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# Does the presence of wind turbines have negative externalities for people in their surroundings? Evidence from well-being data

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ABSTRACT

Throughout the world, governments foster the deployment of wind power to mitigate negative externalities of conventional electricity generation, notably $CO_2$ emissions. Wind turbines, however, are not free of externalities themselves, particularly interference with landscape aesthetics. We quantify these negative externalities using the life satisfaction approach. To this end, we combine household data from the German Socio-Economic Panel Study (SOEP) with a novel panel dataset on over 20,000 installations. Based on geographical coordinates and construction dates, we establish causality in a difference-in-differences design. Matching techniques drawing on exogenous weather data and geographical locations of residence ensure common trend behaviour. We show that the construction of wind turbines close to households exerts significant negative external effects on residential well-being, although they seem both spatially and temporally limited, being restricted to about 4000 m around households and decaying after five years at the latest. Robustness checks, including view shed analyses based on digital terrain models and placebo regressions, confirm our results.



## Introduction

Since the 1990s, there has been a world-wide trend towards renewable resources for electricity generation. In OECD countries, the share of renewables, excluding hydro power, quadrupled from 1.8% to 7.2% between 1990 and 2012 (IEA, 2013). Wind power has been a major driver of this development: in the same time period, capacity and production grew by more than 20% annually (IEA, 2013). In Germany, for example, more than 20,000 wind turbines contributed 9% to total electricity consumption in 2014 (BMWi, 2015). Also in non-OECD countries, wind power plays an ever increasing role, for example, in China, being the world's biggest market by 2012 (WWEA, 2013). The economic rationale behind this trend is to avoid negative environmental externalities common to conventional electricity generation technologies. Beyond noxious local emissions from burning fossil fuels, carbon dioxide emissions are responsible for global climate change. Nuclear power is subject to unclear long-term storage of waste and low-probability but high-impact accidents.

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While wind power is largely free of emissions, waste, and risks, it is not free of externalities itself. Thereby, it is important to distinguish between *wind power* and *wind turbines*. Wind power, that is, electricity generated by wind turbines, might require costly changes within the electricity system, including the need to build more flexible backup capacities or expand the transmission grid. Wind turbines, in contrast to large centralised conventional power plants, which foster out-of-sight-out-of-mind attitudes, are more spatially dispersed and in greater proximity to consumers, increasing the salience of energy supply (Pasqualetti, 2000; Wüstenhagen et al., 2007). In fact, beyond unpleasant noise emissions (Bakker et al., 2012; McCunney et al., 2014) and impacts on wildlife (Pearce-Higgins et al., 2012; Schuster et al., 2015), most importantly, wind turbines have been found to have negative impacts on landscape aesthetics (Devine-Wright, 2005; Jobert et al., 2007; Wolsink, 2007). In general, no market prices exist for these negative externalities, so that they must be valued using alternative methods such as stated (Groothuis et al., 2008; Jones and Eiser, 2010; Meyerhoff et al., 2010) or revealed preference approaches (Gibbons, 2015; Heintzelman and Tuttle, 2012).

We investigate the effect of the presence of wind turbines on residential well-being and quantify their negative externalities using the so-called *life satisfaction approach*. To this end, we combine household data from the German Socio-Economic Panel Study (SOEP) with a novel panel dataset on more than 20,000 installations for the time period between 2000 and 2012. Trading off the decrease in life satisfaction caused by the presence of wind turbines against the increase caused by income, we value the negative externalities monetarily. As this approach has already been applied to various other environmental externalities, including air pollution (Ambrey et al., 2014; Ferreira et al., 2013; Levinson, 2012), landscape amenities (Kopmann and Rehdanz, 2013), noise pollution (Rehdanz and Maddison, 2008; van Praag and Baarsma, 2005), or flood disasters (Luechinger and Raschky, 2009), we contribute to a steadily growing stream of literature.

To estimate the causal effect of the presence of wind turbines on residential well-being, we employ a difference-in-differences design that exploits variation in wind turbine construction across space and over time: residents are allocated to the treatment group if a wind turbine is constructed within a pre-defined radius around their households, and to the control group otherwise. To ensure comparability of the treatment and control group, we apply, first, propensity-score matching based on socio-demographic characteristics, macroeconomic conditions, and exogenous weather data; and second, spatial matching techniques based on geographical locations of residence.

We show that the construction of a wind turbine within a radius of 4000 m has a significant and sizeable negative effect on life satisfaction. For larger radii, no negative externalities can be detected. Importantly, the effect seems to be transitory, vanishing after five years at the latest, and does not intensify with proximity or cumulation of installations. Robustness checks, including view shed analyses based on digital terrain models and placebo regressions, confirm these results. We arrive at a monetary valuation of these negative externalities for the current resident population between 564 Euro per affected household in total and 258 Euro per affected household and year, depending on the specification. Complementary hedonic regressions indicate a willingness-to-pay to avoid wind turbine construction in surroundings, which is internalised in annual rental prices, of up to 200 Euro.

To our knowledge, there exists only one working paper that investigates the effect of the presence of wind turbines on residential well-being, von Möllendorff and Welsch (2015), showing that they have a temporary negative impact. However, it differs from our paper in at least two important aspects: the authors do not account for self-selection of residents, and the data are only analysed at the post code level, i.e. life satisfaction is regressed on the number of wind turbines in a given post code area.

The rest of this paper is organised as follows: Section "Literature review" reviews the literature on negative externalities of wind turbines and different valuation approaches. Section "Data" describes the data, and Section "Empirical model" the empirical model. Results are presented in Section "Results", and discussed in Section "Discussion". Finally, Section "Conclusion" concludes and outlines avenues for future research.

## Literature review

### *Stated and revealed preference approaches*

Throughout contingent valuation studies, landscape externalities in the form of visual disamenities are found to be a crucial trigger of opposition to wind turbine projects (Groothuis et al., 2008; Jones and Eiser, 2010; Meyerhoff et al., 2010). Opposition is found to be shaped by two potentially opposing forces: proximity and habituation. Concerning proximity, most studies find a significant willingness-to-pay to locate planned installations further away from places of residence (Drechsler et al., 2011; Jones and Eiser, 2010; Meyerhoff et al., 2010; Molnarova et al., 2012). Concerning habituation, evidence is more mixed: while some papers detect decreasing acceptance (Ladenburg, 2010; Ladenburg et al., 2013), others find unchanged attitudes (Eltham et al., 2008) or adaptation (Warren et al., 2005; Wolsink, 2007) over time.

Likewise, hedonic studies, drawing on variations in real estate prices, find evidence for negative externalities caused by the construction of wind turbines, for example, in the United States (Heintzelman and Tuttle, 2012), Denmark (Jensen et al., 2014), the Netherlands (Dröes and Koster, 2016), Germany (Sunak and Madlener, 2016), and England and Wales (Gibbons, 2015). The decrease in real estate prices is found to range between 2% and 16%.

*Life satisfaction approach*

The life satisfaction approach (LSA) is an alternative to stated and revealed preference approaches. It specifies a microeconometric function relating self-reported life satisfaction to the environmental disamenity to be valued, along with income and other variables. Parameter estimates are then used to calculate the implicit marginal rate of substitution, that is, the amount of income a resident is willing to pay in order to avoid the environmental disamenity (Frey et al., 2004). Conceptually, life satisfaction, which is also referred to as subjective well-being (Welsch and Kühling, 2009) or experienced utility (Kahneman et al., 1997), can be defined as cognitive evaluation of the circumstances of life (Diener et al., 1999).

Compared to contingent valuation studies, the LSA avoids bias resulting from the expression of attitudes or the complexity of valuation. Stated preference approaches, in particular, are subject to symbolic valuation: what is measured may be intrinsic attitudes rather than extrinsic preferences. At the same time, they are prone to framing and anchoring effects (Kahneman and Sugden, 2005). The LSA, in contrast, does not ask residents to monetarily value a complex environmental disamenity in a hypothetical situation, which reduces cognitive burden. Likewise, it does not reveal the relationship of interest, mitigating the incentive to answer in a strategic or socially desirable way (Kahneman and Sugden, 2005; van der Horst, 2007).

Compared to hedonic studies, the LSA avoids bias resulting from the misconception that the real estate market is in, or close to, equilibrium. Typically, this occurs in case of slow adjustment of prices, incomplete information, and transaction costs (especially direct and indirect migration costs). It also avoids potentially distorted future risk expectations common to market transactions, as well as bias resulting from the misprediction of utility (Frey et al., 2004; Frey and Stutzer, 2014).

Intuitively, the LSA is not entirely free of methodological issues itself. For subjective well-being data to constitute a valid approximation of welfare, they have to be at least ordinal. Moreover, the microeconometric function relating self-reported life satisfaction to the environmental disamenity has to be specified correctly. These requirements are typically met in practice (Welsch and Kühling, 2009).

