

Schütt, Marvin

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Wind Turbines and Property Values: A Meta-Regression Analysis

Marvin Schütt¹ 

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Abstract

A key concern for property owners about the set up of proximate wind turbines is the potential devaluation of their property. However, there is no consensus in the empirical hedonic literature estimating this price-distance relationship. It remains unclear if the proximity to wind turbines reduces, increases, or has no significant effect on property values. This article addresses this ambiguity, combining 720 estimates from 25 hedonic pricing studies in a first comprehensive meta-analysis on this topic. Using Bayesian model averaging techniques and novel publication bias correction methods, I calculate an average of the reported estimates that is free from misspecification and publication bias. In economic terms, I find an average reduction in property values of -0.68% for properties 1.89 miles away, which turns to zero beyond 2.8 miles. Next to publication selection, the studies' ability to control for confounding factors such as pre-existing price differentials and spatial effects explains the variance in reported effect sizes.

Keywords Meta-analysis · Wind turbines · Property values · Hedonic pricing · Publication bias · Bayesian model averaging

1 Introduction

Annual global electricity generation from wind farms has grown steadily over the past decade(s) and is expected to continue to grow. For example, the International Energy Agency predicts that annual wind capacity additions will reach 210 GW in 2030, up from 95 GW in 2021 (IEA 2022, 293). Obvious reasons include the low CO₂ emission compared to fossil fuel-based generation, low operating costs, and reduced energy import dependencies from the countries' perspective. While these advantages can be perceived as valuable for society in general, wind farms are simultaneously sources of various negative externalities (Zerrahn 2017; Sovacool et al. 2021). Residents in close proximity may particularly be affected by noises, shadow flicker, and visual deterioration of the landscape, which may result in negative welfare changes (Mattmann et al. 2016; Onakpoya et al. 2015; Liebich et

✉ Marvin Schütt
schuett@economics.uni-kiel.de

¹ Institute for Environmental, Resource and Spatial Economics, Kiel University, Wilhelm-Selig-Platz 1, 24118 Kiel, Germany

al. 2021). In this context, many hedonic pricing studies investigate if these externalities translate to price variations in property values proximate to wind farms. Notably, the empirical evidence is ambiguous, with many studies failing to find a significant (negative) effect on property values due to the presence of wind turbines (Parsons and Heintzelman 2022; Dorrell and Lee 2020; Brinkley and Leach 2019). Furthermore, studies that find a significant effect show a large variance in its magnitude, mostly between -10% and 10% , but also with estimates well beyond this range. Given this ambiguity, it is of great interest for researchers, decision-makers, and property owners to understand whether and under which conditions wind turbines significantly affect local property values.

I address this open question by combining 720 estimates from 25 hedonic pricing studies estimating price-distance relationships for wind turbines and residential properties in the first comprehensive meta-analysis on this topic. Bayesian Model Averaging (BMA) techniques are used to identify sources of heterogeneity attributable to the data characteristics of the primary studies and their ability to control for confounding factors. In addition, I apply a series of novel tests for publication selection bias that allow the calculation of a bias-corrected mean effect size.

Some authors contributing to this literature have already speculated about reasons for the significant differences across studies in terms of reported effect sizes. In particular, Hoen and Atkinson-Palombo (2016) argue that insignificant findings may reflect underpowered studies, having access to only small samples of truly affected properties, i.e., few observations close to wind turbines. Accordingly, for a small effect size, the respective study would be unable to identify such an effect if it exists. There is also suggestive evidence that site conditions such as local opposition (Heintzelman et al. 2017), degree of visibility (Sunak and Madlener 2016) or the number of turbines in proximity (Dröes and Koster 2016) can partly explain the studies' differing findings. In excellent reviews, Parsons and Heintzelman (2022) and Möllney (2022) show that the econometric specification (e.g., avoidance of omitted variables bias, treatment of endogeneity problems) and data characteristics (e.g., distance of treated properties, type of analysed property prices) differ across studies, potentially leading to contrasting results. Similarly, Möllney (2022) acknowledges the difficulties in comparing the empirical findings due to the diversity of employed metrics to measure wind turbine impacts. Parsons and Heintzelman (2022) also conduct what they call a "mini meta-analysis" as part of their qualitative review. That is, they calculate the simple mean effect size for different wind-turbine-to-property distances using 18 observations. They document -4.5% devaluation for houses at 1 km of distance, with the effect fading out after 4.5 kms. However, this finding should be interpreted with caution since they do not (i) correct for publication bias, (ii) include other control variables, (iii) give more weight to more precise estimates, and (iv) use only a subset of the available estimates. I address all of these issues in the methodological setup of this meta-analysis, thus allowing to systematically investigate the existence of price effects of wind turbines on property values at the aggregate level. Building upon these lines of argumentation, I contribute to this literature (i) with a systematic assessment of the causes for effect size heterogeneity and (ii) by calculating an average effect size corrected for publication and misspecification bias.

The results do not confirm a substantial effect of the presence of wind turbines on residential property values. In fact, after correcting for publication and misspecification bias, the resulting effect size corresponds to a reduction of property values of -0.68% for properties 1.89 miles away, which turns zero beyond 2.8 miles of distance. The simple average of reported effect size instead would have indicated a decrease of property values by 2.14% . Selective under-reporting of significant positive findings is responsible for a 22%

overestimation of the effect (based on the preferred publication bias correction method, RoBMA-PSMA , suggested by Bartoš et al. (2023)). Important study characteristics to avoid misspecification bias are, e.g., the accuracy of the distance calculation, usage of spatial controls, and reliance on a difference-in-difference estimation design.

The remainder of the paper is structured as follows. Section 2 summarizes the data collection process and gives an overview of the resulting sample. Section 3 assesses the severity of publication selection bias. Section 4 explores the heterogeneity-explaining factors relying on BMA and presents the results. Section 5 discusses the findings and concludes. The study selection process and coding decisions are explained in detail in the appendix. Finally, model diagnostics for BMA and results for alternative specifications are also reported in the appendix.

2 Data

The strategy to identify relevant studies followed the current guidelines for meta-analyses in economics (Havránek et al. 2020). I used a predefined search query with placeholders for “property values”, “wind turbines” and “hedonic pricing” to find relevant studies. I complemented the list with a forward- and backward search using citations and reference lists of already identified studies, respectively (see Table 4 for a list of search terms). For consideration, studies must use the hedonic pricing method to estimate a price-distance relationship for residential properties and wind turbines. Accordingly, I refrain from combining estimates with different (i) underlying welfare measures (e.g., contingent valuation and hedonic pricing studies), (ii) wind turbine impact metrics (e.g., distance and view), or (iii) property types (e.g., residential properties and farmlands). In the appendix, I outline in more detail that these study selection criteria ensure a consistent set of effect size estimates that can reasonably be meta-analysed. There, I also summarize the study selection process (Fig. 6) and list the included studies (Table 5) with their details (Table 6) and excluded studies by reason for exclusion (Table 7). The resulting final dataset consists of 720 observations from 25 studies. The search was conducted in December 2021 and documented using the reference management software Citavi. The data and code are available via <https://doi.org/10.17605/OSF.IO/UB37W>.

All included studies rely on a semi-logarithmic functional form to elicit the price-distance relationship such that the reported distance coefficients represent semi-elasticities. Accordingly, these semi-elasticities serve as the dependent variable. They can be interpreted as the *percentage change of residential property values at the average distance of a treated house*.¹

Figure 1 shows the distribution of estimates. Most estimates are negative but small in magnitude, with a mean of -2.15% and a median of -1.67% . Still, the range of estimates is substantial, with a minimum and maximum of -66% and $+109\%$, respectively. Given the large variation, I exclude three outliers exceeding $+100\%$ and -50% . These are at the

¹ Here, the average distance of a treated house is 1.89 miles. Note, however, that the definition of *treatment* varies across studies. Some studies consider the announcement of a wind turbine as treatment, while others use the construction date (or both points in time). Similarly, the distance from a wind turbine, at which a house is no longer considered as treated, differs. These aspects are reflected in the selection of moderators, introduced in Sect. 4.1.

extreme ends of the effect size distribution, well beyond all other observations.² The main results are not affected by using different outlier criteria or no outlier criterion at all, see Sect. 4.4. This discrepancy is also reflected in Fig. 2, in which the effect size estimates of each study are summarized in boxplots, ordered by the studies' publication year. Clearly, the estimates vary considerably, both across and within studies. Additionally, there seems to be a trend toward more negative findings and more narrow confidence intervals over time. This might indicate, e.g., better data availability or more refined methodological choices in more recent studies (Hoen and Atkinson-Palombo 2016; Parsons and Heintzelman 2022).

3 Publication Bias

Publication selection bias commonly describes the distortion of a field of literature due to journals' selection of studies or researchers' selection of findings based on statistical precision, magnitude, direction of effect, or any combination thereof (Stanley and Doucouliagos 2012). Accordingly, if researchers or editors expect the presence of wind turbines to either reduce or not affect property values, significantly positive estimates may not be selected for publication, leading to a distorted picture of the literature. Figure 3 shows a funnel plot where the effect size (horizontal axis) is set in relation to its precision $= \frac{1}{SE}$ (vertical axis).³ With no publication bias present, the most precise estimates at the top should mirror the genuine magnitude of the effect. A symmetric dispersion around this *true* mean should result in lower precision levels. Here, the most precise observations have a corresponding effect size close to zero. There is a tendency for relatively more negative results to be found for more imprecise estimates, i.e., moving down the inverted funnel. This is a first indication that significant positive findings of wind turbine effects on property values may be under-represented due to publication bias.⁴

Next to visual tools, many more formal methods exist designed to detect and correct publication bias. Simulation studies show that there is no single best method, but instead indicate that the performance of these methods depends on the magnitude of the effect size, level of heterogeneity in the literature as well as severity and type of publication bias (Alinaghi and Reed 2018; Hong and Reed 2021). Here, I employ the recently developed R package `ROBMA-PSMA` that combines the competing techniques (Bartoš et al. 2023). This method is beneficial for two main reasons. First, it aims for objectivity by testing 36 competing models simultaneously, weighting the result by the fit to the data using BMA. Second, it captures uncertainty about the publication selection mechanism by including models assuming reporting based on p-values (so-called selection models) and those assuming selection based on significance and effect magnitude. Bartoš et al. (2023) show

² This changes the mean and median only little (−2.14% and −1.58%).

³ There are several options for the choice of precision measure used on the vertical axis, as well as the style of the funnel plot (e.g., including pseudo 95% confidence intervals, using contour-enhanced funnel plots), see Sterne and Egger (2001) for a detailed discussion. Here, $\frac{1}{SE}$ is used because the resulting funnel plot gives a clear visual impression of the relationship between effect size and precision. An alternative contour-enhanced funnel plot using *SE* as the precision measure gives a similar impression (see Fig. 9 in the appendix).

⁴ This interpretation is supported by a histogram of t-values shown in Fig. 8, which shows a sharp decrease in the number of reported estimates in the significant positive range.

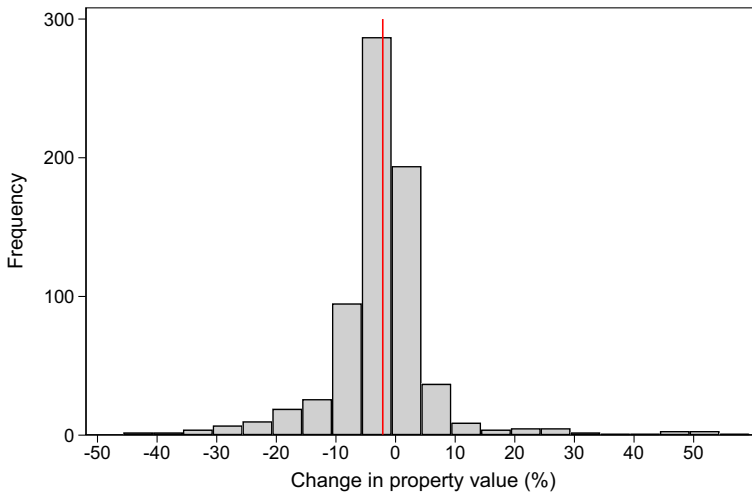


Fig. 1 Histogram of effect size estimates after removing three outliers exceeding +100% and −50%, respectively. The solid vertical line depicts the sample mean

that their approach outperforms most conventional techniques but also advise to complementing their method with other concepts. Accordingly, I also employ other techniques shown to perform well under conditions frequently met in empirical economic settings, i.e., multiple estimates per study, omitted variables bias, or a continuous dependent variable (Alinaghi and Reed 2018; Bom and Rachinger 2019). This encompasses the Endogenous Kink method introduced by Bom and Rachinger (2019),⁵ the selection model advocated by Andrews and Kasy (2019) and the p-uniform* method by van Aert and van Assen (2023). Finally, I also consider methods that only use a subset of the available observations to calculate a bias-corrected mean effect size (and are hence not covered by the RoBMA-PSMA framework). This includes the stem-based method introduced by Furukawa (2019) that is based on the funnel plot logic and the “Top Ten”, which uses only the ten percent most precise observations (Stanley et al. 2010).

The results of the tests for publication bias are summarized in Table 1. Regardless of the chosen method, the corrected effect size is considerably smaller (in absolute terms) than the unweighted mean (OLS estimate) (−0.06 to −1.76 vs. −2.14), confirming publication bias. This is also in line with the visual impression gained from the funnel plot depicted in Fig. 3. Stanley and Doucouliagos (2015) show that with publication bias present, the unrestricted weighted least squares (WLS) estimator, i.e., using inverse-variance weights, gives a more realistic representation of the true unconditional mean and I report it for completeness. Indeed, with −0.38 this simple estimator is within the range of bias-corrected means. With RoBMA-PSMA, the corrected mean effect size is −1.67. This corresponds to an absolute reduction in effect size magnitude by about 22% compared to the unweighted

⁵ The Endogenous Kink method is a non-linear extension to the popular PET-PEESE framework, which formalizes the funnel plot relationship introduced by Stanley and Doucouliagos (2012). Since it reduces to the PET-PEESE method as a special case, I do not consider the latter technique individually.

