ (Transport for London, 2011). Later analysis by Zhai and Wolff (2021) examines the effect of the LEZ on PM_{10} concentrations. Using a stepwise difference-in-differences model they found that the second and more stringent phase of the LEZ did curtail PM_{10} concentrations, mainly near major roads. Interestingly, Zhai and Wolff also identified a negative spillover effect in sites beyond the LEZ boundaries where polluting vehicles have become increasingly concentrated.

The LEZ is not the first or the last policy to be implemented in London to reduce the negative externalities of vehicle use. The Congestion Charge Scheme (CCS), introduced in 2003 and still active today, subjects most vehicle drivers to a daily charge to enter a 21 square kilometres area in Central London (approximately 1.3% of Greater London) during high-traffic hours. The amount charged per day started at £5, and has been kept mostly stable in real terms. The push to reduce vehicle pollution and traffic has continued in recent years with the introduction of the T-charge (in 2017), and the Ultra Low Emission Zone introduction and expansion in 2019 and 2024. Following the London example, many UK cities have recently applied (between 2021 and 2023) or planned the application of their own Low Emission Zones.[5]

4.3 Data and Empirical Strategy

Our analysis aims to comprehensively assess the impact of London's LEZ on standardised exam scores among elementary school students in England. In this section, we describe the various sources of datasets that we used in our study, the resulting data, and our empirical strategy.

4.3.1 Data Sources and Descriptive Statistics

The primary data sources for this study are drawn from various governmental agencies and educational authorities in the United Kingdom. This provides comprehensive administrative information on exam results and various school-level characteristics, enabling us to examine varied treatment impacts across different subgroups of the student population. The primary dataset is the Key Stage 2 Performance Measures, obtained from the GOV.UK website.[6] This dataset provides annual information on Key Stage 2 test scores alongside various school-level characteristics for schools in England, covering an extensive period from 2005 to 2015[7], with the exception of a brief interruption in 2010 attributed to a national boycott of assessments. It includes essential variables such as test results, identifiers for schools, and demographic data, allowing for a detailed investigation into the evolution of examination scores before and after the introduction of the policy.

This dataset includes the Average Point Score (APS) in Key Stage 2, which quantifies student achievement across English and Maths in the following way: Initially, raw test scores by subject are transformed into Total Point Scores (TPS) based on year- and subject-specific thresholds. Then, these scores are averaged into a single APS that

[5] Between 2021 and 2023 the cities of Bath, Birmingham, Portsmouth, Aberdeen, Bradford, Bristol, Dundee, Edinburgh, Newcastle, and Sheffield introduced their LEZ. Greater Manchester is expected to introduce a LEZ by 2026.

[6] Available at https://www.compare-school-performance.service.gov.uk/download-data

[7] New performance measures were introduced in 2016, going from reporting an absolute value to a relative one that compared pupils' results of similar prior attainment.

represents the academic achievement of students according to the following formula: $APS = ((TPS_{Reading} + TPS_{Writing})/2 + TPS_{Math})/2$ (Middlemas, 2014). This is then further averaged across the school to provide a school-wide average APS.

In addition to the Key Stage 2 dataset, the study utilises the School Characteristics dataset, also known as the Pupil Census Data. This dataset contains school-level variables collected during the January school census each year and includes information on a wide variety of students, school, and teacher characteristics including ethnicity, teacher information, and enrolment figures. The School Characteristics data complements the Key Stage 2 data by providing additional contextual information about the schools and their student populations.

In the last decade, the UK has witnessed a meaningful number of school mergers and splits. To account for potential changes in school organizational structures and to have a consistent panel data of schools, we also use the "School Links" dataset from the Get Information About Schools service[8]. This dataset provides information on school links, including predecessor-successor relationships, mergers, and other relevant details. It allows for tracking schools across time and ensuring accurate identification, even if their unique reference numbers (URNs) have changed.

Overall, the final dataset comprises 32,919 observations from a total of 3,445 schools, split into 1,199 schools within the treatment group and 2,246 schools designated as the control group. Detailed summary statistics of the main variables before treatment are presented in Table 4.3.1. The table reveals that the average point scores were similar between the treatment and control groups, with scores of 27.6 and 27.5, respectively. The standardised Average APS in KS2 were marginally higher for the treatment group, consistent with the row APS. Interestingly, the standard deviations were negative, which suggests that the schools in our sample, located in large cities, exhibit lower test scores on average compared to other regions in the country. Moreover, the treatment group featured a notably lower percentage of native English speakers and individuals from the

[8]Available to download at https://get-information-schools.service.gov.uk/Downloads

white ethnic group compared to the control group.

Table 4.3.1: Summary Statistics

	Control			Treatment		
	Mean	SD	N	Mean	SD	N
Average point score (APS) in KS2	27.50	2.22	2,145	27.66	2.23	1,139
Standardized APS in KS2	-0.21	1.07	2,145	-0.14	1.07	1,139
Number of full-time equivalent pupils	269.16	116.49	2,245	310.66	127.11	1,196
% Eligible to free school meals	21.58	16.28	2,211	27.36	17.05	1,191
% English as first language	85.20	23.97	1,849	62.03	24.67	1,180
% White British ethnic origin	79.67	25.24	2,227	38.71	25.39	1,186
% Other white ethnic origin	1.39	2.05	1,462	8.96	7.16	1,136
% Indian ethnic origin	2.96	8.50	1,766	4.95	8.13	900
% African ethnic origin	1.98	5.08	1,752	12.99	11.44	1,140
Pupil-teacher ratio	21.44	4.57	2,245	21.51	5.28	1,197

Notes: All values from 2006. SD = Standard Deviation. N = Number of schools.

4.3.2 Empirical Strategy

In order to estimate the impact of London's LEZ on standardised exam scores, we adopt an empirical strategy that leverages the strengths of our data. In particular, we estimate the causal effect of the LEZ policy by employing the following two-way fixed effects differences-in-differences (DiD) model:

$$TestScores_{it} = \beta\{LEZ_i \times Post_t\} + \gamma_i + \delta_t + \varepsilon_{it} \tag{4.1}$$

Where $TestScores_{it}$ is the average test score of school i at academic year t, LEZ_i is an indicator that has value 1 only if school i is inside the London LEZ, and $Post_t$ is an indicator if it is after the LEZ was applied (2008 or latter). Finally, we allow for ε_{it} to be heteroskedastic and correlated at the postcode district level.

The above DiD estimation yields valid causal inferences of the average treatment effect if several key assumptions hold. First and foremost, the key identifying assumption in such models is the parallel trends assumption which implies that the application of the policy is exogenous to the potential outcome paths of treated and control units, and thus treatment and control units would exhibit parallel outcome trajectories for all periods in the absence of treatment. In the next paragraph, we discuss a potential threat to this assumption and describe the measures we take to address this concern. Additionally, we offer evidence that this assumption likely holds in our case by visually inspecting the pre-treatment trends in the following section.

A possible threat to this assumption, and therefore to the causal interpretation of our empirical strategy, is the application of the "Pupil Premium" in England. This policy, which started in the 2011-2012 school year, gives extra funding to schools for each pupil who meets certain characteristics, the most common being eligibility for Free School Meals (FSMs) at any point in the last six years[9]. The extra funding per pupil steadily increased in real terms (2022-2023 prices) from £629 in the 2011-2012 school year to £1596 in 2014-2015 (the last year in our sample) (Roberts, 2023). This national policy would not be a concern if treated and control schools had a similar proportion of FSM eligibility, and thus received a similar amount of financial help. However, treated schools happen to have a larger (although not statistically significant) percentage of FSM eligible pupils (27% instead of 21%, from Table 4.3.1).

To control for any possible differential effect on treated and control regions, we amend Equation 4.1 and estimate the following model:

$$
\begin{aligned}
TestScores_{it} = {} & \beta\{LEZ_i \times Post_t\} + \\
& \alpha_1\{FSM_{2006,i} \times PostPremium_t\} + \alpha_2\{FSM_{2006,i} \times PostPremium_t \times t\} + \\
& \gamma_i + \delta_t + \varepsilon_{it}
\end{aligned}
\tag{4.2}
$$

with $FSM_{2006,i}$ being the percentage of pupils eligible for free school meals on school i in the year 2005-2006 (pre-treatment) and $PostPremium_t$ being 1 only after the

[9] The other conditions are being a "Looked after" or "previously looked after" children (these are pupils who are in charge of the local council instead of their family) and pupils with a parent serving in the regular armed forces.

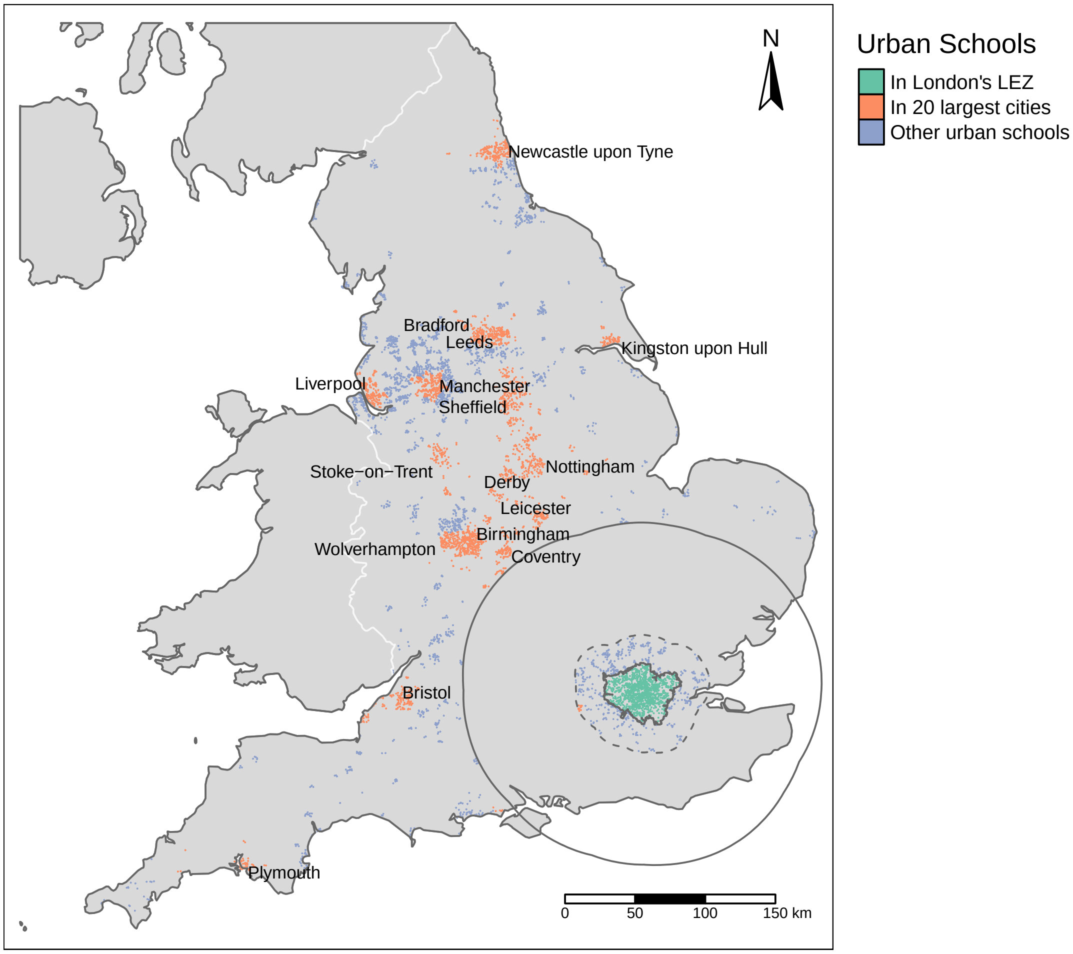
introduction of the Pupil Premium and 0 otherwise. This allows α_1 and α_2 to control for the resources received by school i due to the pupil premium and its steady increase in funding per pupil during our study period[10]. Equation 4.2 is our preferred specification for all the results presented below.

Another key identification assumption is that the potential outcomes for any unit should not vary with the treatment assigned to another unit. This is the non-interference (spillovers) assumption. For our specific case, this can happen through various channels including the spillover of air pollution via wind current and changes in traffic volume and/or vehicle fleets of neighbouring regions. In fact, prior literature provides evidence for such cases. For example, Wolff (2014) shows how the application of a LEZ correlates with a change towards cleaner vehicles in surrounding cities, and Sarmiento et al. (2021) find air pollution spillover effects up to 25km.[11] Therefore, we define our treatment and control group very carefully. In particular, the treatment group comprises urban schools located within London's LEZ boundary. These schools are directly affected by the LEZ policy measures aimed at reducing total vehicular emissions. Conversely, the control group consists of urban schools which are located more than 100km outside the LEZ boundary, similar to Zhai and Wolff (2021), and within the 20 largest cities in England. This creates a control group that is similar to our treated sample and minimizes the risk of contamination between treated and control units. Figure 4.3.1 shows the treatment and control schools used in our study and the 100 kilometer buffer that we use around the London's LEZ.

Another possible concern would be that treated and control schools change in the overall numbers, or in the social or ethnic composition during the time of the study due to external factors (such as migratory shocks from other regions of the world), and these have, by themselves, an impact on the outcome. To show the robustness of our results to these differential changes in composition, the next section presents a series of results for a sub-sample of matched schools in which treated and control schools experience the

[10]Reassuringly, the results are robust to not including these controls as shown in section 4.4.4.
[11]These are positive (increases in pollution) for the first 500m in the case of O_3, PM_{10} and NO_2, and negative from 500m up to 10-25km for CO, PM_{10} and NO_2.

Figure 4.3.1: Urban Schools in England



Notes: This map illustrates the urban schools included in the analysis, with each point representing a school. The solid line represents the 100km distance from the border of the LEZ (as in Zhai and Wolff (2021)) while the dotted line represents the 20km distance used for spillover effects. Treated schools are in green while the control schools are those in orange that are outside the solid line. In the spillover analysis the blue schools are included in both treated and control groups.

same changes in overall number of pupils, the share of pupils eligible for Free School Meals, the share of pupils that have English as their main language and share of pupils with the most prevalent ethnic origins in our sample (white British, other white origin, Indian, and African).

The last relevant assumption for our model is that the policy should not have unaccounted anticipation effects. This is a possible concern in our setting as the London LEZ was announced 1 year before its implementation, aiming to encourage people to upgrade their vehicles beforehand. As demonstrated by Wolff (2014), this proactive behaviour was observed in Regensburg, a German city, where there was a notable increase in environmentally friendly vehicles between the LEZ announcement and its enforcement. In the next section we explore this empirically and conclude this is not the case in our study.

We also study possible spillover effects of the policy on schools located outside, but in close proximity to the LEZ boundary to explore the broader impacts of the policy. As in Butts (2021), this is done simply by taking the urban schools in the first 20km buffer around the LEZ as treated units and using urban schools that are more than 100km away from the LEZ boundary as controls.

