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Do wind turbines have adverse health impacts?

Christian Krekel
Johannes Rode
Alexander Roth

The London School of Economics and Political Science

UKRI Economic and Social Research Council

Abstract
While wind power is considered key in the transition towards net zero, there are concerns about adverse health impacts on nearby residents. Based on precise geographical coordinates, we link a representative longitudinal household panel to all wind turbines in Germany and exploit their staggered rollout over two decades for identification. We do not find evidence of negative effects on general, mental, or physical health in the 12-Item Short Form Survey (SF-12), nor on self-assessed health or doctor visits. We also do not find evidence for effects on suicides, an extreme measure of negative mental health outcomes, at the county level.

Keywords: wind turbines, externalities, health, renewable energy, difference-in-differences, event study
JEL Codes: D62; I10; Q20; Q42; R10

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Christian Krekel, Centre for Economic Performance (CEP), London School of Economics (LSE); Department of Psychological and Behavioural Science, LSE. Johannes Rode, TU Darmstadt, International Economics. Alexander Roth, German Institute for Economic Research (DIW Berlin and; Technische Universität Berlin..

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1 Introduction

Wind power is considered key in the transition towards net zero. About 100 gigawatts of onshore capacity – roughly 500,000 wind turbines – were built in Europe between 2011 and 2020 alone, satisfying about 7% of Europe's electricity demand as of 2020 (WindEurope, 2021). Wind power is expected to contribute large shares to electricity supply in Europe (Child et al., 2019) and worldwide (IEA, 2021) by 2050, making it the most important renewable energy after solar.

Yet, wind power is not without controversy. Although its importance is generally acknowledged, local residents often strongly oppose new wind turbines near their homes, a phenomenon referred to as *not-in-my-backyard effect* which is seen as a major contributing factor behind the slow expansion of wind power. In fact, negative impacts on house prices and the subjective wellbeing of nearby residents have been documented (cf. Heintzelman and Tuttle, 2012; Gibbons, 2015; Dröes and Koster, 2016; Möllendorff and Welsch, 2017; Krekel and Zerrahn, 2017). Importantly, local residents regularly cite concerns about adverse health impacts of wind turbines as one reason behind their opposition, and these concerns have led to a heated debate about potential public health consequences of living close to installations. In fact, Baxter, Morzaria, and Hirsch (2013) find that health concerns are the strongest predictor for local resistance. However, systematic, causal evidence on potential health externalities is scarce.

In this paper, we ask: do wind turbines have systematic, negative causal effects on the health of nearby residents? If so, which health dimensions are affected and by how much? And are effects, if any, spatially or temporally limited? To answer these questions, we use quasi-experimental methods and representative longitudinal household data from Germany – a country with a fast expansion of wind power in recent decades and hence a suitable case study – linked to a nationwide dataset on wind turbines, based on precise geographical coordinates, covering the universe of almost 24,000 installations built in Germany between 2000 and 2017.

In theory, adverse health impacts of onshore wind turbines may be driven by several factors.[1] First, and most important, there are concerns about the technology, with visual pollution from both shadow flicker and night-time anti-aircraft lights, as well as noise pollution from both audible and (especially) sub-audible (low-frequency or infra) sound as often cited mechanisms. Whether feared or actually realised, these may result in worry, anxiety, and sleep disturbances, thereby resulting in mental or physical health issues (cf. Bolin et al., 2011; Onakpoya et al., 2014; Freiberg et al., 2019). Besides technological concerns, residents may feel overwhelmed and annoyed by not having been involved in local planning and decision-making processes, aspects of fairness and procedural justice (cf. Pohl, Gabriel, and Hübner, 2018; van Kamp and van den Berg, 2021; Ki et al., 2022). Once installations have been built, they may feel disturbed by violations of their natural landscape prefer-

[1] For general reviews of wind turbine externalities, see Mattmann, Logar, and Brouwer (2016) or Zerrahn (2017).

ences or their psychological attachment to their places of residence (cf. Devine-Wright, 2005; Jobert, Laborgne, and Mimler, 2007; Wolsink, 2007; Waldo, 2012).[2] Each of these factors may provoke negative emotional reactions and stress, leading to, if sufficiently strong, adverse health impacts.

To provide systematic, causal evidence on such health externalities, we link the health outcomes of household members to the nearest wind turbine based on precise geographical coordinates of both households and installations. We measure general, mental, and physical health using the 12-Item Short Form Survey (SF-12) (RAND, 2022), a routine instrument for monitoring health in the general population. In addition, we measure self-assessed health and the number of doctor visits as a retrospective behavioural outcome, as well as the frequency of experiencing negative emotions, sleep satisfaction, and the number of hours of sleep as cited mechanisms behind potential health problems. To estimate causal effects, we exploit the staggered rollout of installations over a two-decade period in a spatial difference-in-differences design, using two-way fixed-effects estimators and, in addition, the robust estimator by Sun and Abraham (2021) to explicitly account for potential treatment effect heterogeneity due to changing technology over time (cf. Goodman-Bacon, 2021).[3] Depending on outcome and treatment and control radii, our estimation samples include between 700 and 1,963 individuals who are treated by between 111 and 462 wind turbines, distributed across the entire country, who are compared to a control group of between 8,002 and 10,533 individuals.[4]

We are the first to study the direct health effects of wind turbines using quasi-experimental methods and nationwide data on households and installations that span over two decades, while explicitly accounting for potential treatment effect heterogeneity due to changing technology over time. We find no evidence of negative effects on either general, mental, or physical health – neither on aggregate nor on any of the different mental or physical health sub-scales – in the SF-12. There is no evidence for dynamic effects over time nor for cumulative effects. We do not detect impacts on self-assessed health or the number of doctor visits either. When looking at often cited mechanisms in the literature, we find no evidence that residents living closer to installations experience more negative emotions, are less satisfied with their sleep, or sleep fewer hours than residents living further away. In our baseline specification, we use a treatment group within 4,000 metres and a control group between 4,000 and 8,000 metres to the nearest installation. Individuals within 4,000 metres are previously shown to experience negative externalities of wind turbines on their subjective well-being (cf. Krekel and Zerrahn, 2017). Our results are robust to different treatment and control radii as well as different bins around plants, to different plant sizes, and to accounting for residential sort-

[2]A similar argument can be made for residents who hold lower environmental attitudes (Hobman and Ashworth, 2013), who have less experience in and knowledge of renewables (Aitken, 2010), or who hold more conservative political beliefs (Eltham, Harrison, and Allen, 2008; Karlstrom and Ryghaug, 2014).

[3]As an alternative to Sun and Abraham (2021), we also use the estimator by Gardner (2022) as a robustness check, which confirms our results.

[4]An *ex-post* power calculation confirms that our study is sufficiently powered to detect a small effect size, if present.

ing. Taken together, our findings cast doubt on health externalities on the local population, which has important implications for the public and scientific debate around wind power.

Suicide is an extreme outcome of mental distress (Harris and Barraclough, 1997), and has been used as an objective measure of adverse mental health impacts of environmental stressors, for example air pollution in the US (Molitor, Mullins, and White, 2023) or high temperatures in Mexico and the US (Burke et al., 2018). The paper most closely related to ours is Zou (2020), who studies the impact of wind turbines on suicides by exploiting administrative data on 800 new utility-scale wind farms and official suicide rates at the county level in the US from 2001 to 2013. The author uses a spatial difference-in-differences design and two-way fixed-effects estimators, finding significant increases in suicides in counties closer to wind farms. However, impacts are small and detectable only for individuals between 15 to 19 and for those over 80 years of age. Leveraging additional survey data, the author shows that increases in suicides are likely driven by sleep insufficiency.[5] Exploiting administrative data on suicide rates at the county level in Germany during our observation period and replicating our analysis on health outcomes for suicides, we do not find evidence of effects on suicides.

We contribute to a body of evidence that is – despite a clear, theoretical causal chain from environmental stressor to health – largely inconclusive and that relies mostly on cross-sectional analyses and local case studies.[6] Most studies find that being located close to a wind turbine is associated with increases in noise annoyance (Bakker et al., 2012; Michaud et al., 2016; Pohl, Gabriel, and Hübner, 2018; Radun et al., 2022), health concerns (especially when installations are visible) (Michaud et al., 2016), sleep disturbances (Bakker et al., 2012; Turunen et al., 2021; van Kamp and van den Berg, 2021), and increases in psychological distress (Bakker et al., 2012), with similar patterns across countries (Hübner et al., 2019). Besides issues of causality and a focus on local case studies, a common concern with many of these studies is that they are often framed as or are seemingly related to wind turbines, which may elicit attitude expression rather than the reporting of genuine impacts. Given the quality of the evidence base, meta-analyses and systematic reviews are, likewise, inconclusive (Bolin et al., 2011; Knopper and Olson, 2011), concluding that "experimental and observational studies investigating the relationship between wind turbine noise and health are warranted" (Onakpoya et al., 2014) and that "more high-quality research is needed" (Freiberg et al., 2019). In a systematic review, Schmidt and Klokker (2014) find that exposure to wind turbines increases the risk of annoyance and sleep disturbance, yet find no conclusive evidence of other claimed health effects, noting

[5] In a study not related to health, Brunner, Hoen, and Hyman (2022) use a spatial difference-in-differences design that exploits the nationwide rollout of wind turbines in the US between 1995 and 2016. The authors estimate the causal effects of wind turbines on test scores, high-school completion, and long-run outcomes of local students, finding precisely estimated zero effects. Like our paper, the authors use both two-way fixed-effects estimators and the robust estimator by Sun and Abraham (2021).

[6] There is also a proliferating grey and pseudo-scientific literature suggesting that proximity to wind turbines is causing a wide range of health issues, from autism to cancer or even death. We limit our literature review to peer-reviewed articles.

that "selection bias and information bias of differing magnitudes were found to be present in all current studies." Given this inconclusive evidence base, the World Health Organization, in its *Environmental Noise Guidelines*, takes a cautionary stance, and recommends "reducing noise levels produced by wind turbines below 45 dB L_{den} [decibel day-evening-night-weighted sound pressure level], as wind turbine noise above this level is associated with adverse health effects" and that "policy-makers implement suitable measures to reduce noise exposure [...] above the guideline values". However, it also acknowledges that the quality of evidence is "low" or even entirely missing (World Health Organization, 2018).

Interestingly, some studies point towards psychological salience, personality, and individual differences to explain some of these findings. For example, Crichton and Petrie (2015) show that concerns about adverse health impacts created by the media may trigger symptom reporting, while Taylor et al. (2013) find perceived symptoms only amongst residents who score high in terms of neuroticism, negative affect, and frustration intolerance. Similarly, Jalali et al. (2016) find reported sleep disturbances only amongst residents who have negative attitudes towards wind turbines, concerns about property devaluations, and who can see installations from their homes.

We also contribute to the literature in health, environmental, and public economics that looks at the external effects of infrastructure, either directly on health and health-related quality of life, such as freeways and associated congestion (Currie and Walker, 2011; Brinkman and Lin, 2022) or shale gas development and fracking (Hill, 2018), or indirectly via noise pollution, such as airports or neighbourhood structure (Bilger and Carrieri, 2012; Boes, Nüesch, and Stillman, 2013); via air pollution such as industrial facilities, power plants, or heating and agricultural systems (Agarwal, Banternghansa, and Bui, 2010; Luechinger, 2014; Currie, Davis, et al., 2015; Sheldon and Sankaran, 2017; Fan, He, and Zhou, 2020), or the impacts of air quality on health and societal welfare more generally (Currie, Neidell, and Schmieder, 2009; Muller, Mendelsohn, and Nordhaus, 2011; Coneus and C. K. Spiess, 2012; Tanaka, 2015; Deryugina et al., 2019; Anderson, 2020; Giaccerini, Kopinska, and Palma, 2021), and specifically, the societal benefits and costs of wind power (Cullen, 2013; Novan, 2015). Our paper adds a particular type of infrastructure – renewable energy facilities, specifically wind turbines – that is being deployed in many countries at fast pace in close proximity to households.

2 Data

2.1 Health

Our health data come from the German Socio-Economic Panel (SOEP), a representative panel of private households in Germany (SOEP, 2021). It has been conducted annually since 1984 and includes almost 40,000 individuals living in more than 19,000 households in its most recent 2022 wave. Importantly, the panel provides, besides interview dates, the exact geographical coordinates of every household in every year since 2000, which allows us to merge the health outcomes of individuals living in a representative sample of German households with data on wind turbines based on precise geographical information and timing (Goebel, Grabka, et al., 2019).[7]

We select several health outcomes. Our main outcomes come from the 12-Item Short Form Survey (SF-12) (RAND, 2022), which is incorporated into the SOEP every second year (i.e. 2000, 2002, 2004, ..., and so on). It includes summary scales for *general health*, *mental health*, and *physical health*, alongside respective sub-scales.[8] The SF-12 is a standard instrument on health-related quality of life, allowing for group comparisons involving multiple health dimensions. It relies on self-reporting and is widely used in healthcare for monitoring and assessment of health outcomes in general and patient populations (Ware, Kosinski, and Keller, 1995). All scales from the SF-12 are normalised to be between zero and 100, with a mean of 50 and a standard deviation of 10 (cf. Andersen, Mühlbacher, and Nübling, 2007).

Moreover, we obtain data on the subjective *self-assessed health* of individuals and, as a retrospective behavioural outcome, the reported *number of doctor visits* in the year prior to their interview, both of which are asked every year. The former is obtained from a five-point Likert scale question that asks "How would you describe your current health?", with answers ranging from five ("Very good") to one ("Bad"). The latter is obtained from a question that asks "Have you gone to a doctor within the last year? If yes, please state how often." Finally, we obtain data on the frequency of experiencing certain emotions, sleep satisfaction, and the number of hours of sleep to look at often cited mechanisms through which adverse health impacts of wind turbines may come about, in particular those related to noise pollution from both audible and (especially) sub-audible (low-frequency or infra) sound.

Besides these outcomes, we select a wide range of demographic and socio-economic characteristics as covariates, including marital status, employment status, log annual net household income, the ownership status of the dwelling and its log annual rent, as well as the number of adults and children in the household.[9]. Importantly, neither surveys nor questions are framed as being related to the presence of wind turbines, so that priming of respondents is of no concern.

[7]The SOEP is subject to rigorous data protection: it is not possible to derive household data from geographical coordinates as both are not visible to the researcher at the same time. See Goebel and Pauer (2014) for details.

[8]For mental health, these are *role-emotional* and *social functioning*, which are defined as the extent to which individuals are capable of mastering work or other daily and social activities without being affected by emotional problems, as well as *general mental health* and *vitality*, which are defined as the absence of mental disorder and fatigue. For physical health, these are *role-physical* and *physical functioning* as well as *bodily pain*. Each sub-scale is obtained from a five-point Likert scale, whereby the respective summary scale combines these with equal weights.

[9]The SOEP asks renters to report their actual and owners to report their *estimated* rent in the hypothetical case in which they would not own their dwelling. We combine both in a single variable.

Appendix Table A.I shows summary statistics for outcomes and covariates in our baseline specification. Overall, individuals in our estimation sample are 71% married, 34% full-time and 12% part-time employed (with a median annual net household income of about €31,200), 4% unemployed, 70% owning their dwelling and 30% renting, and have, on average, slightly less than three individuals in their household.[10] Individuals in our sample also tend to be rather healthy: for our main outcomes based on the SF-12, individuals have mental and physical health scores above the median of 50 (which can be interpreted as a cut-off for being healthier as opposed to unhealthier), and they themselves assess their health as good (though not necessary very good). The median number of doctor visits in the last year is four. Note that the divergence between mean and median for some of our health outcomes suggests that there is a longer tail of individuals who have below-average health.

2.2 Wind Turbines

Our data on wind turbines come from Unnewehr et al. (2021) and include all 23,628 onshore wind turbines connected to the grid in Germany from 2000 to the end of 2017. In particular, the data contain information on the exact location of each installation in form of precise geographical coordinates, the starting year of operation, and further details such as hub height, rotor diameter, and installed capacity in megawatts.

