Centre for Economic Performance

Discussion Paper ISSN 2042-2695

No. 1942
August 2023

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

Cheng Keat Tang
Stephen Gibbons

THE LONDON SCHOOL OF ECONOMICS AND POLITICAL SCIENCE

Economic and Social Research Council

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 3.6% for properties up to 1200 meters away, suggesting the impacts extend further than previously estimated.

Keywords: externalities, overhead power lines, pylons, house prices, revealed preferences
JEL Codes: R32; Q48; Q51

This paper was produced as part of the Centre's Community Wellbeing Programme. The Centre for Economic Performance is financed by the Economic and Social Research Council.

We would like to thank National Grid for providing locations of power lines and the year they are constructed.

Cheng Keat Tang, Nanyang Technological University. Stephen Gibbons, London School of Economics and Centre for Economic Performance at LSE.

Published by
Centre for Economic Performance
London School of Economic and Political Science
Houghton Street
London WC2A 2AE

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 landscape. 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, wellbeing 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 neurodegenerative 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 overhead (Vi-

---

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

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

dal, 2012). 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-in-differences 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. [3]

Results from our difference-in-differences specifications show that pylons reduce prices of houses within 1200 metres, by an average of 3.6%, relative to those further away. [4] 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 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 determin-

---

[3] There 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, 2019), externalities from windfarms (Gibbons, 2015) and health risks (Davis, 2004) etc.

[4] The estimated effects are from our preferred estimate from column (4) of Table 3.

ing 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 voltagess 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 linr 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.[5]

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 households affected are relatively small.[6] 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.

---

[5] For more information on the list of groups, activists and research institutes on Electromagnetic Fields, refer to http://www.emfs.info/more/links/

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

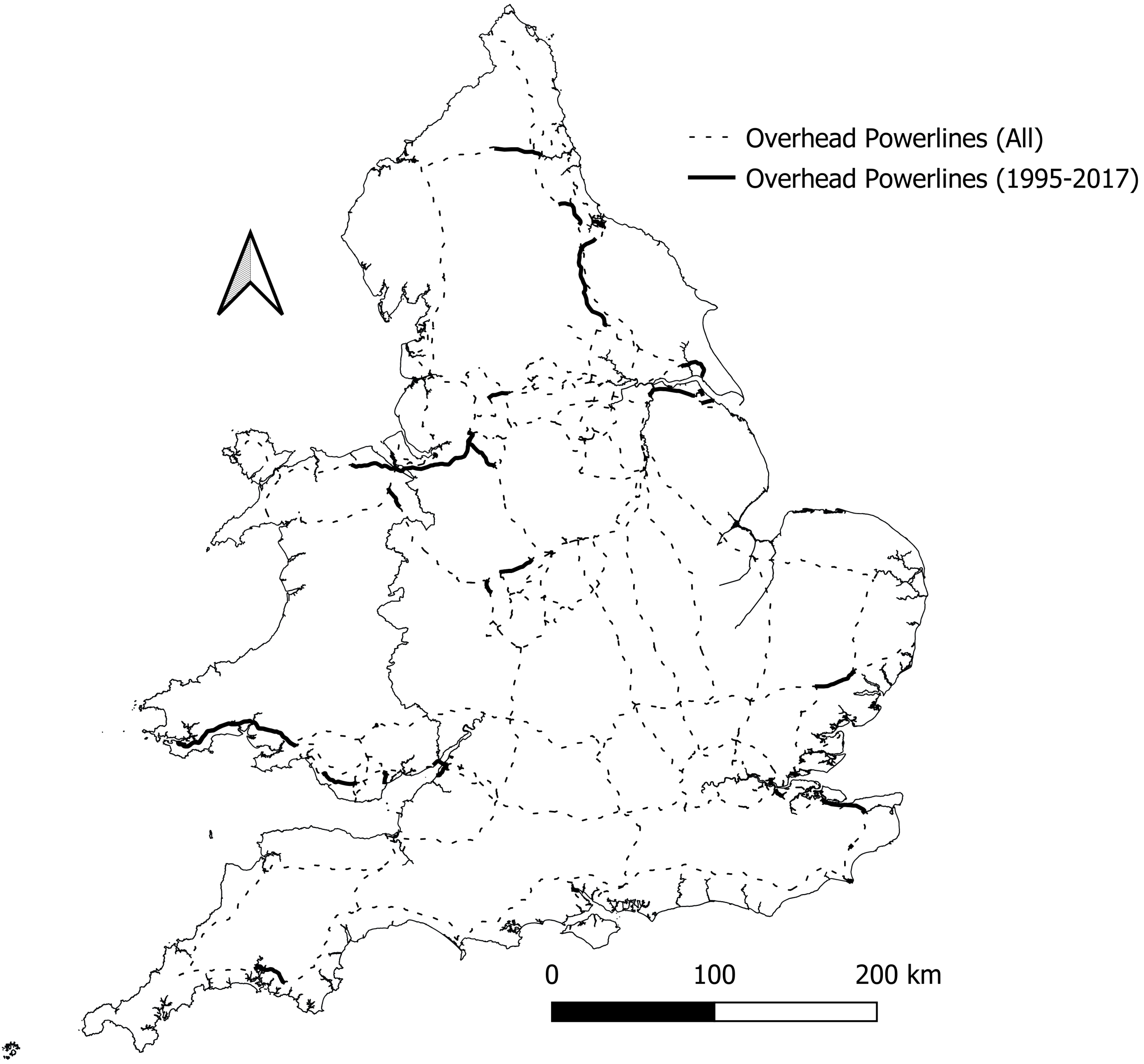


Figure 2: Overhead Power Lines across England and Wales. Map denote the OHLs managed by National Grid. Thicker dark lines denote newer OHLs constructed by National Grid from 1995 to 2017.
Source: Authors own illustration. Locations of OHLs provided by National Grid.

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.

