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Essays on Environmental and Energy Challenges

Advisor: Valentina BOSETTI

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Ireri HERNANDEZ CARBALLO
ID number: 3110642

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Introduction

This dissertation contains four chapters at the intersection of public policy and political economy, addressing three interconnected themes: environmental health, the political economy of climate and energy policies, and the labor impacts of decarbonization. Leveraging quasi-experimental methods, survey data, and large geocoded datasets, it offers new insights into the multifaceted challenges of transitioning to a sustainable future.

The first part of this dissertation examines the relationship between air pollution and the COVID-19 pandemic. In collaboration with David Stuckler and Maria Bakola, I conducted a systematic review, published in Environmental Research, that synthesizes evidence on how air pollution influences the spread, severity, and mortality of COVID-19. Our findings indicate a significant link between higher air pollution levels and increased COVID-19 incidence, spread, and mortality. The effects of short-term air pollution exposure on COVID-19 are inconclusive, primarily due to confounding factors and measurement biases in existing studies. Future research should focus on study designs that control for confounding factors, for example by using quasi-experimental and natural experiments, and use measures of COVID-19 that are less likely to be measured with systematic errors, such as excess mortality. We conclude with suggestions for future research on this topic. This review provides important insights for the relationship between the largest worldwide environmental health risk factor and the largest pandemic in recent history, to understand the health implications of air pollution in the context of pandemics.

Having addressed a critical environmental health issue, the dissertation then shifts focus to the socio-economic dimensions of the energy transition. The second part of the dissertation focuses on the energy transition and decarbonization policies more broadly. Avoiding the catastrophic consequences of climate change requires support

for environmental policies and will entail substantial shifts in energy production, consumption and modes of transportation. However, the costs and benefits of reducing fossil fuel usage are likely to be unevenly distributed across members of society and regions. Furthermore, individuals incurring losses from decarbonization regulation might perceive these policies to be unfair, decreasing public support.

A key question I aim to investigate is how individuals form their support around climate and energy policies and whether this is influenced by factors such as energy prices and socio-demographic characteristics, and how these impacts vary geographically. The second and third chapters of my dissertation focus on this. Particularly, I study how energy prices impact the acceptability of environmental policies and furthermore, how the impacts of the energy transition vary across socio-demographic groups and regions.

One of my chapters investigates how support for climate policies is impacted by gasoline prices. In joint work with Matthew R. Sisco, published in Nature Energy, we use weekly retail gasoline prices combined with individual survey data from the International Panel on Climate Change and Air Pollution and the Gallup Poll Social Series to analyze this question. The empirical methods employed exploit time and regional variation in gasoline prices, and we find that policy support for some (but not most) environmental policies decreases with higher fuel prices. Specifically, we find that in weeks where gasoline prices are higher, there is lower support for phasing out of combustion-engine cars and slightly lower support for protecting the environment at the risk of economic growth. Further analysis suggests that this is driven by increased worry about the availability and affordability of energy and worry about the economy and not by decreased concern about the environment. We further find that lower-income individuals and those identifying as more liberal, while more likely to support phasing out gasoline- and diesel-fueled vehicles relative to more conservative and to higher-income individuals, respectively, decrease their support faster with increases in gasoline prices. We don't find any significant association between gas prices and other climate policies surveyed on, such as funding research into renewables, regulating CO2, and requiring companies to pay a carbon tax. Our findings suggest that rising energy prices should be accompanied by access to affordable clean transportation options, especially for lower-income individuals and those with limited transportation alternatives.

Additionally, another of my chapters extends this work to study how shifts in

other energy prices, namely natural gas prices, impact support for climate policies. Shifts in energy prices due to global market dynamics, increased climate variability, or more recently, crises such as COVID-19 and the invasion of Ukraine, are likely to have an impact on household energy expenditures and costs of living. A key question relevant to the political economy of the energy transition is how individuals will react to these shifts in energy prices: will this affect the public acceptance of climate and energy policies, or drive political outcomes such as voting for green, or on the other extreme, populist or right-winged parties? Through what channels will the effect operate (e.g., income and energy expenditures shocks, perceptions of fairness, or trust in the government)? Which regions and social groups are more likely to be affected and respond more? While high energy prices may increase support for policies that make the transition to renewable energy more feasible, support for policies that are perceived to increase energy prices may decrease. The third chapter of my dissertation begins to study this question.

Finally, regions that engage in fossil fuel production or have a substantial amount of employment in energy-intensive industries may be more impacted by environmental policies. To study this, chapter 4, joint with Gian Mallarino and Marco Percoco, focuses on the regional distributional impacts of the clean energy transition by studying the local impacts of the EU ETS on economic outcomes. Environmental policies such as the European Union Emissions Trading System (EU ETS) raise concerns about their impact on employment and competitiveness. Yet, existing EU ETS studies focus on firm-level outcomes during the initial phases of the program. We construct a panel dataset of about 900 European provinces across 2008 to 2020 to assess the effects of a significant policy change in Phase 3 of the EU ETS. Specifically, we investigate how the changes in the free allocation mechanism affected local economic performance, including employment, gross value added (GVA) and productivity per employee (PPE) of European provinces. We employ data on the net change of paid emissions from Phase 2 to Phase 3 to construct a measure of local level exposure at the NUTS-3 level. Using synthetic difference-in-differences, we find that being more exposed to the EU ETS is associated with negative and significant results on employment and GVA of the most impacted industries but not at the overall regional level. Furthermore, we find strong reductions in emissions and no strong evidence of increasing inequality, although with some differences across outcomes in the most impacted sectors.

Chapter 1

The impact of air pollution on COVID-19 incidence, severity, and mortality: A systematic review of studies in Europe and North America

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The Impact of Air Pollution on COVID-19 Incidence, Severity, and Mortality: A Systematic Review of Studies in Europe and North America

Ireri Hernandez Carballo[1,2*], Maria Bakola[3], and David Stuckler[1,4]

[1] – Department of Social and Political Sciences, Bocconi University, Milan, Lombardy, Italy

[2] – RFF-CMCC European Institute of Economics and the Environment, Centro Euro-Mediterraneo sui Cambiamenti Climatici, Milan, Lombardy, Italy

[3] – Research Unit for General Medicine and Primary Health Care, Faculty of Medicine, School of Health Science, University of Ioannina, Ioannina, Greece

[4] – DONDENA Centre for Research on Social Dynamics and Public Policy, Bocconi University, Milan, Lombardy, Italy

* - Corresponding author: Ireri Hernandez Carballo, ireri.hernandez@phd.unibocconi.it, Via Roentgen, 1, 20136, Milan, Lombardy, Italy

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Abstract:

***Background*:** Air pollution is speculated to increase the risks of COVID-19 spread, severity and mortality.

***Objectives*:** We systematically review studies investigating the relationship between air pollution and COVID-19 cases, non-fatal severity and mortality in North America and Europe.

Methods: We searched PubMed, Web of Science, and Scopus for studies investigating the effects of harmful pollutants, including particulate matter with diameter ≤ 2.5 or 10 micrometers ($PM_{2.5}$ or PM_{10}), ozone (O_3), nitrogen dioxide (NO_2), sulfur dioxide (SO_2) and carbon monoxide (CO), on COVID-19 cases, severity, and deaths in Europe and North America through to June 19th, 2021. Articles were included if they quantitatively measured the relationship between exposure to air pollution and COVID-19 health outcomes.

Results: From 2,482 articles screened, we included 116 studies reporting 355 separate pollutant-COVID-19 estimates. Approximately half of all evaluations on incidence were positive and significant associations (52.7%); for mortality the corresponding figure was similar (48.1%), while for non-fatal severity this figure was lower (41.2%). Longer-term exposure to pollutants appeared more likely to be positively associated with COVID-19 incidence (63.8%). Specifically, $PM_{2.5}$, PM_{10}, O_3, NO_2 and CO were most strongly positively associated with COVID-19 incidence, and $PM_{2.5}$ and NO_2 with COVID-19 deaths. All studies were observational and most exhibited high risk of confounding and outcome measurement bias.

Discussion: Air pollution may associate with worse COVID-19 outcomes. Future research is needed to better test the air pollution-COVID-19 hypothesis, particularly using more robust study designs and COVID-19 measures that are less prone to measurement error and by considering co-pollutant interactions.

1. Introduction

Air pollution is the greatest environmental health risk factor worldwide [1]. But has it also contributed to worse COVID-19 outcomes? Evidence that air pollution has systemic effects on cardiopulmonary and respiratory systems has led several researchers to speculate that it could increase COVID-19 incidence, severity, and mortality [2,3].

Air pollution could exacerbate COVID-19 prognosis in multiple ways. Both acute and long-term exposure to air pollution increase susceptibility to and severity of respiratory and cardiovascular diseases by increasing oxidative stress and inflammation [4,5]. Particulate matter can reach the alveolar sacs in the lungs and travel further into the bloodstream, causing an inflammatory response that triggers and exacerbates respiratory diseases, including COVID-19 [6]. Furthermore, other pollutants such as NO_X, O_3, SO_2 and *CO* create oxidative stress, lung damage, and endothelial dysfunction [6]. In addition to weakening the respiratory and immune system, air pollution has been hypothesized to aggravate COVID-19 infection severity through the overexpression of the angiotensin converting enzyme 2 (ACE-2), a coronavirus receptor, on surfaces of the respiratory tract [7].

In Europe and North America, air pollution is particularly harmful to aging and urban populations and continues to harm vulnerable groups. In 2016, more than 400,000 and 70,0000 deaths were attributable to $PM_{2.5}$ and NO_2, respectively, and the urban population of the EU-27 and the UK continue to be exposed to pollution levels exceeding WHO thresholds [8,9]. Likewise, in Canada and in the U.S., three out of 10 people live in areas where ambient air quality standards are not met [10]. Furthermore, around 26% of the population in Europe and 23% of the U.S and Canada population is 60 or older [11]. For the elderly, long-term exposure to even low levels of air pollution can increase the risk of respiratory conditions [12].

It is currently unclear to what extent air pollution is exacerbating significant COVID-19 risks. Since 2020 hundreds of studies have investigated the association of exposure to air pollution on COVID-19. Many of the earlier studies use limited datasets and are prone to several confounding factors, which warrants caution in interpreting these results [13]. While existing prior systematic reviews suggest that air pollution may be linked to COVID-19 outcomes (see Marquès and Domingo [14] for a recent review), most studies are from the very early stages of the pandemic and/or include limited evidence from North America and Europe, places where both air pollution and COVID-19 outcomes are likely to be prone to less measurement error [14–16]. In this short time the air pollution-COVID literature has increased exponentially and now covers additional pollutants, making it possible to ascertain potential heterogeneity of effects across PM, nitrous oxides, ozone, carbon monoxide and other known pollutants to be harmful to human health.

Here we perform a systematic review on studies investigating the relationship between air pollution and COVID-19 outcomes in Europe and North America, disaggregating the evidence by air pollutant type and length of exposure, as well as COVID incidence, non-fatal severity, and mortality. Our focus on these regions is motivated by the similar pollution exposures, COVID-19 responses, and institutional and healthcare capacity relative to the rest of the world. Furthermore, the evidence for these settings is likely to be more reliable given the better air quality monitoring and comparable COVID-19 testing rates and reporting of outcomes [17]. Our final sample includes 69 studies covering populations in Europe, 31 in North America, and one which spans both.

2. Methods

Databases and Search Strategy

We searched PubMed, Web of Science, and Scopus for studies investigating the relationship of air pollutants and COVID-19 outcomes. We included published studies written in English with no restriction on the database inclusion start date through to June 19th, 2021. The keywords used in the database search are composed of the concepts of 1) air pollution and 2) COVID-19. We included the most commonly measured air pollutants: particulate matter less than 10, 2.5, and 1 micrometers in diameter (PM_{10}, $PM_{2.5}$ and PM_1, respectively), nitrogen oxides (NO_X), nitrogen dioxide (NO_2), ozone (O_3), sulfur dioxide (SO_2), and carbon monoxide (CO).

For example, the search strategy on PubMed consisted of the following sets of keywords:

1) ((air pollutant) OR (air pollution) OR (air pollut*) OR (air contam*) OR (particulate matter) OR (PM_{10}) OR ($PM_{2.5}$) OR (PM1) OR (nitrogen oxides) OR (NO_X) OR (nitrogen dioxide) OR (NO_2) OR (ozone) OR (O_3) OR (sulfur dioxide) OR (SO_2) OR (carbon monoxide))
2) AND ((COVID-19) OR (SARS-COV-2))

The full search strategy is available in the Online Resource.

Figure 1 reports the PRISMA (Preferred Reporting Items for Systematic Reviews and Meta-Analyses) flow diagram that led to inclusion of the studies. The initial search resulted in 1,662 articles from PubMed, 1,670 from Web of Science, and 1,456 from Scopus. After removing duplicates and ineligible items such as books and videos, two authors independently screened the title and abstract of 2,482 articles. Of these, we retrieved and analyzed 466 full-text reports for eligibility, including those where there was uncertainty based on title and abstract screening. We identified four additional texts from citations of included studies. Disagreements about inclusion were discussed between two authors and

resolved by a third author. A total of 116 studies met the inclusion and geographical criteria and are included in the review.

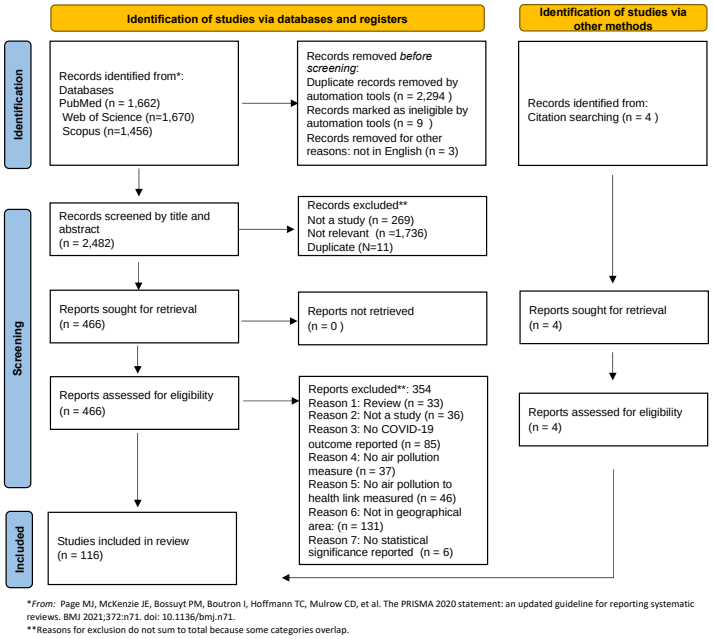


Figure 1: PRISMA (Preferred Reporting Items for Systematic Reviews and Meta-analysis) flow diagram of study selection

Selection of articles and extraction of data

We included studies examining the relationship between ambient air pollutants, such as particulates ($PM_{2.5}$, PM_{10}) or gases (NO_2, NO_X, O_3, CO, SO_2), and COVID-19 spread (e.g., transmission, incidence, prevalence), non-fatal severity (e.g., hospitalization), deaths (e.g., mortality, morbidity, excess deaths) and related outcomes in North America and Europe. We included quantitative analytical studies, such as ecological, cross-sectional, cohort, time-series, case-control, and quasi-experimental studies. We excluded qualitative or purely descriptive studies, reviews, letters to the editor, commentaries, responses, hypotheses, and editorials. We omitted animal studies and studies where the relevant gases (e.g., ozone) were administered in a clinical setting as part of a therapy. This review was registered with the International Prospective Register of Systematic Reviews (PROSPERO CRD42021275236).

Two authors independently extracted the following: authors, title, journal, aim or the study, location, unit of analysis, study period, air pollution type, exposure length, data sources, details on the COVID-19 health outcome, study design, method for data analysis, covariates, and main results.

Summary tables of the main features and results for each of the 116 studies are found in the Online Resource, Tables 1-3.

Synthesis of Studies

We synthesized the studies by COVID-19 outcome, pollutant type, length of air pollution exposure measured (short-term vs long-term exposure), and country. We classify the measure of air pollution exposure as long-term if is one year or longer. Due to large heterogeneity in the measures of COVID-19 outcomes and air pollution exposures, we did not perform a meta-analysis. Furthermore, given that many of the studies are at risk of bias, a meta-analysis may be misleading since it is likely to compound the errors. In general, it is difficult to compare result magnitudes among all studies, given the different outcome measures and since many do not interpret their coefficients beyond their value and statistical significance. However, to facilitate the identification of general patterns across the different sets of outcomes, we classify the estimates reported into positive associations, negative associations, non-significant associations, and mixed associations (that is, within the same study, the direction of the relationship is dependent on e.g., the model specification and/or sample). Only estimates that are statistically significant at the 5% significance level are reported as positive or negative and are otherwise classified as non-significant. 6 studies were omitted because they did not indicate p-values or whether their results were statistically significantly different than zero [18–23]. We create summary figures graphing the direction of the association found by on outcome, pollutant, length of exposure, and country.

Risk of Bias Assessment

We used the World Health Organization's Risk of Bias (ROB) Assessment Instrument for Systematic Reviews Informing Global Air Quality Guidelines [24] developed for the assessment of studies examining the relationship between air quality and health. We assessed the studies using the following six domains 1) confounding, 2) selection bias, 3) exposure assessment, 4) outcome measurement, 5) missing data, and 6) selective reporting. Each of these domains is scored either "low", "moderate", or "high" risk of bias based on the specific guidelines provided by the WHO evaluation instrument. Potential confounders are identified based on the ROB instrument suggestions and further adapted to consider confounders that are particularly relevant to COVID-19 outcomes. For long-term studies, critical confounders include age, sex, individual or area-level socioeconomic status, and body mass index or co-morbidities. For short-term exposure studies, critical confounders include temperature, seasonality, long-term trends, and other pandemic specific variables such as population levels of interactions and mobility, lock-down measures. A separate evaluation was made for each COVID-19

outcome that a study investigates and independently assessed by two authors. A detailed explanation of the ROB assessment is included in the Online Resource Risk of Bias Assessment section.

3. Results

3.1 Overview of Studies

The 116 included studies vary in research design, unit of observation, study period, and of length of air pollution exposure measured. Most studies use either an ecological or time-series design, comparing air pollution and COVID-19 outcomes across regions or within a region across time. Three studies implement quasi-experimental designs [26–28]. The unit of analysis varies from individual-level (n = 8) to sub-regional level (e.g., city, county, province, state). Most studies analyze COVID-19 outcomes from the first half of 2020. 64 studies study long-term pollution exposure, 47 focus on short-term exposure and 3 investigate both (2 do not specify the length). The studies provide evidence from eight European countries including Italy (n = 35), the U.K. (n = 11), Spain (n = 6), Germany (n = 3), France (n = 3), Austria (n = 2), Poland (n = 2) and the Netherlands (n = 1), and from North America including Canada (n = 3), the U.S. (n = 39), and Mexico (n = 4). Seven studies analyze multiple countries together: six study European countries, and one reports results for both Europe and North America at the continent-level [2].

We first describe the COVID-19 outcomes used to measure spread/cases, followed by non-fatal severity, and deaths. 80 studies focused on how various air pollutants relate to the spread of COVID-19, resulting in 184 evaluations. These studies employed measures such as incidence, prevalence, total cases, and other transmission dynamics based on officially reported cases. The papers studying incidence (n = 43) mostly use incidence case counts or crude incidence rates – one uses a standardized incidence ratio [29], and another uses the daily number of new cases divided by the number of existing cases [30]. The incidence measurement period varies across studies from daily to weekly to all "new" cases from the beginning of the pandemic up to a specified date. Instead, 4 studies define their COVID-19 outcome as prevalence [31–33]. However, several studies did not differentiate between incidence and prevalence and use the total cumulative number of cases as their measure of COVID-19 (n =24). A few studies use other COVID-19 measures such as positivity rates [34,35], age-standardized positivity ratios [36], standardized morbidity ratios[37], seeding and doubling time [38], the reproductive number R0 and Rt [39,40], and another measure of the outbreak rate [41]. We refer to these spread-related outcomes collectively as spread/cases.

11 studies measure COVID-19 non-fatal severity or hospitalizations, for a total of 17 pollutant-outcome evaluations. The studies on non-fatal severity mostly employ hospitalization-related measures, which include number of hospitalizations, hospitalization rates, odds of hospitalization, cases in intensive care units (ICU), rate of urgent hospitalizations, and daily ICU admissions; one study measures non-fatal

severity as the probability of developing COVID-19 pneumonia [42]. Finally, 70 studies investigate the relationship between air pollution and COVID-19 mortality-related outcomes, for a total of 154 pollutant-outcome evaluations. These measures include total counts of COVID-19 deaths (daily new deaths or cumulative up to a given date), mortality rates, case fatality rate, and excess mortality.

The studies analyze how COVID-19 outcomes relate to ambient exposure to the following air pollutants: $PM_{2.5}$ (n = 82), PM_{10} (n = 42), O_3 (n = 34), NO_2 (n = 41), NO_X (n = 4), SO_2 (n = 6), CO (n = 8) and a more general multi-pollutant air quality index, AQI (n = 4). Nine studies included other pollutant measures such as ammonia (NH_3), benzene, benzidine, acetaldehyde sulfate, aerosol optical depth (AOD), sulfate (SO4) and other generic non-methane volatile organic compounds (NMVOC). Most studies evaluate more than one pollutant.

3.2 Risk of Bias (ROB) Assessment

For each study, we assessed each COVID-19 outcome separately, resulting in 171 evaluations. The ROB varies widely across the six evaluated categories. There was high risk of confounding in most evaluations (n = 115), moderate risk in 8 evaluations and low risk in 48 of the evaluations. Most of the studies classified as high risk of confounding fail to adjust for critical variables, such as age, sex, socioeconomic status, and body-mass index. Time series studies fail to account for confounders such as mobility, lockdown measures, and seasonality. Selection-bias risk was low in most evaluations (n = 134), moderate in 24, and at high risk in 13. Air pollution exposure assessment risk was high in 11 assessments, moderate in 6 evaluations, and low in 154, as studies mostly employed high quality air pollution data that is measured by government or environmental agencies. COVID-19 outcome measurement risk was high in most of the evaluations (n = 148), moderate in 14, and low in only 9, where studies used excess deaths or explicitly accounted for testing capacity. The main concern in COVID-19 measurement are potential errors in the measurement of officially reported COVID-19 outcomes that are systematically related to air pollution exposure, particularly COVID-19 testing, healthcare capacity, socioeconomic status, and comorbidities. We classified missing data risk of bias as unclear in 150 of the evaluations since most studies did not report whether and how much data was missing or whether it was imputed. Selective reporting is mostly at low risk of bias, as in 156 of the evaluations all effect estimates were presented for all hypotheses that that study aimed to test. An overview graph of the results and a detailed description of the risk of bias assessment is found in the Online Resource, ROB Assessment and Figure 5.

3.3 Direction of Relationship across COVID-19 Spread/Cases, Hospitalizations and Deaths:

We first analyze results by COVID-19 outcomes. Many studies examine more than one COVID-19 outcome and air pollutant, and we report on each pollutant-outcome pair as a separate evaluation. Figure 2

summarizes the distribution of the 355 total evaluations across COVID-19 spread/cases, non-fatal severity as hospitalizations (including one study that measures severity as risk of developing COVID-19 pneumonia), and deaths, coded by the direction of association found with air pollution. Across all evaluations, close to half reported positive associations with air pollution (n=177, 49.9%). Non-significant associations were found in 105 evaluations (29.6%), while negative and mixed associations were found in 34 (9.6%) and 39 (11.0%) of the evaluations, respectively. We next disaggregate the results by COVID-19 outcomes as shown in Figure 2.

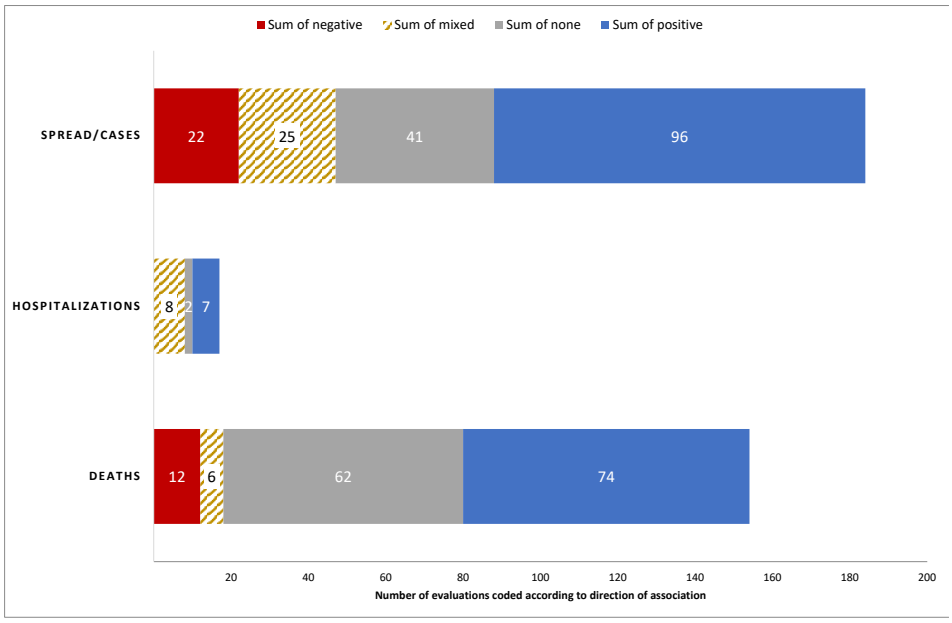


Figure 2: Direction of the relationship between air pollution and COVID-19 cases/spread, hospitalizations, and deaths. The relationship was coded separately for each pollutant and COVID-19 outcome pair for each study. Includes all countries and pollutants. None is defined as having found no statistically significant association.

Spread/cases

The category "spread/cases" includes all outcomes related to spread as previously defined. 96 out of 184 evaluations (52.2%) find that COVID-19 spread/cases are positively associated with higher air pollution. Furthermore, 41 evaluations (22.3%) find no statistically significant results, 25 find mixed results, and 22 find negative associations. The analyses on spread/cases are at high risk of confounding bias in 76.7% of the outcome evaluations, and low risk in 19.8% of the cases. Only 16 papers studying COVID-19 spread have low confounding bias, for a total of 31 pollutant-COVID-19 evaluations. Among these evaluations, 15 (48.4%) find positive air pollution-spread associations, 14 (45.2%) find no significant associations, one finds mixed results and one finds a negative association between air pollution

and COVID-19 spread. Compared to all evaluations, those with low risk of confounding bias find a higher share with no significant associations (45.2% vs 22.3% in full sample) and almost none find mixed or negative associations. A slightly lower percent finds positive associations (48.4% vs 52.2% in full sample). This suggests that confounding bias is likely to increase the number of studies that find negative or mixed associations and increase the number of evaluations that fail to find a significant relationship.

Focusing on the 84 evaluations that measure spread/cases as incidence, 48 (57.1%) find that increasing levels of air pollution exposure are positively associated with higher incidence, 12 evaluations find non-statistically significant results, 10 negative, and 14 mixed associations. For example, Fang et al.[43] find that in the U.S., each 1 μg/m^3 (microgram per cubic meter) increase in the annual average concentration of $PM_{2.5}$ exposure was associated with a 7.56% (95% CI: 3.76%, 11.49%) increase in COVID-19 incidence risk, while in Italy, De Angelis et al.[44] find that a 1 μg/m^3 higher in annual concentrations of $PM_{2.5}$ is associated with a 5.8% increase in the incidence rate. Similarly, among 68 evaluations that measure spread as the cumulative total number of cases, (52.9%) find positive associations, 14 find non-significant associations, 10 find negative, and 8 find mixed results. In terms of magnitude, for example, Travaglio et al.[45] find that in the United Kingdom, increases of 1 μg/m^3 in the long-term average of $PM_{2.5}$ and PM_{10} were associated with a 12% and 8% increase in COVID-19 cases, respectively.

14 evaluations from four studies use prevalence to measure COVID-19 spread [31–33,46] with varying results according to pollutant. Five evaluations (35.7%) find a positive relationship between air pollution and higher prevalence, eight evaluations (57.1%) from one single study find no significant association [46], and one evaluation finds a negative association. Bilal et al.[31] find that in Germany, daily $PM_{2.5}$, PM_{10}, and O_3 are positively associated with active cases but NO_2 is negatively associated. For Northern Italy, Filippini et al.[32] find a positive association between short-term satellite-measured tropospheric NO_2 levels and SARS-CoV-2 prevalence at levels beyond 130 μmol/m^2, suggesting there may be non-linearities in the relationships. Finally, Hendryx & Luo [33] find that long-term exposure to $PM_{2.5}$ is positively associated with prevalence in the U.S., but no significant association is found for O_3. The mixed results within studies indicate potential interactions among pollutants.

