Headwind at the Ballot Box? - The Effect of Visible Wind Turbines on Green Party Support

Melanie Krause* Vincent Stegmaier†

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Abstract

Whether pro-renewable political parties win or lose at the ballot box when wind turbines are built near voters' homes is still not well understood, particularly with regard to voter motivation and channels of influence. We contribute by using new fine-grained data on the location of wind turbines in Germany to determine the visual exposure of residential areas to wind turbines. This allows us to estimate the change in the vote share for the German Green Party after voters see a wind turbine from their neighborhood for the first time. In most election periods, we find no significant effect of visible wind turbines on the Green Party vote share, suggesting that voters did not change their support for pro-renewable policies. Yet, for municipalities first visually exposed in the 2017 and 2021 election period, we find a negative effect. In these municipalities, a growing number of citizens' initiatives have emerged prior to construction, indicating that wind energy expansion is expanding to less supportive areas where strong opposition has formed. With the exception of two legislative periods from 1998 to 2005, the party had little influence on fundamental expansion strategies and hardly any on local site decisions, implying a shift in the general attitude towards the expansion of renewable energies, rather than a punishment effect. The negative effect of visual exposure decreases with increasing proximity, but does not increase with the number of visible turbines.

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*Leipzig University, Faculty of Economics and Management Sciences, Grimmaische Straße 12, 04109 Leipzig, Germany. E-mail: *melanie.krause@uni-leipzig.de*

†Leipzig University, Faculty of Economics and Management Sciences, Grimmaische Straße 12, 04109 Leipzig, Germany. E-mail: *vincent.stegmaier@uni-leipzig.de*

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1 Introduction

The expansion of renewable energy is a vital measure of environmental policy for countries around the world in order to achieve climate neutrality and to comply with the goals of the Paris Climate Change Agreement (Intergovernmental Panel on Climate Change, 2022). Along with solar and hydro power, wind energy is one of the main sources of renewable energy generation. Yet, while many people support wind energy generation in general, there are arguably some disamenities for those living in the vicinity of wind turbines. Understanding voters' concerns and reactions is important for policymakers that decide on the construction of new wind turbines.

A backdrop to this situation is formed by the increasing political polarization as well as regional inequality that many industrialized countries have experienced in recent years. It is well documented that people in urban centers tend to vote differently from rural areas (see e.g. Kenny and Luca, 2020, Scala and Johnson, 2017, MacLeod and Jones, 2018, Rodríguez-Pose, 2018). This phenomenon increases the relevance of the local effects of environmental policy. Douenne and Fabre (2022) argue that in several instances, people outside of big urban centers have to bear the brunt of the transition in energy and mobility, as evidenced by the *Gilets Jaunes* protests against a carbon tax in France in 2018/19. In a similar way, wind turbines are typically built in rural areas.

This paper deals with voters' reaction to the construction of a visible wind turbine in their proximity. In particular, we study whether a change in the general support for renewables is reflected in the pro-renewable Green party at the federal election after voters being visually exposed. In theory, wind turbines in people's vicinity might affect them through various negative or positive channels that include noise pollution, bird endangerment, visual intrusion of the landscape, but also active contribution towards a cleaner energy supply with potentially cheaper prices and/or jobs for locals (Wolsink, 2000, Liebe and Dobers, 2019, Diermann, 2023). It is thus an empirical questions, whether pro-renewable parties will lose local votes after the construction of a wind turbine. Previous papers from various settings around the world have yielded ambiguous results (see for example Urpelainen and Zhang, 2022, Stokes, 2016, Germeshausen et al., 2021, Otteni and Weisskircher, 2021).

Here, we provide new insight on this topic by estimating the effects of visible wind turbine construction on Green party voting behavior from 1998 to 2021. The new contribution of this paper is threefold:
(i) Combining municipal-level data on voting behavior over two decades with the precise location of each wind turbine, we have much more fine-grained data at our disposal than

previous studies.
(ii) We employ robust econometric methods. We address concerns that plague fixed effects estimations by working with difference-in-difference methods with different combinations of treatment and control groups, also accounting for the anticipation effect.
(iii) Crucially, we focus on visibility of a wind turbine from a settlement area rather than its mere presence. We are the first study to do so and can thus empirically elucidate an important channel through which wind turbines might affect residents' attitudes.

We work with German data for a number of reasons: Germany is the most populous country in Europe and an industrial powerhouse with coal and gas as traditional energy sources, but the *Energiewende* (energy transition) has driven the expansion of renewables. In 2021 - even before the invasion of Ukraine by Russia -, 42.4% of electricity in Germany came from renewable sources, half of which was generated by wind energy (Destatis, 2022). This expansion of wind energy across Germany occurred gradually over the last decades, with sizeable variation across both time and space. With the geo-locations of wind turbines and their building date at our disposal, we can exploit this variation in our econometric analysis.

Yet, Germany brings another advantage in terms of analyzing the local political effects of building wind turbines: It has a Green party that has run on a strongly pro-environmental platform since its foundations in the 1980s, arguing against nuclear energy and in favor of renewable energy sources (Bukow, 2016). The party is thus strongly associated with the climate topic in public opinion (Wagner and Meyer, 2014), arguably much more so than other, more comprehensive progressive parties, such as the Democrats in the U.S. This mitigates concerns that the voting behavior might be dominated by other issues.

In our analysis, the preliminary results suggest no sizable local blacklash against pro-renewable at the ballot box most of the time. Constructing a wind turbine that is visible from a nearby settlement is not followed by a decrease in the Green party's local vote share in most of the last decades from 1998 to 2021. However, our preliminary findings also point to a change in that overall pattern at the 2021 election period, where the Green party loses in those municipalities that were treated by wind turbine construction at that time. We discuss these developments and point to various factors, including a more polarized political debate and the expansion of wind turbines to less supportive areas.

The remainder of this paper is organized as follows: In Section 2, we anchor our contribution in the literature. Section 3 gives an overview of the German wind power expansion and its geographical and temporal distribution. Section 4 discusses the visual perception of turbines, which is crucial in our data analysis. Section 5 gives more insights on the data we use. Section 6 contains a discussion of the econometric methods employed. In Section 7, we present and interpret our main results, whose implications we discuss in Section 9. Section 10 concludes.

2 Relation to the existing literature

Just as climate change and its mitigation measures have become increasingly important topics of the political debate in countries around the world, the environmental economics literature on people's support and attitudes has expanded. Understanding who supports and who opposes renewable energy projects under which circumstances is vital for policymakers designing such measures.

Recent research has examined the impact of climate-change mitigation measures in light of the broader background of political polarization and regional inequality in many industrialized countries. It is well-documented that people in big cities tend to vote differently from those in rural areas and that this so-call 'urban-rural political divide' has deepened in recent years, in particular since the Great Recession of 2008. In the U.S., the political divide between Democrat-leaning urban centers and Republican strongholds in the countryside has been analyzed extensively (e.g. McKee, 2008, Scala and Johnson, 2017). Strong geographical differences in voting behavior have also been studied in many European countries, including Britain (e.g. Jennings and Stoker, 2016, MacLeod and Jones, 2018), France (e.g. Ivaldi and Gombin, 2015, Agnew and Shin, 2020) and the meta-study by Kenny and Luca (2020). This matters for climate-change mitigation measures: In what McKann (2020) calls 'the geography of discontent', the right-wing populist voting share tends to be particularly high in formerly industrialized regions that are losing out to globalization, structural change and as well as the shift towards greener energy (Rodríguez-Pose, 2018).

At the same time, Douenne and Fabre (2022) suggest that people outside of big urban centers may have to bear the brunt of the transition in energy and mobility. They point to the Gilets Jaunes protests that erupted in France in 2018/19 and were sparked by a carbon tax that threatened the purchasing power of people in rural areas, who are dependent on cars as means of transport. This concern for purchasing power went in line with an anti-elite sentiment, and Douenne and Fabre (2022) show that the protests shifted the public perception of the carbon tax as regressive and environmentally ineffective.

Wind energy generation also constitutes a vital feature of the transition to a carbon-free economy, but, just like carbon taxes, the construction of wind turbines does not affect every citizen equally. It is concentrated in rural areas. While it is important for politicians to obtain public support, the literature has so far yielded ambiguous and contradictory results about the impact of new wind turbines on election outcomes.

Theoretically, the reaction to the construction of wind turbines is not clear-cut and residents tend to be aware of advantages and disadvantages. Among the opponents, two groups are typically distinguished: NYMBYists ('Not in my backyard') see the necessity of wind energy as a public good but want to free-ride by not having turbines in their own vicinity. By contrast, while NIABYists ('Not in any backyard') oppose that kind of energy generation in general (Wolsink, 2000, van der Horst, 2007). Despite the overall large public support of wind energy projects (Aldy et al., 2012), both groups are empirically relevant (Liebe and Dobers, 2019, Wolsink, 2000). Often-cited negative effects include noise pollution and interference with natural areas (such as bird endangerment). Yet survey respondents' attitudes towards wind projects are most strongly shaped by their "perceived impact on scenery, visual intrusion of the landscape" (Wolsink, 2000, p.51). In line with these arguments, visibility of a wind turbine from urban settlements plays a key role in our analysis. On the positive side, wind turbines actively contribute towards a cleaner and more sustainable energy supply, potentially going in line with cheaper electricity and new jobs. To what extent these benefits accrue not only at the global, but also at the local level, might depend on the circumstances. Diermann (2023) analyzes cheaper electricity prices offered by suppliers to local residents of German wind parks. Participation opportunities for citizens have also been found to matter for acceptance (Langer et al., 2017). Whenever longer time horizons are considered, self-selection as well as habituation may play a role: Hoen et al. (2019) find that Americans that live closer to wind turbines have more positive attitudes towards them, in contrast to the negative impacts of noise and visual dominance that increase with proximity.

Which effect dominates empirically and whether or not voters close to wind turbines change people's attitudes toward wind energy reflected in pro-climate parties vote share, is far from clear. The literature has found very heterogenous results so far.
On the positive end of results, Urpelainen and Zhang (2022) finds that one more megawatt of wind power capacity within U.S. Congressional districts in 2003 to 2012 has lead to a 0.03 percentage point increase in vote shares for the Democratic party. They suggest that policies might endogenously create their political support. Yet, U.S. Congressional districts are comparatively large and it is also conceivable that overall economic benefits at the aggregate might mask discontent of those living closest to the

turbine. In fact, Stokes (2016) finds that proximity to turbines plays a crucial role in determining voting outcomes. She works with municipal-level data from Ontario, Canada, between 2006 and 2013, to show that voters tend to punish incumbents after the construction of a wind turbine with a decrease in vote share by 4-10%. A negative effect of incumbents' vote share at the local level is also found for Denmark from 2000 to 2019 by Larsen et al. (2021). The vote share decreases by 3.5% on average after a construction of a wind turbine, with the effects on local incumbents much larger than on national incumbents.
For Germany, Germeshausen et al. (2021) look at the federal elections of 2009 and 2013 to find a sizable 17% decrease in vote share of the Green party resulting from a wind turbine in a municipality. On the other hand, Otteni and Weisskircher (2021) analyze German federal and regional elections between 2013 and 2019. They find a small positive rather than negative effect of wind turbine construction on the vote shares of both the Greens and the far-right, anti-renewable AfD party, suggesting an increase in voters' polarization.

This wide heterogeneity of estimated effects might be due a number of factors, including different countries with different political systems and parties (that might be single-issue or broad parties), different time horizons (when climate change was a more or less dominant topic compared to other issues), different units of observations (at which the presence of a wind turbine might yield different effects), as well as the precise data and measurement. It is conceivable that the first wind turbine in a municipality has a different impact from adding one more to a large exiting wind park. Another contributing factor to the widely varying results might be different econometric methods. Two-way fixed effects is the typical panel data estimator (Otteni and Weisskircher, 2021); however, some studies employ instrumental variable techniques to contour the potential endogeneity of turbine location. These instruments can be wind speeds (Stokes, 2016) or expected revenues (Germeshausen et al., 2021).