Bulk 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. After controlling for a limited set of observable housing characteristics, including size, number of bathrooms, garage size etc, he concludes that properties closer to transmission wires are sold at a cheaper price. This negative impact reduces with distance from the power lines and over time. 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. This study also shows that transmission lines have a detrimental impact on housing values although these effects are highly localized and dissipate quickly moving away. Specifically, properties within 100 m from transmission towers are sold 5.8% cheaper (USD$6,740) while the effects for properties from 200 to 300m are smaller at 2.8% (USD $3,438). François (2002) conducted a similar analysis for 507 single family houses in Greater Montreal in Canada from 1991 to 1996, finding a 10-20% drop for properties facing the pylon. This study also examines how the WTP to avoid electric power lines changes after widely publicity on studies investigating electromagnetic fields (EMF)-induced health hazards but these findings remain inconclusive. Sims and Dent (2005) broaden the analysis to the UK and use two different approaches. First, they conducted a survey to seek real estate professionals' perception towards electric power lines and transmission towers on housing values. Their contingent valuation approach on 257 valuers and 176 agents suggests power transmission lines and towers reduce valuations by 5 to 10 %. Second, they examine the effects on prices of 620 properties in Scotland sold between 1994 to 2010. They show that proximity and a view of these pylons reduce prices by up to 21% within 250m.

Thomas et al. (2017) do more than earlier studies to address biases from unobserved confounders, by investigating the effect of new pylons for the Tehachapi Renewable Transmission Project (TRTP) intended to transmit electricity from wind farms in California. They estimate the impact of the project by comparing changes in property values for 2,569 transactions before and after 16 pylons were in constructed, although the pylons were never used and subsequently dismantled. They document significant loss of home values of between 8.3% for encumbered and 4.9% for abutting properties. Although they improve identification of effects by adopting a Difference-in-Difference 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 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 22,000 of them made within 1200m 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.

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.[7]

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.[8] 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 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 address postcode and euclidean distances from the nearest pylons and power lines are computed using Geographic Information System (GIS) software. Our main analysis focuses on the distance to pylons, because pylons are the more salient features of the landscape and because distances to pylons and lines are very highly correlated. Our dataset, which limits to properties not more than 2000m from the nearest pylon, covers more than 1.5 million property transactions from 1995 to 2017. In similar fashion, we computed euclidean distance of each postcode from smaller pylons that are not managed by National Grid and include them as control variables.

We further compute the proximity of each postcode from the nearest waterways, green space based on data from Ordinance Survey Open Rivers and Open Greenspace (Ordnance Survey, 2018a,b). These variables are used to control for natural amenities that are likely to be corre-

---

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

[8] This data linking was done for another project by colleagues at LSE.

lated 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 properties, 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:

$$Y_{ijnt} = \alpha_n + \sum_k \gamma^k Pylon_j^k + X_{jt}'\phi + N_{it}'\rho + \tau_t + \epsilon_{ijnt}, \tag{1}$$

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, $Pylon_j^k$, is a binary variable that takes the value of 1 if postcode j is within k meters from nearest pylon. The coefficients, γ^k, are interpreted as the percentage difference in sale prices between properties within k meters from pylon and properties further awayin the omitted, baseline reference group.

To minimize salient differences between properties, these cross sectional methods use a 'kitchen-sink' 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 (α_n) 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 $E[\epsilon_{ijnt}|Pylon_j^k] = 0$, 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_{ijnt}. 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 these identification challenges, we estimate a panel-based difference-in-difference (DID) specification, that identifies the effect of pylons on house prices from new pylon construction:

$$Y_{ijt} = \alpha_j + \delta Pylon_{jt} + \sum_k \beta^k Pylon_{jt}^k + X'_{jt}\phi + \tau_t + \varepsilon_{ijt}, \tag{2}$$

When estimating this regression specification, we limit the sample to sales in postcodes within a specific distance buffer of a new pylon constructed from 1995 onwards, the year from which we have data on new pylon construction. In our main specifications, we set this distance to 2km, beyond which any changes in prices are unlikely to be due to pylon construction. In this regression equation, $Pylon_{jt}$ is a binary variable taking the value 1 for all years t after the nearest pylon within 2km of postcode j is constructed, zero otherwise. $Pylon_{jt}^k$ is a binary variable taking the value of 1 for all periods t after the pylon is constructed if postcode j is within k meters from this nearest pylon, zero otherwise. The key parameter of interest β_k measures the percentage change in sale prices within k meters from pylon after it is constructed, compared to the baseline change within the 2km buffer.

This estimation method allows us to partial out time-invariant confounders related to sales in the same location by controlling for postcode fixed effects (α_j). There are, on average, only 17 housing units sharing a postcode in United Kingdom, meaning that postcode fixed effects capture most potential cross-sectional confounders. In this specification, we are identifying the effects of pylons from the changes in distance to properties that occurs as new properties are constructed. The specification also partials out changes occurring in general within 2km of new pylons, through $Pylon_{jt}$. In other words, we are comparing price changes for properties within postcode j at k meters from the pylon before and after they are installed with price changes of comparable properties further away but within our 2km boundary. Coefficient $delta$ 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 in postcodes that had a pylon constructed within 2km 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 heterogenous 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 adopted two intuitive strategies. First, we constrain our analysis to a balanced time window around the construction dates of each pylon. Figure 3 provides a simple illustration of this strategy associated with two pylons. In particular, we restrict our analysis to transactions within 5 years from construction. In a simple 2-by-2 DID setup, we will be comparing price changes for properties close to the pylon (treated) with price changes further away (control). By doing so, we ensure that the variance of treatment is similar for all pylons regardless of their construction dates. Another notable advantage of this strategy is that it reduces the risk from unobserved temporal con-founders, such as changes in neighborhood and/or housing characteristics that could occur in the long run.

Second, after restricting our data to sales within 5 years from the construction year, we collapse these transactions to two different time periods, before and after pylon construction, at postcode level. This means that we will now have only two observations per postal code (before and after pylon construction). Put differently, we are estimating the effects of pylons on housing values using a simple 2-by-2 DID setup, comparing changes in housing values before and after pylon construction for postal codes closer ($<$ 1200m) and further ($>=$ 1200m) from pylon via a first-difference model.

