This is a non-peer-reviewed preprint submitted to EarthArXiv.
Subsequent versions of this manuscript may have slightly different content. If accepted for publication, the final version will be available via the 'Peer-reviewed Publication DOI' link on this preprint's EarthArXiv page.

What Does a Country-Level Difference-in-Differences on Satellite NO_2 Identify?

Baseline Composition, Winter Retrievals and the European Climate Law

Sakshi D. Maske

Independent Geospatial Researcher
sakshimaske303@gmail.com

Version 1 · October 2026

Code & data: github.com/sakshimaske303-commits/GPIE

License: CC BY 4.0

What Does a Country-Level Difference-in-Differences on Satellite NO_2 Identify? Baseline Composition, Winter Retrievals and the European Climate Law

Sakshi D. Maske

Independent Geospatial Researcher

Abstract

Satellite observations make it easy to compare air pollution in countries that adopted a policy with countries that did not. This paper examines what such a comparison identifies. It uses Sentinel-5P TROPOMI tropospheric NO_2 for 2019–2024 and a country-level difference-in-differences (DiD) model of the European Climate Law that compares the EU-27 with nine neighbouring non-EU countries. NO_2 fell by 1.34×10^{-6} mol/m^2 more in the EU-27 (4.3% of its pre-treatment level, $p = 0.022$). The estimate is stable under year-month fixed effects, group-specific seasonality, meteorological and COVID-19 controls, a wild cluster bootstrap and the removal of any single comparison country. It loses significance without the winter months ($p = 0.11$), with countries weighted by area ($p = 0.11$), with log GDP ($p = 0.21$), with real GDP per capita in place of nominal GDP ($p = 0.072$) and under randomization inference on the coefficient ($p = 0.17$), and it appears only from the third year after the law. Most of it reflects the composition of the two groups. When the treatment label is given to random sets of countries, the estimate tracks how much more polluted the labelled group was before treatment ($r = -0.81$), and the EU-27 estimate is close to what its pre-treatment gap predicts. On a 0.1° grid, comparing EU and comparison-group pixels that started at the same NO_2 level reduces the estimate to -0.21×10^{-6} (95% CI −0.67 to +0.24). The same comparison does detect a known 2.5% reduction added to EU-27 NO_2 after July 2021 (-0.80×10^{-6}, $p = 0.002$). The satellite values agree with 1,543 ground stations of the European Environment Agency (monthly $r = 0.68$). The stations give a relative EU-27 decline of similar size that is not significant (−4.0%, $p = 0.12$), and they do not show the December 2023 drop that the satellite records in Denmark and Sweden. Descriptively, polluted areas lost about 15–25% of their NO_2 in north-western Europe, including the non-EU United Kingdom and Switzerland, but little in south-eastern Europe, including several EU members, in western Turkey and in Serbia. The data cannot separate EU membership from region, shared emission standards or winter retrieval conditions. Country-level DiD on satellite NO_2 should be checked against comparisons matched on baseline pollution before it is read as a policy effect.

Keywords: nitrogen dioxide; TROPOMI; difference-in-differences; aggregation; parallel trends; policy evaluation

1. Introduction

Tropospheric nitrogen dioxide (NO_2) is one of the few air pollutants that can be observed from space every day, almost everywhere, with a single instrument. Since the launch of Sentinel-5P in 2017, the TROPOMI instrument (Veefkind et al., 2012) has made it possible to compare NO_2 across borders without relying on national monitoring networks or self-reported inventories. Satellite-derived pollution is now a common outcome in quasi-experimental studies in which the unit of analysis is an administrative area: counties exposed to a trade shock (Chen et al., 2020), cities that opened subway systems (Gendron-Carrier et al., 2022),

and countries or first-level regions under COVID-19 lockdowns (Dang & Trinh, 2021; Keola & Hayakawa, 2021). The design is simple. It compares the change in pollution in treated areas with the change in untreated ones.

The design is harder to read when the treated and comparison areas differ in how polluted they are to begin with, because the change in a satellite NO_2 column depends on its level. Clean areas have little to lose, a background contribution from the free troposphere does not respond to surface emissions (Silvern et al., 2019), and the retrieval itself is biased by amounts that depend on pollution level (Verhoelst et al., 2021). If NO_2 falls more where it is higher, two groups of countries with different shares of polluted area will diverge even when comparable places change in the same way, and a difference-in-differences (DiD) in levels will record the divergence as a treatment effect.

This paper asks: when treated and comparison countries differ in baseline pollution, what does a country-level DiD on satellite NO_2 identify? It has two parts. RQ1: how much of the country-level estimate is explained by the difference in pre-treatment NO_2 between the two groups? RQ2: when EU and comparison areas are compared at the same starting level, where did NO_2 fall, and does the pattern follow the EU border?

The case studied is the European Climate Law (Regulation (EU) 2021/1119), adopted on 30 June 2021. It applies to all 27 member states from the same date and not to their neighbours, which gives a clean two-group design. It is also a case in which a dated effect on NO_2 is implausible. The law sets a binding target of climate neutrality by 2050 and an intermediate target for 2030. NO_2 in Europe is governed by other instruments, among them vehicle emission standards, the Industrial Emissions Directive (2010/75/EU) and the National Emission reduction Commitments Directive ((EU) 2016/2284), and road transport is its main source (European Environment Agency, 2025). The law is used deliberately as a negative-control case, a treatment with a clear date and a clear border but no plausible dated effect on NO_2. A country-level DiD around July 2021 then tests the method more than the law, and any estimate that looks like an effect needs another explanation.

The paper proceeds in three steps. It first estimates the country-level DiD and reports its behaviour under a wide set of specifications in one table, without sorting them into those that support the estimate and those that do not. It then tests whether the estimate reflects baseline composition (RQ1), using a country-level interaction, a placebo exercise with random assignment of the treatment label, and a pixel-level comparison matched on pre-treatment NO_2, and it checks with a positive control, a known reduction added to the data, that these models can detect an effect of plausible size. Finally it describes where NO_2 fell, country by country, including areas outside the study sample (RQ2), and checks the satellite data against ground stations.

The country-level estimate is negative and significant under many standard specifications, and fragile under others that are rarely reported: the treatment of winter retrievals, country weighting and the measure and functional form of the GDP control. Most of the estimate is accounted for by the difference in pre-treatment pollution between the two groups. Comparisons matched on that level leave an estimate that is small and not distinguishable from zero, although they would detect a uniform reduction of about 3%. And the decline in polluted areas is geographically uneven in a way that does not follow the EU border, although the data cannot say why.

Section 2 reviews related work. Section 3 describes the data and Section 4 the methods. Section 5 presents the results, Section 6 discusses them, Section 7 lists limitations and Section 8 concludes.

2. Related Work

2.1 Satellite NO_2: Trends, Retrieval and Meteorology

TROPOMI NO_2 columns have been used widely to measure the atmospheric response to discrete events, above all the COVID-19 lockdowns, during which reductions of tens of percent were reported over European cities (Barré et al., 2021; Vîrghileanu et al., 2020). Longer satellite records show that European NO_2 has declined for two decades, at rates that differ between regions and that have slowed or reversed in places (Georgoulias et al., 2019).

Four properties of the measurement matter for policy evaluation. Columns do not scale one-to-one with emissions: the free-tropospheric background is insensitive to surface sources (Silvern et al., 2019), and the lifetime of NO_x varies non-linearly with its concentration (Laughner & Cohen, 2019). TROPOMI tropospheric columns are biased low against ground-based instruments, by more over highly polluted areas than over clean ones (Verhoelst et al., 2021). The processor has changed several times, with version 2.2 raising columns by 10–40% relative to earlier versions depending on pollution level and season (van Geffen et al., 2022). And weather modulates concentrations strongly, which is why satellite-based estimates that neglect it can misstate changes (Barré et al., 2021) and why meteorological normalization is standard in the analysis of surface air-quality trends (Grange et al., 2018).

2.2 Quasi-Experimental Designs with Satellite Pollution

The studies cited in the introduction aggregate satellite pollution to administrative units and compare treated with untreated units over time. Two results from that literature are relevant here. Gendron-Carrier et al. (2022) find no average effect of subway openings on aerosol optical depth but a reduction in cities that were initially more polluted, which shows that effects measured on satellite pollution can depend on the starting level. Sager and Singer (2025) show that a standard DiD overstates the effect of the 2005 fine-particle standard in the United States several times over, because treated and untreated areas were on different paths. Evaluations with a direct mechanism and ground monitors can reach sharper conclusions: for London's Ultra Low Emission Zone, an augmented synthetic control found reductions in NO_2 after its 2019 introduction and no detectable effect of the 2023 expansion (Tong et al., 2025).

2.3 Difference-in-Differences When Groups Differ in Levels

DiD identifies a treatment effect under the assumption that treated and comparison units would have followed parallel trends without treatment (Roth et al., 2023). Three strands of recent work bear on this study. First, parallel trends is not invariant to the scale of the outcome. When groups start from different levels, trends that are parallel in levels are not parallel in logs, and the assumption can hold for all transformations only under strong conditions (Kahn-Lang & Lang, 2020; Roth & Sant'Anna, 2023). The remedy is to make the assumption conditional on pre-treatment characteristics (Abadie, 2005; Sant'Anna & Zhao, 2020), although matching on a noisy pre-treatment outcome can introduce regression to the mean (Daw & Hatfield, 2018). Second, tests of pre-treatment trends have low power, and conditioning on passing them can distort inference (Roth, 2022), and sensitivity analysis is preferable to a pass-or-fail reading (Rambachan & Roth, 2023). Third, with few clusters, cluster-robust standard errors (Bertrand et al., 2004) can be too small. Recommended practice is to use the t distribution with degrees of freedom tied to the number of clusters (Cameron & Miller, 2015), the wild cluster bootstrap (Cameron et al., 2008), or randomization inference (Conley & Taber, 2011; MacKinnon & Webb, 2020).

