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The Green Shock: Carbon Pricing, Local Decline, and the Rise of the Populist Right in Europe[Ψ]

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Abstract

Does carbon pricing fuel the populist radical right? This paper exploits the Phase 3 reduction in free emission allowances beginning in 2013 in the EU Emissions Trading System as a quasi-natural experiment in a difference-in-differences model for 889 NUTS-3 regions in 23 European countries. Regions more exposed to the shock saw higher populist radical-right (PRR) shares, concentrated in European Parliament (EP) elections, pointing to expressive voting that channels EU-policy grievances toward the EU ballot box. The shock reaches voters through broader regional stagnation rather than deindustrialisation, as services employment falls, GDP per capita declines, and population shrinks while manufacturing employment remains unchanged. A sectoral decomposition links the EP concentration to regions with greater power-sector exposure, consistent with allowance-cost pass-through to household electricity bills. Replacing PRR with populist radical-left or Green vote shares flips the coefficient to negative, suggesting a right-wing rather than generic protest response. Instrumental variable estimates based on the manufacturing free-allocation component support the finding, which survives controls for Eurozone-crisis exposure and migration patterns. The paper contributes to the political economy of climate policy by showing that carbon pricing can generate geographically concentrated right-wing backlash and EU-directed protest voting.

Keywords: EU Emissions Trading System, carbon pricing, populist radical right, climate policy backlash, regional decline, expressive voting

JEL codes: D72, H23, Q52, Q58, R11

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1 Introduction

The costs of carbon pricing fall unevenly across regions. In the case of the EU Emissions Trading System, this unevenness became especially pronounced when Phase 3, which took effect in 2013, redesigned the system asymmetrically. Electricity generation moved to full auctioning, so power producers had to buy every allowance they used, while manufacturing kept only a declining share of its allowances for free.[1] Compliance costs therefore arrived on regional economies along two paths: directly, when local manufacturers paid for emissions above their free allocation, and indirectly, when local power plants priced the cost of allowances into wholesale electricity, raising the bills paid by households and downstream firms in the same region. This paper asks whether that regionally concentrated cost shock shifted electoral support toward populist radical-right parties (PRR), and whether voters attributed the burden to the level of government that imposed it.

The question connects climate policy to a well-established empirical pattern. Trade liberalisation, austerity, and financial crises all generate spatially concentrated losses, and a large body of evidence shows that such losses increase support for populist radical-right parties in the affected regions (Autor et al., 2020; Guriev and Papaioannou, 2022, among others). Carbon pricing operates through a comparable channel, since it raises costs for specific industries in specific places, and the incidence depends on local industrial composition. If the political reaction follows the same pattern, the sustainability of ambitious climate regulation depends partly on how its costs land geographically and whether affected communities receive adequate compensation.

To test whether this cost shock shifted voting behaviour, the paper constructs a region-level measure of carbon cost exposure, *GreenShock*, defined as the rise in market-purchased allowances per manufacturing worker from Phase 2 to Phase 3. Combining this measure with NUTS-3 election data for 889 regions across 23 countries, I estimate a difference-in-differences model that compares changes in PRR vote shares between more and less exposed regions before

[1] The architecture is set out in Articles 10 and 10a of Directive 2003/87/EC (European Parliament and Council, 2003) and operationalised in Commission Decisions 2011/278/EU (European Commission, 2011), 2010/2/EU (European Commission, 2010), and 2013/448/EU (European Commission, 2013), with successive carbon-leakage lists in 2014/746/EU and 2019/708/EU. Section 3.1 sets out the allocation schedule in detail.

and after 2013. To address the concern that the observed shock partly reflects pre-existing industrial decline or endogenous firm adjustments, an instrumental variable strategy isolates the regulatory component of the shock: the decline in free allocations determined by EU-wide benchmarks.

The results confirm this expectation. Regions more exposed to the carbon cost shock experienced a significant increase in PRR vote shares after Phase 3. The effect is robust across specifications and concentrated in European Parliament (EP) elections, where the total GreenShock effect is roughly three times larger than in national elections. It also survives pre-treatment controls for Eurozone-crisis exposure and migration patterns. This pattern fits the public-choice account of expressive voting in second-order elections (Hamlin and Jennings, 2019; Reif and Schmitt, 1980), since EP contests do not determine national government formation and their low instrumental stakes make them a natural outlet for protest. Replacing PRR with populist radical-left or Green vote shares as the outcome yields negative effects for both, an asymmetry that rules out a generic anti-establishment reading. The instrumental variable estimates are smaller but point in the same direction, and the EP-versus-national contrast holds under both approaches.

The mechanism analysis traces how the carbon cost shock reaches the ballot box. Manufacturing employment in exposed regions does not decline, as firms appear to absorb the cost increase through the compliance flexibility built into the ETS, purchasing allowances rather than shutting down production. The regions that lost ground did so through a broader economic erosion, as services employment fell, GDP per capita declined, and population shrank. This gradual stagnation, rather than factory closures, is consistent with the political reaction documented here. Moreover, heterogeneity analysis confirms that the effect scales with local manufacturing density and is amplified where PRR parties already had a foothold.

The paper contributes to three strands of literature. First, it extends the literature on economic shocks and populism to climate policy, showing that carbon pricing can generate the same kind of spatially concentrated political backlash as trade or fiscal shocks. Second, it extends Konradt and Mangiante (2026), the closest existing study on EU ETS exposure and far-right voting, by adding finer NUTS-3 resolution, an instrumental variable strategy, a sectoral decomposition of the shock, and a mechanism analysis of the downstream regional adjustments. Third, it provides causal evidence for expressive voting in second-order elections by documenting that voters direct policy-specific grievances at the level of government they hold responsible, suggesting that national election results alone understate the political cost of EU-wide regulation, since the backlash disproportionately shifts the composition of the European Parliament toward parties that oppose further climate legislation.

The remainder of the paper is organised as follows. Section 2 reviews the related literature and develops the theoretical framework. Section 3 describes the data and sets out the empirical strategy. Section 4 reports the results, including the mechanism analysis and the robustness checks, and Section 5 concludes.

2 Related Literature and Theoretical Framework

This section reviews the most relevant contributions on the political consequences of economic shocks, identifying both established findings and gaps that motivate the present study. The review is organised around three questions: whether spatially concentrated economic shocks increase populist radical-right support, whether climate policy generates a comparable backlash, and whether voters direct policy-specific grievances at the level of government they hold responsible. A final subsection draws these strands together into the theoretical framework and the testable claims that the empirical analysis evaluates.

2.1 Economic shocks and the populist radical right

Among the competing explanations for the rise of the populist radical right, economic grievances have received the strongest and most consistent empirical support. The central finding is that spatially concentrated economic shocks increase PRR vote shares in affected regions, particularly when existing institutions fail to compensate the losers (Guriev and Papaioannou, 2022; Bolet et al., 2024).

The most developed strand of this literature exploits variation in exposure to Chinese import competition. Autor et al. (2020) show that US commuting zones hit harder by the China trade shock experienced greater political polarisation, a rightward shift in media consumption, and more ideologically extreme campaign contributions. Colantone and Stanig (2018a) demonstrate the European parallel: NUTS-2 regions more exposed to Chinese imports saw higher vote shares for nationalist and radical-right parties, an effect they trace to economic insecurity rather than cultural attitudes. In a companion paper, Colantone and Stanig (2018b) show that the same import shock causally increased the Leave vote in the Brexit referendum and worsened anti-immigration attitudes, even though immigration was not the source of the shock. Dippel et al. (2016) find that in Germany, about two-thirds of the trade-to-far-right-voting effect runs through labour market deterioration. Barone and Kreuter (2021) find a similar pattern across roughly 8,000 Italian municipalities, and Hee (2026) extends the finding to France, showing that less trade-competitive regions vote more populist and that migration shares shape whether the response tilts left- or right-populist. The common finding across these studies is that trade shocks reach the ballot box primarily through local economic decline.

Yet the same pattern does not appear at the country level. Bergh and Kärnä (2021) analyse 33 European countries over 1980–2017 and find no evidence that greater economic globalisation increases populist vote shares across countries, suggesting that the political effects of economic shocks operate at the regional rather than the national level. This finding motivates the sub-national focus of the present study.

Beyond trade, austerity and financial crises generate a comparable political response. Fetzer (2019) provides evidence that welfare-state retrenchment under British austerity increased support for UKIP and contributed to the Brexit vote. Baccini and Sattler (2025) generalise this across 12 Western European countries, finding that fiscal austerity raises populist support among economically vulnerable voters and regions, with the effect driven by the radical right. Funke et al. (2016) document that financial crises have been followed by a roughly 30 per cent increase in far-right vote shares across 20 advanced economies since 1870, whereas ordinary recessions produce no comparable effect. Gyöngyösi and Verner (2022) provide micro-level evidence from Hungary, where household exposure to foreign-currency mortgage losses during the 2008 crisis led to a large and persistent increase in far-right (Jobbik) support, explaining roughly a fifth of the party's overall rise.

