The Effect of Land Use Regulations: Evidence from the Overturning of Local Planning Authorities' Decisions in the United Kingdom*

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Abstract

Housing supply is often constrained by local politics. In this paper, we examine what happens to the prices of existing homes when developments that were originally blocked by local governments end up being built. We do so in the context of the United Kingdom where developers who have their plans rejected by local authorities can appeal to the national government. Our empirical strategy exploits the quasi-random assignment of inspectors to appeals. We find that overturning local authorities' decisions does not lead to a large fall in housing prices and in some cases, may even lead to an increase in prices. We provide theoretical and empirical evidence to rationalize this puzzle of local authorities blocking developments that could actually raise the value of existing homes in the vicinity.

Keywords: Urban economics, housing supply and demand, housing prices, land use regulations.

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1 Introduction

Housing supply is often constrained by local politics (Glaeser, 2014; Gyourko and Molloy, 2015) and is particularly salient as many countries around the world have both local and national governments. In some countries like the United Kingdom (UK), there is even an increasing shift towards localism (Cheshire et al., 2014).

Traditional urban political economy theories (Fischel, 2001) tend to emphasize that local governments restrict housing supply to keep housing prices high for the benefit of homeowners. Such theories would suggest that an increase in new housing supply, which was originally blocked by local governments, will lead to a decline in the prices of existing homes. However, in this paper, we show that this may not always be the case. Instead, we highlight that the effect on the prices of existing homes depends on the type of developments being built (particularly, whether they come with attractive amenities) and the amount of local power in the area.

The setting of our study is the UK where ownership alone does not confer the right to develop the land. Despite owning the land, individuals and developers have to still apply for development permission from their local planning authority. Applicants who have their plans rejected can appeal to the Secretary of State, via the Planning Inspectorate. The Planning Inspectorate then assigns an inspector to decide whether to overturn the local authority's decision. As the data on the appeals were extremely patchy and often involved "greenfield" sites with no existing addresses, we had to manually geocode and check the location of thousands of appeals.

Having assembled the dataset, the results from our OLS "stacked" difference-in-differences (DiD) suggest that successfully overturning the local authority's decision has a small negative effect of around 0.6% on the prices of existing homes in the area (compared to an unsuccessful appeal). The 95% confidence interval is able to rule out an impact more negative than -1.2% and we do not find evidence of pre-trends in our DiD. Despite being relatively small in magnitude, this negative result is consistent with the argument that local authorities restrict housing supply to maintain housing values in the area.

While an instrumental variable (IV) approach is typically used to address "endogeneity" issues, in the presence of heterogenous treatment effects, it can also be used as an estimation method to recover the Local Average Treatment Effect (LATE) for the "compliers", even when there are no "endogeneity" issues.[1] We leverage this characteristic to examine

[1]Under homogenous treatment effects, in the absence of "endogeneity", both OLS and IV estimators are consistent but the OLS estimator is more precise. With heterogenous treatment effects, in the absence of "endogeneity", OLS estimates an average treatment effect while IV estimates the treatment effect for "compliers" – those that are affected by the instrument. We verify this in Appendix A3.

whether the effect of successful appeals on the prices of existing homes differ based on whether the appeals were assigned more lenient inspectors (i.e., the "compliers"). Our IV strategy exploits the fact that inspectors were quasi-randomly assigned to the appeal cases and that they have different preferences. The leniency of the inspectors can hence be used as an instrument for whether an appeal is successful.[2]

For these "compliers", we find that overturning the local authority's decision does not lead to a large fall in housing prices. Our IV estimates show a positive impact of around 5.6% from overturning decisions, suggesting that in some instances, the local authorities' decision to constrain housing supply may actually be harming instead of benefitting homeowners. This positive effect allows us to infer some characteristics about the "complier" subpopulation that underlies our IV's LATE. Given that housing supply has increased, the increase in the prices of existing homes must be due to the presence of positive demand shifters. Using annual point-of-interest (POI) data by location, when running an IV regression, we find statistically significant evidence that the count of local services increased but there was no effect in the OLS regression. This suggests that the positive impact for the "compliers" may be driven by an improvement in local amenities which increased demand for the neighborhood.

Our results are consistent with a simple demand and supply model. For the OLS, while there was an increase in supply, there was no effect on amenities. This explains the negative results from the OLS. For the "compilers" in the IV, while there was an increase in supply, there was an accompanying increase in local amenities which more than offset the supply shock. This accounts for why the IV estimates are positive.

The IV results suggest that local authorities are blocking attractive developments that actually raise the value of existing homes in the vicinity. Why would they do this? To rationalize this puzzle, we provide a theoretical framework which models the development application and approval process as a multi-stage sequential game between local planning authorities and inspectors. We model the local authorities' utility as a weighted sum of the utility of homeowners who choose to remain in the area ("insiders") and the utility of prospective homeowners moving into the area ("outsiders"). Existing homeowners prefer the value of their property to be high while prospective homeowners prefer lower housing prices so that they can afford to move into the location.[3]

[2]The use of expert leniency as an instrumental variable has been used widely in the economics literature. For example, the leniency of judges has been used to estimate the impact of eviction on poverty (Humphries et al., 2018); incarceration on economic and family outcomes (Aizer and Doyle Jr, 2015; Bhuller et al., 2020); pretrial detention on legal and economic outcomes (Dobbie et al., 2018); foster care on child outcomes (Doyle Jr, 2008); disability on labour supply (Dahl et al., 2014; Kostøl et al., 2019); and even the effect of patents on innovation (Galasso and Schankerman, 2015).

[3]Another interpretation is that these "outsiders" are renters.

Depending on whether demand or supply effects dominate, new developments may either raise or lower the prices of existing homes in the area. Therefore, in deciding which developments to approve, local authorities have to balance the interest of "insiders" and "outsiders". We show that this results in a "threshold" that determines which developments are allowed or blocked by the local authorities (proposition 1). Appeals that are overturned by the inspectors are likely to be those involving developments that are just to the right or left of this "threshold". We term such developments marginal ones.

One prediction from our framework (proposition 2) is that in areas with a greater share of "insiders", overturning the local authority's decision should not lead to large changes in housing prices. This is because the local authority has already taken locals' interest into consideration by allowing most of the attractive developments to be built. Consequently, the marginal developments that go through as a result of the inspectors' decisions are those that do not contain as many attractive amenities and hence, do not lead to big changes in the prices of existing homes.

By contrast, in areas where there are more "outsiders", overturning the local authority's decision will lead to a bigger increase in housing prices. This is because in these areas where "outsiders" dominate, the marginal developments around the local authority's "threshold" have substantially stronger demand effects. When these marginal developments end up being built, they lead to bigger changes in the prices of existing homes. This could explain why our IV estimates are positive – the "compliers" in our IV are from areas with more "outsiders".

To provide further support for our theoretical explanation, we split our sample into areas with a high share of households who owned their accommodation ("insiders") and areas with a low share of homeowners ("outsiders"). Consistent with the predictions of our theoretical framework, when we estimate our IV regressions on the different subsamples, we indeed find that successful appeals in areas with more "outsiders" led to a larger increase in housing prices. On the other hand, there was no statistically significant effect in areas with more "insiders".

Our paper contributes to the burgeoning literature on the effects of land use regulations in two ways.[4] First, our paper demonstrates why it is incomplete to view land use

[4]Examples of empirical studies that examine the effects of land use regulations include Albouy and Ehrlich (2018); Shertzer et al. (2018); Turner et al. (2014); Libecap and Lueck (2011); Saiz (2010); Glaeser et al. (2005); Quigley and Raphael (2005); Hilber and Vermeulen (2016); Hsieh and Moretti (2019); Koster (2020); and Cheshire and Sheppard (2002). More broadly, our paper is related to the literature which examines the effects of housing policies. Examples of housing policies other than land use regulations include, urban revitalization (Greenstone and Gallagher, 2008; Rossi-Hansberg et al., 2010); direct provision of housing (Collins and Shester, 2013); price controls (Autor et al., 2014; Diamond et al., 2019); and taxes and subsidies (Baum-Snow and Marion, 2009; Collinson and Ganong,

regulations as simply being a supply shock.[5] Before Turner et al. (2014), the literature often took contradictory positions as to whether increases in housing prices indicate welfare increases or decreases. One position was that price increases reflected welfare improvements. This view is predicated on viewing land use regulations as improving the amenities of an area and hence a demand shock. The other position viewed land use regulations as a supply shock and hence led to welfare losses for existing homeowners in the locality. Following Turner et al. (2014), we model land use regulations as being both a demand and supply shock. We then show how this is important in determining what happens to housing prices when local authorities' decisions are overturned.

Second, we show how the incentives of actors in the local political process are crucial in explaining why attractive developments are sometimes prevented from being built. Hilber and Robert-Nicoud (2013) and Glaeser et al. (2005) show how developers react to the local political process while Parkhomenko (2023), Ortalo-Magné and Prat (2014), Calabrese et al. (2007), Epple et al. (1988) and Hamilton (1975) show that land use regulations are ways to prevent certain types of households from entering a community. We contribute to this literature by showing how the interaction of local politics and national politics has interesting implications not only on housing supply, but also on the types of developments that are allowed to be built.

Finally, our paper is related to the literature on the economic effects of the decentralization of policy responsibilities to local governments. A review by Pike et al. (2012) suggests that there is no clear relationship between decentralization and economic outcomes.[6] However, much of this literature does not establish causality. By examining how the decisions of local governments are exogenously overturned, our paper attempts to contribute to this literature.

2018; Diamond and McQuade, 2018). Since land use regulations are a form of place-based policies, our paper relates to the literature on the use of place-based polices to rectify regional disparities. Kline and Moretti (2014) provide a good overview on the economics of place-based policies. In addition, because land use regulations limit the size of cities, our paper is also related to the literature on optimal city size. Au and Henderson (2006) find that migration restrictions have resulted in many undersized cities in China. To the extent that there are huge benefits from urban agglomeration, the costs of being undersized due to land use regulations are potentially high.

[5] Gyourko and Molloy (2015) note that the predicted effects of regulations become less obvious if households can move freely among cities. This is because under spatial equilibrium, utility must be equal across cities. Therefore, population flows can erode away price differences across locations. The price differences across areas thus reflect the amenity value of growth controls and not the lower elasticity of housing supply.

[6] For example, Zhang and Zou (1998) find that decentralization is associated with lower economic growth at the provincial level in China. However, Akai and Sakata (2002) and Stansel (2005) find a positive relationship between decentralization and economic growth in the US. Yet another group of studies such as Davoodi and Zou (1998), Xie et al. (1999) and Rodríguez-Pose and Bwire (2004) find that there is no link between decentralization and economic outcomes.

The rest of the paper proceeds as follows. Section 2 provides the background to land use regulations in the UK and the rise of local power. Section 3 describes the data sources which we use for our analysis. Section 4 explains our empirical strategy and Section 5 reports our main results. Section 6 presents a theoretical framework to explain the puzzle behind our results. We also provide empirical evidence in support of the theoretical framework and empirically rule out alternative explanations. Finally, Section 7 concludes.

2 Background to land use regulations in the UK

Land use regulations in the UK has a long and storied history – Corkindale (1999) notes that as early as 1540, Queen Elizabeth I forbade any new buildings within three miles of the City gates of London. The present-day land use regulations in the UK have their roots in the Town and Country Planning Act which came into law in 1947. The purpose of the 1947 act was to contain urban areas and stop them from spilling out into the surrounding countryside, as well as preserve amenities of various kinds. The act also separated land uses which might be incompatible (e.g., industry from residential). At the same time, the act also aimed to provide lower density and greener living conditions in the new towns.

To do so, the act set out the principle of "development control" which means that planning permission is required for land development – ownership alone no longer conferred the right to develop the land. As a result, owners had to apply and seek permission from their local planning authorities whenever they wanted to develop their land. The local planning authorities would only approve the development if it is consistent with the local development plan.[7] The UK system therefore controls independently of prices, the amount of land available not just for housing, but for all urban uses of land including offices, retail and commercial space.

There have been many modifications since 1947.[8] For example, in the 1950s, the restrictions were tightened as "Greenbelt" boundaries were established. This resulted in increasing amounts of land around towns and cities being taken out of the effective land supply. Later revisions of the act were legislated in 1962, 1971 and 1990. While the 1990 act is the current legislation, the act has been substantially amended and added to, for example, through the use of legislative orders. Nonetheless, the 1947 act established an

[7]Today, many parts of the UK (particularly England) have three tiers of local government – (i) county council; (ii) district; borough or city councils; and (iii) parish or town councils. The district councils are the ones that are typically responsible for most planning matters including preparing local development plans and approving planning applications.

[8]Each country within the UK has its own variations in their planning system. Since the English system is the dominant one, the details which we provide here are based on the system in England.

approach and framework that has not been superseded.