2.4 Aggregation and the Unit of Analysis

That relationships estimated on aggregates need not hold for the units within them has been known since Robinson's (1950) work on ecological correlation, and the dependence of statistical results on the choice of

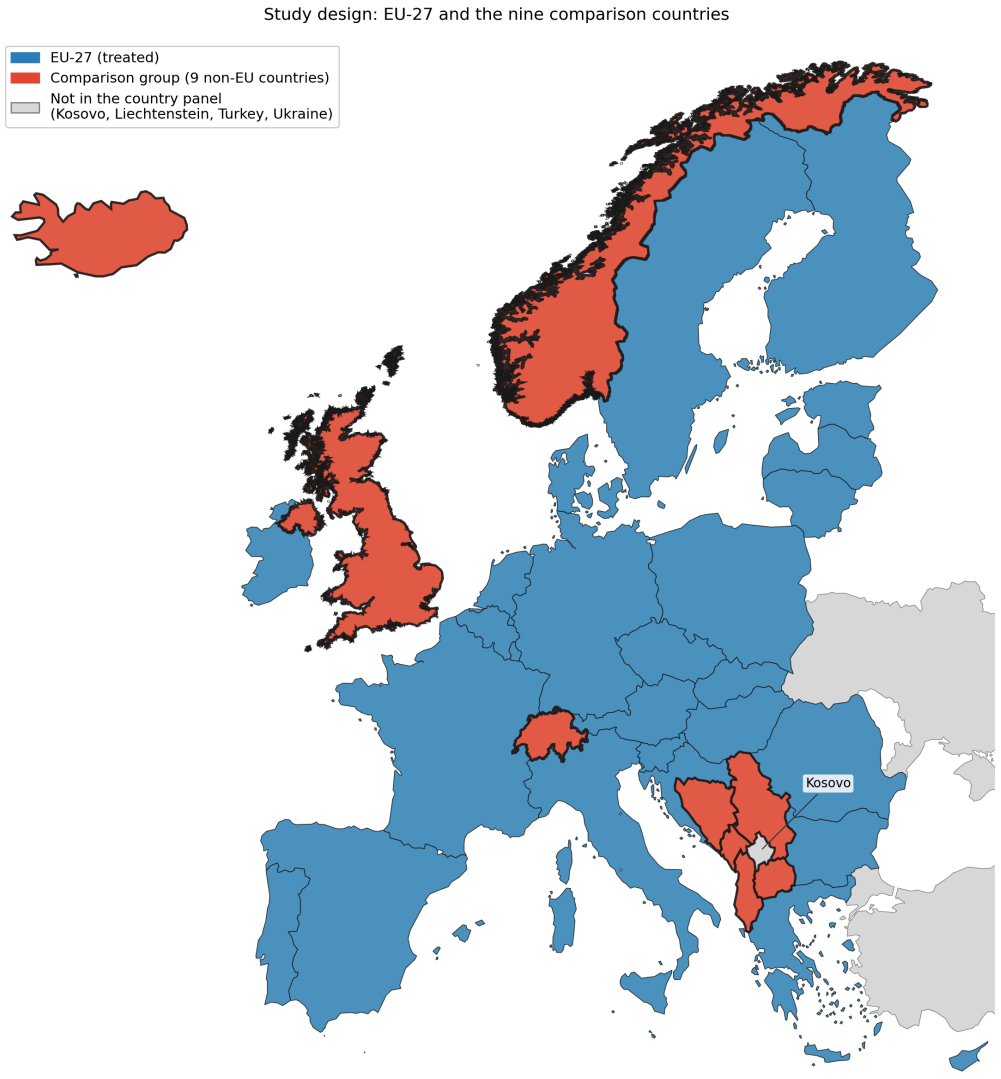
areal unit is the modifiable areal unit problem (Fotheringham & Wong, 1991). Satellite data are unusual in that the analyst chooses the unit. The same retrievals can be averaged to countries, regions or pixels. A country mean of NO_2 combines cities, industrial areas and rural background in proportions that differ greatly between countries. RQ1 asks how much of a country-level DiD estimate comes from that difference.

3. Data

3.1 Study Area and Period

The treated group is the EU-27. The comparison group consists of nine non-EU countries in Europe for which all covariates are available: the United Kingdom, Norway, Switzerland, Iceland, Albania, Bosnia and Herzegovina, Montenegro, North Macedonia and Serbia (Figure 1). The study period is January 2019 to December 2024, giving 30 pre-treatment and 42 post-treatment months around the adoption of the Climate Law on 30 June 2021 (Regulation (EU) 2021/1119; in force 29 July 2021). July 2021 is the first post-treatment month.

Figure 1. Study design: EU-27 (treated) and the nine comparison countries. Kosovo, Liechtenstein, Turkey and Ukraine (grey) are not in the country panel.



The comparison group is not free of EU climate policy. Norway and Iceland have taken part in the EU Emissions Trading System since 2008 (European Commission, 2016), the Swiss system has been linked to it since 1 January 2020 (Swiss Federal Council, 2019), and the Western Balkan countries endorsed the Green

Agenda for the Western Balkans in November 2020 (Regional Cooperation Council, 2020). The United Kingdom was an EU member in 2019 and applied EU law during the 2020 transition period. Shared exposure of this kind would bias a DiD estimate toward zero.

3.2 NO_2

The outcome is the tropospheric NO_2 vertical column density from the Sentinel-5P TROPOMI Level-2 product (mol/m^2), retrieved through the Copernicus Data Space Sentinel Hub Process API. For each of the 2,192 days in the study period, one raster was requested over the study area (27.5–71.5° N, 31.5° W–35° E) on a regular 0.1° grid (665 × 440 pixels), keeping only retrievals with a quality value of at least 0.75 (Eskes & Eichmann, 2023). The requests leave resampling and mosaicking at the Process API defaults, so each 0.1° value is the nearest Level-2 ground pixel rather than an area average, and where orbits overlap within a day a single orbit is used. The native ground pixel is 3.5 × 7 km before 6 August 2019 and 3.5 × 5.5 km afterwards.

Monthly means pool all valid pixel-days. For a country, the monthly mean is the sum of all valid retrievals inside its boundary divided by the number of valid pixel-days, so days and areas with more valid, mostly clear-sky, retrievals carry more weight. For a pixel, it is the mean of its valid days in the month, requiring at least three. Country means rebuilt from the pixel grid reproduce the country series, so the two levels of analysis use the same data. Boundaries are Eurostat GISCO NUTS level 0 (2024), and GADM 4.1 for the United Kingdom, Norway and Switzerland.

The panel has 2,592 country-months, of which 2,572 have an NO_2 value. The 20 missing country-months are in Iceland (9), Finland (6), Norway (4) and Estonia (1), all in December or January, when high-latitude retrievals are unavailable. On average 35% of pixel-days have a valid retrieval, with 24% in November–February and 41% in May–September. Eight country-months have a non-positive mean, all in December or January in Nordic and Baltic countries, and four of them in December 2023 (Denmark, Sweden, Latvia, Lithuania). A negative column is physically impossible and indicates retrieval error.

The processor version matters for comparisons over time. On 24 sample days (the 15th of January, April, July and October in each year), the values served are from the reprocessed record up to July 2022 and from operational products afterwards. The product catalogue confirms version 2.4.0 on 14 of the 15 reprocessed sample days (it returned no entry for 15 April 2019), and versions 2.4.0 (October 2022, January 2023), 2.5.0 (April–October 2023), 2.6.0 (January–July 2024) and 2.7.1 (October 2024) for the operational products. The large change to version 2.2 on 1 July 2021 (van Geffen et al., 2022), which coincides with the treatment date, therefore does not enter the series. The later operational updates do, and Section 6 returns to them.

3.3 Covariates

Monthly mean 2 m temperature, total precipitation, 10 m wind speed and boundary-layer height come from the ERA5 reanalysis (Hersbach et al., 2020), averaged over each country. Annual GDP at current prices comes from Eurostat for the EU-27 and from the World Bank for the comparison countries, converted from US dollars to euros at approximate annual average exchange rates. As a check, real GDP per capita, in purchasing power parity at constant 2021 international dollars and in constant 2015 US dollars, is taken from the World Bank for all 36 countries. The national stringency index of the Oxford COVID-19 Government Response Tracker (Hale et al., 2021) is averaged to months. It covers 34 of the 36 countries (not Montenegro or North Macedonia) and is set to zero outside 2020–2022. Table 1 lists the sources.

Table 1. Data sources.

Variable	Source	Resolution used
Tropospheric NO_2 column	Sentinel-5P TROPOMI Level-2, quality value ≥ 0.75	Daily 0.1° rasters; monthly country and pixel means
Temperature, precipitation, wind speed, boundary-layer height	ERA5 reanalysis	Monthly, country means
GDP; real GDP per capita (check)	Eurostat; World Bank	Annual, country
NO_2 at ground stations (check)	European Environment Agency, verified air-quality data (E1a)	Hourly, station; monthly means
COVID-19 stringency	Oxford COVID-19 Government Response Tracker	Monthly, country (34 of 36)
Boundaries	Eurostat GISCO NUTS 0 (2024); GADM 4.1	Country

Pooled over country-months, mean NO_2 before treatment is 31.3 × 10^{-6} mol/m² in the EU-27 and 20.8 × 10^{-6} in the comparison group. Pre-treatment country means range from 14 to 74 × 10^{-6} (Netherlands) in the EU-27, and from 5 (Iceland) to 38 × 10^{-6} (United Kingdom) in the comparison group.

4. Methods

4.1 Country-Level Models

The baseline model is the two-group DiD most often used in applied work:

$$NO2_{it} = \beta\,(EU_i \times Post_t) + \gamma\,Post_t + X_{it}'\delta + \alpha_i + \mu_{m(t)} + \varepsilon_{it}$$

where i indexes countries and t months, EU_i indicates EU-27 membership, $Post_t$ indicates July 2021 or later, X_{it} contains temperature, precipitation and GDP, α_i are country fixed effects and $\mu_{m(t)}$ are calendar-month fixed effects common to both groups.

The main model replaces the post indicator and the common seasonal cycle with year-month fixed effects λ_t and lets the EU-27 have its own seasonal cycle $\theta_{m(t)}$:

$$NO2_{it} = \beta\,(EU_i \times Post_t) + X_{it}'\delta + \alpha_i + \lambda_t + EU_i \times \theta_{m(t)} + \varepsilon_{it}$$

The EU-specific seasonal terms are needed because NO_2 has a larger winter peak in more polluted countries, so a common seasonal cycle leaves a seasonal pattern in the difference between the groups. Unless stated otherwise, country-level results refer to the main model, countries are unweighted, and standard errors are clustered by country.

The main model is re-estimated under alternative specifications (Table 2): additional controls; exclusion of the COVID-19 and energy-crisis periods and of each comparison country in turn; exclusion of winter months, of December 2023, of the eight non-positive country-months and of country-months with few valid retrievals; area weights; GDP in logs, omitted or replaced by real GDP per capita; a log outcome; the sample restricted to 2020–2024; and the interaction between each country's pre-treatment NO_2 level and the post indicator.

4.2 Timing

The treatment indicator is replaced with interactions between EU_i and blocks of months relative to July 2021, with July 2020–June 2021 as the reference. The four full blocks cover twelve months each, so seasonality does not affect them. The first block (January–June 2019) and the last (July–December 2024) cover six months. Two further diagnostics shift the treatment date by 6 and 12 months in both directions and add an EU-specific linear trend, and placebo dates are placed inside the pre-treatment period.

4.3 Inference

P-values and confidence intervals for coefficients use the t distribution with degrees of freedom equal to the number of countries minus one (Cameron & Miller, 2015). Because only nine of the 36 clusters are comparison countries, the main estimate is also tested with a restricted wild cluster bootstrap with Rademacher weights (Cameron et al., 2008; 9,999 replications) and with randomization inference, in which the EU label is reassigned to 27 randomly chosen countries (MacKinnon & Webb, 2020; 999 assignments), reported for both the coefficient and the t-statistic. Spatial dependence in the residuals is assessed with Moran's I (Moran, 1950), computed month by month on the residuals of the baseline model with four-nearest-neighbour weights.

