When Entrepreneurs Move In: Evidence from North Carolina

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This paper provides the first causal evidence that entrepreneurial activity diffuses through immediate neighborhood ties. I exploit residential moves in North Carolina to study whether the arrival of an entrepreneurial next-door neighbor increases business formation among incumbent residents. Merging statewide business registrations, voter files, and property transaction data, I construct a panel linking individuals to their closest neighbors and entrepreneurial outcomes over nearly two decades. Using a nearest-neighbor design with highly granular fixed effects, I find that exposure to an entrepreneurial neighbor raises the probability of business entry by 4–9 percent within five years. Effects are concentrated among immediate neighbors, attenuate sharply with distance, and are driven by arrivals who actually reside nearby. Most entry occurs in unincorporated, lower-cost businesses, while exposure to high-quality entrepreneurs increases the likelihood of starting incorporated firms. These findings show that face-to-face residential interactions are a powerful and highly localized channel through which entrepreneurship spreads.

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1. Introduction: Neighborhoods as Gateways to Entrepreneurship

Entrepreneurship has long been viewed as a cornerstone of economic growth and innovation, dating back to Schumpeter's classic insights on creative destruction. Yet despite extensive policy efforts, rates of entrepreneurial entry remain modest, and the factors that help individuals overcome the risks and frictions of starting a business are not fully understood.

One promising channel is social interaction: by shaping access to information, role models, and networks, exposure to entrepreneurs may lower barriers to entry and influence who becomes an entrepreneur. Recent studies document that entrepreneurial activity often clusters within schools, workplaces, and broader residential areas. Yet little is known about the most immediate and pervasive form of exposure—interactions with one's neighbors. Because neighbors are geographically proximate, repeatedly encountered, and often socially comparable, they represent a uniquely plausible channel through which entrepreneurial behavior may spread. This paper asks whether living next to an entrepreneurial neighbor increases the likelihood that an incumbent resident starts a business.

To address this question, I exploit residential moves that generate quasi-random variation in the entrepreneurial background of new arrivals: some newcomers have prior entrepreneurial experience, while others do not. Focusing on incumbent homeowners who receive new next-door neighbors, I examine whether exposure to entrepreneurial neighbors increases subsequent business formation. This design provides a unique opportunity to isolate the causal effect of direct neighbor-to-neighbor influence on business formation and to shed light on how neighborhood social interactions shape economic activity.

The results show that the arrival of an entrepreneurial neighbor significantly increases the likelihood that incumbents start a business, providing the first direct evidence of neighborhood-based entrepreneurial spillovers. The impacts are economically meaningful, concentrated among immediate neighbors, and more pronounced when newcomers are high-quality entrepreneurs. Importantly, the estimated effects are robust to alternative definitions of local exposure and to stricter control-group constructions that eliminate potential attenuation from nearby entrepreneurial arrivals. Together, these findings demonstrate that entrepreneurial diffusion through neighborhood ties is both real and highly localized, consistent with transmission through direct local interaction rather than broader neighborhood conditions.

As with any study of peer effects, a central challenge is establishing causality. Neighboring households are not randomly assigned, and several concerns arise. Incumbents and entrepreneurial newcomers may sort into the same neighborhoods based on unobserved traits or shared preferences for local amenities, creating spurious correlations. Outcomes may also move together in response to common neighborhood shocks, making it difficult to separate genuine spillovers from correlated effects. To address these challenges, my empirical design exploits residential moves that change the entrepreneurial background of the nearest arriving households—comparing incumbents whose up to four closest new neighbors include an entrepreneur with those whose new neighbors do not, within the same Census block group and year. Because housing markets are highly illiquid and properties sell one at a time, buyers can choose the neighborhood they move into but not which exact nearby house becomes available, generating plausibly exogenous variation in who their immediate

neighbors turn out to be. I further show that the results are insensitive to how neighbors are spatially defined and remain stable when excluding control observations with nearby entrepreneurial exposure. This design allows me to isolate the causal impact of direct neighbor-to-neighbor entrepreneurial exposure.

To implement this strategy, I adapt the nearest-neighbor design of Bayer et al. (2022) to the context of entrepreneurial exposure. I construct a novel dataset by merging three administrative sources: business registration records from the North Carolina Secretary of State, voter registration files, and CoreLogic property data, which contain detailed information on real estate transactions and property characteristics. Following the address-linking methodology of McCartney, Orellana-Li, and Zhang (2024), I identify individuals living directly next door to one another and track changes in neighbor composition as properties turn over. The resulting 2005–2019 panel covers millions of resident–year observations, with detailed information on incumbents' entrepreneurial status, demographics, and housing characteristics, as well as those of their nearest neighbors. This linkage enables me to observe entrepreneurial exposure and subsequent business formation at an exceptionally granular scale.