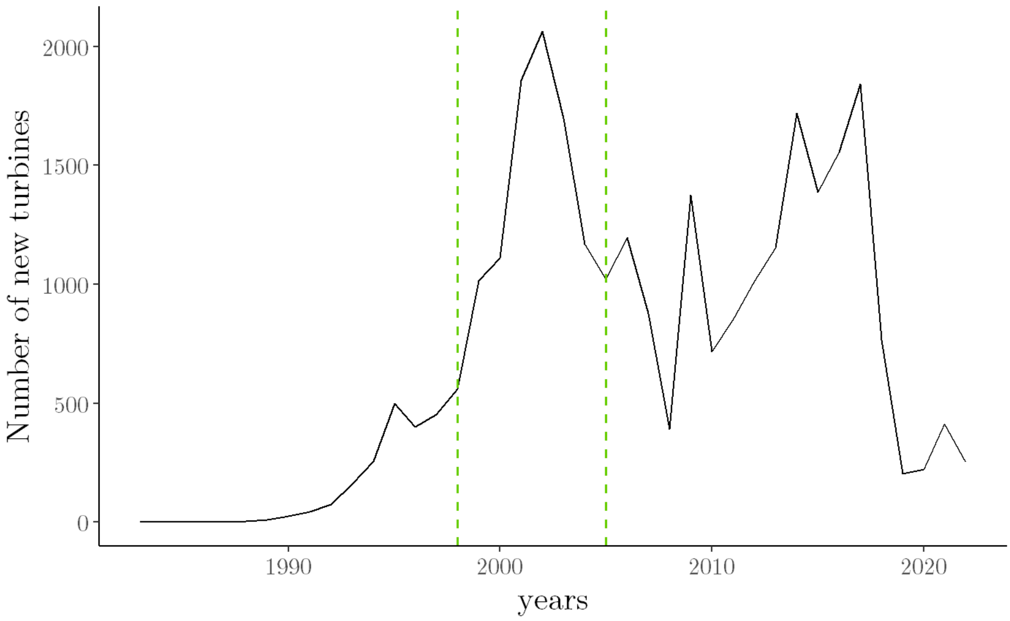
In this paper, we seek to advance the literature in various respects. With a comprehensive approach, we aim to gain new insights as well as to reconcile previous results.
Focusing on Germany and its pro-renewable Green party, we work with fine-grained municipal-level data for local effects, but use a larger time span of elections reaching back several decades. Moreover, we avoid the issues associated with the two-way fixed effects estimator in settings with multiple periods and treatment timings (Abraham and Sun, 2018) by estimating the effect for each group of municipalities treated at the same time separately. We also account for anticipation and inherent differences in municipalities by using as controls those units that get treated later on. Finally, we exploit the precise location of wind turbines as well as settlements. Rather than taking the mere presence of a

wind turbine in a municipality, we build on the survey literature that has highlighted both the proximity and the visibility of a turbine from the settlement as vital characteristics for shaping attitudes (Wolsink, 2000). As we will explain in more detail in the following, we compute the viewshed of each wind turbine to see if visibility leads to a decrease in the vote-share of the Green party. An analysis of the visibility feature on election outcome is, to the best of our knowledge, novel to the empirical literature.

3 The German wind turbine expansion

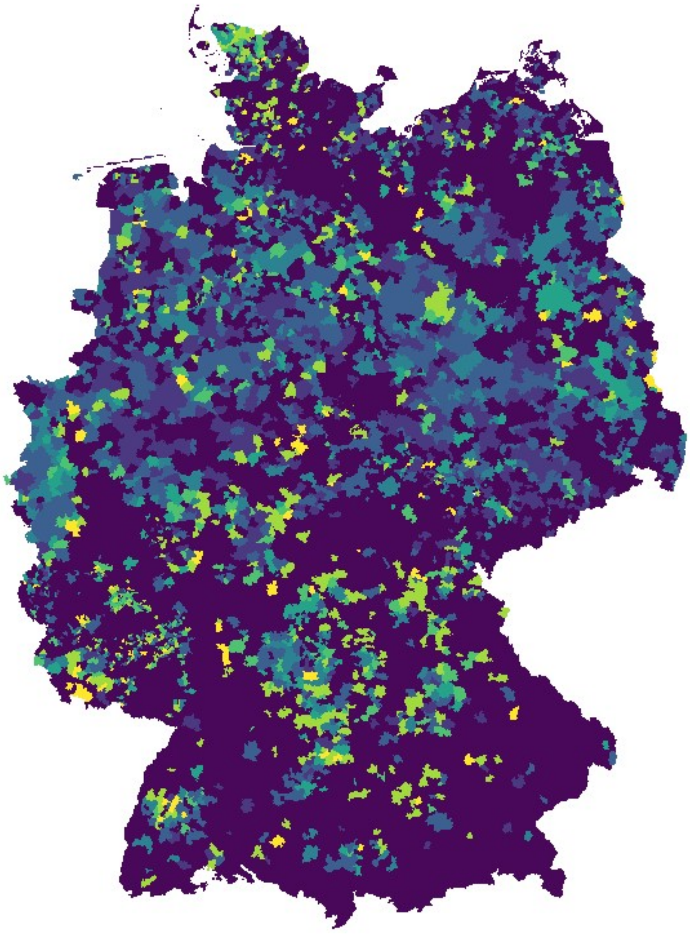
Whereas the expansion of wind energy in Germany already began in the late 1980s, it accelerated rapidly during the time the Green Party was part of the government from 1998 to 2005 (Figure 1). In 2000, the Renewable Energy Sources Act (EEG) was passed, introducing feed-in tariffs (i.e., a fixed price per unit of energy generated) and a feed-in priority for wind energy. Although between 2008 and 2011 the expansion was low, a second surge began in 2012, commonly explained by reforms to the EEG and a refocus on renewable energy generation following the Fukushima accident in 2011 and the subsequent phase-out of nuclear power (Fuchs, 2021).

Figure 1 – Number of turbines installed over the years. The time frame marked by the green lines indicates the period during which the Green Party was involved in government



The map in Figure 2 visualizes in which election period the first wind turbine in each municipality was built. While many municipalities in the north had their first turbine in the earlier expansion periods, many in the south had their first turbine in more recent election periods or had no turbine until 2021. In addition to worse topographical characteristics (Blankenhorn and Resch, 2014), protests by local residents, particularly in Southern parts of the country are often used as an explanation of these differences.

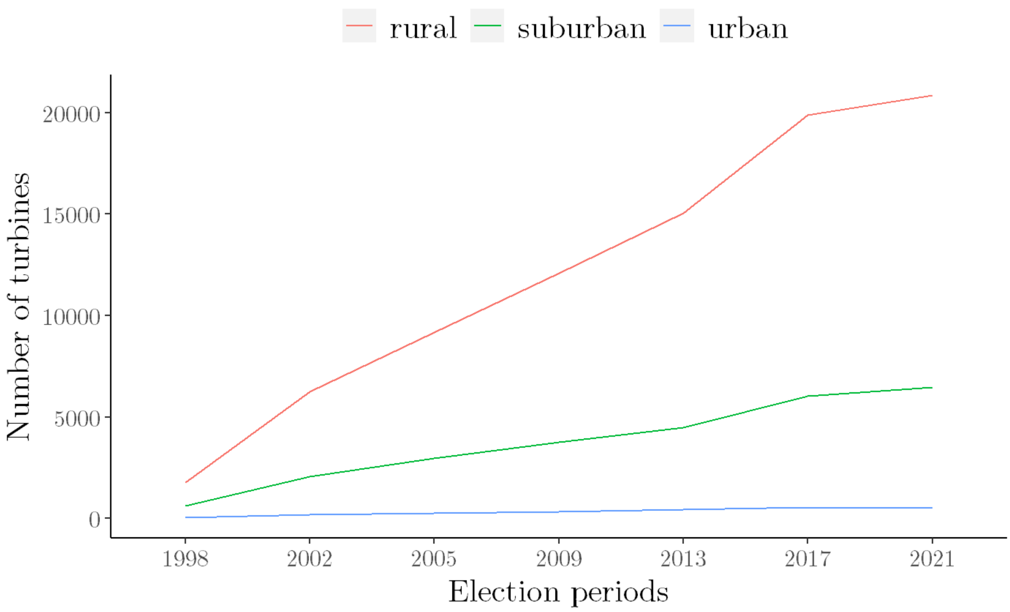
Figure 2 – Spatio-temporal distribution of municipalities where a turbine is installed for the first time. The dark blue areas are municipalities without turbines until the 2021 election period. The lighter the color, the later the first turbine was installed (yellow is the most recent 2021 election period).



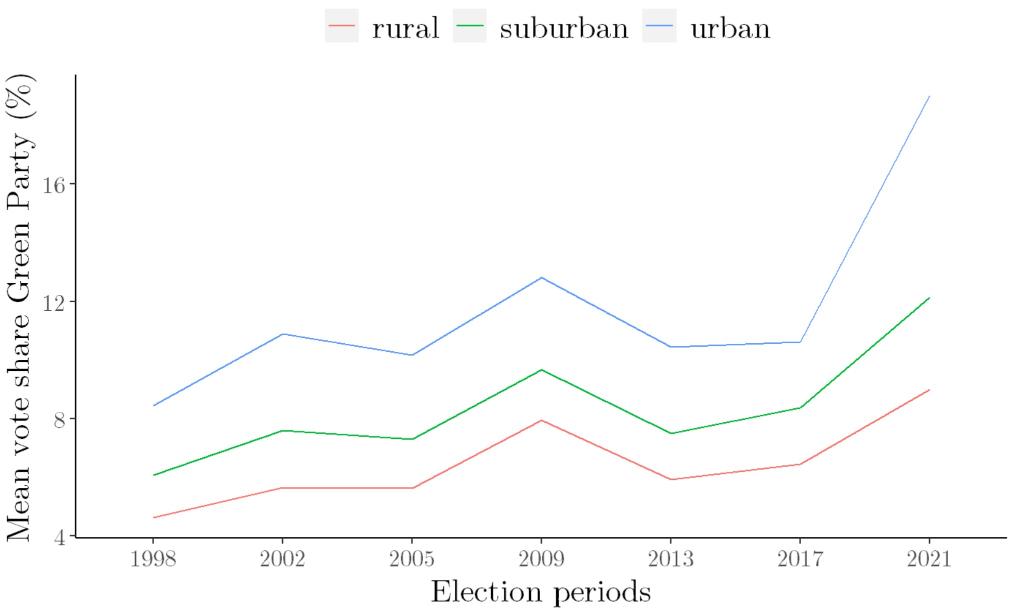
Furthermore, expansion has occurred almost exclusively in rural areas due to inexpensive and available land, but support for the Green Party is significantly lower there than in more urbanized areas (Figure 3). Even if there is no direct relationship, this suggests that where the renewable energy is generated, support for renewable energy is relatively low.