4.4 Random Assignment and Baseline Composition

The random assignments also provide a direct test of the composition explanation. For each assignment, I record the DiD estimate and the difference in mean pre-treatment NO_2 between the countries labelled treated and the rest. If a DiD in levels responds to baseline composition, the estimates will be related to that difference even though the labels are random. The share of random assignments with a significant estimate at the 5% level shows how often the clustered t-test finds an "effect" for a label that carries no treatment.

Three further country-level checks address composition directly. The model is re-estimated without the two most polluted EU members, the Netherlands and Belgium; on the countries whose pre-treatment level does not exceed that of the most polluted comparison country, the United Kingdom (common support); and, on the log scale, with the interaction between each country's log pre-treatment level and the post indicator.

4.5 Pixel-Level Comparison Matched on Pre-Treatment Level

Each pixel's pre-treatment level is its mean NO_2 over January 2019–June 2021. All period means are computed by averaging within calendar months first and then across the twelve calendar months, so that every season has the same weight in every period. A period mean requires at least nine calendar months with data. Pixels with a valid pre-treatment mean (about 54,500 of the roughly 68,000 inside the 36 countries) are assigned to eight classes by that level: below 1.5, 1.5–2, 2–2.5, 2.5–3, 3–4, 4–5, 5–7 and above 7 × 10^{-5} mol/m^2. The data are collapsed to country × class × month cells. Cells with fewer than five pixels are dropped, and a cell-month is used if at least three pixels and 30% of the cell's pixels have a value. The model is

$$NO2_{ict} = \beta\,(EU_i \times Post_t) + X'_{it}\delta + \alpha_{ic} + \lambda_{ct} + EU_i \times \theta_{m(t)} + \varepsilon_{ict}$$

where c indexes classes, α_{ic} are country-by-class fixed effects and λ_{ct} are class-specific year-month fixed effects. With λ_{ct}, EU pixels are compared only with comparison-group pixels of the same class in the same month. This is stratification on the pre-treatment level rather than one-to-one matching, and "matched" is used below for brevity. Cells are weighted by their number of pixels, rescaled so that each country has the same total weight in every month, as in the country-level model. Replacing λ_{ct} with λ_t gives the unmatched counterpart on the same cells. Standard errors are clustered by country. Malta (three pixels) has no class with five pixels and drops out, so the pixel-level models have 35 clusters and use t(34). The difference between the

unmatched and matched estimates is tested with a cluster bootstrap that resamples countries within each group (499 draws).

Two checks address regression to the mean (Daw & Hatfield, 2018): classes are redefined from 2019 alone with the model estimated on 2020–2024, and the relationship between change and starting level is re-estimated with a starting level (2019) that is not part of the change (2020–June 2021 to July 2023–December 2024). The number of classes is varied between 4 and 16. The matched model is also estimated in the 12-month blocks of Section 4.2, and with March 2020–June 2021, the months of COVID-19 restrictions, left out of the estimation.

4.6 Descriptive Geography

To describe where NO_2 changed, the change from the pre-treatment period to July 2023–December 2024 is reported by pre-treatment class and, for pixels above 3×10^{-5} mol/m^2, by country (countries with at least 20 such pixels). The later period was chosen after the block estimates of Section 4.2 showed where the country-level estimate originates, and the threshold of 3×10^{-5} mol/m^2 for polluted pixels was set at the same stage, so these comparisons are descriptive. The daily rasters also cover three areas outside the country panel: Turkey and Ukraine west of 35° E, and Kosovo. They have no covariates here and are used only descriptively and in matched models without controls. Finally, the matched model is estimated on subsets that hold region fixed: north-western EU members (Netherlands, Belgium, Luxembourg, Germany, Denmark, France, Austria, Ireland) against the United Kingdom and Switzerland; south-eastern EU members (Croatia, Hungary, Romania, Bulgaria, Greece, Slovenia) against the Western Balkans; and north-western against south-eastern EU members. This grouping was also chosen after inspecting the data.

4.7 Positive Control

The law is a negative-control case, so a null result is informative only if the models could have detected an effect. To check this, a known reduction is added to the observed data and the models are re-estimated. The reduction is proportional, since a policy that cuts emissions by a given share lowers the column by roughly that share: NO_2 in the EU-27 from July 2021 onwards is multiplied by 0.975, 0.95 or 0.90, a reduction of 2.5%, 5% or 10%. The main country-level model and the matched pixel model are re-estimated on the modified data, and the change in each estimate is compared with the average reduction that was added. The same reductions are added to the 999 random groups of Section 4.4, which shows how often a reduction of each size is detected when the treated group is not the EU-27. For the matched model, the smallest effect detectable with 80% power at the 5% level is computed as $(t_{0.975} + t_{0.80}) \times \text{SE}$ with 34 degrees of freedom and expressed as the uniform percentage reduction that would move the estimate by that amount.

4.8 Ground Stations

The satellite data are compared with NO_2 measured at the European Environment Agency's air-quality stations. Hourly verified data for 2019–2024 are averaged to months when at least 75% of hours are valid, and a sampling point is kept if it has a monthly value in at least 80% of the 72 months. The main comparison uses background stations, with traffic stations as a check. Each station is matched to the 0.1° pixel that contains it. Three questions are asked: whether station and pixel values agree in level, over time and in their change from the pre-treatment period to July 2023–December 2024 (period means balanced by calendar month, as for pixels); whether the main country-level model, applied to country-month means of station NO_2, gives an estimate of similar relative size to the same model on the satellite pixels at the same stations; and whether the stations show the December 2023 drop in the countries whose satellite means were negative. Stations

measure surface concentrations in μg/m³ and the satellite a tropospheric column, so the comparison is of relative changes and patterns, not of levels.

5. Results

5.1 The Country-Level Estimate

Between the pre- and post-treatment periods, mean NO_2 fell from 31.3 to 29.7 × 10^{-6} mol/m² in the EU-27 and from 20.8 to 20.7 × 10^{-6} in the comparison group. The baseline DiD estimate is −1.52 × 10^{-6} (p = 0.018), 4.9% of the EU-27's pre-treatment mean, and the main model gives −1.34 × 10^{-6} (p = 0.022), or 4.3%. Table 2 reports the estimate under all specifications.

Table 2. Country-level estimates of the EU × post coefficient (10^{-6} mol/m²; log outcomes in log points).

Specification	Estimate	95% CI	p
Model			
Baseline: country and common calendar-month fixed effects	−1.52	−2.77 to −0.27	0.018
Country and year-month fixed effects	−1.53	−2.79 to −0.28	0.018
Main model: + EU-specific calendar-month effects	−1.34	−2.47 to −0.21	0.022
Added controls and excluded periods (main model)			
+ wind speed and boundary-layer height	−1.36	−2.50 to −0.21	0.022
+ COVID-19 stringency (34 countries)	−1.48	−2.71 to −0.26	0.019
Excluding March 2020–June 2021	−2.25	−4.04 to −0.46	0.015
Excluding July 2021–December 2022	−2.08	−3.58 to −0.58	0.008
Excluding 2023–2024	−0.26	−1.12 to +0.61	0.55
2020–2024 only	−0.59	−1.37 to +0.19	0.13
Winter and data quality			
Without November–February	−0.81	−1.81 to +0.20	0.11
Without December–February	−1.06	−2.14 to +0.01	0.053
May–September only	−0.54	−1.43 to +0.35	0.23
Without December 2023	−1.16	−2.24 to −0.09	0.035
Without the eight non-positive country-months	−1.38	−2.50 to −0.26	0.017
Country-months with at least 10% valid pixel-days	−1.28	−2.38 to −0.18	0.024
Weighting and functional form			
Countries weighted by area	−0.96	−2.15 to +0.23	0.11
Log GDP in place of GDP	−0.94	−2.44 to +0.56	0.21

Specification	Estimate	95% CI	p
No GDP	−1.31	−2.80 to +0.18	0.082
Real GDP per capita (PPP) in place of GDP	−1.36	−2.84 to +0.13	0.072
Log real GDP per capita (PPP) in place of GDP	−0.81	−2.15 to +0.53	0.23
Real GDP per capita (constant US$) in place of GDP	−1.32	−2.84 to +0.21	0.088
Log outcome	−0.030	−0.06 to +0.00	0.069
Log outcome, no GDP	−0.029	−0.07 to +0.01	0.15
Baseline composition			
+ pre-treatment NO_2 level × post: EU × post	+0.58	−0.44 to +1.60	0.26

The estimate is stable under the changes that applied work most often reports. Year-month fixed effects and group-specific seasonality change it little. Boundary-layer height is strongly related to NO_2 ($p < 0.001$), but adding it and wind speed leaves the estimate unchanged, as does the stringency index. Excluding the months most affected by COVID-19 restrictions, or the months of the 2021–2022 energy-price shock, makes it larger. Leaving out each comparison country in turn gives estimates between −1.20 and -1.42×10^{-6} (p between 0.016 and 0.032), and leaving out each EU member in turn gives estimates between -0.98×10^{-6} (without the Netherlands) and -1.46×10^{-6} (without France). The wild cluster bootstrap gives $p = 0.018$.