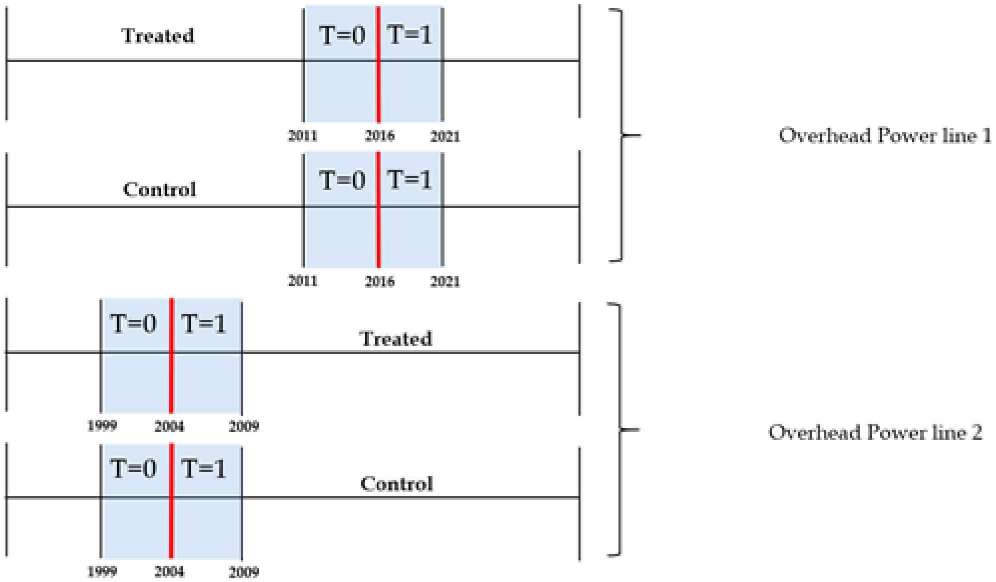


Figure 3: An illustration of the five year window surrounding two pylon construction dates. Treated denote properties that are close to newly constructed pylon while control denotes properties further away. T=1 (T=0) denotes five year window after (before) pylon construction date. Un-shaded areas are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

Although we partial out time-invariant unobservables by controlling for α_j, and time variant unobservables through $Pylon_jt$, there are still concerns regarding unobserved time-variant shocks between sales closer and further from these transmission lines that could bias our WTP estimates (β_k). For instance, inferior properties nearer to power lines could be sold after these power lines are constructed. We adopt the following strategies in our estimation to address this issue. First, we directly test for whether there are any changes to the composition of houses sold after the overhead power lines are constructed by conducting a battery of balancing test on various housing characteristics. [9] Second, we limit our analysis to properties not more than 2000 meters from overhead power lines to ensure that sales are in fairly similar neighbourhoods with different exposure to the negative externalities from power lines. Third, we allow the properties in different areas to have

---

[9] Another way is to include address fixed effects (θ_i). This is akin to estimating a repeated sales model, comparing price changes of the same property overtime. However, we are unable to do so because of the small number of repeated transactions in our sample. The inclusion of address fixed effects reduces our sample to 3485 sales from 80520 sales

their own unique price trends by interacting indicator variables for various administrative boundaries - Middle Super Output Area and Lower Super Output Area - with year trends. Finally, we 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: Descriptive Statistics for Hedonic and DID Sample

			Panel A: Housing Characteristics			
	Cross Sectional Sample			**DID Sample**		
	All(<=2km)	< 1.2km	1.2km to 2km	All(<=2km)	< 1.2km	1.2km to 2km
Sale Price (2015 values)	202542.89	199331.54	206051.09	170707.18	165644.86	172866.96
Log Price	11.60	11.60	11.61	11.48	11.46	11.49
New Builds	0.07	0.07	0.06	0.09	0.09	0.08
Size (sqm)	88.55	88.10	89.04	88.86	89.28	88.68
Detached House	0.19	0.21	0.18	0.20	0.23	0.19
Flat/Mansionette	0.14	0.13	0.14	0.08	0.06	0.09
Semi-Detached House	0.34	0.35	0.33	0.42	0.46	0.41
Terraced House	0.33	0.32	0.35	0.29	0.24	0.31
Freehold	0.74	0.75	0.73	0.81	0.83	0.80
Fireplace	0.15	0.14	0.15	0.13	0.13	0.13
Energy Consumption	59.41	59.68	59.13	60.17	60.09	60.20
Number of Rooms	4.60	4.60	4.60	4.65	4.66	4.64
Number of Rooms with Heating	4.24	4.24	4.24	4.22	4.18	4.24
Number of Extensions	0.46	0.45	0.47	0.46	0.50	0.45
Solid Walls	0.24	0.22	0.25	0.17	0.16	0.18
			Panel B: Neighbourhood Characteristics			
	Cross Sectional Sample			**DID Sample**		
	All(<=2km)	< 1.2km	1.2km to 2km	All(<=2km)	< 1.2km	1.2km to 2km
Pop Size	314.30	314.50	314.09	306.12	305.70	306.30
Pop Density	53.47	50.92	56.26	44.63	40.16	46.44
Unemployment Rate (%)	0.05	0.05	0.06	0.05	0.06	0.05
Non-white Residents (%)	0.12	0.12	0.13	0.06	0.04	0.06
Social Renters (%)	0.12	0.12	0.13	0.12	0.10	0.12
Home Owners (%)	0.74	0.76	0.73	0.77	0.81	0.75
Non-EU Residents (%)	0.08	0.07	0.08	0.03	0.02	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	0.27	0.25
Households w/o cars (%)	0.23	0.22	0.25	0.21	0.19	0.22
No. of Transactions	1372694	716667	656027	73703	22041	51662
No. of Postcodes	78054	40997	37057	4169	1311	2858
			Panel C: Number of Sales & Postcodes around power lines			
	Cross Sectional Sample			**DID Sample**		
Distance Bandwidths	Number of Sales	Postcodes		Number of Sales	Postcodes	
0-600m	256475	14817		3469	191	
600-1200m	460192	26180		18572	1120	
1200-2000m	656027	37057		51662	2858	

Means of various housing and census characteristics between properties less than 1200 from the nearest pylon and properties more than 1200m but less than 2000m from the nearest pylon for Cross Sectional sample and DID sample.