Today, the main planning acts that are in force in England are the Town and Country Planning Act (1990), Planning and Compulsory Purchase Act (2004), Planning Act (2008) and Localism Act (2011). These acts are also read alongside a series of planning policy documents and guidelines such as the National Planning Policy Framework which was published in March 2012. Other national policies and restrictions such as "Greenbelts", "Sites of Special Scientific Interest" and "Areas of Outstanding Natural Beauty" also form part of the planning regulations. Many of the recent reforms, such as the Localism Act (2011), were aimed at giving locals more power in deciding land use in their neighborhood.[9,10]

A planning application needs to be submitted to the local authorities if an individual or developer wants to (i) build something new; (ii) make a major change to the building (e.g., building an extension); or (iii) change the use of the building. In determining whether to grant planning permission, the local authority has to assess whether the proposed development is consistent with (i) national policies; (ii) the local plan; and (iii) the neighborhood plans (if any exists).[11]

Applicants who have their plans rejected can appeal to the Secretary of State, via the Planning Inspectorate. The Planning Inspectorate then assigns one inspector to the case to decide whether to overturn the local planning authority's decision.[12] Data from

[9]For example, neighborhood plans give communities the direct power to shape the development and growth of their local area. The neighborhood plans are extremely powerful. This is because when considering whether to grant planning permission, the local authorities have to ensure that the proposed development is consistent with the neighborhood plans.

[10]The UK's land use regulations are typical of the planning systems in OECD countries. Common features include: (i) land use planning is predominantly a local task but involves some coordination between different levels of government; (ii) formal stakeholder engagement involves a public consultation process; (iii) land use regulations are well enforced; (iv) taxes are levied on the increases in land value from redevelopment; and (iv) the public expropriation of land for the construction of infrastructure is possible.

[11]Since most developments put a strain on existing infrastructure such as roads, schools and open spaces, the local planning authorities can impose a Community Infrastructure Levy (a charge which new developments pay, based on the size and type of development) to mitigate the impact of the proposed development. The Levy collected is then used to fund a wide range of infrastructure needed to support the development of the area. Alternatively, under Section 106 of the Town and Country Planning Act (1990), the local planning authority can also grant permission in return for some specified gain to the community. For example, the local planning authority may require that the developer provides a certain amount of affordable housing. This gives rise to a negotiation process between the local planning authority and the developer. More details on the stages involved in the planning application process can be found at GOV.UK.

[12]The Secretary of State has the power to take over the decision making from the Planning Inspectorate if the case raises issues that justify a Ministerial decision. In such cases, a planning inspector will submit a report and recommendation to the Secretary of State. Taking into account the inspector's

the UK's Ministry of Housing, Communities & Local Government indicate a rejection rate of around 20% for all planning applications for major residential developments in the UK from the fourth quarter of 2012 to the fourth quarter of 2017. Based on our reading of a sample of appeals, common reasons cited for the rejection of plans include (i) detrimental impact on neighboring amenities (e.g., loss of green or communal spaces); (ii) negative effect on character and appearance (e.g., proposed architecture of housing is not consistent with that of other buildings in the area); and (iii) traffic and parking pressures (e.g., additional residents would lead to severe congestion of local roads).

Among the major residential developments that were rejected, 83% appealed the decision even though the mean turnaround time for appeals was around 26 weeks.[13] Of these appeals, about 43% of them were successful. The high rate of appeals suggests that the costs of not appealing (relative to appealing) is high. By not appealing, developers may not be able to build at all, or may have to rework their plans according to the local planning authority's requirements, resulting in the loss of expected profits. By contrast, the administrative costs of lodging an appeal is fairly low (the developer just needs to fill out a form and forward the documents that were part of the submission to the local planning authority).

In order to understand how inspectors are assigned to cases, we spoke to a representative from the Planning Inspectorate. The representative shared with us that the Planning Inspectorate uses an algorithm as well as human judgment to decide on the assignment of inspector to cases. First, the algorithm uses the (i) complexity of the case;[14] (ii) inspectors' qualifications and experience; and (iii) how far away each inspector lives from the location of the proposed development to identify a list of suitable inspectors.[15] Second, a case worker then manually goes through the list and excludes inspectors who (i) already have a heavy case load; (ii) recently had a case in the area where the development is being proposed; (iii) live in the area of the proposed development; (iv) previously worked for a consultancy firm that is involved in the appeal; and/or (v) have personal difficulties such as illness. This means that conditional on these variables, the assignment of inspectors to cases is as good as random. This forms the basis of our empirical strategy to identify the causal effects of having a successful appeal.

assessment of the proposals, the Secretary of State will then make a decision.

[13] Analysis from LandTech.

[14] For example, the size of the development, whether the proposed development is in a "Greenbelt", agricultural area, "Area of Outstanding Natural Beauty", "Sites of Special Scientific Interest", etc.

[15] The Planning Inspectorate maintains a list of inspectors where they classify inspectors based on the inspectors' suitability to assess cases according to the different levels of case complexity.

3 Data

3.1 Description of datasets

The data we use are from four sources: (i) the Planning Inspectorate's Appeals Casework Portal (ACP); (ii) the UK Land Registry's Price Paid Database (PPD); (iii) the Domestic Energy Performance of Buildings Registers; and (iv) the Royal Mail Postal Address File (PAF).

Planning Inspectorate's Appeals Casework Portal (ACP). The ACP is the same database that the Planning Inspectorate caseworkers use to assign cases to inspectors and to manage cases. The ACP dataset that we use contains decisions on appeals starting from October 1, 2012 to December 28, 2018.

Table 1: Summary of ACP dataset

Fields	Description
Local Planning Authority (LPA) details	Name of local planning authority (LPA), LPA code, LPA's case ID (the case ID allows us to search for documents related to the appeal, e.g., site maps)
Key dates	Date when the appeal was received, started and decided
Inspector	Names of the inspectors
Development details	Residential or commercial; address; floor space; site area; number of residences; whether the proposed development is in a "Greenbelt", "Area of Natural Beauty", "Sites of Special Scientific Interest", or agricultural area; whether it involves a conservation area or historical building
Others	Type of appeal; whether a planning consultant was hired; whether a bespoke timeline was agreed between the parties; whether the inspector ordered the local planning authority to bear the cost of the developer's appeal

Since the ACP database is also used by the Planning Inspectorate for casework management, most fields are accurate.[16] There is, however, one key field that is subjective because it is filled in by the developer – development type. Therefore, we do not use this variable to construct our sample. In addition, the addresses for the site of the appeals were extremely patchy or often involved "greenfield" sites with no existing addresses. To overcome this, we downloaded the application and supporting documents to manually check and geocode the location of thousands of appeals.

UK Land Registry's Price Paid Database (PPD). For tax purposes, the PPD

[16]At the very least, they are consistent with the information the Inspectorate had in deciding the appeal.

contains the registered price of all residential property transactions. The version of the dataset that we are using runs from January 1, 2010 to January 31, 2020. It includes the address of the property; the property type (i.e., detached, semi-detached, terrace or flat); whether the property is a new built; and whether the property is freehold or leasehold. Crucially, the PPD does not include specific characteristics of the property such as the floor area or the number of rooms. Therefore, to obtain these variables, we use the Domestic Energy Performance of Buildings Registers.

Domestic Energy Performance of Buildings Registers. By law, Energy Performance Certificates (EPCs) are needed whenever a property is built, sold or rented. Besides the address of the property and its energy rating, this dataset also includes specific characteristics of the property like property type, floor area and number of rooms. We match the EPC data to the PPD data using the matching key that was put together by the Urban Big Data Centre (UBDC), a research centre based at the University of Glasgow (UBDC, 2023).

Royal Mail Postal Address File. This database contains all addresses and location of residential units in the UK. We use it to construct estimates of housing stock near the appeal site.

3.2 Sample for regression

We are interested in estimating the effect of a successful appeal on the prices of existing homes in the vicinity. To obtain the sample for our regression, we apply a number of sample restrictions.

3.2.1 Selection of appeals sample

Include only appeals involving residential developments that add 10 or more dwellings. There are a number of reasons why we apply this sample restriction. First, as noted in our background to land use regulations in the UK, almost any substantial changes one makes to a property requires planning permission – this includes, for instance, a loft conversion. This means that the ACP database includes appeals for a myriad of issues such as displaying advertisement on a building, house rear extensions and changes to commercial store front. Therefore, for this paper, we focus specifically on major residential developments, defined in the UK legislation as any development that adds 10 or more dwelling units.[17] Second, appeals involving smaller developments (less

[17] Town and Country Planning Order (2010). Note that this definition would include mixed use or primarily commercial developments that also add 10 or more residential units. In addition, there are certain requirements that "major developments" have to adhere to and these requirements make it easier for locals to exercise their power to influence the local planning authority. For example, the developer is required to give notice of the planned development at the development site, serve the notice to adjoining

than 10) tend to comprise mainly of homeowners who are seeking to divide their house into two apartments to maximize rental yield or adding units to house family members. Such extensions and modifications are likely to be very different from a developer seeking to build residential units for sale. Third, there is a long tail of very small developments. For instance, more than 50% of residential developments add only one dwelling unit, and more than 75% of developments add two units. Since minor developments tend to have smaller externalities, we would be skewing our results towards finding zero effect if we include them in our sample.

Exclude variation in conditions and caravan parks. Variation in conditions are appeals that seek to amend the original plan approved by the local authority. For instance, a developer who planned for extensive landscaping, but now finds it prohibitively costly, may appeal to the inspectorate to remove that part from the agreed plan. These appeals typically involve minor amendments and, if successful, barely change the development. We also exclude caravan parks because it is not clear whether they are residential in nature. Some may serve as long term lots for families living in caravans but others may serve as winter parking or tourist lots.

Choose earliest appeal if several are near each other. This reduces the need to account for cross-appeal effects in our regressions. We can also think of the number of subsequent appeals in the area as an outcome variable that is influenced by the earliest appeal. This may introduce endogeneity into our regressions.

Location plans available online. We had to geocode the planned development manually because many were developed on "greenfield" sites without existing addresses.

Include appeals for which inspector leniency is estimable. Section 4.4 details how we construct the instrument for our IV regressions: we need a sufficient sample of cases per inspector to ensure estimates of inspector leniency converge to their true leniency. Thus, we excluded appeals for which the inspectors did not have many cases.

Table 2 shows us the summary statistics of the appeals data, after applying the above sample restrictions.

occupiers and publish the notice in a local newspaper. These requirements mean that locals are more likely to be aware of such developments, enabling them to voice their opinions and hold the members of the local planning authority accountable if they approve the development.

Table 2: Summary statistics (appeals data)

	Mean	SD	Min	Max	N
Year appeal started	2015.42	1.65	2012	2018	3105
Year appeal decided	2015.86	1.61	2012	2018	3105
No. of dwellings	56.18	101.07	10	4022	3105
Site area (hectres)	6.90	141.93	0	6780	2971
East Midlands	0.11	0.31	0	1	3105
East of England	0.14	0.35	0	1	3105
London	0.09	0.28	0	1	3105
North East	0.03	0.16	0	1	3105
North West	0.09	0.29	0	1	3105
South East	0.24	0.43	0	1	3105
South West	0.15	0.36	0	1	3105
West Midlands	0.09	0.29	0	1	3105
Yorkshire and The Humber	0.06	0.23	0	1	3105
Appeal successful	0.43	0.50	0	1	3105

3.2.2 Selection of price paid sample

Include only resale transactions of residential properties that are ever within 1km of any appeal. The reason for including only resale transactions is because we want to see how the appeals affect existing properties and not the new properties that are being built. As for the 1km restriction, we apply this because if we were to increase the radius beyond 1km, we end up having many properties that are linked to multiple appeals. In addition, the effect of having an appeal nearby is likely to decay with distance. If we define too big a radius, we would be skewing our results towards finding zero effect.

Exclude "others" category of residential transactions. The dataset includes residential-related transactions such as garages and parking spaces that we exclude.

Include only resale transactions that took place within three years before or after an appeal. We have fewer and fewer resale transactions beyond three years of an appeal. Statistical inference might become unreliable if the number of resale transactions in certain years before or after an appeal becomes too small.

Trim top and bottom 1% in prices. Regressions may be sensitive to outliers so we trimmed extreme prices to reduce that sensitivity. Our results hold with an untrimmed or a Winsorised sample. Table 3 shows us the summary statistics of the resale transactions data.

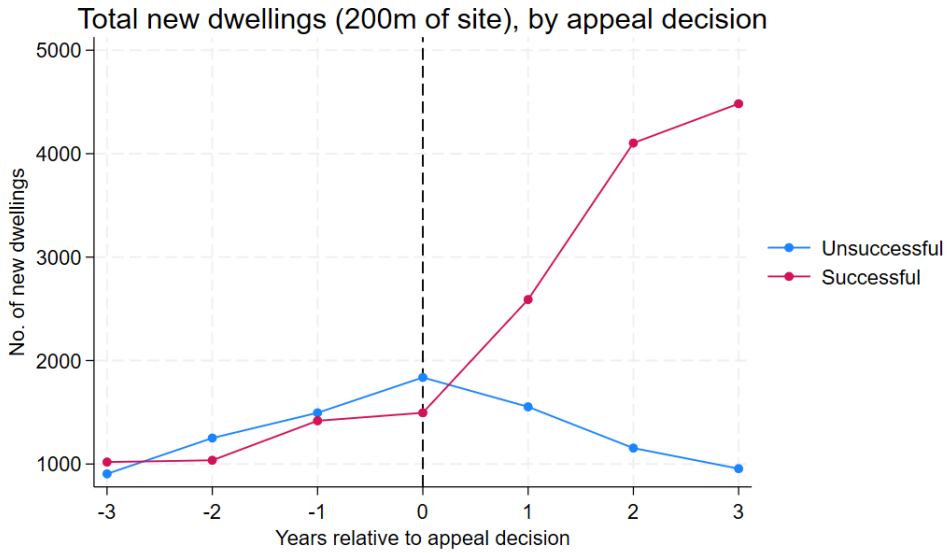
Table 3: Summary statistics (resale transactions data)

	Mean	SD	Min	Max	N
Year of transaction	2015.88	2.07	2010	2020	1233780
Transacted price	297549.99	209270.43	54950	1504000	1233780
Terrace	0.29	0.45	0	1	1233780
Flat	0.27	0.45	0	1	1233780
Semi-detatched	0.24	0.42	0	1	1233780
Detached	0.20	0.40	0	1	1233780
Freehold	0.70	0.46	0	1	1233780
Distance from first appeal	631.31	246.16	1	1000	1233780

3.3 What happens after a successful appeal?

So far we have assumed that successful appeals lead to new residential developments at the appeal site. Concretely, what it does lead to is an option to develop the appeal site. Developers may choose not to develop at all if property markets are poor.[18] Figure 1 plots the number of new dwellings, within 200m of the center of appeal sites, before and after an appeal decision, and indicates that options do translate to new housing.