To explore the dynamic effects of the policy and also to provide another test for pre-trends we use a slightly more complex event-study specification. This is done by interchanging $\beta(LEZ_i \times Post_t)$ with $\sum_{k=2005}^{2015} \beta_k 1\{(LEZ_i \times Post_t) = 1\}$ in Equation 4.2.

To investigate if treatment effects varied by a given school-level characteristic Z_i we estimate the following model:

$$
\begin{aligned}
TestScores_{it} = \beta_1\{LEZ_i \times Post_t\} + \beta_2\{LEZ_i \times Post_t \times Z_i^{hi}\} + \\
\alpha_1\{FSM_{2006,i} \times PostPremium_t\} + \alpha_2\{FSM_{2006,i} \times PostPremium_t \times t\} + \\
\eta\{Z_i^{hi} \times Post_t\} + \gamma_i + \delta_t + \varepsilon_{it}
\end{aligned}
\tag{4.3}
$$

with Z_i^{hi} (or Z_i^{low}) being a dichotomous variable that has value of 1 if the school pre-treatment value of Z is above (below) the median and 0 otherwise. This allows us to estimate the treatment effect for schools that have a high- and low-level of characteristic

Z separately, and to interpret β_2 as a test of that difference.

Finally, we conduct a sensitivity analysis by exploring various additional variations in our sample and specification to show the robustness of our main findings. These include restricting our sample to those schools in which we have information for all years (balanced panel), using an specification without controlling for the 'Pupil Premium' program, using an alternative and much more restricted control group which consist of the 5 largest cities in England, and looking at the results by changing the minimum distance buffer around the LEZ border from 100km (main specification) to 75km, 50km, and 20km.

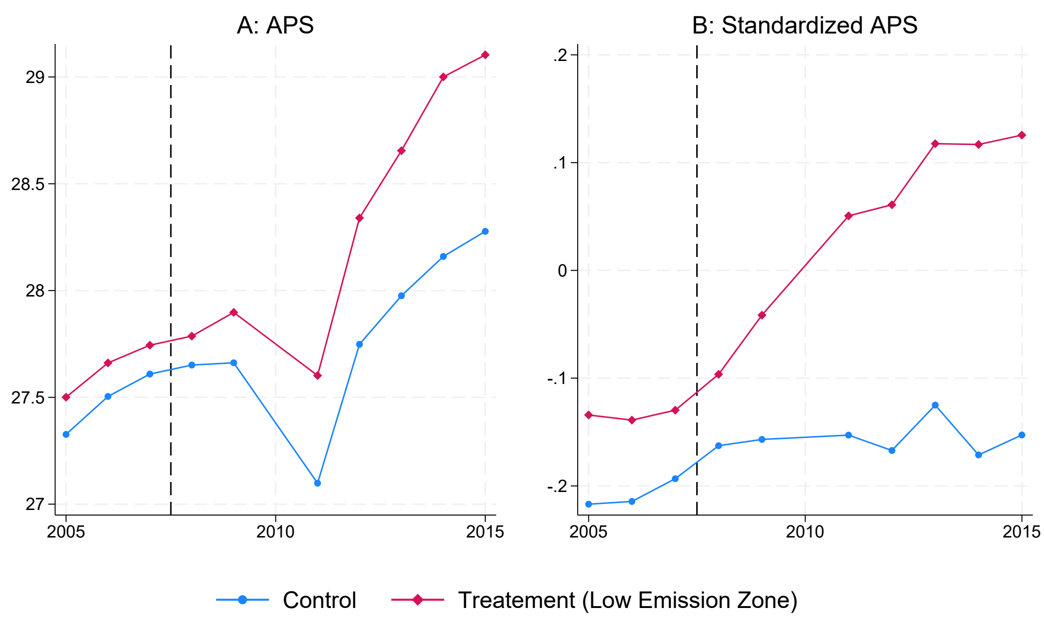
4.4 Results

In this section, we present and discuss the empirical findings of our study, examining the impact of London's LEZ on KS2 standardised test scores. Our analysis not only assesses the overall effect of the LEZ but also investigates how this impact varies over time and across schools with different income and achievement levels. This nuanced approach allows us to understand the broader implications of environmental policies on educational outcomes.

4.4.1 Main Results

Our primary analysis is visually represented in Panels A and B of Figure 4.4.2, which track the evolution of average KS2 test scores from 2005 to 2015, both in terms of raw Average Point Score (APS) and standardised scores, respectively. There are two important observations from these figures. First, the figures show parallel trends in the pre-treatment period across the treatment and control groups. This is a critical observation as it supports the parallel trend assumption underlying our difference-in-differences empirical strategy, suggesting that any post-treatment divergences can be attributed to the intervention rather than pre-existing trends. Second, the figures illustrate a clear divergence between the treatment group (schools within the LEZ area, depicted in red) and the control group (schools outside the LEZ area, depicted in blue) following the implementation of the LEZ policy. This divergence is notable very soon after the introduction of the LEZ, with the gap in standardised test scores between the two groups widening in favor of the treatment group. Such findings suggest that the LEZ policy has had a positive impact on educational achievement within the LEZ area, supporting the hypothesis that improving air quality can contribute to better academic outcomes. Following this visual analysis, Table 4.4.2 presents more formal estimates using our difference-in-differences estimator described in Section 4.3.2. The results show that the introduction of the LEZ improves test scores by 0.37 APS, or 0.88 of a standard deviation. This estimate is statistically significant at the 0.1 percent level and also economically significant, underscoring the policy's significant positive impact on educational achievement. To put the effect size in context, it is slightly smaller than the estimated effects of increasing average teacher quality by one standard deviation and similar to the estimated effect of reducing class size by 10 students or paying teachers large financial bonuses (Lavy, 2009; Rivkin et al., 2005).

Figure 4.4.2: Average Point Score (APS) Over Time



Notes: Yearly averages of APS and standardized APS by year and treatment status.
Data for 2010 is not reported due to a marking boycott.

Table 4.4.2: Impact of LEZ on KS2's Average Point Score (APS)

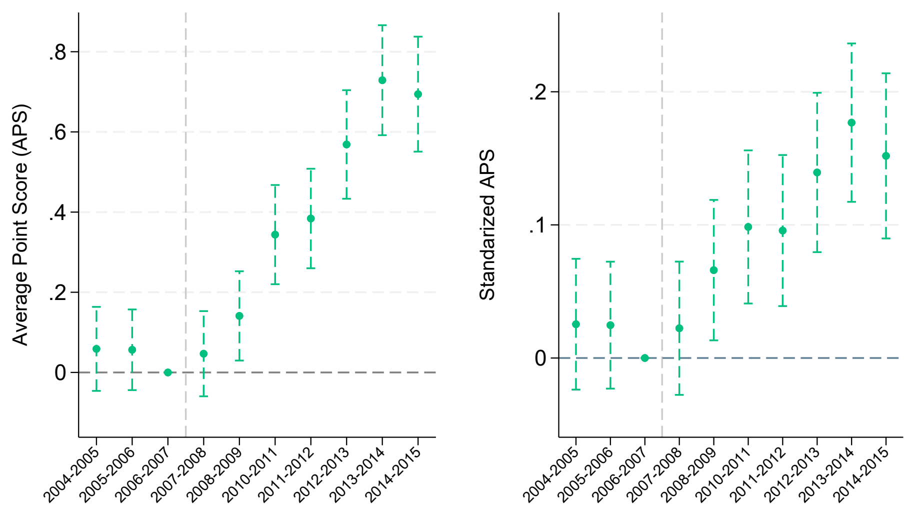
	APS (1)	Standardised APS (2)
Post × Treatment	0.37***	0.088***
	(0.043)	(0.020)
Constant	27.2***	-0.25***
	(0.020)	(0.0092)
School FE	Yes	Yes
Year FE	Yes	Yes
R2	0.21	0.12
Schools	3386	3386
Clusters	632	632
Observations	32415	32415

Notes: Standard Errors in parenthesis and clustered by postcode district.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

We continue our analysis by exploring the temporal dynamics of the LEZ policy's effects on KS2 scores through a dynamic difference-in-differences analysis (Event Study). This approach allows us to observe both the immediate and the evolving impacts of the LEZ policy over the years, with detailed results presented in Figure 4.4.3 for both raw and standardised scores. The dynamic analysis exhibits a notable trend: the positive effect of the LEZ policy on KS2 scores not only persists but appears to amplify over time. This finding is consistent with the hypothesis that sustained reductions in pollution exposure yield increasingly significant improvements in educational outcomes. The right panel of Figure 4.4.3 reveals that the effects range from 0.07 of a standard deviation in the 2008-2009 school year to 0.17 in 2014-2015. Such a trend is of great importance as it suggests that the benefits of the LEZ extend beyond initial impacts, accumulating positively as students experience prolonged periods of improved air quality. The gradual enhancement of test scores within the LEZ area, relative to the control, highlights the importance of continuous and long-standing policy measures in achieving substantial educational improvements.

Relatedly, we also study the effect of the policy by modeling the effect on KS2 standardised test scores as a function of the duration of exposure to the policy, rather than merely comparing before and after the policy's implementation. This approach, which is

Figure 4.4.3: Event Study Results of the Impact of LEZ on KS2's Average Point Score (APS)



Notes: Estimates by year with 95% CI, SE clustered by postcode district.
Data for 2010 is not reported due to a marking boycott.

a parametric version of the event study above, allows us to interpret the LEZ's influence on educational outcomes through the lens of cumulative effects, acknowledging that the benefits of improved air quality on test scores may accumulate over time. To study this, we introduce an interaction term in our model which is the number of years a cohort has been exposed to the LEZ policy interacted with an indicator for being in the LEZ area. This modification enables us to capture the yearly incremental effect of the policy over time on academic performance, captured in a single coefficient representing the slope of improvement. Our findings, presented in Table 4.A.1, show that for each additional year a student is educated within the LEZ, there is a statistically significant increase of 0.02 in standardised test scores. This result not only underscores the positive influence of the LEZ on educational attainment but also highlights the policy's cumulative benefits over time.

Overall, our analysis corroborates the initial findings of a significant positive effect on educational achievement and also illuminates the increasing magnitude of this impact as the duration of exposure extends. Such results lend strong support to the argument for sustained environmental interventions as a means to foster academic success, highlighting the critical role of time in the realization of policy benefits.

4.4.2 Heterogeneous effects

We now delve into the heterogeneous effects of the LEZ policy on KS2 standardised test scores, with a particular focus on initial test scores before the policy implementation and varying income groups as proxied by eligibility for free school meals. Our results of this analysis, detailed in Table 4.4.3 and estimated as described in Equation 4.3, document the differential impact of the LEZ policy across these distinct subgroups, revealing insightful patterns about whom the policy benefits the most. First, we explore the policy's effects relative to the average test scores of schools before the LEZ implementation. Our findings indicate that the observed benefits, in terms of higher test scores post-policy, predominantly accrue to schools that had lower (below median) test scores before the LEZ policy was introduced. In particular, the point estimates for the interaction terms are 0.18 and 0.66 in terms of APS and standardised scores respectively, and are statistically significant. This pattern is consistent with prior research on the unequal effects of air pollution on cognition, which find larger negative impacts in those with lower abilities (La Nauze and Severnini, 2021) and highlights the LEZ policy's capacity in contributing to narrowing the achievement gap.

We also examine the interaction between the LEZ policy and the proportion of students eligible for free school meals within a school, a reliable indicator of the socio-economic status of the student body. In particular, schools with a higher percentage of students eligible for free school meals (defined as having above-median eligibility) are considered high-eligibility schools. Our findings, also presented in Table 4.4.3, indicate a pronounced increase in test scores within these high-eligibility schools compared to schools with lower free school meal eligibility. Specifically, we find that schools below the median in terms of eligibility increased their tests scores by 0.26 and 0.04 in terms of raw average points and standardised scores respectively while those above the median experienced an *additional* improvement of 0.14 and 0.04 in APS and standardised scores, respectively. Whilst the interaction results are slightly noisy, they still highlight the significance of the LEZ policy's impact on schools, with additional benefits for more economically disadvantaged students, and underscore its potential role in reducing educational disparities.

Table 4.4.3: Heterogeneity in the impact of LEZ on KS2's Average Point Score (APS)

	APS		Standardized APS	
	(1)	(2)	(3)	(4)
Post × Treatment	0.32***	0.26***	0.077***	0.049*
	(0.044)	(0.045)	(0.019)	(0.020)
Post × Treatment × Low APS	0.18*		0.066*	
	(0.070)		(0.031)	
Post × Low APS	0.63***		0.36***	
	(0.042)		(0.019)	
Post × Treatment × High eligibility		0.14		0.043
		(0.074)		(0.035)
Post × High eligibility		0.22***		0.12***
		(0.048)		(0.022)
Constant	27.2***	27.2***	-0.24***	-0.25***
	(0.020)	(0.020)	(0.0088)	(0.0091)
School FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
R2	0.23	0.22	0.15	0.12
Schools	3386	3386	3386	3386
Clusters	632	632	632	632
Observations	32415	32415	32415	32415

Notes: Standard Errors in parenthesis and clustered by postcode district.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The analysis presented in Table 4.4.3 suggests that the LEZ policy can not only improve environmental conditions, and therefore alleviate some of the environmental injustice concerns, but can also foster educational equity. By enhancing test scores in schools with lower initial test scores, the LEZ policy demonstrates a potentially impactful approach to addressing educational disparities. These heterogeneous effects of the policy shed light on the broader social implications of environmental interventions and their potential to promote social mobility through improved educational outcomes of vulnerable populations.

4.4.3 Spillover Effects

Motivated by prior research indicating that LEZ implementation can lead to increased traffic and pollution just outside the boundaries of the zone (Zhai and Wolff, 2021), we move on to examine the potential spillover effects of London's LEZ on the test scores of surrounding areas. This displacement could potentially neutralize some benefits of the LEZ or even exacerbate conditions in adjacent areas. To empirically test the hypothesis that these changes might also affect educational outcomes, we shift our focus to urban schools within a 20km buffer outside the LEZ (see dotted line in Figure 4.3.1) instead of schools within the LEZ, and compare them to any urban school outside of the 100km buffer. Our results, detailed in Appendix Table 4.A.5, indicate a relatively modest positive impact on the test scores of schools situated just outside the LEZ, with an increase of 0.026 of a standard deviation. This estimate is less than a third of the positive impact observed within the schools directly inside the LEZ boundary (0.09 of a standard deviation) and statistically insignificant. Nevertheless, this analysis highlights the empirical danger of using adjacent areas as controls, providing further support to our research design.

