

Supplementary Appendix to:

Headwind in Sight? Wind Turbine Visibility
Spillovers and Support for Renewable Energy
Policy (JEEM, 2026)

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A Appendix: Descriptive Statistics

A.1 Summary Statistics

Figure A-1 – Distribution of the Mean Number of Visible Turbines (Weighted by the Area of the Settlement) Within the 6km Treatment Zone.

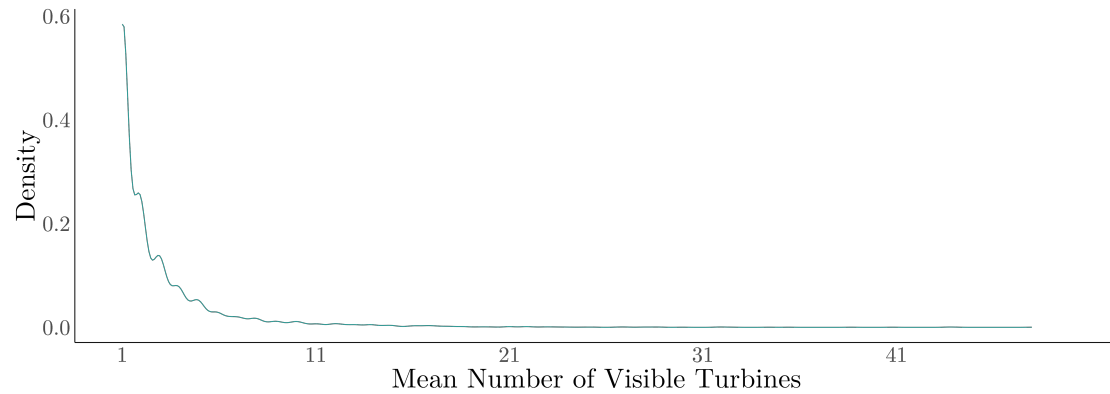


Figure A-2 – (a) Mean Share of Settlements visually exposed per Cohort.
(b) Geographic Distribution per Cohort.

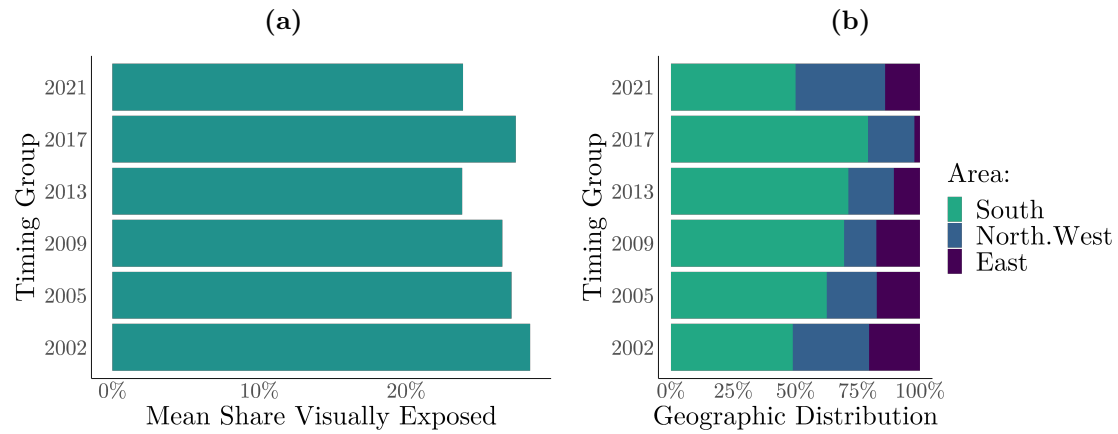


Table A-1 – Cohort Summary: Baseline Model

Cohort	Variable	Pre					Post				
		Treatment		Control		T-Test	Treatment		Control		T-Test
		Mean	SD	Mean	SD		Mean	SD	Mean	SD	
2002	Vote Share Greens (%)	4.83	2.27	5.04	2.65	0.012	6.07	2.98	6.35	3.26	0.009
	Uni. degree (%)	6.05	2.19	6.12	2.24	0.388	6.37	2.27	6.41	2.30	0.645
	Unemployed (%)	4.36	2.50	4.28	2.46	0.386	16.84	11.05	16.30	10.69	0.154
	Female (%)	50.35	1.59	50.39	1.82	0.453	50.25	1.56	50.37	1.70	0.038
	Young (%)	13.62	1.87	13.49	2.20	0.071	12.54	2.11	12.40	2.20	0.057
	PC Income Tax (Log)	5.19	0.58	5.22	0.59	0.146	6.58	0.59	6.62	0.60	0.078
	Immigrants (%)	0.84	0.49	0.89	0.67	0.036	0.56	0.25	0.55	0.23	0.506
2005	Vote Share Greens (%)	6.35	3.38	6.74	3.44	0.109	6.28	3.11	6.54	3.07	0.233
	Uni. degree (%)	6.24	2.05	6.34	2.17	0.525	6.64	2.11	6.75	2.22	0.488
	Unemployed (%)	15.40	10.28	14.92	9.62	0.496	13.01	8.33	12.86	8.01	0.797
	Female (%)	50.25	1.71	50.34	1.74	0.478	50.23	1.93	50.43	1.63	0.127
	Young (%)	12.63	2.27	12.74	2.32	0.470	12.90	2.29	12.82	2.04	0.591
	PC Income Tax (Log)	6.65	0.55	6.64	0.57	0.696	6.32	0.52	6.32	0.51	0.851
	Immigrants (%)	0.56	0.36	0.57	0.38	0.738	0.33	0.23	0.34	0.24	0.924
2009	Vote Share Greens (%)	6.19	2.79	6.16	2.42	0.907	8.33	3.86	8.17	3.31	0.638
	Uni. degree (%)	6.23	1.99	6.32	2.21	0.627	6.76	2.06	6.86	2.25	0.632
	Unemployed (%)	13.20	8.18	12.70	7.74	0.513	13.82	9.23	12.86	7.82	0.233
	Female (%)	50.26	1.83	50.36	1.64	0.518	50.27	1.62	50.25	1.65	0.887
	Young (%)	13.18	2.23	12.89	2.06	0.159	13.10	2.01	12.79	2.19	0.112
	PC Income Tax (Log)	6.32	0.55	6.34	0.54	0.715	6.91	0.44	6.93	0.44	0.722
	Immigrants (%)	0.33	0.21	0.34	0.21	0.818	0.29	0.22	0.30	0.22	0.705
2013	Vote Share Greens (%)	8.93	3.62	9.10	3.80	0.631	6.61	2.90	7.13	3.05	0.067
	Uni. degree (%)	7.62	2.73	7.81	2.93	0.485	8.85	3.09	9.09	3.28	0.443
	Unemployed (%)	12.56	8.38	12.28	7.55	0.720	9.65	6.19	9.35	5.32	0.586
	Female (%)	50.24	1.37	50.20	1.57	0.772	50.17	1.39	50.11	1.43	0.676
	Young (%)	12.83	1.76	12.79	2.10	0.847	12.48	2.08	12.43	2.46	0.830
	PC Income Tax (Log)	7.04	0.41	7.07	0.41	0.366	7.20	0.32	7.22	0.34	0.588
	Immigrants (%)	0.28	0.15	0.28	0.16	0.803	0.41	0.19	0.41	0.20	0.994
2017	Vote Share Greens (%)	7.23	2.76	7.33	3.09	0.644	8.25	3.39	8.43	3.81	0.483
	Uni. degree (%)	8.80	3.43	9.20	3.46	0.098	11.09	4.03	11.57	4.07	0.093
	Unemployed (%)	7.77	3.49	7.93	3.80	0.515	6.98	3.07	7.13	3.23	0.528
	Female (%)	50.15	1.56	50.18	1.47	0.749	49.91	1.47	49.87	1.53	0.649
	Young (%)	12.83	2.01	12.78	2.17	0.749	13.06	1.95	12.94	2.23	0.400
	PC Income Tax (Log)	7.26	0.24	7.29	0.25	0.204	7.51	0.28	7.56	0.33	0.032
	Immigrants (%)	0.41	0.17	0.42	0.18	0.345	1.44	0.56	1.51	0.62	0.091
2021	Vote Share Greens (%)	6.50	3.04	7.56	3.67	0.119	8.53	3.84	11.08	4.97	0.005
	Uni. degree (%)	10.18	2.96	10.43	2.92	0.672	11.88	3.37	12.21	3.37	0.633
	Unemployed (%)	8.36	3.79	7.83	3.98	0.500	1.95	0.93	1.83	0.89	0.519
	Female (%)	49.50	1.34	49.61	1.64	0.705	49.65	1.57	49.54	1.78	0.727
	Young (%)	12.54	2.27	12.52	2.46	0.977	11.97	2.38	12.23	2.06	0.566
	PC Income Tax (Log)	7.38	0.27	7.42	0.31	0.522	6.87	0.25	6.89	0.32	0.667
	Immigrants (%)	1.67	0.56	1.71	0.77	0.759	1.70	0.67	1.78	1.01	0.644

Table A-2 – Cohort Summary: External Exposure Model

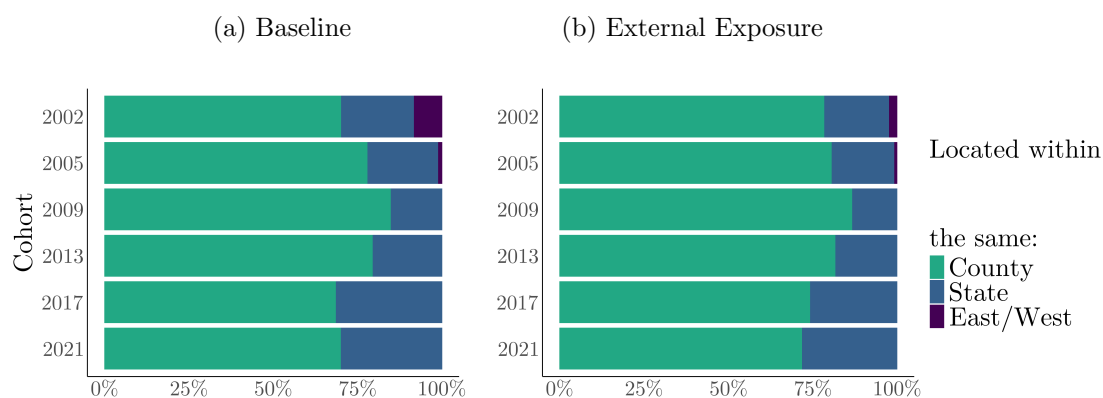
Cohort	Variable	Pre					Post				
		Treatment		Control		T-Test	Treatment		Control		T-Test
		Mean	SD	Mean	SD		Mean	SD	Mean	SD	
2002	Vote Share Greens (%)	4.80	2.28	4.92	2.58	0.212	6.09	2.97	6.25	3.15	0.208
	Uni. degree (%)	6.01	2.21	6.08	2.25	0.420	6.32	2.27	6.38	2.31	0.528
	Unemployed (%)	4.29	2.47	4.30	2.45	0.898	16.56	10.88	16.33	10.62	0.586
	Female (%)	50.32	1.64	50.32	1.83	0.975	50.22	1.62	50.29	1.71	0.307
	Young (%)	13.57	1.91	13.51	2.29	0.459	12.47	2.16	12.40	2.24	0.389
	PC Income Tax (Log)	5.19	0.57	5.21	0.59	0.379	6.58	0.59	6.60	0.60	0.233
	Immigrants (%)	0.83	0.49	0.88	0.65	0.052	0.55	0.23	0.55	0.23	0.971
2005	Vote Share Greens (%)	6.31	3.18	6.67	3.46	0.161	6.28	2.95	6.46	3.09	0.447
	Uni. degree (%)	6.19	2.00	6.28	2.13	0.579	6.57	2.05	6.68	2.18	0.510
	Unemployed (%)	15.71	10.49	15.16	9.92	0.485	13.21	8.53	13.04	8.29	0.799
	Female (%)	50.22	1.77	50.35	1.79	0.332	50.22	2.02	50.45	1.66	0.116
	Young (%)	12.62	2.18	12.72	2.39	0.571	12.89	2.24	12.85	2.05	0.833
	PC Income Tax (Log)	6.62	0.56	6.60	0.58	0.683	6.29	0.53	6.29	0.52	0.933
	Immigrants (%)	0.57	0.38	0.58	0.41	0.815	0.34	0.24	0.34	0.25	0.939
2009	Vote Share Greens (%)	6.20	2.80	6.27	2.47	0.791	8.46	3.94	8.33	3.44	0.745
	Uni. degree (%)	6.20	2.01	6.22	2.06	0.918	6.73	2.09	6.76	2.10	0.918
	Unemployed (%)	12.89	7.58	12.23	6.99	0.379	13.47	8.43	12.48	7.34	0.224
	Female (%)	50.25	1.95	50.34	1.66	0.598	50.27	1.70	50.25	1.65	0.899
	Young (%)	13.11	2.31	12.80	2.05	0.174	13.05	2.07	12.74	2.20	0.164
	PC Income Tax (Log)	6.33	0.54	6.36	0.50	0.548	6.92	0.43	6.95	0.40	0.514
	Immigrants (%)	0.33	0.21	0.34	0.21	0.891	0.30	0.23	0.30	0.23	0.804
2013	Vote Share Greens (%)	8.91	3.57	9.08	3.81	0.663	6.57	2.89	7.16	3.12	0.061
	Uni. degree (%)	7.72	2.75	7.87	2.92	0.620	8.96	3.14	9.13	3.29	0.602
	Unemployed (%)	12.42	7.90	12.22	7.50	0.803	9.53	5.82	9.36	5.42	0.770
	Female (%)	50.18	1.41	50.20	1.38	0.892	50.17	1.45	50.08	1.23	0.515
	Young (%)	12.82	1.83	12.92	2.10	0.632	12.45	2.14	12.64	2.36	0.444
	PC Income Tax (Log)	7.05	0.40	7.07	0.40	0.511	7.21	0.32	7.22	0.33	0.596
	Immigrants (%)	0.28	0.16	0.28	0.16	0.949	0.42	0.19	0.42	0.21	0.801
2017	Vote Share Greens (%)	7.11	2.76	7.22	3.17	0.627	8.14	3.39	8.29	3.92	0.612
	Uni. degree (%)	8.84	3.49	9.02	3.47	0.530	11.14	4.11	11.35	4.09	0.526
	Unemployed (%)	7.86	3.52	7.97	3.74	0.709	7.07	3.17	7.15	3.29	0.771
	Female (%)	50.15	1.67	50.22	1.49	0.580	49.93	1.53	49.90	1.56	0.775
	Young (%)	12.68	2.09	12.73	2.20	0.752	12.99	2.02	12.86	2.25	0.449
	PC Income Tax (Log)	7.26	0.24	7.27	0.26	0.578	7.51	0.28	7.55	0.34	0.109
	Immigrants (%)	0.41	0.17	0.42	0.18	0.782	1.47	0.56	1.51	0.61	0.342
2021	Vote Share Greens (%)	6.38	3.21	7.22	3.36	0.262	8.33	4.02	11.00	5.10	0.012
	Uni. degree (%)	10.01	3.06	10.27	2.82	0.696	11.67	3.48	11.99	3.27	0.670
	Unemployed (%)	8.56	4.17	8.08	4.36	0.620	2.00	1.03	1.87	0.98	0.576
	Female (%)	49.53	1.40	49.69	1.76	0.653	49.65	1.67	49.68	1.84	0.940
	Young (%)	12.55	2.43	12.39	2.39	0.768	11.96	2.59	12.18	2.01	0.677
	PC Income Tax (Log)	7.38	0.27	7.42	0.33	0.645	6.86	0.25	6.89	0.34	0.716
	Immigrants (%)	1.63	0.53	1.69	0.68	0.699	1.66	0.62	1.73	0.88	0.666