Table 1 presents summary statistics of housing (in Panel A) and neighbourhood characteristics (in Panel B) for three groups: properties within 2000m, those within 1200m and those more than 1200m 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 600m, 600 to 1200m and 1200 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 3469 sales from 191 postcodes within 600m from power lines, 18572 sales from 1120 postcodes from 600 to 1200m and 51662 sales from 2858 postcodes from 1200 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 1500m (e.g 0-300m, 300-600m .... 1200-1500m). Vertical bars on the plots denote 95% confidence intervals constructed from standard errors clustered at postcode level.

In the cross-sectional graph, estimation of the coefficients stems from comparing differences in property prices at different distances to pylons. Coefficients are interpreted as the percentage difference between mean house prices in a given distance band, compared to mean prices from 1500 to 2000m, the omitted reference group. For the cross sectional regression, we include output area (OA) fixed effects to partial time-invariant unobserved differences across space, 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).

Coefficients 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 difference-in-difference (DID) research design that compares house price changes close to pylons before and after they are constructed, with house price changes further away. Figure 4 presents these coefficients for every 300m up to 1500m 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.[10] For details on the actual estimated effects, refer to Table A4. We restrict the analysis to sales less than 2000m from power lines constructed from 1995 onwards, reducing the sample size to more than 71,000 property sales from 3966 postcodes.

In contrast to the cross-sectional regressions, these Difference-in-Difference estimates suggest that home owners do pay to avoid the negative externalities associated with new transmission lines. Properties within 0 to 300m experience a 9.6% reduction in their market value after pylons are installed.[11] These effects remain stable at around 3.8% for properties between 300 and 600m, around 6.4% for those between 600 and 900m and around 4.8% for those between 900 and 1200m. Beyond 1200m, the estimated effects are quite imprecisely estimated at 2.0%. 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 £6,460 for properties between 900 and 1200m to £16,320 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).

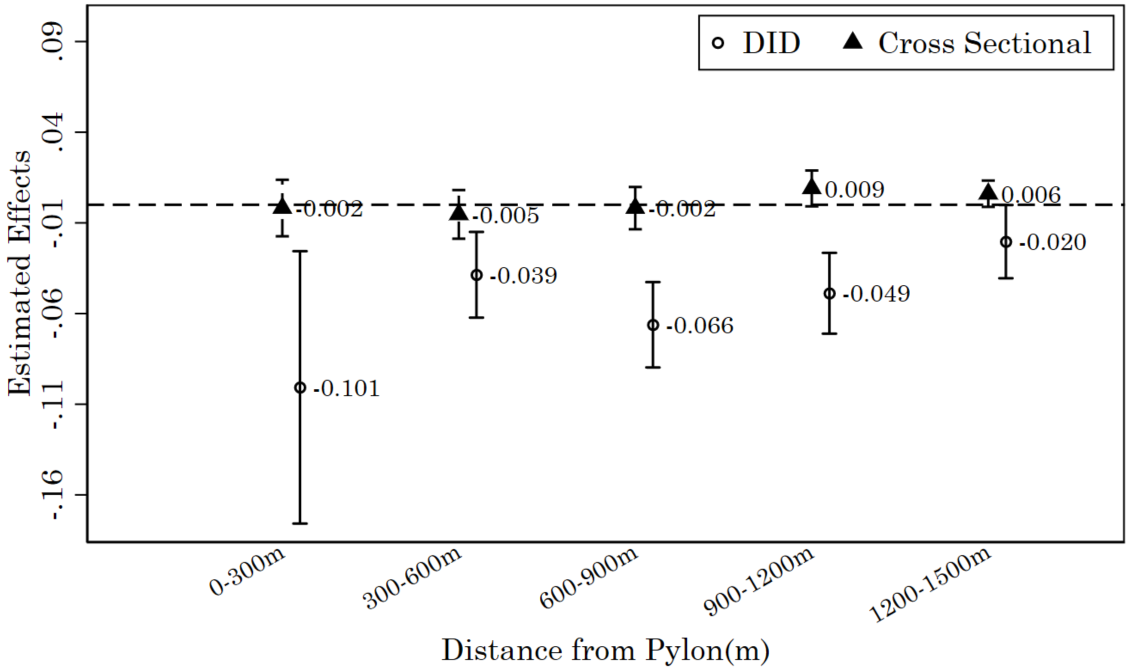
We present additional DID estimates by sequentially adding control variables into our empirical specifications to understand whether these estimates are stable across specifications. We relegate these results to Table A4 in the Data Appendix. Here, we show that, unlike the estimates from cross sectional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

---

[10] We 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.

[11] Percentage changes are computed from taking the exponential of the estimated effects, before subtracting by 1 and multiplying by 100%. For instance, $(exp(-0.101) - 1) \times 100\% = 9.61\%$.

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....,1200-1500m) 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 **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. 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 1500 and 2000 meters from the nearest OHL act as baseline group for comparison. For details on the reported estimated effects and sample sizes, refer to Table A2 in data appendix.

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

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. 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 600 meters and from 600 to 1200 meters, with sales beyond 1200 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 600m 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 600 to 1200m 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 600 meters and at 600 to 1200 meters from the nearest pylon for comparison. 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.

In column 6 we restrict the sample of transactions to those within 1500m 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 1200 and 1500m. While this reduces the sample to 38,384 sales from 2273 postcodes, it is comforting to record estimates that are not statistically different from baseline findings. Price effects range from 2.3 to 3.4%.

