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Estimating the Impact of the Minimum Energy Efficiency Standard on Property Prices

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Ben Lockwood (Head of the Department of Economics, University of Warwick) and Michael Ward (Head of the Department of Economics, Monash University)

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Housing Externalities

Estimating the Impact of the Minimum Energy Efficiency Standard on Property Prices

Eleni Sandi*

Abstract

The Minimum Energy Efficiency Standard (MEES) aims to improve the energy efficiency of privately rented properties in England and Wales. Previous literature identifies this policy intervention as a driver of transition risk as it devalues sub-standard real estate. This paper reveals that MEES also devalues neighbouring houses meant to be unaffected by the policy, i.e. above-standard properties. The study leverages a dataset that combines energy efficiency and transaction data at the postcode level to capture this spatial externality. A concentration measure for sub-standard properties within a neighbourhood is constructed, which is applied to aggregate and property level analyses using a difference-in-difference specification. The aggregate analysis reveals that an incremental increase in the concentration of sub-standard housing within a postcode sector after introducing the standard leads to a 20.1% decrease in aggregate prices for above-standard houses. A repeated sales regression run on property-level data finds that an increase in concentration leads to a more plausible 4.03% decrease in prices for above-standard properties. These results imply potential problems for homeowners who may find themselves in negative equity due to the aggregate price drop, which may also negatively impact their pro-environmental investments.

JEL Classifications: C43; Q54; Q58; R31

Keywords: Climate Policy; Transition Risk; House Prices; Concentration Measure

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To view appendices please click here (including full regression tables, robustness checks and spatial graphs).

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1 Introduction

Several seminal papers find that location-specific factors significantly determine housing prices (Li and Brown, 1980). Proximity to parks, accessibility to schools and workplaces, and the aesthetics and quality of nearby houses fundamentally impact a property's valuation. For instance, one resident's negligence of their house maintenance may depress house prices in the surrounding neighbourhood without compensating other homeowners for their property's devaluation (Rossi-Hansberg and Sarte, 2012). These exogenous non-market interactions between neighbourhoods and individual property prices have been called housing market externalities or spillovers.

The housing market literature has extensively explored different types of spatial externalities. The current study contributes to this body of literature by focusing on housing spillovers caused by properties subject to climate change transition risk - a potential externality driver left unexplored by the literature. Climate change transition risk refers to the reassessment of asset values triggered by the transition to a more sustainable economy, i.e. environmental policies or regulatory changes. This study aims to assess whether property price fluctuations due to environmental regulatory changes spill over to properties initially unaffected by the policy change.

The paper explores this question in the context of the 2018 Minimum Energy Efficiency Standard (MEES) in England and Wales. Building energy efficiency standards impose specifications on properties leading to increased retrofit costs and impacting housing valuations. A recent paper by Ferentinos et al. (2022) quantifies this impact by finding that the introduction of MEES led to a devaluation of £5,000 - £9,000 in houses below the energy efficiency threshold. The current paper builds on these findings by estimating the impact of this devaluation on neighbouring houses that are above the energy standard.

This study's motivation stems not only from its academic contribution but also from its policy relevance. Housing externalities give a reason for government intervention because they lead to equilibrium allocations that are not Pareto efficient (Rossi-Hansberg et al., 2010). Crucially, the correct intervention form depends on the externality's nature and direction. Estimating the direction is especially relevant in this context since there are two plausible yet contradicting hypotheses for the direction of the potential MEES externality.

Under the first hypothesis, a high concentration of sub-standard housing in a neighbourhood leads to a negative externality. Specifically, the property devaluation of sub-standard housing would attract lower-income residents into the area. Such residents may have a lower demand for neighbourhood amenities or be unable to invest in regular property maintenance. As a result, the image of the surrounding area may change, depressing prices for neighbouring properties that were not directly affected by MEES.

The second hypothesis is a positive externality caused by the concentration of sub-standard housing. In this case, the more sub-standard houses in an area, the larger the supply shock for above-standard houses as they become relatively scarcer. This supply shock drives up prices for above-standard houses in a neighbourhood.

This paper leverages a novel panel dataset that combines transaction prices with Energy Performance Certificate (EPC) information at the postcode level to test the above hypotheses. It uses this data to construct the concentration measure for sub-standard houses within a neighbourhood. The chosen measure is a simple ratio defined by the number of sub-standard houses divided by the total number of houses in a neighbourhood. Notably, the concentration metric is applied to the stock of houses up to but not including 2018 to avoid endogeneity problems.

Building on Campbell et al. (2011), the concentration measure is a continuous treatment variable in the aggregate neighbourhood-level analysis, which employs a difference-in-difference approach. However, the validity of this identification strategy relies on the parallel trends assumption, whereby the control and treatment group would have followed the same trend even in the absence of the treatment. Running three different diagnostic exercises revealed that the assumption may not confidently hold. This result prompted a more conservative repeated sales approach in the spirit of Gerardi et al. (2012) and Harding et al. (2009).

The subsequent sections survey the existing literature (1.1) and policy background (1.2), summarise the property-level (2.1) and aggregate-level data (2.2), present the concentration metric (3), report the difference-in-difference (4.1), parallel trends test (4.2) and repeated sales results (4.3), explore robustness (5) and provide concluding remarks (6).

1.1 Literature Review

Empirical economic literature has assessed the direction and magnitude of housing market externalities in various contexts. Examples include identifying the spatial spillovers from urban revitalisation programs (Rossi-Hansberg et al., 2010) and housing subsidies, such as mortgage assistance programs (Di et al., 2010).

After the 2007 - 2010 US housing crisis, a significant portion of the housing externality literature has focused on the spillovers caused by residential foreclosures in the US. As foreclosure externalities cover a large segment of the literature, it will inform the approach of the current paper.

Typically, the empirical model of choice in the housing market externality literature is a hedonic price regression[1] of the following form:

$$log(Price)_{it} = \alpha + \beta_1 F_{it} + \beta_2 X_{it} + \epsilon_{it} \tag{1}$$

The dependent variable in the above expression is the log of transaction price for property i at year t. In the foreclosure context, the main explanatory variable F_{it} is the number of foreclosed properties within a certain distance (usually measured in miles) from property i at year t. The consensus among foreclosure studies is that β_1 is negative, meaning the higher the concentration of foreclosures within a specific distance from property i, the lower the price of house i. Most papers also include a set of hedonic controls X_{it}, to address characteristics that might affect property prices and increase the likelihood of foreclosure.

However, authors have argued that equation (1) does not adequately control the heterogeneity between properties that may impact their valuation. This limitation stems from the controls only capturing the house characteristics available in the dataset, which may be narrow. For instance, there may not be data on whether the property is south or north facing or if it has river or seaside views. Therefore, multiple papers (e.g., Gerardi et al., 2012 and Harding et al., 2009) use a repeated-sales approach, which only utilises observations for properties that are transacted before and after the treatment. This method compares the same property over time, allowing the authors to control for all property characteristics that remain constant during the chosen time frame. Gerardi et al.

[1] Hedonic price models are used to identify the price/demand of a commodity by decomposing it into the estimates of characteristics that affect it (Herath and Maier, 2010).

(2012) further augment the repeated sales approach model by adding area fixed effects at the Census Tract Group (CTG) level.

