Are friends electric? Valuing the social costs of power lines using house prices*

Cheng Keat Tang Nanyang Technological University Stephen Gibbons London School of Economics

April 15, 2024

Abstract

Overhead electrical power lines and pylons have long raised concerns regarding the effects of electromagnetic fields on health, noise pollution and the visual impact on rural landscapes. These issues are once again salient because of the need for new lines to connect sources of renewable energy to the grid. In this study we provide new evidence on the cost implied by these externalities, as revealed in house prices. We use a spatial difference-in-difference approach that compares price changes in neighbourhoods that are close to overhead power-lines, before and after they are constructed, with price changes in comparable neighbourhoods further away. Our findings suggest that the construction of new overhead pylons reduces prices by 3.9% for properties up to 1500 meters away, suggesting the impacts extend further than previously estimated.

(JEL: R32, Q48, Q51)

Keywords: Externalities, Overhead Power lines, Pylons, House Prices, Revealed Preferences **Acknowledgements:** We would like to thank National Grid for providing locations of power lines and the year they are constructed.

^{*}The title was inspired by the song, Are Friends Electric?, by Tubeway Army.

1 Introduction

High voltage overhead power lines and their associated pylons are ubiquitous features of developed landscapes and provide an essential role in cost-effective electricity transmission. They have, nevertheless, long raised objections from those concerned about their impact on the land-scape. John Maynard Keynes is reportedly wrote to the Times newspaper in the late 1920s when the national electrical grid was first developed in Britain, describing the impact of pylons on the Sussex downs as 'the permanent disfigurement of a familiar feature of the English landscape' (Hicks, 2018; Thomson, 2014). Contingent valuation studies have shown that respondents perceive pylons as unattractive and are willing to pay to remove them and put them underground (Delaney and Timmons, 1992; Priestley and Evans, 1996; Atkinson et al., 2004). The routing of transmission lines is thus a contentious planning issue.

Alongside the visual environmental impacts, there are concerns about other effects on health, well-being and safety. Electromagnetic fields (EMFs) emitted from high voltage cables have been linked to various health conditions such as childhood leukemia (Wertheimer and Leeper, 1979; Feychting and Alhbom, 1993), various forms of adult cancers (Feychting and Ahlbom, 1994; Elliott et al., 2013), suicide and depression (Baris and Armstrong, 1990), heart disease (Sorahan and Nichols, 2004) and neuro-degenerative disorders (Sobel et al., 1995; Savitz et al., 1998). Although the evidence on health effects is inconclusive and often contradictory, reports of concerns appear regularly in the media (Brown, 2000; Northern Echo, 2004). Power lines also generate noise pollution. Corona noise (crackle or hum) is emitted when air around electric cables is ionized, particularly on wet days.¹ Aeolian noise is generated from vibrations when strong winds blow against the cables and pylons. The bigger the transmission lines, the greater the noise from both sources. Strong winds and natural disasters can topple power lines, causing fire risk, though this risk is negligible in the UK due to safety cut-out features. In other contexts it is bigger issue: One of the deadliest wildfire that completely burnt down the town of Paradise in California in 2018 was due to power transmission lines.²

These issues are back in the news today in the UK and US, with many high voltage lines needed to connect renewable energy generation sources (Milman, 2023; Seddon, 2023). Construction of these new power lines in the UK is inevitably stalled by planning objections and the Electricity Networks Commissioner has recommended monetary payments to households in their path, to speed up the process (Winser, 2023). Other organisations advocate burying cables underground to avoid their environmental impacts, which is much more costly than running them

¹For more technical details, see https://www.scientificamerican.com/article/ what-causes-the-noise-emi/

²For more information, refer to https://www.nytimes.com/2019/05/15/business/pge-fire.html.

overhead (Vidal, 2012). Other groups campaign for an undersea ring main around the UK, to link up offshore wind farms and avoid new overhead power lines altogether. ³ Good estimates of the magnitude and spatial extent of the perceived environmental cost of power lines to local residents are therefore crucial.

This paper provides new evidence on the monetary value of these negative externalities, by estimating the impact of high voltage pylons on local housing values in England and Wales. Well established theories (Rosen, 1974) and a mass of empirical literature on the valuation of amenities suggest that we can interpret price differences between properties close by, and comparable properties further away, as household marginal willingness to pay (WTP) to avoid living close to power lines. In practice, estimation of WTP from house prices is empirically challenging, due to unobserved confounding factors. In the case of power lines, routing is likely endogenous to house price formation, given that lines are run in ways that minimise the costs of planning, construction and transmission, and in such a way as to minimise impacts on residential population.

The paper mitigates these challenges by estimating the price changes occurring in response to the construction of new pylons. We adopt a quasi-experimental fixed effects/difference-indifferences research design that compares price changes occurring in postcodes before and after pylons are constructed with price changes in comparable postcodes further away. Kuminoff et al. (2010) provides a discussion of the advantages of quasi-experimental approaches of this type in the context of hedonic methods for environmental valuation. Recent work has confirmed the validity of these reduced form causal estimation methods for estimating marginal willingness to pay, without the need for structural approaches (Banzhaf, 2021). Our study is, therefore, a significant advance over previous studies, which are nearly all based on cross-sectional price comparisons on relatively small samples of transactions. The paper closest in design to ours is Thomas et al. (2017), although their context is quite niche, looking at a single 4 mile stretch of 16 pylons, which were never operational and only in place for two and a half years. In contrast, we look at the impact of over 650km of new lines and 790 new pylons. ⁴

Results from our difference-in-differences specifications show that pylons reduce prices of houses within 1500 metres, by an average of 3.9%, relative to those further away.⁵ This estimate remains robust and stable across a battery of robustness tests that accounts for both observed and unobserved differences between properties. An event-study design looking at the timing of the

³See for example http://www.svu.org.uk/uk-ring-main.html or https://pylonseastanglia.co.uk.

⁴There are numerous papers that rely on quasi-experimental variation of non-market amenities to measure their values using the housing market. This includes air quality (Chay and Greenstone, 2005), traffic (Tang, 2021), crime (Gibbons, 2004; Tang and Le, 2023), externalities from wind farms (Gibbons, 2015; Dröes and Koster, 2016) and health risks (Davis, 2004) etc.

⁵This estimate is obtained from the preferred empirical specification of Column 1 in Table 3. In particular, we combine the estimated effects from 0-750m and 750-1500m into one group - 0-1500m.

impacts suggests the results are causal, and not attributable to pre-existing differences in price trends. Conversely, estimates from our cross-sectional hedonic regressions exhibit many signs of mis-specification with substantial variation across regressions with different set of controls.

We further disentangle whether this WTP is driven by the visibility of pylons by determining whether each postcode has a view of the electric pylons using Digital Elevation Models that combines height data and pylon locations. While we do not find any discernible differences in estimates between properties with and without a view of pylons, our results suggest that the impacts of these infrastructures are more widespread than previously estimated, collectively causing a loss in home values of around £19 billion. Although these effects on the housing market are sizable, a simple back of the envelope analysis suggests that the benefits from burying overhead power lines are unlikely to outweigh the cost for doing so.

The remainder of the paper is structured as follows. Section 2 discusses the background policy issues of power transmission lines in England and Wales and reviews the existing literature on power lines and housing values. Section 3 describes the data used for the analysis. Section 4 outlines the empirical strategy and Section 5 explains the findings for this paper. Finally, Section 6 discuss the implications of our findings and concludes.

2 Background & Literature Review

A transmission line is a high-voltage overhead power line for long-distance distribution of electricity. There are more than 7,200 kilometers of overhead transmission lines, carrying voltages of 132 kV, 275 kV or 400 kV, across England and Wales. They are owned and operated by National Grid plc. Figure 1 shows a typical pylon in the UK, used to carry overhead transmission lines in the UK.



Figure 1: 132kV overhead lines and pylons in a suburban setting in England ©Stephen Gibbons 2023.

Figure 2 maps the location of overhead power transmission lines across England and Wales, carrying voltage lines of between 275kV and 400kV. These power lines typically carry two separate circuits on each side of the pylon with each side carrying three bundles of wires. Our main source of variation for identifying the effects of pylons stem from newly constructed power lines from 1995 onwards. A total of 653km of new transmission lines and 790 towers were added over this period. The location of these features are denoted in thicker dark lines.

Despite the purported concerns with living close to overhead power lines, there are no restrictions on how close a home can be to an overhead power line, underground cable, or substation in United Kingdom. However, power lines carrying 132kV and above are considered to be 'major infrastructures' that require development consent from the Infrastructure Planning Commission (IPC) for construction. IPC will have to consider the evidence provided by the applicant and any other relevant evidence on the impacts of the project. They have to ensure that the proposal is in accordance with the guidelines highlighted in the National Policy Statement for Electricity Networks Infrastructure (NPS EN-5) before consenting to the project. Given the lack of conclusive evidence indicating that EMFs have any causal detrimental impacts, many projects receive approval for their development despite opposition. For instance, a power line in Lancashire in 2007, power lines from Beauly to Denny in Scotland in 2010 and Hinkley Point C connection in 2016 were all granted development consent. A large number of citizen and campaign groups have been formed to fight against the infringement of pylons and overhead cables into residential areas.⁶

⁶For more information on the list of groups, activists and research institutes on Electromagnetic Fields,

Because transmission lines are generally run away from residential areas, and homeowners can seek for compensation if pylons or power lines infringe on their properties, the number of house-holds affected are relatively small.⁷ Based on the estimates from National Grid, approximately 46,000 homes are within 100m, which constitutes around 0.2% of all the homes across England and Wales. Still, this figure increases exponentially to 600,000 homes (2.7% of all homes) within 500m and up to 3.1 million homes at 1200m.

Existing studies on the effects of pylons and transmission lines on house prices typically base their analysis on cross-sectional, spatial variation in exposure. They measure the willingness to pay (WTP) to avoid pylons by comparing housing values close to pylons with those further away, holding all other differences constant via a multi-variate regression framework.

Most of these studies are conducted in North America. Colwell (1990) examines the effects of power lines for a small sample of 200 sales within 400 meters from transmission towers in Decatur, Illinois from 1968 to 1978. Hamilton and Schwann (1995) extend the analysis for a large sample of 12,907 single detached house sales from four neighbourhoods in Vancouver, Canada from 1985 to 1991. Both studies show that transmission lines have a detrimental impact on housing values although these effects are highly localized and dissipate quickly moving away. François (2002) conducted a similar analysis for 507 single family houses in Greater Montreal in Canada from 1991 to 1996, and focused on how the WTP to avoid electric power lines changes after widely publicity on studies investigating electromagnetic fields (EMF)-induced health hazards. Their findings remain inconclusive. Sims and Dent (2005) broaden the analysis to the UK and use two different approaches. They conducted a contingent valuation on 257 valuers and 176 agents to seek their perceptions towards electric power lines and transmission towers on housing values, and report a 5 to 10 % discount. They then adopted a revealed preference approach on 620 properties in Scotland sold between 1994 and 2010 and report price effects of up to 21% within 250m.