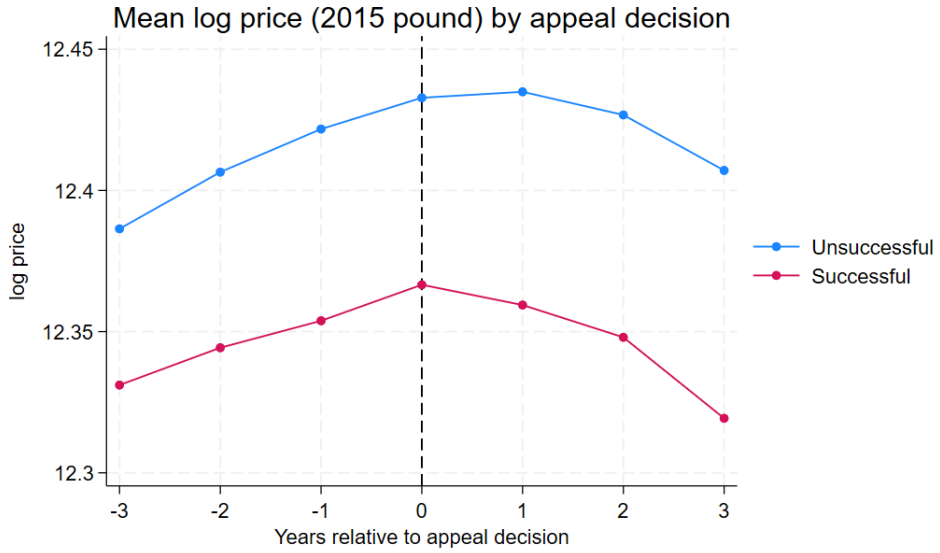
Figure 1: Comparison of new dwellings in appeal sites, by success of appeal



[18] What they can do, however, is to sell the option to other developers. Regardless of market conditions, developers who have gone through the appeals process would have sunk in a lot of money (e.g., purchasing the land, drafting plans, hiring architects and consultants) and would usually develop the site except in exceptional circumstances.

Figure 2 plots the average log real price by appeal decision. We see that successful appeals tend to be in neighborhoods with lower house prices. This suggests neighborhood characteristics, which determine market prices, may affect appeal decisions. For instance, properties near a green belt are priced higher due to the supply restrictions, and the bar for an appeal there would also be higher. However, we see that the price trends prior to appeal decisions are similar across both groups.

Figure 2: Unconditional mean log price by appeal decision



4 Empirical strategy

4.1 Overview

We use a DiD approach to estimate the causal effect of overturning the local authority's decision on the prices of existing homes in the vicinity:

$$ln(price_{ijt}) = \alpha_j + \delta_t + \beta Success_i \times Postappeal_t + \gamma' X_{ijt} + \epsilon_{ijt} \quad (1)$$

$ln\,(price_{ijt})$ is the log price of existing property i within 1km of appeal j in year-month t. $Success_i$ is an indicator variable that denotes whether property i is ever within 1km of a successful appeal. $Postappeal_t$ is an indicator variable for the period that the appeal took place and periods after the appeal. X_{ijt} is a vector of individual property characteristics such as property type (i.e., detached, semi-detached or a flat); whether the property is leasehold or freehold; the number of rooms; floor area; the property's current

and potential energy efficiency; and whether the property was built before 2012.[19] We also included dummies for the deciles of the supply of houses in the 1km vicinity of the appeal one year prior to the appeal decision interacted with relative year dummies; and dummies for the deciles of the average wages across the LPAs one year prior to the appeal decision interacted with relative year dummies.

α_j are appeal-by-postcode sector fixed-effects for properties around appeal j and δ_t are time fixed-effects. These consist of calendar year-month fixed-effects and event year fixed-effects. For example, the event year fixed-effect takes on the value of one if the sale of the property is within one year after the appeal. We cluster the standard errors at the postcode sector level.

Using the example of the postcode SE16 7BB, Table 4 shows how the varying levels of postcode granularity are defined in the UK. Each postcode sector corresponds to around 3,000 addresses.

Table 4: Postcode format in the UK

Postcode			
Outward code		Inward code	
Area	District	Sector	Unit
SE	16	7	BB

4.2 OLS approach

We implement our OLS DiD using a "stacked" approach – a similar approach used by Cengiz et al. (2019) and Deshpande and Li (2019). Each stack consists of housing resale transactions within 1km of an appealed development. If the appeal is successful ($Success_i = 1$), that stack is considered to be "treated". If the appeal is unsuccessful ($Success_i = 0$), the stack that it is associated with is considered to be a "control". Event years are specified relative to the date of the appeal decision.[20] Similar to Deshpande and Li (2019), we include both calendar time and event time fixed-effects in our regressions.

In our setting, appeals take place throughout the year. As a result, different locations will experience variation in treatment timing. Multiple researchers (see Roth et al. (2023) for an excellent review) have cautioned that in such settings involving multiple periods and variation in treatment timing, the coefficients from standard two-way fixed

[19] We include this as a control because our appeals' data only start from October 2012 even though there were appeals that took place before that.

[20] Cengiz et al. (2019) note that aligning events by event time instead of calendar time is equivalent to a setting where the events happen all at once and are not staggered.

effects (TWFE) models may not give an appropriate weighted average of treatment effects when treatment effects are heterogeneous. This is because DiD using a TWFE regression makes both "clean" comparisons between treated and not-yet-treated/never-treated units, and "forbidden" comparisons between units that are both already-treated.

We take several steps to avoid the issue of "forbidden" comparisons. First, if a housing transaction is within 1km of multiple appeals, we assign the transaction to the earliest of the appeals. For example, if the earliest appeal is a successful one and the subsequent ones are unsuccessful, then this transaction is considered to have already been "treated" and will not be part of the "control" group in later time periods. Second, if there are both successful and unsuccessful appeals that took place on the same day, we split such ties by assigning that transaction to the "treated" group. This ensures that our "control" group will not be contaminated by "treated" observations. Both steps also ensure that there are no duplicated observations in our sample.

4.3 Instrumental variable (IV) approach

The OLS results could be masking substantial heterogeneity. For example, it does not tell us whether the effect of successful appeals differ based on whether the appeals were assigned more lenient inspectors. To study this specific type of heterogeneity, we turn to an IV approach. Our IV strategy exploits the fact that inspectors have different preferences and are quasi-randomly assigned to the cases. The leniency of the inspectors can hence be used as an instrument for whether an appeal is successful.

We emphasize that while an IV approach is typically used to address "endogeneity" issues, in the presence of heterogenous treatment effects, it can also be used as an estimation method to recover the Local Average Treatment Effect (LATE) for the "compliers" even when there are no "endogeneity" issues. We verify this in Appendix A3. In our context, the "compliers" refer to existing homes that were within 1km of appeals that were successful because they were assigned more lenient inspectors. When reporting our empirical results, we report both the OLS and IV results.

4.4 Instrumental variable calculation

Step 1: Run regression to obtain "residuals". To construct the instrument, we follow the existing literature on expert leniency. This involves regressing whether the appeal is successful on the variables that the Planning Inspectorate uses to assign the inspectors to cases, and then saving the "residuals" from the regression.[21] Including the

[21] Although our regression sample involves only major dwellings, we also use the success rate for appeals involving non-major dwellings to construct the instrument. The purpose of doing this is to improve the power of the instrument.

assignment criteria is important because it accounts for the fact that randomization by the Planning Inspectorate occurs within the pool of available and suitable inspectors.

Ideally, we would control for the assignment criteria based on the exact way that the Inspectorate's algorithm and case workers filter out inspectors to reach the final pool of suitable inspectors. However, we were unable to obtain these exact procedures from the Inspectorate. This is not surprising; the Inspectorate needs to keep these procedures opaque to prevent developers and local planning authorities from gaming the allocation process. For instance, altering case characteristics to obtain a "favorable" inspector. To overcome this, we proxy for the broad criteria revealed to us by the Inspectorate using available data (see Table 5). The detailed regression results of the relationship between these variables and the probability of a successful appeal are included in the online Appendix, Table A1.

Table 5: Assignment criteria for inspectors

Assignment criteria	Proxies available in data
Case characteristics	
Complexity of the case	Key words (e.g., redevelopment, commercial); number of residences; whether case has bespoke timeline; whether developer is appealing for costs; whether intermediary is involved (e.g., consultant); whether linked to another appeal; type of appeal procedure (e.g., hearing, written); number of past appeals; and success rate of these appeals in the LPA
Inspector characteristics	
Qualification and experience	Total days taken to resolve appeals handled, case characteristics of appeals handled (see above)
Distance lived from appeal	LPA of appeal X year fixed-effects
Caseload	Number of live appeal cases
Another case near appeal	LPA of appeal X year fixed-effects
Lives in area appeal	LPA of appeal X year fixed-effects
Worked for consultancy firm	Whether intermediary involved
Personal difficulties	Not able to find appropriate proxy in data

Step 2: Take averages of the "residuals". For each appeal, we calculate the leniency of the inspector based on taking the average of the "residuals" from all the cases that the inspector handled. These cases include both past and future appeals but not the existing appeal. This leave-out measure is important because it avoids introducing mechanical reverse causality.

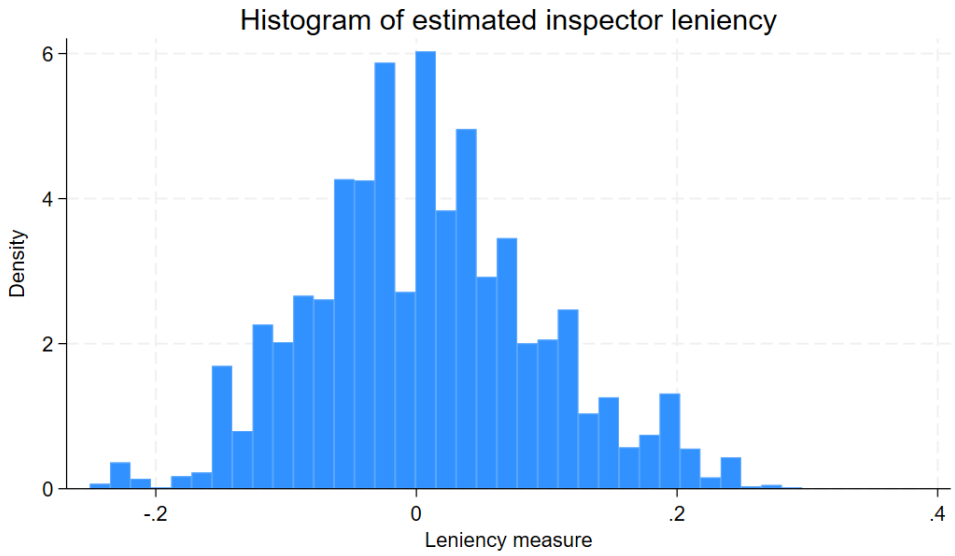
Step 3: Drop appeals involving inspectors who presided over ≤30 appeals.

Since we estimate the leniency of inspectors using her mean residualized probability of approving an appeal, this estimate would be very noisy for inspectors with only a few appeals. For instance, the leniency would be estimated off one appeal if an inspector only judged two appeals in total.[22] Therefore, we dropped appeals involving inspectors who presided over ≤30 appeals.[23] This excludes 32.4% of inspectors from our sample, leaving us with 269 inspectors. For these remaining inspectors, each of them presided over an average of around 90 appeals. The highest number of appeals presided over by an inspector is 313 and the smallest number of appeals presided over by an inspector is 31.

Figure 3 shows the variation of our leniency measure. The inspector leniency measure ranges from -0.25 to 0.30 with a standard deviation of 0.09. The histogram suggests that there is a wide variation in whether an inspector is likely to allow an appeal to be successful.

Step 4: Interact leniency measure with the post appeal dummy. We interact the leniency measure with the post appeal dummy. $Leniency_i \times Postappeal_t$ is thus our instrument for $Success_i \times Postappeal_t$.

Figure 3: Distribution of inspector leniency (regression sample)



[22]Indeed, the predictive power of leniency on approval, without dropping inspectors, is very poor.
[23]Our results are robust to using various arbitrary cut-offs.