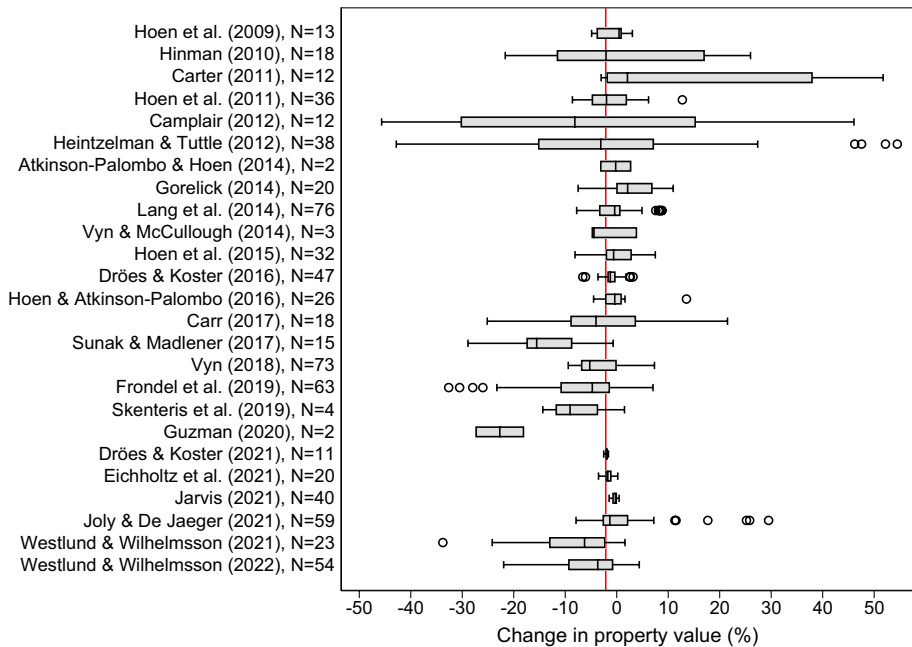


Fig. 2 Boxplots of effect sizes estimates for every primary study after removing three outliers exceeding +100% and -50%, respectively. Studies are sorted in ascending order by publication year. The boxes denote the inter-quartile range (P75-P25), with the mean shown as a solid vertical line. Whiskers indicate the distance up until 1.5 times the IQR starting from the P25 and P75, if applicable. Dots reflect outlying observations within a study. The solid vertical line depicts the sample mean

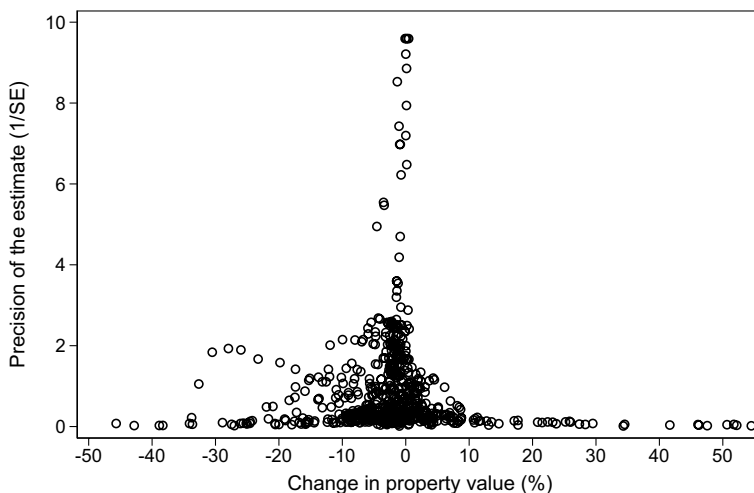


Fig. 3 Funnel plot relating effect size estimates to their reported precision ($\frac{1}{SE}$). Without publication bias the plot should take the shape of an inverted funnel. The most precise estimates are omitted for the ease of exposition but included in all calculations

Table 1 Mean effect size without and with correction for publication bias

	No correction		Correction with full sample				Correction with reduced sample	
	OLS	WLS	RoBMA	A&K	p-uniform*	End. Kink	Furukawa	Top Ten
Mean effect size	-2.14*** (0.37)	-0.38*** (0.06)	-1.67*** (0.30)	-0.29*** (0.21)	-1.76** (0.83)	-0.16** (0.06)	-0.06 (0.35)	-1.03*** (0.17)
N	717	717	717	717	717	717	27	72

***, ** and * denote statistical significance at the 0.01, 0.05 and 0.1 level, respectively

mean.⁶ Turning to the other methods, the p-uniform* estimate is of similar magnitude (-1.76), while the Andrews and Kasy (2019) and the Endogenous Kink method correct more strongly (-0.29 and -0.16).⁷ The remaining methods that only use small subsets of the most precise observations similarly induce a strong correction of the mean effect size (“Top Ten”: -1.03 ; Furukawa: -0.06). This is no surprise, considering that the most precise estimates are clustered around zero in this case.

This section confirms that publication bias is present in the hedonic literature on wind turbines and proximate property values. The unweighted mean of -2.14% is inflated, and the bias-corrected effect size is likely around -1.67% .

In economic terms, the range of estimates translates to a slight decrease in property values on average if a wind turbine is present. None of the estimates, however, take into account the large differences in reported effect size magnitude within and across studies. This could be problematic for two main reasons. First, other sources of bias linked to measurement error or misspecification in the primary studies could systematically influence the magnitude of the reported effect. This could lead to an over- or underestimated mean effect size if only publication bias is corrected. Second, if study design choices are systematically related to the likelihood of publication or the precision of the estimates, the supposed presence of publication bias could, in fact, mirror true heterogeneity. Accordingly, I explore the drivers of the observed heterogeneity in the next section.

4 Heterogeneity

The goals of this section are threefold: first, to find elements of study design that are accountable for the observed effect size heterogeneity; second, to establish if publication bias can still be confirmed even with other controls for study differences in place; and third, to calculate the expected effect of wind turbines on property values corrected for misspecification and publication bias.

⁶ The RoBMA-PSMA package does not calculate significance levels but Bayes factors in determining the likelihood of the effect being different from zero. Here, with $BF = 206$ there is a strong indication of the presence of an effect, following the classification of Lee and Wagenmakers (2014, 105).

⁷ When applying the Andrews and Kasy (2019) approach, researchers have some degrees of freedom. Here, I assume a one-sided selection, i.e., under-reporting of significant positive findings that would signal increases in property values. Assuming a two-sided selection, i.e., underreporting of insignificant findings, changes the effect size estimate to -1.34 , which is close to the RoBMA-PSMA estimate.

4.1 Moderator Selection

A rich set of 42 moderators was coded to identify relevant dimensions in which the selected studies differ. The selection of moderators is based on recommendations in the general meta-analytic literature (Stanley and Doucouliagos 2012; Havránek et al. 2020), existing reviews (Parsons and Heintzelman 2022; Möllney 2022; Brinkley and Leach 2019) and study differences becoming apparent during the data-coding process.⁸ The moderators are grouped into four categories reflecting the primary studies' (1) data characteristics, (2) control variables, (3) specification of the wind turbine impact, and (4) publication-related information. I summarize the variables in Table 2.

4.1.1 Data Characteristics

The primary studies included in the analysis examine very different samples of properties and wind turbines to estimate the wind turbine effect. Accordingly, I define ten variables controlling for data characteristics. For an accurate calculation of the distance between wind turbine(s) and properties, exact coordinates are required. Two variables control if more coarse data, e.g., using wind farm or postcode centroids lead to significantly different results in the reported effect size (*Wind coordinates*, *Property coordinates*). Similarly, I include a variable reflecting the reported mean distance of treated properties from the wind turbine site to test if the price-distance relationship experiences a distance decay (*Distance*). Turning to the sampled properties, some studies have access to actual sales transaction data, while others rely on asking- or assessed prices. Likewise, while most studies analyse effects on already built properties, some focus on residential land. The dummies *Sales* and *Res. land* reflect these differences. Next, I control for the sample size and the time span of the sampling period (*Sample size*, *Sample duration*). Finally, three moderators reflect the general context in which the data were sampled, i.e., a dummy controls if the study was conducted in the USA or elsewhere (*USA*), and two variables reflect the share of wind energy (*Share wind*) and share of renewables (*Share renewables*) in the respective country's electricity mix at the midpoint of the studies' sampling period, respectively.

4.1.2 Control Variables

Although the hedonic pricing framework does not define any exact set of variables to be included in the analysis, omission of relevant control variables may lead to misspecified models and, in turn, to biases in the reported effect sizes (Wooldridge 2010; Phaneuf and Requate 2016). Consequently, the dummy variables *Structure var*, *Neighbourhood var*, *Access* and *Demoecon* capture if studies control for characteristics of the sampled houses (e.g., age, number of rooms), the neighbourhood (e.g., road noise, prison presence), access options (e.g., distance to central business district or highway), or socio-economic factors (e.g., income levels, crime rate, population density), respectively. Similarly, the estimated wind turbine effect may differ for studies that control for other proximate (dis-)amenities (e.g., distance to a park, beach, industrial facility or landfill). The dummies *Oth amen* and

⁸ Following the guidelines referred to in Havránek et al. (2020), a second coder separately coded a substantial proportion of the final dataset, i.e., 12 studies or about 50%. Reassuringly, coding ambiguities were attributable to the varying levels of reporting detail in the primary studies and were reconciled.

Table 2 Definition and summary statistics of variables

Moderators	Definition	N	Mean	SD	Min	Max
<i>Data characteristics</i>						
Wind effect	Change in property value (%)	717	-2.14	9.99	-45.66	54.50
Wind coordinates	= 1 if exact coordinates of wind turbines are used to calculate distances, 0 else					
Property coordinates	= 1 if exact coordinates of the properties are used to calculate the distance, 0 else	717	0.98	0.15	0	1
Sales	= 1 if actual sales price data were used, 0 else	717	0.90	0.30	0	1
Res. land	= 1 if the analysed price refers to the land parcel without the building, 0 else	717	0.90	0.30	0	1
Distance	Mean distance of treated properties in miles	717	0.02	0.14	0	1
Sample duration	Count of years the study's sample spans	717	1.90	1.93	0.13	10.56
Sample total	Total number of observations in the estimate's corresponding regression (in logs)	717	12.72	7.37	0	34.00
Sample stage*	Total number of observations at the construction stage to which the estimate refers (in logs)	717	10.67	2.65	3.22	15.92
Sample treated*	Number of observations to which the estimate refers (in logs)	457	9.00	2.64	3.22	13.85
Sample 1 mile*	Total number of observations within 1 mile of a turbine in the study (in logs)	393	6.57	3.40	0.69	12.58
PC 1 mile*	Number of observations within 1 mile after construction (in logs)	570	8.11	3.13	2.08	13.21
Turbines*	Number of wind turbines	462	7.39	2.85	2.40	12.22
Height*	Average hub height of the turbine(s) in meters	596	5391.36	10439.34	1	40,000
Capacity*	Average installed capacity of the turbine(s) in MW	421	80.19	14.71	59.50	104.55
Farms*	Number of distinct wind farms	449	1.56	0.67	0.76	3.00
National*	= 1 if the studied properties are distributed nationwide, 0 else	461	180.30	494.07	1	1775
USA	= 1 if studied properties are located in the USA, 0 else	717	0.49	0.50	0	1
Share renewables	Share of renewables in national electricity generation (%) in the mid-year of the study's data	717	0.11	0.31	0	1
Share wind	Share of wind energy in national electricity generation (%) in the mid-year of the study's data	717	19.45	20.59	1.13	59.88
<i>Control variables</i>						
Structure var	= 1 if structural property characteristics are used, 0 else	717	2.30	3.06	0.08	9.91
Neighbourhood var	= 1 if neighbourhood-related property characteristics are used, 0 else	717	0.94	0.24	0	1
Access	= 1 if access options (e.g., highway, central business district) are controlled for, 0 else	717	0.82	0.38	0	1
Oth disamen	= 1 if other disamenities are considered in the regression (e.g., industrial facility, landfill), 0 else	717	0.54	0.50	0	1
Oth amen	= 1 if other amenities are considered in the regression, (e.g., park, beach), 0 else	717	0.33	0.47	0	1
Demoecon	= 1 if socio-economic factors (e.g., income, poverty, crime rate) are controlled for, 0 else	717	0.47	0.50	0	1
HPI	= 1 if a housing price index is used to account for time trends, 0 else	717	0.09	0.29	0	1
		717	0.07	0.26	0	1

Table 2 (continued)

Moderators	Definition	N	Mean	SD	Min	Max
Time dummy	= 1 if a dummy is used to account for time trends, 0 else	717	0.53	0.50	0	1
One period	= 1 if no control for time control is needed (one-period data), 0 else	717	0.01	0.12	0	1
DID	= 1 if a difference-in-difference design is used, 0 else	717	0.65	0.48	0	1
Spatial	= 1 if a control for unobserved spatial factors is included (e.g., spatial fixed effects, repeat sales approach), 0 else	717	0.97	0.16	0	1
OLS	= 1 if OLS is used for estimation, 0 else	717	0.74	0.44	0	1
<i>Wind turbine impact</i>						
Dist binary	= 1 if distance variable is binary instead of categorical, 0 else	717	0.24	0.43	0	1
Other wind	= 1 if other wind turbine related variables are used (e.g., view, number of turbines), 0 else	717	0.25	0.43	0	1
Announcement effect	= 1 if wind turbine announcement instead of construction is considered, 0 else	717	0.28	0.45	0	1
AE and CE	= 1 if anticipation and construction effect are both controlled for in the same regression, 0 else	717	0.53	0.50	0	1
Adjustment control	= 1 if price adjustment over time after construction is controlled for in the same regression, 0 else	717	0.06	0.23	0	1
Pooled	= 1 if wind turbines sites are pooled into one impact variable, 0 else	717	0.93	0.25	0	1
One site	= 1 if only one wind turbine site is analysed, 0 else	717	0.06	0.23	0	1
<i>Publication</i>						
SE	Standard error of effect size	717	4.29	6.82	0.02	76.54
Sig	= 1 if 'B' is significant (up to a p-value of 0.1), 0 else	717	0.47	0.50	0	1
Reviewed	= 1 if published in a peer-reviewed journal, 0 else	717	0.65	0.48	0	1
Citations*	Mean of annual citations (in logs)	717	1.26	1.72	-4.09	3.12
Impact*	Recursive discounted RePEc impact factor of the journal or series	717	0.12	0.17	0	0.70
Year publish	Year in which the study was published	717	2016.84	3.73	2009	2022
Precision set	= 1 if precision was set by meta-analyst, 0 else	717	0.15	0.35	0	1

SD standard deviation. *Denotes variables that are only used in robustness checks

Oth disamen reflect this possibility. Furthermore, some studies can address the endogeneity problem using a difference-in-difference design (Kuminoff et al. 2010; Bishop et al. 2020; Greenstone and Gayer 2009), i.e., taking advantage of the information on prices before and after treatment (temporal variation) as well as on prices of proximate and distant properties (spatial variation). This type of study can account for pre-existing price differentials and frequently interpret the estimated coefficient as a causal price change due to the presence of proximate wind turbines. Accordingly, I include the dummy *DID* to differentiate DID studies from those with a standard hedonic pricing framework.⁹ Moreover, the hedonic pricing literature has a consensus that time-invariant unobservable spatial effects should be reflected in the econometric specification for proper identification (Parsons and Heintzelman 2022). Accordingly, I control if the omission of spatial controls results in changes in reported effect sizes using the dummy variable *Spatial*. Finally, the moderator *OLS* distinguishes studies using ordinary least squares from those with other estimation approaches (e.g., instrumental variables or maximum likelihood).