4 Visual Perception

Qualitative work such as Wolsink (2000) suggests that support for renewables is based on subjective perception influenced by physical sensory impressions, particularly visibility and audibility. Some voters might perceive the presence of wind turbines as annoyance or disruptive to the landscape. In addition, there is a risk of shadow flicker, which can be caused by the shadows cast by rotating rotor blades, although this effect is very small



(a)



(b)

Figure 3 – Number of turbines constructed (a) and the vote share for the Green Party (b) in areas with different types of urbanization

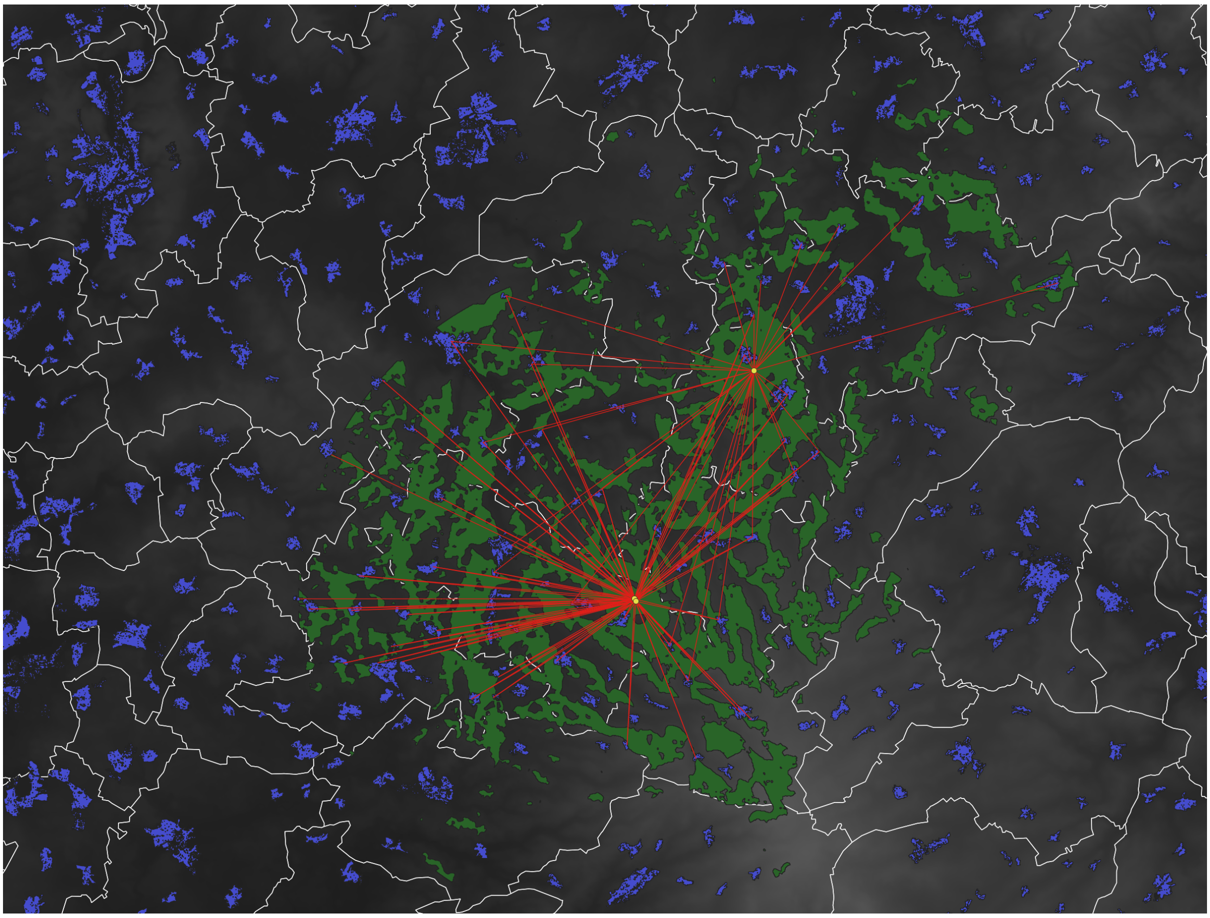
with modern generations of turbines (Freiberg et al., 2019). Others may have concerns about noise generated by wind turbines, which is strongly correlated with visibility, as physical obstructions block both light rays and (in part) sound waves and the exposure decreases with increasing distance. Actual noise exposure also depends on various factors such as aerodynamic processes, and the audible radius is much smaller than the visible one (Bakker et al., 2012). While there is no visibility regulation, a plant can only be built in Germany with a noise protection permit, which is granted if the surrounding area is not affected by sound to a certain degree (4th BImSchV), so arguably, most of the sensory perception of turbines that is relevant in this natural experiment is visual. As far as we know, this is the first attempt to isolate the effect of visual exposure from the overall effect of a wind turbine on election outcomes. Obviously, election data is only published at an aggregated level, so there is no clear treatment boundary where a proportion of a municipality's visible area is sufficiently exposed to have an effect on the support of renewable through the vote share of the Green party. Moreover, multiple turbines may be visible from one point and an even larger number at another point, so different components shape each municipality's treatment intensity. Thus, to estimate voter response to visual exposure, it is necessary to identify when in which part of a residential area how many turbines can be seen and from what distance. We explain the construction of the treatment variable in section Section 6.1.

5 Data

For our spatial analysis, we use fine-grained data on the position of wind turbines in Germany based on the federal network agencies data base adjusted by Eichhorn et al. (2019). We combine the geo-coded turbine data including their hub heights and construction dates with the digital surface model EU-DEM, a representation of the elevation including the height of ground features such as trees and non-natural structures in Europe (First-Surface Model) with a resolution of 25 m. To assess how many potential voters can see how many turbines from a given distance in a given election period, we calculate the viewshed of all installed turbines, i.e., the area around the turbine from which a person with an eye height of 1.6 m can see the hub. To further analyze the relationship between distance and voting responses, we also calculate the intervisibility distances, i.e., the distance between each settlement and the visible turbines.[1] Figure 4 visualizes the intervisibility network for turbines constructed within the 2013 election period (2010-2013) in Hesse. Each cell of the resulting viewshed grid represents the sum of visible turbines within a certain distance. Second, we superimpose the EU's Global Human Settlement Layer (GHSL), which represents the global settlement area based on satellite imagery, and the viewshed grid to calculate which settlement area is visually exposed to what extent in each election period. We merge this with the municipal boundary map as of 2021. Socioeconomic data from the INKAR data base were provided by on a municipal level by The Federal Office for Building and Regional Planning (BBSR, 2020) and the federal election data by The German Federal Returning Officer which we also adjusted on the 2021 administrative boundaries.

[1]The visible distance was calculated from the centroid of the settlement's visible part to the turbine.

Figure 4 – Intervisibility network of turbines constructed in 2013 (yellow points) and residential areas (blue polygons) in the state of Hesse. The green area represents the viewshed of the turbines and the red lines the distances between the settlements and all visible turbines. The lighter the background, the higher the elevation



6 Econometric Methods

Our empirical model is based on Difference-in-Difference, as it internally controls for nationwide trends in the support for the Green party as well as time invariant differences between groups. As wind turbines are constructed gradually over time, municipalities are visually exposed at different points in time. Previous research (e.g. Otteni and Weisskircher, 2021) estimates the impact of this staggered treatment adaptation (i.e., the impact of building an additional turbine or the kW/hr generated by those turbines within the administrative boundary) on election outcomes using a two-way fixed effects (TWFE) model over multiple time periods and treatment timings. We use a similar approach, but estimate the effect separately for each group of municipalities visually exposed in the same election period, following a framework resembling the group-time average treatment effect proposed by Callaway and Sant'Anna (2021). From now on, we are referring to all municipalities first visually exposed in the same election period as a timing group g.[2] Since the data covers eight election periods in which all turbines were built, we have eight timing groups, for each of which we estimate the immediate effect on the results of the subsequent election.[3]

Analysing each timing group individually can reveal how voting responses might change over time. Furthermore, Goodman-Bacon (2021), Callaway and Sant'Anna (2021) and Abraham and Sun (2018) show that any TWFE model with multiple treatment timings can be decomposed into a weighted average of all possible 2x2 Difference-in-Difference (DiD) estimators in the panel. This implies that municipalities visually exposed in earlier election periods also serve as control groups for municipalities treated at a later time, while the weights of each 2x2 DiD estimator depend not only on the relative size of the timing groups, but also on the variation in the treatment variables. If the effects of wind turbines on election outcomes vary over time or are heterogeneous between municipalities treated at different points in time (i.e., between timing groups), these comparisons will bias the results. Given the long observation period (23 years), it is highly plausible that impacts change over time or between groups, since, for example, the first visible wind turbine in a municipality built in the early 2000s might be perceived differently than it was in late the 2010s due to changing policy debates about renewable energy and climate change mitigation. In addition, turbines have evolved over the years, e.g., they have become larger, but also quieter.

While Goodman-Bacon (2021), Callaway and Sant'Anna (2021) and Abraham and Sun (2018) consider the case of a binary treatment, these problems with staggered adoptions of the TWFE models also arise with a treatment with finite number of ordered values (De Chaisemartin and d'Haultfoeuille, 2020) or continuous treatment (Callaway et al., 2021). By comparing only the change in election outcomes between one pre-visible election period and one post-visible election period for each timing group separately, we avoid these potential problems of TWFE models, since each estimate is a simple 2x2 DiD setup with equal treatment time windows (one election period prior the intstallation versus one election period after) and a comparison only between treated and untreated units (or treated municipalities with a similar treatment intensity in the response estimation).

[2]For example, municipality m is first visually exposed in 2007, which falls in the 2009 election period (the year the next federal election is held), so m belongs to the timing group $g = 2009$.

[3]For the first timing group ($g = 1998$), we cannot estimate the effect as we do not have a pre-treatment period in the panel.

6.1 Treatment groups

The treatment group in each election period (i.e., timing group) consists of municipalities visual exposed for the first time to turbines up to four kilometer, an estimated threshold distance for dominant visual impact (Breuner, 2001, CPRW, 1999). As most of the installed turbines are only partially visible from residential areas, we limit the treatment group to municipalities where at least one turbine is visible for the first time in more than ten percent of residential areas. The distance at which a wind turbine is perceived as intrusive is subjective, so that a cut-off value cannot be clearly defined. To ensure that there is not already an effect of turbines at a distance greater than 4 km in the pre-treatment period ($g-1$), we restrict the treatment group to municipalities with no visual exposure in their pre-treatment period up to a distance of 8 km, due to the ambiguity of the extent to which turbines have an effect in the buffer zone located located between 4 and 8 km to residential areas.

There are systematic differences between the timing groups. First, the proportion of municipalities visually exposed for the first time in the 1990s is larger in eastern and western/northern Germany than in the south, while the share of southern municipalities is higher from the 2000s onward ((a) Figure A-1). Furthermore, the share of municipalities classified as smaller towns and suburbs rather than rural according to the DEGURBA classification (Eurostat, 2020) is considerably larger in the latest timing group than in municipalities of earlier timing groups, revealing that the later expansion is spreading within sight of more densely populated areas ((b) Figure A-1). Correspondingly, the average population density is lower than the national average in municipalities of the 1998 to 2017 groups while municipalities in the 2021 timing group have an above-average density ((c) Figure A-1). Last, the number of municipalities in each group decreases over time ((d) Figure A-1).

6.2 Control groups

Similar municipalities that are geographically close to the treated ones, but have no turbines in sight, are used as a control group, up to a buffer distance of 8 km, again to avoid treatment spillover to turbines further away than the treatment threshold of 4 km. If the change in vote share of the control group is equal to the counterfactual change in vote share of the treated group, we estimate the average effect of turbine visibility on support for renewable energy policies. Thus, it is essential for a comparison with untreated units to find municipalities that are similar to those of the treated group with the exception of turbine visibility. To do this, we exploit the geographic and administrative hierarchy within the unique ID assigned to each municipality. The ID is a eight digit number, which starts with a state identifier (first two digits), followed by the government district index (third digit), an identifier for the county (fourth digit and the fifth digit) as well as the municipality (last three digits). Thus, municipalities with a similar numeric ID value are approximately geographically close to each other and might share administrative and socioeconomic characteristics (Tobler's first law of geography, Tobler (1970)). For each municipality in timing group g, a municipality that is not or not yet visually exposed to wind turbines is matched based on the smallest Euclidean distance of their ID values.[4] We restrict the matching process to pairs within the same part of Germany (South, North/West or East) and with the same urbanization status according to the DEGURBA classification, as voters in urban and rural areas might have systematically different voting behaviours regardless of their geographic proximity. Hence, municipalities j without visual exposure in both periods within the same area, having the same degree of urbanization and the smallest differences in ID value of the treated municipality m are used as comparisons ($min_j||ID_m - ID_j||$).

