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ARTICLE

The Effects of Infrastructure Projects on House Prices and Rents : Evidence from the HS2 Extension Cancellation in the UK

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ABSTRACT

This paper uses the HS2 extension cancellation in November 2021 as a quasi-experiment to study its impact on house prices and rents in Leeds. Using a DiD approach on repeat sales and monthly rents, I compare property values near the HS2 station and proposed construction site before and after the announcement. Results show a 3.6% decrease in house prices and a 3.9% decline in rents near the station, while properties near the construction site experienced a 2.4% increase in prices and a 2.1% rise in rents. This is the first paper to analyse the HS2 cancellation effect using panel data methods.

Keywords: HS2 extension cancellation; Externalities; House price effects; Transport infrastructure; Difference-in-Differences Model

1. Introduction

The High-Speed Two (HS2) project, a significant investment in transport infrastructure in the United Kingdom (UK), aims to stimulate economic growth, enhance connectivity and rebalance the economy. However, since its announcement in 2009, the project has faced numerous challenges and revisions by policymakers. The cancellation of the eastern leg from Birmingham to Leeds in November 2021 was part of the government's "Integrated Rail" Plan and marked a major shift in the project's scope and objectives. My main research question is to what extent this cancellation led to changes in average house prices and monthly rents in the affected Leeds area. I also study how this impact differs between properties

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near the station (which would have benefited from improved accessibility) and those near the planned construction site (which would have experienced negative externalities). Despite the large body of literature on the relationship between transport accessibility and property values, there is limited empirical evidence addressing this question in the context of the UK, particularly in the case of a major infrastructure project cancellation like the HS2 extension.

The consensus in the literature suggests that transport accessibility positively affects house prices, while long-term construction activities have a negative impact. Early studies using multivariate OLS techniques suffer from omitted variable bias due to unobserved factors, leading to biased estimates. More recent work, advanced by Gibbons (2013), employs boundary discontinuity designs to control for unobservable neighbourhood characteristics, but issues may persist if these factors change discontinuously along railway track boundaries or if infrastructure projects are located in more developed neighbourhoods, leading to reverse causality. Some researchers have used instrumental variables to address such issues, but finding a valid and strong instrument remains a challenge. The most common approach to evaluate the impact of transport infrastructure changes on property prices is the quasi-experimental or difference-in-differences (DiD) methodology. For example, Brandth (2004) studied the ex-ante and ex-post effects of the introduction of the rapid Seattle transit line and found that house prices within a 2.5 km vicinity of the station increased in value by 4.3%. However, one limitation in such analyses is the difficulty in cleanly separating the positive direct effects of enhanced transport accessibility from the negative indirect effects of construction and noise pollution, especially when both effects occur within the same local area. While these studies differ in their exact approach, they generally compare property price changes in an impact area subject to transport policy changes with price changes in a 'control group' with neighbourhoods designated as mostly unaffected.

In this paper, I leverage the surprise HS2 extension policy cancellation in November 2021 as a quasi-experiment to estimate the premium for transport accessibility and the penalty for being located near a construction site. The abandonment of the HS2 line meant that transport links from Leeds to major cities like London and Birmingham were weakened, with HS2 predicted to improve travel times by 45% when fully operational. By employing a robust DiD specification with repeat sales of houses and monthly rents of properties brought onto the market during the study period, I employ 'within-variation' to more effectively control for unobserved differences in neighbourhood characteristics. This approach is less reliant on somewhat crude assumptions compared to instrumental variable or regression discontinuity designs. The novelty of my paper lies in its first-of-its-kind use of quasi-experimental methods to identify the impact of a major policy shock. To conduct my analysis, I manually constructed a dataset based on the spatial location of properties using the mapping program ArcGIS. By comparing the changes in house prices and monthly rents between affected and unaffected areas before and after the policy announcement, I find that properties near the HS2 station experienced a significant decrease in value (3.6% for house prices and 3.9% for rents) due to the loss of anticipated transport accessibility improvements. Conversely, properties near the planned construction site saw an increase in value (2.4% for house prices and 2.1% for rents), attributed to the elimination of expected negative externalities.

The rest of the paper is organised as follows: Section II offers a literature review. Section III provides information on the policy and how I constructed my dataset. Section IV contains the details of my empirical approach. Section V presents the main results and extensions while Section VI contains some robustness-control analysis. Finally, Section VII concludes.

2. Literature review

2.1 Theoretical aspects

The first strand of the literature is grounded in urban economic theory, which delves into the role of spatial factors in determining land values. The monocentric city model, developed by Alonso (1964) and extended by Muth (1969), provides a theoretical groundwork for understanding the relationship between transport costs and land rents. In this model, households trade off accessibility to the central business district (CBD) against housing consumption, with land prices declining with distance from the CBD to compensate for higher commuting costs. After the improvement of transport infrastructure, such as the introduction of a new rail transit line, the model implies that property values will increase due to a reduction in commuting times and an increase in attractiveness of purchasing nearby properties. However Anas (1998), among others, have critiqued this model for its simplifying assumptions: the homogeneity of household preferences and there being only one employment centre. He argued that cities exhibit polycentric structures with multiple centres of employment and complex spatial patterns of economic activity. Further, spatial equilibrium patterns may diverge from the predictions of the monocentric city model due to the heterogeneous preferences of households with respect to location, property attributes and amenities.

Recent academic thought has sought to address these complexities with the bid-rent theory, originally postulated by Fujita (1989), being the main framework. It allows for heterogeneous preferences and multiple CBDs when analysing the spatial structure of cities. The theory argues that land rent at each location is determined by the highest bidder, among different types of land users such as firms, developers, or households. The bid-rent function for these agents incorporates factors like accessibility, employment opportunities, production technology or speculative upside potential. When applied to transport infrastructure, the theory is more inconclusive and suggests that the impact may vary across the different segments of the housing market and is influenced by the spatial distribution of economic activities. For example, Mathur and Ferrell (2013) highlight that the impact of a new rail line in San Jose, USA, on property values was mediated by the adoption of transit-oriented development policies and zoning changes around the stations.

Another important theoretical consideration is the potential for transport improvements to lead to wider economic benefits, beyond the direct user benefits; this motif has been captured by cost-benefit analysis. Increased labour mobility, resulting from transport improvements, can lead to better job accessibility and higher wages, which in turn can increase the demand for housing and drive-up property values. Similarly, productivity gains and agglomeration economies, stemming from improved connectivity and knowledge spill overs, can enhance the attractiveness and economic viability of living in an area. Despite the theoretical significance of wider economic benefits, empirical studies quantifying these impacts has been limited due to data constraints. For instance, Chatman et al. (2012) attempted to measure the agglomeration benefits of transit investments in San Diego, USA, but faced challenges in isolating the effects of transit from other confounding factors. Similarly, Ahlfeldt (2011) investigated the wider economic impacts of a new high-speed rail line in London, UK, but acknowledged the limitations of available data in capturing the full extent of these benefits.