There is more debate about whether self-reported life satisfaction is an approximation of welfare in the first place. Recent research shows that people do not necessarily make choices that maximise their life satisfaction, for example, when making moving decisions (Glaeser et al., 2016). This seems to suggest that life satisfaction is not equal to utility, but rather one component in an individual's utility function, besides others such as income (Becker and Rayo, 2008; Benjamin et al., 2012). An emerging stream of literature argues that one other such component could be sense of meaning or purpose in life (White and Dolan, 2009). On the other hand, individuals might make prediction errors when trying to maximise their life satisfaction, be it white noise or systematic. This might be even more so the case when trading off losses in well-being today for gains in the future (Odermatt and Stutzer, 2015).

An extensive treatment of the validity of subjective well-being measures is beyond the scope of this paper. To be clear, we do not advocate to use life satisfaction as an exclusive criterion for environmental policy evaluation, but only use it as a vehicle to measure a negative externality. The life satisfaction approach itself does not hinge on the assumption that life satisfaction is equal to utility: rather, it assumes that it is a valid approximation. Adler et al. (2015), using a large population survey, show that people by and large tend to make life choices that score high on life satisfaction.

## Data

*Data on residential well-being*

We use panel data from the German Socio-Economic Panel Study (SOEP) for the time period between 2000 and 2012. The SOEP is a representative panel of private households in Germany, covering almost 30,000 individuals in 11,000 households every year (Wagner et al., 2007, 2008). Importantly, it provides information on the geographical locations of the places of residence, allowing to merge data on residential well-being with data on wind turbines.[1] Our dependent variable is *satisfaction with life*, which is obtained from an eleven-point single-item Likert scale that asks “How satisfied are you with your life, all things considered?”.[2]

*Data on wind turbines*

At the heart of our analysis lies a novel panel dataset on onshore wind turbines in Germany. For its creation, we drew on a variety of dispersed sources, mostly the environmental authorities in the sixteen federal states. If data were not freely accessible, we contacted the body in charge for granting access and filed a request for disclosure.[3] We brought together data

[1] The SOEP is subject to rigorous data protection legislation. It is never possible to derive the household data from coordinates since they are never visible to the researcher at the same time. See Goebel and Pauer (2014) for more information.

[2] In unreported regressions, we also examined whether wind turbine construction has an effect on health, using self-assessed health, as well as the mental and physical health items from the Short-Form (SF12v2) Health Survey, which has been incorporated into the SOEP. Overall, we find little evidence that these outcomes are affected.

[3] See Online Appendix B.6 for a detailed account and information on data protection.

on more than 20,000 wind turbines with construction dates ranging between 2000 and 2012. The core attributes rendering an observation suitable for our empirical analysis are (i) the exact geographical coordinates, (ii) the exact construction dates, and (iii) information on the size of the installation.

The exact geographical coordinates constitute the distinctly novel feature of our dataset: postal codes or addresses, as provided by the public transparency platform on renewable energy installations in Germany, would render an exact matching between individuals and installations impossible.[4] Moreover, the exact construction dates of installations are required in order to contrast them with the interview dates of individuals. Finally, we focus only on installations that exceed a certain size threshold: small installations are less likely to interfere with landscape aesthetics. It is also more likely that they are owned by private persons, and we might therefore measure effects other than negative externalities. Naturally, there is some degree of arbitrariness in determining a size threshold: beyond those without any information on size at all, we exclude all installations with a hub height of less than 50 m or a capacity of less than 0.5 MW. In doing so, we focus only on large installations that are typically constructed by utilities.[5]

Out of more than 20,000, we are left with a set of 10,083 wind turbines relevant for our analysis. These constitute the *included group*.[6] The other 10,554 constitute the *excluded group*.

### *Merge*

We merge the data on residential well-being with the data on wind turbines by calculating the distances between households and the nearest installation. Specifically, a treatment radius around each household is specified within which wind turbines of the *included group* trigger the household members to be allocated to the treatment group. If no such wind turbine is located within the treatment radius, the household members are allocated to the control group.

We subsequently check for each individual and year whether a wind turbine from the *excluded group* is located within the treatment radius at the interview date. Turbines from the *excluded group* receive special attention as households in their proximity should be discarded: they do not belong to either the treatment or control group. If both a turbine from the *included* and *excluded group* are present, however, then the individual is allocated to the treatment group if the first turbine built is from the *included group*, and discarded otherwise. See Fig. B.1 in the Online Appendix for a graphical illustration.

Some further data adjustments are made. Due to currentness of data, only years up to 2010 are included for the state of *Mecklenburg-Vorpommern*, up to 2011 for *Saxony*, and up to 2012 for all other states. Moreover, we discard individuals for which the interview date is given with insufficient accuracy in the year in which the first wind turbine is constructed in their surroundings: for those individuals, we cannot be sure whether they should be allocated to the treatment or control group. Finally, we discard individuals who "start" in the treatment group, for example, if they enter the panel while a wind turbine is already present in their surroundings: for them, no pre-treatment information to base inference on is given. Note that the size of the treatment and control group depends on the treatment radius chosen.

## Empirical model

### *Treatment radius*

As default treatment radius, we choose 4000 m, motivated by three considerations. First, we consider this radius close enough for wind turbines to unfold negative impacts. Second, it allows for a sufficient sample size, especially when stratifying the final sample to study different sub-groups. Finally, there is no uniform legislation in Germany that could serve as reference. Across time and states, the so-called *impact radius*, based on which intrusions into the environment are evaluated, varies between 1500 and 6000 m for a wind turbine with a hub height of 100 m. Beyond the 4000 m default treatment radius, we carry out various sensitivity analyses with other radii.

In addition, to achieve a clear-cut distinction between treatment and control group at the margin, we introduce a ban radius of 8000 m, twice the length of the treatment radius: residents who experience the construction of a turbine within the ban radius, but outside the treatment radius, are discarded.

### *Identification strategy*

To establish causality, we have to make three identifying assumptions. First, the interview date is random and unrelated to the construction date. In other words, residents should not strategically postpone interviews due to construction. We checked the distribution of interviews, and it seems that this is not the case. Second, in the absence of treatment, treatment and control group would have followed a common trend in the outcome over time. While this *common trend assumption* is

[4] The public transparency platform on renewable energy installations can be found at http://www.netztransparenz.de/de/Anlagenstammdaten.htm (in German), accessed June 1, 2015.

[5] We also focus only on installations that are built past 2000: before that, the SOEP does not provide information on the geographical locations of places of residence.

[6] See Table B.1 in the Online Appendix for descriptive statistics.

not formally testable, as the counterfactual is not observable, we apply propensity-score and spatial matching techniques, as described in Section "Matching treatment and control group", to ensure comparability between treatment and control group. In addition, we control for confounders that could cause remaining differences in time trends.[7] Finally, conditional ignorability implies that, conditional on covariates, construction is independent of the outcome, and therefore exogenous. In our setting, endogeneity may arise through two channels: endogenous construction or endogenous residential sorting. In other words, for certain residents it could be systematically more likely that either new wind turbines are constructed in their surroundings, or that they move away from or towards existing installations. In both cases, estimates would be biased if such endogenous assignment to treatment or control group is correlated with the outcome. We argue that both channels are mitigated.

Concerning endogenous construction, the siting process in Germany is driven business interests of project developers, which must adhere to governmental zoning law and the regulations on ecological impacts. Negotiation with affected residents or the legal right to appeal is, in general, not provided for. As such, we omit residents who live near small wind turbines; those installations are more likely to be built and run by private persons. Instead, we focus only on large installations that are typically constructed by utilities. Moreover, we omit residents who are farmers: these are more likely to let land to commercial operators.[8] Finally, we control for individual fixed effects and a rich set of time-varying observables at the micro level, originating from the SOEP, and at the macro level, originating from the Federal Statistical Office. The micro controls include demographic characteristics, human capital characteristics, and economic conditions at the individual level, as well as household characteristics and housing conditions at the household level; the macro controls include macroeconomic conditions and neighbourhood characteristics at the county level.[9] In doing so, we net out systematic differences between treatment and control group over time and at any point in time, ensuring common trend behaviour.[10]

In case of endogenous residential sorting, residents with lower (higher) preferences for wind turbines self-select into areas with greater (smaller) distances to them, whereby the preferences are correlated with the outcome. This can happen either prior to the observation period, so that we have an issue of *preference heterogeneity*, which we already account for by including individual fixed effects, or during the observation period, so that we have an issue of *simultaneity*.

In our baseline specification, we work around simultaneity by excluding residents who move, motivated by two reasons. First, if residential sorting is endogenous to wind turbine construction, the direction of bias resulting from the inclusion of movers is unclear. Depending on the type of move, theory predicts an attenuation or augmentation of estimates. For instance, hypothesising that wind turbines exert a negative effect on residential well-being, most adversely affected individuals are most likely to move away from installations, leading to a downward bias. On the other hand, individuals who move from the control to the treatment group may exhibit a lower aversion against wind turbines, leading to an upward bias. To this end, estimating for stayers provides clearer and undistorted evidence. Section "Robustness: residential sorting" provides a robustness check including movers. Besides that, endogenous residential sorting seems to be a quantitatively minor issue: geographical mobility in Germany is traditionally low. As a matter of fact, in our final sample, between 4% and 7% of all individuals move per year. Therefore, we expect bias resulting from the exclusion of movers not to overly blur results.