6.3 Level effect: Seen versus unseen

In the first specification, we define the treatment variable as binary, since the potential impact of visibility is arguably driven by the first turbine, while it diminishes with each additional one. The treatment variable takes the value of one if turbines are seen for the first time in election period $t = g$ in more than ten percent of the residential areas of municipality m within a radius of 4 km:

$$D_{mt} = \begin{cases} 1 & \text{if} sharevisible_{mt} > 0.1 \\ 0 & otherwise \end{cases}$$

Hence, we compare the change in vote share before and after the visual exposure with the change of similar municipalities not visually exposed at time $t = g$, defined by eq. (1), with G_g indicating if the municipality belongs to timing group g and D_g an indicator if a municipality is already treated at $t = g$.

$$ATT(g) = E[Y_g - Y_{g-1}, G_g = 1] - E[Y_g - Y_{g-1}, D_g = 0] \tag{1}$$

Assuming parallel trends and no anticipation, eq. (1) targets the average effect of visual exposure for municipalities seeing a turbine for the first time. While common pre-treatment outcomes are not necessary nor sufficient to provide evidence for post parallel trends (Kahn-Lang and Lang, 2019), it supports the assumption's plausibility. Figure A-2 plots the vote share for each timing group and it's corresponding control group over time, supporting the assumption. Moreover, we shift the treatment timing for each timing group to all possible $t < g$ pre-treatment election periods and aggregate the results in

[4] We allow up to three matches for each treated municipality, which gives us the most variation in the control group while keeping the geogrpahic distribution similar to the treatment group.

event time, given by eq. (2), with e referring to the event time and P to the relative group size of g. The results are small and insignificant (Figure A-3) for all pre-treatment election periods, further providing evidence for an unbiased estimation. The assumption of no anticipation is discussed and addressed in Section 6.6.

$$\theta(e) = \sum_{g=2}^{T} 1\{g + e \leq T\} ATT(g, g + e) P(G_g = 1 | g + e \leq T) \tag{2}$$

6.4 Slope effect: Average number of visible turbines

We further investigate if the effect is actually driven by the fact a municipality is visually exposed to any number of turbine and is not linearly increasing with the number of turbines. To do so, we also estimate the average response of a marginal change in the average number of visible turbines within the visible part of residential areas for municipalities of timing group g visually exposed for the first time ($t = g$) at a particular intensity. We split each timing group in municipalities with a visual exposure to an average number up to two ($D \leq 2$) and municipalities with a visual exposure to an average number above two ($D > 2$). The continuous treatment variable for both intensity groups is constructed by calculating the average number of turbines visible within 4 km from the residential areas, weighted by the share of the residential area from where the turbines are visible:[5]

$$D_{mt} = \sum_{i=1}^{n} wt_{it} * sharevisible_{imt}$$

Most of the municipalities in the treatment group are on average visually exposed by less than or equal to one turbine (Figure 5) and only 86 are on average visually exposed to over ten turbines.

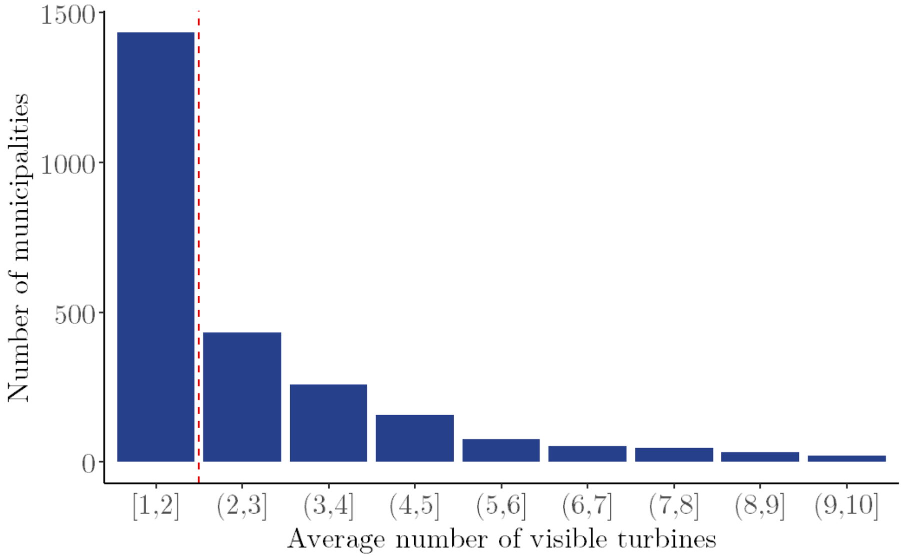
With the additional assumption of homogeneous responses across different exposure intensities,[6] the Average Causal Response (ACR) for a particular intensity d is given by eq. (3).

$$ACR(g, d) = \frac{\partial [Y_g - Y_{g-1} | G_g = 1, D_g = d]}{\partial d} \tag{3}$$

[5]For example, if 25 percent of the visible part of the residential area of municipality m is visually exposed to one turbine and 75 percent is visually exposed to two turbines, the value of the treatment variable in the post-treatment period $t = g$ is equal to: $D_{mt} = 0.25 \cdot 1 + 0.75 \cdot 2 = 1.75$

[6]This assumption implies that proportional changes in vote share in response to certain visual exposure would have been the same for all municipalities independent of their actual visual exposure intensity. Thus, changes in the vote share for municipalities with a low visual exposure are a good counterfactual for municipalities with a high visual exposure had they had the same treatment intensity (Callaway et al., 2021).

Figure 5 – Distribution of the treatment variable up to an average number of ten visible turbines. The red dashed line visualizes the split between low and high intensity subgroups.



6.5 Estimation

Both the ATT and ACR are estimated via eq. (4) given by the slope parameter of the treatment variable $D_{m_g t}$ for each timing group $g = 2002, ..., 2021$. η_{m_g} and ψ_t are municipal and election period fixed effects. In order to make the parallel trend assumption more plausible, we also estimate the model with an inclusion of potential relevant socioeconomic covariates $X_{m_g t}$, controlling for the municipal population density, the share of workers with an university degree, the unemployment rare and the per person income tax revenue.

$$Y_{m_g t} = \eta_{m_g} + \psi_t + \beta D_{m_g t} + X'_{m_g t}\gamma + \epsilon_{m_g t} \tag{4}$$

In the level specification, β corresponds to the average effect of visual exposure on the election outcome, whereas in the continuous specification, the parameter targets a weighted average of responses to different visual exposure intensities for both subgroups ($D \leq 2$ and $D > 2$) of each timing group g.

6.6 Accounting for anticipation

In Germany, the duration of the planning and approval of a wind turbine is on average 4.75 years (FA Wind, 2015). These procedures include location assessments such as sound and shadow forecasts and public debates. Thus, voters might reveal their support or rejection at the ballot box before the turbine is commissioned at an earlier election period and therefore bias the estimates. To control for potential anticipation effects, we re-estimate the model by shifting the base pre-treatment period from $g - 1$ to $g - \delta - 1$ where δ

represents the number of anticipation periods (eq. (5), eq. (6)). An anticipation of one election period ($\delta = 1$) increases the difference between the pre-treatment election and the earliest date a turbine is constructed to four years which arguably should account for most of the anticipation effect. Although this relaxes the assumption of no anticipation, it also limits the number of timing groups for which we can estimate the response, since it is not possible to estimate the effect for the second timing group ($g = 2002$) given that voters in these municipalities already anticipate visual exposure or are already exposed to the construction site in the first election period, implying that there is no untreated period to compare to.

$$ATT(g, \delta) = E[Y_g - Y_{g-\delta-1}, G_g = 1] - E[Y_g - Y_{g-\delta-1}, D_{g+\delta} = 0] \tag{5}$$

$$ACR(g, d, \delta) = \frac{\partial[Y_g - Y_{g-\delta-1}|G_g = 1, D_g = d]}{\partial d} \tag{6}$$

7 Preliminary Results

Figure 6, Figure 7 and Figure 8 illustrate the ATT and ACR at a distance of 4 km for the six timing groups (2002 to 2021). The results of both the estimated ATT and ACR suggest that visual exposure from turbines has little impact for all timing groups except the last one.

While the point estimates are small in magnitude and insignificant up to timing group of 2013 for the ATT as well as the ACR (Table A), visibility is associated with a significant decrease in Green party vote share for the municipalities first visually exposed in the 2017 and 2021 election period (Table 7). For the 2017 timing group, visual exposure is associated with a 0.26 to 0.27 percentage point decrease in Green Party vote share, significant at the 5 percent level, and 0.29 to 0.3 percentage points when anticipation is accounted for, suggesting that voters barley respond in the planning and construction phases. The estimated effects of the 2021 timing group are almost ten times higher with an effect of -2.4 to -2.6 percentage points, significant at the 1 percent level. While the marginal effects are insignificant for the 2017 timing group, municipalities of the 2021 timing group with up to two turbines visible associates an additional turbine visible in the settlement area with a 0.9 percentage point decrease in the Green vote share, significant at the 5 percent level and up to -1 percentage point when anticipation is accounted for (Figure 8). The ACR for municipalities where more than two turbines are visible is not significant (Figure 7), suggesting that the effect is mainly driven by the first visible turbines and diminishes with additional turbines.

Table 1 – ATT results for 2017

Dependent Variable:	Vote share Green Party (%)			
	No Anticipation		Anticipation	
Model:	(1)	(2)	(3)	(4)
Variables				
log(population density)	7.7*** (2.7)		1.6 (1.9)	
Share university degree (%)	0.37* (0.19)		0.08 (0.10)	
Unemployment rate (%)	0.25* (0.13)		0.19 (0.13)	
Income tax revenue (PC)	0.04 (0.10)		-0.10 (0.12)	
post × treat.view.majority.4.did	-0.27* (0.14)	-0.26* (0.14)	-0.30** (0.14)	-0.29** (0.14)
Fixed-effects				
Election Period	Yes	Yes	Yes	Yes
Municipality	Yes	Yes	Yes	Yes
Fit statistics				
Observations	1,640	1,640	1,640	1,640
R^2	0.92154	0.91644	0.92884	0.92778

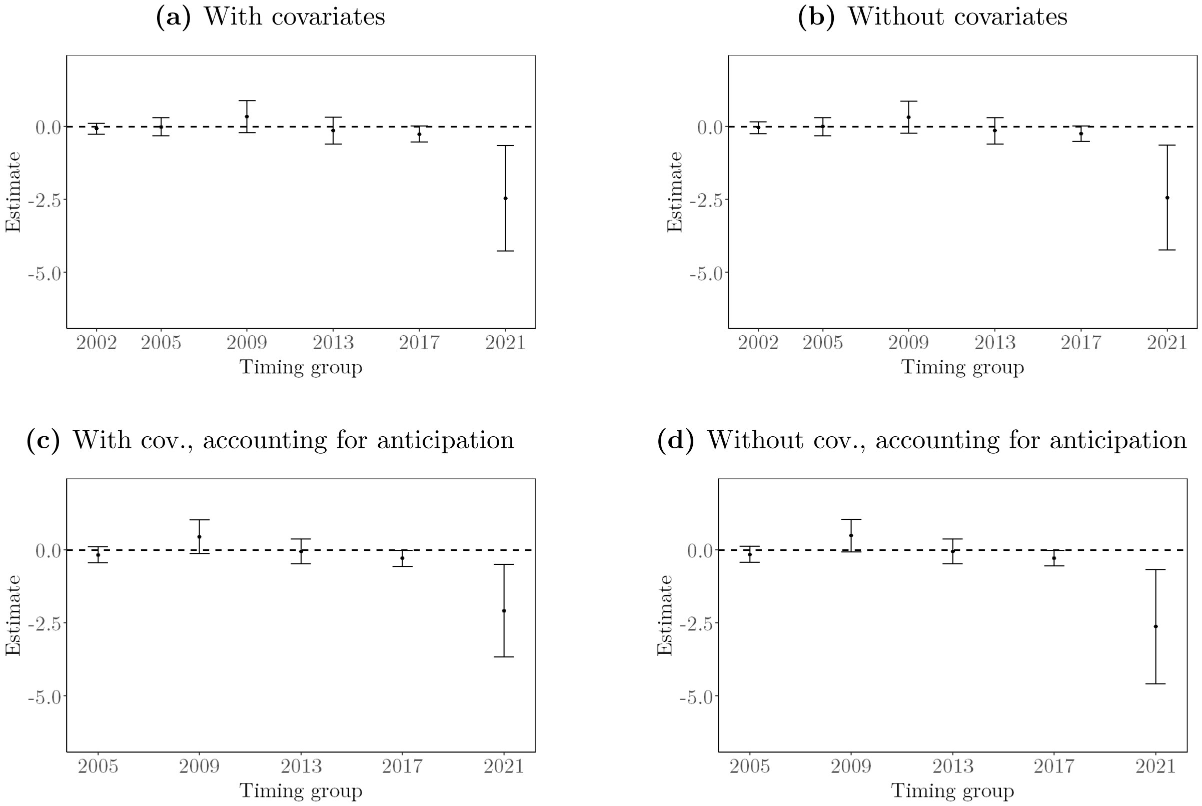
Clustered (County) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 2 – ATT results for 2021

