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Empty homes, longer commutes: The unintended consequences of more restrictive local planning☆

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ABSTRACT

We investigate the impact of land use regulation on housing vacancy rates. Using a 30-year panel dataset on land use regulation for 350 English Local Authorities (LAs) and addressing potential reverse causation and other endogeneity concerns, we find that tighter local planning constraints increase local housing vacancy rates: a one standard deviation increase in restrictiveness causes the local vacancy rate to increase by 0.9 percentage points (23%). The same increase in local restrictiveness also causes a 6.1% rise in commuting distances. The results underline the interdependence of local housing and Labour markets and the unintended adverse impact of more restrictive planning policies.

1. Introduction

To an economist it might seem self-evident that vacancies in the housing stock are a natural feature of how any market must work. There even are 'uneaten' apples in a well-functioning fruit market. The Labour market is very much more comparable to the housing market and virtually all mainstream economists expect to observe at least frictional unemployment when the Labour market is in equilibrium (see Pissarides, 1985; Mortensen and Pissarides, 1994; Pissarides, 1994). It is the same in any normally functioning housing market. In equilibrium there must be vacant houses as people move and 'house-hunt', as people die or houses wait to be demolished and sellers wait to find a buyer (Han and Strange, 2015).

But this view is often not shared by those who design buildings and influence urban policy or with those who plan housing supply – at least in England. Even in what was then one of the least restrictive English Regions, the East Midlands, in calculating how much land should be allocated for housing to meet their estimate of their region's 'housing needs', planners argued that they could allocate less land because they assumed they would reduce the number of vacant homes:

> '*The annual average housing provision reflects a number of factors, transactional vacancies in new stock (about 2%) add 7,000 to the requirement, but offset against that is an assumption that vacancies in the existing stock should be reduced by a half per-cent, which will bring 8,600 dwellings back into use.*' (Government Office for the East Midlands, 2005, Appendix 4, p. 91).

It is surely true that using ones stock of capital more intensively is a way of increasing efficiency. That is just how cut price airlines operate: they keep their seats full and their aircraft in the air. They, however, had an analysis of how to achieve this. They did not just assume planes would spend more of their lives in the air and seats would be fuller. Unless we understand why houses are vacant we cannot rationally hope to reduce the number of vacant houses just by being more restrictive. To help improve our understanding of the factors which determine vacancy rates in the housing market, this paper investigates the causal,

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albeit reduced-form, impact of regulatory restrictiveness. Moreover, since housing and Labour markets are interdependent, we also investigate the related issue of how local regulatory restrictiveness affects the average commute distance of those working in the jurisdiction. These are not the only outcomes of greater regulatory restrictiveness. We find that there are other measurable effects apart from raised house prices, all apparently responses to poorer housing market matching (discussed below); more households are in temporary homes, crowding is greater, and in-migration lower.

These results stem from the insight that policy imposed restrictions on housing supply may have two opposing effects.[1] The first of these we call the 'opportunity cost effect'. Tighter restrictions on supply imply fewer available houses and therefore more demand pressure for existing homes, increasing house prices and thus the opportunity cost of keeping housing empty. This will lead to a lower vacancy rate all else equal. If this 'opportunity cost effect' was the only effect at work, tighter supply constraints should unambiguously lower vacancy rates.

There is however a second effect, which we refer to as the 'mismatch effect'. Tighter supply constraints not only reduce supply of new houses but also influence the composition and adaptability of the bundle of attributes of both the existing housing stock and those of new built homes. Over time the structure of households' demand for housing attributes changes because incomes rise, the demographic structure of the population changes and preferences themselves may change. For example, as real incomes rise, so does the demand for certain attributes depending on the varying income elasticity of demand for them.[2] In addition there may be demographic changes such as an increase in the proportion of single adults, which mean that market preferences change.

If the attributes of the housing stock, as a consequence of planning constraints, cannot, or can only more slowly adjust to these changes on the demand side, matching the demand for housing attributes with the supply of those available will inevitably become more difficult. Hence, in line with Wheaton (1990), mismatched households may have to stay longer in a less restrictive housing market while searching in a more restrictive one, implying a relatively lower vacancy rate in the less restrictive market and a higher vacancy rate in the more restrictive one. Mismatched households may also take temporary accommodation and search for longer in more restrictive markets or have to search further afield for a suitable home; they become mismatched on the locational characteristics of houses implying longer commutes.

Our aim in this paper is to determine the net effect of these two opposing forces – the opportunity cost effect versus the mismatch effect – in order to identify the role that regulatory restrictiveness plays in determining the vacancy rate in local housing markets. To do so, we analyse panel data on housing vacancies from 1981 to 2011 for 350 English Local Authorities (LAs), the basic local jurisdictional unit that implements planning policies and approves or rejects individual planning applications. One key concern in this analysis is the endogeneity of local planning restrictiveness. The stylised fact that policy makers and local planners may respond to higher vacancy rates by restricting supply suggests possible reverse causation. Regulatory constraints may also be endogenous to unobserved demand factors (Hilber and Robert-Nicoud, 2013; Davidoff, 2016) and those demand factors may directly affect vacancy rates. To account for possible reverse causation and omitted variable bias and thus identify the causal effect of regulatory restrictiveness, we employ an instrumental variable strategy by exploiting specific features of the British voting system which induces a substantial 'randomness' of seats won (or lost) beyond the vote share. That is, we use the share of Labour seats in LAs, controlling for the share of Labour votes in a flexible way, as an instrumental variable to identify local planning restrictiveness. One could query this identification strategy because, for example, the political composition of an LA could influence local government expenditures and those, in turn, might influence house prices and vacancy rates. Based on a series of placebo regressions, we show that these alternative explanations do not plausibly invalidate the main conclusions.

Our two key empirical findings are as follows. First, when we naively look at cross-sectional data, we find a negative relationship between more restrictive local planning and local vacancy rates, superficially appearing to confirm "planners' assumptions". However, when we (*i*) use first differencing and so control for time-invariant unobservable characteristics, (*ii*) properly account for the endogeneity of restrictiveness by instrumenting for it and (*iii*) control for other relevant factors, more restrictive places have a significantly – and substantially – *higher* vacancy rate. That is, the underlying causal relationship appears to be exactly the opposite to that which planners assume. Based on our most rigorous empirical specification, a one standard deviation increase in local regulatory restrictiveness causes the average local vacancy rate to increase by about 0.9 percentage points (23%).

Second, we find that regulation-induced mismatch has spatial implications for Labour markets. Workers with jobs in LAs with more restrictive planning have to search for housing they can afford and match their preferences further afield; so they are more likely to be locationally mismatched and have to commute further. Using a similar approach to that used for investigating the underlying relationship between the vacancy rate and restrictiveness we find that a one standard deviation increase in local regulatory restrictiveness causes an increase in average commuting distance of some 6.1%. We also provide additional suggestive evidence relating to other proxies for mismatch, such as the share of crowded or non-permanent properties and the share of migrants.

Our findings, therefore, strongly suggest that tighter local planning restrictiveness not only leads to less efficient housing market matching but also this effect dominates the opportunity cost effect, resulting in higher local vacancy rates overall and longer average commutes. Hence, local efforts to reduce the number of vacant homes by imposing supply restrictions have three unintended effects: they increase the local vacancy rate and they increase the average commuting distance of those who work in the jurisdiction – thereby causing a welfare cost. In addition, as the literature shows, they increase local house prices (see e.g. Cheshire and Sheppard, 2002; Glaeser and Gyourko, 2003; Hilber and Vermeulen, 2016).

We proceed as follows. In the next section we discuss in more depth the link between land use regulation and mismatch in the housing market and how that affects the local vacancy rate and the average commuting distance. We then describe our data and set out our main results. The final section draws conclusions.

2. Land use regulation, housing market search and vacant housing

The price of housing services is a function of both demand and supply in the relevant local markets. Various empirical studies document a positive effect of regulatory restrictiveness on house prices (Cheshire and Sheppard, 2002; Glaeser and Gyourko, 2003; Glaeser et al., 2005a, 2005b; Quigley and Raphael, 2005; Ihlanfeldt, 2007; Hilber and Vermeulen, 2016).

What these studies do not consider is the fact that, on the seller's side, it takes time to sell a house and, on the buyer's side, search for a new house is costly too. These search frictions lead to housing vacancies (Merlo and Ortalo-Magné, 2004; Han and Strange, 2015). It has been documented – and our data also suggest – that housing vacancies are

[1] Regulation may have more than two effects. We discuss one potential additional mechanism – a real options argument – in Section 3.7. If greater restrictiveness led to greater price volatility then under certain assumptions this might induce owners to postpone renting or selling their properties, implying a higher vacancy rate (Grenadier, 1995, 1996). Empirically, however, we can find no evidence that such a mechanism plays a significant part in explaining what we find. Other mechanisms are also discussed in that section.

[2] The income elasticity of demand for space both inside houses and in gardens seems to be particularly strong: Cheshire and Sheppard (1998) estimate an elasticity of close to two.

not constant across space and time and depend on the characteristics and preferences of households living in a housing market, as well as on those of the location such as characteristics that are systematic of persistently weak housing demand (Rosen and Smith, 1983; Gabriel and Nothaft, 1988; Gabriel and Nothaft, 2001; Deng et al., 2003; Molloy, 2016). However, the impact of land use restrictions on housing vacancies has not yet been studied.

In the context of this paper we use data on local jurisdictions – in Britain, LAs – which we refer to as local housing markets.[3] On the demand side, households often search in a local housing market while still living in another local market, for example due to changes in where they work (Mulalic et al., 2014; Koster and Van Ommeren, 2017). On the supply side, the characteristics of housing are the result of both the characteristics of new build housing and the adaptation of the characteristics of the existing stock.

The degree of regulatory restrictiveness influences the characteristics of new construction and of the existing stock in very great detail. Both new construction and significant changes to the characteristics of existing houses – converting loft to living space, for example – likely require 'development control' permission. This is the responsibility of the LA's Planning Committee made up of locally elected politicians. This decision making process tends to be politicised and unlike a Zoning or Master Planning system, such as in force in the US or in most of Continental Europe, decisions are not very predictable.

As noted in the introduction, planning induced housing supply restrictions will have two opposing effects on the housing vacancy rate: an 'opportunity cost effect' and a 'mismatch effect'. The opportunity cost effect works via restrictions of supply reducing the availability of land for development (see for example Cheshire and Sheppard, 2005, or Hilber and Vermeulen, 2016). This reduces the rate of new building and so over time the size of the stock of housing relative to demand within the market. This, all else equal, increases prices and thus the opportunity cost of keeping housing vacant. The effect of this is unambiguously to *reduce* vacancy rates. It will also be likely to increase price volatility.

However, more restrictive planning policies will also change the bundle of attributes on offer and, other things equal, slow the rate of adaptation of housing characteristics to changes in the structure of demand with respect to them – the mismatch effect. The latter effect is expected to *increase* vacancy rates. This will come about via two separate forces, one working on the characteristics of new build and the other on the adaptation of the characteristics of the existing stock of houses.

The first force may imply that new build houses become smaller, more distant from jobs and are more likely to be in the form of flats or terraced houses, because there is less land available for dwellings. The second force arises because the structure of demand for housing characteristics changes over time and to accommodate this, the characteristics of the existing stock of housing need to be constantly adjusted. For example, entry to the best state schools in Britain is determined by the exact location of houses. As the relative standing of different schools changes over time, people seeking to 'buy' entry to better state schools will want more bedroom space in the best schools' catchment areas. However, the more restrictive is the LA, the more difficult it will be to adapt existing houses to provide more space or for developers to build additional family housing near better schools.

Another example is that as more cars have been bought (car ownership has increased 13-fold since the current form of land use planning in England was introduced in 1947 and doubled since our vacancy data starts in 1981, Department of Transport, 2013), the demand for garages and off street parking has increased. Such examples of ways in which the demand for housing attributes changes over time could be increased almost indefinitely. However, what it means is that if the supply and demand for the structural characteristics of housing are to be efficiently matched to each other, there will need to be constant adaptation of the characteristics of the existing stock of houses. So more restrictive LAs will slow the adaptation of the existing stock to (changes in) the structure of demand for housing attributes.

Over time, in more restrictive LAs the characteristics of new and existing housing available will be less adapted to preferences of households. Hence, other things equal, if people have a (strong) idiosyncratic preference for locations and house type (e.g. a double-earner household with children that needs at least two-bedrooms and garden space), they will spend more time searching for housing that matches their preferences. When households live in a less restrictive housing market while searching in the more restrictive local market, this will imply a decrease in the vacancy rate in the former and an increase in the latter housing market.[4] In other words, given idiosyncratic preferences, households stay longer in the 'wrong' places.

This may imply that younger people live longer with their parents in 'crowded' properties, or that households are induced to stay in temporary accommodation while searching. Because the housing stock does not match their current preferences, this implies a higher vacancy rate, other things equal, in the more restrictive housing market. We provide some evidence on some of these different symptoms of mismatch in Section 3.6 where we look at how regulation influences the share of non-permanent homes and 'crowded' properties.

We discuss also a more obvious measure of mismatch in Section 3.4: commuting distances from the workplace in the LA. We do indeed find that for workers in more restrictive LAs commuting distances increase significantly. This result is consistent with house hunters finding it more difficult to match their preferences in more restrictive local housing markets so becoming 'mismatched' locationally. This has interesting implications for the boundaries of local Labour markets – they appear to be determined not just by transport costs but also by local planning policies – and how these affect the total supply of housing and the supply of individual housing characteristics. Because households may decide not to move to the desired more restricted place, the share of in-migrants is expected to be lower in the more restrictive housing market. We provide evidence for the latter in Section 3.6.

The well-documented fact that tighter local regulation leads to higher prices is indicative that the opportunity cost effect may be important in determining local vacancy rates. However, we lack evidence on the importance of the offsetting mismatch effect. Thus the net effect of local regulatory restrictiveness on local vacancy rates is ambiguous. The empirical analysis that follows aims to identify this *net* effect while eliminating alternative explanations.

One may question whether changes in vacancy rates are a sufficient statistic when one is interested in the welfare effects of land use regulation. We do not argue that vacancy rates *in general* are a sufficient statistic. However, when one is specifically interested in the change in welfare due to an increase in mismatch caused by more restrictive local planning, an increase in the vacancy rate (beyond the natural rate) is a sufficient statistic for the former.

In line with a large Labour market literature on matching, and as demonstrated by Koster and Van Ommeren (2017), from a welfare perspective housing search may be either too low or too high. However,

[3] It might be argued that Travel to Work Areas (TTWA) approximate more closely to spatial housing markets but as our results demonstrate, the geographical extent of both housing and Labour markets is jointly determined. Planning policy is implemented by the local jurisdiction, the LA, so it is only by using these as our units of analysis that the relationship between restrictiveness and commuting distances can be revealed. Not only do TTWAs not correspond to any political jurisdiction but our finding on the impact of planning restrictiveness on distance commuted shows their boundaries are partially determined by the policy actions of their constituent jurisdictions.

[4] In the Web Appendix we demonstrate this in a standard search model setting, building on the seminal paper by Wheaton (1990). Using numerical simulations, we formally demonstrate that under realistic parameter assumptions an increase in the (relative) regulatory restrictiveness in a particular market increases the local vacancy rate in that market (and lowers it in the comparably less restrictive market), even with perfectly inelastic total demand for housing.

when search is too low, that is caused by an externality for sellers: if buyers search more they will find a suitable property sooner, thereby reducing the time on the market and the associated costs for the seller. Buyers do not take this into account when increasing search effort. However, a regulation-induced increase in vacancy rates while increasing search effort, neither reduces sales times, nor increases matching quality, so the welfare effects are unambiguously negative.

This is important because planning policies that aim to reduce vacancies by reducing new construction but end up leaving more houses empty, cause an under-utilisation of a major capital asset. According to ONS by the end of 2013 houses accounted for 61% of the UK's net worth: up from 48.7% 20 years previously (ONS, 2016). So the capital stock represented by housing is very significant indeed and so its underutilisation represents a significant economic inefficiency.

The effects on commuting are important in their own right as again they represent a welfare loss resulting from increased difficulty of matching.

Of course planning policies per se have the potential to increase social welfare via correcting market failures and we are not claiming here that our evidence on the effect of land use regulation on vacancy rates and commuting distances in isolation suggests that local planning restrictions reduce net welfare. However, there is evidence at least for the UK and the US that an increase in the restrictiveness of planning policy (from current levels) has a net negative effect on welfare (see Cheshire and Sheppard, 2002 for the UK and Turner et al. (2014) for the US). In this context, our finding that more restrictive local planning increases the local vacancy rate and commuting distance via raising mismatch in the local housing market, adds to the alleged net negative effect on welfare. Both effects (on vacancy rates and commuting distances) have been ignored in the literature so far.