4.4.4 Robustness

Finally, we provide a rich set of robustness tests to show that our results are robust across various model specifications and choice of control groups. As mentioned in the Empirical Strategy, it would be ideal to be able to rule out the effect of external factors, such as migratory shocks from other regions of the world, on our estimates. In order to advance on that objective, we create a sample in which treated and control schools start with very similar characteristics and have experienced very similar compositional changes by matching schools by their pre-treatment values of Standardized APS, share of pupils with English as their first language, and the shares of 'white British' and 'other white' ethnic origin. The intuition behind this is that schools with similar pre-treatment characteristics are more likely to be equally affected by external shocks. This can be directly applied to compositional changes as new immigrants tend to move to neighbourhoods with a higher presence of their own ethnicity, as first shown by

Bartel (1989) and used widely in the migration literature (e.g. Altonji and Card, 2018; Card, 2001). We use this known phenomenon to control and rule out the effect of exogenous *changes* in composition determined by using the pre-treatment levels of variables on ethnic origin.

This matching reduces the number of schools from 1197 to 568 in the treated area and from 2245 to 1763 in the control area and creates a much stronger balance of various covariates of interest (see Table 4.A.3). More importantly, we test if this matching eliminates any differential compositional *changes* between treated and control schools by inserting various variables in the left-hand side of Equation 4.2. The results, in Table 4.A.4, show that, after matching, treated and control schools experience the same changes in overall number of pupils, the share of pupils eligible to free school meals, the share of pupils that have English as their first language, and the share of pupils with the most prevalent ethnic origins in our sample (white British, other white origin, Indian, and African). The main results with this matched sample (reported in Table 4.A.2) are almost identical to our main estimates in Table 4.4.2. This shows that controlling for these migratory flows has no impact in the main results.

Finally, in Table 4.4.4 we provide a rich set of additional robustness tests. Column 1 presents our preferred specification results for raw APS in Panel A and standardized APS in Panel B, serving as a benchmark for subsequent analyses. In Column 2, we apply the same preferred specification but on a balanced sample and reassuringly find that results align closely with our initial findings. Column 3 changes the year of treatment from the year of implementation (2008) to the year of announcement (2007) to account for any pre-intervention effects, finding virtually no change with respect to the main results. Column 4 estimates the main results without controlling for the potential effect of the "Pupils Premium" (estimates Equation 4.1 instead of 4.2), finding an even larger effect that in our preferred specification. In Column 5 we use only schools situated in the 5 largest cities in England as our control group. The rationale for this analysis is to use a very restricted set of relatively comparable cities to further test the robustness of our results to the choice of the control. Again, we find very similar results even with this restricted sample. We conclude our analysis by testing the sensitivity of our findings to variations in our buffer zone by adjusting the distance buffer around the LEZ to 75, 50, and 20 kilometers, deviating from our original 100-kilometer buffer. The results, which are presented in Table 4.A.6 show that these adjustments do not affect our estimates significantly.

Overall, the robustness tests presented in Table 4.4.4 and Table 4.A.6 provide strong reassurance that our findings are stable across different specifications and methodological choices, reinforcing the reliability of our conclusions regarding the impact of LEZ policies on educational outcomes.

Table 4.4.4: Robustness

	Outcome: Average Point Scores (APS) in KS2				
	Main Results *(Table 4.4.2)*	With balanced panel	Treatment at Announcement	No Pupil Premium controls	Restricted Control Group
A: APS					
Post × Treatment	0.37***	0.36***	0.38***	0.47***	0.34***
	(0.043)	(0.046)	(0.042)	(0.045)	(0.049)
Constant	27.2***	27.4***	27.3***	27.3***	27.2***
	(0.020)	(0.022)	(0.019)	(0.020)	(0.024)
School FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
R2	0.21	0.23	0.21	0.19	0.25
Schools	3386	2541	3672	3740	2225
Clusters	632	602	696	696	429
Observations	32415	24751	35166	35760	21304
B: Standardised APS					
Post × Treatment	0.088***	0.089***	0.092***	0.17***	0.088***
	(0.020)	(0.021)	(0.019)	(0.022)	(0.022)
Constant	-0.25***	-0.19***	-0.23***	-0.21***	-0.25***
	(0.0092)	(0.010)	(0.0088)	(0.0097)	(0.011)
School FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
R2	0.12	0.12	0.11	0.043	0.13
Schools	3386	2541	3672	3740	2225
Clusters	632	602	696	696	429
Observations	32415	24751	35166	35760	21304

Notes: Standard Errors in parenthesis and clustered by postcode district. Main results as in Table 4.4.2 with 100km buffer and treatment at the application of the LEZ (2008). The restricted control group includes the 5 largest cities in England outside of London (in 2005): Birmingham, Liverpool, Bristol, Sheffield, and Manchester.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

4.5 Conclusions

This paper provides compelling evidence of the beneficial impact of London's Low-Emission Zone on standardised exam scores among elementary school students, marking a significant addition to both academic literature and policy discussions. Utilising the National Pupil Database, and employing a rigorous difference-in-differences approach complemented by fixed effects, our findings reveal that primary schools within the LEZ experienced a meaningful improvement in test scores, comparable to traditional education interventions such as enhancing teacher quality or reducing class sizes.

Importantly, we also show that the benefits are disproportionately greater in schools serving a larger share of economically disadvantaged populations and those with historically lower academic performance. This indicates that LEZs can play a critical role in leveling the educational playing field and supporting vulnerable communities. Furthermore, our results suggest that previous evaluations of LEZs may have underestimated their broader societal benefits, focusing primarily on immediate health and pollution reductions.

Several potential mechanisms could explain the observed relationship between LEZ implementation and improved test scores. For example, reduced air pollution may directly enhance cognitive performance and cognition. Additionally, by lowering air pollution, LEZs likely reduce respiratory-related school absences, allowing students to attend more consistently, which can enhance academic performance. Changes in the demographic composition within LEZs could also influence results as improved environmental conditions may attract families with higher socio-economic status, bringing additional educational resources and high-performance students that boost overall student performance. Future research should aim to disentangle such influences, providing clearer insights into the underlying mechanisms of this newly documented relationship between LEZs and educational outcomes.

Overall, this research provides a valuable framework for future studies and policy discussions, emphasizing the importance of incorporating educational outcomes into the assessment of environmental policies. As we continue to explore and understand these intersections, it is clear that environmental interventions can and should be part of a holistic strategy to enhance both public health and educational achievement.

4.A Appendix: Additional Tables and Figures

Table 4.A.1: Impact of LEZ on KS2's Average Point Score (APS) by Duration of Exposure

	APS (1)	Standardised APS (2)
Post × Treatment	0.0013	0.017
	(0.037)	(0.017)
Years since treatment × Treatment	0.10***	0.020***
	(0.0081)	(0.0033)
Constant	27.2***	-0.25***
	(0.019)	(0.0085)
School FE	Yes	Yes
Year FE	Yes	Yes
R2	0.22	0.12
Schools	3386	3386
Observations	32415	32415

Notes: Standard Errors in parenthesis and clustered by postcode district.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4.A.2: Impact of LEZ on KS2's Average Point Score (APS) — Matched Sample

	APS (1)	Standardised APS (2)
Post × Treatment	0.28***	0.088*
	(0.079)	(0.035)
Constant	27.3***	-0.24***
	(0.052)	(0.023)
School FE	Yes	Yes
Year FE	Yes	Yes
R2	0.20	0.080
Schools	2295	2295
Clusters	568	568
Observations	22272	22272

Notes: Standard Errors in parenthesis and clustered by postcode district.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4.A.3: Summary Statistics — Matched Sample

	Control			Treatment		
	Mean	SD	N	Mean	SD	N
Average point score (APS) in KS2	27.36	2.01	1,726	27.74	2.07	554
Standardized APS in KS2	-0.28	0.96	1,726	-0.10	1.00	554
Number of full-time equivalent pupils	295.70	118.56	1,762	322.80	123.59	566
% English as first language	66.52	28.29	1,437	66.60	27.71	553
% White British ethnic origin	48.71	29.27	1,754	47.45	28.56	558
% Other white ethnic origin	2.94	2.92	1,152	4.92	3.55	525
% Indian ethnic origin	8.57	14.97	1,365	5.90	9.89	436
% African ethnic origin	6.03	7.91	1,356	11.62	12.49	528
Pupil-teacher ratio	21.44	3.59	1,762	22.05	4.39	567

Notes: All values from 2006. SD = Standard Deviation. N = Number of schools.

Table 4.A.4: Changes in Composition — Full and Matched Sample

	Outcomes:						
	Full-time equivalent pupils	% With English as first language	% Ethnic British origin	% Ethnic other white origin	% Ethnic Indian origin	% Ethnic African origin	% Free School Meals Eligible
A: Full sample							
Post × Treatment	17.9***	3.67***	-4.42***	4.30***	0.60	1.89***	-3.00***
	(2.37)	(0.92)	(0.66)	(0.40)	(0.31)	(0.47)	(0.29)
Constant	285.9***	78.8***	65.6***	4.06***	3.40***	5.85***	24.5***
	(0.85)	(0.30)	(0.29)	(0.16)	(0.12)	(0.18)	(0.14)
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.21	0.64	0.55	0.36	0.11	0.26	0.36
Schools	3402	3402	3402	3401	3397	3400	3402
Clusters	632	632	632	632	632	632	632
Observations	32323	42825	44018	38507	37621	39816	44565
B: Matched sample							
Post × Treatment	3.32	1.42	0.090	1.20	-0.019	-0.24	-0.67
	(3.74)	(1.65)	(1.14)	(1.07)	(0.73)	(0.93)	(0.45)
Constant	302.3***	69.0***	50.0***	2.63***	7.74***	6.43***	27.7***
	(1.56)	(0.84)	(0.65)	(0.76)	(0.46)	(0.51)	(0.34)
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.26	0.59	0.47	0.43	0.17	0.36	0.47
Schools	2298	2298	2298	2297	2295	2296	2298
Clusters	568	568	568	568	568	568	568
Observations	21770	28574	29689	25309	25264	26177	29974

Notes: Standard Errors in parenthesis and clustered by postcode district. Matching performed with Coarsed Exact Matching on pre-treatment average Standardized APS, 2006-2007 values of share of pupils with English as their first language, the shares of pupils' ethnic origin, and the share of pupils with ethnic origin.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4.A.5: Impact of LEZ on Neighbouring Areas KS2's Average Point Score (APS)

	APS (1)	Standardised APS (2)
Post × 20km buffer outside LEZ	0.037	0.026
	(0.049)	(0.022)
Constant	27.4***	-0.19***
	(0.016)	(0.0073)
School FE	Yes	Yes
Year FE	Yes	Yes
R2	0.14	0.071
Schools	4802	4802
Clusters	987	987
Observations	45872	45872

Notes: Standard Errors in parenthesis and clustered by postcode district. Treated and control units are urban schools. Control schools are >100km from LEZ buffer but do not have to be in the largest 20 cities as the main specification.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

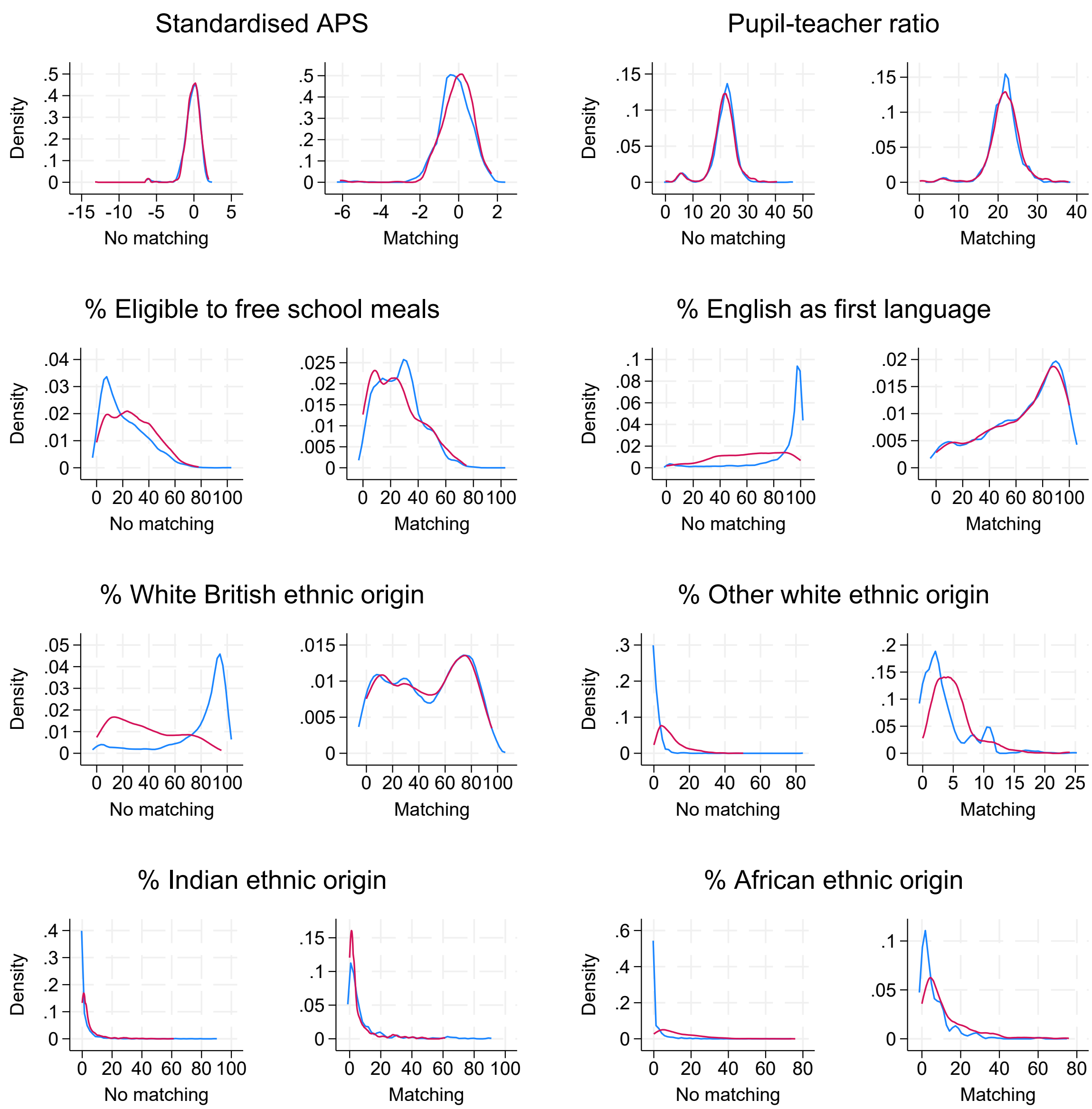
Table 4.A.6: Robustness by Distance Buffer

	Outcome: Average Point Scores in KS2							
	Raw values				Standardised values			
	(1) > 100km	(2) > 75km	(3) > 50km	(4) > 20km	(5) > 100km	(6) > 75km	(7) > 50km	(8) > 20km
Post × Treatment	0.37***	0.37***	0.38***	0.38***	0.088***	0.089***	0.091***	0.091***
	(0.043)	(0.043)	(0.043)	(0.042)	(0.020)	(0.020)	(0.019)	(0.019)
Constant	27.2***	27.2***	27.3***	27.3***	-0.25***	-0.24***	-0.24***	-0.23***
	(0.020)	(0.020)	(0.019)	(0.019)	(0.0092)	(0.0090)	(0.0089)	(0.0088)
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.21	0.21	0.21	0.21	0.12	0.11	0.11	0.11
Schools	3386	3517	3577	3663	3386	3517	3577	3663
Clusters	632	665	679	696	632	665	679	696
Observations	32415	33665	34247	35077	32415	33665	34247	35077

Notes: Standard Errors in parenthesis and clustered by postcode district. Treated and control units are urban schools in the largest 20 cities that are further away than the threshold. The main specification corresponds with columns (1) and (5) with a threshold of >100km from LEZ buffer.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure 4.A.1: Distribution and Balance of Covariates:



Notes: Epanechnikov kernel density functions for treated (red) and control (blue) samples with and without CEM matching. Distributions are for the 2006-2007 school year (before treatment).

