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Title: Health impact, process, and economic evaluation of selective licensing schemes for private rented housing in England

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Sponsor: London School of Hygiene & Tropical Medicine.

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Contents

Part A: GENERAL INFORMATION

1. Title

Health impact, process, and economic evaluation of selective licensing schemes for private rented housing in England.

2. SAP version

Version	Protocol version	Effective date	Authors	Protocol deviations
1.0	4.0	11 Mar 2025	Ben Barr, Matt Egan, Katie Fahy, Katharina	1) Nearly 18% of all standardised LSOA11 intervention unit were fragmented while this was only true for a few units in the feasibility study. By 'fragmented' we mean that the LSOA11 units were only partly exposed to the intervention. This is particularly the case for

			Janke, Jakob Petersen, Tanith Rose, Frank De Vocht	smaller schemes that have 'bespoke' (rather than administrative) boundaries. SAP now details how this problem is approached in a set of sensitivity analyses weighing up selection bias against measurement error (Section 18. Analysis Plan). 2) At the data extract stage, University of Liverpool recommended focusing on emergency hospital admissions rather than just hospital admissions for the outcome indicators they produce under licence with NHS. The authors agreed to this change by email in July 2024 as this is in line with the literature on preventable hospitalisation (Section 18. Analysis Plan) [1,2]. 3) University of Glasgow (pers. comm., 17 Jun 2024), the owner of the Zoopla rent data, does not recommend combining data from before and after they changed the way they scrape the data from the Zoopla website, referred to as Generations 1 and 2. Consequently, the time series available for analysis is based on Generation 1 data. The first and last full year of data were 2012 and 2018, respectively. Therefore, 2012-2018, will be studied for this outcome instead of 2011-2019 (Section 18. Analysis Plan). 4) Staggered Difference-in-Differences with Propensity Score Matching is the primary evaluation method. Staggered Synthetic Difference-in-Differences (SDID) will now be carried out too. In case the Parallel Trends assumption is violated for a given outcome, SDID will become the primary method instead. We will restrict the analyses to schemes implemented 2012-2019 as the SDID method requires at least 1 year of pre-intervention data (Section 18. Analysis Plan). 5) Annual precipitation (mm) has been added as a falsification outcome (Section 17. Potential bias). 6) Some outcomes would originally be log-transformed for analysis. We have amended this in line with recent recommendations (Section 18. Analysis Plan). 7) The sample has changed slightly for two reasons, a) we now restrict the analyses to schemes implemented 2012-2019 as the SDID method requires at least 1 pre-intervention time period. Before the sample included three small schemes implemented in 2011: 2011_NewcastleuponTyne, 2011_Stoke-on-Trent, and 2011_Thanet (Section 18. Analysis Plan), b) the SL scheme data have been revised following a data validation consultation (25 Sep-8 Nov 2024): Sheffield 2014 has been recorded as a street-based scheme (N=2 LSOA) and newly treated areas of 2020_Wirral has been recoded as a 'Future treated scheme 2020-21' (N=2 LSOA). 8) In consultation with ISC, we have dropped or simplified some of the secondary subgroup analyses due to multiple testing concerns. False Discovery Rate corrections will furthermore be applied to secondary analyses (Section 18. Analysis Plan). 9) In consultation with DMEC, we have updated the Conflict of Interest statement to reflect that some members reside in local authorities that have or had Selective Licensing schemes.

3. Summary

Scientific abstract (WP1, 3)

Research question

Can selective licensing (SL) schemes improve England's private rented housing sector leading to health and social benefits for residents?

Background

English local authorities can implement SL to help improve conditions and reduce antisocial behaviour (ASB) linked to private rented housing. SL requires private rented landlords to register, purchase a license with various conditions, and allow inspections to ensure housing standards are maintained. A London study of SL found evidence of impacts but a national study is needed.

Study Aim

To evaluate SL schemes for the private rented housing sector in England. The evaluation will assess health and social impacts, implementation and change mechanisms, and economic consequences.

Work package objectives

WP1: To assess individual-level and area-level impacts of SL schemes on health and social outcomes. To assess impacts on equity.

WP3: To assess whether SL schemes are value for money.

Methods

WP1: The proposed impact evaluation uses a difference-in-differences design with matched controls. Individual-level outcomes are self-reported anxiety (primary outcome), wellbeing, general health and residential stability from the Annual Population Survey (APS) and area-level outcomes are an index of mental health and wellbeing (SAMHI: primary outcome), emergency hospital admissions (all cause, cardiovascular, respiratory, asthma, injuries due to falls or burns), ASB, population turnover and rent.

41 Local Authorities operated 61 SL schemes between 2012 and 2019: n=1,635 Lower Layer Super Output Areas (LSOAs);approx. 5.5% of the English population. For our individual-level outcomes, we estimate that the APS includes around 1000 participants per year from these LSOAs. Over our 2012 to 2019 sample period, we expect to observe around 8,000 individuals in the intervention group and three times as many in the matched comparison LSOAs if we match three control LSOAs to every intervention LSOA. We will estimate the average treatment effect on the treated (ATT) using recently developed difference-in-differences methods for staggered treatment adoption.

WP3: We will conduct a cost-benefit analysis using the subjective wellbeing approach to obtain a monetary value for the benefits of SL.

Timeline and Milestones

Start date: 1st December 2023

Length: 26 months.

- 0-3 months: ethics approval, PPI & AG meetings.
- 2-6 months: WP1 update data, submit protocol
- 7-17 months: WP1 Analysis plan and analysis; WP3 prepare data

- 18-22 months: All WPs: Analysis, interpretive workshops; refine and begin write-up
- 22-26 months: final analysis and write-up.

Key words

Housing system; private rented sector, public health, health equity, natural experiment evaluation.

4. Sponsor and funder detail

Sponsor: London School of Hygiene & Tropical Medicine, Keppel Street, London, WC1E 7HT. +44 (0)20 7636 8636

Funder: National Institute for Health and Care Research, Public Health Research (NIHR PHR 154797)

5. Conflict of interest statements

ME is co-chair of the London Public Health and Housing Network. This is an unfunded role. The Network links third sector and community stakeholders, local (London) public sector practitioners, national policy stakeholders, Greater London Authority and researchers interested in Public Health and Housing.
The research team includes private rented housing tenants, private rented housing landlords, and owner occupiers. Some members of the team reside in local authorities that have implemented selective licensing schemes.

6. Investigators

6a. Investigator names

Principal investigators: Prof Matt Egan[1] and Dr Jakob Petersen[1].

Co-investigators: Dr Alexandros Alexiou[2*], Prof Ben Barr[2], Dr Katie Fahy[2], Dr Katharina Janke[3], Dr Tanith Rose[2], Prof Frank De Vocht[4]

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Part B: INTRODUCTION

This is the statistical analysis plan for the NIHR PHR funded project NIHR154797: *Health impact, process, and economic evaluation of selective licensing schemes for private rented housing in England*.

It follows our previous production of the study protocol. The protocol introduced the study and the intervention being evaluated. The intervention is selective licensing, a discretionary power local authorities have to license landlords of private rented homes that are for single households (i.e. not houses of multiple occupancy). The policy has a number of goals but at its simplest it provides a means of regulating landlords so that they meet legal requirements and maintain the quality of the homes they rent out. Our protocol gave details of evaluation work divided into four work packages (WPs): WP1 – the impact evaluation; WP2 – the process evaluation; WP3 – the economic evaluation; and WP4 – knowledge exchange.

The protocol, and our original proposal to the funder, stated that we would also draw up a statistical analysis plan (SAP). The SAP is intended to provide more information on the quantitative analysis in WP1 and WP3, and especially provide more details on our analysis of the primary outcomes for the impact evaluation. We said we would develop the SAP in the first half the project timeline. The timing allows us to finalise our data sources; finalise and update access to those sources across the team; and discuss any emerging methodological issues and options including any methods that have been more recently developed or that we have recently familiarised ourselves with. The SAP will, for the most part, stay in line with the protocol but we allow ourselves some space to change methods if we can justify why it improves the analysis and providing we are transparent about any changes. Importantly, the SAP is developed before we do any outcome analysis. To sign off the SAP, we send the draft SAP to our Independent Steering Committee, which includes two members with extensive statistical expertise, a member with experience of leading NIHR mixed methods evaluations, and a public partner. Following comments and revisions we will then send it to NIHR for approval and make the finalised version publicly available. Then we will conduct the outcome analysis for the project. The intention is to follow the SAP when doing that analysis – any subsequent changes to the plan would need to be justified and made transparent in publicly available revisions to the SAP.