Hospitalizations/Non-fatal Severity

Among 17 evaluations from 11 studies on hospitalizations/non-fatal severity, eight evaluations (47.1%) find mixed results. In the U.S., Mendy et al. [47] find that $PM_{2.5}$ is positively associated with hospitalization among COVID-19 patients with pre-existing respiratory diseases but surprisingly, negatively associated among those without. Instead, both Lolli et al. [48] and Linares et al.[49] find that the results depend on location, as they find significant positive relationships in only some cities/regions in

their samples. Furthermore, Diaz et al.[50] find that the results are either positive or null depending on whether air pollution is measured daily or averaged over 14 days. Seven evaluations find positive associations between pollutants and hospitalizations: five of these study $PM_{2.5}$ [26,51–54] and one NO_2 [55]. Additionally, one study finds a positive association between exposure to PM_{10} and developing COVID-19 pneumonia [42]. Finally, two evaluations from one study find non-significant relationships between NO_2, SO_2, and hospitalizations [26]. The analyses on hospitalization are at high risk of confounding bias in 63.6% of the outcome evaluations, and low risk in 36.4% of the cases. Only four papers studying hospitalizations and non-fatal severity had low risk of confounding bias, for a total of six evaluations. Of these, half (n = 3) find positive associations, two non-significant associations, and one finds mixed associations.

Deaths

Among 154 evaluations on COVID-19 mortality, about half (n = 74; 48.1%) find positive associations with pollutants, 62 (40.3%) evaluations find non-significant associations, 12 (7.8%) find negative associations, and six find mixed results. The analyses on mortality are at high risk of confounding bias in 56.8% of the outcome evaluations, and low risk in 36.5% of the cases. From the 23 papers with low confounding bias studying COVID-19 deaths, there are a total of 62 evaluations. Of these, 27 (43.5%) of evaluations find positive associations between air pollution and COVID-19 deaths, 32 (51.6%) find no significant effects, two (3.2%) find negative results, and one (1.6%) finds no significant results. Relative to all evaluations on COVID-19 deaths, those with low confounding bias have a higher percentage of nonsignificant results, and a lower percentage of positive, negative, and mixed results. We next stratify the results by type of mortality measures used.

44 out of 154 evaluations on mortality use measures of mortality rate or ratio. Among these, about half (n = 23) find positive associations with air pollutants and 19 find no significant associations. The magnitude of the relationship for those finding positive results varies across countries, pollutants, and models. For instance, in one of the most cited studies, Wu et al. [56] use a negative binomial model and find that in the U.S., an increase of 1 μg/m^3 in the 2000-2016 $PM_{2.5}$ average (8.4 μg/m^3; SD 2.5 μg/m^3) is associated with an 11% (95% CI: 6%, 17%) increase in a county's COVID-19 mortality rate. Also in the U.S. but focusing on NO_2, Lipsitt et al. [57] find that an interquartile range increase of 8.7 ppb in the long-term mean annual NO_2 leads to an increase in COVID-19 mortality of 35% (95% CI: 23%, 48%), 44% (95% CI: 11%, 86%), and 60% (CrI: 37%, 88%), using Poisson, negative binomial, and spatial random effect models, respectively, highlighting the sensitivity of using different models. Instead in Italy, Borro et al.[58] find that when short-term average $PM_{2.5}$ concentration levels increase from 10 to 25 μg/m^3 the mortality rate doubles from 4.5 to 9%.

Other evaluations focus on total cumulative death counts (n = 41), of which 21 find positive associations with pollutants, 10 find no significant associations, and nine find negative associations. Instead, focusing on case fatality rates or ratios, six out of 19 evaluations find positive relationships with pollutants, 10 find no significant associations, and three find mixed results. For example, in a U.S. study that uses multi-pollutant models, an interquartile range increase (4.6 ppb) in NO_2 was associated with a COVID-19 case-fatality rate increase of 11.3% (95% CI: 4.9%, 18.2%), while in contrast, $PM_{2.5}$ and O_3 were not significantly associated with deaths [59].

3.4 *Results by Pollutant*

Figure 3 graphs the direction of the relationships found among specific pollutants and COVID-19 spread/cases, deaths, and hospitalizations. Each pollutant-outcome estimate is treated as a different evaluation. Overall, among the top five pollutants studied, $PM_{2.5}$, PM_{10}, O_3, NO_2, and *CO*, studies most frequently find that COVID-19 spread/cases increase as these pollutants increase. $PM_{2.5}$ and NO_2 are also most frequently associated with more deaths. Instead, for PM_{10} and O_3 the most frequent result is no statistically significant relationship with COVID-19 deaths.

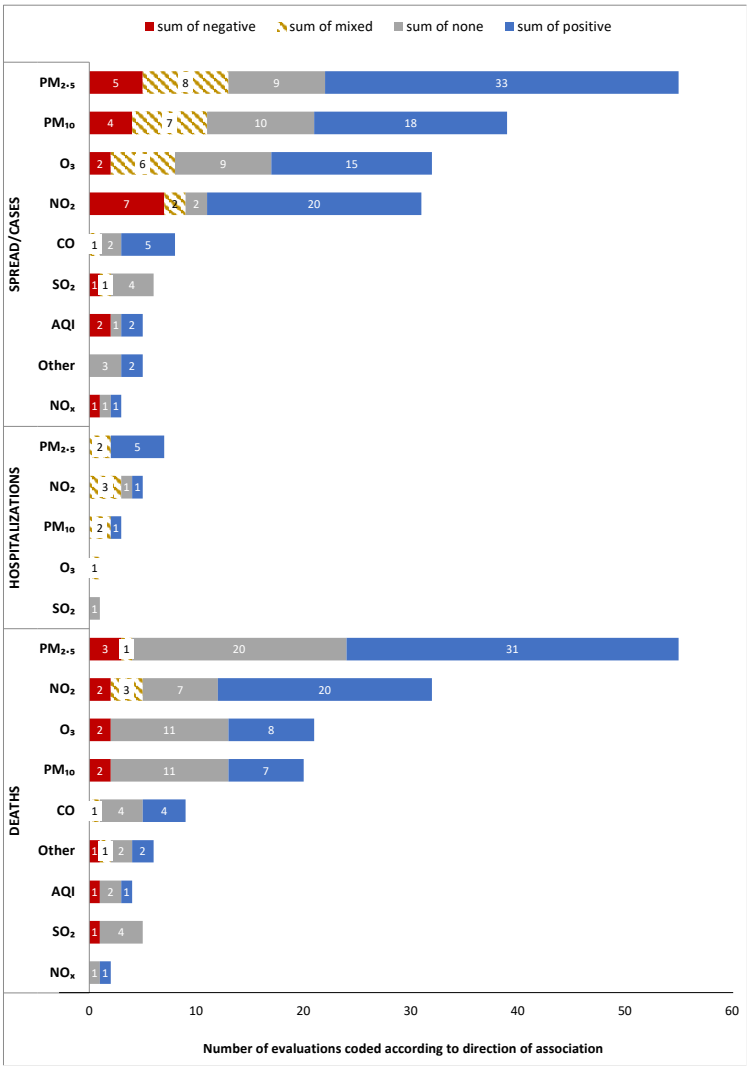


Figure 3: Direction of the relationship between air pollution and COVID-19 cases/spread, hospitalizations, and deaths split by pollutant. The relationship was coded separately for each pollutant and COVID-19 outcome pair for each study. Pollutants included: particulate matter less than 10 and 2.5 micrometers in diameter (PM_{10} and $PM_{2.5}$ respectively); ozone (O_3); nitrogen dioxide (NO_2), nitrogen oxides (NO_X), sulfur dioxide (SO_2), carbon monoxide (CO) and a more general multiple-pollutant air quality index, AQI . The category "other" includes other measures of air pollution such as ammonia (NH_3), benzene, benzidine, acetaldehyde SO4, aerosol optical depth (AOD), Sulfate (SO4) and other non-methane volatile organic compounds (NMVOC).

$PM_{2.5}$ is the most-studied pollutant, and among 55 evaluations, more than half (n = 33; 60.0%) find that higher levels are associated with increasingly higher COVID-19 spread: Nine find non-significant results, Seven find mixed results, and five find negative relationships. For hospitalizations, five out of seven evaluations find positive associations, and for deaths 31 out of 55 (56.4%) find that higher levels are related to higher COVID-19 mortality.

The evidence for PM_{10}, is less consistent than that of $PM_{2.5}$, particularly for deaths. A total of 18 out of 39 (46.2%) evaluations find that PM_{10} leads to more spread/cases, while 10 find non-significant associations. Instead, among 20 evaluations on COVID-19 deaths, 11 (55.0%) find no statistically significant results, while seven (35.0%) find that higher levels of PM_{10} lead to more deaths. Among three evaluations of PM_{10} and hospitalizations, one finds a positive association[42] and two find mixed results [49,50].

The results for O_3 are similar to those of PM_{10}. Out of the 32 evaluations between O_3 and COVID-19 spread/cases, 46.9% (n = 15) find positive associations, 9 find non-significant associations, and only two find negative associations. For COVID-19 deaths 11 out of 21 evaluations (52.4%) find no significant association and 8 find that higher O_3 levels lead to more COVID-19 deaths. Only one evaluation analyzed hospitalizations, finding that O_3 is positively associated with the rate of urgent hospitalizations in some regions of Spain but not others [49].

Across all pollutants, NO_2 is the one with the highest percentage of evaluations finding a positive relationship with spread/cases and deaths: 20 out of 31 evaluations (64.5%) find that increasing levels of NO_2 lead to higher spread/cases, while 20 out of 32 evaluations (62.5%) on deaths find a positive relationship. Instead, seven out of 31 evaluations find a negative relationship between NO_2 and spread/cases, and for deaths this is only two out of 32.

Many studies evaluate more than one pollutant, but only some consider interactions among the pollutants in their models. Notably, Liang et al. [59] use co-pollutant models and find that an increase of 4.6 parts per billion of NO_2 and a 2.6 mg/m^3 increase in $PM_{2.5}$ were associated with case-fatality and mortality rates increases of 11.3% (95% CI 4.9%–18.2%) and 16.2% (95% CI 8.7%– 24.0%), respectively; similarly an increase in $PM_{2.5}$ of 2.6 mg/ m^3 was associated with a 14.9% (95% CI 0.0%–31.9%) increase in the COVID-19 mortality rate.

3.5 Results by Length of Exposure

Figure 4 displays the direction of the association between pollutants and COVID-19 outcomes stratified by length of exposure to air pollution. We classify the measure of air pollution exposure as long-term if is one year or longer. In this section, we omit 2 studies whose measure of the length of air pollution exposure was unclear. Additionally, three studies examine both long-term and short-term exposure and we code each relationship separately.

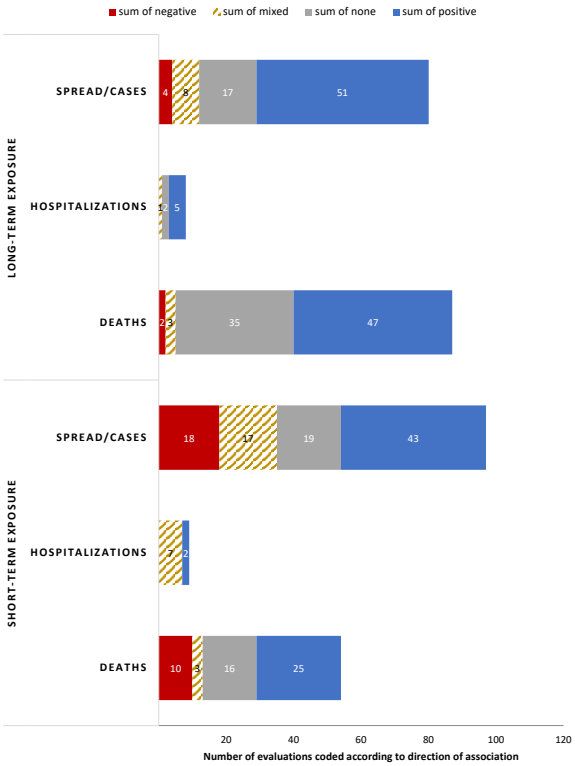


Figure 4: Direction of the relationship between air pollution and COVID-19 cases/spread, hospitalizations, and deaths split by length or air pollution exposure measured. The relationship was coded separately for each pollutant and COVID-19 outcome pair for each study. We classify the measure of air pollution exposure as long-term if is one year or longer. Two studies investigated both short-term and long-term exposure and their findings are reported separately for each short- and long-term evaluation. Two studies where the length of pollution measure is unclear (14 evaluations) are omitted.

Long-term exposure to air pollution is more frequently positively associated with higher COVID-19 cases and deaths compared to short-term exposure. Specifically, for COVID-19 spread/cases 63.8% (51 out of 80) of evaluations studying long-term exposure to pollutants find positive associations, compared with 44.3% (43 out of 97) of the evaluations on short-term exposure. For deaths the gap is smaller, with 54.0% (47 out of 87) of the evaluations finding positive associations with long-term exposure to air pollution, compared to 46.3% (25 out of 54) of the evaluations studying short-term exposure. There are fewer evaluations on hospitalizations, and 5 out of 8 are positively associated with air pollution in the long-term exposure studies, while 2 out of 9 are positively associated in the short-term exposure studies.

Conversely, short-term exposure to air pollution is more frequently found to be negatively associated with COVID-19 spread/cases and deaths (in 18.6% and 18.5% and of the evaluations, respectively) compared with the results for long-term exposure to air pollutants, in which only 5.0% and 2.3% of evaluations find that higher levels of pollutants are associated with less spread/cases and deaths,

respectively. Overall long-term exposure to air pollution is more consistently positively associated with aggravated COVID-19 outcomes, relative to short-term exposure.

3.6 Results of Individual-level Studies

A total of eight studies use individual-level data. Seven investigate the relationship between long-term exposure to air pollution and COVID-19 outcome and one analyzes both long- and short-term air pollution exposure. Most of the studies come from the UK (n = 4) and these use UK Biobank data, while two studies come from the U.S, one from Mexico, and one from Italy. Nine evaluations from three studies focus on the relationship between pollutants and COVID-19 spread: among these, five find that pollution is positively associated with COVID-19 spread and four find no significant relationships. From three evaluations on hospitalizations, two find that pollution is positively associated with risk of hospitalization, and one finds mixed results: a positive association for patients with pre-existing conditions and negative associations with patients without pre-existing conditions. Finally, for COVID-19 deaths, among 10 evaluations from three studies, six evaluations do not find evidence that air pollution is associated with COVID-19 deaths, three find positive associations, and one finds a negative association. Overall, for each of the main outcomes (COVID-19 spread, hospitalizations, and deaths) there are only three separate studies mostly coming from the UK and using the same datasets, calling for a need for more evidence using individual-level data and for those studying the consequences of short-term exposure to air pollution, since only one study focused on this.

3.7 Country-specific Results

Figure 5 shows the results of the direction of the relationships by country for cases/spread, hospitalizations, and deaths.

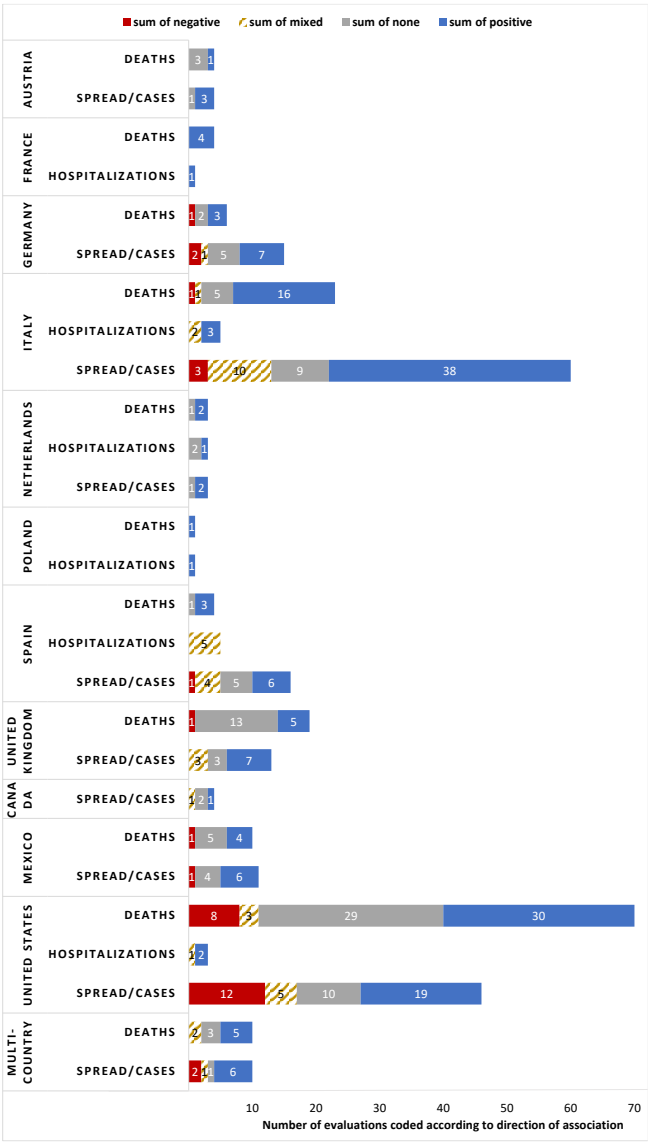


Figure 5: Direction of the relationship between air pollution and COVID-19 cases/spread, hospitalizations, and deaths split by country. The relationship was coded separately for each pollutant and COVID-19 outcome pair for each study.

Europe

Austria (n = 2)

In two studies from Vienna, results vary by the length of air pollution exposure and pollutant. Hutter et al.[60] report that levels of average 2019 PM_{10} and NO_2 above the upper quartile (30 and 20 µg/m^3, respectively) are significantly associated with higher risk of a COVID-19 diagnosis (hazard ratios of 1.44 and 1.16), while only NO_2 is found to increase deaths. Instead, Moshammer et al.[30] find that

that a short-term increase in 1 μg/m^3 of NO_2 led to a 3.2% incidence increase, but found no significant association between PM_{10} and cases, nor for either pollutant and COVID-19 deaths.

France (n = 3)

Three studies in France find positive associations between pollution, hospitalizations, and deaths. Two related studies on the cities of Lyon, Marseille, and Paris during March and April of 2020 use Artificial Neural Networks (ANNs) experiments and machine learning techniques and find that short-term exposure to $PM_{2.5}$, PM_{10}, and NO_2 beyond certain thresholds is positively associated with COVID-19 deaths [61,62]. Furthermore, Deguen & Kihal-Talantikite[55] use nation-wide data and find that 2014-2018 average NO_2 is positively associated with hospitalizations, cases in intensive care units, and deaths, particularly for those regions in the highest tercile of housing overcrowding.

Germany (n = 3)

10 out of 21 evaluations in Germany find that air pollutants are positively associated with higher COVID-19 spread/cases and deaths. In one of the few quasi-experimental studies Isphording & Pestel [27] find that PM_{10} significantly increases both COVID-19 deaths and cases, but find no significant effect of O_3 in a period with relatively low ozone levels. Instead, Bilal et al.[31] use Spearman correlations to find that O_3 is the only pollutant that is consistently associated with higher COVID-19 spread/cases and deaths. Finally, Huang and Brown[29] find that a 1 μg/m^3 increase in long-term exposure to NO_2 increases the COVID-19 incidence rate by 5.58% (95% CI: 3.35%, 7.86%), while SO_2 is positively associated with incidence in one out of 2 models. However, they find no association for other pollutants ($PM_{2.5}$, PM_{10}, Benzene, Arsenic, Cadmium and Nickel).

Italy (n = 35*)*

Italy is the most-studied European country and most evaluations find positive relationships between air pollutants and COVID-19 cases/spread (38 out of 60; 63.3%); instead 15.0% and 16.7% find null and mixed results, respectively. The results differ according to length of air pollution. Among the evaluations on long-term exposure, 85.7% (24 out of 28) find positive associations with spread/cases, while 41.9% of short-term exposure studies find the same (13 out of 31). Fiasca et al.[63] investigate both short- and long-term exposure to air pollution and find that a 1 μg/m^3 increase in $PM_{2.5}$ and NO_2 rises incidence rates by 1.56 and 1.24 × 10^4 people, respectively, considering average 2016–2020 pollution, and 2.79 and 1.24 × 10^4 for pollution during March-May 2020.

Among the 23 evaluations on deaths, 16 (69.6%) find positive associations with pollutants, 5 find no associations, and 1 finds a negative association. Using excess mortality, one of the most accurate

COVID-19 measures, Coker et al.[64] find that a 1 μg/m^3 increase in $PM_{2.5}$ is associated with a 9% (95% confidence interval: 6–12%) increase in COVID-19 related mortality.

For hospitalizations, 3 out of 5 evaluations find positive associations. Frontera et al.[54] and Cascetta et al.[53] find positive associations between hospitalizations and short- and long-term exposure to $PM_{2.5}$, respectively. For PM_{10}, Pegoraro et al.[42] use individual-level patient data and report that higher exposure the month before COVID-19 diagnosis increased the likelihood of developing COVID-19 pneumonia. Finally, Lolli et al.[48] find mixed positive and null associations for PM_{10} and NO_2 and hospitalizations, depending on the city studied.

All but one study are at high risk of confounding bias. The exception is Cazzolla Gatti et al.[65] who consider critical confounders such as the number of smokers in each region, income levels, the regional levels of obesity, as well as the number of hospital beds and COVID-19 tests performed. They use a machine learning model to find that air quality and $PM_{2.5}$ were among the most important predictors of COVID-19 positivity and mortality rates. Furthermore, the Italian evidence may be overrepresented by five similar papers from one author finding that PM_{10} and O_3 are positively associated with COVID-19 cases [66–70].

Netherlands (n = 1)

For the Netherlands, Cole et al.[26] find that long-term exposure to $PM_{2.5}$ leads to higher COVID-19 cases, hospitalizations, and deaths; similarly, increases in NO_2 lead to more COVID-19 cases and deaths. They find no association between SO_2 and COVID outcomes.

Poland (n = 2)

One study in the city of Warsaw uses Spearman rank correlations tests and finds that daily ground-level O_3 is negatively associated with the number of COVID-19 cases during April to June 2020. Instead, a nationwide study finds that average PM2.5 during 2019-2020 is positively associated with COVID-19 cases and excess deaths [71].

Spain (n = 6)

In Spain, 6 out of the 16 evaluations on spread/cases find positive associations, 5 find no associations and 4 find mixed results. The results vary widely across and within studies according to pollutants, region studied, and measures used. Only Zaldo-Aubanell et al.[72] find a negative association between NO_2 and COVID-19 incidence, but alongside a positive association with PM_{10}. Five evaluations on hospitalizations from two studies find mixed results according to region and model used [49,50]. For

COVID-19 deaths, 3 out of 4 evaluations find that higher air pollution is associated with increasing COVID-19 mortality [72,73].

United Kingdom (n = 12)

In the UK, most evaluations find positive associations with spread and no associations with deaths. 7 out of 13 evaluations find that COVID-19 spread/cases is positively associated with air pollution. Instead, for COVID-19 deaths the results are mostly null: 13 out of 19 evaluations find no significant associations and 5 find positive associations. Only one evaluation finds a negative association between O_3 and COVID-19 deaths, but in this same study a positive association is found between NO_X and deaths [45].

North America

Canada (n=3)

To et al.[40] find that O_3 is positively associated with COVID-19 incidence but only in institutional settings (e.g., long-term care home, hospital, correctional facilities) and no significant associations with the reproductive number R_t. Additionally, Steib et al.[74] find no significant associations between long-term exposure to $PM_{2.5}$ nationally, but positive associations for the city of Toronto [75].

Mexico (n=4)

6 of the 11 evaluations on COVID-19 spread/cases find positive associations with air pollution while 4 find no significant results. For deaths, 4 out of 10 evaluations find positive relationships and 5 find no associations. In Mexico City, Kutralam-Muniasamy et al.[76] find negative associations with PM_{10}, positive associations with CO and O_3, and nonsignificant associations with $PM_{2.5}$. Consistent with this, López-Feldman et al.[77] use individual-level data in Mexico City and report that short-term exposure to $PM_{2.5}$ is not significantly associated with COVID-19 deaths, but finds positive associations for long-term exposure. Instead, a in the city of Victoria $PM_{2.5}$, PM_{10}, and CO have positive associations with COVID-19 cases [78]. Finally, a study in the state of Nuevo Leon finds that PM_{10} is positively significantly related to COVID-19 cases and deaths but $PM_{2.5}$ is not significantly associated with any outcome [79].

United States (n = 39)

For spread/cases, among a total of 46 evaluations, 19 find positive associations, 10 find no significant associations, and 12 find negative associations. Only 3 evaluations analyze hospitalizations.

Instead, 30 out of 70 evaluations find positive associations between air pollution and COVID-19 deaths, another 29 find no significant associations, and 8 negative associations.

The overall direction of the associations varies with the length of air pollution exposure, with no clear consensus for short-term exposure to air pollution, and more consistently positive and null associations for long-term exposure to air pollution. Out of 26 evaluations for COVID-19 spread/cases and short-term exposure to air pollution, the outcomes are split across positive and negative associations: 10 find positive associations while 11 find negative associations. Instead, for long-term exposure to air pollution, 8 out of 14 evaluations find a positive associations with COVID-19 cases, 4 find no significant association and one study finds a negative association: Ingram et al.[34] find that NO_X was negatively associated with COVID-19 positivity rate but also find positive associations with $PM_{2.5}$ and non-significant associations for O_3.

A similar pattern as in COVID-19 spread/cases emerges when focusing on COVID-19 deaths. Of 22 evaluations on short-term exposure to air pollution and deaths, 8 find positive correlations, 7 find negative associations, and 6 find no significant association between air pollutants and COVID-19 deaths. Instead, for those evaluation on long-term exposure to pollution, 20 out of 36 (55.56%) find a positive relationship between air pollutants and COVID-19 deaths, while 13 find no statistically significant association. Only Hu et al.[80] find a negative association between long-term exposure to benzidine and COVID-19 deaths, but a positive association between $PM_{2.5}$ and deaths and no significant association with other pollutants ($PM_{2.5}$, PM_{10}, O_3, CO, SO_2). Regarding hospitalizations, only long-term exposure to air pollution is assessed, and the three existing evaluations find that air pollution is positively associated with increased hospitalizations [47,51,52].

The only quasi-experimental study in North America and the U.S. comes from Persico and Johnson[28], who use a difference-in-difference analysis to provide causal evidence. The authors use county-level data and variation in air pollution caused by rollbacks in environmental regulation enforcement and find that an 11.8% increase in pollution is associated with a 53% and 10% in increase in COVID-19 and deaths, respectively.

