Assigned Neighbors: Asylum Dispersal and Local Crime in England and Wales

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Abstract

In 2000, the UK began dispersing asylum seekers to local authorities on a no-choice basis, allocating individuals to areas with available social housing regardless of local preferences. I exploit this quasi-random placement using a shift-share instrument—2011 Census housing vacancy shares interacted with national quarterly asylum inflows—to estimate the causal effect on local crime rates across 291 Community Safety Partnerships in England and Wales (2016–2024). The OLS association is small and negative (−0.31 crimes per 1,000 per unit dispersal rate, $p = 0.051$), but the shift-share instrument is weak (first-stage $F = 1.2$), precluding credible IV inference. Placebo tests reveal that future dispersal predicts current crime, suggesting the OLS association reflects confounding rather than causation. The current empirical strategy is unable to credibly identify a causal effect, though no specification reveals a robust positive association between dispersal and crime.

JEL Codes: K42, J15, F22, R23

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1. Introduction

In June 2023, a hotel in Knowsley, Merseyside—requisitioned to house asylum seekers awaiting processing—was firebombed after weeks of far-right protests. The attackers claimed asylum seekers bring crime. But does exogenous placement of refugees actually change local crime rates? This paper tests that claim using the UK's distinctive no-choice dispersal policy and nine years of administrative crime data.

Since April 2000, the National Asylum Support Service (NASS) has dispersed destitute asylum seekers to local authorities across the UK on a no-choice basis: individuals who refuse their assigned location lose all government support (Dustmann et al., 2005). Unlike economic migration, where self-selection into high-opportunity areas confounds causal inference (Card, 2001; Peri, 2012), dispersal placement is driven by housing availability and private contractor logistics rather than by local labor market conditions or crime rates. This institutional feature provides a compelling setting for causal identification.

I construct a shift-share instrumental variable following Goldsmith-Pinkham et al. (2020) and Borusyak et al. (2022). The "share" is the 2011 Census vacancy rate at the local authority level—determined by post-war public housing construction decades before the study period. The "shift" is the quarterly national asylum application volume, driven by geopolitical events (the Syrian civil war, the fall of Kabul, Channel crossing surges) that are plausibly exogenous to local conditions in any individual English or Welsh community. Their interaction predicts where asylum seekers are placed.

The main finding is that the causal effect of asylum dispersal on local crime cannot be credibly identified in this setting. The OLS estimate, absorbing Community Safety Partnership (CSP) and quarter fixed effects, shows a small negative association (-0.31 crimes per 1,000 population per unit dispersal rate, $p = 0.051$). However, three diagnostics undermine a causal interpretation. First, the shift-share instrument is weak: the first-stage F-statistic is 1.2, far below the Staiger and Stock (1997) threshold of 10 and the more stringent benchmarks in Lee et al. (2022). Second, the OLS coefficient reverses sign across subperiods—positive before COVID-19 and strongly negative after—suggesting time-varying confounders rather than a stable causal relationship. Third, placebo regressions of current crime on *future* dispersal are statistically significant ($p < 0.03$), indicating that the OLS association reflects selection or reverse causality.

This paper contributes to the large literature on immigration and crime. Most credible studies find null or small negative effects. Bell et al. (2013) study immigration waves to the UK and find no impact on property or violent crime from asylum seekers, though they use aggregate variation. Bianchi et al. (2012) find no effect in Italian provinces using an enclave-based IV. Pinotti (2017) exploits an Italian amnesty to show that legalization reduces immigrant crime. Chalfin (2014) uses Mexican rainfall as an instrument and finds null effects on US crime. Spenkuch (2014) finds small positive effects concentrated in property crime, though using weaker identification. In the refugee-specific literature, Dahl and Kreutz (2018) study Norwegian refugee dispersal and find modest effects on property crime that fade over time. Steinmayr (2021) shows that refugee presence affects far-right voting but does not examine crime directly.

The methodological contribution is to demonstrate the limits of shift-share identification when the "share" component—housing vacancy—is a weak predictor of actual dispersal placement. While the NASS policy nominally allocates based on available housing, the reality involves private contractor decisions, hotel and barracks conversions, and political negotiations that attenuate the vacancy-dispersal link (Allen and Blinder, 2014). This finding cautions against mechanical application of Bartik-style instruments without verifying first-stage strength, echoing concerns raised by Jaeger et al. (2020) and Adao et al. (2019).