Survey-based evidence reinforces these findings at the individual level. Guiso et al. (2024)

show that economic insecurity, measured through job loss and financial distress, is a robust predictor of populist party support across Europe, even after controlling for cultural attitudes. Alabrese et al. (2019) find the same individual-level pattern behind the Brexit vote, where Leave support concentrated among older, less educated, and economically marginal voters. Rodríguez-Posé (2018) frames the broader pattern as the political consequence of places that have lost industries, population, and relevance over decades. Broz et al. (2021) stress that this decline strikes whole communities rather than individuals, eroding local services and population alongside jobs. Scheiring et al. (2024) confirm these regularities in a meta-analysis of 36 causal-inference studies on the globalisation–populism link, finding a small but robust average effect that is stronger where social protection is weak and industrial employment is concentrated (see also Milner, 2021).

One pattern that recurs across these studies is that voters react more strongly to losses imposed by foreign or supranational actors than to comparable losses from domestic sources such as automation. Rodrik (2021) explains this asymmetry: external shocks provide identifiable outsider targets like foreign competitors and international institutions, which makes identity-based mobilisation easier. EU climate policy fits this logic precisely, since the costs of the ETS are imposed by a supranational authority on local industries, giving affected communities a clear external target to blame.

2.2 Climate policy and political backlash

Climate policy can generate political backlash through the same channel as trade and austerity, since it imposes concentrated costs on specific regions within a short period while its benefits, such as lower emissions and improved air quality, are spread across the population and materialise over decades. Stokes (2016) provides early evidence from Ontario, where local exposure to wind turbines reduced support for the incumbent Liberal government. Lockwood (2018) argues that right-wing populist parties have strong incentives to oppose climate policy, both because their voter base is disproportionately exposed to its costs and because climate governance relies on the kind of technocratic, supranational institutions that populists define themselves against.

The closest paper to the present study is Konradt and Mangiante (2026), who examine the effect of EU ETS exposure on far-right voting across European regions. Their identification strategy exploits regulatory events across 2005–2019, and they find that regions more exposed to carbon costs experienced higher support for radical-right parties. The present paper departs from their work in five respects: it anchors identification on the Phase 3 allocation reform, which generates installation-level variation in exposure rather than the country-wide carbon-price movements their design captures; it uses NUTS-3 rather than NUTS-2 regions; it distinguishes European Parliament from national elections to test where voters direct the backlash; it constructs an instrumental variable from the decline in free allocations; and it traces the mechanisms through a yearly panel.

Several related papers examine other dimensions of this relationship. Colantone et al. (2024) find that the cost-benefit asymmetry holds empirically: green policies generate a backlash when their costs are geographically concentrated, even when the aggregate welfare effect is positive. This finding is corroborated by Heddesheimer et al. (2025), who document that after the AfD adopted an anti-green-transition platform in 2016, German counties with higher shares of brown-industry employment showed larger far-right gains. Voeten (2025) provides a clean identification in the Dutch context: Dutch natural-gas-tax rises shifted energy-cost-exposed renters toward the radical right by 5–6 percentage points, showing that a visible change in household energy costs can produce measurable far-right gains. Bolet et al. (2024) show that targeted compensation programmes, among other channels, can mitigate the political costs of decarbonisation in coal-dependent areas, indicating that the backlash depends on how the transition is managed. Dickson (2025) documents that green backlash can also be manufactured through partisan blame attribution, as PRR parties in the UK pinned rising energy prices on net-zero policies even when global fossil fuel markets were the actual driver.

Whether the carbon cost shock reaches voters at all depends on cost pass-through. The mechanism for power generation is well established: Fabra and Reguant (2014), Sijm et al. (2006), and Hintermann (2016) estimate near-complete pass-through of allowance costs into wholesale electricity prices in Spain, Germany, and the Netherlands, since wholesale electricity is priced at marginal cost and the marginal generator's marginal cost includes the allowance price. Pass-through in manufacturing is less automatic but empirically substantial: in a Commission-funded ex-post study covering Phases 2 and 3 across six energy-intensive sectors, de Bruyn et al. (2015) estimate pass-through rates of 20 to 40 per cent for cement, 55 to 100 per cent for steel, and 80 to 100 per cent or higher for refineries and fertilisers, even where allowances were received free of charge, because firms include the opportunity cost of the unused allowance in their pricing decision. Taken together, these estimates suggest that the Phase 3 burden surfaced in electricity bills and product prices rather than remaining inside the installations that paid for the allowances.

Understanding the political backlash also requires understanding the policy's economic effects, which indicate that Phase 3 imposed meaningful costs on covered installations without triggering large-scale plant closures. Dechezleprêtre et al. (2023) find that the EU ETS reduced emissions at regulated installations by about 10 per cent without significantly affecting employment or revenue. Petrick and Wagner (2014), Curtis (2018), and Karlsson (2025) report similar patterns for German manufacturing under the earlier ETS phases, the United States NO_x Budget Trading Program, and Swedish carbon taxation, with employment effects concentrated in energy-intensive industries. Verde (2020) finds limited evidence of carbon leakage, suggesting that firms absorbed the cost increase rather than relocating production abroad. Hernández Carballo et al. (2025) provide the most granular evidence to date, using synthetic difference-in-differences at the NUTS-3 level to show that Phase 3 caused significant contractions in employment and gross value added in the most carbon-intensive industries even as aggregate employment held steady. Känzig and Konradt (2023) confirm from a macroeconomic perspective that carbon price increases reduce activity in the short run. Taken together, these findings indicate that the carbon cost shock did not destroy manufacturing jobs wholesale, but undermined the broader economic vitality of exposed regions, the channel the mechanism analysis in this paper examines directly.

2.3 Expressive voting and second-order EP elections

The distinction between European Parliament elections and national elections is central to identifying whether voters attribute the carbon cost shock to the EU. Hamlin and Jennings (2019) identify two strands in the rational-choice account of voting: an instrumental strand, in which voters cast ballots to influence the outcome and thereby advance their interests, and an expressive strand, in which voters gain value from the act of voting itself, regardless of whether it affects the outcome. They formalise the latter: when votes are unlikely to be decisive, voters can use the ballot to express commitments rather than to influence outcomes. This margin is sharpest where the instrumental stakes are lowest.

Political scientists have documented this pattern under the heading of second-order elections. Reif and Schmitt (1980) characterise EP elections as “second-order national elections” in which turnout is lower, governing parties lose votes, and smaller or radical parties perform better, since EP elections do not determine national government formation. Hix and Marsh (2007) refine this empirical regularity using data from the first six rounds of EP elections, confirming that voters systematically use EP contests to express dissatisfaction, particularly at the midpoint of the national electoral cycle.

Lechler (2019) provides empirical support for this reasoning, showing that regional employment declines increase both anti-EU sentiment and Eurosceptic voting in EP elections, with effects concentrated among the unemployed and low-skilled. More recently, Toygür and Sojka (2026) document that climate policy scepticism has emerged as a new dimension of Eurosceptic mobilisation in EP elections, as Eurosceptic parties increasingly frame EU climate policy as an economic burden imposed by Brussels. Both expressive voting and the second-order framework have been used to explain why Eurosceptic and radical parties are over-represented in the European Parliament. Neither prediction, however, has been tested with causal evidence from a specific EU policy shock that also distinguishes its effect on EP from national elections. The present study fills this gap by exploiting the EU ETS Phase 3 reform as an exogenous source of variation in EU-imposed costs and testing whether the resulting backlash concentrates in EP elections, as both frameworks predict.

2.4 Theoretical framework and testable claims

The three strands reviewed above combine into a single theoretical argument that links the Phase 3 reform to populist radical-right voting. The argument has a demand side and a supply side. On the demand side, the reform creates an uneven regional cost shock that voters experience as a deterioration of local economic conditions. On the supply side, populist radical-right parties convert that diffuse discontent into a directed electoral choice. Whether a regional cost shock becomes a populist vote therefore depends on both steps, and the chain below states each as a conditional claim that the empirical analysis can test.

The demand side has two parts. If a region's installations face a larger Phase 3 cost increase, then the cost does not remain inside those installations, because power producers pass the cost of auctioned allowances into electricity prices and manufacturers pass part of their compliance cost into product prices (Fabra and Reguant, 2014; de Bruyn et al., 2015). And if firms can buy allowances rather than close plants, as the compliance flexibility of the ETS allows, then the visible result is not factory closures but a slower erosion of local economic vitality, as higher energy bills and weaker demand reduce services employment and population. What matters for the theory is how voters perceive this decline. Voters do not need to observe the ETS, follow allowance markets, or attribute their situation to a named policy. They experience the lived deterioration of their region directly. The grievance that results is therefore real but diffuse, attached to the sense that local conditions are worsening rather than to carbon pricing as such (Rodríguez-Posé, 2018; Broz et al., 2021).