Thomas et al. (2017) adopt a Difference-in-Difference (DID) empirical strategy to investigate the effect of new pylons from the Tehachapi Renewable Transmission Project (TRTP), which are planned to transmit electricity from wind farms, on housing values in California. They document significant loss of home values of between 8.3% for encumbered and 4.9% for abutting properties surrounding 2,569 transactions before and after 16 pylons are constructed but not operational. Although they improve the estimation using a quasi experimental strategy, it remains hard to see how their findings can be generalised to the other contexts involving permanent pylons and active transmission lines at a larger scale.

The limitations of these existing studies raise concerns about both their internal and external

refer to http://www.emfs.info/more/links/

⁷For more information, refer to https://www.nationalgridet.com/ network-and-assets/landowners-occupiers-and-grantors



Figure 2: Overhead Power Lines across England and Wales. Map denote the OHLs managed by National Grid. Thicker dark lines denote newer OHLs constructed by National Grid from 1995 to 2017. *Source: Authors own illustration. Locations of OHLs provided by National Grid.*

validity. Point estimates differ substantially between studies and even within studies when a different set of control variables are accounted for. Perverse (positive) estimates are documented in some instances, raising concerns whether cross-sectional regressions are mis-specified and yield biased and inconsistent WTP estimates. In contrast, our study extends the analysis to a universe of transactions of nearly 1.4 million sales across England and Wales from 1995 to 2018. We mitigate endogeneity concerns using a quasi-experimental DID strategy that exploits variation in the exposure to 790 newly constructed pylons, supporting 653km of power transmission lines. Even when we restrict our analysis to properties near new pylons, we still have a sample of more than 73,000 sales, more than 38,000 of them made within 1500m from the nearest pylon. There is, therefore, a much greater chance than in previous work of accurately estimating generalisable causal effects of power lines on local housing values.

Another stream of literature related to this paper is the burgeoning line of papers measuring the negative externalities of renewable energy infrastructures, which include wind farms and solar panels. Despite planners' attempts to combat the existential problem of climate change by replacing traditional energy sources with renewable energy to reduce carbon footprint, these infrastructure could, unintentionally, impose a cost on local communities. Wind farms, for instance, impose tremendous environmental cost through visual disturbance and noise pollution. Researchers have shown that the negative externalities from new wind farms have a detrimental impact on housing values in England, Wales and Netherlands (Gibbons, 2015; Dröes and Koster, 2016). Negative house effects are also reported for the construction of new solar panels in the United States and Netherlands. These solar panels could impose visual disamenities, destroy rural character and increase fire risk (Dröes and Koster, 2021; Gaur and Lang, 2023). In similar fashion, our study contributes to this stream of literature by examining whether transmission cables and towers, which are integral in transmitting electricity from renewable energy sources, could impose negative externalities to local communities.

3 Data

Our data come from a range of sources. Information on the location (latitude and longitude), characteristics and year of construction of the pylons are provided by National Grid. Characteristics recorded include the height of the pylon and the voltage it is carrying (275 or 400 kV). We further collect information on the location smaller pylons carrying a lower voltages that are not managed by National Grid. Data is provided by OS Vector Map district.⁸

Housing transactions data come from the England and Wales Land Registry 'price paid' housing transactions data. This dataset records basic information on sales price, basic property types detached, semi-detached, terraced or flat/maisonette - whether the property is new or secondhand, and whether it is sold on freehold or leasehold basis. We link this dataset to information from Energy Performance Certificates (EPC), which are required for all properties bought and sold in England and Wales.⁹ The EPC data provides a much richer description of the structure of the property. Additional information includes size of the unit, number of rooms, whether the unit

⁸More details can be found in https://www.ordnancesurvey.co.uk/ business-government/products/vectormap-district

⁹This data linking was done for another project by colleagues at LSE.

has a fireplace, and estimated energy consumption. Although the EPC is given to properties from 2008 onwards, the information can be tracked back for properties with EPCs when they are sold in earlier periods (assuming that the basic structure of the property did not undergo massive changes over this period).

The housing transactions are geocoded using the centroid of the address unit postcode. Unit postcodes are the smallest geographical units we have available. There are around 1.5 million postcodes in England and Wales, which has a land area of 150000km squared and 25 million households. This implies a mean land area of around 0.1km squared and about 17 households per postcode. Most unit postcodes have a much smaller land area as the distribution is right skewed by large postcodes in sparse areas with few houses. The postcode centroids we use come from the UK Office for National Statistics Postcode Directory and are based on the mean coordinates of the addresses of buildings within the postcode, which provides a more precise estimate of the location of houses than a geometric centroid. We compute Euclidean distances from postcode centroids to the nearest pylons, power lines and other geographic features using Geographic Information System (GIS) software. Our main analysis focuses on distance to 275kV and 400kV power transmission lines managed by the National Grid, but we also control for distance to smaller 132kV lines. We focus mainly on distance to pylons, rather than cables, because pylons are the salient visible features of the landscape and because distances to pylons and cables are very highly correlated. Our dataset, which limits to properties not more than 2000m from the nearest pylon, covers more than 1.3 million property transactions from 1995 to 2017.

Information on other geographical features comes from various sources. We compute proximity to the nearest waterways and green space based on data from Ordnance Survey Open Rivers and Open Greenspace (Ordnance Survey, 2018a,b). These variables are used to control for natural amenities that are likely to be correlated with pylon location, given the predominantly rural and suburban location of the latter. We also classify the land use surrounding each postcode relying on information from Landsat remote sensed data that is derived from satellite imagery and provides land cover information at 25 meters by 25 meters rasters (Rowland, 2017). Each postcode, based on the centroid, is matched onto the land use rasters and classified into 9 major land uses, including urban, suburban, rural land uses etc. From Ordnance Survey Strategi data (Ordnance Survey, 2015), we measure the distance of each postcode from the nearest rail lines and stations to mitigate the risk of accessibility to public transit from biasing our estimates.

We link each postcode to Census data units, the Local Area Districts (LAD), Middle Layer Super Output Areas (MSOA), Lower Super Output Areas (LSOA) and Output Areas (OA). There are around 180,000 OAs and 35,000 LSOAs, 7,200 MSOAs, and 317 LADs across England and Wales. OAs are the smallest geographical area in which Census data from the Office of National Statistics is collected at every decade. To control for neighbourhood differences between proper-

ties, we account for a wide array of characteristics, specifically unemployment rate, percentage of households owning cars and lone-parent households, percentage of residents under social renting, with no education, in minority ethnic groups, who are non-EU, homeownership rates, population and population density, all at OA level. These data are collected from the 2001 and 2011 Census and matched to different sales according to the closest year of transaction.

4 Research Methodology

4.1 Cross-sectional regression strategy

To value how much homeowners pay to avoid power lines, existing studies typically adopt a cross-sectional empirical set up. As a starting point to our analysis, we follow this tradition and estimate regressions of the following form:

$$Y_{ijt} = \alpha + \sum_{k \in K} \gamma^{k-l,k} Py lon_j^{k-l,k} + X'_{jt} \phi + N'_{it} \rho + \tau_t + \epsilon_{ijt},$$
(1)

where Y_{ijt} is the natural logarithm of sale price for property *i* located in postcode *j* and sold in time t. The key explanatory variable of interest, $Pylon_i^{k-l,k}$, is a binary indicator taking a value of 1 if postcode j is between k - l and k meters from the nearest pylon and 0 otherwise. These indicators define non-overlapping distance bands of width l meters, at intervals of k meters in a set K, up to some maximum distance beyond which we do not expect power lines to have any impact on prices. We set this limit initially to 2000m. In this cross-sectional analysis, we restrict the sample to sales in postcodes that already had pylons within 2000m before 1995, at the beginning of our house sale transactions data. There is then no variation in the distances between postcodes and pylons over time within our estimation sample and estimation of the coefficients $\gamma^{k-l,k}$ is based solely on cross-sectional variation in the proximity of houses to pylons. Distances are Euclidean distances from the centroid of postcode j to the nearest pylon. We set k to be in $K = \{300m, 600m, 1500m, 1800m\}$ and l = 300, such that there is an indicator for each 300m distance band from 0-1800m. The coefficients, $\gamma^{k-l,k}$, are interpreted as the percentage difference in sale prices between properties between k - l to k meters from the nearest pylon and properties further away in the omitted reference group. We expect the price effects to diminish as k increases because the negative externalities from pylons decay as distance from pylons increases.

To minimize salient differences between properties, these cross-sectional methods use a 'kitchensink' control variable strategy, controlling for observable characteristics of housing and locality. In our regressions we control for housing (e.g size, property type, tenure), denoted by N'_{it} , location (e.g distance to transportation nodes, schools, parks) and neighbourhood characteristics (e.g unemployment rate), denoted by X'_{jt} . We also control for various forms of neighbourhood fixed effects, including Local Area District (LAD), Middle Super Output Area (MSOA), Lower Super Output Area (LSOA) and Output Area (OA) fixed effects, to partial out time-invariant unobserved differences between locations across space. τ_t denotes time dummies that control for general trends in property prices across areas over time.

For γ^k to be consistently estimated, the assumption is that $E[\epsilon_{ijt}|Pylon_j^{k-l,k}] = 0$, conditional on observable control variables. This assumption, however, is very likely to be violated because the locations of pylons are endogenously determined. In the case of power lines, routing is related to land and house prices because lines or pylons are typically run in such a way as to minimise impacts on residential population, and reduce planning and construction costs (especially given home owners are entitled to compensation for lines crossing their property). Consequently, there will be many unobserved confounding factors, meaning distance to pylons is almost certainly correlated with the unobserved factors affecting house prices ϵ_{ijt} . These empirical weaknesses are reflected in the wide variation in magnitude and direction of the estimates within and across existing studies.