The literature highlights the importance of the size of a neighbourhood or locality in detecting spatial externalities. In the foreclosure literature, papers have differing views on whether externalities are aggregate or localised. For instance, Campbell et al. (2011) use ZIP code level transaction data to identify aggregate spillovers. This analysis reveals that foreclosures have little predictive power over ZIP code level prices. The authors then use a difference-in-difference approach on a property-level dataset to identify localised spillovers. In this case, they find that foreclosures reduce the prices of houses within a 0.1 - 0.25-mile radius of a foreclosed property.

However, Campbell et al. (2011) suffer from the limitation that the parallel trends assumption may not hold. In the absence of foreclosures, the prices in the control group, i.e., houses beyond 0.25 miles, could be trending differently to the treatment group, i.e., houses within 0.25 miles. Recent papers provide more reliable estimates than Campbell et al. (2011). For instance, Mian et al. (2015) use the exogenous variation in legal foreclosure laws across states to tackle identification problems and find evidence in favour of ZIP code level foreclosure spillovers. Favara and Giannetti (2017) find similar results and argue that ZIP codes are the most aggregate spatial area in which they can identify housing externalities.

Studies using policy changes as sources of housing externalities are relatively sparse in the literature, as Rossi-Hansberg et al. (2010) note. Among the few policy-oriented papers, environmental policies have yet to be addressed. This paper seeks to fill this gap in the literature by exploring the spillover effects of MEES on the prices of properties that should have been unaffected by the policy. The study builds on Ferentinos et al. (2022), who find that MEES leads to a £5,000 - £9,000 drop in valuation for carbon-intensive properties. Their identification strategy was to employ a difference-in-difference specification with a repeated-sales dataset.

Given the environmental policy dimension of this paper, it also contributes to a segment of the literature that explores the impact of environmental characteristics on housing prices. According to Herath and Maier (2010), hedonic pricing models have been extensively used to assess how environmental amenities and clean air impact property values. The same econometric concerns of unobserved heterogeneity carry over to this part of the literature. For instance, concerning the impact of air pollution, Chay and Greenstone (2005) argue that the hedonic pricing literature suffers from omitted variable bias. They show that the inclusion of fixed effects can largely impact the magnitude and direction of estimates, i.e., changing signs from positive to negative.

The existence of policy-oriented papers is scarce in this literature segment as well. Exceptions include Casado et al. (2017), who address the impact of the East Sussex and Brighton and Hove Waste Local Plan - a spatial planning policy minimising the distance between waste and its treatment, which ultimately led to waste incinerators being placed close to residential areas. The authors compare the prices of houses in postcode sectors containing incinerators with those in nearby sectors at different distances (Casado et al., 2017). They find that the presence of incinerators causes a 0.4 - 1.3% decrease in the mean prices of houses within a sector.

1.2 Policy Background

The Minimum Energy Efficiency Standard (MEES) was introduced in England and Wales on the 1st of April 2018. This legal standard is part of the Energy Efficiency (Private Rented Property) (England and Wales) Regulations 2015, which aim to improve the energy efficiency of properties (UK Statutory Instruments, 2015). The Regulations apply to privately rented domestic and non-domestic properties with specific tenancy agreements[2] and that are legally obliged to report an EPC rating (Department for Business, Energy & Industrial Strategy, 2021). Under the MEES 2018 standard, the minimum energy efficiency for privately rented properties is set to band E[3]. Landlords of properties with sub-standard EPC, i.e. F and G, are restricted from renewing existing tenancies or granting new ones (UK Statutory Instruments, 2015). Additionally, from 1st of April 2020, landlords are no longer able to continue letting properties covered by the Regulations with sub-standard EPC. For below threshold properties, the landlord is expected to undertake energy efficiency improvements to meet the minimum band E or otherwise register for a legal exemption.

Legal exclusions from MEES include 'devaluation', 'high cost', 'all improvements made' and 'consent' exemptions. Specifically, properties whose energy efficiency improvements would lead to a 5 percent devaluation of the property's market value or where the cheapest energy efficiency improvement option is more expensive than £3,500 are exempt from the standard. Also, where all recommended energy efficiency improvements have been installed but the property remains below band E, landlords do not have to adhere to MEES. An exemption for 5 years may also apply if the energy efficiency improvement requires third party consent e.g. local authority or mortgage lenders (Department for Business, Energy & Industrial Strategy, 2019).

Enforcement of the Regulations is undertaken by local authorities. In the event of non-compliance with MEES, the authorities may decide on a penalty for the responsible landlord. The maximum total fine reaches £5,000 (Department for Business, Energy & Industrial Strategy, 2021).

[2]Assured, regulated and domestic agricultural tenancies.
[3]E starts from 39 on a 1-100 EPC scale.

2 Data

This study uses a novel dataset that merges postcode-level transactions from the Land Registry's Price Paid Data with energy efficiency information from Energy Performance Certificates (EPC) (UCL, 2022). The data covers England and Wales during the period 1995 - 2021.

A key advantage of this dataset is the granularity of housing prices at the property level, which allows flexibility for both property and aggregate-level analyses. Additionally, the extensive time period provides multiple observations in pre- and post-intervention periods. However, a limitation is the restricted selection of property-level characteristics. Previous papers have controlled for detailed features, such as a garage or terrace, in their hedonic price regressions, which is not possible with the given dataset.

Although house prices from listing data are preferable for maintaining the exogeneity of price variation compared to transaction data, most papers use the latter[4]. This is due to restrictions on accessing listing data prices.

2.1 Property Level Data

Data cleaning was applied to the property-level dataset. Firstly, all observations prior to 2010 were removed as they were too dated to be of value to the analysis[5]. The time window 2010 – 2021 was chosen as it included multiple years before and after the 2018 intervention.

Furthermore, transaction datasets commonly include mistakes, e.g. houses sold for £1. Such reporting mistakes were removed at the property level following Ferentinos et al. (2022) and Campbell et al. (2011) by excluding properties with prices below the 1st and above the 99th percentile. Similarly, the total floor area variable (m^2) included improbably low values, e.g. zero. Hence, any observations below the 1st percentile were replaced with missing values. Additionally, where zero rooms were registered for a property, this was replaced with a missing value, as done by Campbell et al. (2011). After data cleaning, the total number of observations at property level is 8,035,254.

New variables were also created to act as property characteristic controls. The original "property type" variable was categorical, where each type was indicated by a letter e.g., D = detached. This variable was split into 4 dummy variables[6], one for each property type. Similarly, the original categorical "duration" variable was split into 2 dummies indicating whether the property is freehold or leasehold. Additionally, the "occurrence" variable was constructed, which captures the number of times the same property is transacted within the dataset. Also, an "EPC" variable was created, which captures the EPC ratings for above-standard properties only.

Table 1 provides summary statistics for the property level variables (i=property and t=year). The price variable refers to prices of above-standard houses, i.e., houses unaffected by MEES with energy rating E and above[7]. The resulting minimum house price is £50,000, and the maximum is £1,210,000. However, graphing the distribution of prices revealed a strong leftward skew (Figure 8 Appendix A). This common characteristic of housing

[4]Listing data is considered to be better because of the exogeneity in the timing of new listings (Anenberg and Kung, 2014).

[5]EPC is only available from 2008 onward.

[6]Property types: detached, semi-detached, terraced, flat/maisonette.