The supply side explains how diffuse grievance becomes a particular vote. Grievance does not translate into a vote on its own, because it must first be interpreted, attributed to a culprit, and connected to a political alternative. This is the work of a political entrepreneur, and populist radical-right parties have both the incentive and the means to do it. The incentive is that their core electorate is concentrated in the exposed, declining regions, and that supranational, technocratic climate governance is precisely the kind of institution against which the populist radical right defines itself. The means is framing and blame attribution, and these parties deploy it early. They campaign against EU climate policy from the outset, so the interpretive frame is in place before the regional decline becomes visible, and the green shock then activates a narrative voters have already been offered rather than creating one (Lockwood, 2018; Dickson, 2025). The effect is amplified because populist radical-right parties tend already to be established in left-behind regions, so the shock deepens a grievance they were cultivating rather than building support from scratch (Guiso et al., 2024). By naming Brussels and EU climate policy as the source of rising costs and regional decline, they convert diffuse local discontent into a directed grievance against the EU, which makes the backlash right-specific and EU-directed rather than a generic anti-incumbent protest. Because European Parliament elections carry low instrumental stakes, the expressive-voting logic predicts that this backlash surfaces most strongly at the EU ballot box.

The argument yields three testable claims, each a conditional prediction. First, if the demand-side step operates, then regions more exposed to the carbon cost shock should record higher populist radical-right vote shares after 2013. Second, if the supply-side step channels grievance toward the EU, then the effect should concentrate in European Parliament elections, where low instrumental stakes encourage expressive voting, whereas a pure material-interest response would instead punish incumbents in national elections, where votes influence policy. Third, if the demand-side step works through regional decline rather than direct job destruction, then the operative channel should be broader economic erosion in services employment, GDP per capita, and population rather than manufacturing job losses. The empirical analysis tests each claim in turn.

3 Data and Empirical Strategy

The analysis draws on four data sources: installation-level records from the EU Emissions Trading System (EUTL), regional election results from the EU-NED dataset (Schraff et al., 2023), a party classification from PopuList (Rooduijn et al., 2023, 2024), and regional economic indicators from Eurostat. The unit of analysis is the NUTS-3 region, the finest administrative geography for which consistent election and economic data are available across the EU. The panel covers 889 regions in 23 countries over the period 2002–2020.[2]

[2]The sample includes Austria, Bulgaria, Cyprus, Czechia, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Italy, Latvia, Lithuania, Luxembourg, Netherlands, Norway, Poland, Portugal, Romania, Slovakia, Spain, and Sweden. Five EU member states are excluded due to incomplete coverage in either the NUTS-3 election data or the EUTL installation records: Belgium, Croatia, Ireland, Malta, and Slovenia.

3.1 The EU Emissions Trading System and the GreenShock measure

The EU Emissions Trading System (EU ETS) is the world's largest cap-and-trade programme for greenhouse gas emissions (European Commission, 2023). Launched in 2005, it requires covered installations in the power, manufacturing, and heavy industry sectors to hold emission allowances for each tonne of CO_2 they emit. During the first two phases (2005–2007 and 2008–2012), the vast majority of allowances were distributed free of charge. Phase 3, which began in January 2013, redesigned allocation asymmetrically across covered sectors and required affected installations to purchase any missing allowances at market-determined auction prices, creating an ongoing operating cost that scaled with each installation's carbon output. Electricity generators moved to 100 per cent auctioning under Article 10(1) of Directive 2003/87/EC, so power producers had to buy every allowance for which they were liable. Manufacturing installations retained benchmark-based free allocation under Article 10a, but on a declining schedule that fell from 80 per cent of the benchmark entitlement in 2013 to 30 per cent in 2020 for sectors not on the carbon-leakage list. Sectors classified as exposed to a significant risk of carbon leakage kept 100 per cent of their benchmark throughout the phase.[3] Phase 3 therefore generated a single, dated shock on top of three different regulatory floors: power producers paid in full from 2013, manufacturers outside the leakage list paid an increasing share each year, and manufacturers inside the leakage list remained largely shielded.

I exploit this regulatory shift to construct a region-level measure of carbon cost exposure, *GreenShock*. Its construction starts from installation-level compliance records in the EU Transaction Log (EUTL), obtained from the cleaned redistribution maintained by Abrell (2024), which reports verified emissions, free allocations, and surrendered allowances for every covered installation, along with geographic coordinates. I assign each installation to a NUTS-3 region based on its geographic coordinates and the 2016 NUTS boundary map. For each re-

[3] Free allocation to manufacturing is calculated as the product of an installation's historical activity level and a sector benchmark, set equal to the average emissions intensity of the top-decile most efficient installations in the EU and EEA-EFTA states, with two fallback methods (a heat benchmark and a fuel benchmark) for processes lacking a product benchmark; the methodology is set out in European Commission (2011). A uniform Cross-Sectoral Correction Factor reduced free allocation by a further 5.7 per cent in 2013, rising to 17.6 per cent in 2020 (European Commission, 2013). Ten lower-income Member States could also derogate under Article 10c, granting power plants limited free allowances tied to verified modernisation investments, which provided partial relief without lifting the auctioning rule.

gion, I compute the average annual CO_2 deficit (verified emissions minus free allocation) in Phase 2 (2008–2012) and Phase 3 (2013–2020), take the difference, and normalise by regional manufacturing employment in 2012 to scale the shock relative to the size of the local industrial workforce. The resulting measure is standardised.

Formally, let $D_{i,t}$ denote the annual CO_2 deficit (verified emissions minus free allocation) for installation i in year t, and let T_p denote the number of years in phase p. For each NUTS-3 region r, the raw GreenShock is the difference between the average annual deficit in Phase 3 and Phase 2:

$$\text{GreenShock}_r^{\text{raw}} = \frac{1}{T_3}\sum_{t\in P3}\sum_{i\in r} D_{i,t} \; - \; \frac{1}{T_2}\sum_{t\in P2}\sum_{i\in r} D_{i,t} \tag{1}$$

where $P2 = \{2008, \dots, 2012\}$ and $P3 = \{2013, \dots, 2020\}$, with $T_2 = 5$ and $T_3 = 8$. The per-worker measure is:

$$\text{GreenShock}_r^{\text{pw}} = \frac{\text{GreenShock}_r^{\text{raw}}}{\text{Mfg Employment}_{r,2012} \times 1000} \tag{2}$$

and the standardised variable used in all regressions is:

$$\text{GreenShock}_r = \frac{\text{GreenShock}_r^{\text{pw}} - \overline{\text{GreenShock}^{\text{pw}}}}{\text{SD}(\text{GreenShock}^{\text{pw}})} \tag{3}$$

By construction, GreenShock sums the gap between verified emissions and free allocation across every covered installation in a region, regardless of which of these three regulatory floors the installation fell under. Section 4.5 reports a decomposition that isolates the power-sector component, yielding results consistent with the asymmetric incidence the regulation imposed.

Figure 1 illustrates the regulatory shift. Free allocation roughly matched verified emissions through Phase 2, then dropped sharply in 2013 while emissions declined only gradually, opening the persistent deficit that GreenShock measures. A higher value of GreenShock_r therefore indicates that a region's industrial base absorbed a larger cost increase from this reform.[4]

[4]The most exposed NUTS-3 regions in the sample include the lignite-power and heavy-industry belt of eastern Germany (for example, Chemnitz), central and southern Poland, and the Ústí region in Czechia, where coal-fired electricity generation and energy-intensive manufacturing have historically concentrated and where the Phase 3 auctioning rule for power producers therefore had its largest absolute incidence.

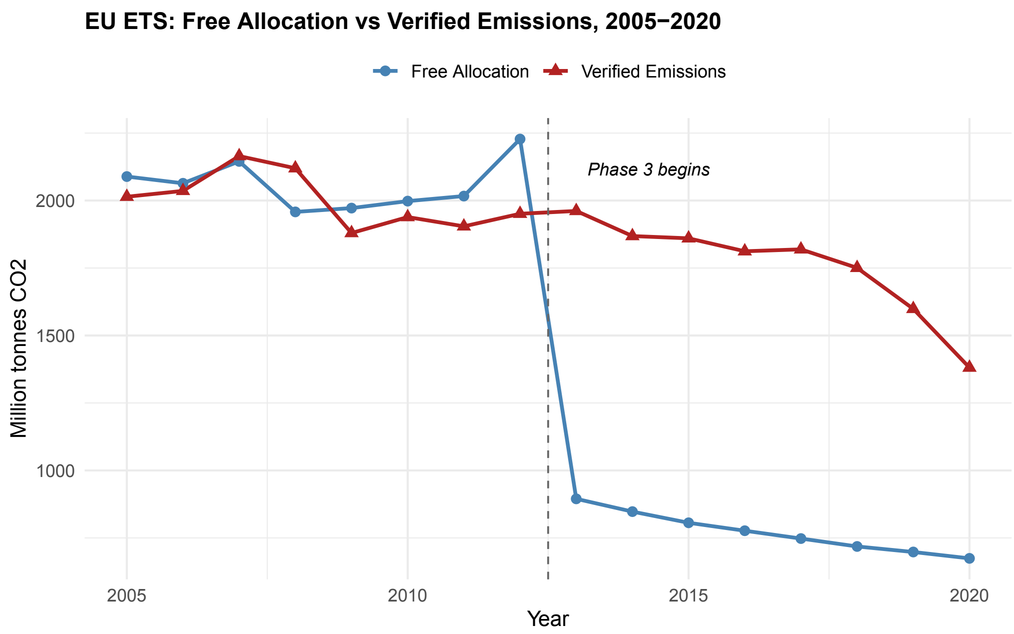

Figure 1: EU ETS: aggregate free allocation versus verified emissions, 2005–2020. The dashed vertical line marks the start of Phase 3 in 2013.