4.2 Difference-in-difference strategy

To mitigate the identification challenges inherent in the cross-sectional approach, we estimate a panel-based difference-in-difference (DID) specification, that identifies the effect of pylons on house prices from new pylon construction:

$$Y_{ijt} = \alpha_j + \sum_{k \in K} \gamma^{k-l,k} Py lon_j^{k-l,k} + \sum_{k \in K} \beta^{k-l,k} Py lon_j^{k-l,k} \times Post_{jt} + \delta Post_{jt} + X'_{jt} \phi + \tau_t + \varepsilon_{ijt},$$
(2)

When estimating this regression specification, we limit the sample to sales in postcodes within a 2000m distance buffer of a new pylon constructed from 1995 onwards, the year from which we have data on new pylon construction. ¹⁰ This sample restriction is in contrast to the cross-sectional approach described above, in which we limit the sample to postcodes which already had pylons within 2000m distance by 1995. Similar to before, $Pylon_j^{k-l,k}$ is a binary variable that takes the value of 1 if postcode j is k - l to k meters from nearest pylon, 0 otherwise. Initially we set k and l to define 300m bands, as in the cross-sectional analysis described above, but we use other values of l and K elsewhere. $Post_{jt}$ is a single indicator variable that takes the value of 1 for all years t after the nearest pylon to postcode j is constructed and 0 otherwise. The key variable of

¹⁰Assessments of the sensitivity to the choice of this maximum distance buffer are provided in the robustness checks

interest is the interaction of $Post_{jt}$ and $Pylon_j^{k-l,k}$. The estimates of the parameters $\beta^{k-l,k}$ are the difference-in-difference estimates. These parameters estimate the mean difference between the percentage change in mean transaction prices for properties k - l to k meters from the nearest pylon, and the mean percentage change in transaction prices for properties in the omitted baseline reference group, where the percentage change is between the periods before and the periods after the nearest pylon is constructed.

In this specification, we are identifying the effects of pylons from the changes in distance to properties that occurs as new pylons are constructed. The specification partials out time-invariant confounders related to sales in the same location using postcode fixed effects (α_j) . The specification also partials out changes occurring in general within 2000m of new pylons, through $Post_{jt}$. Coefficient δ is identified by comparison of changes in prices in a postcode occurring at the time a pylon is constructed within 2km, with changes in prices occurring across the whole estimation sample - i.e., all transactions in postcodes that had a pylon constructed within 2000m in the past or will do so in the future.

There is a growing literature highlighting that DID two-way fixed effects (TWFE) estimators of this type could be problematic when there is staggered adoption timing, i.e., when relying on staggered roll-out of treatment with no groups left untreated (De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Callaway and SantAnna, 2020). When some units are treated earlier and others later, units treated in one period serve as control units in another period. This can be an issue when there is heterogeneity in the effects of treatment over the study period, because regression estimates are a variance weighted average of these heterogeneous effects. Specifically, the DID TWFE estimator is a weighted average of treatment-control comparisons in different subgroups (Goodman-Bacon, 2021). This includes differences between early treated and later treated groups over the periods when the later treated groups are not yet treated (group 1), differences between early treated and later treated groups over the periods when the early groups are treated and they are used as benchmark for later treated group (group 2) and differences between early or later treated groups with the never-treated group (group 3). Weights given to the different subgroups are determined by the sample size and the variance of treatment. In our context, properties that are treated in the middle will have the largest variance of treatment and will be given higher weights while properties treated in the beginning or the end will receive smaller weights because of smaller variance of treatment.

To allay concerns over DID TWFE estimates, we adopt two intuitive strategies in our robustness tests. First, we constrain our analysis to a balanced time window of 5 years around the construction dates of each pylon. Figure 3 provides a simple illustration of this strategy for two pylons. By doing so, we ensure that the variance of treatment is the same for all pylons regardless of their construction dates. Another notable advantage of this strategy is that it reduces the risk from unobserved temporal confounders, such as changes in neighborhood and/or housing characteristics that could occur in the long run. As a second check, after restricting our data to sales within 5 years from the construction year, we collapse these transactions to means in two time periods, before and after pylon construction, at postcode level. This means that we have only two observations per postcode (before and after pylon construction). Using these data, we then estimate the effects of pylons on housing prices using a simple 2-by-2 DID setup, comparing changes in housing prices between the periods before and after pylon construction in postcodes close to pylons, with the changes in housing prices occurring in postcodes further away.



Figure 3: An illustration of the five year window surrounding two pylon construction dates. Treated denote properties that are close to newly constructed pylon while control denotes properties further away. T=1 (T=0) denotes five year window after (before) pylon construction date. Un-shaded areas are omitted from our analysis.

Although we partial out time-invariant unobservables by controlling for α_j , and time variant unobservables through $Post_{jt}$, time-variant factors that affect prices near pylons differently to those further away could bias our results. The restriction of the difference-in-difference estimation sample to transactions within 2000 meters of pylons is one step to minimise this risk. However, we take other steps to check for the possibility of unobserved time varying confounders. Firstly, in robustness tests we control in our regressions for differential trends at various geographical levels, by interacting fixed effects for census zones (Lower Layer or Middle Layer Super Output Areas) with year trends. One other issue could be changes in sample composition in terms of the characteristics of houses sold close to pylons, relative to those further away. Therefore, we check directly whether there are any changes to the composition of houses sold after power lines are constructed, using 'balancing tests' in which we replace prices with various housing characteristics in our difference in difference regressions. ¹¹ We also test for the presence of pre-existing differential trends in prices, between treated and control units, by conducting an event study regression examining property price changes before and after the pylons are installed with the inclusion of temporal leads and lags. All these results can be found in robustness section and will be explained in greater detail.

5 Results

5.1 Descriptive statistics

Table 1 presents summary statistics of housing (in Panel A) and neighbourhood characteristics (in Panel B) for three groups: properties within 2000m, those within 1500m and those more than 1500m but less than 2000m from the nearest overhead power line. We report results for both the cross-sectional and DID sample. Panel A shows that properties closer to power lines are transacted at lower prices compared to those further away. Houses closer to power lines are more likely to be detached homes, less likely to be flats/apartments and more likely to be freehold properties. Surrounding neighbourhood characteristics are shown in Panel B. Places near power lines are more likely to have home owners, car owners, and have lower population densities. These differences illustrate the fact that transmission lines are primarily constructed between cities, through rural and low density areas, for the reasons discussed already - to minimise construction costs, planning costs and the impact on residential areas. The patterns are similar in the full sample, and the DID sample of transactions close to pylons constructed since 1995, suggesting our smaller DID sample is representative of the entire population.

Panel C further summarizes the number of sales and postcodes from 0 to 750m, 750 to 1500m and 1500 to 2000m for our sample of sales from cross-sectional and DID regressions. The sample of transactions from DID regressions is noticeably smaller because they are postcodes within 2000m from newly constructed power lines from 1995 onwards. There are around 6219 sales from 377 postcodes within 750m from power lines, 32165 sales from 1896 postcodes from 750 to 1500m and 35319 sales from 1896 postcodes from 1500 to 2000m.

5.2 Baseline results

Figure 4 summarizes coefficients and confidence intervals from cross-sectional hedonic and difference-in-difference regressions of house prices on proximity to pylons for every 300m up to

¹¹Another way would be to estimate a repeated sales model, with house rather than postcode fixed effects. However, estimation based on repeat sales is weighted towards a selected sample of houses which sell frequently, and reduces our sample to 3485 sales from 73703 sales

		1					
	Panel A: Housing Characteristics						
	Cross-sectional Sample			DID Sample			
	All(<=2km)	<= 1.5km	1.5km to 2km	All(<=2km)	<= 1.5km	1.5km to 2km	
Sale Price (2015 values)	204295.12	202584.09	208303.13	170707.18	169079.50	172476.10	
Log Price	11.61	11.61	11.61	11.48	11.47	11.49	
New Builds	0.07	0.07	0.06	0.09	0.08	0.09	
Size (sqm)	88.54	88.34	89.00	88.86	88.64	89.11	
Detached House	0.19	0.20	0.17	0.20	0.22	0.19	
Flat/Mansionette	0.14	0.13	0.15	0.08	0.07	0.09	
Semi-Detached House	0.34	0.34	0.32	0.42	0.45	0.40	
Terraced House	0.34	0.32	0.36	0.29	0.26	0.32	
Freehold	0.74	0.74	0.73	0.81	0.82	0.80	
Fireplace	0.15	0.15	0.16	0.13	0.14	0.13	
Energy Consumption	59.38	59.52	59.05	60.17	60.31	60.01	
Number of Rooms	4.60	4.60	4.59	4.65	4.65	4.64	
Number of Rooms with Heating	4.24	4.25	4.23	4.22	4.18	4.27	
Number of Extensions	0.46	0.45	0.47	0.46	0.48	0.45	
Solid Walls	0.24	0.23	0.27	0.17	0.15	0.20	

Table 1: Descriptive Statistics

	Panel B: Neighbourhood Characteristics					
	Cross-sectional Sample			DID Sample		
	All(<=2km)	<= 1.5km	1.5km to 2km	All(<=2km)	<= 1.5km	1.5km to 2km
Pop Size	314.76	314.81	314.62	306.12	304.99	307.32
Pop Density	53.96	52.52	57.32	44.63	41.13	48.33
Unemployment Rate (%)	0.05	0.05	0.06	0.05	0.05	0.06
Non-white Residents (%)	0.13	0.12	0.14	0.06	0.04	0.07
Social Renters (%)	0.12	0.12	0.13	0.12	0.10	0.13
Home Owners (%)	0.74	0.75	0.72	0.77	0.80	0.74
Non-EU Residents (%)	0.08	0.07	0.09	0.03	0.03	0.04
Lone Parent Households (%)	0.07	0.07	0.07	0.07	0.06	0.07
Residents w/o education qualifications (%)	0.26	0.26	0.26	0.25	0.26	0.25
Households w/o cars (%)	0.24	0.23	0.26	0.21	0.19	0.23
No. of Transactions	1300982	911754	389228	73703	38384	35319
No. of Postcodes	73902	51950	21952	4169	2273	1896
	Panel C: Number of Sales & Postcodes around power lines					
	Cross-sectional Sample			DID Sample		
Distance Bandwidths	Number of Sales	Postcodes		Number of Sales	Postcodes	
0-750m	337492	19472		6219	377	
750-1500m	574262	32478		32165	1896	
1500-2000m	389228	21952		35319	1896	

Means of various housing and census characteristics between properties less than 1500m from the nearest pylon and properties more than 1500m but less than 2000m from the nearest pylon for cross-sectional sample and DID sample.

1500m (i.e., 0-300m, 300-600m 1200-1500m, 1500-1800m). Vertical bars on the plots denote 95% confidence intervals constructed from standard errors clustered at postcode level.