[7]The price variable for energy efficient properties has 405,602 missing values as this was the number of sub-standard properties in the sample.

prices is customarily corrected by log-transforming the price variable to follow a normal distribution (Figure 9 Appendix A). Notably, the property with the largest number of rooms, 112, is a family farm with postcode SA19 8YU, sold in 2016.

Table 1: Property Level Variables - Summary Statistics

Variable	Obs.	Mean	S. Dev	Min	Max
Price_{it}	7,629,652	253,180.9	168,084.6	50,000	1,210,000
$\log(\text{Price})_{it}$	7,629,652	12.26	0.59	10.82	14.01
Floor area_{it}	7,950,558	94.16	43.47	35	10,815.25
No. rooms_{it}	6,841,418	4.66	1.64	1	112
Detached_{it}	8,035,254	0.25	0.43	0	1
$\text{Semi-detached}_{it}$	8,035,254	0.29	0.45	0	1
Flat_{it}	8,035,254	0.18	0.38	0	1
Terraced_{it}	8,035,254	0.28	0.45	0	1
Leasehold_{it}	8,035,254	0.22	0.42	0	1
Freehold_{it}	8,035,254	0.18	0.38	0	1
EPC_{it}	7,629,652	65.47	11.58	39	347

Table 2 shows the share of properties by characteristic in the cleaned sample. The most transacted properties by type are semi-detached houses (28.81%), followed by terraced houses (28.48%). Moreover, the most transacted properties are freehold (77.57%) rather than leasehold (22.43%). Concerning the composition of properties in the sample by number of rooms, transactions of 1-room properties (i.e., studios) are the minority (0.27%), whilst the majority of sales are for 5-room houses (27.62%).

Table 2: Property Level Variables - Characteristics

Variable	No.	Share (% of sample)
Detached	2,023,869	25.19
Terraced	2,288,320	28.48
Semi	2,315,324	28.81
Flat	1,407,741	17.52
Freehold	6,233,274	77.57
Leasehold	1,801,980	22.43
Number of rooms		
1 room	18,625	0.27
2 rooms	364,154	5.32
3 rooms	1,238,251	18.1
4 rooms	1,688,795	24.68
5 rooms	1,889,554	27.62
6 rooms	824,605	12.05

Table 3 presents a decomposition of the EPC for all properties in the dataset. This variable is used to determine which properties are below or above the standard (band E being the minimum threshold). Most transacted properties in the sample have an EPC within band D (40.96%). Overall, the properties below the regulatory standard are 5.04% of the sample, with the rest being above-standard housing.

Table 3: EPC - Summary Statistics

EPC band	Numerical	Obs.	Share (% of sample)
A	92+	10,752	0.13
B	81-91	1,035,145	12.88
C	69-80	1,937,463	24.11
D	55-68	3,291,315	40.96
E	39-54	1,354,977	16.86
F	21-38	323,176	4.02
G	1-20	82,425	1.02

2.2 Aggregate Data

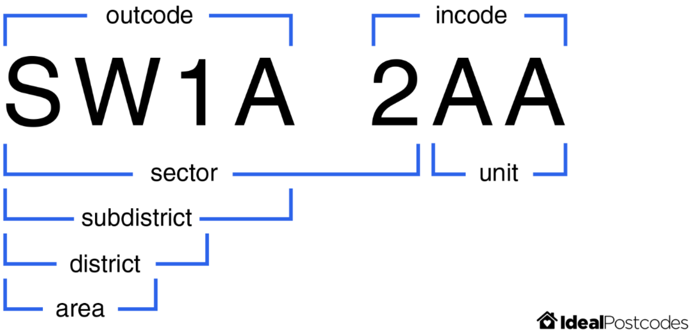
Past papers have used different aggregation levels to capture spatial externalities.

Most papers conduct property-level analyses, whereby the spillover effect of each treated property is estimated. Studies assessing the spillovers caused by foreclosed properties find that the strength of the externality decreases as the distance from the foreclosure increases.

Other papers conduct neighbourhood-level analyses. Various aggregation levels have been used to define the neighbourhood cluster within which the externality is expected to be captured. These studies highlight that if the area defining a neighbourhood is too broad, the magnitude of the externality could be underestimated or not detected. As both property- and aggregate-level analyses are prevalent in the literature, this study will conduct both analyses.

The housing externality literature is dominated by US studies, which use US spatial aggregation measures, e.g. Census Tract Group (CTG). Others, however, argue that the ZIP code level is the largest area in which externalities can be detected (Favara and Giannetti (2017))[8]. However, translating ZIP code areas into a spatial measure suitable for England and Wales is more complex. This is because the structure of ZIP codes in the US and postcodes in the UK differ. Namely, ZIP codes refer to larger areas that include multiple blocks. On the other hand, postcodes refer to specific buildings. The difference lies in the last two units of a UK postcode, which specify the building unit, as is shown in Figure 1 below.

Figure 1: UK Postcode Decomposition

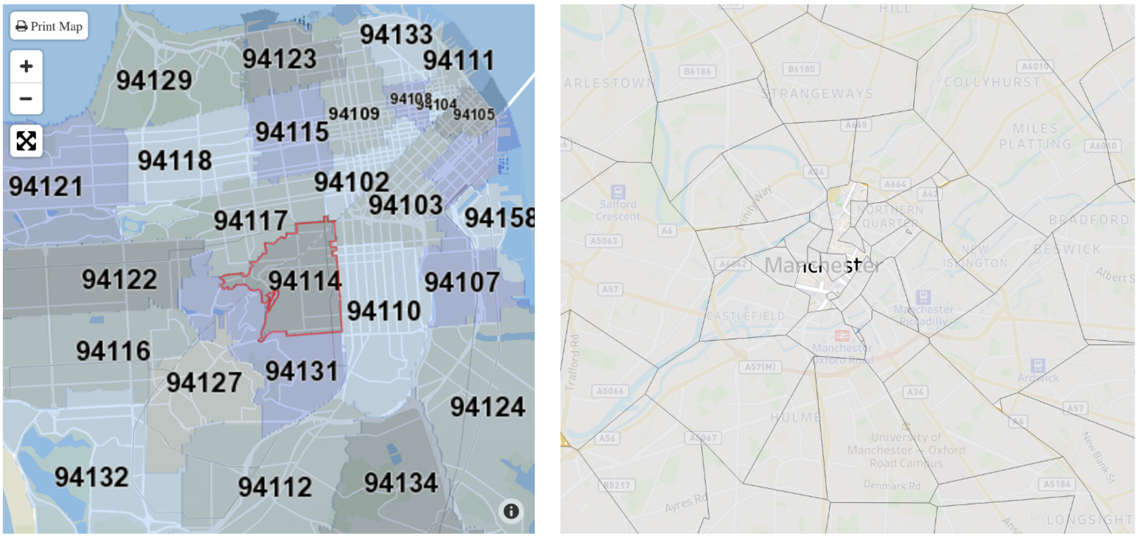


[8]Earlier papers e.g., Campbell et al. (2011) that don't find externalities prevalent in ZIP code levels have been criticised for questionable identification strategies (Literature Review)

A UK postcode itself can be aggregated into sector (5 digits), sub-district (4 digits), district (3 digits) and area (2 digits). The areas were mapped and compared to each other to determine which of these levels would be analogous to ZIP codes (see Figure 2 below). Based on this mapping exercise, the closest comparison between ZIP codes and postcodes was the sector level (5-digit postcode level)[9]. Postcode sectors are still quite granular and contain an average of around 3,000 addresses Casado et al. (2017).