In general, our empirical strategy can be characterised as intention-to-treat approach: the definition of our treatment variable proxies the effect of the presence of wind turbines on residential well-being by a treatment radius. It implicitly assumes that every wind turbine is visible to every resident at any time, which is unlikely to be the case. For example, local topography and land cover might block the view from a household to a wind turbine.[11] On the other hand, households might adopt mitigating behaviour to block the view themselves, for example, by planting a tree or building a fence. Finally, we only have information on private households: some individuals, however, might spend considerable amounts of time outside their homes, for example, at work. They might thus be less permanently affected. Moreover, wind turbines can also unfold negative externalities on actual and potential temporary visitors like tourists, or non-use values, which cannot readily be captured by our approach. Therefore, our estimates can be interpreted as a lower bound, specifically for individuals who do not move away.

### *Matching treatment and control group*

Under the basic definition, the treatment group is relatively small, and concentrated in remote and rural areas, whereas the much larger control group is dispersed over the whole country. Individuals may thus not be comparable to each other, questioning the assumption of a common time trend between treatment and control group. We therefore restrict both

[7] Implicitly, we also require the *stable unit treatment value assumption* to hold: whether a wind turbine is constructed in the surroundings of one household should not depend on the outcome of another household. There is no a priori reason to believe that this is the case.

[8] In unreported robustness checks, we do not find that wind turbine construction increases income from renting out or leasing of nearby residents. The results are robust to the inclusion of farmers.

[9] The results are robust to replacing the macro controls with state–year fixed effects. Moreover, they are robust to including linear and quadratic time trends, both individually and jointly, and to including month and quarter-of-year fixed effects.

[10] The results are robust to omitting all of these controls, which reinforces the notion of ignorability, that is, wind turbine construction as an exogenous event.

[11] We investigate this issue further in Section "Robustness: view shed analysis" by performing a view shed analysis.

treatment and control group to individuals living in rural areas, excluding individuals living in city counties (*kreisfreie Städte*) and counties ranked in the top two deciles according to population density.[12] Moreover, we use two types of matching, prior to running our difference-in-differences regressions. See Fig. B.2 in the Online Appendix for a graphical illustration of both types of matching.

The first type of matching is *propensity-score matching*. Specifically, we use one-to-one nearest-neighbour matching on macro controls, including the unemployment rate, average monthly net household income, and population density at the county level, as well as state dummies. We match residents on the mean values of these variables, taken over the entire observation period. Alternatively, one could match individuals on their values in either the first year of the observation period or, in case that individuals enter the panel at a later point, in the first year in which they enter the panel. The resulting point estimates are similar in terms of significance, and slightly smaller in size.[13] We also match on a variable that captures local wind power adequacy, defined as the average annual energy yield of a wind turbine in kilowatt hours per square metre of rotor area, based on weather data from 1981 to 2000 (DWD, 2014). It encompasses a multitude of exogenous climatic and geographical factors. Specifically, it is based on wind velocity and aptitude, taking into account between-regional factors, such as coasts, and within-regional factors, such as cities, forests, and local topographies. Wind power adequacy is recorded on the basis of 1 km $\times$ 1 km tiles, distributed over the entire country. We match households with the nearest tile, and calculate the mean expected annual energy yield of a wind turbine from the 25 tiles surrounding it. See Fig. B.3 in the Online Appendix for a graphical illustration of this calculation.

Fig. 1 visualises how the dependent variable, *satisfaction with life*, evolves over time. The annual mean life satisfaction is shown for the matched control group (solid line) and the treatment group prior to treatment (dashed line).[14] All graphs control for confounders. As can be seen, the matched control and pre-treatment group co-move in a similar pattern over time; there is no evidence for a diverging time trend.

The second type of matching is *spatial matching*. It is based on the first law of geography, which states that closer things are more similar to each other. In this vein, it follows the idea that residents in close proximity to wind turbines are sufficiently similar to those living close but slightly farther away. We define a matching radius around each place of residence: individuals who are neither treated nor discarded, but experience the construction of a wind turbine within the matching radius, constitute the control group. In other words, we match residents who live close to an installation and close enough to be treated with those who live close but not close enough to be treated. We choose 10,000 and 15,000 m as matching radii, whereby the latter serves as default. Through spatial matching, the scope of the analysis is narrowed down to residents who are comparable in terms of local living conditions. Likewise, potential positive effects of wind turbines, in particular local economic benefits, can be mitigated: while both treatment and control group could profit to a certain extent from a wind turbine, only the treatment group within 4000 m distance is likely to be negatively affected by its presence.

Fig. 2 is constructed analogous to Fig. 1, using the default matching radius of 15,000 m. Again, there is no evidence for a diverging time trend between matched control and pre-treatment group. A similar picture arises for the matching radius of 10,000 m.

The descriptive statistics for the propensity-score matching specification are given in Table 1:[15] it shows the means of all covariates, overall and separately for treatment and control group, along with their scale-free normalised differences. Imbens and Wooldridge (2009) suggest that a normalised difference above 0.25 indicates covariate imbalance. Clearly, this is not the case for any of our covariates. Thus, we conclude that the final sample is well-balanced on observables.[16]

### *Regression equation*

We employ a linear model estimated by the fixed-effects (within) estimator.[17] The specification test by Wu (1973) and Hausman (1978), as well as the robust version by Wooldridge (2002), indicates that a fixed-effects specification is strictly preferable over a random-effects one: all tests reject the null of identical coefficients at the 1% significance level.[18] Robust standard errors are clustered at the federal state level.

Regression equation (1) estimates the overall treatment effect, with $Construction_{it,r}$ as the regressor of interest. $Construction_{it,r}$ is a dummy variable that equals one in time period $t$ if a wind turbine is present within treatment radius $r$

[12] The results are robust to the inclusion of individuals living in urban areas.

[13] The results are available upon request.

[14] The horizontal axis is restricted to the time period between 2000 and 2008. Thereafter, the pre-treatment group mean is based only on very few observations, and hardly delivers insightful information.

[15] See Table B.2 in the Online Appendix for the spatial matching specifications.

[16] Note that covariance imbalance between treatment and control group would not necessarily be a threat to our identification strategy: we control for a rich set of time-varying observables. Moreover, including individual and year fixed effects net out systematic differences in both time-invariant observables and unobservables between individuals and years, respectively.

[17] Note that using a linear model introduces measurement error, as *satisfaction with life* is a discrete, ordinal variable. However, this has become common practice, as discrete models for ordinal variables are not easily applicable to this type of estimator, and the bias resulting from this measurement error has been found to be negligible (see, for example, Ferrer-i-Carbonell and Frijters, 2004 for panel data, and Brereton et al., 2008; Ferreira and Moro, 2010 for repeated cross-section data).

[18] The empirical values of the test-statistic, 204.20 and 220.38 under propensity-score matching and 211.12 and 243.20 under spatial matching, exceed the critical value 56.06 of the $\chi^2$-distribution with 34 degrees of freedom.

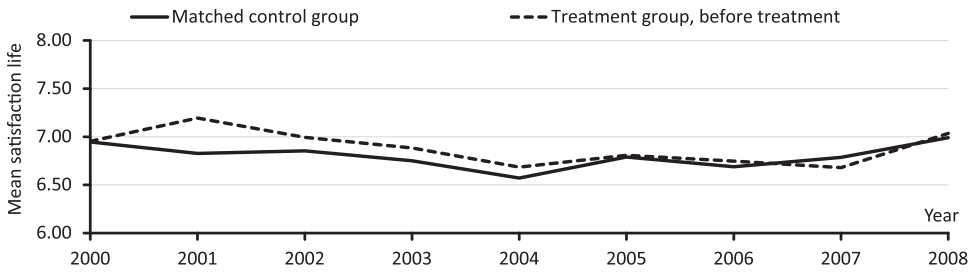


**Fig. 1.** Common Time Trend (Propensity-Score Matching).
*Source*: SOEP, v29 (2013), 2000–2012, individuals aged 17 or above, own calculations.

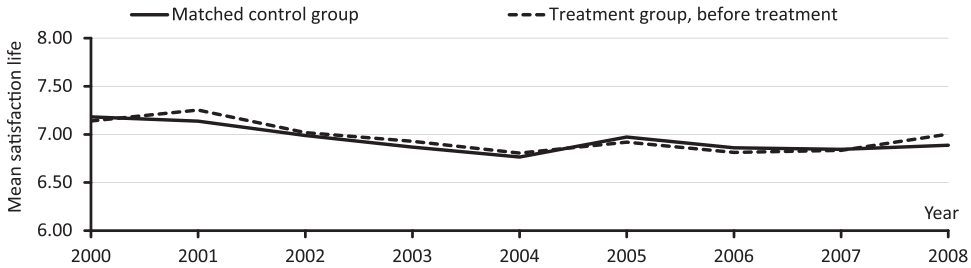


**Fig. 2.** Common Time Trend (Spatial Matching, 15,000 m).
*Source*: SOEP, v29 (2013), 2000–2012, individuals aged 17 or above, own calculations.