Column 7 restricts the sample to a shorter 5-year window around pylon construction dates to minimize the risk fromunobserved temporal shocks around the time of treatment that could influence housing prices (e.g construction of transport infrastructure, shopping malls). Doing so

Table 2: Balancing Tests of Housing Characteristics

	(1) New	(2) Size	(3) Detach	(4) Flats	(5) Semi-D	(6) Rooms	(7) Rooms(Heat)
$\mathbf{Pylon^{0-600m}}$	-0.131**	-0.924	0.037*	-0.018***	-0.035**	-0.017	0.056
	(0.065)	(1.306)	(0.020)	(0.005)	(0.015)	(0.058)	(0.116)
$\mathbf{Pylon^{600-1200m}}$	0.006	0.448	0.004	0.001	0.004	0.027	0.088**
	(0.023)	(0.782)	(0.008)	(0.003)	(0.010)	(0.028)	(0.044)
	(8) Terrace	(9) Freehold	(10) Fire Place	(11) Energy Consump.	(12) Ext. Count	(13) Solid Walls	
$\mathbf{Pylon^{0-600m}}$	0.015	0.009	-0.020	0.889	-0.026	0.006	
	(0.015)	(0.011)	(0.023)	(0.606)	(0.044)	(0.023)	
$\mathbf{Pylon^{600-1200m}}$	-0.008	0.007	-0.001	-0.021	0.005	-0.019**	
	(0.008)	(0.006)	(0.009)	(0.267)	(0.019)	(0.008)	

Dependent variable is as denoted in the column header. Key variable of interest is a binary variable that takes the value of 1 if property is within 0 to 600m & 600 to 1200m from the nearest pylon after it is constructed. Properties between 1200 and 2000m from OHL act as baseline group for comparison. For all regressions, we control for postcode fixed effects and year-quarter fixed effects. Standard errors, clustered at postcode level, are reported in parenthesis. ***, ** and * denote significance level at 1%, 5% & 10% respectively.

Table 3: Robustness Tests

	(1) Baseline	(2) LAD Year-Trends	(3) MSOA Year-Trends	(4) Loc*Year	(5) Land Use*Year
$\mathbf{Pylon^{0-600m}}$	-0.035***	-0.019*	-0.029***	-0.037***	-0.033***
	(0.011)	(0.011)	(0.011)	(0.012)	(0.011)
$\mathbf{Pylon^{600-1200m}}$	-0.049***	-0.035***	-0.037***	-0.037***	-0.047***
	(0.009)	(0.008)	(0.008)	(0.009)	(0.009)
Observations	73703	73601	73601	73703	73698
R2	0.88	0.88	0.88	0.88	0.88
Mean Dep Variable	170707.18	170829.81	170829.81	170707.18	170709.97
No.of Postcodes	4169	4158	4158	4169	4168
	(6) <=1500m	(7) -5,+5 years	(8) Rem Outliers	(9) Bad controls	(10) 1st Diff
$\mathbf{Pylon^{0-600m}}$	-0.023*	-0.034***	-0.028***	-0.037***	-0.060***
	(0.013)	(0.013)	(0.011)	(0.013)	(0.015)
$\mathbf{Pylon^{600-1200m}}$	-0.035***	-0.040***	-0.049***	-0.044***	-0.023**
	(0.011)	(0.010)	(0.008)	(0.008)	(0.009)
Observations	38384	29524	72203	71706	7642
R2	0.88	0.88	0.89	0.88	0.94
Mean Dep Variable	169079.50	163428.01	167723.90	170705.48	173499.55
No.of Postcodes	2273	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 600m & 600 to 1200m 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 restrict the sample to property sales less than 1500 meters from the nearest pylon.
In Column 7, we restrict our analysis to sales that were made 5 years before and after the pylon is constructed.
In Column 8, we remove top and bottom 1% of the transactions in sale prices to negate the influence of outliers.
In Column 9, we include a vector of time-varying neighborhood controls collected at output area level that could potentially be influenced by pylon installation.
In Column 10, 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 OHL other than Columns 6 and 7. Standard errors, clustered at postcode level, are reported in parenthesis. ***, ** & * denote significance level at 1%, 5% & 10% respectively.

reduces the sample to 29,524 sales but we document stable effects of around 3.1 to 3.9%.

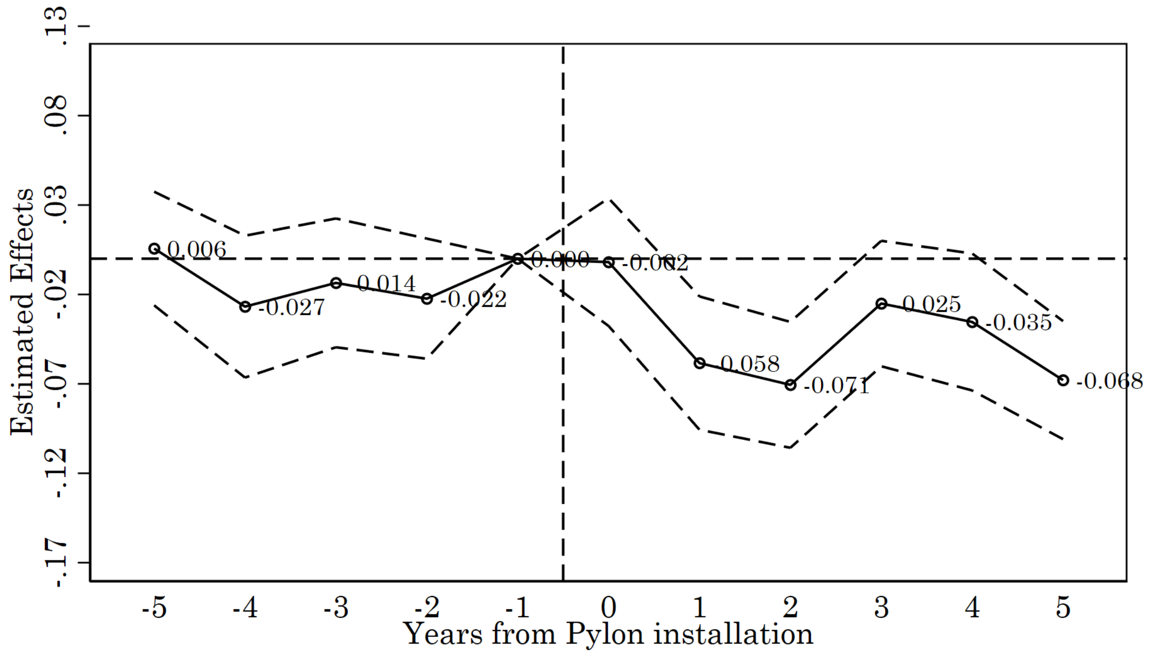
In column 8, we check for the influence of outliers by truncating the top and bottom 1% of the transactions in sale prices. While this reduces the sample size by 1,500 observations, we do not report any discernible change compared to our baseline estimates.