Chapter 5

The Role of Gender in Firm-level Climate Change Adaptation Behaviour: Insights from Small Businesses in Senegal and Kenya

5.1 Introduction

Small and medium enterprises (SMEs) are crucial for livelihoods and employment opportunities and play a key role in the African business landscape and in inclusive growth (Abisuga-Oyekunle et al., 2020; Crick, Gannon et al., 2018). They contribute to local economic development, including through economic growth and poverty reduction. They are also often the most realistic livelihood opportunity for a wide range of disadvantaged groups, including women and youth, who face additional barriers to accessing other forms of employment. SMEs also dominate the business landscape. In Senegal, for example, they represent 90% of all enterprises and employ 60% of the active population (USAID, 2017).

Yet SMEs in Sub-Saharan Africa, hereafter called 'Africa', are highly exposed to climate risk, and businesses often have experienced climate extremes causing wide-ranging impacts on their business activities (Crick, Eskander et al., 2018). SMEs in Africa, particularly in rural areas, are often concentrated in agricultural sectors, and the impacts of extreme climate events interact and propagate along agricultural value chains, including through effects on supply and demand (Carabine and Simonet, 2017). However, research has shown that even fairly moderate climate events can produce wide-ranging but under-recognised impacts on businesses across a range of rural and urban sectors through events such as flooding, water supply disruption and hydroelectric load shedding (Gannon, Conway et al., 2018; Siderius et al., 2018). The importance of firm-level adaptation is therefore increasingly recognised in policymaking and academia, including in the context of climate justice agendas.

Literature increasingly highlights that businesses in Africa are generally highly aware of the climate risks that they face and tend to take action to try to manage climate risk within their operations. In some businesses, this includes taking steps to try to prepare for future climate change (Crick, Eskander et al., 2018). However, business level adaptive capacity is often very constrained by wide ranging deficits in African business enabling environments (Crick, Gannon et al., 2018). Additionally, the effectiveness of adaptation actions to establish business level resilience to climate risks varies.

Various distinctions are made between different types of adaptation in existing literature, including reactive vs. proactive, autonomous vs. planned, and incremental vs transformative adaptation (Begum et al., 2022). At the business level, Crick, Eskander et al. (2018) made a distinction between sustainable and unsustainable adaptation behaviours – depending on whether an action is likely to increase the capacity of a business to adapt to weather shocks in the future, maintaining current business activity, or if they result in a temporary (or sometimes permanent) reduction in business activity. Unsustainable adaptation behaviour, as outlined in Crick, Eskander et al. (2018), constitutes coping behaviour, because it helps businesses to address immediate needs and withstand and minimise the negative impacts of shocks and stressors in the short term (IPCC, 2018). However, coping behaviour can prevent businesses from engaging in long-term adaptation and, crucially, it may harm their ability to absorb future shocks. Comparatively, sustainable adaptation behaviour is an indicator of adaptive capacity and longer-term resilience.

This paper adopts Crick, Eskander et al. (2018) definition of sustainable and unsustainable adaptation behaviours, to consider how female representation in the management and ownership of a business impacts adaptation and resilience outcomes. The motivation for this analysis is the literature's emphasis that climate change has very disparate impacts on different social groups who, in turn, have different adaptive capacities and risk preferences. Different dimensions of social identity, such as gender, age, ethnicity, socioeconomic status, social roles, and geographical location, shape exposure to climate shocks and condition access to resources that shape climate change adaptive capacity and vulnerability (Nyukuri, 2016; Omolo, 2010).

Gender is a particularly well-documented influence not only on exposure to climate-related risks, but also on individuals' and communities' risk perceptions, susceptibility to harm, and their capacity to cope and adapt. In business contexts, literatures have shown that cultural norms and established gender roles influence the types of business activities that women are likely to be concentrated in and constrain female entrepreneurs' ability to access key resources for adaptation and to participate in decision-making processes (Gannon, Pettinotti et al., 2022). Literature in different contexts and geographical regions has also found that there are often differences in adaptation behaviour between men and women (Codjoe et al., 2012; Djoudi and Brockhaus, 2011; Jost et al., 2016; Swai et al., 2012) and that gender often shapes different risk perceptions and preferences. Yet there has been limited empirical research that has explored gender dimensions of adaptation behaviour at the business level, with most focused at the household level. This is particularly problematic since, in Africa, most entrepreneurs are women.

This paper contributes to this gap in the literature by exploring how female gender representation at the firm level impacts the propensity of a firm to undertake (un)sustainable adaptation behaviour. Crick, Eskander et al. (2018) found that repeated exposure to extreme events among SMEs in Kenya and Senegal is associated with a higher likelihood of a business engaging in more adaptation. However, they also found that the otherwise almost linear link between climate stress and businesses undertaking sustainable adaptation behaviours levels off among firms that have faced three extreme events or more. Given this effect of exposure on adaptation behaviour, we also consider how gendered propensity to (un)sustainable adaptation varies in the context of increasing exposure to extreme weather events.

Using survey data from 325 SMEs in semi-arid regions of Senegal and Kenya, we employ a Poisson regression model to empirically investigate how female representation in ownership and management of SMEs in Kenya and Senegal affects the adoption of firm-level sustainable and unsustainable adaptation strategies with increasing exposure to extreme weather events that affect the SMEs. Our results suggest that firms with female representation are less likely to resort to unsustainable adaptation behaviours in the context of increasing exposure to extreme events. We also acknowledge that access to factors enabling adaptation can be gendered-differentiated. Thus, we investigate how different dimensions of the business enabling environment (access to assistance and training) condition the differences between businesses with or without gender representation within the business management and ownership structure.

The structure of the paper is as follows. Section 5.2 provides the analytical and literature context to the role of gender in private sector adaptation in Africa and introduces our approach. The subsequent sections discuss our empirical strategy (Section 5.3) and the results (Section 5.4). Section 5.5 discusses policy implications of the findings, and finally, Section 5.6 concludes.

5.2 Gendered Dimensions of Adaptation at the Business Level

5.2.1 Women Entrepreneurs face a 'Triple Differential Vulnerability' to Climate Change

Women entrepreneurs in Africa are generally considered to be particularly at risk from the impacts of climate change. Recognising that climate risk results from the interaction of vulnerability, exposure and the likelihood of a climate-related hazard occurring (IPCC, 2018), this differential risk profile occurs at the level of exposure and vulnerability. Traditional gender roles and responsibilities, coupled with additional barriers to entrepreneurship, mean women entrepreneurs are often involved in small-scale livelihood activities and sectors, such as agriculture, small-scale agricultural processing, informal trade, and hospitality; sectors that are highly vulnerable to climate disruption (UN Women, 2018). Some literature also suggests that women are more likely to be confined to more marginal, degraded or flood-prone land that is less resilient to climate shocks (e.g. Davies, 2017; Djoudi and Brockhaus, 2011). Additionally, women entrepreneurs are also often responsible for tasks at the household level, such as water fetching, food preparation and childcare, that are susceptible to disruption through climate variability and extreme weather events and which may limit the time, mobility, and resources that they have to manage climate risks within their business or diversify their livelihoods (e.g. Agol et al., 2023).

Gender also shapes barriers to adaptation within the business and business environment. Businesses need to have the incentives, resources, knowledge, and skills to adapt to climate change (Fankhauser et al., 1999), and the ability of businesses to adapt effectively to climate change is highly influenced by the external business enabling environment, as well as by internal firm characteristics. This includes businesses' ability to access supportive policies, information, technologies, infrastructure, markets, and finance (Crick, Gannon et al., 2018).

In recent years, there has been a surge in political commitments focused on redressing gendered disadvantage in wide-ranging policy areas among national and international governments and development institutions (C. Moser and A. Moser, 2005; Sweetman, 2012; Woodford-Berger, 2004). Indeed, gender often forms a fundamental part in the design and evaluation criteria in development programming[1]. As such, adaptation and development institutions often implement actions and policies directly targeted at women. Some studies have suggested that, in some contexts, such gender mainstreaming actions have led to women having greater access to public services and other community-based and government-funded programmes than men (e.g. Ado et al., 2019; Mersha and van Laerhoven, 2019).

Nevertheless, the effectiveness of gender mainstreaming in policy and development programming remains unclear. Development programming targeting the poorest and most vulnerable is often more challenging and costly to implement. Moreover, micro and small enterprises in informal (unregistered) sectors — where many women's businesses are located — have often not been well recognised as economic actors and tend to be overlooked in private sector adaptation and development policies (Gannon, Crick et al., 2020). Thus, literature suggests that women entrepreneurs in Africa continue to face a differential disadvantage in access to training, finance (Adegbite and Machethe, 2020; Singh and Belwal, 2008), markets (Stevenson and St-Onge, 2005), and assistance (Ado et al., 2019; Mersha and van Laerhoven, 2019), among other factors.

Reflecting literature on intersectional vulnerability, which recognises that the effects of concurrent forms of inequality are often more than the sum of their parts (Crenshaw, 1989; Kaijser and Kronsell, 2014), maintains that differential disadvantage in relation to these factors interacts and compounds. For example, women's more limited mobility to travel within and between regions means they are often less likely

[1]In the African continent, out of the 54 countries that have submitted National Contributions to the Paris Agreement, 78% mentioned the vulnerability of women and other vulnerable groups, as well as the need to enhance their resilience (Chingarande et al., 2020). In Kenya, one third representation of women is required in different decision-making fields, including climate-change related platforms (Atela et al., 2018). Senegal in 2010 enforced one of the most radical quota laws in the world, requiring all parties to have a female-male ratio of 50% in their candidate lists (Tøraasen, 2017).

to attend trainings and observe climate-resilient practices directly, or to access large and diverse social and business networks which support access to new information and the acquisition of new business and adaptation skills (Mayoux, 1995; Nyantakyi-Frimpong, 2019; World Bank, 2014). These same factors are also likely to constrain business growth, as well as the financial resources and collateral to access technologies for adaptation or business development. These factors led Gannon, Pettinotti et al. (2022) to suggest that women entrepreneurs face a "triple differential vulnerability" to climate change, wherein they: "1) are often more sensitive to climate risk as a result of their concentration in certain sectors, activities and types of enterprises (e.g. micro SMEs in agricultural production in remote regions); 2) face additional barriers to adaptation across policy, institutional, regulatory and financial environments and in accessing supportive infrastructure, markets, technology, data, information and training; and 3) are also on the frontline of managing climate risk at household levels".

5.2.2 Gender Shapes Adaptation Choices and Preferences

Alongside literature which suggests that women are especially vulnerable to climate change, is a parallel and integrated thesis which indicates women's potential as strategic actors in strengthening inclusive resilience and suggests that women may also be especially valuable in adaptation. Much of the literature that focuses on the role of women's empowerment in fostering resilience is grounded in traditional participation rationales which propose that incorporating diverse knowledges, experiences, and values into adaptation efforts fosters innovation and leads to more robust outcomes (e.g. Kaijser and Kronsell, 2014; Quisumbing et al., 2014). However, some literature goes further and begins to suggest that women may be more likely to engage in adaptation actions that are comparatively more sustainable, equitable or effective than those pursued by men, based on their unique knowledges, experiences, and values.

Studies in cognitive science, behavioural economics and psychology have tended to conclude, for example, that women have a more precautionary approach to risk management, which may mean that women are more likely to prioritise adaptation strategies that minimise uncertainties. Behavioural science has been strongly concentrated in

WEIRD – Western, Educated, Industrialized, Rich and Democratic – countries, and there can be no assumption of transferability of findings across culturally and contextually diverse contexts. Indeed, some research indicates strongly that gender differences in individual and social risk-taking are likely to be culturally specific (Friedl et al., 2020). Nevertheless, women's different responsibilities and participation in different economic sectors and activities, including within communities and households, shapes differential exposure to climate extremes (Wong, 2016), contribute to different perceptions of climate change (Habtemariam et al., 2016; Mason and Agan, 2015) and to different adaptation preferences, knowledges, and responses (Codjoe et al., 2012; Kom et al., 2020; Smucker and Wangui, 2016).

In this context, some literature has begun to consider how gendered adaptation knowledge and preferences position women well for finding equitable, resilient and nature-based solutions to socio-climatic risks. Djoudi and Brockhaus (2011), for example, posit this thesis within their study of women in livestock-dependent communities in northern Mali. Within this context, they suggest that women's focus on ensuring household wellbeing results in a long-term perspective in their adaptation preferences, which leads them to focus on adaptation options oriented around household educational investments and decreasing livelihood dependency on natural resources. Codjoe et al. (2012) similarly find that women's traditional roles as providers of household food and water security influence their preferred adaptation choices, motivating them to implement adaptation action that prioritises these activities (Codjoe et al., 2012). A case study of small-scale farmers in South Africa, meanwhile, found that households headed by female farmers are more likely to employ new crops and crop diversification in response to climate variability and change (Kom et al., 2020).

These findings reflect parallel literatures which: 1) have argued that women-led communities tend to have more inclusive and effective responses to natural disasters and promote more sustainable natural resource management (B. Agarwal, 2009; ILO, 2009; UN WomenWatch, 2009); 2) suggest that as caregivers and managers of household resources, women often consider the long-term impacts of their actions on family livelihoods and community resilience; and 3) link women's potential contribution in climate change adaptation to their experiences of historical exclusion, social disadvantage and longstanding advocacy for social justice (Enarson, 2013; Smucker and Wangui, 2016). Empirical evidence on women's inherent propensity towards sustainable adaptation, nevertheless, remains extremely limited and is generally linked to their societal roles as caretakers, rather than developed in the context of entrepreneurship.

This gap in existing literature motivates our analysis of how gender-differentiated SMEs adapt to extreme climate events, considering unsustainable adaptation a potential manifestation of limited adaptive capacity. Moreover, we look at the effect of key aspects of business enabling environments for private sector adaptation: assistance and access to training, particularly since literature highlights that access to these resources can be highly gendered.