Multi-country (n = 7)

Among the 10 evaluations on COVID-19 spread/cases, 6 find a positive association with pollutants, 2 find a negative association, and one finds no significant association. Furthermore, Zerefos et al.[81] find a positive association between $PM_{2.5}$ and COVID-19 cases in March-April but not in May-June. For COVID-19 deaths, among 10 evaluations, 5 find positive associations with air pollution, 3 find non-significant results, and 2 find mixed results. At continent level, Pozzer et al.[2] find that in Europe 19% (CI: 8%, 41%) of COVID-19 mortality is due to $PM_{2.5}$ anthropogenic emissions and 17% (CI: 6%,

578 39%) in North America. Instead, focusing on 23 European countries, Rodríguez-Pose & Burlina[82]
579 report that $PM_{2.5}$ is positively but not statistically significantly associated with COVID-19 excess
580 mortality rates. Mixed results are found by Li et al.[83] in Germany, Italy, and Spain: whether NO_2 and
581 AOD are statistically significant depends on the model, covariates and country under investigation.
582
583 **4. Discussion**
584 In this systematic review, we present the evidence on the relationship between air pollution and
585 COVID-19 cases, deaths and hospitalizations for Europe and North America. Most of the studies come
586 from U.S and Italy. Studies focus on both short- and long-term exposure to air pollution of mostly $PM_{2.5}$,
587 PM_{10}, O_3 and NO_2. More than half of the evaluations (52.2%) find that COVID-19 spread/cases increase
588 with higher air pollution, 22.3% find no statistically significant results and 12.0% find negative
589 associations between air pollution and spread/cases. For COVID-19 deaths, 48.1% of the evaluations find
590 positive associations, 40.3% find no associations and 7.8% find negative associations. The evidence for
591 hospitalizations points to mostly mixed (47.1%) and positive associations (41.2%), although there are
592 relatively fewer evaluations on this outcome.
593 Among the top five pollutants studied, our analysis finds that, $PM_{2.5}$, PM_{10}, O_3, NO_2 and CO are
594 most frequently positively associated with COVID-19 spread/cases. For deaths, the evidence is less
595 consistent: $PM_{2.5}$, NO_2 are most frequently associated with more deaths but for PM_{10} and O_3 the results
596 are mostly null. Evaluations of CO find the same number of positive and non-significant relationships
597 with deaths. We cannot make clear conclusions about the other pollutants such as SO_2 and other NO_X,
598 since the number of evaluations is small. Many studies focus on $PM_{2.5}$, given that it is likely to penetrate
599 deep into the respiratory and cardiovascular system creating inflammation and leading to various health
600 repercussions
601 To summarize, we find that the largest share of evaluations finds a link between higher levels of
602 air pollution and COVID-19 incidence/spread and mortality. There are relatively fewer studies on non-
603 fatal COVID-19 hospitalizations/severity and the existing evidence on this is mostly mixed. The impact
604 of short-term exposure to air pollution on COVID-19 remains the most inconclusive and risk of
605 confounding bias and COVID-19 measurement bias remains the main issue with most of the existing
606 studies. Future research should focus on study designs that control for confounding factors, for example
607 by using quasi-experimental and natural experiments, and use measures of COVID-19 that are less likely
608 to be measured with systematic errors, such as excess mortality.
609 Differences in results across studies depend mainly on the length of air pollution exposure
610 measured, the COVID-19 outcome studied, and differences in adjustments for risk of bias. More studies
611 find negative or mixed relationships when studying short-term exposure or COVID-19 spread. It is

612 important to consider that the evidence on short-term air pollution exposure is in general at high risk of
613 confounding bias. One of the biggest concerns in measuring the relationship between short-term air
614 pollution and COVID-19 outcomes is reverse causality and adjustment for omitted variables, such as
615 mobility, lockdowns, and weather patterns during the pandemic. Indeed, the timing lockdowns may be
616 one of the reasons why some studies find negative associations in time-series studies of the effect of
617 short-term exposure to pollution [84]. Furthermore, many studies report a positively significant
618 relationship in some of their models, but after accounting for covariates, this relationship disappears,
619 suggesting there is confounding or omitted variable bias. When focusing only on the studies with low risk
620 of confounding bias, we find that there are significantly fewer evaluations that find negative or mixed
621 associations, but also slightly fewer evaluations that find positive evaluations, with more overall
622 evaluations that fail to find a significant relationship. However, the number of studies that are at low risk
623 of confounding bias is relatively low to make more general conclusions and other biases such as COVID-
624 19 outcome measurement error should also be considered when interpreting these results. Across
625 countries, the main differences in results stem from the factors mentioned above and the number of
626 available studies. Furthermore, there may be interactions among pollutants, as some studies find positive
627 results for some pollutants, and negative or null results for other pollutants within the same sample.
628 Air pollution has been proposed to affect COVID-19 outcomes through several mechanisms.
629 Some studies suggest that exposure to pollutants could lead to oxidative stress and a more stressed
630 respiratory tract, which increases the likelihood of COVID-19 infection, hospitalization or death. The
631 results from the studies in this review suggest that long-term exposure to air pollution weakens the
632 respiratory system and increases susceptibility to COVID-19.
633 Another early proposed mechanism, particularly during the beginning of the pandemic, was
634 through airborne transmission of COVID-19 through its attachment to pollution particles. However, this
635 interpretation has not been supported by successive studies [85,86].. Furthermore, in our review we find
636 limited evidence of this interpretation, as found by the less clear relationship between short-term
637 contemporaneous exposure to air pollutants and COVID-19. These results suggest that there is limited
638 evidence that COVID-19 is carried by air pollution particles.
639 While the evidence for North America and Europe is important, a major limitation of this review
640 is the limited regional scope and the fact that most of the countries are high-income countries (except for
641 Mexico). In low- and middle-income countries, COVID-19 and the impact of air pollution are likely to be
642 significantly more catastrophic, given fewer resources, less healthcare capacity, and higher exposure to
643 outdoor air pollution. Nevertheless, the results for the relationship between air pollution and COVID-19
644 outcomes in North America and Europe can still inform the relationship and policy decisions in other
645 parts of the world.

Another limitation of this review is that we do not carry out a meta-analysis due to the heterogeneity in the measurement of COVID-19 outcomes, even within spread/cases, hospitalizations, and deaths. However, given the large risk of confounding and other biases found in the studies, a meta-analysis may be misleading, as it will simply compound the errors. Furthermore, given that there is inconsistency in the direction of the effect for some of the outcomes, it may be misleading to quote an average value for the effect [87]. Nonetheless, an attempt was made to synthesize the studies according to the direction of the relationship between pollutants and COVID-19. Future analysis should seek to quantify the and present the associations in a meta-analysis.

In this analysis, we are not able to analyze how lockdowns or quarantine measures and the subsequent changes in air pollution interacted with the COVID-19 outcomes to moderate the results. There were various levels of lockdown measures implemented during the pandemic, and these lockdown measures likely lead to different air pollution changes and COVID-19 outcomes. The studies we include are carried out throughout various stages of the pandemic, and some other studies account for differences in air pollution lockdown by only focusing on measuring pollution only before the lockdown [88–90]. As discussed by Gujral and Sinha [91], it is important to consider lockdown restrictions, as this could explain why some studies find a negative relationship between air pollution and COVID-19 spread [84,92,93]. Lockdowns cause large instantaneous drops in pollutants which may not be immediately followed by COVID-19 health outcomes due to the timing of the lockdown and the dynamics of the virus. However, some studies find that PM is correlated with cases both before and after the lockdown [94,95], and other studies find different mixed results during the different stages of the lockdown period and by pollutant [76]. Other studies control for the lockdown stage but do not report different results, while some do not control for lockdown measures. We acknowledge that we cannot generalize beyond this and suggest that future research study how lockdown dynamics and quarantine measures affect the relationship between air pollution and COVID-19, as there may be potential non-linear relationships between the level of air pollution and COVID-19 outcomes, which was influenced by lockdown measures.

Our study allows us to disaggregate the evidence of the effects on COVID-19 by outcome, pollutant, length of air pollution exposure, and country. This is important, as different institutional and contextual settings are likely to moderate the effects or pollutants on COVID-19 outcomes. We complement existing systematic reviews and focus on the evidence in Europe and North America, where there is more comparable air pollution exposure, institutional and healthcare capacity, and COVID-19 response relative to the rest of the world. Furthermore, the higher quality of data on both air pollution and COVID-19 outcomes in these regions relative to the rest of the world allow for more reliable results.

The included studies are subject to limitations. Many of the studies used correlation analysis such as Pearson’s or Spearman’s coefficients, which do not allow for the addition of covariates to account for

confounding. Those using multivariate models are usually ecological. While the ecological study design is the most easily accessible given the data available, this study design is prone to ecological bias and has several limitations [56]. Only three studies applied quasi-experimental designs, which aim to estimate causal relationships and are more likely to account for confounding factors more robustly. In addition to employing more quasi-experimental designs, future evidence should include more individual-level data, and control for critical confounders.

Another challenge most studies face is that measurement data on COVID-19 cases and deaths is prone to measurement error that is likely to vary across regions and time. Testing capacity and death classification are likely to be related to timeframe of the pandemic, resources and potentially air pollution exposure. Even confirmed COVID-19 deaths counts are likely underestimated as they depend on testing for classification. Excess mortality is likely to be a better measure of the pandemic burden and future studies should focus on this, although methods for this calculation differ [96].

Studying country-specific results is important since there may be need for specific policies that reduce air pollution. Indeed, rollbacks in environmental protections during COVID-19 have been causally found to be related to higher COVID-19 cases and mortality [28]. In Europe, most of the evidence focuses on Italy, highlighting regions that were chronically exposed to air pollution and hit particularly hard by COVID-19 in the early stages of the pandemic. However, there are very few Italian studies that are at a low risk of confounding bias and no studies that aim to estimate casual results. Furthermore, the evidence would benefit from more studies outside of Italy, and particularly from Eastern Europe, where daily limits of air pollution are most widely exceeded [9].

Research investigating the impacts of long-term exposure to air pollution typically measures average air pollution exposure across one or more years and links it to later health outcomes. To interpret and directly link the relationship between past exposure and later outcomes, studies assume that the population studied remained in the same geographical area, or that people moved at random. Studies would benefit from addressing or making this limitation clear. Furthermore, the effect of acute exposure to air pollution is likely to depend on the cumulative or long-term exposure to air pollution; future studies should aim to study these interactions. Since air pollution is often a localized problem measuring exposure across large geographical areas, such as a state, may lead to large measurement errors. Furthermore, many studies investigate several pollutants but these are often studied in separate models; incorporating multi-pollutant models, for example as in Liang et al.[59] is a key priority for future research.

Reducing air pollution is a way to reduce harmful health effects that are responsible for millions of global premature deaths each year. Furthermore, we find that mitigating air pollution is likely to alleviate COVID-19 severity and deaths and to reduce the harms of any future potential respiratory

pandemics. Immediate action is needed to implement policies that reduce air pollution, particularly for the most vulnerable populations that are more likely to be affected by air pollution and its deadly health consequences.

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Figure Captions

Figure 1: PRISMA (Preferred Reporting Items for Systematic Reviews and Meta-analysis) flow diagram of study selection

Figure 2: Direction of the relationship between air pollution and COVID-19 cases/spread, hospitalizations, and deaths. The relationship was coded separately for each pollutant and COVID-19 outcome pair for each study. Includes all countries and pollutants. None is defined as having found no statistically significant association.

Figure 3: Direction of the relationship between air pollution and COVID-19 cases/spread, hospitalizations, and deaths split by pollutant. The relationship was coded separately for each pollutant and COVID-19 outcome pair for each study. Pollutants included: particulate matter less than 10 and 2.5 micrometers in diameter (PM_{10} and $PM_{2.5}$ respectively); ozone (O_3); nitrogen dioxide (NO_2), nitrogen oxides (NO_X), sulfur dioxide (SO_2), carbon monoxide (CO) and a more general multiple-pollutant air quality index, AQI . The category "other" includes other measures of air pollution such as ammonia (NH_3), benzene, benzidine, acetaldehyde SO4, aerosol optical depth (AOD), Sulfate (SO4) and other non-methane volatile organic compounds (NMVOC).

Figure 4:Direction of the relationship between air pollution and COVID-19 cases/spread, hospitalizations, and deaths split by length or air pollution exposure measured. The relationship was coded separately for each pollutant and COVID-19 outcome pair for each study. We classify the measure of air pollution exposure as long-term if is one year or longer. Two studies investigated both short-term and long-term exposure and their findings are reported separately for each short- and long-term evaluation. Two studies where the length of pollution measure is unclear (14 evaluations) are omitted.

Figure 5: Direction of the relationship between air pollution and COVID-19 cases/spread, hospitalizations, and deaths split by country. The relationship was coded separately for each pollutant and COVID-19 outcome pair for each study.

Chapter 2

Examining the Effects of Gasoline Prices on Public Support for Climate Policies

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Examining the effects of gasoline prices on public support for climate policies

Ireri Hernandez Carballo[1,2*] and Matthew Ryan Sisco[2,3]

[1]Department of Social and Political Sciences, Bocconi University
[2]RFF-CMCC European Institute on Economics and the Environment (EIEE), Centro Euro-Mediterraneo sui Cambiamenti Climatici
[3]School of Public and International Affairs, Princeton University
*corresponding author email: ireri.hernandez@unibocconi.it

Abstract

Successful implementation of climate policies requires public acceptability. Fuel prices may impact policy support. Here we examine this relationship by analyzing retail gasoline prices and individual-level survey data from Italian and U.S. metropolitan areas. We do not find a significant association between gasoline prices and support for funding renewable energy research, regulating CO2 emissions, and requiring companies to pay carbon taxes. We do find that support for phasing out combustion-engine cars decreases when gasoline prices increase. In exploratory analyses, we find that lower-income and more liberal individuals reduce their support more as gasoline prices rise. Using an additional U.S. nationally representative survey dataset, we find that higher gasoline prices are associated with lower support for environmental protection when it comes at the risk of curving economic growth. The negative effects of gasoline prices on climate policy support do not seem driven by reduced environmental concerns.

Introduction

Mitigating climate change requires public support for policies that reduce greenhouse gas emissions. Balancing trade-offs between environmental sustainability, economic conditions, and energy security is a major challenge in the clean energy transition (1). A key related question is whether shifts in energy prices (caused by climate policies or otherwise) impact public support for climate policies. Understanding whether and how energy prices impact individuals' support for climate policies is important to implement such policies strategically and successfully.

Out of the various types of energy that consumers purchase, gasoline prices are highly visible and engaged with by people frequently, making them arguably the most salient energy prices (2,3). Gasoline prices are mostly determined by the price of crude oil (4,5). Other components include refining costs, distribution expenses, marketing costs, and taxes (6). In the current paper, we analyze changes in retail gasoline prices that are mainly due to oil price fluctuations. Existing research on the impacts of gasoline prices on individual behavior has studied its effects on consumer spending (7) and finds that rising gasoline prices lead to more pessimistic consumer attitudes about the economy (8,9). Here we study the relationship between gasoline prices and individuals' attitudes toward climate policies. Furthermore, we investigate differences across policies and groups of people.

Past work offers mixed predictions for how gasoline prices may affect individuals' worries about climate change and support for climate policies. Some studies find that making the costs of climate

policies more salient to individuals reduces their support. A few studies report that support for renewable energy policies is sensitive to their effects on electricity prices (10–12). Furthermore, support for automotive fuel economy requirements decreases after respondents are informed this would raise new car prices (10). Additionally, some past work suggests that worse economic conditions decrease support of climate change mitigation (13–15) and reduce environmental concern (16). These findings are consistent with a prediction that as gasoline prices increase, support for climate policies will decrease, especially for climate policies the public perceives will increase costs.

In contrast, other past findings would predict that increases in gasoline prices will not decrease climate policy support. One study finds that climate change worry is not significantly related to the price of oil (15). Several studies do not find that economic conditions or financial concerns affect attitudes about climate change. Another study reports that richer countries are less concerned with environmental quality than poorer countries (17). Other studies find that the influence of the state of the economy on support for climate policies is small or not associated with the prioritization of climate policies and that individuals' own economic situations are not significantly related (18,19). This is in line with evidence that many people believe growth and environmental protection are compatible and that when people have to choose, environmental protection is often prioritized (20). Furthermore, there is evidence that climate worry is not crowded out by other concerns (21). The mixed predictions from past findings regarding the important question of how gasoline prices affect climate policies make the current study's aim of answering this question compelling.

Past research has posited a variety of factors that affect climate policy support and pro-environmental decisions more generally. These past findings helpfully contextualize the relationship between gasoline prices and climate policy support. A recent meta-analysis of determinants of public opinion about climate change policies finds that perceived fairness and policy effectiveness are the strongest determinants of public acceptability of climate policies (22). Furthermore, another key predictor of support is policies' perceived economic impacts on people's own households (23). Thus, two potential channels through which energy prices may influence support of climate policies are through their impacts on people's economic situations and perceived policy fairness. People sometimes perceive carbon taxes that increase gasoline prices as unfair because they think gas prices are already high, because of the need to drive, and because of the disproportionate burden for those living in rural areas, among other factors (24).

Additional drivers of support for climate policies include concern about climate change, perception of climate change risk and seriousness (25), and experiences with climate change (26). Other individual factors also play a role – holding self-transcendent values, trusting institutions, and identifying as politically liberal have been found to be positively related with stronger climate policy support (27). The demographic factors of being younger, having higher income, and having completed higher levels of education are found to be weakly positively associated with public support for climate policies (22). While a variety of contextual and individual factors associated with climate policy support have been identified to date, we contribute to the literature by analyzing how gasoline prices impact support for climate policies. Drawing from the past literature, we conduct analyses that examine how this relationship differs across demographic subgroups.

In the current paper, we analyze weekly gasoline price data combined with individual-level survey data to estimate the impacts of retail gasoline prices on climate policy support and related measures. We use two survey datasets, including 1) survey data collected by the authors referred to as the International Panel on Climate Change and Air Pollution (ICAPP) and 2) the Gallup Poll Social Series (GPSS) survey data (28). We first look at impacts on policy support for several climate policies, allowing us to compare the effects of gasoline prices across various policy alternatives. Our analyses use fixed effects panel regression models to estimate these relationships, leveraging time and regional variation in gasoline prices, while controlling for unobserved heterogeneity and time patterns (see Methods for more details). Additionally, we further explore whether the impact of gasoline prices differs according to other individual characteristics related to policy support, namely income and ideology. Furthermore, we utilize the GPSS survey data to analyze how gasoline prices impact individuals' responses to environmental versus economic tradeoffs, and explore potential mechanisms behind our results.

Gasoline prices and climate policy support

First, we analyze the ICAPP survey data with respondents from metropolitan areas in the U.S. and Italy, sampled from December 2019 through December 2020 (more details in Methods). We regress climate policy support on weekly gasoline prices. For our main ICAPP analyses, we analyze the sample of individuals that use a vehicle as their main mode of transportation. Table 1 shows the regression results. This table shows results for four different climate policies separately, as specified in the column titles (columns 1-4), as well as a composite measure (column 5). In all of our results, we adjusted the significance levels using the false discovery rate (FDR) method (29).

Our results suggest that when gasoline prices were higher, individuals decreased their support for phasing out gasoline and diesel vehicles, but we do not see significant effects for the other policies. We include in Table 1 an estimated coefficient for ideology to contextualize the magnitude of the results. Higher ideology scores correspond to being more liberal. A single dollar-per-gallon increase in gasoline prices is associated with a 0.45 point decrease (se: 0.13; 95%CI: -0.70 to -0.20, p-value: 0.003; FDR adjusted p-value: 0.016) in support for the phasing out of combustion-engine cars (on a scale from 1-5). This is about twice as large in magnitude as the association of a one-unit shift in ideology (e.g., going from 'neither conservative nor liberal' to 'slightly conservative' on a 7-point ideology scale) on the support for this policy. The coefficients for all other policies are negative, relatively small in magnitude, and not significantly different from zero.

We implemented several robustness checks. A potential concern is that people were not driving much during the period in which the ICAPP responses were collected (due to COVID-19), which may attenuate our results. We show in the Supplementary Table 1 that our results are similar when excluding the second quarter of 2020, the period in which cities we sampled from were under strict lock down. Furthermore, in the Supplementary Table 2, we show the results for the subset of

individuals that do not use a car as their main mode of transportation (i.e., individuals that walk, take public transportation, or bike). We find a non-significant and near zero impact of gasoline prices for non-car users, implying that the negative results are particular to car users. Furthermore, to analyze country-specific impacts, we estimate results separately by country in the Supplementary Tables 3-4. The country-specific results for the policy to phase out gasoline cars are negative for both the U.S. and Italy. The results for the U.S. sample become non-significant after adjusting for multiple hypothesis testing. The decrease is larger in magnitude and remains significant at the 1% level for the sample in Italy.

Table 1: Estimated impacts of gasoline prices on climate policy support

	(1) Fund renewable research	(2) Regulate CO2	(3) Phase out gas cars	(4) Require fossil fuel companies pay carbon tax	(5) Composite measure
Gas price dollars/gallon	−0.06	−0.02	−0.45*	−0.03	−0.14
	(0.06)	(0.08)	(0.13)	(0.06)	(0.06)
Ideology	0.14***	0.19***	0.22***	0.23***	0.19***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
N	9745	9745	9745	9745	9745

The asterisks indicate the cutoff p-values from a two-sided t-test with the null hypothesis being that the coefficient is equal to zero: ***p < 0.001; **p < 0.01; *p < 0.05. P-values and significance are adjusted for all coefficients using the Benjamini and Hochberg (1995) false discovery rate method using 5 comparisons. In this analysis we use the ICAPP sample and include only individuals using a personal vehicle as their main source of transportation. Dependent variables are specified in the column titles. All specifications include city fixed effects, quarter-of-year fixed effects, city-by-quarter-of-year fixed effects, and individual controls including gender, race, education, age, ideology, income, employment status, regional monthly employment and quarterly GDP. Standard errors are clustered at the city-by-quarter level and shown in parentheses.

Impact of gasoline prices across ideologies

We next show an exploratory analysis of how gas prices relate to support for phasing out combustion-engine cars across political ideologies. Table 2 shows the results of a regression specified similarly to the one described above but additionally including an interaction between gas prices and a continuous measure of ideology. We see that individuals identifying as more liberal have higher baseline levels of support for phasing out gasoline and diesel fueled vehicles relative to more conservative individuals. However, as gasoline prices increase, support decreases more for more liberal individuals, as shown by the significantly negative beta coefficient for the interaction term.

Figure 1 shows the graphical representation for the relationship between gasoline prices and support for phasing out combustion-engine cars, by ideology. The y-intercepts show the main effects of each level of ideology compared to those identifying as extremely conservative. Individuals identifying as somewhat liberal, liberal, and extremely liberal are significantly more likely to support phasing out of gasoline and diesel cars, relative to extremely conservative individuals (see Supplementary Table 5 for the coefficients and significance levels). However, as gasoline prices

increase, individuals identifying as liberal (interaction coefficient: -0.14; 95% CI: -0.22 to -0.05; p-value: 0.007; FDR adjusted p-value: 0.007) and extremely liberal (interaction coefficient: -0.24; 95% CI: -0.36 to -0.12; p-value: 0.002; FDR adjusted p-value: 0.007) decrease their support for phasing out of gas and diesel cars significantly more relative to extremely conservative individuals. One interpretation of the results is that the pattern is due to floor effects for conservatives; given generally lower levels of support, they may have less room to decrease. However, even among extremely conservative individuals, the average support is -0.07 on a -2 to 2 scale which leaves room for them to continue to decrease their support with higher gasoline prices.

Table 2: Ideology heterogeneity in impacts of gasoline prices

	(1) Phase out gas cars	(2) Composite measure
Gas price dollars/gallon	−0.46**	−0.15*
	(0.13)	(0.06)
Ideology	0.31***	0.31***
	(0.03)	(0.02)
Gas price x ideology	−0.03***	−0.03***
	(0.01)	(0.00)
N	9745	9745

The asterisks indicate the cutoff p-values from a two-sided t-test with the null hypothesis being that the coefficient is equal to zero: ***$p < 0.001$; **$p < 0.01$; *$p < 0.05$. P-values and significance are adjusted for all coefficients using the Benjamini and Hochberg (1995) false discovery rate method using 2 comparisons. In this analysis we use the ICAPP sample and include only individuals using a personal vehicle as their main source of transportation. Dependent variables are specified in the column titles. All specifications include city fixed effects, quarter-of-year fixed effects, city-by-quarter-of-year fixed effects, individual controls including gender, education, age, ideology, income, employment status, regional monthly employment and quarterly GDP. Standard errors are clustered at the city-by-quarter level and shown in parentheses.

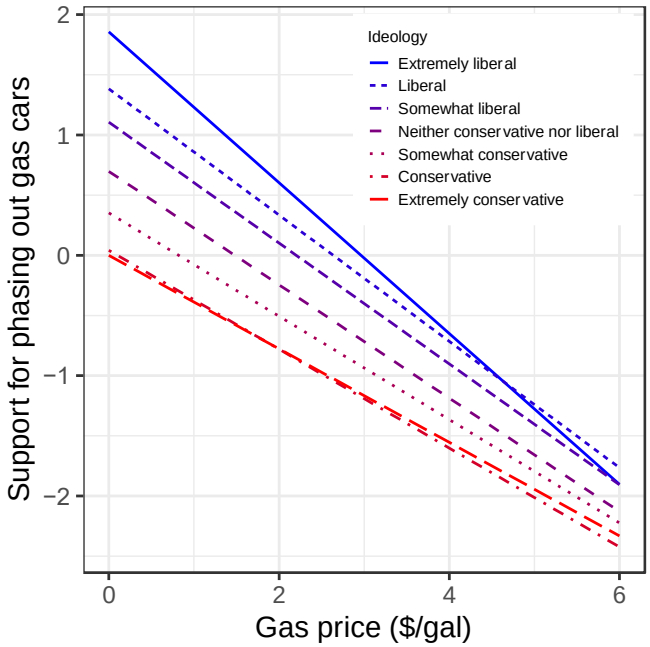


Figure 1. Association between gasoline price and support for fossil fuel car phaseout by ideology. The figure displays the linear projection of the estimated marginal effects of gasoline prices on support for phasing out gasoline and diesel vehicles by ideology. The y-intercept shows the main effect for each ideology group relative to those identifying as extremely conservative (scaled at zero). The interaction terms between gas prices and ideology groups are projected as the slope of the linear projection for each ideology group. Estimates are based on interaction models with fixed effects using the ICAPP data (n = 9745) and coefficients and standard errors are displayed in the Supplementary Table 5, model 1. All estimates control for city fixed effects, quarter of year fixed effects, city-by-quarter of year fixed effects, individual controls including gender, education, age, race, income, employment status, regional monthly employment and quarterly GDP.

Impact of gasoline prices across income levels

Next, we examine exploratory interaction analyses of how gasoline prices impact support for phasing out combustion-engine vehicles according to income levels. Table 3 shows the results from regressing this policy support variable on gasoline prices interacted with three income brackets (with similar sample sizes): below $45k, between $45-89k, and more than $90k. We find that individuals in the middle-income bracket of $45-89k are less likely to support phasing out gasoline and diesel cars relative to those with income less than $45k. However, the positive interaction terms for gasoline prices and the income brackets of $45k-89k and more than $90k in Table 3 indicate that as gasoline prices increase, people in those income brackets decrease their support less relative to those with income less than $45k. Figure 2 shows the visualization of our regression estimates by income brackets (corresponding with the coefficients in Table 3). The y-intercepts show the main effects for income relative to those earning less than $45k (which has policy support scaled to zero). As a robustness check, we also compute the results using six income brackets and find a similar pattern of results which is shown in Supplementary Table 6

Table 3: Income heterogeneity in impacts of gasoline prices

	(1) Phase out gas cars	(2) Composite measure
Gas price dollars/gallon	−0.51**	−0.18**
	(0.12)	(0.06)
Income 45-89k	−0.22***	−0.18***
	(0.05)	(0.04)
Income 90k+	−0.19*	−0.18**
	(0.08)	(0.04)
Gas price x [income 45-89k]	0.06**	0.04**
	(0.01)	(0.01)
Gas price x [income 90k+]	0.10***	0.06***
	(0.02)	(0.01)
N	9745	9745

The asterisks indicate the cutoff p-values from a two-sided t-test with the null hypothesis being that the coefficient is equal to zero: ***p < 0.001; **p < 0.01; *p < 0.05. P-values and significance are adjusted for all coefficients using the Benjamini and Hochberg (1995) false discovery rate method using 2 comparisons. In this analysis we use the ICAPP sample and include only individuals using a personal vehicle as their main source of transportation. Dependent variables are specified in the column titles. All specifications include city fixed effects, quarter-of-year fixed effects, city-by-quarter-of-year fixed effects, individual controls including gender, education, age, ideology, income, employment status, regional monthly employment and quarterly GDP. The base comparison group is composed of those earning less than $45k. Income in U.S. dollars. Standard errors are clustered at the city level and shown in parentheses.

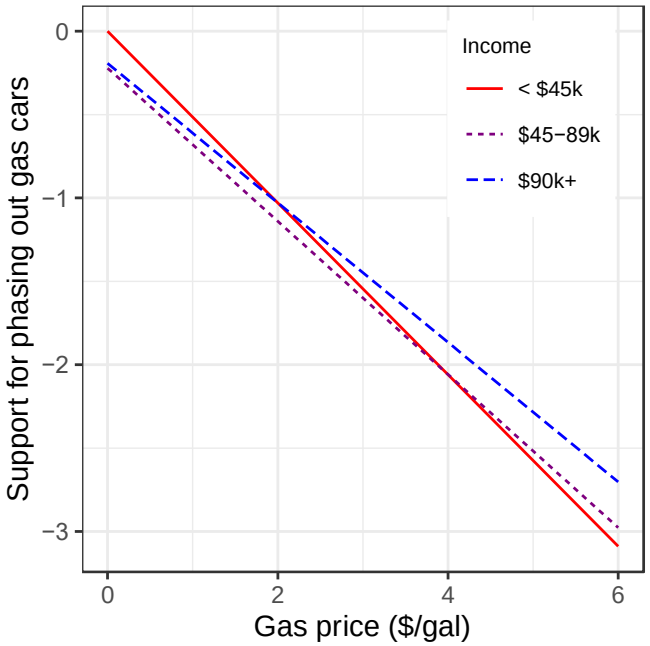


Figure 2: Association between gasoline price and support for fossil fuel car phaseout by income. The figure displays the linear projection of the estimated marginal effects of gasoline prices on support for phasing out gasoline and diesel vehicles by income groups. The y-intercept shows the main effect for each income group relative those earning less than $45k (scaled at zero). The interaction terms between gas prices and income

groups are projected as the slope of the linear projection for each ideology group. Estimates are based on interaction models with fixed effects using the ICAPP data (n = 9745) and coefficients and standard errors are displayed in Table 3 model 1. All estimates control for city fixed effects, quarter of year fixed effects, city-by-quarter of year fixed effects, individual controls including gender, education, age, race, ideology, employment status, regional monthly employment and quarterly GDP.

