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Retail density and category overlap condition street-level shop survival near large commercial complexes: evidence from Zhengzhou, China

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Research Article

Keywords: Street-level retail, Large commercial complex, Shop survival, Category overlap, Retail density, Zhengzhou

Posted Date: November 4th, 2026

DOI: https://doi.org/10.21203/rs.3.rs-11403954/v1

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Additional Declarations: No competing interests reported.

Abstract

A planning authority must rule on a large commercial complex before it exists, and the evidence points both ways: complexes are held both to crowd out and to anchor nearby small shops. Much of that disagreement reflects conditions never estimated. We estimate two of them, and ask whether either can be observed when permission is sought. Following 45,558 street-level retail units in Zhengzhou, China, from 2019 to 2025 across three contrasting case areas containing four complexes, we estimate how survival varies with street-network distance to the nearest complex. Its direction depends on the retail intensity of the location, reversing in the low hundreds of shops within 500 metres, at a turning point the estimates place between 287 and 398. Its steepness depends on the overlap between what a shop sells and what the complexes sell: the survival gradient is about three percentage points per kilometre steeper for shops in categories a complex carries than for the rest. Only one condition is available when permission is granted: surrounding retail can be counted from open data, but what a complex will sell is not fixed until it leases. Category shares can be written into land transfer conditions, as one Chinese parcel shows, and would close that gap. We estimate conditional gradients rather than an average effect, comparing shops at different distances from the same complex. Where earlier studies assign category similarity by judgement, we compute overlap from a complete enumeration of the 1,914 tenant records inside the complexes.

1. Introduction

Large commercial complexes have been inserted into Chinese cities at a pace and scale with few precedents, and they are inserted into streets that are already trading. Whether a new complex displaces that fabric or sustains it is a question a planning authority must answer before granting approval, and the evidence available to it points both ways. What is at stake is the ground floor of the ordinary city: small, largely independent shops whose survival depends directly on where they are and on what is sold around them, and which together carry much of the employment, everyday provisioning and street life of the districts they occupy.

The evidence points in both directions for a reason that is easy to miss. Large-format retail entry has been found to reduce employment and raise exit among nearby small retailers (Haltiwanger et al., 2010), to raise customer visits to surrounding non-chain shops (Vallapuram et al., 2022), and to raise the revenue of retailers close to it while reducing that of same-market retailers further out (Daunfeldt et al., 2019). These are not three estimates of one quantity but estimates of three: employment and exit, footfall, and revenue. A development can raise the number of people walking past a shop, raise the rent it pays, and shorten its life, all at once. Before the sign of the effect can be settled, the conditions under which it takes each sign have to be stated and measured.

Recent work has begun to state them. Chung et al. (2022) find the effect of a single mega complex in suburban Seoul to differ across space, over time and across retail types, and Kang and Yoon (2025), following ten such complexes with business registration records, show that their effect on nearby

businesses depends on the services those businesses offer. Two things are left open. The condition is a category the analyst assigns: shops are grouped by whether the analyst judges the complex to sell something similar, a judgement that cannot easily be checked, because the complex's own composition is not observed. The condition is also a property of the shop alone. The property of the place enters only as context. How much retail was already trading there plausibly decides whether a complex supplies custom a street could not generate on its own or competes for custom the street was already capturing, but it is not treated as a condition on the effect. The two properties are unlikely to act independently: Daunfeldt et al. (2019) report that agglomeration outweighed competition only among complementary retailers.

The setting in which these complexes are built makes both problems tractable. Complexes in Chinese cities are typically inserted into fabric that is already built and already trading, rather than placed on suburban sites where the surrounding retail is thin, so the retail intensity a complex meets varies widely within a single city, from saturated old-city lanes to new districts whose frontage has not yet filled. That variation, within one city rather than between municipalities, is what makes the property of the place estimable at all. At the same time, the tenants of a Chinese complex are recorded individually in open point-of-interest databases, under the same classification as the shops trading on the streets outside. What a complex sells can therefore be enumerated and compared directly with what each surrounding shop sells, rather than inferred.

This study estimates the two conditions jointly, and measures the shop condition rather than assigning it. We follow 45,558 street-level retail units recorded in Zhengzhou in 2019 to 2025 across three morphologically contrasting case areas containing four complexes, and ask how survival varies with street-network distance to the nearest complex, and how that variation depends on the retail intensity of the location and on the overlap between what a shop sells and what the complexes sell. That overlap is computed from the development's own tenant list: a complete enumeration of the 1,914 tenant records inside the four complexes, classified under the same scheme as the shops on the streets outside, so that it covers shopping services, the largest category in the sample and the one the closest comparable study had to leave out, its registration data withholding retail businesses on privacy grounds (Kang & Yoon, 2025). Both conditions are properties the place and the shop already have before the complex opens, but they are not knowable at the same moment: the retail around a site can be counted when permission is sought, while what a complex will sell is not settled until it leases.

2. Framing the problem

2.1 Divergent findings and the outcomes they rest on

Research on how a large retail development affects the small shops around it has produced results in both directions, and the disagreement is ordinarily presented as one about sign. Haltiwanger et al. (2010) find big-box entry a significant negative shock to employment growth among nearby small retailers in the Washington, DC metropolitan area, sharpest in the entrant's own detailed industry and

operating chiefly through exit. Vallapuram et al. (2022) report the opposite direction in Greater London: an anchor store raises customer traffic to surrounding non-chain restaurants by 14.2 to 26.5 per cent, traffic being measured from the tips users post rather than observed directly. Daunfeldt et al. (2019) find retailers within one kilometre of a newly opened IKEA gaining roughly seven per cent in revenue, while retailers serving the same market two to five kilometres away lose.

These need not conflict, because they do not measure the same outcome, and a fourth study on the same IKEA entries measures a fourth again: the total factor productivity of incumbent retailers (Han et al., 2018).

The samples differ as much as the outcomes, and in a way that bears directly on what follows. Vallapuram et al. restrict their analysis to non-chain restaurants, and none of their fifteen covariates describes what the surrounding shops or the anchor store sell, while Haltiwanger et al. identify their effect most sharply in the entrant's own detailed industry. One study therefore excludes the relationship between what the entrant sells and what its neighbours sell; the other makes it the sharpest margin. The conditions under which a large development displaces or supports the retail around it are thus implicit in these designs, entering through the choice of outcome and of sample rather than as quantities to be estimated.

Our own choice follows from the same observation. The question asked here is about survival: whether a shop recorded in the baseline year is still trading six years later. That outcome has three properties the others do not. It is the quantity an approval decision turns on, since what an authority is asked to weigh is whether the fabric persists, not what any shop takes. It is identifiable for every unit in the record rather than for one sector or one sample, which is why the estimates below rest on 45,558 shops across all eight retail categories rather than on restaurants, on food stores, or on firms with published accounts. And it is the channel through which the strongest negative result in this literature runs: Haltiwanger et al. (2010) attribute much, if not all, of the employment effect they measure to increased exit.

A second literature shares that outcome but asks a different question of it, and so cannot settle this one. Since Berry's (1963) account of commercial blight in Chicago it has asked what makes a retail street durable in the first place, relating durability to pre-existing retail structure (Wrigley & Dolega, 2011), to locational obsolescence (Hughes & Jackson, 2015), and to land-use diversity and network centrality (Krenz et al., 2025). It explains durability, not what a single large entrant does to a street.