3.2 Electoral and regional data

Voting data come from the EU-NED dataset (Schraff et al., 2023), which provides constituency-level returns for both European Parliament (EP) and national parliamentary elections across EU member states, mapped to the 2016 NUTS-3 classification. The sample includes all EP elections between 2004 and 2019 and all national parliamentary elections held within the same period. The earliest EP election (2004) predates the EU ETS launch and serves as a pre-treatment baseline. The data produce an unbalanced panel of 7,407 region-election observations across 19 elections. The dependent variable is the combined vote share of populist radical-right (PRR) parties, expressed as a percentage of total valid votes.

Parties are classified as populist radical right (PRR) using the PopuList dataset (Rooduijn et al., 2023, 2024), version 3.0, a peer-reviewed expert classification with time-specific validity windows. A party is coded as PRR if it is classified as either "far-right" or both "populist" and "right-wing" during the relevant election period. The inclusion of far-right parties without a separate populist flag follows Mudde (2007, pp. 22–26), who argues that the contemporary European radical right is defined by a core ideology combining nativism, authoritarianism, and populism. Therefore, populism is treated as part of the family definition rather than as a separate additional flag.[5] For each region-election, the PRR vote share is the sum of vote shares of all PRR-classified parties. Two placebo outcomes are constructed for use in Section 4.7: a populist radical-left (PRL) vote share built from PopuList 3.0's far-left flag, and a Green vote share built by matching party names in EU-NED against green-related keywords, with parties already classified as PRR or PRL removed to avoid double-counting.

Regional economic and demographic variables come from Eurostat (Eurostat, 2024). Employment data (*nama_10r_3empers*) report total and manufacturing employment at the NUTS-3 level; GDP per capita comes from *nama_10r_3gdp*. I construct manufacturing employment share and services employment (total minus manufacturing) as additional variables. Demographic data (*demo_r_gind3*) provide population and net migration at the NUTS-3 level. These variables enter the analysis both as controls and as outcomes in the mechanism tests.

Table 1 reports summary statistics for the analysis panel.

Table 1: Summary statistics

Variable	Mean	SD
PRR vote share (%)	8.10	10.95
Turnout (%)	58.62	17.42
GreenShock (standardised)	−0.07	0.25
Mfg employment share (%)	17.85	8.25
GDP per capita (EUR)	25,779	14,504
Observations	7,407	
NUTS-3 regions	889	
Countries	22	
Elections	19	

Notes: The sample covers both European Parliament and national parliamentary elections at the NUTS-3 level, 2002–2020. GreenShock is the standardised increase in annual CO_2 deficit per manufacturing worker from EU ETS Phase 2 to Phase 3. PRR vote share is the combined vote share of parties classified as populist radical right by PopuList 3.0.

[5] Examples include the Rassemblement National (France), the Alternative für Deutschland (Germany), the Lega (Italy), Fidesz (Hungary), and the Partij voor de Vrijheid (Netherlands).

3.3 Estimating the green-shock effect

The empirical strategy exploits the sharp increase in carbon compliance costs that EU ETS Phase 3 imposed from 2013 on regions with carbon-intensive manufacturing and power generation. This section sets out two estimating equations, a baseline model for the average effect on populist radical-right voting and a pooled interaction model that separates European Parliament from national elections, while Section 3.4 takes up the identifying assumptions.

The estimating equation

The baseline specification is a difference-in-differences model with continuous treatment intensity:

$$\mathrm{PRR}_{r,t} = \alpha_r + \gamma_{c(r),t} + \beta\ (\mathrm{GreenShock}_r \times \mathrm{Post}_t) + \varepsilon_{r,t} \tag{4}$$

where $\mathrm{PRR}_{r,t}$ is the populist radical-right vote share in NUTS-3 region r at election t. Region fixed effects (α_r) absorb all time-invariant differences across regions, including geography, industrial composition, and political culture. Country-by-election fixed effects ($\gamma_{c(r),t}$) absorb any shock common to all regions within a country in a given election, such as national political cycles or macroeconomic conditions. Post_t equals one for elections held after Phase 3 began in 2013, and $\mathrm{GreenShock}_r$ is the standardised measure of carbon cost exposure defined in Section 3.1.

Because $\mathrm{GreenShock}_r$ is continuous, β measures a slope: a one-standard-deviation increase in carbon cost exposure is associated with a β-percentage-point change in PRR vote share after 2013, relative to less exposed regions in the same country at the same election. All comparisons are therefore within-country and within-election, so the estimate is not confounded by country-level political swings or election-specific shocks. Standard errors are clustered at the NUTS-2 level, following standard guidance on cluster-robust inference (Cameron and Miller, 2015), since neighbouring NUTS-3 regions tend to share industrial structures and voting patterns.

In extended specifications, the baseline model is augmented with time-varying controls (manufacturing employment share, GDP per capita, voter turnout) to examine whether the baseline effect survives their inclusion. Net migration is excluded here as a “bad control”

(Angrist and Pischke, 2009), since it lies on the causal path between GreenShock and PRR voting rather than being a pre-existing characteristic; a specification that includes it is reported in Section 4.

The EU-attribution channel

As discussed in Section 2.3, EP elections do not determine national government formation, which lowers the instrumental cost of voting and makes them a natural outlet for expressive backlash against EU-level policies (Hamlin and Jennings, 2019; Reif and Schmitt, 1980). To test whether the GreenShock effect differs across election types, I estimate a pooled interaction model:

$$\mathrm{PRR}_{r,t} = \alpha_r + \gamma_{c(r),t} + \beta_1\,(\mathrm{GS}_r \times \mathrm{Post}_t) + \beta_2\,(\mathrm{GS}_r \times \mathrm{Post}_t \times \mathrm{EP}_t) + \varepsilon_{r,t} \tag{5}$$

where EP_t equals one for European Parliament elections and zero for national elections. The model includes β_1 to allow the GreenShock effect to be non-zero in national elections, letting the data determine the national-election baseline rather than assuming it away. β_1 captures the effect in national elections alone. β_2 measures whether the effect is larger in EP elections beyond that baseline. The total effect in EP elections is $\beta_1 + \beta_2$, combining the general post-2013 backlash (β_1) with the additional EU-specific protest channel (β_2). In other words, voters in EP elections are affected both by the general economic backlash (which would make them shift toward the PRR in any election) and by the additional incentive to protest against the EU specifically, so $\beta_1 + \beta_2$ captures the full EP effect. If the EU-attribution channel operates, β_2 should be positive.

3.4 Identification

Instrumental variables

Two concerns may threaten the causal interpretation of the OLS estimate. First, regions with industries already in structural decline before 2013 might exhibit larger CO_2 deficits mechanically, and if these regions were also trending toward the PRR for unrelated reasons, the estimate would be upward biased. Second, firms could respond to the new carbon costs by adjusting production and emissions endogenously after 2013, so that the observed GreenShock partly reflects firm choices rather than the pure policy shock. The pre-trend test discussed below addresses the first concern, but cannot rule out all unobserved confounders. The instrumental variable strategy addresses both.

To address these concerns, the instrument isolates the *regulatory* component of the shock: the decline in freely allocated allowances between Phase 2 and Phase 3. The actual GreenShock reflects both changes in free allocation and changes in verified emissions, but the free-allocation decline was determined by EU-wide rules applied uniformly on the basis of sector benchmarks and product classifications, independent of any region's economic trajectory or political conditions. By isolating this component, the instrument strips out both pre-existing decline and post-2013 firm responses. A separate concern, that the static GreenShock includes data realised after early post-treatment elections, is addressed by a 2013-only robustness in Section 4.7. The approach follows the logic of shift-share instrumental variable designs (Bartik, 1991; Goldsmith-Pinkham et al., 2020): a plausibly exogenous aggregate policy shock (the EU-wide free-allocation cut) is combined with pre-determined local exposure (each region's pre-reform industrial composition) to generate predicted variation in treatment intensity. The instrument, FreeDecline_r, is constructed identically to GreenShock but uses only the change in free allocations. Because power plants moved to full auctioning, they had no annual free allocation left to decline, so FreeDecline tracks the year-by-year decline in manufacturing's free-allocation schedule rather than the separate auction-driven channel by which power producers pass allowance costs through to electricity prices. Let $A_{i,t}$ denote the free allocation for installation i in year t:

$$\text{FreeDecline}_r^{\text{raw}} = \frac{1}{T_3} \sum_{t \in P3} \sum_{i \in r} A_{i,t} \; - \; \frac{1}{T_2} \sum_{t \in P2} \sum_{i \in r} A_{i,t} \tag{6}$$

As with GreenShock, the raw measure is normalised by manufacturing employment in 2012 and standardised. The first stage regresses the actual GreenShock interaction on the predicted shock:

$$\text{GS}_r \times \text{Post}_t = \pi_0 + \pi_1 \left(\text{FreeDecline}_r \times \text{Post}_t\right) + \alpha_r + \gamma_{c(r),t} + \nu_{r,t} \tag{7}$$

The second stage uses fitted values from equation (7) in place of $\mathrm{GS}_r \times \mathrm{Post}_t$ in equation (4). The exclusion restriction requires that the regulatory reduction in free allowances affects PRR voting only through regional carbon costs, which is plausible given the EU-wide, benchmark-based nature of the allocation rules noted above.