3. Empirical analysis

3.1. Data and descriptive statistics

Our data come from several sources. The vacancy rates are from the UK Census for the years 1981, 1991, 2001 and 2011.[5] For the first three Census years we have information on the number of vacant dwellings and we are able to distinguish between primary dwellings and second homes.[6] The 2011 Census reported only information on the number of unoccupied dwellings *including second homes*. To estimate vacancies for 2011 in the most consistent way possible, therefore, we assume that the share of second homes remained constant between 2001 and 2011. In a robustness check we use an alternative dataset for vacancy rates (available for 2001 and 2011 only) to test whether our findings are sensitive to this adjustment. The latter dataset is provided by the Department of Communities and Local Government (DCLG) using the LA returns for the Council Tax.[7]

Our measures of regulatory restrictiveness come from the DCLG's Planning Statistics. Following the literature, our key measure is the refusal rate for major residential projects available for each LA on an annual basis. The refusal rate for 'major' projects is defined as the share of applications for residential developments of ten or more dwellings that is refused by an LA in any year during the process of 'development control'. We calculate this for each LA using data on all applications and refused applications of major developments for the Census year itself plus the two years preceding it.[8] In what follows we call this variable the *refusal rate*.

As a proxy for local (housing) demand we use LA-level male weekly earnings for the period from 1981 to 2011. Our earnings data come from the Annual Survey of Hours and Earnings (ASHE) for 2001 and 2011 and from the New Earnings Survey (NES) for 1981 and 1991. We obtained the ASHE data at the LA-level but the NES data for earlier years are only available at the county and London borough level. We then geographically matched all earnings data to the LA-level and deflated the nominal earnings figures by the Retail Price Index to obtain real earnings. For more details on the data and procedures used, see Hilber and Vermeulen (2016).

A number of other factors may influence vacancy rates, in particular housing tenure, demographics and socio economic characteristics. We obtain these control variables from the Population Censuses. Our list of controls includes the local homeownership rate. Homeowners tend to move less often than renters, and this is likely to be reflected in higher vacancy rates for rental housing. We also control for the share of council housing. Because rents of council houses are usually below market value, there are waiting lists for them. This is likely to imply a shorter duration of vacancies (Pawson and Kintrea, 2002). However, this effect could be offset to the extent councils have less efficient housing management.

The Population Censuses also provide data on the share of people between 30 and 64 and the share of elderly, 65 and over. Young people may be more flexible in their housing choices than older people, and they may be less selective because they are more income constrained or have lower search costs (perhaps because of lower opportunity costs of time) leading to lower vacancy rates in LAs where there are proportionately more young adults. On the other hand, younger people tend to have a higher mobility rate, leading to higher vacancy rates. The mortality rate is of course highly correlated to the share of elderly. Death frequently implies that houses become vacant and, moreover, because of probate and perhaps other reasons (the new owner may not be a local resident or the house has suffered a period of neglect so is more likely to need refurbishment) houses that become vacant on the death of their owner are likely to remain vacant for longer. Other control variables derived from the Population Censuses are the share unemployed, the share of highly educated, and the share of residents with permanent illnesses.

As a proxy for mismatch and as a significant focus of interest in its own right, we gather data on the average commuting distance from the workplace for all the Census years. The data provide us with the share of people per commuting distance band (0–2 km, 2–5 km, etc.). We then calculate the average commuting distance by taking the midpoint of each category and weighting it by the number of persons in each category. We further gather data on other variables that may relate to spatial mismatch from the Census, such as the share of crowded properties, the share of shared properties, the share of migrants and the share of non-permanent dwellings.

Our instrumental variable strategy employs information on the political composition of the LA and local vote shares. We obtained the local election data from various sources: (*i*) the British Local Election Database (1889–2003) compiled by Rallings and Thrasher (2004), (*ii*)

[5] The Census does not distinguish between short-term and long-term vacant housing units. Molloy (2016) points out that in the United States, long-term vacancies are on average rare but there are substantial spatial differences. Our data do not allow us to explore differences between short-term and long-term vacancies. However, it is worth pointing out that due to the extremely inflexible planning system in England, overbuilding is highly unlikely anywhere in the country. Long-term vacancies as a consequence of weak housing demand will likely be concentrated in the north of the country and the change in the unemployment rate will likely capture the effect of declining areas in our empirical analysis.

[6] The Census uses the term 'household space', which is a space taken by one household, including that of just one person. Almost no household shares facilities like bathrooms (< 0.1%), implying that the number of (vacant) household spaces is essentially the same as the number of (vacant) dwellings. Hence, in what follows, we will refer to dwellings as household spaces.

[7] The cross-sectional correlation between the 2001 Census and the DCLG data is 0.68, indicating that there are non-trivial differences in the measurement of vacant dwellings arising from the different methodologies. As is discussed later, this hardly affects our results however.

[8] In a sensitivity analysis – see Appendix 2 – we also use additional information on the refusal rate of minor projects and show that our results are robust when we include this additional information.

the Local Election Handbooks (1999 to 2008), (*iii*) the Local Elections Archive Project (LEAP) (2006 to 2010) and (*iv*) the BBC (2009 to 2011). We do not have data on four LAs, so these are excluded from the analysis, leaving us with a sample of 350 LAs and four Census years (1981, 1991, 2001 and 2011).[9] Since it might be argued that turnout is unrepresentatively low at local elections in the sensitivity analysis, we also use data on general elections, by matching each Census year to the nearest general election year (i.e., 1983, 1992, 2001 and 2010). The LA-level share of votes for the Labour party in the general elections is derived from the British Election Studies Information System. For more information on the election data, see Appendix 1.

We also gather data on net local expenditures from the Chartered Institute for Public Finance and Accountancy (CIPFA) annual reports on finance and general estimates available for each LA. We choose spending categories that remain robust over time, such as spending on education, personal and social services (such as social care), highways, housing services, local planning and the total local net expenditures. Because these are net expenditures, they may be negative in certain instances. We express the local expenditures in £ per head of the population. We note that for education, personal and social services, and highways, the largest share of the spending is done at the county level. Although LAs have some freedom to spend extra money, we add the net spending per head at the county level to the local expenditures in these categories (otherwise most values would be zero). This also explains why the *total* local expenditures of an LA are lower than, for example, the net spending on education: the total expenditures only refer to expenditures by the LA itself. In a few instances data are missing for individual LAs (in particular for a dozen LAs in Greater London in 1981). In cases such as this we impute the missing values from the average spending in a county, implying (a small) measurement error. However, in the placebo regressions in Section 3.4 the spending is the dependent variable. As long as this measurement error is random, it does not affect the estimated coefficients.

We obtained data on house prices from the Land Registry (1995–2011) and the Council of Mortgage Lenders (CML) (1974–1995). We do so by taking account of the composition of sales in terms of housing types by adopting a mix-adjustment approach (see Wall, 1998). The real price index is obtained by again deflating the nominal series with the Retail Price Index. We then use the price index to create a measure of local price volatility; for more information see Hilber and Vermeulen (2016).

Table 1 presents the descriptive statistics. The average overall vacancy rate is about 4%. The vacancy rate in 2011 was 3.6%. This is only slightly lower than in the United States, where it was 4.5% in 2012. This might seem surprising when one takes into account the enormous excess supply of housing in the wake of the Great Recession that made housing extremely affordable in the US. In Fig. 1 we plot the cross-sectional relationship between the vacancy rate and house prices. Vacancy rates are somewhat lower in areas with high prices ($\rho = -0.246$), consistent with the opportunity cost argument discussed above. There is little response to the housing market cycle; the correlation between the change in the vacancy rate and the change in house prices is very low with $\rho = -0.069$.

We map the average local vacancy rates over the sample period in Fig. 2. There is meaningful variation in vacancy rates over space. They are generally higher in the less prosperous north. Cities like Liverpool and Bradford, which respectively relied on traditional port and port-related manufacturing or textiles, experienced decline from the 1950s.

Table 1
Descriptive statistics (repeated cross-section).

	Mean	Std. dev.	Min	Max
Vacancy rate *(in %)*	3.886	1.340	0	12.06
Refusal rate, *t-2 (in %)*	27.43	14.63	0	78.57
House price *(in £)*	141,665	91,729	43,804	1.277e + 06
Male weekly earnings *(in £)*	545.7	147.3	258.3	1793
Share owner-occupied housing *(in %)*	66.86	11.22	4.611	89.52
Share council housing *(in %)*	16.09	11.34	0.416	81.96
Share age 30–65 *(in %)*	44.87	2.783	36.42	51.62
Share age > 65 *(in %)*	16.42	3.642	6.128	31.36
Share unemployed *(in %)*	6.516	2.960	2.042	22.40
Share highly educated *(in %)*	13.74	11.39	0.244	53.57
Share permanent illness *(in %)*	3.478	1.878	0.745	12.15
Predicted employment ('Bartik instrument')[a]	57,913	47,674	9832	474,473
Share Labour seats, *t-2, (in %)*	30.87	26.86	0	99.17
Share Labour voters, local elections *(in %)*	31.49	16.76	0	76.95
Share Labour voters, general election *(in %)*	29.53	15.21	2.426	75.25
Mean commuting distance from workplace (in km)	6.853	3.465	2.141	18.66
Share crowded properties *(in %)*	1.952	1.567	0.399	14.99
Share non-permanent properties *(in %)*	57.23	59.05	0.670	586.2
Share migrants *(in %)*	10.84	3.328	0.466	36.51
Coefficient of variation house prices, *t + 3*	0.104	0.0745	0.00755	0.460
Coefficient of variation earnings, *t + 3*	0.0755	0.0400	0.00685	0.674
Room diversity	4.589	0.419	3.496	6.259
Unitary authority	0.123	0.328	0	1
Net expenditures on education (in £ per head)	436.9	278.0	17.67	1643
Net expenditures on social services (in £ per head)	169.4	147.9	17.32	741.8
Net expenditures on highways (in £ per head)	38.96	18.99	− 72.89	119.2
Net local expenditures on housing services (in £ per head)	12.81	24.02	− 3.605	210.2
Net local expenditures on planning (in £ per head)	12.15	16.16	− 29.26	308.1
Total local net expenditures (in £ per head)	373.0	554.6	15.04	3054

Notes: The number of observations is 1400, as we have 4 observations for 350 local authorities. *t-2* denotes that we include applications up to two years preceding and including the year of observation, *t + 4* denotes that we include data up to four years after and including the year of observation.
[a] Measure is based on 1971 local industry composition.

Apart from high unemployment and lower earnings there was outward migration tending to generate a more obsolete housing stock and higher housing vacancy rates. Also in areas where mining was historically important (in County Durham and Lancashire for example), vacancy rates tend to be higher. We implicitly control for these geographical differences in the industry composition by first differencing our empirical specification, thus capturing all time-invariant characteristics that vary over space. The inclusion of the first difference in the local unemployment rate as a further control should effectively control for any relevant influence of changes in industrial structure on housing vacancy rates.

Refusal rates over the last 30 years have been clearly highest in the Greater London Area and in the south of England and lowest in the north of the country (Fig. 3). The south of England has not only been economically considerably more successful than northern regions over the period, but it has (perhaps relatedly) had much tighter planning restrictiveness. This – despite strong housing demand – has constrained the growth of housing supply in southern England relative to the north.

[9] Since in our empirical analysis we first difference the Census data to account for time-invariant unobservable characteristics, we end up with $350 \times (4 - 1) = 1050$ observations. We note that some LAs have been amalgamated in 2011, reducing the total number of LAs to 326. To achieve consistency in our analysis over time we geographically match the 2011 LA information to 2001 LA boundaries with the help of official 'lookup tables'. In a robustness check we exclude those LAs that were affected by amalgamation. Results are very similar.

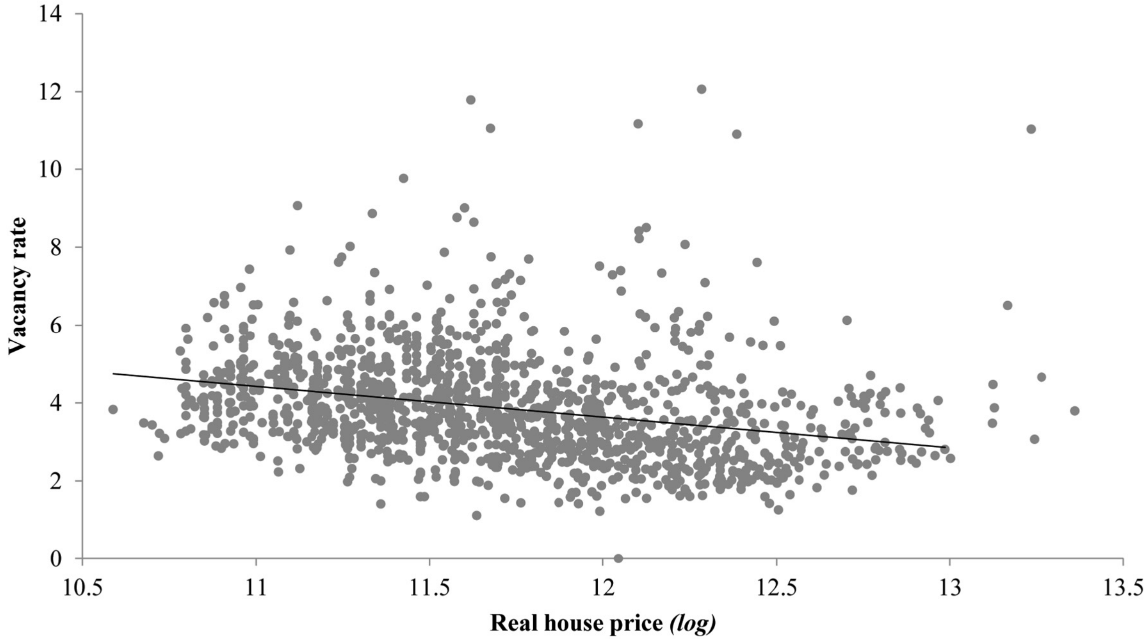


Fig. 1. Correlation between vacancy rate and real house price.

3.2. Econometric framework and identification

We aim to test the impact of housing supply restrictions (as captured by the refusal rates of major projects) on vacancy rates. Let $v_{\ell,t}$ be the vacancy rate in LA ℓ in year t. $r_{\ell,t-2}$ is the refusal rate, where the refusal rate is calculated using all applications and refused applications in years $t-2$, $t-1$ and t. We use data up to two years before and including the year of observation to avoid random yearly fluctuations and because some LAs receive no or very few applications in a particular year.[10] θ_t are year fixed effects that capture any aggregate economic shocks and also any policy changes at the national level that might affect vacancy rates. Then:

$$v_{\ell,t} = \alpha r_{\ell,t-2} + \theta_t + \epsilon_{\ell,t}, \tag{1}$$

where α is the parameter of interest and $\epsilon_{\ell,t}$ is an independently and identically distributed error term. Policy makers expect that $\alpha < 0$, implying that supply restrictions lead to a lower vacancy rate. The problem with estimating this specification using OLS is that there are potentially important endogeneity concerns with respect to $r_{\ell,t}$. First, there may be several omitted variables that have a joint impact on regulation and vacancy rates. For example, areas with more demand (higher earnings) likely have lower vacancy rates and more stringent planning (Hilber and Robert-Nicoud, 2013). Another concern is that due to durable housing, the north of England with its declining industries can be expected to have higher (long-term) vacancy rates (Molloy, 2016). It is also observed that these areas are less restrictive, so there may be spurious correlation. This may lead to a (strong) downward bias of the estimated coefficient α. A second source of bias is that if developers know that a particular LA is more restrictive and so more likely to reject applications, they will be less likely to apply in the first place because applications cost significant resources. At some limit, one might argue, the refusal rate could become completely uninformative. Developers may know how many (few) projects will be accepted in any given LA and year. If this is costly, they will be strategic in the way they play this lottery—at some extreme margin, refusal rates may be equalised in equilibrium although the payoff from success would be likely to also rise with the refusal rate. It is important to note that this point will likely be only a theoretical and not an observed equilibrium.[11] Nevertheless, these considerations imply a measurement error in the regulatory restrictiveness measure. A third concern is that vacancy rates also influence regulatory restrictiveness (reverse causality). When policy makers observe a high vacancy rate, they may become more reluctant to permit new development.