In the cross-sectional graph (marked by diamonds), estimation of the coefficients stems from comparing property prices at different distances to existing pylons. Coefficients are interpreted as the percentage difference between mean house prices in a given distance band, compared to mean prices from 1800 to 2000m, the omitted reference group. For the cross-sectional regression, we include output area (OA) fixed effects to partial time-invariant unobserved geographical factors, year-quarter fixed effects to control general trends in property prices across space, and a vector of housing, neighbourhood and location controls to control for salient differences between properties (equation 1).

Coefficient estimates from the cross-sectional regression are small and insignificant, which if interpreted causally, would suggest no willingness to pay to avoid overhead power lines. However, for all the reasons discussed earlier, we doubt these regressions have a causal interpretation. We provide more evidence on the empirical weaknesses of cross-sectional hedonic regressions in Table A3 in the Data Appendix. It is evident from these results that cross-sectional estimates of the impacts of infrastructure are highly sensitive to specification and unreliable, echoing the findings reported by Chay and Greenstone (2005) for the WTP to avoid air pollution.

To obtain more plausibly causal estimates, we apply the postcode fixed effects/differencein-difference (DID) research design that compares house price changes close to pylons before and after they are constructed, with house price changes further away. For these estimates, we restrict the analysis to sales less than 2000m from power lines constructed from 1995 onwards, reducing the sample size to around 73,000 property sales from more than 4,000 postcodes. The plot with circular markers in Figure 4 presents these coefficients for every 300m up to 1800m from the nearest pylon constructed after 1995. In these regressions, we control for postcode fixed effects, year-quarter fixed effects, and a vector of housing characteristics.¹² For details on the actual estimated effects, refer to Table A4.

In contrast to the cross-sectional regressions, these Difference-in-Difference estimates suggest that home owners do pay to avoid living near new transmission lines. Properties within 0 to 300m experience a 9.8% reduction in their market value after pylons are installed.¹³ These effects remain stable at around 4.0% for properties between 300 and 600m, around 6.6% for those between 600 and 900m, around 4.5% for those between 900 and 1200m and around 2.3% for those between 1200 and 1500m. Beyond 1500m, the estimated effects are small at 0.4% and statistically insignificant. ¹⁴ Given that the average housing price (in 2015 values) around power lines is around £170,000, the absolute impact of overhead power lines on housing values could range from £3,865 for properties between 1200 and 1500m to £16,638 for properties between 0 and 300m. These estimates suggest that overhead power lines might have a far-reaching impact on housing prices than previously estimated (Colwell, 1990; Hamilton and Schwann, 1995; Sims and Dent, 2005).

We present additional DID estimates by sequentially adding control variables into our empirical specifications to understand whether these estimates are stable across specifications. We relegate these results to Table A4 in the Data Appendix. Here, we show that, unlike the estimates

¹²We are unable to add location characteristics because all these variables are constructed from postcode locations and there is no variation within postcodes. In our robustness tests, we interact these characteristics with year dummies and added them as controls. Doing so has an immaterial effect on our estimates.

¹³Percentage changes are computed as $(exp(\beta) - 1) \times 100\%$

¹⁴This decay in house price effects over distance bands is also quite consistent with our semi-parametric results reported in Figure A2. Due to space constraints, we relegate this figure and the discussion of the results in the data appendix.

Figure 4: Cross-sectional and Difference-In-Difference Regressions of the effect of proximity to pylons on housing values across distance at 300m intervals



Reported estimates are the reported house price effects at 300m intervals (0-300m, 300-600m....,1500-1800m) from both cross-sectional and DID regressions. For cross-sectional regressions, we control for a vector of housing characteristics that include size, number of rooms, number of heated rooms, number of extensions, wall type, property type, tenure, whether the unit is new build, has a fire place, and energy consumption, **neighbourhood characteristics** that include population size, population density, percentage of social renters, minority race residents, non-EU residents, residents without education qualifications, lone-parent households and households without cars, unemployment rate, homeownership rates collected annually at Output Area level (OA), and location characteristics that include distance from nearest pylon that is not managed by National Grid and its second polynomial, distance from rail stations, railways and rapid stations, green space and waterways. We also control for Output Area fixed effects, Local Area District by year fixed effects and year-quarter fixed effects. For DID regressions, we exploit the variation from the installation of new Pylons and include Postcode fixed effects, year-quarter fixed effects and a same set of controls on housing characteristics (See above). For more information on the definition of the control variables, refer to Table A1. Tails denote 95% confidence intervals from standard errors clustered at postcode level. Sales between 1800 and 2000 meters from the nearest pylon act as baseline group for comparison. For details on the reported estimated effects and sample sizes, refer to Table A2 in data appendix.

from cross-sectional regressions reported in Table A3, both the size and direction of the estimated effects across bandwidths are unaffected by the inclusion of more control variables. We also show the effects of distance to the overhead power lines in Panel B, for comparison with the effects of distance to pylons in Panel A. Given pylons are closely spaced, around 300m apart on average, we would not expect big differences between these estimates, and the results from our preferred DID

specifications for pylons and power lines are comparable. Given this similarity, We focus the rest of our analysis on proximity to pylons which are the more salient visual features.

5.3 Robustness tests and event study

In this section, we discuss a range of tests to assess whether the estimates reported above in 4 are causal. Given we found no significant effects beyond 1500m, we simplify the analysis to consider two distance bands 0-750m and 750-1500m, with 1500m to 2000m as the omitted baseline group.

A potential threat to our identification strategy is that property sales that took place after pylons are constructed are different from those made earlier. For instance, if housing units of inferior quality (e.g older, not well-maintained) are sold after the pylons are installed, we could overestimate the causal impacts on prices, holding housing quality constant. To address this concern, we conduct a battery of balancing tests by estimating a specification similar to equation 2, but replacing the dependent variable with various housing characteristics. We report estimates separately for sales from 0 to 750 meters and from 750 to 1500 meters, with sales beyond 1500 meters acting as the reference group for comparison. Results are summarized in Table 2. We control for postcode and year-quarter fixed effects in all regressions. If the composition of houses did not change after the overhead power line is constructed, we expect coefficients across the board to be statistically insignificant.

Looking across Table 2, we detect some changes in the composition of housing transactions after the construction of pylons. Specifically, properties 0 to 750m from power lines are more likely detached houses than houses of other types. Sales after pylons are constructed are less likely to be new builds, which suggests a possible effect on supply. Properties sold from 750 to 1500m have more heated rooms and are less likely to have solid walls (i.e., more likely to have cavity walls). Otherwise, differences are small and non-significant. All our regressions control for these property characteristics, and are insensitive to their inclusion, so these changes in composition do not threaten the interpretation of our findings.

Table 3 presents estimates from several additional robustness tests. Column 1 presents baseline results for comparison at 0 to 750 meters and at 750 to 1500 meters from the nearest pylon for comparison. Overall, we observe that house prices are around 6.5% lower within 750m from the pylons, and around 3.5% lower between 750 and 1500m from the pylons after they are installed. These results are also quite comparable to the negative house price effects around other renewable energy infrastructure such as wind farms (around 5% within 2km) reported by Gibbons (2015) and solar panels (up to 3.6% within 1km) reported by Gaur and Lang (2023) that have adopted similar quasi-experimental empirical strategies.

		Table	2: Balancing	Tests of Housing Chara	cteristics		
	(1)	(2)	(3)	(4)	(5)	(9)	(2)
	New	Size	Detach	Flats	Semi-D	Rooms	Rooms(Heat)
$Pylon^{0-750m}$	-0.109^{**}	-0.237	0.008	-0.012^{**}	0.003	0.027	0.158^{*}
	(0.049)	(1.187)	(0.014)	(0.005)	(0.014)	(0.045)	(0.087)
$\mathrm{Pylon}^{750-1500\mathrm{m}}$	-0.001	0.900	0.001	-0.005	0.005	0.019	0.064
	(0.022)	(0.678)	(0.007)	(0.003)	(0.00)	(0.025)	(0.039)
Observations	73703	73703	73703	73703	73703	73703	73703
R2	0.45	0.64	0.58	0.73	0.49	0.60	0.51
	(8)	(6)	(10)	(11)	(12)	(13)	
	Terrace	Freehold	Fire Place	Energy Consump.	Ext. Count	Solid Walls	
$Pylon^{0-750m}$	0.000	0.013	-0.025	0.189	0.002	-0.015	
	(0.013)	(0.00)	(0.016)	(0.430)	(0.033)	(0.017)	
$\mathrm{Pylon}^{750-1500\mathrm{m}}$	-0.001	0.004	-0.003	-0.174	0.025	-0.006	
	(0.007)	(0.006)	(0.008)	(0.259)	(0.017)	(0.007)	
Observations	73703	73703	73703	73703	73703	73703	
R2	0.58	0.72	0.40	0.53	0.42	0.64	
Dependent variable	is as deno	ted in the co	olumn heade	r. Key variable of inte	erest is a bina	y variable tha	it takes the value
of 1 if property is w	ithin 0 to 7	⁷ 50m & 750	to 1500m fr	om the nearest pylon	after it is con	structed. Prop	erties more than
1500 but within 200	00m from J	pylon act as	baseline grc	up for comparison.	For all regress	sions, we con	trol for postcode
fixed effects and yes	ar-quarter 1	fixed effects	. Standard ei	rrors, clustered at pos	stcode level, a	re reported in	parenthesis. ***,
** and * denote signi	ificance lev	vel at 1%, 5	% & 10% re	spectively.			

Table 3: Robustness Tests							
	(1)	(2)	(3)	(4)	(5)	(6)	
	Baseline	LAD Year-Trends	MSOA Year-Trends	Loc*Year	Land Use*Year	Weighted	
Pylon ^{0-750m}	-0.065***	-0.049***	-0.058***	-0.063***	-0.063***	-0.094***	
	(0.011)	(0.011)	(0.012)	(0.012)	(0.012)	(0.014)	
$\operatorname{Pylon^{750-1500m}}$	-0.035***	-0.032***	-0.033***	-0.022**	-0.032***	-0.059***	
	(0.009)	(0.008)	(0.008)	(0.009)	(0.009)	(0.012)	
Observations	73703	73601	73601	73703	73698	73703	
R2	0.88	0.88	0.88	0.88	0.88	0.87	
Mean Dep Variable	170707.18	170829.81	170829.81	170707.18	170709.97	170707.18	
No.of Postcodes	4169	4158	4158	4169	4168	4169	
	(7)	(8)	(9)	(10)	(11)	(12)	
	<=1800m	Exclude	-5,+5 years	Rem Outliers	Bad controls	1st Diff	
Pylon ^{0-750m}	-0.065***	-0.052***	-0.055***	-0.059***	-0.068***	-0.066***	
	(0.013)	(0.012)	(0.013)	(0.011)	(0.012)	(0.012)	
$\operatorname{Pylon}^{750-1500\mathrm{m}}$	-0.032***	-0.033***	-0.023**	-0.034***	-0.029***	-0.015*	
	(0.010)	(0.009)	(0.009)	(0.008)	(0.008)	(0.008)	
Observations	56602	64563	29524	72203	71706	7642	
R2	0.87	0.88	0.88	0.89	0.88	0.94	
Mean Dep Variable	172216.52	174661.08	163428.01	167723.90	170705.48	173499.55	
No.of Postcodes	3302	3569	3379	4138	4138	3821	

Dependent variable is the natural logarithm of sale prices. Results from Column 1 is from our baseline estimation. Key variable of interest is a binary variable that takes the value of 1 if property is within 0 to 750m & 750 to 1500m from the nearest pylon after it is constructed.