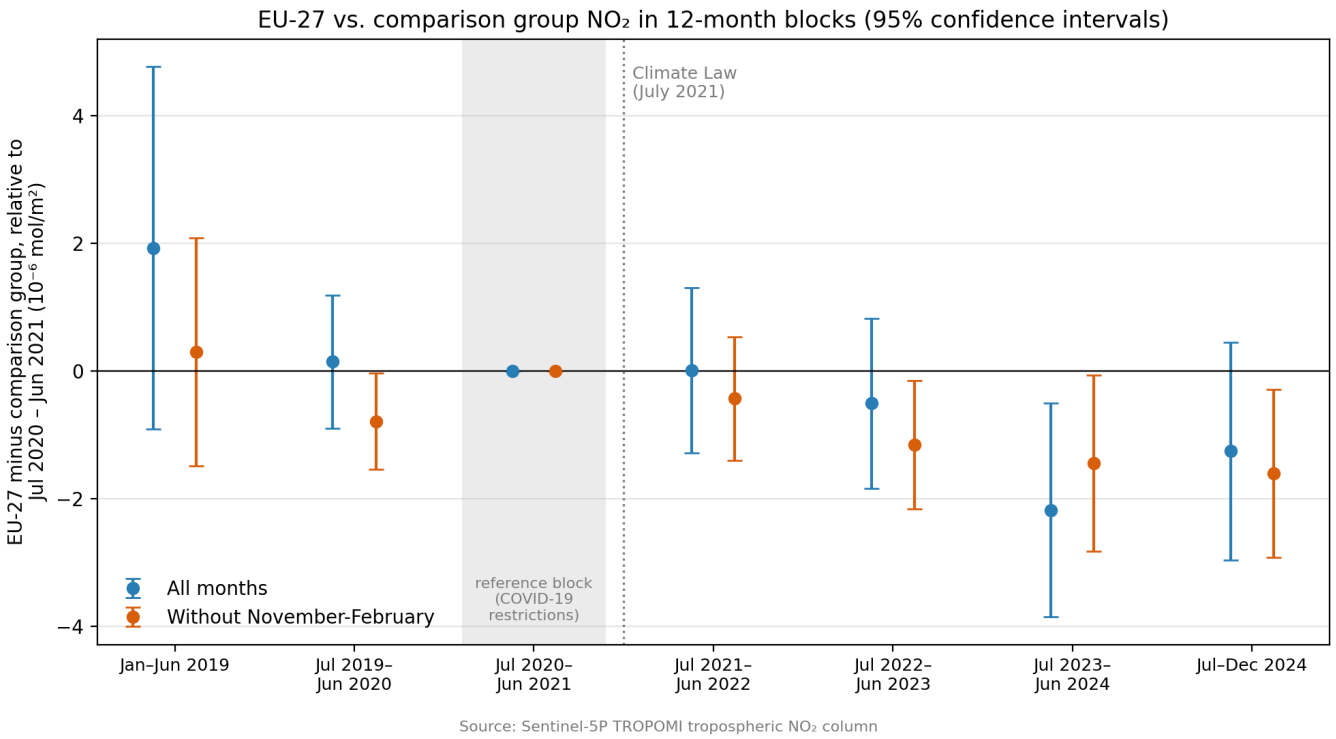
It is not stable under several other changes. Without November–February it falls by 40% and is no longer significant. Winter is when valid retrievals are fewest and when the non-positive country means occur. The estimate also depends on two modelling choices. Every country gets the same weight, so Malta (three pixels) counts as much as Germany, and nominal GDP enters in levels. With area weights or with log GDP the estimate falls by about 30% and loses significance, and without GDP its confidence interval includes zero. Replacing nominal GDP with real GDP per capita from a single source leaves the estimate about the same size (-1.36×10^{-6}) but its confidence interval includes zero ($p = 0.072$). With log real GDP per capita it falls to -0.81×10^{-6} ($p = 0.23$). On the log scale, which measures proportional change, the estimate is −3.0% ($p = 0.069$), and $p = 0.15$ without GDP. Randomization inference gives $p = 0.046$ when based on the t-statistic and $p = 0.17$ when based on the coefficient.

Moran's I on the residuals is positive and significant in 59 of 72 months (median 0.35). Neighbouring countries' errors are correlated, which clustering by country does not address, so the p-values in Table 2 are, if anything, too small.

5.2 Timing

In blocks of months relative to the reference block July 2020–June 2021 (Figure 2), the pre-treatment estimates are $+1.93 \times 10^{-6}$ for January–June 2019 (95% CI −0.91 to +4.77) and $+0.15 \times 10^{-6}$ for July 2019–June 2020 (−0.89 to +1.19). A Wald test of both gives $p = 0.29$. They do not differ significantly from the reference block, but the first is imprecise and larger in size than the estimate itself, so the absence of a pre-existing trend is not established. After the law, the first two blocks show no change ($+0.01 \times 10^{-6}$, $p = 0.98$; -0.51×10^{-6}, $p = 0.45$). The estimate comes from July 2023–June 2024 (-2.18×10^{-6}, $p = 0.012$), with -1.25×10^{-6} ($p = 0.15$) in July–December 2024, and without 2023–2024 it is -0.26×10^{-6} (Table 2).

Figure 2. EU-27 minus comparison-group NO_2 in blocks of months relative to July 2020–June 2021, with and without November–February. The shaded reference block falls in the period of COVID-19 restrictions.



This timing depends on the winter months. Without November–February, the blocks after the law are −0.43, −1.15, −1.44 and −1.60 × 10^{-6}, and July 2019–June 2020 is −0.79 × 10^{-6} (p = 0.04): a gradual drift that starts before the law. Removing December 2023 alone reduces the July 2023–June 2024 block from −2.18 to −1.49 × 10^{-6}.

The estimate is not specific to the treatment date. Shifting the date gives −1.42 × 10^{-6} (p = 0.049) twelve months earlier, −1.14 × 10^{-6} (p = 0.038) six months earlier, −1.38 × 10^{-6} (p = 0.024) six months later and −1.60 × 10^{-6} (p = 0.010) twelve months later. With an EU-specific linear trend the treatment coefficient is +0.86 × 10^{-6} (p = 0.28), and with a linear trend for every country it is +1.12 × 10^{-6} (p = 0.15). Placebo dates inside the pre-treatment period give p = 0.07, 0.39 and 0.89. Whether read as a late step or as a gradual drift, the relative decline is not tied to July 2021.

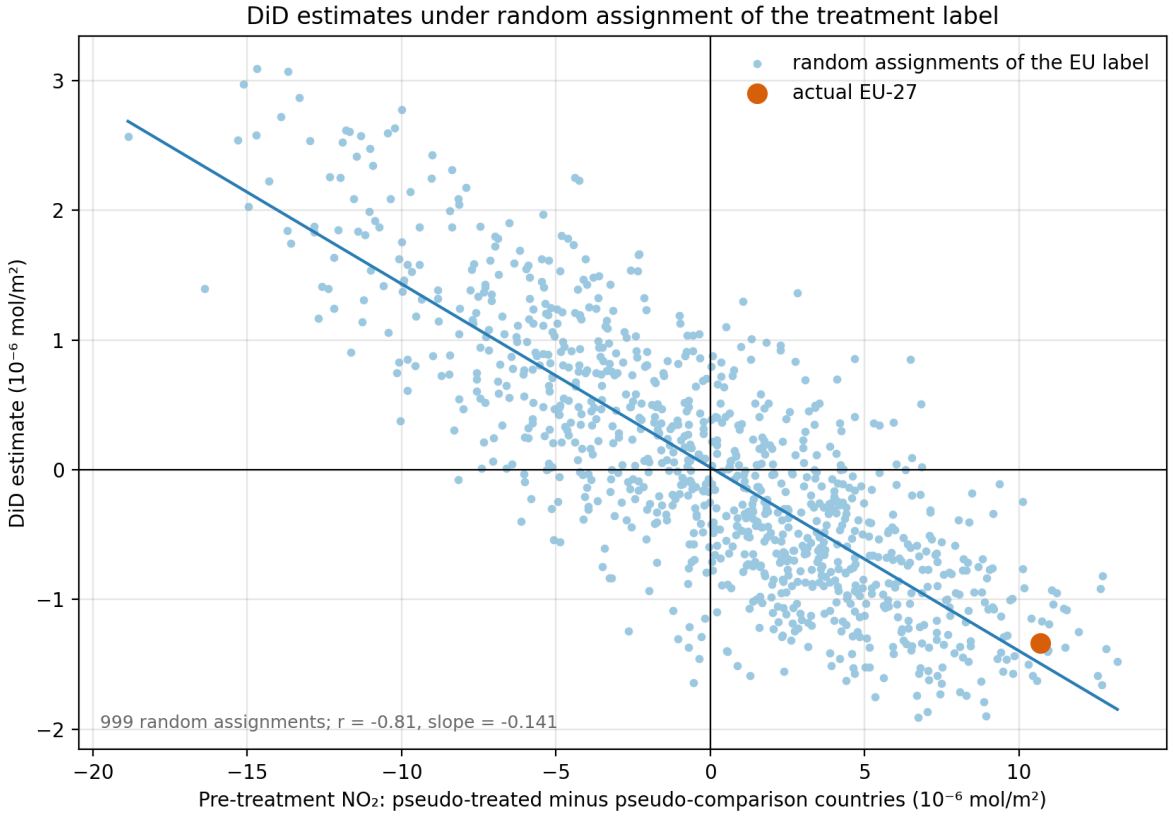
5.3 Baseline Composition at Country Level

The last row of Table 2 adds the interaction between each country's pre-treatment NO_2 level and the post indicator. The interaction is −0.18 per unit of pre-treatment NO_2 (p < 0.001): countries that started with more NO_2 lost more of it. With this term in the model the EU coefficient is +0.58 × 10^{-6} (p = 0.26). The same holds on the log scale: the interaction of the log pre-treatment level with the post indicator is −0.080 (p = 0.005) and the EU coefficient is +0.005 log points (p = 0.72), so more polluted countries lost a larger share of their NO_2, not only more of it in absolute terms.

The estimate also rests on the most polluted members. Without the Netherlands and Belgium it is −0.73 × 10^{-6} (95% CI −1.48 to +0.01; p = 0.054). Restricted to the 29 countries whose pre-treatment level does not exceed the United Kingdom's, which drops Belgium, Czechia, Germany, Italy, Luxembourg, the Netherlands and Poland, it is −0.11 × 10^{-6} (95% CI −0.69 to +0.47; p = 0.71).

The random assignments show that this is a general property of the design and not of this grouping (Figure 3). Across 999 random assignments of the treatment label to 27 of the 36 countries, the DiD estimate is strongly related to how much more polluted the labelled group was before treatment ($r = -0.81$): each additional 10^{-6} mol/m^2 of pre-treatment gap lowers the estimate by 0.14×10^{-6}. The EU-27 is 10.7×10^{-6} more polluted than the comparison group before treatment, for which this relationship predicts an estimate of about -1.5×10^{-6}, against an actual estimate of -1.34×10^{-6}. The random assignments also give a significant estimate at the 5% level in 8.3% of cases, against a nominal 5%. These are not only the errors of a test that is too liberal: random groups that differ in pre-treatment level diverge for the same reason as the EU-27 and its comparison group.

Figure 3. DiD estimates under 999 random assignments of the treatment label, against the pre-treatment NO_2 gap between the labelled and unlabelled countries. The actual EU-27 assignment is highlighted.