Dependent Variable:	Vote share Green Party (%)			
	No Anticipation		Anticipation	
Model:	(1)	(2)	(3)	(4)
Variables				
log(population density)	10.5 (12.7)		5.0 (8.2)	
Share university degree (%)	0.83** (0.38)		1.1*** (0.30)	
Unemployment rate (%)	-1.3 (0.78)		-0.24 (0.54)	
Income tax revenue (PC)	0.38 (0.70)		7.0 (8.0)	
post × treat.view.majority.4.did	-2.5** (0.92)	-2.4** (0.92)	-2.1** (0.81)	-2.6** (0.99)
Fixed-effects				
Election Period	Yes	Yes	Yes	Yes
Municipality	Yes	Yes	Yes	Yes
Fit statistics				
Observations	264	264	264	264
R^2	0.89520	0.88744	0.86389	0.83755

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

Figure 6 – Estimated ATT for each timing group



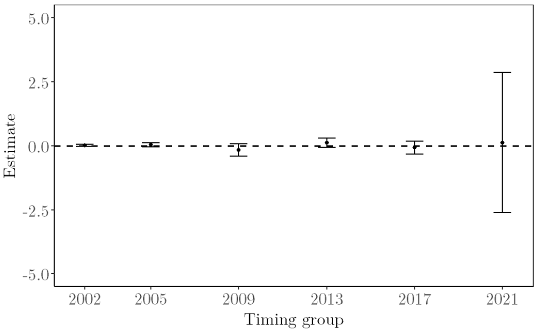
8 Robustness Tests

One of the major threats to the validity of the results is the small number of municipalities in later timing groups, especially in the 2021 group. To increase the number of observations per group, we extend the distance threshold to six kilometers. Figure A-4 shows that with the extended threshold, the number of municipalities in each timing group increases considerably. Since the perceived size of turbines decreases approximately linearly at these distances, increasing the maximum distance also reveals whether impacts actually decrease with increasing proximity. The results are very similar to the baseline cut-off specification, but of a smaller magnitude and insignificant results for the 2017 timing group (Figure A-5), confirming this relationship. While the number of municipalities in the 2021 timing group is still relatively small, the lower but similar estimation results are consistent with visual theory.

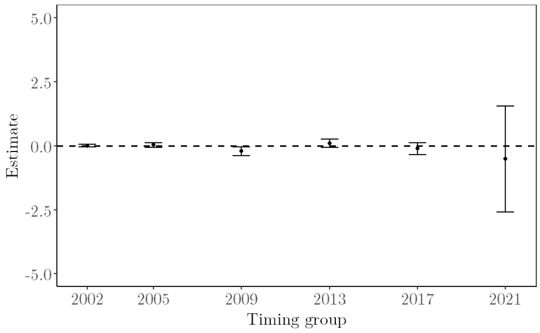
Next, we also test whether and to what extent the effects are affected by endogeneity. For example, the estimate is biased if turbine construction is more likely within the viewshed of areas where Green Party support is high. A potential negative effect of visual exposure for early timing groups could be masked by an opposing positive effect of higher support for these projects relative to the control group, resulting in small and insignificant estimates. To reduce such potential problems, we exploit the staggered treatment roll-out

Figure 7 – Estimated ACR ($D \geq 2$) for each timing group

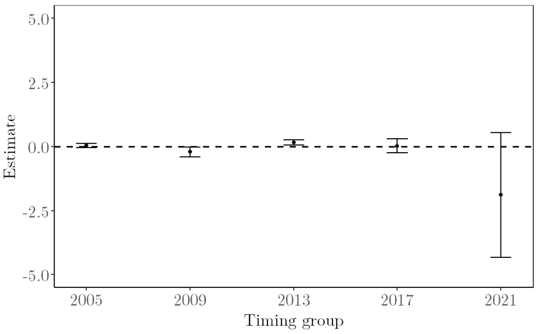
(a) With covariates



(b) Without covariates



(c) With cov., accounting for anticipation



(d) Without cov., accounting for anticipation

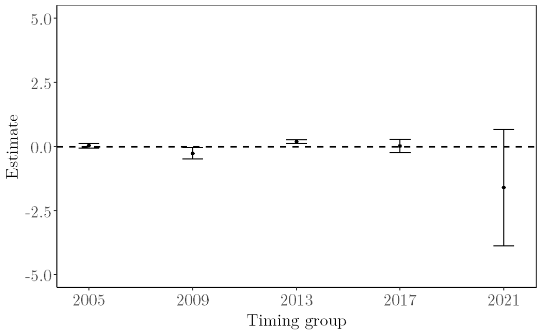
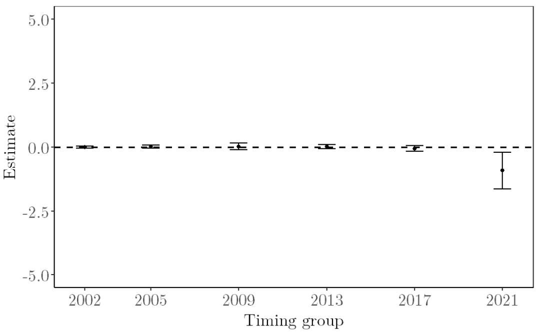
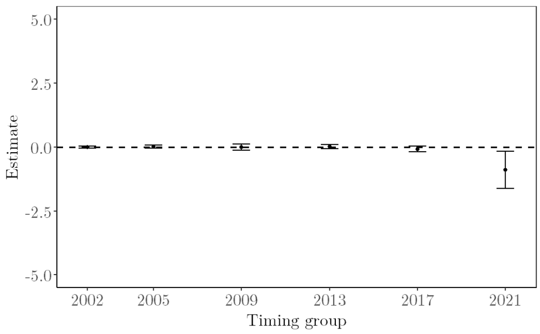


Figure 8 – Estimated ACR ($D \leq 2$) for each timing group

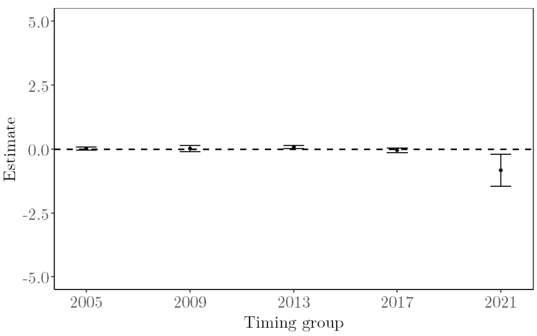
(a) With covariates



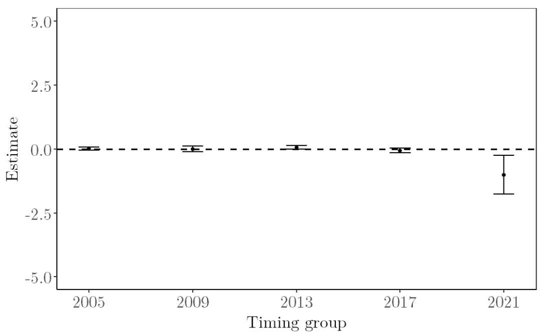
(b) Without covariates



(c) With cov., accounting for anticipation



(d) Without cov., accounting for anticipation



by restricting the control group to municipalities visually exposed one period later than the treatment group, i.e., $g+1$ and $g+2$ when accounting for anticipation (eq. (7) and eq. (8)).

$$ATT(g) = E[Y_g - Y_{g-1}, G_g = 1] - E[Y_g - Y_{g-1} | D_g = 0, D_{g+1} = 1] \tag{7}$$

$$ATT(g, \delta) = E[Y_g - Y_{g-\delta-1}, G_g = 1] - E[Y_g - Y_{g-\delta-1} | D_{g+\delta} = 0, D_{g+\delta+1} = 1] \tag{8}$$

Assuming that support is similar for municipalities treated within two (or three) election periods, effects induced by reverse causality should be cancelled out. Due to the restriction that the control groups consist only of municipalities with future visual exposure, there is no longer a control group for the last group ($g = 7$) to compare to. Thus, we can recover the ATT only for the 2002 to 2017 elections. Accounting for anticipation further limits the number of estimable ATT's to timing group three, four and five. Since we chose $\delta = 1$, municipalities visually exposed in the last election period ($g = 7$) already anticipate their treatment in the penultimate period ($t = g - 1$). Thus, for municipalities exposed in the second to last election period ($g = 6$), there is no longer a control group with untreated outcomes. Analogous to the baseline specification, it is also not possible to estimate the effect for the second group ($g = 2$) because voters in these municipalities already expect construction in the first election period ($t = g - 1$). Figure A-6 shows that the results are comparable to those of the baseline specification, suggesting that the estimated effects are not due to endogeneity. A limitation of the test is that it cannot detect the temporal endogeneity of the different treatment timings between each treatment and control group. It is possible that residents in municipalities seeing a turbine in their settlement area in an earlier election period were less likely to resist, which could also explain the switch to a negative effect of the last two timing groups. We will discuss this in Section 9. On the other hand, the pre-treatment vote share is less than or equal to that of the control group, indicating that support in these areas is similar to that in the control group, an argument for treatment exogeneity.

9 Interpretation and Discussion

Our preliminary results suggest that building a wind turbine that is visible from a settlement does not come with strong negative impact on the Green party's local vote share in most of the last decades. Nevertheless, there seems to be a recent trend in this direction, as suggested by the jump in the only significant coefficients between the last election periods in 2017 and 2021. Various factors might play a role in explaining this development.

First of all, wind turbines have already been built for many years in those regions which were geographically appropriate and where arguably local politicians as well as voters might have been more supportive. Once these low-hanging fruits have been grasped, locations have been chosen that might have been less inclined. These inherent differences between early wind turbine adopters and laggards that explain the varying effects over time. Moreover, this might be mirrored in the finding of Allcott (2015) about energy conservation programs in the U.S., namely that results from first adopters overstate the overall efficiency because of their concentration in the most environmentalist-friendly areas, which changes as the measure expands to the rest of the country. Similarly, wind turbines can now be thought to be expanding to some less supportive areas.

Several studies have shown that while wind farms are generally supported by a majority, political engagement by local citizens' initiatives can in turn have a negative impact on support for expansion (Hobman et al., 2012, Horbaty et al., 2012, Gardt et al., 2021). Azau (2011) estimates that 30 percent of unfinished wind farm projects in Europe are stopped due to litigation and public opposition. Similar to Gardt et al. (2021), we use data from Germany's largest anti-wind protest platform to identify the location of each initiative and determine in which municipality a group was active.[7] Up to the 2009 timing group, the share of municipalities reporting a citizens' initiative is below ten percent, after which it increases with the largest jump from the penultimate to the last timing group, where almost a quarter of all municipalities had an initiative within their municipal borders (Figure 9).[8] Thus, these initiatives may have delayed the installation of turbines until the last election periods by swaying public opinion to a rejectionist side, which could subsequently be reflected in the Green Party's vote share once the turbines were installed. Additionally, municipalities in the 2021 timing group are on average more densely populated and consist of more suburbs, implying that expansion in recent years has moved closer to the homes of more voters in the respective municipalities, increasing the likelihood of affecting voters who change their attitudes after exposure.