2.2 Empirical evidence

With the increased availability of spatial data and advances in econometric theory, the empirical literature on the impact of transport accessibility on property values has significantly grown over the decades. The literature showcases a methodological evolution; from basic OLS regression models to advanced spatial econometrics, reflecting deeper insights into the causal impacts of transport accessibility. Most of the research has been concentrated on the effects of rail transit systems, such as light, metro or commuter rail on residential and commercial property values. A variety of econometric methods, such as hedonic pricing model, spatial econometrics or quasi-experimental designs have been employed. It is noteworthy that the evidence on the impact of transit accessibility on property values is mixed, with some studies finding significant positive effects and others reporting insignificant or even negative effects. This may be the case because researchers have not been able to separate the positive effect of increased transport accessibility with the negative effects of externalities on prices. A summary of the key papers is outlined in **Table 1**. Meta-analyses by Debrezion (2007) and Mohammad et al. (2013) have sought to synthesise the findings from multiple sources. The consensus is that the impact of rail transit improvement on property values is generally positive, but the size of the effect depends on factors such as the type of property, the distance to the nearest station and the methodological quality of the study.

In this strand of the literature, one of the most influential papers is by Rosen (1974) where he formulated the hedonic pricing model as a general framework for estimating the implicit price of housing attributes, including accessibility. In his model, the market price of a property is assumed to be a function of its locational, neighbourhood and structural characteristics. This stems from an optimisation problem where homebuyers choose house characteristics and access to local facilities (e.g., public transport infrastructure) based on heterogeneous budget constraints and preferences. At the optimum, the agent equalises the marginal benefit and cost of improving any attribute. The coefficient on each variable thus captures the marginal willingness to pay (MWTP) for that attribute.

To quantitatively estimate the MWTP for transportation accessibility, early research utilised multivariate cross-sectional regressions, adjusting for house and neighbourhood attributes that affect property values. For instance, an increase of up to 6.5% in house prices was found by Norman (1987), who used a dataset of over 1500 sales of residential homes in Germany in the 1970s, attributed to the improved interconnectedness of the Berlin-Hamburg highway. Several studies have also extended Rosen's basic framework by controlling for other relevant factors such as Brockman (2013) who uses pricing data for over 600,000 properties and concludes that the construction of the Mumbai high speed rail network increased prices by 3.2% for homes within a 2 km vicinity.

However, to interpret the hedonic price model estimates as causal effects in this context is problematic due to the potential for omitted variable bias and endogeneity. Neighbourhood quality or local economic conditions (both variables which are unobserved) may be correlated with both property values and transport infrastructure projects, leading to biased and inconsistent estimation of the premiums. Cervero and Landis (1997) further argue that the location of stations may be endogenous, as they may be more likely to be constructed in areas with greater development potential. To address such issues, researchers have used instrumental variables or fixed effects approaches to control for the unobserved heterogeneity. For example, Smith and Johnson (2019) employ a fixed effects model to investigate the impact of a new rapid bus system on residential property prices in New Mexico, USA, accounting for both direct and indirect (spillover) effects of transport accessibility. They conclude that a 10% increase in proximity to bus stations leads to a 2.5% increase in property values, with significant positive spillover effects extending up to 1.5 km from the bus stops.

The temporal variation in transportation accessibility allows for the application of panel data methods to control for unobserved heterogeneity between houses and neighbourhoods. These approaches exploit exogenous changes arising from policy shocks or quasi-experiments. By linking changes in housing markets to the change in transport infrastructure, researchers have identified the associated premium while accounting for any unobserved differences across properties. For example, Machin (2005) uses a DiD approach to estimate the impact of the Jubilee Line Extension in London on property values, comparing the changes in prices between affected and unaffected areas before and after the opening of the new line. He concludes that the extension led to a significant increase in property values, with a 1 km reduction in distance to the nearest station associated with a 2.1% increase in prices. This strand of the quasi-experimental literature also highlights the importance of considering the potential for heterogeneous effects and the role of complementary policies in shaping the impact of transport accessibility on property values. For example, Bowes and Ihlanfeldt (2001) find that the effect of rail transit proximity on property values in Atlanta varied depending on the neighbourhood income level and the distance to the CBD, with higher-income neighbourhoods and those farther from the CBD experiencing greater accessibility premiums of approximately 5.8%.

2.3 Research gaps and contribution

Despite the growing body of empirical research on the impact of transport accessibility on property values, several gaps and limitations remain. First, most studies have focused on the effects of rail transit in North America. This means results cannot be generalised to other countries, like the United Kingdom, as the institutional, economic and social contexts differ significantly. Second, only a few studies have analysed the effect on both residential house prices as well as monthly rent rates. This is important to investigate for welfare reasons because most people in the UK, about 60%, rent their homes. Finally, most studies focus on quantifying the positive effects of increased transport accessibility but have not been able to isolate the indirect effect of negative externalities, such as noise pollution and inconvenience caused by construction, on house and rent prices.

To date, limited research has explored the impact of transport policy on housing market dynamics in the UK using panel-data methodologies. Employing a quasi-experimental research design to identify causal effects, my paper contributes to the literature by providing evidence on the impact of cancelling high-speed rail access in an UK context, analysing both house prices and monthly rents. Following the second strand of the literature, I also investigate heterogeneous effects by property type to qualify my results. With my classification of two treated areas ("Near station" and "Near track"), I attempt to quantify the positive direct effect of better transport accessibility as well as the negative indirect effect of externalities, associated with large public infrastructure projects. Because of the spatial pattern of construction sites and railway line, my paper leverages a more robust identification strategy. My paper introduces a novel contribution by adopting a quasi-experimental design to examine the repercussions of a significant UK policy shift without relying on stringent assumptions about unobserved household attributes or neighbourhood qualities. Notably, I am the first to analyse the implications of the HS2 extension cancellation on property values, meaning I had to manually construct the dataset.

3. Context and Data

The HS2 project is a proposed high-speed railway network in the United Kingdom, aimed at enhancing connectivity between London and major cities in the Midlands and North of England. The project was first announced in 2009 by then Prime Minister Gordon Brown, who pledged a £20bn investment in a North-South High-Speed Rail Network. Developed by HS2 Ltd, a non-departmental public body wholly funded by the Secretary of State for Transport and sponsored by the Department for Transport, the project aimed to address capacity constraints on existing rail lines, reduce travel times and stimulate economic growth in regions outside of the capital. Due to the large scale of this investment, the network was originally supposed to be built in several phases, with Phase 1 connecting London to the West Midlands, Phase 2a extending to Crewe and Phase 2b completing the network to Manchester and Leeds.