8. Aims and objectives

8a. Aim

To evaluate SL schemes for the private rented housing sector in England. The evaluation will assess health and social impacts, implementation and change mechanisms, and economic consequences.

8b. Objectives

WP1: To assess individual-level and area-level impacts of SL schemes on health and social outcomes. To assess impacts on equity.

WP3: To assess whether SL schemes are value for money.

Research Question

Can selective licensing (SL) schemes improve England's private rented housing sector leading to health and social benefits for residents?

PICO

- Population: Residents in areas exposed to SL; Individual PRS tenants exposed to SL. (For qualitative work, relevant stakeholders we will sample include tenants, landlords, implementers and service providers).
- Intervention: SL schemes.
- Control: Matched control areas.
- Outcomes:
    - Area level: mental health and wellbeing (SAMHI: primary outcome); physical health (operationalised as all-cause emergency hospital admissions, cardiovascular emergency admission in 65+yr, respiratory emergency admission in 65+yr, emergency admission injuries falls/burns in 65+yr (any place of occurrence), asthma emergency admission (all ages, 65+yr), all-cause A&E attendance (all ages, 0-14yr, 65+yr)); ASB, population turnover, £ rent.
    - Individual level: self-reported anxiety (primary outcome); general self-reported health, residential stability (ONS Annual Population Survey - APS).

8c. Purpose

The impact assessment intends to estimate causal effects.

Part C: METHODS

9. Design and setting of study

9a. Study design

WP1 Impact evaluation: Individual-level outcomes

The impact evaluation has a difference-in-differences design with matched controls. The idea of a difference-in-differences design is to supplement a simple before-after comparison of the outcomes of interest in the treated units with a before-after comparison of the outcomes in a set of control units to remove any changes that would have happened in the absence of the intervention. The underlying assumptions is that in the absence of the intervention the average before-after change would have been the same in the treated units and in the control units, known as the parallel trends assumption.

When considering individual-level outcomes, treated units are private rented sector tenants in LSOAs with SL. Potential control units are individuals who do not live in private rented sector housing or who do not live in LSOA with SL. However, SL might have spillover effects. For example, many SL schemes are set up to tackle anti-social behaviour. If they were effective at addressing anti-social behaviour, individuals in SL areas who do not live in private rented housing might also benefit from SL. Using these individuals as controls might therefore result in underestimating the impact of SL. Similarly, individuals in neighbouring LSOAs who do not have an SL scheme in place might be affected by the SL scheme if, for example, SL leads to displacement of certain types of private sector tenants such as tenants engaged in anti-social behaviour. Using these individuals as controls might result in overestimating the impact of SL.

We therefore use as our controls individuals who live in private rented housing in LSOAs without SL that are not direct neighbours of LSOAs with SL. From among these controls, we choose those individuals who live in LSOAs that we have matched to the treated LSOAs using characteristics of the LSOAs such as proportion of individuals who are under 16, proportion of individuals who are non-white and proportion of households classed as overcrowded.

Our individual-level outcome measures come from repeated cross-sectional data, so each year different individuals are being observed in each LSOA. The starting point for our analysis is the canonical two-way fixed effects regression specification:

$$Y_{iat} = \alpha_a + \varphi_t + \beta D_{at} + \varepsilon_{iat}$$

Y_{iat} indicates one of our outcome measures for individual *i* in LSOA *a* in year *t*. α_a is an LSOA effect, which accounts for time-invariant differences between the LSOAs, and φ_t is a year effect, which accounts for changes over time happening in all LSOAs. D_{at} takes the value 1 if LSOA *a* is subject to an SL scheme in year *t* and 0 otherwise. β is the treatment effect, i.e. it captures the impact of the SL scheme. In the special case of only two years *t* (before and after the intervention) the ordinary least squares (OLS) estimate $\hat{\beta}$ is equivalent to the difference-in-differences estimate $\hat{\gamma}$:

$$\hat{\gamma} = \left(\bar{Y}_{SL,AFTER} - \bar{Y}_{SL,BEFORE}\right) - \left(\bar{Y}_{COMPARATOR,AFTER} - \bar{Y}_{COMPARATOR,BEFORE}\right)$$

$\bar{Y}_{SL,AFTER}$ is the mean of the outcome measure in the LSOAs with an SL scheme after implementation of the SL scheme and $\bar{Y}_{SL,BEFORE}$ is the mean before implementation. $\bar{Y}_{COMPARATOR,AFTER}$ is the mean of the outcome measure in the matched comparator LSOAs in the year after SL was implemented in the SL areas and $\bar{Y}_{COMPARATOR,BEFORE}$ is the mean in the year before implementation. Unfortunately, this intuitive causal interpretation of the parameter estimate $\hat{\beta}$ is unlikely to apply in the more general case of more than two years *t* and the intervention being rolled out over several years. As the implementation of SL schemes is at the discretion of local authorities, they have been rolled out at different times over many years.

In this setting with staggered intervention timing the OLS estimator of β in the canonical two-way fixed effects regression yields an unbiased estimate of the treatment effect only if there is no treatment effect heterogeneity, i.e. if all LSOAs have the same treatment effect and if the treatment has the same effect regardless of how long it has been since the treatment started. The bias occurs because the OLS estimator is a weighted average of comparisons between treated and not-yet-treated units as well as between treated and already-treated units. If, for example, the treatment effect gets stronger over time, the parallel trends assumption discussed above does not hold for the latter comparison [3,4].

The recent literature on difference-in-differences has proposed alternative estimators that overcome the problem of the "forbidden" comparison between treated and already-treated units by making transparent exactly which units are being used as comparison [5]. We will apply the Callaway and Sant'Anna estimator as our main method [6]. This estimator allows for arbitrary treatment effect heterogeneity and dynamic effects by deconstructing the estimation of the overall treatment effect into estimation of so-called group-time average treatment effects. We will apply different aggregation schemes to explore the overall effect as well as treatment effect heterogeneity over time and across groups.

To assess the plausibility of the parallel trends assumption, we will examine the trends in the outcome measures for the treatment and control units in the before period by plotting the raw data over calendar time for the treatment and control units, with the treatment units and their matched control units.We will test if the estimated treatment effects for the three years before the intervention are jointly statistically significantly different from zero. Recent reviews discuss the difficulties with formally testing parallel trends as the analysis may be underpowered [5,7]. Analysts are advised to also assess event time plots and to exercise a degree of pragmatism in cases of small deviations from no-effect before the intervention.

If the parallel trends assumption seems implausible, we will use Synthetic Difference-in-Differences (SDID) instead of DiD as the primary method [8,9]. If the parallel trend assumption is plausible, we will use SDID for our primary outcome to assess the sensitivity of our results to the specific method. SDID finds controls matched on the pre-intervention evolution of outcomes through a weighting strategy and thereby ensures parallel trends in the pre-intervention period. Simulations have shown that SDID performs compared to other commonly used methods [10]. We will use the same set of LSOA-level covariates for the synthetic controls as for the propensity score matching (see Table 4).

We will explore spillover effects by repeating the difference-in-differences analysis with matching for (1) individuals who do not live in private rented sector housing and (2) for individuals who live in LSOAs that are first-order neighbours of SL areas. For (1) we will compare individuals who do not live in private rented sector housing in SL areas to individuals who do not live in private rented sector housing in the control areas that we have matched to treatment areas for the main analysis. For (2) we will treat the first-order neighbours of treated LSOAs as if they are intervention LSOAs and match them to comparable LSOAs that are neither SL areas nor SL area neighbours. We will report the average effects over the first five years after the intervention (0 to 5 years) as the main result. We will report this together with an event time plot showing all available years before and after the interventions. In addition, we will report delayed effects and longer-term effects as secondary results.

WP1 Impact evaluation: Area-level outcomes

When considering area-level outcomes, treated units are LSOAs with SL. Potential control units are LSOAs without LSOA. However, SL might have spillover effects. Neighbouring LSOAs who do not have an SL scheme in place might be affected by the SL scheme if, for example, SL leads to displacement of certain types of private sector tenants such as tenants engaged in anti-social behaviour. Using these LSOAs as controls might result in overestimating the impact of SL. We therefore use as our potential controls LSOAs without SL that are not direct neighbours of LSOAs with SL. From among these potential controls, we choose LSOAs that we have matched to the treated LSOAs using characteristics of the LSOAs such as proportion of individuals who are under 16, proportion of individuals who are non-white and proportion of households classed as overcrowded.