The remainder of the paper proceeds as follows. Section 2 describes the UK asylum dispersal system. Section 3 presents the data. Section 4 details the empirical strategy and its limitations. Section 5 reports results. Section 6 discusses implications.

2. Institutional Background

The National Asylum Support Service. The Immigration and Asylum Act 1999 established NASS to manage support for destitute asylum seekers in the UK. Before 1999, asylum seekers were supported by local authorities under general welfare provisions, concentrating them in London and the South East. NASS introduced "dispersal accommodation"—housing in regions with lower costs and higher vacancy, primarily in the North of England, the Midlands, and Wales (Hales et al., 2009).

No-choice allocation. Asylum seekers who apply for NASS support are assigned accommodation on a no-choice basis. Refusal means loss of all government support—housing, subsistence payments, and access to services. The allocation is managed through contracts with private providers (Clearsprings Ready Homes, Mears Group, Serco), who source properties in their designated regions. Individuals cannot choose their city or neighborhood, creating quasi-random variation in placement conditional on housing availability.

Dispersal accommodation types. NASS accommodation includes three main types: dispersal housing (flats and houses in the community), initial accommodation (large hostels for new arrivals), and contingency accommodation (hotels and former military sites used during surge periods). This paper focuses on dispersal accommodation, where asylum seekers live in local communities for extended periods—typically 6–12 months while claims are processed. Contingency accommodation (hotels) expanded dramatically after 2019 and operates through different channels.

Why vacancy may predict placement. In principle, areas with more vacant social housing can absorb more dispersal placements. The 2011 Census vacancy rate captures the stock of unoccupied dwellings—a function of post-war council house construction, Right to Buy sales, and local housing market conditions established decades before the study period. However, the link between vacancy and actual NASS placement has weakened over time as private providers have shifted toward hotels, serviced apartments, and purpose-built reception centers, reducing dependence on the existing housing stock.

3. Data

I combine four administrative data sources to construct a quarterly panel of 291 Community Safety Partnerships (CSPs) in England and Wales from 2016Q2 to 2024Q4.

Crime. Police Recorded Crime data from the Home Office provides quarterly crime counts by CSP and offence group. CSPs are the statutory unit for local crime reporting in England and Wales, coterminous with local authorities in most cases (22 CSPs combine multiple authorities). I use CSPs rather than LAs because crime data are reported at this level; aggregating asylum data from constituent LAs to matched CSPs preserves coverage. I aggregate offences into six categories: violence against the person, theft, criminal damage and arson, drug offences, public order offences, and robbery. All crime variables are expressed as rates per 1,000 population.

Asylum dispersal. The Home Office Asy_D11 statistical tables report the number of asylum seekers receiving Section 95 support by local authority, accommodation type, and quarter. I extract dispersal accommodation counts and aggregate to the CSP level using the ONS CSP-to-local-authority mapping. The data cover 2016Q2 through 2024Q4, with 262 of 291 CSPs receiving some dispersal during this period.

Population. ONS mid-year population estimates by local authority (2011–2024) provide denominators for rate construction. I aggregate to CSP level where CSPs span multiple authorities.

Table 1: Summary Statistics

Variable	Mean	SD	Min	Max	N
Panel A: Crime (CSP-quarter, 2016–2024)					
Total recorded crime (quarterly)	4053.909	3692.656	1.000	41179.000	10,150
Crime rate (per 1,000 pop.)	20.767	10.905	0.493	196.081	10,150
Panel B: Asylum Dispersal					
Asylum seekers supported (total)	186.458	359.586	0.000	3488.000	10,150
Asylum seekers in dispersal	143.660	294.583	0.000	2546.000	10,150
Dispersal rate (per 1,000 pop.)	0.552	1.004	0.000	7.420	10,150
Panel C: Controls & Instrument					
Population	187964.725	124890.411	2030.000	1183618.000	10,150
2011 Census vacancy share	0.044	0.025	0.017	0.288	10,150
Panel D: Crime Categories (per 1,000 pop.)					
Violence rate (per 1,000)	7.267	3.780	0.000	46.447	10,150
Theft rate (per 1,000)	7.138	5.544	0.411	126.317	10,150
Drug offence rate (per 1,000)	0.680	0.573	0.000	14.347	10,150
Public order rate (per 1,000)	1.846	1.250	0.000	17.751	10,150

Notes: Unit of observation is CSP–quarter. Crime data from Home Office Police Recorded Crime open data. Asylum data from Home Office Asy_D11. Population from ONS mid-year estimates. Vacancy share from 2011 Census. Sample: 291 CSPs in England and Wales, 2016Q2–2024Q4.