A.2 Balance and Validity Tests

In this section, we evaluate the performance of the matching process and conduct a series of tests to assess the assumptions underpinning the theoretical framework and empirical approach of both models.

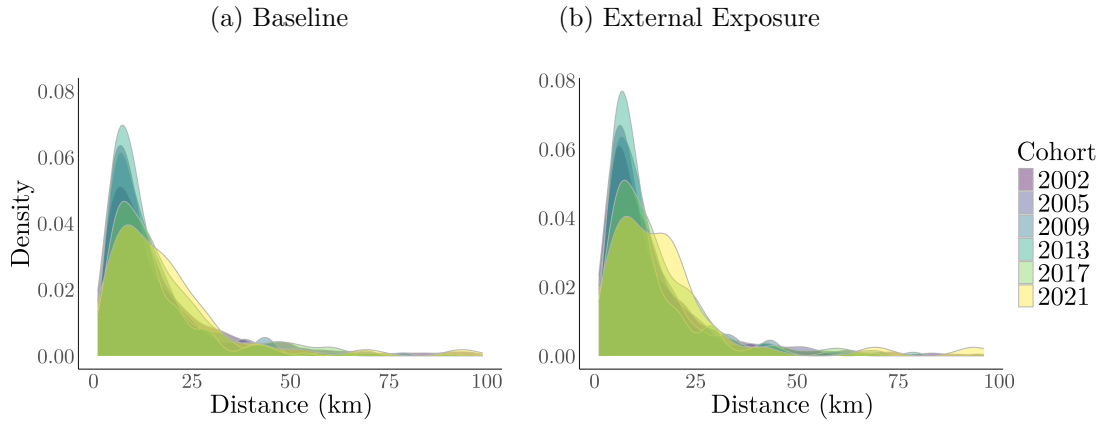
After matching, 73% and 79% of the treated municipalities across the cohorts are located in the same county (NUTS-3) as their matched control municipality for the Baseline and External Exposure Models, respectively. Looking at the rest, 22% and 19% are in the same federal state (NUTS-1), and 5% and 1% are only in the same East/West region of Germany. [Figure A-3](#) shows that the administrative unit overlap between the treatment and control groups are similar across the cohorts.

Figure A-3 – Shared Administrative Levels With Control Municipalities per Cohort



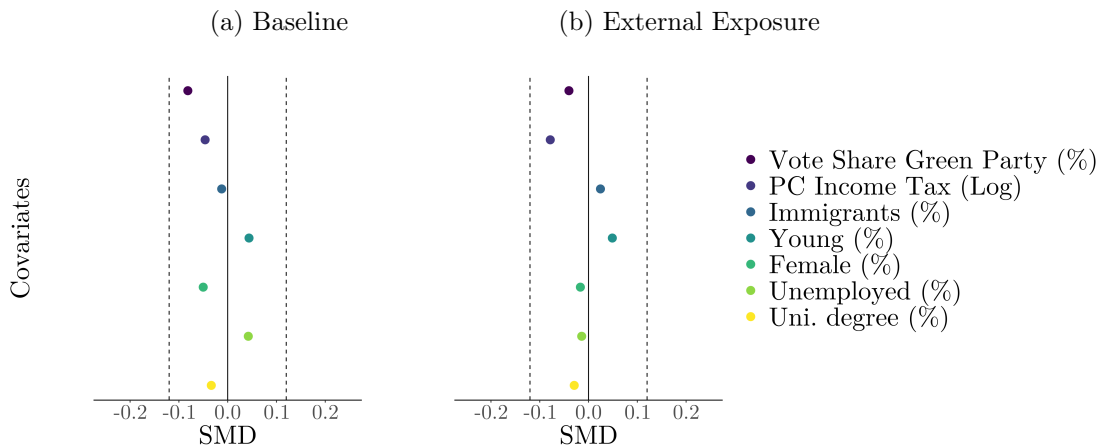
[Figure A-4](#) visualizes the distance distribution per cohort. Overall, the median distance between matched pairs' centroids is 12 km, with the 90th percentile at a distance of 43 km for the Baseline Model. For the External Exposure Model, the median distance is 10.5 km and has the 90th percentile at 32 km.

Figure A-4 – Distance Distribution Between Pairs (up to 100km) per Cohort



The standardized mean differences (SMD) of the covariates and the outcome between the treatment and control groups across the cohorts ranges from -0.08 to 0.05 (Figure A-5). Note that we did not specifically target these covariates during our matching process. Their similarity confirms the strength of our matching procedure: the large majority of treatment and control municipalities are located in the same county and therefore share many socioeconomic, administrative, and cultural characteristics, often belonging to the same labor market.

Figure A-5 – Covariate Balance (SMD)



Furthermore, in [subsubsection A.2.2](#) we plot the mean time trends of each covariate for treatment and control groups across cohorts, confirming that balance is achieved across all election periods.

A.2.1 Parallel trends

[Figure A-6](#) shows the mean vote share of the Green Party over time for each cohort and their control group for the Baseline Model. [Figure A-7](#) is the same for the External Exposure Model. Differences in the levels and trends are minimal, supporting the parallel trend assumption. For the 2021 cohort, the mean share of votes already deviates in the 2017 election period, possibly due to anticipation effects associated with long planning and approval durations, which we discuss in Section 6.4 in the main paper and account for in [subsection C.11](#).

Figure A-6 – Baseline Model: Green Party Vote Share per Cohort.
 Red: Treatment Group, Blue: Control Group

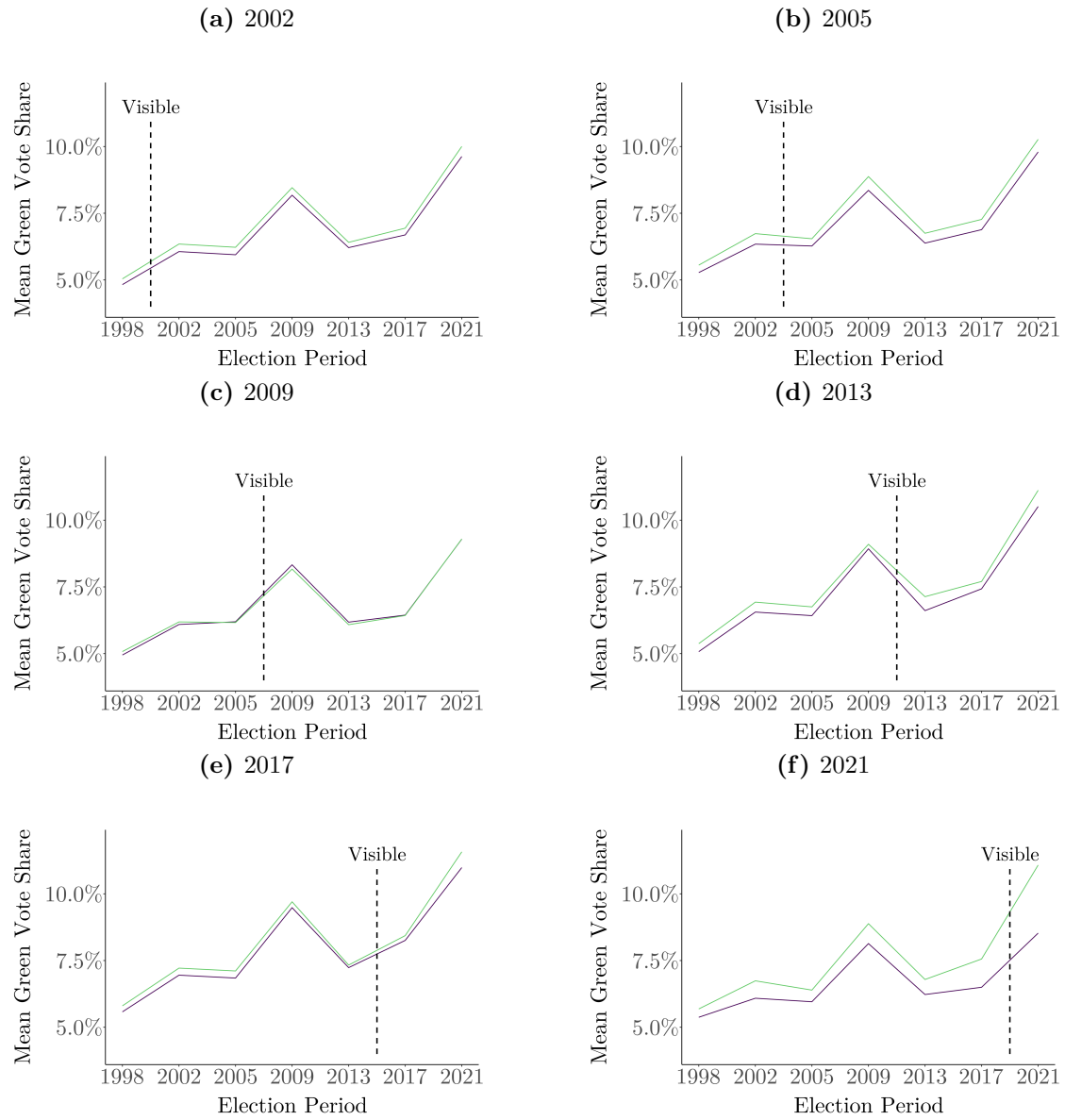
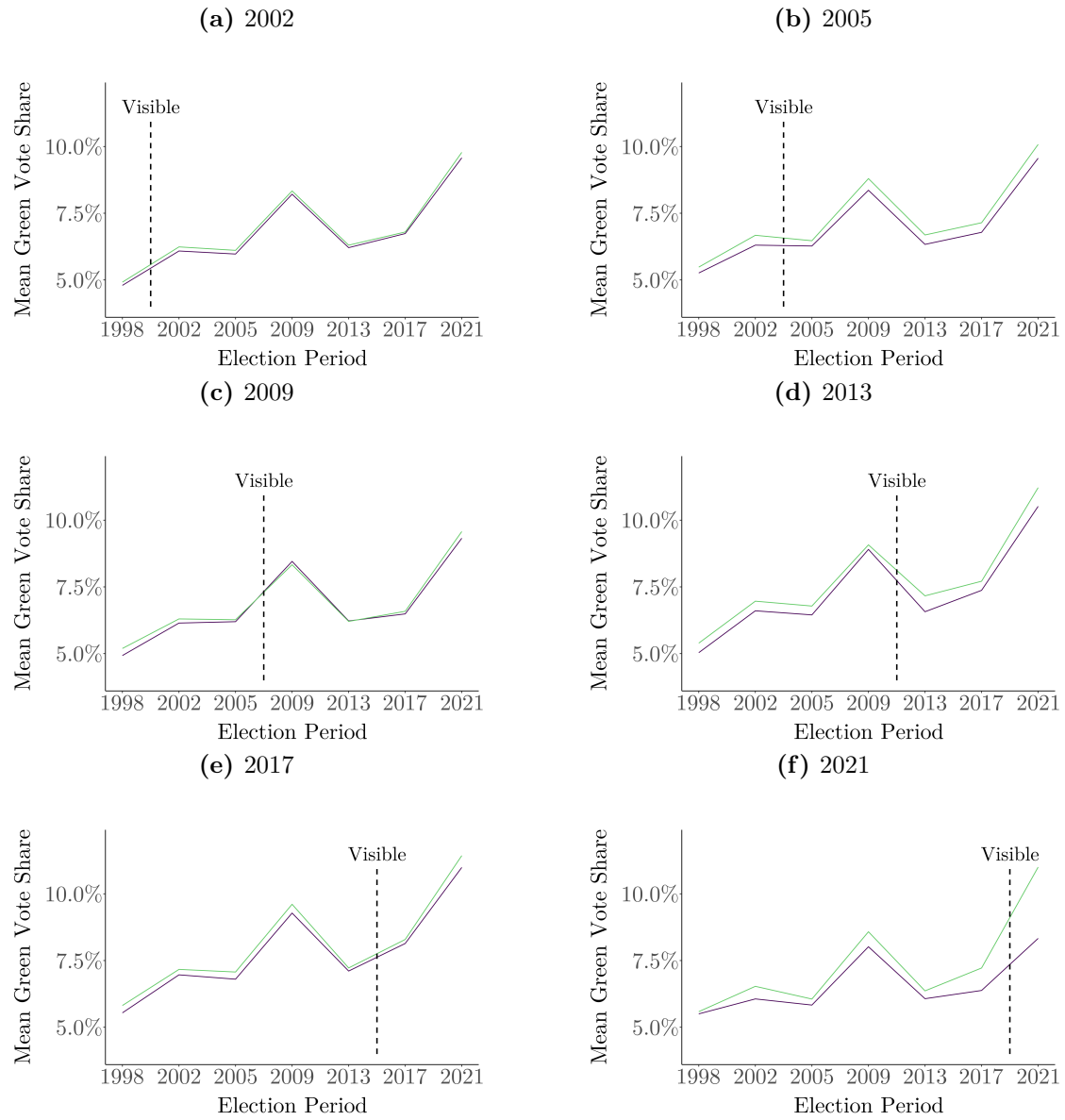


Figure A-7 – External Exposure Model: Green Party Vote Share per Cohort.
Red: Treatment Group, Blue: Control Group



A.2.2 Covariate Balance

Figure A-8 and Figure A-9 illustrate that covariate means across cohorts are well balanced between the Baseline and External Exposure Models over all election periods after matching.