The timeline of key HS2 decisions and developments from 2009-2023 has been marked by several milestones, controversies and revisions. In 2010, the Cameron-Clegg coalition approved the development of the high-speed rail network policy. In 2012, Transport Secretary Justine Greening gave the green light to HS2, despite predicted costs rising to £32.7bn in his annual statement. The following year, the government announced that HS2 would cost almost £50bn, with the line expected to become operational in 2026 and be completed in 2033. However, costs continued to escalate, with estimates reaching £55.7bn by 2015 and £98bn by 2020 (in 2019 prices). The proposed route has been shown in **Figure 1** for a visual understanding.

Table 1: Summary of Literature on the Impact of Transit on Property Values

Authors	Year	Property Type	Transit Type	Study Area	Model	Main Finding
Insignificant Effects						
Gatzlaff and Smith	1993	Single-family detached house sales	Metrorail	Miami, US	DID	The announcement of new rail transit had a weak impact on residential property values.
Mulley	2014	Residential	Bus rapid transit	Sydney, Australia	Hedonic regression in the log transformed form	Property values are primarily influenced by individual property features and the neighborhood.
Forrest et al.	1996	Residential	Light rail transit	Manchester, UK	DID	The provision of rail transit has a weak impact on residential property values.
Hess and Almeida	2007	Residential	Light rail transit	Buffalo, US	OSL Box Cox and a spatial econometric model	For every 0.3km closer to stations, average property values rose by 0.99% (network distance), although rail proximity was less influential than property location and characteristics in predicting housing prices.
Positive Effects						
Mulley et al.	2016	Residential	Light rail transit and Bus rapid transit	Brisbane, Australia	DID	Proximity to stations increased property values by 0.14% for every 100m and by 0.36% for every 250m.
Benjamin and Sirmans	1996	Apartment rent	Metrorail	Washington DC, USA	Hedonic pricing model with fixed effects OLS	Rent decreases by 2.5% with every 0.1 mile increase in distance from stations.
Voith	1993	Single-family detached house sales	Rail	Philadelphia, US	Hedonic pricing model	Proximity to stations results in an 8.1% increase in average sales price and a 7.5% increase in average median price across all housing types.
Banister and Goodwin	2011	Residential	Metrorail	London, UK	DID	Housing prices increase with proximity to stations; Southwark experiences a residential value uplift of £59 million, and Canary Wharf sees an uplift of £5.7 million.
Debrezion et al.	2007	Commercial and residential	Meta-analysis of rail, bus rapid transit, light rail transit and metrorail	15 cities in USA	Hedonic model using generalized spatial two-stage least-squares estimation; DID and fixed effects OLS	Commercial properties within station zones were pricier than residential houses. The average prices of commercial and residential properties within station catchments were 16.4% and 4.2% higher, respectively, than those outside. Moreover, CRT stations had a more significant impact on raising housing prices compared to LRT, HRT, and Metro stations.
Al-Mosaind et al.	1993	Residential	Light rail transit	Portland, USA	Hedonic pricing model	Prices increased by 10.6% within 500m of stations.
Mathur, S., & Ferrell, C.	2013	Residential	Light rail transit with suburban development	San Jose, USA	Spatial econometric model	The price effect of transport-oriented development dissipates beyond 1/8 mile. In the post-transport oriented development period, housing prices within 1/8 mile were 18.5% higher than those more than 1/8 mile away; during the construction period, the prices were 7.3% higher; and in the pre-transport oriented development period, the difference was statistically significant.
Negative Effects						
Laakso	1992	Residential with focus on detached homes	Metrorail	Helsinki, Finland	Hedonic pricing model	Land values peak between 250-500m from railway stations and 500-750m from metro stations.
Bowes and Ihlanfeldt	2001	Residential	Bus	Atlanta, US	DID	Transport effects have a greater influence on prices than retail effects. Additionally, prices are lower by 3.4% within a 0.25-mile station buffer due to negative externalities.
Brandt and Maennig	2012	Office, commercial, light industrial properties	Rail	Hamburg, Germany	DID	Rail transit improvements can lead to up to a 4.6% uplift in prices. However, prices are lower within a 250m station buffer due to negative externalities.

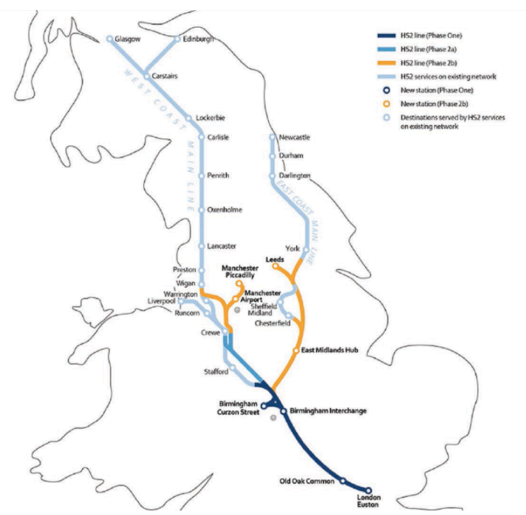

Figure 1i: Map showing the original HS2 route

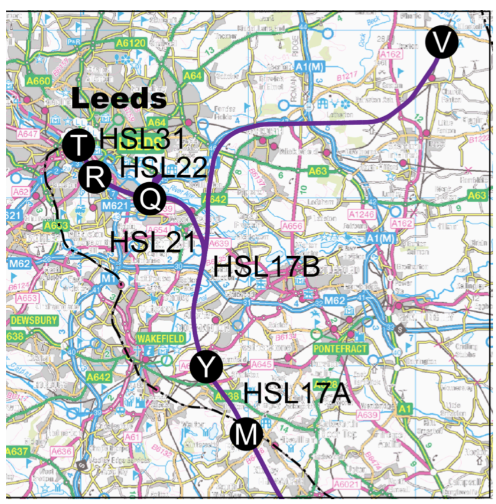

Figure 1ii: Map of the Leeds route

The proponents of the policy argue that HS2 will deliver significant economic benefits, such as increased connectivity, regeneration opportunities and reduced carbon emissions due to more advanced train technology. However, critics have raised concerns about the high estimated costs, potential cost overruns and the need for substantial taxpayer subsidies. There is general consensus in the academic literature that the economic case for HS2 is based on overly optimistic demand forecasts and that the project represents an example of government central planning, with taxpayers bearing a high proportion of the financial risk (DoT, 2018). Unfortunately, I have not been able to supplement my empirical work with a cost-benefit analysis, as outlined by the UK Government Green Book, due to data limitations.

As HS2 progressed, it faced increased criticism and scrutiny, with the House of Lords Economic Affairs Committee questioning the sufficiency of evidence to justify its construction in 2019. Concerns were raised about the accuracy of passenger demand forecasts, the project's ability to reduce inequality between the North and South and the potential environmental consequences due to difficulties in delivering the carbon reduction targets in time. On 18th November 2021, Transport Secretary Grant Shapps scrapped the eastern leg from Birmingham to Leeds, providing the foundation for my quasi-experimental approach. More recently, in 2023, the Sunak government announced that construction of HS2's Birmingham-Crewe leg would be delayed by a further two years and the Birmingham-Manchester leg would be scrapped. Given the unfolding events and recent policy shifts, my work is not only contemporarily relevant but also important in shaping future infrastructure policy and economic debate in the UK.