**Table 1**
Descriptive Statistics for Propensity-Score Matching (PS).

| Variables | Mean | | |
|---|---|---|---|
| | Treatment group (T) | Control group, PS (C) | Normalised difference (T)−(C) |
| *Micro controls* | | | |
| Age | 54.2053 | 52.3441 | 0.0875 |
| Is Female | 0.4991 | 0.5026 | 0.0050 |
| Is Married | 0.7829 | 0.7216 | 0.1006 |
| Is Divorced | 0.0481 | 0.0654 | 0.0530 |
| Is Widowed | 0.0735 | 0.0733 | 0.0006 |
| Has Very Good Health | 0.0566 | 0.0631 | 0.0194 |
| Has Very Bad Health | 0.0433 | 0.0481 | 0.0163 |
| Is Disabled | 0.1447 | 0.1243 | 0.0421 |
| Has Migration Background | 0.0881 | 0.0845 | 0.0089 |
| Has Tertiary Degree | 0.2828 | 0.3065 | 0.0369 |
| Has Lower Than Secondary Degree | 0.1844 | 0.1773 | 0.0130 |
| Is in Education | 0.0101 | 0.0184 | 0.0498 |
| Is Full-Time Employed | 0.3758 | 0.3779 | 0.0030 |
| Is Part-Time Employed | 0.1112 | 0.0770 | 0.0829 |
| Is on Parental Leave | 0.0068 | 0.0060 | 0.0069 |
| Is Unemployed | 0.0732 | 0.0954 | 0.0566 |
| Log Monthly Net Individual Income[a] | 6.4513 | 6.3143 | 0.1009 |
| Has Child in Household | 0.2277 | 0.2652 | 0.0616 |
| Log Annual Net Household Income[a] | 10.3718 | 10.2929 | 0.0984 |
| Lives in House[b] | 0.5538 | 0.5283 | 0.0376 |
| Lives in Small Apartment Building | 0.0896 | 0.0866 | 0.0067 |
| Lives in Large Apartment Building | 0.1589 | 0.1745 | 0.0312 |
| Lives in High Rise | 0.0113 | 0.0145 | 0.0211 |
| Number of Rooms per Individual | 1.7996 | 1.7686 | 0.0245 |
| *Macro controls* | | | |
| Unemployment Rate | 12.0116 | 13.7700 | 0.2139 |
| Average Monthly Net Household Income[a] | 1364.0120 | 1311.0680 | 0.1959 |
| Number of Observations | 3975 | 2662 | – |
| Number of Individuals | 498 | 488 | – |

[a]In Euro/Inflation-Adjusted (Base Year 2000). [b]Detached, Semi-Detached, or Terraced.
*Note*: The third column shows the normalised difference, which is calculated as $\Delta x = (\bar{x}_t - \bar{x}_c) \div \sqrt{\sigma_t^2 + \sigma_c^2}$, where $\bar{x}_t$ and $\bar{x}_c$ are the sample mean of the covariate for the treatment and control group, respectively. $\sigma^2$ denotes the variance. As a rule of thumb, a normalised difference greater than 0.25 indicates a non-balanced covariate, which might lead to sensitive results (Imbens and Wooldridge, 2009). All figures are rounded to four decimal places.
*Source*: SOEP, v29 (2013), 2000-2012, individuals aged 17 or above, own tabulations.

around the household of individual $i$, and zero else. Regression equation (2) estimates the treatment effect intensity, with the interaction $Construction_{it,r} \times Intensity_{it,r}$ as the regressor of interest. $Intensity_{it,r}$ is a place holder for different measures of treatment intensity: $InvDist_{it,r}$ is the inverse of the distance to the nearest installation in kilometres, $RevDist_{it,r}$ is the treatment radius minus the distance to the nearest installation, and $Cumul_{it,r}$ is the number of installations within the treatment radius. As more or more closely located wind turbines can be constructed during the observation period, the intensity can change over time. The two distance measures make different parametric assumptions. Regression equation (3) estimates the treatment effect persistence. The regressor of interest, $Trans_{it-\tau,r}$, is a dummy variable that equals one in time period $t$, which is $\tau$ periods after the construction of the first turbine within the treatment radius, and zero else:

$$y_{it} = \beta_0 + MIC_{\mathbf{it}}'\boldsymbol{\beta}_1 + MAC_{\mathbf{it}}'\boldsymbol{\beta}_2 + \delta_1 Construction_{it,r} + \sum_{n=1}^{12} \gamma_n Year_{2000+n} + \mu_i + \epsilon_{it} \tag{1}$$

$$y_{it} = \beta_0 + MIC_{\mathbf{it}}'\boldsymbol{\beta}_1 + MAC_{\mathbf{it}}'\boldsymbol{\beta}_2 + \delta_1 Construction_{it,r} \times Intensity_{it,r} + \sum_{n=1}^{12} \gamma_n Year_{2000+n} + \mu_i + \epsilon_{it} \tag{2}$$

$$y_{it} = \beta_0 + MIC_{\mathbf{it}}'\boldsymbol{\beta}_1 + MAC_{\mathbf{it}}'\boldsymbol{\beta}_2 + \sum_{\tau=1}^{9} \delta_\tau Trans_{it-\tau,r} + \sum_{n=1}^{12} \gamma_n Year_{2000+n} + \mu_i + \epsilon_{it} \tag{3}$$

where $y_{it}$ is *satisfaction with life* as the regressand; $\boldsymbol{MIC}_{it}$ and $\boldsymbol{MAC}_{it}$ are vectors of controls at the micro and macro level, respectively; and $Year_{2000+n}$ is a full set of yearly dummy variables. $\mu_i$ captures time-invariant unobserved heterogeneity at the individual level. $\epsilon_{it}$ is the idiosyncratic disturbance. $Construction_{it,r}$, $Construction_{it,r} \times Intensity_{it,r}$, and $Trans_{it-\tau,r}$, defined for all years past 2000, are the regressors of interest. The corresponding average treatment effects on the treated are captured by $\delta_1$ and $\delta_\tau$.

## Results

### *Overall treatment effect*

Table 2 reports the results of our difference-in-differences propensity-score and spatial matching specifications using the default treatment radius of 4000 m. For convenience, we only show our treatment variable here; detailed tables showing all covariates can be found in the Online Appendix.

For both matching specifications, a central result emerges: the presence of a wind turbine within the default treatment radius of 4000 m around households has a significant negative effect on life satisfaction at the 1% and 5% level, respectively. The size of this effect is also economically significant: under propensity-score matching, for instance, life satisfaction decreases by 8% of a standard deviation. Combining propensity-score with the spatial matching yields point estimates that are very similar to those of the standalone spatial matching specifications, regardless of matching

**Table 2**
Results – FE Models, Propensity-Score (PS) and Spatial (S) Matching, $Construction_{it,4000}$.

| Dependent Variable: Satisfaction With Life | | | |
|---|---|---|---|
| Regressors | PS | S (10, 000 m) | S (15, 000 m) |
| $Construction_{it,4000}$ | −0.1405*** | −0.1088*** | −0.1138** |
| | (0.0399) | (0.0222) | (0.0366) |
| Micro Controls | Yes | Yes | Yes |
| Macro Controls | Yes | Yes | Yes |
| Number of Observations | 6637 | 8609 | 16,378 |
| Number of Individuals | 986 | 1317 | 2586 |
| *of which in treatment group* | 498 | 506 | 506 |
| *of which in control group* | 488 | 811 | 2080 |
| Adjusted $R^2$ | 0.0657 | 0.0678 | 0.0632 |

*Note*: $Construction_{it,4000}$ is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a treatment radius of 4000 m in interview year $t$, and zero else. The dependent variable is life satisfaction on a 0/10 scale. All regression equations include dummy variables for interview years, individual fixed effects, and a constant. See Table 1 for the complete list and descriptive statistics of the micro and macro controls. All figures are rounded to four decimal places. Robust standard errors clustered at the federal state level in parentheses.
*Source*: SOEP, v29 (2013), 2000-2012, individuals aged 17 or above, sources in Online Appendix B.6, own calculations.
*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

**Table 3**
Results – FE Models, Propensity-Score (PS) and Spatial (S) Matching, $Construction_{it,4000} \times$ Intensity.

| Dependent Variable: Satisfaction With Life | | | | | | |
|---|---|---|---|---|---|---|
| | PS Intensity measure | | | S (15, 000 m) | | |
| Regressors | $InvDist_{it,4000}$ | $RevDist_{it,4000}$ | $Cumul_{it,4000}$ | $InvDist_{it,4000}$ | $RevDist_{it,4000}$ | $Cumul_{it,4000}$ |
| $Construction_{it,4000} \times Intensity$ | −0.2090 | −0.0128 | −0.0178 | −0.1862* | −0.0181 | −0.0174 |
| | (0.1605) | (0.0550) | (0.1556) | (0.0940) | (0.0338) | (0.0106) |
| Micro Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Macro Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of Observations | 6637 | 6637 | 6637 | 16,378 | 16,378 | 16,378 |
| Number of Individuals | 986 | 986 | 986 | 2586 | 2586 | 2586 |
| *of which in treatment group* | 498 | 498 | 498 | 506 | 506 | 506 |
| *of which in control group* | 488 | 488 | 488 | 2080 | 2080 | 2080 |
| Adjusted $R^2$ | 0.0650 | 0.0646 | 0.0659 | 0.0630 | 0.0629 | 0.0630 |