Parallel trends

The difference-in-differences design requires that more and less exposed regions were trending similarly in PRR voting before Phase 3. If both groups were on comparable trajectories before 2013, any divergence afterwards can be attributed to the policy shock. I probe this assumption by interacting the exogenous predicted shock ($\mathrm{FreeDecline}_r$) with a linear time trend over pre-treatment elections. A zero coefficient on this interaction indicates no differential pre-trend.

No anticipation

The design assumes that firms and voters did not adjust behaviour before 2013 in anticipation of Phase 3. Two features of the ETS support this. First, ETS-covered installations involve large fixed costs in physical capital and site-specific permits, making preemptive restructuring difficult. Second, the compliance mechanism works against anticipatory adjustment: firms expecting a future shortfall in free allowances can plan to purchase additional allowances on the carbon market rather than altering production before the costs materialise. For voters, anticipatory behaviour may not be plausible, since the political reaction appears only after 2013 when costs materialised, and voters do not typically follow EU regulatory proceedings closely enough to adjust their behaviour years in advance.

3.5 Mechanisms and heterogeneity

Mechanism analysis

To trace the channels through which carbon cost exposure translates into political backlash, equation (4) is re-estimated replacing the PRR vote share with economic and demographic outcomes: services employment, total employment, manufacturing employment, manufacturing employment share, GDP per capita, population, and net migration. These regressions use an annual panel of NUTS-3 regions (2000–2024), which provides finer temporal resolution than the election-year panel. The identifying variation and fixed-effect structure remain the same.

Heterogeneity

To examine where the baseline effect concentrates, the sample is split along three dimensions: the share of manufacturing in local employment, the level of pre-existing PRR support, and the distinction between Eastern and Western Europe. If the carbon cost shock operates through the local economy, regions that depend more on manufacturing, that already had PRR parties to channel grievances, or that lack institutional buffers should react more strongly.

4 Results

4.1 Main result: the green shock and populist voting

This subsection tests the first claim, that regions more exposed to the carbon cost shock recorded higher populist radical-right vote shares after 2013. Carbon cost exposure significantly increased PRR vote shares after Phase 3, as Table 2 shows. Column (1) reports the baseline specification without time-varying controls. A one-standard-deviation increase in carbon cost exposure is associated with a 0.92-percentage-point larger increase in the PRR vote share after Phase 3, relative to less exposed regions in the same country at the same election. Against a sample mean of 8.1%, this corresponds to roughly an 11% increase, meaning that carbon cost exposure produced a politically relevant shift in PRR support even in the baseline specification without controls.

The effect attenuates as controls are added but remains stable and significant throughout. Column (2) adds manufacturing employment share, and the GreenShock coefficient falls to 0.807, indicating that part of the effect runs through the local industrial base. Manufacturing share enters positively and significantly, consistent with the link between industrial structure and PRR support. Column (3) adds GDP per capita, which enters negatively and significantly. Accordingly, the GreenShock coefficient declines to 0.735 but remains significant. The results are also robust to using log GDP per capita: the GreenShock coefficient rises slightly to 0.842

while log GDP becomes insignificant, confirming that the finding does not depend on the functional form of the income control. Column (4) includes voter turnout; the coefficient barely moves (0.739), and turnout itself is insignificant.

Table 2: Main result: EU ETS Phase 3 exposure and populist radical-right voting

	Dep. var.: PRR vote share (%)			
	(1)	(2)	(3)	(4)
GreenShock × Post	0.923**	0.807*	0.735*	0.739*
	(0.457)	(0.439)	(0.436)	(0.436)
Mfg employment share		0.284***	0.301***	0.301***
		(0.082)	(0.083)	(0.083)
GDP per capita			−0.0001**	−0.0001**
			(0.00004)	(0.00004)
Turnout				−0.005
				(0.008)
Region FE	Yes	Yes	Yes	Yes
Country × Election FE	Yes	Yes	Yes	Yes
Observations	7,407	7,406	7,258	7,258
R^2	0.943	0.943	0.944	0.944

Notes: Each column reports a separate OLS regression of the PRR vote share on the interaction of the standardised GreenShock with a post-Phase 3 indicator. The sample includes both European Parliament and national parliamentary elections at the NUTS-3 level, 2002–2020. GreenShock is standardised (mean zero, unit variance). Standard errors clustered at NUTS-2 in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

That the effect survives the inclusion of manufacturing share, itself a plausible mediator, suggests that carbon cost exposure carries political consequences beyond what shifts in industrial composition alone can explain, a question the mechanism analysis in Section 4.4 examines directly. The sectoral decomposition in Section 4.5 revisits this aggregate effect by separating the shock by sector, testing whether the political response reflects the asymmetric Phase 3 design.

4.2 The EU-attribution channel

This subsection tests the second claim, that the effect concentrates in European Parliament elections. To examine whether the political backlash targets the level of government responsible for the carbon cost shock, Table 3 reports estimates from the pooled interaction model (equation 5).

In the OLS specification (column 1), the coefficient on GreenShock × Post, capturing the effect in national elections, is 0.246 and statistically insignificant. By contrast, the triple interaction GreenShock × Post × EP is 1.253 and significant at the 1% level, and the total EP effect of 1.498 is jointly significant. Taken together, the GreenShock effect on PRR voting is concentrated in EP elections, consistent with the expressive-voting prediction: voters direct their backlash at the EU ballot box, where the instrumental cost of protest is lowest, while in national elections the effect is not statistically detectable. A pure material-interest response would affect national elections similarly, yet the EP-concentration fits the expressive prediction more cleanly.

Table 3: Pooled interaction model: EP versus national elections

	(1) OLS	(2) 2SLS
GreenShock × Post	0.246	0.455
	(0.515)	(0.549)
GreenShock × Post × EP	1.253***	0.487
	(0.386)	(0.471)
Total EP effect ($\hat{\beta}_1 + \hat{\beta}_2$)	1.498***	0.943*
	[0.473]	[0.495]
Region FE	Yes	Yes
Country × Election FE	Yes	Yes
KP F-statistic		33.4
Observations	7,407	7,407
R^2	0.943	0.943

Notes: The dependent variable is PRR vote share (%). Standard errors clustered at the NUTS-2 level in parentheses; standard errors for the total EP effect in brackets. The instrument in column (2) is FreeDecline × Post. KP F-statistic = Kleibergen-Paap rk Wald F-statistic. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Column (2) reports the 2SLS estimates using FreeDecline as the instrument. The triple interaction remains positive but loses individual significance, and the total EP effect is 0.943, marginally significant. Moreover, the first-stage F-statistic of 33.4 is well above conventional thresholds for instrument strength, confirming that the free-allocation instruments have sufficient predictive power for both endogenous variables. The IV estimates are uniformly smaller than OLS, consistent with OLS capturing the full economic burden experienced by exposed regions while the IV isolates the manufacturing free-allocation component of the policy shock, leaving the auction-driven electricity-price channel outside its identifying variation. Despite this attenuation, the core pattern is preserved: the EP effect remains roughly twice the national effect, and the total EP estimate falls within the OLS confidence interval.

4.3 Event-study evidence

To examine how the effect evolves over time and to provide visual evidence on pre-trends, Figure 2 presents event-study estimates for EP elections, with a separate GreenShock coefficient for each election period and the last pre-Phase 3 EP election normalised to zero as the baseline.