In this section of the paper, we have highlighted that the existing literature suggests that gender — and its interactions with other determinants of vulnerability — may enable or constrain firm-level adaptive capacity and sustainable adaptation. Nevertheless, evidence on gender barriers to business development and adaptive capacity is widespread, historically persistent and varied in its forms. Thus, broadly speaking, we anticipate the resulting impacts on adaptive capacity to be conditioned by any strategic role women may play in supporting sustainable adaptation behaviour. For these reasons, we not only expect gender to shape adaptation behaviour at firm-level, but we also expect SMEs that are female-owned/led to have more limited adaptive capacity.

5.3 Research Design and Methodology

The adoption of sustainable and unsustainable adaptation strategies by SMEs is determined by their exposure to climatic events in addition to their respective adaptive capacity, with adaptive capacity being represented by a set of internal firm characteristics and external business environment (Crick, Eskander et al., 2018). We built on this premise to investigate how female representations in ownership and management affect the adoption of adaptation strategies by SMEs. In particular, we empirically investigate 1) how female ownership and management of SMEs in Kenya and Senegal correlates with their adoption of sustainable and unsustainable adaptation strategies, and 2) the mitigating effects of training and assistance.

First, in the context of their respective adaptive capacity, exposure to climate events and gender representation, SMEs decide how many sustainable and unsustainable adaptation strategies to adopt. Here, our outcome variables are count variables and therefore are non-negative. While the population regression that we estimate takes the form $E(y|x)$, the linear model $E(y|x) = x\beta$ might not provide the best fit over all values of the explanatory variables. Although the outcome variables are strictly non-negative, there can be values of x such that the predicted value of y can be negative, i.e., $x'\hat{\beta} < 0$. In this case, the ideal approach is to adopt a Poisson regression model that possesses the desirable features especially in count data context. In fact, conditional maximum likelihood estimators are fully efficient if $E(y|x)$ follows Poisson distribution.

Therefore, we fit the following Poisson regression model:

$$y_{id} = exp\left(\beta_1 N_{id} + \beta_2 N_{id}^2 + \beta_3 F_{id} + \beta_4 N_{id} \times F_{id} + \beta_5 N_{id}^2 \times F_{id} + \alpha_d + z_{id}\delta + u_{id}\right) \tag{5.1}$$

where y_{id} represents the number of sustainable (or unsustainable) strategies by the SME i in district d, N_{id} denotes the number of extreme weather events experienced, and F_{id} denotes female representation in management or ownership. α_d are district fixed effects and z_{id} includes a set of controls representing internal firm characteristics and external business environment. We elaborate on these variables in the following section. The error u_{id} is further allowed to be heteroskedastic and correlated within district and distance-to-market groups, a more conservative choice than the usual assumption that the errors u_{id} are all independent and identically distributed.

Since $exp(.)$ is always positive, Equation (5.1) ensures that predicted values for y_{id} will also be positive. We are interested in the joint effect of the estimated coefficients of the interaction terms, i.e., β_4 and β_5, that shows the additional number of adaptation strategies the SMEs experience due to female representation when exposed to climate events.

Using the Poisson distribution necessarily assumes that the conditional variance and mean are equal. That is, $Var(y|x) = E(y|x)$. However, this Poisson variance assumption of the variance-mean equality often does not hold. Table 5.A.3, in the Appendix, reports the results of the test for overdispersion, concluding that our main specification does not suffer from it. Nonetheless, we also report negative binomial and zero-inflated Poisson regression results as robustness tests that would address this potential overdispersion problem, if any.

We next investigate the potential mitigating or exaggerating effects of financial barriers and assistance. Both these variables are included in z. For this purpose, we include additional interactions to Equation 5.1 according to

$$y_{id} = exp(\beta_1 N_{id} + \beta_2 N_{id}^2 + \beta_3 F_{id} + \beta_4 N_{id} \times F_{id} + \beta_5 N_{id}^2 \times F_{id} + \gamma_1 N_{id} \times M_i + \gamma_2 N_{id}^2 \times M_{id} + \gamma_3 F_i \times M_{id} + \gamma_4 N_{id} \times F_i \times M_{id} + \gamma_5 N_{id}^2 \times F_{id} \times M_{id} + \alpha_d + z_{id}\delta + u_{id}). \tag{5.2}$$

Here in Equation (5.2), γ_3, γ_4 and γ_5 denote the mitigating or exaggerating effects of financial barriers or assistance (M_i). All other variables and estimation strategy are as described for Equation (5.1).

5.3.1 Data

We use the dataset collected in 2016 by Crick, Eskander et al. (2018) that comprises 325 SMEs. The dataset covers the Senegalese regions of Louga, Saint Louis and Kaolack and the county of Laikipia in Kenya. These are semi-arid regions, which can be defined as areas where annual rainfall is generally between 500 and 800mm (Mongi et al., 2010). These regions are experiencing high climate variability and extremes, which are expected to increase in the coming decades IPCC (2014). Research has found that semi-arid lands in Kenya (Marigi et al., 2016; J. O. Ouma et al., 2018) and Senegal (Wade et al., 2015) are experiencing decreasing annual rainfall and steady warming patterns. Our data records self-reported weather extremes that SMEs experienced in the previous five years, and shows that SMEs in the sample are mostly affected by drought. Other types of events in the dataset are flooding; extreme rainfall events and storms; extreme heat; extreme cold; and extreme wind or dust storms. Diop et al. (2022) and Agol et al. (2023) interviewed women entrepreneurs in some of these regions, and found consistent overlap between the events identified in the survey and those that the women interviewed highlighted as the most severe events affecting their businesses.

For our analysis, we restrict the sample to relatively homogenous SMEs in terms of size, exposure to climate events, and ownership structures. We exclude SMEs that were exposed to more than 8 extreme events in the previous five years (this only removes one outlier), and/or have more than 10 employees and/or more than 5 owners. The final estimating sample consists of 205 SMEs: 125 in Kenya and 80 in Senegal, distributed among 10 districts. Although the World Bank definition for small companies is 20 or less employees, it is common practice in the literature to restrict this to 10 employees (e.g., Brixiová et al., 2020). Given the diversity in our original sample, these changes are designed to make the sample within the dataset more comparable, since medium firms have quite different profiles in terms of physical capital, adaptive capacity, etc. Moreover, as discussed, micro and small enterprises represent a significant portion of total employment in Africa (Dougherty-Choux et al., 2015), while medium and large enterprises represent a minority, which makes smaller SMEs more representative of the African business landscape. Small businesses tend to have lower barriers to entry, operate in local markets and make entrepreneurship more accessible to women, youth, and other disadvantaged groups. Therefore, a focus on adaptation in smaller businesses is also more relevant to inform action designed to enhance climate resilient inclusive employment and economic growth agendas, as set out under the Sustainable Development Goals (SDG8 on Decent Work and Economic Employment; SDG 10 on Reduced Inequalities; and SDG13 on Climate Action).

5.3.2 Variables

Following Crick, Eskander et al. (2018), we categorize adaptation strategies into sustainable and unsustainable strategies. Sustainable strategies are defined as those which are aimed at business preservation and reduction of the risk and negative impacts of extreme climate shocks. We classify uptake of loans and insurance, switching to different commodities or crops, trading or farming additional commodities or crops, and switching to different varieties of the same commodities or crops as sustainable adaptation strategies. On the other hand, unsustainable strategies are defined as actions that, taken as a response to an extreme climate event, help businesses to cope with an event, but result in a temporary or permanent contraction in business activity. These actions include reducing the number of employees, selling productive assets, mortgaging assets, and selling assets at lower prices. We then define our outcome variables for Equation (5.1) as the number of these sustainable and unsustainable strategies adopted by SMEs. Table 5.A.1 reports the average number of sustainable and unsustainable strategies adopted by SMEs in our sample.

Surveyed firms report the number of extreme weather events that they have experienced in the previous five years. Examples of such events include drought, flood, extreme rainfall, storms, extreme heat, extreme cold, and extreme wind or dust storms. Equations (5.1) and (5.2) include the squared number of climatic extremes to control for the potential nonlinearity in the relationship between adaptation and exposure. Most SMEs experienced between 0 and 2 extreme events (76.1%), with firms with and without female representation experiencing a similar average number of events as shown in Table 5.1.

To model the representation of women in the ownership and management of the SME, we construct a variable that takes the value of 1 if there is at least one female owner, or if the main manager of the firm is a woman, and 0 otherwise. The maximum number of owners in our sample is 5. Therefore, according to this definition, a firm has female representation if either at least 20% of owners are female, or the firm is managed by a woman. This definition is consistent with the common measures of gender representation

Table 5.1: Summary Statistics

Variables	Description	Representation		
		No female	Female	
		[1]	[2]	[2] - [1]
Nº of events	Nº of extreme events encountered by the surveyed firms	1.806 (1.307)	1.755 (1.459)	-0.051 [0.193]
Nº of sustainable practices	Nº of sustainable adaptation practices adopted by the surveyed firms	0.777 (1.238)	1.127 (1.318)	0.351* [0.178]
Nº of unsustainable practices	Nº of unsustainable adaptation practices adopted by the surveyed firms	0.291 (0.636)	0.510 (0.865)	0.219** [0.106]
Financial barriers	If the firm encounters any financial barriers, 0 if otherwise	0.969 (0.461)	0.843 (0.365)	0.144** [0.058]
Assistance	If the firm receives any financial assistance	0.359 (0.482)	0.598 (0.493)	0.239** [0.068]
Training	If the owners receive any training	0.485 (0.502)	0.559 (0.499)	0.073 [0.070]
Membership	If the firm owners have any membership to professional or gender-specific organization	0.524 (0.502)	0.539 (0.501)	0.014 [0.070]
Market distance	Distance to market (in kilometers)	5.794 (8.479)	4.750 (7.094)	-1.044 [1.096]
No secondary education	No owner had secondary education or higher	0.485 (0.502)	0.275 (0.448)	-0.210*** [0.066]
Observations		103	102	

Notes: We restrict the summary statistics to our estimating sample of 205 micro and small firms with 10 or less employees, 5 or less owners, and 8 or less extreme weather events. Standard deviations are reported in parentheses, whereas standard errors are reported in brackets. Female representation implies having at least one female owner or manager. *** p<0.01, ** p<0.05, * p<0.1.

in SMEs[2] and allows us to have a balanced sample, where around half of the SMEs in the sample have female representation in their leadership.

The vector of controls includes different indicators of adaptive capacity such as financial barriers, assistance, training, membership of a professional or gender-specific organisation, and market distance. These are all factors that literature has acknowledged as being not only key for adaptive capacity (Crick, Gannon et al., 2018), but that are also shaped by gender dynamics and socio-cultural roles of men and women in society (Gannon, Pettinotti et al., 2022).

[2] such as the gender of the top manager (Ayalew et al., 2020; TM and Joseph, 2021), presence of at least one female owner or manager (Brixiová et al., 2020)

Financial barriers take the value of 1 in the dataset if the firm reported encountering any financial barriers when exposed to climate events, and 0 if not. Examples of such financial barriers include not being able to access sufficient, or any, finance, including access to finance having a condition that the business could not satisfy, like a requirement for collateral.

SMEs were asked whether they had received support of any kind (financial, material, technical, or other) in the previous five years from a set of possible sources (national government, local administrations, insurance companies, NGOs, family and friends, and other businesses) to deal with the impact of extreme weather events. Our assistance variable takes the value of 1 if the SME has received at least one kind of support from at least one source, and 0 otherwise. Table 5.A.1 shows that almost half of the firms in our sample (47%) have received some kind of support.

Surveyed SMEs report whether any owner or manager received any relevant professional or vocational training since starting with this SME. The dummy variable, training, is denoted as 1 if the business has received any relevant training, and 0 if not. Similarly, the membership variable takes the value of 1 if the firm owners have any membership to a professional or gender-specific organization, and 0 if not. Finally, market distance measures the reported distance, in kilometres, from the closest market.

5.4 Results and Findings

5.4.1 Results

Our analysis combines two separate factors affecting adaptation choices: the number of extreme events that each SME experiences, and female representation in the firm. Columns (1) and (2) of Table 5.2 present results where the roles of these variables are investigated separately after controlling for common district characteristics.

Table 5.2: Initial Results

	Baseline Results		With Covariates		Main results	
Variables	Sustainable (1)	Unsustainable (2)	Sustainable (3)	Unsustainable (4)	Sustainable (5)	Unsustainable (6)
Nº of events	1.467***	0.588***	1.422***	0.696***	2.329**	0.009
	(0.212)	(0.225)	(0.285)	(0.209)	(0.973)	(0.330)
Nº of events^2	-0.264***	-0.075	-0.252***	-0.098**	-0.504**	0.028
	(0.043)	(0.047)	(0.057)	(0.041)	(0.237)	(0.051)
Female rep.	0.472***	0.591*	0.127	-0.208	0.729	-1.465***
	(0.169)	(0.325)	(0.178)	(0.301)	(0.943)	(0.462)
Female rep. × Nº of events					-1.037	1.329***
					(1.012)	(0.390)
Female rep. × Nº of events^2					0.294	-0.246***
					(0.243)	(0.062)
Financial barriers			-0.244	0.683	-0.299	0.601
			(0.319)	(0.517)	(0.328)	(0.535)
Assistance			0.530***	0.093	0.527***	0.122
			(0.195)	(0.307)	(0.197)	(0.334)
Training			0.429**	0.296	0.437**	0.335
			(0.168)	(0.277)	(0.185)	(0.271)
Membership			-0.255*	-0.114	-0.236*	-0.077
			(0.153)	(0.241)	(0.142)	(0.225)
Market distance			0.028***	0.059***	0.028***	0.060***
			(0.010)	(0.009)	(0.010)	(0.010)
No secondary education			0.012	0.414	-0.035	0.383
			(0.228)	(0.293)	(0.238)	(0.292)
Constant	-1.754***	-1.968***	-2.032***	-4.883***	-2.631***	-4.194***
	(0.306)	(0.381)	(0.499)	(0.427)	(0.851)	(0.464)
Observations	205	205	204	204	204	204
District FE	YES	YES	YES	YES	YES	YES

Notes: Poisson regression coefficients are estimated according to Equation (5.1). The dependent variables are the number of sustainable and unsustainable adaptation strategies as reported by the SMEs. Robust standard errors clustered at district by distance-to-market group are reported in parentheses.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

First, the baseline results show that as SMEs experience more extreme weather events, like floods or droughts, they change the way they adapt or respond to these challenges. Interestingly, there is an inverse U-shaped relationship between the number of extreme events and how much businesses engage in both sustainable and unsustainable adaptation practices. These findings are supported by Crick, Eskander et al. (2018). Secondly, and once we control for covariates, we see that female leadership representation by itself is not a significant explanatory variable. As discussed in Section 5.2, literature suggests that gender and exposure can influence adaptation choices and capacities and also that exposure can influence by gender dynamics. To know the extent to which gender plays a role in adaptation choices, in a context of varying exposure, our main econometric specification interacts these two variables ("Female representation" and "Number of events"). This allows us to estimate the difference female representation makes in the context of exposure to various numbers of events. All the following results control for common district characteristics, and various SME-level variables, including experience of financial barriers, participation in assistance or training programs, membership of a professional or gender-specific organization, distance to the closest market, and whether all owners have an educational level below secondary education.