4.5 Validating the IV strategy

4.5.1 Relevance of instrument

We estimate the following linear probability model to examine the first-stage relationship between inspector leniency and whether an appeal is successful:

$$Success_i \times Postappeal_t = \alpha_j + \delta_t + \rho Leniency_i \times Postappeal_t + \theta' X_{ijt} + \upsilon_{ijt} \quad (2)$$

where X_{ijt} is a vector of control variables that is the same as in equation 1. Table 6 presents the first-stage results. Column 1 shows the results from the specification with just the basic fixed-effects while in column 2, controls are added. In both columns, the estimates are highly significant, suggesting that being assigned to an inspector who is 10 percentage points more lenient increases the probability of the appeal being successful by around 6.6 to 6.8 percentage points. Furthermore, the first-stage has a KP F-statistic value of over 40, suggesting that we can reject the null hypothesis of weak instruments. Columns 3 and 4 show that we can reject the null hypothesis of weak instruments even if the data are aggregated at the appeal-year level.

Table 6: First-stage estimates

	(1)	(2)	(3)	(4)
VARIABLES	Success*Postappeal			
Leniency*Postappeal	0.679***	0.660***	0.559***	0.534***
	(0.101)	(0.102)	(0.104)	(0.104)
Observations	1,233,078	1,020,940	20,172	19,900
Time FE	✓	✓	✓	✓
Appeal X Postcode sector FE	✓	✓	✓	✓
Controls		✓		✓
KP F-stat	44.92	42.24	28.73	26.59

Notes: Controls include transaction level property characteristics such as property type (i.e., detached, semi-detached or a flat); whether the property is leasehold or freehold; the number of rooms; floor area; the property's current and potential energy efficiency; whether the property was built before 2012; dummies for the deciles of the supply of houses in the 1km vicinity of the appeal one year prior to the appeal decision interacted with relative year dummies; and dummies for the deciles of the average wages across the LPAs one year prior to the appeal decision interacted with relative year dummies. Time fixed-effects in columns 1 and 2 consist of calendar year-month fixed-effects and relative year fixed-effects, while in columns 3 and 4, they consist of year fixed-effects and relative year fixed-effects. Standard errors are clustered at the postcode sector level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

4.5.2 Validity of instrument

Conditional independence. In order for inspector leniency to be a valid instrument, the assignment of appeals to inspectors must not be correlated with variables which also affect the outcome variable (housing prices). We compiled the characteristics of the local planning authority and of property transactions that were within 1km of **future** appeals and run a balancing test. This test checks if historical characteristics of the local planning authority and the type of properties transacted nearby can predict the leniency of the inspector assigned to an appeal.

Column 1 of Table 7 shows that past transaction characteristics of properties within 1km of an appeal and the characteristics of the local planning authority are jointly predictive of whether an appeal is successful (p-value of 0.00), However, they are not jointly predictive of our IV, inspector leniency (column 2; p-value of 0.22). With the exception of two coefficients (which though statistically significant, are extremely close to zero), all the other estimates are close to zero and statistically insignificant.

In column 3, we aggregated these variables to the appeals level to check if this conclusion is robust to different levels of aggregation. Although the coefficients on several of the potential energy efficiency variables are significant, the variables are not jointly significant (p-value of 0.547). This gives us greater confidence that conditional on the Planning Inspectorate's assignment rules, inspectors are randomly assigned to cases.

The random assignment of inspectors (conditional on the Planning Inspectorate's assignment criteria) gives us consistent estimates of the reduced form effect of inspector leniency on housing prices. However, interpreting the IV estimates as the causal effects of a successful appeal on housing prices requires two further assumptions.

Exclusion restriction. This restriction requires that inspector leniency affects housing prices (outcome variable) only through its effect on whether an appeal is successful. Although there is no direct way to test the exclusion restriction, the fact that inspectors are randomly assigned to cases (conditional on the assignment criteria), coupled with the results from our balancing test, lend support to the exclusion restriction.

Monotonicity. If the causal effect of a successful appeal is constant across all cases, then the instrument only needs to satisfy the exclusion assumption. However, with heterogeneous effects, monotonicity must also be assumed. Monotonicity gives the IV estimate a Local Average Treatment Effect (LATE) interpretation – the average causal effect among the subgroup of cases that would have received a different appeal decision had the case been assigned to a different inspector. In our setting, the monotonicity assumption requires that appeals that are ruled successful by a strict inspector would also be ruled successful by a lenient inspector.

One testable implication of the monotonicity assumption is that the first-stage estimates should be non-negative for any subsample. To test this, we split the sample into subsamples based on (i) geographic regions; (ii) year of appeal decision; and (iii) number of dwellings in the development. Table 8 shows that for all of these subsamples bar one, the first-stage estimates are positive. For the region, West Midlands (region 11 in Table 8), which accounts for around 6% of appeals, the coefficient is negative but not statistically significant. Excluding this region does not change our main results.

Table 7: Balancing test (transactions within 1km of future appeals)

VARIABLES	(1) Success	(2) Inspector	(3) Leniency
ln(price)	-0.038*	0.003	0.001
	(0.020)	(0.003)	(0.006)
ln(floor area)	0.004	-0.002	-0.008
	(0.015)	(0.003)	(0.017)
3 rooms	0.011	-0.002	-0.026
	(0.007)	(0.001)	(0.040)
4 rooms	0.017*	-0.001	0.016
	(0.009)	(0.002)	(0.042)
5 rooms	0.011	-0.002	-0.002
	(0.011)	(0.002)	(0.042)
6 rooms or more	0.006	-0.002	0.000
	(0.012)	(0.002)	(0.044)
Current energy B	0.040	0.001	-0.591
	(0.067)	(0.010)	(0.602)
Current energy C	0.036	0.001	-0.527
	(0.066)	(0.010)	(0.600)
Current energy D	0.027	0.000	-0.524
	(0.066)	(0.010)	(0.600)
Current energy E	0.022	0.002	-0.521
	(0.066)	(0.010)	(0.599)
Current energy F	0.025	0.003	-0.511
	(0.067)	(0.010)	(0.600)
Current energy G	0.024	0.002	-0.502
	(0.067)	(0.010)	(0.604)
Potential energy B	0.004	-0.002	-0.076**
	(0.011)	(0.002)	(0.033)
Potential energy C	0.002	-0.003	-0.091***
	(0.011)	(0.002)	(0.033)
Potential energy D	0.004	-0.001	-0.111***
	(0.011)	(0.002)	(0.035)
Potential energy E	0.009	-0.001	-0.093**
	(0.013)	(0.002)	(0.039)
Potential energy F	-0.005	-0.001	-0.124**
	(0.016)	(0.003)	(0.057)
Potential energy G	0.034	0.004	-0.152**
	(0.027)	(0.005)	(0.059)
Detached house	0.035**	-0.000	0.000
	(0.014)	(0.003)	(0.012)
Semi-detached house	-0.006	-0.003	-0.021
	(0.009)	(0.002)	(0.014)
Flat	-0.042	0.008*	0.033
	(0.026)	(0.005)	(0.030)
Leasehold property	-0.003	-0.007	-0.014
	(0.026)	(0.005)	(0.022)
Built before 2012	0.041	-0.009	-0.056
	(0.034)	(0.007)	(0.099)
Supply just before appeal	0.000	-0.000*	-0.000
	(0.000)	(0.000)	(0.000)
LPA acceptance rate	11.075**	0.019	-0.197
	(4.396)	(0.859)	(0.876)
LPA log(mean gross weekly wages)	1.450**	-0.003	-0.042
	(0.571)	(0.113)	(0.114)
LPA acceptance rate*	-1.666**	-0.003	0.037
LPA log(mean gross weekly wages)	(0.697)	(0.136)	(0.139)
Observations	491,565	491,565	2,896
Adjusted R-squared	0.019	0.003	-0.000
Time FE	✓	✓	
F-stat for joint test	3.181	1.201	0.943
p-value for joint test	5.95e-08	0.218	0.547

Notes: Time fixed-effects consist of calendar year-month fixed-effects and relative year fixed-effects. Standard errors are clustered at the postcode sector level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Testing monotonicity assumption

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
	Success*Postappeal												
Leniency*Postappeal	1.105***	0.402	1.154***	1.858***	1.234***	0.366*	0.803***	-0.677*	0.496	1.014***	0.385***	0.992***	0.432***
	(0.280)	(0.313)	(0.207)	(0.548)	(0.350)	(0.192)	(0.279)	(0.389)	(0.348)	(0.154)	(0.137)	(0.154)	(0.132)
Observations	91,704	119,720	194,985	19,405	88,598	235,016	132,122	72,716	66,304	407,908	613,032	501,012	519,928
Time FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Appeal X	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Postcode sector FE													
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Region	1	2	3	4	5	8	9	11	12				
KP F-stat	15.61	1.647	31.17	11.48	12.41	3.647	8.268	3.036	2.033	43.32	7.921	41.40	10.65
No. of dwellings												10-25	>25
Decision year										2012-2015	2016-2018		

Notes: Controls include transaction level property characteristics such as property type (i.e., detached, semi-detached or a flat); whether the property is leasehold or freehold; the number of rooms; floor area; the property's current and potential energy efficiency; whether the property was built before 2012; dummies for the deciles of the supply of houses in the 1km vicinity of the appeal one year prior to the appeal decision interacted with relative year dummies; and dummies for the deciles of the average wages across the LPAs one year prior to the appeal decision interacted with relative year dummies. Time fixed-effects consist of calendar year-month fixed-effects and relative year fixed-effects. The geographic regions are as follows: (1) East Midlands; (2) East; (3) London; (4) North East; (5) North West; (8) South East; (9) South West; (11) West Midlands; and (12) Yorkshire and The Humber. Standard errors are clustered at the postcode sector level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

5 Effect of successful appeals

5.1 Main results

Table 9 presents the OLS and IV estimates of the effect of overturning the local authority's decision on housing prices. Column 1 reports the OLS estimate from a specification with just the time and cross-section fixed-effects. The estimate in column 2 where controls are included remains relatively similar to column 1. This is despite some of controls – dummies for the deciles of the supply of houses in the 1km vicinity of the appeal one year prior to the appeal decision interacted with relative year dummies; and dummies for the deciles of the average wages across the LPAs one year prior to the appeal decision interacted with relative year dummies – aborbing a lot of variation from the data.

Our OLS result in column 2 indicates that a successful appeal has a small negative effect of around 0.6% on housing prices compared to an unsuccessful appeal. The coefficient is significant at the 5% level and the 95% confidence intervals are able to rule out an impact more negative than -1.2%. The rejection rate, of major developments, for all local authorities in the UK was about 20% from 2012 to 2017. This suggests that local authorities were rejecting only the most egregious developments. Hence, it is surprising that we can rule out a large negative impact on prices in the neighborhood.

Table 9: Effect of overturning the local authority's decision

	OLS		IV	
	(1)	(2)	(4)	(5)
VARIABLES	ln(Price)			
Success*Postappeal	-0.009***	-0.006**	0.072***	0.056**
	(0.003)	(0.003)	(0.027)	(0.024)
Observations	1,233,078	1,020,940	1,233,078	1,020,940
R-squared	0.609	0.890	-0.002	0.705
Time FE	✓	✓	✓	✓
Appeal X Postcode sector FE	✓	✓	✓	✓
Controls		✓		✓
KP F-stat			44.92	42.24

Notes: Controls include transaction level property characteristics such as property type (i.e., detached, semi-detached or a flat); whether the property is leasehold or freehold; the number of rooms; floor area; the property's current and potential energy efficiency; whether the property was built before 2012; dummies for the deciles of the supply of houses in the 1km vicinity of the appeal one year prior to the appeal decision interacted with relative year dummies; and dummies for the deciles of the average wages across the LPAs one year prior to the appeal decision interacted with relative year dummies. Time fixed-effects consist of calendar year-month fixed-effects and relative year fixed-effects. Standard errors are clustered at the postcode sector level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

The IV estimates are presented in columns 3 and 4. The IV result with controls shows that instead of depressing prices, overturning the local authorities' decision actually increased the value of properties in the neighborhood by around 5.6%. The coefficient is significant at the 5% level. The 95% confidence intervals are able to reject an impact smaller than 0.9%. Similar to the OLS, we are not finding that developments that are

rejected have a large detrimental impact on the prices of existing homes in the neighborhood.

The 95% confidence intervals of the OLS and IV estimates do not overlap, so we can reject the null that the IV and OLS estimates are the same. In the absence of endogeneity, the difference between the OLS and IV estimates is because the IV estimates the LATE of "compliers". In our context, these "compliers" refer to existing homes that are within 1km of appeals that were successful because they were assigned more lenient inspectors. By contrast, the OLS estimate gives the weighted effect of both lenient and strict inspectors.

5.2 Robustness checks

We subject our main results to a series of robustness checks. First, we run a dynamic version of regression 1 and show in Appendix Table A2 that there is no evidence of pre-trends for our OLS estimate. Second, we test if our results are robust to limiting the sample's bandwidth to two years before and after the appeals. The results in Appendix Table A3 show that the OLS estimates are very similar to our baseline estimates. The IV estimate (with controls) falls to 3.7% but continues to be positive and statistically significant. Third, we show in online Appendix Table A4 that our results are robust to dropping data from 2020 which is the year that experienced the impact of COVID.

Fourth, we address potential concerns about spillovers. Some "treated" stacks could be neighboring other "treated" stacks. Consequently, these stacks may end up seeing a greater intensity of treatment. Similarly, some "control" stacks could be neighboring "treated" stacks and may hence experience some effect from being relatively near to a successful appeal. To address these concerns, we dropped stacks that had appeals within 2km of each other. Online Appendix Table A5 shows that the OLS estimates continue to be extremely close to zero while the IV estimates become larger, albeit imprecisely estimated. This imprecision is expected given that more than half of the sample is dropped under this robustness check.