The starting point for our analysis of the area-level outcomes is the following two-way fixed effects regression specification:

$$Y_{at} = \alpha_a + \varphi_t + \beta D_{at} + \varepsilon_{at}$$

Y_{at} indicates one of our outcome measures for LSOA *a* in year *t*. α_a is an LSOA effect, which accounts for time-invariant differences between the LSOAs, and φ_t is a year effect, which accounts for changes over time happening in all LSOAs. D_{at} takes the value 1 if LSOA *a* is subject to an SL scheme in year *t* and 0 otherwise. β is the treatment effect, i.e. it captures the impact of the SL scheme. Because of the staggered intervention timing we will apply the Callaway and Sant'Anna estimator and estimate all relevant group-time average treatment effects and then summarise them using different aggregation schemes [6].

As for the individual-level outcomes, we will assess the plausibility of the parallel trends assumption by examining the trends in the outcome measures for the treatment and control units in the before period by plotting the raw data and by running an event study regression for each group. In case the parallel trends assumption is implausible, we will use Synthetic Difference-in-Differences (SDID) instead of DiD as the primary method [8,9].

We will explore spillover effects by repeating the difference-in-differences analysis with matching for LSOAs that are first-order neighbours of SL areas by treating the first-order neighbours of treated LSOAs as if they are intervention LSOAs and matching them to comparable LSOAs that are neither SL areas nor SL area neighbours.

WP3 Cost-benefit analysis using subjective wellbeing approach

The economic evaluation will produce a cost-benefit analysis that uses the subjective wellbeing approach to monetise the benefits of SL. The starting point for the calculation of the benefits of SL will be the estimate of the impact of SL on the life satisfaction variable from the APS, one of our individual-level outcome measures. We will then follow the procedure described in Dolan et al. (2019) [11]. Essentially, we will use an estimate of the impact of income on life satisfaction from previous research to determine the income change that would be required to achieve the same change in life satisfaction as achieved by SL. Multiplying this income change amount by the number of individuals affected by SL will provide the total benefit of SL from a societal perspectives. All our calculations will be retrospective and estimate the benefits of SL over our 2012 to 2019 sample period. Depending on the results of our analysis of spillover effects, we will adjust the benefit estimate accordingly.

We will calculate the costs of SL from a public sector perspective and attempt to estimate the costs of SL from a societal perspective. To determine the total cost of SL from a public sector perspective over the 2012 to 2019 sample period, we will measure the direct and indirect cost of SL in four case study sites. Accounting data provided by the case study sites will be the starting point for the cost calculation. However, the economic concept of opportunity cost goes beyond accounting cost. For example, local authorities might employ staff specifically for the administration of the SL schemes in their area, so these costs would be included in the accounting data as staff salaries and on-costs. However, staff not specifically employed for SL purposes might also be involved in administering local schemes. Any staff involved in SL will require office space and might need to be trained and supervised. Local authorities need to disseminate information about their SL schemes using their website or other means and maintain the online application system. All these activities incur costs, either directly in the form of money being handed over in payment or indirectly in the form of resources such as staff time or office space not being available for alternative uses.

The four case study sites have been selected to represent different types of SL (smaller and larger) in different regional contexts (north and south). We will use extrapolation methods that use these two broad characteristics as well as cost information obtained through FOI requests to assign costs to the other local authorities that have implemented SL. We will subtract income from license fees, fines and civil penalties to obtain net costs.

In addition, we will estimate the potential costs saved by the NHS through SL's impact on antidepressant prescribing, mental-health related hospital admissions and all-cause hospital admissions over our sample period. If possible, we will also estimate potential cost savings through impacts on mental health-related benefits. If we find substantial cost savings, we will subtract these from the cost of SL to local authorities to obtain net costs from a public sector perspective.

To estimate net costs from a societal perspective we will add back in any income local authorities generate from license fees, fines and civil penalties and try to estimate the costs landlords incur to remove hazards following the receipt of an improvement notice issued by the local authority following an SL inspection.

9b. Study setting

WP1 and WP3 will examine impacts across England.

10. Study schedule

Start date: 1st December 2023

Length: 26 months.

- 0-3 months: ethics approval, PPI & AG meetings
- 2-6 months: WP1 update data, submit protocol
- 7-17 months: WP1 Analysis plan and analysis; WP3 prepare data
- 18-22 months: All WPs: Analysis, interpretive workshops; refine and begin write-up
- 22-26 months: final analysis and write-up.

11. Sample size

Individual-level outcomes

For the individual-level outcome measures, we estimate that the APS includes about 1,000 respondents per year who live in private rented accommodation in LSOAs covered by SL schemes, so over our 2012 to 2019 sample period we expect to observe around 8,000 individuals in the intervention group (5.5% of the England population live in the schemes included in the study). The number of APS participants in the matched comparison LSOAs will be approximately 3000 per year if we match 3 control LSOAs to every intervention LSOA.

As the power calculation for our staggered treatment timing difference-in-differences design with repeated cross-section data would be very complicated, we ran a power calculation for the canonical difference-in-difference design with one pre- and one post-intervention observation to get a broad idea of the statistical power of our analysis. The power calculations are based on a bootstrapping algorithm with 1,000 repetitions with 95% significance level and a 3:1 ratio between control to intervention units. The outcome variable was designed as normal-distributed and z-standardised in an artificial dataset. With just 1,000 respondents – the potential annual number of respondents in treated areas – the minimum detectable difference would be 30% of an SD (Table 1).

Table 1 Power calculations based on bootstrapping algorithm with 1,000 repetitions for a DiD design with 95% significance level, one pre- and one post-intervention observation, and 3:1 ratio between control to intervention units (Burlig et al. 2020) [12]. The outcome variable was designed as normal-distributed and z-standardised. Cells shaded in grey indicate power calculations at or above 80%. A unit can correspond to either a respondent or a LSOA area depending on the context. N corresponds to the number of treated units.

N	10%SD	15%SD	20%SD	25%SD	30%SD
1000	0.167	0.291	0.523	0.694	0.837
1500	0.195	0.440	0.637	0.864	0.953
2000	0.272	0.540	0.800	0.934	0.989
2500	0.333	0.659	0.884	0.972	0.997
3000	0.379	0.706	0.923	0.987	0.998
3500	0.423	0.787	0.958	0.989	1.000
4000	0.515	0.833	0.979	0.997	1.000
4500	0.568	0.876	0.988	0.999	1.000
5000	0.593	0.894	0.993	0.998	1.000
5500	0.652	0.930	0.993	1.000	1.000
6000	0.685	0.946	1.000	1.000	1.000
6500	0.727	0.962	0.998	1.000	1.000
7000	0.767	0.973	1.000	1.000	1.000
7500	0.755	0.982	1.000	1.000	1.000

8000	0.797	0.986	1.000	1.000	1.000
8500	0.811	0.992	1.000	1.000	1.000
9000	0.839	0.994	1.000	1.000	1.000
9500	0.850	0.994	1.000	1.000	1.000
10000	0.870	0.999	1.000	1.000	1.000

Area-level outcomes

For the area-level outcomes, our main analysis will examine 1,635 treated LSOAs, which would give a minimum detectable difference of 25% of an SD (Table 1).

12. Sampling procedure

Our intervention group will include all selective licensing schemes in England with start dates between 2012 and 2019, as identified by Freedom of Information (FOI) requests to all English local authorities. Comparison areas will be identified through matching (see Section 18).

13. Participant selection

The impact evaluation (WP1,3) is based on secondary data analysis and does not involve recruitment of participants.

14. Variables and data sources

14a. Variables

Individual-Level Outcomes

We have chosen outcome measures based on previous research on housing interventions and policy aims. Poor quality homes and unsafe neighbourhoods present both mental and physical risks. Our primary outcome will be **high levels of anxiety self-reported in the Annual Population Survey (APS)**, measured as people reporting a score of more than 6 in response to the question "Overall, how anxious did you feel yesterday?", where 0 is `not at all anxious' and 10 is `completely anxious'. A threshold of more than 6 on the 11-point scale has been identified by the ONS as a measure of high anxiety levels.

Our secondary outcomes will be the **three other measures of subjective wellbeing (happiness, life satisfaction and feeling worthwhile)** included in the APS. We will also examine **the general self-reported health measure collected by the APS** as an indicator of health that includes both mental and physical health. To investigate potential unintended consequences of SL, we will also examine the **APS respondent's number of years at the current address** as an indicator of residential stability.

Area-Level Outcomes

Our primary area-level outcome will be the **Small Area Mental Health Index (SAMHI)** and its individual domain scores. The index is available for LSOAs on an annual basis [13]. It combines the following measures of mental healthcare use into a single index: mental health-related hospital admissions, antidepressant treatment days per population, proportion of the population diagnosed with depression, proportion of population in receipt of mental health-related benefits.