In Column 2 and 3, we control for LAD year trends and MSOA year trends respectively.

In Column 4, we control for location characteristics-by-year fixed effects.

In Column 5, we control for land use-by-year fixed effects.

In Column 6, we weight our estimates inversely based on the land area of postcodes. More emphasis is placed on smaller postcodes that we can more accurately measure distance to nearest pylons.

In Column 7, we restrict the sample to property sales less than 1800 meters from the nearest pylon.

In Column 8, we exclude any sales that are within 1000 meters from substations, solar panels and wind farms.

In Column 9, we restrict our analysis to sales that were made 5 years before and after the pylon is constructed.

In Column 10, we remove top and bottom 1% of the transactions in sale prices to negate the influence of outliers.

In Column 11, we include a vector of time-varying neighborhood controls collected at output area level that could potentially be influenced by pylon installation.

In Column 12, we construct a two period data, before and after pylon construction, from sales not more than 5 years before and after the year the pylon is constructed. We then estimate a first-difference model that controls for postcode fixed effects, a binary variable denoting post construction period, housing characteristics.

All regressions include postcode fixed effects, year-quarter fixed effects, controls on housing characteristics unless otherwise specified. For more information on the definition of the control variables, refer to Table A1. Sample is constrained to properties no further than 2000 meters from the nearest pylon other than Columns 6 and 7. Standard errors, clustered at postcode level, are reported in parenthesis. ***, ** & * denote significance level at 1%, 5% & 10% respectively.

Firstly, in column 2-5 we allow for separate time trends, at different geographical levels or according to initial area characteristics. Columns 2 and 3 include controls for Local Authority District (LAD) and MSOA level trends respectively. In column 4 we control for interactions between fixed local characteristics (distance to rail stations, waterways and green space, as in our cross-sectional regressions) and year dummies. In column 5 we control for interactions between land use categories and year dummies. All these modifications make little substantive difference to the results.

One limitation we face is that we do not have the coordinates of the exact addresses in our dataset. Instead the distance to pylons are measured from postcode centroids, determined by the

average coordinates of a group of addresses that shares the same postcode. One concern is whether the distances to pylons are inaccurately measured for larger postcodes. Hence, in column 6 we weight our regressions using the inverse of the postcode land area, to place more weight on smaller area postcodes. These weighted estimates are larger in magnitude than our baseline estimates, though it is impossible to say whether this is because we are measuring distances more accurately, or whether the effects on price are heterogeneous and larger in denser urban areas with smaller postcodes.

In column 7, we restrict the sample of transactions to those within 1800m rather than the 2000m used in our main estimates to see whether our choice of buffer is important. These estimates should be interpreted in reference to price changes for sales between 1500 and 1800m. While this reduces the sample to 56,602 sales from 3302 postcodes, it is comforting to record estimates that are not statistically different from baseline findings. Price effects range from 2.3 to 3.4%.

In column 8, we exclude any properties that are within 1000 meters from any substations, wind farms and solar panels to allay concerns that our estimates could be biased by impacts from this new infrastructure. We obtain the locations of solar panels and wind farms from the Renewable Energy Planning Database provided by Department for Energy Security and Net Zero from the UK government¹⁵. Removing these sales reduces our sample by about 12.5%, from 73,703 to 64,563 sales, but our estimates are similar to before, suggesting that the estimates are due to the new pylons, not other associated energy infrastructure.

In column 9, we restrict the sample to a shorter 5-year window around pylon construction dates to minimize the risk from unobserved temporal shocks around the time of treatment that could influence housing prices (e.g construction of transport infrastructure, shopping malls). Doing so reduces the sample to 29,524 sales but we document stable effects of around 2.3 to 5.5%.

In column 10, we check for the influence of outliers by truncating the top and bottom 1% of the transactions in sale prices. While this reduces the sample size by 1,500 observations, we do not report any discernible change compared to our baseline estimates.

In column 11, we control for time-varying neighborhood variables, which include unemployment rate, home ownership rates, education levels etc. collected at an output area level. While these are useful controls if the changes in characteristics are exogenous, they are potentially endogenous given that households could sort themselves across space in response to price changes. Either way, we find their inclusion does not change the key results.

In column 12, we tackle the concerns that our estimates could be driven by weighting issues from the different treatment timing for different subgroups in a difference-in-difference estimation with two-way fixed effects. As highlighted by De Chaisemartin and d'Haultfoeuille (2020);

¹⁵For more information, one can refer to https://www.gov.uk/government/publications/ renewable-energy-planning-database-monthly-extract.

Goodman-Bacon (2021), these estimates are a weighted average of the estimated effects from different subgroups and the weight for each group is determined by the variance of treatment, which is affected by timing of treatment. Groups that are treated earlier or later could receive smaller or even negative weights that could bias the estimates. We allay these concerns by estimating a first-difference model that involves limiting sales to within 5 years from the construction years of pylon, before collapsing the panel data into two periods (before and after pylon construction) at a postcode level. Although the first-difference specification is less efficient, our results remain robust as we continue to document pronounced price discounts of around 1.5 to 6.6% after the pylons are constructed.¹⁶

Figure 5: Event Study Regression of the effect of proximity to pylons on housing values before and after year of installation



Reported estimates are the annual price effects for sales up to 1500m from pylon from DID regressions before and after pylon installations. Similar to earlier specifications, we restrict our analysis to sales $\langle = 2000 \rangle$ meters from the nearest pylon. Omitted group includes sales made 1 year before the construction of pylon. Empirical specification include postcode fixed effects, year-quarter fixed effects and controls on housing characteristics. For more information on the definition of the control variables, refer to Table A1. Tails denote 95% confidence intervals from standard errors clustered at postcode level.

¹⁶We also conducted our analysis using the estimator from De Chaisemartin and d'Haultfoeuille (2020) to allay concerns that negative weights from some subgroups could bias our estimates. Due to space constraints, we report these results in Table A5. They are quite consistent with our main findings. For more information, refer to data appendix.

In Figure 5 we explicitly test for pre-existing differential trends in property prices between places close to pylons (within 1500m) and those further away (between 1500 and 2000m). Here, we are concerned that home owners might anticipate the construction of pylons and respond beforehand, or that there are pre-existing differences in price trends which might indicate some unobserved confounders. The figure plots the coefficients from an event study regression with coefficients and confidence intervals for estimated effects in each year before and after pylon installation. The coefficients can be interpreted as annual changes in housing values relative to properties sold one year before the pylons are constructed. These results suggest that pre-trends and anticipation effects are unlikely to bias earlier estimates. This is evidenced by the lack of house price effects are likely to occur after one year, and the discount ranges between 3.0 and 5.6%. A plausible explanation for the absence of house price effects in year 0 is that we only have the year of installation for the pylon, but not the exact date. Hence, it is possible that some of the sales that we classified as "treated" could be sold before the pylon is installed, explaining the absence of house price effects around the year of installation.¹⁷

5.4 Mechanisms: visibility and transmission voltage

In this section, we consider whether the disamentity of pylons is driven by their visual impacts, or by impacts of electro-magnetic fields and other effects related to transmission voltage.

We assess pylon visibility by combining the height and location of the pylons with a Geographic Information System (GIS) digital elevation model to generate 'viewsheds' on 200m grid to measure visibility.¹⁸ These viewsheds are used to differentiate residential postcodes (geographical units with approximately 17 houses) into those from which the pylons is visible, and those from which it is less likely they are visible. Where pylon height is missing, we set the height of pylons to be 40 meters. ¹⁹. If anything, this approximated height is likely to overestimate the height of pylons and hence the visibility of them from residential postcodes. For a visualization of these viewsheds, refer to Figure 7. The location of the pylon is denoted in a larger dot while postcodes

¹⁷One concern is whether classifying these pre-installation sales as "treated" could bias our estimates. Therefore, we repeat the analysis removing the sales made in year 0. Doing so reduces our sample size marginally from 73703 to 70417 but yields similar results to before. See Figure A2 of the data appendix.

¹⁸GB SRTM Digital Elevation Model 90m, based on the NASA Shuttle Radar Digital Topography Mission and available from the EDINA ShareGeo service http://www.sharegeo.ac.uk/handle/10672/ 5

¹⁹This height is approximated based on information from http://www.emfs.info/sources/ overhead/ohl-calculating/geometries/ It is estimated that a more modern design for new pylons typically takes the height of 40 meters, with a ground clearance of 12 meters and earth wire of around 28 meters.

are represented by smaller dots. Darker shaded rasters denote areas with a clear view, while those in lighter shaded rasters denote areas without a clear view of the pylon.

To estimate the impact of visibility, we include interactions between this binary visibility indicator and the distance band dummies indicating proximity to new pylons. This is a difference-in-difference-in-difference empirical strategy, comparing price changes for those properties near power lines with and without a view of a pylon. Given that these pylons are tall, they are visible from all properties within 600m. Hence, we redefine our treatment variables to group properties at 1000m intervals from pylons and extended our analysis to properties up to 5000m. ²⁰ In short, for each distance interval, we estimate the effects separately for properties with and without a clear view of the nearest pylon and we report these results in figure 6.



Figure 6: Effect of pylon visibility on housing values from DID regressions

Reported estimates are the reported house price effects at 1000m intervals from pylons for properties with and without a clear view of pylons from DID regressions. Empirical specification include Postcode fixed effects, year-quarter fixed effects and controls on housing characteristics. For more information on the definition of the control variables, refer to Table A1. Tails denote 95% confidence intervals from standard errors clustered at postcode level. Sales between 4000 and 5000 meters from the nearest pylon act as baseline group for comparison.