5.4 Pixel Level: Comparing Like with Like

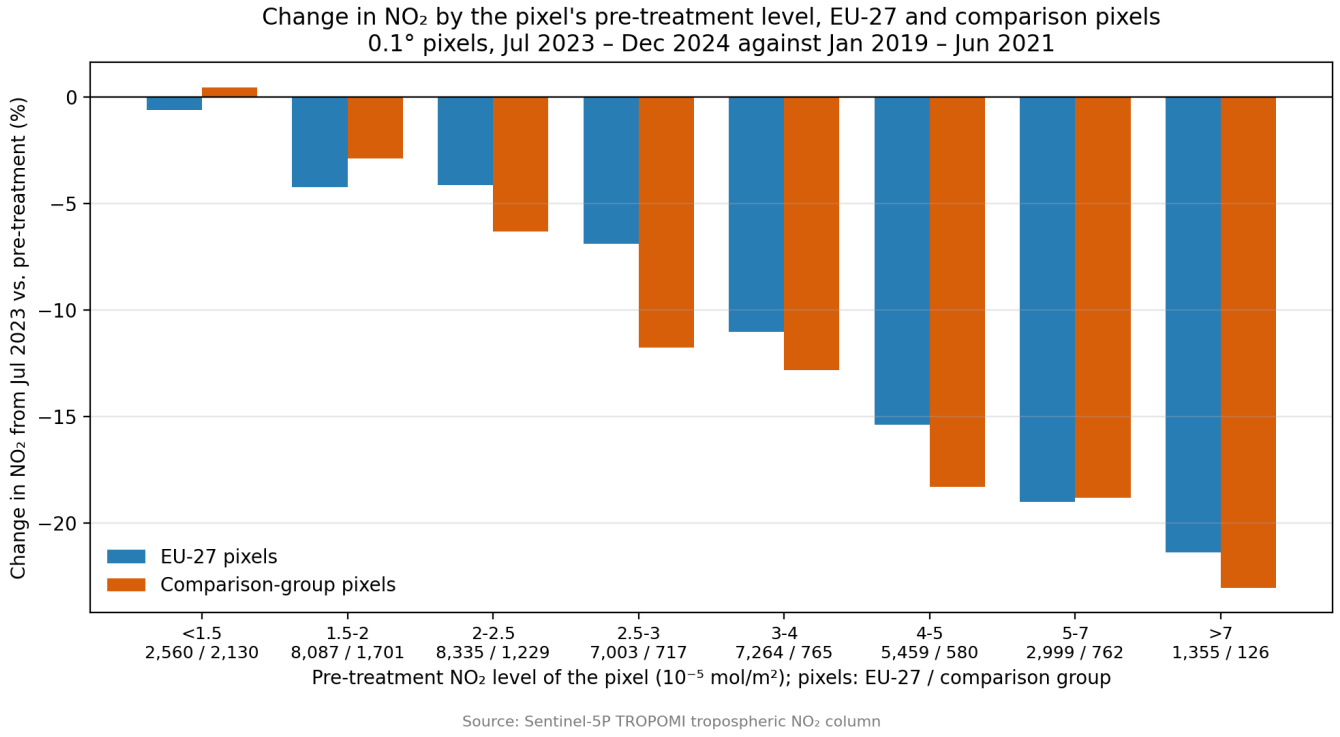
Table 3 and Figure 4 show the change between the pre-treatment period and July 2023–December 2024 by pre-treatment class, for the 51,072 pixels (43,062 in the EU-27 and 8,010 in the comparison group, counting 15 border pixels in both) with a valid mean in the pre-treatment period, in July 2021–June 2023 and in July 2023–December 2024. Most of the pixels that drop out are at high latitudes.

Table 3. Change in NO_2 from the pre-treatment period to July 2023–December 2024, by pre-treatment class.

Pre-treatment level (10^{-5} mol/m^2)	**EU-27 pixels**	**EU-27 change**	**Comparison pixels**	**Comparison change**
< 1.5	2,560	−0.6%	2,130	+0.5%
1.5–2	8,087	−4.2%	1,701	−2.9%

Pre-treatment level (10^{-5} mol/m^2)	EU-27 pixels	EU-27 change	Comparison pixels	Comparison change
2–2.5	8,335	−4.1%	1,229	−6.3%
2.5–3	7,003	−6.9%	717	−11.8%
3–4	7,264	−11.0%	765	−12.8%
4–5	5,459	−15.4%	580	−18.3%
5–7	2,999	−19.0%	762	−18.8%
> 7	1,355	−21.4%	126	−23.0%

Figure 4. Change in NO_2 by pre-treatment class, EU-27 and comparison-group pixels. The numbers under each class are the EU-27 and comparison-group pixel counts.



In both groups the decline is larger in more polluted classes, from almost nothing in the cleanest class to more than 20% in the most polluted, and in five of the eight classes the comparison-group pixels fell at least as much as the EU pixels. The percentage decline is not constant across classes, consistent with a background column that does not respond to surface emissions. In a regression of the change on the pre-treatment level, the change falls by 0.247 mol/m^2 for each mol/m^2 of pre-treatment NO_2 (R^2 = 0.69; 53,298 pixels with a valid mean in both periods). Part of this slope is mechanical, because the change is measured from the level it is regressed on. With the 2019 level as the starting point and the change measured from 2020–June 2021, the slope is −0.152 (R^2 = 0.47). Conditional on the pre-treatment level, EU pixels do not differ significantly from comparison pixels in level (+0.79 × 10^{-6}, p = 0.23) or in slope (p = 0.83), with standard errors clustered by country.

Table 4 gives the cell-level DiD with and without matching on class.

Table 4. Pixel-level DiD on country × class × month cells (10^{-6} mol/m^2; log outcome in log points).

Model	Estimate	95% CI	p
Not matched on class	−1.34		0.017
Matched on class	−0.21	−0.67 to +0.24	0.35
Difference, unmatched minus matched	−1.13	−2.18 to −0.28	
Matched, July 2021–June 2023	−0.00		0.99
Matched, from July 2023	−0.52		0.23
Matched, without November–February	+0.04	−0.42 to +0.51	0.85
Matched, classes from 2019, estimated on 2020–2024	−0.20		0.41
Matched, 4 classes	−0.39	−0.90 to +0.11	0.12
Matched, 16 classes	−0.22	−0.66 to +0.23	0.33
Matched, log outcome	+0.001	−0.02 to +0.02	0.92
Matched, without March 2020–June 2021	−0.23	−0.79 to +0.33	0.42
Matched, real GDP per capita (PPP) in place of GDP	−0.21	−0.73 to +0.32	0.43
Matched, log real GDP per capita (PPP) in place of GDP	+0.02	−0.43 to +0.47	0.94

Without matching, the cell-level model gives -1.34×10^{-6}, close to the country-level estimate. The two are not identical because the cell model uses only pixels with a valid pre-treatment mean and averages pixel means within cells instead of pooling pixel-days. Comparing EU pixels only with comparison-group pixels of the same class reduces the estimate by about 84%, to -0.21×10^{-6}, and the bootstrap interval for the difference between the two excludes zero. The matched estimate is similar across class definitions and when the classes are defined on years outside the estimation sample. Its confidence interval excludes effects larger than about half of the country-level estimate but includes zero and includes small effects of either sign. On the 2020–2024 sample the unmatched estimate is itself smaller (-0.56×10^{-6}), so there matching removes about two thirds. Leaving out the months of COVID-19 restrictions (March 2020–June 2021) makes the unmatched estimate larger (-2.30×10^{-6}) and leaves the matched estimate at -0.23×10^{-6}. With real GDP per capita in place of nominal GDP the matched estimate is -0.21×10^{-6} ($p = 0.43$), and $+0.02 \times 10^{-6}$ with its log.

In the 12-month blocks of Section 4.2, the matched estimate shows no break at the law. The two pre-treatment blocks are -0.57 and -0.12×10^{-6}, and the four post-treatment blocks are -0.16, -0.16, -0.68 and -0.62×10^{-6}. None is significant ($p \geq 0.22$). The same blocks without matching repeat the country-level pattern, with -1.96×10^{-6} ($p = 0.014$) in July 2023–June 2024.

The matched estimate averages over a comparison group that is itself heterogeneous. Against the United Kingdom, Norway, Switzerland and Iceland it is $+0.20 \times 10^{-6}$ ($p = 0.49$). Against the Western Balkans it is -0.38×10^{-6} ($p = 0.15$), and -1.08×10^{-6} ($p = 0.014$) from July 2023. The two halves of the comparison group differ from each other by -1.00×10^{-6} ($p = 0.014$). The next section looks at this heterogeneity directly.

5.5 Where NO_2 Fell

Figure 5 maps the change per pixel, in absolute terms and as a share of the pre-treatment level, and Figure 6 shows the change in pixels that started above 3×10^{-5} mol/m², country by country.

Figure 5. Change in tropospheric NO_2 per 0.1° pixel, July 2023–December 2024 minus January 2019–June 2021: (a) in 10^{-6} mol/m^2, with the colour scale clipped at ±15; (b) as a percentage of the pre-treatment level, for pixels that started at or above 1 × 10^{-5} mol/m^2, clipped at ±40%. Thick outlines mark the comparison countries. Pixels without a valid mean in both periods, mostly at high latitudes, are blank; Iceland and Cyprus lie outside the frame. Because the change grows with the starting level, the absolute map shows polluted areas most strongly, while the percent map shows the same contrast between north-western and south-eastern Europe, less sharply.

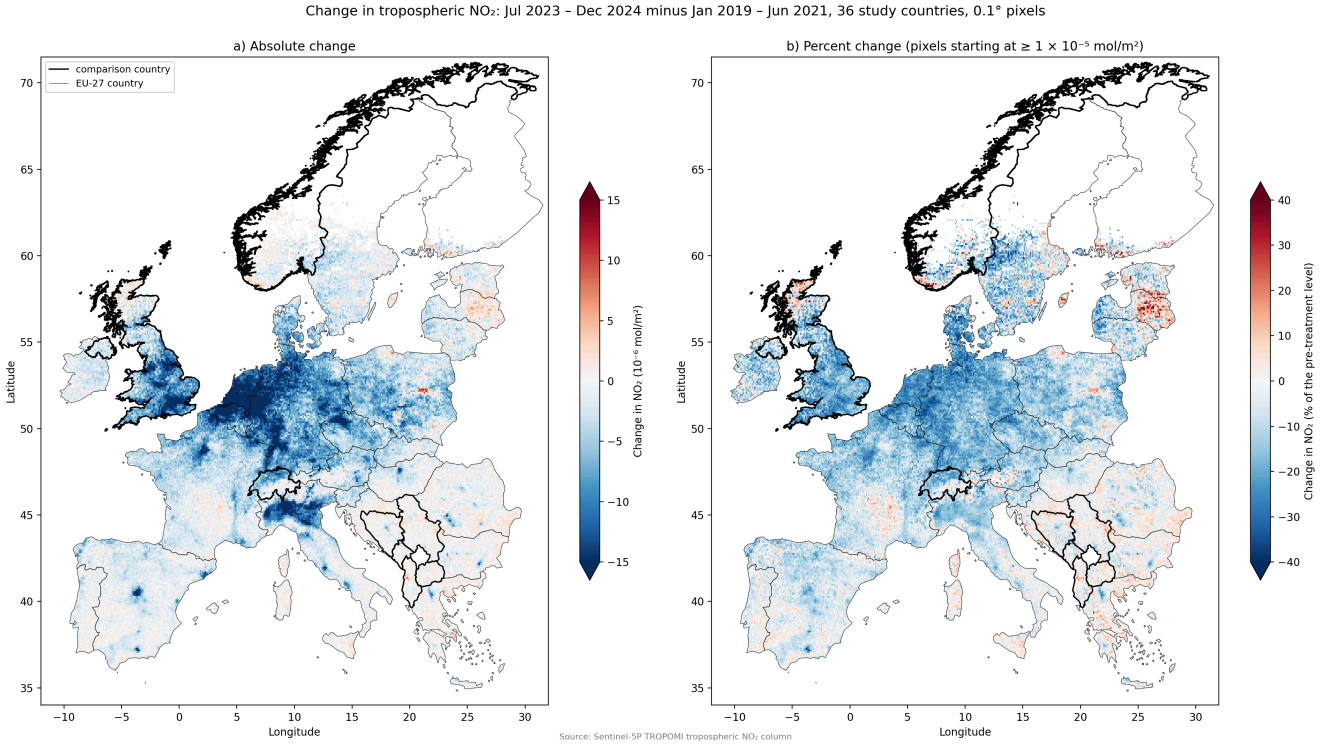
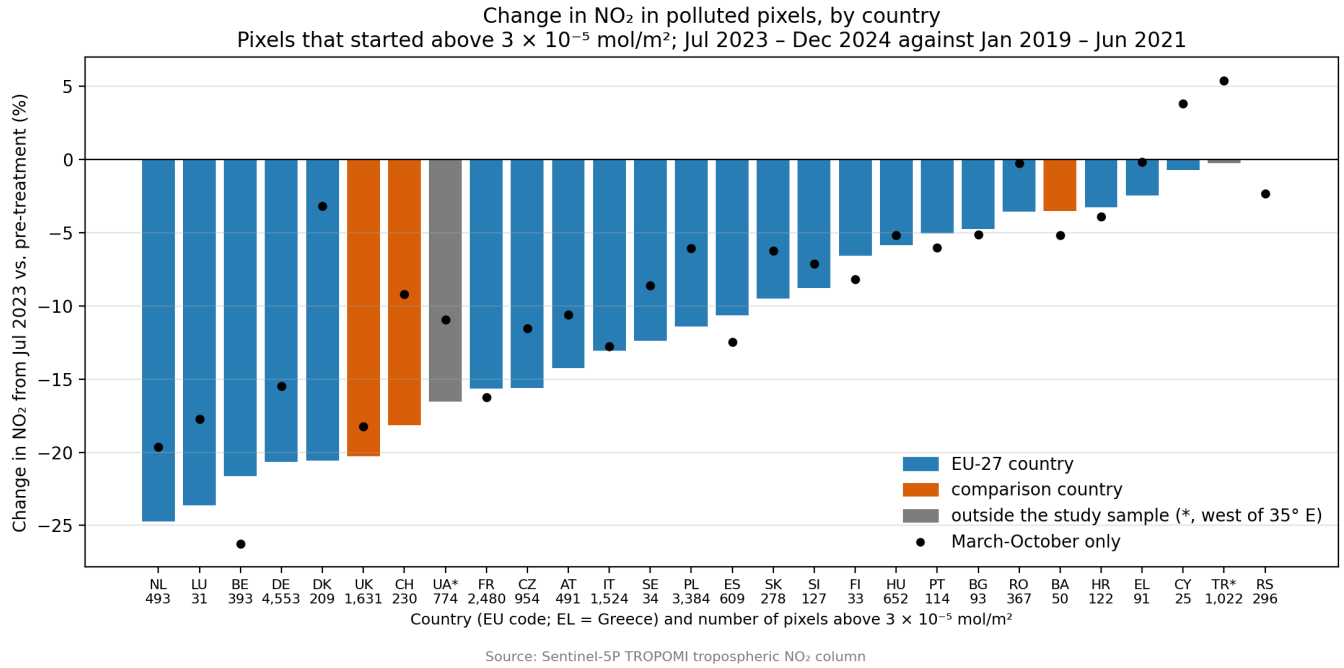