We will complement the analysis with an exploration of physical health outcomes. Since the treated areas are relatively small, most physical health outcomes are not available at the spatial granularity needed for the analysis. However, we can use **1. All-cause emergency hospital admissions** as an

indicator of the population's overall health. Similar measures have been used to evaluate public health and social care interventions, including housing [14,15]. Hospital admissions data are available for LSOAs on an annual basis. This indicator is equivalent and a counterpart to the mental health-related hospital admissions element of the SAMHI and can be extracted from the Hospital Episode Statistics data (NHS Digital), which the University of Liverpool has access to. In addition, we will examine **2) Cardiovascular emergency admission in 65+yr, 3) Respiratory emergency admission in 65+yr, 4) Emergency admission injuries falls/burns in 65+yr (any place of occurrence), 5) Asthma emergency admission, a) All ages, b) 0-14yr, c) 65+yr, 6) All-cause A&E attendance, a) All ages, b) 65+yr**. There is evidence for these outcomes showing associations with the home environment [16–18]. Older people are particularly vulnerable to poor quality housing. By the same token, we will study emergency asthma admissions among 0-14-year-olds. Rodgers et al. 2018 studied emergency admission injuries falls/burns in 65+yr occurring in the home. This level of detail, i.e. place of occurrence, is not available in the data available to us and we will study this is as 'any place of occurrence'.

Incidence of police-recorded ASB: High levels of ASB is one of the most common reasons for local authorities to implement SL [19] . These data are available for LSOAs on a monthly basis [20].

Population turnover index: An increase in population turnover might indicate unintended consequences of SL. The population turnover index is based on a combination of electoral roll and consumer data [21].

Tenants' rent (£): We have gained access to Zoopla Property Data through the Urban Big Data Centre at the University of Glasgow [22]. Zoopla is one of the largest online property advertising companies. The Zoopla data affords a much higher level of granularity than government data on property rents, which are only available at regional level. The data are based on daily extractions, so they reflect day-to-day advertising rather than stock value at any given moment.

University of Glasgow (pers. comm., 17 Jun 2024), the owner of the Zoopla rent data, does not recommend combining data from before and after they changed the way they scrape the data from the Zoopla website in 2019. The first full year of data is 2012. Therefore, for this outcome measure we will study only 2012-2018 instead of 2011-2019.

Listings were selected that qualified on the following criteria (Figure S2): Generation 1 Zoopla data, rentals, residential 2-bedroom properties, non-missing last marketed data 2012-2018, England, first listing per property per year.

We will use the mean rent for a 2-bedroom home with the bottom and top centiles removed to address outlier issues. We will also study the same for 2-bedroom flats ('flat' or 'block of flats') as the most commonly occurring property type.

Matching variables

We have chosen matching variables measured before the interventions or as close as possible to this time point from Census 2011, Indices of Multiple Deprivation, and council tax data. From Census 2011 we use the proportion of the population that are <16 years, proportion of the population aged 16 to 59 years, proportion of the population who are non-white and proportion of households from private rented sector. From the English Indices of Deprivation 2015, we use the proportion of population experiencing deprivation related to income, proportion of social and private homes that fail to meet the Decent Homes standard, proportion of households classed as overcrowded and an indicator of inability to afford to enter owner-occupation in the private rental market. Finally, we create a built pre-1945 indicator. The data on dwelling construction era were only 96.9% complete

and were hence imputed sequentially with non-missing values from nearest LSOA11 neighbours (geodetic distance) (Table S3). For each LSOA11, the most common construction era was selected. In cases with ties (2.95%), the earliest construction era was selected.

14b. Data sources

The APS data for the individual-level outcomes come from ONS and access to the data through the Secure Research Service (SRS) was approved on 2 Aug 2024 [23].The sources of the area-level outcome measures are the University of Liverpool Place-based Longitudinal Data Resource [13], NHS Digital [24], Police.uk [25], University of Glasgow Urban Big Data Centre [22], and ONS [23]. The sources of LSOA characteristics used for matching are the Department for Levelling Up, Housing and Communities [26], and ONS [27,28].

15. Data collection and management

Quantitative analyses will only involve secondary data and be carried out on password-protected university servers unless otherwise specified by agreements with data providers. Agreements with data providers will be adhered to – including use of secure data environments.

16. Blinding

Blinding is typically not feasible for natural experiments, e.g. if – as in this study - there are multiple control units per treated unit.

17. Potential bias

We will assess the robustness of our results in two ways: exposure falsification, i.e. assigning intervention status to a matched control area and outcome falsification, i.e. assessing an outcome measure that is unlikely to be affected by SL. At area-level we will use annual precipitation as a placebo outcome. Each LSOA unit will be assigned the annual precipitation value (mm) of the nearest 12-km grid node. At individual-level we will explore the variables available in the APS for potential placebo outcomes.

18. Analysis plan

Selection of treatment areas

We have identified all Lower layer Super Output Areas 2011 (LSOA11) with 5 to 100% coverage of Selective Licencing (SL) schemes initiated between 2007 and 2021. These data were obtained through FOI requests to local authorities who provided information of the geographical areas covered by SL schemes. These could be based on various geographies (e.g wards, LSOAs, streets or other bespoke geographies). We mapped these geographies to LSOAs to provide the % of area in each LSOAs covered by a SL scheme. This process inevitably resulted in some small slivers and to avoid analysing areas that were potentially unexposed, LSOA11 with <5% area coverage were ignored.

We distinguish between treated areas, matched control areas, other untreated areas, and exclusion areas. The exclusion areas are defined as street-based schemes, pre-treated areas (2007-11), and future treated areas (2020-21). In addition, we define exclusive spillover areas as an alternative treatment category (see below).

Street-based schemes were taken out of the treated group and put in a separate category as there is insufficient contextual information on streets for the analysis, e.g. from official data sources such as Census (Table 3). We encountered three local authorities with street-based schemes, two solely street-based schemes in London and a small street-based scheme in a local authority that otherwise predominantly operated area-based schemes, Sheffield. In the case of the former, we have chosen to classify the whole local authority as 'Street-based schemes'.

LSOA11 treated before 2012 were removed from the treatment category and put in a separate category due to lack of outcome data for the early years of SL. Similarly, LSOA that were newly treated after the end of the 2012-19 study period, i.e. 2020-21, were put in a category of 'Future treated 2020-21' (Figure 1, Table 2).

We track how small areas (LSOA) have been part of one or more SL schemes over time within a local authority. The SL intervention will be analysed by the first year each LSOA11 was treated and treatment allocation has been fixed for methodological reasons regardless of whether a scheme got renewed after the first five years or not. Where the proportion of the LSOA covered by an SL scheme varied over time, the highest area proportion an LSOA11 unit had been exposed over time is used.

In the feasibility study in Greater London, partially treated areas were ignored. In the national study, however, many schemes could not be mapped 1:1 with LSOA11 units. They were either too small and/or defined with incongruent area units. Table S1 (Appendix I) shows the distribution of treated units by scheme and area coverage categories. The fragmented areas are likely to be different from the areas with larger and intact schemes such as London and Liverpool. It could hence create a selection bias problem if these more fragmented areas were excluded. Including the fragmented LSOA11 units in the analysis (N=279), on the other hand, creates a measurement error problem (Figure S1). The main analytical approach will therefore be analysis of treated areas with 100% coverage accompanied by sensitivity analyses that gradually adds the more thinly covered areas until all areas with at least 5% coverage are included.

A total population of 2,807,326 (Census 2011) resided in the standardised LSOA11 areas across 64 SL schemes in 41 different local authorities (Table 2, Table S1). Within these schemes, 777,688 persons were living in private rental accommodation (Census 2011).

Table 2 Population estimates (Census 2011) and number of 2011 Lower Layer Super Output Area (LSOA11) units of Selective Licencing (SL) schemes in England 2012-19 by SL area coverage categories. The main analysis will be of the 5%-100% category as if fully treated. Sensitivity analyses will gradually deselect the more thinly covered areas until only fully treated areas are left, i.e. the '100% only' category.