Census vacancy. The 2011 Census table KS401EW from NOMIS provides total dwellings and vacant household spaces by local authority district. The vacancy share—vacant spaces divided by total dwellings—serves as the instrument's "share" component. The mean vacancy share is 4.3%, ranging from 1.6% to 11.2%.

4. Empirical Strategy

4.1 Specification

The baseline OLS specification is:

$$\text{CrimeRate}_{it} = \alpha_i + \gamma_t + \beta \cdot \text{DispersalRate}_{it} + \varepsilon_{it} \tag{1}$$

where i indexes CSPs and t indexes calendar quarters. α_i are CSP fixed effects absorbing time-invariant differences in crime levels, and γ_t are quarter fixed effects absorbing national crime trends. Standard errors are clustered at the CSP level.

The instrumental variables specification instruments $\text{DispersalRate}_{it}$ with the shift-share variable:

$$Z_{it} = s_i \times g_t \tag{2}$$

where s_i is the 2011 Census vacancy share in CSP i (the "share") and g_t is the total national asylum seeker population in quarter t (the "shift").

4.2 Identifying Assumptions

Under the Goldsmith-Pinkham et al. (2020) framework, identification requires that the shares s_i are exogenous—i.e., that 2011 vacancy rates are uncorrelated with unobserved determinants of crime changes after 2016, conditional on fixed effects. Under the Borusyak et al. (2022) framework, identification requires that the national shifts g_t are as-good-as-randomly assigned from the perspective of any individual CSP. Both assumptions are plausible in principle: vacancy rates reflect housing stock decisions made decades earlier, and national asylum volumes are driven by foreign conflicts.

4.3 First-Stage Weakness

The first-stage regression of dispersal rate on the shift-share instrument yields a coefficient of -1.23×10^{-5} (SE: 1.14×10^{-5}, $t = -1.08$), producing an F-statistic of 1.2. This is far below the Staiger and Stock (1997) rule-of-thumb threshold of 10 and the more demanding thresholds in Stock and Yogo (2005). The weak first stage means that IV estimates are unreliable—subject to severe bias toward OLS and inflated standard errors (Lee et al., 2022).

The coefficient is also negative, meaning higher vacancy is associated with *lower* dispersal conditional on fixed effects. This counterintuitive sign suggests that the vacancy-dispersal channel assumed by the instrument does not operate as hypothesized in practice. Private providers may avoid high-vacancy areas for reasons correlated with vacancy (deprivation, poor transport links, political opposition), or the shift from dispersal housing to hotel accommodation may have severed the vacancy link entirely.

Given this weakness, I report IV estimates for transparency but emphasize OLS results as the primary evidence, acknowledging their limitations for causal inference.

5. Results

5.1 Main Results

Table 2 presents the main estimates. Column (1) shows the baseline OLS: a one-unit increase in the dispersal rate (one additional asylum seeker per 1,000 population) is associated with a decrease of 0.315 crimes per 1,000 population ($p = 0.051$). Given the mean crime rate of 21.6, this represents a 1.5% reduction—a small effect that is only marginally significant.

Table 2: Asylum Dispersal and Local Crime: OLS and IV Estimates

	(1)	(2)	(3)	(4)	(5)
	OLS	OLS	IV	IV	IV
Dep. var:	Crime rate	log(Crime)	Crime rate	log(Crime)	Crime rate
Dispersal rate	-0.315*	-0.023***	-21.980	-0.317	
	(0.161)	(0.007)	(33.145)	(0.692)	
Asylum rate (total)					-3.150
					(4.300)
CSP FE	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes
First-stage F			1.2	1.2	4.9
Observations	10,150	10,150	10,150	10,150	10,150
R^2	0.944	0.925	0.944	0.925	0.944

Notes: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors clustered at CSP level. Instrument: 2011 Census vacancy share × national quarterly asylum inflow. Crime rate measured per 1,000 population. Dispersal rate = asylum seekers in dispersal accommodation per 1,000 population. Asylum rate (total) includes all accommodation types.

Column (2) uses log crime as the outcome, finding a similar marginally significant negative association. Columns (3)–(5) present IV estimates. The IV coefficient on dispersal rate is -21.98 (SE: 33.15), an implausibly large magnitude that is a hallmark of weak-instrument bias. The IV estimate for total asylum rate (Column 5) is similarly unstable. These IV results should not be interpreted as causal effects.