*Note*: $Construction_{it,4000}$ is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a treatment radius of 4000 m in interview year $t$, and zero else. The intensity measures are defined as follows: $InvDist_{it,4000}$ is the inverse distance, $RevDist_{it,4000}$ is equal to four minus the distance to the next wind turbine in kilometres, $Cumul_{it,4000}$ is equal to the number of wind turbines within a treatment radius of 4000 m, all in interview year $t$. The dependent variable is life satisfaction on a 0/10 scale. All regression equations include dummy variables for interview years, individual fixed effects, and a constant. See Table 1 for the complete list and descriptive statistics of the micro and macro controls. All figures are rounded to four decimal places. Robust standard errors clustered at the federal state level in parentheses.
*Source*: SOEP, v29 (2013), 2000-2012, individuals aged 17 or above, sources in Online Appendix B.6, own calculations.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

radius chosen, and significant at the 5% level.[19] The baseline specification thus provides evidence for significant negative local externalities.[20]

What happens if we increase the treatment radius? For 8000 and 10,000 m under propensity-score matching, coefficient estimates are negative but considerably smaller in size, $\delta_1 = -0.0348$ and $\delta_1 = -0.0074$, respectively, and insignificant at any conventional level. Likewise, no effect can be detected in case of a 15,000 m treatment radius.[21] An analogous result emerges for an increased treatment radius of 8000 m under spatial matching. This corroborates that we indeed systematically pick up negative local externalities triggered by the presence of wind turbines rather than local peculiarities: while closer proximity serves as a proxy for an undesired impact, for larger distances such an effect cannot be detected anymore.

### *Treatment effect intensity*

We explore treatment effect intensity next. In Table 3, for inverse distance, reverse distance, and cumulation, coefficient estimates have the expected sign, but none of them is significant for any matching specification.[22] It seems that the presence of a wind turbine in a 4 km radius itself is sufficient for negative externalities to arise, and specific intensity measures matter little in addition.

To explore this finding further, we investigate closer treatment radii below 4000 m under spatial matching (with propensity-score matching, the control group would have to be determined anew for each treatment radius, rendering comparability difficult). Specifically, we use 2000, 2500, and 3000 m as treatment radii, and in addition analyse different distance bands around treated individuals. For example, in band $[2000; 3000]$, only individuals experiencing wind turbine construction between 2000 and 3000 m around their places of residence are assigned to the treatment group; residents with wind turbines in closer proximity are dropped. Analogously, we specify bands between 2000 and 4000 m, 2500 and 4000 m, and 3000 and 4000 m. Table 4 reports the results for both spatial matching radii. For distances below 4000 m, no significant effects are detected, and neither is for the $[2000; 3000]$ band. For larger bands, however, coefficient estimates are negative, significant at the 1% or 5% level, and large in size.[23]

This finding can have several explanations. First, results can be driven by smaller sample sizes. In the baseline 4000 m specification, there are 506 treated individuals, decreasing to only 183 for 2000 m. Beyond such a potential statistical artefact,

[19] See Table B.10 in the Online Appendix for the combined matching specification.
[20] In Fig. B.4 in the Online Appendix, we illustrate the identified effect graphically. Here, we carried out a post-estimation analysis in an event study framework: we re-estimated the baseline specifications, normalised the point in time of treatment to $t$=0, and calculated the mean predicted life satisfaction for periods $t-5$ to $t+5$.
[21] For larger treatment radii, we apply no ban radius. See the Online Appendix for detailed results.
[22] The results for spatial matching with a 10,000 m matching radius are analoguous. See the Online Appendix for detailed results.
[23] Alternatively, instead of estimating separate sub-samples, one could interact the main effect with a dummy variable for the respective distance band: the results remain qualitatively the same.

**Table 4**
Results – FE Models, Closer Proximity and Distance Bands, Spatial (S) Matching, $Construction_{it,r/b}$.

| Dependent Variable: Satisfaction With Life | | | |
|---|---|---|---|
| Treatment radius $r$ | S (10, 000 m) $Construction_{it,r}$ | S (15, 000 m) $Construction_{it,r}$ | # treated |
| *2000* | −0.0254 | 0.0232 | 183 |
| | (0.1278) | (0.1107) | |
| *2500* | −0.0119 | −0.0169 | 274 |
| | (0.0717) | (0.0613) | |
| *3000* | −0.0450 | −0.0442 | 356 |
| | (0.0575) | (0.0589) | |
| *4000* | −0.1088*** | −0.1138** | 506 |
| | (0.0222) | (0.0366) | |
| Treatment band $b$ | $Construction_{it,b}$ | $Construction_{it,b}$ | # treated |
| *[2000; 3000]* | −0.0783 | −0.0827 | 243 |
| | (0.0549) | (0.0614) | |
| *[2000; 4000]* | −0.1711*** | −0.1749** | 411 |
| | (0.0423) | (0.0551) | |
| *[2500; 4000]* | −0.1860** | −0.1869** | 329 |
| | (0.0635) | (0.0754) | |
| *[3000; 4000]* | −0.1735** | −0.1799* | 232 |
| | (0.0725) | (0.0842) | |

*Note*: $Construction_{it,r}$ ($Construction_{it,b}$) is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a treatment radius of $r$ metres (treatment band $b$ in metres) in interview year $t$, and zero else. The treatment band $[x_1; x_2]$ comprises only those households that are located between $x_1$ and $x_2$ metres from the wind turbine. The dependent variable is life satisfaction on a 0/10 scale. All regression equations include micro controls, macro controls, dummy variables for interview years, individual fixed effects, and a constant. See Table 1 for the complete list and descriptive statistics of the micro and macro controls. All figures are rounded to four decimal places. Robust standard errors clustered at the federal state level in parentheses.
*Source*: SOEP, v29 (2013), 2000-2012, individuals aged 17 or above, sources in Online Appendix B.6, own calculations.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

residents in closer proximity may exhibit certain peculiarities: some could effectively profit from installations, for instance, by directed compensation measures. The turbine planning process in Germany prescribes an ecological impact compensation scheme, which could include, for example, a landscape upgrade by planting trees beside a road or the demolition of an abandoned building. As a rule of thumb, compensations should be close to impacts and in the same domain. Alternatively, individuals in particularly close distance could also actively erect wind turbines in their surroundings, and profit monetarily.[24] Although unlikely, we cannot fully exclude this case since we do not have information on the ownership structure of particular installations.

Concerning size and significance of coefficient estimates, this result is in line with the treatment effect for the default 4000 m radius: while the effect is much stronger within the [2000; 4000] band, it is insignificant for closer distances. Concerning directed compensation measures or active wind turbine erection by residents, results are in line with a lower-bound interpretation: as it cannot be excluded that some individuals in closer distances may profit, estimates are, if anything, attenuated, given that a significant negative overall treatment effect remains a robust finding. As discussed above, this lower-bound interpretation is consistent with the intention-to-treat definition of the treatment variable.[25]

In this respect, insignificant coefficient estimates for the different intensity measures are explained by non-significance of effects for smaller distances: if coefficients are insignificant for individuals living closer to wind turbines, treatment intensity increasing in proximity is obsolete.

### *Treatment effect transitoriness*

Intuitively, the question arises whether the presence of wind turbines has a permanent or transitory effect on residential well-being. Table 5 reports results on transitoriness for all matching specifications, including coefficient estimates for up to nine transition periods after the construction of a wind turbine within the default treatment radius of 4000 m. As can be seen, the effect seems to be temporally limited. It is significant at the 1% or 5% level from transition period two, that is, one year after the construction of a wind turbine, to at most transition period five. The size of the effect in each time period is

[24] In unreported robustness checks, we do not find that wind turbine construction decreases electricity costs of nearby residents. Recall that we neither find that it increases income from renting out or leasing.

[25] Impact compensation tends to be greater the closer and the larger the project. In this regard, point estimates for closer distance bands and for cumulation could be downward biased.