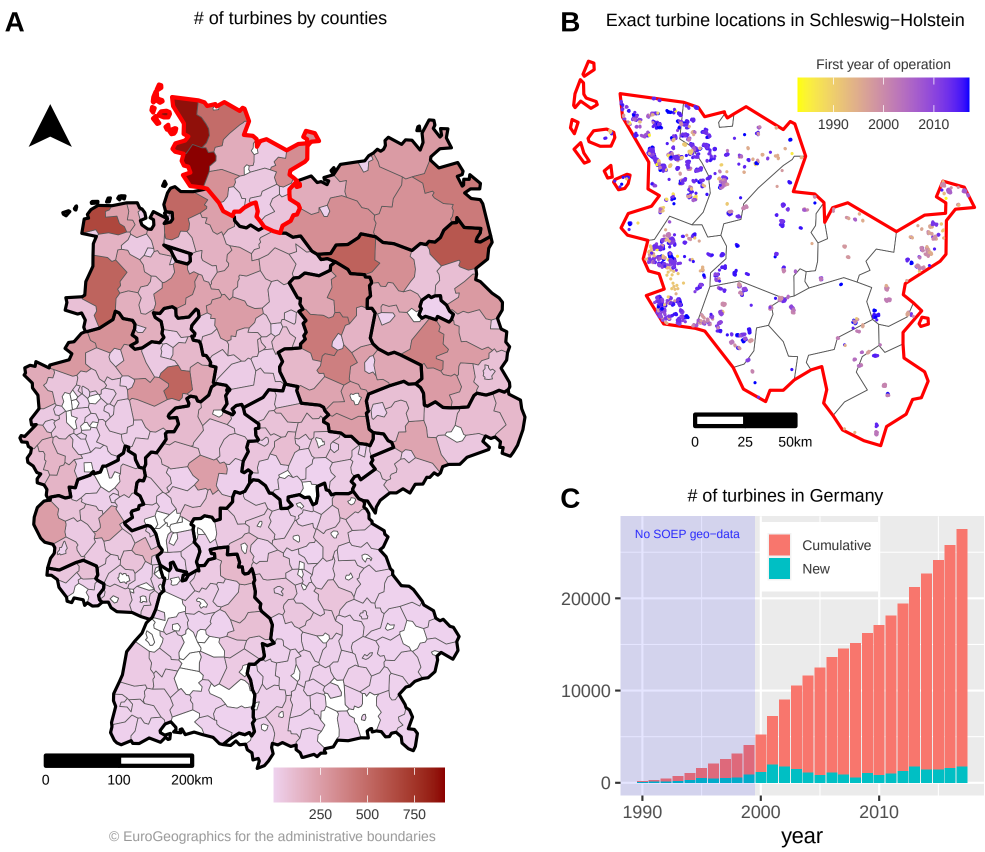
The exact location of each installation is essential for our analysis, and we carried out extensive plausibility checks to ensure high data quality. In particular, we drew a 10% random sample of wind turbines and then verified the location of each randomly drawn installation based on satellite imagery from Google Earth. We found that about 95% of installations had the correct geographical coordinates.[11] We conclude that our data on wind turbines are of high quality.

Based on our data, Figure 1 shows the diffusion of onshore wind turbines in Germany until 2017. In particular, Panel A shows the geographical distribution of wind turbines at the level of counties (NUTS-3 areas) in Germany, whereby counties coloured in darker shades of red exhibit more installations. We observe that 327 out of 401 counties had installations by the end of 2017. Most can be found in the north of Germany, near the sea where wind intensity tends to be highest, especially in the federal states of *Lower Saxony*, *Mecklenburg-Western Pomerania*, and *Schleswig-Holstein*, which are adjacent to the North Sea, as well as to a lesser extent in the federal states of *Brandenburg* and *Saxony-Anhalt*, which are landlocked yet still in the north of the country.

Panel B shows, as an example, the exact location of each wind turbine in the federal state of

[10] As described in Section 3.2, our sample is restricted to individuals living in rural areas (where wind turbines are more common). Our results are robust to lifting this restriction.

[11] More specifically, 93.9% of the random draw had exactly the same geographical coordinates as in Google Earth. For 1.4% of the draw, the geographical coordinates were almost the same. For the rest, we found that 2.8% of installations no longer existed, while 1.6% could not be found, 0.1% were under construction, and 0.25% came with similar geographical coordinates as another installation nearby.



Panel A shows the geographical distribution of wind turbines across counties (NUTS-3 areas: *Landkreise und Kreisfreie Städte*) in Germany in 2017. The thick black lines indicate the borders of federal states (NUTS-1 regions), whereas the red thick line indicates the border of the federal state of *Schleswig-Holstein*, the most northern German state. *Panel B* is a close-up of *Schleswig-Holstein* and shows, as an example, the exact location of each installation in that federal state, whereby each dot indicates one installation, coloured by the first year of operation. *Panel C* plots the annual number of cumulative and new installations in Germany since 1990.

Figure 1: Diffusion of Onshore Wind Turbines in Germany until 2017.

Schleswig-Holstein, whereby installations that are older are coloured in yellow and those that are newer are coloured in blue. In total, there were 3,310 installations in *Schleswig-Holstein* at the end of 2017.[12]

Finally, Panel C plots the annual number of cumulative and new installations in Germany since 1990. While new builds increased in the 1990s, their number peaked in 2002, two years after the German *Renewable Energy Sources Act* established an attractive feed-in-tariff system for electricity generated from wind power. After fewer new builds in 2008 and an increase in the following years, the number of new builds per year remained roughly stable at around 1,500 between 2013 and 2017.

[12] Appendix Figure A.I shows the exact locations of all 27,739 onshore wind turbines connected to the grid in Germany through the end of 2017.

Our analysis focuses on the period between 2000 and 2017, for which the SOEP provides the precise geographical coordinates of every household in every year.

Appendix Table A.II shows summary statistics for wind turbines in our baseline specification. On average, wind turbines have a power capacity of 1.6 megawatts (standard deviation of 0.8), a hub height of 88.4 metres (standard deviation of 32.2), and a rotor diameter of 76.2 metres (standard deviation of 23.3). Appendix Table A.III then shows how these summary statistics have evolved over time during our observation period: capacity has almost doubled, from 1.3 megawatts in 2002 to 2.4 in 2015, and so have hub height (from 75.7 metres in 2002 to 122.1 in 2015) and rotor diameter (from 64.2 metres to 112.2).

2.3 Estimation Sample

Our estimation sample consists of all individuals who are interviewed from 2000 through 2017 (for whom we have precise geographical coordinates), who have at least one pre-treatment and one post-treatment observation, and who have no missings on either outcomes or covariates. The number of observations in our estimation sample depends on the availability of outcomes in a given year (some are available every year, others only every second) and on our treatment and control radii.

In our baseline specification, which uses a treatment group within 4,000 metres and a control group between 4,000 and 8,000 metres to the nearest installation, we have 700 individuals in our treatment group and 8,002 individuals in our control group for our main outcomes based on the SF-12, being treated by 111 wind turbines. For self-assessed health and the number of doctor visits, this amounts to 1,510 individuals in our treatment group and 10,533 individuals in our control group, being treated by 399 wind turbines. For a treatment group within 6,000 metres, we have 902 treated and 8,002 controlled individuals for our main outcomes, being treated by 116 wind turbines. For self-assessed health and the number of doctor visits, there are 1,963 treated and 10,533 controlled individuals, being treated by 462 wind turbines.

To ascertain whether our study is sufficiently powered to detect a small effect size, we conduct an *ex-post* power calculation. In particular, we assume a small effect size of $d = 0.2$, an error probability of $\alpha = 0.05$, and a power of $1 - \beta = 0.95$. This yields a required total sample size of 1,084 individuals, with 542 individuals in the treatment group and in the control group. As our group sizes exceed this threshold for each of our outcomes, we conclude that our study is sufficiently powered to detect a small effect size, if present.

3 Empirical Strategy

3.1 Model

Our empirical strategy rests on a spatial difference-in-differences design that compares the health outcomes of individuals living in households near wind turbines with those of individuals living further away, from before to after the start date of operation. We begin with the following regression model:

$$
\begin{aligned}
Y_{ijd,t} = \beta_0 + \beta_1(1\{Near\}_{ijd} \times 1\{Operating\}_{ij,t}) + \beta_2 1\{Near\}_{ijd} + \beta_3 1\{Operating\}_{ij,t} \\
+ \beta_4' X_{ijd,t} + r + s + t + s \times t + u_i + \epsilon_{ijd,t}
\end{aligned}
\tag{1}
$$

where $Y_{ijd,t}$ is the health outcome of individual i in year t, given the nearest installation j and its distance d to the household of the individual. The indicator $1\{Near\}_{ijd}$ is a time-invariant dummy that takes on one if the household is located within distance band $[0; d]$ metres to the installation (i.e. our treatment group), and zero if it is located within distance band $(d; x]$ metres (i.e. our control group, whereby $x > d$). That is, individuals in our control group are located close to an installation but not close enough to be treated. The indicator $1\{Operating\}_{ij,t}$ is a time-varying dummy that takes on one if the installation is operational in a given year, and zero else.[13] The vector $X_{ijd,t}$ are time-varying covariates, including demographic and socio-economic characteristics. The variables r, s, and t are county, federal state, and year fixed effects,[14] whereas u_i is an individual fixed effect. Together, r, s, t, and u_i net out time-invariant unobserved heterogeneity at the county, federal state, year, and individual level. We also include interactions between federal state and year fixed effects to flexibly account for trends in health across federal states over time. Because plants determine treatment, we cluster robust standard errors at the plant level.

Equation 1 implements our spatial difference-in-differences design as a two-way fixed-effects estimator, generalising the canonical difference-in-differences design to treatment at multiple points in time.[15] Noting that $1\{Near\}_{ijd}$ and u_i as well as $1\{Operating\}_{ij,t}$ and t are collinear, and defining $D_{ijd,t} = (1\{Near\}_{ijd} \times 1\{Operating\}_{ij,t})$, Equation 1 can be rewritten as:

[13] We use the start date of operation to define our time dummy, as adverse health impacts are mostly attributed to the operation rather than construction of installations. Note that the construction (excluding planning and project management) of a wind turbine is rather fast: for example, it only takes about two months to build a smaller, ten megawatts wind farm and about six months for a larger, 50 megawatts farm, each comprising several wind turbines (European Wind Energy Association, 2023). As Figure 2 shows, we find no evidence for anticipation effects (adverse health effects prior to treatment) that could be attributed to construction.

[14] In Germany, there are 401 counties (NUTS-3 areas) and 16 federal states (NUTS-1 regions).

[15] This closely resembles the model by Currie, Davis, et al. (2015) for estimating the causal effect of toxic plant closings on health, the main difference being that our model takes the level of analysis from the aggregate to the individual level.

$$Y_{ijd,t} = \beta_0 + \beta_1 D_{ijd,t} + \beta_2' X_{ijd,t} + r + s + t + s \times t + u_i + \epsilon_{ijd,t} \quad (2)$$

As we are also interested in whether individuals adapt to nearby installations or whether continued exposure potentially aggravates adverse health impacts, we also estimate this model as an event study:

$$Y_{ijd,t} = \beta_0 + \sum_{l} \beta_1^l D_{ijd,t}^l + \beta_2' X_{ijd,t} + r + s + t + s \times t + u_i + \epsilon_{ijd,t} \quad (3)$$

where $D_{ijd,t}^l$ is a set of dummies that take on one for the l^{th} lead before (i.e. from $l = -6$ to $l = -1$) or lag after construction (i.e. from $l = 0$ to $l = 8$), and zero otherwise.[16]

We are interested in β_1 in Equation 2 and β_1^l in Equation 3, which can be interpreted as the average causal effects on health from being located within distance band $[0; d]$ metres to the nearest wind turbine if our identifying assumptions in Section 3.2 are satisfied.

3.1.1 Treatment Effect Heterogeneity

Chaisemartin and D'Haultfœuille (2020), Callaway and Sant'Anna (2021), Goodman-Bacon (2021), Sun and Abraham (2021), Athey and Imbens (2022), and Borusyak, Jaravel, and J. Spiess (2023) show that Equations 2 and 3 yield unbiased estimates of β_1 and β_1^l only if treatment effects are homogeneous.[17] This may not be true in our case: we exploit the staggered rollout of installations over a two-decade period during which technology may have changed. In fact, Appendix Table A.III shows that capacity, as well as hub height and rotor diameter of wind turbines, almost doubled between 2002 and 2015, suggesting that treatment effects may be heterogeneous during our observation period.

In essence, Equations 2 and 3 may yield biased estimates of β_1 and β_1^l as they compare individuals who are being treated at the time not only to those who are later treated or who are never treated but also to those who were earlier treated.[18] However, individuals who were earlier treated may have been exposed to a different technology, resulting in, for example, different trajectories of adaptation to nearby installations. The direction of potential bias is not *ex-ante* clear.[19]

[16] We normalise the year of first treatment as $t = 0$ and use the pre-treatment year $t = -1$ as the reference category in our regression. Note that, due to sample size, i.e. a small number of individuals many years before and many years after treatment, we trim observations before the sixth lead and after the eighth lag.

[17] See also Chaisemartin and D'Haultfœuille (2022) and Roth et al. (2023) for recent reviews of this issue.

[18] Individuals who are always treated are generally excluded, as they do not allow for inference.

[19] Accounting for potential treatment effect heterogeneity over time, we also look at heterogeneous treatment effects by plant size in Section 5.

To eliminate potential bias, we adopt the robust estimator by Sun and Abraham (2021) for difference-in-differences with treatment at multiple points in time, which formalises this setting as an event study. This approach has several advantages in our case: first, it allows us to show an unbiased common trend between treated and controlled pre-treatment, by looking at leads, as well as an unbiased trajectory of adaptation to nearby installations post-treatment, by looking at lags. We can then aggregate lags into a single parameter to obtain an unbiased average effect. Second, it allows us to elicit the extent of bias arising from treatment effect heterogeneity, by directly comparing estimates from our two-way fixed-effects estimator in Equation 3 with those from Sun and Abraham (2021), which is a contribution in its own right.

Sun and Abraham (2021) use cohort-specific average treatment effects on the treated as building blocks, which in our case can be defined as $CATT_{e,l} = E[Y^1_{ijd,e+l} - Y^0_{ijd,e+l} | E_{ijd} = e]$, where $E_{ijd} = min\{t : D_{ijd,t} = 1\}$ is the year of first treatment, individuals in cohort $e \in \{1, 2, ...T, \infty\}$ are first treated in year $\{i : E_{ijd} = e\}$ (with ∞ denoting cohorts that are never treated), and $Y^1_{ijd,e+l}$ and $Y^0_{ijd,e+l}$ are potential outcomes of treatment and control group, respectively.[20] Hence, $CATT_{e,l}$ is the average treatment effect l periods relative to the year of first treatment for the cohort of individuals who are first treated in year e. The authors show that, for a non-empty cohort e, some pre-periods $s < e$, and some set of non-empty control cohorts $C \subseteq \{c : e + l < c \leq T\}$, an estimate $\hat{\delta}_{e,l}$ of $CATT_{e,l}$ can be obtained from:

$$\hat{\delta}_{e,l} = \frac{\frac{1}{N}\sum_{i=1}^{N}(Y_{ijd,e+l} - Y_{ijd,s}) \times 1\{E_{ijd} = e\}}{\frac{1}{N}\sum_{i=1}^{N} 1\{E_{ijd} = e\}} - \frac{\frac{1}{N}\sum_{i=1}^{N}(Y_{ijd,e+l} - Y_{ijd,s}) \times 1\{E_{ijd} \in C\}}{\frac{1}{N}\sum_{i=1}^{N} 1\{E_{ijd} \in C\}} \tag{4}$$

Then, estimates of the l^{th} lead before or lag after construction, $\hat{\beta}^l_1$, can be calculated as weighted averages of $\hat{\delta}_{e,l}$ using estimated weights $\hat{PR}\{E_{ijd} = e | E_{ijd} \in [-l, T - l]\}$, which are obtained from sample shares of each cohort in relevant periods l:

$$\hat{\beta}^l_1 = \sum_l \sum_e \hat{\delta}_{e,l} \hat{PR}\{E_{ijd} = e | E_{ijd} \in [-l, T - l]\} \tag{5}$$

Finally, an overall estimate, $\hat{\beta}_1$, can be calculated as the average across all lags after construction. Sun and Abraham (2021) show that, if our identifying assumptions in Section 3.2 are satisfied, $\hat{\delta}_{e,l}$ is a consistent estimate of $CATT_{e,l}$ and sample shares $\hat{PR}\{E_{ijd} = e | E_{ijd} \in [-l, T - l]\}$ are consistent

[20] The data structure of our event study can be described as *hybrid* (Miller, 2022), considering that treatment occurs at multiple points in time and that it includes both individuals who are later treated and individuals who are never treated.

estimates of population shares, implying that $\hat{\beta}_1^l$ and $\hat{\beta}_1$ are consistent estimates even if treatment effects are heterogeneous.

Note that, regardless of our estimator, we assume that treatment is an absorbing state, i.e. once a wind turbine becomes operational, it remains so until the end of our observation period.[21]

Appendix Figure A.II, Panel A, shows the number of individuals who are treated by year in our estimation sample; Panel B the number of individuals who are never treated, exemplary for our outcome *self-assessed health*, which is available in every year. Appendix Figure A.III replicates this figure for *general health* in the SF-12, which is available every second year. As seen, in both cases, the number of individuals who are treated is almost constant during our observation period, except for a slight increase in 2016 and a much stronger increase around 2002, when the feed-in-tariff system for electricity generated from wind power was established in Germany. In line with this, Panel C shows the cumulative density of individuals who are treated by year, with a much steeper increase during the first years of our observation period. Finally, Appendix Figure A.IV shows the share of individuals who are treated by one, two, or more newly built wind turbines. Most are treated by one turbine or wind farms with less than five turbines.

3.2 Identification

We choose our control group to be close enough to installation j to capture highly localised area conditions such as local demography, labour markets, deprivation, or health clusters in its surroundings, yet far enough not to be treated.