2.2 Conditional accounts and what they leave open

A third body of work treats the effect as conditional from the outset, and comes closest to the question asked here. Chung et al. (2022) find the effect of one complex in suburban Seoul heterogeneous across space, over time and across retail types. Kang and Yoon (2025), following ten mega shopping complexes in the Seoul metropolitan area on business registration records, find that they affect local businesses differently according to the services those businesses offer: pubs, and recreation businesses offering services the complexes also provide, face sharply higher hazards, while general restaurants, cafes and

beauty businesses are not significantly affected. Daunfeldt et al. (2019) likewise split their sample by what each incumbent sells and locate their gains among complementary retailers alone; Amin et al. (2026) find the same conditioning in United States food retail, where openings reduce visits to incumbents of the same store format while complementarity appears, within about a mile, where entrant and incumbent are not direct competitors.

The mechanism is standard in retail economics: co-location of substitutes is the basis of comparison shopping, which brings customers but also competition, while co-location of complements is the basis of one-stop shopping, which brings customers alone (Han et al., 2018). What a shop sells relative to the entrant therefore determines not whether it receives the footfall a development generates, but whether it also pays for it.

These studies establish that the conditions matter. Two things are nevertheless left open, and both are what this paper acts on.

The first is how the condition is obtained. In every one of them it is a category the analyst assigns: by mirroring the composition of the complex (Chung et al., 2022), by judging whether complexes frequently house a service (Kang & Yoon, 2025), by store format (Amin et al., 2026), or by labelling a listed set of furnishing trades substitutes for IKEA and every other retail store a complement (Daunfeldt et al., 2019). Each is a judgement about what the entrant sells rather than a measurement of it, and none can be checked against the development itself. That judgement is easy to make for a single-format store such as IKEA, and not for a complex housing several hundred tenants across every retail category.

The second is the scale at which the location enters. A condition on the location is not absent from this literature, but it is established at the scale of the municipality: Han et al. (2018) find productivity gains among complementary retailers in two of four IKEA entry regions, both small, and none in the two larger ones, while Daunfeldt et al. (2019) estimate a separate effect for firms in city centres and find none. Neither can say at what level of retail intensity the effect changes, because neither is measured on a scale along which retail intensity varies continuously: a municipality is rural or urban, a firm is in the city centre or it is not. Within one city, and within a few hundred metres of one development, retail intensity varies by an order of magnitude, and that is the scale at which an approval decision is taken.

The two should therefore be estimated jointly: category overlap measured from the development rather than assigned to it, and the retail intensity of the location on a scale along which it actually varies.

2.3 The Chinese context

The setting in which these complexes are built decides whether either condition can be estimated at all. The Seoul and Swedish cases were built towards the edge of their metropolitan areas, where the surrounding retail was thin. Complexes in Chinese cities are frequently inserted into fabric that is already built and already trading: in Shanghai, shopping centres have concentrated in the city centre and within the inner ring, with no tendency towards suburbanisation over two decades (Shi et al., 2015). Insertion therefore happens into fabric of very different retail thickness, from saturated old-city lanes to newly built

districts whose frontage has not yet filled, so the location condition varies enough within one city to be estimated there. And the tenants of a Chinese complex are recorded individually in open point-of-interest databases, under the same classification system as the shops outside, so the condition that Chung et al. and Kang and Yoon assign can here be measured against the development itself.

That fine-grained street properties govern where retail locates (Sevtsuk, 2014) and bear on how it performs once there (Yoshimura et al., 2022) is in any case well established, and work on Chinese cities has concentrated on such properties (Ye et al., 2018; Zhou et al., 2023; Efeoglu et al., 2025). Which of them condition the effect of a single large entrant is not.

3. Study area, data and methods

3.1 Three sites in one city

Zhengzhou is a provincial capital of some ten million people in central China. Four large commercial complexes opened in its central districts within three years of one another: two in Site 1, an established high-density area of the old city, where Zhenghong City opened in December 2018 and Wanda Fang in December 2019 some 290 metres away, and which we treat as a single site with two origin points; one in Site 2, a planned district of wide arterials, where Langyue Park Mall opened in October 2018; and one in Site 3, a newly built district near the Yellow River, where Huiji Wanda Plaza opened in November 2016 (Fig. 1).

The three sites were selected for morphological contrast, and that contrast is characterised by measurable indicators rather than type labels (Table 1): Site 2 is the coarsest of the three on all three grain measures yet the most orthogonally regular, with an orientation-order index of 0.812 computed after Boeing (2019). Site 3 is the instructive case. Its network and intersection densities are close to Site 1's, yet its street-frontage density is one fifth of Site 1's: the network has been built but the commercial frontage has not yet formed.

Table 1
Street morphology of the three sites

Dimension	Indicator	Site 1	Site 2	Site 3
Grain	Network density (km per km²)	9.81	8.34	9.39
	Intersection density (per km²)	23.5	18.0	23.3
	Median segment length (m)	107	144	113
Order	Orientation-order index φ	0.777	0.812	0.701
Frontage	Street-frontage density (shops per km of main road)	26.7	11.9	5.6
	Cul-de-sac density (per km²)	27.8	5.8	1.4
Reach	Network reachable within 5 km (km)	520	290	327

Notes: Grain and frontage indicators are computed over a 500 m Euclidean neighbourhood of each shop and averaged by site; the orientation-order index follows Boeing (2019) and is computed over all segments within 3 km of each site's origin point. Reachable network length is obtained by Dijkstra search to 5,000 m along the street network from each origin point. Source: calculated by the author.

Sites 1 and 3 also differ at the regional scale despite their near-identical grain: 5,000 metres along the network reaches 520 kilometres of street at Site 1 against 327 at Site 3. Local density is not regional accessibility, and the distinction matters because a complex reaches the shops around it along the network (Online Resource 1).

3.2 Data and the determination of survival

The data are annual point-of-interest records from Amap (Gaode), one of the two dominant Chinese mapping platforms, covering the five central districts, from which street-level retail units are selected by category. The annual extracts were supplied by a commercial data vendor rather than drawn from the platform directly, and they are not uniform: field naming, coordinate datum and identifier system all vary between vintages. Each year was therefore reconciled to a common schema before use, with the datum established file by file, because the offset between China's encrypted national datum and WGS-84 reaches several hundred metres in Zhengzhou, the same order as the distance effect estimated here. The baseline is the 2019 cohort of 45,558 shops, 34,071 at Site 1, 6,910 at Site 2 and 4,577 at Site 3; 33,431 of these are independent and 12,127 belong to chains.

Survival is determined by matching each record's unique identifier across years: a baseline shop counts as surviving if its identifier still appears in the 2025 records. Name-similarity matching was tried and rejected: it retained as few as 41 per cent of chain-brand outlets, chain names being too similar for a similarity measure to discriminate.

Both the choice of baseline year and the exclusion of one year follow from the identifier system rather than from judgement. Identifier matching between adjacent years is stable at 69 to 74 per cent, but falls

to 41.1 per cent between 2018 and 2019, the 2018 identifiers having subsequently been re-issued; 2019 is therefore the baseline. The 2022 records use an identifier system incompatible with every other year, so no shop can be tracked into or out of it, and its coverage gap is independently not spatially random, concentrating almost entirely in Site 1 (Fig. S1, Online Resource 1) and so introducing a difference along the very dimension on which the sites are compared. The year 2017 is excluded for the converse reason, a substantial expansion of collection.