Coverage	LSOA11 units	Population	Private rental tenants
5%-100%	1,635	2,807,326	777,688
50%-100%	1,506	2,586,069	712,661
100% only	1,356	2,329,792	637,541

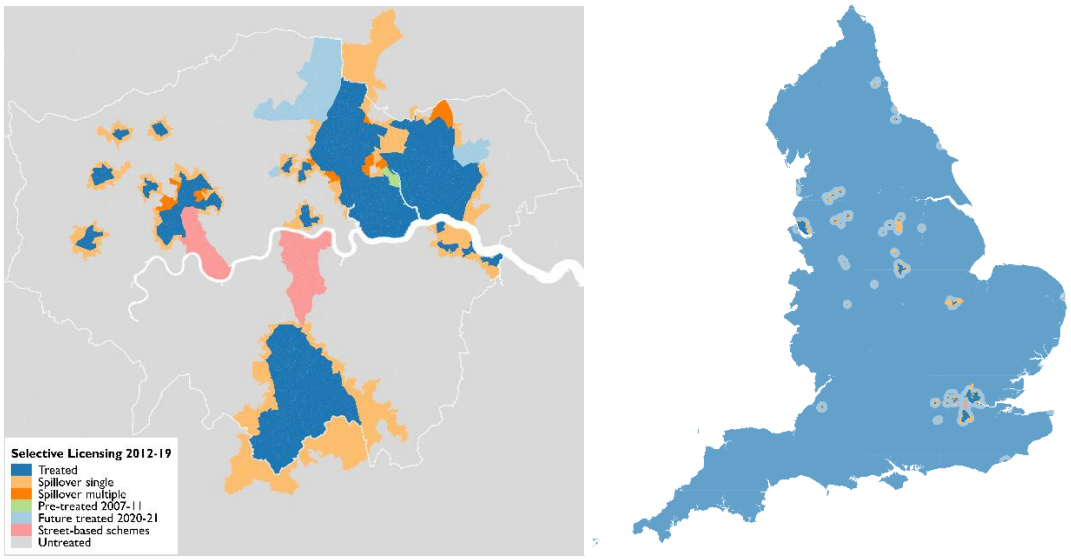

Figure 1 Selective Licencing treatment, spillovers, and exclusion areas (2012-19) for London (left panel) and England (right panel with 5km buffer to highlight smaller schemes).

Table 3 Selective Licencing schemes in England 2012-19. Number of standardised 2011 Lower Layer Super Output Area (LSOA11) by region. Regions were defined as 1) The North (North East, North West, Yorkshire and The Humber), 2) The South (East Midlands, East of England, South East, South West, West Midlands), and 3) Greater London.

Category	The North	The South	London	Total
Untreated	7,561	17,465	1,916	26,926
Treated	470	235	930	1,635
Matched control	967	581	1,271	2,819
Spillover single	293	233	271	797
Spillover multiple	28	3	15	46
Street-based schemes	2	-	279	281
Pre-treated 2007-11	94	7	9	110
Future treated 2020-21	56	14	144	214
Total	9,471	18,538	4,835	32,844

Definition of spillover areas

An area-based policy could have spillover effects, e.g. if tenants get forced out by increasing rents or a more ardent use of antisocial behaviour clauses. At the same time, landlords and letting agencies could potentially move their businesses away from SL areas to avoid regulation. To study such spillover effects, we have selected areas adjacent to treatment areas. We are acknowledge there are limitations with this definition of spillover – given that displacement of tenants (or landlord properties) may not necessarily be to those adjacent areas. However we have no means of tracking individual displacement events, so our use of adjacent areas represents what we believe to be the best way of assessing this issue that our data will allow. Nonetheless, we will make clear the limitations and clearly designate this part of our analysis as 'secondary.' For simplicity, spillover, and control areas have been defined as mutually exclusive.

Specifically, spillover areas were selected as LSOAs adjacent to treated LSOAs by the year they first gained spillover status. In the selection, treatment status would 'overrule' spillover status. This is particularly evident in London where schemes in some cases are adjacent to other schemes. The exclusion areas, i.e. street-based, 'pre-treated 2007-11', and 'future treated 2020-21' schemes, also

'overruled' potential spillover status. A small proportion of spillover areas gained that status more than once (N=46, Table 3). This is noted as a limitation but the multiple spillovers will pragmatically be included in the analysis of any spillover effects overall.

A set of spillover control areas will be selected following the same methodology as for the treatment control areas (see next section). All treated units were urban areas and only urban areas were included in the pool of control candidates for treated areas. Since some spillover areas were in fact rural, all areas were included in the pool of candidates for the spillover controls regardless of urbanity.

Selection of control areas

Only areas that were untreated and did not qualify for any of the exclusion criteria were entered into the pool of control candidates (Table 2). SL schemes were only introduced in urban areas and rural LSOA11 were accordingly excluded from the matching pool.

Variables (LSOA11-level) for matching were proportions of the population in receipt of income benefits, self-reported non-white ethnicity, and proportions of households from the private rental sector, living in housing in poor condition, overcrowded, or unaffordable, whether buildings were mainly built before 1945 (Table 4). Skewed variables were logarithmically transformed (natural logarithm) prior to matching.

Propensity score matching (PSM) with 1:3 controls and replacement within each super region was used for the treatment (Table 4, Table S4) and spillover units (Table S5-S6) [29].

Table 4 Baseline characteristics for Treated, Never-treated, and propensity score matched control areas (1 treated:3 controls) with replacement. Number of standardised 2011 Lower Layer Super Output Area (LSOA11).

A **Matching variables**	B **Treated**	C **Never-Treated**	D **Matched Controls**	E **Std. dif. (B-D)**
THE NORTH	N=470	N=7,255	N=1,410 (967 unique)	
Age 0-15 years	.18	.19	.18	.06
Age 16-59 years	.63	.58	.64	-.07
Ln(Income deprivation)	-1.42	-2.04	-1.47	.06
Ln(Private rental)	-1.58	-2.19	-1.57	-.02
Unaffordability	.93	-.53	.82	.06
Ln(Poor housing cond.)	-1.34	-1.62	-1.34	<-.01
Ln(Overcrowding)	-2.56	-3.09	-2.59	.05
Ln(Non-White)	-2.52	-3.18	-2.56	.04
Pre-1945 construction	.76	.45	.76	<.01
THE SOUTH	N=235	N=13,746	N=705 (581 unique)	
Age 0-15 years	.19	.19	.19	-.11
Age 16-59 years	.66	.58	.65	.10
Ln(Income deprivation)	-1.61	-2.24	-1.57	-.06
Ln(Private rental)	-1.37	-2.08	-1.42	.08
Unaffordability	1.30	-.11	1.21	.05
Ln(Poor housing cond.)	-1.26	-1.64	-1.28	.07
Ln(Overcrowding)	-2.10	-2.90	-2.15	.06
Ln(Non-White)	-1.48	-2.73	-1.47	-.01
Pre-1945 construction	.65	.35	.65	.01

LONDON	N=930	N=3,179	N=2,790 (1,271 unique)	
Age 0-15 years	.22	.19	.22	-.06
Age 16-59 years	.64	.65	.64	.07
Ln(Income deprivation)	-1.75	-2.12	-1.75	.01
Ln(Private rental)	-1.51	-1.63	-1.58	.11
Unaffordability	2.43	1.55	2.32	.08
Ln(Poor housing cond.)	-1.52	-1.53	-1.53	.03
Ln(Overcrowding)	-1.59	-1.87	-1.65	.08
Ln(Non-White)	-.72	-1.24	-.76	.06
Pre-1945 construction	.76	.70	.76	.01

Data sources: Indices of Multiple Deprivation 2015 [30]; Census 2011 [27]. Abbreviation: Households (hh), Standardised distance (Std. dif.). Regions were defined as The North, The South, and London.

Variable transformations

We will apply non-linear DID models [31] such as Poisson models for counts or, alternatively, using logit models and dichotomising the specific outcome variables. This way we can avoid the usual approach of log transforming outcome variables, which has implications for the parallel trend assumption [32]. Log transformation would require as-random treatment allocation (not our case) or checking that the outcome distribution is not affected by the intervention itself. Log transformation also affects the interpretation depending on the model, e.g. from measuring additive to multiplicative effects [33].

Sub-Group Analysis

We will repeat our analyses of individual- and area-level outcomes for the following subgroups:

- **Small versus larger SL schemes**. We propose this partly because the larger schemes have been politically contentious (following the change of government in 2024, changes were announced to make it easier for LAs to set up large schemes without needing national government permission), and partly because we hypothesise that resident displacement occurs less in larger schemes – particularly where the schemes extend across the whole local authority area. For this purpose, we will define larger schemes as those covering 90%+ of the Local Authority population (Census 2011): 2012_Newham, 2014_BarkingandDagenham, 2015_Croydon, 2015_Liverpool, 2015_WalthamForest. Small schemes will in this analysis be defined as those covering up to 20% of the population. Schemes of intermediate sizes will be ignored in this analysis.
- **London versus outside London.** Comparisons with London are useful because it has a unique housing market and some of its areas were settings for the Olympic Games (a potential confounder).