In column 9, 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 10, 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); 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 2.3 to 5.8% after the pylons are constructed.

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 1200m from pylon from DID regressions before and after pylon installations. Similar to earlier specifications, we restrict our analysis to sales <= 2000 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.

In 5 we explicitly test for pre-existing differential trends in property prices between places close to pylons (within 1200m) and those further away (between 1200 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 (close to zero) prior to the construction of the pylons. Our results further suggest that the effects are likely to occur after one year, and the discount ranges between 5.6 and 6.9%.

5.4 Mechanisms: visibility and transmission voltage

In this section, we consider whether the dis-amentity 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.[12] 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 to the height of pylons to be 40 meters. [13]. 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 how these viewsheds are created, refer to Figure 7. The location of the pylon is denoted in a larger dot while postcodes are represented by smaller dots. Darker shaded rasters in denote areas with a clear view, while those in lighter shaded rasters does not.

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

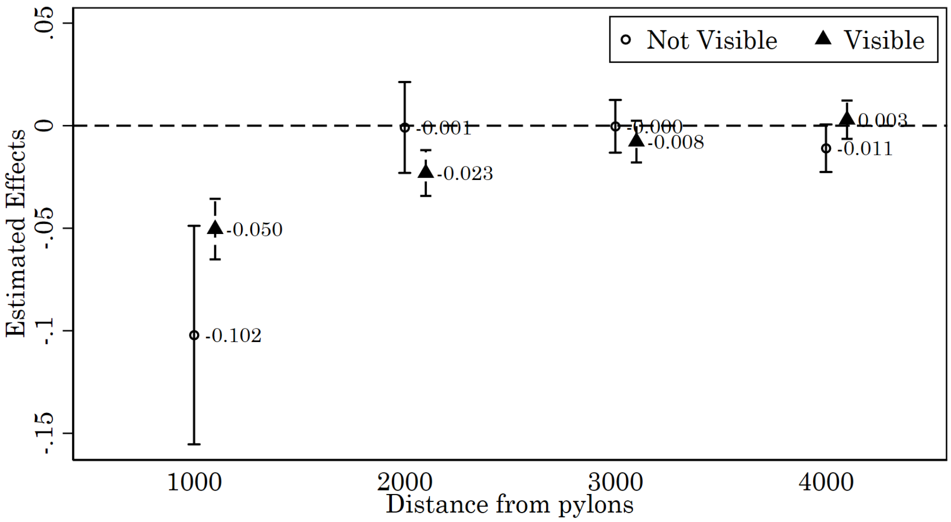
---

[12] GB 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`

[13] This 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.

1000m intervals from pylons and extended our analysis to properties up to 5000m. [14] 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 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.

---

[14] In 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.