Two further construction decisions shape the sample. The treatment variable is street-network rather than Euclidean distance from each shop to the origin point of the nearest complex, solved by Dijkstra's algorithm on the OpenStreetMap network with service roads removed, and truncated at five kilometres following the ranges used by Haltiwanger et al. (2010) and Chung et al. (2022), a bounded trading area in the sense of Reilly (1931) and Huff (1964). Tenants inside the complexes are not street-level retail and are removed: building-footprint data for Zhengzhou are too incomplete to use, so we retain only points whose names contain a keyword identifying the complex, build a concave hull from that point set after Duckham et al. (2008), and buffer it outward by about eleven metres to capture units against the external wall. This removes 1,914 records from the 2019 baseline (Fig. 2), and the same 1,914 records define the tenant mix used in Section 4.3.

Two distance metrics are used, and they are not interchangeable here. Network distance is reserved for the treatment, since what determines a shop's exposure to a complex is the route a customer actually walks or drives. Every neighbourhood aggregation is Euclidean: baseline retail density, the morphological indicators of Table 1, and the clustering units described below. Measuring the moderator along the same network that defines the treatment would build an association between them into their construction. No quantity reported below combines the two, and each metric is labelled where it appears.

3.3 Models and identification strategy

This design is built to estimate a conditional gradient, and that choice sets what it does and does not deliver. The quantity is the change in survival with street-network distance within each site, together with the way the slope of that change varies with observable properties of the shop and of its location. It asks only whether shops at different distances from the same complex, in the same city and over the same six years, survive at different rates, so no counterfactual in which the complex was never built is required and none is constructed. An average treatment effect is a different quantity: estimating it would require the same shops observed before and after opening alongside a comparison area, or otherwise comparable locations without a complex, while our baseline cohort begins at the year of opening and every shop in the sample lies within the catchment of a complex. We therefore state our results as conditions rather than magnitudes.

Four estimates carry the results, and they are made at two scales because the two conditions are properties of different objects. The location condition is estimated first on a grid of cells, where the gradient can be estimated separately at each site under spatially robust inference, and then on shops, where the retail intensity of the location enters as a continuous moderator. The shop condition is

estimated on shops. Two further specifications support the findings rather than carrying them: a near-zone threshold model, which locates the radius within which survival is depressed, and a random forest with SHAP attribution, which recovers the ordering of retail categories without imposing a functional form.

The grid-scale model belongs to the spatial Durbin family, which nests a spatially lagged dependent variable and spatially lagged regressors,

$$y = \rho W y + X\beta + WX\theta + \varepsilon \qquad (1)$$

and we estimate its spatial lag of X (SLX) member, which retains the lagged regressors and sets ρ to zero,

$$y = X\beta + WX\theta + \varepsilon \qquad (2)$$

where y is the cell-level survival rate, X the regressors listed below, W a row-standardised contiguity matrix over cells, and ε the error term. The shop-scale model is a linear probability model of survival Y on street-network distance D, a condition M and their interaction, with controls Z,

$$Y = \beta_0 + \beta_1 D + \beta_2 M + \beta_3 (D \times M) + \gamma' Z + \varepsilon \qquad (3)$$

so that the effect of distance at any value of the condition is

$$\partial Y / \partial D = \beta_1 + \beta_3 M \qquad (4)$$

Where M is continuous, setting expression (4) to zero solves for the value at which the distance effect changes sign, $M^* = -\beta_1 / \beta_3$. Where M is binary, β_3 is the difference in gradient between the two groups. A positive distance coefficient therefore means survival rises with distance: proximity to a complex is associated with a lower chance of still trading.

At the grid scale the unit is the H3 resolution 9 hexagon, and 1,260 cells enter estimation. Because survival is a proportion and baseline counts differ greatly between cells, we weight by baseline count and report the empirical Bayes corrected Moran's I (Assunção & Reis, 1999) alongside the raw statistic. The SLX member is selected by diagnostics rather than in advance: the tests reported in Section 4.1 leave a lagged dependent variable and a lagged error term indistinguishable (Anselin et al., 1996), which points to the spatial Durbin family, and the member we estimate (Halleck Vega & Elhorst, 2015) carries no lagged dependent variable and therefore does not admit a decomposition into direct and indirect effects; none is reported. Standard errors are Conley spatially robust (Conley, 1999) under a Bartlett triangular kernel truncated at 1,500 metres, which covers roughly five rings of neighbours given cell centres about 300 metres apart; Section 4.1 reports the sensitivity of the result to that choice across cut-offs from 500 to 3,000 metres.

The grid-scale regressors are each cell's street-network distance to the nearest complex, the spatial lags of that distance and of the other regressors, competition exposure from other large commercial complexes within five kilometres, and cell-level controls for population, transit access and baseline retail stock, with site fixed effects. Because a regressor and its own spatial lag are strongly correlated here, the lagged terms are assessed by a joint Wald test rather than by individual t statistics; none rejects, and the lags are retained for consistency with the diagnostic rather than interpreted (Online Resource 1). The model is estimated separately by site, since the question is whether a gradient exists at each, not what its pooled magnitude is.

The linear form at the shop scale is used so that β_3 is itself the quantity of interest, which it is not in logit or probit (Ai & Norton, 2003), and every interaction carries both constituent terms (Brambor et al., 2006). Standard errors are clustered on 500 metre grid cells, for two reasons, neither of them the result. The level of clustering should match the level at which the regressor of interest varies (Abadie et al., 2023), and the location condition is itself constructed on a 500 metre Euclidean radius. That scale is also coarser than the unit within which shops plausibly share landlords, rents and footfall, mean intersection spacing at the three sites being 206, 236 and 207 metres, so clustering at this scale is conservative relative to clustering at the block. Section 4.2 reports the estimate at four cell sizes.

The first condition is the retail intensity of the location, which we measure as baseline retail density: the number of street-level retail units within a 500 metre Euclidean radius of a shop in the baseline year, logged and mean-centred. It is a continuous shop-level quantity, so the interaction gives the distance effect at any density and can be solved for the density at which that effect changes sign. Because density is monotonic in site identity across the three sites, we estimate a ladder of four specifications of increasing stringency, ending with site-specific distance slopes that identify the interaction from within-site variation alone.

The second condition is the overlap between what a shop sells and what the complexes sell, and it is measured rather than assigned. The tenant records removed in Section 3.2 are classified under the same eight-category scheme as the shops outside, and for each category we form the ratio of its share among tenants to its share among street-level retail in the study area. Because the categories rest on very unequal numbers of tenant records, the ratio defines a binary low-overlap indicator rather than entering continuously in the main specification: assignment then requires only that a shop's category be classified correctly, and the continuous version is retained as a robustness check. Estimates are reported without controls, with one control drawn from each of four domains, and with a set of twenty shop- and location-level controls; no control coefficient is given a causal reading (Westreich & Greenland, 2013).

The two supporting specifications are as follows. The near-zone threshold model replaces the linear distance term with an indicator for lying within γ network kilometres of a complex, γ being chosen by grid search over the observed range of distances. Because γ is searched rather than fixed in advance, the fit of the selected model is not a valid test for the presence of a threshold, and we report the radius and the

step in survival as a description of where the effect is concentrated rather than as a test of it. The random forest is fitted to survival with no reference to the overlap ratio, under the settings reported in Online Resource 1, and the ordering of categories is recovered from the fitted surface by SHAP attribution (Lundberg & Lee, 2017). Attribution units are not percentage points of survival, so the forest is used for ordering alone and no attribution magnitude is quoted.

4. Results

4.1 A distance gradient at one of the three sites

Survival between 2019 and 2025, tabulated by rings of street-network distance from the complex, differs sharply across the three sites (Fig. 3). At Site 1, the dense old-city site, it rises ring by ring from 18.0 per cent in the innermost ring to 28.0 per cent in the outermost; Sites 2 and 3 show no such progression, their innermost rings being too thinly populated to read as gradients in either direction. Overall survival is nonetheless almost identical at the three sites, at 27.0, 25.5 and 25.6 per cent, so the contrast does not follow from one site being a worse trading environment than the others. What has to be explained is not the level of survival but its arrangement with distance.