Figure 6. Change in NO_2 in pixels that started above 3 × 10^{-5} mol/m^2, by country (countries with at least 20 such pixels; the number of pixels is shown under each code). Bars use all months; dots use March–October only. Grey bars are areas outside the study sample, west of 35° E.



Polluted pixels lost 25% of their NO_2 in the Netherlands, 24% in Luxembourg, 22% in Belgium, 21% in Germany and Denmark, 20% in the United Kingdom and 18% in Switzerland; 16% in France and Czechia, 14% in Austria, 13% in Italy, 12% in Sweden, 11% in Poland and Spain, 10% in Slovakia, 9% in Slovenia and 7% in Finland; and between 0 and 6% in Hungary, Portugal, Bulgaria, Romania, Bosnia and Herzegovina, Croatia, Greece, Cyprus and Serbia. Luxembourg, Sweden, Finland and Cyprus have fewer than 35 such pixels each. The two non-EU countries with large polluted areas in north-western Europe are among their EU neighbours at the top of the ranking, and the EU members of south-eastern Europe are among their non-EU neighbours at the bottom.

The areas outside the study sample add two observations. In western Turkey, which has 1,022 pixels above 3×10^{-5} mol/m² (218 above 5×10^{-5}, including Istanbul and Ankara), polluted pixels did not change (−0.3%; +5.4% in March–October). This shows that the decline is not a mechanical property of polluted pixels. Where the pre-treatment level was high but the location was outside north-western and central Europe, NO_2 did not fall. Western Turkey's area comes from the generalized NUTS boundary cut at 35° E, and it has no covariates. In Ukraine west of 35° E, polluted pixels lost 17%, which cannot be separated from the effects of the war that began in February 2022.

Table 5. Regional contrasts, matched on pre-treatment class (10^{-6} mol/m²).

Contrast	Estimate	95% CI	p
North-west: EU members vs United Kingdom, Switzerland	−0.41	−1.55 to +0.73	0.44
South-east: EU members vs Western Balkans	+0.22	−0.45 to +0.88	0.48
Inside the EU: north-west vs south-east members	−1.43	−2.56 to −0.31	0.016
Inside the EU: north-west vs south-east, without November–February	−0.29	−1.19 to +0.61	0.50
EU-27 vs western Turkey (no controls)	−1.43	not reported	not reported
North-west EU members vs western Turkey (no controls)	−2.16	not reported	not reported
South-east EU members vs western Turkey (no controls)	−0.30	not reported	not reported

The three contrasts with western Turkey have a single comparison country, so cluster-robust standard errors are not valid for them and only the point estimates are given. They are of the same order as the estimates before matching: at the same pre-treatment level, NO_2 fell by 1.43×10^{-6} mol/m² more in the EU-27 than in western Turkey, by 2.16 more in north-western members and by 0.30 more in south-eastern members. Matching on the starting level therefore removes the EU difference when the comparison pixels are in the United Kingdom, Switzerland and the Western Balkans, and does not remove it when they are in Turkey. What the matched model returns depends on where the comparison pixels are.

Table 5 holds region fixed. Within the north-west and within the south-east, the estimated difference across the EU border is small and not significant, but these contrasts rest on few comparison countries (two in the north-west) and their confidence intervals are wide: the north-west interval includes the whole country-level estimate. They show that the data do not require an effect at the EU border; they do not show that there is none. The contrast between north-western and south-eastern EU members is as large as the country-level estimate with all months and is small and not significant without November–February.

The regional pattern depends on winter as much as the country-level estimate does. In percentage terms, a difference between the two ends of the ranking remains in March–October: −20% in the Netherlands, −18% in the United Kingdom and −15% in Germany, against −0.3% in Romania, −0.2% in Greece and −2% in Serbia. But the ranking in between changes: Denmark falls from −21% to −3% and Switzerland from −18% to −9%.

5.6 Positive Control

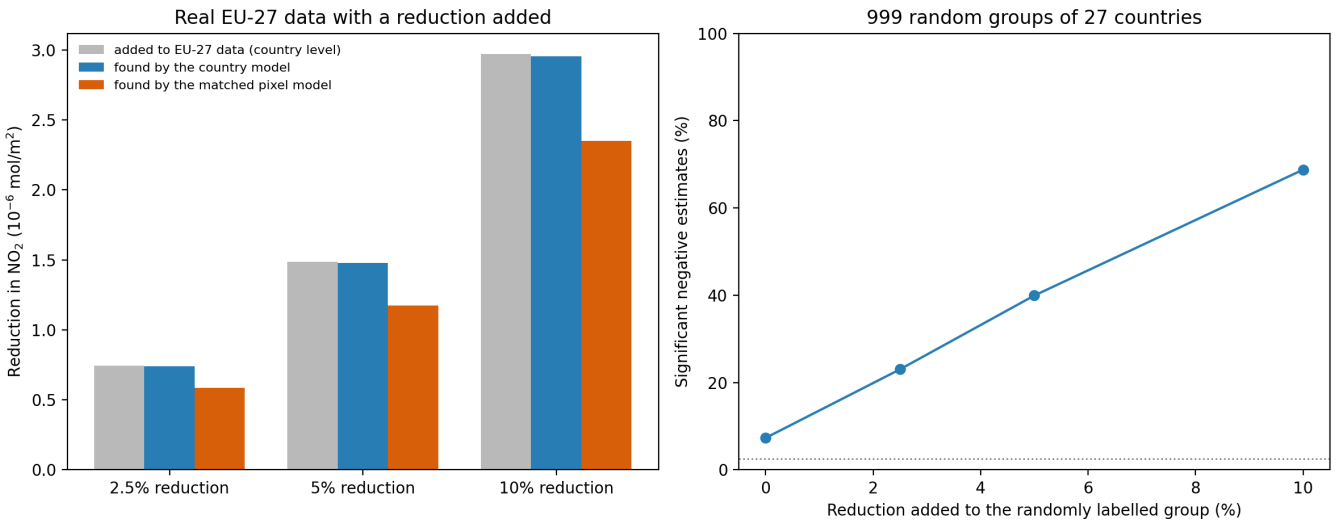
Table 6. Estimates after adding a known reduction to EU-27 NO_2 from July 2021 (10^{-6} mol/m²). "Added" is the average reduction added, in country-months for the country model and in weighted pixel cells for the matched model.

Reduction added	Added	Country model	p	Added	Matched pixel model	p
None (observed data)	—	−1.34	0.022	—	−0.21	0.35
2.5%	−0.74	−2.08	0.002	−0.75	−0.80	0.002
5%	−1.48	−2.81	<0.001	−1.50	−1.39	<0.001
10%	−2.97	−4.29	<0.001	−2.99	−2.56	<0.001

Both models find the added reductions (Table 6). The country-level model recovers them in full: its estimate moves by −0.74, −1.48 and −2.95 × 10^{-6} for added reductions of −0.74, −1.48 and −2.97 × 10^{-6}. The matched model recovers about four-fifths of them (−0.59, −1.18 and −2.35 for −0.75, −1.50 and −2.99). An added constant is recovered in full, so the shortfall comes from the proportional form of the reduction: with class-specific month effects, the matched estimate gives the less polluted classes more weight than their share of EU pixels, and a proportional reduction is smaller there. A 2.5% reduction moves the matched estimate from −0.21 to −0.80 × 10^{-6} ($p = 0.002$), and the smallest effect the matched model detects with 80% power is 0.65 × 10^{-6} mol/m². Because the matched model recovers about four-fifths of a proportional reduction, this corresponds to a uniform reduction of 2.7% in EU pixels. The matched estimate close to zero is therefore not a result of low power: a uniform reduction of about 3% or more would very probably have been detected. A smaller reduction, or one confined to part of the EU, could have been missed.

With random groups of 27 countries, the country-level model detects added reductions less reliably (Figure 7). Without an added reduction, 7.3% of the random groups have a significant negative estimate (8.3% in either direction). A 2.5% reduction is detected in 23% of them, a 5% reduction in 40% and a 10% reduction in 69%. On average the estimate equals the added reduction (−1.35 against −1.37 × 10^{-6} for 5%), so the model is not biased across random groups but is imprecise, because each random group differs from the rest in pre-treatment level by a different amount (Section 5.3). For the EU-27, the added reductions are detected easily because they add to an estimate that is already −1.34 × 10^{-6}, which is about the size the composition of the two groups predicts.