These results show no statistically different effects on between properties with and without a view of a pylon within 1000m. This could stem from the small number of sales without a view of pylons: only around 2% of sales within 1000m. This proportion increases to 9.4%, 21.6% and 19.2% once we consider sales between 1000 and 2000m, 2000 and 3000m, 3000 and 4000m. An

²⁰In reality, it is unlikely that pylons remain visible to the naked eye at 5000m. Here, we are measuring potential visibility based on topography around pylons.

interesting finding that emerges is that properties between 1000 and 2000m appear to be affected by pylon visibility as they are sold at a discount of 2.3% after the pylon is constructed while properties without a view appear to be unaffected by pylons. Most of the estimated effects are too imprecise and small to be statistically different from zero beyond 2000m from the pylons.



Figure 7: Pylon visibility for Postcodes within 5000 meters

Figure 8: Effect of proximity to pylons of different transmission voltages (≤ 275 kV and 400kV) on housing values from DID regressions



Reported estimates are the reported house price effects at 750m intervals (0-750m, 750-1500m) for pylons of different transmission voltages (<=275kV and 400kV) from DID regressions. Empirical specification include Postcode fixed effects, year-quarter fixed effects and controls on housing characteristics. For more information on the definition of the control variables, refer to Table A1. Tails denote 95% confidence intervals from standard errors clustered at postcode level. Sales between 1500 and 2000 meters from the nearest pylon act as baseline group for comparison.

Next, we examine whether the voltage of the transmission lines could affect the house price impact of pylons. These estimates are plotted in Figure 8. Higher voltages are more likely to generate concerns about health effects from electromagnetic fields and noise pollution from electrical discharge. Specifically, we allow our estimates to vary for power lines of different voltage. The empirical setup is similar to our visibility analysis but we now interact distance dummies with transmission voltage dummies. Most of these pylons managed by National Grid carry high voltages of either 275kV or 400kV, with some carrying 132kV. More than 78.7% of our transactions are near 400kV pylons, with around 18.8% near 275kV pylons and 2.5% around 132kV pylons. Therefore, we group sales near 132kV pylons with those near pylons carrying 275kV lines. Although the estimated effects for 400kV transmission lines induce statistically larger house price effects than 275kV transmission lines, suggesting that both are perceived as equally harmful. A plausible reason is because 275kV and 400kV lines appear very similar to each other because they are carried by similar pylons. The size of the pylons is determined by safety, topographical, operational and environmental reasons, rather by the voltage of transmission lines (Nationalgrid, 2008).

6 Discussion and Conclusions

In this paper, we provide new quantitative estimates of the costs to households associated with living near overhead high-voltage power lines. We estimate these costs from the impact of new pylon construction on local housing prices in England and Wales. Our findings suggest that overhead power lines depress housing prices within 1500 meters from power lines by around 3.9%, which is around £6,657 in absolute value (in 2015 values). These findings remain robust across a variety of specifications that relaxes various identification assumptions.

If the estimates in this paper are taken seriously as the mean willingness to pay to avoid the negative externalities of overhead power lines, the implied costs are quite substantial. According to census estimates in 2011, there are around 4.0 million dwellings within 1500 meters from an overhead power line. This means that the total implied impact of existing power lines on home owners is more than £26 billion (2015 values) given that the average decrease in house values within 1500 meters is around \pounds 6,657.

Our estimates have important implications, given the need to run new lines to connect to new sources of renewable energy. The figures are relevant in determining compensation payments to households affected by new transmission lines and in evaluating the benefits of running cables underground. Our results imply compensation figures that are larger than currently applied in the UK. As of now, only home owners whose properties are infringed by power lines are compensated, but our analysis shows that the pylons imply costs on households that extend over a much larger area. A rough estimate of the average number of households within 1500 meters of a 1km stretch of transmission line in England is 425 (an area of 2.5 km squared, multiplied by the average household density of 170 per km squared). Based on our estimates, the cost to these households from the environmental impacts of 1km of overhead lines is around £2.8 million. One alternative to providing compensation to households, is to bury cables underground. In the UK, the construction and maintenance cost associated running electricity lines overhead was £2.2-4.2 million per km over their lifetime compared to £10.2-24.1 million per km for burying them underground in 2012, towards the end of our study period (IET, 2012). The direct construction cost difference is therefore around £8-20 million per km. On average then, the benefits to households of burying the cables is way below the additional construction costs. Unfortunately, this analysis cannot answer the question of the value of burying cables in places of great natural beauty, where the visual damage may well far exceed the mean cost to residential households which we provide in this study. Neither can it answer the question of whether a strategic offshore, underwater ring main - as proposed by some organisations - would be a cost effective solution, given the costs and feasibility of such a project are still very uncertain. It is worth noting though that the costs of compensating households for the construction of land-based pylons could become very large. Compensating just 10% of the 28 million households in the UK for pylons within 1500m would cost £24 billion in 2024 prices. So a strategic infrastructure project to route power lines offshore could become a cost effective solution.

References

- Atkinson, Giles, Brett Day, Susana Mourato, and Charles Palmer, "'Amenity' or 'eyesore'? Negative willingness to pay for options to replace electricity transmission towers," *Applied Economics Letters*, 2004, *11* (4), 203–208.
- Banzhaf, H.Spencer, "Difference-in-differences hedonics," *Journal of Political Economy*, 2021, *129* (8), 2385–2414.
- **Baris, D and B Armstrong**, "Suicide among electric utility workers in England and Wales.," *British journal of industrial medicine*, 1990, 47 (11), 788.
- Brown, Paul, "New cancer link to power lines," https://www.theguardian.com/uk/2000/sep/21/paulbrown 2000.
- **Callaway, Brantly and Pedro HC SantAnna**, "Difference-in-differences with multiple time periods," *Journal of Econometrics*, 2020.
- Chaisemartin, Clement De and Xavier d'Haultfoeuille, "Two-way fixed effects estimators with heterogeneous treatment effects," *American Economic Review*, 2020, *110* (9), 2964–96.
- Chay, Kenneth Y and Michael Greenstone, "Does air quality matter? Evidence from the housing market," *Journal of Political Economy*, 2005, *113* (2), 376–424.
- **Colwell, Peter**, "Power lines and land value," *Journal of Real Estate Research*, 1990, 5 (1), 117–127.
- C.S, Morton R.D. & Carrasco L. & McShane G & O Neil A.W. & Wood C.M. Rowland, "Land Cover Map 2015 (vector, GB)," *NERC Environmental Information Data Centre*, 2017.
- **Davis, Lucas W**, "The effect of health risk on housing values: Evidence from a cancer cluster," *American Economic Review*, 2004, *94* (5), 1693–1704.
- **Delaney, Charles and Douglas Timmons**, "High voltage power lines: do they affect residential property value?," *Journal of Real Estate Research*, 1992, 7 (3), 315–329.
- **Dröes, Martijn I and Hans RA Koster**, "Renewable energy and negative externalities: The effect of wind turbines on house prices," *Journal of Urban Economics*, 2016, *96*, 121–141.
- _ and _, "Wind turbines, solar farms, and house prices," *Energy Policy*, 2021, 155, 112327.

- Elliott, Paul, Gavin Shaddick, Margaret Douglass, Kees de Hoogh, David J Briggs, and Mireille B Toledano, "Adult cancers near high-voltage overhead power lines," *Epidemiology*, 2013, pp. 184–190.
- Feychting, Maria and Anders Ahlbom, "Magnetic fields, leukemia, and central nervous system tumors in Swedish adults residing near high-voltage power lines," *Epidemiology*, 1994, pp. 501– 509.
- and Michael Alhbom, "Magnetic fields and cancer in children residing near Swedish highvoltage power lines," *American journal of epidemiology*, 1993, *138* (7), 467–481.
- **François, Rosiers**, "Power lines, visual encumbrance and house values: a microspatial approach to impact measurement," *Journal of Real Estate Research*, 2002, *23* (3), 275–302.
- Gaur, Vasundhara and Corey Lang, "House of the rising sun: The effect of utility-scale solar arrays on housing prices," *Energy Economics*, 2023, *122*, 106699.
- **Gibbons, Steve**, "The costs of urban property crime," *The Economic Journal*, 2004, *114* (499), F441–F463.
- _, "Gone with the wind: Valuing the visual impacts of wind turbines through house prices," *Journal of Environmental Economics and Management*, 2015, 72, 177–196.
- Goodman-Bacon, Andrew, "Difference-in-differences with variation in treatment timing," *Journal of Econometrics*, 2021.
- Hamilton, Stanley W and Gregory M Schwann, "Do high voltage electric transmission lines affect property value?," *Land Economics*, 1995, pp. 436–444.
- Hicks, Jan, "Pylons: Controversial Giants on the Landscape," Science and Industry Museum Blog http://https://blog.scienceandindustrymuseum.org.uk/ pylons-controversial-giants-in-the-landscape/ 2018.
- **IET**, "Electricity Transmission Costing Study," https://www.theiet.org/media/ 9376/electricity-transmission-costing-study.pdf 2012.
- Kuminoff, Nicolai V, Christopher F Parmeter, and Jaren C Pope, "Which hedonic models can we trust to recover the marginal willingness to pay for environmental amenities?," *Journal of Environmental Economics and Management*, 2010, *60* (3), 145–160.
- Milman, Oliver, "America's big shift to green energy has a woolly mammoth problem," https://www.theguardian.com/environment/2023/may/22/ america-green-energy-obstacles-fossils 2023.