Figure 2 compares ZIP code and postcode sectors for San Francisco (left) and Manchester (right). The two cities were chosen because they have comparable land areas i.e., 121.4 km² and 115.6 km², respectively.

Figure 2: ZIP/Sector Comparison



Therefore, this study has chosen postcode sectors to define neighbourhood clusters. The aggregation method follows Campbell et al. (2011), who create a ZIP code level dataset from their individual property database. Namely, for each sector-year pair, transactions are weighted equally to construct the average price of above-standard houses, the average number of rooms and the average total floor area. The additional property characteristic variables are no longer binary as in the property level case because the dataset does not refer to individual properties. Instead, they continuously capture the number of houses that are detached, semi-detached, terraced, flats, leasehold and freehold for each sector-year. The aggregate dataset also includes an occurrence variable that counts the average number of times the same house is transacted within the dataset. Graphing the distribution of raw aggregate level prices, revealed a leftward skew yet again. Therefore, to correct this non-normality, the price variable was log-transformed using the aggregate prices as the level variables.

[9]Postcode sectors can be 4 or 5 digits long depending on the length of the original postcode as not all are 7 digits long. E.g., postcode sector for CV1 1AH is CV1 1.

After aggregation, the number of total sectors in the sample of transactions is 8,170, and the number of observations is 92,899. Table 3 presents summary statistics for the sector-level variables (s = sector and t = year).

Table 4: Neighborhood Level Variables - Summary Statistics

Variable	Obs.	Mean	S. Dev	Min	Max
Price_{st}	92,691	261,057.2	150,734.3	50,000	1,210,000
$\log(\text{Price})_{st}$	92,691	12.33	.54	10.82	14.01
Floor area_{st}	92,834	96.1	21.79	35.14	1,339
No. rooms_{st}	92,451	4.68	.84	1	76
Detached_{st}	92,899	21.79	24.93	0	315
$\text{Semi-detached}_{st}$	92,899	24.92	23.53	0	212
Flat_{st}	92,899	15.15	28.03	0	1,188
Terraced_{st}	92,899	24.63	24.03	0	304
Leasehold_{st}	92,899	19.4	30.55	0	1,187
Freehold_{st}	92,899	67.1	54.56	0	556
Occurrence_{st}	92,899	1.42	.23	1	5
EPC_{st}	92,691	64.28	5.43	39	99

3 Concentration Measure

Some papers choose to capture externalities by estimating the impact of a concentration measure on housing prices. Seo and Craw (2017) calculate the concentration of foreclosures at the property level by drawing inventory rings around each transaction, i.e. the number of foreclosures within 0.125 of a mile, 0.125 to 0.25 and 0.25 to 0.5. However, drawing rings around each property affected by MEES would have been infeasible due to time constraints and the size of the property-level dataset.

A more feasible method is that proposed by Favara and Giannetti (2017). They report evidence that local foreclosures are positively affected by a ZIP code level concentration measure for mortgages. The measure is calculated by dividing the sum of the top 4 mortgage lenders in a ZIP code by the total number of lenders in the ZIP code.

Therefore, this paper applies the idea of a simple ratio to the context of properties affected by the MEES threshold. The spatial concentration measure is defined as the average concentration of sub-standard properties in each neighbourhood prior to the intervention. The resulting variable serves as the treatment effect because as the concentration measure increases, so would the relative price change among properties not affected by MEES.

The resulting expression can be seen below:

$$C_s = \frac{L_s}{T_s} \tag{2}$$

Where, L_s is the number of sub-standard properties in a sector, and T_s is the total number of properties within a sector. Notably, the metric does not vary over time as it captures the stock rather than the flow of properties in each neighbourhood. Gerardi et al. (2012) stress that in terms of identifying externalities, the inventory of houses matters. Note that when taking the stock of houses using a transaction-level dataset, a higher weight is assigned to those properties transacted more than once. Note that since the dataset is at transaction-level, the same house is sometimes reported multiple times within the same sector. To avoid this multiple-counting of properties, the concentration measure only captures the first occurrence of a house based on its ID.

Additionally, the metric was computed from 2010 to 2017 without including the stock of houses in 2018. MEES intended to change the average EPC of the stock of properties, so to avoid endogeneity, the measure will only capture the average concentration per neighbourhood prior to 2018.

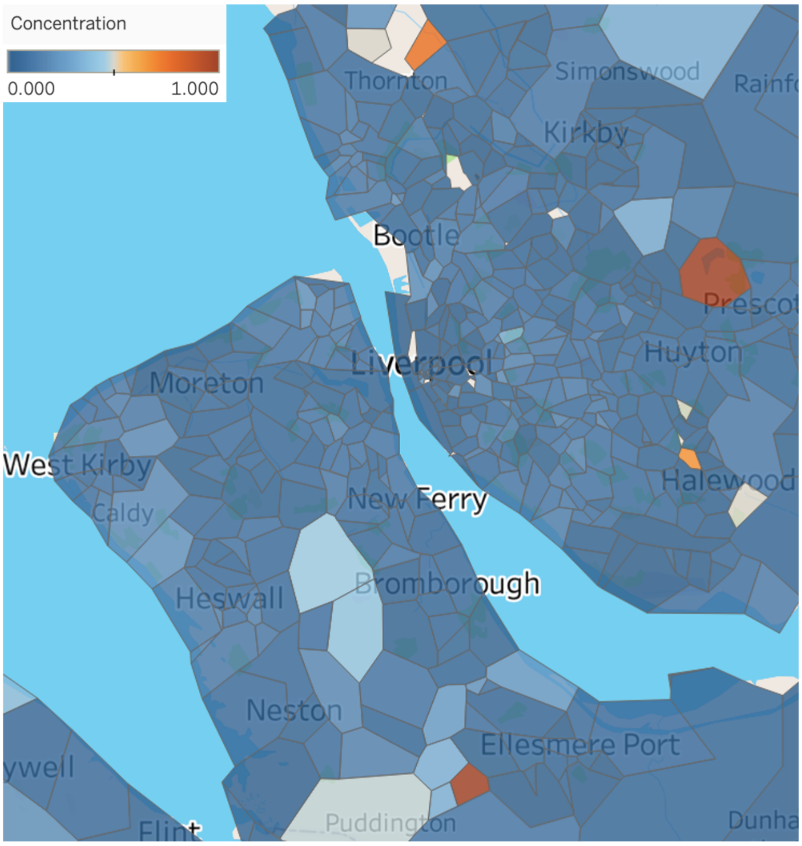
Table 5 shows that the concentration measure takes values between 0 and 1. A postcode sector with no stock of sub-standard houses up to 2018 would have a zero concentration measure, whilst a sector whose stock is only comprised of sub-standard properties has a concentration measure of 1. Moreover, the distribution of the measure is skewed to the left as most sectors have few sub-standard houses (Figure 9 Appendix A). In terms of percentiles, the 25th is 0.03, and the 75th is 0.08.