To quantitatively study the effects of HS2 policy cancellation, my use of house prices and monthly rent rates is justified on two grounds. Firstly, the market price for a property is indicative of its discounted future expected utility to buyers. Even if the current buyer may not directly benefit from improved transport links (e.g., due to a short work commute), the property can be readily sold or rented to someone who values this feature. This transferability of benefits ensures that the accessibility premium persists in the housing market, as the price reflects the aggregate willingness to pay of all potential buyers, not just the current owner. Even though the benefits of the station would not have been realised until completion (projected to be 2033) house buyers will have included this in their decision, albeit slightly discounted. Second, houses are primarily transacted through estate agents, who are responsible for communicating all relevant information about the property to potential buyers, including details on nearby transport stations and future infrastructure projects. This is also done by landlords who rent their properties to renters and this information disclosure ensures that buyers are well-informed about the property's attributes, which is subsequently reflected in the market price. Assuming an efficient market, changes in the housing attributes will be reflected in the prices quickly, especially in the era of online marketplaces where nominal price rigidities are minimal (Suchindler, 2010).

To construct my dataset, I collected and merged several ones together. The primary data source on house prices was obtained from HM's Land Registry. This contained over 135,000 transactions across the Leeds area, covering commercial and residential property sales from 2010 to 2023. The dataset contained valuable information such as the date of each transaction and various characteristics of the properties being bought or sold. To ensure the accuracy and comparability of the data, I refined the dataset by focusing on houses that were sold at least twice in the time period, while maintaining the same features throughout. For houses with sales both before and after the reform, I selected only the transactions that occurred closest to the policy announcement date from either side. This refinement process helped to minimise the potential impact of time-varying factors and ensure that the observed changes in property values could be more confidently attributed to the reform itself. The houses were mapped geographically using ArcGIS to attribute the effects of HS2 construction and improved connectivity to specific spatial locations.

With regards to rent data, it was scraped using a Python script from Zoopla, a large online marketplace and data provider, between January 2010 to December 2023, covering a predicted 65% of the Leeds rental market. This dataset included details on net rent, the time it first appeared on the market, postcode of the property and a range of housing characteristics. In this context, posted rents are particularly valuable because they tend to be more accurate than surveyed rents. Surveyed rents can be less precise, as households often struggle to separate net rent from total shelter costs, which include heating and additional services. Once again, the rented properties were geocoded with ArcGIS, a tool that transforms traditional written addresses into longitudinal and latitudinal coordinates which are then plotted on a map (**Figure 2ii** shows some of the rented properties in the "Near station" area).

For the radii defining the "Near track" and "Near station" areas, I selected a 1 km distance based on the prevailing norms in relevant literature (Kim, 2014). The "Near station" area was centred around New Lane station, designated as the primary hub for the HS2 scheme. Conversely, the "Near track" zone was oriented around Rothwell interchange, one of the six main sites for HS2 railway construction in the Leeds area. For my robustness tests, I increased the radii to 1.2 km and the areas can be seen by the dashed line circles in **Figure 2iii**. The control group included properties situated at least 1.2 km from the main HS2 station and 3 km from the Rothwell construction site. This group, located within Leeds, shares similar macroeconomic characteristics with the treatment groups, which is critical for isolating the causal impacts of HS2. **Table 2** provides summary statistics for the house sales and rented properties as well as a brief description of the control and treated areas.

```
import pandas as pd
import logging
from datetime import datetime
import matplotlib.pyplot as plt

# Set up logging
logging.basicConfig(level=logging.INFO, format='%(asctime)s - %(levelname)s
    - %(message)s')

def fetch_rent_data(sdfahasdfz, Leeds, Jan_2010, Dec_2023):
    base_url = "https://api.zoopla.co.uk/api/v1/property_listings.js"
    headers = {"Accept": "application/json"}
    all_data = [sdfahasdfz]

    while current_date <= end_date:
        params = {
            'area': Leeds,
            'listing_status': 'rent',
            'include_rented': '1',
            'api_key': sdfahasdfz,
            'page_size': '100',
            'order_by': 'price',
            'ordering': 'descending',
            'listed_from': current_date.strftime('2010Y-1m-1d'),
            'listed_to': (current_date.replace(day=28) + pd.DateOffset(days
                =4)).current_date.endtime('2023Y-12m-31d')
```