- Nationalgrid, "Development near overhead lines: Planning and amenity aspects of high voltage electricity transmission lines and substations," Downloaded from https://www.nationalgrid.com/sites/default/files/documents/ Development%20near%20overhead%20lines_0.pdf 2008.
- Northern Echo, "Overhead power line protestors to step up campaign," https://www.thenorthernecho.co.uk/news/6973328. overhead-power-line-protestors-step-campaign/2004.
- Ordnance Survey, "Strategi," Downloaded from EDINA Digimap Ordnance Survey Service http://digimap.edina.ac.uk 2015.
- _, "OS Open Greenspace," Downloaded from EDINA Digimap Ordnance Survey Service http: //digimap.edina.ac.uk 2018.
- _, "OS Open Rivers," Downloaded from EDINA Digimap Ordnance Survey Service http:// digimap.edina.ac.uk 2018.
- **Priestley, Thomas and Gary W Evans**, "Resident perceptions of a nearby electric transmission line," *Journal of Environmental Psychology*, 1996, *16* (1), 65–74.
- **Rosen, Sherwin**, "Hedonic prices and implicit markets: product differentiation in pure competition," *Journal of Political Economy*, 1974, 82 (1), 34–55.
- Savitz, David A, Harvey Checkoway, and Dana P Loomis, "Magnetic field exposure and neurodegenerative disease mortality among electric utility workers.," *Epidemiology (Cambridge, Mass.)*, 1998, 9 (4), 398–404.
- Seddon, Paul, "Give cash to households in path of new pylons, government urged," BBC News https://www.bbc.co.uk/news/uk-politics-66397256 2023.
- Sims, Sally and Peter Dent, "High-voltage overhead power lines and property values: a residential study in the UK," *Urban Studies*, 2005, *42* (4), 665–694.
- Sobel, Eugene, Zoreh Davanipour, Raimo Sulkava, Timo Erkinjuntti, Juhani Wikstrom, Victor W Henderson, Galen Buckwalter, Joseph D Bowman, and Pey-Jiuan Lee, "Occupations with exposure to electromagnetic fields: a possible risk factor for Alzheimer's disease," *American Journal of Epidemiology*, 1995, 142 (5), 515–524.
- **Sorahan, Tom and Linda Nichols**, "Mortality from cardiovascular disease in relation to magnetic field exposure: findings from a study of UK electricity generation and transmission workers, 1973–1997," *American journal of industrial medicine*, 2004, *45* (1), 93–102.

- Tang, Cheng Keat, "The Cost of Traffic: Evidence from the London Congestion Charge," *Journal* of Urban Economics, 2021, 121, 103302.
- _ and Thao Le, "Crime risk and housing values: Evidence from the gun offender registry," Journal of Urban Economics, 2023, 134, 103526.
- **Thomas, Charles, Gerd Welke et al.**, "The Effect of HVTLs on Property Values: An Event Study," *International Real Estate Review*, 2017, *20*, 167–187.
- Thomson, Alice, "We all profit if beauty comes before pylons," http://https://www.thetimes.co.uk/article/ we-all-profit-if-beauty-comes-before-pylons-m5ks2qml89n 2014.
- Vidal, John, "Burying electricity power lines 'cheaper than National Grid claims'," https://www.theguardian.com/environment/2012/jan/31/ burying-electric-pylons-cheaper-government 2012.
- Wertheimer, Nancy and ED Leeper, "Electrical wiring configurations and childhood cancer," *American journal of epidemiology*, 1979, *109* (3), 273–284.
- Winser, Nick, "Electricity Network Commissioner's principle areas of recommendation," https://www.gov.uk/government/publications/ accelerating-electricity-transmission-network-deployment-electricity-netw 2023.

Data Appendix

CHENG KEAT TANG

STEVE GIBBONS

The appendix reports auxiliary details and analysis to the main paper. We first provide more details of the data used in the empirical analysis. We then report additional results that either correspond to the figures we produced in our main analysis or provide robustness checks to our main findings.

Description of Variables

Variable	Source	Description
Dependent Variable		•
Sale Price	Land Registry	Transacted price of property
Ln Price	Land Registry	Natural logarithm of the transacted price of prop-
	8 ,	ertv
Powerline/Transmission Pyle	on Characteristics	
Overhead Cables (OHL)	National Grid	Latitude and Longitude of Transmission Lines
Pylon	National Grid	Latitude and Longitude of Pylon
Voltage	National Grid	Voltage (in KV) carried by lines
Year of Installation	National Grid	Year pylons are installed
Housing Characteristics		
Tenure	Land Registry	Binary variable = 1 if unit is Freehold or Lease-
Tenure	Duna negistry	hold
Property Type	Land Registry	Binary variable – 1 if unit is Detached Semi-
Topenty Type	Land Registry	Detached, Elat/Mansionette or Terrace House
New Build	Land Peristry	Binary variable – 1 if unit is newly build
Fire Diace	OPC	Binary variable $= 1$ if unit is newly build Binary variable $= 1$ if unit has a firmlage
Size	OPC	Size of the unit cold (in com)
Size	OPC	Size of the unit sold (in sqiff)
No. of Doorse	OPC	Number of Decreasing with
No. of Rooms	OPC	Number of Rooms in unit
No. of Heated Rooms	OPC	Number of Heated Rooms in unit
No. of Extensions	OPC	Number of extensions within unit
Wall type	OPC	Binary variable = 1 if unit has solid or cavity
Neighbourhood Characteric	tics	waiis
Population Density	Census	Number of residents in OA divided by area
Unamployment Pate	Consus	% of residents in OA who are economically ac
Onemployment Rate	Cellsus	tive but are unemployed
\mathcal{O} of long parent households	Conque	\mathcal{O} of households in OA with long parents
% of non EU residents	Census	% of noisenoids in OA with lone parents
% of non-EU residents	Census	% of residents in OA outside of European Union
% of residents without educa-	Census	% of residents in OA without any education qual-
tion qualifications	0	incations
% of residents of minority	Census	% of residents in OA of minority race
race	0	
% of social renters	Census	% of residents in OA who are social renters
Homeownership rates	Census	% of residents in OA who are home owners
Car ownership	Census	% of households in OA without cars
Location Characteristics		
Distance from nearest water-	Ordinance Survey	Dist of unit from nearest in-land waterway (m)
way		
Distance from nearest green	Ordinance Survey	Dist of unit from nearest green space (m)
space		
Distance from rail station	Ordinance Survey	Dist of unit from nearest railway station (m)
Distance from rapid station	Ordinance Survey	Dist of unit from nearest rapid station (m)
Land Use Classification	Ordinance Survey	Binary variable = 1 if unit is in Arable land,
		Grassland, Improved Grass, Heather-Bog-Rock,
		Urban, Woodland or Marsh

I I I I I I I I I I I I I I I I I I I

Cross-sectional hedonic regression results against DID results

Table A2 reports estimates corresponding to Figure 4.

Table A2:	Cross-sect	ional Hed	onic & DID	Regressions	of the	effect of	Pylons on	housing	values	across
distance										
	=			Full Sa	ample	DID	Sample	_		
				(1)		(2)			

	(1)	(2)
	Hedonic Pylon	DID Pylon
0-300m	0.003	-0.103***
	(0.009)	(0.039)
300-600m	-0.001	-0.041***
	(0.008)	(0.014)
600-900m	0.003	-0.068***
	(0.007)	(0.014)
900-1200m	0.012^{*}	-0.051***
	(0.006)	(0.013)
1200-1500m	0.009	-0.023*
	(0.005)	(0.013)
1500-1800m	0.001	-0.004
	(0.004)	(0.013)
Observations	1300756	73703
R2	0.87	0.88
Mean Dep Variable	204285.85	170707.18

Dependent variable is the natural logarithm of transacted prices. Key variable of interest reported is a binary variable that takes the value of 1 if property is within the respective distance bandwidth from the nearest Pylon. For instance, 0-300m denotes sales within 300m from the nearest Pylon. For hedonic regressions, we control for a vector of housing, neighbourhood and location characteristics, year-quarter FE, OA FE and LAD*YEAR FE, and restrict the analysis to housing transactions no more than 2000 meters from the nearest pylon. For DID regressions, we control for a vector of housing characteristics, postcode fixed effects and year-quarter fixed effects. For all regressions, sales from 1500 to 2000m from the nearest Pylon are the baseline group for comparison. Standard errors, clustered at postcode level, are reported in parenthesis. ***, ** and * denote significance level at 1%, 5% & 10% respectively.

Semi-parametric analysis

We further present graphical evidence to show how home values change after a pylon is installed. Figure A2 plots the residuals of the natural logarithm of housing transaction prices as a function of distance from the nearest pylon before (solid) and after (dash) it is installed using local polynomial regressions. We construct residuals of house prices by regressing the natural logarithm of house prices with observable housing characteristics, postcode and year quarter fixed effects. The concern is whether any disparity in housing values could be driven by differences in the quality of homes sold after the installation of pylons. We observe that house prices experienced a significant dip after pylons are installed, indicating that such infrastructures confer negative externalities that home owners pay to avoid. These externalities appear to be localized as negative effects are no longer discernible beyond 1500m. The absence of house price effects beyond this threshold also support the notion that houses far enough from newly installed pylons represent a valid control group that is unaffected by the installations.



Figure A2: House price gradient of distance from new pylons. Results from local polynomial regressions (Bandwidth: 200m, Kernel: Epanechnikov) of residuals of log sale prices on distance from nearest pylons before (solid line) and after (dashed line) the pylons are installed. Shaded areas denote 95% confidence interval.

Comparing estimates between full sample and sub-sample removing sales made in the year of installation

A data limitation we face is that we do not have the exact date of pylon installation as we only have the year of installation. This means that it is possible for a subset of properties to be classified as treated despite being sold before the pylon is installed. Grouping these sales into our treatment group could bias our estimates. To allay this concern, we exclude sales that were made in the year of installation and re-estimate our baseline results across distance analogous to the empirical specification adopted in Figure 4. We further compare these results with results from the full sample as summarized in Figure A2. Excluding sales made in year 0 reduces our sample size marginally from 73703 to 70417 but has an immaterial impact on our estimates. As observed, we do not report statistically different estimates across the different distance bands. Since the exclusion of these transactions are unlikely to make material differences from our current set of results, and it is hard to justify the exclusion because some of these sales could be made after the pylons are installed, we decide to keep these sales in our estimation.

Figure A2: Difference-In-Difference Regressions of the effect of proximity to pylons on housing values across distance at 300m intervals for full sample and sub-sample removing sales made in the year of installation.



Reported estimates are the reported house price effects at 300m intervals (0-300m, 300-600m....,1500-1800m) from DID regressions for full sample and sub-sample removing sales made in the year of installation (or year zero). We exploit the variation from the installation of new Pylons and include Postcode fixed effects, year-quarter fixed effects, and control for a vector of **housing characteristics** that include size, number of rooms, number of heated rooms, number of extensions, wall type, property type, tenure, whether the unit is new build, has a fire place, and energy consumption. For more information on the definition of the control variables, refer to Table A1. Tails denote 95% confidence intervals from standard errors clustered at postcode level. Sales between 1800 and 2000 meters from the nearest pylon act as baseline group for comparison.