As there exists no uniform legislation in Germany that could serve as a point of reference (like a mandated setback distance), we are agnostic and use different treatment radii, i.e. $d = \{2000, 3000, 4000, 5000, 6000\}$, as well as different control radii, i.e. $x = \{4500, 5000, 5500, 6000, 8000, 10000\}$. A treatment radius of $d = 4000$ and a control radius of $x = 8000$ are our default, as individuals within 4,000 metres are previously shown to experience negative externalities of wind turbines on their subjective wellbeing (cf. Krekel and Zerrahn, 2017). This is a common approach in the literature (cf. Gibbons, 2015; Krekel and Zerrahn, 2017), in case a treatment radius cannot be endogenously determined, for example by estimating how far a pollutant travels (cf. Currie, Davis, et al., 2015). It also allows us to test for spatial decay of potential wind turbine externalities on health.

Left with these treatment and control group definitions, our empirical strategy rests on two identifying assumptions:

[21] Our data on wind turbines do not include the date of decommissioning, if applicable. However, the average lifespan of a wind turbine is 20 years (Environmental Protection Agency, 2013). Decommission is, therefore, likely to be a minor issue during our observation period. In any case, it is likely to bind our treatment effects from below. The same is true if wind turbines are taken off-grid for maintenance or repair (which usually takes only very short time).

1. ***Exogeneity of Treatment.*** Whether an individual is allocated to our treatment or control group is as good as random, conditional on time-varying covariates $X_{ijd,t}$, county and federal state fixed effects r and s, year fixed effects t, and individual fixed effects u_i. That is, $D_{ijd,t} \perp 0, 1 | X_{ijd,t}, r, s, t, u_i$. This also implies no anticipatory behaviour prior to treatment.

2. ***Common Trend.*** In a hypothetical absence of treatment, our treatment group would have followed the same trend in health outcomes as our control group, conditional on time-varying covariates $X_{ijd,t}$, county and federal state fixed effects r and s, year fixed effects t, and individual fixed effects u_i. That is, $E[Y_{ijd,t} - Y_{ijd,t-1} | X_{ijd,t}, r, s, t, u_i, D_{ijd,t} = 1] = E[Y_{ijd,t} - Y_{ijd,t-1} | X_{ijd,t}, r, s, t, u_i, D_{ijd,t} = 0]$.

Regarding exogeneity of treatment, Appendix Table A.IV shows means and variances of our covariates separately for our default treatment and control group, including normalised differences between them. According to Imbens and Wooldridge (2009), a normalised difference greater than 0.25 suggests covariate imbalance. As seen, none of our covariates exceeds this value, implying that they are well-balanced between groups. Note that *not* controlling for time-varying covariates, county and federal state fixed effects, year fixed effects, and individual fixed effects in our regressions does not change our results (Appendix Figures A.V and A.VI). This suggests that exogeneity of treatment is likely satisfied, even unconditionally. As Figure 2 shows, we do not find evidence for anticipatory behaviour prior to treatment.

Regarding common trend, we plot leads before the year of first treatment in our event studies, for our two-way fixed-effects estimator and for the robust estimator by Sun and Abraham (2021). As will be seen, none of these leads turns out significantly different from zero in our baseline specification, suggesting common trend behaviour between treated and controlled, pre-treatment.

A threat to identification may come from *endogenous sorting*. In particular, some individuals may be more likely to move away from wind turbines, for example because they are concerned about adverse health impacts or are experiencing them. Other individuals, however, may move towards wind turbines, where rental prices may be lower, potentially mispredicting adverse health impacts or even deliberately taking them into account. The direction of resulting bias is not *ex-ante* clear. Thus, in our baseline specification, we omit individuals who move and focus entirely on stayers.[22] Note that mobility in Germany is rather low compared to other countries: in the SOEP, only about 5% of individuals move every year.

Another threat to identification may come from *endogenous construction*. In particular, some individuals may be more likely to have wind turbines constructed nearby, while others may even construct installations themselves. For example, wind turbines may be more likely to be placed in

[22] In a robustness check in Section 5, we return to the issue of endogenous sorting. As will be seen, our results remain robust to the inclusion of movers (Appendix Table A.V).

deprived areas, where local resistance may be lower. On the other hand, private persons may be generating income from wind turbines, for example farmers who build a wind farm on their land or who lease their land to utilities to do so. To the extent that endogenous construction is correlated with health, as is found for deprivation and income (cf. Frijters, Haisken-DeNew, and Shields, 2005; Lindahl, 2005; Jones and Wildman, 2008), it may bias our estimates, the direction of which is again not *ex-ante* clear.

We deal with endogenous construction in three ways. First, recall that our control group is located within distance band $(d; x]$ metres to the nearest installation, hence far enough not to be treated but close enough to capture highly localised area conditions such as deprivation and income. Second, we use different treatment and control radii d and x to capture different aspects of these conditions. Additionally, we control for county fixed effects r to capture localised area conditions such as local attitudes (as well as federal state fixed effects s and their interaction with years to capture regional socio-political conditions and their trends over time). Finally, we exclude farmers and urban counties, so that our estimation sample is restricted to a relatively homogeneous group of individuals living in rural areas.[23]

4 Results

We first look at average treatment effects. Table 1 shows the estimates from our baseline specification, which compares the health outcomes of individuals who are treated (i.e. living within 4,000 metres to the nearest newly built wind turbine) with those who are not (i.e. living between 4,000 and 8,000 metres). Panel A shows the estimates from our two-way fixed-effects estimator, Panel B those from the robust estimator by Sun and Abraham (2021). All models routinely control for time-varying covariates, county fixed effects, federal state times year fixed effects, and individual fixed effects. We standardised outcomes to have a mean of zero and a standard deviation of one (i.e. z-scores) for comparability.

We do not find a statistically significant effect of a newly built wind turbine on either the mental or physical health summary scale (Columns 2 and 3 in each panel), our main outcomes from the SF-12. If anything, we detect a *positive* effect on general health as an overall measure of health (Column 1). However, it is only small in size (about 6% SD), significant at the 5% level (i.e. P value of about 0.04 for each estimator), and should be de-emphasised due to the number of hypotheses we are testing. In particular, considering that we are testing five hypotheses, a standard Bonferroni correction suggests a critical value of $(0.10/5) = 0.02$ for a 10% level of statistical significance, which is clearly below our empirical P value. Going on, we do not find a statistically significant effect on self-assessed health (Column 4) or on the reported number of doctor visits (Column 5), a

[23] Our results do not change when including urban counties (Appendix Figure A.VII).

retrospective behavioural outcome that allows us to capture potential impacts that go beyond self-assessment. Estimates from our two-way fixed-effects estimator generally resemble those from the robust estimator by Sun and Abraham (2021).

Appendix Tables A.VI and A.VII disentangle the mental and physical health summary scales from the SF-12 into their respective sub-scales, which are *role-emotional* and *social functioning*, *general mental health*, and *vitality* for the mental health summary scale, and *role-physical* and *physical functioning* as well as *bodily pain* for the physical health summary scale. In line with our previous results, we do not find a statistically significant effect of a newly built wind turbine on any of these sub-scales.

Table 1: Average Treatment Effects.

(a) Two-Way Fixed-Effects Estimator.

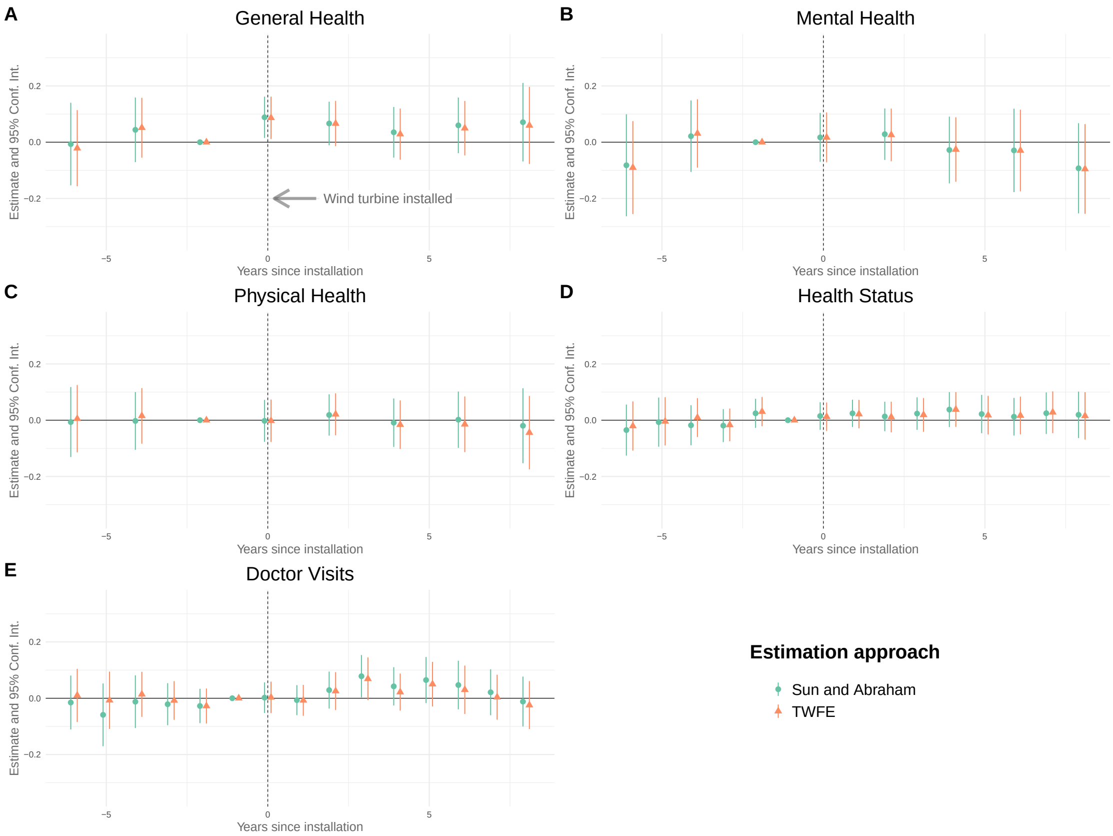
	SF-12 Health Survey			Other Health Outcomes	
Dependent Variable:	General Health (1)	Mental Health Summary Scale (2)	Physical Health Summary Scale (3)	Self-Assessed Health (4)	Doctor Visits (5)
Variable					
Treated 0-4 km	0.06**	0.009	-0.002	0.01	0.02
	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)
Controls	Yes	Yes	Yes	Yes	Yes
Fixed-effects					
Individual	Yes	Yes	Yes	Yes	Yes
County	Yes	Yes	Yes	Yes	Yes
State-Year	Yes	Yes	Yes	Yes	Yes
Statistics					
Adjusted R^2	0.591	0.484	0.668	0.601	0.357
Obs.	26,903	26,903	26,903	68,289	65,068
N treated	700	700	700	1,509	1,508
N never treated	8,002	8,002	8,002	10,533	8,767

(b) Robust Estimator by Sun and Abraham (2021).

	SF-12 Health Survey			Other Health Outcomes	
Dependent Variable:	General Health (1)	Mental Health Summary Scale (2)	Physical Health Summary Scale (3)	Self-Assessed Health (4)	Doctor Visits (5)
Variable					
Treated 0-4 km	0.07**	-0.007	0.0009	0.02	0.03
	(0.03)	(0.04)	(0.03)	(0.02)	(0.02)
Controls	Yes	Yes	Yes	Yes	Yes
Fixed-effects					
Individual	Yes	Yes	Yes	Yes	Yes
County	Yes	Yes	Yes	Yes	Yes
State-Year	Yes	Yes	Yes	Yes	Yes
Statistics					
Adjusted R^2	0.591	0.485	0.668	0.601	0.357
Obs.	26,903	26,903	26,903	68,289	65,068
N treated	700	700	700	1,509	1,508
N never treated	8,002	8,002	8,002	10,533	8,767

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1; clustered (plant) standard-errors in parentheses; treatment group 0-4 km; control group 4-8 km.
Outcomes in z-scores; more indicates better health (but for doctoral visits more indicates worse).

Next, we move from static to dynamic effects and look at treatment over time. Figure 2 shows the estimates from our baseline specification implemented as an event study, with six leads before and eight lags after a new wind turbine is built, whereby the period in which an installation is built is normalised to zero and the first lead serves as the reference category. Panels A to E plot these leads and lags for each of our outcomes in Table 1. The remainder is the same as before.



Outcomes are in z-scores. Higher values indicate better health (but for doctor visits higher indicates worse).

Figure 2: Dynamic average treatment effects for two-way fixed-effects estimator and robust estimator by Sun and Abraham (2021). Difference in health outcomes between individuals living nearby a newly built wind turbine (i.e. within 4,000 metres) and individuals further away (i.e. between 4,000 and 8,000 metres).

A visual inspection of the leads indicates no difference in time trends between our treatment and control groups in any of the panels, suggesting common trend behaviour pre-treatment.

Again, we do not find a statistically significant effect of a newly built wind turbine on either the mental or physical health summary scales from the SF-12, neither for any lead nor for any lag. We also do not find a consistent effect on self-assessed health or on the reported number of doctor visits. We observe that the small, positive effect on general health is only significant in the year in which a new wind turbine is built (i.e. P value of about 0.02 for each estimator), with no evidence of a lasting positive effect on general health. Considering that we are testing 15 hypotheses (i.e. six leads and eight lags), a standard Bonferroni correction suggests a critical value of $(0.10/15) = 0.007$ for a 10% level of statistical significance, which is again below our empirical P value. Estimates from our two-way fixed-effects estimator once more resemble those from the robust estimator by Sun and Abraham (2021).

It could be the case that potential effects only emerge from more than one turbine. Appendix Figure A.VIII replicates Figure 2 for different treatment intensities, i.e. being treated by one, two to five, or more than five wind turbines, using the robust estimator by Sun and Abraham (2021), to capture potential cumulative impacts, for example by wind farms. As before, we do not find a statistically significant effect of one or several newly built wind turbines on any of our health outcomes.[24]

Perhaps effects only manifest themselves for different age groups. Appendix Figure A.IX replicates Figure 2 for different age groups, defined as younger (between 18 and 40 years), middle-aged (between 41 and 59 years), and older (from 60 years of age onwards). Again, we do not detect consistent impacts on any of these age groups for any of our outcomes.

Although we are unable to detect impacts on our health outcomes, there may still be externalities from newly built wind turbines, though perhaps not sufficiently strong to manifest themselves in adverse health impacts. Because noise annoyances and sleep disturbances are often cited as mechanisms through which adverse health impacts may come about, we also look at the *frequency of experiencing certain emotions* (i.e. happiness, sadness, anxiety, and anger) as well as *sleep satisfaction* and the *number of hours of sleep* on a normal weekday and a normal weekend day as additional outcomes.[25]

Appendix Figure A.X replicates Figure 2 for these additional outcomes. As seen, we again do not find consistent evidence of systematic, statistically significant effects on either happiness, sadness, anxiety, or anger, nor on the number of hours respondents report to sleep or their sleep satisfaction.

[24]There is indication for a temporal effect on doctor visits from 2-5 turbines but in this case the common trend assumption does not hold.

[25]The frequency of experiencing certain emotions is obtained from a five-point Likert scale question that asks "Please indicate for each feeling how often or rarely you experienced this feeling in the last four weeks: angry, worried, happy, and sad", with answers including one ("Very rarely"), two ("Rarely"), three ("Occasionally"), four ("Often"), and five ("Very often"). Moreover, sleep satisfaction is obtained from an eleven-point Likert scale question that asks "How satisfied are you with your sleep?", with answers ranging from zero ("Completely dissatisfied") to ten ("Completely satisfied"). Finally, the number of hours of sleep is obtained from free-text questions that ask "How many hours do you sleep on average on a normal day during the working week? How many hours on a normal weekend day?".

5 Robustness

We conduct a series of tests to investigate the robustness of our results. If not stated otherwise, estimates are based on the robust estimator by Sun and Abraham (2021), a treatment group that lives within 4,000 metres to the nearest newly built wind turbine, and a control group that lives between 4,000 and 8,000 metres, i.e. our baseline specification.[26] For consistency, we conduct each robustness check for each of our health outcomes.

We first look at our standard errors, which, in our baseline specification, are clustered at the plant level, where randomisation takes place. Appendix Table A.V Column 1 shows that clustering our standard errors at the level of households, i.e. at a lower, and hence, less conservative level, does not change our results.[27] Next, we look at endogenous sorting. Recall that we focused on stayers in our baseline specification as movers may move towards or away from installations, depending on preferences, potentially biasing our estimates. Movers may also bias our estimates because moving itself may have health effects. Column 2, however, shows that including movers leaves our results unchanged, suggesting that endogenous sorting is, if anything, only a minor concern. Also recall that we trim observations before the sixth lead and after the eighth lag in our baseline specification, as these are only identified by few observations. We now include these observations in Column 3, thus also capturing potential effects that may occur many years after a new wind turbine was built. As shown, there is no evidence for such effects as our results remain unchanged. Finally, in Columns 4 and 5, we split our estimation sample into the years before and after 2010, i.e. the years in which wind power was still relatively novel and later years, whereas in Columns 6 and 7, we differentiate small from large plants, i.e. plants with a hub height below 100 metres from those with a hub height above. Especially for the latter, a potential concern could be that for a given treatment and control radius, plants with a higher hub height may contaminate our control group, thereby reducing our treatment effect. Focusing on smaller plants should mitigate such concerns.[28] As shown, there are no statistically significant treatment effects (at the 5% level) across Columns 4 to 7.[29]

Next, we look at whether modifying our control group changes our results. What if individuals in our control group living close to an installation (but just outside our treatment radius) are also,

[26]We also implemented the two-stage difference-in-differences framework by Gardner (2022) and Gardner and Butts (2022) as an alternative to Sun and Abraham (2021). In essence, this framework identifies group and period effects in a first stage from the sample of untreated observations, then, in the second stage, it identifies treatment effects by comparing treated and untreated outcomes after removing these group and period effects. We obtain qualitatively similar results using this framework (Appendix Figure A.XI).