That arrangement survives estimation at Site 1 and is not detected at the other two (Fig. 4). Survival clusters spatially on the empirical Bayes corrected Moran's I (Assunção & Reis, 1999), and the robust Lagrange multiplier forms leave a lagged dependent variable and a lagged error term indistinguishable (Anselin et al., 1996), which selects the SLX specification of Section 3.3. Its focal distance coefficient is + 0.0116 at Site 1 (SE 0.0055, p = 0.034), and is distinguishable from zero at neither Site 2 (-0.0052, SE 0.0071) nor Site 3 (+ 0.0139, SE 0.0094). In survival terms the Site 1 estimate is 1.16 percentage points per kilometre: a shop one kilometre further from the complex was about 1.2 percentage points more likely to still be trading six years later. The coefficient is invariant to the width of the Conley kernel, holding at + 0.0116 at every cut-off from 500 to 3,000 metres, while its p value runs from 0.057 to 0.012, so the gradient is marginal rather than significant under the shortest kernels (Online Resource 1).

What three sites cannot establish is that the gradient differs between them. Significance here tracks precision rather than magnitude: Site 3's point estimate is the larger of the two positive ones, and the difference between it and Site 1's is nowhere near significance, at + 0.23 percentage points per kilometre with a standard error of 1.09, Site 1 simply contributing 34,071 shops against Site 3's 4,577. The three-site comparison therefore poses the question that Section 4.2 answers with a continuous measure of the location, identified from variation within sites rather than between them.

Two further results at this scale concern where the effect sits rather than whether it exists. Competition exposure from other complexes is significant only at Site 2 (-0.239, SE 0.080, p = 0.003), and is identically zero at Site 3, there being no other large commercial complex within five kilometres. The Site 1 gradient is also not spread evenly across the five-kilometre catchment. A near-zone threshold model places the radius at 1.3 network kilometres, inside which survival is 7.3 percentage points lower than outside it

(Online Resource 1); the zone covers 49 of Site 1's 632 cells and 13.0 per cent of its shops, and we refer to it below as the crowded-out zone. This is the sharpest statement the data make about the scale at which a complex and the street compete: the effect is a near-field one, confined to about a quarter of the radius over which the trading area was drawn. It is also the least portable of the results reported here: the radius marks where the effect is concentrated rather than a tested boundary, because γ is chosen by search (Section 3.3), and it is estimated at one site in one city, so it describes Zhengzhou's old city rather than supplying a parameter for anywhere else.

4.2 Baseline retail density sets the direction of the gradient

Only one of the three sites shows a gradient, so what distinguishes that site has to be identified. Street morphology does not do it. Site 3's grain is nearly identical to Site 1's, at 9.39 against 9.81 kilometres of street per square kilometre and 23.3 against 23.5 intersections per square kilometre (Table 1), yet it shows no gradient; Site 2, the coarsest of the three and the most orthogonally regular, shows none either. What separates Site 1 from both is how much retail was already trading on its streets. We therefore take the retail intensity of the location as the condition, and measure it as baseline retail density: the number of street-level retail units within a 500 metre Euclidean radius in the baseline year.

Two restrictions define the sample. Independent shops alone are used, here and in Section 4.3: chain outlets survive at 33.9 against 24.0 per cent, a gap reflecting headquarters resources rather than location, and letting chain status into either condition would let it dominate both. The restriction is substantive as well as technical: the long-run structural change in retail is the replacement of single-establishment retailers by chains (Jarmin et al., 2009). Shopping services are excluded here because they are crowded out at every level of density and never reverse; Section 4.3 returns to them across all eight categories. What remains is 16,481 independent shops in catering and everyday services.

One feature of that sample sets the identification problem. Median baseline densities at the three sites are 673, 288 and 199 shops within 500 metres in this sample, an ordering monotonic in site identity and therefore collinear with it, so density acting causally cannot be distinguished from some other characteristic of Site 1 that does not vary with distance. Four progressively more stringent specifications are designed to rule this out (Table 2): M1 keeps a morphological dummy as the moderator for comparison, and the other three use baseline density, ending with M4, which adds site-specific distance slopes so that the interaction is identified only from within-site variation in density. All standard errors are clustered on 500 metre grid cells.

Table 2
Baseline retail density moderates the distance effect on survival

Moderator	M1	M2	M3	M4
	Morphological dummy	Baseline density	Baseline density	Baseline density
Site fixed effects	No	No	Yes	Yes
Site-specific distance slopes	No	No	No	Yes
Distance × moderator	+ 1.57	+ 1.08	+ 1.23	+ 1.14
Cluster-robust SE	(0.86)	(0.45)	(0.41)	(0.59)
t	+ 1.82	+ 2.38	+ 2.99	+ 1.93
p	0.069	0.017	0.003	0.054
Reversal density (shops within 500 m)	—	287	398	361
Residualised variance of interaction	—	1.455	1.441	0.789
Observations	16,481	16,481	16,481	16,481
Clusters (500 m cells)	508	508	508	508

Notes: Linear probability models of survival from 2019 to 2025 on street-network distance to the nearest complex and its interaction with the moderator. The sample is the 16,481 independent shops in catering and everyday services. Coefficients are percentage points of survival per kilometre, and per log unit of density where the moderator is continuous; a positive coefficient means survival rises with distance. Standard errors are clustered on 500 m Euclidean grid cells. The reversal density is the density at which the distance effect changes sign, solved from each specification. Source: calculated by the author.

The interaction between distance and baseline density is positive under every specification in which it appears, and of stable magnitude: +1.08 percentage points per kilometre per log unit of density under M2 (t = 2.38), + 1.23 under M3 (t = 2.99) and + 1.14 under M4 (t = 1.93, p = 0.054) (Table 2). The morphological moderator M1 retains for comparison is larger and less precise, at + 1.57 with p = 0.069, as a dummy standing for the whole site would be. Because density enters continuously, each specification can be solved for the density at which the distance effect changes sign, and the three place that reversal at 287, 398 and 361 street-level retail units within 500 metres (Fig. 5). We carry forward M4, which surrenders all between-site contrast, and with it a reversal at 361 units.

The complex therefore does not act in one direction on the street around it. On frontage already thinly occupied, by a couple of hundred shops within 500 metres, the shops nearer the complex survived better than those further away; on frontage carrying many more, the order is reversed. The reading that fits both ends is the standard one in retail economics: where there is little else to shop for, a complex supplies the

custom a street cannot generate on its own, and where there is already a great deal, it competes for custom the street was capturing already (Han et al., 2018).

The position of the reversal is less precisely determined than its existence, and we report it as an order of magnitude rather than a point: the three specifications span 287 to 398, and estimating the same ladder on a sample that also includes chain outlets moves it to between 312 and 386. What is stable is that the sign changes somewhere in the low hundreds of shops per 500 metre neighbourhood. The two ends of the gradient are not equally well identified. A Johnson-Neyman calculation (Johnson & Neyman, 1936) locates where the distance effect is distinguishable from zero under M4: crowding out is significant above about 789 shops within 500 metres, a condition met by 29 per cent of the sample, while the anchoring effect at low density does not reach significance anywhere on the continuous margin. We therefore treat the crowding-out end as the better-identified of the two, though a less conservative split by density quintile does find both, at -1.37 percentage points per kilometre in the lowest and + 1.18 in the highest.