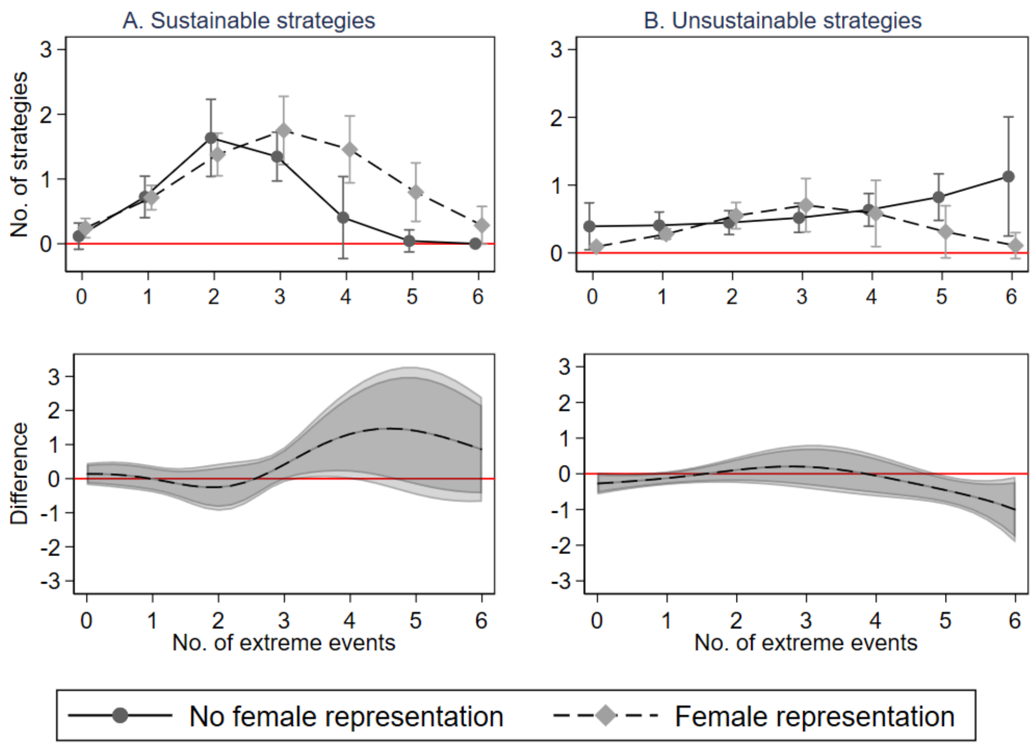
5.4.2 Adaptation Behaviour and Gender Representation in SMEs

Table 5.2 also reports the Poisson regression results according to our main specification in Equation (5.1). The dependent variables are the number of sustainable and unsustainable adaptation strategies a business reported having adopted. Although we report all the coefficients, we focus the following discussion on our coefficients of interest. Specifically, we are interested in understanding how gender representation in the SME influences adaptation choices in the context of varying exposure to extreme events. We use standard errors clustered at the district by distance-to-market level (39 clusters) in all the specifications.

Our main results show that in the absence of any exposure to extreme events, SMEs with female representation and no exposure to extreme events are less likely to undertake unsustainable adaptation strategies than businesses without female representation. The coefficients are visible in Table 5.2, but the marginal effects, which are visually easier to interpret, can be seen in Figure 5.1. This gendered effect does not hold, however, when firms are exposed to an increasing number of extreme events and businesses with female representation become similarly likely to adopt unsustainable adaptation strategies to cope with climate shocks as businesses without female representation. Finally, this changes again when firms are faced with 5 or more extreme events. Here results again show that SMEs with female representation adopt a lower number of unsustainable strategies than firms without female representation when faced with 5 or more extreme events (see Table 5.2, Figure 5.1).

Considering next the effects of gender and exposure to extreme events on the adoption of *sustainable* adaptation strategies, in most cases our main results do not signal clear, statistically significant gendered differences in the adoption of sustainable adaptation strategies. However, among firms that have experienced 3-4 extreme events, we do find that women-led SMEs are more likely to adopt sustainable adoption strategies.

Figure 5.1: Marginal Effects of Gender



Notes: Panels A and B show the number of sustainable and unsustainable adaptation strategies, respectively, that the SMEs with and without female representations have adopted, with their 95% CI. The shaded area represents the 90 and 95% CI of the difference between marginal effects of gender representation. Standard errors are robust and clustered at the district by distance-to-market group.

To test the strength of our results, we first considered alternative estimation strategies for Equation (5.1). Table 5.A.5, within the Appendix, reports results for negative binomial and zero-inflated Poisson regression models. Table 5.A.4 and Figure 5.A.3, also in the Appendix, report the results and marginal effects with plain OLS. These results are consistent with our main results in Table 5.2. Additionally, we considered a specification that does not assume a functional form, but models the number of extreme weather events as 3 categorical groups (0-1, 2-3, and 4-6). This more flexible non-parametric approach also yielded similar results to our main specification (see Appendix Figure 5.A.2). We therefore conclude that our results are robust to alternative specifications.

As an additional robustness test, we plot countfit graphs for both the number of sustainable and unsustainable adaptation strategies for the estimated results according to Equation (5.1). As shown in Figure 5.A.1, in the Appendix, each of the specifications predicts the actual counts almost perfectly, suggesting that our results are not biased by the presence of outliers.

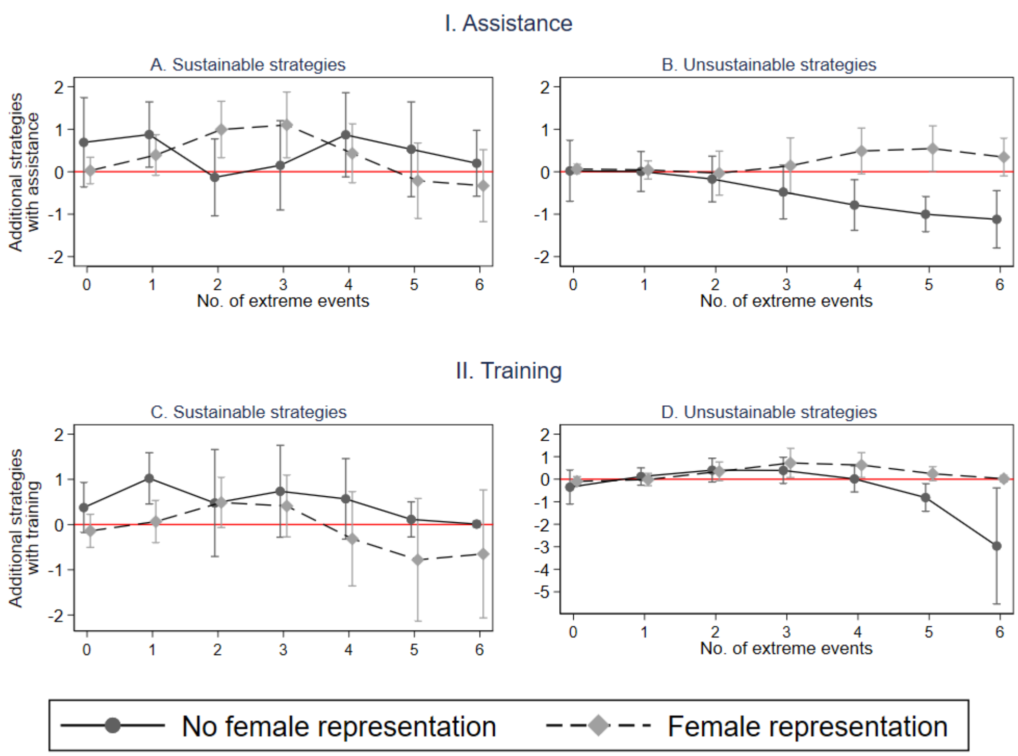
Finally, we consider the possibility that the SMEs make simultaneous decisions about adopting both sustainable and unsustainable adaptation strategies. This is modelled using seemingly unrelated regression (Zellner, 1962), which estimates Equation 5.1 for sustainable and unsustainable strategies jointly. The results, reported in Table 5.A.5 in the Appendix, are consistent with our main results in Table 5.2 and thus show that our results reported above are robust to this change in model assumption.

5.4.3 Mitigating Effects

We analyse whether having assistance or training affects the relationship between the adoption of adaptation strategies and the exposure to extreme events. We do this according to the econometric specification in Equation (5.2) and report the results in Table 5.3. To ease interpretation, Figure 5.2 calculates the marginal effects and reports the number of *additional* sustainable or unsustainable strategies applied by SMEs that received assistance or training (relative to those that have not), by the number of extreme events experienced, separately for those with or without female representation.

From Figure 5.2.I. we can see that female-led SMEs who were affected by between 2 and 3 extreme weather events (most of our sample) and received assistance to overcome these adversities, adopted a higher number of sustainable strategies than those that did not receive this assistance. On the other hand, among female-led SMEs that received assistance, only those that faced 5 or 6 events adopted a higher number of unsustainable

Figure 5.2: Marginal Effects of Assistance and Training



Notes: This shows the number of additional sustainable (A, C) and unsustainable (B, D) adaptation strategies that SMEs with assistance (Panel I) or training (Panel II) report relative to those without assistance or training. This is calculated separately for SMEs with and without female representations. Standard errors are robust and clustered at the district by distance-to-market group. Bars represent the 95% CI.

strategies, than those that did not receive assistance. For the case of non-female-led SMEs, receiving assistance has no effect on adoption of sustainable strategies among firms within our dataset, while there is a significant drop in unsustainable strategies when faced with 3 or more extreme weather events.

Figure 5.2.II. describes the differences between firms with or without training, again, separately for those with or without female representation. Regarding subfigure C, we find that SMEs without female representation seem to have a higher adoption of sustainable strategies when faced with only 1 extreme weather event. We find no statistical differences for SMEs with female representation. On the other hand, subfigure D shows that having training is linked with adopting a higher number of unsustainable

Table 5.3: Mitigating Effects of Assistance and Training

	Role of *Assistance*		Role of *Training*	
Variables	Sustainable (1)	Unsustainable (2)	Sustainable (3)	Unsustainable (4)
Nº of events	4.301***	0.436	6.008***	-0.642
	(0.789)	(0.426)	(2.309)	(0.647)
Nº of events^2	-0.909***	-0.037	-1.335**	0.162
	(0.180)	(0.068)	(0.571)	(0.100)
Female rep.	2.932***	-1.781*	5.126**	-1.346
	(1.027)	(1.003)	(2.222)	(0.854)
Female rep. × Nº of events	-3.347***	1.570	-5.157**	1.575*
	(0.879)	(1.092)	(2.388)	(0.809)
Female rep. × Nº of events^2	0.770***	-0.347*	1.216**	-0.338***
	(0.197)	(0.210)	(0.592)	(0.109)
Financial barriers	-0.230	0.636	-0.427	0.384
	(0.326)	(0.556)	(0.324)	(0.561)
Assistance	4.073***	0.166	0.535***	0.133
	(1.246)	(1.154)	(0.190)	(0.314)
Training	0.479***	0.358	5.198**	-0.613
	(0.182)	(0.274)	(2.096)	(0.792)
Membership	-0.241*	-0.134	-0.226	-0.198
	(0.135)	(0.240)	(0.163)	(0.213)
Market distance	0.032***	0.061***	0.030***	0.063***
	(0.009)	(0.010)	(0.009)	(0.011)
No secondary education	-0.007	0.372	-0.130	0.581**
	(0.241)	(0.278)	(0.246)	(0.249)
Assistance × Nº of events	-3.584***	-0.049		
	(1.099)	(1.059)		
Assistance × Nº of events^2	0.754***	-0.118		
	(0.238)	(0.234)		
Female rep. × Assistance	-3.960***	0.752		
	(1.205)	(1.542)		
Assistance × Nº of events × Female rep.	4.180***	-0.864		
	(0.998)	(1.512)		
Assistance × Nº of events^2 × Female rep.	-0.889***	0.350		
	(0.225)	(0.324)		
Training × Nº of events			-4.435*	1.532**
			(2.335)	(0.760)
Training × Nº of events^2			0.971	-0.341***
			(0.591)	(0.132)
Female rep. × Training			-5.764***	-0.157
			(2.235)	(1.332)
Training × Nº of events × Female rep.			5.279**	-0.699
			(2.501)	(0.996)
Training × Nº of events^2 × Female rep.			-1.164*	0.255*
			(0.640)	(0.143)
Constant	-4.765***	-4.417***	-6.162***	-3.932***
	(0.967)	(0.747)	(1.957)	(0.607)
Observations	204	204	204	204
District FE	YES	YES	YES	YES

Notes: Poisson regression coefficients are estimated according to Equation (5.2). The dependent variables are the number of sustainable and unsustainable adaptation strategies as reported by the SMEs. Robust standard errors clustered at district by distance-to-market group are reported in parentheses.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

strategies for those SMEs that faced 2-4 extreme weather events. This is similar for firms with and without female representation, but only significant for those with it. Finally, we find that training reduces the overall number of adaptation strategies adopted among SMEs without a female as owner or manager that faced 5 or 6 extreme weather events.

5.5 Discussion

The results above suggest that female representation in the ownership or management of small businesses in our sample shapes firm-level adaptation behaviour. This section discusses these findings in the context of the broader literature on gender in business and climate change adaptation to consider how gender-specific vulnerabilities and capabilities shape sustainable adaptation decision-making practices in small and medium-sized enterprises in Africa. Specifically, we propose an interpretation of the main results (seen in Table 5.2 and Figure 5.1), in the context of literature that depicts women in Africa as both particularly vulnerable to climate change and particularly valuable for sustainable adaptation. These insights are valuable for understanding the intersection of gender dynamics, development, and environmental economics, especially in the context of developing economies like Senegal and Kenya.

We preface this discussion with recognition that, despite the robustness tests carried out within our analysis, there are various statistical and epistemological reasons why our results need to be interpreted cautiously. First, we are limited to a relatively low number of observations, which constraints the precision of our estimates and which, although well distributed geographically (Crick, Eskander et al., 2018), are not a representative sample of SMEs in Senegal and Kenya. This implies that although the results apply to these observations, the average in the population of SMEs in these two countries, sub-Saharan Africa, or other developing countries might be different. Secondly, we do not aim for our estimates to be interpreted as causal relationships. In other words, the differences we find are not representative of the change we would expect if an individual firm exogenously started (or stopped) having female representation in its ownership or management, but the current differences between both groups after controlling for various internal business characteristics and external business environment conditions.