To partially address the first source of endogeneity, we estimate a first-difference equation, so that we can control for all time-invariant unobserved factors. Hence:

$$\Delta v_{\ell,t} = \alpha \Delta r_{\ell,t-2} + \theta_t + \Delta\epsilon_{\ell,t}, \tag{2}$$

where Δ denotes the change.[12]

This specification only partly addresses the first endogeneity concern because there might still be correlation with unobserved shocks. For example, in locations with increasing demand, house prices and regulatory restrictiveness may increase simultaneously. Anecdotal evidence suggests that in England regulatory restrictiveness is strongly pro-cyclical. In times of high demand, planners reject more proposals in attractive areas, perhaps to avoid what they perceive as a threatened 'oversupply' and perhaps because the system cannot cope with the workload. Because housing supply takes time to adjust, this will lead to lower local vacancy rates during boom periods. This again implies that α is likely strongly downward biased if we estimate (2) by OLS.

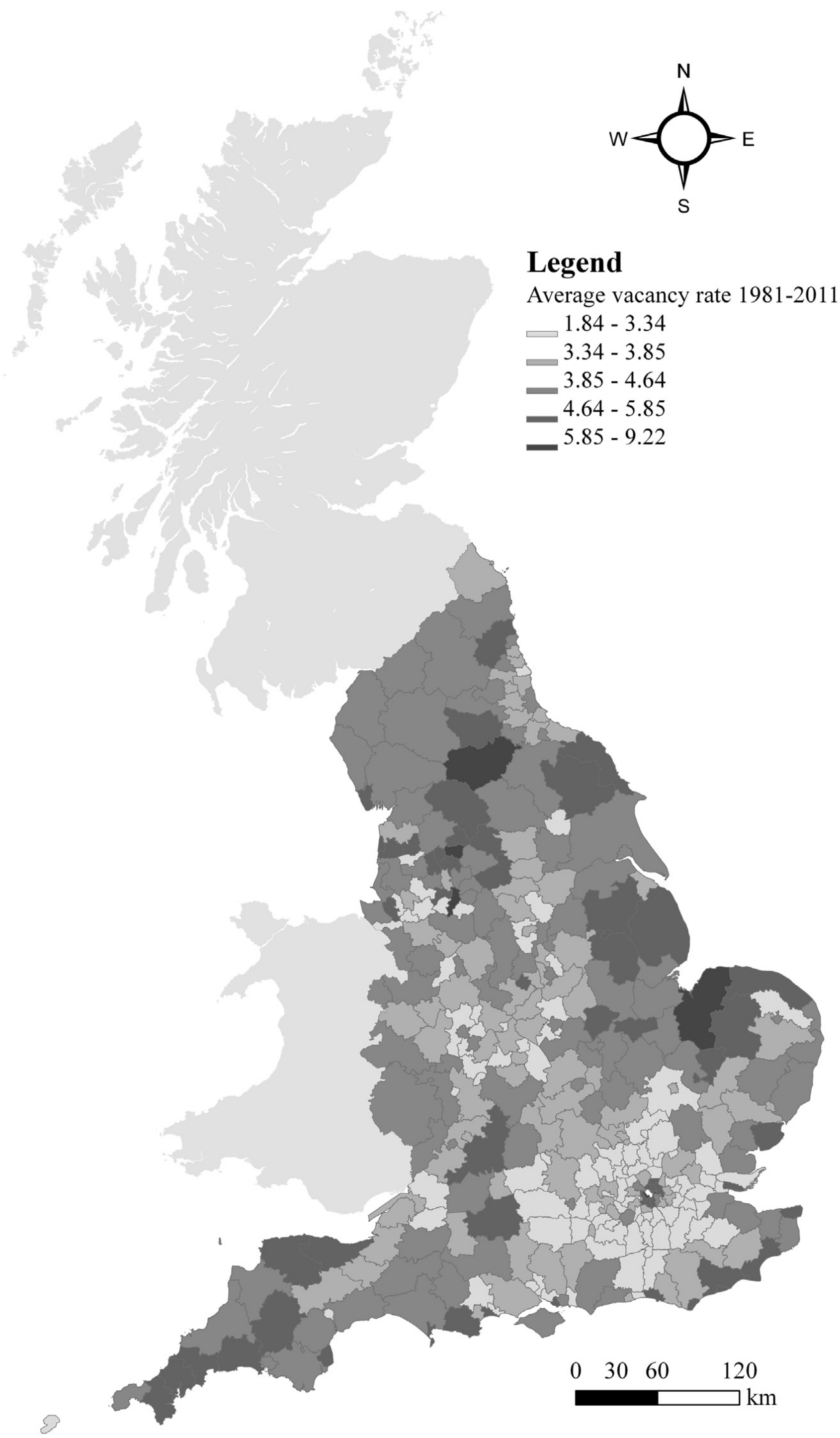
We therefore have to find an instrumental variable to identify refusal rates that is uncorrelated with local unobserved shocks. Bertrand and Kramarz (2002) exploit the cumulative representation of each political party at regional level as an instrument for how restrictive French *départments* are likely to be towards new retail entrants to document that stronger deterrence of entry by regional zoning boards increased retailer concentration and slowed down employment concentration. In a similar vein, Cheshire et al. (2015), Sadun (2015) and Hilber and Vermeulen (2016) use the share of party representation at LA-level as

[10] We experimented with leads and lags. It appears that results become weaker when moving away from year t, while regulation in $t + 1$ and $t + 2$ do not have an effect on vacancy rates. The results are available upon request.

[11] Moreover, the elasticity between log of refused applications and log of total applications is essentially equal to one. This is suggestive that developers do not participate in this kind of strategic behaviour. Nevertheless, to fully address this concern we employ an instrumental variable strategy, discussed in more detail below.

[12] One might also use a fixed effects approach. We test the robustness of our results to using a fixed effects approach in Appendix 2 and show that results are very similar.

Fig. 2. Vacancy rate across England.



an instrument to identify the impact of local regulatory restrictiveness on, respectively, retail store-level output, the entry of large retail stores, and house prices. Our study, to the best of our knowledge, is the first to *(i)* exploit the fact that the particular details of the electoral system of local government in England generates random variation in local party influence relative to vote share and *(ii)* use this exogenous variation to identify the impact of local regulatory restrictiveness on housing and Labour market outcomes.

Our specific instrument is the change in the number of seats for the Labour party between election years close to the Census years. Traditionally Labour voters and politicians have been less opposed to new residential construction than their Conservative counterparts. Labour councillors typically represent a part of the population that has less housing equity and so is less subject to NIMBY pressures aiming to protect house values. Labour councillors are also likely to be more interested in the job generating effects of construction. Thus, we can expect that an increase in the share of Labour seats may induce LAs to become less restrictive, yet, a change in the share of Labour seats should not directly affect the local vacancy rate other than through any effects it has on planning restrictiveness.

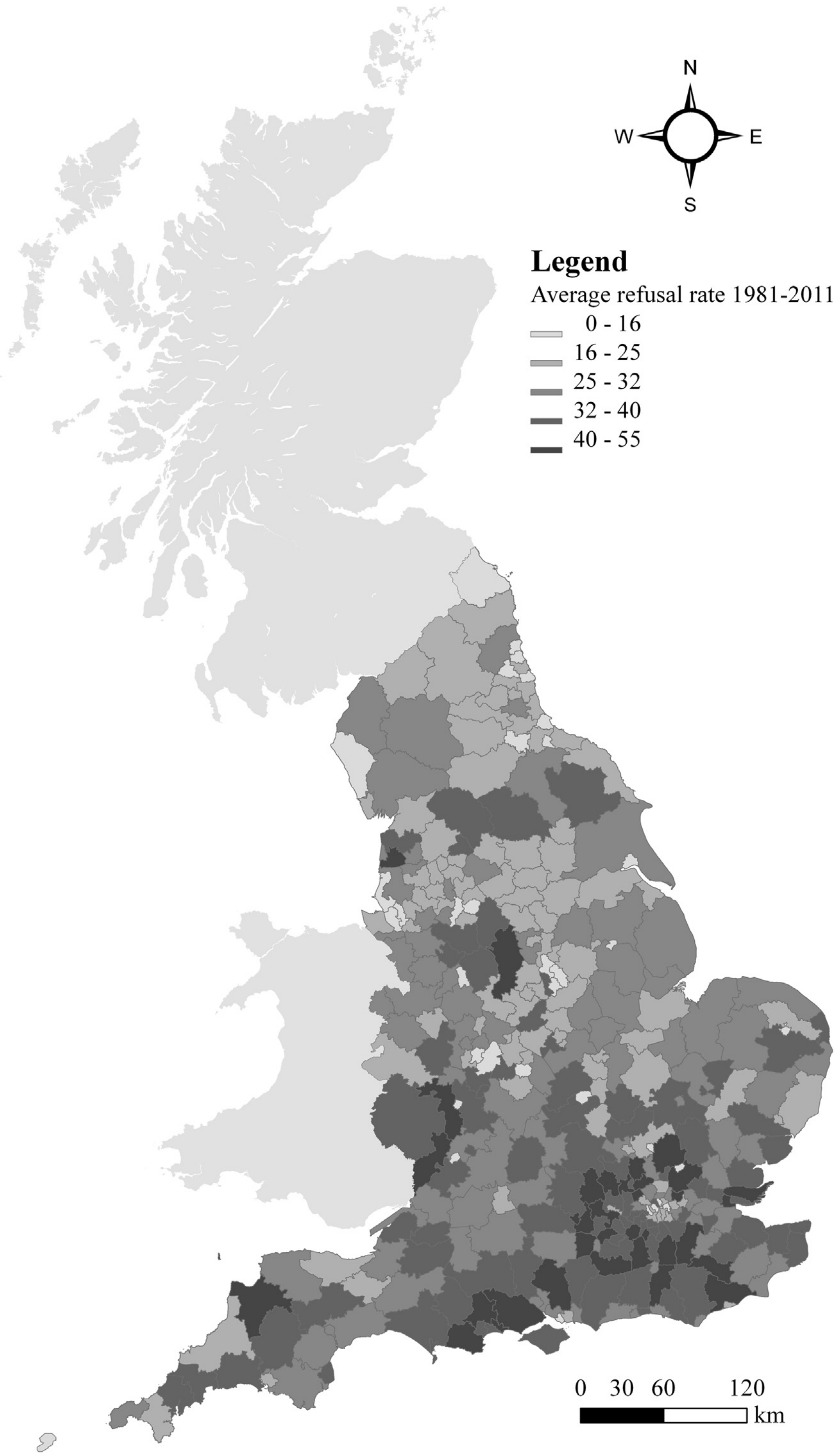


Fig. 3. Housing supply restrictions across England (refusal rate by LA).

To make the reasons for our choice of instrument clearer it may help to have a brief explanation of the English local government system and of the mechanics of the local electoral system.[13] There is a recent, succinct account available in Sandford (2016). The English system is very heterogeneous but has certain common features. It is highly centralised. As is discussed below, for most purposes, LAs are hardly more than agents of central government with legal obligations but little fiscal autonomy; property taxes, for example, are essentially national taxes (see Section 3.5) and there is no local income tax. The major area of autonomy is with respect to planning decisions. There are a total of 354 LAs of three different types: County Councils; District Councils; and Unitary Authorities. The 125 Unitary Authorities – which have the fullest range of functions – include the London and Metropolitan Boroughs.

Members of the Councils of all LAs are elected for 'wards' – geographical subdivisions of the council's area – and all elections are conducted on a 'first-past-the post' system. Some LAs have single-member wards, others multi-member wards. Voters vote for as many councillors as there are vacant seats at any date there is an election. So

[13] Since all our analysis is only for English LAs we only discuss the English system here. Systems in Scotland and other countries of the UK differ.

if, for example, all members of a three-member ward face re-election on the same date, the elector will have three votes. To complicate matters further some councils elect all their members every three years; others elect one third of their members at any given election while a few elect half their members each year, so political control can change rapidly.

Over the period of our analysis there were three main political groups: The Labour, Conservative and Liberal-Democrat parties. As with any first-past-the post system the party winning a seat contested by three parties may have a minority of votes; in wards where one party is dominant, their candidate may have only token opposition or even none at all. Equally, councils may be quite evenly split in terms of vote shares for the different parties.

Thus there are two independent reasons why the share of votes at any election and the share of seats on the council may differ. The first is just the way that the first-past-the-post voting system works when there are three parties all gaining significant vote shares but those shares are highly variable between constituencies. The second is that in many councils only one third or a half of the elected members are voted for at any election. So the composition of the council is a moving average of past votes. And, of course, the share cast for any party may change significantly over the course of even a year. The result is that the share of votes and the number of members on a council is not perfectly correlated—for the purposes of our identification strategy important: the discrepancy between the two can be considered random. The correlation between the share of Labour votes and seats, for example, is 0.77. As is explained in Appendix 1, the variable we use for 'seats' is the closest measure we can find for 'seats controlled on the council' so allows for the fact that in many councils only a third or a half of members are elected in any given election.

To illustrate the random element of seats won beyond vote shares, in Fig. 4, we provide a scatterplot of the share of Labour votes and share of Labour seats from 1978 to 2011 for each year and LA. Not surprisingly there is a strong positive correlation between seats and votes. However, below a vote share of about one third, any vote share translates into a less than proportional number of seats (denoted by the dashed line). In a number of cases, votes did not translate into any seats at all. Furthermore, because of the first-past-the-post feature of the system, above a vote share of about one third, an increase in the vote share leads to a more than proportional number of seats. If the Labour party has a vote share of > 70%, this usually implies that all seats are assigned to the Labour party.

So first, we only use the variation in the change in number of Labour seats in an LA:

$$\Delta v_{\ell,t} = \alpha \boldsymbol{\Delta r_{\ell,t-2}} + \theta_t + \Delta\epsilon_{\ell,t}, \tag{3}$$

$$\boldsymbol{\Delta r_{\ell,t-2}} = \widetilde{\alpha}\Delta s_{\ell,t-2} + \widetilde{\theta}_t + \Delta\widetilde{\epsilon}_{\ell,t}, \tag{3.1}$$

where **bold** indicates that changes in the regulatory constraints measure $\Delta \boldsymbol{r}_{\ell,\ t\ -\ 2}$ are instrumented by changes in the share Labour seats $\Delta s_{\ell t}$ and the ~ refers to first stage parameters.

One might still be concerned that our instruments are correlated with $\Delta\epsilon_{\ell,\ t}$, so the next step is to include LA fixed effects η_ℓ:

$$\Delta v_{\ell,t} = \alpha \boldsymbol{\Delta r_{\ell,t-2}} + \eta_\ell + \theta_t + \Delta\epsilon_{\ell,t}, \tag{4}$$

$$\boldsymbol{\Delta r_{\ell,t-2}} = \widetilde{\alpha}\Delta s_{\ell,t-2} + \widetilde{\eta}_\ell + \widetilde{\theta}_t + \Delta\widetilde{\epsilon}_{\ell,t}. \tag{4.1}$$

By including LA fixed effects η_ℓ, we control for all linear trends caused by unobservable factors, which increases the likelihood that changes in the instruments are uncorrelated with $\Delta\epsilon_{\ell,\ t}$.

If the instruments are valid (so uncorrelated with omitted variables and therefore the error term), adding additional control variables, should not influence the parameter of interest α, but also should not have an impact on the first-stage coefficients of the instrument. To test this, we include other, potentially endogenous, control variables, like changes in the demographic composition:

$$\Delta v_{\ell,t} = \alpha \boldsymbol{\Delta r_{\ell,t-2}} + \beta\Delta x_{\ell,t} + \eta_\ell + \theta_t + \Delta\epsilon_{\ell,t}, \tag{5}$$

$$\boldsymbol{\Delta r_{\ell,t-2}} = \widetilde{\alpha}\Delta s_{\ell,t-2} + \widetilde{\beta}\Delta x_{\ell,t} + \widetilde{\eta}_\ell + \widetilde{\theta}_t + \Delta\widetilde{\epsilon}_{\ell,t}, \tag{5.1}$$

where $\Delta x_{\ell,\ t}$ is a vector of changes in the control variables. One of our control variables is the change in log local average earnings as a proxy for local demand. One might be particularly concerned about the endogeneity of earnings and we are also interested in the impact of this variable on local vacancy rates. Thus, in a robustness check, following Hilber and Vermeulen (2016), we instrument for this variable, also, using a measure that captures local demand shocks (a Bartik, 1991-type instrument). We do not include local house prices as a control since, as we discuss in Section 2, we would expect that regulatory restrictiveness influences vacancy rates in part through house prices. Moreover, house prices and vacancy rates are jointly determined by restrictions.