Environmental and economic tradeoffs and concerns

Now we turn to our analysis of the GPSS data. This sample is representative of the U.S. population and spans the years 2005-2018. When individuals decide on their support for climate policies they may evaluate the perceived tradeoffs of protecting the environment versus 1) the economic costs this may have and/or 2) the impact the policy may have on energy supply. The GPSS survey asks participants to self-report which they think should be prioritized more in these trade-offs. We analyze these responses as dependent variables. Specifically, we examine how gasoline prices relate to individuals' prioritization of protection of the environment at the expense of 1) economic growth and 2) development of fossil fuel energy supplies. We use these questions to shed light on how individuals may think about climate policies that involve energy security or economic tradeoffs.

Table 4 shows the regression results. We find that people are less likely to agree that protection of the environment should be given priority at the risk of curbing economic growth as gasoline prices increase (coefficient: -0.08; 95% CI:-0.13 to -0.02; p-value 0.007; FDR adjusted p-value: 0.013). As we did above, to help contextualize the effect sizes, we also report in Table 4 the coefficient for ideology which is about three times as large as that of the gas price variable. We see the beta coefficient of the impact of a one dollar increase in real retail gasoline prices on how people prioritize the environment versus the production of energy is also negative, but not statistically significant.

Table 4: Impact of gasoline prices on tradeoff variables

	(1) Protecting environment vs econ growth	(2) Protecting environment vs energy production
Gas price dollars/gallon	−0.08*	−0.06
	(0.03)	(0.03)
Ideology	0.23***	0.26***
	(0.02)	(0.01)
N	12769	12769

The asterisks indicate the cutoff p-values from a two-sided t-test with the null hypothesis being that the coefficient is equal to zero: ***p < 0.001; **p < 0.01; *p < 0.05. P-values and significance are adjusted for all coefficients using the Benjamini and Hochberg (1995) false discovery rate method using 2 comparisons. Dependent variables are specified in the column titles. All specifications include state fixed effects, time fixed effects, state-by-3-year fixed effects, individual controls including gender, education, age, ideology, income, employment status, state-level monthly employment and quarterly GDP per capita. Standard errors are clustered at the state level and shown in parentheses. In this analysis we use the GPSS sample using the provided sampling weights to make the results more representative.

Exploring attitudes and worries as potential mechanisms

To explore the possible mechanisms behind the negative association between gasoline prices and climate policy support, we analyze five different variables that are plausible mechanisms: two measures on environmental attitudes, a measure of worry about the environment, a measure of worry about the availability and affordability of energy, and a measure of worry about the economy (see Methods section). Table 5 reports these results. Examining the beta coefficient for gas price, we find that people's worry about the environment remains unchanged as gasoline prices increase, but that worry about energy security and the economy increases. We do not find statistically significant associations between gas prices and individuals' interest in environmental movements or perceptions on how serious the news covers global warming. These results suggest that higher gas prices do not lead to less worry about the environment, interest in protecting the environment, or perceived seriousness of the issue. Thus, we find evidence contrary to the notion that gas prices affect climate policy support by reducing (or crowding out) worry about or motivation to address environmental problems. Rather our results imply that increasing worry about the economy and/or energy supply when gas prices increase could be a mechanism through which increasing fuel prices impact climate policy support for measures perceived as costly to individuals.

Table 5: Mechanisms

	(1) Worry energy	(2) Worry economy	(3) Worry environment	(4) Participate env. movement	(5) Global warming serious coverage
Gas price dollars/gallon	0.15***	0.14***	−0.01	0.06	−0.00
	(0.03)	(0.04)	(0.04)	(0.05)	(0.02)
Ideology	0.06***	0.01	0.23***	0.21***	0.28***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
N	10421	11367	11357	9933	12504

The asterisks indicate the cutoff p-values from a two-sided t-test with the null hypothesis being that the coefficient is equal to zero: ***p < 0.001; **p < 0.01; *p < 0.05. P-values and significance are adjusted for all coefficients using the Benjamini and Hochberg (1995) false discovery rate method using 2 comparisons. Dependent variables are specified in the column titles. All specifications include state fixed effects, time fixed effects, state-by-3-year fixed effects, individual controls including gender, education, age, ideology, income, employment status, state-level monthly employment and quarterly GDP per capita. Standard errors are clustered at the state level and shown in parentheses. In this analysis we use the GPSS sample using the provided sampling weights to make the results more representative.

Discussion and conclusions

We evaluated the impacts of gasoline prices on public support for a variety of climate policies. We find that support for phasing out combustion-engine vehicles is negatively associated with retail gasoline prices. However, we do not find significant associations between gas prices and policy support for funding renewable energy research, regulating CO2 emissions, or taxing fossil fuel companies,

policies where the economic costs may be less salient and direct to consumers. In the GPSS analyses we find marginal evidence that when gas prices are higher, people are less likely to support environmental protection at the risk of curving economic growth. As fuel prices increase, the decreased prioritization of environmental protection does not appear to be due to a reduction in concern for the environment, but rather from increased economic and energy supply concerns. We find that although individuals worry more about the economy and energy security as gasoline prices rise, their worry about the environment is not associated with gasoline prices.

We report some differences in the effects of gasoline prices on support for phasing out combustion-engine vehicles based on people's ideology and income. Specifically, in the ICAPP analyses we find that while liberal individuals are more likely to support the phaseout of combustion-engine vehicles, they decrease their support of this policy more as gasoline prices increase. This may be due to liberal individuals having a higher baseline support for the phaseout to begin with. We also find that, relative to those in higher income groups, lower-income individuals decrease their support of this policy by more as gasoline prices increase. This suggests that the negative effects of gasoline prices on support for phasing out combustion-engine vehicles may be driven by economic or financial concerns. These results regarding the roles of ideology and income are exploratory and this is an important potential avenue for future research.

We find gas prices lower support for phasing out combustion-engine vehicles for individuals that currently use a personal car as their main mode of transportation. We do not find this relationship among non-car users. This is consistent with the fact that gasoline price increases affect those who use a personal vehicle more immediately and directly compared with those who use other modes of transportation.

One interpretation of the policy-specific results is that higher gasoline prices decrease support for phasing out combustion-engine vehicles due to elevated economic pressures. This is in line with evidence that the perceived economic impact on one's own households is negatively associated with climate policy support (23). As gasoline prices increase, we find lower support for the climate policy that would lead individuals to incur direct and salient costs to which gasoline prices are most relevant. Additionally, the command-and-control feature of the phaseout may be perceived as more costly than the other proposed market-based policies or the proposal to fund more research into renewable energy sources, for which we find no significant results. However, we do not make any strong conclusions on the non-significant findings. Furthermore, we do not directly test the hypothesis that increased costs are the mechanism by which gasoline prices affect support for phasing out combustion-engine vehicles and encourage future research to further investigate this and other mechanisms more closely.

Higher gasoline prices increase the costs of using combustion-engine vehicles and one therefore might expect preferences for this type of vehicle to decrease, increasing support for their phaseout. However, higher gasoline prices also decrease individuals' purchasing power, which may temporarily make phasing out combustion-engine vehicles less attractive, due to large switching costs. Vehicles are durable goods and combustion-engine vehicle users would have to substitute theirs with electric ones or change their mode of transportation if a ban were to take place. We expect that in the short term, with increasing fuel costs individuals decrease their support for the phaseout itself, not necessarily lowering their interest in alternative-fuel vehicles. In the long term, if individuals were to experience

consistently high gasoline prices, we would expect them to consider not only upfront switching costs, but also lifetime operating costs, which would increase for combustion-engine vehicles.

One of the strengths of our analyses is the use of multiple datasets that complement each other and provide a cross-cultural view of the relationship between gasoline prices and support for climate policies. Banning the sales of new gas-powered vehicles within the next two decades is a relevant climate policy that has been passed across various jurisdictions. The European Union, California, and New York State have introduced these bans in their efforts to address climate change. While most of these bans take the form of restrictions on the sales of new combustion-engine vehicles, we believe our results can provide insights for these types of policies. Future research could study how individuals react to the implementation of these bans and test the mechanisms proposed here more carefully. For example, these restrictions are likely to be more costly in non-urban areas where people rely more heavily on cars since there are fewer alternatives. Thus, the resistance to banning combustion-engine vehicles may be even stronger in these areas, where policies such as carbon taxing are perceived as particularly unfair (24). We think our results for samples from metropolitan areas are likely a lower-bound estimate for what we would find if sampling rural populations and encourage further analyses on this.

Our study points to several promising areas for future research in addition to those mentioned above. In the current analyses, we focus on retail gasoline prices which are driven mostly by oil market fluctuations. One avenue is to investigate is whether people respond differently to gasoline price changes that come from carbon pricing and gasoline taxes instead of oil market fluctuations, as individuals may react differently to policy-driven rather than market-driven price changes (30,31). Another consideration is that peoples' perceptions of gasoline prices are likely dependent on reference points (32) and future work could investigate how sensitive individuals are to deviations from these.

We encourage future research to study the association between gasoline prices, other energy prices, and additional climate policies. The current study is limited by analyzing only a subset of relevant climate policies and prices for one type of energy. Response to the policies are likely dependent on the salience of the energy price that is the most relevant for each policy. For instance, in line with our results, support for renewable energy policies has been found to be sensitive to electricity prices (10–12).

While we found that gasoline prices are negatively associated with support for phasing out gasoline-powered cars and have no significant association with the other specific policies we evaluated, responses to other types of climate policies may be positive. For example, increases in gasoline prices may induce more openness to policies favoring alternative technologies such as subsidizing electric vehicles or expanding clean public transportation. Previous research finds that pull-measures such as subsidies for electric vehicles (EVs) and charging infrastructure attract more support than regulation to phase out fuel cars or carbon taxes (33). Furthermore, studies on the impact of gasoline prices on vehicle fleet composition find that individuals adopt more fuel-efficient and hybrid vehicles when gasoline prices increase (34–38).

Our results have important policy implications for the timing and framing of climate policies. Our findings imply that the phasing out of combustion-engine vehicles may face less public support when

fuel prices are high. It may be more politically feasible to introduce such bans when gasoline prices are low or in parallel with policies that improve affordable access to alternative clean energy modes of transportation. Additionally, since we find that individuals worry about the economy more as gasoline prices increase, compensating individuals for their losses due to climate policies or increases in transportation costs may reduce public resistance (39).

Strategic communication and framing of climate change policies that are perceived as costly can be critical to maximize public support for them. For some climate policies, such as phasing out combustion-engine vehicles, citizens may be unclear about how the policy mitigates climate change and what the direct costs would be to them. They may perceive the policy as unfair or costly in the context of high fuel prices. This is in line with Douenne and Fabre (40), who find that opposition to carbon pricing in France during the Yellow Vest movement, in the context of rising gasoline prices, was driven by misconceptions about its effectiveness, fairness, and its impact on individuals' purchasing power. Communicating to the public the expected costs and benefits to them can help minimize reluctance due to uncertainty or overestimation of a policy's costs to individuals (23).

Methods

International Panel on Climate Change and Air Pollution (ICAPP) data

This study complies with all relevant ethical regulations. Ethical approval for research with human subjects was provided by the Human Research Protection Office Institutional Review Board at Columbia University (IRB-AAAS3097), by the Research Integrity and Assurance Institutional Review Board at Princeton University (IRB# 11220), the Bocconi Ethical Committee for Research at Bocconi University, and by the Guanghua School of Management Institutional Review Board at Peking University. Informed consent was obtained from all participants before they answered the survey. Participants received compensation for their participation.

Our first set of analyses use individual measures of policy support from a longitudinal online survey fielded from Dec 3, 2019 through Dec 31, 2020. We analyzed data collected from individuals living in the metropolitan areas of four cities: New York City, Dallas, Milan, and Rome. The main reason we surveyed in metropolitan areas (instead of distributing responses across the countries) was to have larger sample sizes in local areas to measure responses to local events. Prior to collecting data, we conducted power analyses and confirmed that with our sample sizes at the metropolitan level we had enough power to detect moderately sized effects from locally changing variables (such as local energy prices). Sampling participants from metropolitan areas of cities rather than city centers alone gives us a wider range of demographics. We chose not to sample participants at the national level, as this would not have given us sufficient statistical power to analyze effects of local events and locally changing variables.

Since we surveyed a set of cities each with its own demographic distribution, we found it infeasible to target quota samples specific to each city as this was outside of the scope of what our participant recruitment firm could execute. However, we decided a feasible and sensible solution was to set common quotas across cities. The respondents were balanced on ideology, gender, and age (i.e., we ensured comparable sample sizes in each bracket of ideology, gender, and age). We used quota

sampling, with our quotas being; female: 51%, male: 49%, age 18-34: 33%, age 35-54: 33%, age 55+: 33%, political affiliation – conservatives: 35%-65%, liberals: 35%-65%. Participants responded to the survey on a near daily basis across the full time frame of the study.

All survey questions were originally devised in English, then translated by native speakers to Italian, for the Italian sample. To ensure the original meaning was preserved in these translations, the questions were back-translated into English by different translators and any discrepancies in meaning were resolved.

A single participant recruitment firm was in charge of recruiting all participants. The median completion time was 6.18 minutes for the full survey. Participants that sped through the study (faster than three minutes completion time, as this is the minimum amount of time necessary to complete the survey while meaningfully answering the questions) and those that failed an attention check were automatically excluded by our recruitment firm. We also excluded participants who answered with non-sensical open-ended responses (responses that were clearly unrelated to the survey question, indicating that the participant did not read the question). Non-sensical responses were assessed by a single coder and only indisputably non-sensical responses were excluded. For example, responses like "asdlkfjasld3" or "blah blah blah" would be deemed non-sensical responses. Overall, 1.5% of responses were removed for being deemed non-sensical.

In our main analyses we restrict our sample to individuals that use a personal vehicle, since these are the individuals most impacted by gasoline prices, but we also carry out robustness checks on the sample of individuals whose main mode of transportation is not a personal vehicle. We also restrict our sample to those that are first-time survey respondents since some individuals participated more than once and responses would not be independent for individuals that are repeatedly surveyed, violating I.I.D. sampling if not accounted for. This leads to a total of 3,728 individuals from Italy (Milan and Rome) and 6,017 individuals from the U.S. (New York City and Dallas). Our final ICAPP sample of 9745 participants is 50% female and all participants are 18 and older. The full sample characteristics can be found in Supplementary Table 11. Our focus is on particular survey questions related to climate policy support. We analyze the answers to four questions (questionnaire section shown in Supplementary Figure 1), asking individuals how much they support or oppose (ranging from 1 to 5: strongly oppose, somewhat oppose, neither support nor oppose, somewhat support, and strongly support) the following policies:

1. "Fund more research into renewable energy sources, such as solar and wind power."
2. "Regulate carbon dioxide (the primary greenhouse gas) as a pollutant."
3. "Phase out gasoline and diesel fueled cars in the [local city] area within the next 10 years."
4. "Require fossil fuel companies to pay a carbon tax."

We also constructed a composite measure, defined as the average support across all 4 policies. For demographic variables, we measured age bracket, income bracket, ideology (coded so that higher values indicate more left-leaning), gender (coded female = 1), and education level.

Gallup Poll Social Series (GPSS) data

For our second set of analyses, we use the 2005-2018 Gallup Poll Social Series (GPSS) survey data (28). The GPSS data complements the data of our first study as it covers a longer time span of multiple years, which allows us to analyze how responses change with larger gasoline price fluctuations. Furthermore, this dataset is representative of the U.S. population, and includes more non-urban respondents. The Gallup organization during the time period of our data conducted yearly public opinion surveys on topics related to the environment in March via telephone interviews on a random sample of around 1,000 adults per year. The respondents are aged 18 and older living in all 50 U.S. states with representative quotas for each region. Gallup survey respondents are sampled to match the U.S. population according to gender, age, race, Hispanic ethnicity, education, region, population density, and phone type used. We use the provided population weights in our analyses.

Our measures of support for environmental policies come from questions asked about trade-offs between environmental protection and energy production/the economy. Specifically, we analyze responses to the following question (environment vs. energy production):

> "With which one of these statements about the environment and energy production do you most agree: development of U.S. energy supplies – such as oil, gas and coal – should be given priority, even if the environment suffers to some extent (or) protection of the environment should be given priority, even at the risk of limiting the amount of energy supplies – such as oil, gas and coal – which the United States produces".

We also analyze responses to a question on prioritizing the environment versus the economy:

> "With which one of these statements about the environment and the economy do you most agree: economic growth should be given priority, even if the environment suffers to some extent (or) protection of the environment should be given priority, even at the risk of curbing economic growth."

We scale responses from -1 (prioritize energy/the economy) to 1 (prioritize protection of the environment). These questions were asked every year from 2005-2018.

Furthermore, to study other measures of environmental attitudes, we analyze responses to the following question:

> "Thinking specifically about the environmental movement, do you think of yourself as – unsympathetic towards the environmental movement, (or) neutral, (or) sympathetic towards the movement but not active, (or) an active participant in the environmental movement."

We scale answers from -1.5 (unsympathetic) to 1.5 (active). We also analyze a question regarding news coverage of global warming:

> "Thinking about what is said in the news, in your view is the seriousness of global warming – generally exaggerated, (or) generally correct (or) generally underestimated?"

We scale answers from -1 (exaggerated) to 1 (underestimated).

Finally, to investigate mechanisms through which gas prices may affect policy support, we analyze several questions regarding individuals' worries. Specifically, the survey included the following section:

> "Next I'm going to read a list of problems facing the country. For each one, please tell me if you personally worry about this problem a great deal, a fair amount, only a little, or not at all. First, how much do you personally worry about..."

Among the list of items, the following were included:

1. The quality of the environment
2. The availability and affordability of energy
3. The economy

We code answers from 0 to 3, with zero being "not at all worried" and 3 being "a great deal".

Other data

In supplementary robustness checks, we complement our analysis with nationally representative U.S. data of similar policy-specific questions from Yale's Climate Change in the American Mind (CCAM) dataset (41,42) shown in Supplementary Table 10. All the robustness checks mentioned can be found in the Supplementary Information section.

Fuel price and economic data

For the U.S., we use Energy Information Administration weekly regional gasoline prices at the Texas and New York state level for the ICAPP analysis and at the Petroleum Administration for Defense Districts level for the GPSS analysis. We obtain daily-level Italian gasoline prices from the Italian Ministry of Ecological Transition, which we aggregate at the weekly province level for Milan and Rome. We convert all gasoline prices to 2020 dollars per gallon for the ICAPP analyses and to real 2012 dollars per gallon for the U.S. GPSS analyses.

We include in our analyses time-varying region-specific variables that if omitted could be confounding the effect of gasoline prices on policy support. We use the most regionally disaggregated data that is available at the quarterly level. For the U.S., we use monthly metro-area employment data from the U.S. Bureau of Labor Statistics and quarterly state-level GDP data from the U.S. Bureau of Economic Analysis. We obtain national Italian employment and quarterly GDP data from the National Statistics Institute (ISTAT). While the more granular metro-area GDP and unemployment data is unavailable quarterly, the measures we use should be highly correlated with the local municipality employment dynamics.

Estimation approach

Our analyses use individual measures of policy support merged with data on regional retail gasoline prices during the weeks individuals were interviewed. In the ICAPP analyses, we leverage

weekly variation in gasoline prices while in the longer-term multi-year GPSS analyses the variation is at the yearly level. We furthermore incorporate regional variation in gasoline prices across Italian and U.S. cities in the ICAPP analysis and U.S. regions in the GPSS analysis. Our regressions control for individual covariates that have been found to be associated with environmental policy support by past literature including age, income, education, race, and ideology. Our specifications include region (city in the ICAPP sample and state in the GPSS sample) fixed effects, which condition out time invariant unobservables that vary across regions and could confound the effect of interest. Furthermore, time fixed effects are used to capture unobserved drivers of policy support that impact all regions but vary over time. Therefore, time-varying unobservables within regions that are not captured with our time controls could be confounders. Of particular concern would be regional time-varying variables that are correlated with gasoline prices and influence support for environmental policies. To address this we include region-time fixed effects and include regional monthly employment and quarterly GDP to control variation in support for environmental policies that might be driven by the effects of gasoline price changes on the local economy.

Our main regression specification is a fixed effects model as follows:

$$Y_{irt} = \beta gasprice_{rt} + \sigma X_i + \pi Z_{rt} + \gamma_r + \delta_t + \eta_{rt} + \varepsilon_{irt} \quad (1)$$

Y_{irt} is the outcome variable (stated support for gasoline car phaseout, support for other climate policies, energy vs environment trade-off, etc.,) for individual *i* in region *r* at time *t*. We test the model separately for each outcome variable. Gas prices are weekly retail gasoline prices, all converted to 2012 U.S. dollars per gallon, at the state level in the ICAPP analysis and at the Petroleum Administration for Defense District level in the GPSS analysis, during the week individuals were surveyed. X_i are individual covariates such as gender, education, age, ideology, and employment status. Z_{rt} are region- and time-varying economic variables including monthly employment and quarterly GDP of the relevant time period. γ_r is a region fixed effect (city for the ICAPP analyses and state for the GPSS analyses), and δ_t are time fixed effects (quarter of year for the ICAPP analyses and 3-year time periods for the GPSS analyses). Finally, as there may be region-specific variation in the evolution of omitted variables, we add region-by-time fixed effects, η_{rt} ;these allow to flexibly control for other unobserved factors that are changing across time in each specific region (city in the ICAPP analysis and state in the GPSS analysis), separately controlling for time-varying responses that are region-specific.

In additional analyses, we interact the gas price variable with a variable measuring individuals' ideology and income separately to study heterogeneous responses across groups. We cluster standard errors at the city-by-quarter level in the ICAPP analyses and at the state level in the GPSS analyses since residuals are likely to be correlated within quarter and regions (Cameron & Miller, 2015). In robustness checks, we estimate the results separately by country (Supplementary Tables 3 and 4). Our main identification assumption is that there are no omitted region-specific characteristics correlated with gasoline prices (at the weekly level in the ICAPP analysis and for the week the respondent was surveyed in the GPSS analysis) that change over time and impact support for climate policies. We feel

this is a relatively safe assumption given our inclusion of region-specific, time-varying control variables.

We report several robustness checks. A potential concern with the GPSS analyses is that less populated states may have individuals that are outliers which are driving the results. However, we show in Supplementary Tables 7-8 that that our results are robust to omitting the 5 least-populated states. Another concern may be that we are capturing changes in prices due to environmental policies driven by already-changing climate policy support, leading to reverse causality. For example, California's cap-and-trade 2013 policy started to cover transportation fuels in 2015 and might have impacted gasoline prices there. We control for this by adding a dummy variable for California post-2015. Our results remain robust and similar as shown in Supplementary Table 9. To the best of our knowledge, there were no other climate policies that significantly changed gasoline prices during the time of our study mitigating concerns for reverse causality (i.e., environmental policy support being the main driver of gasoline prices). Finally, we complement our main analyses with an additional analysis of policy-specific questions from Yale's Climate Change in the American Mind (CCAM), a U.S. representative survey dataset, shown in Supplementary Table 10. In line with the ICAPP results, we find only a relatively small and marginally significant impact of gasoline prices on support for regulating CO2 and funding renewable research.

While we control for many possible omitted factors through fixed effects, given the non-experimental framework we cannot be sure we have controlled for all omitted factors that are correlated with gasoline prices and may influence support for climate policies. There may still be omitted variables that vary at other, more granular time frequencies. In the ICAPP analyses, the concern would mainly be about unobserved within-region changes at the weekly level that are correlated with gasoline prices and impact climate policy support. In the GPSS analyses, the changes are on a longer-time spans and therefore omitted variables are more likely to co-vary with gasoline prices. If gasoline prices are correlated with omitted factors that drive policy support, such as policy trust and perceived effectiveness, climate-policy specific beliefs, or climate change attitudes, our results could be confounding these effects. However, we find that gasoline prices are not significantly correlated with worry about the environment, suggesting that changes in climate change attitudes are an unlikely omitted or co-determinant factor in the main regressions.

Data availability

The ICAPP data analyzed in the current study are not currently publicly available due to ongoing unpublished analyses but available from the authors upon reasonable request. The retail gasoline price data we used can be retrieved from the U.S. Energy Information Administration (EIA) (https://www.eia.gov/petroleum/gasdiesel) for the U.S. and for Italy from the Italian Ministry of Environment and Energy security (https://dgsaie.mise.gov.it/open-data). U.S. state and regional employment data is available from the U.S. Bureau of Labor Statistics (https://download.bls.gov/pub/time.series/la/la.data.2.AllStatesU https://download.bls.gov/pub/time.series/la/la.data.60.Metro. GDP data is available from the U.S. Bureau of Economic Analysis (https://apps.bea.gov/regional/histdata/index.cfm). Italian

employment and quarterly GDP data from the National Statistics Institute (ISTAT) (http://dati.istat.it/Index.aspx?QueryId=12578&lang=en http://dati.istat.it/Index.aspx?QueryId=25308&lang=en). The Gallup survey data we analyzed are retrievable from the Gallup organization with an appropriate license. Data from Climate Change in the American Mind: National Survey Data on Public Opinion (2008-2022) is from the following website https://osf.io/k9sfc

Code availability

The replication code used to produce the figures and analyses in this study is available from the corresponding authors on reasonable request.

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Contributions

M.R.S. developed and oversaw the ICAPP survey data collection. M.R.S. and I.H.C jointly conceptualized the study design, developed the analysis, collected, analyzed, and interpreted the data, and wrote and edited the paper.

Competing interests

The authors declare no competing interests.

Ethics statement

Ethical approval for research with human subjects was provided by the Human Research Protection Office Institutional Review Board at Columbia University (IRB-AAAS3097), by the Research Integrity and Assurance Institutional Review Board at Princeton University (IRB# 11220), the Bocconi Ethical Committee for Research at Bocconi University, and by the Guanghua School of Management Institutional Review Board at Peking University.

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Chapter 3

Energy Costs and the Political Economy of the Green Energy Transition: Evidence from the EU

Energy Costs and the Political Economy of the Green Energy Transition: Evidence from the EU

Ireri Hernandez Carballo*

Department of Social and Political Sciences, Bocconi University
RFF-CMCC European Institute on Economics and the Environment

Abstract

Do changes in energy prices impact individuals' attitudes towards the energy transition? Global energy markets are highly interconnected, making them a source of globalization that can impact public policy preferences. Shifts in energy prices due to the energy transition, global supply shocks, or other crises impact household energy expenditures, costs of living, income, and inequality. A key question is how individuals will react to shifts in energy prices: will this affect environmental concern, public support of climate and energy policies, or other political attitudes? Which regions and social groups are more likely to be affected and respond more? These relationships are important understand to successfully manage the energy and climate transition. I study changes in attitudes in the time frame of the 2022 Russian invasion of Ukraine, which led to a drastic increase in energy prices. I combine energy price data with geographical variation in energy use in an instrumental variables framework to study individual-level changes in public attitudes to answer this question. The focus is on the EU, which in 2020 imported 57% of the energy it consumed, 24.4% coming from Russia. Initial results suggest that higher in energy prices increase energy concerns but have very small relative impact on environmental concerns, suggesting that worry about climate change is not crowded out by higher energy prices in this context.

1 Introduction

Avoiding the catastrophic consequences of climate change requires public support for policies that advance the transition away from polluting energy sources. However, this transition involves substantial structural and social changes, raising complex challenges. A central concern is the energy trilemma of balancing energy security, sustainability, and affordability (Costa-Campi et al., 2017). These trade-offs are likely to shape public support for clean energy policies, making it essential to understand and manage their implications.. In particular, increases in energy prices (whether driven by carbon pricing or global shocks) affect household energy expenditures and overall cost of living, with potential consequences for inequality (Känzig, 2023; Schulte and Heindl, 2017). Do energy prices impact individuals' attitudes towards the energy transition and their political behavior? Which regions and sociodemographic groups are more likely to be affected and how do they respond? This project aims to answer these questions.