Figure 2i: Scraping for rent data

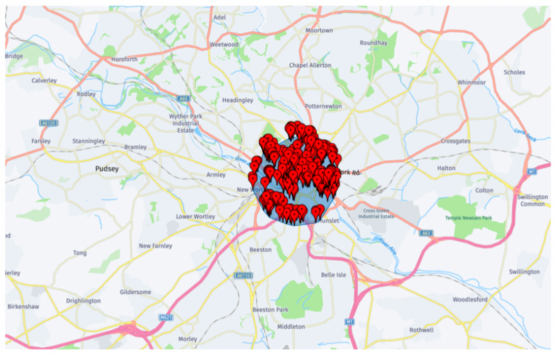

Figure 2ii: Geolocating the properties

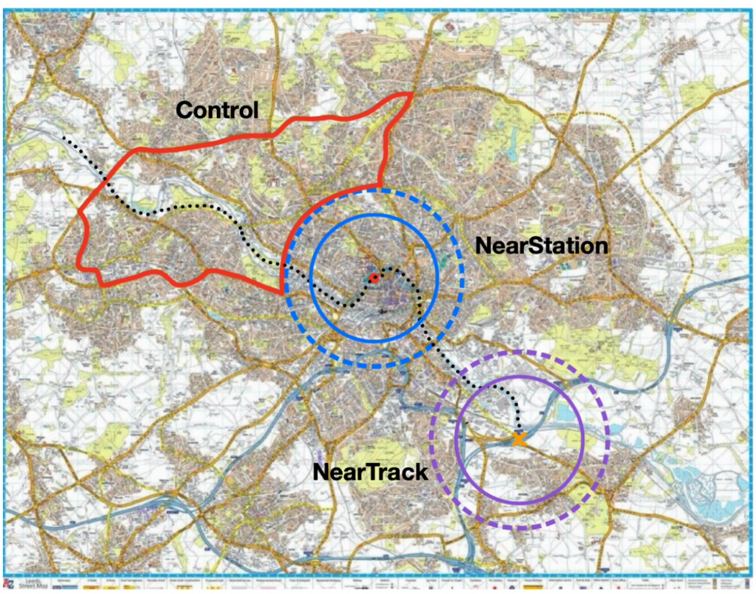

Figure 2iii: Visual representation of the groups

Table 2: Summary statistics for control and treatment groups

Group	Control	Near station	Near track
Description	The control group consists of houses located at least 1.2km from the main HS2 station and 3km from the proposed construction site on Rothwell interchange. By selecting properties within Leeds, the control group shares similar macro-level characteristics with the treatment groups, essential for isolating the causal effect of the HS2 project. The most common postcodes within this group are LS21 and LS29.	Treatment group 1 consists of properties within 1km of New Lane station, chosen to examine the benefits of increased transport access from the HS2 project, such as better connectivity and shorter travel times, without the downsides of construction. These properties will also retain access to current railway services alongside the new HS2 line. The most common postcodes within this group are LS1, LS2 and LS3.	Treatment group 2 includes properties within a 1km radius of the planned railway construction near Rothwell interchange, chosen to examine the impact of negative externalities like noise pollution and construction inconveniences. The construction is set to begin in 2026 and is expected to finish within three years. The most common postcodes within this group are LS25 and LS26.
Mean (house price)	£214,200	£225,800	£188,200
SD (house price)	£127,000	£133,300	£114,280
Transaction date range	1/1/2010 - 30/12/2023	1/1/2010 - 31/12/2023	3/1/2010 - 30/12/2023
Mean (rents)	£850	£935	£815
SD (rents)	£90	£105	£85
Transaction date range	2/1/2010 - 30/12/2023	1/1/2010 - 31/12/2023	3/1/2010 - 30/12/2023
Characteristics for house sales (%)			
New build	3.1	3.2	0.6
Semi-detached or detached	51.9	5.3	42.1
Terraced	8	30.5	48.3
Flats	37	61	9
Number of observations	3971	1682	855
Characteristics for rented properties (%)			
New build	4.5	4	1.6
Semi-detached or detached	38	12	45.1
Terraced	12.5	18	38.3
Flats	45	66	15
Number of observations	1372	1564	538

4. Empirical specification

I estimate benchmark DiD models to compare the impact of the policy cancellation on house prices (HP_{it}) and monthly rent prices (RP_{jt}) between treatment and control groups, before and after the announcement. The two equations are:

$$\ln(HP_{it}) = \beta_0 + \beta_1(TreatStation_i \times Post_t) + \beta_2(TreatTrack_i \times Post_t) + \beta_3 TreatStation_i \tag{1}$$

$$\ln(RP_{jt}) = r_0 + r_1(TreatStation_j \times Post_t) + r_2(TreatTrack_j \times Post_t) + r_3 TreatStation_j + r_4 TreatTrack_j + r_5 Post_t + v_{jt} \tag{2}$$

where $TreatStation_i$ and $TreatTrack_i$ are dummy variables indicating whether property i or j is near the station or track and $Post_t$ is a dummy variable that denotes the time period after the treatment (post November 2021). This econometric approach is consistent with the recent literature on DiD estimation and is based on comparing prices (before and after the policy announcement of cancelling HS2) for a treatment and control group. Specifically, we follow Kuminoff (2010) which showed that the "DiD estimator is the most suited to hedonic estimation for panel data." The parallel-trend assumption is of great importance in ensuring the internal validity of DiD models, yet it is the most challenging assumption to satisfy. This assumption asserts that, in the absence of the treatment, the price differential between the treatment and control groups remains constant over time. To assess the validity of this assumption, two sets of time-series data were analysed. **Figure 3** illustrates the average house price for the houses "Near track" and "Near station" in treated areas compared to the non-treated control area for 11 years preceding the announcement of the extension cancellation. Similarly, **Figure 4** depicts the average rent rates for the houses "Near track" and "Near station" in treated areas juxtaposed with the rent rates of houses in non-treated control area for the same pre-treatment period. Upon visual examination of both figures, it is evident that the price series exhibit a approximately parallel trajectory, strongly suggesting that the parallel-trend assumption holds. This finding lends credibility to the validity of my DiD model in the study, as it implies that any observed post-treatment differences in prices between the treatment and control groups can be attributed to the intervention itself, rather than confounding factors or pre-existing disparities.

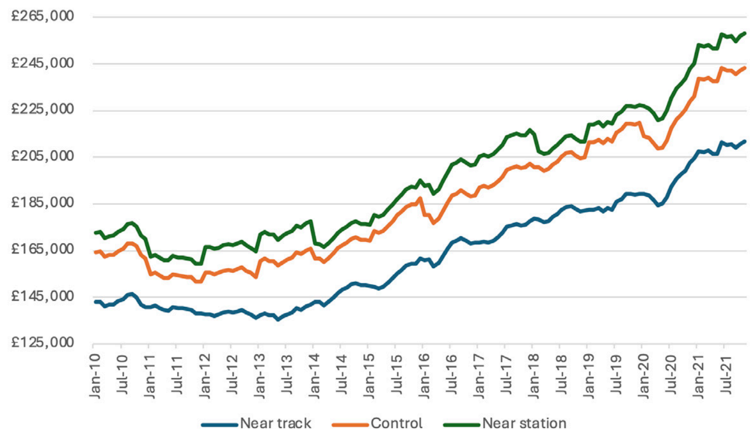

Figure 3: testing parallel trends assumption – house prices.

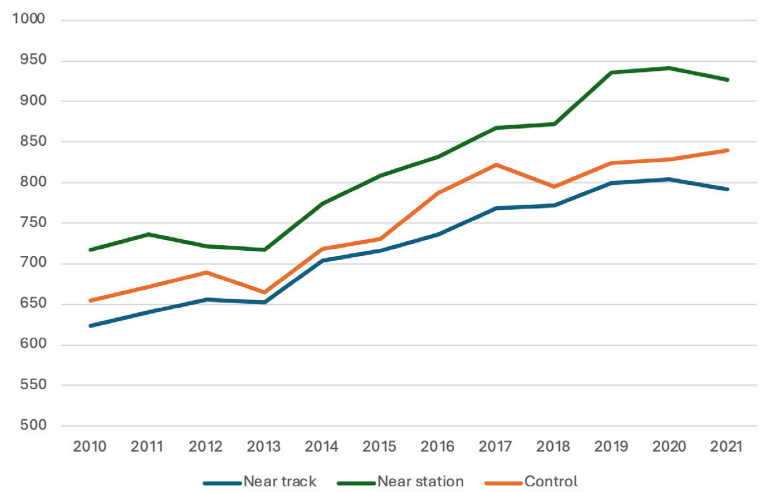

Figure 4: testing parallel trends assumption – rent prices.