The main objection to the validity of the change in the share Labour

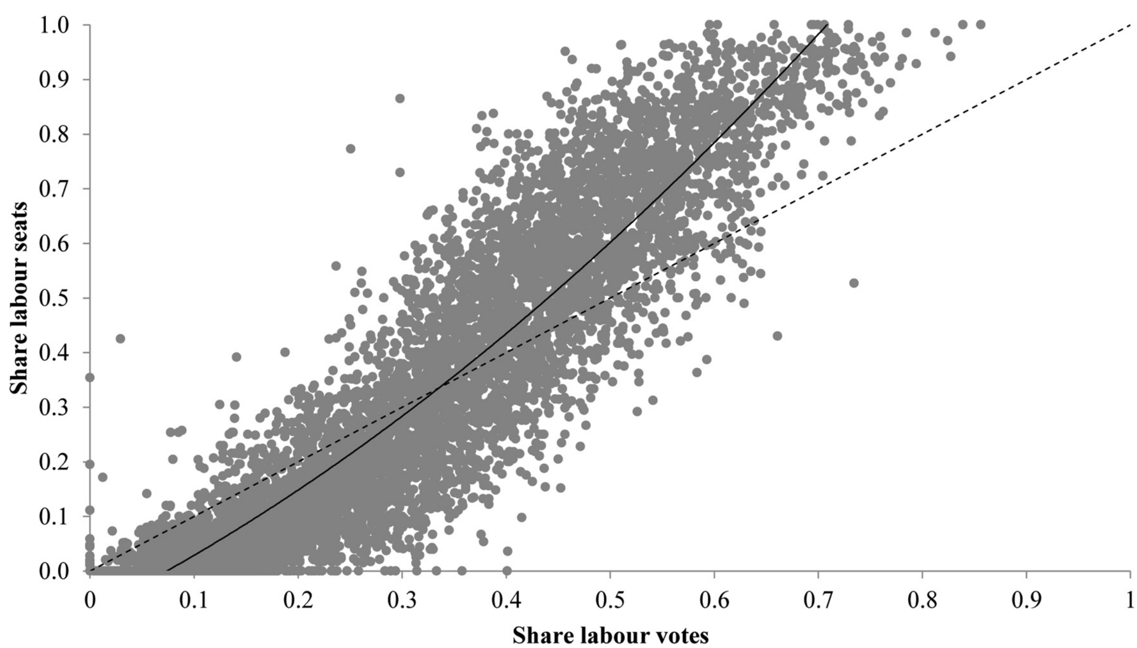


Fig. 4. Correlation between Labour votes and Labour seats.

seats-instrument is that it may be correlated with (potentially non-linear) unobserved trends. For example, some local housing markets in the Greater London Area have experienced a substantial inflow of wealthy residents during the last two decades, leading to changes in the demographic composition of the local market and therefore also to changes in voting behaviour. We thus control for a flexible function of local vote shares of the previous local election, identifying regulatory restrictiveness from the random component generated by the particular features of the English local government and electoral systems discussed above which ensure the seats allocated to parties are very seldom proportional to the number of votes. So what we effectively use to identify regulatory restrictiveness is *the number of seats that Labour won (or lost) beyond their vote share*. While Labour's local vote share may be correlated with various demographic and socio-economic characteristics of the constituency, holding local vote shares constant, seats won (or lost) above and beyond should be uncorrelated with the error term. We can express our final estimating (base) equation as:

$$\Delta v_{\ell,t} = \alpha \boldsymbol{\Delta r_{\ell,t-2}} + \beta \Delta x_{\ell,t} + \Omega(\Delta \pi_{\ell,t}) + \eta_{\ell} + \theta_t + \Delta \epsilon_{\ell,t}, \tag{6}$$

$$\boldsymbol{\Delta r_{\ell,t-2}} = \widetilde{\alpha} \Delta s_{\ell,t-2} + \widetilde{\beta} \Delta x_{\ell,t} + \widetilde{\Omega}(\Delta \pi_{\ell,t}) + \widetilde{\eta}_{\ell} + \widetilde{\theta}_t + \Delta \widetilde{\epsilon}_{\ell,t}, \tag{6.1}$$

where $\pi_{\ell,\,t}$ is the share of Labour votes in the closest previous local elections, and

$$\Omega(\Delta \pi_{\ell,t}) = \sum_{n=1}^{N} \gamma_n \Delta(\pi_{\ell,t}^n) \quad \text{and}$$
$$\widetilde{\Omega}(\Delta \pi_{\ell,t}) = \sum_{n=1}^{N} \widetilde{\gamma}_n \Delta(\pi_{\ell,t}^n). \tag{6.2}$$

Hence, $\Omega(\cdot)$ and $\widetilde{\Omega}(\bullet)$ are N^{th} order polynomials of local vote shares $\pi_{\ell,\,t}$ and γ_n and $\widetilde{\gamma}_n$ are parameters to be estimated.

Despite the fact that we identify changes in regulatory restrictiveness from the random component generated by the particular features of the English local government system, one might still be concerned that greater Labour representation does not only affect regulatory restrictiveness but may also affect other local variables that may separately affect local vacancy rates. That is, the exclusion restriction may be violated. To address this crucial concern we first argue and provide evidence to support the claim that — unlike in countries with decentralised government structures — LAs in England, especially since 1972, have very little fiscal discretion or power other than making planning decisions.[14] Next, we show that even for those LAs — Unitary Authorities — that provide more local services than others, the effect of a random increase in the local Labour representation has a very similar effect on local restrictiveness. This suggests that the relation between the share of Labour seats and local restrictiveness may not be significantly biased by other local policies and services that may be correlated with both regulatory restrictiveness and local vacancy rates. Most reassuringly, when we run a battery of placebo (first-stage) specifications, in which we replace the change in local refusal rates with changes in local expenditures — our placebo variables — we find no significant relationship between share Labour seats and these placebo measures (see Section 3.5). This is in contrast to a strong and statistically significant negative relationship between the random change in the share of Labour seats and local refusal rates. Overall, these results provide a strong indication that the exclusion restriction is not violated and our identification strategy is valid.

3.3. Results for housing vacancies

We start by ignoring any potential endogeneity issues and simply regress the vacancy rate on the refusal rate of major residential projects (Eq. (1)). From Fig. 5 we can see that the cross-sectional relationship between the major refusal and the vacancy rates is negative. The regression line implies that a one standard deviation increase in refusal rates is associated with a 0.23 percentage point decrease in the vacancy rate (s.e. 0.040). This naïve correlation provides 'common sense' evidence supporting the view that vacant houses can be 'regulated away'. However, the quantitative impact is not very large.

Table 2 reports estimates for Eqs. (2) to (6). In the cases of Eqs. (3) to (6) these are the second stage results of our IV-estimates. In column (1) we regress *the change* in the vacancy rate on *the change* in the refusal rate still ignoring potential endogeneity issues (Eq. 2). We first difference controls to offset for any time invariant omitted characteristics such as differences in income levels across LAs. We see that even without instrumenting for the refusal rate or adding control variables, the relationship between (the change in) planning restrictiveness and (the change in) the vacancy rate is no longer negative and statistically significant.

However, because of the endogeneity concerns discussed above, the coefficient on the refusal rate cannot be interpreted as a causal effect. So in column (2) we include LA fixed effects. The coefficient on the change in the refusal rate variable now becomes positive and statistically significant at the 10% level. In column (3) of Table 2 we add further controls as discussed in Section 3.1 above. The estimated coefficient for the change in the refusal rate is hardly affected, although it is not statistically significant at conventional levels anymore. The control variables often have a statistically significant impact on the change in the vacancy rate with the anticipated sign. For example, areas with an increasing share of elderly people or of council housing experience an increase in the vacancy rate. Also, areas with an increasing unemployment rate, from which people may have been tending to move away, experience an increase in the vacancy rate. In areas with a rising share of highly educated people, vacancies tend to decrease.

Still, however, regulatory restrictiveness is likely measured with error (because developers may not apply in the first place in more restrictive places). It may also be correlated with unobserved shocks. Moreover, we should address the potential reverse causality issue that higher vacancy rates may induce policy makers to be more restrictive. We therefore instrument for the change in the refusal rate with the change in the share of Labour seats in column (4). This specification corresponds to Eq. (3) above.

Kleibergen-Paap *F*-statistics indicate that there are no issues of weak identification of regulatory restrictiveness. The results suggest that a one standard deviation increase in the refusal rate leads to an increase in the vacancy rate of 0.82 percentage points. As noted in the previous subsection, one objection to the instrument is that it may be correlated with unobserved characteristics of the area. To control for this, we include LA fixed effects in column (5) – corresponding to Eq. (4). The coefficient on the refusal rate hardly changes and remains statistically significant at the 5% level. Column (6), corresponding to Eq. (5), includes the same range of control variables as in column (3). This makes almost no difference to the estimated coefficient of primary interest.

One might still be worried that changes in the share of Labour seats are correlated with unobservable shocks (e.g. gentrification) that simultaneously have an impact on voting behaviour and vacancy rates. So in column (7) we estimate our final model (6). That is, we additionally include a flexible function of changes in the share of Labour *votes* in local elections, approximated by a fifth-order polynomial to isolate the impact of voting behaviour caused by any change in the demographic and socio-economic composition of the LA from political power (measured by *seats*). In the sensitivity checks, discussed below, we report results for different orders of polynomials. Reassuringly, the estimated effect of regulatory restrictiveness in column (7) is very similar to the previous specifications. The instrument is somewhat less strong (with a Kleibergen-Paap *F*-statistic of 8.2). Still, we find a positive and economically meaningful effect of regulatory restrictiveness on the vacancy rate: a one standard deviation increase in the refusal rate increases the vacancy rate by 0.90 percentage points. Due to the

[14] Perhaps surprisingly, Ferreira and Gyourko (2009) find that in the US – where municipalities may be thought to have greater local discretion – whether the city mayor is a Democrat or a Republican makes little difference to a range of outcomes at the city level, including total expenditure or its allocation.

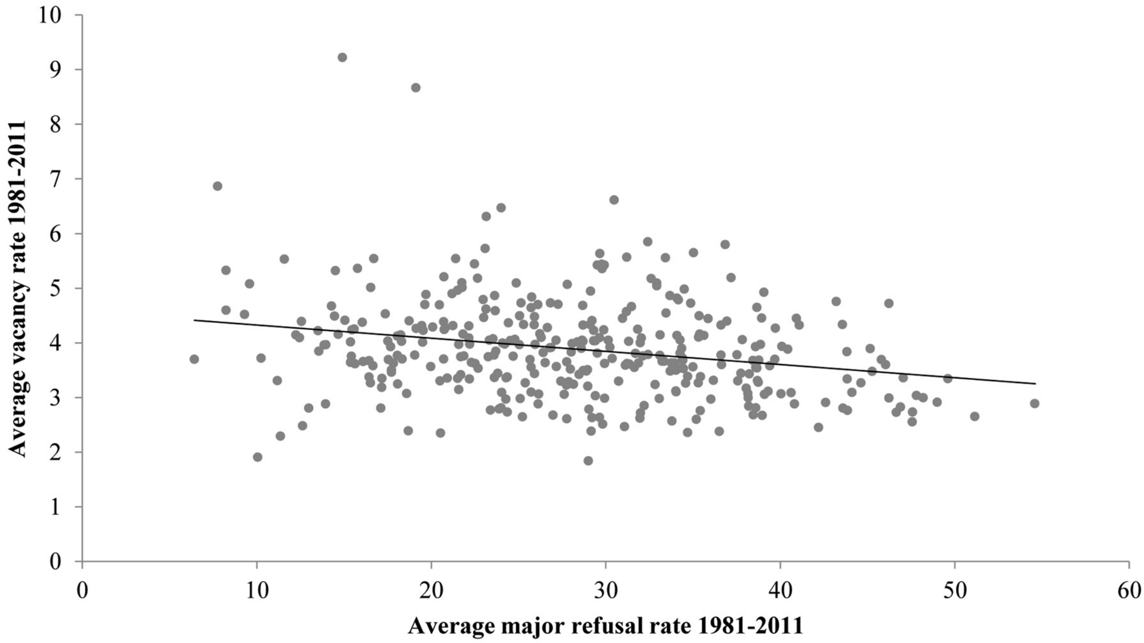


Fig. 5. Cross-sectional relationship between refusal rate and vacancy rate.

Table 2
Baseline results – second stage. (Dependent variable: Δ vacancy rate).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	OLS	OLS	2SLS	2SLS	2SLS	2SLS
Δ Refusal rate, *t-2*	0.0434	0.101*	0.0801+	**0.820****	**0.855****	**0.840****	**0.895***
	(0.0382)	(0.0532)	(0.0501)	**(0.417)**	**(0.405)**	**(0.354)**	**(0.530)**
Δ Earnings *(log)*			0.373			− 0.0701	− 0.125
			(0.677)			(0.692)	(0.719)
Δ Share owner-occupied housing			− 0.129			− 0.262	− 0.321
			(0.356)			(0.369)	(0.418)
Δ Share council housing			0.736***			0.655***	0.631***
			(0.235)			(0.219)	(0.233)
Δ Share age 30–65			− 0.0989			0.0550	0.0754
			(0.208)			(0.202)	(0.211)
Δ Share age > 65			1.281***			1.432***	1.362***
			(0.317)			(0.311)	(0.319)
Δ Share unemployed			0.368***			0.270**	0.276**
			(0.138)			(0.114)	(0.136)
Δ Share highly educated			− 0.493**			− 0.581***	− 0.617***
			(0.191)			(0.189)	(0.214)
Δ Share permanent illness			0.192**			0.151	0.123
			(0.0972)			(0.0934)	(0.0985)
Year fixed effects (3)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Local authority fixed effects (350)	No	Yes	Yes	No	Yes	Yes	Yes
Δ Share Labour voters local elections Ω(·)	No	No	No	No	No	No	Yes
Observations	1050	1050	1050	1050	1050	1050	1050
R-squared	0.373	0.488	0.559				
Kleibergen-Paap *F*-statistic				18.52	17.49	19.01	8.151

Notes: All independent variables (except for earnings) are standardised with mean zero and unit standard deviation. **Bold** indicates instrumented. In Models (4)–(7), the instrument for Δ Refusal rate is Δ Share of Labour seats in the LA. Ω(·) is approximated by a fifth-order polynomial of share Labour voters in local elections. Standard errors in parentheses are clustered at the LA level.

+ $p < 0.15$.
* $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$.

Table 3
Baseline results – first stage. (Dependent variable: Δ refusal rate, t-2).

	(1)	(2)	(3)	(4)
	OLS	OLS	OLS	OLS
Δ Share Labour seats, *t-2*	− 0.301***	− 0.322***	− 0.343***	− 0.264**
	(0.0700)	(0.0943)	(0.0966)	(0.114)
Δ Earnings *(log)*			0.579	0.504
			(0.461)	(0.468)
Δ Share owner-occupied housing			0.250	0.318
			(0.327)	(0.323)
Δ Share council housing			0.0880	0.113
			(0.186)	(0.188)
Δ Share age 30–65			− 0.194	− 0.184
			(0.150)	(0.151)
Δ Share age > 65			− 0.160	− 0.148
			(0.220)	(0.218)
Δ Share unemployed			0.112	0.134
			(0.0880)	(0.0874)
Δ Share highly educated			0.167	0.189
			(0.171)	(0.175)
Δ Share permanent illness			0.0828	0.0673
			(0.0826)	(0.0865)
Year fixed effects (3)	Yes	Yes	Yes	Yes
Local authority fixed effects (350)	No	Yes	Yes	Yes
Δ Share Labour voters local elections $\widetilde{\Omega}(\bullet)$	No	No	No	Yes
Observations	1050	1050	1050	1050
R-squared	0.168	0.310	0.324	0.331

Notes: All independent variables (except for earnings) are standardised with mean zero and unit standard deviation. $\widetilde{\Omega}(\bullet)$ is approximated by a fifth-order polynomial of share Labour voters in local elections. Standard errors in parentheses are clustered at the LA level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$, $^{+}$ $p < 0.15$.

correlation between changes in the Labour vote shares and changes in the share of Labour seats, it is no surprise that the coefficient is now only statistically significant at the 10% level.[15]

In Table 3 we report the corresponding first-stage estimates: a standard deviation increase in the share of Labour seats leads to a decrease in the refusal rate of 0.26–0.34 standard deviations. It is notable that the first-stage coefficients of the change in the share of Labour seats instrument are highly statistically significant and are hardly affected by the inclusion of LA fixed effects and other control variables. If we include vote share controls, the coefficient on change in Labour seats becomes slightly lower, but it is still statistically significant at the 5% level.