Figure 7. Positive control. Left: reduction added to EU-27 NO_2 from July 2021 and the change it causes in the country-level and matched pixel estimates. Right: share of 999 random groups of 27 countries with a significant negative estimate, by size of the reduction added to the group; the dotted line is the 2.5% expected by chance in one direction.



5.7 Ground Stations

The comparison uses 1,543 background sampling points in 33 countries. It covers six of the nine comparison countries: the United Kingdom has EEA data only for 2019, and Albania and Montenegro have no station with enough months. It therefore lacks the comparison country with most of the polluted comparison-group pixels.

Satellite and station values agree. Across 104,905 station-months the correlation is 0.68. Within a station over time the median correlation is 0.70, and between stations, in their pre-treatment means, it is 0.68. Agreement is weaker for the change from the pre-treatment period to July 2023–December 2024 (r = 0.34 over 1,496 stations), and lower at traffic stations (monthly r = 0.59).

Table 7. The main country-level model on country-month means of background-station NO_2 and of the satellite pixels at the same stations (33 countries; log outcomes in log points).

Specification	Stations (μg/m³)	% of EU-27 pre-treatment mean	p	Satellite at the stations (10^{-6} mol/m²)	% of EU-27 pre-treatment mean	p
Main model	−0.48	−4.0%	0.12	−1.91	−4.5%	0.026
Without November–February	−0.72	−7.0%	0.076	−0.86	−2.3%	0.30
Log outcome	−0.040		0.14	−0.042		0.033
+ pre-treatment level × post	−0.58		0.090	−0.45		0.55

In relative terms the stations give an EU-27 decline of the same size as the satellite at the same places (−4.0% against −4.5%), but it is not significant (Table 7). The median station declined by 19% from July 2023 in the EU-27 and by 18% in the comparison countries, against 13% and 9% for the satellite pixels at the same

stations. The stations do not depend on winter: without November–February their estimate is larger (−7.0%, p = 0.076), while the satellite estimate at the same stations falls to −2.3% (p = 0.30). In December 2023, station NO_2 in Denmark and Sweden was 16% and 7% below the average of December 2022 and December 2024, while the satellite values at the same stations were 150% and 111% below it, that is, negative. In these two countries the December 2023 anomaly is therefore a retrieval artefact, not a change in air quality.

6. Discussion

The country-level analysis produced a negative and significant DiD estimate under the baseline model, and it remained so under year-month fixed effects, group-specific seasonality, meteorological and pandemic controls, a wild cluster bootstrap and leave-one-out tests. The same estimate was not significant without the winter months, with area weights, with log GDP or real GDP per capita, or under randomization inference on the coefficient. A reader shown only the first set would call the result robust, and a reader shown only the second would call it fragile. The first conclusion is that both sets have to be reported.

The second is that most of the estimate reflects baseline composition. The country-level interaction with the pre-treatment level, the link between placebo estimates and the pre-treatment gap under random assignment, and the pixel-level comparison within pollution classes all agree on this. In the last of these the estimate falls from -1.34 to -0.21×10^{-6}, and the difference is itself significant. The estimate also roughly halves without the Netherlands and Belgium, and it is close to zero when the sample is restricted to countries no more polluted than the United Kingdom. The mechanism is the one the DiD literature describes for groups that differ in levels (Kahn-Lang & Lang, 2020; Roth & Sant'Anna, 2023). When the change in the outcome depends on its starting level, trends cannot be parallel in levels between a heavily polluted group and a much cleaner one. Fixed effects, weather controls and resampling over clusters do not detect this, because none of them changes the composition of the two groups. The interaction of the pre-treatment level with the post indicator, the log specification, the random-assignment plot and a common-support restriction do detect it, and all four are cheap to run at country level. In this sample, however, baseline level and location are closely related. Most polluted comparison-group pixels are in the United Kingdom, and the polluted pixels of western Turkey did not decline. Here, baseline composition also means a contrast between north-western and south-eastern Europe.

What remains after matching is uncertain. The matched estimate is close to zero, but its confidence interval admits a relative decline of up to half the country-level estimate, and against the Western Balkans alone the matched estimate from July 2023 is negative and significant. The positive control narrows this: the matched model would very probably have detected a uniform reduction of about 3% in EU pixels (Section 5.6), so a relative decline of that size across the EU is unlikely, while a smaller one, or one confined to some regions, is not ruled out. This paper does not show that EU membership had no effect on NO_2. It shows that the country-level estimate is not evidence for one, and that the data, analysed like with like, do not require one.

The geography of the decline suggests where an explanation should be sought. NO_2 fell most in the polluted areas of north-western and central Europe, on both sides of the EU border, and little in the polluted areas of south-eastern Europe and western Turkey. Several accounts are compatible with this pattern and the present data do not discriminate between them. One is regulation of EU origin that reaches beyond the EU: vehicle emission standards (Regulation (EC) No 715/2007 for light vehicles and Regulation (EC) No 595/2009 for heavy vehicles; European Commission, n.d.) are shared by the United Kingdom and Switzerland, and if newer vehicles enter the fleet faster in richer countries, the same standards would produce a faster decline there than in south-eastern member states. A second is the pace of coal-plant closures and industrial change. A third is that part of the pattern is not real. Winter months account for a large share of the country-level estimate and

of the regional contrasts. Winter is also when valid retrievals are fewest and when the physically impossible negative country means occur. And the decline coincides with the operational processor versions 2.5 to 2.7, with the anomalous month of December 2023 falling between the last sampled day on version 2.5.0 (October 2023) and the first on version 2.6.0 (January 2024). A change in the retrieval that affected polluted, high-latitude winter scenes more than others would produce a similar picture. The ground stations support this account for December 2023, which they do not record as a drop, and for the dependence on winter, which the station estimate does not share (Section 5.7). They do not, however, show an EU-specific decline that is significant on its own.

Some practical points follow for satellite-based policy evaluation. Units should be compared at the same starting level, by matching pixels or regions on pre-treatment pollution or by modelling the dependence on the starting level explicitly. Results should be reported with and without the winter months, together with the share of valid retrievals. And choices such as country weighting and the functional form of economic controls should be reported, because here each of them alone moves the estimate across the 5% threshold.

For the Climate Law itself, the conclusion is limited. The design cannot detect an effect of a framework law with no dated mechanism, and the relative decline it does detect is not tied to the law's date. A policy with a direct mechanism, a known start date and variation in where it applies would be a better target for the same data.

7. Limitations

The matched pixel comparison has thin common support at high pollution levels. Of the comparison-group pixels above 4×10^{-5} mol/m², 86% are in the United Kingdom, with the rest mostly in Switzerland and Serbia. Western Turkey adds polluted pixels outside the country panel, but without covariates and from one country. The comparison therefore separates EU membership from pollution level and, descriptively, from region. It does not separate regulation of EU origin from its absence.

The period from July 2023, the threshold for polluted pixels and the regional grouping were chosen after seeing the data, so the descriptive results of Section 5.5 and the p-values in Table 5 are not confirmatory. No formal sensitivity analysis for violations of parallel trends (Rambachan & Roth, 2023) is reported.

Sixteen of the thirty pre-treatment months, including the whole reference block of Section 4.2, fall in the period of COVID-19 restrictions, when NO_2 fell most in polluted areas. Leaving them out makes the country-level estimate larger (-2.25×10^{-6}, Table 2) and leaves the matched estimate unchanged (-0.23×10^{-6}, Table 4), but the pre-treatment period remains short and unusual.

The positive control adds a uniform proportional reduction to the observed data. A real policy effect could differ by region, season or pollution level, and the observed data may already contain one. The control shows what the models can detect, not what the true effect is.

The outcome is a tropospheric column, not an emission rate or a surface concentration. It is observed only under mostly clear skies, the 0.1° grid resamples ground pixels of about 5 km, and the comparison with ground stations (Section 5.7) lacks the United Kingdom, Albania and Montenegro and sets surface concentrations against a tropospheric column. Meteorological controls are available only as country means, so the pixel-level models do not adjust for local weather, and a difference in the weather of the 2023–2024 winters between regions is not ruled out. The processor version was checked on 24 sample days, not for every file, and no day was screened out for low coverage. Boundaries come from the generalized 1:20 million NUTS file for 33 countries and from the GADM 4.1 GeoJSON files for the United Kingdom, Norway and Switzerland,

which are much more detailed, so pixels along some borders and coasts are assigned approximately. Spain and Portugal include their Atlantic islands. Class-specific time effects absorb version changes that depend on pollution level in the matched models, but not in the country-level models.

Inference rests on 36 clusters, nine of them in the comparison group, the residuals are spatially correlated, and random assignments of the treatment label are significant at the 5% level in 8.3% of cases. GDP in the main model is nominal, annual and partly post-treatment, and the comparison countries' GDP is converted at approximate exchange rates. Replacing it with real GDP per capita from a single source leaves the estimate about the same size but no longer significant ($p = 0.072$), so the measure and functional form of this one control move the country-level estimate across the 5% threshold (Table 2). The matched estimate is unaffected (Table 4).

8. Conclusion

A country-level difference-in-differences on satellite NO_2 finds that pollution fell about 4% more in the EU-27 than in nine neighbouring countries after the European Climate Law. The estimate is stable under many standard specifications and unstable under others, and most of it reflects the fact that the EU-27 was more polluted to begin with: placebo estimates under random assignment follow the pre-treatment gap between groups, and, within the study sample, comparing pixels with the same starting level leaves an estimate that is small and not distinguishable from zero. NO_2 fell most in the polluted areas of north-western and central Europe, inside and outside the EU, and little in the polluted areas of south-eastern Europe and western Turkey. The size of that contrast depends on winter retrievals of uncertain quality, and its cause is not identified.

The data used here are public, and the pixel-level comparison requires nothing beyond what the country-level analysis already downloads. Evaluations of policy with satellite air-quality data should compare like with like, and report what happens without the winter months, before presenting a country-level difference as a policy effect.

Data and Code Availability

All data are from public sources (Table 1). The code that downloads the daily rasters, builds the country and pixel panels and produces every estimate, table and figure in this paper is available at github.com/sakshimaske303-commits/GPIE. The daily rasters (about 1 GB) and the monthly 0.1° grid built from them are not stored in the repository and can be regenerated with the download script. The paper contains modified Copernicus Sentinel data (2019–2024), processed by Sentinel Hub, and modified Copernicus Climate Change Service information (2019–2024). GADM boundaries are subject to the GADM licence and are downloaded by the code rather than stored in the repository. EEA air-quality station data are downloaded with the airbase package, and the station-month means used in Section 5.7 are stored in the repository.