Fifth, we show that our results are robust to clustering our standard errors at different levels of variation. Online Appendix Table A6 shows the results when we cluster by (i) outward postcode; (ii) appeal-by-postcode sector; and (iii) 1km grids. The 1km grids are arbitrarily created grids of 1km-by-1km across the whole of the UK. These alternate methods of clustering do not affect the statistical inference of our estimates.

Sixth, we trim off the top and bottom values in our instrument (inspector leniency) to check if our IV results are driven by "outlier" values (Online Appendix Table A7). After trimming the top and bottom 1% of inspector leniency (column 2), the IV esti-

mate remains positive and significant at 6.5%. Trimming the top and bottom 5% causes the estimate to become even larger at 12.3% and still statistically significant (column 4).

Seventh, in online Appendix Table A8 we show that our IV results are robust to using only inspectors who presided over at least 40 appeals (baseline analysis was based on at least 30 appeals). The IV estimate with controls falls to 5.1% (versus 5.6% in the baseline) but remains statistically significant at the 5% level.

5.3 Mechanisms

The positive IV estimates suggest that on average, the appeals that were successful because they were assigned more lenient inspectors, have demand effects that dominate the increase in housing supply. To validate the presence of demand effects, we collect annual point-of-interest (POI) data by location and test if successful appeals lead to higher counts of these points. We categorize POIs into three categories, summarized in Table 10. These categories reflect the possible amenities that major developments could introduce: (i) integrated developments often include commercial spaces for local businesses; (ii) population growth in an area may attract more businesses; and (iii) developers may be asked to contribute to local infrastructure by building them (e.g., green spaces) or via a tax.

Table 10: POI categories

Category	POIs included
Local services	Accommodation, Eating and Drinking, Retail
Local economy	Commercial services, Manufacturing and Production
Local infrastructure	Education and Health, Public Infrastructure, Transport

We use the count of POIs in each category as an outcome variable and study if there are more POIs within 1km of a successful appeal site (Table 11). The OLS estimates are all small and close to zero. However, for the IV, despite the high annual turnover in POIs (businesses open and shut down, bus stops are added, etc.), we find statistically significant evidence that the count of local services increased after successful appeals (column 4). If we disaggregate local services (Table 12), we see that the increase is coming from retail shops. This suggests that the positive impact for the "compliers" may be driven by an improvement in local amenities which increased demand for the neighborhood.

Table 11: Possible demand shifters

	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Local serv.	Local econ.	Local infra.	Local serv.	Local econ.	Local infra.
Success*Post Appeal	-0.060	1.015	0.607	10.755**	5.847	1.459
	(0.579)	(0.969)	(0.771)	(5.452)	(5.667)	(7.176)
Observations	22,464	22,464	22,464	22,434	22,434	22,434
R-squared	0.995	0.994	0.991	-0.040	-0.003	-0.000
Time FE	✓	✓	✓	✓	✓	✓
Appeal FE	✓	✓	✓	✓	✓	✓
KP F-stat				39.27	39.27	39.27

Notes: Time fixed-effects consist of calendar year-month fixed-effects and relative year fixed-effects. Standard errors are clustered at the appeal level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 12: Demand shifters within local services

	IV	
	(1)	(2)
VARIABLES	Accommodation, Eating and Drinking	Retail
Success*Post Appeal	1.164	10.268**
	(2.311)	(4.885)
Observations	21,186	21,186
R-squared	-0.001	-0.061
Time FE	✓	✓
Appeal FE	✓	✓
KP F-stat	34.93	34.93

Notes: Time fixed-effects consist of calendar year-month fixed-effects and relative year fixed-effects. Standard errors are clustered at the appeal level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Moving on to examine supply effects, Table 13 shows heterogeneity by the supply shock from a successful appeal. We define the supply shock as the number of new dwellings that would be introduced by the appeal, divided by the total number of dwellings within 1km of the appeal site in the year prior to the appeal decision. Small and large shocks are categorized based on whether they were below or above the median magnitude of supply shocks.

For the OLS, the estimated coefficients by the size of the supply shocks were similar to two decimal places. While the estimated negative effect was significant for large supply shocks, it was statistically insignificant for small supply shocks. As for the IV, small supply shocks have a positive and significant impact on prices in the neighborhood while the effect of larger supply shocks was statistically insignificant.[24]

[24]The results presented in Table 13 include all controls. In online Appendix Tables A9 (small shock) and A10 (large shock), we show how the coefficient estimate changes when we move from a specification with just the basic fixed-effects to one where controls are added.

To summarize this section on mechanisms, in general, our results are consistent with a simple demand and supply model. For the OLS, while there was an increase in supply, there was no effect on amenities (and hence demand). This explains the negative results from the OLS. For the "compliers" in the IV, in the case of developments with a small supply shock, the demand shifters dominated the supply shock; while for larger developments the two seem to cancel out.

Table 13: Effect by supply shock

	OLS		IV	
	(1)	(2)	(3)	(4)
VARIABLES	ln(Price)			
Success*Postappeal	-0.005	-0.007*	0.060**	0.022
	(0.003)	(0.004)	(0.025)	(0.077)
Observations	788,720	232,220	788,720	232,220
R-squared	0.894	0.871	0.702	0.721
Time FE	✓	✓	✓	✓
Appeal X Postcode sector FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Sample	Small shock	Large shock	Small shock	Large shock
KP F-stat			45.69	2.478

Notes: Controls include transaction level property characteristics such as property type (i.e., detached, semi-detached or a flat); whether the property is leasehold or freehold; the number of rooms; floor area; the property's current and potential energy efficiency; whether the property was built before 2012; dummies for the deciles of the supply of houses in the 1km vicinity of the appeal one year prior to the appeal decision interacted with relative year dummies; and dummies for the deciles of the average wages across the LPAs one year prior to the appeal decision interacted with relative year dummies. Time fixed-effects consist of calendar year-month fixed-effects and relative year fixed-effects. Standard errors are clustered at the postcode sector level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

6 Explaining why local authorities block attractive projects

In the previous section, we showed evidence that the positive effect is due to demand effects dominating supply effects. However, this begs the question of why would local authorities block developments if they actually cause existing houses in the vicinity to become more valuable? In this section we rationalize this puzzle by providing a theoretical framework that models the development application and approval process as a multi-stage sequential game between local planning authorities and inspectors. We then provide further empirical evidence to support the reason proposed by our framework. Finally, we run additional regressions to rule out alternative explanations.

6.1 Theoretical framework

The aim of our theoretical framework is to help interpret our results from the empirical analysis, and in so doing, enables us to explain the positive effects from our IV regressions. The projects that went through the appeals process are self-selected and would

not be similar to projects that that did not appeal. Hence, our theoretical framework analyzes this selection effect and how it changes with local power. The model is a sequential game between the local planning authority (L) and inspector (N) played out in three stages:

Table 14: Stages of the game

Stage	Player	Action set	Consequence of action
0	Nature	Draws κ, s and η	All players observe κ and s, only N observes η
1	L	Accept	$\{\kappa, s\}$ implemented; move to stage 3
		Reject	Move to stage 2
2	N	Accept	$\{\kappa, s\}$ implemented; move to stage 3
		Reject	No development
3	Housing market clears		

A land development plan, defined as the pair $\{\kappa, s\}$, is submitted to L. If accepted, this development adds an exogenous amenity value of $\kappa \in (-\infty, \infty)$ to the location. The destruction of green spaces can be represented as a negative κ, while the removal of an abandoned building as a positive κ. A value of $\kappa = 0$ means there is not change to the amenity value relative to the status quo. s is the intensity of residential development, where $s \in [0, \infty)$. A higher s means more residents in the location which creates congestion and counteracts the amenity value of the location.[25] If the development is accepted, $P(\kappa, s)$ is the market clearing price of the development.

We assume that all residents (current and potential) are homeowners, which allows us to study the impact of wealth effects (i.e., property prices affecting the local land use decisions). However, our framework can be viewed more generally as one where the local planning authority has to trade off the interests of "insiders" versus "outsiders". In our framework, "insiders" refer to current homeowners in the location while "outsiders" refer to potential homeowners who are looking to move into the location. We could also view "outsiders" as "renters" – while this would require adjustments to the model, the key predictions of the model would not change substantially.

We proceed to analyze the game backwards from stage 3.

6.1.1 Stage 3: Household behavior and market clearing

Resident homeowners ("insiders"). There is a mass $\underline{s} \in [0, 1]$ of resident homeowners who are deciding between residing in the location or selling their property and moving elsewhere (outside option). Staying provides a utility of $W_r + \psi(\kappa, s)$, where

[25] One way to think about congestion is that of more residents competing for scarce local services like schools and transport.

W_r is the wealth (and hence composite consumption) of the residents. $\psi_\kappa(.) > 0$ and $\psi_s(.) < 0$. The residents have homogenous utility from residing but heterogeneous outside utility that is increasing in the price from selling their property (P) and moving elsewhere. Specifically, resident i has an outside utility of $P(\kappa, s) + u_i$ and would choose to leave if $u_i \geq W_r + \psi(\kappa, s) - P(\kappa, s)$.[26] The mass of potential residents with an outside utility of u and lower is $F_r(u)$. Therefore, the mass of residents who want to leave ($\dot{s}$) is such that:

$$\dot{s}(\kappa, s) = \underline{s} - F_r(W_r + \psi(\kappa, s) - P(\kappa, s))$$

And the mass of residents who stay is $\tilde{s}(\kappa, s) = F_r(W_r + \psi(\kappa, s) - P(\kappa, s))$

Potential homeowners ("outsiders"). There is a mass of potential resident who are deciding between purchasing a property in the location and living elsewhere (outside option). Moving to the location provides a utility of $C + \phi(\kappa, s)$ subject to $C + P \leq W_p$, where W_p and C are the wealth and the composite consumption of the potential residents. The potential residents have homogenous utility from residing but heterogeneous outside utility. Specifically, potential resident j has an outside utility of u_j and would choose to reside in the location if $u_j \leq W_p + \phi(\kappa, s) - P(\kappa, s)$. The mass of potential residents with an outside utility of u and lower is $F_p(u)$. Therefore, the mass of potential residents who want to reside in the location ($\dot{m}$) is such that:

$$\dot{m}(\kappa, s) = F_p(W_p + \phi(\kappa, s) - P(\kappa, s))$$

For the the market to clear, the residents who want to leave and the new supply of housing from the development must be taken up by potential residents. Therefore:

$$\begin{aligned} s + \dot{s}(\kappa, s) &= \dot{m}(\kappa, s) \\ \implies s + \underline{s} - F_r(W_r + \psi(\kappa, s) - P(\kappa, s)) &= F_p(W_p + \phi(\kappa, s) - P(\kappa, s)) \end{aligned} \tag{3}$$

6.1.2 Stage 2: Inspector decides whether to overturn the local planning authority's decision

The inspector (N) compares the social utility of accepting the project versus rejecting it. She observes η, which is the weight of current residents ("insiders") who stay relative to the weight of new residents ("outsiders"). So the social utility of accepting is:

$$\eta * \tilde{s}(\kappa, s) * (W_r + \psi(\kappa, s)) + (s + \dot{s}(\kappa, s)) * (W_p + \phi(\kappa, s) - P(\kappa, s))$$

[26] This is consistent with u_i reflecting the amenity and composite consumption value of residing elsewhere, and $P(\kappa, s)$ reflecting the additional consumption value from the sale of their property (i.e., wealth effect).

Current residents, who choose to stay are weighted η, get their initial level of consumption (W_r) and enjoy the net stock of amenities. New residents, who are weighted 1, enjoy the same stock of amenities.

The social utility of rejecting is simply:

$$\eta * \tilde{s}\,(0,0) * (W_r + \psi(0,0)) + \dot{s}\,(0,0) * (W_p + \phi(0,0) - P(0,0)) \equiv \eta R_r + R_p$$

That is the market clears with the initial level of housing and amenities.

The local planning authority (L) does not observe the value of η but know its distribution (cdf, $G(\eta)$ and pdf, $g(\eta)$). Given a value of s, we can rewrite the condition for accepting as the range of values of η for which N will accept:

$$\eta \leq \frac{(s + \dot{s}\,(\kappa, s)) * (W_p + \phi(\kappa, s) - P(\kappa, s)) - R_p}{R_r - \tilde{s}\,(\kappa, s) * (W_r + \psi\,(\kappa, s))} \equiv I(\kappa, s)$$

Hence, the probability of a successful appeal is $G\,(I(\kappa, s))$.

6.1.3 Stage 1: Local planning authority decides whether to accept or reject the developer's proposed plan

The local planning authority (L) knows that it can accept a development and it will be built. But if it rejects, N may still accept it. The social utility of L is similar to N but instead of η, she weighs current residents ("insiders") with a value of ω relative to new residents ("outsiders"). She does not know the exact η but she knows the distribution of η. Assuming L is risk neutral, she will accept a development if and only if:

$$\omega * \tilde{s}\,(\kappa, s) * (W_r + \psi(\kappa, s)) + (s + \dot{s}\,(\kappa, s)) * (W_p + \phi(\kappa, s) - P(\kappa, s)) \geq \omega R_r + R_p$$

Existing homeowners want the value of their property to be high while new homeowners moving into the area prefer lower housing prices so that they can afford to move into the location.

6.1.4 Selection into appeals

We hope to understand the range of κ that will result in appeals which will help us to better understand what is driving the positive IV effects. To make some progress we make the following assumptions:

1. $\omega \geq \frac{W_p}{W_r} \geq 1$. The assumption $W_p \geq W_r$ is natural because potential homeowners have not spent their wealth on housing. $\omega W_r \geq W_p$ constrains social utility such that L would not strictly prefer all current residents to be replaced by new ones simply because new residents are substantially wealthier.