4.1.3 Specification of Wind Turbine Impact

The chosen econometric specification of the wind turbine impact may influence the effect size. Accordingly, I distinguish specifications with one treatment zone from those with several (*Dist binary*). Similarly, I test if the consideration of additional wind-turbine-impact variables, such as the number of turbines or the degree of visibility next to the distance variable leads to differences in the estimated effect (*Other wind*). Furthermore, three dummies test the influence of differences in the definition of the treatment period. First, I distinguish if the estimated effect size refers to the announcement or the construction date of a wind turbine (*Announcement effect*). Second, some studies consider both announcement and construction date in their econometric specification, while others define treatment for only one point in time (*AE and CE*). Finally, in a few cases, authors test if the effect size adjusts over time, mirroring a habituation effect (*Adjustment control*).

4.1.4 Publication

Taking the presence of publication bias into account, I include three variables to test if it can still be confirmed when the controls for heterogeneity introduced above are in place. This includes the standard error of the reported coefficient (*SE*), a dummy signaling its significance (*Sig*), and a variable reflecting peer-review status in a scientific journal (*Reviewed*). Including the standard error formalizes the funnel plot logic on which the methods by Furukawa (2019) and Bom and Rachinger (2019) are based. The significance dummy mirrors selection based on p-values (Andrews and Kasy 2019; van Aert and van Assen 2023). The peer-review control captures two aspects. First, it should reflect some

⁹ Parsons and Heintzelman (2022) note that concerns about biases in estimating treatment effects have been raised when treatments are staggered over time (de Chaisemartin and D'Haultfoeuille 2020, 2022; Steigerwald et al. 2021). Since most sampled studies pool observations from different wind farms with varying corresponding construction dates, treatments are clearly staggered over time in this literature. I am aware of only one study (Möllney 2022) that explicitly considers this methodological aspect. However, this Master's thesis could not be considered for the meta-sample as distance coefficients were not estimated. Jarvis (2021) and Dröes and Koster (2016) are the only included studies that approach this issue using an event study design. Jarvis (2021) states, however, that this is only a partial solution to the problem of staggered treatments.

particularities of the studies linked to their quality not captured by the variables introduced above. Second, since the peer-review process critically examines, e.g., the methodological choices or econometric specification and other decisions made by the respective authors, this process may have induced changes in the reported results. Thus, this dummy moderator measures the magnitude of another potential facet of publication selection. Next, I include the publication year to account for time-trend effects (*Year publish*). Finally, in some cases, the standard error was not reported. If other precision measures were available instead (e.g., t-value or p-value), they were converted accordingly (see also the detailed explanation on the standardization of the precision measure in the appendix and Fig. 7). In other cases, however, only information on the significance level of an estimate was given. For these estimates, I set the precision at a conservative level. The dummy *Precision set* is included to assess the influence of this coding decision.

These moderators reflect the most prominent aspects of the included studies and, thus, account for key differences in data and methodology. Additional moderators, e.g., the level of urbanity in the area of the sampled properties, the degree of turbine visibility, or the presence of local opposition, were initially considered but dismissed. These variables were inconsistently defined and could, therefore, not be used in a comprehensive comparison across studies.¹⁰ Additionally, I tested the added value of finer-grained categorical moderators instead of dummies (e.g., using different significance levels instead of a significance dummy). If there were no changes to the results, I opted for model parsimony using a dummy specification (see the code available via <https://doi.org/10.17605/OSF.IO/UB37W> for details). Finally, for another subset of moderator candidates (i.e., wind turbine and additional sample size characteristics), information was missing for several studies. I include these variables in Table 2 for completeness (marked with an asterisk). However, to conserve sample size, I conduct separate analyses with this type of moderator, see Sect. 4.4.

4.2 Estimation

The choice of the correct meta-analytic model is a context-specific and data-driven issue, which involves (at least) decisions on treating publication bias as well as meta-model and moderator selection. Publication bias has clearly been confirmed, and I subsequently defined several relevant variables that can be considered to address this issue in a regression framework. Regarding model selection, I adopt a standard multivariate meta-regression model in the baseline specification. That is

$$\text{wind effect}_i = \beta_0 + \sum_{k=1}^K \beta_k * M_{k,i} + v_i + \epsilon_i \quad i = 1, \dots, N \quad (1)$$

where each reported effect size i is put in relation to a set of k study characteristics used as moderators $M_{k,i}$, with v_i reflecting unobserved heterogeneity assumed to be $v_i \sim N(0, \tau^2)$ and $\epsilon_i \sim N(0, \hat{\sigma}_i^2)$ representing the error term. Equation 1 is estimated using WLS with random effects weights, i.e., $w_i = \frac{1}{\hat{\sigma}_i^2 + \tau^2} = \frac{1}{\widehat{SE}_i^2 + \tau^2}$, where \widehat{SE}_i is the standard error corresponding to the reported effect size i . This gives greater weight to more precise observations in

¹⁰ One reviewer suggested a dummy moderator reflecting the market extent of the data and a journal quality indicator as additional moderators. However, due to coding ambiguities for these moderators, I refrain from using them in the main specification and instead report them in separate extensions to the baseline regression, see Table 12. For completeness, I include these moderators in Table 2.

the dataset and addresses heteroscedasticity in the error term, which naturally occurs when combining estimates with different variances from several studies.¹¹ I assess the effect of choosing this baseline specification on the results using a series of robustness checks, including different assumptions on the error term, weighting schemes, and data dependency. The main results are unaffected by the model selection, as discussed in Sect. 4.4.

Finally, while all moderators introduced above may systematically influence the effect size, using all of them jointly in the regression may obscure true data patterns since some moderators will prove collinear in explaining wind turbine effects on property values. Accordingly, I use BMA to address model ambiguity. This technique does not require selecting one particular specification for the meta-regression.¹² BMA has become a frequently used tool in economics in general (Steel 2020) and meta-analysis in particular (Havranek et al. 2015; Matousek et al. 2022) to address model uncertainty. For a recent overview, see Steel (2020). The following brief description covers the basics of this technique and introduces relevant terms needed for inference in the subsequent analysis.

In BMA, all possible combinations of moderators are estimated in individual regressions, and a weighted average of these models is constructed. The weights correspond to the posterior model probabilities. This measure reflects the model fit of individual specifications conditional on the data and model parsimony, analogous to adjusted R^2 in a frequentist setting. The importance of each moderator is measured in terms of posterior inclusion probability (PIP), i.e., the sum of all posterior model probabilities for all regressions that include the specific variable. This corresponds to statistical significance in frequentist econometrics (Steel 2020). Here, with 34 potential moderators with full observations 2^{34} models could be estimated. To reduce computational complexity, I follow common practice and rely on the Markov Chain Monte Carlo algorithm included in the `bms` package for R (Zeugner and Feldkircher 2015), which considers only the most promising models, i.e., those with the highest posterior model probabilities.¹³

BMA requires the selection of priors for the model space and regression coefficients (so-called g-prior). Without sufficient knowledge about the coefficients' magnitude, I choose the popular unit information prior as g-prior in the baseline specification, which performs well in simulations (Eicher et al. 2011). This prior assumes a zero mean for all coefficients and has about the same information content as one observation. For the model space, I use the uniform model prior (Eicher et al. 2011), which gives each model the same probability. I assess the sensitivity of results using other common choices for the prior structure in robustness checks, see Sect. 4.4.

¹¹ Note that there is some controversy regarding the decision for or against random effects weights in the literature. Stanley and Doucouliagos (2017) show in simulations that fixed effects estimation with inverse-variance weights is, in general, preferable to random effects estimation if publication bias is present. In this study, however, the reported standard errors differ substantially in magnitude, resulting in excessive influence of very precise observations and barely any weight for imprecise ones in the fixed effects WLS framework. Moreover, Irsova et al. (2023) show that exaggerated precision of observations entering the meta-sample leads to biases in fixed effects WLS estimation. Even excluding the most precise observations does not address the weighting problem adequately. Additionally, there are many observations with high and low precision simultaneously with no clear cut-off point. Accordingly, I decided to use the WLS-RE framework in the baseline setting, which offers more evenly distributed weights due to adding the parameter τ^2 .

¹² Another popular option is to follow a general-to-specific approach, which includes a step-wise exclusion of moderators that fail to fall within a certain significance threshold. While commonly applied, this technique is not statistically valid and may lead to omitting relevant moderators (Havranek et al. 2015).

¹³ I employ 50 million iterations and 10 million burn-ins to ensure convergence.

4.3 Results

Figure 4 illustrates the results of BMA. The columns represent individual regression models sorted by their posterior model probability starting with the best model on the left. The vertical axis lists the variables in descending order, sorted by posterior inclusion probability indicating importance. A blank cell indicates the variable is not included in the respective model. The red colour implies that the corresponding regression coefficient is positive, while a blue colour signals the negative sign of the coefficient. The best model in terms of posterior probabilities on the left includes twelve out of the 34 variables used in the analysis. These variables are also the only ones with a posterior inclusion probability above 0.5. This threshold signals a non-negligible effect of these variables on the effect size in the classification of Kass and Raftery (1995). According to this rule of thumb, the moderators have a weak, positive, strong, or decisive impact of the effect size if the PIP lies between 0.5 and 0.75, 0.75 and 0.95, 0.95 and 0.99, or 0.99 and 1, respectively. All other variables do not systematically influence the magnitude of the estimated effect.

Table 3 gives the corresponding numerical results of BMA¹⁴. The left panel reports the posterior mean, posterior standard deviation, and posterior inclusion probability for each explanatory variable's regression coefficient. The right panel shows the results of a frequentist WLS check, including the twelve variables with a posterior inclusion probability of 0.5 and higher. The estimated coefficients in both panels have the same sign and similar magnitudes as well as the same statistical importance (posterior inclusion probability in the BMA setting and its frequentist equivalent, p-value). Accordingly, the results of the frequentist check are consistent with the baseline BMA. In the following, I present results by moderator category.

4.3.1 Data Characteristics

The type and quality of the analysed data are central determinants of the estimated effect size. In particular, using exact wind turbine coordinates for the distance calculation seems essential to estimate the price-distance relationship reliably. Relying instead on, e.g., wind farm centroids induces imprecise estimation of this price-distance relationship, reflected in the large coefficient for this variable.¹⁵ Moreover, the type of analysed property price seems to be an important dimension of effect size variance. Using actual sales data instead of asking prices or assessed values is associated with significantly more positive findings of about 3.26 percentage points. One possible interpretation for this finding is that residents who decide to offer their property include a price discount in their asking price, mirroring the (subjectively perceived) lower value due to wind turbine presence. Similarly, assessors seem to devalue properties with a proximate wind turbine on average. Apparently, both types of property prices (asking and assessed) lead to inflated negative estimates of the effect of wind turbines compared to actual sales prices.¹⁶ The type of analysed property also seems to affect the effect size. Investigating the price effect on undeveloped residential

¹⁴ The corresponding posterior model size distribution and convergence are shown in Fig. 10.

¹⁵ Note, however, that only 16 observations from two studies do not rely on exact wind turbine coordinates. Even though the finding is robust to including study-level fixed effects, see Sect. 4.4, I caution against generalizing this finding accordingly.

¹⁶ While this finding is robust in almost all sensitivity analyses, note that including study-level fixed effects leads to a loss in significance for this variable, see Sect. 4.4.

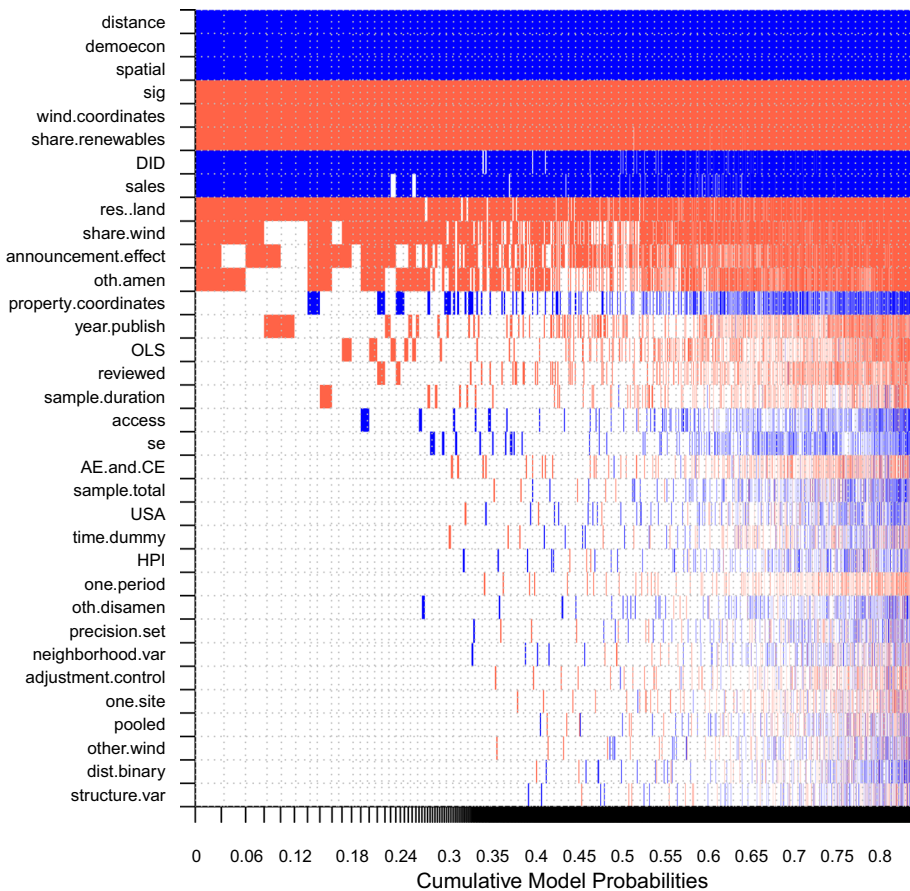


Fig. 4 Model inclusion probability of moderators. The response variable is the estimated price-distance coefficient relating wind turbines and property values. The columns represent individual models sorted by posterior model probability. The variables are depicted on the vertical axis, ordered by posterior inclusion probability in a descending array. A blue (red) cell indicates the inclusion of the variable in the model and that the estimated sign is positive (negative). A blank cell indicates that the variable is not included in the model. The uniform model prior and the unit information prior (Eicher et al. 2011) are used for the model space and the coefficients, respectively. Corresponding numerical results are presented in Table 3

land instead of residential buildings leads to more negative estimates on average (−9.54 percentage points). This may be due to the increased visibility of wind turbines when no structure has been built yet. Note, however, that only one study (Sunak and Madlener 2017) contributing 15 observations relies on residential land values. Accordingly, I caution against generalizing this finding even though it remains robust to the inclusion of study-level fixed effects. In line with expectations, I find that the values of properties that are located at greater distances from a wind turbine are less affected, i.e., for each additional mile of distance from a wind turbine, property values increase by 0.73 percentage points, *ceteris paribus*. Additionally, for countries with a comparatively higher share of wind power in their electricity mix (or renewable energies in general) when the respective study