Cross-sectional Hedonic Regressions

Table A3 present results from our cross-sectional hedonic regressions associated with proximity with pylons in Panel A and with overhead cables (or OHL) in Panel B. In these regressions, we compare house price changes for properties within 600m and between 600 and 1200m with house price changes for properties between 1200 and 2000m from the nearest pylon/OHL. The reported estimated effects between pylons and OHL are quite similar because the two features are quite close to one another. Our paper focuses on the externalities from pylons given that they are likely to be more salient compared to OHL. Although we document analogous results with our DID regressions in the most parsimonious cross-sectional specification without any spatial fixed effects (in column 1), these results are not robust to variations in the empirical specification. Once we control for LSOA fixed effects or OA fixed effects (from column 5 onwards), we no longer report any significant house price effects associated with these infrastructures. The sensitivity of the estimates across different cross-sectional hedonic regressions suggests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			P	anel A: Pylo	n		
${ m Pylon^{0-750m}}$	-0.032***	-0.014***	-0.007**	-0.002	0.004	0.003	0.003
	(0.004)	(0.003)	(0.003)	(0.004)	(0.005)	(0.005)	(0.005)
$\mathrm{Pylon}^{750-1500\mathrm{m}}$	0.009^{***}	0.008^{***}	0.010^{***}	0.008^{***}	0.009^{**}	0.009^{**}	0.008^{**}
	(0.003)	(0.002)	(0.002)	(0.003)	(0.004)	(0.004)	(0.004)
Observations	1300982	1300982	1300973	1300927	1300828	1300828	1300828
R2	0.73	0.82	0.84	0.84	0.86	0.86	0.86
Mean Dep Variable	204295.12	204295.12	204294.73	204291.97	204294.51	204294.51	204294.51
Estimated Effect (%)	-3.11	-1.41	-0.69	-0.20	0.38	0.34	0.32
	Panel B: OHL						
$ m OHL^{0-750m}$	-0.031***	-0.014***	-0.007**	-0.001	0.005	0.005	0.005
	(0.004)	(0.003)	(0.003)	(0.004)	(0.005)	(0.005)	(0.005)
$\mathrm{OHL}^{750-1500\mathrm{m}}$	0.009^{***}	0.008^{***}	0.010^{***}	0.008^{***}	0.010^{**}	0.009^{**}	0.009^{**}
	(0.003)	(0.002)	(0.002)	(0.003)	(0.004)	(0.004)	(0.004)
Observations	1300982	1300982	1300973	1300927	1300828	1300828	1300828
R2	0.73	0.82	0.84	0.84	0.86	0.86	0.86
Mean Dep Variable	204295.12	204295.12	204294.73	204291.97	204294.51	204294.51	204294.51
Estimated Effect (%)	-3.09	-1.39	-0.66	-0.14	0.55	0.50	0.47
No. of Postcodes	73902	73902	73893	73847	73748	73748	73748
Year-Qtr FE	1	1	1	1	1	1	1
LAD FE		\checkmark					
MSOA FE			1				
LSOA FE				1			
OA FE					\checkmark	✓	\checkmark
MSOA * Year Trends						\checkmark	
LSOA * Year Trends							1

Table A3: Cross-sectional Hedonic Regressions of the effect of proximity to Pylons (Panel A) & Overhead lines [OHL] (Panel B) (0-1200m) on housing values

Dependent variable is the natural logarithm of transacted prices. Key variables of interest include a binary variable that takes the value of 1 if property is within 0 to 750 metres , and a binary variable that takes the value of 1 if property is within 750 to 1500 metres from either pylon (Panel A) or OHL (Panel B). Properties between 1500 and 2000m act as baseline group for comparison. For all regressions, we control for a vector of **housing characteristics** that include size, number of rooms, number of heated rooms, number of extensions, wall type, property type, tenure, whether the unit is new build, has a fire place, and energy consumption, **neighbourhood characteristics** that include population size, population density, percentage of social renters, minority race residents, non-EU residents, residents without education qualifications, lone-parent households and households without cars, unemployment rate, homeownership rates collected annually at Output Area level (OA) , and **location characteristics** that include distance from nearest pylon that is not managed by National Grid and its second polynomial, distance from rail stations, railways and rapid stations, green space and waterways. Standard errors, clustered at postcode level, are reported in parenthesis. ***, ** and * denote significance level at 1%, 5% & 10% respectively.

DID Hedonic Regressions

Table A4 reports DID hedonic regressions that exploit the variation in negative externalities from newly constructed pylons in Panel A and from overhead power lines (OHL) in Panel B from 1995 to 2018. In these regressions, we benchmark house price changes for properties within 600m and between 600 and 1200m from the nearest newly constructed pylon or OHL with house price changes for properties between 1200 and 2000m. In our most parsimonious specification with only year quarter fixed effects, we document an increase in housing values for properties closest to the newly constructed pylons/OHL. These effects, however, disappear upon controlling postcode fixed effects in column 2. In particular, we report negative house price effects that ranged between 4.0 and 4.4% (2.5 and 3.1%) from newly constructed pylons (OHL). In column 3 and 4, we control for housing characteristics and location-by-year fixed effects. This did not matter much as the reported effects remain quite stable across various specifications, unlike those reported from cross-sectional hedonic regressions in Table A3.

	(1)	(2)	(3)	(4)		
	Panel A: Pylon					
$\mathrm{Pylon^{0-750m}}$	0.083^{*}	-0.072***	-0.065***	-0.063***		
	(0.050)	(0.014)	(0.011)	(0.012)		
$\mathrm{Pylon}^{750-1500\mathrm{m}}$	-0.042	-0.031***	-0.035***	-0.022**		
	(0.033)	(0.010)	(0.009)	(0.009)		
Observations	73703	73703	73703	73703		
R2	0.38	0.85	0.88	0.88		
Mean Dep Variable	170707.18	170707.18	170707.18	170707.18		
	Panel B: OHL					
$ m OHL^{0-750m}$	0.088^*	-0.037***	-0.041***	-0.033***		
	(0.048)	(0.012)	(0.011)	(0.011)		
$\mathrm{OHL}^{750-1500\mathrm{m}}$	-0.077^{**}	-0.005	-0.011	0.007		
	(0.034)	(0.010)	(0.009)	(0.009)		
Observations	73703	73703	73703	73703		
R2	0.38	0.85	0.88	0.88		
Mean Dep Variable	170707.18	170707.18	170707.18	170707.18		
Year-Qtr FE	1	1	1	1		
Postcode FE		1	1	1		
Housing			\checkmark	1		
Location*Year FE				1		

Table A4: DID Regressions of the effect of proximity to Pylon (Panel A) & Overhead line [OHL] (Panel B) on housing values

Dependent variable is the natural logarithm of transacted prices. Key variables of interest include a binary variable that takes the value of 1 if property is within 0 to 750 metres , and a binary variable that takes the value of 1 if property is within 750 to 1500 metres from either pylon (Panel A) or OHL (Panel B). Properties between 1500 and 2000m from OHL act as baseline group for comparison. For all regressions, we control for a vector of **housing characteristics** that include size, number of rooms, number of heated rooms, number of extensions, wall type, property type, tenure, whether the unit is new build, has a fire place, and energy consumption, and **locational characteristics** that include distance from nearest pylon that is not managed by National Grid and its second polynomial, distance from rail stations, railways and rapid stations, green space and waterways. Standard errors, clustered at postcode level, are reported in parenthesis. ***, ** and * denote significance level at 1%, 5% & 10% respectively.

Computing Viewsheds

We rely on Shuttle Radar Topography Mission's (SRTM) high resolution digital elevation models (DEM) at 90m resolution to determine whether each postcode has a view of the pylons. Due to computation constraints, we aggregate these height rasters to 180m by 180m resolution (aggregating four rasters into one). We restrict the analysis to postcodes not more than 5000 meters from each pylon. We then conduct the analysis one at a time for each pylon (there are in total 823 pylons) and compute whether each postcode has visibility of a pylon. As some of these pylons are considerably close to one another, it is likely that there will be overlapping postcodes within a 5000 meter buffer. We consider a particular postcode to have a view of these pylons if they are able to view at least one pylon. If a particular postcode has the view of multiple pylons, we will only consider the pylon. Figure 7 in the main text illustrates the visibility rasters constructed for a particular postcodes within 5000m from the pylon. Rasters in purple are areas that have a clear view of the electric pylons while postcodes located in grey rasters do not have a clear view of pylons.

Comparing Two-way fixed effects (TWFE) estimators

Column 1 of Table A5 reports DID hedonic regressions using TWFE estimators from De Chaisemartin and d'Haultfoeuille (2020). This is estimated using *did_multiplegt* command using *STATA*. The concern is whether DID estimate, which is a weighted sum of the average treatment effects from different subgroups, could be bias because of negative weights for certain subgroups. In columns 2 and 3, we further report analogous results from tradition TWFE estimators and from our first difference regressions. The rationale is to compare these estimators to see whether earlier estimates are comparable with the estimates from De Chaisemartin and d'Haultfoeuille (2020). In these regressions, we combine our treatment groups such that our main variable of interest is a binary variable that takes the value of 1 if a property is within 0 to 1500m from the pylon, 0 otherwise. We also aggregate our observations to postcode and year level to ensure our data is more balanced to facilitate the computation of estimators using *did_multiplegt* that is more computationally intensive. It is comforting to see that the estimated effects are quite consistent across specifications. While the DID TWFE estimates from De Chaisemartin and d'Haultfoeuille (2020) are the most conservative, it is not statistically different from our traditional TWFE and first-difference estimates.

	(1)	(2)	(3)
	did_multiplegt	TWFE	First-Difference
Pylon ^{0-1500m}	-0.021**	-0.026***	-0.023***
	(0.011)	(0.009)	(0.008)
Observations	15375	15375	7642
R2	-	0.88	0.94

Table A5: DID Regressions of the effect of proximity to Pylon on housing values: Comparing first difference, DID TWFE and DID-TWFE estimator from De Chaisemartin and d'Haultfoeuille (2020)

Dependent variable is the natural logarithm of transacted prices. Key variables of interest include a binary variable that takes the value of 1 if property is within 0 to 1500 metres. Properties between 1500 and 2000m act as baseline group for comparison. For all regressions, we control for a vector of **housing characteristics** that include size, number of rooms, number of heated rooms, number of extensions, wall type, property type, tenure, whether the unit is new build, has a fire place, and energy consumption, postcode and year-fixed effects.

In column 1, we estimate treatment effects using TWFE estimator from De Chaisemartin and d'Haultfoeuille (2020). In column 2, we use traditional TWFE regression. In column 3, we employ a first-difference estimator comparable to column (12) of Table 3. Analysis is conducted in postcode and year level for columns (1) and (2) and postcode pre-post level for columns (3). Standard errors, clustered at postcode level, are reported in parenthesis. ***, ** and * denote significance level at 1%, 5% & 10% respectively.

Standard errors for column (1) are computed from 25 bootstrap replications.