The decline in significance from M3 to M4 is a loss of identifying variation rather than a weaker effect: site-specific slopes absorb the between-area contrast in full, and the residualised variance of the interaction term falls by 46 per cent. Three checks leave the estimate where it is. Varying the clustering scale from 250 metres to two kilometres holds the point estimate at + 1.14 percentage points and moves only the p value, from 0.018 to 0.097 as the number of clusters falls to 56 (Cameron & Miller, 2015); a placebo interaction replacing baseline density with the site ordinal is not significant (p = 0.125); and including chain outlets leaves the interaction at + 1.17 percentage points and the reversal at 318. Full numbers are in Online Resource 1 (Table S1).

4.3 Category overlap steepens the distance gradient

The second condition is a property of the shop rather than of its location, and we measure it from the complexes themselves rather than assigning it. The 1,914 point-of-interest records inside the four complexes are classified under the same scheme as the shops outside, so for each of eight categories we compute an overlap ratio: the share of that category among the complexes' tenants divided by its share among street-level retail in the study area. The ratios run from 1.19 for shopping services down to 0.00 for vehicle repair (Table 3). They rest on very unequal numbers of in-complex records, from 932 for shopping services to 16 for health care and none at all for vehicle repair, so we do not estimate a separate effect for each category and instead split the eight at a ratio of 0.40. The cut falls in the gap between health care at 0.34 and everyday services at 0.81, so no category lies near it, and it separates the three categories resting on fewest in-complex records from the five resting on 69 or more. The sample is the 33,431 independent shops of the baseline cohort across all eight categories, 2,122 of them in the low-overlap group.

Table 3
Category overlap between the complexes and the surrounding street-level retail

Retail category	Tenant records inside the complexes	Share of tenants (%)	Share of street-level retail (%)	Overlap ratio	Exact 95% CI	Group
Shopping services	932	48.7	41.1	1.19	1.131–1.241	High
Sports and leisure	21	1.1	0.9	1.17	0.726–1.783	High
Catering services	586	30.6	29.8	1.03	0.957–1.097	High
Education and culture	69	3.6	3.9	0.92	0.717–1.156	High
Everyday services	289	15.1	18.6	0.81	0.728–0.903	High
Health care	16	0.8	2.5	0.34	0.195–0.552	Low
Vehicle services	1	0.1	1.8	0.03	0.001–0.161	Low
Vehicle repair	0	0.0	1.4	0.00	0.000-0.138	Low

Notes: The overlap ratio is a category's share among the 1,914 tenant records enumerated inside the four complexes divided by its share among street-level retail in the study area. Intervals are Clopper-Pearson exact binomial intervals propagated to the ratio. The low-overlap group is defined by a ratio of 0.40 or below; no category lies near that cut. Source: calculated by the author.

The quantity of interest is the difference between the two groups in how survival varies with distance. Estimating survival on distance, the low-overlap indicator and their interaction, with site fixed effects and standard errors clustered on 500 metre cells, the interaction is negative and significant under all three control sets: -2.47 percentage points per kilometre with no controls (t = -2.00), -2.88 with one control drawn from each of four domains (t = -2.34), and − 2.90 with the full set of twenty shop- and location-level controls (t = -2.30). The fourth of those domain controls is incumbency, included because exit hazards are known to depend on establishment age (Bruderl & Schussler, 1990; Fichman & Levinthal, 1991).

The two kinds of shop therefore meet the complex on different terms (Fig. 6). Among shops selling what the complex sells, moving a kilometre further out is worth about three percentage points more survival than it is among shops selling something else, which is what one would expect if the complex takes custom from shops it duplicates and passes custom to shops it does not (Han et al., 2018). The claim concerns how survival varies with distance within each group, not which group survives better overall:

raw survival does differ across categories, but that ordering confounds overlap with each trade's own durability.

That difference of about three percentage points per kilometre is the one quantity here that is stable across every control set, and it is the only one we carry forward. The high-overlap slope is not: it falls from + 1.10 without controls to + 0.35 under the full set. Nor is the low-overlap slope, which is negative throughout, at -1.22 to -2.26, but is not distinguishable from zero in any of the three. We therefore make no claim that shops in low-overlap categories benefit from proximity.

Three checks bear on the measure. Ranking the eight categories by a distance-attribution slope recovered from a random forest (Breiman, 2001) with SHAP attribution (Lundberg & Lee, 2017), fitted with no reference to the overlap ratio, reproduces the ranking by overlap at a Spearman correlation of 0.929; the ordering is thus recovered by a method that imposes no functional form at all. Replacing the indicator with the continuous ratio, and stratifying the three largest categories, leave the contrast where it is (Table S1). We also asked whether overlap moves the density at which the distance effect changes sign, and found no evidence that it does: the three-way interaction is indistinguishable from zero and the category-level reversal densities are not ordered by overlap (Online Resource 1). We therefore report the two conditions as acting additively, baseline density governing the direction of the gradient and overlap its steepness.

5. Discussion and conclusion

5.1 Reconciling the divergent findings

We opened by noting that the literature disagrees about the sign of the effect, and that the studies concerned do not measure the same outcome. The results above suggest the disagreement is also, and perhaps mainly, about conditions that were never estimated.

The two conditions act on the distance effect in different ways, and both act within a single city. The location condition moves it across zero, at a density that can be solved from the estimates themselves: below roughly 361 street-level retail units within 500 metres, proximity to a complex is associated with higher survival, and above it with lower survival, with the specifications placing that turning point between 287 and 398. The shop condition acts on the steepness of the same gradient, which is about three percentage points of survival per kilometre steeper among shops in the categories a complex carries than among the rest. Where the gradient is detected it is also a near-field one, concentrated inside 1.3 network kilometres rather than spread across the five-kilometre catchment. We found no evidence that overlap also moves the density at which the effect reverses, so we report two margins on one gradient rather than a nested structure. A study sampling only dense old-city streets and only categories the complex also sells would find crowding out; a study sampling thinner fabric, or categories in which overlap is low, would find the opposite. Neither would be wrong about its sample. The direction

is not new in substance, only in scale: Han et al. (2018) find IKEA's productivity spillover among complementary retailers in the two smallest of four entry regions and not in the two larger ones.

The same split can be seen inside a single design, and inside a single trade. Daunfeldt et al. (2019) report significant revenue effects and statistically insignificant employment effects for the same entries, the same firms and the same window, so two outcomes can disagree without any difference in sample. And within one trade: Chung et al. (2022) find daily traffic to restaurants and bars within five kilometres of a complex to rise after it opens, while Kang and Yoon (2025), working on survival in the same country, find pubs the most harmed of the groups they examine. These are not answers to one question but measurements taken at different points of a conditional surface, with different outcomes.

The complement-substitute distinction has organised this literature since Haltiwanger et al. (2010) posed it as a question, and in every case we are aware of it is drawn by the analyst. Ours is computed from the tenant list, so the distinction the literature has been assigning becomes something another study can recompute. On the one group where our result can be set beside an existing one it points the same way: Kang and Yoon find a survival benefit from proximity among recreation businesses offering services a complex does not, and our low-overlap slope is negative throughout but not distinguishable from zero, which is why we report the difference between groups rather than a benefit to either.