2. $\psi(\kappa, s) \geq \phi(\kappa, s)$ and $\psi_\kappa(\kappa, s) \geq \phi_\kappa(\kappa, s)$. These assumptions can be thought of as a type of endowment effect, where residents value their amenities, as well as any improvement in amenities, greater than outsiders. They also constrain the social utility such that L would not strictly prefer all current residents to be replaced by new ones simply because new residents value the amenities more.

3. $s = 0$. To simplify the analysis, we shut down the supply effects. This constrains the social utility such that L would not accept developments due to the fact that there is more housing and a mechanical higher utility because of more residents in the location. This assumption is also justified because our data suggests that the supply shocks are quantitatively small.

If we denote the weighted social utility of accepting as $U(\kappa)$ and the weighted social utility of rejecting as the constant R then:

Proposition 1 (Negative selection into appeals) *Given assumptions 1, 2 and 3,* $\exists \kappa^* : \kappa = \kappa^* \Rightarrow U(\kappa) = R$ *and* $\kappa < \kappa^* \Rightarrow U(\kappa) < R$ *and* $\kappa \geq \kappa^* \Rightarrow U(\kappa) > R$

Proof: See Mathematical Appendix A1. □

There exists a "threshold" (κ^*) such that appeals take place only when $\kappa < \kappa^*$. This says that the developments we observe in appeals are likely to be the ones with a more negative (or less positive) impact on amenities, because they are the ones that result in a lower weighted social utility. An empirical implication is that we should expect a fall in housing prices when appeals are successful. This is indeed what we found for our OLS results.

Proposition 2 (Greater local power exacerbates selection) *If* $\kappa^* < 0$ *and* $\omega' \geq \omega \Rightarrow \kappa^*(\omega) \leq \kappa^*(\omega')$. *If* $\kappa^* > 0$ *amd* $\omega' \geq \omega \Rightarrow \kappa^*(\omega) \geq \kappa^*(\omega')$.

Proof: See Mathematical Appendix A2. □

This proposition suggests that for $\kappa < 0$ (deleterious projects), the "threshold" (κ^*) for areas with more "outsiders" (ω) is to the left of the "threshold" for areas with more "insiders" (ω'). For $\kappa > 0$ (attractive projects), the reverse holds – the "threshold" (κ^*) for areas with more "outsiders" is to the right of the "threshold" for areas with more "insiders".

Focusing on developments where $\kappa < 0$ (deleterious projects), an increase in local power makes marginal developments that reduce amenities (i.e., $\kappa = \kappa^* - \epsilon$) less desirable. The local authority thus blocks a greater number of these undesirable projects, resulting in more undesirable projects ending up in the pool of appeals. As for developments where $\kappa > 0$ (attractive projects), an increase in local power makes marginal developments that increase amenities (i.e., $\kappa = \kappa^* + \epsilon$) more desirable. The local authority blocks a smaller number of these attractive projects, resulting in fewer of them appearing in the

pool of appealed projects.

The part of the proposition relating to $\kappa^* > 0$ can help to explain our "puzzling" positive IV estimates. Appeals that are overturned by the inspectors are likely to be the marginal ones and are very susceptible to the leniency of inspectors. When $\kappa^* > 0$, in areas with more "insiders" (ω'), overturning the local authority's decision should not lead to large changes in housing prices. This is because the local authority has already taken locals' interest into consideration by allowing most of the attractive developments to be built. Consequently, the marginal developments that go through as a result of the inspectors' decisions are those that do not contain as many attractive amenities and hence, do not lead to big changes in housing prices.

By contrast, in areas where there are more "outsiders" (ω), overturning the local authority's decision will lead to an increase in housing prices. This is because in these areas, the local authority has a relatively higher "threshold" as compared to areas with more "insiders"; $\kappa^*(\omega) \geq \kappa^*(\omega')$. As a result, the marginal developments around this "threshold" have substantially stronger demand effects. When they end up being built, they lead to bigger changes in the prices of existing homes. This could explain why our IV estimates in Section 5 are positive – the "compliers" in our IV are from areas with more "outsiders".

6.2 Empirical evidence in support of theoretical framework

To find empirical evidence in support of Proposition 2, we need to (i) verify that our IV regressions are picking up the effect of successful appeals involving attractive marginal developments ($\kappa > 0$) and then (ii) run IV regressions on subsamples with different levels of local power (ω and ω').

For (i), we know from our characterization of the "compliers" in Section 5.3 that our IV regressions do indeed pick up the effect of successful appeals where demand effects (i.e., $\kappa > 0$) dominate supply effects. This is in contrast to the OLS regressions where the effect is based on a mix of $\kappa > 0$ and $\kappa < 0$ developments.

As for (ii), in Table 15, we split our sample into areas based on whether they had a high share of households that owned (either fully paid or owned with a mortgage or loan) their accommodation. An area is considered to have low (high) local power if its share of homeowners is below (above) the national median based on Census 2011's data. This relies on the assumption that a high share of homeownership reflects more "insiders" and a greater propensity for residents to be involved in the local political process.

Focusing on the IV estimates, column 3 of Table 15 shows that successful appeals in local

planning areas that have a low share of homeowners (ω) increased the value of properties within 1km of the appeal by around 5.8% and this is statistically significant at the 5% level. By contrast, column 4 shows that in areas with a high share of homeowners (ω'), the effect on properties within 1km of the appeal was statistically insignificant. Notably, the magnitude of the estimate for areas with a low share of homeowners was almost twice that of areas with a high share of homeowners.[27] Given that the IV estimate of 5.6% in Section 5 is a weighted average of the estimates from areas with low (5.8%) and high homeownership (3.0%, but statistically insignificant), we can infer that it is predominantly driven by areas with low homeownership.

These results line up with what Proposition 2 predicts, suggesting that our "puzzling" positive IV estimates are indeed because the IV regressions are picking up the effect of successful appeals (i) involving attractive marginal developments which are (ii) in areas where there is less local power.

Table 15: Effect by homeownership share

	OLS		IV	
	(1)	(2)	(3)	(4)
VARIABLES	ln(Price)			
Success*Postappeal	-0.009**	-0.001	0.058**	0.030
	(0.004)	(0.003)	(0.028)	(0.034)
Observations	546,267	474,305	546,267	474,305
R-squared	0.896	0.882	0.676	0.742
Time FE	✓	✓	✓	✓
Appeal X Postcode sector FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Sample	Low ownership	High ownership	Low ownership	High ownership
KP F-stat			39.50	13.22

Notes: Controls include transaction level property characteristics such as property type (i.e., detached, semi-detached or a flat), whether the property is leasehold or freehold, the number of rooms, floor area, the property's current and potential energy efficiency, whether the property was built before 2012; dummies for the deciles of the supply of houses in the 1km vicinity of the appeal one year prior to the appeal decision interacted with relative year dummies; and dummies for the deciles of the average wages across the LPAs one year prior to the appeal decision interacted with relative year dummies. Time fixed-effects consist of calendar year-month fixed-effects and relative year fixed-effects. Standard errors are clustered at the postcode sector level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

6.3 Ruling out alternative mechanisms

6.3.1 Selection into transaction

One limitation of the data is that we only observe transaction prices. However, the outcome of an appeal may affect the choice to buy or sell a nearby property (selection into transaction). For instance, richer households in the neighborhood, who have the

[27]The results presented in Table 15 include all controls. In online Appendix Tables A11 (low ownership sample) and A12 (high ownership sample), we show how the coefficient estimate changes when we move from a specification with just the basic fixed-effects to one where controls are added.

finances to move quickly, may choose to sell after a successful appeal as they anticipate that the new supply would depress the value of their housing asset. If these properties are also more valuable (e.g., bigger and more luxurious), then our IV result of a price increase from a successful appeal might be due to this selection into transaction. We check if this is an issue by regressing some characteristics of the transacted properties on the treatment variable ($Success_i * Postappeal_t$). If there is selection into transaction, then we should expect the types of properties being transacted to be different between neighborhoods with successful and unsuccessful appeals.

Table 16 summarizes our findings on three property characteristics: (i) floor area; (ii) number of rooms; and (iii) energy rating (a higher rating is more energy efficient). The first two measures size and the third is a proxy for the type of materials used in the property.[28] The estimated effects are all close to zero. As such, both the OLS and the IV regressions cannot reject the null that there are no differences in transacted properties near successful versus unsuccessful appeals. We interpret this as evidence that selection into transaction is not a major driver of our main results above.

Table 16: Testing for selection into transaction

	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	ln(Area)	No. rooms	Energy	ln(Area)	No. rooms	Energy
Success*Postappeal	0.002	0.008	-0.002	-0.004	-0.003	-0.019
	(0.001)	(0.006)	(0.003)	(0.012)	(0.049)	(0.028)
Observations	1,020,940	1,020,940	1,020,940	1,020,940	1,020,940	1,020,940
R-squared	0.447	0.443	0.331	0.338	0.330	0.250
Time FE	✓	✓	✓	✓	✓	✓
Appeal X Postcode sector FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
KP F-stat				42.06	42.06	42.06

Notes: Controls include transaction level property characteristics such as property type (i.e., detached, semi-detached or a flat); whether the property is leasehold or freehold; the number of rooms; floor area; the property's current and potential energy efficiency; whether the property was built before 2012; dummies for the deciles of the supply of houses in the 1km vicinity of the appeal one year prior to the appeal decision interacted with relative year dummies; and dummies for the deciles of the average wages across the LPAs one year prior to the appeal decision interacted with relative year dummies. In each column, we exclude as a control the outcome variable. For example, in column 1, floor area is not included as a control. Time fixed-effects consist of calendar year-month fixed-effects and relative year fixed-effects. Standard errors are clustered at the postcode sector level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

6.3.2 The role of distance

Another possible reason to explain our positive IV results could be that we are not looking at transactions that are near enough to the appeal site of the new development.

[28] This is an imperfect measure of "quality" because there may be luxurious building materials that are not energy efficient, and rich households may be willing to pay higher heating costs to maintain them. We recognize this and are simply interested to test if the materials used are different as opposed to better.

The intuition is that opposition against new housing supply is likely to be driven by immediate neighbors (who would be affected by construction noise or hampered views) rather than by the majority of neighbors in the vicinity of a development project (who may benefit from amenities associated with the new development). If this is the case, we should expect to see a negative effect for existing homes near to the successful appeal and a positive effect for those further from the successful appeal.

To test this, columns 1 and 3 of Table 17 show the estimated impact when we restrict the sample to properties within 500m of the appeal site, while columns 2 and 4 show the estimated impact when we restrict the sample to properties 500m to 1km from the appeal site. The OLS estimates across the two sample are similar, suggesting little heterogeneity in the impact by distance from the appeal site. Our IV results are both positive and relatively similar in magnitude, allowing us to rule out different effects by distance.[29]

Table 17: Effect by distance from appeal site

	OLS		IV	
	(1)	(2)	(3)	(4)
VARIABLES	ln(Price)			
Success*Postappeal	-0.007*	-0.006**	0.048*	0.063**
	(0.004)	(0.003)	(0.027)	(0.027)
Observations	311,922	708,882	311,922	708,882
R-squared	0.893	0.895	0.701	0.702
Time FE	✓	✓	✓	✓
Appeal X Postcode sector FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Sample	<=500m	>500m & <=1km	<=500m	>500m & <=1km
KP F-stat			37.87	36.51

Notes: Controls include transaction level property characteristics such as property type (i.e., detached, semi-detached or a flat); whether the property is leasehold or freehold; the number of rooms; floor area; the property's current and potential energy efficiency; whether the property was built before 2012; dummies for the deciles of the supply of houses in the 1km vicinity of the appeal one year prior to the appeal decision interacted with relative year dummies; and dummies for the deciles of the average wages across the LPAs one year prior to the appeal decision interacted with relative year dummies. Time fixed-effects consist of calendar year-month fixed-effects and relative year fixed-effects. Standard errors are clustered at the postcode sector level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

7 Conclusion

In this paper, we contribute to the literature on the causes and effects of land use regulations. In terms of the effects, we examine what happens to the prices of existing homes when developments originally blocked by local governments end up being built. Our OLS results show that on average, overturning the local authority's decision leads

[29]The results presented in Table 17 include all controls. In online Appendix Tables A13 (500m sample) and A14 (500m to 1km sample), we show how the coefficient estimate changes when we move from a specification with just the basic fixed-effects to one where controls are added.

to a small negative effect of around 0.6% on housing prices in the vicinity. This negative result is consistent with the argument that local authorities constrain housing supply to maintain housing values in the area.

Nevertheless, this result could be masking substantial heterogeneity in the types of developments that are successfully appealed. Leveraging the LATE interpretation of an IV estimation strategy, we show that for a subset of projects that were successfully appealed (in particular, marginal developments that are very susceptible to the leniency of inspectors), there is little evidence that these developments reduce housing prices in the vicinity. Instead, our IV estimates suggest a positive impact of around 5.6% from overturning the local authorities' decisions.

This finding that the local authorities are blocking a substantial subpopulation of attractive projects that could actually raise the value of existing homes in the vicinity appears somewhat surprising. Our paper suggests that the cause for this is due to local authorities having to balance the interest between "insiders" and "outsiders". This echoes the observation by Gyourko and Molloy (2015) that land use regulations are often "shaped by the incentives and influence of actors in the local political process", leading to drastic implications on the amount of housing supply as well as the type of developments that end up being built or vetoed in each locality.