Figure A-8 – Baseline Model: Covariate Means Across Cohorts for all Election Periods

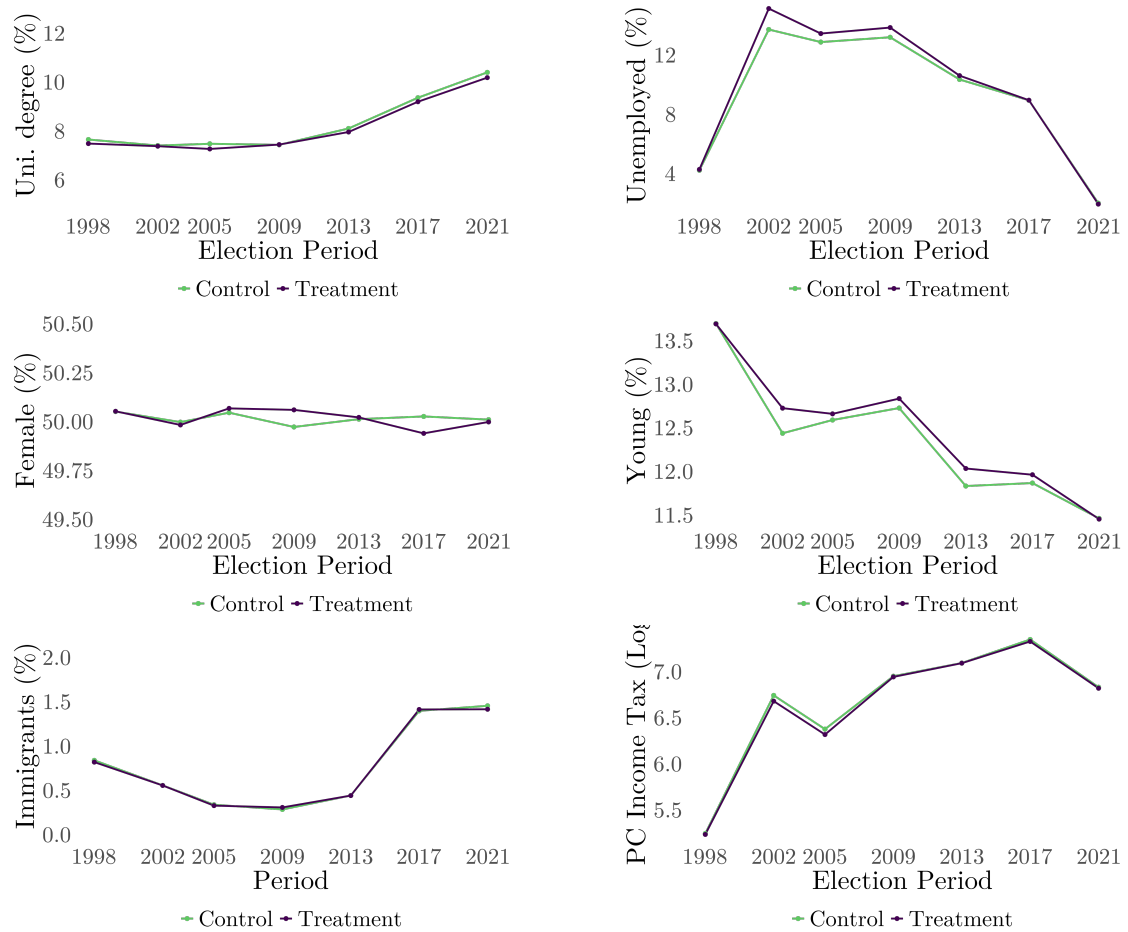
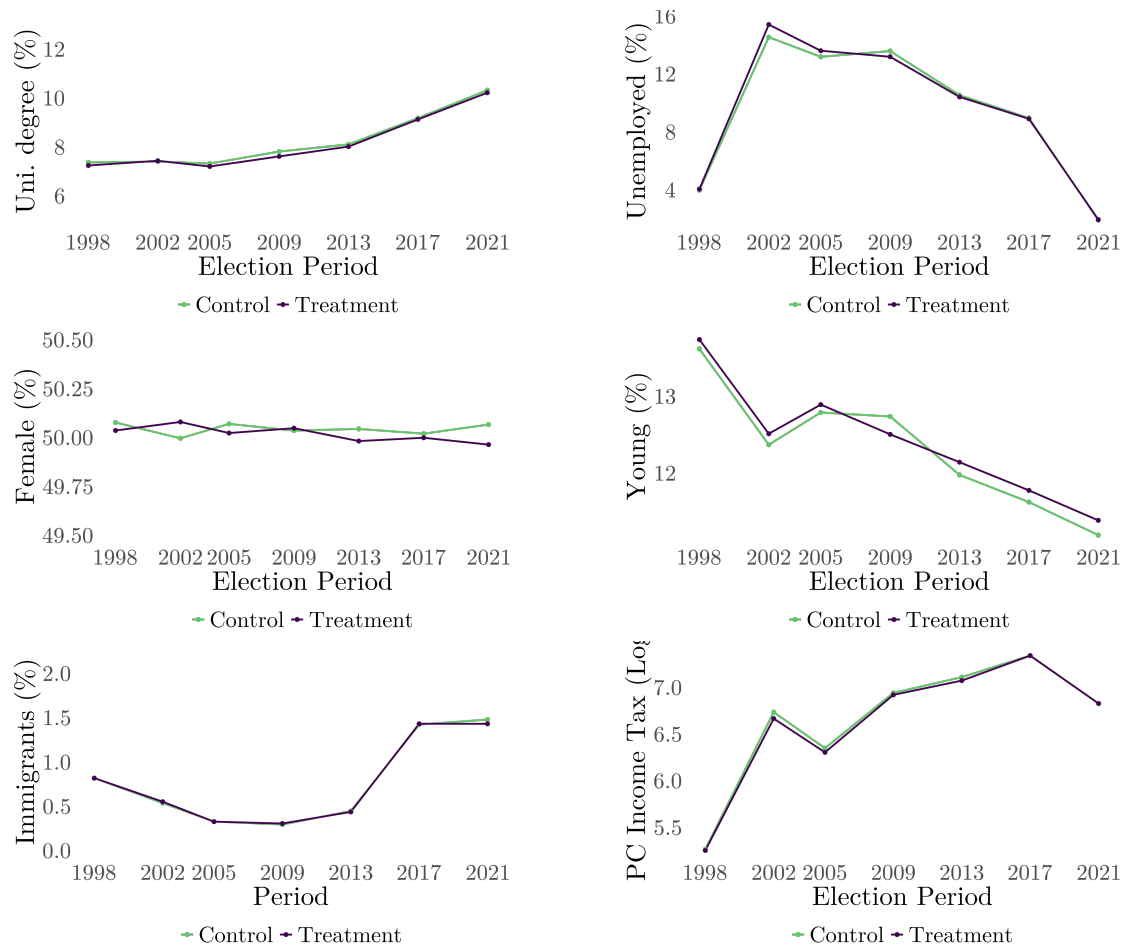


Figure A-9 – External Exposure Model: Covariate Means Across Cohorts for all Election Periods

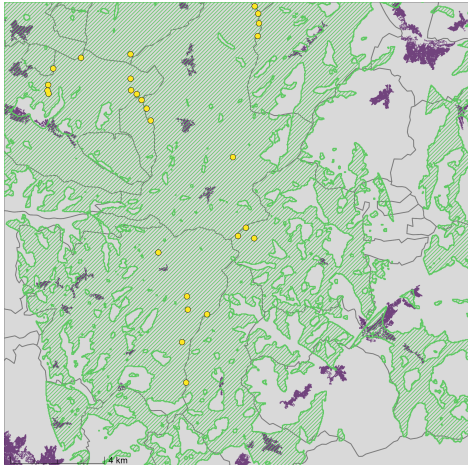


A.3 Additional Examples of Viewshed Analysis

Figure A-10 illustrates several examples of the viewshed analysis. The top panels (a and b) show rural areas in southern and western Germany, which are generally more elevated than the north, resulting in a more rugged viewshed. The middle panels (c and d) depict rural areas in the north, where the terrain is largely flat, producing almost circular viewsheds indicating that turbines are visible from all cardinal directions. The bottom panels show more urbanized areas, (e) in the south and (f) in the north.

Figure A-10 – Examples of Visually Exposed Areas. 6 km Viewshed (Green Area) of Wind Turbines (Yellow Points). Purple Polygons Indicate Residential Areas, Grey Areas are Municipalities.

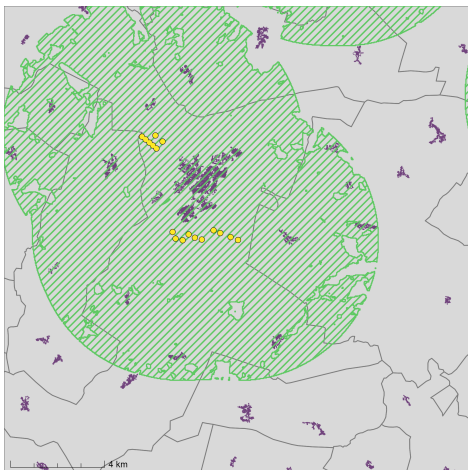
(a) Biederbach, Baden-Wuerttemberg



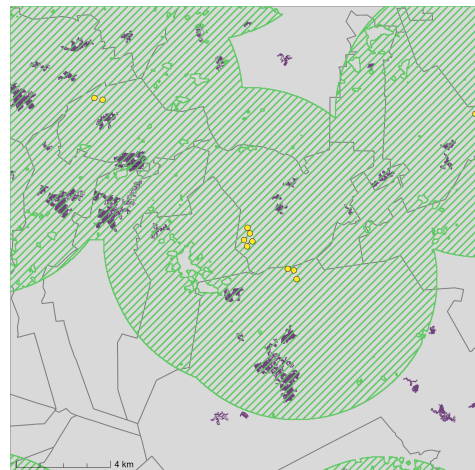
(b) Norath, Rhineland-Palatinate



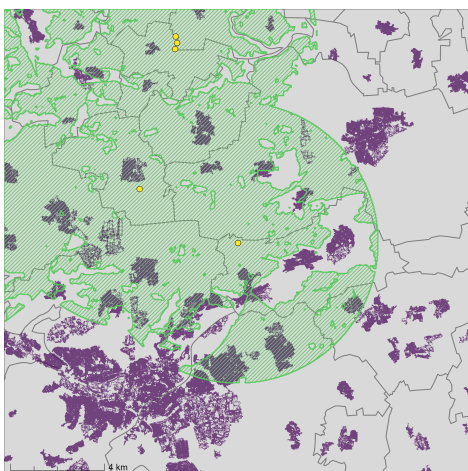
(c) Friedland, Mecklenburg–West. Pom.



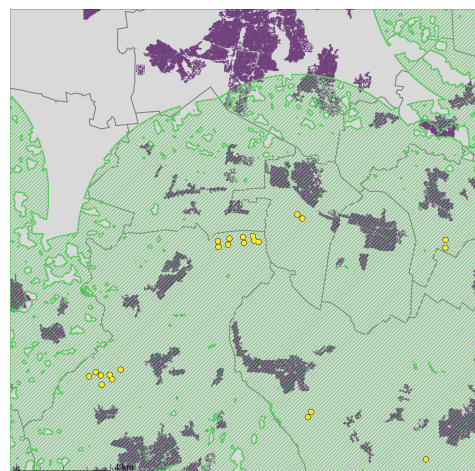
(d) Barver, Lower Saxony



(e) Elchingen, Bavaria



(f) Burgdorf, Lower Saxony



B Appendix: Extensions of the Theoretical Framework

B.1 Perception Updating

Let $v_i(k_i)$ be the perception of individual i on the impact of $\phi_{mr}w_r$ on $e_{imr}(\phi_{mr}, w_r, v_i(k_i))$, which depends on i 's information set k_i . The larger perception $v_i(k_i)$ is, the less disturbing individuals find the sight of wind turbines in their immediate surroundings. A citizens' initiative against turbine installations can provide new (subjective) information that decreases i 's perception ($\delta v_i / \delta k_i < 0$). Given that the negative impact of $\phi_{mr}w_r$ on e_{imr} increases with decreasing perception, the new information provided amplifies the negative change of regional wind turbines on the environmental good and therefore utility. Equation (B-1) represents the change of information k_i on individuals utility:

$$\frac{dU_{imr}}{dk_i} = \frac{\delta^2 e_{imr}}{\delta(\phi_{mr}w_r)\delta v_i} \frac{dv_i}{dk_i} \quad ((B-1))$$

In Section 6.3 in the main paper, we discuss how citizens' initiatives might shape the perception of wind turbines through (subjective) information campaigns, protest organization, and pursued lawsuits and run an additional estimation, testing for heterogeneity in opposition formation (subsection C.14). Moreover, planning processes can also influence sentiments toward these projects (Section 6.4 in the main paper).