Table 3 Bayesian model averaging results

Moderators	Bayesian model averaging			Frequentist check (WLS)		
	Post. mean	Post. SD	PIP	Coef	SE	p-value
<i>Data characteristics</i>						
Wind coordinates	-14.956	2.803	1.000	-11.605	2.603	0.000
Property coordinates	0.398	0.873	0.227			
Sales	3.262	1.153	0.955	3.543	0.817	0.000
Res. land	-9.541	3.924	0.931	-10.470	2.859	0.000
Distance	0.730	0.089	1.000	0.721	0.083	0.000
Sample duration	-0.005	0.020	0.089			
Sample total	0.006	0.051	0.050			
USA	0.020	0.233	0.048			
Share renewables	-0.059	0.013	0.997	-0.052	0.010	0.000
Share wind	-0.265	0.189	0.753	-0.301	0.100	0.003
<i>Control variables</i>						
Structure var	-0.002	0.148	0.027			
Neighbourhood var	0.007	0.119	0.033			
Access	0.060	0.258	0.081			
Oth disamen	0.013	0.133	0.038			
Oth amen	-0.728	0.756	0.550	-1.046	0.440	0.018
Demoecon	4.651	0.775	1.000	5.068	0.697	0.000
HPI	0.022	0.259	0.042			
Time dummy	-0.012	0.170	0.042			
One period	-0.296	2.196	0.040			
DID	2.429	0.826	0.963	2.327	0.538	0.000
Spatial	11.365	1.075	1.000	11.172	1.058	0.000
OLS	-0.163	0.453	0.152			
<i>Wind turbine impact</i>						
Dist binary	0.002	0.084	0.029			
Other wind	0.002	0.100	0.031			
Announcement effect	-0.761	0.657	0.649	-1.050	0.398	0.009
AE and CE	-0.058	0.301	0.068			
Adjustment control	-0.010	0.151	0.032			
Pooled	-0.002	0.369	0.032			
One site	-0.029	0.414	0.032			
<i>Publication</i>						
SE	0.001	0.004	0.076			
Sig	-2.459	0.371	1.000	-2.560	0.356	0.000
Reviewed	-0.133	0.437	0.119			
Year publish	-0.001	0.001	0.206			
Precision set	0.008	0.168	0.035			
Constant	-0.063		1.000	-4.559	2.683	0.090
R^2				0.669		
N	717			717		

The response variable is the estimated price-distance coefficient relating wind turbines and property values. On the left, the BMA results are presented relying on the uniform model prior and the unit information prior (Eicher et al. 2011). The results for a frequentist WLS regression with the most important variables ($PIP > 0.5$) identified by BMA are shown on the right

SD standard deviation, *PIP* posterior inclusion probability, *SE* standard error

took place, corresponding estimates document more negative effects on property values on average. One possible explanation might be that with an increased share, wind turbines are built closer to residential areas since more remote areas have already been used. Finally, neither the sampling duration, sample size nor the USA dummy influence the reported effect size systematically.

4.3.2 Control Variables

Using adequate control variables proves essential to disentangle the wind turbine effect from other price-influencing factors. In particular, studies lacking sufficient data to control for unobserved local price differentials using, e.g., spatial fixed effects or a repeat sales approach (*Spatial*), report more negative effect sizes (about eleven percentage points). Additionally, studies unable to control for pre-existing price-differentials via a difference-in-difference design (*DID*) generally report more negative effect sizes, as expected (about -2.43 percentage points).¹⁷ Similarly studies accounting for socio-economic factors like income levels or population density (*Demoecon*) document less adverse wind turbine effects. Additionally, if other amenities like a park or beach are present, the estimated wind turbine effect is more negative on average (*Oth amen*). Apparently, having a living-quality enhancing element in close vicinity worsens the effect of wind turbines.¹⁸ Other moderators reflecting the inclusion of control variables for house-structure characteristics, neighbourhood aspects, infrastructure access options, or the presence of other disamenities do not systematically influence the wind turbine effect. Similarly, using estimation approaches different from OLS or the choice of approaches to control for time trends are not affecting the reported wind turbine impact systematically.

4.3.3 Specification of Wind Turbine Impact

Studies differ to a great extent in the way the wind turbine impact is specified. However, the reported effect size is largely unaffected by design choices in this dimension. The only exception is the dummy *Announcement effect*, which shows that choosing the announcement date of a wind turbine as treatment results in more negative estimates compared to using the construction date.¹⁹ Using one treatment zone (*Dist binary*), i.e., a binary distance specification, does not lead to significantly different findings compared to categorical specifications with multiple treatment zones. In the same vein, controlling for announcement and construction effect simultaneously (*AE and CE*) or for a potential habituation effect (*Adjustment control*) does not change the effect size systematically. Additionally, the standard approach to pool observations from different sites to increase sample size (*Pooled*) does not alter the reported estimates. Surprisingly, studies using *other wind* turbine controls next to the distance variable (e.g., view, number of turbines) do not report estimates smaller in absolute terms, i.e., more positive price-distance effect sizes. This would be expected, assuming that other wind turbine controls take on some of the effect. Apparently, other moderators prove more important in explaining the effect size variance.

¹⁷ This finding is in line with an average location effect, i.e., a pre-existing price-differential, of -3.01% , which could be calculated for a subset of studies included in this meta-sample that reported this value.

¹⁸ Note, however, that this moderator loses importance if other prior settings are considered, see Sect. 4.4.

¹⁹ It is worth noting, however, that this dummy loses importance using other model priors, see Sect. 4.4.

4.3.4 Publication

The combined findings for three variables show the existence and type of publication bias: First, I find that significant estimates of wind turbine effects (*Sig*) are more negative on average (−2.46 percentage points). Second, the standard error (*SE*) is not a systematic factor in explaining the heterogeneity, i.e., selection does not take place based on effect size magnitude and its significance. Finally, studies that are published in peer-reviewed journals do not differ systematically in terms of effect size magnitude compared to unpublished manuscripts. Linking these findings, I conclude that publication bias is still confirmed with heterogeneity-explaining factors in place. The type of publication bias is a one-sided selection that disfavours significantly positive estimates of the impact of wind turbines on property values.²⁰ The publication year is not affecting the effect size. Reassuringly, the coding decision to set the precision level at specific values in cases when this metric is reported imprecisely (see again Fig. 7 for details) does not affect the estimated mean effect size.

4.4 Robustness Checks

The robustness of the results is assessed from several perspectives. First, I change the BMA prior settings: This includes (i) substituting the uniform model prior with the dilution prior, which allows for collinear moderators in each particular model (George 2010), (ii) combining the benchmark g-prior with the beta-binominal model prior (Fernandez et al. 2001; Ley and Steel 2009), which implies equal prior probability for each model size. I compare the results in terms of variable importance in Fig. 5. I conclude that the results are largely insensitive to the selection of priors with the exceptions mentioned above for the dummies *Announcement effect* and *Oth amen*, which lose importance using the alternative priors. The corresponding numerical results are summarized in Table 8. They confirm the main findings.

Next, I run a series of frequentist robustness checks based on the set of moderators identified by the baseline BMA summarized in Table 3 with a PIP > 0.5. I document the findings in the appendix. First, several moderators with missing observations that were not considered in the main specification are added in separate regressions, respectively. These moderators reflect wind turbine characteristics (average turbine height, installed capacity, number of turbines, number of wind farms) as well as additional aspects related to the property sample used in the respective primary study (number of treated properties, number of properties at the construction stage of the wind turbine(s) to which the coefficient refers, number of properties within one mile of a turbine, number of properties within one mile of a turbine after turbine construction). Both types of variables are suspected to influence the effect of wind turbines on property values, e.g., multiple turbines are presumably related to more negative effects due to the increased likelihood of visual impacts or noises (Jensen et al. 2014; Jensen et al. 2018). At the aggregate level, however, these factors do not translate to economically significant changes in the reported effect size, see Table 9.²¹ Only one of the additionally included moderators is statistically significant, i.e., a larger number of treated properties analysed by the primary studies is associated with

²⁰ See also again the histogram of t-values shown in Fig. 8 that supports this interpretation.

²¹ The same pattern of insignificant changes is observed for the additional moderators with ambiguous coding (market extent of the primary data, journal quality indicator), see Table 12.

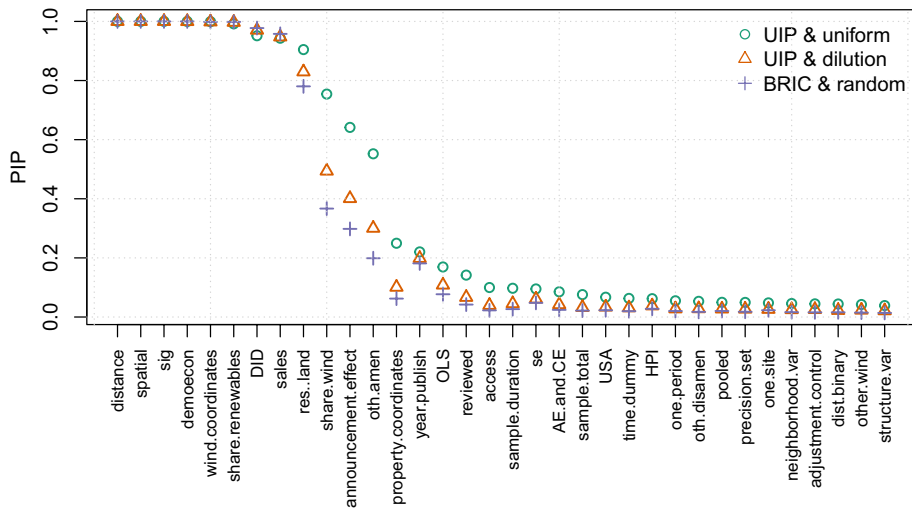


Fig. 5 Posterior inclusion probabilities with changed priors. UIP and uniform = Baseline setting used in Table 3 with priors following Eicher et al. (2011). UIP and Dilution = Uniform model prior exchanged for dilution prior (George 2010). BRIC and Random = Benchmark g-prior for coefficients (Fernandez et al. 2001) combined with the beta-binominal model prior (Ley and Steel 2009)

more negative findings. Most of the other moderators are robust, with only minor changes that can be attributed to the reduced number of observations considered for these subsample regressions (717 in the baseline specification vs. 393 if the number of treated properties is included as moderator).

In a second set of specifications, I investigate the effect of different definitions of outliers see Table 10. These cover the range from *no outlier* to the omission of 89 observations using the inter-quartile range criterion following Tukey (1977). The results are robust to changes in this dimension. Finally, in Table 11, I document the sensitivity of results to modifications of the meta-analytic model. This includes (i) changes to the estimation (using heteroscedasticity-robust standard errors and standard errors clustered at the study-level), (ii) adding study-level fixed effects and, (iii) changing the weighting scheme to inverse-variance weights (WLS-FE), or using no weights (OLS) (reported for completeness). While the main findings are confirmed, some points are worth noting. Altering the assumptions for calculating the error term does not change the results. Using OLS reduces the explanatory power as expected (R^2 drops from 0.669 to 0.218) and several variables lose significance. This again demonstrates the need to rely on WLS estimation in meta-regression analysis. Instead, the choice of weights (fixed or random effects) is less critical, with only minor changes documented in the WLS-FE framework due to the unbalanced weighting scheme.

Finally, when study-level fixed effects are added, the dummies *Sales* and *Oth amen* lose significance. This is probably due to the fact that only a few studies do not use sales data and *Oth amen* already found to be less robust in other BMA settings. The change in sign and significance of *Share renewables* is due to the high share of renewables in Sweden of about 60% in the study period of Westlund and Wilhelmsson (2021, 2022). This is significantly higher than the average of 19.45% in this meta-dataset. Omitting these observations results in coefficients similar to those in the baseline specification, but with a loss

of significance for *Share renewables*. I therefore caution against generalising the findings related to this variable.

4.5 Implied Effect Size

As the final step of the analysis, I compute the wind turbine impact on residential property values conditional on the absence of publication and misspecification bias. To this end, I calculate an average effect size that can be expected for a hypothetical study that follows “best practice” regarding methodology and data quality. Specifically, I use the results from the baseline BMA analysis and compute fitted values of the effect size when specific values for the variables with $PIP > 0.5$ are used. While arguably certain aspects of study design are preferable to others, any best-practice specification remains subjective by design. In order to increase plausibility, I follow a conservative calculation approach: When there is good reason to prefer a particular type of study design, I use the preferred value (e.g., I use 1 for the dummy variable corresponding to DID design); otherwise, I use the respective study mean to reflect my indifference.

To permit an accurate distance calculation, I prefer exact wind turbine coordinates. Similarly, I consider estimates from actual sales prices to reflect wind turbine impacts more realistically. For the *distance* of treated properties as well as *Share wind* and *Share renewables*, I consider the respective study means (1.89 miles, 19.45% and 2.29%). Similarly, I multiply the *Res. land* coefficient by 0.5 to reflect my indifference. In terms of control variables, I prefer rich data sets allowing to control for socioeconomic factors (*Demoecon*) and unobserved spatial factors (*Spatial*) and the application of a DID design (*DID*), which - all - may lead to misspecification bias if not accounted for. Instead, the presence of other amenities is not a study quality dimension, so I remain agnostic regarding this moderator. Finally, to correct for publication bias, I set the significance dummy to 0.