[27]Clustering our standard errors at the plant times year level does not change our results either (Appendix Figure A.XII).

[28]In another robustness check, we additionally controlled for hub height, which left our results unchanged (Appendix Figure A.VI).

[29]Appendix Figure A.XIII shows dynamic treatment effects over time when splitting our estimation sample into the years before and after 2010. Again, we find no consistent evidence of adverse health impacts for any lag after a new wind turbine was built.

though minorly, affected by its presence? To answer this question, in Appendix Figure A.XIV, we narrow our control group to individuals living in 500-metre bins between 4,000 and 6,000 metres to the nearest newly built wind turbine. As before, we do not find consistent evidence of adverse health impacts across bins. In Figure A.XV, we then further adjust our control group, by selectively including individuals living further away, as it could be the case that adverse health impacts (for example, due to low-frequency noise emissions) may manifest themselves only at distances greater than 4,000 metres. As before, we find no consistent evidence of such impacts.

Next, we change our treatment radius. Although a treatment radius of 4,000 metres in our baseline specification seems reasonable, and is shown to capture negative externalities of newly built wind turbines on the subjective wellbeing of nearby residents (cf. Krekel and Zerrahn, 2017), we vary our treatment radius in Figure A.XVI. As seen, we also do not find systematic evidence of adverse health impacts at 2,000, 3,000, or 6,000 metres. There is some evidence of a higher number of reported doctor visits two, three, and four years after a new wind turbine was built at a distance of 2,000 metres, but in this case we also observe a potential violation of the common trend assumption prior to treatment. Note that, for a distance of 2,000 metres, the size of the treatment group drops: here, we only observe 318 individuals in our treatment group for our bi-annual health outcomes (i.e. the SF-12) and 584 for our annual outcomes (i.e. self-assessed health and the reported number of doctor visits).

Finally, Appendix Figure A.V shows that excluding and including various fixed effects (i.e. individual, year, county and federal state, and their interactions) does not change our results; results also do not change with the inclusion of fixed effects for different distance bins around newly built wind turbines (e.g. a fixed effect for all households that are located within 1,000 metres to the nearest installation, another for all households that are located within 1,000-2,000 metres, and so on).

6 Additional Analysis: Suicides

We move on to an alternative approach for measuring potential adverse health impacts of wind turbines. In particular, we use suicide rates as an objective measure of adverse mental health impacts, as has been used for air pollution in the US (Molitor, Mullins, and White, 2023) or high temperatures in the US and Mexico (Burke et al., 2018). The advantage of information on suicides is that it relies on administrative records as opposed to self-reports and that it is consistently measured across a population over time. In doing so, we follow Zou (2020), who exploits administrative data on 800 wind farms and suicides at the county level in the US from 2001 to 2013 in a spatial difference-in-differences design and two-way fixed-effects estimators. The author finds significant increases in suicide rates in counties closer to wind farms. In what follows, we replicate our analysis for annual suicides per million population in the 401 counties in Germany. The Statistical Offices of the

German federal states provided us with the data.

We control for covariates shown in Appendix Table B.I. These include unemployment per capita, GDP per capita, and the average age, which are obtained from INKAR (2023).[30] Further, we include county and federal state times year fixed effects.[31]

Table 2 Column 1 shows our baseline results using the robust estimator by Sun and Abraham (2021). It shows differences in suicides per million population between treated counties (those with at least one new wind turbine) *versus* untreated counties (those with no wind turbines).[32] In our baseline specification, we focus on non-urban counties as wind turbines are mainly installed there.[33] We observe no statistically significant differences in suicides between treated and untreated counties.

Next, we move to dynamic effects and look at treatment over time. Appendix Figure B.III, Panel A, shows the estimates from our baseline specification implemented as an event study, with six leads before and eight lags after the first wind turbine is built.[34] A visual inspection of the leads indicates no difference in time trends between our treatment and control groups, which suggests common trend behaviour pre-treatment.[35]

Suicides are extreme events. It could be the case that potential effects only emerge from more than one installation. Thus, we increase the threshold that we regard as treatment. In Table 2, Columns 2 and 3 reveal no effect, neither for ten or more installations nor for counties that reach an installation density of 0.1 or more per square kilometre.[36] The threshold of 0.1 installations per square kilometre indicates a high turbine density. The value lies between the 90th percentile value of 0.09 installations per square kilometre and the 95th percentile value of 0.13 installations per square

[30] Appendix Table B.II shows normalised differences between treated and never-treated counties. Note that we are not particularly concerned about differences greater than 0.25 for GDP per capita as our county fixed effects account for GDP imbalances which should mainly be time-invariant. We also control for the log transformed level of suicides, lagged by 10 years. As we trim our data to observations with six leads before and eight lags after a new wind turbine was built, we only include lagged suicide information before treatment.

[31] County fixed effects capture time-invariant county-specific determinants of suicides, whereas federal state times year fixed effects control for characteristics that vary on the state level and change over time, for example changes in the health care system.

[32] Appendix Figure B.I Panel A illustrates that there are many counties with at least one wind turbine in 2000, i.e. always treated counties. In our estimations, we focus on counties without a wind turbine in 2000 as only these allow us to estimate potential causal effects on suicides from a new wind turbine. Appendix Figure B.I, Panel C, gives a first indication that the average number of suicides per million between counties with and without turbines in 2000 follows a similar trend. Figure B.II, Panel A, shows the number of counties that are treated by year; Panel B the number of counties that are never treated. We observe that the number of counties that are treated is largest at the beginning of our observation period. In line with this observation, Panel C shows the cumulative density of individuals that are treated by year, with a much steeper increase during the first years, as for our self-reported health outcomes.

[33] Concentrating on non-urban counties allows us to analyse a homogeneous group of counties. Nevertheless, we include urban counties in a robustness check below.

[34] As before, the period in which an installation is built is normalised to zero.

[35] Appendix Figure B.III also indicates a common trend before treatment based on the two-way fixed effects estimator.

[36] We drop observations close to thresholds. In Column 2, we neglect observations with between one and nine installations and counties with an installation in 2000. In Column 3, we drop counties with more than 0.1 installations per square kilometre in 2000 and observations with between 0.075 and 0.1 installations per square kilometre. In Table 1 Columns 2 and 3, we include urban counties in order to have a large enough control group.

Table 2: Wind turbines on suicides.

Treatment	At least one turbine	Ten or more turbines	0.1 or more turbines per sqkm
Dependent Variable:		Suicides per million population	
	(1)	(2)	(3)
Variable			
ATT	0.49	0.41	1.5
	(1.1)	(1.3)	(1.6)
Controls	Yes	Yes	Yes
Fixed-effects			
County	Yes	Yes	Yes
State-Year	Yes	Yes	Yes
Statistics			
Adjusted R^2	0.959	0.929	0.940
Observations	1,190	2,843	6,273
N treated	73	126	71
N never treated	20	136	324

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1; clustered (county) standard-errors in parentheses;
Controls are GDP per capita, unemployment rate, average age and the log of number of suicides lagged by 10 years.
In column (1), we focus on non-urban areas only and neglect counties with a turbine installed in 2000.
In column (2), we neglect observations with between 1 and 9 turbines and those with 10 or more turbines in 2000.
In column (3), we neglect counties with more than 0.1 turbines per sqkm in 2000.
In column (3), we also neglect observations between 0.075 and 0.1 turbines per sqkm.

kilometre (for the pooled dataset of counties between 2000 and 2017). A visual inspection of the corresponding event studies in Appendix Figure B.III, Panels B and C, reveals no difference in time trends between our treatment and control groups, neither before nor after treatment, for both alternative treatment thresholds. The only noticeable exception is the third lead in case of counties that had ten or more turbines installed in 2000, which turns out significant with our two-way fixed-effects estimator only.

Table B.III shows that our results are robust in various dimensions. We look at the treatment threshold of at least one wind turbine. In Column 1, we find no effects using the log-transformed level of suicides as an outcome. This approach allows for capturing potentially heterogeneous effects for counties with different suicide levels. In Columns 2 and 3, we focus on suicides per million population again, controlling for the number and the log-transformed number of wind turbines, respectively. Still, we find no evidence for effects of wind turbines on suicides. In Column 4, we include urban areas. Again, there is no evidence of effects. In Column 5, we only focus on the years between 2000 and 2009 (when wind turbines were smaller), and in Column 6, the years between 2010 and 2017 (when they were larger). There is no evidence of effects in either period. In Column 7, we also look at wind turbines nearby a county (within 4,000 metres) as treatment.[37] Again, this

[37] Appendix Figure B.I Panel B is a close-up of the federal state of *Schleswig-Holstein*. For example, blue dots indicate a wind turbine relevant for *Pinneberg* county (in yellow). Here, we consider not only blue dots within the county but also

alternative definition of treatment does not reveal any effects. Finally, in Column 8, we also include counties with a wind turbine in 2000, i.e. always-treated counties. This approach only reveals a correlation but allows for including counties in the north of Germany, where installations are common due to more favourable wind conditions near the North Sea. If there is an effect of wind turbines on suicides, we would still expect significant effects. Again, we find no difference in suicides per population between counties with and those without wind turbines. We conclude that we find no evidence of adverse health impacts of wind turbines on suicides as an extreme measure of negative mental health outcomes.

7 Discussion and Conclusion

It is estimated that, by 2050, wind power will become the most important renewable energy after solar (IEA, 2021). Despite its importance in the transition towards net zero, there is a heated, ongoing debate about potential adverse health impacts of wind turbines on nearby residents, which in many cases manifests itself in vocal resistance against new installations locally. This resistance is often based on a body of evidence that is largely inconclusive and that relies mostly on cross-sectional analyses and local case studies.

This paper set out to determine whether wind turbines have systematic, negative causal effects on the health of nearby residents and, if so, which health dimensions are affected and by how much. It also asked whether effects, if any, are spatially or temporally limited.

For this, we used representative longitudinal household data linked, based on precise geographical coordinates, to a nationwide dataset on wind turbines and a spatial difference-in-differences design that exploited the staggered rollout of installations in Germany, a country that witnessed a fast expansion of wind power since the year 2000. We used both two-way fixed-effects estimators and the robust estimator by Sun and Abraham (2021) to explicitly account for potential treatment effect heterogeneity due to changing technology over time. To our knowledge, we are the first to do so.

We do not find evidence of temporary or even permanent negative effects on either general, mental, or physical health in the 12-Item Short Form Survey (SF-12) (RAND, 2022). There are also no effects on self-assessed health or on the number of doctor visits of nearby residents. Often cited mechanisms through which adverse health impacts of wind turbines may come about include visual and, in particular, noise pollution, potentially resulting in annoyance and sleep disturbances. When looking at the frequency of experiencing negative emotions, sleep satisfaction, and the number of hours of sleep, however, we do not find impacts. Finally, by exploiting administrative data on suicide rates at the county level in Germany during our observation period and by replicating our analysis

those within 4,000 metres distance to the county border.

on health outcomes for suicide rates, we also do not find impacts. Our results are robust to different treatment and control radii as well as different bins around plants, to different plant sizes, and to accounting for residential sorting. By calculating statistical power *ex post*, we confirm that our study is sufficiently powered to detect a small effect size, if present.

While these findings cast doubt on systematic, causal negative effects of wind turbines on the local population, our study has several limitations that warrant readers' attention. For one, while reliance on secondary data and quasi-experimental methods avoids priming respondents and ensures external validity, our sample size and inference are limited when it comes to residents who live in very close proximity to installations, i.e. below 2,000 metres. Similarly, our sample size requires us, in most cases, to estimate average treatment effects. Although these are most relevant for policy applications, they may cast potentially important heterogeneities. For example, theory and evidence in psychology shows that some individuals are more sensitive to (changes in) their environment than others (Pluess et al., 2023). Likewise, individuals who score high in terms of neuroticism, negative affect, and frustration intolerance (Taylor et al., 2013), or who already have a negative predisposition towards wind turbines pre-treatment (Jalali et al., 2016), have been suggested to react more adversely to new installations. Unfortunately, we have no data to capture such individual differences. Finally, the context of Germany itself, in terms of culture and political climate where residents are generally aware of climate change and favourably disposed toward renewable energy, may itself impose limitations when it comes to transferability of findings to other countries.

Although we find no evidence of adverse health impacts, this does not preclude that other externalities do not exist. Negative impacts on the house prices and the subjective wellbeing of nearby residents, for example, are well documented (cf. Gibbons, 2015; Krekel and Zerrahn, 2017). Furthermore, concern or fear of potential negative health consequences is a real phenomenon (cf. Michaud et al., 2016), with actual consequences, including local protests or voting outcomes (cf. Financial Times, 2021). However, recent studies suggest that residents develop more favourable attitudes towards the technology *after* having been exposed to it (cf. Bayulgen et al., 2021; Urpelainen and Zhang, 2022), suggesting learning about one's preferences or rationalisation *ex-post*. In fact, Baxter, Morzaria, and Hirsch (2013) find that residents in communities without wind turbines are *more* concerned about the technology and show *lower* support than residents in communities with installations. Finally, wind turbines can also have positive externalities, for example on local fiscal outcomes or air pollution, which for a balanced assessment need to be taken into account (Kahn, 2013).

In any case, local resistance may slow the transition to renewable energy and risks missing climate action goals, which is why these concerns must be taken seriously and addressed by policy, for example by actively involving resident communities in local planning and decision-making processes and disseminating targeted, factual information grounded in scientific evidence regarding potential impacts. Promising avenues for future research include how to achieve fairness and procedural justice during new build projects, as well as distributional equity in sharing the burden of external effects amongst the general population.

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Appendix

A Health

A.1 Descriptives

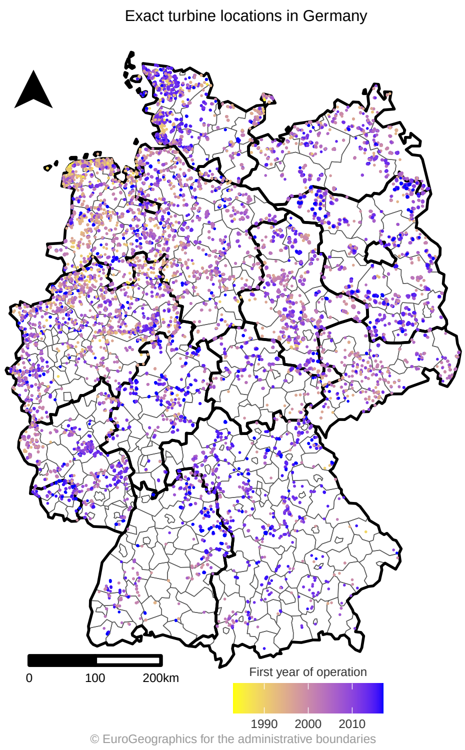


Figure A.I: Exact locations of on-shore wind turbines in Germany until 2017. Each dot indicates a turbine coloured by the first year of operation. Thick black lines indicate the borders of federal states.

Table A.I: Summary statistics.

Variable	Mean	Median	SD	Minimum	Maximum	Observations
Outcomes						
General Health	48.96	45.61	9.75	24.85	66.37	31395
Mental Health: Summary Scale	51.17	52.85	9.79	3.11	79.33	31395
... General	51.13	50.26	9.75	19.73	68.58	31395
... Role-Emotional Functioning	50.34	58.08	9.96	13.34	58.08	31395
... Social Functioning	50.20	57.12	9.97	14.69	57.12	31395
... Vitality	49.64	48.71	9.92	26.82	70.60	31395
Physical Health: Summary Scale	48.19	49.88	10.13	9.21	77.65	31395
... Role-Physical Functioning	49.02	50.27	10.39	21.92	59.72	31395
... Physical Functioning	48.54	50.58	10.35	27.25	58.35	31395
... Bodily Pain	49.17	50.64	10.25	23.00	59.85	31395
Self-Assessed Health	3.32	3.00	0.94	1.00	5.00	31395
Doctor Visits	9.72	4.00	15.45	0.00	396.00	30229
Covariates						
Age	53.50	54.00	16.70	16.00	99.00	31395
Gender [1: male, 2: female]	1.51	2.00	0.50	1.00	2.00	31395
Is Married	0.71	1.00	0.46	0.00	1.00	31395
Is in Civil Partnership	0.00	0.00	0.02	0.00	1.00	31395
Is Divorced	0.06	0.00	0.24	0.00	1.00	31395
Is Widowed	0.07	0.00	0.26	0.00	1.00	31395
Is Unemployed	0.04	0.00	0.20	0.00	1.00	31395
Is on Parental Leave	0.01	0.00	0.09	0.00	1.00	31395
Is in Training	0.02	0.00	0.14	0.00	1.00	31395
Is Part-Time Employed	0.12	0.00	0.33	0.00	1.00	31395
Is Full-Time Employed (baseline)	0.34	0.00	0.48	0.00	1.00	31395
Number of Individuals in Household	2.79	2.00	1.30	1.00	13.00	31395
Number of Children in Household	0.49	0.00	0.92	0.00	8.00	31395
Is Owner	0.70	1.00	0.46	0.00	1.00	31395
Is Renter (baseline)	0.30	0.00	0.46	0.00	1.00	31395
Annual Rent (in 1000)	4.34	2.40	5.73	0.00	119.99	31395
Annual Net Household Income (in 1000)	36.63	31.20	28.85	0.12	1199.99	31395

Summary statistics for outcomes are before standardising.