Table 5: Concentration Measure - Summary Statistics

Variable	Obs.	Mean	Median	S. Dev	Min	Max	25th	75th
Concentration$_s$	92,899	.07	.05	.07	0	1	.03	.08

To visualize the geographical distribution of the concentration of sub-standard houses at sector level, the concentration measure was mapped using Tableau. Blue areas are those with a concentration lower than 0.5, and orange areas are those with a concentration of about 0.5. Neutral-coloured sectors are those closest to the median. For a map of England and Wales by concentration measure, refer to Figure 3 Appendix A. Unfortunately, this mapping was not very useful, as postcode sectors are very granular and are not easily discernible on a large-scale map of England and Wales. Therefore, two illustrative case studies were selected. Figure 3 shows Liverpool, whose city centre neighbourhoods have a low concentration of sub-standard housing, with a higher concentration in some suburban areas.

Figure 3: Concentration Measure - Liverpool



The second sector-level case study is Wales, which exhibits the highest substandard housing concentration clusters. Figure 4 zooms into the Isle of Anglesey and the Llyn Peninsula, where the concentration ratio is particularly high.

Figure 5 shows the geographical distribution of the concentration measure constructed at the postcode area level, i.e., first 2 characters of a postcode. This level was chosen for the geographical visualisation because it was aggregate enough to distinguish the area boundaries clearly. There are a total of 106 postcode areas depicted in the figure below[10]. Wales has the highest concentration of sub-standard housing up to 2018, which matches the sector-level concentration map. The area-level map also reveals that the Southwest Peninsula, the Isles of Scilly (bottom left), Carlisle and Galashiels (North-East England) also have relatively higher concentration levels. The area with the lowest concentration ratio is the Dumphries postcode area in North-West England.

[10]The concentration measure has significantly smaller values at area compared to sector level because of the relatively larger number of total houses than sub-standard houses within each area e.g., in AL the ratio of sub-standard/total houses is 885/25,165.

Figure 4: Concentration Measure - Wales

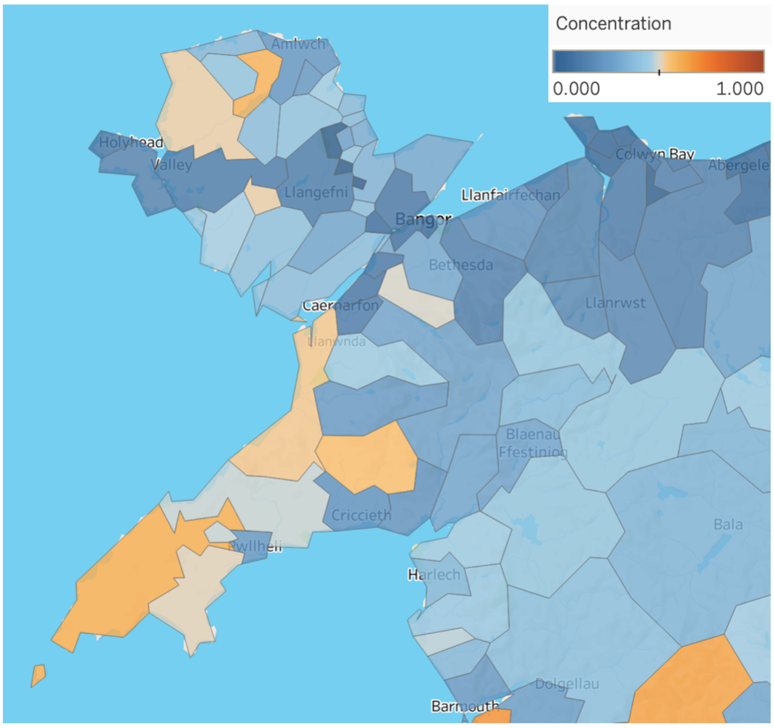
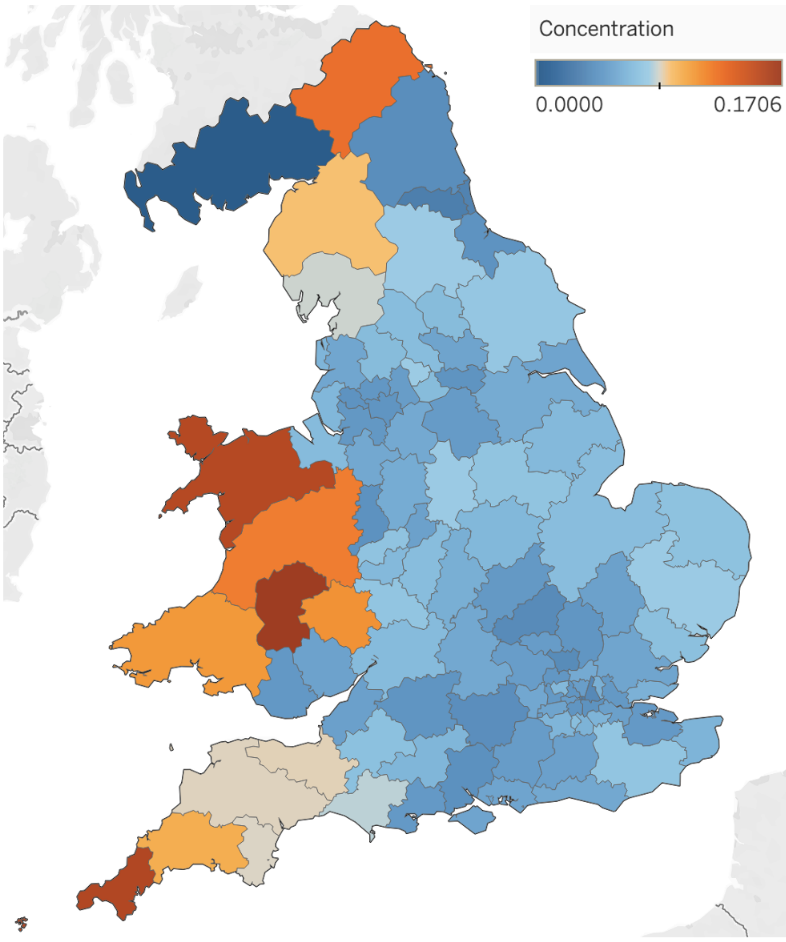


Figure 5: Area Level Concentration Measure - England and Wales



4 Main analyses

4.1 Difference-in-difference

Next, the concentration measure is used to determine whether a sector-level increase in the concentration of sub-standard houses impacts the prices of above-standard properties. To estimate this effect, a difference-in-difference method is employed. This strategy compares the effect of a change in concentration before and after the introduction of MEES on average log prices for properties above the energy efficiency threshold within the same sector. The pre-introduction period is 2010-2017, and the post-introduction period is 2018-2021.

The estimated difference-in-difference regression takes the following form:

$$log(Price)_{st} = \beta_1 Concentration_s * Post_t + \beta' X_{st} + \eta_s + \sigma_t + \epsilon_{st} \quad (3)$$

The impact of a change in the concentration measure on the prices of above-standard houses in sector s is captured by β_1. $Concentration_s$ indicates the continuous 'treatment' measure, and $Post_t$ is a dummy equal to 1 for 2018 onward and zero otherwise[11]. X_{st} is a vector of sector-level characteristics, i.e., the number of terraced, detached, flats, semi-detached, freehold and leasehold houses. These controls aim to address the plausible change in the composition of property transactions within a sector over time. For instance, the recent 2021 'race for space' involved people demanding bigger houses in urban areas, which increased housing prices. The type of transactions was also altered, with more detached houses and fewer flats being bought and sold (Daher et al. (2021)). Additionally, X_{st} includes the occurrence variable, which controls for average transaction volume (the average number of times the same house is transacted in a sector-year) and the EPC variable, which controls for the energy rating of above-standard houses by sector-year.