**Table 5**
Results - FE Models, Propensity-Score (PS) and Spatial (S) Matching, $Trans_{it-\tau,4000}$.

| Dependent Variable: Satisfaction With Life | | | | | |
|---|---|---|---|---|---|
| | PS | | S (10, 000 $m$) | S (15, 000 $m$) | |
| Regressors\Transitoriness Measure | $Trans_{it-\tau,4000}$ | # treated | $Trans_{it-\tau,4000}$ | $Trans_{it-\tau,4000}$ | # treated |
| $Trans_{it-1,4000}$ | −0.0546 | 498 | −0.0401 | −0.0392 | 506 |
| | (0.0642) | | (0.0657) | (0.0642) | |
| $Trans_{it-2,4000}$ | −0.1616** | 444 | −0.1212** | −0.1262** | 450 |
| | (0.0697) | | (0.0482) | (0.0697) | |
| $Trans_{it-3,4000}$ | −0.192** | 424 | −0.1381*** | −0.1506** | 430 |
| | (0.0609) | | (0.0411) | (0.0609) | |
| $Trans_{it-4,4000}$ | −0.2242** | 376 | −0.1808** | −0.1902* | 382 |
| | (0.0917) | | (0.0687) | (0.0917) | |
| $Trans_{it-5,4000}$ | −0.2253** | 335 | −0.1311 | −0.1472 | 341 |
| | (0.0924) | | (0.0837) | (0.0924) | |
| $Trans_{it-6,4000}$ | −0.2637 | 288 | −0.1664 | −0.1519 | 291 |
| | (0.1495) | | (0.1264) | (0.1495) | |
| $Trans_{it-7,4000}$ | −0.2215 | 240 | −0.0963 | −0.0744 | 243 |
| | (0.1271) | | (0.0941) | (0.1271) | |
| $Trans_{it-8,4000}$ | 0.0305 | 204 | 0.1847 | 0.2104 | 207 |
| | (0.1846) | | (0.1483) | (0.1846) | |
| $Trans_{it-9,4000}$ | −0.0679 | 167 | 0.0378 | −0.0778 | 170 |
| | (0.2816) | | (0.2452) | (0.2816) | |
| Micro Controls | Yes | | Yes | Yes | |
| Macro Controls | Yes | | Yes | Yes | |
| Number of Observations | 6,637 | | 16,378 | 16,378 | |
| Number of Individuals | 986 | | 1317 | 2586 | |
| *of which in control group* | 488 | | 811 | 2080 | |
| Adjusted $R^2$ | 0.0659 | | 0.0680 | 0.0635 | |

*Note*: $Trans_{it-\tau,4000}$ is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a 4,000 metres treatment radius in interview year $t-\tau$, and zero else. For example, $Trans_{it-3,4000}$ is the treatment dummy in the third year after the construction of the wind turbine. The dependent variable is life satisfaction on a 0/10 scale. All regression equations include dummy variables for interview years, individual fixed effects, and a constant. See Table 1 for the complete list and descriptive statistics of the micro and macro controls. All figures are rounded to four decimal places. Robust standard errors clustered at the federal state level in parentheses.
*Source*: SOEP, v29 (2013), 2000-2012, individuals aged 17 or above, sources in Online Appendix B.6, own calculations.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

somewhat larger than the size of the combined effect.

Note that a non-significant effect in transition period one is not surprising. While we use the construction date as reported in the data sources, in reality there might be some blur, which is picked up by the first-period coefficient: a wind turbine is usually not erected within a single day, and it is not stated explicitly whether the construction date marks the beginning or the end of the construction process. Additional sensitivity checks including a dummy variable for the time period before the construction of a wind turbine, on the contrary, provide no evidence of anticipation effects.[26]

This finding can have several explanations. First, current residents may adapt to the presence of wind turbines in their surroundings (it is difficult to make any inference on future residents, or temporary visitors, as they do not appear in the data). Alternatively, they may adjust to their presence, for example, by adopting mitigating behaviour such as planting a tree or building a fence. Second, the decay effect may be due to disamenities related to the construction process rather than the presence wind turbines. We believe that this is less likely to be the case, though, as the construction process of wind turbines is rather quick. Moreover, the non-significant effect in transition period one and the prolonged significant effects in transition periods thereafter point against this explanation. Finally, results may be driven by smaller sample sizes, as the treatment group size decreases over time. For a lag of nine years, construction from 2000 to 2003 is possible, whereas for shorter intervals more years are relevant. Note, however, that the point estimates remain reasonably robust as significance decreases. Non-significance may thus arise as a statistical artefact due to loss of power rather than a genuine decay effect.

### *Heterogeneity analysis*

To gain a more diverse picture, we apply our treatment effect analysis to different sub-groups. Table 6 reports the results for house owners versus renters, as well as for residents who are very concerned about the environment or climate change,

[26] See also Section "Robustness: placebo tests" for placebo tests using leads of the treatment variable.

**Table 6**
Results – Sub-Samples, FE Models, Spatial Matching (15, 000 m), $Construction_{it,4000}$.

| Dependent Variable: Satisfaction With Life | | | | | | |
|---|---|---|---|---|---|---|
| Regressors | (1) | (2) | (3) | (4) | (5) | (6) |
| $Construction_{it,4000}$ | −0.1261** | −0.0937 | −0.0711 | −0.1356** | 0.0634 | −0.2127*** |
| | (0.0488) | (0.1132) | (0.0686) | (0.0436) | (0.0499) | (0.0605) |
| Micro Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Macro Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of Observations | 12,570 | 3808 | 3934 | 12,350 | 5469 | 10,909 |
| Number of Individuals | 2047 | 700 | 1380 | 2400 | 722 | 1864 |
| *of which in treatment group* | 388 | 155 | 308 | 488 | 148 | 358 |
| *of which in control group* | 1659 | 545 | 1072 | 1912 | 587 | 1,506 |
| Adjusted $R^2$ | 0.0635 | 0.0733 | 0.0668 | 0.0636 | 0.0669 | 0.0650 |

(1) House-owners, (2) Non-house-owners, (3) Worries environment high, (4) Worries environment not high, (5) Worries climate change high, (6) Worries climate change not high.
*Note*: *Construction* $_{it,4000}$ is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a treatment radius of 4000 m in interview year *t*, and zero else. The dependent variable is life satisfaction on a 0/10 scale. All regression equations include dummy variables for interview years, individual fixed effects, and a constant. See Table 1 for the complete list and descriptive statistics of the micro and macro controls. All figures are rounded to four decimal places. Robust standard errors clustered at the federal state level in parentheses.
*Source*: SOEP, v29 (2013), 2000-2012, individuals aged 17 or above, sources in Online Appendix B.6, own calculations.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

respectively, versus residents who are not. The indicators on environmental and climate change concerns are obtained from single-item three-point Likert scales that ask respondents to rate how concerned they are about "environmental protection" or "climate change", respectively. We collapse these items into binary indicators that equal one for the highest category of concerns, and zero otherwise. Throughout all models, we use the difference-in-differences spatial matching specification with the default matching radius of 15,000 m; results are robust to using the matching radius of 10,000 m.

Stratifying along real estate ownership, the coefficient estimate for house owners shows a significant negative effect (first column), which is not the case for renters (second column). The size of the coefficient estimate is somewhat larger than at the aggregate level. Sensitivity analyses including land price at the county level as an additional control leave results on average and for the different sub-groups unchanged. One explanation for this finding may be that renters are more swiftly compensated through a decrease in rents, as the negative external effect is internalised through the price mechanism in rental markets, whereas for house owners this channel does not operate. In case of full internalisation for renters, we may not be able to detect any residual negative effect of the externality on life satisfaction. We explore this possibility in more detail by performing an additional hedonic analysis in Section "Discussion".[27]

Stratifying along environmental concerns, coefficient estimates for non-concerned individuals show significant negative effects (fourth column for environment, sixth for climate change), which is not the case for concerned individuals (third and fifth columns, respectively). Again, the size of coefficient estimates is higher than at the aggregate level. In this respect, we interpret environmental concerns as referring to more global rather than local impacts. Generally, wind turbines are regarded as environmentally friendly, and findings for residents who are environmentally aware are in line with that interpretation. Likewise, less environmentally aware individuals may have lower preferences for emission-free electricity production and, thus, be more sensitive towards intrusions into their surroundings.

### *Robustness: placebo tests*

To check the robustness of our results regarding confounding factors, we conduct placebo tests. Specifically, we include up to three leads of the treatment variable, first individually and then jointly in combination with the contemporary treatment variable, in both our default difference-in-differences propensity-score and spatial matching specifications. Table 7 reports the results.

As can be seen, none of the leads is significant at any conventional level, neither in the propensity-score – first to third column – nor spatial – fifth to seventh column – matching specification. They are also much smaller in size, and in case of the third lead even of opposite sign. When included jointly in combination with the contemporary treatment variable – fourth and eighth columns – they remain insignificant without clear pattern in terms of sign and size. The contemporary treatment variable, however, is still significant at the 1% level, negative, and large in magnitude. We take this as evidence that our estimates indeed systematically pick up the effect of wind turbine construction rather than confounding factors.[28]

[27] In this context, Luechinger (2009) provides a discussion of this complementarity between the life satisfaction approach and the hedonic method in the context of air pollutant emissions from fossil-fuelled power plants.
[28] This is also evidence that the construction of a wind turbine is a rather sudden, short-lived, and unanticipated event.