To examine impacts on equity, we will run the following area-level subgroup analyses by ranking local authorities:

- **Level of deprivation**: lower half (least deprived), upper half (most deprived)
- **Proportion of population from a Non-White ethnic background**: lower half (low proportion), upper half (high proportion)
- **Proportion of population under 16**: lower half (low proportion), upper half (high proportion)
- **Proportion of population over 75**: lower half (low proportion), upper half (high proportion)

APS includes items that are relevant to the following equity dimensions covered in the PROGRESS+ framework: Place of residence (which we operationalise as region); Ethnicity, Occupation, Gender, Religion, Education as well as participants who live on their own and participants with child dependents. Occupational status and education can be considered proxies for socio-economic status. Furthermore, the survey asks about long term and limiting health conditions, allowing us to consider these dimensions of disability. It also asks about cohabiting same sex partners. We will incorporate these dimensions into the equity analysis where sample sizes allow us to do so.

To address potential multiple testing issues with the secondary analyses, will adjust the significance tests according to a Benjamini–Hochberg False Discovery Rate of 5% [35].

Software

The analyses will be carried out in Stata (version 18 or later)[36] or R.

19. Handling of missing data

Missing data does not constitute a significant issue with the listed data sources and the analyses will be conducted as complete case only.

20. Handling of withdrawals and replacements

Not applicable.

21. Outcome

Primary and secondary outcomes are described in detail in Section 14.

22. Data confidentiality statement

Quantitative analyses will only involve de-identified secondary data and be carried out on password-protected university servers unless otherwise specified by agreements with data providers. Agreements with data providers will be adhered to – including use of secure data environments.

23. Follow-up

The quantitative part of the study will only involve de-identified data where follow-up is not possible.

26. Quality assurance

The quantitative data have been through quality assurance at the source. We will still carry out checks of data completeness, consistency, and outliers. Any changes will be justified and documented in syntax files.

Part D: ETHICAL CONSIDERATION

27. Ethical approval

The project has gone through the ethical approval process at London School of Hygiene & Tropical Medicine (LSHTM submission number: 29809). On obtaining approval from LSHTM, we will then follow ethics requirements for each of the other collaborating universities (Liverpool, Lancaster, Greenwich, Bristol).

The quantitative components of this study are based on routine data analysis so for this part there is no primary data collection. Ethical issues related to this part of the study focus on data privacy and security. We have access agreements with the following data providers: UK Data Service Secure Lab (Annual Population Survey - APS); NHS England / NHS Digital (Hospital Episode Statistics - HES); and Urban Big Data Centre, University of Glasgow (Zoopla data). These will be updated if necessary and adhered to.

We will conduct the quantitative analysis of Annual Population Survey data in ONS's Trusted Research Environment and only be able export results such as regression tables and graphs once these have been subjected to statistical disclosure control checks and cleared for release.

University of Liverpool's HES data sharing agreement (University of Liverpool - data sharing agreement DARS-NIC-16656-D9B5T-v6.1) allows members of the team based at Liverpool access to de-identified and aggregated HES data after a risk assessment within a trusted research environment. The Liverpool team will furthermore ensure that no patient will be indirectly identifiable in the outputs they produce (by e.g., suppressing small numbers or increasing the aggregation level).

Non-personal data from University of Glasgow Urban Big Data Centre (Zoopla property rental price data) will only be stored and handled on collaborating universities' secure servers. The analytical team will ensure that no individual address will be identifiable in the outputs they produce.

The other data we intend to use is publicly available, anonymous and does not pose a risk to individual confidentiality.

28. Consent and assent

The impact evaluation (WP1,3) is based on secondary data analysis with deidentified data and does thus not involve primary data collection.

29. Risk/harm to participants

The impact evaluation (WP1,3) is based on secondary data analysis with deidentified data under secure settings according to data owners' conditions. Residual risks and harms are considered minimal.

30. Adverse and serious adverse event reporting

Not applicable.

Part E: REPORTING AND DISSEMINATION

32. Dissemination/publication plan

Dissemination will include: website blog and twitter posting from NIHR School of Public Health Research (SPHR); mail out and presentation to the London Public Health and Housing Network; emails to personal/professional contacts at a number of government, charitable organisations, think tanks and research organisations including: UK Collaborative Centre for Housing Evidence (CaCHE); Institute for Health Equity, Centre for Homelessness Impact, Greater London Authority, local authority contacts; Cambridge House; CRISIS; Shelter, MEDACT, Centre for London, Local Government Association; Equal North; relevant national government departments. In our submitted response to the peer reviewers, we added that dissemination plans will include the National

Residential Landlords Association, Chartered Institute of Environmental Health and Chartered Institute of Housing. We will look for emergent opportunities such as government consultations. We will at minimum produce the following academic outputs: two articles for WP1 (individual and area level impacts), one for WP3. We anticipate at least 4 conference presentations.

Part F: OTHERS

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APPENDIX I

Table S1 Selective Licencing schemes 2012-19 by region and area coverage categories. Population estimates based on Census 2011.

Schemes	Number of LSOA11 units							Population
	5-19%	20-39%	40-59%	60-79%	80-99%	100% only	5-100%	
The North (N=32)								
2012_Blackpool	2			3	2		7	9,536
2012_Gateshead	2	2					4	6,580
2013_BlackburnwithDarwen		1		1	1		3	4,861
2013_Salford	3	1					4	7,097
2014_Blackpool	1				5		6	8,881
2014_Burnley	2	5			1		8	11,500
2015_Doncaster	1	3					4	6,898
2015_Hartlepool	2						2	2,738
2015_Liverpool						298	298	465,656
2015_Oldham						12	12	19,429
2015_Rotherham	3	3	1	2	4		13	22,464
2015_Salford	2	3	1		2		8	12,771
2015_Wirral						4	4	5,940
2016_Burnley	3	1					4	6,004
2016_Middlesbrough	1			1	1		3	4,596
2016_Oldham						4	4	7,573
2017_BlackburnwithDarwen	2	2					4	5,494
2017_Manchester		1					1	1,869
2017_Salford	1						1	3,699
2017_Scarborough	1	1		1			3	5,120
2018_Doncaster	2	1		1			4	5,510
2018_Gateshead	1						1	1,770
2018_Hyndburn	3	2	2	1	3		11	18,497
2018_Manchester	2	5	1		1		9	18,460
2018_Sefton						19	19	26,857
2018_Sheffield	9						9	17,213
2019_Blackpool	1				7		8	10,898
2019_Burnley	1		1	1			3	4,372
2019_Gateshead		1					1	1,763
2019_Middlesbrough	3		1		1		5	8,424
2019_Scarborough	1	1		1			3	4,993
2019_Wirral						4	4	6,394
Sub-total	49	33	7	12	28	341	470	743,857
The South (N=14)								
2012_Wolverhampton	2						2	3,040
2013_Bristol						3	3	7,071
2014_Stoke-on-Trent	2	2					4	5,841
2015_Hastings						22	22	37,190
2016_Bristol						14	14	24,352
2016_Peterborough						22	22	42,604
2016_WestLindsey	1			1			2	2,965
2017_Ashfield	1	2					3	5,039
2017_EastStaffordshire						1	1	1,701
2018_Gedling	1	1	1	1	1		5	7,841
2018_Nottingham	6	10	6	8	14	91	135	228,736
2018_Woking	1	1		1	1		4	7,643
2019_GreatYarmouth	1				3		4	7,069
2019_Slough	3	3	1	2	3	2	14	28,356
Sub-total	18	19	8	13	22	155	235	409448
London (N=15)								
2012_Newham						155	155	293,736
2014_BarkingandDagenham						110	110	187,029
2015_Brent						23	23	47,632
2015_Croydon						220	220	364,815
2015_Harrow						7	7	11,756
2015_WalthamForest						144	144	259,742
2016_Harrow						6	6	11,483
2016_TowerHamlets						22	22	38,717
2017_Ealing						43	43	77,337
2017_Redbridge					2	14	16	29,203
2018_Bexley					11	2	13	23,798
2018_Brent					4	39	43	78,160
2018_Hackney				1		21	22	37,890
2018_Harrow						14	14	24,646
2018_Redbridge	1				51	40	92	168,077
Sub-total	1	0	0	1	68	860	930	1,654,021
ENGLAND TOTAL	68	52	15	26	118	1,356	1,635	2,807,326

Table S2 Selective Licencing Master file variables.