B.2 Turbine Ownership Structure

Let $\zeta_{ir} \in [0, 1]$ be the share of regional wind turbines w_r in which individual i can participate financially. The budget constraint of i can therefore be extended to: $y_{imr}(\phi_{mr}, \zeta_{ir}, w_r) = x_i$. Thus, an additional positive income opportunity exists for i through participation in a cooperative investment ($\partial y_{imr} / \partial (\zeta_i w_r) \geq 0$). Since participation opportunities can vary between individuals within the same region ($\zeta_{ir} \neq \zeta_{jr}$), it might explain differences in support between individuals independent of visual exposure.

C Appendix: Additional Results

Here we provide supplementary information on the empirical results and the results of the validity and robustness tests.

C.1 Main Estimation Results

Table C-1 reports the estimation results of both estimation methods with the main specification of six kilometer treatment zone and two kilometers of buffer for the Baseline and External Exposure Model. These results are discussed in Section 5 in the main paper.

Table C-1 – Main DiD Estimates for Each Cohort

	Dependent Variable: Vote Share Green Party (%)	
	Base	External
Visible 2002	-0.06 (0.06)	-0.03 (0.07)
Visible 2005	0.14 (0.11)	0.20 (0.12)
Visible 2009	0.14 (0.20)	0.20 (0.23)
Visible 2013	-0.40 (0.17)	-0.50* (0.20)
Visible 2017	-0.04 (0.12)	-0.01 (0.14)
Visible 2021	-1.42** (0.50)	-1.73** (0.58)
Sub-Exp. $\times$ Period FE	Yes	Yes
Muni. $\times$ Sub-Exp. FE	Yes	Yes
Controls	Yes	Yes
Obs	11,656	9,170

^a Each cell reports the coefficient estimate with standard error in parentheses.

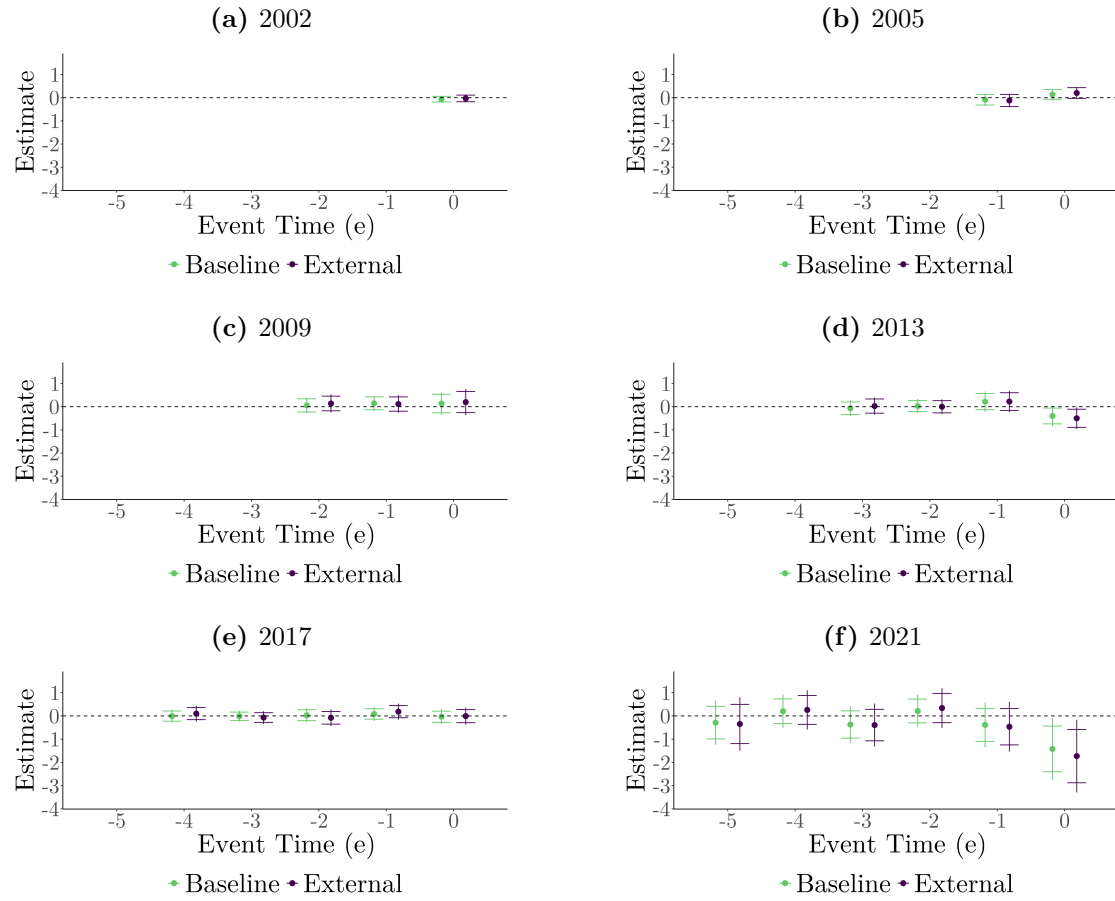
^b Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

^c Standard errors clustered at the municipal level.

C.2 Validity Check: False Treatment Timings

We shift the treatment timing to all possible pre-treatment election periods ($t < g$) with the number of periods before treatment denoted as e . This approach entails that, at an event time of $e = -5$, solely the the 2021 cohort is incorporated. Moreover, we cannot shift the treatment timing for the 2002 cohort, as we do not have two pre-treatment election period for any $e < 0$. [Figure C-1](#) reports the results for the Baseline and External Exposure Model, respectively. All coefficients for the false treatment timings are insignificant. The negative estimates for the 2021 cohort one election period before visual exposure ($e = -1$) could possibly be related to anticipation effects, discussed in more detail in [subsection C.11](#).

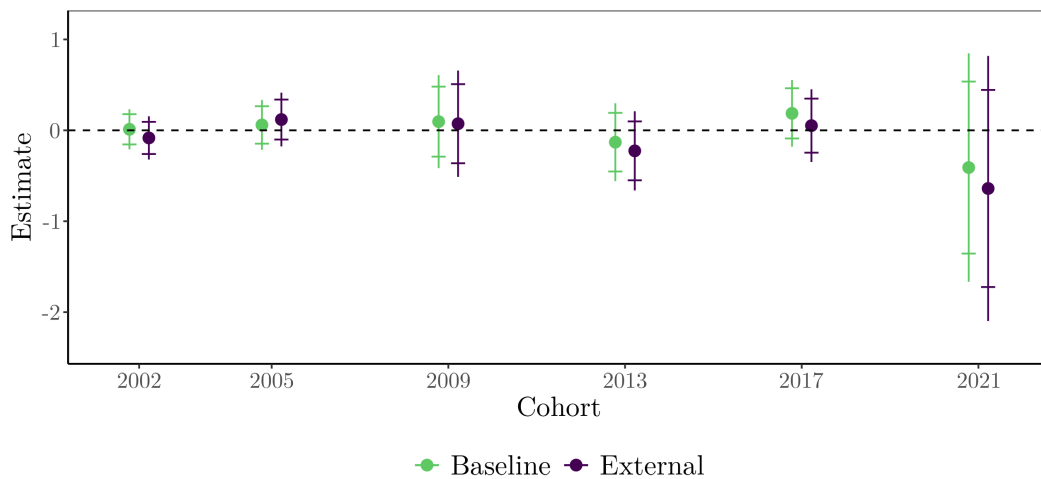
Figure C-1 – DiD Estimates With False Treatment Timings for Each Cohort: Outer Lines Represent 95% Simultaneous Confidence Bands, Inner Bars 95% Pointwise Confidence Intervals



C.3 Validity Check: Placebo Treatment Groups

As an additional falsification test, we construct placebo treatment groups using the remaining untreated municipalities. To reduce the likelihood that any estimated placebo effect is confounded by broader geographical disparities, we restrict these to municipalities within a 20 km radius that share the same urbanization status as the control group units of each cohort. All estimated effects are small in magnitude and statistically insignificant at conventional levels ([Figure C-2](#)).

Figure C-2 – DiD Estimates With Placebo Cohorts (False Treatment Groups): Outer Lines Represent 95% Simultaneous Confidence Bands, Inner Bars 95% Point-wise Confidence Intervals



C.4 Validity Check: Residential Sorting

It is conceivable that the electorate’s composition changes if individuals relocate after turbines become visible, i.e., some residents may face higher net costs from visual exposure than from moving. We assess this sorting channel by estimating the effects of visual exposure on in- and out-migration shares using the same empirical framework as in Section 4 in the main paper. The overall effect is small and statistically indistinguishable from zero for both in- and out-migration (Table C-2).

Table C-2 – Main DiD Estimates of in-, out- Migration Shares Across all Cohorts

	Dependent Variable: Vote Share Green Party (%)			
	Inflow		Outflow	
	Base	External	Base	External
Visible	-0.02 (0.08)	-0.01 (0.10)	0.05 (0.08)	0.04 (0.10)
Sub-Exp. × Period	Yes	Yes	Yes	Yes
FE				
Muni. × Sub-Exp.	Yes	Yes	Yes	Yes
FE				
Controls	Yes	Yes	Yes	Yes
Obs	11,640	9,154	11,640	9,154

^a Each cell reports the coefficient estimate with standard error in parentheses.

^b Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

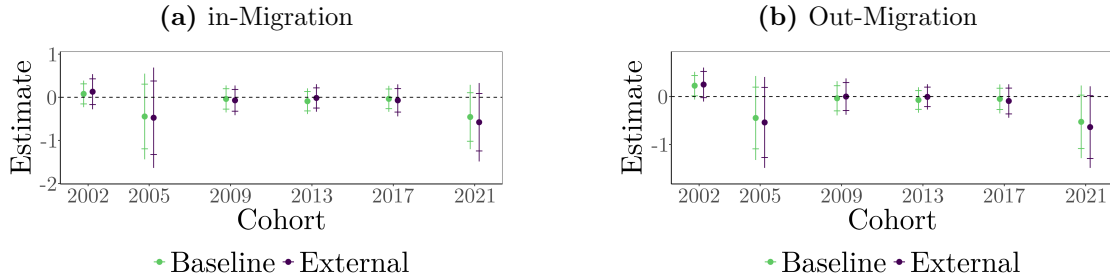
^c Standard errors clustered at the municipal level.

For the cohort specific effects, while not significant on any common level, declines are fairly large for out-migration for cohort 2005 and 2021, appearing contrary to canonical sorting mechanisms (Figure C-3).¹

¹For the 2002 cohort, we find weak evidence of higher gross migration, but the magnitude is small and does not translate into discernible changes in local support shares.

Yet, it is consistent with a housing market liquidity channel. As visible wind turbines are capitalized into lower house prices (e.g., [Gibbons \(2015\)](#), [Dröes and Koster \(2016\)](#)), liquidity decreases. When prices fall or sellers anchor to past (higher) valuations, time-on-market rises, matching slows, and transactions thin out ([Genesove and Mayer, 2001](#), [Ngai and Tenreyro, 2014](#), [Merlo and Ortalo-Magne, 2004](#)). In predominantly rural areas with high homeownership rates, such illiquidity can dampen both in- and out-migration in the short term. Overall, the (at most) slight decline in gross migration for more recent cohorts suggest that moving is unlikely to be a meaningful mechanism behind the decline in local support we estimate.