Table A.II: Wind power plants: summary statistics.

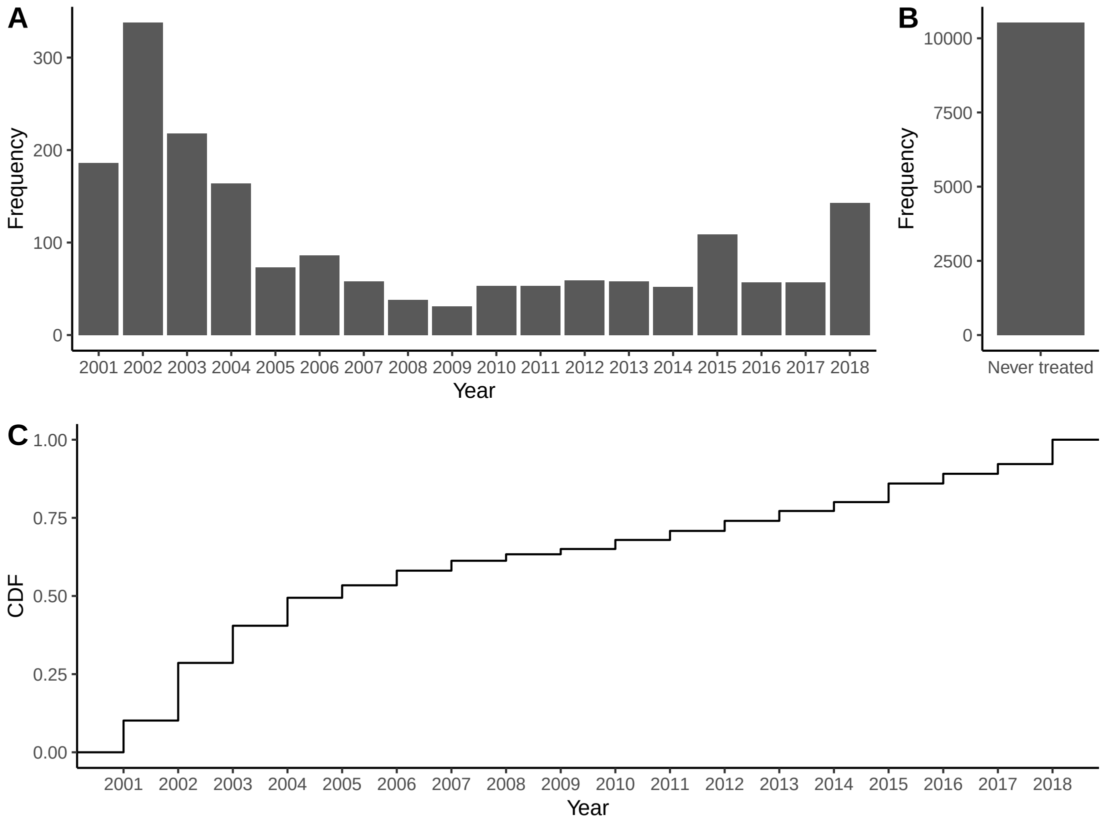
Variable	mean	md	sd	min	max
Power capacity [MW]	1.55	1.5	0.82	0	3.4
Hub height [m]	88.42	85.0	32.15	4	149.0
Rotor diameter [m]	76.23	77.0	23.26	6	126.0

Table A.III: Wind power plants: summary statistics per year.

Variable	year	mean	md	sd	min	max
Power capacity [MW]	2002	1.28	1.50	0.48	0.01	2.00
	2010	1.72	2.00	0.74	0.01	3.05
	2015	2.39	2.40	0.99	0.05	3.30
Hub height [m]	2002	75.71	74.00	18.07	10.00	100.00
	2010	98.11	98.00	34.29	10.00	138.00
	2015	122.06	140.00	35.36	32.00	149.00
Rotor diameter [m]	2002	64.24	70.00	15.44	6.00	80.00
	2010	75.74	82.00	15.17	48.00	101.00
	2015	112.24	115.35	12.84	77.00	126.00

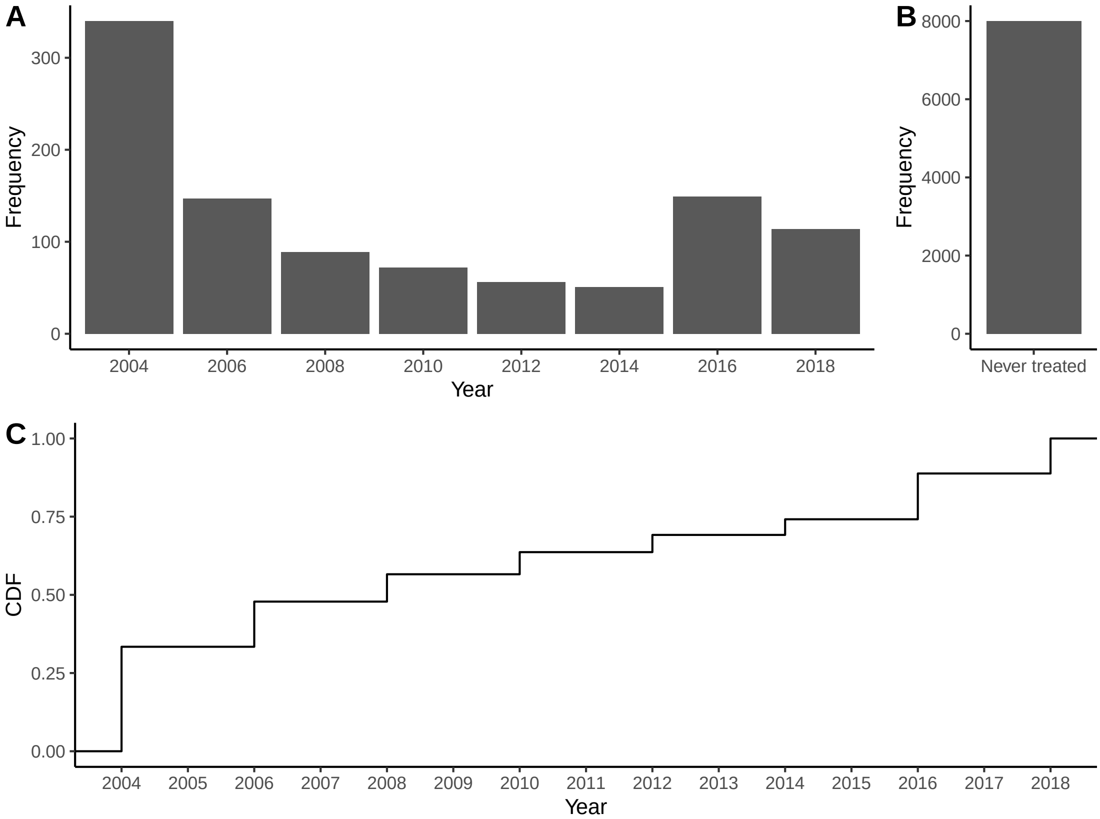
Table A.IV: Normalised differences between treatment (4 km) and control (4-8 km) group.

	Mean		Variance		
Variable	Treatment	Control	Treatment	Control	Normalised Difference
Age	54.91	53.01	252.84	287.36	0.08
Gender [1: male, 2: female]	1.49	1.52	0.25	0.25	0.03
Is Married	0.74	0.69	0.19	0.21	0.08
Is in Civil Partnership	0	0	0	0	0
Is Divorced	0.05	0.06	0.05	0.06	0.03
Is Widowed	0.07	0.07	0.06	0.07	0.01
Is Unemployed	0.04	0.04	0.04	0.04	0.01
Is on Parental Leave	0	0.01	0	0.01	0.04
Is in Training	0.02	0.02	0.02	0.02	0.02
Is Part-Time Employed	0.11	0.12	0.1	0.11	0.03
Is Full-Time Employed (baseline)	0.35	0.34	0.23	0.23	0.01
Number of Individuals in Household	2.72	2.82	1.43	1.76	0.05
Number of Children in Household	0.41	0.52	0.69	0.89	0.09
Is Owner	0.77	0.67	0.18	0.22	0.15
Is Renter (baseline)	0.23	0.33	0.18	0.22	0.15
Annual Rent (in 1000)	4.38	4.33	30.03	33.86	0.01
Annual Net Household Income (in 1000)	35.09	37.17	420.58	976.04	0.06
Observations	8178	23217			



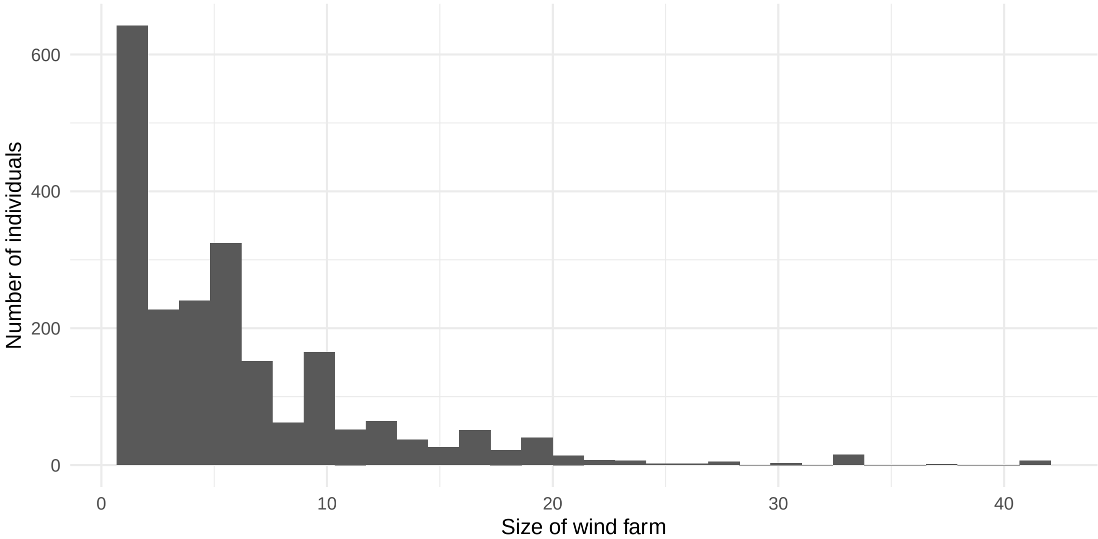
The figure relates to our baseline specification (0-4 km treatment group, 4-8 km control group, outcome: self-assessed health (Table 1 Column 4)). In *Panel A*, we show the frequency of firstly treated individuals (new wind turbine installed nearby individual by year). In *Panel B*, we show the frequency of never-treated individuals. *Panel C*, the cumulative density function of individuals from Panel A.

Figure A.II: Frequency (Panel A) and cumulative density (Panel B) of treated individuals by year and frequency of never treated individuals (Panel C) for outcome self-assessed health.



The figure relates to our baseline specification (0-4 km treatment group, 4-8 km control group, outcome: general health (Table 1 Column 1)). In *Panel A*, we show the frequency of firstly treated individuals (new wind turbine installed nearby individual by year). In *Panel B*, we show the frequency of never treated individuals. *Panel C*, the cumulative density function of individuals from Panel A.

Figure A.III: Frequency (Panel A) and cumulative density (Panel B) of treated individuals by year and frequency of never treated individuals (Panel C) for outcome general health.



The figure depicts the number of treated individuals by the size of the wind park. As seen, most individuals are treated by single wind turbines or by wind farms consisting of less than five wind turbines.

Figure A.IV: Treatment intensity.

A.2 Results

A.2.1 Static

Table A.V: Robustness Checks.

	SF-12 Health Survey: General Health Summary Scale						
	SE Clust. at household (1)	Incl. movers (2)	Incl. all leads and lags (3)	Years < 2010 (4)	Years $\geq$ 2010 (5)	Small plants only (< 100m hub height) (6)	Large plants only ($\geq$ 100m) (7)
Variables							
Treated 0-4 km	0.07*	0.07**	0.05	0.07	0.01	0.07*	0.05
	(0.04)	(0.03)	(0.03)	(0.04)	(0.06)	(0.04)	(0.05)
Fixed-effects							
Individual, County and State-Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics							
Adjusted R^2	0.591	0.585	0.588	0.611	0.592	0.594	0.590
Obs.	26,903	41,051	27,731	10,724	14,432	25,405	24,707
N treated	700	923	700	385	197	417	280
N never treated	8,002	12,669	8,002	3,479	6,007	8,002	8,002

	SF-12 Health Survey: Mental Health Summary Scale						
	SE Clust. at household (1)	Incl. movers (2)	Incl. all leads and lags (3)	Years < 2010 (4)	Years $\geq$ 2010 (5)	Small plants only (< 100m hub height) (6)	Large plants only ($\geq$ 100m) (7)
Variables							
Treated 0-4 km	-0.007	0.009	-0.03	0.03	0.02	-0.007	-0.006
	(0.05)	(0.03)	(0.04)	(0.05)	(0.08)	(0.05)	(0.05)
Fixed-effects							
Individual, County and State-Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics							
Adjusted R^2	0.485	0.481	0.485	0.508	0.487	0.479	0.483
Obs.	26,903	41,051	27,731	10,724	14,432	25,405	24,707
N treated	700	923	700	385	197	417	280
N never treated	8,002	12,669	8,002	3,479	6,007	8,002	8,002

	SF-12 Health Survey: Physical Health Summary Scale						
	SE Clust. at household (1)	Incl. movers (2)	Incl. all leads and lags (3)	Years < 2010 (4)	Years $\geq$ 2010 (5)	Small plants only (< 100m hub height) (6)	Large plants only ($\geq$ 100m) (7)
Variables							
Treated 0-4 km	0.0009	0.006	-0.001	0.01	-0.10	0.01	-0.02
	(0.04)	(0.03)	(0.03)	(0.04)	(0.06)	(0.04)	(0.05)
Fixed-effects							
Individual, County and State-Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics							
Adjusted R^2	0.668	0.663	0.664	0.683	0.675	0.670	0.672
Obs.	26,903	41,051	27,731	10,724	14,432	25,405	24,707
N treated	700	923	700	385	197	417	280
N never treated	8,002	12,669	8,002	3,479	6,007	8,002	8,002

	Self-Assessed Health						
	SE Clust. at household (1)	Incl. movers (2)	Incl. all leads and lags (3)	Years < 2010 (4)	Years $\geq$ 2010 (5)	Small plants only (< 100m hub height) (6)	Large plants only ($\geq$ 100m) (7)
Variables							
Treated 0-4 km	0.02	0.009	0.02	0.05*	-0.02	0.03	-0.008
	(0.02)	(0.02)	(0.02)	(0.03)	(0.04)	(0.02)	(0.03)
Fixed-effects							
Individual, County and State-Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics							
Adjusted R^2	0.601	0.591	0.597	0.620	0.600	0.604	0.601
Obs.	68,289	101,396	71,869	32,488	32,558	63,774	58,944
N treated	1,509	2,038	1,510	1,062	378	1,023	481
N never treated	10,533	16,148	10,533	4,650	7,697	10,533	10,533

	Doctor Visits						
	SE Clust. at household (1)	Incl. movers (2)	Incl. all leads and lags (3)	Years < 2010 (4)	Years $\geq$ 2010 (5)	Small plants only (< 100m hub height) (6)	Large plants only ($\geq$ 100m) (7)
Variables							
Treated 0-4 km	0.03	0.02	0.03	0.05*	-0.03	0.04	-0.009
	(0.03)	(0.02)	(0.03)	(0.03)	(0.06)	(0.03)	(0.03)
Fixed-effects							
Individual, County and State-Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics							
Adjusted R^2	0.357	0.339	0.346	0.366	0.363	0.357	0.348
Obs.	65,068	97,343	68,652	32,446	29,391	60,573	55,746
N treated	1,508	2,037	1,509	1,064	375	1,022	480
N never treated	8,767	13,941	8,767	4,647	5,933	8,767	8,767

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1; clustered (plant, unless differently) standard-errors in parentheses; treatment group 0-4 km; control group 4-8 km*

Table A.VI: Average Treatment Effects: Mental Health.