To formally test this assumption, I estimate these OLS equations following Rambachan (2020).

$$\text{In}(HP_{it}) = \alpha + \pi_1 TreatStation_{it} + \pi_2 TreatTrack_{it} + \sum_{t=2011}^{2021} \lambda_t D_t + \sum_{t=2011}^{2021} \delta_{1t}(D_t \times TreatStation_{it}) + \sum_{t=2011}^{2021} \delta_{2t}(D_t \times TreatTrack_{it}) + \mu_i + e_i \quad (3)$$

$$\text{In}(RP_{jt}) = \upsilon + \eta_1 TreatStation_{it} + \eta_2 TreatTrack_{it} + \sum_{t=2011}^{2021} \lambda_t D_t + \sum_{t=2011}^{2021} \delta_{1t}(D_t \times TreatStation_{it}) + \sum_{t=2011}^{2021} \delta_{2t}(D_t \times TreatTrack_{it}) + \mu_i + \mu_{it} \quad (4)$$

In this regression, D_t are yearly dummies and the coefficients δ_t are of interest. The results in **Table 3** (shown in the appendix) provide support for the parallel trends assumption in both the "Near track" and "Near station" treatment groups, for both house prices and rents. The majority of the lead coefficients are statistically insignificant at conventional levels, suggesting that the treatment and control groups followed similar trends in the outcome variables prior to the treatment in 2021. For the "Near station" group, the coefficients for house prices and rents are consistently positive but insignificant throughout the pre-treatment period. This indicates that while the "Near station" group had slightly higher house prices and rents compared to the control group, the difference was not statistically significant and remained stable over time. Similarly, for the "Near track" group, most of the lead coefficients are insignificant for both house prices and rents. The coefficients alternate between positive and negative values, but the magnitudes are small and not statistically different from zero, suggesting no systematic differences in the trends between the "Near track" group and the control group. The lack of significant lead coefficients in the pre-treatment period supports the validity of the DiD approach in this context. It suggests that the treatment and control groups were comparable before the treatment, and any differences in the outcome variables after the treatment can be attributed to the causal effect of the treatment, rather than pre-existing differences in trends. However, it is important to note that the parallel trends assumption is fundamentally untestable, as it relies on the counterfactual scenario of what would have happened in the absence of the treatment. While the insignificant lead coefficients provide supporting evidence, they do not guarantee that the assumption holds perfectly.

5. Main results with extensions

My main findings from the DiD estimation for the period 2010 to 2023 are detailed in columns (1) and (2) of **Table 4**. Following Hansen's (2007) recommendations, I have clustered standard errors at the group level to mitigate the issue of intragroup correlation thus enhancing the robustness of my estimates. This approach is particularly suited for my panel data structure, where the non-independence of observations within groups can bias standard error estimates.

The results reveal that the announcement of the HS2 extension cancellation led to a decrease in house prices by approximately 3.6%, significant at the 5% level, near the station. This reduction in price, which translates to a £9,800 decrease in property value based on the average house price of £255,800 in the "Near station" group, highlights the diminished attractiveness of these properties due to reduced transport accessibility. Conversely, properties near the construction site witnessed a 2.4% increase in value, also significant at the 5% level, suggesting an increase in property attractiveness likely due to the elimination of negative externalities like noise pollution and congestion, equating to an increase of £4,500 based on the average house price of £188,200 in the "Near track" group.

Regarding rental prices, it is assuring that a similar trend is observed. Properties near the proposed station experienced a 3.9% decrease in monthly rent, averaging a reduction of £37 based on the average rent of £955 in the "Near station" group. This decline is significant at the 5% level and may reflect the high value placed on transport accessibility by renters, possibly driven by a higher proportion of young working professionals in this demographic. Conversely, rent prices near the construction site saw a 2.1% increase, adding a monthly premium of £17 based on the average rent of £815 in the "Near track" group, significant at the 5% level. My results show that rental prices are slightly more responsive to changes in local amenities and economic conditions than house prices, as renters are generally more mobile than homeowners as reiterated by Glaeser (2007).

These results, while significant, show a smaller magnitude of change compared to existing literature on transport infrastructure impacts on property values. For instance, studies such as Smith and Gihring (2006) report larger impacts, potentially due to their immediate operational timelines compared to the HS2 station's expected operational date in 2033 for the Leeds area, with construction expected to begin only in 2026. This suggests that the estimated effects in this study are discounted for the future, reflecting a delayed realisation of benefits and costs.

5.1 Extension 1: controlling for dates

In my DiD estimation, spanning an extensive pre- and post-reform period, there is a risk of bias if the control and treatment groups have properties sold at different times. It also might be the case that rented properties appear again on the market at systematically different times. This concern persists even if the average transaction dates are similar across groups, as the distribution of sales might vary, introducing bias during specific economic cycles, such as those influenced by the COVID-19 pandemic. For example, a concentration of sales in one group during a boom year like 2022 could skew the results. To mitigate this, the results in columns (3) and (4) of **Table 4** use the UK house price index and the UK private housing rental index to normalise all sales prices to the 2015 baseline. This normalisation aligns the prices to a common reference year, controlling for potential discrepancies in timing between groups. This method not only preserves our full sample size, enhancing the statistical power and precision of our estimates, but also ensures that our findings are not distorted by temporal fluctuations in the market. This approach provides further robustness for my results in evaluating the true impact of the reform, free from the biases associated with varying sales dates. With normalised prices, the coefficients are slightly larger in magnitude but in the same expected direction and significant. This suggests the effect of policy cancellation is more pronounced on house and rent prices after I control for time-confounding factors like inflation or general market trends.

It's also worth noting that my paper only considers repeat sales of houses sold or rented multiple times between 2010-2023. Houses sold or rented more often, which are more likely to be included, may fundamentally differ from other houses. For example, families with a low MWTP for transport accessibility and short commutes might keep their homes for longer and be indifferent to transport improvements, potentially leading to their exclusion from the dataset and an overestimation of the true MWTP for the entire population. However, it can be argued that the extensive window period of approximately 13 years mitigates the impact of such bias, rendering it relatively minimal.

5.2 Extension 2: heterogenous effects by property type

An interesting question to test is whether the transport accessibility premium is more prominent for property owners who are more likely utilise public transport, such as for commuting. My dataset includes various residential properties, including flats and houses (detached, semi-detached, terraced). Flat owners, often younger professionals, may rely more on nearby transport accessibility compared to higher-income homeowners (Femenias, 2020). To test this hypothesis, I'll examine heterogeneous effects of transport accessibility on prices based on property type. Houses in the full sample may attenuate the estimated premium, as their owners are less reliant on public transport. To isolate the premium for the hypothesised target demographic, I re-estimated models (1) and (2) using only flats as a subsample. This removes the influence of other property types, allowing me to estimate the transport accessibility premium for the group hypothesised to value it most. It is reasonable to assume that the impact of being located near the construction site ("Near track" area) and the corresponding coefficient estimate will be similar across property types, as the negative externalities from the construction activities are likely to affect all properties in proximity with minimal heterogeneity in resident preferences. My work extends the existing literature by investigating heterogeneous effects of transport accessibility on residential property prices based on property type, an area that has received minimal attention thus far. My results are reported in **Table 5** and are broadly in line with the hypothesis. It is important to note that standard errors are noticeably larger due to a smaller sample size, rendering some of the leading DiD coefficients not significant at the 5% level. My results show that house prices fell by 4.1% with rents falling by 4.3% in the "Near station" group whereas house prices increased by 3.2% with rents going up by 1.9% in the "Near track" group. Intuitively these results are as expected which is comforting and in line with the limited literature on this topic (Wardrip, 2011). I also report the results after normalisation in columns (3) and (4).