Number	Variable	Type	Value label	Variable label
1	lsoa11	str9		LSOA11CD
2	la_name	str22		LA_NAME
3	tyear	float		Year first treated
4	cat	float	cat	SL classification
5	lsoa_area_pc	float		(max) lsoa_area_pc
6	gor	str9		Government Region (Aug 2016)
7	oslauanm	str36		Local Authority name (Aug 2016)
8	nsl	float	nsl	Super Region: North/South/London
9	lat	float		Mean latitude of live small user postcodes (Aug 2016)
10	lon	float		Mean longitude of live small user postcodes (Aug 2016)
11	tla	str24		SL scheme 20YY+LA Name
12	cover	float	cover	Treatment area coverage category
13	syear2012	float		Spillover 2012
14	syear2013	float		Spillover 2013
15	syear2014	float		Spillover 2014
16	syear2015	float		Spillover 2015
17	syear2016	float		Spillover 2016
18	syear2017	float		Spillover 2017
19	syear2018	float		Spillover 2018
20	syear2019	float		Spillover 2019
21	nspill	float		Number of times a spillover
22	syear	float		Year first spillover
23	age0_15	float		Aged 0-15yr (2011)
24	age16_59	float		Aged 16-59yr (2011)
25	age75	float		Aged 75+yr (2011)
26	incomescorerate	float		Income Score (rate)
27	hh_privaterent	float		Household tenure: private rented (Census2011)
28	hh_poorcond	double		Housing in poor condition indicator
29	hh_overcrowd	double		Household overcrowding indicator
30	hh_afford	double		Housing affordability indicator
31	nonwhite	float		Non-White (Census2011)
32	prewar	float		Built before 1945 (most frequent)
33	urban	float		Urban 2011 (DEFRA)
34	cfw	float		Matched treatment control frequency weight
35	scfw	byte		Matched spillover control frequency weight
36	treated	float		Treatment allocation
37	prspop11	int		Census 2011 private rental sector tenants
38	pop11	int		Census 2011 usual residents
39	tla_p	float		Scheme population% (Census 2011)

Value label cat
- 0 Untreated
- 1 Treated
- 2 Matched control
- 8 Spillover single
- 9 Spillover multiple
- 97 Street-based schemes
- 98 Pre-treated 2007-11
- 99 Future treated 2020-21

Value label nsl
- 1 North
- 2 South
- 3 London

Value label cover
- 5 5-49%
- 50 50-99%
- 100 100%

Table S3 Sequential imputation of missing dwelling construction era information by 2011 Lower layer Super Output Area (LSOA11). Missing values replaced with the non-missing value of the geodetically nearest neighbour (1-4 neighbours).

Category	LSOA11			Mean distance (km)
	N	%	Cumulative %	
Non-missing singular	30840	93.90	93.90	N/A
Non-missing with ties	970	2.95	96.85	N/A
1st neighbour imputed	884	2.69	99.54	0.83
2nd neighbour imputed	137	0.42	99.96	1.06
3rd neighbour imputed	10	0.03	99.99	0.78
4th neighbour imputed	3	0.01	100	0.88
Total	32,844	100		

Table S4 Selective Licencing treatment propensity-score matching logit regression output by super-region.

Covariates	Individual co-variates			Multiple co-variates		
	OR	P	95% CI	OR	P	95% CI
THE NORTH; Treated units=470; N=7,725; Pseudo-R^2=.236						
Age 0-15 years	**-2.08**	.031	-3.97;-.19	**-12.15**	<.001	-14.97;-9.53
Age 16-59 years	**6.44**	<.001	5.52;7.37	**6.59**	<.001	4.38;8.85
Ln(Income deprivation)	**1.28**	<.001	1.12;1.44	**2.47**	<.001	2.15;2.83
Ln(Private rental)	**1.28**	<.001	1.14;1.42	.39	.004	.12;.65
Unaffordability	**.36**	<.001	.31;.42	.02	.739	-.10;.14
Ln(Poor housing cond.)	**2.19**	<.001	1.91;2.47	**.84**	.001	.34;1.25
Ln(Overcrowding)	**.89**	<.001	.76;1.01	**-1.44**	<.001	-1.75;-1.12
Ln(Non-White)	**.46**	<.001	.39;.54	**.54**	<.001	.41;.67
Pre-1945 construction	**1.36**	<.001	1.14;1.57	**.59**	<.001	.31;.87
THE SOUTH; Treated units=235; N=13,981; Pseudo-R^2=.259						
Age 0-15 years	-1.13	.409	-3.81;1.55	**-6.74**	<.001	-10.24;-3.24
Age 16-59 years	**9.49**	<.001	8.31;10.68	**6.44**	<.001	3.50;9.38
Ln(Income deprivation)	**1.42**	<.001	1.19;1.64	**2.18**	<.001	1.77;2.59
Ln(Private rental)	**1.71**	<.001	1.49;1.92	**1.19**	<.001	.76;1.61
Unaffordability	**.46**	<.001	.38;.53	-.10	.177	-.25;.05
Ln(Poor housing cond.)	**2.68**	<.001	2.31;3.05	.16	.576	-.40;.71
Ln(Overcrowding)	**1.42**	<.001	1.23;1.60	**-1.19**	<.001	-1.60;-.78
Ln(Non-White)	**1.15**	<.001	1.01;1.28	**1.02**	<.001	.83;1.22
Pre-1945 construction	**1.23**	<.001	.96;1.50	-.07	.702	-.43;.29
LONDON; Treated units=930; N=4,109; Pseudo-R^2=.196						
Age 0-15 years	**13.82**	<.001	12.15;15.50	**10.80**	<.001	8.05;13.54
Age 16-59 years	-.65	.208	-1.67;.36	2.00	.152	-.73;4.73
Ln(Income deprivation)	**.97**	<.001	.84;1.10	**1.14**	<.001	.84;1.44
Ln(Private rental)	**.31**	<.001	.19;.42	**.78**	<.001	.51;1.05
Unaffordability	**.38**	<.001	.33;.43	**-.14**	.013	-.25;-.03
Ln(Poor housing cond.)	.09	.555	-.20;.37	.46	.059	-.02;.93
Ln(Overcrowding)	**.57**	<.001	.46;.69	**-1.68**	<.001	-2.07;-1.29
Ln(Non-White)	**1.86**	<.001	1.69;2.04	**2.19**	<.001	1.92;2.47
Pre-1945 construction	**.29**	.001	.13;.46	**.43**	<.001	.22;.64

Table S5 Baseline characteristics for Spillovers, Never-treated, and propensity score matched control areas (1 treated:3 controls) with replacement. Number of standardised 2011 Lower Layer Super Output Area (LSOA11). Balance was tested with a Wilcoxon rank-sum test (t-test for comparison). Binary outcome (Pre-1945 construction) tested with a Chi-square test.

A Matching variables	B Spillovers	C Never-Treated	D Matched Controls	E Std. dif. (B-D)
THE NORTH	N=321	N=8,528	N=963 (863 unique)	
Age 0-15 years	.20	.18	.20	.01
Age 16-59 years	.60	.58	.60	.02
Ln(Income deprivation)	-1.63	-2.11	-1.65	.02
Ln(Private rental)	-1.88	-2.20	-1.91	.06
Unaffordability	.32	-.63	.22	.04
Ln(Poor housing cond.)	-1.43	-1.57	-1.43	<.01

Ln(Overcrowding)	-2.74	-3.18	-2.77	.03
Ln(Non-White)	-2.67	-3.35	-2.67	.01
Pre-1945 construction	.67	.47	.66	.01
THE SOUTH	N=236	N=18,046	N=750 (665 unique)	
Age 0-15 years	.20	.19	.20	-.04
Age 16-59 years	.59	.57	.60	-.07
Ln(Income deprivation)	-2.02	-2.33	-1.97	-.07
Ln(Private rental)	-1.90	-2.11	-1.90	.01
Unaffordability	.05	-.25	.19	-.07
Ln(Poor housing cond.)	-1.55	-1.53	-1.55	<-.01
Ln(Overcrowding)	-2.74	-3.05	-2.66	-.10
Ln(Non-White)	-2.08	-3.01	-2.03	-.05
Pre-1945 construction	.43	.40	.43	<.01
LONDON	N=286	N=3,187	N=858 (666 unique)	
Age 0-15 years	.21	.19	.21	.02
Age 16-59 years	.63	.64	.63	.02
Ln(Income deprivation)	-1.79	-2.12	-1.81	.03
Ln(Private rental)	-1.64	-1.63	-1.64	<-.01
Unaffordability	2.34	1.55	2.33	.01
Ln(Poor housing cond.)	-1.57	-1.53	-1.57	<-.01
Ln(Overcrowding)	-1.67	-1.87	-1.68	<.01
Ln(Non-White)	-.85	-1.25	-.84	-.01
Pre-1945 construction	.66	.70	.66	.01

Data sources: Indices of Multiple Deprivation 2015 [30]; Census 2011 [27]. Abbreviation: Households (hh), Standardised distance (Std. dif.). Regions were defined as The North, The South, and London.