	SF-12 Health Survey: Mental Health			
Dependent Variable:	General (1)	Role-Emotional Functioning (2)	Social Functioning (3)	Vitality (4)
Variable				
Treated 0-4 km	0.02	-0.03	-0.007	-0.008
	(0.04)	(0.04)	(0.04)	(0.04)
Controls	Yes	Yes	Yes	Yes
Fixed-effects				
Individual	Yes	Yes	Yes	Yes
County	Yes	Yes	Yes	Yes
State-Year	Yes	Yes	Yes	Yes
Statistics				
Adjusted R^2	0.484	0.470	0.431	0.452
Obs.	26,903	26,903	26,903	26,903
N treated	700	700	700	700
N never treated	8,002	8,002	8,002	8,002

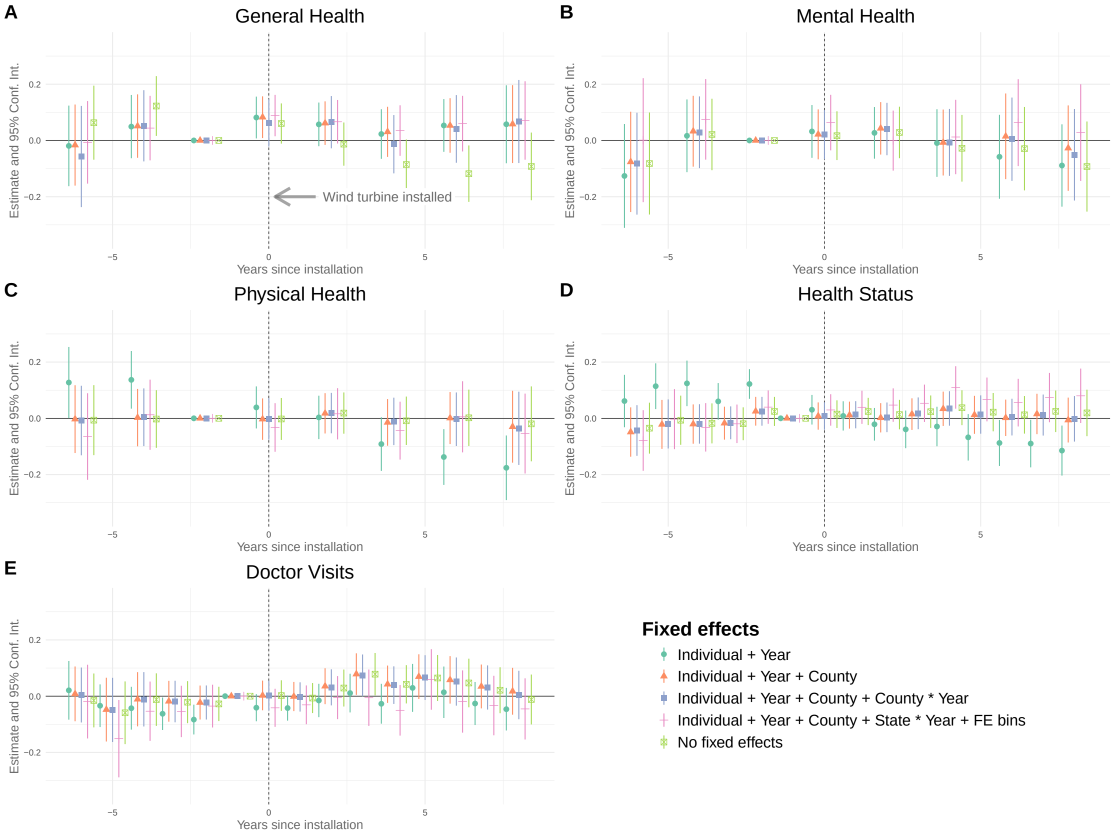
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1; clustered (plant) standard-errors in parentheses; treatment group 0-4 km; control group 4-8 km. Outcomes in z-scores; more indicates better health.*

Table A.VII: Average Treatment Effects: Physical Health.

	SF-12 Health Survey: Physical Health		
Dependent Variable:	Role-Emotional Functioning (1)	Physical Functioning (2)	Bodily Pain (3)
Variable			
Treated 0-4 km	-0.01	-0.01	-0.03
	(0.03)	(0.03)	(0.04)
Controls	Yes	Yes	Yes
Fixed-effects			
Individual	Yes	Yes	Yes
County	Yes	Yes	Yes
State-Year	Yes	Yes	Yes
Statistics			
Adjusted R^2	0.545	0.658	0.522
Obs.	26,903	26,903	26,903
N treated	700	700	700
N never treated	8,002	8,002	8,002

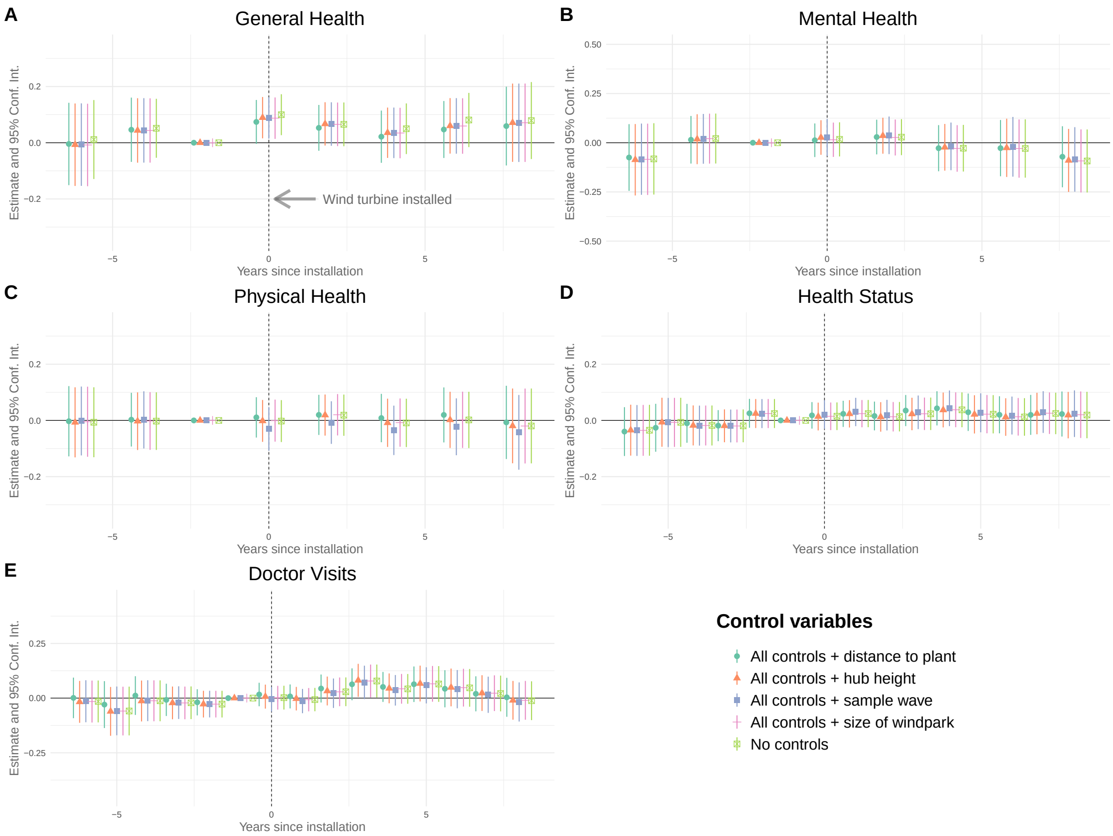
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1; clustered (plant) standard-errors in parentheses; treatment group 0-4 km; control group 4-8 km. Outcomes in z-scores; more indicates better health (but for bodily pain more indicates worse).*

A.2.2 Dynamic



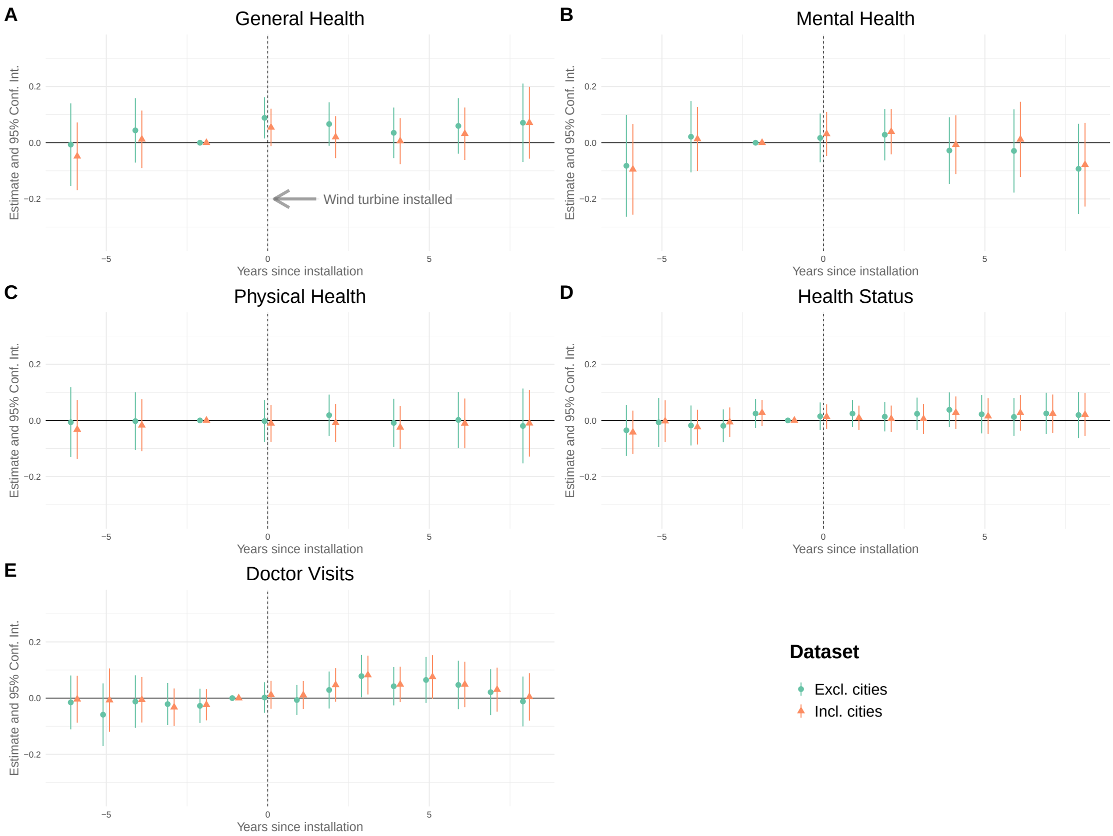
Outcomes are in z-scores. More indicates better health (but for doctor visits more indicates worse).

Figure A.V: Dynamic effects for different fixed effects. Difference in health outcomes between individuals living nearby a newly built wind turbine (i.e. within 4,000 metres) and individuals further away (i.e. between 4,000 and 8,000 metres).



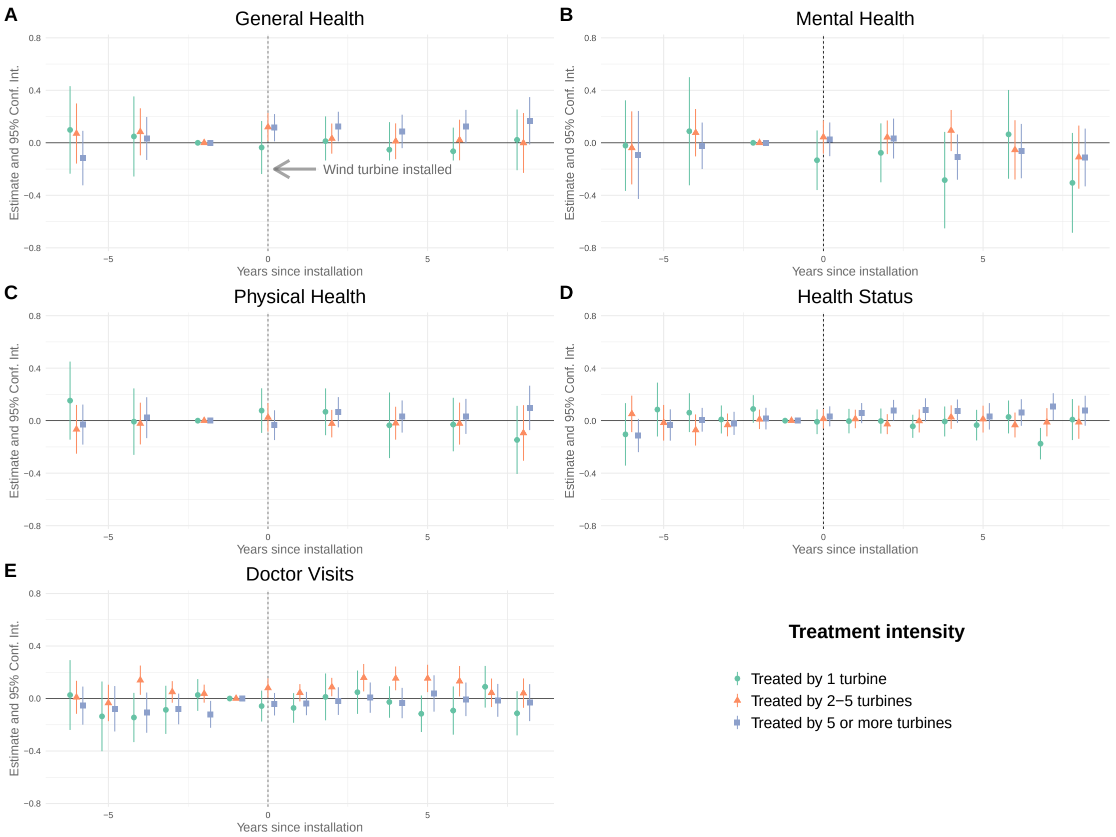
Outcomes are in z-scores. More indicates better health (but for doctor visits more indicates worse).

Figure A.VI: Dynamic effects for different control variables. Difference in health outcomes between individuals living nearby a newly built wind turbine (i.e. within 4,000 metres) and individuals further away (i.e. between 4,000 and 8,000 metres).



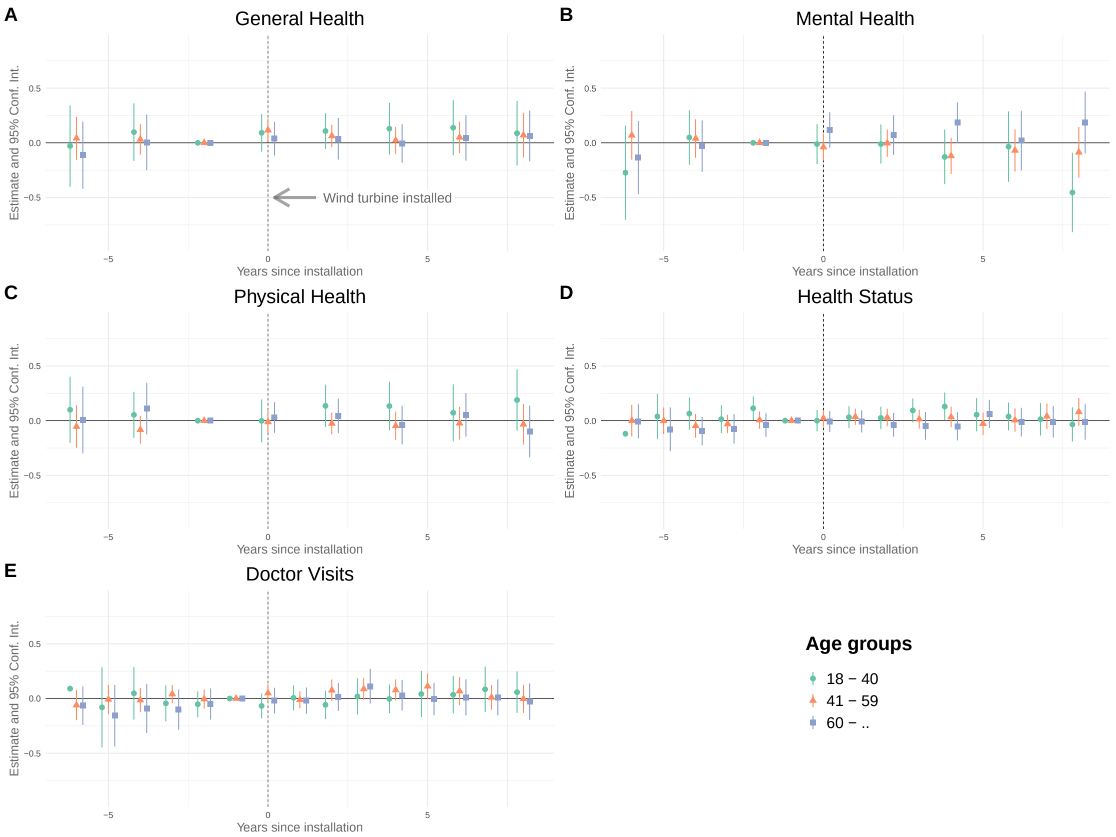
Outcomes are in z-scores. More indicates better health (but for doctor visits more indicates worse).

Figure A.VII: Dynamic effects for different samples, excl. and incl. cities. Difference in health outcomes between individuals living nearby a newly built wind turbine (i.e. within 4,000 metres) and individuals further away (i.e. between 4,000 and 8,000 metres).