*ireri.hernandez@unibocconi.it

Energy prices are important to study for several reasons. First, global energy markets are highly interconnected, making them a source of globalization that can impact public policy preferences. Second, energy price increases can disproportionately affect lower-income households, which allocate a larger share of their income to energy expenditures, raising concerns about exacerbating inequality (Pizer and Sexton, 2019). Third, environmental issues and policies have become highly politicized and parties have increasingly used environmental, economic, and anti-establishment appeals to attract voters.

Understanding the relationship between energy prices and public support for policies is essential to understand how policy-makers can effectively communicate, manage public expectations, and design of measures that mitigate inequality concerns while advancing climate goals. This is particularly important in the European Union, which has adopted a set of ambitious proposals to reduce net greenhouse gas emissions by at least 55% by 2030, relative to 1990 levels, making it one of the world leaders in climate action. While the EU is a global leader in climate policy, public backlash against rising energy costs, underscores the need to understand how to balance climate goals with socioeconomic considerations.

Past literature finds that individuals often reduce their support for climate policies when the associated costs are salient (Drews and van den Bergh, 2016). For instance, support for renewable policies decreases when individuals are informed of increased electricity prices (Bergquist et al., 2020; Stokes and Warshaw, 2017). Similarly, a rise in non-VAT taxes on household electricity bills has been shown to increase the proportion of people who perceive renewable energy targets as overly ambitious(Aklin, 2021). Similarly Hernandez Carballo and Sisco (2023) find that higher gasoline prices can reduce support for phasing out fossil-fuel cars, but find no impact on support for other climate policies. Their results suggest that lower support for phasing out gasoline cars is driven by increased fear about the availability and affordability of energy and economic worry, rather than by decreased concern about the environment.

Evidence also suggests that financial costs of environmental policies can influence voting behavior and political backlash. For example, Dutch households affected by increased taxes on natural gas consumption became more likely to vote for the radical right party and less sympathetic toward the Green Party (Voeten, 2022). Similarly, individuals who experienced losses due to uncompensated bans on polluting vehicles were more inclined to vote for the right-wing populist party in Milan, Italy (Colantone et al., 2022). In the U.S., fossil fuel phase-outs have also been linked to voter backlash (Egli et al., 2022). Furthermore, Furceri et al. (2023) find that market-based climate change policies can result in political backlash if they are adopted during periods of high oil prices, close to elections, or in countries heavily reliant on fossil-fuels.

Furthermore, beyond the direct costs of environmental policies, broader economic conditions can significantly affect support for climate policies. Research has found a negative correlation between GDP and environmental concern (Shum, 2012; Duijndam and van Beukering, 2021), as well as between unemployment rates and climate change attitudes (Brulle et al., 2012). More recently, Bez et al. (2023) use a quasi-experimental design to show that exposure to international trade reduces green voting and worsens environmental attitudes. However, the evidence is mixed. Some studies find little to no relationship between economic conditions or financial concerns and attitudes about

climate change. For example, Kachi et al. (2015) argue that the state of the economy has minimal impact on climate policy support, and that individuals' personal economic situations are often non-significant predictors. Similarly, Mildenberger and Leiserowitz (2017) report that the state of the economy and individual income are not associated with the prioritization of climate policies. Drews et al. (2018) find that individuals view economic growth and environmental protection as compatible and, when forced to choose, often prioritize environmental protection.

This paper contributes to this growing literature by addressing key gaps. Most existing studies focus on hypothetical price increases driven by climate policies (e.g. in survey experiments by Bergquist et al. (2020); Stokes and Warshaw (2017)) or on price increases resulting from taxes (Voeten, 2022; Aklin, 2021). Few examine how global energy price fluctuations shape public attitudes toward the energy transition, (Hernandez Carballo and Sisco, 2023, e.g.,), despite the fact that a large portion of variation in energy prices comes from global market dynamics. This study analyzes the public response to a significant global shock in energy prices, employing a quasi-experimental design across a large set of countries undergoing significant environmental policy changes. Additionally, this study contributes to the broader literature on globalization, economic shocks, and political backlash by analyzing the specific case of energy markets. Through this approach, I aim to shed light on the complex dynamics between energy prices and public attitudes in the context of the clean energy transition.

2 Data

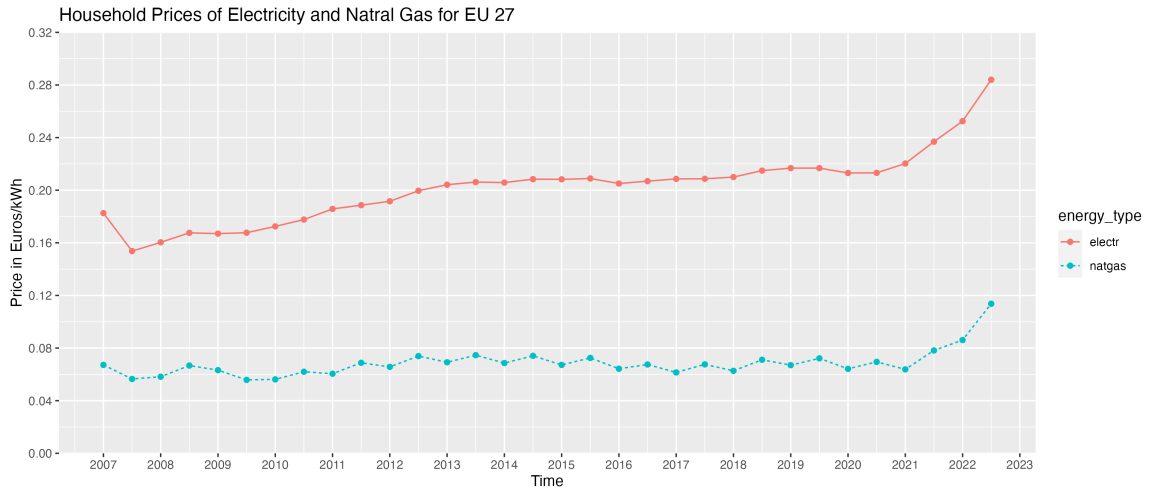
2.1 Energy Data

Residential retail energy price data and regional fuel mix shares come from Eurostat, the statistical office of the European Union. I use the biannual data on natural gas and electricity prices for household consumers. The prices are collected according to different bands of consumption. [1] For both, I use the prices of the middle consumption band, converted to euros per Kilowatt-hour equivalent and inclusive of taxes and levies, to get to the closest retail price that consumers pay. Figure 1 shows the evolution of the prices across the European Union 27 countries.

Figure 2 shows the change in electricity (top) and natural gas prices (bottom) for household consumers in the first half of 2022 compared with the first half of 2019. The decreases in electricity prices in some regions are due to government subsidies and allowances, while in Hungary, the prices for both electricity and natural gas are regulated. Figure 3 shows the mix of energy in EU countries in the residential sector in 2020.

[1] For natural gas the bands are; D1: Customers consuming less than 20 GJ. D2: Customers consuming 20 GJ or more but less than 200 GJ. D3: Customers consuming 200 GJ or more. For electricity the bands are; DA: customers consuming less than 1000 kWh. DB: customers consuming 1000 kWh or more but less than 2500 kWh. DC: customers consuming 2500 kWh or more but less than 5000 kWh. DD: customers consuming 5000 kWh or more but less than 15000 kWh. DE: customers consuming 15000 kWh or more.

Figure 1: EU-27 Household Energy Prices



Note: Prices for the medium consumption band including taxes and levies. Data from Eurostat.

2.2 Public Opinion Data

The Eurobarometer conducts public opinion surveys in Europe several times per year with around 1,000 individuals per country. I use the latest 5 standard Eurobarometer waves (2020-2022) [2]. As part of the questionnaire, individuals are asked to choose from a list the top two issues perceived as the most important problems in their country of residence. For each respondent, I create an environmental concern dummy variable = 1 if individuals mentioned "the environment and climate change" among their top two issues. Similarly, I create an energy concern dummy variable = 1 if individuals mentioned "energy supply" among the top two most important issues facing their country.

Furthermore, individuals are asked: "In your opinion, which of the following objectives should be given top priority in a European Green Deal, a set of measures to protect the environment and fight climate change?" I code two separate dummy variables equal to one if individuals include 1) "Reducing energy consumption" or 2) "Developing renewable energy".

Finally, the following is asked: "What is your opinion on each of the following statements? Please tell for each statement, whether you are for it or against it." I code a binary variable equal to one if the individual answers that they are in favor of : "A common energy policy among EU Member States."

Figure 4 below show the trends across time in these variables at the aggregate European level.

Other measures I will add in the future include the changes to the following EU Green Deal objectives:

[2]In the future I plan to add more past Eurobarometer waves and to incorporate responses from special climate change modules, which are collected approximately every two years

Figure 2: Change in energy prices in % from 1st half of 2022 vs 1st half of 2019

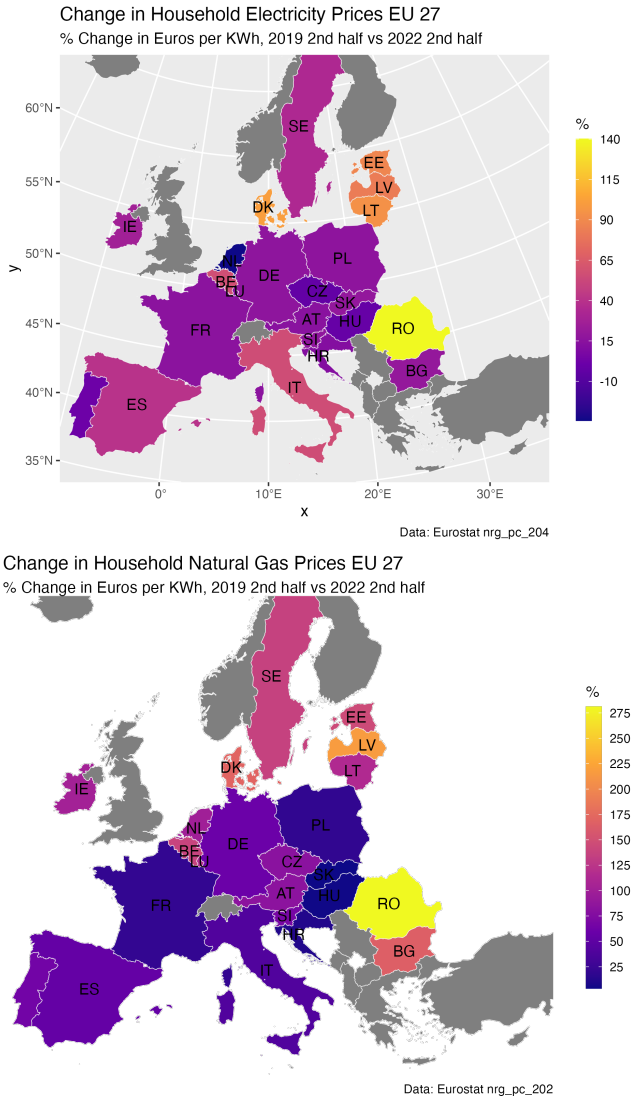


Figure 3: 2020 Country-specific energy mix in the residential sector

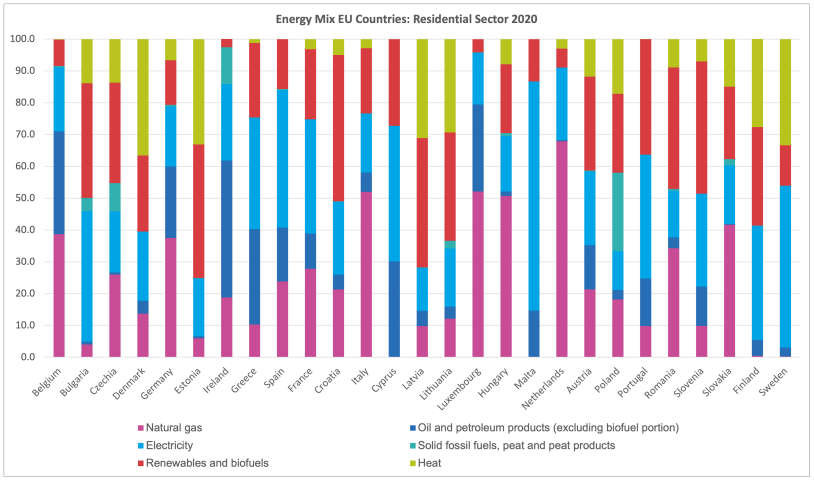
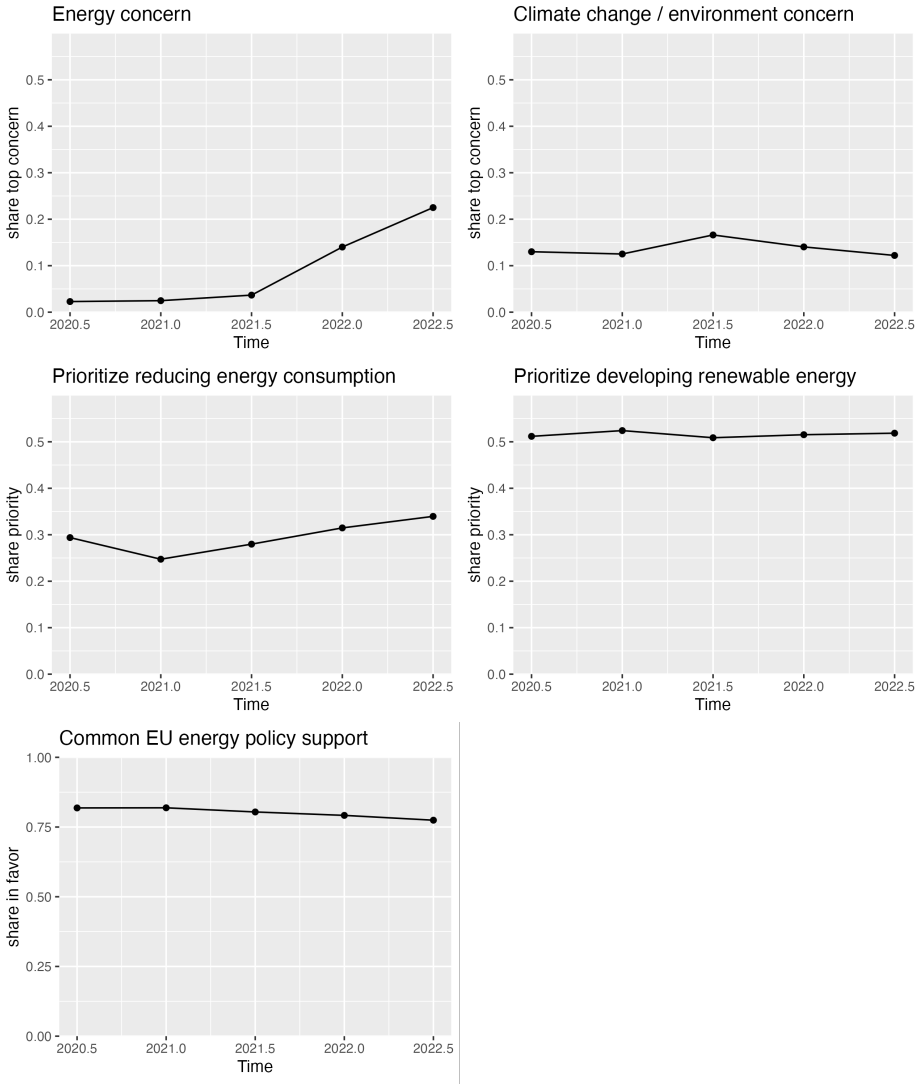


Figure 4: Eurobarometer trends in public opinion



"Measures to make the EU climate-neutral by 2050 by reducing greenhouse gas emissions" and "Taking measures to reduce the risk of environmental disasters".

Furthermore, the survey includes individual demographic variables. Female is a dummy variable = 1 if respondent identifies as female). Ideology is the answer to the following question "In political matters people talk of "the left" and "the right". How would you place your views on this scale?" (on a 1-10 scale, with 1 being left and 10 being right). Occupational category is coded from 1-18 based on different occupational categories.

2.3 Other data

Data on seasonally adjusted chained GDP (in 2005 dollars) in and the employment rate come from Eurostat and are aggregated at the bi-yearly level for each country. In future analyses, I will

aggregate to the smaller geographical level available in the Eurobarometer (mostly NUTS 2).

3 Empirical Strategy

To get at exogenous variation in energy prices, I use the fact that the invasion of Ukraine led to a large unexpected increase in energy prices. The focus in on the EU, which depends on energy imports to a great extent; in 2020 it imported 57% of the energy it consumed, 24.4% coming from Russia (Eurostat, 2022 [3]). While Europe is undergoing a continent-wide energy crisis, the direct impact differs widely according on the import dependency and energy mix of the country. I use the fact that countries have different pre-war (2020) shares of fuels in their final energy consumption mix, creating differential price shocks across regions that are expected to vary according to these shares.

This results in the following sources of variation: i) cross-country variation in energy mix used, ii) over time variation in energy prices induced by the invasion of Ukraine. I thus define country-specific shocks by interacting changes in the country-specific prices of a given fuel type in with the pre-Ukraine invasion share of that fuel used in that country's energy mix.

I am interested in the following structural equation:

$$Y_{irt} = \beta x_{rt} + \sigma W_i + \pi D_{rt} + \gamma_r + \delta_t + \varepsilon_{irt} \tag{1}$$

where r indexes country, t indexes time, Y_{irt} is environmental concern (or any other outcome measure of interest) for individual i in country r at time t, x_{rt} is the household price of energy (natural gas and electricity) in country r at time t. W_i are individual covariates such as gender, education, age, ideology, and occupation category. D_{rt} are region- and time-varying economic variables including the employment rate and GDP of the relevant time period. Finally, γ_r is a region (NUTS2 in most cases) fixed effect, while δ_t are year fixed effects. The parameter of interest is β but the OLS estimation may be biased, due to omitted variable bias if there are omitted factors that are correlated with both environmental concern and energy prices. I therefore propose to use the country's pre-war shares of fuels in their final household energy consumption mix interacted with the national-level prices as an exogenous measure of exposure to energy prices.

Specifically, the goal is to isolate exogenous variation in exposure to energy prices by using country-specific pre-Ukraine invasion energy mix. I propose building a proxy variable, interacting the pre-war (base year 2020) shares of energy (natural gas) fuel mix [4] (e.g., share of household energy coming from natural gas) with the fuel price in that location at time t. Specifically, if Z_{rk} is the pre-war share of country l's energy mix for fuel k and g_{rkt} is the residential price of fuel k in country r at time t. This variable takes the form of:

$$x_{rt} = \Sigma_k Z_{rkt=2020} \times g_{rkt} \tag{2}$$

[3] https://ec.europa.eu/eurostat/statistics-explained/index.php?title=EU_energy_mix_and_import_dependency#Energy_mix_and_import_dependency

[4] https://ec.europa.eu/eurostat/statistics-explained/index.php?title=Energy_consumption_in_households#Energy_products_used_in_the_residential_sector

or when focusing on natural gas

$$x_{rt} = \Sigma_k Share_G as_{rt=2020} \times NtlGasPrice_{rkt} \tag{3}$$

Since energy markets within Europe can vary considerably in terms of prices and regulations, the previously-defined proxy variable could be endogenous to local demand and conditions. To mitigate this endogeneity, I extend the empirical strategy to rely on European-level energy prices combined with the pre-war local variation in the share of household energy used. Specifically, I use an instrument for the price of household natural gas in country r at time t, $NtlGasPrice_{rt}$, with $ShareGas_{r,2020} \times NtlGasPrice_{EU,t}$, the share of natural gas used by households in country r in 2020 times the EU-wide time varying price of natural gas. This strategy is in similar spirit to the one used by Chirakijja et al. (2019).

Specifically, I estimate the following equation using an instrumental variables approach:

$$Y_{irt} = NtlGasPrice_{rt} + \sigma W_i + \pi D_{rt} + \gamma_r + \delta_t + \varepsilon_{irt} \tag{4}$$

Where the first-stage equation is:

$$\begin{aligned} NtlGasPrice_{rt} = \hat{\beta} ShareGas_{r,2020} \times NtlGasPrice_{EU,t} \\ + \sigma \hat{W}_i + \hat{\pi} D_{rt} + \hat{\gamma}_r + \hat{\delta}_t + \varepsilon_{irt} \end{aligned} \tag{5}$$

Again r indexes country, t indexes time, W_i are individual covariates such as gender, education, age, ideology, and occupation category. D_{rt} are region- and time-varying economic variables including the unemployment rate and GDP of the relevant time period. Finally, γ_r is a region (NUTS2 in most cases) fixed effect, while δ_t are year fixed effects.

The identification assumption is that the contemporaneous natural gas household prices in country r should be highly correlated with the contemporaneous EU natural gas prices interacted with pre-war natural gas household use shares. This instrument should impact impacts the outcome variables (energy and environmental concern variables, prioritizing reducing energy use and renewables, a common EU energy strategy) only through the country r natural gas price. The first stage which controls for all covariates, is shown in Table 1, showing a strong and significant association between the instrument and the price of natural gas in country r at time t the price. The F-stat is large enough, 5406.

Table 1: First Stage Result of Country hh Share*EU Price Instrument

	Dependent variable:
	natgas price
$ShareGas_{r,2020} \times NtlGasPrice_{EU,t}(\hat{\beta})$	0.012***
	(0.0001)
nuts and year FE	Yes
F-stat	54.06
Observations	129,700
Note:	*p<0.1; **p<0.05; ***p<0.01

4 Results

4.1 OLS Results

Table 2 shows the estimates of equation (1) using as the main independent variable of interest the time-varying household price of natural gas in the individual's country of survey. The first row shows the estimate of interest, the impact of a euro per kWh increase in the price of natural gas times on the different energy and environmental attitude variables. This is estimated using a linear probability model with fixed effects for region and time. All models control for age, GDP, employment rate, gender, ideology, and occupation indicators.

The coefficient in row 1 of model 1 of Table 2 suggest that the exposure measure, a 1 cent increase in the price of natural gas increases the probability of being concerned about energy supply by .01196 percentage points (49% relative to the pre-2022 energy concern average of 0.03). Instead, a 1 cent increase in the price of natural gas is associated with a -0.002 percentage point decrease in environmental concern (0.01% relative to pre-2022 average concern of 0.14). While this impact is negative, it is very small in terms of magnitude and can be interpreted as a precisely estimated zero impact. Instead, a one cent increase in the price of natural gas (in Kwh equivalent) is associated with a 0.0035 increase in the prioritization of energy use reduction as part of the EU green deal.

While these results are indicative, there may be endogeneity concerns, if countries were able to adjust their prices as a response to the war. The next two analyses use a proxy variable and an IV strategy to try to address these concerns.

4.2 Proxy Results

Table 3 shows the estimates of equation (1) using the main independent variable of interest the interaction of the share of natural gas use in the household energy mix of a respondent's country in 2020, times the time-varying household price of natural gas in the corresponding country. The first row shows the estimate of interest, the impact of a euro per kWh increase in the price of natural gas

Table 2: OLS natural gas prices on environmental and energy attitudes

Dependent Variables:	Energy supply concern	Environmental concern	Reduce energy use	Develop renewables	EU common energy strategy
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Natural gas price Euros/kWh	1.196***	-0.2010**	0.3543**	-0.1572	-0.2319*
	(0.1674)	(0.0821)	(0.1578)	(0.1328)	(0.1227)
age	0.0004***	-0.0004***	0.0006***	-0.0005**	-0.0002
	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)
gdp	-0.0169	0.1492	0.5536***	-0.3324*	0.1288
	(0.2235)	(0.1107)	(0.1599)	(0.1872)	(0.1625)
unemp	0.0104***	-0.0005	-0.0077**	0.0045	0.0006
	(0.0039)	(0.0022)	(0.0034)	(0.0048)	(0.0030)
female	-0.0219***	0.0029	-0.0201***	-0.0348***	0.0112***
	(0.0029)	(0.0037)	(0.0037)	(0.0041)	(0.0037)
ideology	0.0013*	-0.0170***	-0.0014	-0.0035**	-0.0070***
	(0.0007)	(0.0029)	(0.0010)	(0.0015)	(0.0024)
Fixed-effects					
nuts_id	Yes	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes	Yes
as.factor(occupation)	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	105,317	105,317	105,321	105,321	100,258
R^2	0.11285	0.13987	0.03340	0.05655	0.07189
Within R^2	0.00942	0.01123	0.00125	0.00162	0.00182

Clustered (nuts_id) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Sample includes individuals in latest 5 waves of standard Eurobarometer (2020-2022).

times the share used by households, on the different energy and environmental attitude variables. This is estimated using a liner probability model with fixed effects for region and time. All models control for age, GDP, employment rate, gender, ideology, and occupation indicators (estimates with alternative covariates to be added in the Appendix). The coefficient in row 1 of model 1 suggests that the exposure measure, a 1 cent increase in the price of natural gas times the initial share of natural gas used, increases the probability of being concerned about energy supply by 0.0239 percentage points (8.53% relative to the pre-2022 energy concern average of 0.03). Instead, a 1 cent increase in the natural gas price is associated with at 0.0056 percentage point increase in environmental concern (0.04% relative to pre-2022 average concern of a 0.14).

Table 3: Estimation of energy mix natural gas shares times prices on environmental and energy attitudes

Dependent Variables:	Energy supply concern	Environmental concern	Reduce energy use	Develop renewables	EU common energy strategy
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Nat.gas (share x price)	0.0239***	0.0056**	0.0107***	0.0030	0.0004
	(0.0046)	(0.0025)	(0.0039)	(0.0033)	(0.0031)
age	0.0004***	-0.0004***	0.0006***	-0.0005**	-0.0002
	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)
gdp	-0.3029	0.1720	0.4595***	-0.3109*	0.1714
	(0.2408)	(0.1088)	(0.1533)	(0.1875)	(0.1542)
unemp	0.0066*	6.07×10^{-7}	-0.0089**	0.0049	0.0013
	(0.0039)	(0.0022)	(0.0034)	(0.0048)	(0.0031)
female	-0.0217***	0.0030	-0.0199***	-0.0347***	0.0112***
	(0.0030)	(0.0037)	(0.0037)	(0.0041)	(0.0037)
ideology	0.0014*	-0.0170***	-0.0014	-0.0036**	-0.0071***
	(0.0007)	(0.0029)	(0.0010)	(0.0015)	(0.0024)
Fixed-effects					
nuts_id	Yes	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes	Yes
as.factor(occupation)	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	105,317	105,317	105,321	105,321	100,258
R^2	0.10940	0.13985	0.03340	0.05653	0.07176
Within R^2	0.00557	0.01121	0.00125	0.00160	0.00168

Clustered (nuts_id) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Sample includes individuals in latest 5 waves of standard Eurobarometer (2020-2022).

4.3 IV Results

Table 4 shows the estimates of equation (3) using as an instrument the share of natural gas use in the household energy mix of a respondent's country in 2020, times the time-varying price of natural gas at the EU level. The first row shows the estimate of interest, the impact of a euro per kWh increase in the price of EU natural gas times the share used by households in the respective country of the survey, on the different energy and environmental attitude variables. This is estimated using an IV strategy with fixed effects for region and time. All models control for age, GDP, employment rate, gender, ideology, and occupation indicators in the first and second stage.

The coefficients across the first four dependent variables are positive and significant at the 1% level. That is, natural gas prices increase energy concern, environmental concern, the prioritization of reducing energy use and of developing renewables as part of the EU Green Deal. Instead, energy prices decrease support for a common EU energy strategy.

The coefficient in row 1 of model 1 of the IV table 4 suggests that the exposure measure, a 1 cent increase in the price of natural gas (instrumented) increases the probability of being concerned about energy supply by .0393 percentage points. This is relatively large compared to the pre-2022 energy concern average of 0.0394. The second column shows that a 1 cent increase in the natural gas price is associated with at 0.0106 percentage point increase in environmental concern (relative to pre-2022 average concern of 0.14). Furthermore, a one cent increase in the price of natural gas (in Kwh equivalent) is associated with a 0.0132 increase in the prioritization of energy use reduction as part of the EU green deal and a 0.059 percentage point increase in developing renewable energy. Instead, a 1 cent per KWh increase the natural gas price decreases individuals' interest in favoring a common energy policy among EU member states.