The calculated conditional effect size for this best practice specification is -0.68% . This is considerably smaller than the unconditional and only publication bias corrected mean effect size of -1.67% identified with `ROBMA-PSMA` in Table 1. This underlines the importance of correcting for misspecification bias to obtain a realistic effect size estimate. This conditional effect size is by definition sensitive to changes in the best practice specification. For example, setting the distance to a hypothetical value of 0 changes the effect size to -2.04% . For a distance of one mile, a reduction in property values of 1.31% can be expected. The effect becomes zero at a distance of 2.8 miles (i.e., 4.5 kms). This corresponds to a cut-off point often used in primary studies, beyond which no effect of wind turbines is suspected, and is identical to the result of the so-called “mini-meta-analysis” by Parsons and Heintzelman (2022). The effect of closer distances on the conditional effect size is also reflected in the subsample regressions shown in Table 12 that only include observations with a maximum distance of two and one mile, respectively.²² While the main results remain largely unaffected, the conditional effect size is more negative (but insignificant) at a distance of one mile.²³

²² I would like to thank an anonymous reviewer for suggesting that the analysis be restricted to observations at small distances.

²³ Two words of caution about these subsample results are in order. For very close distances, e.g. less than a mile, the number of available property transactions in the primary studies is often limited. For example, Carter (2011), Heintzelman and Tuttle (2012) and Skenteris et al. (2019) report fewer than 50 observations within this distance ring. While this is to be expected, it limits the reliability of the resulting price-distance estimates. This limitation is then transferred to the meta-regression results. In addition, the sub-

5 Discussion and Conclusion

This meta-study is the first to systematically assess the hedonic literature on the price-distance relationship between wind turbines and property values. It addresses the considerable ambiguity in empirical findings investigating its existence and magnitude. Combining 720 observations from 25 studies using BMA and novel publication bias correction methods, I identify the most essential moderators explaining the observed heterogeneity and calculate an average effect size for this relationship.

Three main conclusions emerge from this study. First, selective under-reporting of significant positive findings is responsible for overestimating the effect size by about 22% in absolute terms (correcting the unconditional mean effect size from -2.14% to -1.67%).²⁴ This is in line with ubiquitous publication bias in large parts of the economic literature (Bartoš et al. 2022). Second, next to selective reporting, various data characteristics (e.g., accuracy of the distance calculation, the distance of treated properties, type of property price data) as well as the ability to control for confounding factors, i.e., using a DID framework for accounting for pre-existing price differentials and inclusion of appropriate controls for socio-economic and unobservable spatial factors explain the considerable variation in empirical findings. Third, conditional on the absence of misspecification and publication bias, the effect size is -0.68% for properties 1.89 miles away when calculating a (subjective) best-practice average. For distances greater than 2.8 miles, there is no evidence of a wind turbine effect. These results are robust to changes in the BMA setup, meta-model specification, and outlier treatment.

These findings can inform future research and policymakers in at least three ways. First, future hedonic pricing studies on this subject should rely on a proper identification strategy using a DID design and a rich dataset with sufficient control variables. This ensures that pre-existing price differentials and other confounding factors are not wrongly attributed to the presence of wind turbines. Since the effect is small, studies should also rely on many observations, especially in close vicinity to wind turbines, to have enough statistical power to detect an effect if it exists in the respective setting. In addition, recent methodological advances that reflect the staggered nature of the treatment, i.e., the fact that observations from different wind farms with different corresponding construction dates are pooled, should be adopted (de Chaisemartin and D'Haultfoeuille 2022, 2020; Steigerwald et al. 2021).

Second, future meta-analyses on this topic could construct alternative effect size variables from other impact measures used in this literature (e.g., view, continuous distance, or number of turbines within a certain distance) that could not be used in this study to ensure comparability. This would help to better understand what drives the occasionally documented negative effects of wind turbines on property values. In addition, future meta-analyses focusing on other energy generation facilities (e.g. nuclear power plants or solar farms) could help to place the results of this study in the general context of the effects of

Footnote 23 (continued)

sample regression is based on a smaller set of observations, as fewer estimates are available at these closer distances.

²⁴ Conversion of the effect size into monetary values would be informative, but is not feasible. This requires information on the average prices of the properties analysed in the primary studies and the base year to which these prices were converted for comparability. This type of information was only available for a small subset of observations, so calculating the effect size in monetary terms is not meaningful.

disamenities on property values. Compared to the few existing meta-analyses that consider other types of disamenity and focus on price-distance relationships, the effects of wind turbines are in the lower range of estimates. Schütt (2021), for example, documents that property values increase on average between 1.5% and 2.9% per mile of increased distance from waste sites. Lipscomb et al. (2013) report an increase in value of 6.1% per mile of increased distance for properties close to contaminated water bodies.

Finally, for policy makers, the aggregated evidence from the literature could indicate the appropriateness of financial compensation for homeowners with properties very close to wind turbines. Although the effect size is small on average, payments could be appropriate to acknowledge the “local cost, global benefit” (Fronzel et al. 2019) situation of localised externalities from wind turbines. In addition, the calculated effect sizes for different distances can now be used by policy makers to make informed decisions about distance rules for wind turbines and residential areas that are appropriate to the individual context. As the announcement of future wind turbines appears to have a more negative effect than the actual construction, the results could also be used for early communication with local residents during the wind turbine siting process to reduce concerns. This early communication is also consistent with the results of a related meta-analysis on the non-market valuation of externalities of wind energy, which documents that visual deterioration in particular leads to welfare losses (Mattmann et al. 2016). Given the expected continued growth in global electricity generation from wind farms (IEA 2022), it is clearly in the general interest to increase public acceptance of wind turbines.

Appendix

Study Selection

For inclusion, studies had to fulfil the following requirements: First, I only considered studies that use the hedonic pricing method to estimate the relationship between property values and wind turbines. This excludes studies that investigate simple sales-date and sales-price relationships (e.g., Sterzinger et al. (2003); Baker (2021); Dupont and Etherington (2009)) as well as surveys (e.g., Khatri (2004); Hoen (2019)). Second, I restricted the meta-sample to studies using the price of residential properties as the dependent variable. This thus excluded studies investigating the impact on farmland values (e.g., Sampson et al. (2020); Pates et al. (2020)), which presumably benefit from the presence of wind turbines due to land lease payments. Third, the wind turbine impact must be measured in terms of distance from the respective properties. Distance is a reasonable proxy for wind turbine impact (Parsons and Heintzelman 2022) and is the most frequently used metric. Accordingly, this excludes studies using other impact measures, e.g., view, number of turbines, installed capacity, or turbine density (Dong and Lang 2022; Gibbons 2015; Jensen et al. 2018; Sunak and Madlener 2016)²⁵. Fourth, the distance of properties from a wind turbine has to be defined in discrete terms, i.e., using one or more distance zones for segmentation.

²⁵ Different impact metrics cannot be standardized unless one uses a non-economic, dimensionless effect size such as the partial correlation coefficient (for applications see Doucouliagos and Ulubaşoğlu (2008); de Linde et al. (2014)). Here, I choose economic interpretability over sample size and include only those studies that report a price-distance relationship. This excludes twelve studies.

I thus omit studies using continuous distance-variable definitions.²⁶ Fifth, studies are required to report the respective regression coefficient and its measure of precision. This excludes cases for which this essential information was unavailable despite contact (efforts) with the respective author(s) (e.g., Heblich et al. (2016); Sims and Dent (2007)). Using these study selection criteria ensures a consistent set of effect size estimates that can reasonably be meta-analysed.²⁷ More formally, the study selection criteria ensure that studies included in the meta-sample report results from a variant of the following stylized hedonic pricing specification (omitting indices for simplicity):

$$P = \alpha + \beta * DIST + \gamma * X + u \quad (2)$$

with P reflecting the price of residential properties, $DIST$ the distance to a wind turbine, X a set of control variables and u a common error term. The coefficient of interest is β , which serves as the dependent variable in the subsequent analysis. To arrive at the selection of studies, I use a predefined search query, which I document in Table 4 below, followed by the PRISMA statement (Fig. 6), the list of included studies (Table 5) with their details (Table 6) and the list of excluded studies, ordered by reason for exclusion (Table 7).

Standardization

The precision measures for the included effect size estimates differ in metric (e.g., standard errors, t-values, or p-values) and in accuracy, i.e., exact numbers versus reporting of significance levels. The corresponding standardization process to standard errors is summarized in Fig. 7 below following Schütt (2021). If standard errors are reported, they are considered at face value as a precision measure without further standardization being needed. T-values are converted to standard errors using the relation $SE = \frac{\beta}{t}$, with SE representing the standard error of the reported regression coefficient β . To convert p-values, I use the *t.inv2s* function from Microsoft Excel in conjunction with the reported sample size. If only significance levels are reported, I use the lowest precision as a conservative threshold (e.g., setting $p = 0.05$ or a five percent significance level). If only its insignificance is known for a coefficient, I set $p = 0.3$. In rare cases, the p-value is reported to be 0.000, in which case I set the value to 0.0001, allowing further calculations. This procedure enables the application of common meta-analytic methods that require the standard error (e.g., correcting for publication bias and using inverse-variance weights), as outlined in Sect. 4.2.

²⁶ Studies with continuous distance measures cannot be combined with studies using a discrete distance measure without the loss of information or the introduction of measurement error in the meta-analysis (see the discussions in Guignet et al. (2022), Schütt (2021), Ready (2010) and Debrezion et al. (2007)). Here, most studies use a discrete distance measure leading to its selection as the required metric. Accordingly, nine studies with only continuous distance definitions are dropped.

²⁷ See also the discussion on consistency in meta-analyses in Vedogbeton and Johnston (2020) and Schütt (2021).

Table 4 Search query

Search category	Synonyms / wildcards
Residential property	("Real estate*" OR residential* OR propert* OR hous* OR apartment OR building OR condo* OR dwellin* OR home OR residence OR mansion* OR domicile OR flat)
Hedonic price model	(Hedonic* OR "repeat sale*" OR "Diff*in*Diff*")
Price	(Price* OR price* OR value* OR benefi* OR cost OR premium OR worth OR compensat* OR damag* OR pay* OR sum)
Wind turbine	(Wind* OR turbine)

Search term categories are interconnected via the Boolean operator "AND"

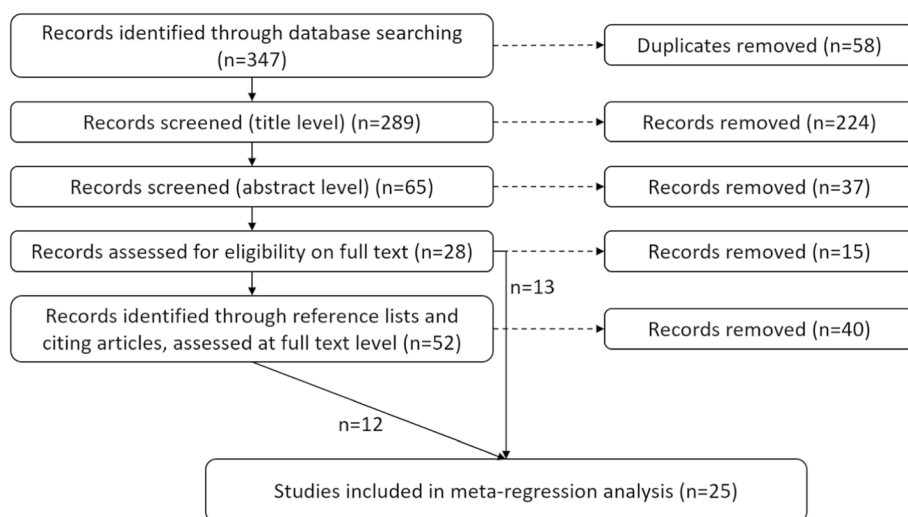


Fig. 6 PRISMA diagram. The PRISMA diagram (Moher et al. 2009) shows the study selection process from initial screening to full-text-level assessment

Table 5 Included studies (N=25)

- C. Atkinson-Palombo and B. Hoen (2014). Relationship between wind turbines and residential property values in Massachusetts. University of Connecticut
- N. Camplair (2012). "Does Proximity to Wind Farms Affect the Value of Nearby Residential Properties? Evidence from Washington and New York States". Macalester College. url: <https://digitalcommons.macalester.edu/econaward/13>
- E. W. Carr (2017). "Three Essays on Environmental Valuation. The Social Value of Carbon Storage in Wetlands, the Impact of Wind Turbines on Home Prices, and the Effect of Flood Risk on Home Prices". Dissertation. University of Delaware. url: <http://udspace.udel.edu/handle/19716/23533>
- J. Carter (2011). "The effect of wind farms on residential property values in Lee County, Illinois". Illinois State University
- M. I. Dröes and H. R. Koster (2016). "Renewable energy and negative externalities. The effect of wind turbines on house prices". In: Journal of Urban Economics 96, pp. 121–141. doi: <https://doi.org/10.1016/j.jue.2016.09.001>
- M. I. Dröes and H. R. Koster (2021). "Wind turbines, solar farms, and house prices". In: Energy Policy 155, p. 112327. doi: <https://doi.org/10.1016/j.enpol.2021.112327>
- P. Eichholtz et al. (2021). "Clean electricity, dirty electricity: the effect on local house prices". In: The Journal of Real Estate Finance and Economics, pp. 1–35. doi: <https://doi.org/10.1007/s11146-021-09878-6>
- M. Frondel et al. (2019). "Local cost for global benefit: The case of wind turbines". In: Ruhr Economic Papers 791. doi: <https://doi.org/10.4419/86788919>
- S. S. Gorelick (2014). "The effect of lake water quality and wind turbines on Rhode Island property sales price". Dissertation. University of Rhode Island. url: <https://doi.org/10.23860/diss-gorelick-susan-2014>
- N. Guzman (2020). "Wind Turbines and Housing Prices. Valuing the Impact of Wind Farms on Transactions". Undergraduate Thesis. Brigham Young University. url: https://scholarsarchive.byu.edu/studentpub_uht/139
- M. D. Heintzelman and C. M. Tuttle (2012). "Values in the Wind: A Hedonic Analysis of Wind Power Facilities". In: Land Economics 88.3, pp. 571–588. doi: <https://doi.org/10.3368/le.88.3.571>
- J. L. Hinman (2010). "Wind farm proximity and property values. A pooled hedonic regression analysis of property values in central Illinois". Graduate thesis. Illinois State University
- B. Hoen and C. Atkinson-Palombo (2016). "Wind Turbines, Amenities and Disamenities. A study of Home Value Impacts in Densely Populated Massachusetts". In: Journal of Real Estate Research, pp. 473–504. doi: <https://doi.org/10.1080/10835547.2016.12091454>
- B. Hoen et al. (2015). "Spatial Hedonic Analysis of the Effects of US Wind Energy Facilities on Surrounding Property Values". In: The Journal of Real Estate Finance and Economics 51.1, pp. 22–51. doi: <https://doi.org/10.1007/s11146-014-9477-9>
- B. Hoen et al. (2009). The Impact of Wind Power Projects on Residential Property Values in the United States. A Multi-Site Hedonic Analysis. doi: <https://doi.org/10.2172/978870>
- B. Hoen et al. (2011). "Wind Energy Facilities and Residential Properties. The Effect of Proximity and View on Sales Prices". In: Journal of Real Estate Research 33.3, pp. 279–316. doi: <https://doi.org/10.1080/10835547.2011.12091307>
- S. Jarvis (2021). The Economic Costs of NIMBYism-Evidence From Renewable Energy Projects. url: <https://eprints.lse.ac.uk/113653/>
- M. Joly and S. de Jaeger (2021). "Not in my backyard. A hedonic approach to the construction timeline of wind turbines in Flanders, Belgium". In: Land Use Policy 108. doi: <https://doi.org/10.1016/j.landusepol.2021.105527>²⁷
- C. Lang et al. (2014). "The Windy City. Property Value Impacts of Wind Turbines in an Urban Setting". In: Energy Economics 44, pp. 413–421. doi: <https://doi.org/10.1016/j.eneco.2014.05.010>
- K. Skenteris et al. (2019). "Implementing hedonic pricing models for valuing the visual impact of wind farms in Greece". In: Economic Analysis and Policy 64, pp. 248–258. doi: <https://doi.org/10.1016/j.eap.2019.09.004>
- Y. Sunak and R. Madlener (2017). "The impact of wind farms on property values. A locally weighted hedonic pricing model". In: Papers in Regional Science 96.2, pp. 423–444. doi: <https://doi.org/10.1111/pirs.12197>