3.4. Results for commuting distance

In Section 2 we hypothesised that a positive relationship between restrictions and vacancy rates might be explained by increased mismatch. There are few obvious measures of mismatch but for reasons discussed earlier we think that the 'average commuting distance from the workplace' does not only provide a useful measure but can potentially illuminate in a useful way the underlying interrelationship between spatial housing and Labour markets. Moreover longer commutes unambiguously signal a welfare loss. One of the most important characteristics of a house is its location with respect to jobs. It seems reasonable therefore that the average commuting distance from the workplace should capture mismatch in this dimension of housing characteristics for any given housing market. In principle, households have a preference to live close to their workplaces. If regulatory restrictions make it more difficult for people to find a home 'matched' to their preferences on other characteristics close to work, their search takes longer and will extend further. This adaptation of search behaviour implies, other things equal, vacancies will tend to be higher in the more restrictive LAs and lower in neighbouring, less restrictive ones, as workers become more locationally mismatched.[16] We provide evidence that regulation also has an impact on other proxies for mismatch in Section 3.6.

Table 4 replicates Table 2 except that the log of average commuting distance replaces the vacancy rate as the dependent variable. The results very closely parallel those for vacancies. Those in the first column suggest that commuting distance is not influenced by regulatory restrictiveness in the LA of the workplace. When we include LA fixed effects and demographic control variables in columns (2) and (3), the results are still statistically insignificant. This is not too surprising as the refusal rate is highly endogenous and correlated with other factors that might explain commuting distances. For example, places that have become denser might have tended to become more restrictive (Hilber and Robert-Nicoud, 2013), but denser places also might have shorter commutes because jobs and households are located closer to each other.

In column (4) we therefore control for other factors that might be correlated with the refusal rate by instrumenting for the change in the refusal rate with the change in the share of Labour seats, as in Table 2. This reveals a positive and significant effect. As restrictiveness in the LA in which a worker is employed increases so does the average commuting distance: a one standard deviation increase in the refusal rate in the workplace LA increases the commuting distance of its employees by 8.5%, a non-negligible effect. The effect becomes somewhat smaller (5.8%) when we include in column (5) LA fixed effects. The effect continues to be essentially the same when we add further control variables in column (6) and a flexible function of the share of Labour votes in column (7), with 5.9 and 6.1% respectively. In the last column the effect is somewhat imprecisely estimated and only statistically significant at the 14% level.[17] On the other hand, the results are consistent in pointing towards a meaningful effect of regulatory restrictiveness on commuting distance, and therefore increasing the spatial mismatch between home and work locations.

3.5. Evidence in support of the identification strategy

The central assumption of our identification strategy is that a random increase in the local representation of the Labour party only influences local regulatory restrictiveness and does not separately affect other local decisions that may themselves be correlated with local vacancy rates. If this assumption does not hold, then the exclusion restriction is violated.

In this context it is important to first re-emphasise that LAs in England have considerable discretion over local planning decisions. However, unlike, for example, in the US, they have almost no local fiscal resources and very limited ability to determine local public service levels since these are largely set by central government. To a large extent local jurisdictions – LAs – are simply agencies of central government in charge of delivering services locally to regulated national criteria using a dedicated budget stream for each purpose (such as education, social services, local roads and street cleaning or refuse collection). These direct grants of revenue from central government have over the past 75 years or so accounted for some 80% of LA

[15] The correlation between the share Labour votes and the share Labour seats for the Census years is 0.88 (note that it is 0.77 when we take all available years into account, see Figure 4). However, the correlation between the change in share Labour votes and the change in share Labour seats is much lower (ρ = 0.481).

[16] The impact of greater restrictiveness in a given LA on vacancies in the aggregate is beyond the scope of this paper. The elasticity of substitution between housing characteristics (including location with respect to job) is unknown; nor is the extent to which people may adopt alternative strategies to accepting longer commutes such as living with parents, friends or taking temporary accommodation.

[17] One may argue that earnings are endogenous, leading to biased results. When we instrument for earnings with a Bartik (1991)-type Labour demand shock variable, the results are very similar. The point estimates related to the refusal rate are around 7%, while the standard errors are even somewhat lower.

Table 4
Baseline results – second stage. (Dependent variable: Δ commuting distance from workplace (log)).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	OLS	OLS	2SLS	2SLS	2SLS	2SLS
Δ Refusal rate, *t-2*	− 0.00274	0.00341	0.00106	**0.0847*****	**0.0584****	**0.0594****	**0.0608**$^{+}$
	(0.00283)	(0.00400)	(0.00384)	**(0.0321)**	**(0.0296)**	**(0.0274)**	**(0.0407)**
Δ Earnings *(log)*			0.179***			0.145***	0.139**
			(0.0567)			(0.0548)	(0.0548)
Δ Share owner-occupied housing			0.0551*			0.0449^{+}	0.0420
			(0.0286)			(0.0296)	(0.0328)
Δ Share council housing			− 0.00297			− 0.00916	− 0.0100
			(0.0177)			(0.0158)	(0.0163)
Δ Share age 30–65			− 0.0216^{+}			− 0.00982	− 0.00803
			(0.0131)			(0.0145)	(0.0153)
Δ Share age > 65			0.0168			0.0285	0.0225
			(0.0319)			(0.0312)	(0.0319)
Δ Share unemployed			0.0181**			0.0106	0.0120
			(0.00755)			(0.00781)	(0.00927)
Δ Share highly educated			0.00129			− 0.00544	− 0.00902
			(0.0123)			(0.0122)	(0.0139)
Δ Share permanent illness			− 0.0245***			− 0.0277***	− 0.0306***
			(0.00915)			(0.00882)	(0.00912)
Year fixed effects (3)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Local authority fixed effects (350)	No	Yes	Yes	No	Yes	Yes	Yes
Δ Share Labour voters local elections Ω(·)	No	No	No	No	No	No	Yes
Observations	1050	1050	1050	1050	1050	1050	1050
R-squared	0.373	0.913	0.559				
Kleibergen-Paap *F*-statistic				18.52	17.49	19.01	8.151

Notes: All independent variables (except for earnings) are standardised with mean zero and unit standard deviation. **Bold** indicates instrumented. In Models (4)–(7), the instrument for Δ Refusal rate is Δ Share of Labour seats in the LA. Ω(·) is approximated by a fifth-order polynomial of share Labour voters in local elections. Standard errors in parentheses are clustered at the LA level.
$^{+}$ $p < 0.15$.
* $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$.

spending. Revenues from taxes on residential property are subject to revenue equalisation across LAs so that local resources are 'needs' based and revenues to LAs become, with only a short delay, independent of the local property tax base: commercial property taxes since 1990 have been a national tax.

Over the first half of the 20th Century LAs did gain an increasing role in the provision of social housing – 'council housing'. This was not initially designed to be 'safety net' housing for the very poor and deprived but rather public housing for those not wanting or not able to become owner occupiers. At the start of the 20th Century owner occupation as a tenure accounted for only about 10% of all housing. Council houses were built to common nationally set design guidelines but delivery was under local control as was the setting of rents (subject to the wider financial rules governing LAs' budgets). This housing role of local government peaked in the 1950s and 1960s and declined thereafter. By the late 1980s the construction of LA (council) housing had almost stopped and local powers to set rents had been all but abolished. The two key changes were the Housing Finance Act of 1972, which required LAs to set nationally defined 'fair' rents, and the introduction of the Right to Buy for LA tenants, introduced in 1980.[18]

Tables 5 and 6 provide empirical evidence to support the above account of how limited the powers of LAs in England are except for their powers over development. In Table 5 we test whether the provision of public services is affected by the political composition of an LA more directly by looking at expenditures on services, such as education, personal services (e.g. social care), highways, (social) housing and planning. We then run a series of placebo tests by replicating the preferred first-stage results in column (7) of Table 3, but using different dependent variables. Instead of the change in the refusal rate we use the log change in the following variables: education expenses, social services expenses, highway expenses, local housing expenses, local planning expenses, and total local expenses.[19] Our identification strategy stipulates that a random change in the share of Labour seats should only significantly reduce the refusal rate but should not affect the expenditures on various public services. Indeed, that is what Table 5 reveals.

The coefficients for the change in the share of Labour seats on expenses are in all cases highly statistically insignificant. The only exception is a weakly significant effect on the total local expenditures (*p*-value = 0.15). The coefficient seems to suggest that a one a standard deviation increase in the share of Labour seats increases local spending by 8.75%. Although the effect is not statistically particularly strong, one may be worried that the share of Labour seats has a direct impact on vacancy rates via the total expenditures of local planning authorities, which would call into question the exclusion restriction.

In Appendix 2 (Table A2.1) we therefore re-estimate the preferred specification in Table 2 (column (7)), but include expenditure by category, one by one, as additional controls (columns (1) to (5)). In column (6), Table A2.1, we instead include total local expenditures as an additional control. The results show that the impact of the refusal rate on vacancy rates is essentially unaffected, even when we control for total local expenditures or for each category of expenditure individually. Planning expenses seem to have some direct negative impact on vacancy rates (column (5)): more planning expenses lead to lower

[18] The Housing Finance Act led to the famous Clay Cross dispute in which local councillors in Clay Cross in Derbyshire were eventually jailed for refusing to set 'fair rents'. In just seven years following the introduction of the Right to Buy over 1 million council houses were sold off. Thus while some council houses still exist, local control of them was effectively abolished from 1972.

[19] Because we cannot take the log of a negative number, we exclude observations with negative expenditures. We also have estimated the same regressions using the expenditure variables in levels, leading to qualitatively the same conclusions.

Table 5
Placebo tests for expenditures.

Dependent variable:	Δ Education expenditures (log)	Δ Social services expenditures (log)	Δ Highway expenditures (log)	Δ Housing expenditures (log)	Δ Planning expenditures (log)	Δ Total local expenditures (log)
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	OLS	OLS	OLS
Δ Share Labour seats, *t-2*	0.0231	−0.0107	0.0224	0.126	0.00629	0.0875^{+}
	(0.0550)	(0.0165)	(0.0321)	(0.110)	(0.0871)	(0.0559)
Year fixed effects (3)	Yes	Yes	Yes	Yes	Yes	Yes
Local authority fixed effects (350)	Yes	Yes	Yes	Yes	Yes	Yes
Control variables (8)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Share Labour voters local elections Ω(·)	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1050	1050	1047	1014	1024	1050
R-squared	0.082	0.583	0.529	0.661	0.431	0.326

Notes: Δ Share Labour seats, *t-2* is standardised with mean zero and unit standard deviation. Ω(·) is approximated by a fifth-order polynomial of share Labour voters in local elections. Standard errors in parentheses are clustered at the LA level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$, $^{+}$ $p < 0.15$.

Table 6
Results for Unitary Authorities and other LAs.

Panel A – (Dependent variable: Δ refusal rate, t-2)	(1)	(2)	(3)	(4)
	OLS	OLS	OLS	OLS
Δ Share Labour seats, *t-2* × other local authority	−0.326***	−0.352***	−0.365***	−0.291**
	(0.0743)	(0.0998)	(0.102)	(0.120)
Δ Share Labour seats, *t-2* × Unitary authority	−0.144	−0.121	−0.187	−0.153
	(0.162)	(0.210)	(0.205)	(0.215)
Year fixed effects (3)	Yes	Yes	Yes	Yes
Local authority FEs (350)	No	Yes	Yes	Yes
Control variables included (8)	No	No	Yes	Yes
Δ Share Labour voters local elections $\widetilde{\Omega}(\bullet)$	No	No	No	Yes
Observations	1050	1050	1050	1050
R-squared	0.169	0.311	0.325	0.326

Panel B – (Dependent variable: Δ vacancy rate)	(1)	(2)	(3)	(4)
	2SLS	2SLS	2SLS	2SLS
Δ Refusal rate, *t-2*	**0.915****	**0.906****	**0.855****	**0.935***
	(0.423)	**(0.399)**	**(0.359)**	**(0.541)**
Δ Share Labour seats, *t-2* × Unitary authority	0.209	0.126	0.0421	0.0786
	(0.207)	(0.216)	(0.196)	(0.212)
Year fixed effects (3)	Yes	Yes	Yes	Yes
Local authority FEs (350)	No	Yes	Yes	Yes
Control variables included (8)	No	No	Yes	Yes
Δ Share Labour voters local elections $\widetilde{\Omega}(\bullet)$	No	No	No	Yes
Observations	1050	1050	1050	1050
Kleibergen-Paap *F*-statistic	19.31	18.65	19.25	8.354

Panel C – (Dependent variable: Δ commuting distance from workplace (log))	(1)	(2)	(3)	(4)
	2SLS	2SLS	2SLS	2SLS
Δ Refusal rate, *t-2*	**0.102*****	**0.0735****	**0.0664****	**0.0720***
	(0.0342)	**(0.0307)**	**(0.0284)**	**(0.0432)**
Δ Share Labour seats, *t-2* × Unitary authority	0.0371**	0.0377***	0.0195	0.0217^{+}
	(0.0164)	(0.0139)	(0.0136)	(0.0141)
Year fixed effects (3)	Yes	Yes	Yes	Yes
Local authority FEs (350)	No	Yes	Yes	Yes
Control variables included (8)	No	No	Yes	Yes
Δ Share Labour voters local elections $\widetilde{\Omega}(\bullet)$	No	No	No	Yes
Observations	1050	1050	1050	1050
Kleibergen-Paap *F*-statistic	19.31	18.65	19.25	8.354

Notes: **Bold** indicates instrumented. The instrument is Δ Share Labour seats, *t-2* × Other Local authority. All independent variables (except for earnings) are standardised with mean zero and unit standard deviation. Ω (·) is approximated by a fifth-order polynomial of share Labour voters in local elections. Standard errors in parentheses are clustered at the LA level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$, $^{+}$ $p < 0.15$.

Table 7
Alternative proxies for mismatch.

Dependent variable:	Δ Dwellings		Δ Share crowded properties		Δ Share non-permanent properties		Δ Share migrants	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Δ Refusal rate, *t-2*	**− 1829****	**− 1821***	**0.505***	**0.576***	**0.181 +**	**0.213 +**	**− 1.139**	**− 1.535***
	(931.7)	**(988.4)**	**(0.284)**	**(0.340)**	**(0.114)**	**(0.132)**	**(0.796)**	**(0.923)**
Δ House price *(log)*		− 2136*		0.546 +		0.249*		− 3.050***
		(1102)		(0.370)		(0.130)		(1.128)
Year fixed effects (3)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Local authority fixed effects (350)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables (8)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Δ Share Labour voters local elections Ω(·)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Δ Dwellings Ψ(·)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1050	1050	1050	1050	1050	1050	1050	1050
Kleibergen-Paap *F*-statistic	8.053	6.708	8.151	6.438	8.151	6.438	8.151	6.438

Notes: The Δ Refusal rate is standardised with mean zero and unit standard deviation. **Bold** indicates instrumented. The instrument for Δ Refusal rate is Δ Share of seats in the LA. Ω(·) is approximated by a fifth-order polynomial of share Labour voters in local elections. Ψ(·) is approximated by a linear term in column (1) and by a fifth-order polynomial of the number of dwellings in column (2). Standard errors in parentheses are clustered at the LA level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$, $^{+}$ $p < 0.15$.

vacancy rates. In column (6) we show that, even if the share of Labour seats might have a positive effect on total local expenditures, it seems that local expenditures do not have a direct impact on vacancy rates. Hence, this provides additional evidence that the exclusion restriction holds. We repeat this exercise in Table A2.2, Appendix 2 with commuting distance as dependent variable and show that the estimated coefficients are also very similar to the baseline specification.

Table 6 provides further evidence supporting this narrative. First we replicate our baseline results but we allow for the coefficient on the share Labour seats to vary between Unitary LAs and all other LAs. Unitary Authorities have a wider remit in terms of the types of services they can provide. We therefore use as an instrument the change in the share Labour seats interacted with a dummy indicating whether an LA is not a Unitary Authority and control directly for the change in share Labour seats interacted with a dummy indicating whether an LA is a Unitary Authority. The first-stage results, reported in Panel A of Table 6, reveal that the coefficient on the share Labour seats is similar for the two types of LAs: the coefficient for Unitary Authorities is somewhat lower but not statistically significantly different from the coefficient for other LAs. This also suggests that the local share of Labour seats does not significantly affect the nature and quality of local service provision (which in turn might be correlated with regulatory restrictiveness and local vacancy rates). Indeed, Panel B of Table 6 shows that the baseline results for vacancy rates are essentially unaffected. Finally if we replace the vacancy rate by the log commuting distance in Panel C of Table 6, the results are again similar. This is indicative that LAs with a greater remit of service provision are not fundamentally different from those that have a more limited remit—presumably this is because even Unitary Authorities have very little discretion over services other than planning decisions.