Time fixed effects σ_t are added to the regression, as local economic shocks may be driving both house prices and the likelihood that sub-standard houses are concentrated in the sector. Sector fixed effects η_s are included in line with Campbell et al. (2011) and Gerardi et al. (2012), who stress the importance of using disaggregated geographic controls to address time-invariant unobserved heterogeneity that impacts house prices. Standard errors are clustered at sector level to account for error term correlations between transactions in the same sector.

Table 6 shows the development of the difference-in-difference specification. Columns (1)-(4) reveal that the coefficient of interest β_1 is negative and significant even with the inclusion of controls and fixed effects. Equation (3) is estimated in column (4), which indicates that after the introduction of MEES, the concentration of sub-standard houses led to a decrease of 21.9% in the prices of houses that were not meant to be affected by the policy. These results favour the negative spillover hypothesis, whereby the devaluation of sub-standard houses attracts lower-income residents into the neighbourhood, depressing prices at the sector level.

When comparing the β_1 estimate with previous findings, it is evident that the spillover on above-standard housing is larger than the direct impact of MEES on sub-standard houses found by Ferentinos et al. (2022). However, it is implausible that the spatial spillovers caused by MEES are larger than the direct impact of the policy on sub-standard properties. This gives reason to suspect that the difference-in-difference analysis is

[11] $Post_t$ and $Concentration_s$ are not added as non-interacted terms in the regression as they are absorbed by the fixed effects.

capturing a composite impact, i.e. both the true MEES spillover and the effect of property characteristics not captured by the model that affect property prices. This links to the criticisms expressed by previous papers on the validity of the hedonic price regression (see Literature Review) in terms of omitted variable bias. The following sub-section explores the possibility that not all relevant property characteristics are controlled for, which causes a violation of the parallel trends assumption.

Table 6: Difference-in-difference - Results

	(1) log(price)	(2) log(price)	(3) log(price)	(4) log(price)
Concentration*Post	-0.236***	-0.210***	-0.343***	-0.201***
	(0.0315)	(0.0220)	(0.0285)	(0.0186)
Concentration	0.764***		0.0138	
	(0.0730)		(0.0983)	
Post	0.241***		0.243***	
	(0.00292)		(0.00419)	
Controls	No	No	Yes	Yes
Year FE	No	Yes	No	Yes
Sector FE	No	Yes	No	Yes
N	92,691	92,667	92,190	92,165
Adj. R^2	0.045	0.944	0.307	0.962

Standard errors clustered at sector-level in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.2 Parallel Trends Tests

The difference-in-difference identification strategy hinges on the assumption of parallel trends. This implies that the changes in log prices of houses in sectors with a low concentration of sub-standard housing are a good counterfactual for the change in log prices that is observed in sectors with larger concentration measures. To assess this claim, papers usually explore the existence of diverging trends pre-intervention (pre-trends). If pre-trends are identified, this is a sign that the sectors with low and high concentrations would not have been trending in parallel post-treatment had the intervention not occurred.

In this sub-section, 3 pre-trend tests are undertaken to determine the validity of the parallel trends assumption concerning regression (3).

4.2.1 Placebo Test

The first parallel trends test is a placebo experiment inspired by Waldinger (2010). The experiment consists of running the baseline regression on a dataset that excludes all post-2018 observations. Moreover, the treatment period was arbitrarily set to 2014 instead of 2018. The purpose is to determine whether sectors with a high concentration of sub-standard houses exhibit evidence of spillovers prior to the 2018 introduction of MEES.

Certain variables were adjusted for the placebo experiment. For instance, to avoid an endogeneity problem, the concentration metric was reconstructed to apply to the stock of houses up to 2014. Additionally, a post-2014 dummy was created, equal to 1 for all observations from 2014 onward and zero otherwise. This dummy was interacted with the concentration measure to generate the difference-in-difference estimate of interest.

Table 7 presents the impact of the placebo MEES introduction in 2014. Column (1) depicts the placebo test run without fixed effects, whereas column (2) includes fixed effects. In both columns, the coefficients on the interaction between concentration and the post 2014 variable are negative and significant. This is a sign of existing pre-trends. Had the treatment and control groups been parallel prior to 2018, there should have been no noticeable impact of the placebo policy introduction in 2014.

Table 7: Parallel Trends Test 1

	(1) log(price)	(2) log(price)
Concentration*Post	-0.370***	-0.388***
	(0.0323)	(0.0195)
Concentration	0.00747	
	(0.106)	
Post	0.151***	
	(0.00551)	
Controls	Yes	Yes
Year FE	No	Yes
Sector FE	No	Yes
N	64,026	63,998
Adj. R^2	0.331	0.971

Standard errors clustered at sector-level in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.2.2 Leads Test

For the second test, the main regression model was augmented with leads for treatment timing, following Autor (2003). This method was chosen because, according to Roth (2019), the most common way to assess pre-trends in the literature is to test whether the leads are statistically significant. Similarly to the placebo test, this would point towards the existence of effects prior to the intervention in 2018.

In the context of this study, leads are dummy variables for years before 2018 up to 2012. Specifically, they equal 1 in the relevant pre-2018 year and zero otherwise. The leads were then interacted with the concentration variable to create several difference-in-difference estimators.

Results from the leads test are presented in table 8. Had there been no differing trends before the intervention, the difference-in-difference estimators would have been insignificant. However, table 8 reports the opposite, with each interaction term coefficient in columns (1)-(2) being negative and significant. This evidence is consistent with test 1, which points towards a violation of the parallel trends assumption.

Table 8: Parallel Trends Test 2

	(1) log(price)	(2) log(price)
Concentration*Post	-0.640***	-0.552***
	(0.0436)	(0.0296)
Concentration	0.217**	
	(0.101)	
Post	0.335***	
	(0.00378)	
Concentration*2017	-0.649***	-0.640***
	(0.0515)	(0.0325)
Concentration*2016	-0.587***	-0.660***
	(0.0497)	(0.0333)
Concentration*2015	-0.459***	-0.538***
	(0.0457)	(0.0306)
Concentration*2014	-0.331***	-0.415***
	(0.0512)	(0.0314)
Concentration*2013	-0.229***	-0.244***
	(0.0502)	(0.0280)
Concentration*2012	-0.102**	-0.138***
	(0.0449)	(0.0343)
Time dummies	Yes	No
Controls	Yes	Yes
Year FE	No	Yes
Sector FE	No	Yes
N	92,243	92,217
Adj. R^2	0.194	0.962

Standard errors clustered at sector-level in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.2.3 DID graph

The final test for parallel trends is the 'classic' difference-in-difference graph. Authors tend to graph the time path of the dependent variable for the treatment and control groups and observe whether, prior to the intervention, the trends are parallel. In the context of this paper, the concentration measure is not binary but has differing treatment intensities across sectors. Therefore, the concentration measure is artificially split into two binary groups to construct a difference-in-difference graph.