Figure C-3 – DiD Estimates of in-, out- Migration Shares for Each Cohort: Outer Lines Represent 95% Simultaneous Confidence Bands, Inner Bars 95% Pointwise Confidence Intervals

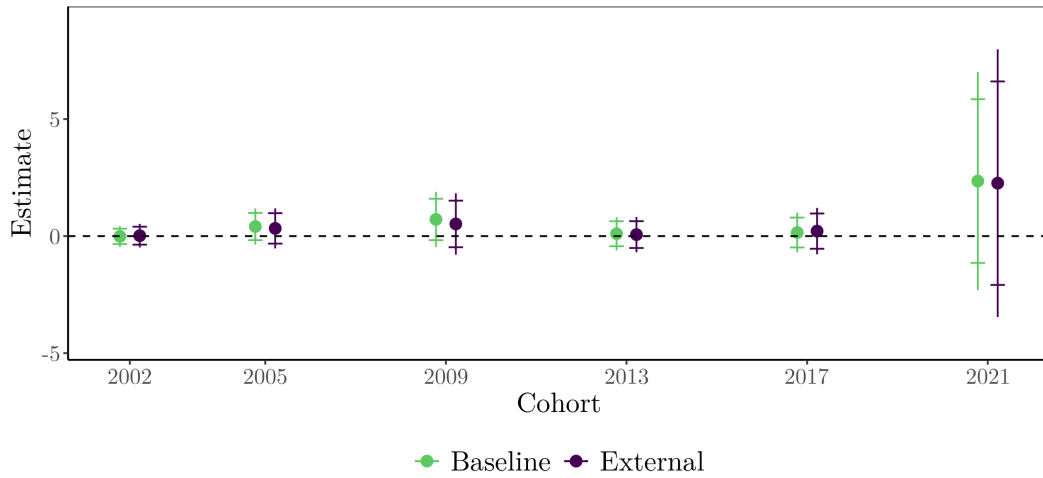


C.5 Validity Check: Effect on Turnout

Next to a decrease in governing party's vote share in reaction to local wind turbine installations, [Stokes \(2016\)](#) also estimates an increase in voter turnout by 2%, arguing that the projects and public opposition might have mobilized former non-voters. Thus, one mechanism might be a change in the vote composition rather than a change in attitudes of existing voters. To investigate if this might be one of the driver of our results, we repeat the estimation exercise in Section 5 in the main paper with the share of casted votes out of the population eligible to vote as outcome. Most coefficients are positive, but only the coefficients for the 2005 cohort are significant based on pointwise confidence intervals ([Figure C-4](#)). As we do not estimate any negative effects of visible wind turbines for this cohort (the point estimates are even positive), we conclude that some mobilization may have taken place in certain areas, providing suggestive but inconclusive evidence of such a mechanism.²

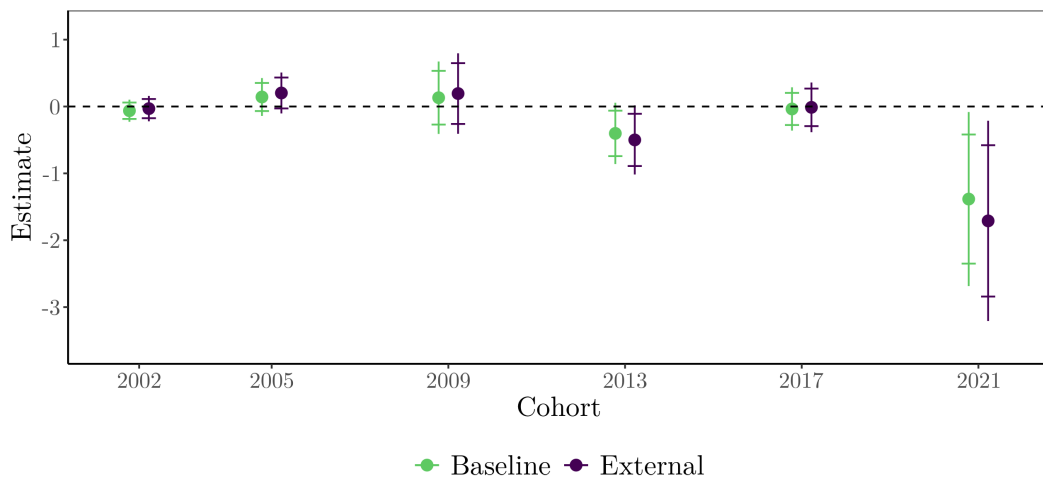
²While we find no large effects on overall turnout, this does not rule out potential changes in voter composition. However, turnout research in Germany indicates that the sociodemographic composition of the electorate has remained largely stable across elections ([Der Bundeswahlleiter, 2022](#)).

Figure C-4 – DiD Estimates With Turnout (%) as Outcome: Outer Lines Represent 95% Simultaneous Confidence Bands, Inner Bars 95% Pointwise Confidence Intervals



Furthermore, we include turnout as an additional control variable, finding minimally smaller estimates, supporting that changes in turnout play a minimal role (Figure C-5).

Figure C-5 – DiD Estimates With Turnout (%) as Additional Covariate: Outer Lines Represent 95% Simultaneous Confidence Bands, Inner Bars 95% Pointwise Confidence Intervals



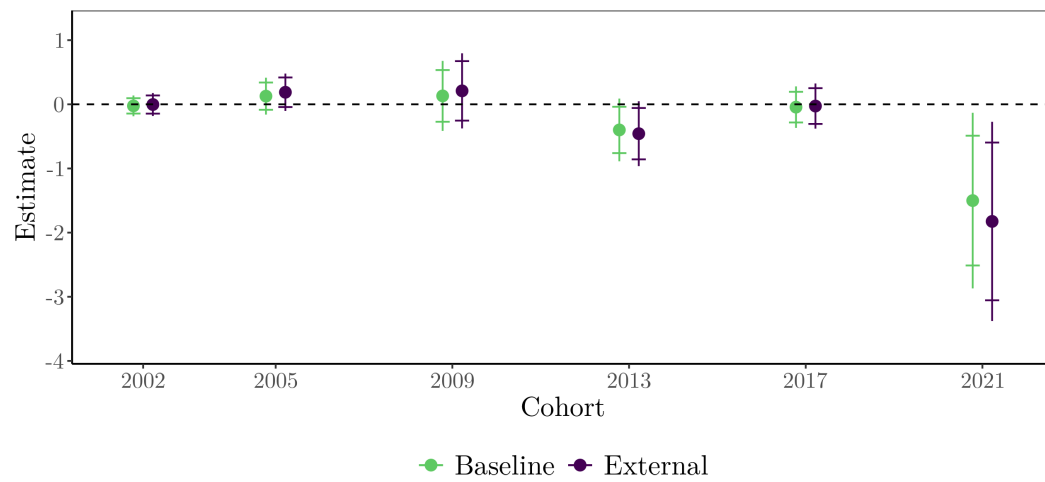
C.6 Robustness Check: Alternative Estimator

Including covariates linearly comes at the cost of making strong assumptions about their relationships. We also estimate the effect semi-parametrically using the Doubly Robust estimator (*DR*) proposed by Sant’Anna and Zhao (2020), which is based on kernel outcome regression (Heckman et al., 1997) and inverse probability weighting (Abadie, 2005). It has become a widely used method in staggered DiD settings (e.g., Alexander and Karger (2023), Brynjolfsson et al. (2023)) and is generally more robust to model misspecifications while conditioning on pre-treatment covariate levels. Given the generalized propensity score $p_g(X_{t=g-4})$, i.e., the probability of being first treated at $t = g$ conditional on covariates at $t = g - 4$, and the population outcome regression for the control group of cohort g , denoted by $a_g = \mathbb{E}[S_{t=g} - S_{t=g-4} | X_{t=g-4}, D_t = 0, G_g = 0]$, the *DR* estimator ($ATT(g) = \beta_g^{DR}$) is given in Equation (C-1):

$$\beta_g^{DR} = \mathbb{E} \left[\left(\frac{G_g}{\mathbb{E}[G_g]} - \frac{\frac{p_g(X_{t=g-4})(1-D_t)(1-G_g)}{1-p_g(X_{t=g-4})}}{\mathbb{E} \left[\frac{p_g(X_{t=g-4})(1-D_t)(1-G_g)}{1-p_g(X_{t=g-4})} \right]} \right) (S_{t=g} - S_{t=g-4} - a_g) \right] \quad ((C-1))$$

The results are almost identical to the OLS estimates, with a slightly stronger effect for the 2021 cohort significant at the 5 % level, while the effect for the 2013 external exposure result is slightly weaker (Figure C-6).

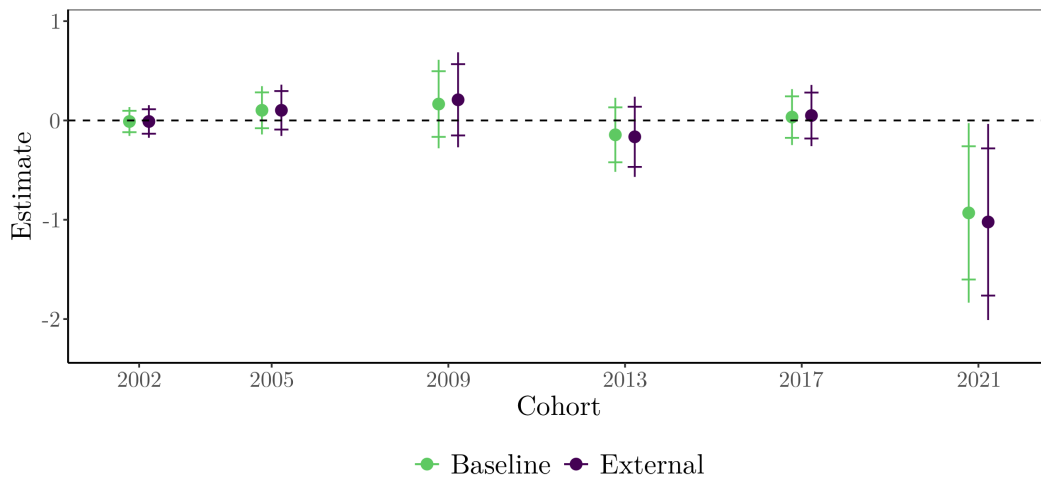
Figure C-6 – DiD Estimates Employing the Doubly Robust Estimator: 95% Simultaneous Confidence Bands, Inner Bars 95% Pointwise Confidence Intervals



C.7 Robustness Check: 8 km Treatment Zone

As discussed in Section 4.1 in the main paper, the distance from which a turbine is perceived as intrusive is unclear and varies among observers. To test the sensitivity of the main results to changes in the treatment zone cut-off, we increase the maximum distance of turbine visibility. In the alternative specification, we also include municipalities only visually exposed in the buffer zone, i.e., we increase the treatment zone to eight kilometer. The point estimates are smaller and only significant for the 2021 cohort, suggesting a decreasing effect with increasing distance (Figure C-7).

Figure C-7 – DiD Estimates With 8 km Treatment Zone: Outer Lines Represent 95% Simultaneous Confidence Bands, Inner Bars 95% Pointwise Confidence Intervals



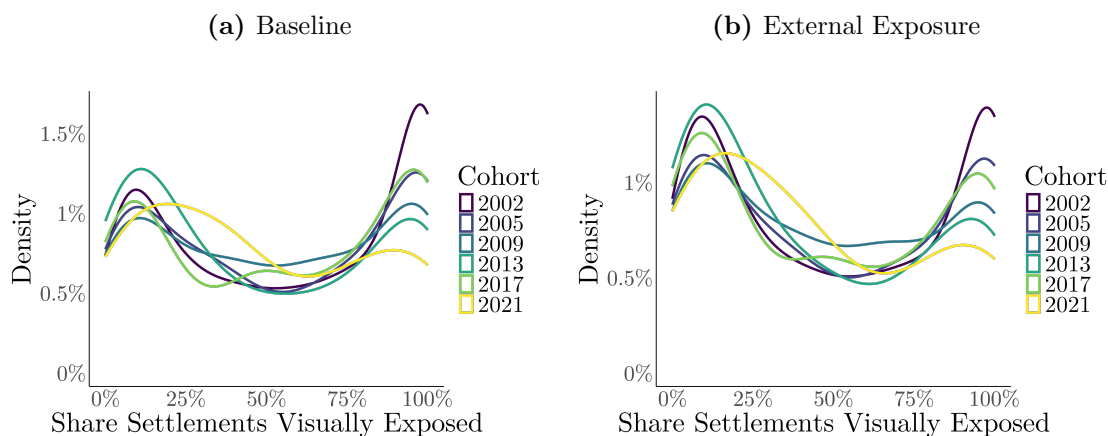
C.8 Robustness Check: Continuous Treatment Dynamics

In Section 4.1 in the main paper, we discuss why we use a binary treatment variable. The choice is a tradeoff between how well this variable can represent the distribution of visual exposure across different intensities (which has many dimensions) and the underlying assumption we have to make in the empirical strategy, given that assuming parallel trends in potential untreated outcomes is not sufficient in continuous difference-in-difference designs ([Callaway et al., 2024](#)). In this section, we test whether the effect is sensitive to different exposure intensities.

As we discuss in Section 4.1 in the main paper, computing individual level panoramic exposure is infeasible with municipal aggregates. As a tractable proxy, we use the share of residential area that is visually exposed (at any intensity). Intuitively, if more residents can see turbines from home, the municipality-wide impact might grow roughly linearly with this share. To probe how effects vary with exposure intensity, we estimate a cubic B-spline dose response for each cohort, following [Callaway et al. \(2024\)](#).³

³For simplicity, we do this unconditional of covariates, as this is not considered in [Callaway et al. \(2024\)](#).

Figure C-8 – Density Distribution of the Share of Residential Areas Visually Exposed per Cohort



For municipalities first visually exposed in the 2021 election cycle, the estimated negative effect increases as the exposed share rises, consistent with intuition. By contrast, for the 2013 cohort, where our binary treatment main specification yields minor statistically significant effects, estimates exceed zero for doses around 60% - 75%. This pattern is plausibly due to sparse support in that range: as shown in [Figure C-8](#), few municipalities exhibit this exposure level. Given that municipalities are heterogeneous, a few outliers might pull the local slope upward away from the overall trend, possibly leading to the divergence from the binarized main summary estimation.