Figure 5: Coefficients on Main Outcomes of Natural Gas Instrumented by Country HH Share*EU Price

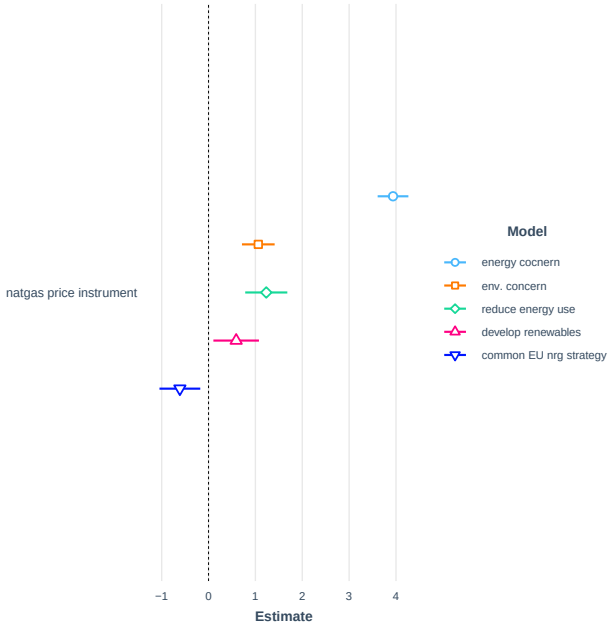


Table 4: Results with Natural Gas Instrumented by Country hh Share*EU Price

	Dependent variable:				
	enrgy concern	env. concern	reduce nrg	develop renew	EU nrg strategy
	(1)	(2)	(3)	(4)	(5)
natgas price instrument	3.936***	1.062***	1.232***	0.590**	−0.612***
	(0.195)	(0.206)	(0.236)	(0.250)	(0.222)
age	0.0004***	−0.001***	0.001***	−0.001***	−0.0002***
	(0.00005)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
gdp	0.717***	0.383***	0.684***	−0.140	−0.008
	(0.068)	(0.079)	(0.107)	(0.113)	(0.090)
unemp	0.008***	−0.002*	−0.005**	0.006***	0.0002
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)
female	−0.021***	0.006***	−0.020***	−0.034***	0.012***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)
nuts and year FE	Yes	Yes	Yes	Yes	Yes
Observations	140,657	140,657	117,261	117,261	132,081

Note: *p<0.1; **p<0.05; ***p<0.01

5 Conclusion and Future Steps

Energy prices are of concern in the energy transition and subject to global macroeconomic conditions. In this paper, I shed light on how the recent increase in energy prices impacts attitudes towards the energy transition and related policies. While I exploit an unexpected increase in energy prices, some concerns remain that prices may be endogenous. To address this, this analysis uses an instrumental variable strategy. I find evidence that the rise in energy prices due to the invasion of Ukraine increased energy supply concern, environmental concern, and led individuals to prioritize reducing energy use and to develop renewable energy. However, higher natural gas prices decreased support for a common EU energy strategy.

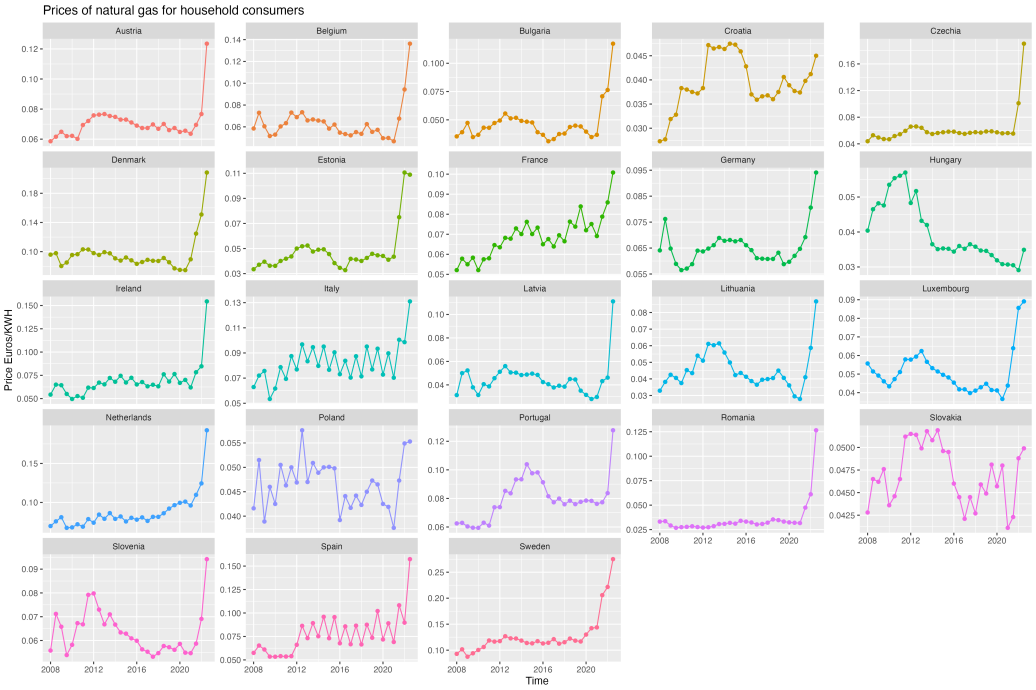
Here I use an instrumental variable strategy, using the prices of natural gas. In future analyses, I may want to integrate electricity prices, although these may be correlated with natural gas prices. Furthermore, I would like to add heterogeneity analyses, particularly to see if the responses vary by sub-region, socio-demographic groups, or whether measures implemented by governments mitigated some of the responses. Of particular interest are people's response to government policies implemented to offset the rising energy costs, which were made in many countries. Furthermore, the response to the government from individuals could have also varied and I also plan to investigate other outcomes such as voting behavior. I would also like to incorporate more waves of the Eurobarmeter data and more data on environmental concerns, climate policies, and energy independence measures. I plan to add data to get a more geographically concentrated measure of energy exposure to exploit the finer aggregation of the Eurobarometer data.

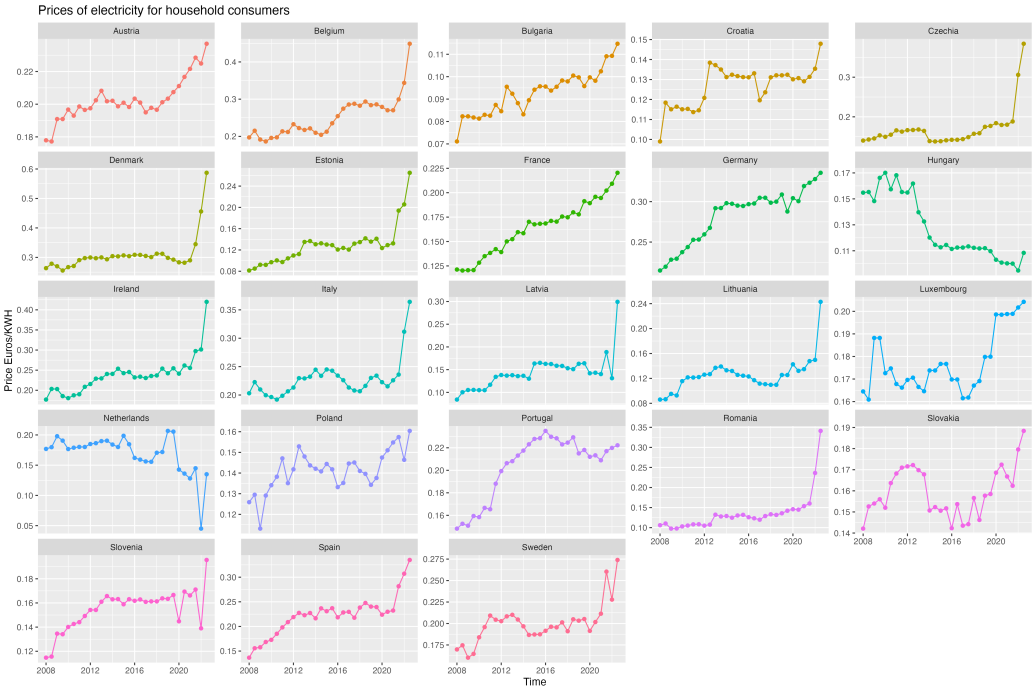
Finally, I would like to add more data that measures revealed preferences such as the adoption of technologies that can help reduce energy use or produce energy more independently. To look at mechanisms, I would like to explore how these changes in energy demand impact labor income and income inequality, since energy expenses are likely to impact lower-income individuals, energy-insecure individuals, and energy intensive regions the most.

6 Acknowledgment

Acknowledgement: I gratefully acknowledge funding from the European Union's Horizon Europe research and innovation programme under grant agreement No 101069880 - AdJUST, Advancing the understanding of challenges, policy options and measures to achieve a JUST EU energy transition and grant agreement No 101056891- CAPABLE - ClimAte Policy AcceptaBiLity Economic framework.

7 Appendix


Prices of natural gas for household consumers
Austria
Belgium
Bulgaria
Croatia
Czechia
Denmark
Estonia
France
Germany
Hungary
Ireland
Italy
Latvia
Lithuania
Luxembourg
Netherlands
Poland
Portugal
Romania
Slovakia
Slovenia
Spain
Sweden
Price Euros/KWH
Time


Prices of electricity for household consumers
Austria
Belgium
Bulgaria
Croatia
Czechia
Denmark
Estonia
France
Germany
Hungary
Ireland
Italy
Latvia
Lithuania
Luxembourg
Netherlands
Poland
Portugal
Romania
Slovakia
Slovenia
Spain
Sweden
Price Euros/KWH
Time

8 Preliminary Results on Country Heterogeneity

The following analyses show the result split by country on each of the 5 main variables of interest. The dependent variable is the endogenous Share X price proxy as in equation 2. In future analyses, these will be instrumented as in the IV strategy above.

8.1 Energy Concern

Figure 6: Impact of Energy Price Proxy on Energy Concern by Country. Main dependent variable is Natural gas (share x price) + Electricity (share x price).

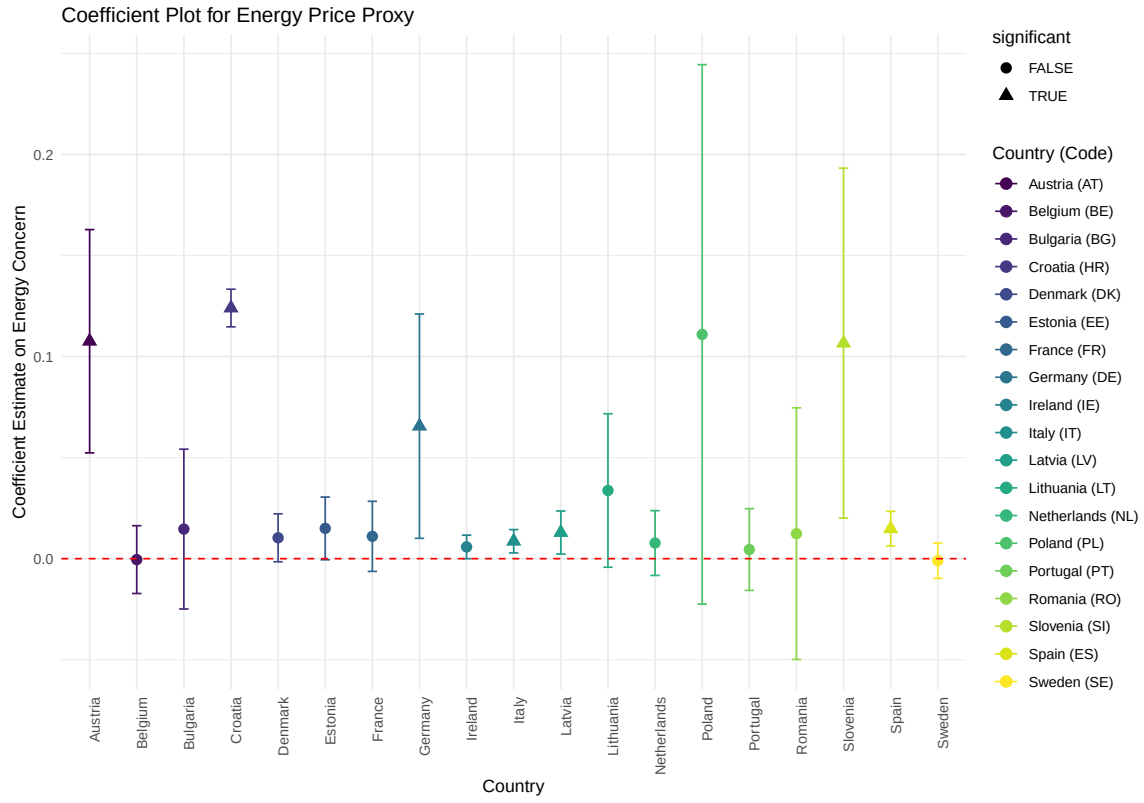
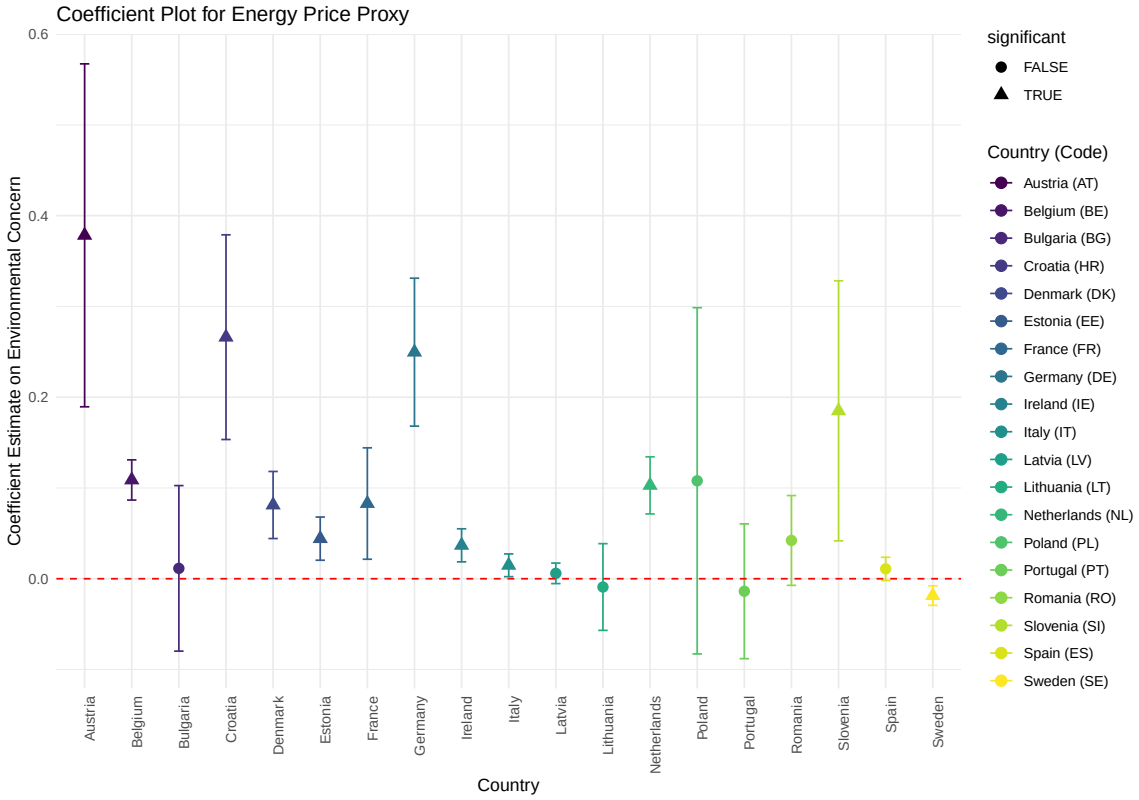


Figure 6 shows the impact of the energy price proxy on attitudes towards energy concern. The samples in Austria, Croatia, Germany, Italy, Latvia, Spain, and Slovenia show positive and significant relationships between energy prices and concern about energy security. Most other countries show a positive relationship, but this is not statistically significant at the 5% level in the non-mentioned countries.

8.2 Environmental Concern

Figure 7 shows the impact of the energy price proxy on environmental concern. The samples in Austria, Belgium, Croatia, Denmark, Estonia, France, Germany, Ireland, Italy, the Netherlands,

Figure 7: Impact of Energy Price Proxy on Environmental Concern by Country. Main dependent variable is Natural gas (share x price) + Electricity (share x price).



Slovenia show a positive relationships between energy prices and concern about the environment that is statistically significant at the 5% level.

8.3 Prioritize Reducing Energy Use as Part of the EU Green Deal

Figure 8: Impact of Energy Price Proxy on Prioritizing Reducing Energy Use by Country. Main dependent variable is Natural gas (share x price) + Electricity (share x price).

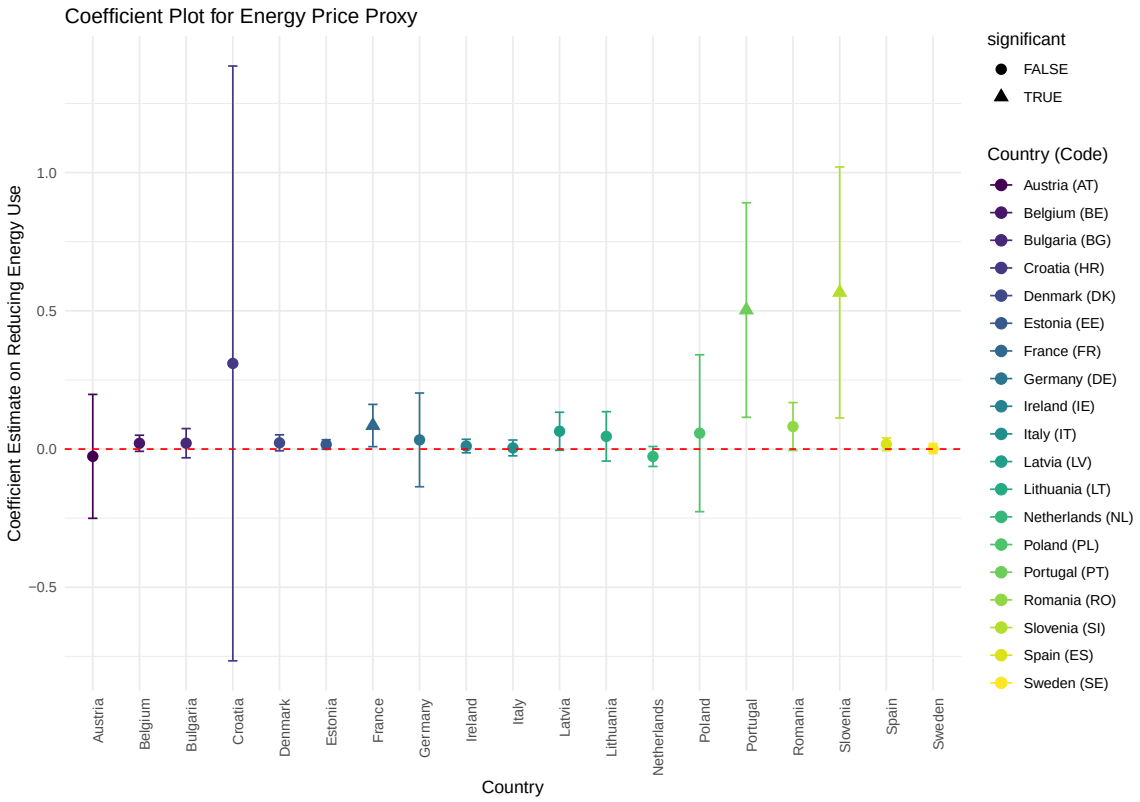
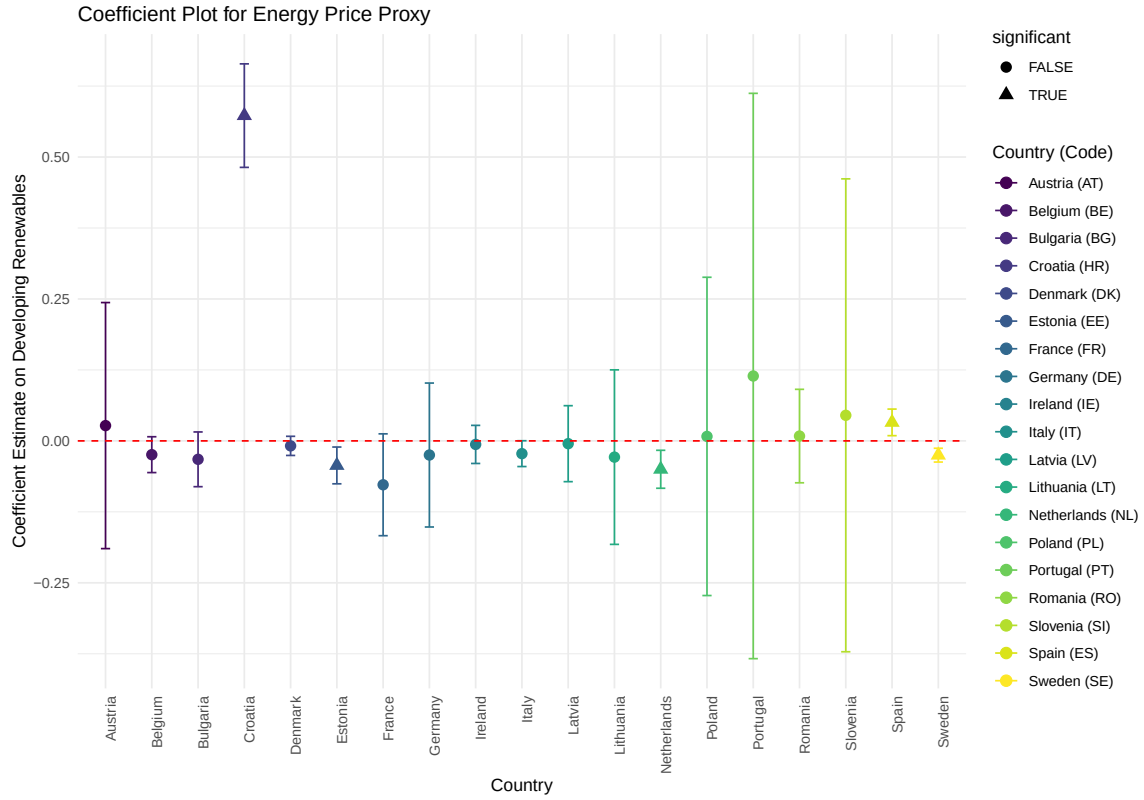


Figure 8 shows the impact of the energy price proxy on prioritizing reducing energy use as part of the EU green deal. While most estimates are positive, only France, Portugal, and Slovenia show a positive relationship between energy prices and prioritizing reducing energy that is significant at the 5% level.

8.4 Prioritize Developing Renewables as Part of the EU Green Deal

Figure 9 shows the impact of the energy price proxy on prioritizing developing renewables use as part of the EU green deal. Only Croatia, Estonia, the Netherlands, and Spain show a positive relationship between energy prices and prioritizing developing renewables that is significant at the 5% level. Most other countries have non-significant negative relationships.

Figure 9: Impact of Energy Price Proxy on Prioritizing Developing Renewables by Country. Main dependent variable is Natural gas (share x price) + Electricity (share x price).



8.5 Prioritizing a Common EU strategy.

Figure 10: Impact of Energy Price Proxy on Prioritizing Developing Renewables by Country. Main dependent variable is Natural gas (share x price) + Electricity (share x price).

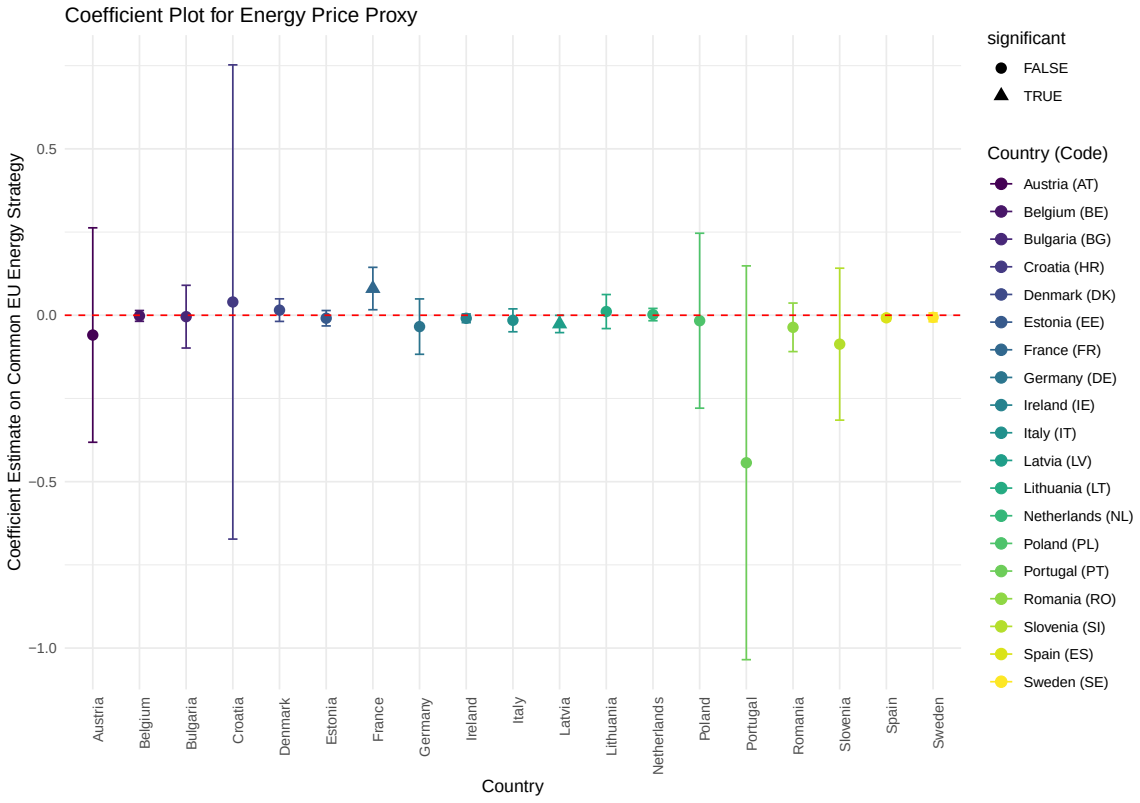


Figure 10 shows the impact of the energy price proxy on developing a common EU energy strategy. Only France and Latvia shows a positive relationship between energy prices and prioritizing developing a common EU energy strategy that is significant at the 5% level.

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Chapter 4

The Impact of Green Policies on Local Economic Performance: Evidence from the EU ETS

The Impact of Green Policies on Local Economic Performance: Evidence from the EU ETS

Ireri Hernandez Carballo[1,2], Gian Maria Mallarino[1], and Marco Percoco[1]

[1]Department of Social and Political Sciences, Bocconi University

[2]RFF-CMCC European Institute on Economics and the Environment (EIEE)

Abstract

Environmental policies such as the European Union Emissions Trading System (EU ETS) raise concerns about their impact on employment and competitiveness. Yet, existing EU ETS studies focus on firm-level outcomes during the initial phases of the program. We construct a panel dataset of about 900 European provinces across 2008 to 2020 to assess the effects of a significant policy change in Phase 3 of the EU ETS. Specifically, we investigate how the changes in the free allocation mechanism affected local economic performance, including employment, gross value added (GVA) and productivity per employee (PPE) of European provinces. We employ data on the net change of paid emissions from Phase 2 to Phase 3 to construct a measure of local level exposure at the NUTS-3 level. Using synthetic difference-in-differences, we find that being more exposed to the EU ETS is associated with negative and significant results on employment and GVA of the most impacted industries but not at the overall regional level. Furthermore, we find strong reductions in emissions and no strong evidence of increasing inequality, although with some differences across outcomes in the most impacted sectors.

Keywords: EU ETS, Carbon policies, CO2 Emissions, Regional Economics, Economic Geography

1 Introduction

As the climate crisis becomes more pressing, governments and adopt various of voluntary as well as command and control policies to contain carbon emissions. Among the many instruments to curb greenhouse gases, carbon pricing has gained momentum as 23% of global greenhouse gas emissions are covered by a considerable variety of carbon pricing schemes, including emission trading systems (World Bank, 2022). Despite their potential environmental benefits, green policies also have potential costs in terms of economic and inequality backlash. The yellow vest protests in France are tangible effects of the social and political discontent associated with public policies aiming to decarbonize economies [Colantone, Di Lonardo, Margalit, and Percoco, 2024, Douenne and Fabre, 2022]. The unrest in the French case was generated by a major increase in the carbon tax on car fuel that disproportionately affected low-income workers. In this paper, we aim to make a further step into the growing literature on the economic costs of green policies by considering explicitly their spatial variation. More specifically, we aim to estimate the impact of the European Union Emission Trading System (EU-ETS) on the European (NUTS3) regions.