3.6. Other evidence for the importance of mismatch

As noted in Section 2 we also investigate the impact of local restrictiveness on other symptoms or measures potentially capturing mismatch, such as the rate of new construction, the share of properties which are crowded, the share of non-permanent properties and migrants. The results are shown in Table 7. We replicate the preferred specification where we instrument for the refusal rate and include fixed effects and control variables (as in column (7) in Tables 2 and 4).[20]

We first investigate a symptom of restrictiveness which would be expected to induce greater mismatch: whether in more restrictive areas, despite the effect on prices, it is more difficult to build additional houses. Because the refusal rate should have an impact on the absolute number of dwellings, we regress the change of the number of dwellings (rather than logs) in an LA on the refusal rate, while additionally controlling for the number of dwellings (in levels).[21] Column (1) of Table 7 shows that on average over a ten year period, a one standard deviation increase in regulatory restrictiveness in an LA reduces the number of additional dwellings by more than half a standard deviation of the growth in the number of dwellings over this time period (about 1800 dwellings per LA). To make sure that this effect is not entirely explained by larger LAs we include a fifth-order polynomial of the number of dwellings and control for house prices in column (2). In line with expectations, a higher price is associated with a slower growth in the number of dwellings, most likely because higher prices are predominantly found in already developed areas with fewer possibilities to extend the building stock (see Hilber and Vermeulen, 2016). More relevantly for present purposes, the effect of regulation on new dwelling supply is essentially unaffected once we control for prices.

Another symptom of mismatch is the share of officially classified ‘crowded’ properties – properties with more than one person per room: some 2% of all properties. If households cannot find a property to their liking and cannot afford larger properties, they are likely to end up in smaller properties, perhaps staying with their partner in the parental home. Column (3) in Table 7 shows that there is indeed a positive effect of regulation on the share of crowded properties. One may argue that this results from the fact that tighter controls make housing more expensive so that households will occupy smaller homes with fewer rooms. However, when we control for house prices in column (4) the effect of regulation on the share of crowded properties is very similar, even somewhat stronger. House prices are, as expected, positively associated with crowding. The coefficient indicates that a standard deviation increase in the refusal rate leads to a 0.58 percentage points

[20] We experimented with alternative datasets to provide further evidence for the mismatch mechanism. Specifically, one implication of mismatch is the proposition that the local housing transaction volumes (or, respectively, time-on-the market) should be less responsive to demand shocks in more restrictive locations. To test this proposition we

(footnote continued)
collected data from the UK Land Registry and replicated the analysis in Hilber and Vermeulen (2016), but using the local transaction volume as the dependent variable rather than house prices. Consistent with our theoretical prior, we find that an increase in (instrumented) regulatory restrictiveness has a highly statistically significant negative effect on the transaction volume-earnings elasticity. The results are available upon request.

[21] To limit the possibility that our results are driven by a few possibly incorrect outliers, we further exclude areas with a growth of > 20,000 dwellings or a decrease in the number of dwellings. Inclusion of those areas, however, does not make a substantial difference.

Table 8
Real options, housing diversity and regulatory restrictiveness.

Panel A – second stage (Dependent variable: Δ coefficient of variation of house prices, t + 3)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	OLS	OLS	2SLS	2SLS	2SLS	2SLS
Δ Refusal rate, *t-2*	0.00501	0.00784	0.00857	**− 0.0892***	**− 0.119****	**− 0.0726***	**− 0.117***
	(0.00453)	(0.00743)	(0.00659)	**(0.0480)**	**(0.0605)**	**(0.0434)**	**(0.0680)**
Δ Refusal rate, *t-2* × Coefficient of variation of earnings, *t + 3*	− 0.0375	− 0.0725	− 0.0758	**0.804***	**1.166****	**0.693***	**0.812***
	(0.0606)	(0.101)	(0.0858)	**(0.411)**	**(0.570)**	**(0.368)**	**(0.468)**
Year fixed effects (3)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Local authority fixed effects (350)	No	Yes	Yes	No	Yes	Yes	Yes
Control variables included (8)	No	No	Yes	No	No	Yes	Yes
Δ Share Labour voters local elections Ω(·)	No	No	No	No	No	No	Yes
Observations	1050	1050	1050	1050	1050	1050	1050
R-squared	0.808	0.826	0.864				
Kleibergen-Paap F-statistic				5.212	4.573	5.419	2.893

Panel B – second stage (Dependent variable: Δ room diversity)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	OLS	OLS	2SLS	2SLS	2SLS	2SLS
Δ Refusal rate, *t-2*	− 0.00186	− 0.00410	− 0.000875	**0.0900***	**0.0294**	**0.00427**	**0.0180**
	(0.00465)	(0.00549)	(0.00492)	**(0.0485)**	**(0.0339)**	**(0.0276)**	**(0.0399)**
Year fixed effects (3)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Local authority fixed effects (350)	No	Yes	Yes	No	Yes	Yes	Yes
Control variables included (8)	No	No	Yes	No	No	Yes	Yes
Δ Share Labour voters local elections Ω(·)	No	No	No	No	No	No	Yes
Observations	1050	1050	1050	1050	1050	1050	1050
R-squared	0.160	0.653	0.726				
Kleibergen-Paap F-statistic				18.52	17.49	19.01	8.151

Notes: All independent variables (except for earnings) are standardised with mean zero and unit standard deviation. **Bold** indicates instrumented. In Models (4)–(7), instruments for Δ Refusal rate and Δ Refusal rate × Coefficient of variation of earnings are Δ Share of Labour seats and Δ Share of Labour seats × Coefficient of variation of earnings. Ω(·) is approximated by a fifth-order polynomial of share Labour voters. Standard errors in parentheses are clustered at the LA level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$, $^{+}$ $p < 0.15$.

increase in the share of crowded properties (about one-third of a standard deviation). Hence, although the results are only significant at the 10% level, the implied magnitude of the point estimates is non-negligible.

A further symptom or proxy for mismatch is explored in columns (5) and (6) of Table 7, the share of non-permanent dwellings. Our underlying explanation for why this measure should proxy for housing market mismatch is that in more restrictive markets there is an incentive to accept even less optimally matched housing characteristics in the short term in order to intensify and increase the efficiency of search. Living in temporary accommodation – mainly caravans or trailers - has a low switching cost associated with it and is a cheaper strategy than buying a suboptimal place to live and then reselling it when a more suitable house is found. The ease with which search can be undertaken and its effectiveness will increase if the house-hunter can be physically present in the local market (Ha and Hilber, 2013). Moreover since the chances of finding a better match in the housing market will improve with length of time spent searching then there will be a payoff to having temporary accommodation available for searchers in markets where matching is more difficult. Thus in more restrictive LAs, other things equal, matching is more difficult and the share of temporary dwellings is greater. Although by improving the efficiency of search, temporary housing may itself reduce vacancies of permanent dwellings, it is such a relatively sub-optimal form of housing we would expect house-hunters to resort to it only when there is extreme difficulty in matching their preferences to available housing supply. So we would expect that the net share of temporary housing would be positively correlated with local restrictiveness. In column (5), Table 7, we find weak but consistent evidence that more restrictive markets have a higher rate of non-permanent homes. Given that the change in non-permanent homes is a somewhat noisy and indirect proxy for mismatch, it may not be too surprising that the results are not statistically significant at conventional levels.[22] Column (6) addresses the issue that this may be entirely the result of higher house prices. This does not seem to be the case; indeed the effect becomes somewhat stronger. The coefficient implies that a standard deviation increase in the refusal rate leads to an increase in the share of non-permanent homes of 0.213 percentage points (about one-third of a standard deviation). This positive relationship is certainly consistent with the proposition that more restrictive local planning increases the costs of matching would-be house buyers in the local housing market to the available permanent housing, inducing people to live temporarily in caravans and mobile homes – clearly inferior substitutes to houses.

In column (7) of Table 7 we show the results for yet another symptom or proxy for mismatch. We expect that tighter regulation in an LA makes it harder for people to move into the preferred area and find a new, satisfactory property in it. Hence, we expect that the share of in-migrants, defined as households that had a different address in the previous year, is lower. We initially find only very weak evidence supporting this proposition with a *p*-value of 0.15. But migration responds not just to spatial differences in wages but also to house price differences across space. So in column (8) we control for house prices. Although house prices are indeed negatively correlated with the share of migrants, controlling for house prices increases the effect of regulation. Not only is the relevant coefficient larger but it is now significant at the 10% level. The coefficient implies that a standard deviation increase in the refusal rate leads to a reduction in the share of migrants of 1.53 percentage points (almost half of a standard deviation), a substantial effect in economic terms.

[22] Resort areas may, for example, quickly acquire or lose a caravan site as their popularity changes.

3.7. Other potential explanations

Are there other explanations for the positive relationship between restrictions and vacancy rates? One might, for example, expect tighter policy restricting the elasticity of supply to be associated with greater price volatility and for that to be associated with higher vacancy rates. This is because price volatility might create a (real) 'option to wait' (McDonald and Siegel, 1986; Grenadier, 1995, 1996). The greater the uncertainty (price volatility) the more valuable is a property owner's option to delay selling or renting out the property. Especially in markets with lengthy leases – such as office markets – this can generate high and persistent ("sticky") vacancy rates; landlords are better off keeping their units empty.

We would expect the real options argument to be less important in the British residential property markets compared to office markets, however, since demand volatility tends to be lower (people have to live somewhere) and, for rentals, the length of tenancy agreements is typically quite short (a year or even less).

The real options argument, nevertheless, could be relevant since tight regulation (more inelastic supply) amplifies demand shocks, so we would expect price volatility to respond more strongly to demand volatility in places with tighter regulatory constraints. We proxy house price volatility by calculating the coefficient of variation of house prices in the year of observation and the three years that follow. Demand volatility is proxied by the coefficient of variation of earnings based on the year of observation and the three years that follow. The results (reported in Panel A of Table 8) show that at least according to the instrumental variable specifications (columns (4) to (7)), the sensitivity of price volatility with respect to earnings volatility increases with regulatory restrictiveness. This provides some evidence that regulatory constraints *potentially* could increase vacancy rates by increasing price volatility (i.e. increased price volatility raises the value of the real option to keep properties empty), but when we investigate this possibility further (see results in Appendix 2, Table A2.3 and discussion below) the evidence does not appear to support it.

Another argument might be that more regulation increases the variance of house types, rather than decreasing the number of available units, so that people have more choice than they would have without regulation. If this were the case vacancy rates would be higher, but this would not necessarily be a negative welfare effect. To test for this possibility we calculate a diversity index based on the share of properties with respectively 1, 2, 3, 4, 5, 6, or 7 + rooms. Hence:

$$div_{\ell,t} = \frac{1}{\sum_{r=1}^{7} s_{r,\ell,t}^{2}}, \tag{7}$$

where $s_{r,\ell,t}$ is the share of properties with r rooms in each local authority in each year. We regress this diversity measure on the refusal rate in Panel B of Table 8. The results show that diversity in the number of rooms is not affected by regulation. The results are generally far from being statistically significant and the point estimates are very close to zero.[23] Of course, any measure of housing diversity is flawed, but this exercise, as well as supporting anecdotal evidence,[24] does not seem to suggest that tighter regulation causes higher variance in housing types so increased variance in housing types as a result of tighter regulation is not a significant driver for the effects we find in the paper.

In Appendix 2 we estimate regressions where we directly control for commuting distance from the workplace, price volatility and room diversity. In Table A2.3 we show that the coefficient on the (instrumented) refusal rate variable decreases by about 15% and ceases to be statistically significant at conventional levels once we control for commuting time (column (1)). This suggests that at least a part of the positive effect of a change in regulatory restrictiveness on the change in vacancy rates is driven by mismatch in the housing market, as proxied by commuting time. When we instead control for price volatility (column (2)), interestingly, the effect of the refusal rate on vacancy rates increases somewhat in magnitude and is statistically significant. This finding could be interpreted as suggesting that the real options argument does not play an important role in explaining the positive link between regulatory restrictiveness and vacancy rates. When we include room diversity in column (3) the effect of regulation on vacancy rates again becomes somewhat stronger, although it is close to that estimated in the baseline specification. Room diversity is, as expected, positively correlated with vacancy rates; when diversity is one standard deviation higher, vacancy rates are 0.65 percentage points higher. We finally control for all variables, including the change in the share of crowded properties, the share of non-permanent properties and the share of in-migrants (column (4)). The coefficient related to the refusal rate is very similar to the previous specifications. We should note two caveats. First, our proxies, for mismatch in particular, are at best partial and, in the case particularly of the share of crowded and temporary properties, as well as the share of in-migrants, more symptoms than measures, so we would not expect these variables to fully account for the positive impact of the (change in the) refusal rate on the (change in the) vacancy rate. Second, the findings reported in Table A2.3 should generally be interpreted with caution. This is because both the commuting distance and the price volatility are likely highly endogenous, as are the alternative proxies for mismatch (some of them have the opposite sign from what might have been expected). Hence, the reported coefficients are likely biased. Still, overall, we interpret the results reported in Table A2.3 as indicating that the increased mismatch between the preferences of the local residents and the characteristics of the available local housing stock causally contributes to our key finding of a positive impact of tighter local restrictiveness on local vacancy rates. This finding is certainly plausible in the context of the extraordinarily rigid British planning system. Whether similar effects can be observed in other countries is an interesting question.

3.8. Sensitivity analysis

Finally, we conducted a set of robustness checks. We tested 1) the sensitivity of our choice of polynomial in relation to changes in the share of Labour votes; 2) the possible endogeneity of earnings; 3) whether results are driven by idiosyncratic factors associated with the Greater London Area; 4) whether the results are sensitive to including all refusals of residential development, not just refusals of applications for major developments; 5) whether a fixed effects approach rather than first differencing makes any difference to the main findings; and 6) whether the change of the Census definition of vacancies in 2011 affects our findings. The main results survive all these tests and alternative specifications essentially unchanged. The results are set out in detail in Appendix 2.

4. Conclusions

This is the first attempt to rigorously analyse the impact of land use regulation on the spatial and temporal variation in housing vacancy rates. It would come as no surprise to economists to observe that in well-functioning Labour markets there was unemployment. Workers search for jobs and employers seek (better) qualified workers. Attempting to regulate unemployment away makes no sense. Vacant

[23] We repeat the same analysis using a diversity measure based on the share of dwellings of a certain type (flats, terraced housing, semi-detached housing and detached housing). However, those data are not available for 1981, so we do not include LA fixed effects on top of first-differencing the variables to avoid the issue that those absorb all effective variation. We again find no effect of regulation on housing diversity. The effects are wholly statistically insignificant and are quantitatively unimportant.

[24] Our prior is that in Britain at least, tight local regulation should *reduce* the local variation of house types. This is because historic preservation in British cities is widespread and so called Conservation Areas ensure that all houses in a road look pretty much the same, thus potentially reducing the variance in types. In addition regulation includes prescriptions as to materials (most commonly matching local houses/traditions) and the external appearance of buildings.

houses are equivalent to unemployed workers yet, at least in Britain, policy does try to 'regulate' vacant homes away by using their existence to justify being more restrictive in the control of the supply of new homes and the structural adaptation of existing ones.

In this paper we argue that such restrictions have two main opposing effects on housing vacancies. The 'opportunity cost effect' leads to a lower vacancy rate in the more restrictive housing markets because supply constraints lead to higher prices, and thus to higher opportunity costs of keeping housing vacant. The 'mismatch effect', however, implies higher vacancy rates in the more restrictive housing markets because households will find it more difficult to match their preferences to the characteristics of the local housing supply given their budget constraints. So search becomes more prolonged and costly and households choose to search for properties further afield and commute longer.

We do indeed confirm that there is a simple negative correlation between local planning restrictiveness and local housing vacancies. Superficially this appears to support the planners' 'common sense' that the existence of empty houses means they can – even should – plan to be more restrictive in supply. This unconditional correlation, however, is the result of a form of joint causation. When we effectively control for unobserved characteristics at the local level by using first differencing, the negative correlation turns positive. When we further control for local linear trends, for other potential explanatory variables and account for the endogeneity of local regulatory restrictiveness, the causal effect of restrictiveness on vacancy rates is firmly positive, statistically significant and economically substantial.