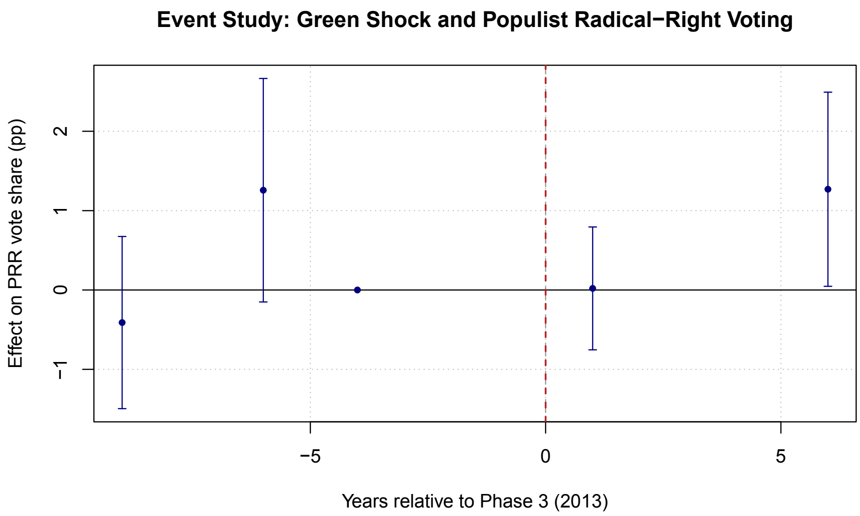

Figure 2: Event-study estimates for EP elections. Each point represents the estimated GreenShock coefficient for a given election period, with the last pre-Phase 3 EP election as the reference category. The dashed vertical line marks the start of Phase 3 (2013). Whiskers denote 95% confidence intervals based on NUTS-2 clustered standard errors.

Before 2013, the coefficients fluctuate without a systematic trend: the earliest estimate is slightly negative, the next shifts positive, and the reference period is set to zero by construction. Importantly, none of the pre-treatment estimates is statistically distinguishable from zero, which supports the parallel-trends assumption. After Phase 3, the pattern changes. The first post-treatment coefficient is close to zero, but the second rises to approximately 1.3 percentage points and approaches significance. With only two EP elections after 2013, the individual estimates are imprecise, but the upward trajectory suggests that the political effect of carbon cost exposure strengthened as the cumulative burden of Phase 3 grew. The pattern coincides with the period in which allowance prices rose sharply, from roughly €5–8 per tonne CO_2 in 2013–2017 to €15–25 in 2018–2020, as the Market Stability Reserve (European Parliament and Council, 2015) began withdrawing surplus allowances from auctions, though we cannot tell whether the increase reflects the rising carbon price itself or a slower behavioural response by voters.

4.4 Mechanisms

The results so far establish the political effect, but not how the cost shock reached voters. The theoretical framework in Section 2 answers that with a demand-side step, in which the shock becomes grievance not through factory closures but through a visible erosion of local economic vitality that voters experience directly. This subsection tests that step, which is the third testable claim and the empirical core of the demand side of the argument. Voters need not know the EU ETS by name, nor trace allowance markets to their electricity bills, for a declining region to register as grievance. If the demand-side step holds, the GreenShock should leave a trace in regional economic and demographic outcomes even while manufacturing employment itself remains stable, and the analysis below examines exactly those outcomes.

Table 4 replaces the PRR vote share with a series of economic and demographic outcomes. Each column estimates the same specification on a different dependent variable using an annual panel (2000–2024), which provides finer temporal resolution than the election-year data. The specification is otherwise identical to the baseline: region fixed effects, country$\times$year fixed effects, standard errors clustered at NUTS-2.

The natural expectation is that carbon costs destroyed manufacturing jobs, but the results suggest otherwise. The GreenShock coefficient on manufacturing employment is -0.024 and statistically insignificant (column 3). Firms facing higher compliance costs appear to have purchased additional allowances on the carbon market rather than shutting down production, consistent with the compliance flexibility built into the ETS. The cost of doing business rose without producing a wave of plant closures.

The damage showed up elsewhere. As households absorbed higher electricity bills and firms adjusted their spending in response to higher compliance costs, demand for local services

Table 4: Mechanism regressions: GreenShock and regional economic outcomes

	(1) Log services empl.	(2) Log total empl.	(3) Log mfg empl.	(4) Mfg share (pp)	(5) Log GDP p.c.	(6) Log pop.	(7) Net mig. rate (‰)
GreenShock × Post	−0.046***	−0.037***	−0.024	0.674***	−0.021*	−0.025***	1.282**
	(0.010)	(0.008)	(0.017)	(0.222)	(0.011)	(0.007)	(0.648)
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each column reports a separate regression of the indicated outcome on GreenShock × Post using the annual panel (2000–2024). All specifications include region and country×year fixed effects. Standard errors clustered at the NUTS-2 level in parentheses. Net migration rate is defined as net inflow per 1,000 inhabitants (Eurostat). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

weakened. Consistent with this, services employment fell significantly by 4.6% (column 1), dragging total employment down by 3.7% (column 2). Moreover, GDP per capita declined by 2.1% (column 5). As economic activity contracted, population followed, dropping by 2.5% (column 6). The regions that bore the carbon cost shock did not lose their industrial base; they lost the broader economic vitality that sustains local communities. Broz et al. (2021) show that it is this place-wide decline, rather than individual hardship, that translates into support for the populist right. On a related note, manufacturing employment share rose by 0.67 percentage points (column 4), though this reflects the faster contraction of services rather than any growth in manufacturing itself.

Migration tells a more complex story. GreenShock-exposed regions experienced significantly higher net inflow of 1.28 per thousand inhabitants per year (column 7), rather than the outflow one might expect from economically declining regions. Modest in any single year, this cumulates over the seven post-treatment years to roughly 9 additional net arrivals per 1,000 inhabitants relative to less exposed regions. In a NUTS-3 region of 50,000 inhabitants, this amounts to roughly 450 additional net arrivals over the post-treatment period, a shift large enough to be visible in smaller communities. Positive net migration into economically declining regions may seem counterintuitive, but a plausible interpretation is that migration patterns shift as local economic conditions weaken, even if the composition of movers lies beyond the scope of the present data. That population still declines despite this inflow is a second apparent contradiction, but it is consistent with the demographic structure of these regions: aging populations tend to produce more deaths than births, and the contraction in services employment may push some existing residents to seek opportunities elsewhere. Population decline despite positive net migration suggests that other demographic forces were also contributing. The data do not allow us to identify the characteristics of movers or to test whether such demographic turnover translates into PRR support, leaving that question open.

These results show that the carbon cost shock reached voters through regional stagnation, not factory closures. The two subsections that follow refine this picture, asking which component of the shock drives the political response and which regions react most strongly.

4.5 Decomposing the shock: power versus non-power installations

As Section 3.1 described, Phase 3 imposed an asymmetric regulatory burden across covered sectors. Electricity generators bought every allowance from 2013 onwards and recovered the cost through wholesale electricity prices, while non-leakage manufacturers paid only for the part of their emissions that exceeded a declining benchmark and leakage-list manufacturers were largely shielded. If the political backlash documented above reflects this asymmetric incidence, decomposing GreenShock into its power and non-power components should reveal that the EP concentration is carried by the power component, since that is the part of the shock that reached households through the visible electricity bill.

Table 5 reports the decomposition. Column (1) reproduces the baseline. Column (2) rebuilds GreenShock using only manufacturing and other non-power installations, excluding NACE 35. Column (3) rebuilds it using only NACE 35 power installations. The non-power measure retains a positive baseline coefficient (0.297, $p < 0.10$), but the EP concentration is no longer detectable. The power-only measure, by contrast, averages an insignificant 0.180 across all elections but carries the EP concentration with a total EP effect of 0.403 ($p < 0.05$). The two measures are correlated at 0.84 across regions, since most ETS regions host both kinds of installation.[6]

[6] A horse-race regression that includes both measures simultaneously washes both coefficients out. This reflects the collinearity of the two decomposed measures rather than an independent economic finding, and it is the reason the decomposition is reported as separate specifications rather than as a single joint regression.

Table 5: Decomposing the Phase 3 shock: original, non-power, and power-only GreenShock

	Dep. var.: PRR vote share (%)		
	(1) Original	(2) Non-power	(3) Power-only
GreenShock × Post	1.240**	0.297*	0.180
	(0.496)	(0.155)	(0.162)
Total EP effect ($\hat{\beta}_1 + \hat{\beta}_2$)	1.912***	0.269	0.403**
	[0.490]	[0.203]	[0.164]
Correlation with original GreenShock	—	0.35	0.84
Region FE	Yes	Yes	Yes
Country × Election FE	Yes	Yes	Yes
Observations	7,325	6,582	5,365

Notes: Columns (1) and (2) use the same set of just over 7,300 regions, after dropping the 1% of regions where the non-power measure produced extreme per-worker ratios driven by small manufacturing-employment denominators; column (1) reproduces the original baseline on this slightly smaller sample than Table 2. Column (3) uses a smaller set of about 5,300 regions, because the power-only measure only exists for regions with at least one NACE 35 power installation, and applies the same 1% trim for the same reason. The Total EP effect is the linear combination $\beta_1 + \beta_2$ from a pooled interaction model with $\text{GS}_r \times \text{Post}_t$ and $\text{GS}_r \times \text{Post}_t \times \text{EP}_t$. Standard errors clustered at the NUTS-2 level in parentheses; standard errors for the Total EP effect in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Read together, the decomposition is consistent with the regulatory architecture. Residual cost pressure on manufacturing produces the modest baseline effect, while the power-sector component of the shock, fully auctioned and passed through to electricity prices, drives the EP concentration. The pattern aligns with the pass-through estimates of Fabra and Reguant (2014), Sijm et al. (2006), and de Bruyn et al. (2015), and with the regulator's own concern, since the Article 10c derogation was introduced specifically to allow lower-income Member States to limit the sharp increases in electricity prices for households that auctioning was expected to cause (European Commission, 2023). The decomposition therefore strengthens the headline finding by locating the EP concentration precisely where the regulator's own justification for the Article 10c derogation implied the largest household-level cost shock would arise, namely in regions whose power-sector exposure made the auctioning rule's pass-through to electricity prices most acute.