References

Abadie, A. (2005). Semiparametric Difference-in-Differences Estimators. *The Review of Economic Studies*, 72(1), 1–19. https://doi.org/10.1111/0034-6527.00321

Barré, J., Petetin, H., Colette, A., Guevara, M., Peuch, V.-H., Rouil, L., et al. (2021). Estimating lockdown-induced European NO2 changes using satellite and surface observations and air quality models. *Atmospheric Chemistry and Physics*, 21(9), 7373–7394. https://doi.org/10.5194/acp-21-7373-2021

Bertrand, M., Duflo, E., & Mullainathan, S. (2004). How Much Should We Trust Differences-in-Differences Estimates? *The Quarterly Journal of Economics*, 119(1), 249–275. https://doi.org/10.1162/003355304772839588

Cameron, A. C., Gelbach, J. B., & Miller, D. L. (2008). Bootstrap-Based Improvements for Inference with Clustered Errors. *The Review of Economics and Statistics*, 90(3), 414–427. https://doi.org/10.1162/rest.90.3.414

Cameron, A. C., & Miller, D. L. (2015). A Practitioner's Guide to Cluster-Robust Inference. *Journal of Human Resources*, 50(2), 317–372. https://doi.org/10.3368/jhr.50.2.317

Chen, S., Lin, F., Yao, X., & Zhang, P. (2020). WTO accession, trade expansion, and air pollution: Evidence from China's county-level panel data. *Review of International Economics*, 28(4), 1020–1045. https://doi.org/10.1111/roie.12480

Conley, T. G., & Taber, C. R. (2011). Inference with "Difference in Differences" with a Small Number of Policy Changes. *The Review of Economics and Statistics*, 93(1), 113–125. https://doi.org/10.1162/REST_a_00049

Dang, H.-A. H., & Trinh, T.-A. (2021). Does the COVID-19 lockdown improve global air quality? New cross-national evidence on its unintended consequences. *Journal of Environmental Economics and Management*, 105, 102401. https://doi.org/10.1016/j.jeem.2020.102401

Daw, J. R., & Hatfield, L. A. (2018). Matching and Regression to the Mean in Difference-in-Differences Analysis. *Health Services Research*, 53(6), 4138–4156. https://doi.org/10.1111/1475-6773.12993

Directive 2010/75/EU of the European Parliament and of the Council of 24 November 2010 on industrial emissions (integrated pollution prevention and control) (recast). https://eur-lex.europa.eu/eli/dir/2010/75/oj/eng

Directive (EU) 2016/2284 of the European Parliament and of the Council of 14 December 2016 on the reduction of national emissions of certain atmospheric pollutants, amending Directive 2003/35/EC and repealing Directive 2001/81/EC. *Official Journal of the European Union*, L 344, 17.12.2016, 1–31. https://eur-lex.europa.eu/eli/dir/2016/2284/oj/eng

Eskes, H. J., & Eichmann, K.-U. (2023). *S5P Mission Performance Centre Nitrogen Dioxide [L2__NO2___] Readme* (S5P-MPC-KNMI-PRF-NO2, issue 2.4). KNMI / S5P Mission Performance Centre for the European Space Agency. https://sentiwiki.copernicus.eu/web/s5p-documents

European Commission. (2016). *The EU Emissions Trading System (EU ETS)* [Factsheet]. https://climate.ec.europa.eu/system/files/2016-12/factsheet_ets_en.pdf

European Commission. (n.d.). *Emissions in the automotive sector*. Directorate-General for Internal Market, Industry, Entrepreneurship and SMEs. https://single-market-economy.ec.europa.eu/sectors/automotive-industry/environmental-protection/emissions-automotive-sector_en

European Environment Agency. (2025). Nitrogen dioxide – NO_2. In *Air quality status report 2025*. https://www.eea.europa.eu/en/analysis/publications/air-quality-status-report-2025/nitrogen-dioxide

Fotheringham, A. S., & Wong, D. W. S. (1991). The Modifiable Areal Unit Problem in Multivariate Statistical Analysis. *Environment and Planning A*, 23(7), 1025–1044. https://doi.org/10.1068/a231025

Gendron-Carrier, N., Gonzalez-Navarro, M., Polloni, S., & Turner, M. A. (2022). Subways and Urban Air Pollution. *American Economic Journal: Applied Economics*, 14(1), 164–196.

https://doi.org/10.1257/app.20180168

Georgoulias, A. K., van der A, R. J., Stammes, P., Boersma, K. F., & Eskes, H. J. (2019). Trends and trend reversal detection in 2 decades of tropospheric NO2 satellite observations. *Atmospheric Chemistry and Physics*, 19(9), 6269–6294. https://doi.org/10.5194/acp-19-6269-2019

Grange, S. K., Carslaw, D. C., Lewis, A. C., Boleti, E., & Hueglin, C. (2018). Random forest meteorological normalisation models for Swiss PM10 trend analysis. *Atmospheric Chemistry and Physics*, 18(9), 6223–6239. https://doi.org/10.5194/acp-18-6223-2018

Hale, T., Angrist, N., Goldszmidt, R., Kira, B., Petherick, A., Phillips, T., et al. (2021). A global panel database of pandemic policies (Oxford COVID-19 Government Response Tracker). *Nature Human Behaviour*, 5(4), 529–538. https://doi.org/10.1038/s41562-021-01079-8

Hersbach, H., Bell, B., Berrisford, P., Hirahara, S., Horányi, A., Muñoz-Sabater, J., et al. (2020). The ERA5 global reanalysis. *Quarterly Journal of the Royal Meteorological Society*, 146(730), 1999–2049. https://doi.org/10.1002/qj.3803

Kahn-Lang, A., & Lang, K. (2020). The Promise and Pitfalls of Differences-in-Differences: Reflections on '16 and Pregnant' and Other Applications. *Journal of Business & Economic Statistics*, 38(3), 613–620. https://doi.org/10.1080/07350015.2018.1546591

Keola, S., & Hayakawa, K. (2021). Do Lockdown Policies Reduce Economic and Social Activities? Evidence from NO2 Emissions. *The Developing Economies*, 59(2), 178–205. https://doi.org/10.1111/deve.12274

Laughner, J. L., & Cohen, R. C. (2019). Direct observation of changing NOx lifetime in North American cities. *Science*, 366(6466), 723–727. https://doi.org/10.1126/science.aax6832

MacKinnon, J. G., & Webb, M. D. (2020). Randomization inference for difference-in-differences with few treated clusters. *Journal of Econometrics*, 218(2), 435–450. https://doi.org/10.1016/j.jeconom.2020.04.024

Moran, P. A. P. (1950). Notes on Continuous Stochastic Phenomena. *Biometrika*, 37(1/2), 17–23. https://doi.org/10.1093/biomet/37.1-2.17

Rambachan, A., & Roth, J. (2023). A More Credible Approach to Parallel Trends. *The Review of Economic Studies*, 90(5), 2555–2591. https://doi.org/10.1093/restud/rdad018

Regional Cooperation Council. (2020). *Sofia Declaration on the Green Agenda for the Western Balkans*. https://www.rcc.int/docs/546/sofia-declaration-on-the-green-agenda-for-the-western-balkans

Regulation (EU) 2021/1119 of the European Parliament and of the Council of 30 June 2021 establishing the framework for achieving climate neutrality and amending Regulations (EC) No 401/2009 and (EU) 2018/1999 ('European Climate Law'). *Official Journal of the European Union*, L 243, 9.7.2021, 1–17. https://eur-lex.europa.eu/eli/reg/2021/1119/oj/eng

Robinson, W. S. (1950). Ecological Correlations and the Behavior of Individuals. *American Sociological Review*, 15(3), 351–357. https://www.jstor.org/stable/2087176

Roth, J. (2022). Pretest with Caution: Event-Study Estimates after Testing for Parallel Trends. *American Economic Review: Insights*, 4(3), 305–322. https://doi.org/10.1257/aeri.20210236

Roth, J., & Sant'Anna, P. H. C. (2023). When Is Parallel Trends Sensitive to Functional Form? *Econometrica*, 91(2), 737–747. https://doi.org/10.3982/ECTA19402

Roth, J., Sant'Anna, P. H. C., Bilinski, A., & Poe, J. (2023). What's Trending in Difference-in-Differences? A Synthesis of the Recent Econometrics Literature. *Journal of Econometrics*, 235(2), 2218–2244. https://doi.org/10.1016/j.jeconom.2023.03.008

Sager, L., & Singer, G. (2025). Clean Identification? The Effects of the Clean Air Act on Air Pollution, Exposure Disparities, and House Prices. *American Economic Journal: Economic Policy*, 17(1), 1–36. https://doi.org/10.1257/pol.20220745

Sant'Anna, P. H. C., & Zhao, J. (2020). Doubly robust difference-in-differences estimators. *Journal of Econometrics*, 219(1), 101–122. https://doi.org/10.1016/j.jeconom.2020.06.003

Silvern, R. F., Jacob, D. J., Mickley, L. J., Sulprizio, M. P., Travis, K. R., Marais, E. A., et al. (2019). Using satellite observations of tropospheric NO2 columns to infer long-term trends in US NOx emissions: the importance of accounting for the free tropospheric NO2 background. *Atmospheric Chemistry and Physics*, 19(13), 8863–8878. https://doi.org/10.5194/acp-19-8863-2019

Swiss Federal Council. (2019, December 9). *Linking of emissions trading schemes: Agreement ratified by Switzerland and EU* [Media release]. https://www.admin.ch/gov/en/start/documentation/media-releases.msg-id-77446.html

Tong, C., Dai, Y., Cole, M., Elliott, R. J. R., Bartington, S. E., Liu, B., & Shi, Z. (2025). Further improvement in London's air quality demands more than the Ultra Low Emission Zone policy. *npj Clean Air*, 1, 29. https://doi.org/10.1038/s44407-025-00030-9

van Geffen, J., Eskes, H., Compernolle, S., Pinardi, G., Verhoelst, T., Lambert, J.-C., et al. (2022). Sentinel-5P TROPOMI NO_2 retrieval: impact of version v2.2 improvements and comparisons with OMI and ground-based data. *Atmospheric Measurement Techniques*, 15(7), 2037–2060. https://doi.org/10.5194/amt-15-2037-2022

Veefkind, J. P., Aben, I., McMullan, K., Förster, H., de Vries, J., Otter, G., et al. (2012). TROPOMI on the ESA Sentinel-5 Precursor: A GMES mission for global observations of the atmospheric composition for climate, air quality and ozone layer applications. *Remote Sensing of Environment*, 120, 70–83. https://doi.org/10.1016/j.rse.2011.09.027

Verhoelst, T., Compernolle, S., Pinardi, G., Lambert, J.-C., Eskes, H. J., Eichmann, K.-U., et al. (2021). Ground-based validation of the Copernicus Sentinel-5P TROPOMI NO2 measurements with the NDACC ZSL-DOAS, MAX-DOAS and Pandonia global networks. *Atmospheric Measurement Techniques*, 14(1), 481–510. https://doi.org/10.5194/amt-14-481-2021

Vîrghileanu, M., Săvulescu, I., Mihai, B.-A., Nistor, C., & Dobre, R. (2020). Nitrogen Dioxide (NO2) Pollution Monitoring with Sentinel-5P Satellite Imagery over Europe during the Coronavirus Pandemic Outbreak. *Remote Sensing*, 12(21), 3575. https://doi.org/10.3390/rs12213575