Table 4: Main regression results

	Without normalization		With normalization	
	(1)	(2)	(3)	(4)
	House prices	Rent prices	House prices	Rent prices
Constant	10.701***	2.234***	10.589***	2.197***
	(0.0214)	(0.0227)	(0.0198)	(0.0209)
TreatStation	0.0542*	0.1015*	0.0624*	0.1132**
	(0.0296)	(0.0604)	(0.0381)	(0.0568)
TreatTrack	-0.1214*	-0.0428***	-0.1362**	-0.0517***
	(0.0736)	(0.0129)	(0.0615)	(0.0117)
Post	0.503***	0.631***	0.521***	0.649***
	(0.0211)	(0.0294)	(0.0203)	(0.0321)
TreatStation × Post	-0.0358**	-0.0391**	-0.0427**	-0.0436**
	(0.0181)	(0.0191)	(0.0199)	(0.0197)
TreatTrack × Post	0.0236**	0.0214**	0.0285***	0.0251***
	(0.0104)	(0.0106)	(0.0097)	(0.0095)
Within R^2	0.3375	0.3606	0.3492	0.3721
Observations	6508	3474	6508	3474

Robust standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Regression results with only flats

	Without normalization		With normalization	
	(1)	(2)	(3)	(4)
	House prices	Rent prices	House prices	Rent prices
Constant	8.105***	1.515***	7.895***	1.474***
	(0.0516)	(0.0924)	(0.0482)	(0.1802)
TreatStation	0.0721**	0.0925*	0.0847*	0.1023**
	(0.0371)	(0.0561)	(0.0484)	(0.0547)
TreatTrack	-0.0944*	-0.0438**	-0.1224**	-0.0531**
	(0.0555)	(0.0237)	(0.0712)	(0.0253)
Post	0.516***	0.591***	0.537***	0.617***
	(0.0351)	(0.0448)	(0.0314)	(0.0401)
TreatStation × Post	-0.0411*	-0.0437	-0.0417*	-0.0436*
	(0.0235)	(0.0273)	(0.0232)	(0.0237)
TreatTrack × Post	0.0326*	0.0195*	0.0385**	0.0231*
	(0.0184)	(0.0115)	(0.0147)	(0.0114)
Within R^2	0.3157	0.3006	0.3529	0.3512
Observations	2572	1730	2572	1730
Flats only	Yes	Yes	Yes	Yes

Robust standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

6. Robustness checks

6.1 Placebo tests

To confirm the validity of my findings and avoid the risk of spurious results, I conducted four placebo tests by changing the treatment date from November 2021 to hypothetical dates. The results, detailed in **Table 6**, show that most of the coefficients are not statistically significant at conventional levels. This lack of significance further lends support to the parallel trends assumption, which is essential for the validity of my methodology. Consistent with Orefice (2010), these placebo tests reinforce the robustness of my main results, demonstrating that the observed effects are genuinely due to the intervention and not to uncontrolled confounding variables.

6.2 Restricting the time period

Adopting a restricted timeframe enhances the likelihood that unobserved housing characteristics remain constant or do not diverge significantly between the treatment and control groups, providing robustness for my results. By narrowing the time period, I reduce potential biases that could violate the common trends assumption and increase the validity of the estimates as a reflection of the MWTP for attributes such as transport accessibility, in line with Botosaru et al (2017).

Table 6: Placebo Test Results

	House prices	Rent prices
Jan 2013		
TreatStation × Post	-0.0140	0.0138
	(0.0152)	(0.0162)
TreatTrack × Post	0.0117	-0.0158*
	(0.0109)	(0.0096)
Within R^2	0.4371	0.4103
Observations	6508	3474
Jun 2016		
TreatStation × Post	-0.0125	-0.0192
	(0.0161)	(0.0173)
TreatTrack × Post	-0.0206*	0.0199*
	(0.0117)	(0.0123)
Within R^2	0.2974	0.3108
Observations	6508	3474
Oct 2019		
TreatStation × Post	0.0238	0.0255
	(0.0156)	(0.0168)
TreatTrack × Post	-0.0225*	0.0182
	(0.0121)	(0.0119)
Within R^2	0.3373	0.3607
Observations	6508	3474
Mar 2022		
TreatStation × Post	0.0309	-0.0359
	(0.0211)	(0.0227)
TreatTrack × Post	0.0247	-0.0227
	(0.0152)	(0.0146)
Within R^2	0.2376	0.1209
Observations	6508	3474

Robust standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Furthermore, this constrained temporal focus helps avoid the influence of external shocks that could affect the groups differently and skew the results. I estimated equations (1) and (2) over a limited time period surrounding the policy shock in November 2021, mirroring the empirical method used by Chay and Greenstone (2005). In their seminal analysis on the impact of air quality regulations on housing values, they concentrated on a narrow two-year window around the policy implementation to avoid biases from unobserved, time-varying factors.

The original results use sales and rent price data from 2010-2023 whereas **Table 7** shows the additional results that restrict my timeline to 2015-2023; 2018-2023 and 2020-2022. Narrowing the time period introduces a notable bias-variance trade-off because standard errors are greater for a smaller sample size. This issue is particularly critical given that transport accessibility may only constitute a small fraction of house values, making it challenging to distinguish this effect from random variation. My analysis indicates that while the coefficients remain relatively stable as the time period is restricted, the standard errors increase significantly. Although the consistency of the magnitudes of coefficients across different time restrictions is reassuring, the larger standard errors lead to most of the coefficients being statistically insignificant at the 10% level.