Inaugurated in 2005, the European Union Emission Trading System (EU ETS) is a carbon pricing mechanism and the main pillar of the European Union's strategy to decarbonize its economy. With currently 31 countries participating, the EU ETS covers 45% of the EU's greenhouse gases (GHG) emissions. Designed to reduce emissions while stimulating heavy emitting companies to innovate their production systems, the EU ETS has raised concerns about its impact on employment, firm competitiveness and subsequent carbon leakage [Commins, Lyons, Schiffbauer, and Tol, 2011, Costantini and Mazzanti, 2012, Reinaud, 2008, Colmer, Martin, Muûls, and Wagner, 2024, Dechezleprêtre, Nachtigall, and Venmans, 2023].

On the one hand the Porter hypothesis suggests that carbon pricing policies may lead to improved competitiveness by enabling new production methods could result in the EU ETS having improved economic outcomes [Porter and van der Linde, 1995]. On the other hand, negative competitiveness impacts may arise if production is shifted to countries with weaker environmental stringency, as predicted by the pollution haven hypothesis [Levinson and Taylor, 2008]. In a detailed literature review of the EU ETS on firm productivity and carbon leakage, Verde [2020] concludes that while the policy has been widely studied to test for eventual economic effects, there is weak empirical evidence of any positive or negative impacts on competitiveness. This wealth of this evidence has mainly focused on the sectorial implication of EU-ETS as it is enforced only in specific sectors. A few more recent studies with more robust research designs analyze firm-level outcomes, also finding limited economic impacts, but mainly focus either on a limited set of countries or on specific sectors or on

emissions alone [Dechezleprêtre et al., 2023, Colmer et al., 2024, Bayer and Aklin, 2020, Jaraitė and Maria, 2016, Marin, Marino, and Pellegrin, 2018].

Previous literature has almost neglected the spatial dimension of the EU-ETS despite the intuition of possible heterogeneous effects arising from the concentration of regulated emissions in a limited number of regions [Robaina and Goncalves, 2019, Roseta-Palma, Robaina, and Rodríguez, 2011]. In this paper, we aim to fill this gap by employing a novel measure of policy exposure at the local and industry level to estimate the causal impact of EU-ETS on regional economies. Contrary to previous studies, that have focused on the first phases of the carbon pricing mechanism, we analyze the impact of Phase 3 which led to a substantial contraction in the free allowances on a subsequent surge into carbon prices and hence in the compliance costs of firms. By employing Synthetic Difference-in-Difference methods, our research design takes advantage of an exogenous variation into policy exposure to estimate the impact of EU-ETS enforcement on a battery of economic indicators.

We find that provinces with the largest changes between Phase 2 and phase 3 in the number of allowances that must be purchased, reduced their employment, GVA, and productivity per employees in targeted industries relative to provinces that were less impacted by the changes between Phase 2 and 3. We find the highest impacts on mining and quarrying, electricity, gas, steam and air conditioning supply, water supply, and construction. We find limited impact on manufacturing outcomes, in line with findings of previous studies. Furthermore, we find a strong reduction on emissions of regulated industries. Finally, we don't find evidence for particularly strong increases in inequality across regions, although there are differences across outcomes in specific sectors.

The rest of the paper is organized as follows: Section 2 provides some background on the EU Emission Trading System and the main innovations introduced in Phase 3 of the policy; Section 3 outlines the research design and the data sources used for the analysis; Section 4 presents and discuss the results and Section 5 summarizes and concludes the study.

2 Background: EU Emission Trading System

The European Union Emissions Trading System (EU ETS), established by Directive 2003/87/EC and launched in 2005, is the cornerstone of the EU's strategy to reduce emissions cost-effectively and incentivize decarbonization. As the world's first and largest international cap-and-trade system for greenhouse gas (GHG) emissions, the system operates by setting a cap on total emissions, which is reduced annually, and issuing Emission Unit Allowances (EUA) that companies can use to cover their emissions. These allowances can be traded on the market, enabling firms to buy or sell them depending on their emission levels. This

limit is progressively lowered each year to align with the EU's climate objectives, ensuring a steady decline in overall emissions. The cap, initially set at the national level during the first two trading phases, became EU-wide starting in Phase 3 (2013–2020). The linear reduction factor, initially 1.74% annually, was increased to 2.2% for Phase 4 (2021–2030), in line with the EU's objective to achieve a 62% reduction in emissions by 2030 compared to 2005 levels. By 2023, the system contributed to a reduction of approximately 47% in emissions from European power plants and industrial facilities compared to 2005 levels.

The EU ETS operates in four distinct trading phases. Phase 1 (2005–2007) was a three-year pilot phase designed to test the system's framework and prepare for Phase 2. The inclusion of installations in the EU ETS is determined based on their activity level. This phase covered only CO_2 emissions from power generators and energy-intensive industries. Nearly all allowances were distributed for free to businesses based on historical emissions, a method known as "grandparenting." and the penalty for non-compliance was set at €40 per tonne of CO_2 equivalent. Due to the lack of reliable emissions data, the caps in Phase 1 were determined based on estimates. This led to the issuance of allowances that exceeded actual emissions, resulting in a significant oversupply [Ellerman, Convery, and De Perthuis, 2010][1]. Consequently, in 2007, the surplus caused allowance prices to drop to zero, as unused Phase 1 allowances could not be carried over into Phase 2.

Phase 2 (2008-2012) was the first full implementation phase of the EU ETS, with a tighter emissions cap (6% lower than Phase 1), the introduction of banking for unused allowances into Phase 3 and the introduction of international credits from the Kyoto Protocol's Clean Development Mechanism (CDM) and Joint Implementation (JI) as an option for companies to offset a portion of their emissions. The system's geographical scope expanded to include Norway, Iceland, and Liechtenstein, while aviation emissions were added in 2012 for flights within, to, and from the European Economic Area (EEA). Although allowances were still predominantly allocated for free, their overall quantity was reduced. The penalty for non-compliance increased to €100 per tonne of CO_2 equivalent. However, the 2008 financial crisis reduced industrial activity, resulting in lower-than-expected emissions and a surplus of allowances, which negatively impacted the price that fell to around 5 Euros per EUA by the end of the phase.

Phase 3 introduced major reforms to harmonize and strengthen the EU ETS. The cap was set at the EU level, declining annually by 1.74%, and auctioning became the primary method of allowance allocation [Mirzaee Ghazani and Jafari, 2021], with free allowances reduced to 43% by 2020. Free allocation was based on benchmarks reflecting the top 10% most efficient

[1]Ellerman et al., (2010) provide a comprehensive overview of the design of the EU ETS and a thorough analysis of the first phase of this policy (2005-2007).

installations, with revenues from auctions used for renewable energy and energy efficiency projects. The scope expanded to include additional sectors and gases, such as nitrous oxide (N_2O) and perfluorocarbons (PFCs), while aviation remained partially covered. To address surplus allowances, the Market Stability Reserve (MSR) was introduced in 2019, with the mandate of stabilizing the market by reducing oversupply. By 2020, emissions from covered sectors had dropped by 35% compared to 2005 levels. As anticipated in the introduction, in this paper, we take advantage of the major changes in the policy between Phase 2 and Phase 3 to estimate the impact of emission trading on regional economies by using data and an econometric design that are discussed in the next sections.

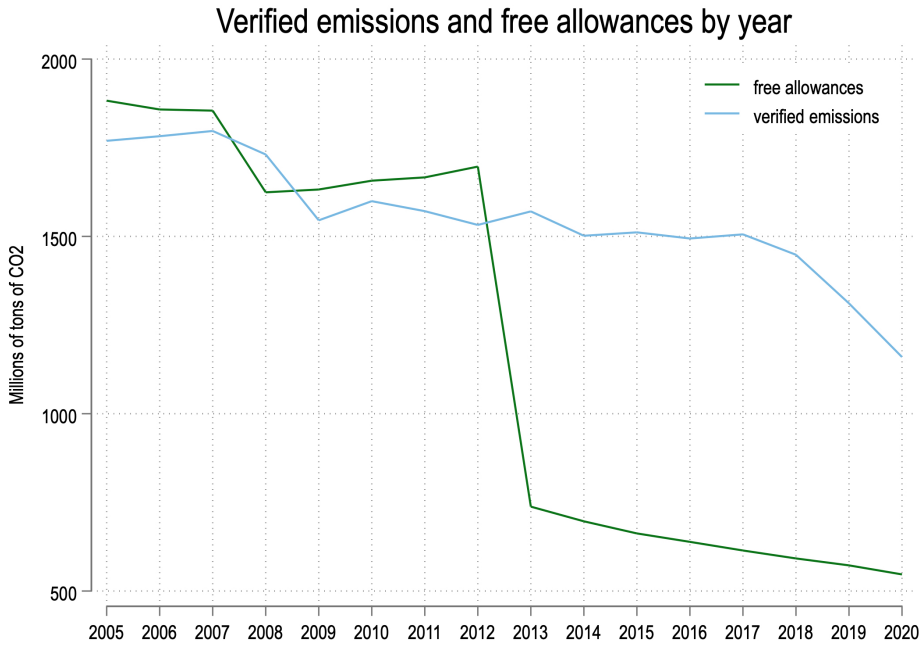


Figure 1: Cumulative Verified emissions and free allowances by year. source: own elaboration

Figure 1 plots the overall levels of free allowances and verified emissions over time, showing the sharp drop in free allowances starting in 2013. Third, the phase encompassed the implementation of a European-wide cap subject to a linear reduction factor of 1.74% per year until 2020.

3 Data and descriptive statistics

3.1 Sources of data and construction of main variables

The econometric analysis on the impact of EU-ETS is conducted by using data from two main sources: the European Union Transaction Log (EUTL) and the Eurostat Regions and Cities Database. The EUTL is the central mandatory reporting and monitoring tool for the EU-ETS, serving as a transaction log for the registration, issuance, transfer, and cancellation of allowances (European Council, 2023). This log is a platform for the European Commission to share public information on the compliance of regulated entities, the active participants in the system and their transactions. We obtain the EUTL data through a publicly available and accessible relational database [Abrell, 2024][2]. We begin with this rich installation-level data, which contains a unique installation id, address, longitude and latitude, industry classification (NACE), year of compliance, number of certificates allocated for free, number of verified emissions, and numbered of surrendered certificates, among other information.

We employ the EUTL data to build a measure of local exposure to the changes in allocation rules starting in Phase 3 of the EU ETS. Each installation in the EUTL registry is assigned to its corresponding NUTS-3 province using latitude, longitude, and zip code. For each year, we calculate the total verified CO_2 emissions and freely allocated allowances by summing these values across all installations within each NUTS-3 province.

To assess the impact of the regulatory changes, we focus on changes in 2013, the first year of Phase 3. Specifically, we compute the net paid emissions in 2013 by subtracting the total freely allocated allowances from the total verified emissions for each NUTS-3 province. This gives us the total net paid emissions in each province. To capture the cost implications of the policy changes, we compared the 2013 net paid emissions with the average net paid emissions during Phase 2 (2008-2012). This difference provides a measure of the increased costs arising from the sharp reduction in free allowance allocations in Phase 3. The higher this number, the higher the costs for the provinces starting in 2013 relative to phase 2. Finally, we divide these differences across provinces into quintiles and assign each NUTS-3 province a variable indicating the quintile it falls into. This categorization allows for a comparative analysis of the varying degrees of exposure to changes across provinces.

In our baseline analysis, we exclude provinces experiencing "false changes", where the calculated exposure does not align with the actual impact of the policy. There are two scenarios that qualify as false changes: false negatives and false positives. False negatives (or false decrease in costs) correspond to provinces where emissions exceeded their free

[2]This data is available from https://www.euets.info/

allowances in both Phase 2 and in 2013 but they experienced a reduction in the number of allowances they needed to purchase (e.g., going from purchasing 100 allowances on average in Phase 2 to 80 allowances in 2013 resulting in a net change of -20). In this case, the calculated exposure measure is negative because the need to purchase carbon allowances decreased, but the province benefitted from the changes in the policy as it is having to pay for less emissions than before. While these provinces still must pay on net, they are different than the provinces that went from having positive costs to having net free allowances starting in Phase 3.

The second case, the false positive, represents provinces where allowances exceeded emissions in both periods, but reduced the amount of free allocations in excess from one period to the other. The net change in paid allowances in this case is positive, driven the reduction of extra allowances from Phase 2 to Phase 3 (e.g. going from having - 50 net paid allowances to having - 25 net paid allowances in 2013, resulting in changes of 25). However, given that the number of allowances still exceeds the level of emissions, these are different than cases when provinces went from having to pay zero on net to some positive number. In the descriptive statistics as well as in the econometric analysis we exclude "false changes", however in the Appendix, as a robustness check, we provide estimates by including also those cases.

Furthermore, we use data on local economic performance from the Eurostat Regions Database (2021). The Eurostat Regions and Cities Database provides a detailed picture of the diverse EU territories and is used to monitor EU regional policy targets. Data retrieved from this database include different social and economic aspects of European provinces such as demography, economy, employment, and education at the smallest Nomenclature of Territorial Units for Statistics (NUTS-3) levels available. The final dataset presents 1,167 NUTS-3 provinces, containing data on 28 countries[3] located in the European continent over the period 2008-2020[4]. We use the following measurements at the NUTS-3 level:

- Economic performance: data on gross value added (GVA) by sector (according to ISIC Rev. 4 categorizations) and total GVA.
- Employment by sector (according to ISIC Rev. 4 categorizationTable A2 in the Appendix provides a list of the ISIC Rev. 4 codes) and total employment.
- Population levels.

[3]Table A1 in the Appendix provides a list of the countries and number of NUTS-3 provinces included in the analysis.

[4]We also provide robustness analyses in the Appendix omitting the year 2020 to exclude changes that may have been driven by COVID-19

- Education: share of population by education levels according to ISCED 11 categorization: less than primary education (levels 0-2); upper secondary and post-secondary non-tertiary education (levels 3 and 4); and tertiary education (levels 5-8).

3.2 Descriptive Statistics

To provide an initial overview of the data, we divide our sample into quintiles based on our measure of exposure: the net change in paid emissions between the average of Phase 2 and 2013, the first year of Phase 3. To examine how net paid emissions vary across regions by quintiles and time, we plot the average yearly net paid emissions for each quintile group, as shown in Figure 2. The figure indicates that quintile 5, and to a moderate extent quintile 4, experienced a significantly higher change in net paid emissions from Phase 2 to phase 3, compared to the rest of the distribution. For subsequent analyses, we group provinces in quintiles 1-3 as this group experienced the least change, and we also group quintiles 4-5 together, as these regions faced the most substantial changes[5].

Table 1 shows the grouped mean and standard deviation for these quintile groups by ETS phase. The table shows that total employment increased slightly from Phase 2 to Phase 3 in both groups: for Quintiles 1-3 (Q1-3), employment grew from 115,230 to 118,843, and for Quintiles 4-5 (Q4-5), it rose from 242,990 to 249,493. Similarly, gross value added (GVA) saw growth in both groups of quintiles, with Q1-3 increasing from 5,510 to 6,417 million euros, and Q4-5 rising from 12,958 to 14,865 million euros. Employment in Industry B-E remained stable in Q1-3, while Q4-5 saw a slight decline. GVA in industry B-E grew across both groups. Employment in manufacturing increased slightly in Q1-3 but declined in the highest quintiles, while GVA in manufacturing increased in both groups of quintiles. Additionally, verified emissions decreased in both groups, with Q4-5 experiencing a larger reduction. Overall, GVA seems to have increased across time with a trend toward reduced emission, despite modest employment declines in some sectors like manufacturing and limited growth in employment in industries B-E.

We next map the quintiles across NUTS-3 provinces. Figure 4 shows the spatial heterogeneity in changes of net paid emissions (phase 2 vs 2013) across NUTS-3 regions in levels (left). To get a sense of the monetary costs of these changes, the average 2013 allowance price was 4.32 euros; the provinces in the highest treated quintile faced an approximate change in costs of between 2.9 million and 94 million Euros. The middle figure shows the regional heterogeneity by net change in paid emissions per capita, while the figure on the right shows the net change per regional GVA in 2013.

[5]We also provide robustness analyses in the Appendix in which we group together quintiles 1-4 vs 5 and 1-2 vs 3-5

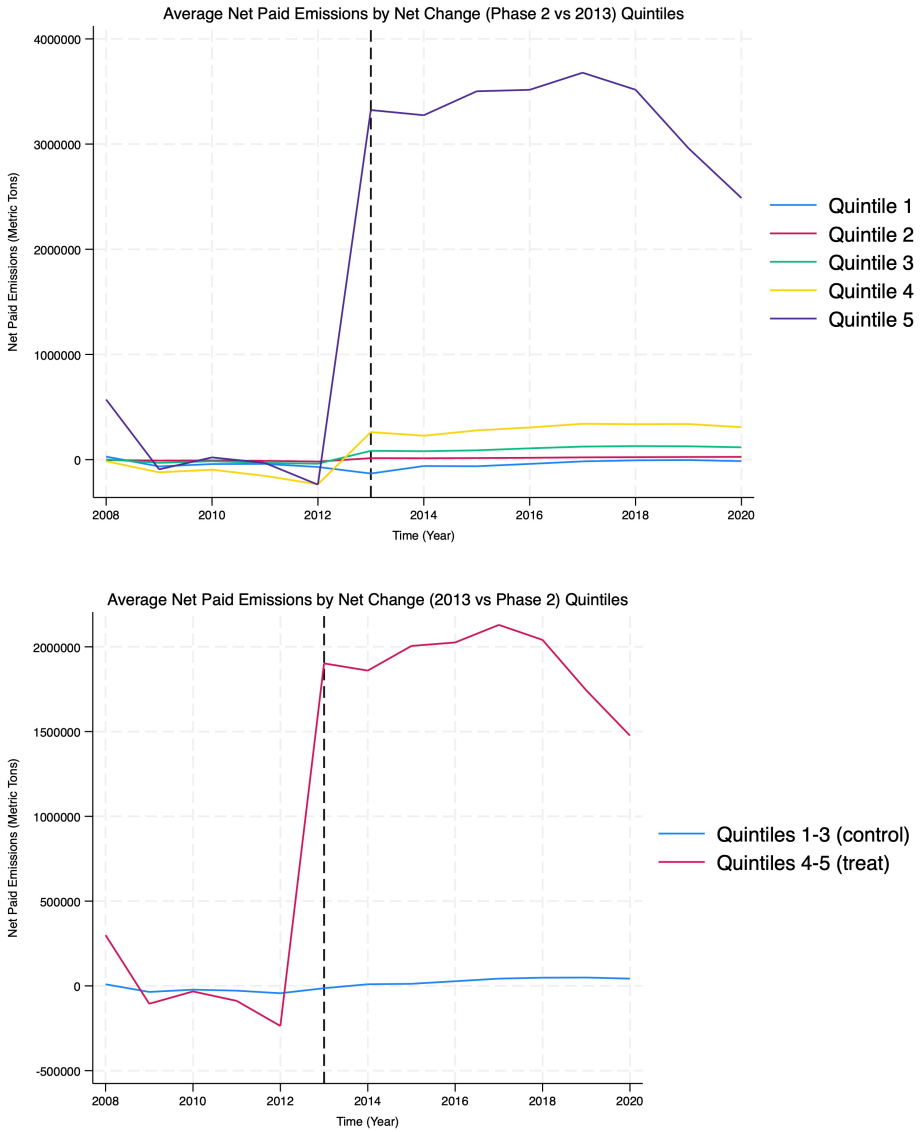


Figure 2: Yearly mean net paid emissions by quintile of net change (Phase 2 vs 2013). All quintiles separately (top) and quintiles 1-3 vs quintiles 4-5 (bottom).

Table 1: Summary Statistics. Quintile 1-3 vs Quintile 4-5: Phase 2 and Phase 3

	Phase 2 2008-2012 Q1-3	Phase 2 2008-2012 Q4-5	Phase 3 2013-2020 Q1-3	Phase 3 2013-2020 Q4-5
employment total (all industries)	115,230.96 (125633.35)	242,990.75 (270550.69)	118,843.10 (133532.55)	249,493.68 (283147.67)
GVA total (all industries)	5,510.60 (7,705.00)	12,958.09 (18,813.11)	6,417.44 (9,093.48)	14,865.03 (21,916.96)
PPE total (all industries)	0.05 (0.02)	0.05 (0.02)	0.05 (0.02)	0.05 (0.02)
employment industry B-E	21,867.76 (21,425.87)	39,033.78 (38,155.10)	21,990.49 (21,525.47)	38,025.79 (36,506.78)
GVA industry B-E (million euros)	1,233.01 (1,469.38)	2,467.42 (2,922.72)	1,457.39 (1,758.05)	2,772.61 (3,287.75)
PPE industry B-E	0.06 (0.03)	0.07 (0.04)	0.07 (0.03)	0.08 (0.04)
employment in manufacturing	20,195.70 (20,141.53)	34,558.46 (34,939.80)	20,262.53 (20,179.09)	33,557.65 (33,107.65)
GVA in manufacturing	1,057.55 (1,319.26)	1,970.54 (2,450.68)	1,266.12 (1,618.86)	2,253.14 (2,820.39)
PPE in manufacturing	0.05 (0.02)	0.06 (0.03)	0.06 (0.03)	0.07 (0.04)
employment in construction	8,183.36 (8,604.26)	16,803.22 (18,814.95)	7,942.85 (8,542.65)	14,998.95 (15,438.06)
GVA in construction	333.25 (429.27)	732.37 (994.89)	378.21 (513.24)	728.66 (909.26)
PPE in construction	0.04 (0.02)	0.04 (0.02)	0.05 (0.02)	0.05 (0.02)
verified emissions	257433.30 (996168.24)	3.46e+06 (5.34e+06)	243296.89 (755334.66)	3.14e+06 (5.12e+06)
education up to upper secondary (share)	52.98 (12.87)	50.05 (15.67)	52.79 (11.60)	49.89 (13.96)
education tertiary and above (share)	24.66 (6.42)	23.92 (7.82)	28.30 (7.32)	28.35 (8.71)
population (thsds)	266.01 (270.32)	528.18 (536.58)	268.27 (277.68)	535.06 (553.62)
Observations	2345.00	2050.00	3752.00	3280.00

Note: Mean of group values. Standard deviations in parentheses.

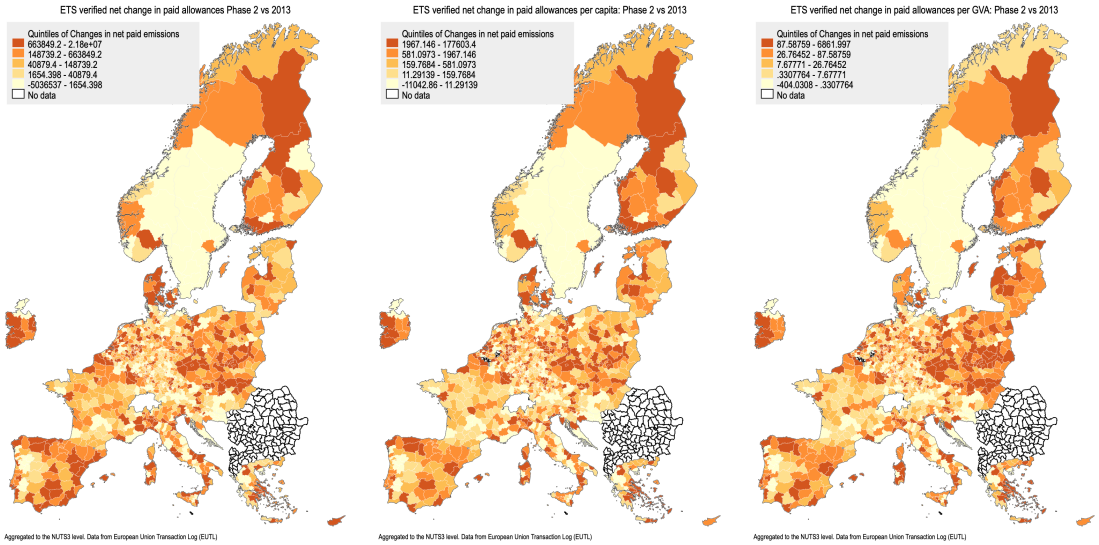


Figure 3: EU ETS 2013 Net Change in Paid Emissions per Capita (L) and per GVA (R)

4 Empirical Strategy

Our research design exploits the variation in the EU-ETS stringency over time as well as the spatial heterogeneity of the policy exposure to estimate the impact of emission trading on regional economic outcomes. To explore the local impact of the EU ETS changes allowance allocation rules, in which auctioning became the default method for distributing allowances starting in 2013, we exploit the differential treatment across provinces as defined above. This allows us to compare provinces that were the most impacted by the new allocation rules (in quintiles 4 and 5 of the net changes) relative to provinces that saw smaller changes in net paid allowances. To find plausible counterfactuals for each province, we rely on synthetic difference-in-differences as formalized by Arkhangelsky, Athey, Hirshberg, Imbens, and Wager [2021].

Using synthetic difference-in-differences (SDID) provides a more robust alternative relative to traditional difference-in-differences (DID) methods. Ideally, we would have similar provinces treated with different "doses" of EU ETS changes in free allowances, whose outcomes would have otherwise evolved similarly. DID is typically applied in settings where some units are exposed to a policy and the "parallel trends" assumption is met, often after including time and unit fixed effects to control for selection effects[6]. Synthetic difference-in-differences weakens the reliance on the parallel trends assumption and is a data-driven way to choose control units that match a set of treated units, exploiting variation in treatment over time and groups. This method re-weights control units and time periods to match the pre-exposure trends of treated and control units, ensuring comparability. These weights put more emphasis on units that on average more similar to the treated units and more weight on years that are more similar to the treated periods, making the estimator more robust relative to DID. Furthermore, it has consistency and statistical normality advantages over traditional difference-in-differences [Arkhangelsky et al., 2021].

In our setting, SDID re-weights the least treated NUTS3 regions (quintiles 1-3) to make their time trend before the change in policy regulation (2013) parallel to the trend of the highest exposed treated regions (quintiles 4-5), then applies DID to this reweighted panel. We use Phase 2 (2008-2012) as our pre-treatment period and Phase 3 (2013-2020) as post-treatment period . We exploit NUTS3 variation in regulation intensity under of the EU ETS based on the change in net allowances paid from Phase 2 to Phase 3. Given the regulatory changes, NUTS3 provinces with higher changes in net paid emissions are expected to be impacted relatively more than provinces with lower changes. For instance, a region that

[6]Before adopting the SDID estimator, we have conducted an extensive analysis using the standard DID approach, however, estimates were unreliable as the parallel trend assumption in the pre-treatment period did not hold. Results, although not reported in the paper, are available upon request.

paid for the same amount of emissions in Phase 2 and in Phase 3 experienced no change in net paid emissions and is expected to be relatively less impacted by the regulatory changes than a region experienced an increase in the net paid emissions from Phase 2 to Phase 3. We split provinces according to quintiles of changes. In our main analysis we define a binary treatment, considering quintiles 4 and 5 to be the "treated" quintiles and quintiles 1-3 as the "control group" [7].

In our setup, we have a panel of N NUTS-3 units and T time periods, Where i indexes the NUTS-3 province, i.e., the province and t the years. $Y_{it}$is our economic outcome variable of interest. W_it is the exposure to the binary treatment as 1) a dummy variable equal to 1 if the region belongs to the highest 2 treated quintiles. Specifically, SDID: i) finds weights, $\hat{\omega}_i$, that align pre-exposure trends in the outcome of units in quintiles 1-3 with those for the units in quintiles 4-5, ii) finds $\hat{\lambda}_t$ time weights that balance pre-exposure (2008-2012) time periods with post-exposure ones (2013-2020). It then uses these weights in a basic two-way fixed effects regression to estimate the average causal effect of exposure, denoted by $\hat{\tau}^{sdid}$ below as in Arkhangelsky et al. [2021].:

$$\left(\hat{\tau}^{sdid}, \hat{\mu}, \hat{\alpha}, \hat{\beta}\right) = \arg\min_{\tau,\mu,\alpha,\beta} \left\{ \sum_{i=1}^{N} \sum_{t=1}^{T} \left(Y_{it} - \mu - \alpha_i - \beta_t - W_{it}\tau\right)^2 \hat{\omega}_i^{sdid} \hat{\lambda}_t^{sdid} \right\} \tag{1}$$

Our coefficient of interest is τ which indicates the average treatment effect of the ETS for the change in net paid emissions in the NUTS3 region in 2013 relative to phase 2. Our estimation includes unit fixed effects α_i and time fixed effects β_t as well as a generalized intercept. We estimate the average treatment effect on the following outcomes: employment, GVA, and PPE at the aggregate level across all industries and on the following sub-industry outcomes, grouped according to their availability in Eurostat for the panel of interest: B-E (B: Mining and Quarrying; C: Manufacturing; D: Electricity, Gas, Steam and Air Conditioning Supply; D: Water Supply); B-E excluding manufacturing; C: Manufacturing; F: Construction; and G-J (G: Wholesale and Retail Trade; H: Transportation and Storage; I: Accommodation and Food Service Activities; J: Publishing, Broadcasting, and Content Production and Distribution Activities)[8]. In a sub-industry analyses, we re-estimate the quintiles of treatment based on the net changes occurring within industries. That is, for each sub-industry group, we calculate the change in net paid emissions for installations belonging to a particular industry, and define quintiles and treatment based on the industry quintiles.