Figure C-9 – Baseline Model B-Spline Continuous DiD Estimates for Each Cohort with 95% Simultaneous Confidence Bands

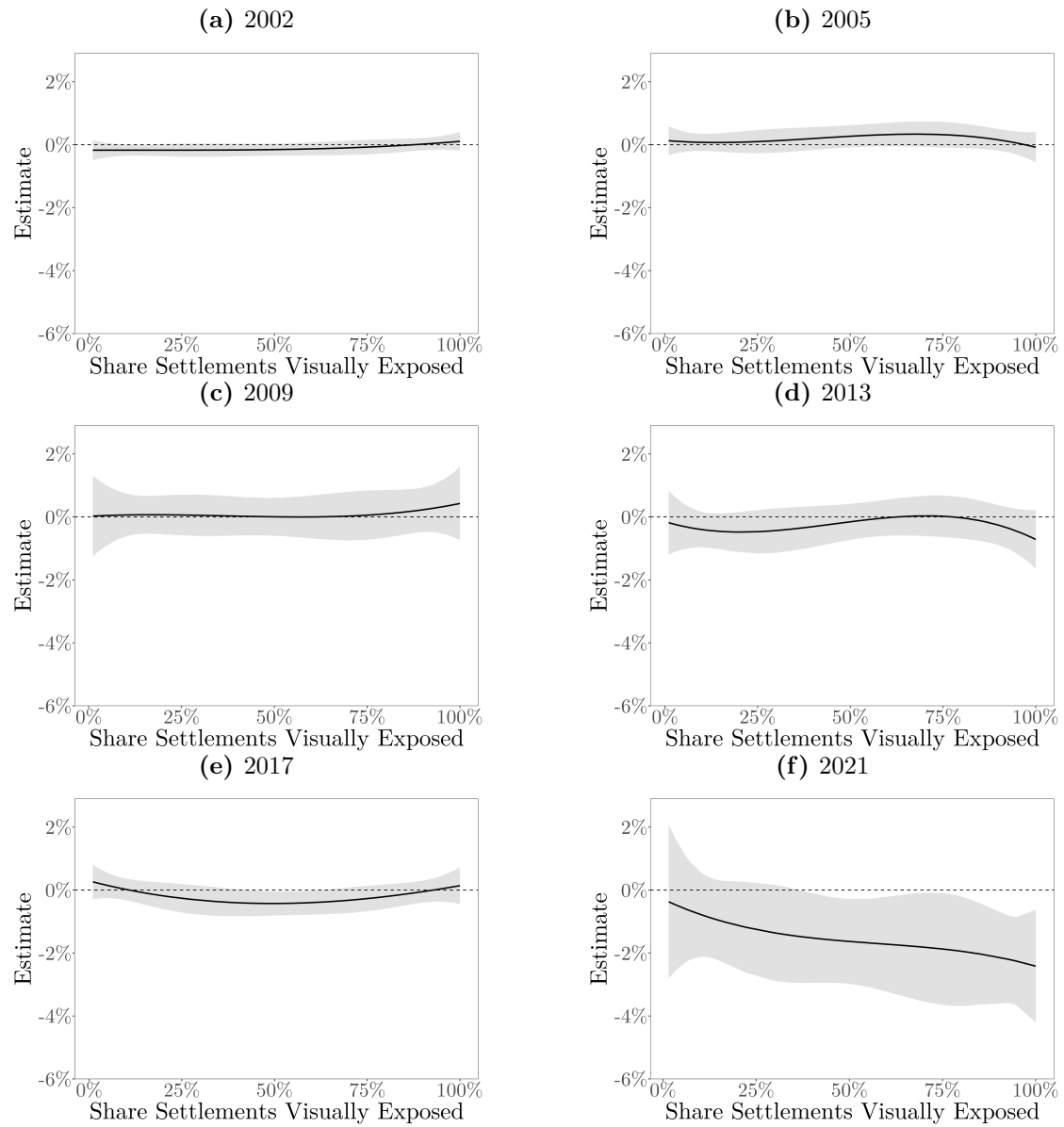
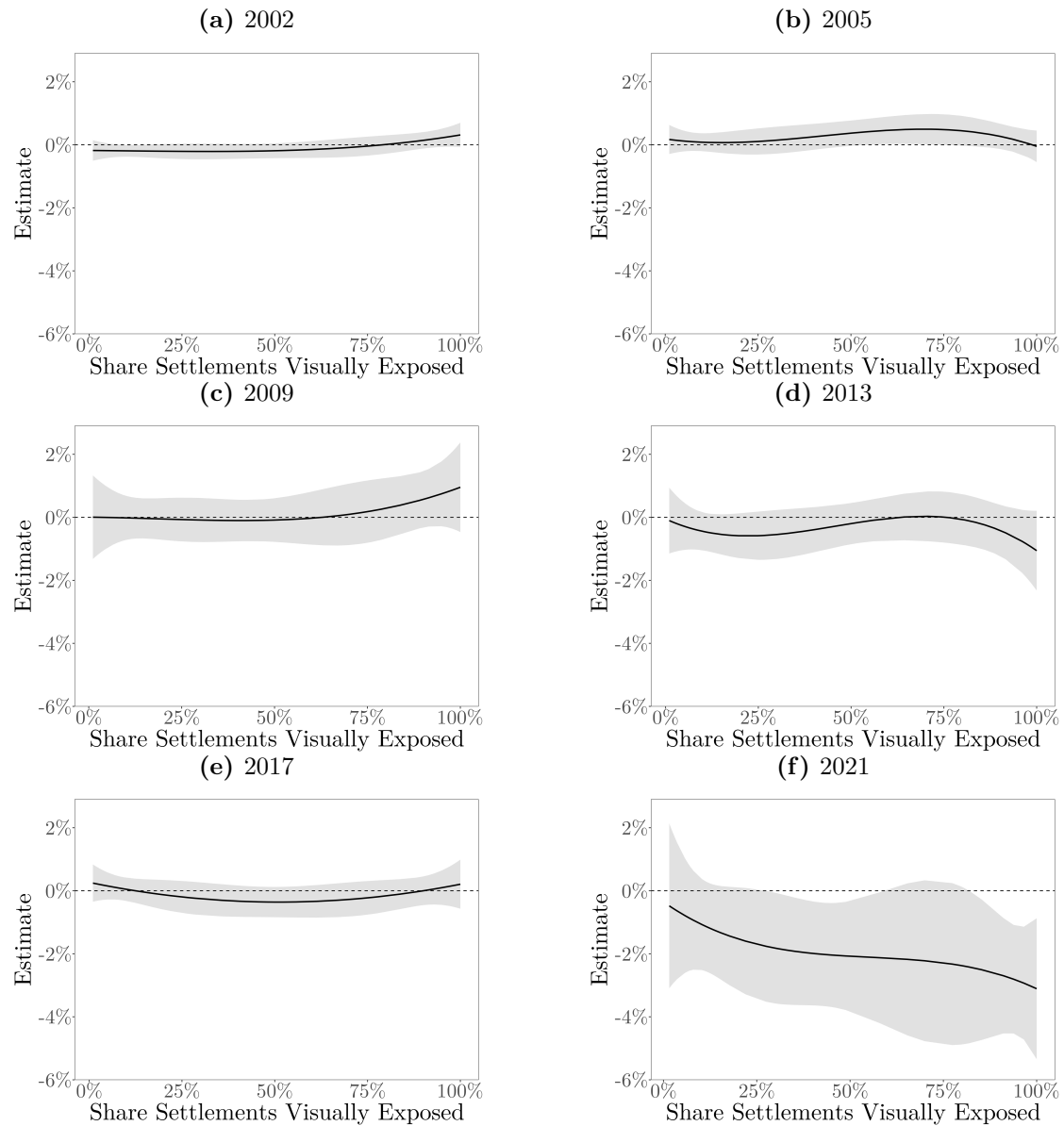


Figure C-10 – External Exposure Model B-Spline Continuous DiD Estimates for Each Cohort with 95% Simultaneous Confidence Bands

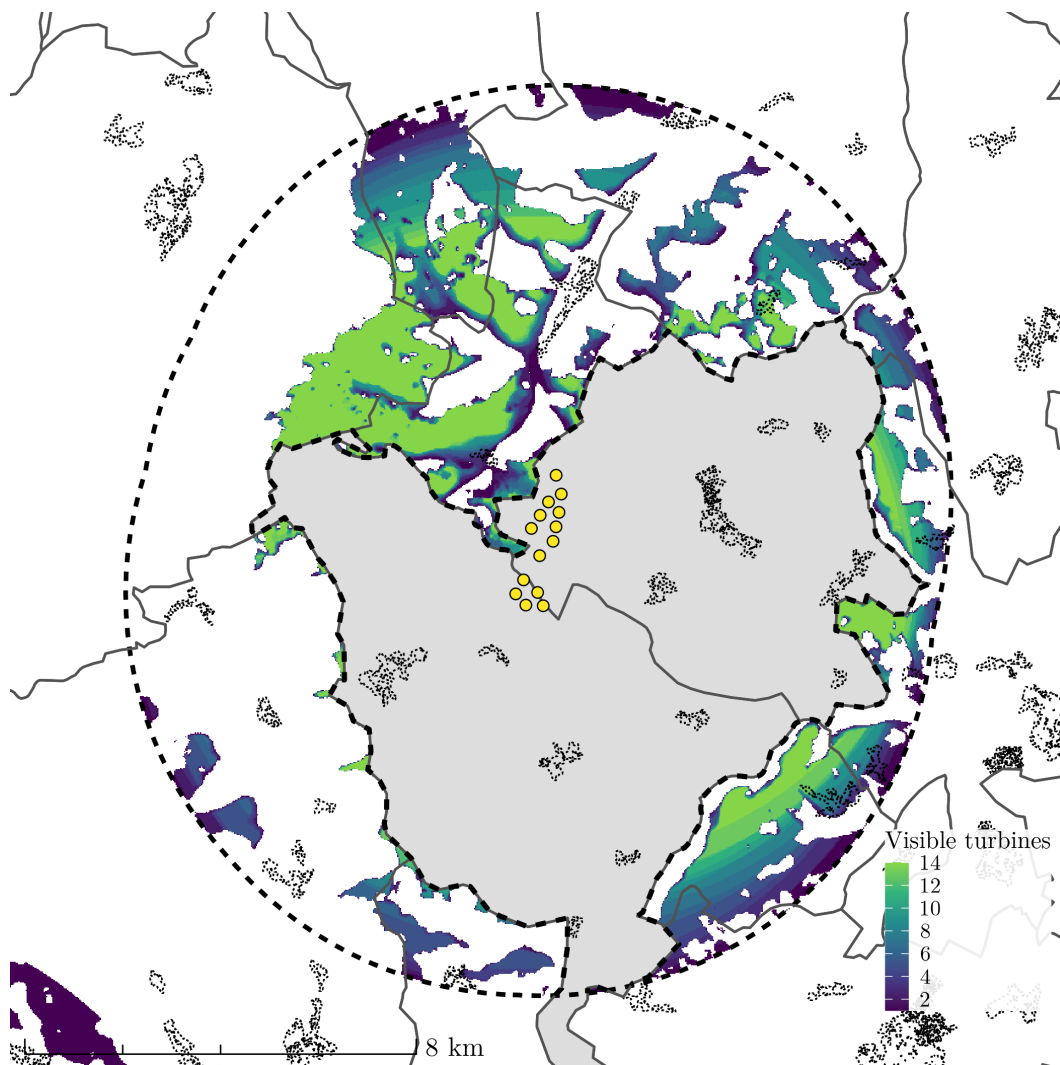


C.8.1 Effect of Mean Number of Turbines Visible

Individual wind turbines are rarely installed as stand alone projects. They are typically developed as wind farm projects with multiple turbines. Within such

projects, compensation arrangements and other project specific conditions for nearby residents are likely to be highly similar. Although our data do not contain a direct project identifier, we approximate projects by grouping turbine clusters that were constructed within the same election period and lie no more than 2 km apart. We then focus on non-hosting municipalities located within the 8 km buffer around the projects. These municipalities have residential areas at broadly comparable distances to the project turbines, but they can still differ in visual exposure share and number of turbines visible because of local topography and other geographic conditions.

Figure C-11 – Example Hosting Municipalities Simmersfeld and Seewald and the Number of Turbines Visible Around Them



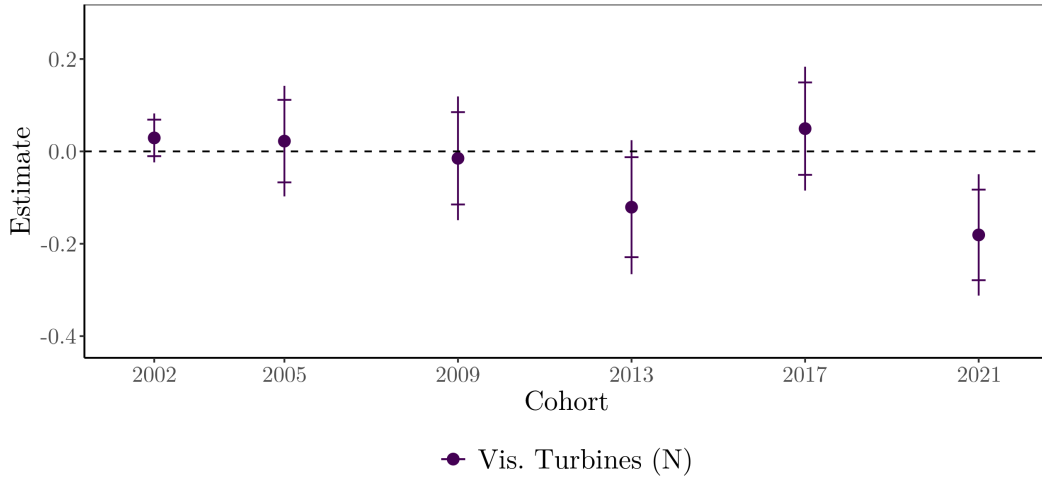
We exploit this variation by replacing the binary treatment indicator with the mean number of project turbines visible from residential areas, allowing us to estimate a dose-response relationship rather than a simple exposed versus unexposed comparison. Because projects are defined within election periods, and as we include municipality and project-by-election-period fixed effects (λ_{pt}), identification comes from comparisons among non-hosting municipalities surrounding the same

project (Equation (C-2)).⁴

$$S_{mpt} = \mu_m + \lambda_{pt} + \sum_h \beta_h^0 VIS_{mpt} \cdot \mathbf{1}\{g_p = h\} + \sum_h \mathbf{1}\{g_p = h\} X'_{mpt} \gamma_h + \epsilon_{mpt}, \quad ((C-2))$$

Now, β_h^0 identifies for each cohort the immediate effect of the average number of turbines becoming visible from residential areas. The patterns of the results mirror the main specification, showing a decline in vote share for the 2013 cohort of 0.12 percentage points significant on a ten percent level and for the 2021 cohort -0.18 percentage points, significant on a one percent level for each additional visible turbine, while they stay insignificant for the other cohorts.