Table 5 (continued)

R. J. Vyn (2018). "Property Value Impacts of Wind Turbines and the Influence of Attitudes toward Wind Energy". In: *Land Economics* 94.4, pp. 496–516. doi: <https://doi.org/10.3368/le.94.4.496>

R. J. Vyn and R. M. McCullough (2014). "The Effects of Wind Turbines on Property Values in Ontario: Does Public Perception Match Empirical Evidence?" In: *Canadian Journal of Agricultural Economics* 62.3, pp. 365–392. doi: <https://doi.org/10.1111/cjag.12030>

H. Westlund and M. Wilhelmsson (2021). "The Socio-Economic Cost of Wind Turbines: A Swedish Case Study". In: *Sustainability* 13.12, p. 6892. doi: <https://doi.org/10.3390/su13126892>

H. Westlund and M. Wilhelmsson (2022). Valuating the negative externality of wind turbines: traditional hedonic and difference-in-difference approaches. KTH Royal Institute of Technology

²⁷Nine observations were dropped from this study since the sign of the reported effect size and its corresponding t-statistic did not fit. Moreover, 15 estimates of effects from pre-existing turbines were not considered to ensure comparability across studies

Table 6 Attributes of included studies

Author(s)	Year	#	Country	Peer reviewed	Journal
Atkinson-Palombo and Hoen	2014	2	USA	No	Report / working paper
Camplair	2012	12	USA	No	Bachelor's thesis
Carr	2017	18	USA	No	Dissertation
Carter	2011	12	USA	No	Bachelor's thesis
Dröes and Koster	2016	47	The Netherlands	Yes	Journal of Urban Economics
Dröes and Koster	2021	11	The Netherlands	Yes	Energy Economics
Eichholtz et al.	2021	20	The Netherlands	Yes	Journal of Real Estate Finance and Economics
Frondel et al.	2019	63	Germany	No	Working paper: Ruhr Economic Papers
Gorelick	2014	20	USA	No	Dissertation
Guzman	2020	2	USA	No	Bachelor's thesis
Heintzelman and Tuttle	2012	38	USA	Yes	Land Economics
Hinman	2010	18	USA	No	Master's thesis
Hoen and Atkinson-Palombo	2016	26	USA	Yes	Journal of Real Estate Research
Hoen et al.	2009	13	USA	No	Working paper
Hoen et al.	2011	36	USA	Yes	Journal of Real Estate Research
Hoen et al.	2015	32	USA	Yes	Journal of Real Estate Finance and Economics
Jarvis	2021	40	England & Wales	No	Dissertation
Joly and De Jaeger	2021	59	Belgium	Yes	Land Use Policy
Lang et al.	2014	76	USA	Yes	Energy Economics
Skenteris et al.	2019	4	Greece	Yes	Economic Analysis and Policy
Sunak and Madlener	2017	15	Germany	Yes	Papers in Regional Science
Vyn	2018	73	Canada	Yes	Land Economics
Vyn and McCullough	2014	3	Canada	Yes	Canadian Journal of Agricultural Economics
Westlund and Wilhelmsson	2021	23	Sweden	Yes	Sustainability
Westlund and Wilhelmsson	2022	54	Sweden	No	Working paper series; Royal Institute of Technology

refers to the number of observations that enter the meta-dataset

Table 7 Excluded studies sorted by reason for exclusion (N=58)*Not hedonic pricing method (N=15)*

- M. Alem et al. (2020). "Qualitative meta-analysis of the socioeconomic impacts of offshore wind farms". In: *Sustinere: Journal of Environment and Sustainability* 4.3, pp. 155–171. doi: <https://doi.org/10.22515/sustinere.jes.v4i3.121>
- C. E. Baker (2021). "The Impact of Wind Turbines on Agricultural Land Values in West Texas". Master's Thesis. Texas Tech University. url: <https://hdl.handle.net/2346/87469>
- J. Dorrell and K. Lee (2020). "The Cost of Wind. Negative Economic Effects of Global Wind Energy Development". In: *Energies* 13.14, p. 3667. doi: <https://doi.org/10.3390/en13143667>
- R. R. Dupont and J. Etherington (2009). Preliminary assessment of the impact of wind farms on surrounding land values in Australia. Preston Rowe Paterson Newcastle and Central Coast. url: https://www.valuergeneral.nsw.gov.au/_data/assets/pdf_file/0006/195315/Preliminary_assessment_impact_of_wind_farms_on_surrounding_land_values_in_Australia.pdf
- P. Haan and M. Simmler (2018). "Wind electricity subsidies. A windfall for landowners? Evidence from a feed-in tariff in Germany". In: *Journal of Public Economics* 159, pp. 16–32. doi: <https://doi.org/10.1016/j.jpubeco.2018.01.011>
- B. Hoen et al. (2019). "Attitudes of U.S. Wind Turbine Neighbors: Analysis of a Nationwide Survey". In: *Energy Policy* 134, p. 110981. doi: <https://doi.org/10.1016/j.enpol.2019.110981>
- J. Jordal-Jorgensen (1996). *Social Assessment of Wind Power: Visual Effect and Noise from Windmills-Quantifying and Valuation*. Copenhagen, Denmark: Institute of Local Government Studies
- M. Khatri (2004). *Impact of wind farms on the value of residential property and agricultural land*. London, UK: Royal Institute of Chartered Surveyors
- P. Lehmann et al. (2021). Optimal siting of onshore wind turbines: Local disamenities matter. UFZ Discussion Paper. Leipzig. url: <http://hdl.handle.net/10419/240195>
- J. Persson and F. Fernqvist (2016). *Socioekonomiska konsekvenser av vindkraftsetablering och tillämpningen av vindbonus. En kunskapssammanställning*. Vol. 2016:4. Landskapsarkitektur trädgård växtproduktionsvetenskap. Alnarp: Fakulteten för landskapsarkitektur, trädgårds- och växtproduktionsvetenskap, Sveriges lantbruksuniversitet. isbn: 978-91-576-8924-5
- J. Rand and B. Hoen (2017). "Thirty years of North American wind energy acceptance research: What have we learned?". In: *Energy Research and Social Science* 29, pp. 135–148. doi: <https://doi.org/10.1016/j.erss.2017.05.019>
- B. K. Sovacool et al. (2021). "The hidden costs of energy and mobility: A global meta-analysis and research synthesis of electricity and transport externalities". In: *Energy Research and Social Science* 72, p. 101885. doi: <https://doi.org/10.1016/j.erss.2020.101885>
- G. Sterzinger et al. (2003). *The Effect of Wind Development on Local Property Values* (Washington, DC: Renewable Energy Policy Project). url: http://www.repp.org/articles/static/1/binaries/wind_online_final.pdf
- M. Torzewski (2016). "The Impact of Wind Farms on the Prices of Nearby Houses in Poland: A Review and Synthesis". In: *Real Estate Management and Valuation* 24.2, pp. 13–24. doi: <https://doi.org/10.1515/remav-2016-0010>
- C. Wen et al. (2018). "Valuing the visual impact of wind farms: A calculus method for synthesizing choice experiments studies". In: *The Science of the total environment* 637–638, pp. 58–68. doi: <https://doi.org/10.1016/j.scitotenv.2018.04.430>

No wind turbine impact measure (N=5)

- P. M. Anglin and R. Gencay (1996). "Semiparametric Estimation of a Hedonic Price Function". In: *Journal of Applied Econometrics* 11.6, pp. 633–648. doi: [https://doi.org/10.1002/\(SICI\)1099-1255\(199611\)11:6<633::AIDJAE414>3.0.CO;2-T](https://doi.org/10.1002/(SICI)1099-1255(199611)11:6<633::AIDJAE414>3.0.CO;2-T)
- W. Athukorala et al. (2019). "Valuing bushfire risk to homeowners. Hedonic property values study in Queensland, Australia". In: *Economic Analysis and Policy* 63, pp. 44–56. doi: <https://doi.org/10.1016/j.eap.2019.04.013>
- L. Chakraborti et al. (2016). *Are Land Values Related to Ambient Air Pollution Levels? Hedonic Evidence from Mexico City*. CIDE, División de Economía
- S. Krumholz (2020). "Three Essays in Applied Microeconomics"
- J. M. Ross et al. (2011). "Inconsistency in Welfare Inferences from Distance Variables in Hedonic Regressions". In: *The Journal of Real Estate Finance and Economics* 43.3. PII: 9221, pp. 385–400. doi: <https://doi.org/10.1007/s1146-009-9221-z>

Table 7 (continued)*Impact measure is not distance (N=13)*

- G. Canning and L. J. Simmons (2010). Wind Energy Study-Effect on Real Estate Values in the Municipality of Chatham-Kent, Ontario
- M. Daams and F. Sijsma (2019). Windparken en Woningprijzen in Groningen en Drenthe. Rapportage in Opdracht van RTV Noord en RTV Drenthe. University of Groningen
- L. Dong and C. Lang (2022). "Do views of offshore wind energy detract? A hedonic price analysis of the Block Island wind farm in Rhode Island". In: *Energy Policy* 167, p. 113060. doi: <https://doi.org/10.1016/j.enpol.2022.113060>
- W. Feilmayr et al. (2018). Windkraftnutzung und der Einfluss auf Grundstückspreise in Österreich. Preisentwicklung von Grundstückspreisen in Österreichs Windkraftgemeinden (1999–2017). Department für Raumplanung, TU Wien
- S. Gibbons (2015). "Gone with the wind. Valuing the visual impacts of wind turbines through house prices". In: *Journal of Environmental Economics and Management* 72.4, pp. 177–196. doi: <https://doi.org/10.1016/j.jeem.2015.04.006>²⁸
- J. Grib (2019). "Wind Turbines and Residential Property Values". Undergraduate Thesis. UC Santa Barbare. url: <https://escholarship.org/uc/item/0z84b44m>
- C. U. Jensen et al. (2018). "The impact of on-shore and off-shore wind turbine farms on property prices". In: *Energy Policy* 116, pp. 50–59. doi: <https://doi.org/10.1016/j.enpol.2018.01.046>
- S. Laposa and A. Mueller (2010). "Wind Farm Announcements and Rural Home Prices. Maxwell Ranch and Rural Northern Colorado". In: *Journal of Sustainable Real Estate* 2.1, pp. 383–402. doi: <https://doi.org/10.1080/10835547.2010.12091798>
- I. McCarthy and H. O. Balli (2014). "Windfarms and residential property values". In: *International Journal of Strategic Property Management* 18.2, pp. 116–124. doi: <https://doi.org/10.3846/1648715X.2014.889770>
- T. Möllney (2022). "Investigating the Causal Impact of Wind Turbines on Housing Prices in Germany". Master's Thesis. Kiel University
- O. Myrna et al. (2019). "The Influence of Wind Energy and Biogas on Farmland Prices". In: *Land* 8.1, p. 19. doi: <https://doi.org/10.3390/land8010019>
- M. Ritter et al. (2015). "Der Einfluss von Windkraftanlagen auf landwirtschaftliche Bodenpreise". In: *Berichte über Landwirtschaft-Zeitschrift für Agrarpolitik und Landwirtschaft* 93.3. doi: <https://doi.org/10.12767/buel.v93i3.83>
- Y. Sunak and R. Madlener (2016). "The impact of wind farm visibility on property values. A spatial difference-in-differences analysis". In: *Energy Economics* 55, pp. 79–91. doi: <https://doi.org/10.1016/j.eneco.2015.12.025>

Distance is continuous (N=10)

- C. Krekel and A. Zerrahn (2017). "Does the presence of wind turbines have negative externalities for people in their surroundings? Evidence from well-being data". In: *Journal of Environmental Economics and Management* 82, pp. 221–238
- B. Castleberry and J. S. Greene (2018). "Wind power and real estate prices in Oklahoma". In: *International Journal of Housing Markets and Analysis* 11.5, pp. 808–827. doi: <https://doi.org/10.1108/IJHMA-02-2018-0010>
- A. Fønnesbech-Wulf et al. (2011). Welfare impacts of landscape dis-amenities. Comparative hedonic approaches
- M. D. Heintzelman et al. (2017). "Understanding the Amenity Impacts of Wind Development on an International Border". In: *Ecological Economics* 137.C, pp. 195–206. doi: <https://doi.org/10.1016/j.ecolecon.2017.03.008>
- B. Hoen (2006). "Impacts of windmill visibility on property values in Madison County, New York". Master's Thesis. Bard College. url: https://www.bcsd.org/site/handlers/filedownload.ashx?moduleinstanceid=1829&dataid=8961&FileName=prop_value1.pdf
- C. U. Jensen et al. (2014). "The Vindication of Don Quixote. The Impact of Noise and Visual Pollution from Wind Turbines". In: *Land Economics* 90.4, pp. 668–682. doi: <https://doi.org/10.3368/le.90.4.668>
- O. Joalland and T. Rambonilaza (2017a). Assessing the impact of renewable energy infrastructure on the "tourist value" in rural landscapes. A spatial hedonic approach. Groupe de Recherche en Economie Théorique et Appliquée (GRE-ThA). url: <http://cahiersdugretha.u-bordeaux.fr/2017/2017-10.pdf>
- O. Joalland and T. Rambonilaza (2017b). "Valeur touristique des aménités environnementales et nuisances associées aux infrastructures d'énergie renouvelable. Une approche hédonique spatiale. (Tourist Value of Environmental Amenities and Nuisances Due to Renewable Energy Infrastructure: A Spatial Hedonic Approach. With English summary.)" In: *Region et Développement* 46, pp. 93–115. url: https://regionetdeveloppement.univ-tln.fr/wp-content/uploads/7_Joalland.pdf
- C. Shultz et al. (2015). Production of wind energy and agricultural land values. Evidence from Pennsylvania. Department of Economics, West Virginia University. url: http://busecon.wvu.edu/phd_economics/pdf/15-11.pdf