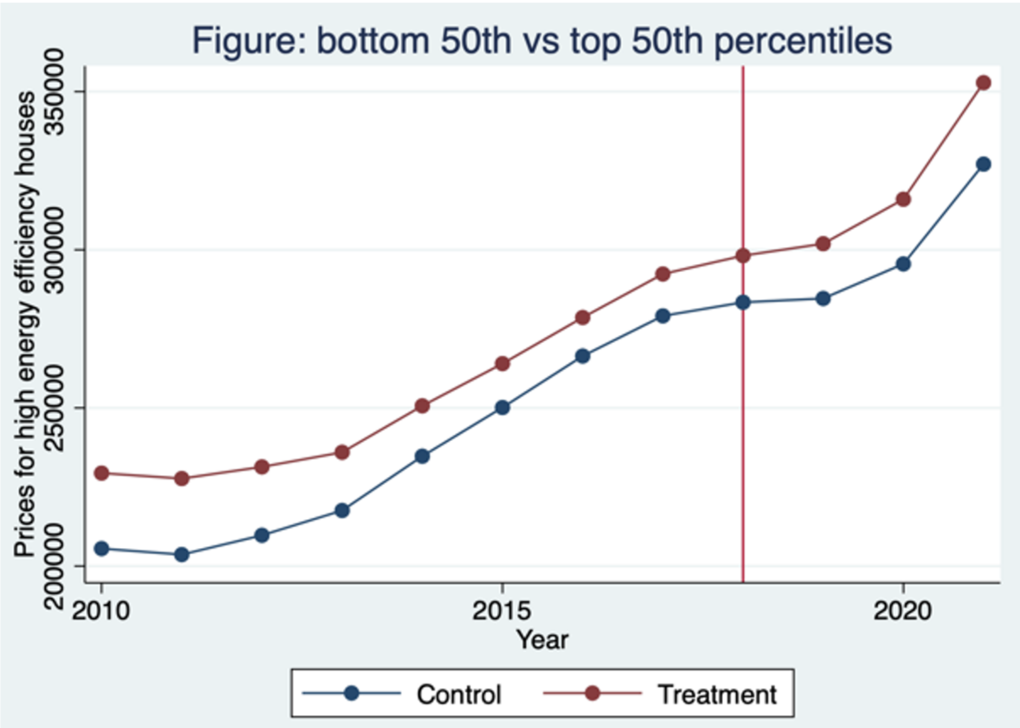
To this author's knowledge, the literature does not provide a standard method for splitting the treatment variable into different intensity groups. For reference, Lindo et al.

(2020) plot a pre-trend graph using a continuous variable. They assess the impact of distance to the nearest clinic on county abortion rates using a difference-in-difference specification. As they use a continuous treatment variable (increase in distance to a clinic), they plot a pre-trends graph by splitting their dataset into 4 categories: zero increase in distance and 3 groups split in terciles.

However, applying this method to the aggregate-level dataset would lead to very few observations per sector. Therefore, the treatment and control sectors were split by the 50th percentile to include as many observations as possible. Specifically, the control group is defined by sectors with bottom 50th percentile concentration values, and the treatment group consists of sectors with concentration in the top 50th percentile. Since this exercise aims to determine whether the prices of above-standard houses in treatment and control sectors are trending in parallel or not, the price variable was plotted over time for sectors in the treatment and the control.

The resulting plot is depicted in Figure 6. It reveals that prior to the treatment year 2018, the prices of above-standard houses in treatment and control sectors were trending parallel. This contradicts the previous two tests, showing a clear parallel trend violation. It is plausible that the results are different in test 3 because of the artificial splitting of treatment and control sectors. A 25th/75th percentile split was also undertaken. This revealed slightly diverging trends, although they are generally parallel (Figure 11 Appendix A). Splitting the sectors into smaller percentiles would not produce valid results as the number of observations would be very restricted (e.g., 5th/95th percentile split).

Figure 6: Trends in prices across treatment intensity groups - 50th/50th



Taking the 3 tests together, with tests 1 and 2 being the most reliable, there is concern that the parallel trends assumption is violated. Relating to the results in section 4.1, the difference-in-difference results are most likely capturing both the diverging pre-trend between treatment and control rather than the true impact of the treatment.

Property level characteristics could be driving the pre-trend, e.g., a garage, balcony, or south-facing property, which may impact the price variation. Additionally, the composition of properties transacted will change over time. This gives reason for a more conservative approach that will control such confounders. This leads to the repeated sales approach.

4.3 Repeated Sales

The baseline regression was re-run on the property level dataset using a repeated sales approach with explanatory variables. This method excludes properties that are transacted once whilst only including properties that are transacted twice - before and after the MEES introduction in 2018. TThe resulting data set has two transactions per property, so it is possible to take the property price difference for each transaction.

The advantage of this approach is that it controls for time-invariant characteristics of houses at the property level (see Literature Review) that the baseline model does not capture. However, it is a restrictive approach that excludes properties without repeated transactions before-after 2018. The final repeated sales dataset consists of 591,488 observations, which is 7.36% of the original property-level dataset.

The results in table 9 explore the relationship between the sector-level concentration measure for sub-standard properties and transaction-level prices for above-standard houses on a repeated sales dataset. Column (1) reports a positive estimate for the post-2018 concentration measure; however, this regression does not include property-level explanatory controls (low adjusted R-square at 0.039). Including controls flips the sign on the difference-in-difference estimate to negative, as in the aggregate-level analysis. Finally, column (3) reports estimates for a specification with controls, year and sector-level fixed effects. This more conservative regression maintains a negative estimate for the concentration measure. The magnitude is largely reduced compared to the aggregate analysis, i.e. a 4.03% reduction in above-standard housing prices compared to 20.1%.

Table 9: Repeated Sales Approach - Results

	(1) log(price)	(2) log(price)	(3) log(price)
Concentration*Post	0.0888***	-0.0490***	-0.0403***
	(0.0178)	(0.0152)	(0.0151)
Concentration	0.306***	-0.399***	
	(0.0827)	(0.0800)	
Post	0.210***	0.235***	
	(0.00154)	(0.00130)	
Controls	No	Yes	Yes
Year FE	No	No	Yes
Sector FE	No	No	Yes
N	575,274	472,274	472,274
Adj. R^2	0.039	0.289	0.846

Standard errors clustered at sector-level in parentheses

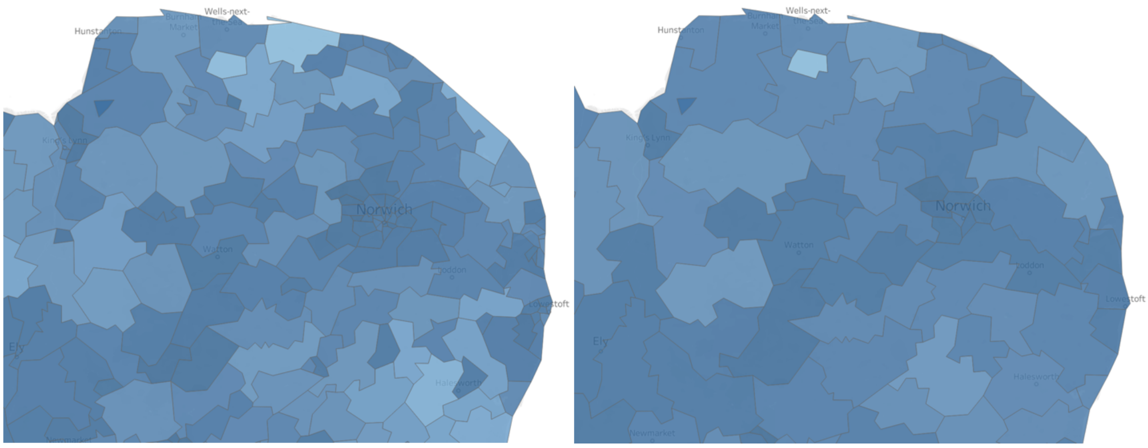
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5 Robustness

This section explores the robustness of the repeated sales and aggregate-level analyses.