Figure C-12 – Project Surrounding DiD Estimates for Each Cohort: Outer Lines Represent 95% Simultaneous Confidence Bands, Inner Bars 95% Pointwise Confidence Intervals

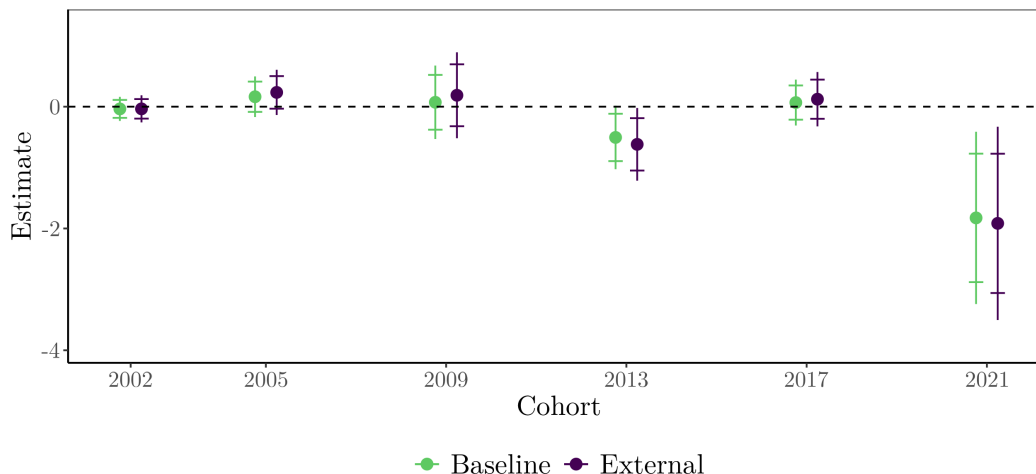


⁴In case multiple projects are installed within 8 km from a municipality, we assign its fixed effect to the closest project

C.9 Robustness Check: Restricting Matches to Within-County Pairs and Distance Calipers

As we have argued in Section 4.1 in the main paper, within-county control municipalities should serve as the most credible counterfactuals for voting outcomes due to their close proximity to their treated counterparts and administrative similarities. Moreover, rural constituencies in Germany are often similar to their respective counties, within which the first vote elects the candidate for the national parliament. While we use the second vote as our outcome variable, which represents the direct vote for a party, individual candidates may influence this vote as well. In addition to potential systematic differences in matched pairs located farther apart, comparing municipalities with different candidates may therefore bias the results. To test whether the estimates are influenced by control municipalities outside the treated municipality's county, we repeat the estimation on the subset of pairs located within the same county, which account for 73% in the Baseline Model and 79% in the External Exposure Model ([Figure A-3](#)).

Figure C-13 – DiD Estimates With Only Within-County Pairs: Outer Lines Represent 95% Simultaneous Confidence Bands, Inner Bars 95% Pointwise Confidence Intervals



The results (Figure C-13) are very similar to the main estimation, showing somewhat larger negative effects for the 2013 cohort (-0.51 and -0.52 for the Baseline Model, -0.67 and -0.63 for the External Exposure Model), with the latter now statistically significant at the 5% level. The External Exposure estimates for the 2021 cohorts are slightly smaller (-1.52 and -1.48), while the Baseline Model results are nearly identical (-1.44 and -1.34).

In addition to restricting matches to within the same county (subsection C.9), we also limit the maximum matching distance using different calipers (25, 35, and 45 km). The effect size decreases as the caliper widens, while the precision increases with the growing sample size (Figure C-14, Figure C-15, Figure C-16).

Figure C-14 – DiD Estimates With 25 km Distance Caliper: Outer Lines Represent 95% Simultaneous Confidence Bands, Inner Bars 95% Pointwise Confidence Intervals

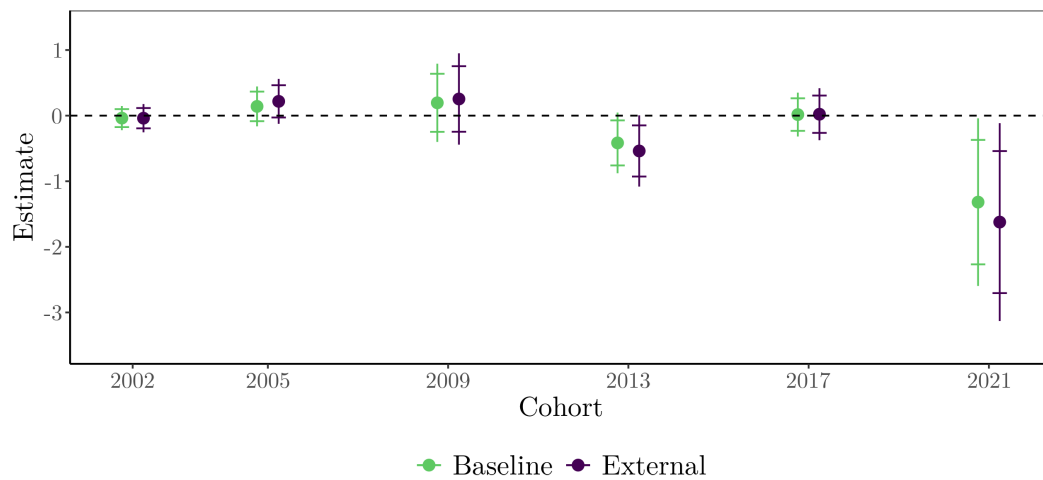


Figure C-15 – DiD Estimates With 35 km Distance Caliper: Outer Lines Represent 95% Simultaneous Confidence Bands, Inner Bars 95% Pointwise Confidence Intervals

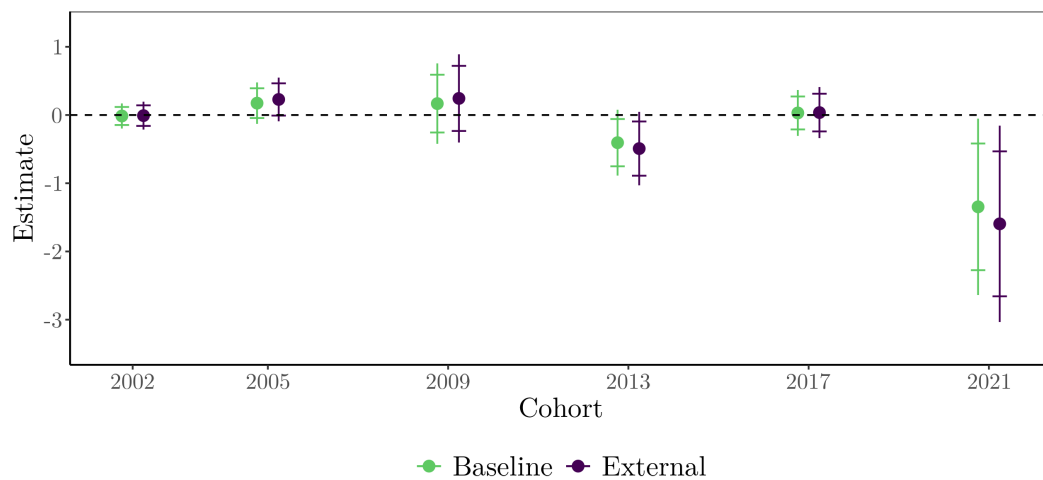
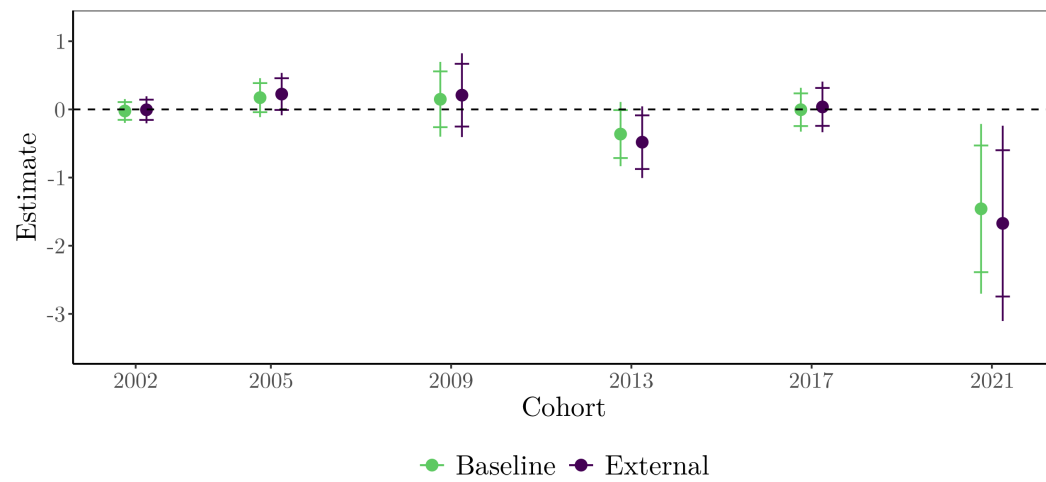


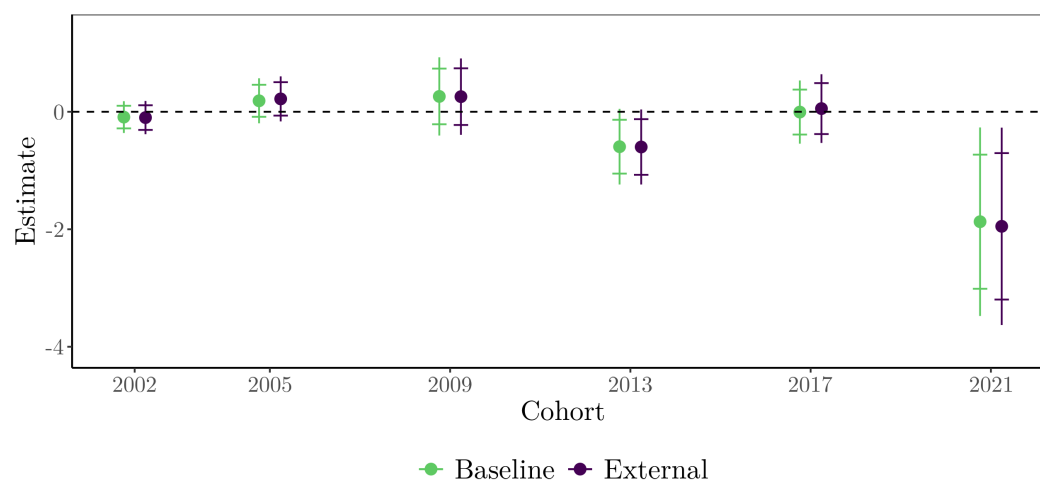
Figure C-16 – DiD Estimates With 45 km Distance Caliper: Outer Lines Represent 95% Simultaneous Confidence Bands, Inner Bars 95% Pointwise Confidence Intervals



C.10 Robustness Check: Alternative Matching Procedures

To test whether the results in Section 5 in the main paper are robust to changes in the matching procedure, we match a control group for each cohort with replacement. For each visually exposed municipality, we find the geographically closest control municipality that is not visually exposed at that time, has the same urbanization status, and has no identified citizens' initiatives. Now, several treatment municipalities can be matched to the same control municipality, which we weight according to the frequency of assignment. The results are similar to the ones in the main specification, with larger point estimates (Figure C-17), providing evidence that the results are not driven by the matching procedure.

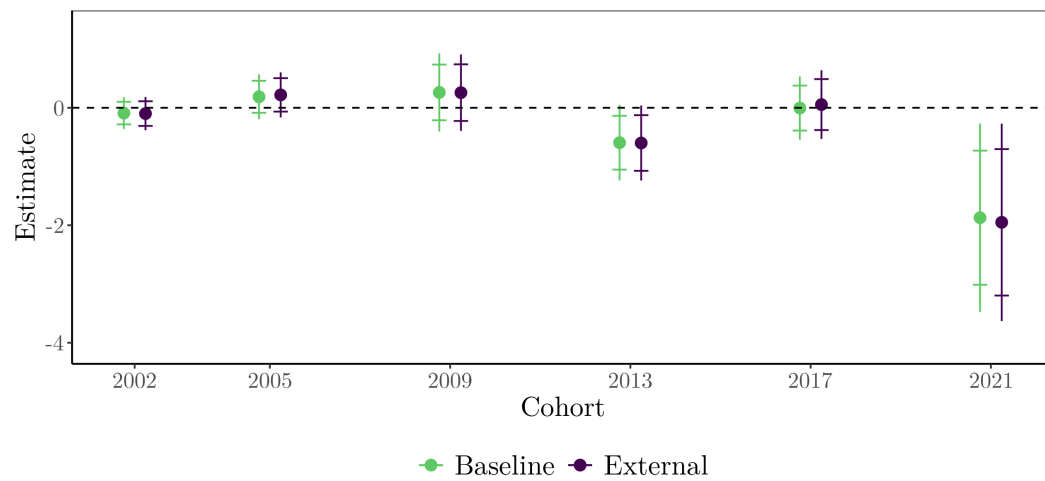
Figure C-17 – DiD Estimates via Matching With Replacement (1:1): Outer Lines Represent 95% Simultaneous Confidence Bands, Inner Bars 95% Pointwise Confidence Intervals



In addition, we double the number of matches for each treated municipality, allowing additional variation in the control group but lower the group balance as worse (more distant) matches enter the control group. Again, the trend is

comparable to the main specification, but the point estimates are smaller and less significant for the 2013 cohort (Figure C-18).