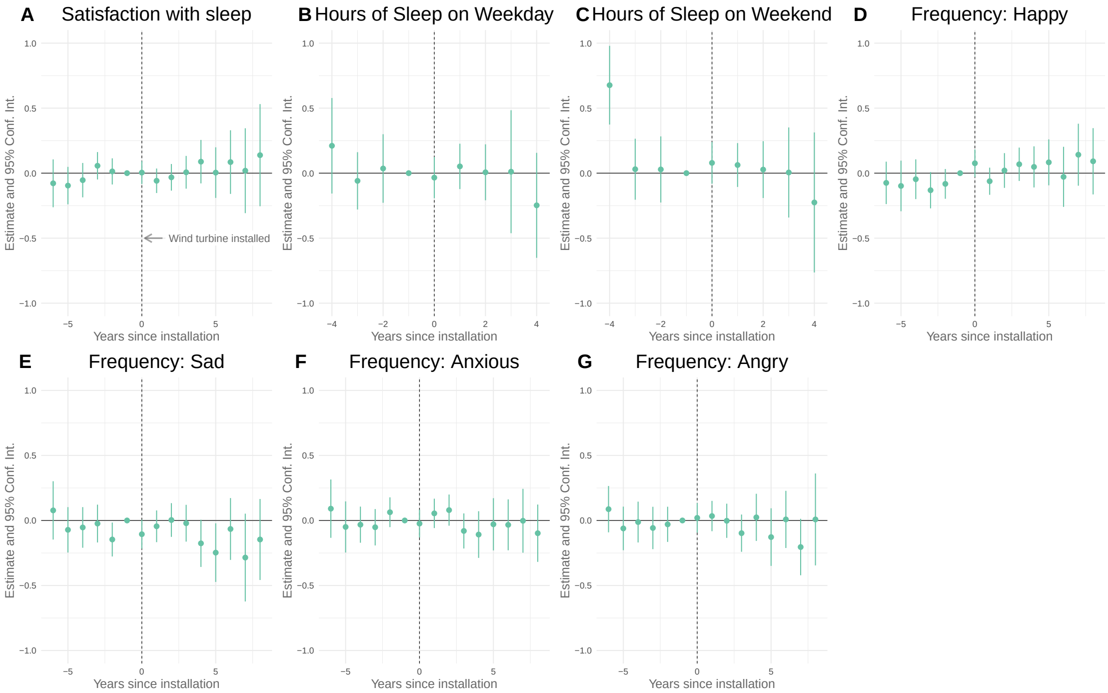
Outcomes are in z-scores. More indicates better health (but for doctor visits more indicates worse).

Figure A.VIII: Treatment intensities. Difference in health outcomes between individuals living nearby one or several newly built wind turbines (i.e. within 4,000 metres) and individuals further away (i.e. between 4,000 and 8,000 metres).



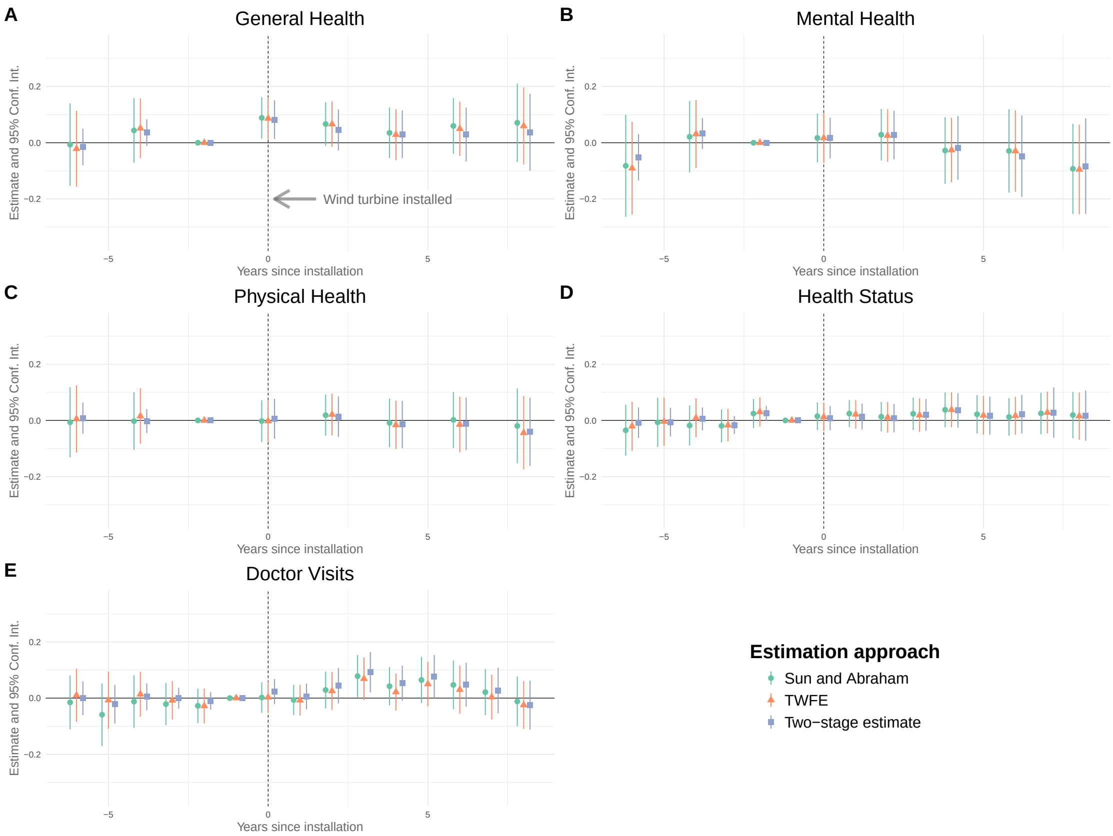
Outcomes are in z-scores. More indicates better health (but for doctor visits more indicates worse).

Figure A.IX: Dynamic effects for different age groups. Difference in health outcomes between individuals living nearby a newly built wind turbine (i.e. within 4,000 metres) and individuals further away (i.e. between 4,000 and 8,000 metres).



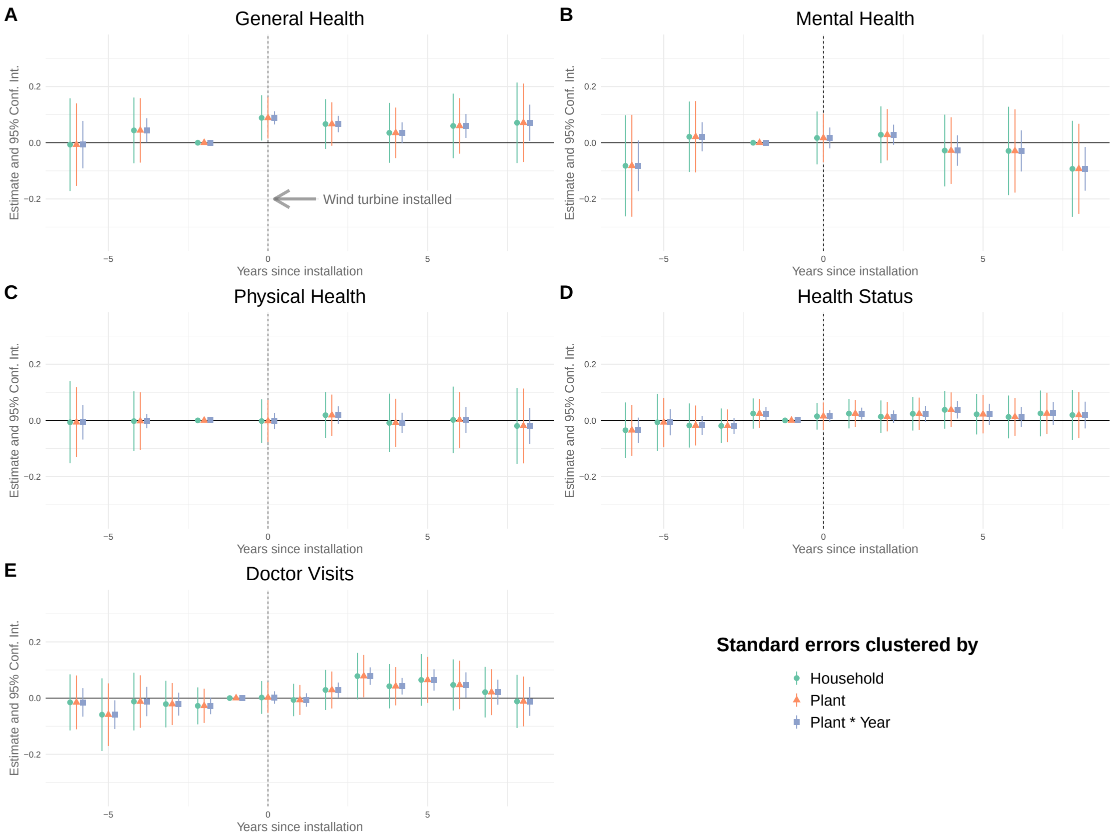
Sun-Abraham estimator. 0-4 km treatment group, 4-8 km control group. Panels A-C as of 2008 (until 2013 for B and C), Panels D-G as of 2007. Outcomes are in z-scores. More indicates better health (but for panels E, F, G more indicates worse).

Figure A.X: Different outcomes. Difference in health outcomes between individuals living nearby a newly built wind turbine (i.e. within 4,000 metres) and individuals further away (i.e. between 4,000 and 8,000 metres).



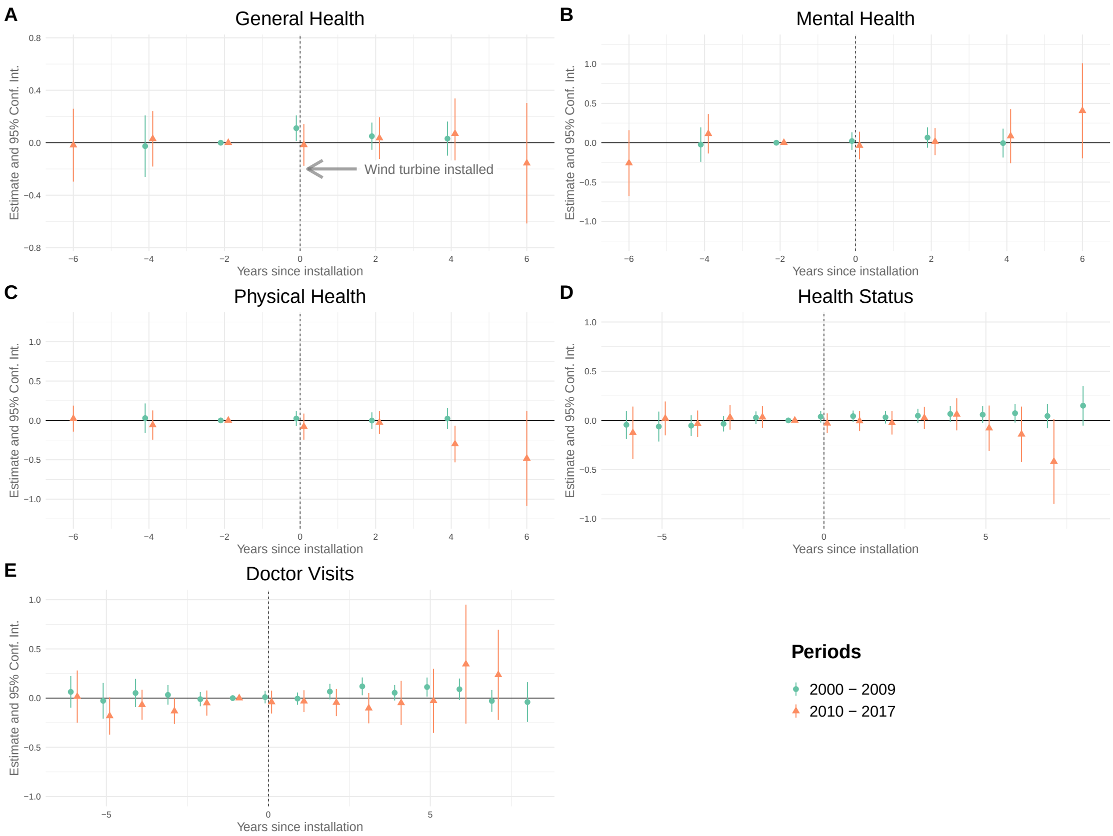
Outcomes are in z-scores. More indicates better health (but for doctor visits more indicates worse).

Figure A.XI: Dynamic effects (different estimators). Difference in health outcomes between individuals living nearby a newly built wind turbine (i.e. within 4,000 metres) and individuals further away (i.e. between 4,000 and 8,000 metres).



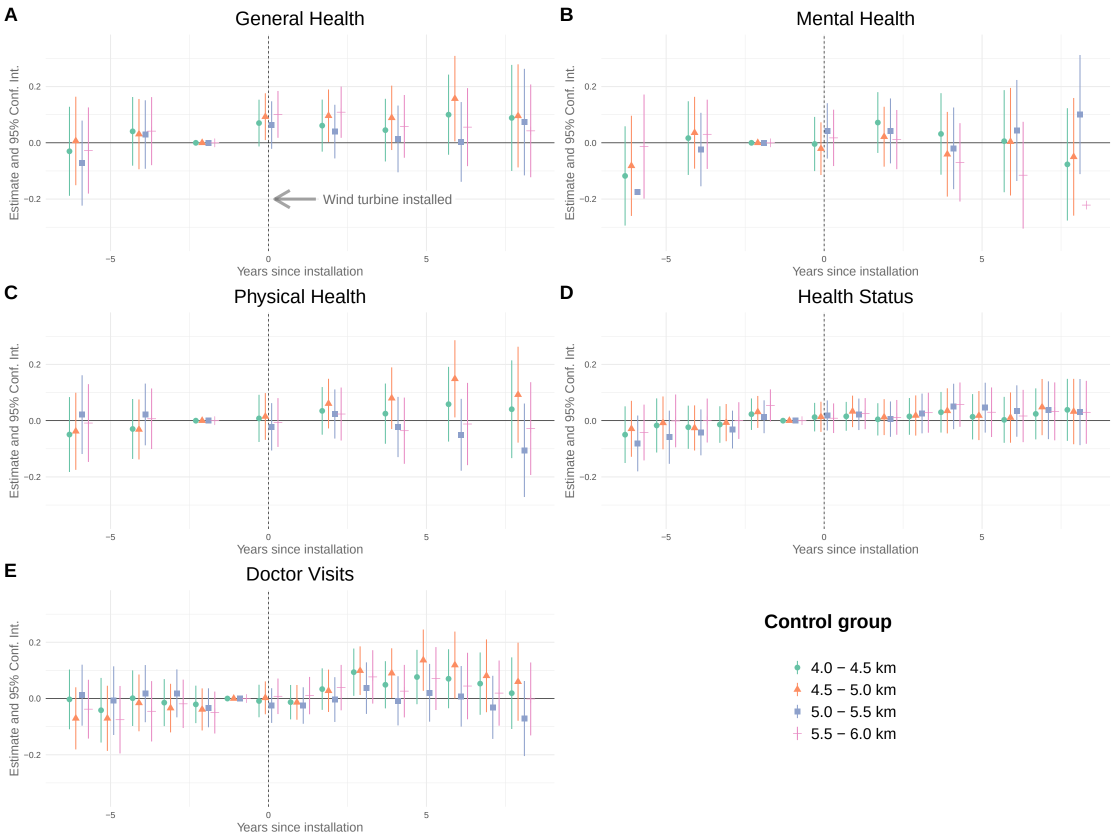
Outcomes are in z-scores. More indicates better health (but for doctor visits more indicates worse).

Figure A.XII: Dynamic effects for different clustering of standard errors. Difference in health outcomes between individuals living nearby a newly built wind turbine (i.e. within 4,000 metres) and individuals further away (i.e. between 4,000 and 8,000 metres).



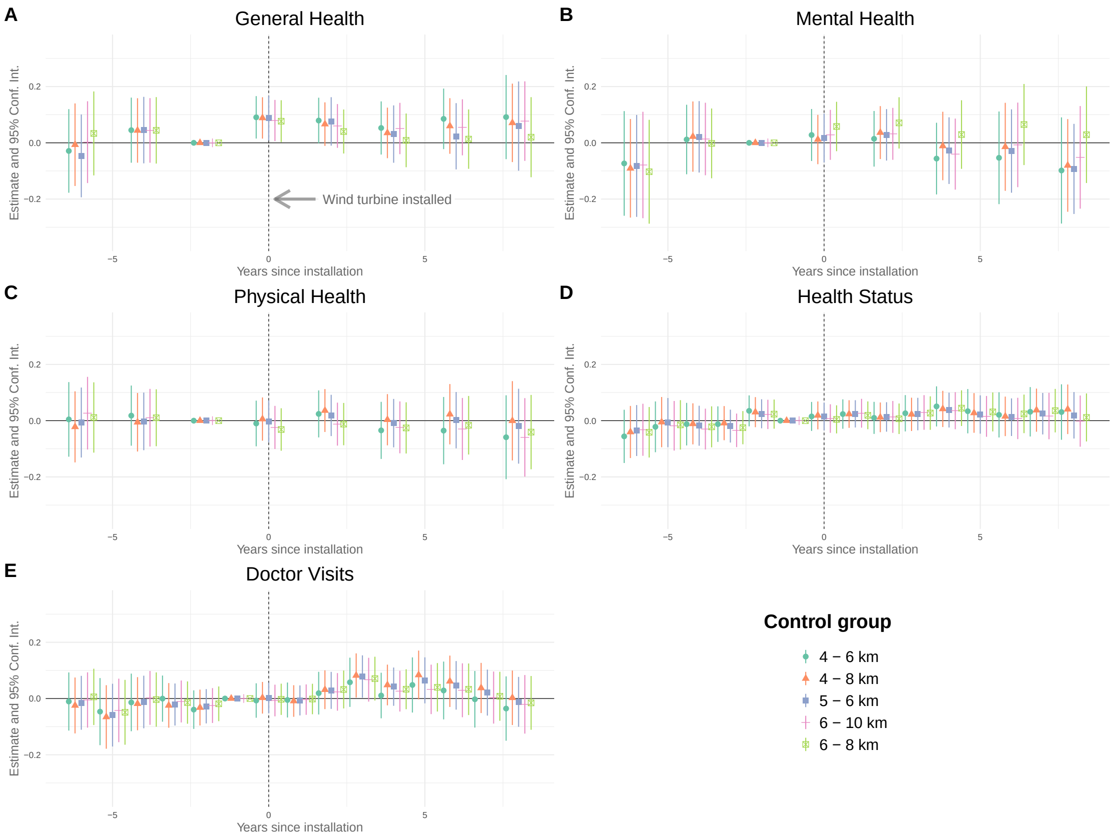
Outcomes are in z-scores. More indicates better health (but for doctor visits more indicates worse).