Besides the effect of early and late adopters of wind energy generation, the public debate about climate action also plays a role. In the years up to the 2021 election, the 'Fridays for Future' movement of young activists have put the issue on the political agenda and raised awareness, but also polarization (Fabel et al., 2022). Against the backdrop of this overall environmental agenda, the local effects of renewable energy propagation, namely the construction of wind turbines close the certain settlements, have been on more people's minds.

[7]The data is taken from the platform "windwahn.de" and includes the link to the website of the initiative as well as the geo-coordinates, but the information cannot be validated externally.

[8]Since the data on initiatives is limited to online registrations, the increase in the early timing groups may reflect the increase in internet usage, while the rise in the last timing group cannot be explained by this, given similar internet use in 2017 as in 2021.

Figure 9 – Share of municipalities with a citizens' initiative per timing group

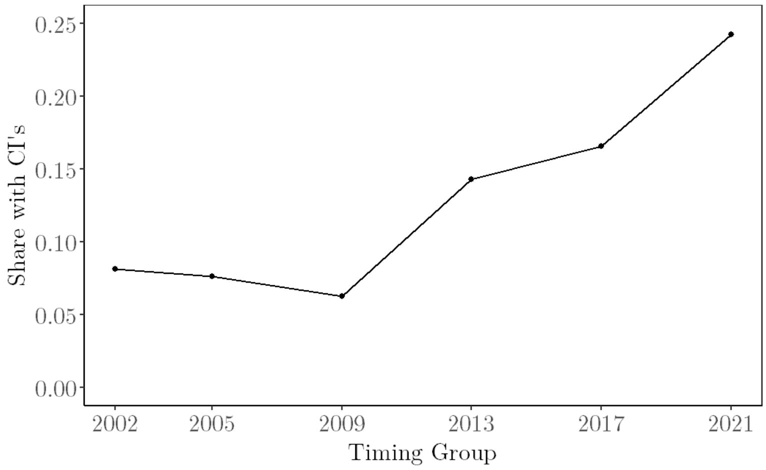
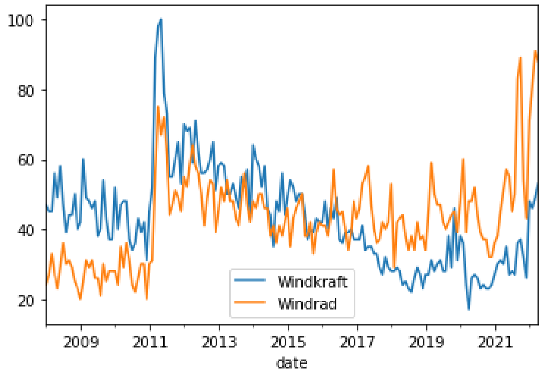


Figure 10 – Google trends search volumes for 'Windkraft' ('Wind energy') and 'Windrad' ('Wind turbine')



This is also illustrated by Google trends results of the German terms "Windkraft" ("Wind energy") and "Windrad" ("Wind turbine") from January 2008 to April 2022. Figure 10 shows that there is a notable jump in spring 2011 coinciding with the Fukushima nuclear disaster and the German government's decision to end the generation of nuclear energy. In the following years, fewer and fewer searches were conducted with the wind energy term as a concept, but more about wind turbines at the individual level. It is conceivable that the strong media coverage and the polarized public climate debate have intensified the reaction of some voters to the construction turbines in the vicinity.

There are obviously other aspects to consider, as well as some caveats. Crucially, our study only measures the reaction of potential Green voters, hence people who might consider voting for the Green party at all and whose voting decision would be affected by a visible wind turbine. Voters who would never even consider voting for the Greens might react to wind turbines in ways which we cannot consider in our study because there would be too many other confounding factors. On the other hand, this focus on the Green party brings with it the advantage of a clean identification. We might go as far as to suggest that any strongly negative effect would capture NIMBY behavior: These are potential Green voters, hence those who are purportedly in favor of renewable energy, yet vote against the Greens once a visible wind turbine 'in their own backyard' is built.

One caveat of our study is that we cannot measure the potential wind energy benefits of local residents in the form of cheaper electricity. Depending on the wind turbine operator, some local households are eligible to cheaper electricity, with the details varying across Germany (Diermann, 2023). It would be interesting to see to what extent these monetary benefits influence the acceptance of wind turbines despite their visibility.

10 Conclusion

We study the reactions of voters after the construction of a wind turbine in their visible neighborhood. Exploiting fine-grained data from Germany from 1998 to 2021 and robust econometric methods based on difference-in-difference with anticipation effects, we are able to reconcile some of the ambiguous empirical results to date. Yet, the prime contribution of this paper is based on our calculation of the wind turbines' viewshed, allowing us the determine to what extent each wind turbine in Germany is visible from nearby settlement areas. The 'visible intrusion of the landscape' (Wolsink, 2000, p.51) is one of the most cited arguments by local opponents of this form of energy generation, yet has never been analyzed in that way. Focusing on the visibility of turbines allows us also to elucidate possible NIMBYism, because people who oppose wind energy in general (e.g. because of bird endangerment) should do so whether or not the wind turbine is visible to them. Our analysis therefore leads to new insights on what drives the acceptance of wind turbines and whether the expansion of wind energy poses a risk to the vote share of pro-renewable parties in rural areas, further deepening the urban-rural divide.

Summarizing, our preliminary results suggest a cautious relief for pro-renewable energy parties. Constructing a wind turbine that is visible from a nearby settlement is not followed by a decrease in the Green party's local vote share in most of the last decades. However, our results also suggest that this general pattern changes from 2017 and intensifies in the 2021 election period, where a backlash is observed. The results echo the widely cited growing tensions over where development should occur, with expansion in more densely populated areas and those with lower levels of support, which is also reflected in an increase in the formation of local citizens' groups against these projects. While more research is needed on the channels to obtain public support, our study illustrates the importance of careful consideration of the local effects of global environmental policy.

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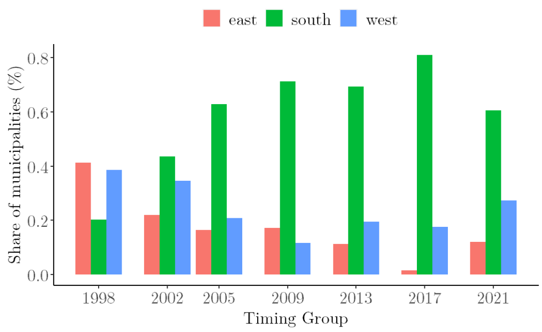
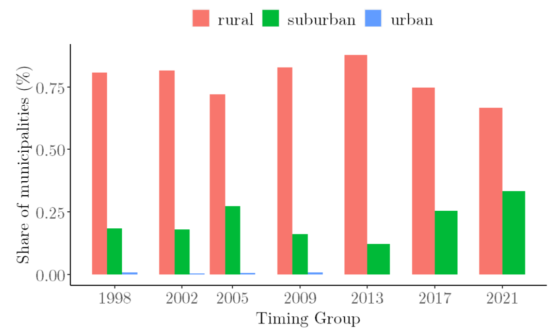
A Appendix: Additional Results and Robustness Checks

Figure A-1

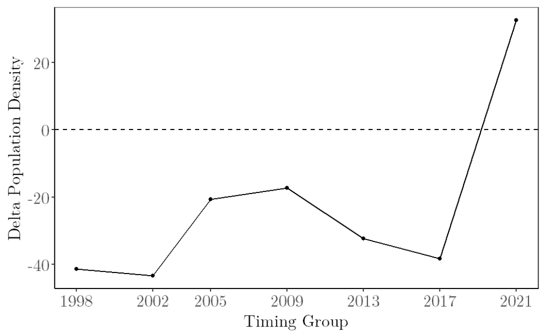
(a) Geographic distribution timing groups



(b) Urbanization distribution timing groups



(c) Population density per timing groups



(d) Number of municipalities per timing group

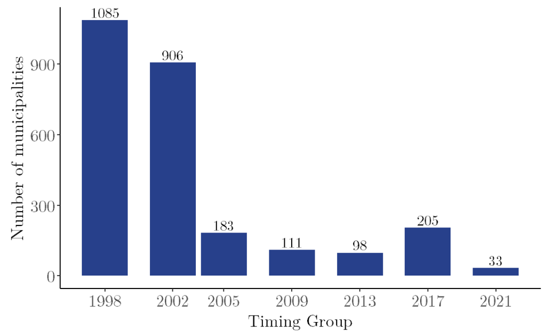
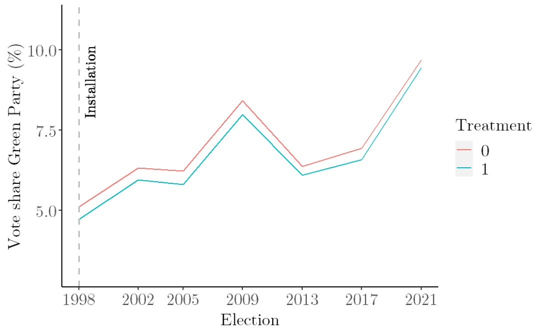
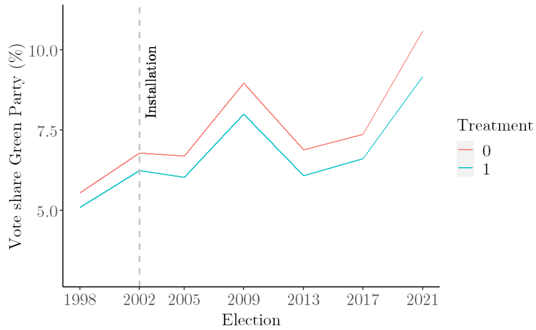