5.2 What a planning authority can act on

The obvious instrument is a restriction on the development itself, and it may be worth reconsidering whether approval should turn on how large a proposed complex is, or how close it stands to existing retail frontage. A rule of that form presumes a single sign for the effect, and the estimates above do not deliver one: they place a change of sign at about 361 street-level retail units within 500 metres, and the gradient is distinguishable from zero only above 789 units, a condition met by 29 per cent of the sample. A restriction keyed to size or to proximity alone would therefore bind across a city in which the gradient it is meant to correct holds over part of the density range and runs the other way over the rest. Provincial guidance has in any case begun to discourage attaching non-spatial requirements to planning conditions (Department of Natural Resources of Shaanxi Province, 2026). What these results support is not a restriction on the development but a differentiated use of the conditions already attached when permission is granted.

Those conditions can carry only one of the two properties at the moment they are written. The retail already trading around a site can be counted at any time from open point-of-interest data, including at the moment permission is sought. The overlap condition cannot: it requires the categories the development will house, and those are not settled when permission is granted.

This is a feature of the approval process rather than of the data. Under the Urban and Rural Planning Law, the conditions attached to a land transfer and the materials required for a building permit concern the location, use class, development intensity and built form of a site (Standing Committee of the National People's Congress, 2019, arts. 38 and 40); neither reaches the composition of retail categories

a complex will let. Neither the published transfer results nor the pre-decision notices for the four complexes studied here contain any clause on retail composition; the mix of trades within the permitted commercial floor area is settled by the developer during leasing. No national rule requires category shares to be recorded; they are attached parcel by parcel. One parcel offered in 2025 required retail to occupy at least half of the permitted floor area and catering at least fifteen per cent (Lingshui Li Autonomous County Bureau of Natural Resources and Planning, 2025). That condition is directed at the programme of the parcel itself and makes no reference to the retail outside it, so the instrument exists but has not been used for this purpose.

What we propose asks for no new instrument, only a further ground for using one that already exists, written at the category resolution our overlap ratio uses. It would move the second condition forward from leasing to approval, and would make retail composition an exception to that guidance rather than an instance of it.

Two readings should be set aside before the conditions are put to use. They do not support the claim that a complex hollows out the street around it, because turnover among street-level retail is high everywhere in this city. Matching identifiers between the 2019 and 2025 citywide records on the same basis used to determine survival, shops absent from the 2019 stock make up 76.8 per cent of the 2025 stock across the five central districts, and between 73.2 and 86.3 per cent within every decile of 2019 baseline density. Inside the crowded-out zone at Site 1, the 1.3 kilometre radius of Section 4.1, the figure is 80.8 per cent, against 76.6 per cent across the rest of the city. Nor do they establish that a complex raises the retail density around it. Density is measured in the baseline year, before the outcome. That is what makes it a condition rather than a consequence. What the estimates identify is narrower: a difference in exit risk along a distance gradient, conditional on two measurable properties. The churn baseline behind it is not the complex's doing, and approval cannot change it.

With both conditions in hand an authority can compute, for a proposed development and the streets around it, which categories of existing shop face high overlap with the proposal, and whether the surrounding frontage lies above or below the density at which the distance effect changes sign. Retail impact becomes something to be anticipated before trading starts rather than attributed afterwards. Two qualifications travel with that use. The reversal density is estimated for Zhengzhou and should be re-estimated rather than transferred: a few hundred shops within 500 metres is a property of this retail economy, not a constant. The overlap ratio is more portable, because it is computed from the development's own tenant mix and the local retail composition and requires no parameter from this study. A screening rule would therefore combine a locally re-estimated density threshold with a locally computed overlap ratio, and would identify which shops are exposed rather than predict how many will close. The decision at approval is therefore not whether shops will close, but which of them stand in the part of the gradient where proximity carries the greater risk. That is the question the two conditions answer, and one of them can already be written into the permission itself.

5.3 Limitations and future research

Three limitations bound what we claim. The first is the number of sites. Baseline retail density is monotonic in site identity across three case areas, so the two are collinear by construction, and no specification on three sites can fully separate them; the placebo of Section 4.2 has little power, and passing it is weaker evidence than failing it would have been. Studying several complexes of one morphological type would hold morphology fixed while density varied, and let the two be separated rather than merely tested against one another.

The second is that we observe only the exit side. A shop that never opened and a shop that was never recorded are indistinguishable in point-of-interest data, so entry cannot be estimated and no claim is made about it. A street with both high exit and high entry is not in the same condition as one with high exit alone, and business registration data, which distinguish non-opening from non-recording, would allow both ends of the flow to be examined together.

The third concerns the resolution of the overlap condition. Overlap is identified across eight categories, far fewer effective clusters than cluster-robust inference ordinarily requires, and those categories rest on very unequal numbers of tenant records. We therefore claim an ordering and a two-group contrast and no more. The null reported in Section 4.3 should be read in the same light: absence of evidence that the two conditions interact is not evidence that they do not. Pooling tenant records across many complexes would make the interaction estimable rather than merely testable.

One prediction is externally testable now. The dividing line between the two overlap groups falls at a ratio of about 0.40, and the ratio can be recomputed for any complex whose tenant list and surrounding retail composition are observable. The ordering of categories by overlap, and the direction in which that ordering moves the distance effect, are claims another case can confirm or refute.

Declarations

Use of generative artificial intelligence. The author used Claude (Anthropic) to assist in writing the code that produced the analysis, the figures and the tables, and to polish the English prose and check it for errors.