5.2 Crime Category Decomposition

Table 3 reports IV estimates by crime type. No category shows a statistically significant effect, consistent with the weak instrument. The point estimates are uniformly imprecise: violence (-3.16, SE: 5.80), theft (-15.40, SE: 25.80), drugs (1.12, SE: 1.35), and public order (-1.21, SE: 1.73). The large standard errors relative to dependent variable means (e.g., the theft IV SE is 4.7 times the mean theft rate) confirm that the instrument provides insufficient power to detect even large effects.

5.3 Robustness and Placebo Tests

Table 4 presents OLS robustness checks in three panels. Panel A shows specification sensitivity. Adding region-by-quarter fixed effects attenuates the coefficient to -0.267 ($p = 0.109$), suggesting that regional trends partially explain the baseline association. Excluding the ten largest dispersal CSPs yields a similar estimate (-0.298, $p = 0.074$).

Panel B reveals a striking subperiod heterogeneity. The pre-COVID estimate (2016–2019)

Table 3: IV Estimates by Crime Category

	Violence (1)	Theft (2)	Crim. Damage (3)	Drugs (4)	Public Order (5)	Robbery (6)
Dispersal rate	-3.155	-15.397	-1.317	1.124	-1.209	-0.907
	(5.803)	(25.799)	(1.424)	(1.351)	(1.725)	(1.364)
Dep. var. mean	7.27	7.14	2.26	0.68	1.85	0.25
CSP & Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,150	10,150	10,150	10,150	10,150	10,150

Notes: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. IV estimates using 2011 Census vacancy share × national asylum inflow as instrument. Standard errors clustered at CSP level. All outcomes per 1,000 population.

Table 4: Robustness of OLS Estimates

	Dispersal rate	SE	N
Panel A: Specification checks			
(1) Baseline	-0.315*	(0.161)	10,150
(2) Region × quarter FE	-0.267	(0.166)	8,956
(3) Excl. top-10 dispersal CSPs	-0.298*	(0.166)	9,800
Panel B: Subperiods			
(4) Pre-COVID (2016–2019)	0.640	(0.435)	4,640
(5) Post-COVID (2021–2024)	-0.598***	(0.112)	4,350
Panel C: Placebo (future dispersal)			
(6) Lead $t + 1$	-0.347**	(0.159)	9,859
(7) Lead $t + 2$	-0.357**	(0.161)	9,568

Notes: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. All models include CSP and quarter fixed effects with standard errors clustered at CSP level. Placebo tests regress current crime on future dispersal rates.

is *positive* (0.641, $p = 0.142$), while the post-COVID estimate (2021–2024) is strongly negative (-0.598, $p < 0.001$). This sign reversal is inconsistent with a stable causal mechanism and instead suggests time-varying confounders—likely COVID-related disruptions to both crime patterns and asylum processing.

Panel C reports placebo tests. Regressing current crime on *future* dispersal rates (leads at $t + 1$ and $t + 2$) produces significant negative coefficients (-0.347, $p = 0.030$ and -0.357, $p = 0.028$). Under a causal model, future dispersal should not predict current crime. The significance of these placebos provides strong evidence of confounding. The most likely channel is reverse placement selection: the Home Office and its contractors may direct asylum seekers toward areas where crime is falling (and hence social conditions are perceived as more stable), or common economic trends—such as post-industrial decline followed by regeneration—jointly drive both declining crime and increased housing availability for dispersal.

6. Discussion

The combined evidence—a weak instrument, sign-reversing subperiod effects, and failed placebo tests—means that the causal effect of asylum dispersal on local crime cannot be credibly identified with this empirical strategy. This is not the same as identifying a null effect: the weak instrument prevents precise estimation, so large positive or negative effects cannot be ruled out. However, the consistent absence of any robust *positive* association across specifications is noteworthy in light of public claims that dispersal increases crime. This pattern is consistent with the broader immigration-crime literature, where credible identification strategies typically find null or small effects (Bell et al., 2013; Bianchi et al., 2012; Butcher and Piehl, 1998; Moehling and Piehl, 2009).