Figure A-2 – 2002

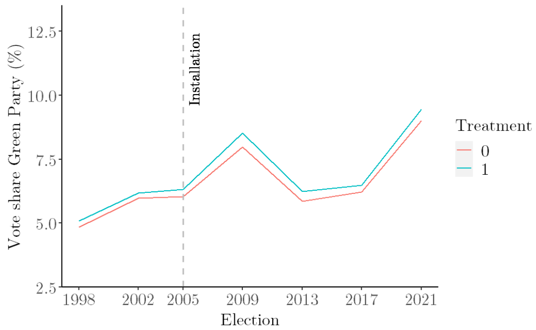
(a) Without covariates



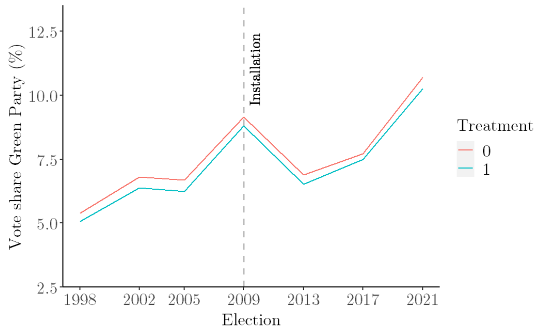
(b) 2005



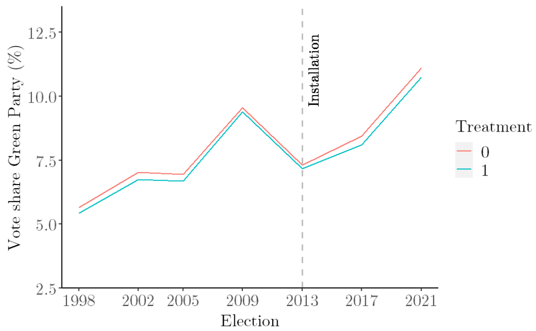
(c) 2009



(d) 2013



(e) 2017



(f) 2021

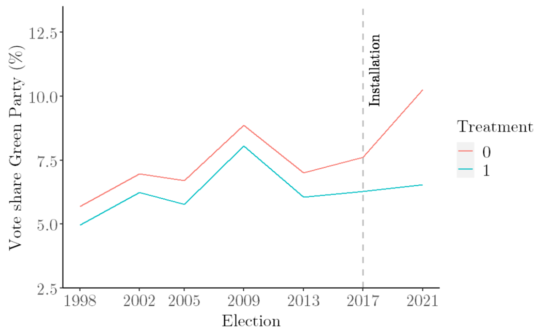


Figure A-3 – Aggregated event study

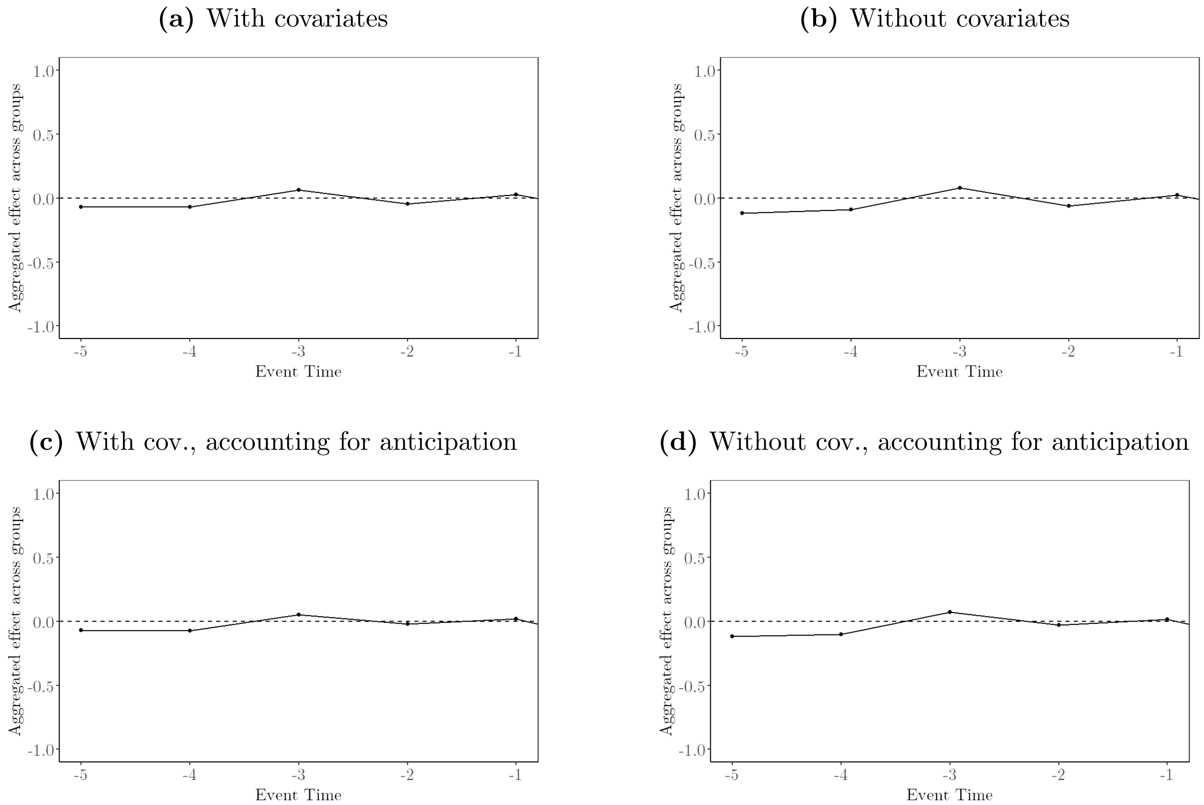


Figure A-4 – Number of turbines visible per timing group up to a 6km threshold

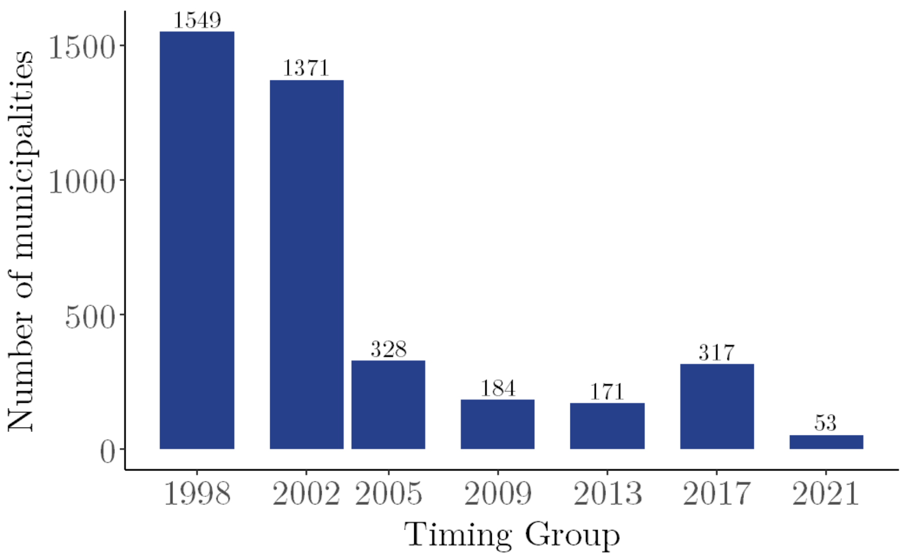
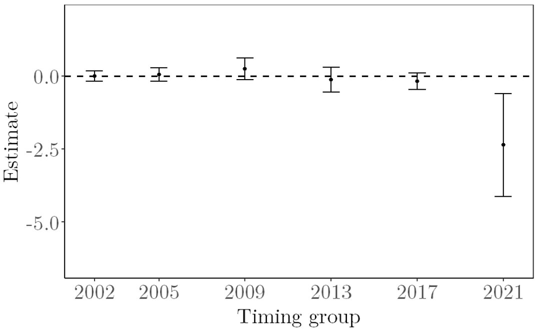
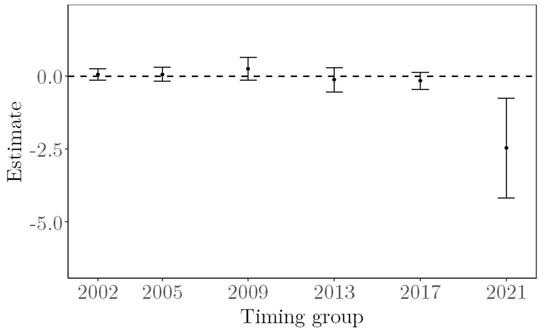


Figure A-5 – Estimated ATT for each timing group 6km

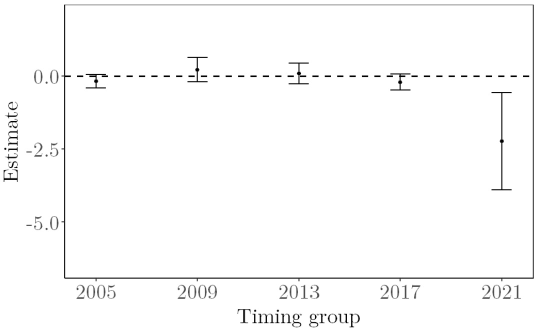
(a) With covariates



(b) Without covariates



(c) With cov., accounting for anticipation



(d) Without cov., accounting for anticipation

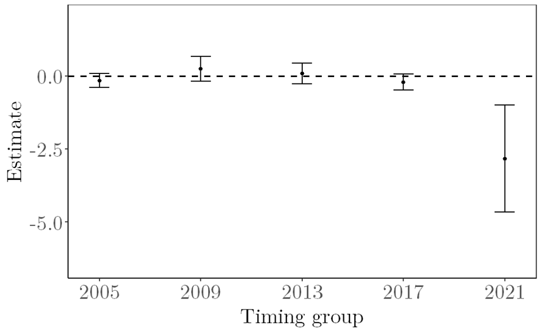


Table A-1 – ATT results for 2002

Dependent Variable:	Vote share Green Party (%)	
	No Anticipation	
Model:	(1)	(2)
Variables		
log(population density)	5.3*** (1.1)	
Share university degree (%)	0.89*** (0.23)	
Unemployment rate (%)	-0.16*** (0.03)	
Income tax revenue (PC)	0.35 (0.25)	
post × treat.view.majority.4.did	-0.09 (0.09)	-0.05 (0.11)
Fixed-effects		
Election Period	Yes	Yes
Municipality	Yes	Yes
Fit statistics		
Observations	6,910	6,910
R^2	0.90218	0.89506

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

Table A-2 – ATT results for 2005

Dependent Variable:	Vote share Green Party (%)			
	No Anticipation		Anticipation	
Model:	(1)	(2)	(3)	(4)
Variables				
log(population density)	-2.2 (2.2)		2.9*** (0.97)	
Share university degree (%)	-0.65* (0.37)		0.21 (0.17)	
Unemployment rate (%)	0.06 (0.07)		0.006 (0.08)	
Income tax revenue (PC)	-0.11 (0.29)		-0.17 (0.24)	
post × treat.view.majority.4.did	-0.02 (0.16)	-0.01 (0.16)	-0.18 (0.14)	-0.16 (0.14)
Fixed-effects				
Election Period	Yes	Yes	Yes	Yes
Municipality	Yes	Yes	Yes	Yes
Fit statistics				
Observations	1,464	1,464	1,464	1,464
R^2	0.94273	0.94173	0.92283	0.92089

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

Table A-3 – ATT results for 2009

Dependent Variable:	Vote share Green Party (%)			
	No Anticipation		Anticipation	
Model:	(1)	(2)	(3)	(4)
Variables				
log(population density)	-1.2 (3.6)		3.5** (1.7)	
Share university degree (%)	0.85* (0.50)		0.23 (0.39)	
Unemployment rate (%)	0.32** (0.13)		-0.006 (0.13)	
Income tax revenue (PC)	0.58 (0.98)		-0.17 (0.64)	
post × treat.view.majority.4.did	0.34 (0.28)	0.31 (0.28)	0.44 (0.29)	0.48 (0.29)
Fixed-effects				
Election Period	Yes	Yes	Yes	Yes
Municipality	Yes	Yes	Yes	Yes
Fit statistics				
Observations	888	888	888	888
R^2	0.88236	0.87503	0.86228	0.86026

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

Table A-4 – ATT results for 2013

Dependent Variable:	Vote share Green Party (%)			
	No Anticipation		Anticipation	
Model:	(1)	(2)	(3)	(4)
Variables				
log(population density)	-2.3 (3.9)		1.7 (2.4)	
Share university degree (%)	-0.009 (0.32)		0.09 (0.13)	
Unemployment rate (%)	0.03 (0.12)		0.05 (0.12)	
Income tax revenue (PC)	-0.35 (1.6)		-0.63 (0.45)	
post × treat.view.majority.4.did	-0.15 (0.23)	-0.16 (0.23)	-0.06 (0.22)	-0.06 (0.22)
Fixed-effects				
Election Period	Yes	Yes	Yes	Yes
Municipality	Yes	Yes	Yes	Yes
Fit statistics				
Observations	784	784	784	784
R^2	0.93624	0.93614	0.91018	0.90810

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

Table A-5 – ACR ($D > 2$) results for 2002

Dependent Variable:	Vote share Green Party (%)	
	No Anticipation	
Model:	(1)	(2)
Variables		
log(population density)	9.0*** (1.8)	
Share university degree (%)	0.17 (0.45)	
Unemployment rate (%)	-0.11* (0.07)	
Income tax revenue (PC)	0.33 (1.2)	
Mean turbines visible (N)	0.02 (0.02)	0.006 (0.03)
Fixed-effects		
Election Period	Yes	Yes
Municipality	Yes	Yes
Fit statistics		
Observations	974	974
R^2	0.89366	0.88185

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

Table A-6 – ACR ($D > 2$) results for 2005

Dependent Variable:	Vote share Green Party (%)			
	No Anticipation		Anticipation	
Model:	(1)	(2)	(3)	(4)
Variables				
log(population density)	-2.1 (3.6)		-2.1 (3.6)	
Share university degree (%)	-0.56 (0.69)		-0.56 (0.69)	
Unemployment rate (%)	0.36*** (0.13)		0.36*** (0.13)	
Income tax revenue (PC)	0.98 (0.88)		0.98 (0.88)	
Mean turbines visible (N)	0.03 (0.04)	0.03 (0.04)	0.03 (0.04)	0.03 (0.04)
Fixed-effects				
Election Period	Yes	Yes	Yes	Yes
Municipality	Yes	Yes	Yes	Yes
Fit statistics				
Observations	166	166	166	166
R^2	0.96572	0.96265	0.96572	0.96265