The first robustness check alters the definition of a neighbourhood from postcode sector to postcode district. Since districts capture the first 4 digits of a postcode, they are more aggregate than sectors. However, in the transaction sample, sectors and districts sometimes refer to the same geographical area, e.g. postcode district NR23 only includes one sector, NR23 1 and hence the two have the same neighbourhood size. A comparison between sector and district area sizes is shown in Figure 7, which zooms into East Anglia:

Figure 7: Sector/District Comparison



Due to the similarity between sectors and districts, it's expected that using either method should yield similar results. To check whether this is the case, the concentration variable was redefined to the district level and was run on the repeated sales dataset. The district-level concentration variable takes values between 0 - 0.57 inclusive. Table 12 Appendix C reports the results. Column (4) indicates that the estimate of the interaction between the post-2018 dummy and concentration defined at the district level is negative and significant but also larger. This indicates that externalities are still present at the district level and that sectors are not the most aggregate level in which spillovers can be detected.

The second robustness check concerns the data cleaning process. Prior papers address the mistakes made during the data entry process for property transactions, e.g. houses transacted for £1. For this reason, they clear out observations below the 1st and above the 99th percentile at the property level. However, papers that run aggregate and property level analyses do not consider cleaning outliers at the aggregate level, i.e. excluding 'freak' neighbourhoods. These would be neighbourhoods hit by exceptional circumstances, such as a natural disaster, leading to abnormally low prices at the neighbourhood level. Therefore, an additional data cleaning process is performed on the aggregate dataset, whereby the bottom 1st and top 99th percentiles of sectors are excluded. The aggregate analysis is re-run on this sample, and the results are reported in Table 13 Appendix C. Excluding the 'freak' neighbourhoods does not impact the aggregate level results. Column (4) reveals that by excluding outliers at the neighbourhood level, the externality is a 21.4% decrease in aggregate level prices. This compares to the 21.9% decrease found in Table 6 from the baseline regression.

The Herfindahl-Hirschman Index (HHI) was used for the third robustness check to calculate the concentration measure instead of the simple ratio. This method is commonly used to measure the concentration of firms in a market or industries in an area. For instance, to measure the concentration of firms, HHI is calculated by taking the squared sums of market shares for all firms in an industry. This index could be applied to the context of the MEES threshold by taking the squared sums of the share for below and above-standard housing within each sector. The formula can be seen below:

$$HHI_s = S_1^2 + S_2^2 \tag{4}$$

Where, s denotes sector, S_1 is the share of sub-standard housing in a sector (the simple ratio), and S_2 is the share of the above-standard housing in a sector. As with the simple ratio, this measure is applied to observations up to but not including 2018.

Taking this expression to the data, the resulting HHI is a variable between 0.5 - 1 inclusive. When HHI is equal to 1, the sector consists of only sub- or above-standard housing. As the sector becomes diversified, the value of HHI drops.

Table 10: HHI - Summary Statistics

Variable	Obs.	Mean	Median	S. Dev	Min	Max	25th	75th
HHI_s	92,843	.88	.9	.09	0.5	1	.84	.93

To compare results with HHI and the simple concentration ratio, it is essential to understand how they relate to each other. When the simple ratio is 0, the sector is concentrated entirely by above-standard housing, meaning HHI equals 1. As the concentration of sub-standard houses increases, the sector becomes more diversified in energy efficiency, leading to a lower HHI. When the simple ratio is 0.5 (sub- and above-standard houses are half/half), then HHI is 0.5 as well. Therefore, if the simple ratio takes values within the range 0 - 0.5, a higher ratio implies a lower HHI, i.e., the two measures move in opposite directions in this range. However, as the simple ratio surpasses 0.5, the sector becomes less diversified as the concentration tips towards relatively more sub-standard housing. Therefore, in the range of 0.5 - 1, the two measures move in the same direction since HHI increases along with the ratio. If the simple ratio is 1 because the sector only consists of sub-standard housing, HHI is also 1, indicating an entirely concentrated sector.

Given that most sectors have a simple ratio of sub-standard housing below 0.5 (see Appendix for distribution of the simple ratio and refer to mapping in section 3), in most cases, it is expected that HHI and the simple ratio move in opposite directions. Hence, when applying HHI to the aggregate regression, the direction of the effect will be positive instead of negative. Indeed, table 13, Appendix C, shows that the interaction term of HHI and a pre-period dummy is positive and significant. In column (4), the estimate is a 14.5% increase in the aggregate price level as the concentration of above-standard housing increases. The magnitude is smaller than the simple ratio, possibly because the HHI index also captures the fact that HHI increases when there is a high concentration of sub-standard housing, although this is less frequent in the data. Overall, the interpretation of HHI in the context of the MEES threshold is more complex than the simple ratio.

6 Conclusion

This study assesses the externalities caused by the 2018 regulatory introduction of the Minimum Energy Efficiency Standard for privately rented properties in England and Wales. Ferentinos et al. (2022) find that introducing an energy efficiency threshold to band E leads to a devaluation of properties with sub-standard efficiency levels based on their EPC. The current study takes these findings a step further by exploring whether the devaluation of sub-standard properties impacts aggregate prices at postcode sector level.

This research question is answered with a novel spatial concentration measure for sub-standard properties. Specifically, the measure is a ratio of the stock of sub-standard properties within each postcode sector and the total stock of properties within the sector. This ratio was applied to all observations prior to and not including 2018 (2010 - 2017). An aggregate-level analysis with a difference-in-difference identification strategy revealed that as the concentration of sub-standard housing increased, the prices of above-standard houses in the area decreased by 20.1%. However, this estimate seems too large compared with Ferentinos et al. (2022) estimate of the direct impact of MEES on house prices. Therefore, there was concern that the aggregate analysis captures different pre-trends between sectors with low and large concentration measures. Therefore, a more conservative repeated sales approach is adopted, revealing a negative and significant impact of concentration on house prices. However, the repeated sales estimate was smaller in magnitude than the aggregate analysis at 4.03%.

These results have brought forward a significant negative externality imposed by MEES on properties that were not intended to be affected by the policy. The aggregate level depression of prices is worrying as this increases the likelihood that homeowners will find themselves in negative equity, i.e. their property valued less than their outstanding mortgage. Owners who sell their property when in negative equity will incur a loss, which may discourage them from selling. As a result, this could lead to overall restricted housing mobility in the economy.

Another implication of the negative externality on above-standard houses could be landlords' pro-environmental attitudes. The devaluation of above-standard houses due to the price change in sub-standard houses may disincentivise landlords from investing in retrofit energy efficiency technologies that will take their property EPC above band E. This could further aggravate the lack of homeowners' engagement with retrofit measures.

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