Table S6 Selective Licencing spillover propensity-score matching logit regression output by super-region.

Covariates	**Individual co-variates**			**Multiple co-variates**		
	OR	**P**	**95% CI**	**OR**	**P**	**95% CI**
			THE NORTH; Spillover units=321; N=8,849; Pseudo-R^2=.090			
Age 0-15 years	**5.55**	<.001	3.36;7.75	-1.89	.210	-4.84;1.07
Age 16-59 years	**4.41**	<.001	3.22;5.60	**4.82**	<.001	2.33;7.30
Ln(Income deprivation)	**.86**	<.001	.69;1.03	**1.52**	<.001	1.17;1.87
Ln(Private rental)	**.74**	<.001	.58;.91	.03	.837	-.26;.32
Unaffordability	**.23**	<.001	.18;.29	**-.19**	.002	-.31;-.07
Ln(Poor housing cond.)	**.78**	<.001	.52;1.04	**.53**	.020	.08;.97
Ln(Overcrowding)	**.70**	<.001	.56;.84	**-.46**	.007	-.80;-.13
Ln(Non-White)	**.45**	<.001	.36;.53	**.40**	<.001	.27;.54
Pre-1945 construction	**.82**	<.001	.58;1.05	**.36**	.016	.07;.65
			THE SOUTH; Spillover units=236; N=18,282; Pseudo-R^2=.091			
Age 0-15 years	**5.28**	<.001	2.58;7.98	-3.54	.026	-6.64;-.43
Age 16-59 years	**3.63**	<.001	210;5.17	**-2.99**	.032	-5.73;-.25
Ln(Income deprivation)	**.63**	<.001	.44;.82	**1.18**	<.001	.84;1.53
Ln(Private rental)	**.56**	<.001	.35;.77	**.67**	<.001	.32;1.02
Unaffordability	**.11**	.005	.03;.19	**-.31**	<.001	-.44;-.17
Ln(Poor housing cond.)	-.06	.667	-.34;.22	-.42	.052	-.85;>.01
Ln(Overcrowding)	**.51**	<.001	.35;.68	**-.51**	.007	-.89;-.14
Ln(Non-White)	**.73**	<.001	.61;.84	**.92**	<.001	.76;1.07
Pre-1945 construction	.15	.269	-.11;.41	.04	.903	-.36;.32
			LONDON; Spillover units=286; N=3,473; Pseudo-R^2=.086			
Age 0-15 years	**8.75**	<.001	6.28;11.23	.70	.728	-3.23;4.63
Age 16-59 years	**-1.88**	.028	-3.55;-.20	-3.66	.058	-7.43;.12
Ln(Income deprivation)	**.80**	<.001	.59;1.00	**.98**	<.001	.54;1.42
Ln(Private rental)	-.01	.908	-.20;.18	**.49**	.016	.09;.88
Unaffordability	**.32**	<.001	.24;.40	-.09	.277	-.26;.08
Ln(Poor housing cond.)	**-.66**	.009	-1.15;-.17	-.49	.193	-1.22;.25
Ln(Overcrowding)	**.39**	<.001	.21;.57	**-.78**	.007	-1.35;-.22
Ln(Non-White)	**1.29**	<.001	1.04;1.55	**1.43**	<.001	1.05;1.81
Pre-1945 construction	-.18	.160	-.44;.07	.12	.428	-.18;.43

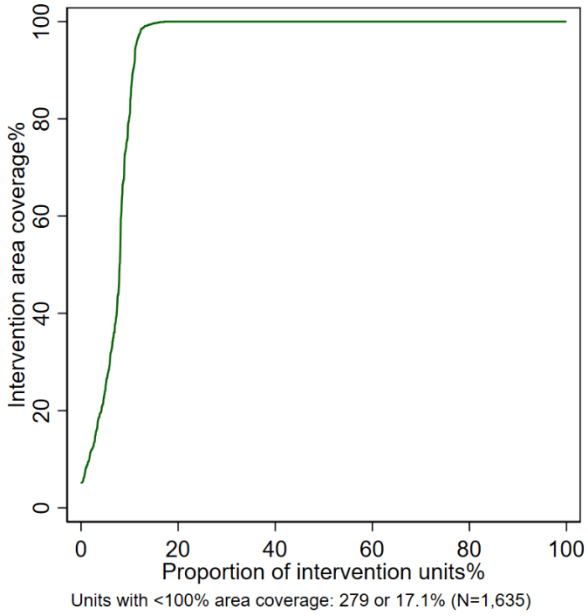

Figure S1 Selective Licensing Intervention units 2012-19 (2011 Lower layer Super Output Area) ordered according to intervention area coverage.

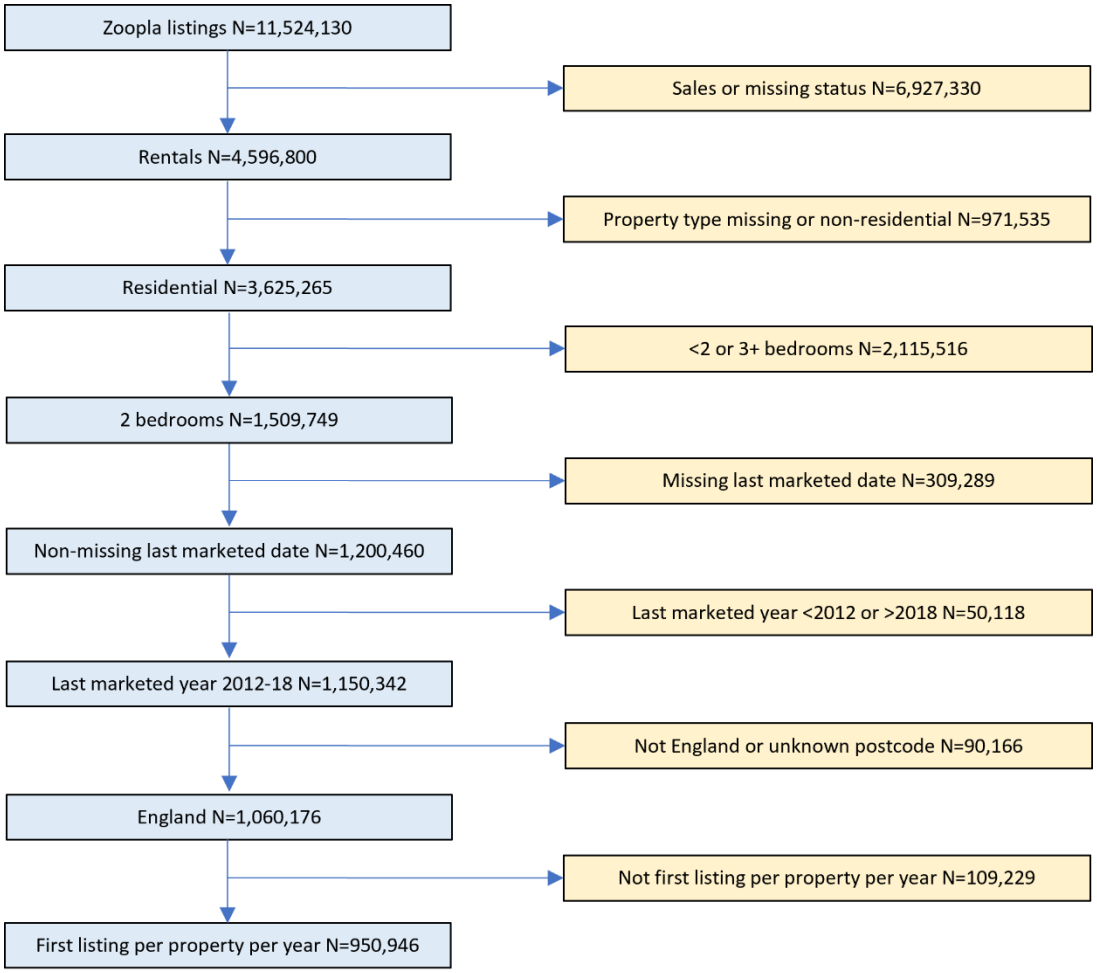

Figure S2 Flow diagram for Zoopla Generation 1 data. Non-residential categories: Business park, Chalet, Equestrian property, Hotel/guest house, Houseboat, Industrial, Land, Leisure/hospitality, Light industrial, Mobile/park home, Office, Parking/garage, Pub/bar, Restaurant/café, Retail premises, Warehouse.