Our empirical analysis does not fully unpack the box of explanations. It is a reduced form telling us what the net impact of increased planning restrictiveness is on housing vacancies. If an LA becomes more restrictive, signalled by an increase in the rate of refusal of residential development proposals, then all else equal, vacancy rates increase in that LA. We also provide direct evidence that mismatch may be an important reason for higher vacancy rates in more restrictive housing markets: The more restrictive in planning terms a local jurisdiction is, the longer is the average commuting distance of workers within it. We subject these findings to an extensive range of sensitivity analyses. They survive remarkably unaltered.

Welfare implications in markets with search frictions are not easy to derive. This is because in such a second best world, households may search less or more than would be welfare optimal (Koster and Van Ommeren, 2017). This paper does not set out to assess the net welfare impact of tightening the supply of housing. However, because regulatory constraints seem to worsen matching for households, the effect of tighter restrictions on vacancies and commute times unequivocally generates a welfare cost.

It is the mismatch between the preferences of households and the housing stock on offer that leads, other things equal, to higher vacancy rates in the more regulated – typically more desirable – places. This is not to say that tighter regulatory constraints in some places necessarily increase vacancy rates at the aggregate level (for the whole country). However, even if on aggregate vacancy rates were unaffected by regulatory restrictiveness, such constraints will still likely cause a significant welfare loss. This is because too much housing stays empty in the most regulated, most desirable and, by implication, most productive places with the strongest demand and highest valuations for living space. So people are induced to commute further, while living in the "wrong" places.

There are important implications for policy, particularly for the UK because of its extraordinarily restrictive planning system. Crucially planners should not allocate less land for development on the grounds that there are empty houses. Some vacancies are integral to the well-functioning of any market and, as our results show, trying to 'regulate housing vacancies away' is counterproductive. There is moreover a nice irony for advocates of the 'compact city'. The most common policy to attempt to implement this ideal is to impose growth boundaries (make land scarcer) and be more restrictive to adaptations of the existing stock or – in the US – make it more difficult to obtain zoning ordinance waivers. In summary aiming for a compact city makes planning policy more restrictive. Our results show this will have exactly the opposite to the intended effect because average commuting distances will lengthen as residents search further afield for housing they can afford and matches their preferences.

Appendix 1. Election data

The local election data are obtained from three different sources. The first source is the British Local Election Database, which is compiled by Rallings and Thrasher (2004). They have combined different data sources on local election outcomes from 1889 to 2003. From 1973, the data contain the universe of local election outcomes. The data are available on a ward level and display for every election the number of candidates, the number of votes per candidate and the number of vacant seats. Councillors that received the most votes will be elected. It is important to note that the share of votes for each party is therefore not perfectly correlated to the assigned number of seats. Based on the number of votes, we determine which candidate is elected as a councillor. The British Local Election Database only provides information on the election results, and not on the current composition of the LA. The problem is that for many local authorities, there are yearly or two-yearly elections of which 33% of the seats are replaced. To estimate the composition of the local council, we use the fact that the full electoral term for *councillors* is usually four years. However, sometimes elections replace the complete council despite the fact that councillors did not complete the electoral term, for example due to changes in boundaries of local authorities. To account for this, we consider full elections as elections where at least 75% of the seats are replaced.

The second data source for local election results is the local election handbooks from 1999 to 2008 (see Hilber et al., 2011). These data provide the number of council seats for each local authority in each year. We measure the correlation of the shares of seats of different parties (Labour, Conservatives, Liberal Democrats, Other) between the British Local Election Database and the latter database for the overlapping years. This is always above 0.95. For the overlapping years, we take the average of shares in seats in both datasets. For 2006–2011, we obtain information on Labour votes (rather than seats) from the Local Elections Archive Project (LEAP). For 55 LAs, we do not have information available for the most recent election in 2011. We then use information on Labour vote shares for the 2007 elections. We made sure that excluding these 55 LA lead to essentially the same results. The final dataset is from the BBC with the outcomes of local council elections for 2009, 2010 and 2011 to complement the LEAP when necessary.

We also use outcomes of general elections to control for demographic changes and general trends in political preferences. We have data of election results for 1983, 1987, 1991, 1997, 2001, 2005 and 2010 obtained from Electoral Calculus. We match each year to the previous election, except for 1981, which is matched to 1983. The results are available at the parliamentary constituency level, which are almost always smaller than LAs. Using geographical information systems, we calculate the geographical overlap of each constituency with each LA and assign the votes accordingly.

In Fig. 4 we provide a scatterplot of the share of Labour votes and the share of Labour seats from 1978 to 2011 for each year and local authority. Not surprisingly there is a strong positive correlation between seats and votes. However, below a vote share of one third, votes translate into a less than proportional number of seats (denoted by the dashed line). In a number of cases votes did not even translate into any seats at all. Furthermore, because of the 'first-past-the-post feature of the system', above a vote share of one third, votes lead to a more than proportional number of seats.

When having > 70% of the votes usually implies that all seats are assigned to the Labour party.

Table A1.1 shows correlations for the election variables of the Labour party in our data: The correlation between the share of Labour council seats and the share of Labour votes is high (0.87). The correlation with the general election vote share is somewhat lower, but still reasonable high (0.67). If we look at the correlation between the changes, these are lower (respectively 0.57 and 0.48).

Table A1.1
Correlations between election variables.

	Share Labour seats	Share Labour votes	Share Labour votes gen. elec.
Share Labour seats, *t-2*	1.0000		
Share Labour votes	0.8761	1.0000	
Share Labour votes general elections	0.6901	0.6060	1.0000

	Δ Share Labour seats	Δ Share Labour votes	Δ Share Labour votes gen. elec.
Δ Share Labour seats, *t-2*	1.0000		
Δ Share Labour votes	0.5706	1.0000	
Δ Share Labour votes general elections	0.4474	0.3834	1.0000

Appendix 2. Sensitivity analysis

To begin with, we re-estimate the preferred specification (column (7), Table 2), but include LA expenditure by category, one by one, as additional controls (columns (1) to (5)). In column (6) we instead include total local expenditures as an additional control. We report the results for vacancy rates in Table A2.1 below. The coefficients unequivocally indicate that the impact of the refusal rate on vacancy rates is unaffected, even when we control for total local expenditures or for each category of expenditure individually. Planning expenses seem to have some direct negative impact on vacancy rates (column (5)), so more planning expenses lead to lower vacancy rates. In column (6) we show that, even when the share of Labour seats might have a positive effect on total local expenditures (see Table 5), it seems that local expenditures do not have a direct impact on vacancy rates. Hence, this provides additional evidence that the exclusion restriction holds.

We also do a similar exercise but take the change in the log of commuting time as dependent variable. The results reported in Table A2.2 suggest that the effect of regulation on commuting distances cannot be explained by differences in expenditures on education, social services, highways, or planning. Column (4) shows that when controlling for housing expenditures the effect of regulation becomes statistically insignificant (*p*-value = 0.222). We do, however, not worry too much because the impact of housing expenditures is highly insignificant and the point estimate is very similar to the baseline estimate. On the other hand, column (6) suggests that the effect of commuting is even somewhat stronger, despite the effect that the total LA expenditures have a positive and statistically significant effect on commuting distance.

Next, we include commuting distance, price volatility and room diversity as additional controls in the regression of vacancy rates on the refusal rate. Table A2.3 reports the results, where column (7) in Table 2 is the corresponding specification. In column (1) of Table A2.3 we show that commuting distance is positively associated with higher vacancy rates. A one kilometre increase in the average commuting distance is associated with an increase in the vacancy rate of about 2 percentage points. We do not interpret this as a strictly causal effect, but the sign is in line with our expectations. The effect of restrictiveness is about 15% lower compared to the baseline specification. The effect is now statistically significantly different from zero only at the 13% level (*p*-value = 0.131). Commuting distance is a crude proxy for mismatch in the housing market, which may explain why the effect of interest is only somewhat smaller. In column (2) we include the coefficient of variation of prices in the year of observation and the three years subsequent to the year of observation. The coefficient is positive and highly statistically significant. Interestingly, the coefficient on regulatory restrictiveness is now somewhat higher compared to the baseline specification. Column (3) investigates the effect of regulation on vacancy rates again, while controlling for room diversity. The effect of the refusal rate becomes somewhat stronger, although it is close to the baseline specification. Room diversity is, as expected, positively correlated to vacancy rates. In column (4), Table A2.3, we control for all variables, including the change in the share of crowded properties, the share of non-permanent homes and the share migrants. The coefficient related to the refusal rate is very similar to the previous specifications.

Recall that in our preferred specification (column (7) of Table 2), we included a fifth-order polynomial of changes in the share Labour votes in each LA. This is to isolate the impact of potentially unobserved demographic and socio-economic variables, which may be reflected in voting behaviour, from local political power. However, the choice of the order of polynomial is somewhat arbitrary. Table A2.4 investigates the robustness of the results to this choice. Panel A reports second-stage results, whereas Panel B reports the corresponding first-stage results. In column (1) of Panel A we include only a linear term of change in the share Labour votes in local elections as a control. Changes in the share of Labour votes do not have a direct effect on changes in vacancy rates. The coefficient on the change in the refusal rate variable is statistically significant at the 11% level. When we include a third or fourth-order polynomial of change in share Labour votes, the coefficient on the change in the refusal rate variable becomes slightly higher and statistically significant at the 10% level. In column (5) we include a fifth-order polynomial of the change in local Labour vote shares, but we also include a fifth-order polynomial of the change in general election vote shares. The latter might be relevant, as one could argue that results of general elections might be a better proxy for the demographic characteristics of an LA, due to the substantially higher turnouts; on the other hand re-working the Parliamentary Constituency vote shares to generate an estimate for LAs must induce some measurement error. The point estimate is very similar to the preferred specification, but the effect is only statistically significant at the 20% level, likely due to a weaker first-stage (the corresponding first-stage Kleibergen-Paap *F*-statistic is only 4.9). In Panel B, we report corresponding first-stage estimates. The instrument has a similar impact across different specifications, but becomes somewhat smaller in magnitude, once we allow for more flexibility in the vote share controls.

In the baseline results we only treat changes in the refusal rate as endogenous. It is possible to argue that changes in earnings are also subject to endogeneity concerns. Earnings, which are a proxy for local demand for housing, are also dependent on the reaction of Labour supply to changes in demand. In turn, local Labour supply depends on the flexibility and adaptability of the housing stock to accommodate new workers, and may therefore depend on the vacancy rate as well (Glaeser et al., 2006; Saks, 2008; Hilber and Vermeulen, 2016). We therefore use a 'Bartik instrument' (Bartik, 1991) based on employment by industry in 1971 to identify earnings. The shock predicts the level of employment in each LA using information on national employment growth in each industry. So, we use exogenous changes (from the local perspective) in employment growth to predict total employment in each LA in each year. Table A2.5 reports the results. Panel A reports second-stage results, while Panels B and C report the corresponding first-stage results. In column (1) of Panel A we only include the endogenous variables change in refusal rate and change in earnings plus the year fixed effects. Changes in regulatory restrictiveness are still strongly positively correlated with changes in vacancy rates. The effect of changes in earnings is statistically insignificant, in line with previous results. Column (2) additionally includes LA fixed effects. The Kleibergen-Paap *F*-statistic of 5.5 now indicates fairly weak identification. Since our model is just identified, the estimated instrumented coefficients are median unbiased, yet they may be too imprecisely estimated to be useful (Angrist and Pischke, 2009). Moreover, if our results are biased, they should be biased towards the corresponding OLS estimates (see columns (2) and (3) Table 2), which in our case would imply that the estimated coefficients are themselves *under*estimates (Stock and Yogo, 2005; Murray, 2006). In any case with these caveats in mind, column (2) indicates that changes in the regulatory restrictiveness have a positive and statistically significant impact on changes in vacancy rates. Although the coefficient on the change in earnings variable now becomes positive and is much larger in magnitude than in previous specifications, it is not statistically significantly different from zero. When we include further control variables in column (3), the results are hardly affected (and coefficients remain weakly identified). In the final – most rigorous – specification, reported in column (4), we also control flexibly for vote shares. The refusal rate has a positive and statistically significant impact (at the 5% level) on vacancy rates: a one standard deviation increase in the refusal rate increases the vacancy rate by 1.1 percentage points, very similar to that implied by the estimates reported in Table 2.

In Panels B and Panel C we report the corresponding first-stage estimates of the models for the change in the refusal rate and the change in earnings respectively. Changes in the share of Labour seats are a reasonably strong instrument for the change in the refusal rate. Changes in the Labour demand shock measure are strongly positively correlated to changes in the refusal rate (i.e. areas that have experienced an exogenous inflow of employment have also become substantially more restrictive). In Panel C, Table A2.5, we observe that changes in the Labour demand shock measure are also positively correlated to changes in earnings, as anticipated.

We consider a number of additional sensitivity checks. The results are reported in Table A2.6. First, we exclude the Greater London Area to test whether the results are driven by the restrictive metropolitan area of London. Column (1) in Table A2.6 shows that the coefficient related to restrictions is even somewhat stronger compared to the baseline specification in column (7), Table 2. Hence, our results are not driven by the Greater London area. In column (2), Table A2.6, we focus on all restrictions as a sensitivity check. Because minor applications are much less important than major applications; the latter referring to the construction of at least 10 dwellings, while the first may refer to an application to construct an attic. We therefore, somewhat arbitrarily, first calculate the major and minor refusal rate and then take the average to arrive at the total refusal rate. The results indicate that the coefficient related to the total refusal rate is somewhat higher, albeit similar to the baseline specification. Because the refusal rate for minor projects is much noisier than the refusal rate for major projects, the Kleibergen-Paap *F*-statistic is much lower than in the baseline specification. This translates into somewhat less precise second-stage estimates, although the effect is still statistically significantly different from zero at the 10% level.

Finally, we pursue a fixed effects approach, rather than first-differencing. In column (3) we regress the vacancy rate on the refusal rate, while controlling for demographic variables, all in levels. We also include LA fixed effects. The results indicate then that a one standard deviation increase in the refusal rate leads to an increase in the vacancy rate of 0.76 percentage points, which is similar to the baseline specification. In column (4) we include 354 LA-specific linear trends. Results are essentially unchanged. Column (5) includes non-linear trends by estimating second-order polynomials. The effect almost doubles to 1.82, but that may be due to weaker identification (the Kleibergen-Paap *F*-statistic is relatively low with 5.1). Nevertheless, this suggests that controlling more carefully for unobserved time-varying factors of locations, the effect of restrictions on vacancy rates does not disappear.

The last concern we address is that the data on vacant housing may be measured with error, because in 2011 the Census also included second homes. We therefore use another data source for 2001 and 2011 from the Department of Communities and Local Government (DCLG - only available for a shorter time period). Because we do not have clear priors which data source provides a better estimate of 'real' vacancies in 2001, we calculate the average vacancy rate using both data sources for the year. We then estimate the same models as in Table 2. The second-stage results are reported in Table A2.7. Column (1) shows that even the bivariate specification suggests a positive and significant correlation between changes in regulatory restrictiveness and changes in vacancy rates. This also holds if we include LA fixed effects in column (2) and control variables in column (3). In column (4), we instrument the change in refusal rate with the change in share of Labour seats. The coefficient is almost identical to previous results: a one standard deviation increase in the refusal rate increases the vacancy rate by 1.0 percentage points. This result is hardly affected if we include LA fixed effects in column (5) and control variables in column (6). In column (7), we finally control flexibly for the change in local vote shares. In the last specification the coefficient on the change in the refusal rate variable becomes slightly but not statistically significantly smaller. It is still statistically significant at the 10% level. Note that the first-stage estimates are identical to the ones presented in Table 3. The results seem highly reassuring and strongly indicate that the potential measurement error in the Census data is not influencing our results.

In Table A2.8 we further test the robustness of our results to the potential measurement problem by excluding 2011 from the analysis. We report OLS and second stage results in Panel A. In columns (1) to (3), where we do not address endogeneity concerns, changes in the refusal rate are positively associated with changes in the vacancy rate, although the effect is not statistically significant in column (3). In column (4), we instrument for the change in the refusal rate. Again, the coefficient is strongly positive, but quite imprecisely estimated. The same holds for the remaining models; due to weak identification (see Panel B), the estimated effects are rather imprecisely estimated. Nevertheless, they seem to point towards a positive and economically meaningful effect of regulatory restrictiveness on vacancy rates, in line with the previous results.

To conclude, the various robustness checks all deliver very similar estimated effects of restrictiveness on vacancy rates in terms of magnitude compared to the baseline models. This provides additional support for the proposition that increased regulatory restrictiveness causes mismatch, leading to higher vacancy rates and longer commutes. We note that the results are not always statistically strong in the more comprehensive specifications. This appears to be mainly due to weak(er) identification.