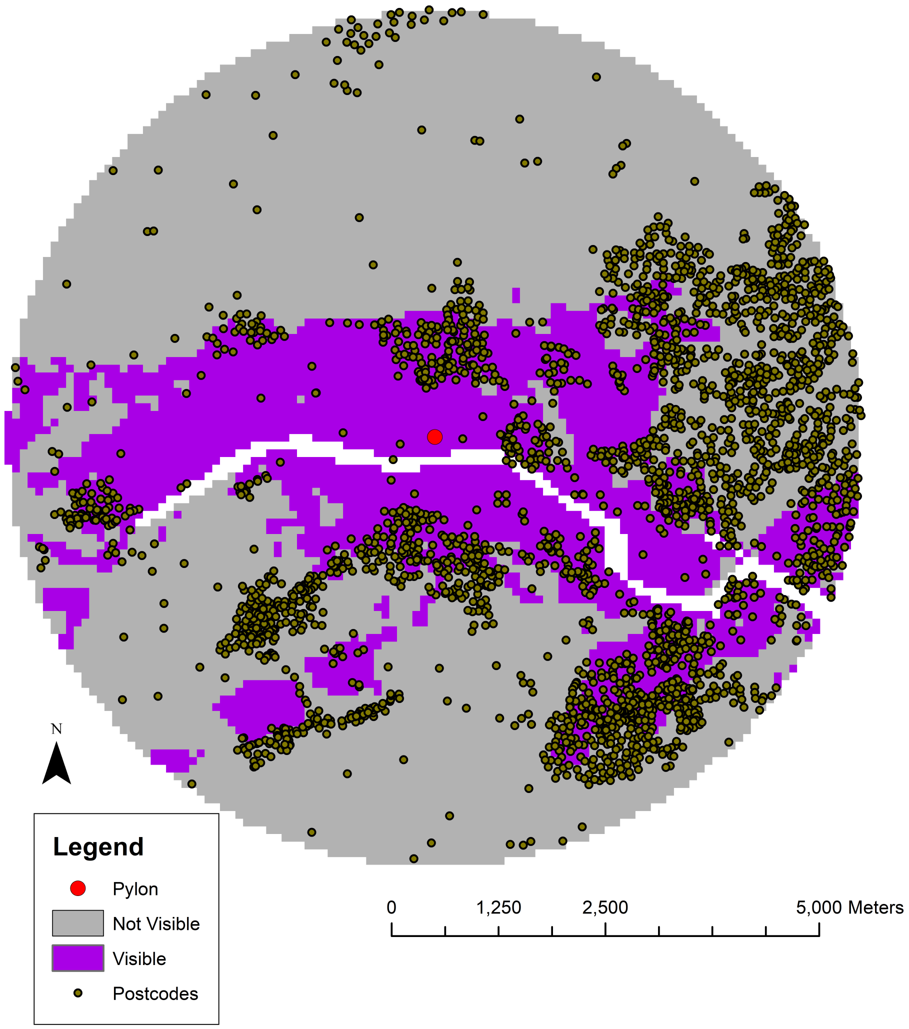
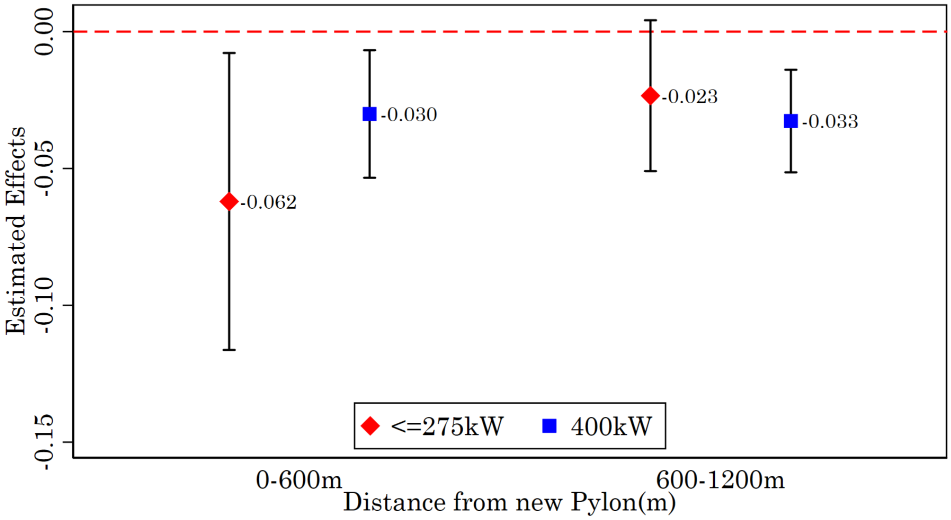


Figure 7: Pylon visibility for Postcodes within 5000 meters

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



Reported estimates are the reported house price effects at 600m intervals (0-600m, 600-1200m) 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 1200 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 WTP to avoid pylons. 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 sizes. 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. Our results, summarized in Figure 8, provide no evidence that 400kV transmission lines induce statistically larger house price effects compared to lower voltage pylons, suggesting that both are perceived as equally harmful.

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 1200 meters from power lines by around 3.6%, which is around £6,120 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 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 3.1 million dwellings within 1200 meters from an overhead power line. This means that the total implied impact of existing power lines on home owners is more than £19 billion (2015 values) given that the average decrease in house values within 1200 meters is around £6,120.

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 1200 meters of a 1km stretch of transmission line in England is 400 (an area of (2.4 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 £2.5 million. An 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 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.

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.

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 high-voltage 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.

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,” 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, “Americaâs big shift to green energy has a woolly mammoth problem,” `https://www.theguardian.com/environment/2023/may/22/america-green-energy-obstacles-fossils` 2023.

Northern Echo, “Overhead power line protestors to step up campaign,” url{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**, "The Cost of Gun Crime: Evidence from Gun Offender Registry on Housing Values," *Available at SSRN 3443631*, 2019.

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

Table A1: 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 property
Powerline/Transmission Pylon 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 Leasehold
Property Type	Land Registry	Binary variable = 1 if unit is Detached, Semi-Detached, Flat/Mansionette or Terrace House
New Build	Land Registry	Binary variable = 1 if unit is newly build
Fire Place	OPC	Binary variable = 1 if unit has a fireplace
Size	OPC	Size of the unit sold (in sqm)
Energy Consumption	OPC	Energy rating of unit sold
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 walls
Neighbourhood Characteristics		
Population Density	Census	Number of residents in OA divided by area
Unemployment Rate	Census	% of residents in OA who are economically active but are unemployed
% of lone parent households	Census	% of households in OA with lone parents
% of non-EU residents	Census	% of residents in OA outside of European Union
% of residents without education qualifications	Census	% of residents in OA without any education qualifications
% of residents of minority race	Census	% of residents in OA of minority race
% 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 waterway	Ordinance Survey	Dist of unit from nearest in-land waterway (m)
Distance from nearest green space	Ordinance Survey	Dist of unit from nearest green space (m)
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

Cross sectional hedonic regression results against DID results

Table A2 reports estimates corresponding to Figure 4.

Table A2: Cross Sectional Hedonic & DID Regressions of the effect of Pylons on housing values across distance

	Full Sample (1) Hedonic Pylon	**DID Sample** (2) DID Pylon
0-300m	-0.002	-0.101***
	(0.008)	(0.038)
300-600m	-0.005	-0.039***
	(0.007)	(0.012)
600-900m	-0.002	-0.066***
	(0.006)	(0.012)
900-1200m	0.009*	-0.049***
	(0.005)	(0.011)
1200-1500m	0.006	-0.020**
	(0.004)	(0.010)
Observations	1372592	73703
R2	0.87	0.88
Mean Dep Variable	202531.77	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.