4.6 Heterogeneity

So far, the baseline estimate has captured the average effect across all regions. This subsection examines where this effect concentrates by splitting the sample along three dimensions: the role of manufacturing in the local economy, the pre-existing level of PRR support, and the distinction between Eastern and Western Europe. Table 6 reports the results.

Table 6: Heterogeneity analysis

	(1) Mfg density	(2) Pre-existing PRR	(3) East vs West
Panel A: Below median / Western			
GreenShock × Post	0.603	0.836	0.730
	(0.491)	(0.592)	(0.524)
Panel B: Above median / Eastern			
GreenShock × Post	1.598**	1.055	1.784**
	(0.656)	(0.649)	(0.849)
Region FE	Yes	Yes	Yes
Country × Election FE	Yes	Yes	Yes

Notes: Each column reports subsample estimates of the baseline specification. Manufacturing density is the pre-treatment share of manufacturing in total employment. Pre-existing PRR is the average PRR vote share in pre-2013 elections. Eastern Europe includes Bulgaria, Czechia, Estonia, Hungary, Latvia, Lithuania, Poland, Romania, and Slovakia. Standard errors clustered at NUTS-2 in parentheses. ** $p < 0.05$, * $p < 0.10$.

The first split tests whether the effect is stronger in regions where manufacturing plays a larger role in the local economy. The rationale is intuitive: higher industrial costs should spill over more visibly where the local economy is more dependent on manufacturing, a channel the mechanism analysis in Section 4.4 documented in detail. In regions with above-median manufacturing density, the GreenShock coefficient is 1.598 and significant at the 5% level; in below-median regions it is 0.603 and insignificant. This pattern is confirmed by a continuous interaction between GreenShock and standardised manufacturing density, which yields a positive and significant moderating effect (0.799, $p = 0.050$), indicating that each additional standard deviation of manufacturing density raises the GreenShock effect by roughly 0.8 percentage points. The political backlash from carbon pricing, in other words, scales with how dependent the local economy is on its industrial base.

The second split examines whether carbon costs created PRR support from scratch or amplified existing grievances. In regions with above-median pre-treatment PRR vote shares, the GreenShock coefficient is 1.055, while in below-median regions it is 0.836; neither is individually significant, but the direction is consistent with amplification. However, a finer tercile split suggests that the amplification concentrates at the top: regions in the highest tercile of pre-existing PRR support show a GreenShock coefficient of 2.019, significant at the 5% level, while the bottom and middle terciles show smaller and insignificant effects. Carbon cost exposure appears to be most politically potent where populist radical-right parties already had a foothold to channel the resulting economic grievances, aligning with the broader literature on how economic shocks activate rather than create populist support (Guiso et al., 2024).

The third split separates Eastern European member states from Western and Northern Europe. Here, the GreenShock coefficient is 1.784 and significant in Eastern Europe, compared to 0.730 and insignificant in the West. This does not come as a surprise, since Eastern European regions tend to have lower incomes and thinner social safety nets, which may make the same regulatory burden harder to absorb economically and easier to mobilise against politically. The pattern also aligns with the regulatory geography of Phase 3, since these regions sit on the lignite-fired power belt of central Europe, where coal-heavy electricity generation produced the largest absolute auction-cost incidence and the most acute electricity-price pressure on already-stretched household budgets.

Across all three dimensions, the pattern is consistent: the political cost of carbon pricing falls most heavily on regions where the local economy depends on manufacturing, where PRR parties already had a presence, and where institutional buffers are weakest.

4.7 Robustness

The preceding subsections established the green-shock effect, its concentration in European Parliament elections, and its mechanism. This subsection subjects that headline result to a series of robustness checks.

Placebo test. To test whether the result could arise from the spatial distribution of GreenShock by chance, the actual GreenShock values are randomly redistributed across regions 500 times and the baseline specification is re-estimated for each permutation. Figure 3 plots the resulting distribution. Only 1% of random permutations produced a coefficient as large as the actual estimate of 0.923, suggesting that the result reflects the actual geographic distribution of industrial exposure rather than a statistical artefact.

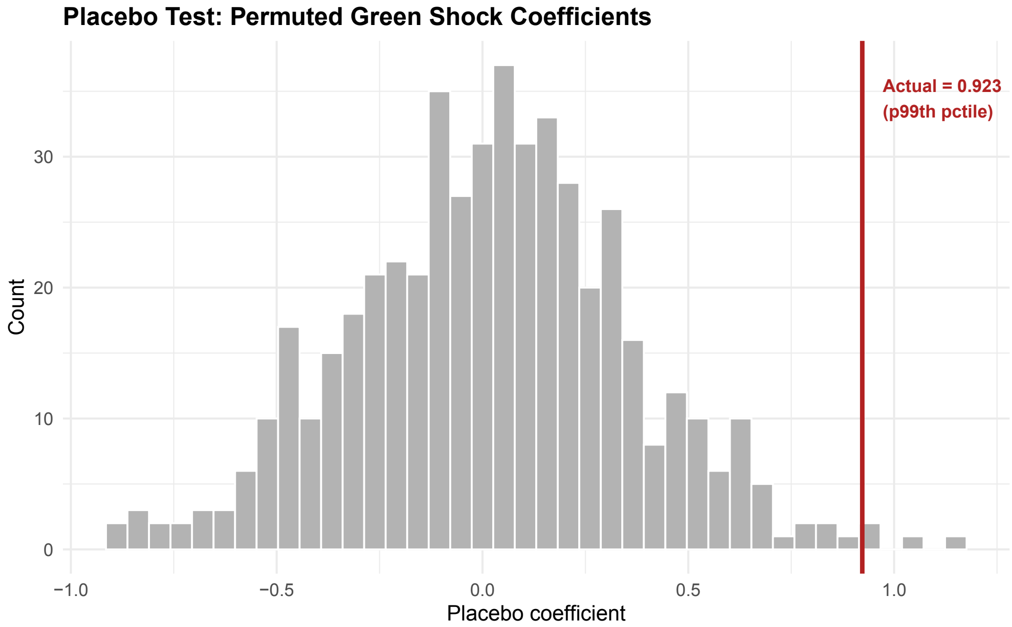

Figure 3: Placebo test: distribution of coefficients from 500 random permutations of GreenShock across regions. The vertical red line marks the actual estimate (0.923), which falls at the 99th percentile of the placebo distribution.

Multi-party placebos. If the GreenShock effect reflects generic anti-establishment grievance rather than a right-specific channel of EU-policy backlash, it should also appear in populist radical-left (PRL) and Green vote shares. I construct PRL vote shares from PopuList 3.0's far-left classification and Green vote shares from party-name patterns in EU-NED, and re-estimate the baseline DiD and the pooled interaction with each as the dependent variable. Table 7 reports the results alongside the headline PRR estimates. PRL falls by a significant 0.52 percentage points at baseline and a marginally significant 0.45 in EP elections; Green vote shares fall by a marginally significant 0.30 at baseline and a significant 0.55 in EP elections. The PRL decline indicates that protest voting shifts toward the right rather than the left; the Green decline reflects cost-exposed voters turning away from the parties most identified with the climate-policy agenda. This mirror-image pattern is consistent with the framing channel discussed in Section 2.2, since opposition to EU climate regulation has been more central to populist radical-right platforms than to radical-left or green ones.

Table 7: Placebo tests: populist radical left and Greens

	(1) PRR *paper's outcome*	(2) PRL *placebo*	(3) Greens *placebo*
GreenShock × Post	0.923**	−0.519**	−0.302*
	(0.457)	(0.225)	(0.164)
Total EP effect ($\hat{\beta}_1 + \hat{\beta}_2$)	1.498***	−0.453*	−0.554**
	[0.473]	[0.240]	[0.280]
Region FE	Yes	Yes	Yes
Country × Election FE	Yes	Yes	Yes
Observations	7,407	7,388	7,388

Notes: Column (1) reproduces the baseline and total EP effect from Tables 2 and 3. Column (2) re-estimates the same specifications with populist radical-left (PRL) vote share as the outcome, constructed from PopuList 3.0's far-left classification. Column (3) re-estimates with Green vote share as the outcome, constructed from party-name patterns in EU-NED, with parties already classified as PRR or PRL removed to avoid double-counting. Standard errors clustered at NUTS-2 in parentheses; standard errors for the total EP effect in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

No look-ahead. Since the static GreenShock averages deficits over the full 2013–2020 phase, the 2014 and 2015 elections are partly explained by data that had not yet been realised when those elections took place. To verify that this look-ahead is not driving the result, I rebuild the treatment using only 2013 deficits, so the 2014 election uses only first-year Phase-3 information, and re-estimate the baseline DiD and the pooled interaction, reported in Table 8. The EP concentration survives and stays significant at the 5% level, while the baseline coefficient attenuates and loses individual significance. The attenuation simply reflects that 2013, with allowance prices still around €5 per tonne CO_2 before the post-2018 spike to €15–25, captures only a small share of the Phase-3 cost shock voters eventually experienced. The headline EP finding is therefore robust to the look-ahead in the static measure.