[7] We also test for robustness to other treatment definitions, e.g. including only quintile 5 in the treatment

[8] We omit results of unregulated industries for which we find no significant impact.

5 Results

5.1 Baseline estimates

We show the set of results comparing the provinces in the highest two quintiles of changes in net paid emissions (treat = 1) with the bottom three quintiles (treat = 0). We estimate the average treatment effect (ATE) including covariates for population and education. Table 2 shows the ATEs with separate results by industry groups within a NUTS-3 region and for the overall NUTS-3 level outcomes across all industries in the bottom panel.

As shown in Table 2, the highest treated provinces experienced a significant negative treatment effect of the EU ETS Phase 3 on employment, GVA, and PPE for the most intensely regulated industries, B-E (in NACE nomenclature), relative to the provinces that received lower treatment impacts. Provinces in the highest treatment quintiles experienced a 1.19% reduction in employment during Phase 3 compared to provinces in the bottom three quintiles of treatment (exp(-0.012) -1) * 100). Similarly, GVA in this group decreased by 2.18% and PPE declined by 1.39% in treated provinces relative to the control group.

We find these negative effects are driven by the most regulated industries (mining and utilities) and to a moderate and limited degree by the manufacturing industry. The negative impacts on seem to be strongest for the industry groups B Mining and quarrying, D Electricity, gas, steam and air conditioning supply, and E Water supply[9]. This group of industries in treated provinces saw decreases in employment, GVA, and PPE of 3.92%, 7.78% and 5.35%, respectively, in Phase 3 compared to these same industries in provinces in the control group. When focusing on the manufacturing industry, we see some smaller decreases in employment (-1.39%) but no significant impacts in GVA or PPE in this sector. Interestingly, we report negative impacts in employment, GVA, and PPE of the construction industry, indicating potential spillovers to this sector .

Pooling all economic indicators across all industries (A-V) we find no significant overall treatment effects for the provinces in the highest treated quintiles relative to the less treated provinces, suggesting the impacts are concentrated across specific industries, particularly those most highly regulated and with the highest changes in free allowances from Phase 2-3, including electricity generators.

Our results are robust to changes in the timeframe and sample definition. We repeat the analyses using the panel from 2008-2019. The results remain similar, but with positive treatment effects on agriculture and positive impacts on employment in industries G-J. We also repeat the analyses including provinces with "false" changes as described in the data section, and again there are positive impacts on agriculture and industry G-J employment of this sample. These results are shown in the Appendix Tables A4-A5.

[9]Due to data availability, we are not able to analyze each of these sub-industries B, D, and E separately.

Table 2: SDID Results by Sub-industry and Total

	log Employment	log GVA	log PPE
B Mining and quarrying; C Manufacturing; D Electricity, gas, steam and air conditioning supply; E Water supply			
treat	-0.012**	-0.022***	-0.014**
	0.005	0.007	0.006
Observations	10803	10803	10803
N NUTS-3	831	831	831
831			
B-E Excluding Manufacturing			
treat	-0.040***	-0.081***	-0.055***
	0.010	0.017	0.017
Observations	10569	10569	10569
N NUTS-3	813	813	813
C Manufacturing			
treat	-0.014**	-0.003	-0.001
	0.006	0.009	0.007
Observations	10803	10803	10803
N NUTS-3	831	831	831
F Construction			
treat	-0.020***	-0.031**	-0.031***
	0.007	0.016	0.012
Observations	10816	10816	10816
N NUTS-3	832	832	832
Total: All Industries			
treat	-0.000	-0.004	-0.006
	0.003	0.005	0.006
Observations	10816	10816	10816
N NUTS-3	832	832	832

Note: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.10

5.2 Heterogeneity analyses by sub-industry

We next show results using a definition of treatment at the sub-industry level, focusing on sub-industry outcomes. Specifically, we re-define the highest treated provinces based on NUTS3 sub-industry changes in net paid allowances from Phase 2 to Phase 3, instead of total changes across all industries. For example, for manufacturing, the highest treated provinces are those whose changes in net paid allowances in the manufacturing sector are in the top 2 quintiles, and similarly for each of the industry groups B-E and B-E excluding manufacturing. The results are shown in Table 3.

Table 3: SDID Results by Sub-industry and Total

	log Employment	log GVA	log PPE
B Mining and quarrying; C Manufacturing; D Electricity, gas, steam and air conditioning supply; E Water supply			
treat	-0.013***	-0.019**	-0.009
	0.005	0.008	0.008
Observations	11622	11622	11622
N NUTS-3	894	894	894
B-E Excluding Manufacturing			
treat	-0.040***	-0.075***	-0.048***
	0.010	0.015	0.012
Observations	12753	12753	12753
N NUTS-3	981	981	981
C Manufacturing			
treat	-0.009	0.006	0.014**
	0.006	0.009	0.007
Observations	10634	10634	10634
N NUTS-3	818	818	818

Note: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.10

The results show similar results for industries B-E, with negative treatment effects on employment and GVA and negative impacts on productivity per employee in sectors B-E when excluding manufacturing. Specifically, provinces in quintiles 4-5 saw a treatment effect on employment on (exp(-0.013) -1) * 100 = - 1.29% in Phase 3, relative to provinces in the bottom 3 quintiles of treatment. Similarly, GVA in this group decreased by 1.88% in Phase 3 relative to the control

group, while there was no significant impact on PPE. When focusing on the industries in mining and utilities (B, D, E), we find larger decreases in employment, GVA and PPE (-3.92%, -7.22%, and -4.69% , respectively.)

When focusing on manufacturing-level changes due to the regulatory impacts of Phase 3, the treatment impact on manufacturing is no longer significantly different than zero. This is in line with the literature on firm-level studies focused on the manufacturing sector, which typically find no significant impacts on manufacturing employment [Colmer et al., 2024, Dechezleprêtre et al., 2023].

5.3 Heterogeneity by economic development

A main concern of the EU ETS is the creation of potential inequalities. In order to explore the impact of the EU ETS's changes in allocation rules starting in Phase 3, we analyze treatment effects on separate sub-samples of provinces by level of output per capita. We split regions into terciles according to their GVA per capita in 2012, representing different levels of economic development. Figure 5 below shows a map of the categorization. We run the SDID analyses on each separate sample of level of development, comparing those provinces in the 4th and 5th quintile of net changes in allowances with those that had smaller changes, as before. Table 4 below shows the average treatment effect for each sub-sample of development. This table presents the results of the SDID analyses that estimate the treatment effects of changes in the allocation rules of the EU ETS starting in Phase 3. We estimate treatment effects across various sub-industries and also on the aggregated results.

In the sample in the first tercile, employment is largely unaffected across industries, with exceptions in the Construction (-0.031**) and B-E excluding manufacturing (-0.036**) industries, where significant effects are found relative to less treated provinces in this group. These two industries in this group also see a decline in GVA. However, only B-E excluding manufacturing sees a decline in PPE. We find positive impacts in employment in agriculture in these regions.

In the second tercile, we find that in sectors B-E excluding manufacturing, treated provinces experienced decreases across employment, GVA, and ppe. No other industry group in this tercile experienced employment decreases (although we find increases in agricultural employment). Furthermore, we find decreases in GVA in industries B-E (including manufacturing) and a modest but significant impact at the 10% level on total GVA across all industries. Furthermore, we find negative treatment effects in PPE for industries B-E, construction, agriculture, and on the aggregate levels across all industries.

Finally, in the highest tercile of economic development, we find negative impacts on employment in industries B-E, including manufacturing (which was not impacted in other terciles), but no significant impact on all other outcomes and industry groups, except for a decrease in productivity per employee in the construction sector. Overall, productivity and output remain largely unaffected across most sectors in the highest economically developed regions. While all terciles experienced

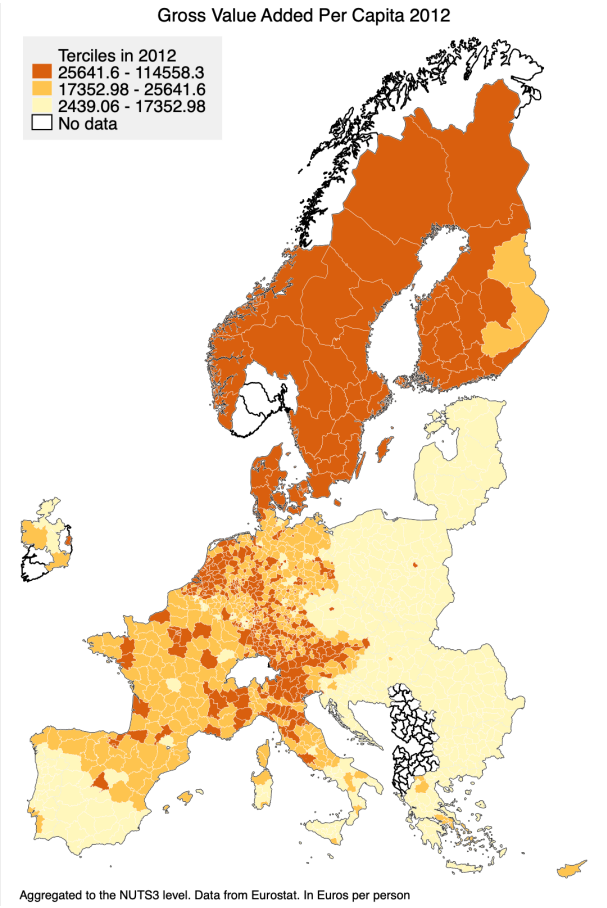


Figure 4: Terciles of GVA per Capita in 2012

Table 4: SDID Results by Tercile of GVA per capita: Sub-industry and Total

	Tercile 1			Tercile 2			Tercile 3		
	log Emp.	log GVA	log PPE	log Emp.	log GVA	log PPE	log Emp.	log GVA	log PPE
B Mining and quarrying; C Manufacturing; D Electricity, gas, steam and air conditioning supply; E Water supply									
treat	-0.016	-0.029	-0.012	-0.006	-0.036***	-0.028***	-0.016***	-0.003	0.007
	0.011	0.018	0.015	0.007	0.012	0.011	0.006	0.015	0.010
Observations	3601	3601	3601	3601	3601	3601	3601	3601	3601
N NUTS-3	277	277	277	277	277	277	277	277	277
B-E Excluding Manufacturing									
treat	-0.036**	-0.124***	-0.097***	-0.037**	-0.079***	-0.055**	-0.039**	-0.044	-0.014
	0.018	0.032	0.034	0.015	0.024	0.024	0.019	0.032	0.025
Observations	3523	3523	3523	3523	3523	3523	3523	3523	3523
N NUTS-3	271	271	271	271	271	271	271	271	271
C Manufacturing									
treat	-0.015	0.006	0.016	-0.004	-0.030**	-0.024***	-0.017**	0.009	0.015
	0.013	0.022	0.019	0.008	0.013	0.009	0.007	0.019	0.012
Observations	3601	3601	3601	3601	3601	3601	3601	3601	3601
N NUTS-3	277	277	277	277	277	277	277	277	277
F Construction									
treat	-0.031**	-0.056*	-0.039	0.002	-0.019	-0.026**	-0.013	-0.017	-0.026**
	0.015	0.033	0.030	0.011	0.019	0.012	0.012	0.018	0.013
Observations	3614	3614	3614	3601	3601	3601	3601	3601	3601
N NUTS-3	278	278	278	277	277	277	277	277	277
A Agriculture, Forestry and Fishing									
treat	0.041**	0.025	-0.004	0.028**	-0.016	-0.037**	-0.010	-0.015	-0.000
	0.018	0.021	0.025	0.013	0.014	0.018	0.015	0.021	0.018
Observations	3588	3588	3588	3588	3588	3588	3575	3575	3575
N NUTS-3	276	276	276	276	276	276	275	275	275
Total: All Industries									
treat	-0.003	-0.008	-0.010	0.006	-0.009*	-0.013***	-0.002	0.001	-0.002
	0.007	0.010	0.012	0.005	0.005	0.004	0.003	0.006	0.005
Observations	3614	3614	3614	3601	3601	3601	3601	3601	3601
N NUTS-3	278	278	278	277	277	277	277	277	277
Total Excluding B-E									
treat	-0.000	-0.003	-0.009	0.006	-0.006	-0.012***	-0.001	-0.001	-0.004
	0.008	0.010	0.014	0.005	0.005	0.004	0.004	0.006	0.005
Observations	3614	3614	3614	3601	3601	3601	3601	3601	3601
N NUTS-3	278	278	278	277	277	277	277	277	277

Note: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

a negative treatment impact on employment of industries in mining and utilities (electricity, gas, steam and air conditioning supply, water supply), only the first and second terciles experienced negative impacts on GVA and PPE. Furthermore, the second tercile seems to be the most impacted in terms of GVA and PPE in industries B-E, while the third tercile sees consistent negative effects on employment in these industries. Overall, the results suggest that there may be a higher degree of output and productivity losses in terciles 1-2, particularly in industries B-E.

5.4 Emissions

We now turn to study the impact of the EU ETS Phase 3 changes in allowance allocation on emissions. Since the main objective of the EU ETS program is to reduce emissions, it is important to consider whether the changes in the program reached this objective. We repeat the SDID analysis, using log emissions of regulated industries as our outcome of interest. The results are shown in Table 5. We find large and significant negative impacts on total emissions of regulated firms and on emissions of regulated firms in industries B-E. Provinces in the highest treated quintiles saw reductions of (exp(-0.146) -1) * 100 = 13.58% in regulated emissions in Phase 3, relative to provinces in the bottom 3 quintiles of treatment. For emissions of regulated industries in B-E, this was a reduction of 11.93%. These figure are significantly higher than the impacts on economic outcomes, but additional analyses are needed to quantify the tradeoffs.

Table 5: SDID Results on Emissions of Regulated Installations

	log Emissions Total	log Emissions B-E
treat	-0.146***	-0.127***
	(0.036)	(0.034)
Observations	9932	9841
N NUTS-3	764	757

Standard errors in parentheses

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

Note: Only balanced sample in emissions is included.

6 Discussion and Conclusion

6.1 Discussion

Our findings indicate that while provinces with the largest increases in net paid allowances achieved significant emissions reductions, this was accompanied by decreases in economic outcomes of mining, electricity, and utility supply industries. When focusing on the manufacturing industry, we find moderate decreases in employment (-1.39%) but no significant impacts in GVA or PPE in this sector. Furthermore, we report evidence for some potential spillover effects, particularly as we find negative impacts in employment, GVA, and PPE of the construction industry .

While we find impacts in the regulated industries, we find no evidence of a significant decrease in total economic activity. Pooling economic indicators across all industries (A-V) we find no significant overall treatment effects for the provinces in the highest treated quintiles relative to the less treated provinces, suggesting the impacts are concentrated across specific industries, particularly those most highly regulated and with the highest changes in free allowances from Phase 2-3, including electricity generators.

When focusing on manufacturing-level changes and outcomes for this industry, the treatment impact is no longer significantly different than zero. This is in line with the literature on firm-level studies focused on the manufacturing sector, which typically find no significant impacts on employment [Colmer et al., 2024, Dechezleprêtre et al., 2023]. While the changes in allowances in the manufacturing industries had no impact on manufacturing economic outcomes, the effect of province-level increases in net paid emissions (e.g., from other industries) had a negative impact on manufacturing employment. This is in line with the possibility that changes in allowances in other industries (i.e. B-E other than manufacturing) could have impacted manufacturing employment indirectly, through e.g. changes in electricity prices or other spillovers. These should be better explored in further analyses.

6.2 Conclusion

This paper investigates the impact of the EU ETS changes in allocation of allowances starting in Phase 3 of the program on economic outcomes at the NUTS-3 level. We apply a synthetic difference-in-differences to estimate and find that the allocation changes starting in 2013 led to a statistically significant decrease in employment, GVA, and PPE in industries B-E. In particular, this study finds that being more exposed to the policy change, meaning being a province with the largest changes between Phase 2 and phase 3 in the amount of allowances that must be purchased, reduces employment, GVA, and productivity per employees in targeted industries relative to provinces with less changes between Phase 2 and 3. We find the highest impacts on mining and quarrying, electricity, gas, steam and air conditioning supply, water supply, and construction. We find limited impact on manufacturing outcomes, in line with findings of previous studies. As with all studies,

our analysis is subject to some limitations. Given the current availability of data, we cover the period ending in 2020 leaving de facto out the last years in which the price of the CO_2 allowances has dramatically increased, remaining stably above 50 euros per tonne since August 2021 and surpassing 95 euros per tonne in February and August 2022. The effects of the policy might be even more accentuated in these last two years, pointing to a potential lower-bound of the reported impact. In the future, we hope to further study the mechanisms driving our outputs and make several robustness checks to our methods and measurements.

Notwithstanding, our study sheds light on the local economic impacts of the EU ETS, and includes data on Phase 3, a previously understudied phase. Following the growing literature focusing on the heterogeneous effects and the social externalities of the so-called green policies, this paper finds that the changes in the EU ETS allocation method decreased economic outcomes on some provinces and sectors, but had limited overall impact across all sectors. Our findings have relevant implications for both researchers and policy makers. From an academic perspective, this evidence raises several questions related to the mechanisms through which local economies have been able to react to the EU ETS.

7 Appendix

Table A1: List of countries in the analysis

Country	Number of NUTS-3 provinces in the analysis
Austria (AT)	35
Belgium (BE)	44
Bulgaria (BG)	28
Cyprus (CY)	1
Czech Republic (CZ)	14
Germany (DE)	401
Denmark (DK)	11
Estonia (EE)	5
Greece (EL)	52
Spain (ES)	59
Finland (FI)	19
France (FR)	100
Croatia (HR)	21
Hungary (HU)	20
Ireland (IE)	5
Italy (IT)	107
Lithuania (LT)	10

Luxembourg (LU)	1
Latvia (LV)	6
Malta (MT)	2
Netherlands (NL)	40
Norway (NO)	5
Poland (PL)	73
Portugal (PT)	25
Romania (RO)	42
Sweden (SE)	21
Slovenia (SI)	12
Slovakia (SK)	8
Total	**1,167**

Table 2: Industry Classifications

ISIC Rev. 4 Industry

Code	**Description**
A	Agriculture, Forestry and Fishing
B	Mining and Quarrying
C	Manufacturing
D	Electricity, Gas, Steam and Air Conditioning Supply
E	Water Supply
F	Construction
G	Wholesale and Retail Trade
H	Transportation and Storage
I	Accommodation and Food Service Activities
J	Publishing, Broadcasting, and Content Production and Distribution Activities
K	Telecommunication, Computer Programming, Consulting, Computing Infrastructure and Other Information Service Activities
L	Financial and Insurance Activities
M	Real Estate Activities
N	Professional, Scientific and Technical Activities
O	Administrative and Support Service Activities
P	Public Administration and Defence, Compulsory Social Security
Q	Education
R	Human Health and Social Work Activities
S	Arts, Sports and Recreation
T	Other Service Activities
U	Activities of Households as Employers and Undifferentiated Goods and Service-Producing Activities of Households for Own Use
V	Activities of Extraterritorial Organisations and Bodies

Note: GVA and employment are available at the following aggregate levels: A; B-E; C; F; G-I; G-J; J; K; K-N; L; M-N; O-U; R-U.

Table A3: All industries Means

	Phase 2 2008-2012 Q1-3	Phase 2 2008-2012 Q4-5	Phase 3 2013-2020 Q1-3	Phase 3 2013-2020 Q4-5
Employment Industry B-E	22,394	36,909	22,396	35,894
	(21597)	(37945)	(21606)	(36260)
GVA Industry B-E (million euros)	1,264	2,250	1,500	2,565
	(1486)	(2844)	(1776)	(3240)
PPE Industry B-E	0.06	0.06	0.07	0.07
	(0.03)	(0.04)	(0.03)	(0.04)
Employment B-E, Excluding Manufacturing	1,754	4,203	1,819	4,214
	(2225)	(5386)	(2404)	(5438)
GVA B-E, Excluding Manufacturing	184	459	211	494
	(332)	(744)	(365)	(779)
PPE B-E, Excluding Manufacturing	0.12	0.13	0.13	0.14
	(0.20)	(0.22)	(0.11)	(0.13)
Employment in Manufacturing	20,639	32,706	20,577	31,680
	(20331)	(34535)	(20267)	(32643)
GVA in Manufacturing	1,080	1,790	1,290	2,071
	(1309)	(2339)	(1594)	(2716)
PPE in Manufacturing	0.05	0.06	0.06	0.07
	(0.03)	(0.03)	(0.03)	(0.04)

Employment in Construction	8,560	16,528	8,238	14,847
	(8643)	(21078)	(8318)	(17071)
GVA in Construction	357	701	398	700
	(455)	(1105)	(512)	(959)
PPE in Construction	0.04	0.04	0.05	0.05
	(0.02)	(0.02)	(0.02)	(0.02)
Employment in G Wholesale and Retail Trade	30,755	64,786	32,045	67,717
	(41692)	(93466)	(44384)	(97560)
GVA in G Wholesale and Retail Trade	1,306	2,982	1,511	3,510
	(2684)	(5611)	(3130)	(6773)
PPE in G Wholesale and Retail Trade	0.04	0.04	0.04	0.04
	(0.02)	(0.02)	(0.02)	(0.02)
Employment in K Telecommunication, Computer Programming	15,753	36,519	17,468	40,445
	(31721)	(65521)	(35374)	(73267)
GVA in K Telecommunication, Computer Programming	1,472	3,264	1,739	3,898
	(3591)	(6441)	(4400)	(7767)
PPE in K Telecommunication, Computer Programming	0.09	0.09	0.10	0.09
	(0.03)	(0.03)	(0.03)	(0.04)
Employment Total (All Industries)	121,246	232,932	124,837	239,628
	(138405)	(294574)	(145989)	(307370)

GVA Total (All Industries)	5,912	12,021	6,913	13,958
	(9841)	(19220)	(11542)	(22445)
PPE Total (All Industries)	0.05	0.05	0.05	0.05
	(0.02)	(0.02)	(0.02)	(0.02)
Verified Emissions	285,542	3,384,628	262,446	2,997,600
	(1037106)	(5165060)	(870246)	(4876101)
Education Up to Upper Secondary (Share)	52.27	49.96	52.03	49.68
	(12.92)	(15.65)	(11.39)	(13.57)
Education Tertiary and Above (Share)	24.25	23.51	28.48	28.44
	(6.64)	(7.79)	(7.66)	(8.81)
Population (Thousands)	280	509	282	515
	(276)	(584)	(282)	(602)
Observations	3325	2510	5985	4518

Table A4: SDID 2008-2019

A Agriculture, Forestry and Fishing

	(1)	(2)	(3)
	log employment	log GVA	Log PPE
treat	0.016	-0.001	-0.009
	(0.011)	(0.011)	(0.011)

N	9924.000	9924.000	9924.000
N_clust	827.000	827.000	827.000

B Mining and quarrying; C Manufacturing; D Electricity, gas, steam and air conditioning supply; E Water supply

	(1)	(2)	(3)
	log employment	log GVA	Log PPE
treat	-0.010**	-0.019***	-0.012*
	(0.005)	(0.007)	(0.006)
N	9972.000	9972.000	9972.000
N_clust	831.000	831.000	831.000

B-E Excluding Manufacturing

	(1)	(2)	(3)
	log employment	log GVA	Log PPE
treat	-0.037***	-0.075***	-0.052***
	(0.009)	(0.016)	(0.016)
N	9756.000	9756.000	9756.000
N_clust	813.000	813.000	813.000

C Manufacturing

	(1)	(2)	(3)
	log employment	log GVA	Log PPE

treat	-0.012**	0.001	0.002
	(0.006)	(0.008)	(0.007)
N	9972.000	9972.000	9972.000
N_clust	831.000	831.000	831.000

F Construction

	(1)	(2)	(3)
	log employment	log GVA	Log PPE
treat	-0.019***	-0.027*	-0.028**
	(0.006)	(0.015)	(0.011)
N	9984.000	9984.000	9984.000
N_clust	832.000	832.000	832.000

G_J: Wholesale and retail trade; H Transportation and storage; I Accommodation and food service activities; J Publishing, broadcasting, and content production and distribution activities

	(1)	(2)	(3)
	log employment	log GVA	Log PPE
treat	0.005	-0.007	-0.011*
	(0.003)	(0.006)	(0.006)
N	9960.000	9960.000	9960.000
N_clust	830.000	830.000	830.000

K-N: K Telecommunication, Computer Programming, Consulting, Computing Infrastructure and Other Information Service Activities; L: Financial and Insurance Activities; M: Real Estate Activities; N: Professional, Scientific and Technical Activities

	(1)	(2)	(3)
	log employment	log GVA	Log PPE
treat	-0.003	-0.002	-0.001
	(0.005)	(0.005)	(0.005)
N	9960.000	9960.000	9960.000
N_clust	830.000	830.000	830.000

Total: All Industries

	(1)	(2)	(3)
	log employment	log GVA	Log PPE
treat	-0.000	-0.003	-0.005
	(0.003)	(0.005)	(0.005)
N	9984.000	9984.000	9984.000
N_clust	832.000	832.000	832.000

Total Excluding B-E

	(1)	(2)	(3)
	log employment	log GVA	Log PPE

treat	0.001	-0.002	-0.004
	(0.003)	(0.005)	(0.006)
N	9984.000	9984.000	9984.000
N_clust	832.000	832.000	832.000

Standard errors in parentheses

="* p<0.10 ** p<0.05 *** p<0.01"

Table A5: Including falsely treated provinces. Panel 2008-2020

A Agriculture, Forestry and Fishing

	(1)	(2)	(3)
	log employment	log GVA	Log PPE
treat	0.014*	0.004	-0.006
	(0.008)	(0.013)	(0.014)
N	14339.000	14339.000	14339.000
N_clust	1103.000	1103.000	1103.000

B-E: B Mining and quarrying; C Manufacturing; D Electricity, gas, steam and air conditioning supply; E Water supply

	(1)	(2)	(3)
	log employment	log GVA	Log PPE
treat	-0.009**	-0.019**	-0.013*

	(0.004)	(0.009)	(0.007)
N	14391.000	14391.000	14391.000
N_clust	1107.000	1107.000	1107.000

B-E: Excluding Manufacturing

	(1)	(2)	(3)
	log employment	log GVA	Log PPE
treat	-0.033***	-0.090***	-0.069***
	(0.011)	(0.015)	(0.015)
N	14092.000	14092.000	14092.000
N_clust	1084.000	1084.000	1084.000

C Manufacturing

	(1)	(2)	(3)
	log employment	log GVA	Log PPE
treat	-0.010*	0.002	0.002
	(0.005)	(0.010)	(0.008)
N	14391.000	14391.000	14391.000
N_clust	1107.000	1107.000	1107.000

F Construction

	(1)	(2)	(3)

	log employment	log GVA	Log PPE
treat	-0.018**	-0.011	-0.011
	(0.007)	(0.014)	(0.011)
N	14404.000	14404.000	14404.000
N_clust	1108.000	1108.000	1108.000

G_J: Wholesale and retail trade; H Transportation and storage; I Accommodation and food service activities;

J Publishing, broadcasting, and content production and distribution activities

	(1)	(2)	(3)
	log employment	log GVA	Log PPE
treat	0.007**	-0.004	-0.011**
	(0.003)	(0.005)	(0.005)
N	14365.000	14365.000	14365.000
N_clust	1105.000	1105.000	1105.000

Total: All Industries

	(1)	(2)	(3)
	log employment	log GVA	Log PPE
treat	0.003	0.001	-0.002
	(0.003)	(0.005)	(0.004)
N	14404.000	14404.000	14404.000

N_clust	1108.000	1108.000	1108.000

Total Excluding B-E

	(1)	(2)	(3)
	log employment	log GVA	Log PPE
treat	0.005*	0.003	0.000
	(0.003)	(0.005)	(0.005)
N	14404.000	14404.000	14404.000
N_clust	1108.000	1108.000	1108.000

Standard errors in parentheses

* p<0.10 ** p<0.05 *** p<0.01

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