Figure C-18 – DiD Estimates via Matching With Replacement (2:1): Outer Lines Represent 95% Simultaneous Confidence Bands, Inner Bars 95% Pointwise Confidence Intervals



C.11 Robustness Check: Accounting for Anticipation

Section 6.4 in the main paper outlines that the average duration from planning to installation nearly doubled within the 2021 election period to almost four years. Since these projects are publicly announced in the planning phase, inhabitants in municipalities of the 2021 cohort could have already anticipated visual exposure in the pre treatment period. Indications of this can be found in the pre-trends in [Figure A-6](#) and the falsification test in [subsection C.2](#), with a negative estimate one election period before the visual exposure. To account for this, we shift the pre-treatment period for this group to $t = g - 8$, i.e. two periods before the visual exposure. Accounting for anticipation implies the assumption that parallel trends must persist over a longer time frame, but mitigates potential biases due to voter reactions prior to treatment.

Table C-3 – 2021 DiD Estimates for Each Timing Group Accounting for Anticipation

	Dependent Variable: Vote Share Green Party (%)	
	Base	External
Visible 2021	-1.81*** (0.49)	-2.19*** (0.58)
Sub-Exp. $\times$ Period FE	Yes	Yes
Muni. $\times$ Sub-Exp. FE	Yes	Yes
Controls	Yes	Yes
Obs	200	156

^a Each cell reports the coefficient estimate with standard error in parentheses.

^b Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

^c Standard errors clustered at the municipal level.

The results are a further indication that individuals in the 2021 cohort may have already adjusted their perceptions in the previous election period due to the

anticipation of visual exposure, as the point estimates are considerably lower than in the main analysis while significance increases ([Table C-3](#)).

C.12 Robustness Check: Alternative Measurements of Support

While the most complete and reliable data on party affiliation come from official election data, there may be municipal-specific effects that we cannot control for or that remain undetected in administrative aggregation. To address this, we also use individual level Green Party preference responses from the Green-SOEP survey as the dependent variable. By controlling for covariates and matching with non-visually exposed control individuals within the same county and in municipalities with the same urbanization status, we estimate the effect using the methods presented in Section 4 in the main paper. As with the other analyses based on Green-SOEP data, we can only estimate the effect for the last two cohorts via Difference-in-Differences due to data coverage limitations. The results align with the findings of our main analysis ([Table C-1](#)), indicating a significantly negative impact for the last cohort at the 10% level (pointwise only), while showing no effect for participants living in municipalities that were first visually exposed during the 2017 election period ([Figure C-20](#)).

Figure C-19 – DiD Estimates of the Effect on Green Party Preference Based on Green-SOEP Survey Responses: Outer Lines Represent 95% Simultaneous Confidence Bands, Inner Bars 95% Pointwise Confidence Intervals



As discussed in Section 3.3 in the main paper, there is a strong connection between preferences for the renewable energy transition and support for the German Green Party. To evaluate whether our results extend to alternative proxies for support for the renewable energy transition, we also use responses regarding opinions on subsidizing renewable energy production as the outcome variable. Unfortunately, this question was only asked in the first five waves (2012 to 2015), which limits us to estimate potential impacts per cohorts by employing a simple fixed effects model across all respondents using the Green-SOEP data with the opinions on subsidizing renewable energy production as the dependent variable for all cohorts except the last one, as this election period starts in 2018 (Equation (C-3)).

$$SUBSI_{imt} = \mu_i + \lambda_t + \sum_{g=1998}^{2017} (G_g * \mathbf{1}\{g \leq t\}) \beta_g^{FE} X'_{it} \gamma + \epsilon_{it} \quad ((C-3))$$

The numeric variable $SUBSI_{imt}$, with its five levels (from 1, "strong rejection,"

to 5, "strong approval"), reflects the intensity of support expressed by individual i in a survey wave falling within election period t . G_g denotes the election period during which the municipality m , where i resides, becomes visually exposed, while individual characteristics X_{it} are the individual's employment status, sex, income, number of children in the household, whether the individual is married, household size, property ownership, and age. It is important to stress that the coefficient of interest, β_g^{FE} , should not be interpreted as causal, as we cannot conduct a clean comparison of changes in approval between treated and control units. Nonetheless, the complementary regression coefficients support the main results with insignificant coefficients for all cohorts except treated during the 2013 election period, where visual exposure is associated with a significant decrease in approval at the 5% level (Table C-4).

Table C-4 – Regression Results Based on the Responses of the Green-SOEP Survey

Dependent Variable: Model Model:	Approval Renewable Energy Subsidization Baseline (1)	External Exposure (2)
<i>Variables</i>		
Visually Exposed 1998	0.23 (0.26)	-0.08 (0.46)
Visually Exposed 2002	-0.11 (0.22)	-0.19 (0.25)
Visually Exposed 2005	-0.02 (0.27)	0.25 (0.25)
Visually Exposed 2009	-0.29 (0.27)	-0.16 (0.36)
Visually Exposed 2013	-0.70** (0.29)	-0.75** (0.38)
Visually Exposed 2017	0.07 (0.09)	0.07 (0.09)
Unemployed	-0.20*** (0.04)	-0.18*** (0.04)
Female	0.58 (0.85)	2.6* (1.5)
HH Income Level	-0.01 (0.01)	-0.01 (0.01)
University Qualification	-0.03 (0.06)	-0.003 (0.07)
N Children in HH	-0.006 (0.03)	-0.01 (0.03)
Married	0.06 (0.07)	0.07 (0.08)
HH Size	0.03 (0.02)	0.03 (0.03)
Property Owner	0.11 (0.10)	0.13 (0.10)
Age	0.02 (0.02)	0.03 (0.03)
<i>Fixed-effects</i>		
ID	Yes	Yes
Survey Wave	Yes	Yes
<i>Fit statistics</i>		
Observations	19,220	16,452
Adjusted R ²	0.57844	0.57457

Standard-errors are clustered on municipal level

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

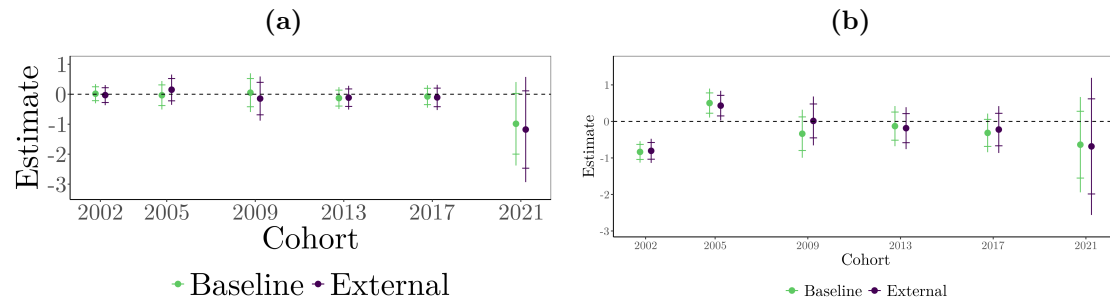
C.13 Heterogeneity Analysis: Voter Weights and Rurality

In addition to the pooled voter-weighted estimation and the rural interaction in Section 6.2 in the main paper, we also estimate cohort-specific effects using Equation (C-4), where η_h^0 captures the additional effect of rurality.

$$S_{mgt} = \mu_{mg} + \lambda_{gt} + \sum_h \beta_h^0 VIS_{mgt} \cdot \mathbf{1}\{g = h\} + \eta_h^0 (VIS_{mgt} \cdot RURAL_m) \cdot \mathbf{1}\{g = h\} + \sum_h \mathbf{1}\{g = h\} X'_{mgt} \gamma_h + \epsilon_{mgt} \quad ((C-4))$$

The voter-weighted estimates are insignificant across all cohorts. The interaction effects, however, are more heterogeneous. For the 2002 and 2021 cohorts, the estimates indicate a sizable difference in the negative effect between rural and non-rural areas, while for the remaining cohorts this difference is comparatively small. For the 2005 cohort, by contrast, the estimated effect in rural areas is significantly more positive than in non-rural areas. Although any interpretation remains speculative, it is notable that the gap in the median number of voters between rural and non-rural municipalities is smallest in the 2005 cohort, while it is largest in the 2002 and 2021 cohorts.

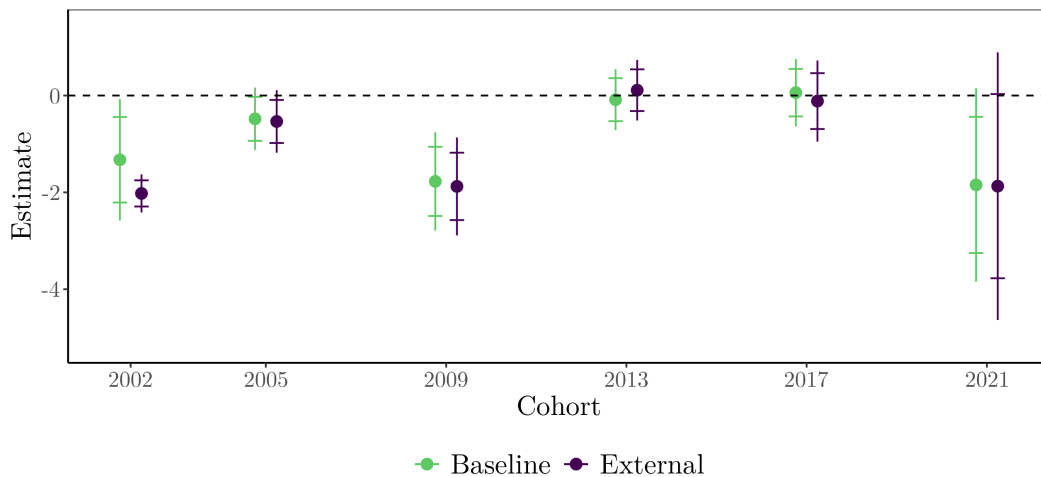
Figure C-20 – (a) Weighted Main DiD Estimates. (b) Rurality Interaction Effect Estimates. Outer Lines Represent 95% Simultaneous Confidence Bands, Inner Bars 95% Pointwise Confidence Intervals



C.14 Heterogeneity Analysis: Impact of Organized Opposition

In Section 6.3 in the main paper, we discuss and estimate the impact of citizens' initiative formations in visually exposed areas on support. As described, we interact the post-treatment indicator with a binary variable equal to one if a citizens' initiative was organized in the pre-treatment period (eq. (7) in the main paper). We estimate negative effects for most cohorts, ranging between -0.47 and -2.03 percentage points, while the 2013 and 2017 results are close to zero. The 2002 and 2009 cohort estimates, as well as the baseline for the 2021 cohort, are significant at the five-percent level based on simultaneous confidence bands, whereas the 2005 results are significant only based on pointwise confidence intervals.

Figure C-21 – Estimates With the Formation of a Citizens' Initiative Indicator Interaction for Each Cohort: Outer Lines Represent 95 % Simultaneous Confidence Bands, Inner Bars 95 % Pointwise Confidence Intervals

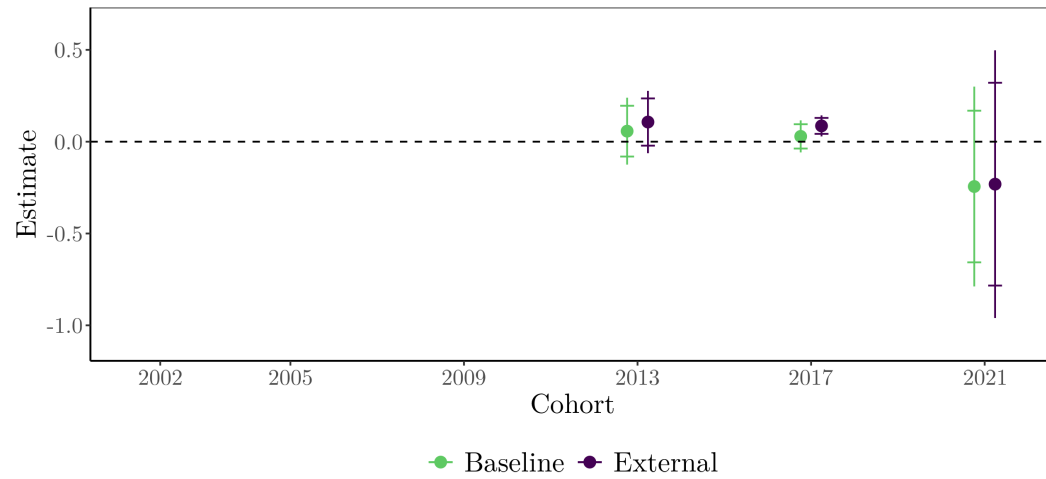


C.15 Heterogeneity Analysis: Impact of Participation Opportunities

In this section, we further examine the influence of cooperative turbine projects as a means of participation opportunities discussed in Section 6.5 in the main paper and report the cohort-specific results. When estimating the interactions separately by cohort, a more nuanced pattern emerges. The 2013 and 2017 cohort coefficients are positive (ranging from 0.06 to 0.11 and 0.03 to 0.09 percentage points, respectively, per additional cooperative turbine nearby), with statistical significance only for the externally exposed group in 2017, while the effect turns negative for the 2021 cohort ([Figure C-22](#)).

Two factors may have contributed to this result. First, the debate surrounding the tendering reform, which unfolded between the 2017 and 2021 election periods, may have diminished residents' perceived opportunities for meaningful participation (Section 6.5 in the main paper). Second, and more trivially, because cooperative installations declined sharply after the first auction rounds, the 2021 cohort includes only nine (Baseline) and seven (External Exposure) visually exposed municipalities located near cooperative turbines, making these estimates prone to small-sample bias. Therefore, these results should be interpreted with caution and not be overemphasized.

Figure C-22 – Estimates with Number of Cooperative Turbines Interaction for Each Cohort: Outer Lines Represent 95% Simultaneous Confidence Bands, Inner Bars 95% Pointwise Confidence Intervals



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