Table A2.1
Controlling for expenditures – second stage. (Dependent variable: Δ vacancy rate).

	(1)	(2)	(3)	(4)	(5)	(6)
	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Δ Refusal rate, *t-2*	**0.888***	**0.915***	**0.930***	**0.841 $^{+}$**	**0.797 $^{+}$**	**0.954***
	(0.522)	**(0.520)**	**(0.528)**	**(0.551)**	**(0.497)**	**(0.569)**
Δ Education expenditures *(log)*	− 0.0781					
	(0.106)					
Δ Social services expenditures *(log)*		− 0.491				
		(0.435)				
Δ Highway expenditures *(log)*			0.0503			
			(0.254)			
Δ Housing expenditures *(log)*				− 0.00581		
				(0.0598)		
Δ Planning expenditures *(log)*					− 0.126 $^{+}$	
					(0.0856)	
Δ Total local expenditures *(log)*						0.179
						(0.143)
Year fixed effects (3)	Yes	Yes	Yes	Yes	Yes	Yes
Local authority fixed effects (350)	Yes	Yes	Yes	Yes	Yes	Yes
Control variables (8)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Share Labour voters local elections Ω(·)	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1050	1050	1047	998	1015	1050
Kleibergen-Paap *F*-statistic	8.265	8.400	8.314	7.543	8.835	7.471

Notes: All independent variables (except for earnings) are standardised with mean zero and unit standard deviation. **Bold** indicates instrumented. The instrument for Δ Refusal rate is Δ Share of Labour seats in the LA. Ω(·) is approximated by a fifth-order polynomial of share Labour voters in local elections. Standard errors in parentheses are clustered at the LA level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$, $^{+}$ $p < 0.15$.

Table A2.2
Controlling for expenditures – second stage. (Dependent variable: Δ commuting distance from workplace (log)).

	(1)	(2)	(3)	(4)	(5)	(6)
	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Δ Refusal rate, *t-2*	**0.0600 $^{+}$**	**0.0605 $^{+}$**	**0.0588 $^{+}$**	**0.0474**	**0.0568 $^{+}$**	**0.0731***
	(0.0402)	**(0.0399)**	**(0.0403)**	**(0.0388)**	**(0.0379)**	**(0.0437)**
Δ Education expenditures *(log)*	− 0.00928					
	(0.00818)					
Δ Social services expenditures *(log)*		0.00778				
		(0.0358)				
Δ Highway expenditures *(log)*			− 0.0169			
			(0.0160)			
Δ Housing expenditures *(log)*				0.000828		
				(0.00442)		
Δ Planning expenditures *(log)*					− 0.000148	
					(0.00594)	
Δ Total local expenditures *(log)*						0.0373***
						(0.0114)
Year fixed effects (3)	Yes	Yes	Yes	Yes	Yes	Yes
Local authority fixed effects (350)	Yes	Yes	Yes	Yes	Yes	Yes
Control variables (8)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Share Labour voters local elections Ω(·)	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1050	1050	1047	998	1015	1050
Kleibergen-Paap *F*-statistic	8.265	8.400	8.314	7.543	8.835	7.471

Notes: All independent variables (except for earnings) are standardised with mean zero and unit standard deviation. **Bold** indicates instrumented. The instrument for Δ Refusal rate is Δ Share of Labour seats in the LA. Ω(·) is approximated by a fifth-order polynomial of share Labour voters in local elections. Standard errors in parentheses are clustered at the LA level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$, $^{+}$ $p < 0.15$.

Table A2.3
Results controlling for commuting time and house price volatility.

Panel A – second stage	(1)	(2)	(3)	(4)
(Dependent variable: Δ vacancy rate)	2SLS	2SLS	2SLS	2SLS
Δ Refusal rate, *t-2*	**0.775**$^{+}$	**1.051****	**0.867***	**1.030***
	(0.513)	**(0.517)**	**(0.510)**	**(0.543)**
Δ Commuting distance from workplace *(log)*	1.977***			1.798***
	(0.660)			(0.685)
Δ Coefficient of variation of house prices, *t + 3*		2.969**		2.484**
		(1.173)		(1.128)
Δ Room diversity			0.651***	0.640***
			(0.192)	(0.227)
Δ Share crowded properties				− 0.594**
				(0.281)
Δ Share non-permanent properties				0.292^{+}
				(0.192)
Δ Share migrants				0.115
				(0.107)
Year fixed effects	Yes	Yes	Yes	Yes
Local authority fixed effects (350)	Yes	Yes	Yes	Yes
Control variables included (8)	Yes	Yes	Yes	Yes
Δ Share Labour voters local elections Ω(·)	Yes	Yes	Yes	Yes
Observations	1050	1050	1050	1050
Kleibergen-Paap F-statistic	8.307	9.052	8.154	8.106

Panel B – first stage (Dependent variable: Δ refusal rate, t-2)	(1)	(2)	(3)	(4)
	OLS	OLS	OLS	OLS
Δ Share Labour seats, *t-2*	− 0.267**	− 0.278**	− 0.265**	− 0.266**
	(0.114)	(0.113)	(0.114)	(0.115)
Δ Commuting distance from workplace *(log)*	− 0.148			− 0.0883
	(0.484)			(0.492)
Δ Coefficient of variation of house prices, *t + 3*		0.962		1.044
		(0.879)		(0.914)
Δ Room diversity			− 0.0430	− 0.148
			(0.171)	(0.176)
Δ Share crowded properties				0.232
				(0.206)
Δ Share non-permanent properties				0.0410
				(0.110)
Δ Share migrants				0.0778
				(0.0946)
Year fixed effects	Yes	Yes	Yes	Yes
Local authority fixed effects (350)	Yes	Yes	Yes	Yes
Control variables included (8)	Yes	Yes	Yes	Yes
Δ Share Labour voters local elections Ω(·)	Yes	Yes	Yes	Yes
Observations	1050	1050	1050	1050
R-squared	0.331	0.333	0.331	0.337

Notes: All independent variables (except for commuting time and house price variation) are standardised with mean zero and unit standard deviation. **Bold** indicates instrumented. The instrument for Δ Refusal rate is Δ Share of Labour seats. Standard errors in parentheses are clustered at the LA level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$, $^{+} < 0.15$.

Table A2.4
Controlling for vote shares.

Panel A – second stage (Dependent variable: Δ vacancy rate)	(1)	(2)	(3)	(4)	(5)
	2SLS	2SLS	2SLS	2SLS	2SLS
Δ Refusal rate, *t-2*	**0.756**$^{+}$	**0.782**$^{+}$	**0.861***	**0.897***	**0.888**
	(0.473)	**(0.490)**	**(0.505)**	**(0.526)**	**(0.689)**
Year fixed effects (3)	Yes	Yes	Yes	Yes	Yes
Local authority fixed effects (350)	Yes	Yes	Yes	Yes	Yes
Control variables included (8)	Yes	Yes	Yes	Yes	Yes
Δ Share Labour votes local elections	− 0.0742	2nd order	3rd order	4rd order	5th order
	(0.158)	Polynomial	Polynomial	Polynomial	Polynomial
Δ Share Labour voters general elections	No	No	No	No	5th order polynomial
Observations	1050	1050	1050	1050	1050
Kleibergen-Paap *F*-statistic	9.213	8.896	8.680	8.285	4.917

Panel B – first stage (Dependent variable: Δ refusal rate, t-2)	(1)	(2)	(3)	(4)	(5)
	OLS	OLS	OLS	OLS	OLS
Δ Share Labour seats, *t-2*	**− 0.274****	**− 0.271****	**− 0.273****	**− 0.267****	**− 0.215***
	(0.111)	**(0.112)**	**(0.114)**	**(0.114)**	**(0.119)**
Year fixed effects (3)	Yes	Yes	Yes	Yes	Yes
Local authority fixed effects (350)	Yes	Yes	Yes	Yes	Yes
Control variables included (8)	Yes	Yes	Yes	Yes	Yes
Δ Share Labour votes local elections	− 0.174*	2nd order	3rd order	4rd order	5th order
	(0.101)	Polynomial	Polynomial	Polynomial	Polynomial
Δ Share Labour voters general elections	No	No	No	No	5th order polynomial
Observations	1050	1050	1050	1050	1050
R-squared	0.329	0.329	0.329	0.331	0.338

Notes: All independent variables (except for earnings) are standardised with mean zero and unit standard deviation. **Bold** indicates instrumented. The instrument for Δ Refusal rate, t-2 is Δ Share Labour seats, *t-2*. Standard errors in parentheses are clustered at the LA level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$, $^{+}$ $p < 0.15$.

Table A2.5
Instrumenting for regulatory restrictiveness and earnings.

Panel A – second stage (Dependent variable: Δ vacancy rate)	(1)	(2)	(3)	(4)
	2SLS	2SLS	2SLS	2SLS
Δ Refusal rate, t-2	**0.814****	**0.966****	**0.816***	**1.055****
	(0.390)	**(0.387)**	**(0.439)**	**(0.498)**
Δ Earnings (log)	**− 0.257**	**4.469**	**6.370**	**4.149**
	(1.717)	**(5.170)**	**(5.820)**	**(5.114)**
Year fixed effects (3)	Yes	Yes	Yes	Yes
Local authority fixed effects (350)	No	Yes	Yes	Yes
Control variables included (8)	No	No	Yes	Yes
Δ Share Labour voters local elections Ω(·)	No	No	No	Yes
Observations	1050	1050	1050	1050
Kleibergen-Paap F-statistic	9.660	5.467	3.714	3.610

Panel B – first stage (Dependent variable: Δ refusal rate, t-2)	(1)	(2)	(3)	(4)
	OLS	OLS	OLS	OLS
Δ Share Labour seats, *t-2*	− 0.305***	− 0.297***	− 0.312***	− 0.224*
	(0.0702)	(0.0957)	(0.0981)	(0.116)
Δ Labour demand shock 1971 *(log)*	2.121***	6.953**	10.29***	10.12***
	(0.666)	(2.843)	(3.102)	(3.159)
Year fixed effects (3)	Yes	Yes	Yes	Yes
Local authority fixed effects (350)	No	Yes	Yes	Yes
Control variables included (8)	No	No	Yes	Yes

Δ Share Labour voters local elections Ω(·)	No	No	No	Yes
Observations	1050	1050	1050	1050
Adj. R-squared	0.172	0.317	0.335	0.342
Angrist-Pischke *F*-statistic	14.12	14.17	19.00	12.24

Panel C – first stage	(1)	(2)	(3)	(4)
(Dependent variable: Δ earnings (log))	OLS	OLS	OLS	OLS
Δ Share Labour seats, *t-2*	0.00575	0.0119	0.00259	0.0140
	(0.00864)	(0.0117)	(0.0106)	(0.0124)
Δ Labour demand shock 1971 *(log)*	0.823***	1.011***	1.245***	1.267***
	(0.0809)	(0.283)	(0.326)	(0.339)
Year fixed effects (3)	Yes	Yes	Yes	Yes
Local authority fixed effects (350)	No	Yes	Yes	Yes
Control variables included (8)	No	No	Yes	Yes
Δ Share Labour voters local elections Ω(·)	No	No	No	Yes
Observations	1050	1050	1050	1050
R-squared	0.406	0.557	0.616	0.622
Angrist-Pischke *F*-statistic	51.73	9.56	11.30	10.67

Notes: **Bold** indicates instrumented. The instruments for Δ Refusal rate and Δ Earnings *(log)* are Δ Share of Labour seats and Δ Labour demand shock 1971 *(log)*. Standard errors in parentheses are clustered at the LA level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$, $^{+}$ $p < 0.15$.

Table A2.6
Additional sensitivity checks – second stage results.

	(1)	(2)	(3)	(4)	(5)
	Exclude Greater London	Refusal rate for all projects	Fixed effects approach – no trends	Fixed effects approach – linear trends	Fixed effects approach – non-linear trends
	2SLS	2SLS	2SLS	2SLS	2SLS
Δ Refusal rate, *t-2*	**1.218****				
	(0.511)				
Δ Refusal rate for all projects, *t-2*		**1.124***			
		(0.673)			
Refusal rate, *t-2*			**0.757****	**0.714****	**1.822*****
			(0.346)	**(0.294)**	**(0.690)**
Control variables included (8)	Yes	Yes	Yes	Yes	Yes
Δ Share Labour voters local elections Ω(·)	Yes	Yes	Yes	Yes	Yes
Share Labour voters local elections Ω(·)	Yes	Yes	Yes	Yes	Yes
Local authority fixed effects (350)	Yes	Yes	Yes	Yes	Yes
Local authority linear trends (350)	No	No	No	Yes	Yes
Local authority non-linear trends (350)	No	No	No	No	Yes
Year fixed effects (3)	Yes	Yes	Yes	Yes	Yes
Observations	954	1050	1400	1400	1400
Kleibergen-Paap *F*-statistic	9.608	2.962	17.52	16.63	5.081

Notes: All independent variables (except for earnings) are standardised with mean zero and unit standard deviation. **Bold** indicates instrumented. The instrument for (Δ) Refusal rate is (Δ) Share of Labour seats in the LA. Ω(·) is approximated by a fifth-order polynomial of share Labour voters. Standard errors in parentheses are clustered at the LA level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$, $^{+}$ $p < 0.15$.

Table A2.7
Results using census data (1981–2001) and DCLG data (2001 – 2011). (Dependent variable: Δ vacancy rate).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	OLS	OLS	2SLS	2SLS	2SLS	2SLS
Δ Refusal rate, *t-2*	0.100***	0.141***	0.115**	**0.961*** **	**1.040*** **	**0.917*** **	**0.780* **
	(0.0375)	(0.0502)	(0.0472)	**(0.350)**	**(0.348)**	**(0.308)**	**(0.429)**
Year fixed effects (3)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Local authority fixed effects (350)	No	Yes	Yes	No	Yes	Yes	Yes
Control variables included (8)	No	No	Yes	No	No	Yes	Yes
Δ Share Labour voters local elections Ω(·)	No	No	No	No	No	No	Yes
Observations	1050	1050	1050	1050	1050	1050	1050
R-squared	0.294	0.468	0.522				
Kleibergen-Paap *F*-statistic				18.56	17.61	19.09	8.212

Notes: All independent variables (except for earnings) are standardised with mean zero and unit standard deviation. **Bold** indicates instrumented. In Models (4)–(7), the instrument for Δ Refusal rate is Δ Share of Labour seats in the LA. Ω(·) is approximated by a fifth-order polynomial of share Labour voters in local elections. Standard errors in parentheses are clustered at the LA level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$, $^{+}$ $p < 0.15$.

Table A2.8
Results using census data (1981–2001, excluding 2011).

Panel A – second stage (Dependent variable: Δ vacancy rate)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	OLS	OLS	2SLS	2SLS	2SLS	2SLS
Δ Refusal rate, *t-2*	0.123**	0.191**	0.114	**1.121**	**2.483* **	**2.321* **	**2.844**
	(0.0490)	(0.0896)	(0.0702)	**(0.697)**	**(1.278)**	**(1.401)**	**(2.467)**
Year fixed effects (2)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Local authority fixed effects (350)	No	Yes	Yes	No	Yes	Yes	Yes
Control variables included (8)	No	No	Yes	No	No	Yes	Yes
Δ Share Labour voters local elections Ω(·)	No	No	No	No	No	No	Yes
Observations	700	700	700	700	700	700	700
R-squared	0.412	0.620	0.736				
Kleibergen-Paap F-statistic				7.528	4.365	2.939	1.355

PANEL B – First stage *(Dependent variable: Δ Refusal rate, t-2)*	(4)	(5)	(6)	(7)
	OLS	OLS	OLS	OLS
Δ Share Labour seats, *t-2*	− 0.243***	− 0.255	− 0.219	− 0.163
	(0.0886)	(0.173)	(0.182)	(0.200)
Year fixed effects (2)	Yes	Yes	Yes	Yes
Local authority fixed effects (350)	No	Yes	Yes	Yes
Control variables included (8)	No	No	Yes	Yes
Δ Share Labour voters local elections Ω(·)	No	No	No	Yes
Observations	700	700	700	700
R-squared	0.070	0.358	0.375	0.385

Notes: All independent variables (except for earnings) are standardised with mean zero and unit standard deviation. **Bold** indicates instrumented. In Models (4)–(7), the instrument for Δ Refusal rate is Δ Share of Labour seats in the LA. Ω(·) is approximated by a fifth-order polynomial of share Labour voters in local elections. Standard errors in parentheses are clustered at the LA level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$, $^{+}$ $p < 0.15$.

Appendix 3. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jpubeco.2017.12.006.

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