My empirical strategy focuses on incumbent–year observations in which a new neighbor moves in next door. I classify incumbents as treated if the incoming neighbor has prior entrepreneurial experience, and as controls otherwise. The design compares business formation rates of treated and control incumbents within the same Census block group and year, thereby absorbing local shocks and broader geographic trends in entrepreneurship. I also control for observable demographic and property characteristics of incumbents. Under this specification, I find that exposure to an entrepreneurial neighbor increases the probability that an incumbent starts a business within five years by 0.04 to 0.09 percentage points—equivalent to a 4–9 percent increase relative to the baseline rate. The results are robust to a wide range of alternative specifications, including models with additional fixed effects and controls for neighborhood composition.

The research design rests on the assumption that, conditional on controls and within block group–year cells, new neighbors are as good as randomly assigned to incumbents. I test this assumption in several ways. First, stacked event-study analyses following Cengiz et al. (2019) confirm the absence of differential pre-trends in business formation between treated and control incumbents prior to neighbor arrivals. Second, I show that treated and control groups are economically indistinguishable on a wide range of demographic and property characteristics, supporting the view that assignment is effectively random within neighborhoods. Third, the evidence indicates that correlated neighborhood shocks are unlikely to drive the results: housing prices show no differential pre-trends, and the estimated effects decline sharply with distance, pointing to highly localized spillovers. Taken together, these exercises provide strong evidence that the design credibly identifies the causal effect of entrepreneurial neighbors.

To further probe the role of direct social interaction, I examine whether entrepreneurial spillovers depend on the residential presence of arriving neighbors. Spillovers are concentrated among entrepreneurial arrivals who actually reside in the neighborhood and attenuate when properties are purchased by non-resident owners. Importantly, the effects re-emerge when such properties are occupied by entrepreneurial renters. These patterns indicate that residential presence, rather than

ownership status or investment activity, is central to the transmission of entrepreneurial spillovers, consistent with a mechanism operating through repeated local interaction.

Beyond the baseline effect, I examine how the strength of entrepreneurial diffusion varies across entrepreneurs and incumbents. Exposure to high-quality arrivals increases the likelihood of more ambitious business formation, while most of the overall impact reflects entry into lower-barrier ventures. Moreover, business formation declines sharply with distance, reinforcing the interpretation that these peer effects are highly localized and driven by close, repeated social interaction among neighbors who actually reside nearby.

Finally, the paper contributes methodologically to the study of localized social interactions. By combining large-scale administrative data with a natural experiment based on residential turnover, I show that next-door neighbor changes can be leveraged to identify peer effects with far greater precision than aggregate neighborhood measures allow. This framework provides a template for studying other settings in which micro-level exposure and face-to-face interactions play a central role in shaping economic behavior.

Contribution to the literature. First, this paper contributes to the literature on entrepreneurial peer effects by showing that direct social interactions influence the decision to start a business. Prior studies document peer effects in selective contexts such as MBA cohorts (Lerner and Malmendier 2013; Hacamo and Kleiner 2024), school peers (Kacperczyk 2013; Mertz, Ronchi, and Salvestrini 2024), and workplace colleagues (Giannetti and Simonov 2009; Nanda and Sørensen 2010; Wallskog 2025). In contrast, I provide the first causal evidence of neighbor-to-neighbor entrepreneurial spillovers within the general residential population, demonstrating that entrepreneurial influence extends well beyond highly professionalized or elite environments.

Second, this paper advances the literature on neighborhood-based entrepreneurship by strengthening evidence on how local entrepreneurial environments influence individual entry decisions. Prior studies show that higher local concentrations of entrepreneurs are associated with greater business formation (Giannetti and Simonov 2009; Andersson and Larsson 2016; Markussen and Røed 2017; Guiso, Pistaferri, and Schivardi 2021), but these analyses typically capture correlations across broader geographic clusters rather than variation in direct interpersonal exposure. By leveraging the timing of new neighbor arrivals and linking individuals to their entrepreneurial outcomes in administrative data, I isolate much finer spatial variation—comparing residents who receive entrepreneurial newcomers with those who receive non-entrepreneurial ones within the same small area. The resulting estimates indicate that true neighbor-to-neighbor spillovers are economically meaningful but more localized and selective than aggregate associations would suggest, operating most strongly through immediate proximity, entrepreneurial quality, and gender homophily. Studying the general population of North Carolina—a context broadly representative of U.S. entrepreneurial patterns—also extends the external validity of this literature, which has largely drawn on Northern European settings (Hacamo and Kleiner 2024).