Leave-one-country-out. The baseline specification is re-estimated 23 times, each time dropping one country. In 21 of 23 cases, the coefficient remains significant at the 5% level. Significance is lost only when Germany is excluded; Poland's exclusion also reduces significance, though the p-value remains close to conventional thresholds ($p = 0.101$). Germany alone accounts for roughly 40% of the sample, so its removal entails a substantial loss of statistical

Table 8: Robustness to look-ahead in the static GreenShock

	(1) Static GreenShock *2013–2020*	(2) 2013-only GreenShock *first year only*
GreenShock × Post	0.923**	0.247
	(0.457)	(0.167)
Total EP effect ($\hat{\beta}_1 + \hat{\beta}_2$)	1.498***	0.379**
	[0.473]	[0.173]
Region FE	Yes	Yes
Country × Election FE	Yes	Yes
Observations	7,407	7,321

Notes: Column (1) reproduces the headline baseline and total EP effect from Tables 2 and 3, using the static GreenShock averaged over Phase 3 (2013–2020). Column (2) re-estimates both specifications with a GreenShock built from 2013 deficits only, so that no election is explained by data realised after it. Standard errors clustered at NUTS-2 in parentheses; standard errors for the total EP effect in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

power. Similarly, Poland contributes substantial industrial variation among Eastern European countries in the sample. In all cases the coefficient remains positive and similar in magnitude, and any loss of significance reflects reduced precision rather than a different effect.

Alternative clustering. The baseline clusters standard errors at NUTS-2. The model is re-estimated clustering at the NUTS-3 level, at the country level, and using two-way clustering by NUTS-2 and election year. The point estimate is mechanically unchanged at 0.923. The standard error ranges from 0.348 (country-level) to 0.440 (NUTS-3) to 0.517 (two-way). The estimate remains significant under all clustering specifications.

Concurrent regional shocks. Other regional shocks of the same era could in principle drive the GreenShock coefficient: the Eurozone sovereign debt crisis of 2008–2012 left lasting labour-market damage in several regions, and European migration patterns since the 2004 enlargement, from southern Europe during the sovereign debt crisis, and from outside the EU have concentrated unevenly across regions. Because these shocks predate Phase 3, they can be addressed with pre-treatment controls that do not risk the bad-control problem discussed below. Table 9 reports three such tests: the regional change in GDP per capita from 2008 to 2012, the regional change in total employment over the same window, and cumulative net migration per 1,000 inhabitants over 2008–2012, each interacted with Post_t. The GreenShock coefficient ranges from 0.71 to 1.03 in the all-elections specification and from 0.86 to 1.33 in the EP-only specification, and the pooled-interaction Total EP estimate stays in the range 1.32–1.65 ($p \leq 0.012$) across all three controls. Pre-treatment labour-market damage is itself associated with higher post-2013 PRR voting, while pre-treatment migrant inflow is associated with lower PRR voting at the within-country level. Neither pattern absorbs the GreenShock effect.

Table 9: Robustness to concurrent regional shocks

	(1) ΔGDP 2008–12	(2) ΔEmpl. 2008–12	(3) Migration 2008–12
GreenShock × Post (all elec.)	1.027**	0.846*	0.707
	(0.471)	(0.453)	(0.471)
Control × Post (all elec.)	−0.388*	0.574***	−0.572**
	(0.220)	(0.216)	(0.262)
GreenShock × Post (EP only)	1.326***	1.155**	0.858*
	(0.502)	(0.478)	(0.510)
Total EP (pooled interaction)	1.649***	1.425***	1.318**
	(0.487)	(0.470)	(0.527)
Region FE	Yes	Yes	Yes
Country × Election FE	Yes	Yes	Yes
N (regions / obs.)	852 / 7,111	889 / 7,407	713 / 6,118

Notes: Each column augments the baseline DiD with one pre-treatment control interacted with Post_t. Column (1) uses the standardised change in GDP per capita 2008–2012; column (2) the standardised change in total employment 2008–2012; column (3) standardised cumulative net migration 2008–2012, per 1,000 inhabitants in 2008. The coefficient on Total EP is the linear combination $\beta_1 + \beta_2$ from a pooled interaction model augmented with the same control. Standard errors clustered at the NUTS-2 level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Net migration as a control. Net migration is excluded from the main specification because it is plausibly a consequence of GreenShock rather than a pre-existing confounder. Even after controlling for net migration, one of the strongest correlates of PRR support across Europe, the GreenShock coefficient retains its positive direction and roughly three-quarters of its magnitude (0.710), though it falls just short of conventional significance levels ($p = 0.106$). This attenuation is consistent with migration being an important transmission channel: controlling for it absorbs part of the causal pathway between carbon costs and PRR voting. Net migration itself enters highly significantly ($p < 0.001$), consistent with its proposed role as a transmission channel between carbon costs and PRR voting, though this interpretation rests on the causal ordering assumed in Section 3.3.

5 Conclusion

The EU Emissions Trading System has reduced emissions across Europe, but the regions that bear the cost of this policy have experienced it as economic decline. This paper has shown that the carbon cost shock imposed by EU ETS Phase 3 significantly increased populist radical-right vote shares in exposed NUTS-3 regions after 2013. The effect is concentrated in European Parliament elections, where the total GreenShock effect is roughly three times larger than in national elections, consistent with voters directing their grievances at the level of government they hold responsible. Instrumental variable estimates point in the same direction, with somewhat smaller magnitudes that reflect the isolation of the purely regulatory component from the risk of pre-existing trends and firm responses biasing the estimate.

Contrary to what one might expect from a carbon cost shock, the mechanism analysis reveals that manufacturing employment did not decline. The compliance flexibility of the ETS allowed firms to purchase allowances rather than cut production, and the asymmetric Phase 3 design partially shielded manufacturing through declining but positive free allocation and the carbon-leakage list. The damage instead ran through the regional economy. As the fully-auctioned power sector raised electricity prices for households and downstream firms, services employment fell, GDP per capita declined, and population shrank. This gradual process of regional economic stagnation, rather than factory closures, is the channel through which carbon costs reached the ballot box. The heterogeneity analysis reinforces this finding, since the effect is strongest in manufacturing-dependent regions, where PRR parties already had a presence, and in Eastern Europe, where institutional buffers are thinnest.

The headline finding survives a series of robustness checks. The EP effect holds when the GreenShock uses only first-year Phase-3 information, ruling out a look-ahead reading of the result. Similarly, pre-treatment regional controls for Eurozone-crisis exposure and migration patterns leave the coefficient largely unchanged, so the backlash reflects the carbon-pricing reform rather than alternative regional shocks of the same era. The effect is also specific to the populist radical right, as placebo regressions on populist radical-left and Green vote shares both turn negative, consistent with the shock activating right-wing rather than generic protest grievance.

These results have direct implications for climate policy design. In particular, if the political costs of carbon pricing are concentrated in specific regions and channelled through EP elections, national-level analyses will underestimate the backlash. Policymakers designing the next phases of the EU ETS, or similar instruments elsewhere, would benefit from anticipating the political geography of the transition alongside its economic geography. The evidence from Bolet et al. (2024) that targeted compensation can mitigate the political costs of decarbonisation suggests that the backlash documented here is not inevitable, but it requires deliberate attention to the manufacturing-dependent regions that bear the costs most acutely, particularly in Eastern Europe where the political reaction is strongest.

Several limitations qualify these findings. The analysis cannot identify the individual-level characteristics of voters who switch to PRR parties, nor the precise profile of migrants entering or leaving exposed regions. Moreover, the post-treatment period contains only two rounds of EP elections, which limits the precision of the event-study estimates. Finally, data availability prevents extending the analysis beyond 2020, leaving open whether the effect has intensified or attenuated as carbon prices rose sharply in recent years. Future work could extend the sample to cover the post-pandemic period and incorporate individual-level voting data to identify which voters within exposed regions shifted toward the PRR.

Taken together, the EU ETS has largely achieved its environmental objective without the large-scale carbon leakage that critics once predicted (Verde, 2020), but the political cost has been borne unevenly across regions and directed at the European Parliament, where future climate legislation is co-decided. As PRR parties gain seats in the EP partly in response to climate policy costs, the institution responsible for advancing the green transition may face growing internal resistance to the very regulation that provoked the backlash, with likely spillovers into national politics as PRR parties translate their EP gains into broader electoral legitimacy and campaign platforms. Whether the green transition can build durable democratic support or will face growing electoral resistance will depend in part on how effectively the regional consequences of carbon pricing are compensated.

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