Mathematical Appendix

A1 Proof of proposition 1

First, we can work out $P_\kappa(\kappa, s)$ by differentiating the 3 with respect to κ, which gives:

$$-f_r\left(W_r + \psi(\kappa, s) - P(\kappa, s)\right) * (\psi_\kappa(\kappa, s) - P_\kappa(\kappa, s)) = f_p\left(W_p + \phi(\kappa, s) - P(\kappa, s)\right) * (\phi_\kappa(\kappa, s) - P_\kappa(\kappa, s))$$

$$\implies P_\kappa(\kappa, s) = \theta_r(W_r, W_p, \kappa, s) * \psi_\kappa(\kappa, s) + \theta_p(W_r, W_p, \kappa, s) * \phi_\kappa(\kappa, s)$$

Where $f_k(.) = \frac{\partial}{\partial \kappa} F_k(.)$ and $\theta_k(.) = \frac{f_k(.)}{f_k(.) + f_{!k}(.)}$ for $k = \{r, p\}$. Therefore, as the exogenous amenity value of the location increases, the clearing price rises as a weighted average of the increase in utility of existing residents and potential residents.

Next, we define $\underline{\kappa} < 0$ as value of κ that clears the market at $P(\underline{\kappa}, 0) = 0$ and satisfies condition 3 (i.e., the lowest value of κ such that the market clears). Values of $\kappa < \underline{\kappa}$ would imply that $P(\kappa, s) < 0$, which we do not consider because residents can simply abandon their properties, and take their outside option, without paying potential residents. Given $s = 0$, the net weighted social utility of rejecting (i.e., the weighted social utility of rejecting subtracted by that of accepting) is:

$$\begin{aligned} &\omega\tilde{s}\left(0, 0\right) * \left(W_r + \psi(0, 0)\right) + \dot{s}\left(0, 0\right) * \left(W_p + \psi(0, 0) - P(0, 0)\right) \\ &- \left[\omega\tilde{s}(\underline{\kappa}, 0) * \left(W_r + \psi(\underline{\kappa}, 0)\right) + \dot{s}(\underline{\kappa}, 0) * \left(W_p + \phi(\underline{\kappa}, 0)\right)\right] \end{aligned}$$

$$\begin{aligned} &\leq \omega\tilde{s}(\underline{\kappa}, 0) * \left(\psi(0, 0) - \psi(\underline{\kappa}, 0)\right) + \omega\Delta\tilde{s} * \left(W_r + \psi(0, 0)\right) \\ &-\Delta\tilde{s} * \max\left\{W_p + \phi(0, 0) - P(0, 0), W_p + \phi(\underline{\kappa}, 0)\right\} \end{aligned}$$

Where $\Delta\tilde{s} = \tilde{s}\left(0, 0\right) - \tilde{s}\left(\underline{\kappa}, 0\right) > 0$ because $\frac{\partial\tilde{s}}{\partial\kappa} \geq 0$. Since $\psi(0, 0) > \psi(\underline{\kappa}, 0)$:

$$\begin{aligned} &\leq \omega\Delta\tilde{s} * \left(W_r + \psi(0, 0)\right) \\ &-\Delta\tilde{s} * \max\left\{W_p + \phi(0, 0) - P(0, 0), W_p + \phi(\underline{\kappa}, 0)\right\} \end{aligned}$$

$$< 0$$

Because of the assumptions that $\omega \geq \frac{W_p}{W_r}$ and $\psi(\kappa, s) \geq \phi(\kappa, s)$. Therefore, L will reject the development.

Finally, we can work out how the weighted social utility changes with an increase in κ:

$$\frac{\partial}{\partial \kappa}\left[\omega * \tilde{s}\left(\kappa,0\right) * \left(W_r + \psi(\kappa,0)\right) + \left(\dot{s}\left(\kappa,0\right)\right) * \left(W_p + \psi(\kappa,0) - P(\kappa,0)\right)\right]$$

$$= f_r(.)\theta_p(.) * \left(\psi_\kappa(\kappa,0) - \phi_\kappa(\kappa,0)\right) * \left(\omega W_r - W_p + \omega\psi(\kappa,0) - \phi(\kappa,0) + P(\kappa,0)\right)$$

$$+ \left(\omega\tilde{s} - \dot{s}\theta_r(.)\right)\psi_\kappa(\kappa,0)$$

The top line is positive because of the assumptions $\psi_\kappa(\kappa,s) \geq \phi_\kappa(\kappa,s)$ and $\omega \geq \frac{W_p}{W_r}$. The bottom line can be negative if $\left(\omega\tilde{s} - \dot{s}\theta_r(.)\right) < 0$; but we know that:

$$\frac{\partial}{\partial \kappa}\left(\omega\tilde{s} - \dot{s}\theta_r(.)\right) = \left(\omega + \theta_r(.)\right)\frac{\partial \tilde{s}}{\partial \kappa} \geq 0$$

Therefore, we know that there are two possibilities as we increase κ from $\underline{\kappa}$: (i) the weighted social utility of accepting increases with κ over the range $[\underline{\kappa}, \infty)$; or (ii) $\exists \bar{\kappa}$ such that the weighted social utility of accepting decreases over the range $[\underline{\kappa}, \bar{\kappa})$ and increases over the range $(\bar{\kappa}, \infty)$.

If we denote the weighted social utility of accepting as $U(\kappa)$ and the weighted social utility of rejecting as the constant R then there are two possibilities.

Under (i), $U(\underline{\kappa}) < R$, $\lim_{\kappa\to\infty} U(\kappa) = \infty > R$ and $U_\kappa(.) > 0$. So $\exists \kappa^*(\underline{\kappa}, \infty)$ such that $U(\kappa^*) = R$, $U(\kappa) < R$ if $\kappa < \kappa^*$ and $U(\kappa) > R$ if $\kappa > \kappa^*$. Under (ii), since $U_\kappa(.) < 0$ for $\kappa \in [\underline{\kappa}, \bar{\kappa}]$, $U(\bar{\kappa}) < U(\underline{\kappa}) < R$. So $U(\bar{\kappa}) < R$, $\lim_{\kappa\to\infty} U(\kappa) = \infty > R$ and $U_\kappa(.) > 0$. So $\exists \kappa^* \in (\bar{\kappa}, \infty)$ such that $U(\kappa^*) = R$, $U(\kappa) < R$ if $\kappa < \kappa^*$ and $U(\kappa) > R$ if $\kappa > \kappa^*$.

A2 Proof of proposition 2

Define:

$$H(\kappa, \omega) \equiv U(\kappa^*, \omega) - R = 0$$

Then, by the implicit function theorem:

$$\frac{\partial \kappa^*}{\partial \omega} = \frac{-H_\omega(\kappa^*, \omega)}{H_\kappa(\kappa^*, \omega)} = \frac{-\left[\tilde{s}(\kappa^*, 0) * \left(W_r + \psi(\kappa^*, 0)\right) - \tilde{s}(0,0) * \left(W_r + \psi(0,0)\right)\right]}{U_\kappa(.)}$$

From the proof in A1 we know that at κ^*, $U_\kappa(.) > 0$. Further, if $\kappa^* < 0$ then $\tilde{s}(\kappa^*, 0) * \left(W_r + \psi(\kappa^*, 0)\right) < \tilde{s}(0,0) * \left(W_r + \psi(0,0)\right)$ so $\frac{\partial \kappa^*}{\partial \omega} > 0$. Also, if $\kappa^* > 0$ then $\tilde{s}(\kappa^*, 0) * \left(W_r + \psi(\kappa^*, 0)\right) > \tilde{s}(0,0) * \left(W_r + \psi(0,0)\right)$ so $\frac{\partial \kappa^*}{\partial \omega} < 0$.

A3 Implications of selection on OLS and IV regressions

Given propositions 1 and 2 in Section 6.1, we can now work out what our regressions are estimating. We consider two regressions: the first is an OLS regression $ln(p_i) = \beta_0 + \beta$**1.**Successful Appeal$_i + e_i$, where the variation in successful appeals is generated by the random assignment of inspectors (or the random variable η). Here our coefficient of interest is β. The second is an IV regression, where we run the regressions $ln(p_i) = \gamma_0 + \gamma_1\eta_i + u_i$ and **1.**Successful Appeal$_i = \pi_0 + \pi_1\eta_i + v_i$, and our coefficient of interest is $\frac{\gamma_1}{\pi_1}$.

The price change when we randomly assign η is: $\Delta P = E\left[P(\kappa, s) - P(0,0) | \kappa < \kappa^*\right]$.

Hence, we know that β_1 in our OLS regression will be:

$$\frac{\Delta P}{P_0} = C \int_{\underline{\kappa}}^{\kappa^*} \left[P(\kappa, s) - P(0, s)\right] dH(\kappa)$$

$$C \equiv \frac{1}{H(\kappa^*) - H(\underline{\kappa})} * \frac{1}{P_0}$$

$\underline{\kappa}$ is the lowest κ, for which the market still clears at a price ≥ 0, and $H(\kappa)$ represents nature's distribution of κ. There are two main effects that influence the sign of $P(\kappa, s) - P(0,0)$:

1. $P(\kappa, s) - P(\kappa, 0)$ is a supply effect, or a move along the demand curve, and is always negative.

2. $P(\kappa, 0) - P(0,0)$ is a demand shifter, and depends on whether κ is greater or less than 0.

We can also work out that $\frac{\gamma_1}{\pi_1}$ in our IV regression will be:

$$C \int_{\underline{\kappa}}^{\kappa^*} \tilde{F}\Big(I(s, \kappa)\Big) \left[P(\kappa, s) - P(0,0)\right] dH(\kappa)$$

where $\tilde{F}(I(s,\kappa)) = \frac{g(I(s,\kappa))}{\int_{\underline{\kappa}}^{\kappa^*} g(I(s,\kappa)) dH(\kappa)}$. When there are no omitted variables, the key difference between the OLS and the IV estimates are the weights (1 versus $\tilde{F}(.)$). The IV estimates a Local Average Treatment Effect (LATE) which skews towards developments that have a high *increase in the probability of success* from being assigned an inspector with a higher η. Under an assumption of homogeneity, that is κ is a constant, $\tilde{F}\Big(I(\tilde{s}, \kappa)\Big) = 1$ and the OLS and IV estimates are the same. This forms the basis of our regression analysis in Section 4.

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Online Appendix A: Additional tables

Table A1: Regression of indicator for successful appeal on proxy variables

VARIABLES	(1) Success
Number of live appeal cases	0.000
	(0.000)
Development with 1 residence	-0.021**
	(0.010)
Development with 2 residences	-0.006
	(0.010)
Number of residences	0.001***
	(0.000)
Number of residences squared	-0.000***
	(0.000)
Bespoke timeline	-0.071**
	(0.035)
Costs applied for	0.176***
	(0.010)
Number of days to resolve appeal	0.000
	(0.000)
Number of linked appeal cases	-0.012
	(0.012)
Intermediary involved	0.060***
	(0.009)
LPA historical number of appeals	0.007***
	(0.001)
LPA historical appeals success rate	-0.023***
	(0.005)
Observations	28,519
R-squared	0.095
Start year FE	✓
Procedure FE	✓
Key words included	✓
LPA linear trends	✓

Notes: The key words included are: frequent; erection; dwellings; access; residential; associated; parking; garage; rear; extension; landscaping; demolition; existing; open; road; works; flats; storey; land; houses; apartments; affordable; site; class; redevelopment; conversion; commercial; vehicular; house; and facilities. Standard errors are clustered at the LPA level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table A2: Dynamic difference-in-differences

VARIABLES	(1) ln(Price)	(2) ln(Price)
Success X Relative Year -3	0.007**	0.004
	(0.004)	(0.003)
Success X Relative Year -2	0.003	0.002
	(0.003)	(0.002)
Success X Relative Year 0	-0.002	-0.002
	(0.003)	(0.002)
Success X Relative Year 1	-0.005	-0.003
	(0.003)	(0.003)
Success X Relative Year 2	-0.009**	-0.007**
	(0.004)	(0.004)
Success X Relative Year 3	-0.016***	-0.007
	(0.005)	(0.005)
Observations	1,233,078	1,020,940
R-squared	0.609	0.890
Time FE	✓	✓
Appeal X Postcode sector FE	✓	✓
Controls		✓

Notes: Controls include transaction level property characteristics such as property type (i.e., detached, semi-detached or a flat); whether the property is leasehold or freehold; the number of rooms; floor area; the property's current and potential energy efficiency; whether the property was built before 2012; dummies for the deciles of the supply of houses in the 1km vicinity of the appeal one year prior to the appeal decision interacted with relative year dummies; and dummies for the deciles of the average wages across the LPAs one year prior to the appeal decision interacted with relative year dummies. Time fixed-effects consist of calendar year-month fixed-effects and relative year fixed-effects. Standard errors are clustered at the outward code level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table A3: Robustness to restricting sample to two years before or after appeals

	OLS		IV	
	(1)	(2)	(3)	(4)
VARIABLES	ln(Price)			
Success*Postappeal	-0.006**	-0.005**	0.058**	0.037*
	(0.003)	(0.002)	(0.024)	(0.021)
Observations	942,809	779,407	942,809	779,407
R-squared	0.613	0.893	-0.002	0.709
Time FE	✓	✓	✓	✓
Appeal X Postcode sector FE	✓	✓	✓	✓
Controls		✓		✓
KP F-stat			44.70	42.07