Figure A.XIII: Dynamic effects for different sample periods. Difference in health outcomes between individuals living nearby a newly built wind turbine (i.e. within 4,000 metres) and individuals further away (i.e. between 4,000 and 8,000 metres).



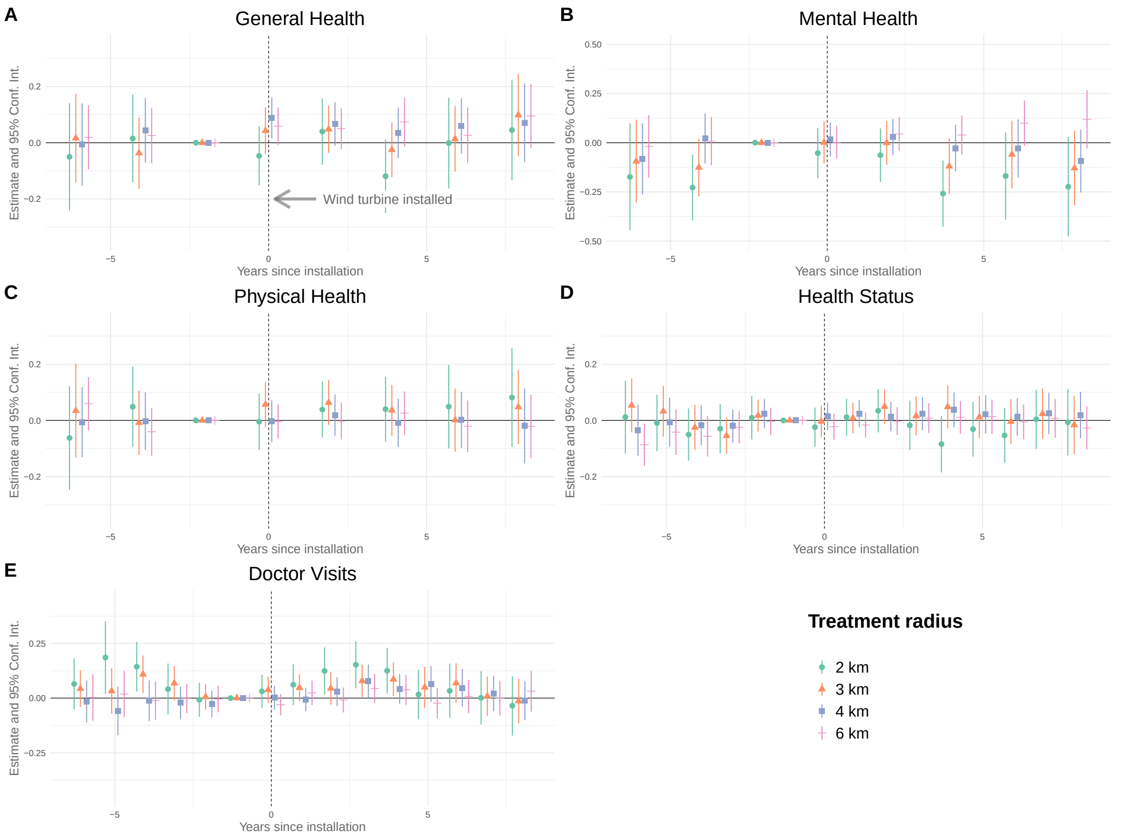
Outcomes are in z-scores. More indicates better health (but for doctor visits more indicates worse).

Figure A.XIV: Dynamic effects for different control groups. Difference in health outcomes between individuals living nearby a newly built wind turbine (i.e. within 4,000 metres) and individuals further away (i.e. between 4,000 and 4,500 metres, within 4,000 and 5,000 metres, within 4,500 and 5,000 metres, within 5,000 and 5,500 metres, or within 5,500 and 6,000 metres).



Outcomes are in z-scores. More indicates better health (but for doctor visits more indicates worse).

Figure A.XV: Dynamic effects for different control groups. Difference in health outcomes between individuals living nearby a newly built wind turbine (i.e. within 4,000 metres) and individuals further away (i.e. between 4,000 and 6,000 metres, within 4,000 and 8,000 metres, or within 6,000 and 10,000 metres).

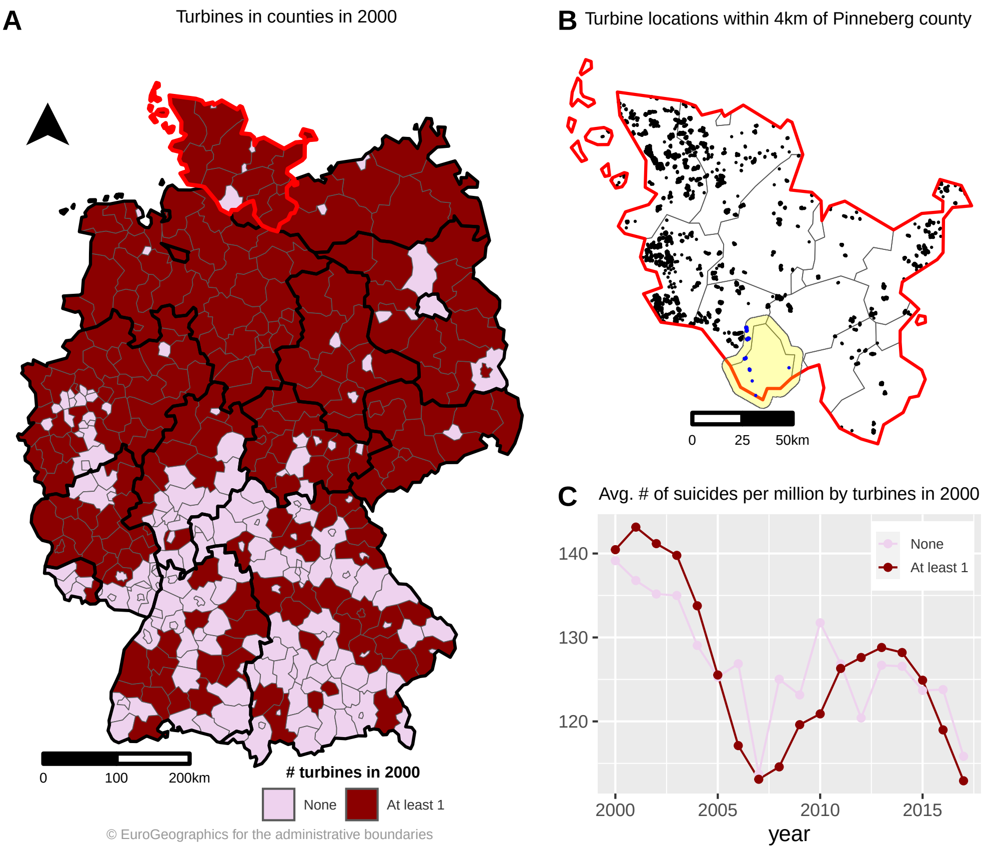


Outcomes are in z-scores. More indicates better health (but for doctor visits more indicates worse).

Figure A.XVI: Dynamic effects for different treatment radii. Difference in health outcomes between individuals living nearby a newly built wind turbine (i.e. within 2,000 metres, within 3,000 metres or within 4,000 metres) and individuals further away (i.e. between 4,000 and 8,000 metres). For treatment of 6,000 metres, the control group is 6,000-10,000 metres.

B Suicides

B.1 Descriptives



Panel A shows counties with and without wind turbines in Germany in 2000. The thick black lines indicate the borders of federal states (NUTS-1 regions), whereas the red thick line indicates the border of the federal state of *Schleswig-Holstein*, the most northern German state. *Panel B* is a close-up of *Schleswig-Holstein* and shows, as an example, the exact location of each installation in that federal state, where each dot indicates one installation. Blue dots highlight turbine locations within 4 km of *Pinneberg* county. *Panel C* plots the average number of suicides per million population by year for counties with and without turbines as of 2000.

Figure B.I: Counties with and without wind turbines in 2000, illustration of turbines nearby a county and average suicides by population over time for counties with and without turbines.

Table B.I: Summary statistics suicides

Variable	Mean	Median	SD	Minimum	Maximum	Observations
Outcomes						
Suicides per million population	128.91	126.38	34.48	22.70	273.98	1190
Covariates						
Unemployed per capita	0.03	0.02	0.01	0.01	0.11	1190
GDP per capita [in thousand EUR]	28.29	26.00	11.29	11.01	107.42	1190
Average age	42.26	42.31	1.75	37.36	48.71	1190

Table B.II: Normalised differences between treated and not treated counties

	Mean		Variance		
Variable	Treatment	Control	Treatment	Control	Normalised Difference
Unemployed per capita	0.03	0.03	0	0	0.03
GDP per capita [in thousand EUR]	30.34	25.8	177.02	56.62	0.3
Average age	42.07	42.5	3.2	2.84	0.17
Observations	539	651			

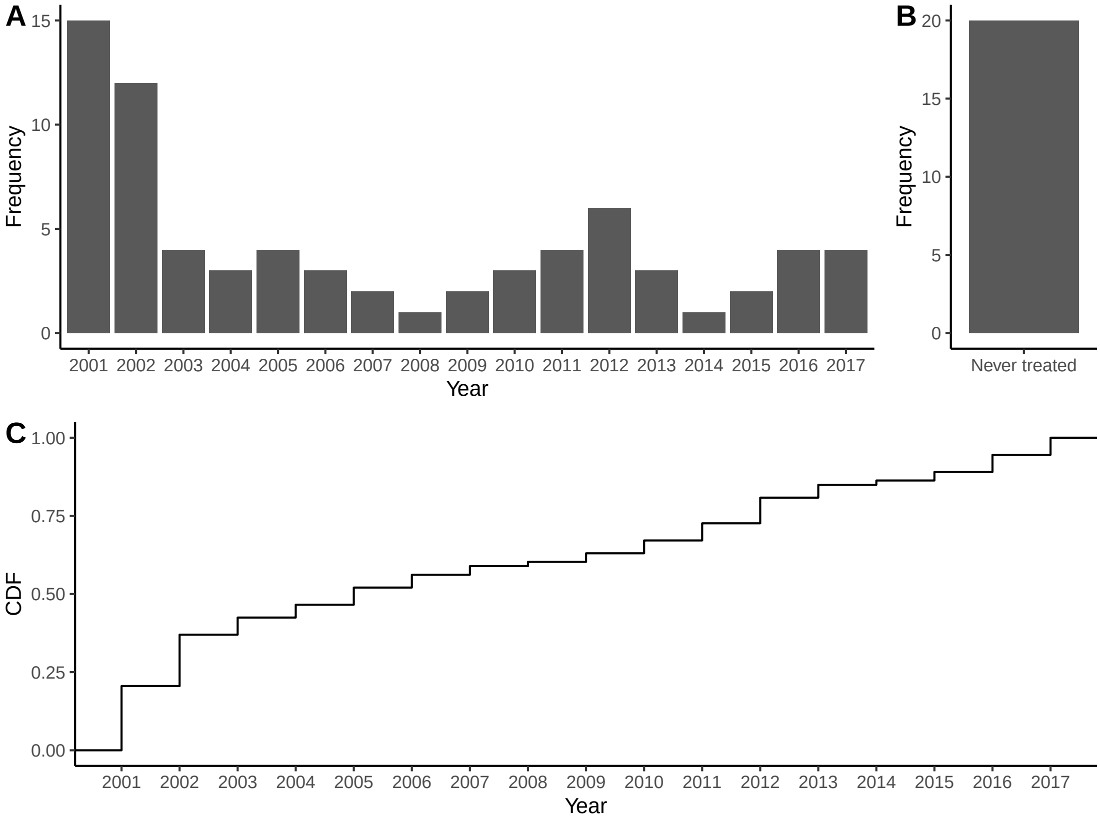


Figure B.II: Frequency (Panel A) and cumulative density (Panel B) of treated counties by year and frequency of never treated counties (Panel C).

B.2 Results

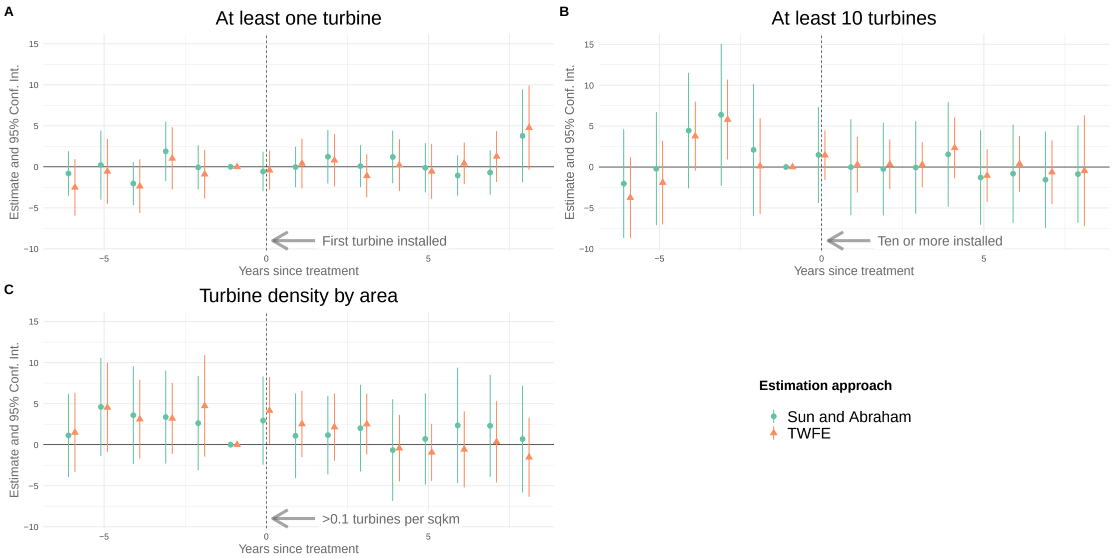
B.2.1 Static

Table B.III: Robustness of wind turbines on suicides.

Description	Other dependent variable	Controlling for intensity		With urban counties	Years 2000-2009	Years 2010-2017	Considering turbines within 4km	With counties with turbine in 2000
Dependent Variables:	ln(Suicides)	Suicides per million population						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variable								
ATT	1.8×10^{-17}	-0.11	-0.78	-1.6	0.90	-3.4	-0.22	-0.39
	(2.6×10^{-17})	(1.4)	(2.2)	(1.1)	(1.3)	(2.9)	(1.4)	(0.93)
# Turbines		0.08						
		(0.08)						
$\ln(1 + \#\text{Turbines})$			0.79					
			(1.0)					
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed-effects								
County	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Statistics								
Adjusted R^2	1	0.959	0.959	0.924	0.963	0.970	0.965	0.949
Observations	1,190	1,190	1,190	2,474	723	390	828	4,700
N treated	73	73	73	102	46	30	55	272
N never treated	20	20	20	74	39	20	11	20

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1; clustered (county) standard-errors in parentheses;
Controls are GDP per capita, unemployment rate, average age and the log of number of suicides lagged by 10 years.
In columns (1-3, 5-8), we focus on non-urban areas only. In columns (1-7), we neglect counties with a turbine installed in 2000.

B.2.2 Dynamic



Standard errors are clustered at the county level. We control for GDP per capita, the unemployment rate and average age. In *Panel A*, we focus on non-urban areas only and also control for the log of number of suicides lagged by 10 years. We neglect counties that had a turbine already installed in 2000. In *Panel B*, we neglect observations with between 1 and 9 turbines installed and counties that had 10 or more turbines already installed in 2000. In *Panel C*, we neglect regions with more than 0.1 turbines per sqkm in 2000. We also drop observations with between 0.075 and 0.1 turbines per sqkm. Table 2 contains further details on the underlying estimations.

Figure B.III: Dynamic effects for wind turbines on suicides per 1,000,000 population for two estimation approaches. Difference between counties with a (new) wind turbine (Panel A), counties with at least 10 turbines (Panel B) or counties with at least 0.1 turbines per sqkm (Panel C) and counties without turbines.

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The Centre for Economic Performance Publications Unit
Tel: +44 (0)20 7955 7673 Email info@cep.lse.ac.uk
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