Table 7: DiD results with restricted dates

	2015-2023		2018-2023		2020-2022	
	House prices	Rent prices	House prices	Rent prices	House prices	Rent prices
Constant	10.362***	2.066***	10.53***	2.032***	10.64***	2.076***
	(0.0227)	(0.0295)	(0.0406)	(0.0527)	(0.092)	(0.119)
TreatStation	0.0825***	0.0948***	0.127**	0.1461*	0.121	0.127
	(0.0313)	(0.0359)	(0.0569)	(0.0712)	(0.1446)	(0.1845)
TreatTrack	-0.067***	-0.0536**	-0.0532	-0.0325	-0.0716	-0.0651
	(0.0204)	(0.0216)	(0.0345)	(0.0276)	(0.0878)	(0.126)
Post	0.461***	0.599***	0.248***	0.285***	0.2338	0.258
	(0.0428)	(0.0374)	(0.0845)	(0.0521)	(0.215)	(0.318)
TreatStation × Post	-0.0392**	-0.0365*	-0.0348*	-0.0357	-0.0214	-0.019
	(0.0167)	(0.0179)	(0.0153)	(0.0268)	(0.132)	(0.275)
TreatTrack × Post	0.0272*	0.0227*	0.0211	0.0251	0.0431	0.0398
	(0.0173)	(0.0163)	(0.0271)	(0.0239)	(0.176)	(0.296)
Within R^2	0.241	0.228	0.134	0.142	0.084	0.079
Observations	4002	2135	2497	1336	936	534
Normalised prices	No	No	No	No	No	No

Robust standard errors in parentheses
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

6.3 Changing the size of treated areas

I selected a 1 km radius for the "Near track" and "Near station" areas, aligning with the consensus in existing literature. I appreciate that my results might be sensitive to this choice, as the effects intuitively diminish with increased distance from the station or construction site. For instance, Benjamin (1996) employs a hedonic pricing model with fixed effects to demonstrate that rents in the Washington DC area rise by 2.5% for every 0.5 km closer to the station. Ideally, I would conduct robustness checks with varying radii; however, practical constraints, primarily time limitations, prevented this. Further, it was not feasible to employ commuting time distance, as sourced from Google Maps, as a metric for radius measurement. I note that commuting distance would serve as a superior indicator, given that actual travel distances bear greater relevance than mere linear straight line measurements. This issue is somewhat mitigated in the context of the "Near Track" group, where the impact of negative externalities such as noise and construction disruption is less reliant on travelling distance but proximity.

As depicted in **Figure 2iii**, the dashed circles represent the areas where I conducted my new benchmark regressions. My results are reported in **Table 8** and although the magnitude of the coefficients is lower than before, this is expected as the inclusion of houses further from key sites dilutes the perceived importance of transport accessibility and the disutility from expected construction. It is noteworthy that the DiD estimators are significant at the 10% level. It is encouraging that my results align with existing literature and offer new insights into the spillover effects of infrastructure projects. These findings can inform future policy decisions, such as the strategic selection of construction sites to minimise the negative impact on property values.

Table 8: DiD results with larger radii

	House prices	Rent prices
Constant	10.537***	1.983***
	(0.0198)	(0.0204)
TreatStation	0.0487**	0.093*
	(0.0234)	(0.0517)
TreatTrack	-0.0813***	-0.0532*
	(0.0473)	(0.312)
Post	0.528***	0.625**
	(0.0281)	(0.0438)
TreatStation x Post	-0.0314*	-0.0212*
	(0.0167)	(0.0115)
TreatTrack x Post	0.0208*	0.0172*
	(0.0129)	(0.0089)
Within R^2	0.2869	0.3065
Observations	7954	4215
Normalised prices	No	No

Robust standard errors in parentheses
***p<0.01, **p<0.05, *p<0.1

7. Concluding remarks

In this study, I utilised the November 2021 policy announcement regarding the cancellation of the HS2 extension as a quasi-experiment to explore the impact of transport accessibility and the associated negative externalities of large infrastructure projects. Employing a robust DiD methodology enabled cleaner comparisons of pre- and post-outcome variations between affected and unaffected areas, enhancing the validity of my results over existing literature (Meha, 2017). My empirical analysis showed that house prices in the "Near station" area decreased by an average of 3.6%, while monthly rents decreased by 3.9% due to diminished future transport accessibility. Conversely, house prices in the "Near track" area rose by 2.4%, with rents increasing by 2.1%, attributed to the reduced exposure of negative externalities. The estimates are slightly muted compared to previous studies (Banister and Goodwin, 2011) possibly due to a discounting effect given the expected operational date of the HS2 station in Leeds was set for 2033, with construction starting in 2026. I also examine heterogeneous effects by property type and find that flats experienced a more pronounced impact compared to houses, in line with expectations.

The generalisability of my findings to other cities facing similar transport infrastructure changes is an important consideration. While the specific context of Leeds, including its housing market characteristics, economic conditions and demographic factors may differ from other areas, the underlying mechanisms through which transport accessibility and construction externalities affect housing markets are likely to be similar. My robust DiD methodology, controlling for unobserved housing characteristics, enhances the external validity of the results.

Future research could extend my static analysis in several directions. A dynamic DiD model would capture anticipatory and adjusting behaviours by households in response to policy changes. Indeed, leading up to 2021, several parts of the project were already cancelled, suggesting that some effects might have already been priced into the housing market. Furthermore, following the Prime Minister's announcement in October 2023 to redirect HS2 funds for the "levelling up" scheme, the observed changes in property values may be offset. Finally, conducting similar studies in the other cities affected by HS2 extension cancellation, such as Manchester or Crewe, would allow for a more holistic assessment of the policy's impact and generalisability of the findings.

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IX. Appendix

Table 3: Non-parametric test for parallel trends assupmtion

	House prices	Rent prices
Near station x 2011	0.0062	0.0041
	(0.0117)	(0.0100)
Near station x 2012	0.0063	0.0037
	(0.0129)	(0.0110)
Near station x 2013	0.0075	0.0042
	(0.0138)	(0.0118)
Near station x 2014	0.0041	0.0022
	(0.0122)	(0.0104)
Near station x 2015	0.0060	0.0033
	(0.0133)	(0.0114)
Near station x 2016	0.0055	0.0030
	(0.0141)	(0.0121)
Near station x 2017	0.0064	0.0035
	(0.0149)	(0.0127)
Near station x 2018	0.0047	0.0025
	(0.0155)	(0.0132)
Near station x 2019	0.0067	0.0036
	(0.0162)	(0.0138)
Near station x 2020	0.0071	0.0038
	(0.0168)	(0.0144)
Near station x 2021	0.0083	0.0045
	(0.0131)	(0.0112)
Near track x 2011	0.0019	0.0010
	(0.0103)	(0.0088)
Near track x 2012	-0.0011	-0.0006
	(0.0114)	(0.0097)
Near track x 2013	0.0017	0.0009
	(0.0121)	(0.0103)
Near track x 2014	0.0025	0.0014
	(0.0107)	(0.0091)
Near track x 2015	-0.0009	-0.0005
	(0.0118)	(0.0101)
Near track x 2016	0.0023	0.0012
	(0.0125)	(0.0107)
Near track x 2017	0.0031	0.0017
	(0.0132)	(0.0113)
Near track x 2018	0.0022	0.0012
	(0.0137)	(0.0117)
Near track x 2019	0.0034	0.0018
	(0.0143)	(0.0122)
Near track x 2020	0.0041	0.0022
	(0.0149)	(0.0127)
Near track x 2021	-0.0018	-0.0010
	(0.0116)	(0.0099)
Within R^2	0.791	0.762
Observations	6508	3474
Unit FE	Yes	Yes