Table 7 (continued)

- S. Sims et al. (2008). "Modelling the impact of wind farms on house prices in the UK". in: *International Journal of Strategic Property Management* 12.4, pp. 251–269. doi: <https://doi.org/10.3846/1648-715X.2008.12.251-269>
- Unpublished version of already assessed studies (N=7)*
- B. Castleberry (2017). "Community-Level Impacts of Wind Power in Oklahoma". Master's thesis. University of Oklahoma. url: <https://hdl.handle.net/11244/50905>
- P. M. A. Eichholtz et al. (2019). *Clean Electricity, Dirty Electricity. The Effect on Local House Prices*. doi: <https://doi.org/10.2139/ssrn.2826508>
- B. Hoen (2013). "Wind Farms in North America". In: *Towers, turbines and transmission lines. Impacts on property value*. Chichester: Wiley, pp. 253–287. isbn: 978-1-4443-3007-6
- B. Hoen et al. (2013). *A Spatial Hedonic Analysis of the Effects of Wind Energy Facilities on Surrounding Property Values in the United States*. Office of Scientific and Technical Information. doi: <https://doi.org/10.2172/1165267>
- S. Jarvis (2020). "Studies in Empirical Policy Evaluation. New Methods and Applications to the Energy Transition". Dissertation, UC Berkeley
- C. U. Jensen et al. (2013). *The Vindication of Don Quijote. The impact of noise and visual pollution from wind turbines on local residents in Denmark*. University of Copenhagen, Department of Food and Resource Economics
- Y. Sunak and R. Madlener (2013). "The Impact of Wind Farms on Property Values: A Geographically Weighted Hedonic Pricing Model". In: *SSRN Electronic Journal*. doi: <https://doi.org/10.2139/ssrn.2114216>
- Missing information (N=6)²⁹*
- C. Andersen and T. Hener (2022). *Wind Turbines and the Housing Market*
- S. Heblich et al. (2016). *Impact of wind turbines on house prices in Scotland*. University of Sheffield
- T. J. Rakitan (2017). *Essays in the economics of energy development and disamenities*. Iowa State University. doi: <https://doi.org/10.31274/etd-180810-5829>
- S. Sims and P. Dent (2007). "Property stigma. Wind farms are just the latest fashion". In: *Journal of Property Investment & Finance* 25.6, pp. 626.651. doi: <https://doi.org/10.1108/14635780710829315>
- V. Svensk (2010). "Vindkraft i Sikt: Hur Påverkas Fastighetspriserna vid Etablering av Vindkraft". In: *Svensk Vindenergi*: Stockholm, Sweden
- P. Yu and J. Levy (2016). "A spatial hedonic analysis of windfarms in Hawaii. The effect of windfarms on property values in the North Shore of Oahu". In: *A selection of papers and abstracts presented at the 5th Global Conference Forum of Economists International (Amsterdam)*. Ed. by M. P. van der Hoek. Forum for Economists International, pp. 1.28
- Farmland values as dependent variable (N=2)³⁰*
- N. J. Pates et al. (2020). *Windfalls or wind falls? The Local Effects of Turbine Development on US Agricultural Land Values*
- G. S. Sampson et al. (2020). "The On-Farm and Near-Farm Effects of Wind Turbines on Agricultural Land Values". In: *Journal of Agricultural and Resource Economics* 45.3, pp. 410–427. doi: <https://doi.org/10.22004/ag.econ.302463>

²⁸For this study, distance and view variables are exclusively used jointly in interactions with no distinct distance variable. This precludes a meaningful comparison with the included studies

²⁹These studies could not be included since essential information were not available despite contact (efforts) with the respective author(s). Andersen and Hener (2022) is a working paper for which citation is currently not allowed. Heblich et al. (2016) displays results in figures for which corresponding exact point estimates and precision measures could not be obtained. For Rakitan (2017), Sims and Dent (2007) and Svensk (2010) descriptive statistics miss to calculate the effect size used in this meta-study, while for Yu and Levy (2016) information on regression results could not be accessed.

³⁰Farmland values tend to increase if wind turbines are placed in the vicinity due to expected payments for land rents. This is the opposite direction of effect expected for residential properties, such that an exclusion seems warranted.

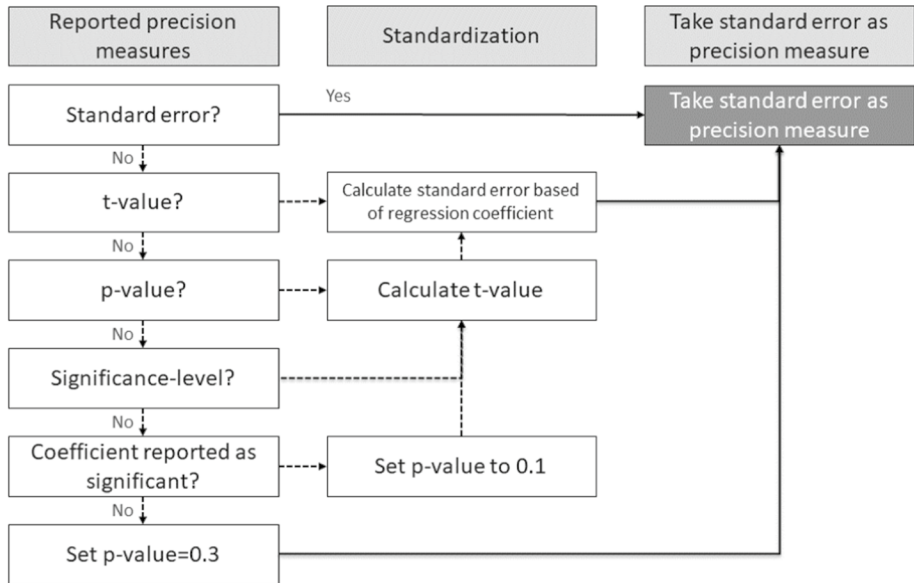


Fig. 7 Standardization of precision measure

Robustness: Figures

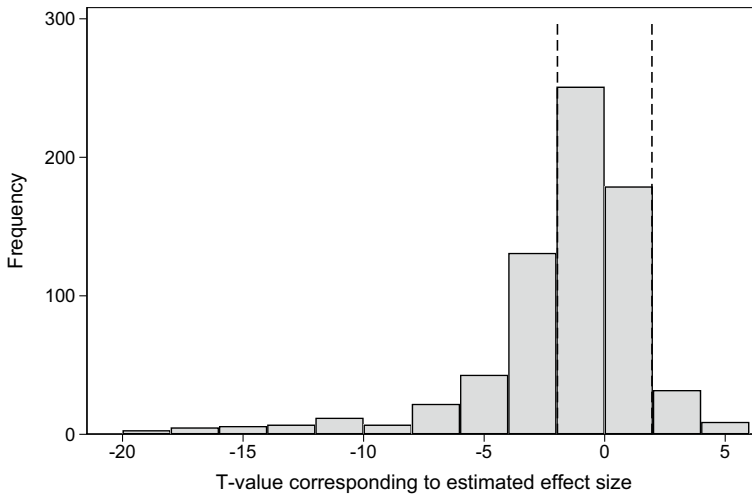


Fig. 8 T-value distribution. The histogram shows the distribution of t-values corresponding to the effect size estimates from the primary studies. The dashed lines indicate the threshold for statistical significance at the 5%-level

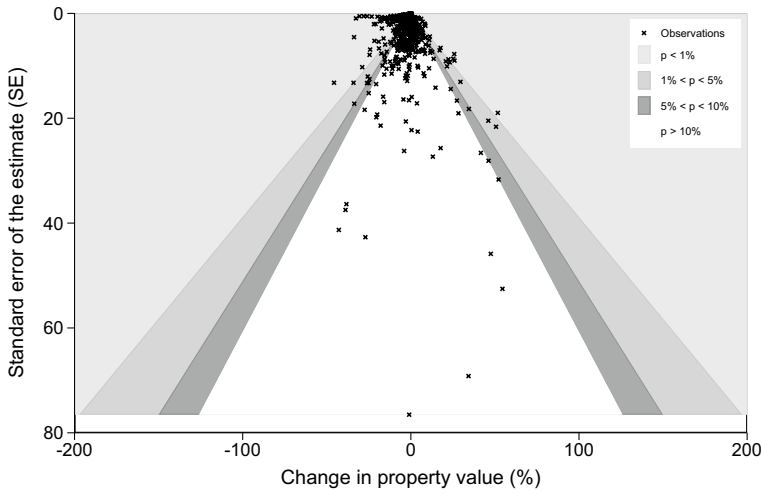


Fig. 9 Contour-enhanced funnel plot relating effect size estimates to their reported standard error (SE). Without publication bias the plot should take the shape of an inverted funnel. If an observation is within one of the shaded areas, the hypothesis of a null effect can be rejected at the corresponding significance level

Robustness: Bayesian Model Averaging

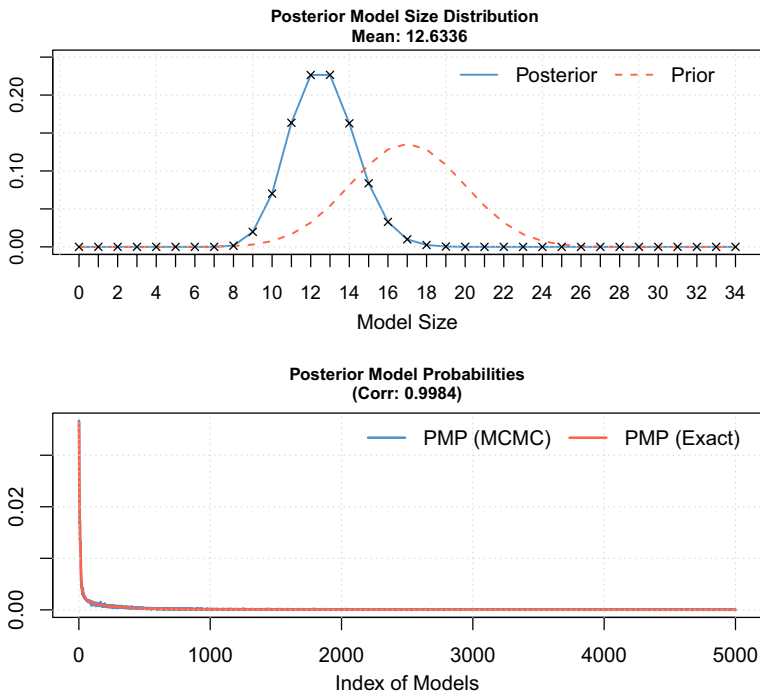


Fig. 10 Model size and convergence for the baseline BMA specification. The figure shows the posterior model size distribution and the posterior model probabilities of the baseline BMA specification reported in Table 3

Table 8 Bayesian model averaging - changing the priors

Moderators	UIP and dilution			BRIC and random		
	Post Mean	Post. SD	PIP	Post. Mean	Post SD	PIP
<i>Data characteristics</i>						
Wind coordinates	-15.301	2.899	0.998	-15.597	2.874	0.998
Property coordinates	0.154	0.576	0.095	0.094	0.451	0.063
Sales	3.356	1.186	0.945	3.429	1.129	0.955
Res. land	-8.518	4.734	0.831	-8.039	4.980	0.786
Distance	0.718	0.087	1.000	0.714	0.087	1.000
Sample duration	-0.002	0.012	0.038	-0.001	0.009	0.027
Sample total	0.003	0.041	0.037	0.002	0.034	0.027
USA	0.025	0.225	0.037	0.011	0.137	0.021
Share renewables	-0.063	0.013	0.997	-0.065	0.013	0.997
Share wind	-0.160	0.189	0.485	-0.119	0.174	0.377
<i>Control variables</i>						
Structure var	-0.000	0.144	0.024	-0.000	0.123	0.018
Neighbourhood var	0.001	0.090	0.022	-0.001	0.069	0.016
Access	0.019	0.154	0.037	0.012	0.123	0.026
Oth disamen	0.005	0.102	0.026	0.003	0.081	0.018
Oth amen	-0.367	0.637	0.290	-0.259	0.555	0.207
Demoecon	4.514	0.759	0.999	4.474	0.749	0.999
HPI	0.023	0.245	0.034	0.023	0.221	0.028
Time dummy	-0.004	0.137	0.031	-0.003	0.117	0.020
One period	-0.214	1.892	0.030	-0.138	1.530	0.020
DID	2.545	0.789	0.967	2.620	0.716	0.980
Spatial	11.421	1.079	1.000	11.435	1.078	1.000
OLS	-0.127	0.419	0.110	-0.094	0.367	0.080
<i>Wind turbine impact</i>						
Dist binary	0.003	0.077	0.023	0.003	0.060	0.016
Other wind	0.004	0.091	0.024	0.003	0.076	0.017
Announcement effect	-0.435	0.607	0.389	-0.324	0.550	0.295
AE and CE	-0.035	0.252	0.044	-0.012	0.148	0.023
Adjustment control	-0.008	0.127	0.023	-0.004	0.096	0.016
Pooled	-0.001	0.380	0.026	-0.002	0.325	0.019
One site	-0.030	0.422	0.027	-0.025	0.374	0.020
<i>Publication</i>						
SE	0.001	0.004	0.057	0.001	0.004	0.047
Sig	-2.462	0.368	1.000	-2.472	0.365	1.000
Reviewed	-0.062	0.302	0.063	-0.042	0.247	0.043
Year publish	-0.001	0.002	0.197	-0.001	0.001	0.179
Precision set	0.003	0.135	0.024	0.002	0.124	0.020
Constant	-0.090		1.000	-0.101		1.000
<i>N</i>	717			717		

The response variable is the estimated price-distance coefficient relating wind turbines and property values. *SD* standard deviation, *PIP* posterior inclusion probability, *UIP and dilution* Unit information prior and dilution model prior (Eicher et al. 2011; George 2010). BRIC and Random = Benchmark g-prior for coefficients (Fernandez et al. 2001) combined with the beta-binominal model prior (Ley and Steel 2009)