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Figures

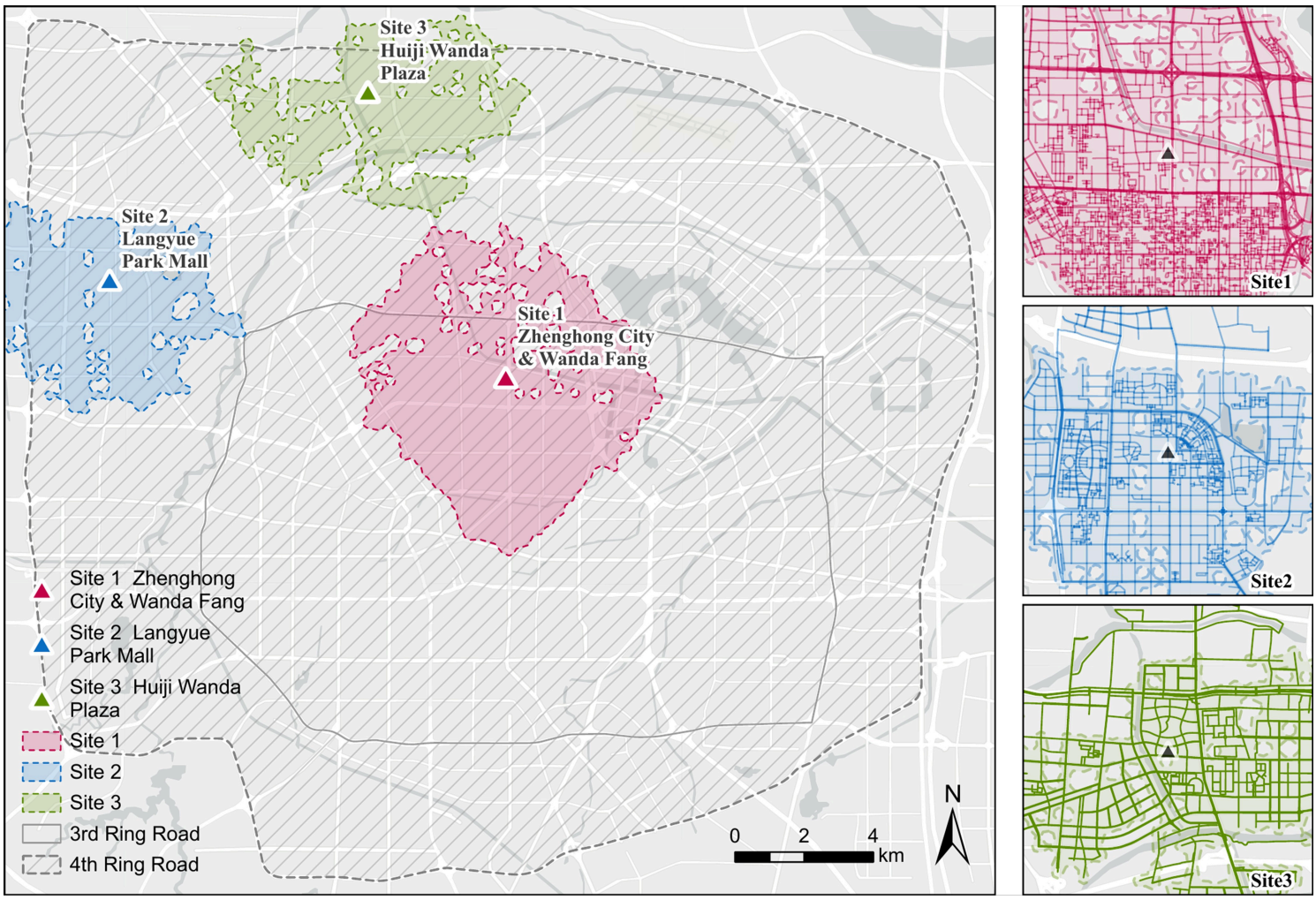


Figure 1

The three case areas in Zhengzhou. Each is the extent reachable within five kilometres of its complex along the street network (dashed outline), and the hatch is the five central districts covered by the point-of-interest data. The insets show each case area's street network, Site 1 to Site 3 from top to bottom, all at the same scale. The third and fourth ring roads are shown for orientation



Site	Commercial complex	Footprint (ha)	POI inside (excluded)	POI within 800 m (retained)
1	Zhenghong City	4.2	562	1,447
1	Wanda Fang	3.4	226	1,477
2	Langyue Park Mall	11.5	597	1,202
3	Huiji Wanda Plaza	7.2	529	355
	Total	**26.3**	**1,914**	**4,481**

Figure 2

The tenant enumeration on which the overlap measure rests, with each case area shown at 1:9,000. Complex footprints are concave hulls built from the keyword-matched point set and buffered outward by about eleven metres (Section 3.2); retail points falling inside a footprint are tenants of the complex and are removed from the street-level sample. The 800 metre counts in the table are given for context only and play no part in the estimation

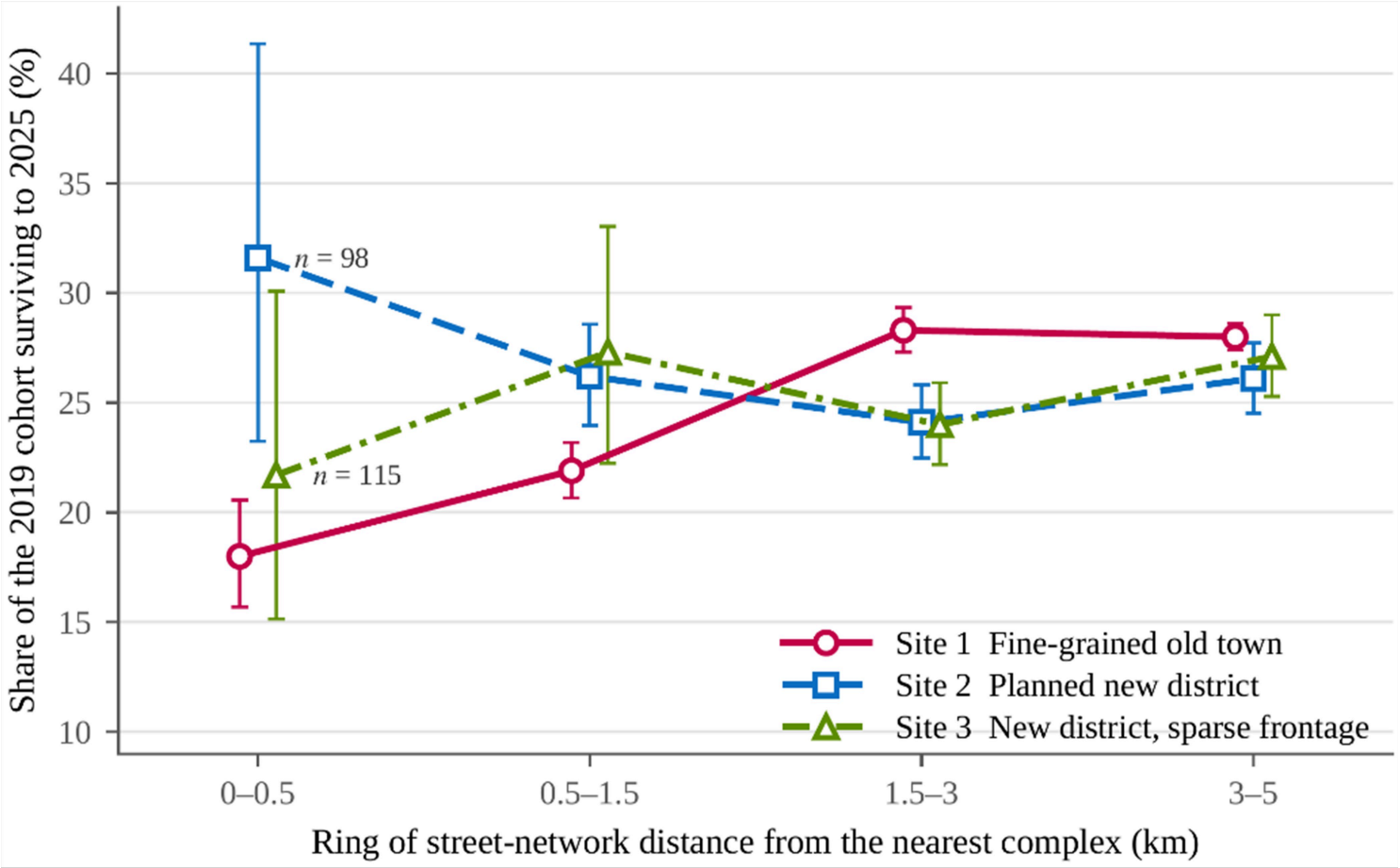


Figure 3

Survival to 2025 of the 2019 street-level retail cohort, by ring of street-network distance from the nearest complex. Bars are Wilson 95 per cent intervals for a binomial proportion. The counts annotated at Sites 2 and 3 are the sample sizes of their innermost rings, which are too small to read as gradients in either direction

Image not available with this version

Figure 4

The effect of street-network distance on cell-level survival, estimated separately at each site. Points are the focal distance coefficients of the specification of Section 3.3, in percentage points of survival per kilometre of additional distance, so that a positive value means survival is lower nearer the complex. Bars are 95 per cent intervals from Conley spatially robust standard errors under a Bartlett kernel truncated at 1,500 metres. Cell counts are the H3 resolution 9 hexagons entering each site's regression