**Table 7**
Results – Robustness (Placebo Tests), FE Models, Propensity-Score (PS) and Spatial (S) Matching, $Construction_{it,4000}$.

| Dependent Variable: Satisfaction With Life | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| Regressors | PS | PS | PS | PS | S (15, 000 m) | S (15, 000 m) | S (15, 000 m) | S (15, 000 m) |
| F3.$Construction_{it,4000}$ (*Third Lead*) | | | 0.0806 | 0.0956 | | | 0.0772 | 0.1083 |
| | | | (0.0894) | (0.1109) | | | (0.0843) | (0.1119) |
| F2.$Construction_{it,4000}$ (*Second Lead*) | | −0.0208 | | −0.0470 | | −0.0163 | | −0.0335 |
| | | (0.0535) | | (0.1104) | | (0.0399) | | (0.1008) |
| F1.$Construction_{it,4000}$ (*First Lead*) | −0.0650 | | | 0.0474 | −0.0593 | | | 0.0421 |
| | (0.0505) | | | (0.0949) | (0.0536) | | | (0.0939) |
| $Construction_{it,4000}$ | | | | −0.1354*** | | | | −0.1239*** |
| | | | | (0.0396) | | | | (0.0313) |
| Micro Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Macro Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of Observations | 6189 | 5843 | 5274 | 5274 | 15,235 | 14,408 | 12,988 | 12,988 |
| Number of Individuals | 897 | 872 | 819 | 819 | 2306 | 2246 | 2090 | 2090 |
| *of which in treatment group* | 496 | 492 | 479 | 479 | 504 | 500 | 486 | 486 |
| *of which in control group* | 401 | 380 | 340 | 340 | 1802 | 1746 | 1604 | 1604 |
| Adjusted $R^2$ | 0.0536 | 0.0517 | 0.0499 | 0.0503 | 0.0561 | 0.0541 | 0.0531 | 0.0532 |

*Note*: $Construction_{it,4000}$ is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a treatment radius of 4000 m in interview year *t*, and zero else. The dependent variable is life satisfaction on a 0/10 scale. All regression equations include dummy variables for interview years, individual fixed effects, and a constant. See Table 1 for the complete list and descriptive statistics of the micro and macro controls. All figures are rounded to four decimal places. Robust standard errors clustered at the federal state level in parentheses.
*Source*: SOEP, v29 (2013), 2000-2012, individuals aged 17 or above, sources in Online Appendix B.6, own calculations.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

*Robustness: view shed analysis*

To check the robustness of our results regarding actual visual relationships between households and installations, we combined our geographical information on households and wind turbines with a digital terrain model for Germany (BKG, 2016). This also provides further insight into disentangling the identified negative externalities into landscape aesthetics and other channels. To be clear, a digital terrain model includes only geographical barriers to visibility such as location-specific elevated terrain, while excluding natural ones such as forests and trees as well as man-made structures such as houses and fences, all of which may equally be barriers to visibility. However, to the extent that the latter are built on purpose in order to block visibility, individuals who built them are presumably those that are most adversely affected. In this vein, our estimates can be interpreted as a lower bound.

We created a new treatment group of households that are located within the default treatment radius of 4000 m and that have a direct view of wind turbines, as well as a corresponding new measure of treatment intensity – the visible height of wind turbines from the viewpoint of households. Based on these, we performed a view shed analysis. The results are presented in Table 8.

As can be seen, the point estimates using the new treatment group definition are very similar to those using the old, in both our propensity-score – first column – and spatial – third column – matching specification. In fact, they are only slightly smaller in size and slightly less significant; the latter is most likely due to the loss of observations resulting from wind turbines covered by terrain. Moreover, the second and fourth columns show that, when using the new treatment group definition and interacting the main effect with the visible height of the nearest installation, life satisfaction drops significantly for each metre rise in visibility. From all measures of treatment intensity, the visible height of wind turbines from the viewpoint of households is the only measure that turns out significant.[29]

We take this as evidence that the identified negative externalities associated with the construction of wind turbines are indeed foremost driven by negative impacts on landscape aesthetics. The aggravating effect of the visible height of the nearest installation suggests that they are mainly driven by households that stand in direct visual relationship to them; however, the vast majority of households in our sample (about 92%) can see at least part of the nearest installation. Likewise, this suggests that negative externalities from wind turbines could be reduced by, for example, creating visual barriers in carefully selected spots in the sight line between affected households and turbines.

*Robustness: residential sorting*

So far, we have excluded movers from all our analyses. To evaluate the extent to which simultaneity and resulting

[29] We also recalculated all of our other intensity measures, including the inverse and reverse distance to the nearest installation, as well as the cumulative number of installations around the household, for the new treatment group. We did the same for our measures of treatment transitoriness. The results, which are available upon request, confirm our baseline results.

**Table 8**
Results – Robustness (View Shed Analysis), FE Models, Propensity-Score (PS) and Spatial (S) Matching, $Construction_{it,4000}$.

| Dependent Variable: Satisfaction With Life | | | | |
|---|---|---|---|---|
| Regressors | PS | PS | S (15, 000 m) | S (15, 000 m) |
| $ConstructionVisible_{it,4000}$ | −0.1388** | | −0.1082** | |
| | (0.0471) | | (0.0381) | |
| $ConstructionVisible_{it,4000} \times HeightVisible_{it,4000}$ | | −0.0013** | | −0.0010** |
| | | (0.0005) | | (0.0004) |
| Micro Controls | Yes | Yes | Yes | Yes |
| Macro Controls | Yes | Yes | Yes | Yes |
| Number of Observations | 6273 | 6273 | 16,013 | 16,013 |
| Number of Individuals | 939 | 939 | 2538 | 2538 |
| *of which in treatment group* | 451 | 451 | 458 | 458 |
| *of which in control group* | 488 | 488 | 2080 | 2080 |
| Adjusted $R^2$ | 0.0623 | 0.0624 | 0.0615 | 0.0616 |

*Note*: $ConstructionVisible_{it,4000}$ is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a treatment radius of 4000 m in interview year $t$ and the household has a direct view on it, and zero else. $HeightVisible_{it,4000}$ is the corresponding visible height of the wind turbine from the viewpoint of the household in metres. The dependent variable is life satisfaction on a 0/10 scale. All regression equations include dummy variables for interview years, individual fixed effects, and a constant. See Table 1 for the complete list and descriptive statistics of the micro and macro controls. All figures are rounded to four decimal places. Robust standard errors clustered at the federal state level in parentheses.
*Source*: Federal Agency for Cartography and Geodesy (BKG) (2016), SOEP, v29 (2013), 2000-2012, individuals aged 17 or above, sources in Online Appendix B.6, own calculations.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

endogeneity plays a role, we conduct two robustness checks on a sample augmented by movers.

First, we analyse moving reasons. Descriptive statistics, as recorded in the SOEP, indicate that about 87% of moves are due to reasons that are not linked to geographical location. To dig deeper, we estimate linear probability models that regress a dummy indicating a move since the last period on the treatment dummy. Otherwise, the models are equivalent to our baseline specifications. Results show that the construction of a wind turbine in the default treatment radius of 4000 m has no significant effect on the probability of moving; see Table B.11 in the Online Appendix. In either matching specification, point estimates are close to zero. Thus, we do not find empirical evidence that residential sorting is endogenous to wind turbine construction.

Second, we re-estimate our baseline spatial matching specification including both movers and non-movers. In doing so, we extend our baseline data quality requirements and exclude individuals who violate one of them before or after a move, for instance when relocating to an urban area, as well as individuals who moved in the period prior to their first observation. An additional dummy captures the effect of having moved as such. To ensure comparability with the treatment group, we impose a minimum timespan an individual must have remained in the control group: it varies between one year, which constitutes no additional restriction, and six years.

Table 9 summarises the results. The additional requirement of a minimum spell in the control group leaves baseline findings without movers virtually unchanged. However, the inclusion of movers attenuates estimates, while significance is preserved for the 10,000 m spatial matching specification. Selectively adding subgroups of movers to the baseline model shows that individuals who relocate from the treatment to the treatment group trigger the strongest attenuation. When dropping such movers, estimates are larger and statistically significant for both matching radii.

Recall that our research design can be characterised as an intention-to-treat approach: treated individuals can be expected to be unequally strongly affected. While we do not find empirical evidence for *endogenous residential sorting* with respect to turbine construction, theory predicts that individuals when relocating due to other reasons – and transaction costs can be regarded as partially sunk – take wind turbines into account and optimise with regard to their actual impacts. Such *conditional residential sorting*, together with the intention-to-treat character of our analysis, provides the lens through which to understand findings from the robustness check: especially for treat-to-treat and control-to-treat movers, one can expect that relocation occurs to sites in which turbines are less salient, thus attenuating estimates of treatment effects. For control-to-control and to a lesser extent for treat-to-control movers, indirect effects can be expected to likewise attenuate estimates: as turbine construction reduces the choice set for relocating, utility will decline, thus yielding a smaller wedge between control and treated individuals.

Taken together, while theory predicts that simultaneity leads to a bias whose direction is unclear, empirical evidence does not support endogenous residential sorting with respect to treatment. Rather, if an individual decides to move and transaction costs can be regarded as partially sunk, self-selection into less affected sites is rational, and attenuating estimates.