Third, we use self-reported exposure to extreme events and adaptation strategies. This allows individual firms to report the same physical event or business action differently based on their exposure and perceptions of it. Notably, this can also be seen as a strength, as it lets us directly understand how SMEs react to their actual perception of climate risk, and the number of strategies implemented that have been sufficiently relevant to be reported.

Despite these limitations, results in this study back a number of potential hypotheses that suggest adaptation support for women entrepreneurs in development programming could serve as a fruitful avenue for upscaling climate resilience within African economies and which warrant additional research.

Our results show that, after exposure to three extreme events, businesses in our dataset with female representation in their management and ownership are more likely to adopt sustainable adaptation behaviours, than firms without female representation. This finding aligns with suggestions in wider literatures that women's roles in ensuring family livelihoods, or their experiences of overcoming gender-based barriers in entrepreneurship, may make women more likely to adopt a long-term perspective in their adaptation preferences (Djoudi and Brockhaus, 2011). However, this result is perhaps surprising in the context of the literature on differential vulnerability to climate change experienced by female entrepreneurs within the business environment. From this literature, discussed in Section 5.2.1, gender determinants of vulnerability can be associated with characteristics of female-owned/led SMEs, which are not specified as control factors, and one starting assumption for this work was that women-led businesses are likely to face additional obstacles to accessing resources for adaptation.

The unprecedented political commitment at national and international levels to promote gender equality, and the adoption of gender mainstreaming policies, is one factor which the literature suggests could be interacting with this result. In this landscape, development and adaptation institutions and programmes often design targeted actions that seek to support gender equality. These actions could have led to some progress in mitigating gender barriers to adaptation in the business environment. Yet, as summarised in Section 5.2.1, the extent of any such progress still appears very limited relative to the differential disadvantage (C. Moser and A. Moser, 2005; Nhamo, 2014; Sweetman, 2012) and literature suggests that there is a significant ongoing gender gap in access to a range of enablers of business level adaptation. This is especially the case in the countries considered in this study, where the integration of gender in adaptation policy is still limited (Crick, Diop et al., 2016). Thus, gender-responsive development programming is unlikely to entirely explain the increased propensity for sustainable adaptation among SMEs with female representation observed among our sample.

This result may also reflect the characteristics of our dataset and analytical approach. It is notable that, compared to unsustainable adaptation strategies (such as selling assets), many sustainable adaptation strategies (such as switching to different commodities or crops), may not necessitate such large financial resources or business assets to implement. Sustainable adaptation strategies, as categorised within our study, may therefore be more likely to be adopted by innovative, dynamic, or motivated business leaders, even if they do not have access to a larger resource base.

Additionally, it is notable that it is not possible to distinguish female-only led businesses through any other publicly available dataset on adaptation in small businesses that we were able to find and access for this study. Our metric for capturing female representation within a business (at least one female owner out of a maximum of five and/or a female manager), therefore, does not exclude the opportunity for a business to also experience the gendered advantage associated with having males within the business environment. Greater use of sustainable adaptation behaviour among businesses with female representation within the business leadership may thus reflect partly an advantage obtained through many firms within this group having a gender-diverse leadership team.

We also find that businesses within our sample with female representation in their leadership may initially (without exposure to extreme climate events) be more resistant to adopting unsustainable adaptation strategies. Recall that unsustainable coping strategies, as categorised in this paper, are likely to reduce the resources available to a business to cope with future shocks and thus have the potential to undermine longer-term business resilience. This finding, therefore, provides further opportunity to advance the hypothesis that women — or perhaps gender-diverse leadership teams — may prioritise business adaptation strategies that support the stability, sustainability, and longer-term health of the business.

Our data, nevertheless, suggests that any differential propensity to avoid adopting adaptation strategies that temporarily or permanently contract business activity that may exist among businesses with female leadership does not persist once they become exposed to extreme climate events. This is to say that our results suggest that businesses with female representation in their leadership may be similarly likely to resort to coping strategies, such as selling or mortgaging assets or reducing their workforce when they face the pressures of climate shocks. This is unsurprising in a context where adaptive capacity, and resources to support adaptation, are generally highly constrained.

We find additional gender differentiation in the use of unsustainable adaptation responses, however, when businesses are exposed to five or more extreme events. For these businesses, those with female leadership adopt a lower number of unsustainable strategies than firms without female leadership. This finding could be interpreted in line with literature on gender differential vulnerability since unsustainable *coping* strategies such as selling or mortgaging assets or reducing numbers of employees require a business to have employees or assets to scale back or sell in the first place. Unsustainable adaptation can therefore also be interpreted as an indication of coping capacity. Thus, in this context, our results could indicate lower coping capacity among businesses within our sample that have female representation within their management and/or ownership structure.

Accessing training on climate-smart practices can support the adoption of new adaptation practices that build business resilience (Clarkson et al., 2019; Gannon, Conway et al., 2018; Kom et al., 2020). Entrepreneurs also see training as a way to build social networks, which are considered key for adaptation (Smucker and Wangui, 2016; Twyman et al., 2014). The results on the effects of assistance and training indicate that assisting female-led SMEs seems to be more effective in increasing the number of adaptation strategies that a firm is likely to adopt, than for SMEs without female representation. This is generally true for both sustainable and unsustainable adaptation strategies[3]. It could be that female entrepreneurs are more receptive to assistance, or that women have more to gain from assistance, because of their disadvantage in other aspects of running a business. At a low number of extreme events (0-1) both assistance and training also increase the number of sustainable strategies that firms that do not have women owners or managers adopt, but this positive effect of assistance and training disappears as the number of extreme events increases. When faced with 2 or 3 extreme weather events in the last 5 years, female-led/managed SMEs that report receiving assistance increase their implementation of sustainable adaptation practices. This is not the case for their only-male led/managed counterparts. A similar result emerges for unsustainable strategies when an SME faced large number of extreme weather events (4-6), with female led/managed SMEs with training or assistance adopting a higher number on unsustainable strategies their only-male led/managed counterparts.

These findings validate the importance of policies that enhance adaptation practices (Cohen et al., 2019), as well as policies that to foster sustainable adaptation specifically target SMEs with female gender representation. Gender mainstreaming in the form of providing assistance and training to female-led or managed SMEs can be a way to upscale sustainable adaptation. These conclusions are in line with the extensive literature advocating for the importance of gender mainstreaming. However, as literature also suggests, gender mainstreaming by itself does not automatically lead to efficient and inclusive policies. Therefore, the quality and content of policy actions is also important. Moreover, our findings present innovative relevance by highlighting the value of assistance that supports adaptation not only at the individual or household level, but at the SME level as well.

[3] The only case this does not apply is for the case of SMEs that faced 5 or 6 extreme weather events (close to the 95th percentile) and received training, and thus are not very representative of the average effect.

This study faces several data limitations due to the context of SMEs in semi-arid regions of South Saharan Africa. Data gathering on adaptation practices and extreme weather impacts is costly, which leads to our use of secondary data from Crick, Eskander et al. (2018). This data explores the necessary outcomes in sufficient depth, but has a limited number of firms in the cross-section, with a limited geographic scope of Kenya and Senegal. Furthermore, as detailed above, the study leans on reported extreme events and adaptation strategies to study the adaptation from the SMEs' perspective, offering valuable insights into their perceptions and responses. However, this approach serves as a relevant but imperfect proxy for the actual occurrence of extreme events and adopted strategies and does not allow us to adopt a more ambitious causal econometric strategy. These challenges are common in the literature. We therefore consider imperative that provision for gender differentiation in research results is put at the heart of the design of future research and survey tools used in the collection of broader datasets on adaptation and entrepreneurship in Africa, such as the World Bank Enterprise Survey. Provision for gender consideration in panel surveys will allow for a more comprehensive analysis of the mechanisms, causality, and generalizability of SME adaptation to weather conditions, but at the moment these datasets rarely exist.

5.6 Conclusions

SMEs with female representation in Senegal and Kenya are less likely to implement unsustainable adaptation when they are either not exposed, or exposed to a very large number of extreme weather events. Nevertheless, when faced with a relatively high number of disasters, female-led SMEs adopt more sustainable adaptation practices than firms without female representation in their ownership or management teams. We associate this increased adoption of sustainable adaptation practices with a higher propensity of female-led SMEs to engage in sustainable behaviour.

Assistance and training appear to increase the likelihood of firms with female gender representation undertaking more adaptation behaviours than firms without female representation when faced with an increasing number of weather extremes. These results provide some evidence in support of the hypothesis that women entrepreneurs may be more inclined to prioritise a long-term perspective in their business adaptation decision-making. Government policies aimed at mitigating the adversities of extreme weather events should incorporate training programs to benefit the entrepreneurs, especially the female entrepreneurs.

Our methodology, though robust in its approach to capturing gender dynamics in SMEs, has its limitations. In particular, we only focused on selected semi-arid regions of Kenya and Senegal, whereas a study should ideally encompass a larger geography. A more comprehensive and representative dataset would allow for a better identification of the average adaptation response to extreme weather events. In this regard, gender-related questions should be incorporated in large country-level surveys such as the enterprise survey conducted by the World Bank.

Despite this limitation, our findings were able to shed lights on the importance of assistance and training as two important mitigation strategies especially for female entrepreneurs in the face of an increasing number of extreme weather events. Policy implications of our results can likely be generalized for other developing countries faced with similar gender norms and climate and weather conditions. Although we have taken care to outline the limits of our analysis in this paper, we suggest that our results are persuasive enough to suggest that these hypotheses all warrant further investigation in future research.

5.A Appendix: Additional Tables and Figures

Table 5.A.1: Summary Statistics — Full Sample

Variables	Description	Mean	S.D.	Min.	Max.
Nº of events	Nº of extreme events encountered by the surveyed firms	1.78	1.38	0	6
Nº of sustainable practices	Nº of sustainable adaptation practices adopted by the surveyed firms	0.95	1.28	0	4
Nº of unsustainable practices	Nº of unsustainable adaptation practices adopted by the surveyed firms	0.40	0.76	0	3
Female rep.	If there is female representation of females in ownership and management of the firm	0.49	0.50	0	1
Financial barriers	If the firm encounters any financial barriers, 0 if otherwise	0.77	0.42	0	1
Assistance	If the firm receives any assistance	0.47	0.50	0	1
Training	If the owners receive any training	0.52	0.50	0	1
Membership	If the firm owners have any membership to professional or gender-specific organization	0.53	0.50	0	1
Market distance	Distance to market (in kilometers)	5.27	7.82	0	42
No secondary education	No owner had secondary education or higher	0.38	0.48	0	1

Notes: We restrict the summary statistics to our estimating sample of 205 micro and small firms with 10 or less employees, 5 or less owners and 8 or less extreme weather events.

Table 5.A.2: Different Adaptation Practices by Female Representation

Variables	No female representation	Some female representation	Difference [Female - Male]
A. *Sustainable adaptation strategies*			
Did you get a loan?	0.078	0.225	0.148***
	(0.269)	(0.420)	[0.049]
Did you get insurance?	0.100	0.010	-0.097**
	(0.310)	(0.099)	[0.032]
Did you switch to a different commodity/good or crop?	0.165	0.245	0.080
	(0.373)	(0.432)	[0.056]
Did you start trading or farming an additional commodity/good or crop?	0.223	0.363	0.139**
	(0.418)	(0.483)	[0.063]
Did you switch to a different variety of the same commodity/good or crop you were operating before?	0.204	0.284	0.080
	(0.405)	(0.453)	[0.060]
B. *Unsustainable adaptation strategies*			
Did you reduce the number of employees?	0.136	0.167	0.031
	(0.344)	(0.375)	[0.050]
Did you sell assets?	0.097	0.196	0.099**
	(0.298)	(0.399)	[0.049]
Did you sell assets at a lower price?	0.058	0.118	0.059
	(0.235)	(0.324)	[0.040]
Did you mortgage/rent out assets?	0	0.029	0.029*
	(0)	(0.170)	[0.017]
No. of Observations	103	102	

Notes: We restrict the summary statistics to our estimating sample of 205 micro and small firms with 10 or less employees, 5 or less owners and 8 or less extreme weather events. There are 102 SMEs with female representations and the remaining 103 SMEs do not have any female representation. Classifications of sustainable and unsustainable adaptation strategies follow Crick, Eskander et al. (2018).
*** p<0.01, ** p<0.05, * p<0.1.

Table 5.A.3: Test for Overdispersion

	Baseline Results		With covariates		Main results	
Variables	Sustainable	Unsustainable	Sustainable	Unsustainable	Sustainable	Unsustainable
N^o of sustainable adaptation practices (est.)	0.394***		0.079		0.052	
	(0.119)		(0.101)		(0.086)	
N^o of unsustainable adaptation practices (est.)		0.799**		0.073		0.036
		(0.331)		(0.190)		(0.128)
Observations	205	205	204	204	204	204
R-squared	0.051	0.028	0.003	0.001	0.002	0.000

Notes: The test of overdispersion is implemented by an auxiliary regression of the generated dependent variable, $(y - \hat{y})^2 - y/\hat{y}$, on estimated dependent variable $\hat{y}$. The dependent variables are the estimated number of sustainable and unsustainable adaptation strategies obtained from Poisson regressions according to Equation (5.1). Statistically insignificant coefficients support equidispersion ($Var(y|x) = E(y|x)$) (null hypothesis) whereas statistically significant support overdispersion ($Var(y|x) > E(y|x)$).
***,** and * represent statistical significance at 1, 5 and 10 percent levels, respectively.