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

Table A-7 – ACR ($D > 2$) results for 2009

Dependent Variable:	Vote share Green Party (%)			
	No Anticipation		Anticipation	
Model:	(1)	(2)	(3)	(4)
Variables				
log(population density)	-7.5 (8.3)		5.9 (6.3)	
Share university degree (%)	-1.8 (1.1)		-0.04 (0.72)	
Unemployment rate (%)	0.53*** (0.14)		0.29 (0.20)	
Income tax revenue (PC)	-3.7** (1.7)		1.6 (2.4)	
Mean turbines visible (N)	-0.17 (0.13)	-0.21** (0.09)	-0.21** (0.10)	-0.26** (0.11)
Fixed-effects				
Election Period	Yes	Yes	Yes	Yes
Municipality	Yes	Yes	Yes	Yes
Fit statistics				
Observations	102	102	102	102
R^2	0.87414	0.85569	0.89263	0.88715

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

Table A-8 – ACR ($D > 2$) results for 2013

Dependent Variable:	Vote share Green Party (%)			
	No Anticipation		Anticipation	
Model:	(1)	(2)	(3)	(4)
Variables				
log(population density)	10.6* (6.0)		-5.5 (3.8)	
Share university degree (%)	-0.39 (0.31)		0.10 (0.20)	
Unemployment rate (%)	-0.81 (0.63)		0.26 (0.20)	
Income tax revenue (PC)	3.7* (2.0)		-0.17 (1.2)	
Mean turbines visible (N)	0.12 (0.09)	0.10 (0.08)	0.16*** (0.05)	0.19*** (0.04)
Fixed-effects				
Election Period	Yes	Yes	Yes	Yes
Municipality	Yes	Yes	Yes	Yes
Fit statistics				
Observations	106	106	106	106
R^2	0.95456	0.94661	0.95357	0.94758

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

Table A-9 – ACR ($D > 2$) results for 2017

Dependent Variable:	Vote share Green Party (%)			
	No Anticipation		Anticipation	
Model:	(1)	(2)	(3)	(4)
Variables				
log(population density)	11.1*** (3.8)		3.1 (3.0)	
Share university degree (%)	0.19 (0.31)		0.04 (0.21)	
Unemployment rate (%)	0.33 (0.44)		-0.30 (0.21)	
Income tax revenue (PC)	-0.11 (0.13)		0.005 (0.14)	
Mean turbines visible (N)	-0.07 (0.13)	-0.11 (0.12)	0.03 (0.14)	0.01 (0.14)
Fixed-effects				
Election Period	Yes	Yes	Yes	Yes
Municipality	Yes	Yes	Yes	Yes
Fit statistics				
Observations	242	242	242	242
R^2	0.93324	0.92546	0.93460	0.93319

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

Table A-10 – ACR ($D > 2$) results for 2021

Dependent Variable:	Vote share Green Party (%)			
	No Anticipation		Anticipation	
Model:	(1)	(2)	(3)	(4)
Variables				
log(population density)	77.4 (84.5)		11.1 (23.8)	
Share university degree (%)	2.8 (1.9)		0.82 (0.92)	
Unemployment rate (%)	-6.5 (3.7)		-4.2** (1.8)	
Income tax revenue (PC)	34.7 (24.0)		6.5 (10.9)	
Mean turbines visible (N)	0.12 (1.4)	-0.51 (1.1)	-1.9 (1.2)	-1.6 (1.2)
Fixed-effects				
Election Period	Yes	Yes	Yes	Yes
Municipality	Yes	Yes	Yes	Yes
Fit statistics				
Observations	34	34	34	34
R^2	0.86347	0.81059	0.88991	0.81646

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

Table A-11 – ACR ($D < 2$) results for 2002

Dependent Variable:	Vote share Green Party (%)	
	No Anticipation	
Model:	(1)	(2)
Variables		
log(population density)	5.4*** (1.1)	
Share university degree (%)	0.87*** (0.23)	
Unemployment rate (%)	-0.16*** (0.03)	
Income tax revenue (PC)	0.36 (0.25)	
Mean turbines visible (N)	0.0007 (0.02)	-0.007 (0.02)
Fixed-effects		
Election Period	Yes	Yes
Municipality	Yes	Yes
Fit statistics		
Observations	6,910	6,910
R^2	0.90214	0.89506

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

Table A-12 – ACR ($D < 2$) results for 2005

Dependent Variable:	Vote share Green Party (%)			
	No Anticipation		Anticipation	
Model:	(1)	(2)	(3)	(4)
Variables				
log(population density)	-2.2 (2.2)		-2.2 (2.2)	
Share university degree (%)	-0.64* (0.37)		-0.64* (0.37)	
Unemployment rate (%)	0.06 (0.07)		0.06 (0.07)	
Income tax revenue (PC)	-0.11 (0.29)		-0.11 (0.29)	
Mean turbines visible (N)	0.02 (0.03)	0.02 (0.03)	0.02 (0.03)	0.02 (0.03)
Fixed-effects				
Election Period	Yes	Yes	Yes	Yes
Municipality	Yes	Yes	Yes	Yes
Fit statistics				
Observations	1,464	1,464	1,464	1,464
R^2	0.94276	0.94178	0.94276	0.94178

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

Table A-13 – ACR ($D < 2$) results for 2009

Dependent Variable:	Vote share Green Party (%)			
	No Anticipation		Anticipation	
Model:	(1)	(2)	(3)	(4)
Variables				
log(population density)	-0.87 (3.5)		3.8** (1.8)	
Share university degree (%)	0.86* (0.50)		0.24 (0.40)	
Unemployment rate (%)	0.32** (0.13)		-0.003 (0.13)	
Income tax revenue (PC)	0.58 (0.98)		-0.11 (0.63)	
Mean turbines visible (N)	0.02 (0.06)	-0.005 (0.06)	0.02 (0.06)	0.006 (0.06)
Fixed-effects				
Election Period	Yes	Yes	Yes	Yes
Municipality	Yes	Yes	Yes	Yes
Fit statistics				
Observations	888	888	888	888
R^2	0.88192	0.87459	0.86149	0.85929

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

Table A-14 – ACR ($D < 2$) results for 2013

Dependent Variable:	Vote share Green Party (%)			
	No Anticipation		Anticipation	
Model:	(1)	(2)	(3)	(4)
Variables				
log(population density)	-2.4 (3.8)		1.5 (2.3)	
Share university degree (%)	-0.004 (0.31)		0.11 (0.13)	
Unemployment rate (%)	0.04 (0.12)		0.06 (0.12)	
Income tax revenue (PC)	-0.43 (1.6)		-0.69 (0.45)	
Mean turbines visible (N)	0.02 (0.04)	0.02 (0.04)	0.08** (0.04)	0.06 (0.04)
Fixed-effects				
Election Period	Yes	Yes	Yes	Yes
Municipality	Yes	Yes	Yes	Yes
Fit statistics				
Observations	784	784	784	784
R^2	0.93620	0.93609	0.91080	0.90849

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

Table A-15 – ACR ($D < 2$) results for 2017

Dependent Variable:	Vote share Green Party (%)			
	No Anticipation		Anticipation	
Model:	(1)	(2)	(3)	(4)
Variables				
log(population density)	7.6*** (2.7)		1.5 (1.9)	
Share university degree (%)	0.36* (0.20)		0.08 (0.10)	
Unemployment rate (%)	0.24* (0.13)		0.19 (0.13)	
Income tax revenue (PC)	0.04 (0.10)		-0.09 (0.12)	
Mean turbines visible (N)	-0.06 (0.06)	-0.08 (0.06)	-0.05 (0.05)	-0.05 (0.05)
Fixed-effects				
Election Period	Yes	Yes	Yes	Yes
Municipality	Yes	Yes	Yes	Yes
Fit statistics				
Observations	1,640	1,640	1,640	1,640
R^2	0.92148	0.91653	0.92864	0.92763

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

Figure A-6 – Estimated ATT for each timing group 'not yet treated' control group

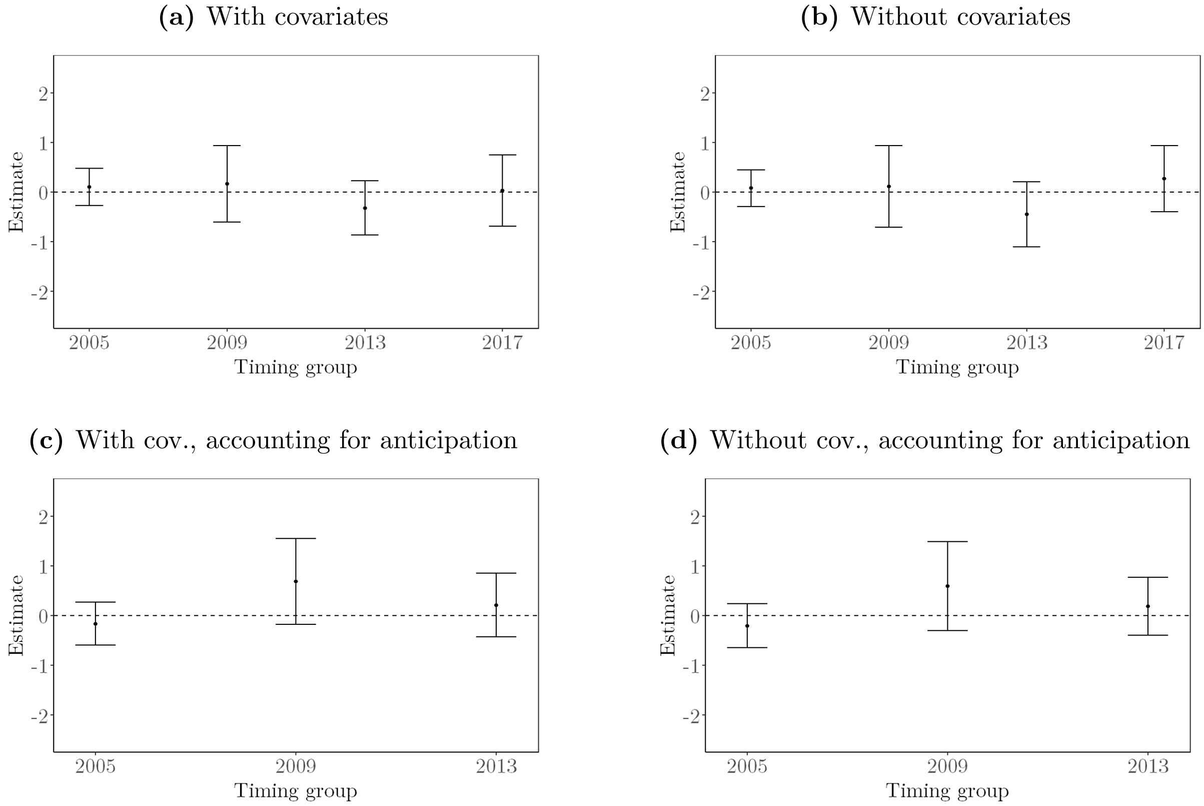


Table A-16 – ACR ($D < 2$) results for 2021

Dependent Variable:	Vote share Green Party (%)			
	No Anticipation		Anticipation	
Model:	(1)	(2)	(3)	(4)
Variables				
log(population density)	7.8 (11.6)		4.4 (7.9)	
Share university degree (%)	0.94** (0.36)		1.1*** (0.31)	
Unemployment rate (%)	-1.4 (0.84)		-0.16 (0.55)	
Income tax revenue (PC)	0.27 (0.64)		7.1 (7.8)	
Mean turbines visible (N)	-0.92** (0.36)	-0.89** (0.37)	-0.83** (0.32)	-1.0** (0.39)
Fixed-effects				
Election Period	Yes	Yes	Yes	Yes
Municipality	Yes	Yes	Yes	Yes
Fit statistics				
Observations	264	264	264	264
R^2	0.89429	0.88597	0.86442	0.83697

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