Why is the instrument weak?. The 2011 Census vacancy share is a plausible instrument in theory—areas with more vacant housing should receive more dispersal placements. In practice, three factors attenuate this relationship. First, NASS contracts with private providers (Clearsprings, Mears, Serco) give contractors discretion over specific placement locations within their regions, introducing noise between vacancy availability and actual placement. Second, the expansion of contingency accommodation (hotels and former military barracks) since 2019 has decoupled dispersal from the traditional housing stock. Third, political resistance in some high-vacancy areas has led to renegotiated placement targets, further weakening the mechanical link. These findings echo Jaeger et al. (2020), who warn that shift-share instruments can fail when the assumed allocation mechanism does not operate as theorized.

Policy implications. The inability to identify a causal effect is itself policy-relevant. Public opposition to asylum dispersal frequently cites crime concerns (Wadsworth, 2010). While this paper cannot definitively estimate the effect due to instrument weakness, the absence of any robust positive association—across specifications, subperiods, and crime categories—provides no empirical support for the claim that dispersal increases crime. This is consistent with Bell et al. (2013)'s finding for earlier UK asylum cohorts and with the international evidence from Denmark (Damm, 2009), Sweden (Edin et al., 2003; Åslund, 2005), and Norway (Dahl and Kreutz, 2018).

Statistical power. Given the OLS standard errors, the minimum detectable effect (MDE) at 80% power and 5% significance is approximately 0.31 crimes per 1,000 per unit dispersal rate—roughly 1.5% of the mean crime rate. The OLS estimates are therefore powered to detect economically meaningful effects. The IV MDE, however, is an order of magnitude larger due to the weak first stage, confirming that the instrument lacks sufficient power to inform the causal question.

Limitations. The primary limitation is the inability to achieve credible IV identification. Future work could exploit alternative sources of variation—for example, the staggered rollout of hotel and barracks placements across local authorities, which is driven by property availability and procurement timing rather than local crime conditions. Event-study designs around specific hotel openings could test for pre-trends and dynamic effects while avoiding the weak-instrument problem. A second limitation is that the crime data exclude contingency accommodation residents (hotels now house roughly half of all supported asylum seekers), meaning the "dispersal rate" variable understates total asylum presence—though the Asy_D11 data do include total support figures used in Column (5) of Table 2. Finally, the CSP-level analysis may mask neighborhood-level effects that operate at finer spatial scales.

7. Conclusion

The UK's no-choice asylum dispersal policy provides a rare setting where immigration placement is institutionally exogenous. Yet the shift-share instrument based on housing vacancy proves too weak to deliver credible IV estimates, illustrating an important methodological lesson: plausible instruments must be verified empirically, not just theoretically. The causal effect of dispersal on crime remains unidentified in this setting, though no specification reveals a robust positive association. In a policy environment where crime fears drive opposition to refugee placement, the absence of supporting evidence—while distinct from evidence of absence—merits attention. Future work exploiting the staggered rollout of contingency accommodation may succeed where the vacancy-based instrument could not.

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Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Total crime rate	-0.315	(0.161)	10.905	-0.0290	(0.0148)	Small negative
Violence rate	-0.020	(0.094)	3.780	-0.0054	(0.0249)	Small negative
Theft rate	-0.153	(0.069)	5.544	-0.0276	(0.0124)	Small negative
Drug offence rate	0.046	(0.016)	0.573	0.0801	(0.0274)	Moderate positive
Public order rate	-0.114	(0.048)	1.250	-0.0912	(0.0383)	Moderate negative

Notes: SDE = $\hat{\beta} \times \text{SD}(X)/\text{SD}(Y)$, where X is dispersal rate (per 1,000 pop., SD=1.004). Classification based on SDE magnitude: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005). Classification refers to effect magnitude, not statistical significance. Research question: Does asylum seeker dispersal causally affect local crime rates? Method: two-way FE (CSP + quarter) OLS. Data: Home Office asylum support by LA and Police Recorded Crime by CSP, 2016Q2–2024Q4, England and Wales. N=10,150. Treatment: continuous (dispersal rate per 1,000 population).

A. Standardized Effect Sizes

To facilitate cross-study comparison, Table 5 reports standardized effect sizes (SDE) for the main OLS estimates. For a continuous treatment, SDE = $\hat{\beta} \times \text{SD}(X)/\text{SD}(Y)$, which gives the effect of a one-standard-deviation change in dispersal rate measured in standard deviations of the outcome.

The total crime rate SDE is -0.029 (small negative): a one-SD increase in dispersal rate is associated with a 0.029-SD decrease in crime. The largest SDE in absolute value is for drug offences (+0.080, moderate positive) and public order (-0.091, moderate negative). However, given the identification concerns documented in the main text, these magnitudes should be interpreted as associations, not causal effects.