Table 5.A.4: Results with OLS

	Baseline Results		With covariates		Main results	
Variables	Sustainable	Unsustainable	Sustainable	Unsustainable	Sustainable	Unsustainable
Nº of events	1.041***	0.210*	0.887***	0.181*	0.758***	-0.097
	(0.136)	(0.108)	(0.136)	(0.091)	(0.195)	(0.111)
Nº of events^2	-0.180***	-0.025	-0.150***	-0.025	-0.138***	0.027
	(0.024)	(0.023)	(0.021)	(0.021)	(0.030)	(0.022)
Female rep.	0.446***	0.235	0.102	-0.003	-0.189	-0.421**
	(0.164)	(0.154)	(0.163)	(0.148)	(0.246)	(0.171)
Female rep. × Nº of events					0.209	0.526**
					(0.192)	(0.206)
Female rep. × Nº of events^2					-0.017	-0.102***
					(0.035)	(0.036)
Financial barriers			-0.075	0.108	-0.096	0.099
			(0.264)	(0.095)	(0.267)	(0.093)
Assistance			0.589***	0.047	0.597***	0.035
			(0.198)	(0.117)	(0.200)	(0.121)
Training			0.305*	0.064	0.319*	0.083
			(0.171)	(0.109)	(0.171)	(0.108)
Membership			-0.258*	-0.047	-0.239	-0.038
			(0.141)	(0.098)	(0.143)	(0.092)
Market distance			0.030	0.026**	0.030	0.026**
			(0.021)	(0.011)	(0.021)	(0.011)
No secondary education			0.013	0.161	-0.000	0.148
			(0.222)	(0.132)	(0.221)	(0.127)
Constant	-0.210	0.037	-0.385	-0.737**	-0.240	-0.515**
	(0.143)	(0.121)	(0.757)	(0.305)	(0.773)	(0.228)
Observations	205	205	204	204	204	204
R-squared	0.153	0.050	0.324	0.251	0.328	0.275
District FE	YES	YES	YES	YES	YES	YES

Notes: OLS regression coefficients are estimated using a similar specification to Equation (5.1). The dependent variables are the number of sustainable and unsustainable adaptation strategies as reported by the SMEs. Robust standard errors are reported in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Table 5.A.5: Results with Alternative Empirical Specifications

	Negative Binomial		Zero-inflated Poisson		Seemingly Unrelated Regressions	
Variables	Sustainable	Unsustainable	Sustainable	Unsustainable	Sustainable	Unsustainable
Nº of events	2.276**	0.008	1.783***	-0.243	0.758***	-0.0965
	(0.948)	(0.332)	(0.669)	(0.394)	(0.250)	(0.155)
Nº of events^2	-0.489**	0.029	-0.403**	0.094	-0.138***	0.0272
	(0.232)	(0.052)	(0.172)	(0.067)	(0.0461)	(0.0285)
Female rep.	0.635	-1.482***	-0.242	-1.048	-0.242	-1.048
	(0.959)	(0.491)	(0.703)	(0.745)	(0.703)	(0.745)
Female rep. × Nº of events	-0.930	1.339***	-0.406	1.666**	0.209	0.526**
	(1.026)	(0.405)	(0.766)	(0.655)	(0.336)	(0.208)
Female rep. × Nº of events^2	0.269	-0.249***	0.182	-0.327***	-0.0165	-0.102***
	(0.244)	(0.067)	(0.184)	(0.109)	(0.0635)	(0.0393)
Financial barriers	-0.305	0.612	-0.516*	0.506	-0.0957	0.0987
	(0.337)	(0.549)	(0.295)	(0.925)	(0.187)	(0.116)
Assistance	0.540***	0.115	0.569***	0.031	0.597***	0.0351
	(0.199)	(0.340)	(0.176)	(0.384)	(0.172)	(0.106)
Training	0.441**	0.344	0.191	0.404	0.319**	0.0826
	(0.192)	(0.275)	(0.159)	(0.271)	(0.161)	(0.0999)
Membership	-0.244*	-0.086	-0.243*	-0.214	-0.239	-0.0381
	(0.148)	(0.216)	(0.125)	(0.221)	(0.164)	(0.102)
Market distance	0.028***	0.061***	0.025***	0.065**	0.0298***	0.0265***
	(0.010)	(0.011)	(0.008)	(0.027)	(0.0106)	(0.00658)
No secondary education	-0.045	0.389	-0.138	0.450	-0.000239	0.148
	(0.241)	(0.283)	(0.243)	(0.348)	(0.175)	(0.108)
Constant	-2.566***	-4.221***	-0.458*	-1.151	-0.458*	-3.738***
	(0.854)	(0.493)	(0.250)	(0.951)	(0.250)	(0.951)
Observations	204	204	204	204	204	204
R-squared/pseudo R2	0.159	0.219			0.328	0.275
District FE	YES	YES	YES	YES	YES	YES

Notes: Negative Binomial and Zero-inflated Poisson regression coefficients are estimated according to Equation (5.1). Seemingly unrelated regressions consider three-stage least squares regression strategy where both the dependent variables are determined simultaneously and follow the specification in Equation (5.1). The dependent variables are the number of sustainable and unsustainable adaptation strategies as reported by the SMEs. Robust standard errors clustered at district by distance-to-market group are reported in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Table 5.A.6: Results with Revised Classifications

	Baseline Results		With covariates		Main results	
Variables	Sustainable	Unsustainable	Sustainable	Unsustainable	Sustainable	Unsustainable
Nº of events (*)	1.841***	0.843**	1.743***	0.952**	3.166***	0.261
	(0.285)	(0.370)	(0.383)	(0.439)	(0.777)	(0.482)
Nº of events^2 (*)	-0.371***	-0.139	-0.345***	-0.164	-0.728***	-0.020
	(0.066)	(0.099)	(0.088)	(0.108)	(0.175)	(0.108)
Female rep.	0.484***	0.612*	0.105	-0.190	1.317	-1.177**
	(0.169)	(0.321)	(0.175)	(0.302)	(0.877)	(0.570)
Female Rep× Nº of events (*)					-1.798**	1.047**
					(0.838)	(0.529)
Female Rep × Nº of events^2 (*)					0.484**	-0.213*
					(0.189)	(0.119)
Financial barriers			-0.259	0.653	-0.298	0.637
			(0.314)	(0.528)	(0.323)	(0.532)
Assistance			0.550***	0.066	0.553***	0.104
			(0.200)	(0.362)	(0.188)	(0.359)
Training			0.420**	0.298	0.416**	0.308
			(0.175)	(0.274)	(0.188)	(0.273)
Membership			-0.271*	-0.144	-0.243*	-0.135
			(0.151)	(0.230)	(0.137)	(0.214)
Market distance			0.027***	0.059***	0.028***	0.059***
			(0.011)	(0.009)	(0.011)	(0.009)
No secondary education			-0.006	0.392	-0.073	0.403
			(0.230)	(0.287)	(0.244)	(0.281)
Constant	-2.015***	-2.161***	-2.233***	-5.013***	-3.269***	-4.378***
	(0.349)	(0.409)	(0.517)	(0.441)	(0.765)	(0.511)
Observations	205	205	204	204	204	204
District FE	YES	YES	YES	YES	YES	YES

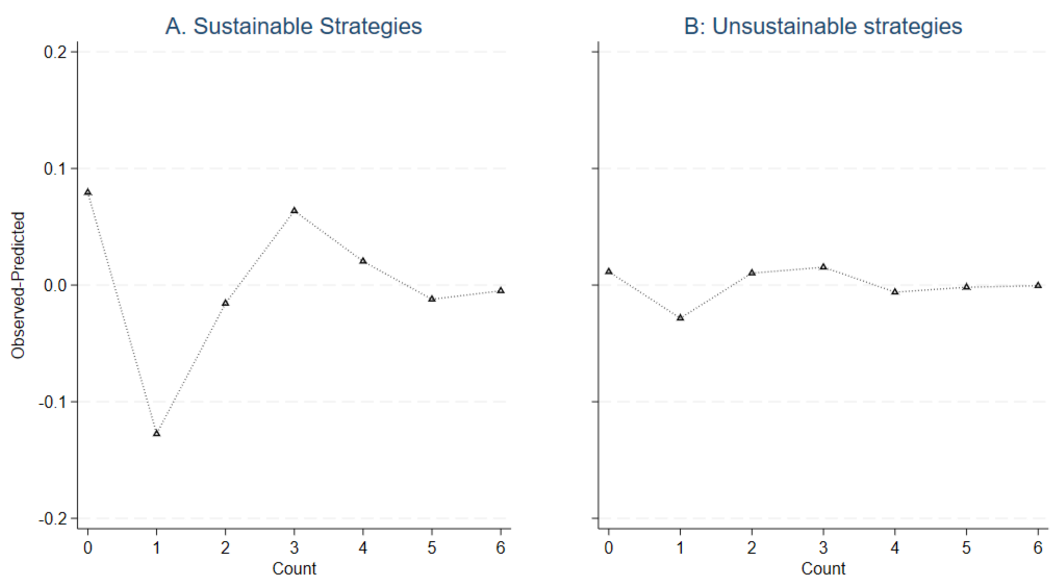
Notes: Poisson regression coefficients are estimated according to Equation (5.1), where more than 3 extreme weather events are classified as 4. The dependent variables are the number of sustainable and unsustainable adaptation strategies as reported by the SMEs. Robust standard errors clustered at district by distance-to-market group are reported in parentheses.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 5.A.7: Results with Revised Gender Definition

	Baseline Results		With covariates		Main results	
Variables	Sustainable	Unsustainable	Sustainable	Unsustainable	Sustainable	Unsustainable
Nº of events	1.462***	0.591***	1.417***	0.708***	1.655***	0.688**
	(0.211)	(0.229)	(0.284)	(0.208)	(0.440)	(0.268)
Nº of events^2	-0.262***	-0.076	-0.251***	-0.100**	-0.335***	-0.089*
	(0.042)	(0.047)	(0.056)	(0.042)	(0.093)	(0.052)
Female Rep.(*)	0.462***	0.617**	0.143	-0.111	0.353	-0.181
	(0.155)	(0.305)	(0.197)	(0.308)	(0.558)	(0.499)
Female Rep.(*) × Nº of events					-0.555	0.190
					(0.478)	(0.509)
Female Rep.(*) × Nº of events^2					0.180*	-0.056
					(0.108)	(0.141)
Financial barriers			-0.243	0.674	-0.278	0.677
			(0.319)	(0.515)	(0.327)	(0.502)
Assistance			0.530***	0.077	0.524***	0.065
			(0.198)	(0.306)	(0.194)	(0.329)
Training			0.424**	0.306	0.454***	0.310
			(0.171)	(0.275)	(0.175)	(0.281)
Membership			-0.260*	-0.110	-0.244	-0.120
			(0.157)	(0.244)	(0.154)	(0.226)
Market distance			0.029***	0.059***	0.030***	0.058***
			(0.010)	(0.010)	(0.010)	(0.010)
No secondary education			0.008	0.416	-0.036	0.417
			(0.228)	(0.295)	(0.224)	(0.294)
Constant	-1.737***	-1.977***	-2.028***	-4.888***	-2.115***	-4.889***
	(0.327)	(0.382)	(0.496)	(0.433)	(0.551)	(0.436)
Observations	205	205	204	204	204	204
District FE	YES	YES	YES	YES	YES	YES

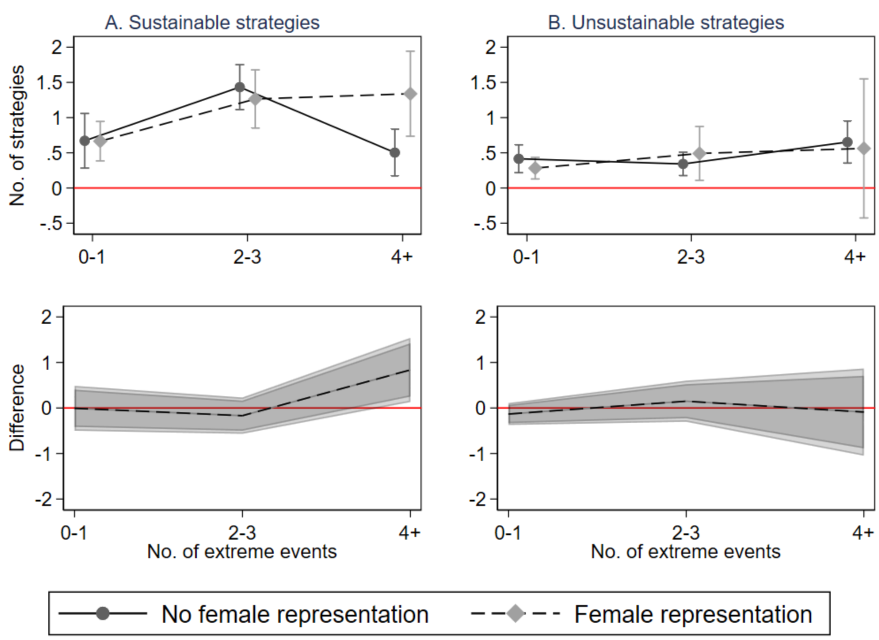
Notes: Poisson regression coefficients are estimated according to Equation (5.1), where female representation has been defined as 1 if any female owners and 0 if no female owners (without considering managers as is the main definition). The dependent variables are the number of sustainable and unsustainable adaptation strategies as reported by the SMEs. Robust standard errors clustered at district by distance-to-market group are reported in parentheses.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Figure 5.A.1: *Countfit* Graphs



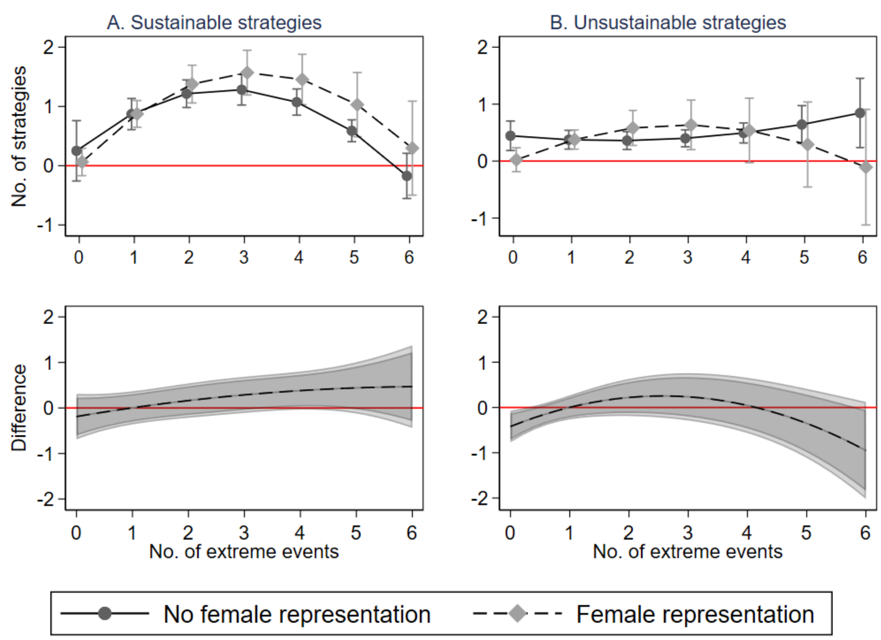
Notes: Countfit graphs plot the residuals for both the sustainable and unsustainable adaptation strategies from main results reported in Table 5.2.

Figure 5.A.2: Marginal Effects of Gender — Non-parametric



Notes: Panels A and B show the number of sustainable and unsustainable adaptation strategies, respectively, that the SMEs with and with female representations have adopted for 3 groups of reported extreme events. Bars represent 95% CI. The shaded area represents the 90 and 95% CI of the difference between marginal effects of gender representation. Standard errors are robust and clustered at the district by distance-to-market group.

Figure 5.A.3: Marginal Effects of Gender — Linear Regression



Notes: Panels A and B show the number of sustainable and unsustainable adaptation strategies, respectively, that the SMEs with and with female representations have adopted for a range of reported extreme events. Bars represent 95% CI. The shaded area represents the 90 and 95% CI of the difference between marginal effects of gender representation. Standard errors are robust and clustered at the district by distance-to-market group.

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