Robustness: Frequentist Meta-Regression Results

Table 9 Robustness regressions - moderators with missing observations

	Turbine characteristics			Sample sizes					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Baseline	# turbines	# farms	Height	Capacity	Stage	Treated	1 mile	1 mile PC
<i>Data characteristics</i>									
Wind coordinates	-11.61*** (2.60)	-7.21 (4.84)	-10.19** (4.80)	-8.29 (5.42)	-8.34 (5.32)	-12.12*** (2.96)	-11.23*** (2.86)	-12.88*** (2.58)	-15.82*** (3.04)
Sales	3.54*** (0.82)	2.44 (1.55)	11.10** (4.30)	12.58*** (4.53)	12.65*** (4.44)	4.02*** (1.19)	0.30 (1.47)	0.99 (0.98)	0.27 (1.08)
Res land	-10.47*** (2.86)	-9.54*** (2.95)	-11.79*** (2.96)	-12.30*** (3.17)	-12.28*** (3.09)	-9.56*** (2.97)	-11.01*** (2.92)	-10.46*** (2.80)	-10.21*** (2.72)
Distance	0.72*** (0.08)	1.02*** (0.12)	0.28*** (0.06)	0.15 (0.14)	0.17 (0.13)	0.92*** (0.12)	0.94*** (0.16)	0.65*** (0.09)	0.80*** (0.10)
Share renewables	-0.05*** (0.01)	-0.03 (0.03)	-0.04*** (0.01)	0.09* (0.05)	0.09** (0.04)	-0.06*** (0.01)	-0.09*** (0.02)	-0.06*** (0.01)	-0.06*** (0.01)
Share wind	-0.30*** (0.10)	-0.53*** (0.18)	-0.68*** (0.34)	-0.47*** (0.13)	-0.44*** (0.13)	-0.56*** (0.17)	-0.36*** (0.16)	-0.37*** (0.13)	-0.46*** (0.14)
<i>Control variables</i>									
Demoecon	5.07*** (0.70)	4.87*** (0.76)	3.56*** (0.60)	2.29*** (0.54)	2.27*** (0.53)	5.08*** (0.96)	4.49*** (0.90)	4.48*** (0.69)	4.43*** (0.70)
Oth amen	-1.05** (0.44)	-1.27** (0.53)	0.04 (0.45)	-0.11 (0.26)	-0.19 (0.26)	-3.05*** (0.85)	-1.56** (0.68)	-1.02* (0.59)	-2.08*** (0.77)
DID	2.33*** (0.54)	3.48*** (0.73)	0.82* (0.47)	0.62 (0.52)	0.65 (0.49)	3.40*** (0.79)	4.48*** (0.80)	3.17*** (0.90)	3.44*** (0.90)
Spatial	11.17*** (1.06)	11.61*** (1.18)	6.31 (5.36)	5.02*** (1.89)	5.06*** (1.85)	12.03*** (1.41)	12.94*** (1.30)	12.96*** (1.09)	13.24*** (1.13)

Table 9 (continued)

	Turbine characteristics				Sample sizes				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Baseline	# turbines	# farms	Height	Capacity	Stage	Treated	1 mile	1 mile PC
<i>Wind turbine impact</i>									
Announcement effect	-1.05*** (0.40)	-1.43*** (0.49)	-0.98*** (0.31)	-0.61* (0.32)	-0.63** (0.28)	-2.04*** (0.63)	-2.44*** (0.65)	-1.56*** (0.47)	-1.56*** (0.51)
<i>Publication</i>									
Sig	-2.56*** (0.36)	-2.04*** (0.46)	-1.87*** (0.25)	-1.19*** (0.25)	-1.18*** (0.24)	-2.61*** (0.59)	-1.16* (0.69)	-2.54*** (0.40)	-2.62*** (0.46)
<i>Missing observations</i>									
Turbines		0.00 (0.00)							
Farms			0.00 (0.00)						
Height				-0.00 (0.01)					
Capacity					-0.12 (0.20)				
Sample stage						0.14 (0.20)			
Sample treated							-0.46*** (0.14)		
Sample 1mile								-0.15 (0.13)	
PC 1mile									-0.26 (0.16)
Constant	-4.56* (2.68)	-9.46* (5.02)	-7.48 (5.33)	-10.17 (7.33)	-10.13 (7.20)	-4.93 (3.44)	-1.06 (3.24)	-1.60 (2.78)	2.89 (3.50)

Table 9 (continued)

	Turbine characteristics				Sample sizes				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Baseline	# turbines	# farms	Height	Capacity	Stage	Treated	1 mile	1 mile PC
R^2	0.669	0.672	0.585	0.811	0.805	0.663	0.693	0.708	0.705
N	717	596	461	421	449	457	393	570	462

The table displays the sensitivity of results to the inclusion of moderators with missing observations, with the baseline WLS specification from Table 3 displaying on the left for ease of comparison. The added variables are defined in Table 2. ***, ** and * denote statistical significance at the 0.01, 0.05 and 0.1 level, respectively

Table 10 Robustness regressions - different outlier specifications

	Baseline	Outlier criterion				
	(1)	(2)	(3)	(4)	(5)	(6)
		No outlier	Win. 2.5%	P 1%	P 2.5%	IQR
<i>Data characteristics</i>						
Wind coordinates	-11.61*** (2.60)	-11.61*** (2.61)	-9.65*** (2.47)	-10.39*** (2.61)	-6.43** (2.52)	-4.02* (2.31)
Sales	3.54*** (0.82)	3.54*** (0.82)	3.42*** (0.76)	3.52*** (0.81)	3.12*** (0.74)	2.71*** (0.60)
Res. land	-10.47*** (2.86)	-10.47*** (2.87)	-10.36*** (2.77)	-10.57*** (2.82)	-9.28*** (2.87)	-5.98 (4.02)
Distance	0.72*** (0.08)	0.72*** (0.08)	0.70*** (0.08)	0.72*** (0.08)	0.65*** (0.07)	0.52*** (0.06)
Share renewables	-0.05*** (0.01)	-0.05*** (0.01)	-0.05*** (0.01)	-0.05*** (0.01)	-0.05*** (0.01)	-0.05*** (0.01)
Share wind	-0.30*** (0.10)	-0.30*** (0.10)	-0.29*** (0.09)	-0.30*** (0.10)	-0.25*** (0.09)	-0.24*** (0.07)
<i>Control variables</i>						
Demoecon	5.07*** (0.70)	5.07*** (0.70)	4.90*** (0.65)	5.01*** (0.69)	4.57*** (0.63)	4.08*** (0.51)
Oth amen	-1.05** (0.44)	-1.06** (0.44)	-0.96** (0.41)	-0.97** (0.43)	-0.84** (0.40)	-0.70** (0.33)
DID	2.33*** (0.54)	2.32*** (0.54)	2.25*** (0.51)	2.28*** (0.53)	2.23*** (0.49)	1.27*** (0.41)
Spatial	11.17*** (1.06)	11.17*** (1.06)	9.93*** (0.99)	11.11*** (1.05)	7.04*** (1.09)	2.90** (1.18)
<i>Wind turbine impact</i>						
Announcement effect	-1.05*** (0.40)	-1.05*** (0.40)	-1.04*** (0.37)	-1.05*** (0.39)	-1.01*** (0.36)	-0.76** (0.30)
<i>Publication</i>						
Sig	-2.56*** (0.36)	-2.56*** (0.36)	-2.57*** (0.33)	-2.55*** (0.35)	-2.65*** (0.32)	-2.40*** (0.26)
Constant	-4.56* (2.68)	-4.55* (2.69)	-5.10** (2.55)	-5.66** (2.68)	-5.14** (2.54)	-2.24 (2.23)
<i>N</i>	717	720	720	706	684	631
<i>R</i> ²	0.669	0.669	0.669	0.668	0.598	0.565

The table displays the sensitivity of results to changes in the outlier specification, with the baseline WLS specification from Table 3 displaying on the left for the ease of comparison. *No outlier* No observation excluded, *Win 2.5%* Observations winsorized at the 2.5% level, *P 1%* Observations excluded based on the 1% percentile, *P 2.5%* Observations excluded based on the 2.5% percentile, *IQR* Observations excluded based on the interquartile range (Tukey 1977). ***, ** and * denote statistical significance at the 0.01, 0.05 and 0.1 level, respectively

Table 11 Robustness regressions - alternative model specifications

	RE weights				FE weights	No weight
	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	Robust SE	Cluster SE	Study FE	WLS-FE	OLS
<i>Data characteristics</i>						
Wind coordinates	-11.61*** (2.60)	-11.61*** (3.40)	-11.61*** (2.80)	-16.61*** (3.75)	-12.06*** (4.43)	-14.72*** (5.21)
Sales	3.54*** (0.82)	3.54*** (1.09)	3.54*** (1.24)	-10.22 (27.56)	4.49*** (0.59)	1.79 (2.71)
Res. land	-10.47*** (2.86)	-10.47*** (1.58)	-10.47*** (0.61)	-7.57** (3.36)	-12.48 (8.39)	-10.95*** (2.37)
Distance	0.72*** (0.08)	0.72*** (0.09)	0.72*** (0.20)	0.83*** (0.09)	0.25*** (0.03)	0.59*** (0.16)
Share renewables	-0.05*** (0.01)	-0.05*** (0.01)	-0.05*** (0.01)	0.59** (0.25)	0.00 (0.00)	-0.06*** (0.02)
Share wind	-0.30*** (0.10)	-0.30*** (0.10)	-0.30* (0.16)	-1.48*** (0.50)	-0.75*** (0.09)	-0.40* (0.22)
<i>Control variables</i>						
Demoecon	5.07*** (0.70)	5.07*** (0.96)	5.07*** (0.73)	5.01*** (0.99)	6.24*** (0.49)	3.46* (1.79)
Oth amen	-1.05** (0.44)	-1.05*** (0.37)	-1.05* (0.52)	-0.93 (1.01)	-0.99*** (0.30)	-1.23 (1.05)
DID	2.33*** (0.54)	2.33*** (0.54)	2.33*** (0.81)	3.40*** (1.30)	-0.39 (0.31)	2.50 (1.62)
Spatial	11.17*** (1.06)	11.17*** (2.27)	11.17*** (3.15)	11.85*** (1.18)	8.13*** (0.55)	14.91*** (2.66)
<i>Wind turbine impact</i>						
Announcement effect	-1.05*** (0.40)	-1.05*** (0.29)	-1.05* (0.52)	-1.17** (0.47)	-0.45 (0.29)	-0.81 (0.89)
<i>Publication</i>						
Sig	-2.56*** (0.36)	-2.56*** (0.28)	-2.56*** (0.60)	-2.43*** (0.38)	-0.61*** (0.10)	-1.28 (0.97)
Constant	-4.56* (2.68)	-4.56 (3.21)	-4.56** (2.06)	8.12 (27.73)	0.20 (4.50)	-3.23 (5.12)
R^2	0.669	0.598	0.598	0.689	0.684	0.218
N	717	717	717	717	717	717

The table displays the sensitivity of results to changes in the model specification, with the baseline WLS specification from Table 3 displaying on the left for ease of comparison. *Robust SE* Standard errors are robust to heteroscedasticity, *Cluster SE* Standard errors are clustered at the study level, *Study FE* Study-level fixed effects are included, *WLS-FE* Inverse-variance weights are used, *OLS* Ordinary least squares. ***, ** and * denote statistical significance at the 0.01, 0.05 and 0.1 level, respectively

Table 12 Robustness regressions - additional moderators and distance subsamples

	Additional moderators				Distance subsamples	
	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	Market extent	Citations	Impact factor	≤ 2 miles	≤ 1 mile
<i>Data characteristics</i>						
Wind coordinates	-11.61*** (2.60)	-11.84*** (2.63)	-11.77*** (2.60)	-11.59*** (2.61)	-15.57*** (2.73)	-10.74* (6.24)
Sales	3.54*** (0.82)	3.49*** (0.82)	3.12*** (0.86)	3.53*** (0.83)	2.37** (1.20)	2.47* (1.36)
Res. land	-10.47*** (2.86)	-10.42*** (2.86)	-10.39*** (2.86)	-10.47*** (2.86)	-11.36*** (3.71)	-11.27*** (4.28)
Distance	0.72*** (0.08)	0.72*** (0.08)	0.73*** (0.08)	0.72*** (0.08)	0.91** (0.38)	-0.95 (0.92)
Share renewables	-0.05*** (0.01)	-0.05*** (0.01)	-0.05*** (0.01)	-0.05*** (0.01)	-0.07*** (0.01)	-0.07*** (0.02)
Share wind	-0.30*** (0.10)	-0.34*** (0.11)	-0.34*** (0.10)	-0.30*** (0.10)	-0.48*** (0.12)	-0.52*** (0.14)
<i>Control variables</i>						
Demoecon	5.07*** (0.70)	5.10*** (0.70)	4.89*** (0.71)	5.07*** (0.70)	5.27*** (0.91)	7.06*** (1.04)
Oth amen	-1.05** (0.44)	-0.73 (0.65)	-1.11** (0.44)	-1.05** (0.45)	-0.83* (0.49)	-0.89* (0.51)
DID	2.33*** (0.54)	2.18*** (0.58)	2.41*** (0.54)	2.33*** (0.54)	3.67*** (0.72)	4.84*** (0.88)
Spatial	11.17*** (1.06)	11.18*** (1.06)	11.28*** (1.06)	11.17*** (1.06)	14.11*** (1.52)	13.42*** (1.61)
<i>Wind turbine impact</i>						
Announcement effect	-1.05*** (0.40)	-0.97** (0.41)	-1.01** (0.40)	-1.05*** (0.40)	-0.78* (0.47)	-0.41 (0.50)
<i>Publication</i>						
Sig	-2.56*** (0.36)	-2.60*** (0.36)	-2.62*** (0.36)	-2.56*** (0.36)	-1.92*** (0.45)	-1.63*** (0.47)
<i>Additional moderators</i>						
National		0.51 (0.77)				
Citations			-0.18 (0.12)			
Impact				-0.06 (0.87)		
Constant	-4.56* (2.68)	-4.57* (2.68)	-3.73 (2.73)	-4.54* (2.71)	-3.72 (2.86)	-8.13 (6.26)
R ²	0.669	0.669	0.671	0.668	0.755	0.788
N	717	717	717	717	491	351

The table displays the sensitivity of results to the inclusion of additional moderators and for distance subsamples, with the baseline WLS specification from Table 3 displaying on the left for ease of comparison. “≤ 2 miles” and “≤ 1 miles” refer to subsamples of observations with a distance to wind turbines of no more than two and one mile(s), respectively. ***, ** and * denote statistical significance at the 0.01, 0.05 and 0.1 level, respectively

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Declarations

Conflict of interest The author has no competing interest to declare that are relevant to the content of the article.

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