**Table 9**
Robustness (Residential Sorting – Sample Includes Movers) – FE Models, Spatial (S) Matching, $Construction_{it,4000}$.

| Dependent Variable: Satisfaction With Life | | | | | | |
|---|---|---|---|---|---|---|
| | S (10, 000 m) | | | S (15, 000 m) | | |
| Control spell | No movers $Construction_{it,4000}$ | All movers $Construction_{it,4000}$ | All movers except TT $Construction_{it,4000}$ | No movers $Construction_{it,4000}$ | All movers $Construction_{it,4000}$ | All movers except TT $Construction_{it,4000}$ |
| 1 | −0.1086*** | −0.0761** | −0.0882*** | −0.1143** | −0.0732 | −0.0809* |
| | (0.0229) | (0.0313) | (0.0269) | (0.0367) | (0.0516) | (0.0428) |
| 2 | −0.1111*** | −0.0712** | −0.0844*** | −0.1160*** | −0.0713 | −0.0799* |
| | (0.0203) | (0.0287) | (0.0259) | (0.0343) | (0.0482) | (0.0399) |
| 4 | −0.1224*** | −0.0729** | −0.0873*** | −0.1236*** | −0.0798 | −0.0894** |
| | (0.0235) | (0.0242) | (0.0214) | (0.0340) | (0.0478) | (0.0398) |
| 6 | −0.1031** | −0.0603** | −0.0763*** | −0.1349*** | −0.0913* | −0.1027** |
| | (0.0332) | (0.0205) | (0.0213) | (0.0314) | (0.0413) | (0.0356) |

*Note*: $Construction_{it,4000}$ is a treatment dummy variable based on the exact interview date that is equal to one if a wind turbine is present within a treatment radius of 4000 m in interview year *t*, and zero else. The dependent variable is life satisfaction on a 0/10 scale. All regression equations include dummy variables for interview years, individual fixed effects, and a constant. See Table 1 for the complete list and descriptive statistics of the micro and macro controls. Rows show results for different minimum control spells, that is the timespan an individual must have remained within the control to be included in the analysis. Columns two and five contain specifications without movers, columns three and six specifications including all movers, and columns four and seven specifications including all movers except individuals who move from the treatment to the treatment group. All figures are rounded to four decimal places. Robust standard errors clustered at the federal state level in parentheses.
*Source*: SOEP, v29 (2013), 2000-2012, individuals aged 17 or above, sources in Online Appendix B.6, own calculations.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

## Discussion

Our findings provide empirical evidence that the presence of wind turbines does entail negative externalities, though limited in both space and time. It is not unequivocal where exactly to delineate effectiveness of these externalities, though: clearly, they impact residents in their surroundings who choose to stay. However, they also influence all potential residents of the area who decide not to move there, just as developers who decide not to project new residences. Likewise, the recreational value of the landscape can be devalued, with impacts on both visitors and potential visitors. Finally, non-use values of natural and cultural landscapes as well as species can be affected.

A monetisation of the negative external effects of wind turbines, let alone a comprehensive cost–benefit account, is therefore difficult to conduct.[30] Based on our findings, however, we can draw some modest conclusions for affected residents in their immediate surroundings who decide not to move away. Also here, some caveats apply. First, regression coefficients capture marginal effects, while changes to be valued are greater than marginal. Likewise, the impact of income on life satisfaction may comprise more subtle aspects like relative comparisons to the past or to others. Moreover, evidence suggests that quantifications using well-being data may overestimate the monetary effect of an environmental externality. Likewise, the life satisfaction approach has been shown to result in relatively low trade-off ratios between the externality to be valued and individual characteristics such as whether an individual is unemployed (Luechinger, 2009). Numbers derived here are thus an informed point of reference.

We provide both a lower and an upper bound for the monetised negative externalities. For the lower bound, we draw on results from the 10,000 m radius matching, as in Table 5, where only coefficient estimates for transition periods two to four are significantly negative at a conventional level. The log annual net household income for the treatment group amounts to 10.4, as in Table 1. A one per cent increase in annual income thus corresponds to 319.5 Euro. Trading off the positive coefficient of income against the three negative coefficients of the treatment, each affected household is on average impacted by a monetised externality of about 564 Euro in total; 155 Euro for the second year, 177 Euro for the third, and 232 Euro for the fourth. For the upper bound, we suppose a permanent effect and take the coefficient estimate largest in size from the propensity-score matching. Applying the same calculus, the monetised negative externality amounts to 258 Euro per year for each affected household.

Recall that in our heterogeneity analysis, we found the negative external effect on the well-being of house owners to be significant and stronger than at the aggregate level, whereas it was insignificant for renters. We conjectured that renters may be more swiftly compensated through a decrease in rents, as the negative external effect is internalised through the price mechanism in rental markets. To put this to test, we conducted an additional hedonic analysis: we re-estimated our baseline specifications using log annual net rents as outcome while controlling for a wide range of dwelling and amenity characteristics.[31] We find that wind turbine construction is associated with a decrease of about 4% in annual net rents,

[30] Additionally, effects of intermittent wind power, that is electricity generated by wind turbines, within the electricity system are nontrivial to quantify (Borenstein, 2012; Hirth et al., 2016).
[31] We estimated the following log-level hedonic regression:

which amounts to a decrease of approximately 200 Euro per year for each affected household, similar to our upper-bound estimate obtained from using well-being data. However, this effect is only present in our spatial matching specifications. With propensity-score matching, estimates are small and insignificant.

Contrary to rental prices, the SOEP does not contain house prices. Instead, it asks house owners to estimate their house prices and convert them into hypothetical rents. We do not find significant effects of wind turbine construction on such hypothetical annual net rents in any of our specifications.[32]

## Conclusion

In many countries, wind power plays an ever increasing role in electricity generation. The economic rationale behind this trend is to avoid negative environmental externalities common to conventional technologies: wind power is largely free of emissions from fossil fuel combustion, as well as waste and risks from nuclear fission. For instance, the German Environment Agency calculated for 2012 that onshore wind energy saved approximately 39 million tons of $CO_2$ emissions in Germany (UBA, 2013). With current estimates of damage costs between roughly 50 and 100 Euro per ton (Foley et al., 2013; van den Bergh and Botzen, 2014, 2015), avoided externalities are large. For wind power to play an effective role, however, wind turbines must be constructed in large numbers, rendering them more spatially dispersed. In fact, the greater proximity of wind turbines to consumers has been found to have negative externalities itself, most importantly negative impacts on landscape aesthetics.

Against this background, we investigated the effect of wind turbines on residential well-being in Germany, combining household data from the German Socio-Economic Panel Study (SOEP) with a unique and novel panel dataset on more than 20,000 wind turbines for the time period between 2000 and 2012. Employing a difference-in-differences design that exploits the exact geographical coordinates of households and turbines, as well as their interview and construction dates, we established causality. To ensure comparability of the treatment and control group, we applied propensity-score and spatial matching techniques based, among others, on exogenous weather data and geographical locations of residence. We showed that the construction of a wind turbine in the surroundings of households has a significant negative effect on life satisfaction. Importantly, this effect seems both spatially and temporally limited, being restricted to about 4000 m around households and decaying after five years at the latest. The results are robust to using different model specifications. Additional robustness checks, including view shed analyses based on digital terrain models and placebo regressions, confirm our results.

We arrived at a monetary valuation of the negative externalities between 564 Euro per household in total when supposing a vanishing effect, and 258 Euro per household and year when supposing a permanent disamenity, in a 4000 m radius around households. An additional hedonic analysis confirms the level of this valuation. From a policy perspective, thus, opposition against wind turbines cannot be neglected. It remains the task of policy-makers to communicate benefits of avoided external costs, moderate decision-making processes in siting, and consider distributional implications and potential local compensation measures, including their temporal components.

Several limitations and open points provide room for further research. First, our data on view sheds and concrete visibility from places of residence is somewhat limited. Advanced digital surface models taking into account natural and man-made structures could provide richer evidence. Second, data on the ownership structure of wind turbines could allow disentangling the nexus between positive and negative spillovers, thus allowing for a more pronounced determination of external effects. Both caveats, however, are consistent with a lower-bound interpretation of our findings: residents in the treatment group might actually not be affected, and wind turbines in community ownership might have potentially positive monetary or idealistic effects on nearby residents. Beyond that, avenues for future research lie in the transfer of the empirical strategy applied in this study to other energy infrastructure, such as biomass plants or transmission towers.

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(*footnote continued*)

$$\ln(R_{dst}) = \beta_0 + \mathbf{DC}'_{dst}\,\beta_1 + \mathbf{AC}'_{dst}\,\beta_2 + \delta Construction_{dst,4000} + \sum_{n=1}^{12} \gamma_n Year_{2000+n} + \eta trend_{st} + \epsilon_{dst}$$

where $R_{dst}$ is the annual rent of dwelling $d$ in state $s$ at time $t$; $DC_{dst}$ is a vector of dwelling characteristics, including whether it is a detached, semi-detached, or terraced house, a small or large apartment building, or a high rise, as well as the number of rooms per individual; $AC_{dst}$ is a vector of amenity characteristics, including whether the dwelling has a kitchen, an indoor bath or shower, an indoor toilet, central or floor heating, a balcony or terrace, a basement, a garden, or a boiler; $Construction_{dst,4000}$ is the treatment dummy variable as in the main specification; $Year_{2000+n}$ is a full set of year dummy variables; $trend_{st}$ are state-specific linear time trends; and $\epsilon_{dst}$ is the idiosyncratic disturbance. We exclude households that have parts of their rents subsidised, or pay no rents at all, as well as non-private households such as nursing homes in order to not distort our estimates. Robust standard errors are clustered at the county times year level.

[32] All hedonic regression results are available upon request.

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## Appendix. Supplementary data

Supplementary data associated with this article can be found in the online version at http://dx.doi.org/10.1016/j.jeem.2016.11.009.

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