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 2000 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

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Panel A: Pylon			
$\mathbf{Pylon^{0-600m}}$	-0.040***	-0.021***	-0.014***	-0.005	-0.001	-0.002	-0.002
	(0.004)	(0.002)	(0.003)	(0.004)	(0.005)	(0.005)	(0.005)
$\mathbf{Pylon^{600-1200m}}$	-0.009***	-0.001	0.004*	0.005*	0.003	0.003	0.002
	(0.003)	(0.002)	(0.002)	(0.003)	(0.004)	(0.004)	(0.004)
Observations	1372694	1372694	1372688	1372677	1372637	1372637	1372637
R2	0.74	0.82	0.83	0.84	0.86	0.86	0.86
Mean Dep Variable	202542.89	202542.89	202542.43	202539.55	202538.88	202538.88	202538.88
Estimated Effect (%)	-3.95	-2.09	-1.39	-0.46	-0.10	-0.17	-0.21
				Panel B: OHL			
$\mathbf{OHL^{0-600m}}$	-0.040***	-0.021***	-0.014***	-0.005	-0.001	-0.001	-0.002
	(0.004)	(0.003)	(0.003)	(0.004)	(0.005)	(0.005)	(0.005)
$\mathbf{OHL^{600-1200m}}$	-0.009***	-0.002	0.003	0.003	0.001	0.000	0.000
	(0.003)	(0.002)	(0.002)	(0.003)	(0.004)	(0.004)	(0.004)
Observations	1372694	1372694	1372688	1372677	1372637	1372637	1372637
R2	0.74	0.82	0.83	0.84	0.86	0.86	0.86
Mean Dep Variable	202542.89	202542.89	202542.43	202539.55	202538.88	202538.88	202538.88
Estimated Effect (%)	-3.94	-2.08	-1.38	-0.46	-0.07	-0.14	-0.18
No. of Postcodes	78054	78054	78048	78037	77997	77997	77997
Year-Qtr FE	✓	✓	✓	✓	✓	✓	✓
LAD FE		✓					
MSOA FE			✓				
LSOA FE				✓			
OA FE					✓	✓	✓
MSOA * Year Trends						✓	
LSOA * Year Trends							✓

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 600 metres , and a binary variable that takes the value of 1 if property is within 600 to 1200 metres from either pylon (Panel A) or OHL (Panel B). Properties between 1200 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 **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.

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.

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

	(1)	(2)	(3)	(4)
		Panel A: Pylon		
$\mathbf{Pylon^{0-600m}}$	0.089	-0.040***	-0.035***	-0.037***
	(0.059)	(0.014)	(0.011)	(0.012)
$\mathbf{Pylon^{600-1200m}}$	-0.011	-0.044***	-0.049***	-0.037***
	(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		
$\mathbf{OHL^{0-600m}}$	0.136**	-0.025*	-0.026**	-0.026**
	(0.058)	(0.013)	(0.012)	(0.013)
$\mathbf{OHL^{600-1200m}}$	-0.004	-0.031***	-0.048***	-0.032***
	(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
Year-Qtr FE	✓	✓	✓	✓
Postcode FE		✓	✓	✓
Housing			✓	✓
Location*Year FE				✓

Dependent variable is the natural logarithm of transacted prices. Key variable of interest is a binary variable that takes the value of 1 if property is within 1000 metres from either pylon (Panel A) or OHL (Panel B). Properties between 1200 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 closest to the property. We will then compute the distance of each postcode from the nearest pylon. Figure 7 in the main text illustrates the visibility rasters constructed for a particular pylon in .The location of the pylon is depicted in the red dot while smaller green dots denote the different 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.

CENTRE FOR ECONOMIC PERFORMANCE
Recent Discussion Papers

1941	Luis Bauluz Pawel Bukowski Mark Fransham Annie Seong Lee Margarita López Forero Filip Novokmet Sébastien Breau Neil Lee Clément Malgouyres Moritz Schularick Gregory Verdugo	Spatial wage inequality in North America and Western Europe: Changes between and within local labour markets 1975-2019
1940	Felipe Carozzi Edward Pinchbeck Luca Repetto	Scars of war: The legacy of WW1 deaths on civic capital and combat motivation
1939	Christos Genakos Costas Roumanias Tommaso Valletti	Is having an expert “friend” enough? An analysis of consumer switching behavior in mobile telephony
1938	Andrew E. Clark Maria Cotofan	Are the upwardly mobile more left-wing?
1937	Priya Manwaring Tanner Regan	Public disclosure and tax compliance: Evidence from Uganda
1936	Philippe Aghion Celine Antonin Luc Paluskiewicz David Stromberg Raphael Wargon Karolina Westin	Does Chinese research hinge on US co-authors? Evidence from the China initiative
1935	Nicholas Bloom Steven J. Davis Stephen Hansen Peter Lambert Raffaella Sadun Bledi Taska	Remote work across jobs, companies and space

1934	Jonathan Colmer Jennifer L. Doleac	Access to guns in the heat of the moment: More restrictive gun laws mitigate the effect of temperature on violence
1933	Ralf Martin Dennis Verhoeven	Knowledge spillovers from clean innovation. A tradeoff between growth and climate?
1932	Christian Krekel George MacKerron	Back to Edgeworth? Estimating the value of time using hedonic experiences
1931	Ernest Dal Bó Karolina Hutkova Lukas Leucht Noam Yuchtman	Dissecting the sinews of power: International trade and the rise of Britain's fiscal- military state, 1689-1823
1930	Nikhil Datta	The measure of monopsony: The labour supply elasticity to the firm and its constituents
1929	Nicholas Bloom Philip Bunn Paul Mizen Pawel Smietanka Gregory Thwaites	The impact of COVID-19 on productivity
1928	Diego Battiston Stephan Maurer Andrei Potlogea José V. Rodriguez Mora	The dynamics of the 'Great Gatsby Curve' and a look at the curve during the Great Gatsby era
1927	Enrico Berkes Davide M. Coluccia Gaia Dossi Mara P. Squicciarini	Dealing with adversity: Religiosity or science? Evidence from the great influenza pandemic
1926	Jose Maria Barrero Nicholas Bloom Steven J. Davis Brent H. Meyer Emil Mihaylov	The shift to remote work lessens wage-growth pressure

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