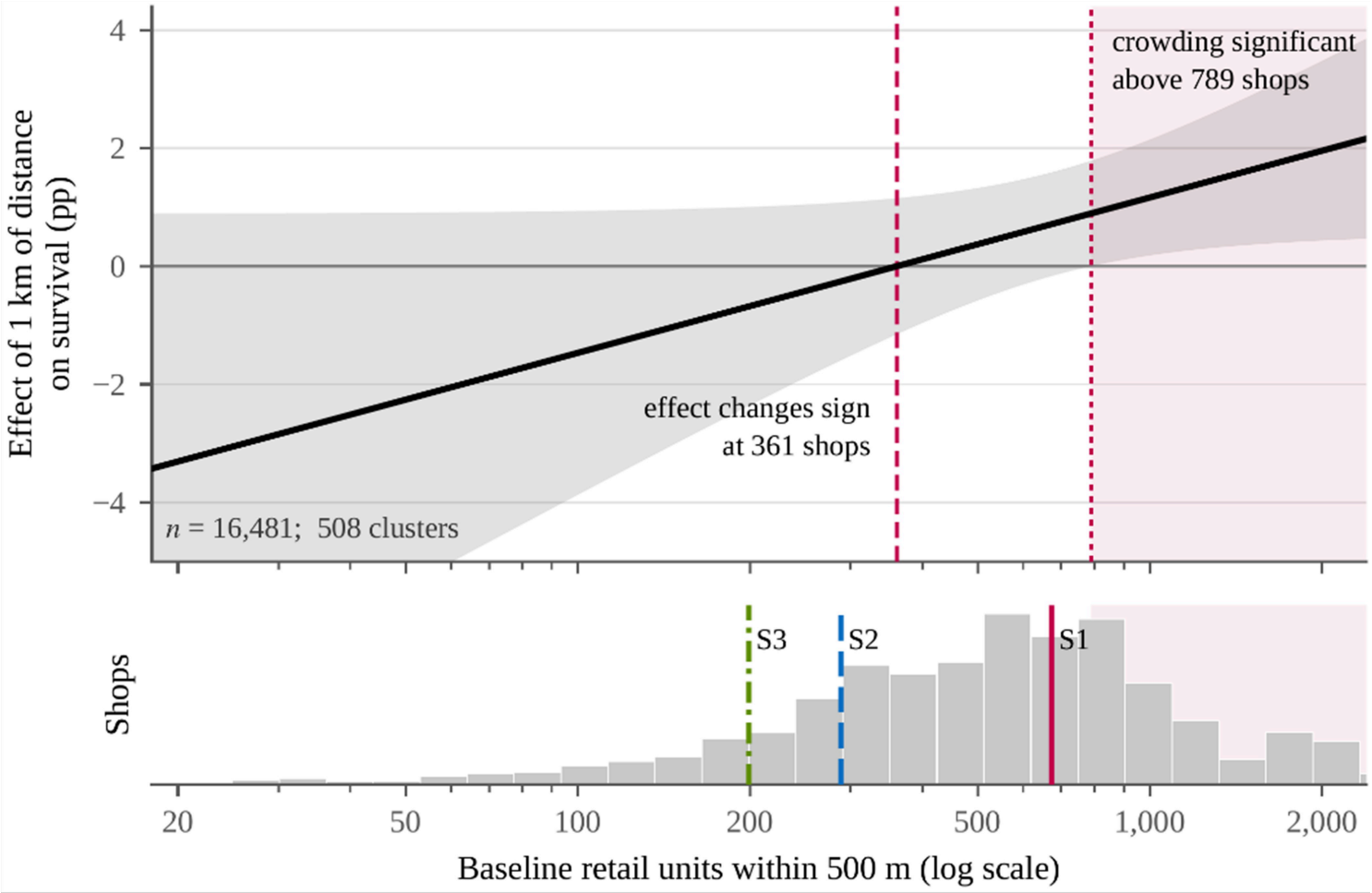


Figure 5

The distance effect on survival as a function of baseline retail density. The line is the marginal effect of one kilometre of street-network distance implied by specification M4 of Table 2, and the band is its 95 per cent interval. The effect changes sign at 361 street-level retail units within 500 metres (dashed line) and is distinguishable from zero only above 789 units (shaded). The lower panel shows how the sample is distributed over baseline density, with each site's median marked

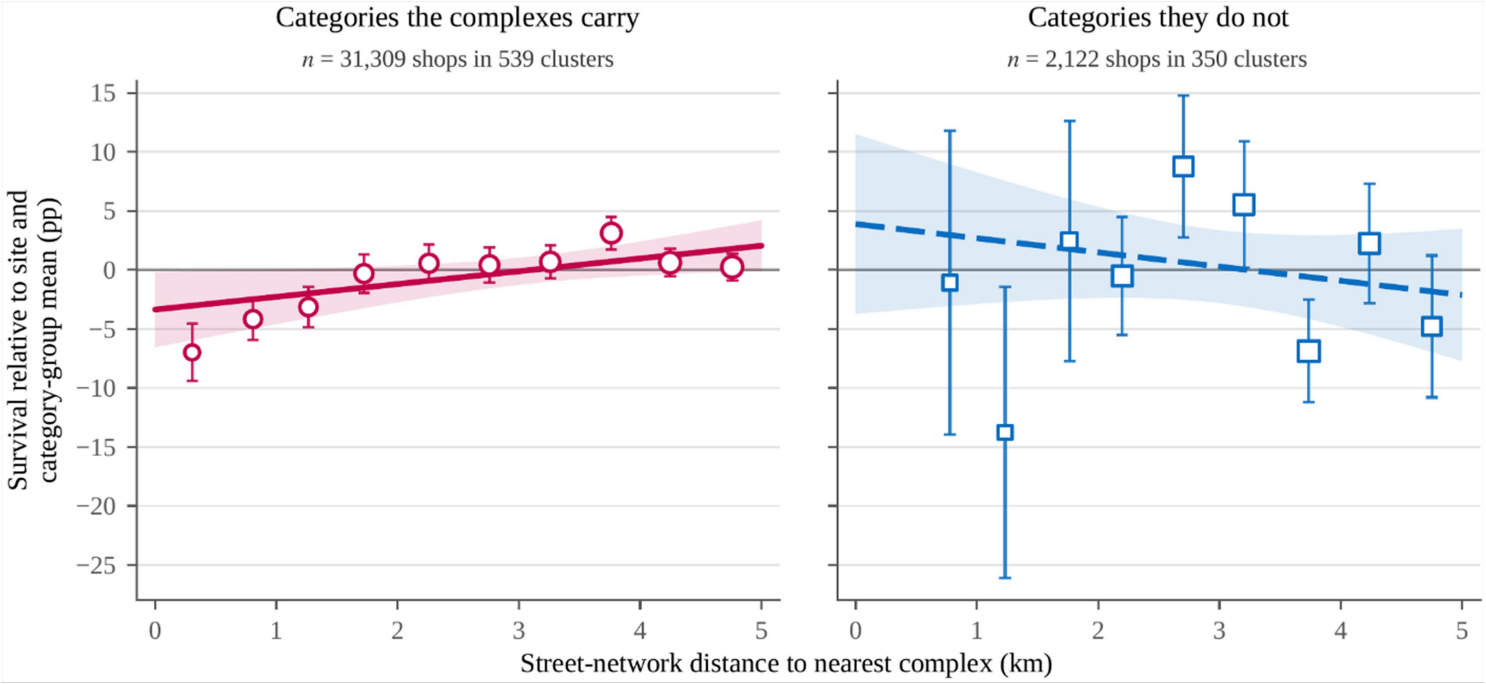


Figure 6

The distance-survival gradient for shops in categories the complexes carry and for shops in categories they do not, with group membership taken from the overlap ratio of Table 3. Survival is expressed relative to the mean of the shop's own site and overlap group, so that each panel shows how survival varies with distance within a group and not which group survives better overall. Markers are binned means with 95 per cent intervals, sized by the number of shops in the bin

Supplementary Files

This is a list of supplementary files associated with this preprint. Click to download.

- ESM1.pdf