Notes: Controls include transaction level property characteristics such as property type (i.e., detached, semi-detached or a flat); whether the property is leasehold or freehold; the number of rooms; floor area; the property's current and potential energy efficiency; whether the property was built before 2012; dummies for the deciles of the supply of houses in the 1km vicinity of the appeal one year prior to the appeal decision interacted with relative year dummies; and dummies for the deciles of the average wages across the LPAs one year prior to the appeal decision interacted with relative year dummies. Time fixed-effects consist of calendar year-month fixed-effects and relative year fixed-effects. Standard errors are clustered at the outward code level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table A4: Robustness to dropping transactions in 2020

	OLS		IV	
	(1)	(2)	(3)	(4)
VARIABLES	ln(Price)			
Success*Postappeal	-0.009***	-0.006**	0.072***	0.056**
	(0.003)	(0.003)	(0.027)	(0.024)
Observations	1,230,016	1,018,270	1,230,016	1,018,270
R-squared	0.609	0.890	-0.003	0.705
Time FE	✓	✓	✓	✓
Appeal X Postcode sector FE	✓	✓	✓	✓
Controls		✓		✓
KP F-stat			44.98	42.25

Notes: Controls include transaction level property characteristics such as property type (i.e., detached, semi-detached or a flat); whether the property is leasehold or freehold; the number of rooms; floor area; the property's current and potential energy efficiency; whether the property was built before 2012; dummies for the deciles of the supply of houses in the 1km vicinity of the appeal one year prior to the appeal decision interacted with relative year dummies; and dummies for the deciles of the average wages across the LPAs one year prior to the appeal decision interacted with relative year dummies.. Time fixed-effects consist of calendar year-month fixed-effects and relative year fixed-effects. Standard errors are clustered at the outward code level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table A5: Robustness to dropping appeals that are within 2km to other appeals

	OLS		IV	
	(1)	(2)	(3)	(4)
VARIABLES	ln(Price)			
Success*Postappeal	-0.008*	-0.007*	0.189	0.100
	(0.004)	(0.004)	(0.118)	(0.083)
Observations	565,794	459,738	565,794	459,738
R-squared	0.601	0.890	-0.014	0.708
Time FE	✓	✓	✓	✓
Appeal X Postcode sector FE	✓	✓	✓	✓
Controls		✓		✓
KP F-stat			5.070	4.951

Notes: Controls include transaction level property characteristics such as property type (i.e., detached, semi-detached or a flat); whether the property is leasehold or freehold; the number of rooms; floor area; the property's current and potential energy efficiency; whether the property was built before 2012; dummies for the deciles of the supply of houses in the 1km vicinity of the appeal one year prior to the appeal decision interacted with relative year dummies; and dummies for the deciles of the average wages across the LPAs one year prior to the appeal decision interacted with relative year dummies. Time fixed-effects consist of calendar year-month fixed-effects and relative year fixed-effects. Standard errors are clustered at the outward code level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table A6: Robustness to different clustering

	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	ln(Price)					
Success*Postappeal	-0.006*	-0.006**	-0.006***	0.056*	0.056**	0.056***
	(0.004)	(0.003)	(0.002)	(0.031)	(0.025)	(0.021)
Observations	1,020,940	1,020,940	1,020,940	1,020,940	1,020,940	1,020,940
R-squared	0.890	0.890	0.890	0.705	0.705	0.705
Time FE	✓	✓	✓	✓	✓	✓
Appeal X Postcode sector FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Clusters	Outward	AppealXSector	1km grids	Outward	AppealXSector	1km grids
KP F-stat				24.48	41.70	62.43

Notes: Controls include transaction level property characteristics such as property type (i.e., detached, semi-detached or a flat); whether the property is leasehold or freehold; the number of rooms; floor area; the property's current and potential energy efficiency; whether the property was built before 2012; dummies for the deciles of the supply of houses in the 1km vicinity of the appeal one year prior to the appeal decision interacted with relative year dummies; and dummies for the deciles of the average wages across the LPAs one year prior to the appeal decision interacted with relative year dummies. Time fixed-effects consist of calendar year-month fixed-effects and relative year fixed-effects. Standard errors are clustered at the outward code level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table A7: Robustness to trimming (IV results)

	(1)	(2)	(3)	(4)
VARIABLES	ln(Price)			
Success*Postappeal	0.069***	0.065**	0.116**	0.123**
	(0.027)	(0.026)	(0.057)	(0.062)
Observations	1,201,166	994,642	1,101,423	912,209
R-squared	-0.002	0.704	-0.006	0.700
Time FE	✓	✓	✓	✓
Appeal X Postcode sector FE	✓	✓	✓	✓
Controls		✓		✓
Trim	1%	1%	5%	5%
KP F-stat	47.74	40.47	13.48	9.926

Notes: Controls include transaction level property characteristics such as property type (i.e., detached, semi-detached or a flat); whether the property is leasehold or freehold; the number of rooms; floor area; the property's current and potential energy efficiency; whether the property was built before 2012; dummies for the deciles of the supply of houses in the 1km vicinity of the appeal one year prior to the appeal decision interacted with relative year dummies; and dummies for the deciles of the average wages across the LPAs one year prior to the appeal decision interacted with relative year dummies. Time fixed-effects consist of calendar year-month fixed-effects and relative year fixed-effects. Standard errors are clustered at the outward code level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table A8: Robustness to using only inspectors who presided over at least 40 appeals (IV results)

	(1)	(2)
VARIABLES	ln(Price)	
Success*Postappeal	0.059**	0.051**
	(0.023)	(0.022)
Observations	1,146,433	949,080
R-squared	-0.002	0.705
Time FE	✓	✓
Appeal X Postcode sector FE	✓	✓
Controls		✓
KP F-stat	62.99	52.36

Notes: Controls include transaction level property characteristics such as property type (i.e., detached, semi-detached or a flat); whether the property is leasehold or freehold; the number of rooms; floor area; the property's current and potential energy efficiency; whether the property was built before 2012; dummies for the deciles of the supply of houses in the 1km vicinity of the appeal one year prior to the appeal decision interacted with relative year dummies; and dummies for the deciles of the average wages across the LPAs one year prior to the appeal decision interacted with relative year dummies. Time fixed-effects consist of calendar year-month fixed-effects and relative year fixed-effects. Standard errors are clustered at the outward code level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table A9: Effect by supply shock (small supply shock)

	OLS		IV	
	(1)	(2)	(3)	(4)
VARIABLES	ln(Price)			
Success*Postappeal	-0.008**	-0.005	0.081***	0.060**
	(0.004)	(0.003)	(0.028)	(0.025)
Observations	958,027	788,720	958,027	788,720
R-squared	0.626	0.894	-0.003	0.702
Time FE	✓	✓	✓	✓
Appeal X Postcode sector FE	✓	✓	✓	✓
Controls		✓		✓
Sample	Small shock	Small shock	Small shock	Small shock
KP F-stat			47.82	45.69

Notes: Controls include transaction level property characteristics such as property type (i.e., detached, semi-detached or a flat); whether the property is leasehold or freehold; the number of rooms; floor area; the property's current and potential energy efficiency; whether the property was built before 2012; dummies for the deciles of the supply of houses in the 1km vicinity of the appeal one year prior to the appeal decision interacted with relative year dummies; and dummies for the deciles of the average wages across the LPAs one year prior to the appeal decision interacted with relative year dummies. Time fixed-effects consist of calendar year-month fixed-effects and relative year fixed-effects. Standard errors are clustered at the outward code level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table A10: Effect by supply shock (large supply shock)

	OLS		IV	
	(1)	(2)	(3)	(4)
VARIABLES	ln(Price)			
Success*Postappeal	-0.004	-0.007*	-0.007	0.022
	(0.005)	(0.004)	(0.096)	(0.077)
Observations	275,051	232,220	275,051	232,220
R-squared	0.518	0.871	0.000	0.721
Time FE	✓	✓	✓	✓
Appeal X Postcode sector FE	✓	✓	✓	✓
Controls		✓		✓
Sample	Large shock	Large shock	Large shock	Large shock
KP F-stat			2.087	2.478

Notes: Controls include transaction level property characteristics such as property type (i.e., detached, semi-detached or a flat); whether the property is leasehold or freehold; the number of rooms; floor area; the property's current and potential energy efficiency; whether the property was built before 2012; dummies for the deciles of the supply of houses in the 1km vicinity of the appeal one year prior to the appeal decision interacted with relative year dummies; and dummies for the deciles of the average wages across the LPAs one year prior to the appeal decision interacted with relative year dummies. Time fixed-effects consist of calendar year-month fixed-effects and relative year fixed-effects. Standard errors are clustered at the outward code level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table A11: Effect by homeownership rate (Low)

	OLS		IV	
	(1)	(2)	(3)	(4)
VARIABLES	ln(Price)			
Success*Postappeal	-0.013***	-0.009**	0.096***	0.058**
	(0.005)	(0.004)	(0.036)	(0.028)
Observations	673,544	546,267	673,544	546,267
R-squared	0.660	0.896	-0.004	0.676
Time FE	✓	✓	✓	✓
Appeal X Postcode sector FE	✓	✓	✓	✓
Controls		✓		✓
Sample	Low ownership	Low ownership	Low ownership	Low ownership
KP F-stat			34.49	39.50

Notes: Controls include transaction level property characteristics such as property type (i.e., detached, semi-detached or a flat); whether the property is leasehold or freehold; the number of rooms; floor area; the property's current and potential energy efficiency; whether the property was built before 2012; dummies for the deciles of the supply of houses in the 1km vicinity of the appeal one year prior to the appeal decision interacted with relative year dummies; and dummies for the deciles of the average wages across the LPAs one year prior to the appeal decision interacted with relative year dummies. Time fixed-effects consist of calendar year-month fixed-effects and relative year fixed-effects. Standard errors are clustered at the outward code level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table A12: Effect by homeownership rate (High)

	OLS		IV	
	(1)	(2)	(3)	(4)
VARIABLES	ln(Price)			
Success*Postappeal	-0.001	-0.001	0.026	0.030
	(0.004)	(0.003)	(0.037)	(0.034)
Observations	559,108	474,305	559,108	474,305
R-squared	0.522	0.882	-0.000	0.742
Time FE	✓	✓	✓	✓
Appeal X Postcode sector FE	✓	✓	✓	✓
Controls		✓		✓
Sample	High ownership	High ownership	High ownership	High ownership
KP F-stat			13.01	13.22

Notes: Controls include transaction level property characteristics such as property type (i.e., detached, semi-detached or a flat); whether the property is leasehold or freehold; the number of rooms; floor area; the property's current and potential energy efficiency; whether the property was built before 2012; dummies for the deciles of the supply of houses in the 1km vicinity of the appeal one year prior to the appeal decision interacted with relative year dummies; and dummies for the deciles of the average wages across the LPAs one year prior to the appeal decision interacted with relative year dummies. Time fixed-effects consist of calendar year-month fixed-effects and relative year fixed-effects. Standard errors are clustered at the outward code level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table A13: Effect by distance from appeal site (500m)

	OLS		IV	
	(1)	(2)	(3)	(4)
VARIABLES	ln(Price)			
Success*Postappeal	-0.010**	-0.007*	0.038	0.048*
	(0.004)	(0.004)	(0.030)	(0.027)
Observations	379,855	311,922	379,855	311,922
R-squared	0.621	0.893	-0.001	0.701
Time FE	✓	✓	✓	✓
Appeal X Postcode sector FE	✓	✓	✓	✓
Controls		✓		✓
Sample	<=500m	<=500m	<=500m	<=500m
KP F-stat			44.69	37.87

Notes: Controls include transaction level property characteristics such as property type (i.e., detached, semi-detached or a flat); whether the property is leasehold or freehold; the number of rooms; floor area; the property's current and potential energy efficiency; whether the property was built before 2012; dummies for the deciles of the supply of houses in the 1km vicinity of the appeal one year prior to the appeal decision interacted with relative year dummies; and dummies for the deciles of the average wages across the LPAs one year prior to the appeal decision interacted with relative year dummies. Time fixed-effects consist of calendar year-month fixed-effects and relative year fixed-effects. Standard errors are clustered at the outward code level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table A14: Effect by distance from appeal site (500m to 1km)

	OLS		IV	
	(1)	(2)	(3)	(4)
VARIABLES	ln(Price)			
Success*Postappeal	-0.009***	-0.006**	0.095***	0.063**
	(0.003)	(0.003)	(0.033)	(0.027)
Observations	853,087	708,882	853,087	708,882
R-squared	0.626	0.895	-0.004	0.702
Time FE	✓	✓	✓	✓
Appeal X Postcode sector FE	✓	✓	✓	✓
Controls		✓		✓
Sample	>500m & <=1km	>500m & <=1km	>500m & <=1km	>500m & <=1km
KP F-stat			36.38	36.51

Notes: Controls include transaction level property characteristics such as property type (i.e., detached, semi-detached or a flat); whether the property is leasehold or freehold; the number of rooms; floor area; the property's current and potential energy efficiency; whether the property was built before 2012; dummies for the deciles of the supply of houses in the 1km vicinity of the appeal one year prior to the appeal decision interacted with relative year dummies; and dummies for the deciles of the average wages across the LPAs one year prior to the appeal decision interacted with relative year dummies. Time fixed-effects consist of calendar year-month fixed-effects and relative year fixed-effects. Standard errors are clustered at the outward code level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.