Third, the paper contributes to the literature on the determinants of occupational choice, which debates the relative importance of innate ability ("nature") and environmental influences ("nurture")

(Nicolaou et al. 2008; Zhang et al. 2009; Nicolaou and Shane 2010). Prior work emphasizes the family as the central channel of nurture, showing that parental characteristics strongly shape children's career trajectories (Lindquist, Sol, and Van Praag 2015; Mertz, Ronchi, and Salvestrini 2024). Beyond family influences, Guiso, Pistaferri, and Schivardi (2021) demonstrate that the broader local economic environment in which a person grows up also affects the likelihood of becoming an entrepreneur. I extend this perspective by showing that entrepreneurial capabilities can likewise be transmitted through direct neighborhood interactions. The evidence that exposure to entrepreneurial neighbors reduces barriers to entry and, in some cases, facilitates higher-quality ventures suggests that entrepreneurship is not solely an innate trait but a learnable skill shaped by local environments.

In addition, this paper contributes to urban economics by showing how residential mobility shapes local economic opportunities. A large literature emphasizes that the movement of people generates productivity gains through knowledge spillovers and the diffusion of skills. I highlight a specific channel: the arrival of entrepreneurial neighbors increases business formation among incumbent residents by transmitting entrepreneurial know-how. This finding complements recent work showing that constraints on housing mobility can suppress aggregate economic growth by limiting access to high-productivity locations (Glaeser and Gyourko 2018; Hsieh and Moretti 2019; Diamond and Gaubert 2022). The results suggest that such constraints may also dampen neighborhood-level entrepreneurial dynamism by restricting exposure to entrepreneurial peers.

A further contribution is to research on neighborhoods as engines of economic opportunity and mobility. The results suggest that neighborhoods are not only sites of residential sorting but also active spaces of entrepreneurial influence, where direct exposure to entrepreneurial neighbors can meaningfully affect economic outcomes. This perspective complements evidence that neighborhood environments shape lifetime earnings and intergenerational mobility (Chetty et al. 2014; Chetty, Hendren, and Katz 2016), and highlights that the local diffusion of entrepreneurial capabilities may be an overlooked mechanism through which neighborhoods influence long-run economic trajectories.

This paper is organized as follows. Section 2 describes the data and the construction of the merged dataset. Section 3 outlines the identification strategy and key assumptions. Section 4 presents the main results. Section 5 reports robustness checks addressing common challenges in estimating peer effects. Section 7 explores extensions on gender, race homophily, distance, and the quality of treatment and outcome. Section 8 concludes.

2. Data: Linking Neighbors, Businesses, and Homes

North Carolina Business Registration Records. I use administrative business registration records from the North Carolina Secretary of State to classify both incumbent residents and newly arriving neighbors according to their entrepreneurial experience and future business activity (measured over horizons ranging from two to ten years). These records are generated when individuals formally register new businesses. Although informal (unregistered) business activity exists, registration offers

significant advantages, including limited liability protection, access to tax benefits, and enhanced credibility with customers and financial institutions (Guzman and Stern 2020).

The data include the universe of business registrations in North Carolina from 2000 to 2024. For each registered entity, the records include the business name, legal structure (e.g., corporation, LLC, or partnership), address of record, registration date, legal status (active or inactive), the date of deactivation (if applicable), and the full name of the registered agent. I treat the registered agent as the founder of the business and, for the purposes of this study, define them as the entrepreneur.

North Carolina Voter Data. To identify incumbent neighbors at the time of each new neighbor's arrival, I use voter registration data from the North Carolina State Board of Elections (NCSBE). These publicly available records, released in quarterly snapshots dating back to 2005, provide comprehensive coverage of the adult population in the state. Each snapshot includes full name, residential address, age, race, sex, state of birth, and political affiliation. Because these data are maintained for official election purposes, they are highly accurate, with minimal entry error.

For this study, I construct a panel using the first available snapshot from each calendar year between 2005 and 2019. I merge the voter data with business registration records using full name matching to determine whether a registered voter has prior entrepreneurial experience or subsequently becomes an entrepreneur.

CoreLogic Dataset. I use two datasets from CoreLogic: the transaction (or transfer) file and the property assessment file. The transaction file captures residential real estate transactions in North Carolina and serves as the basis for identifying the arrival of new neighbors—a key component of the research design. Each transaction record includes the buyer's full name, property address, transaction date, and additional details such as sale price, mortgage usage, and whether the property is newly constructed. By merging buyer names with business registration records, I determine whether each arriving neighbor has prior entrepreneurial experience, which defines treatment assignment in my empirical framework.

The assessment file provides detailed property-level information for all residential parcels in North Carolina beginning in 2008.[1] Key variables include site address, geographic coordinates (latitude and longitude), owner names, year built, parcel size, building square footage, assessment value, and the number of bedrooms and bathrooms. I merge the assessment data with voter registration and business registration records using full name and address. This merge with the voter registration data ensures that the analysis focuses on owner-occupied properties rather than investment holdings. Combined with precise geolocation, the matching process allows me to identify individuals living exactly next door to one another and to construct an annual panel of residential neighbors.

The Final Sample. With the merged dataset, I observe registered voters in North Carolina along with their residential addresses and property characteristics at the beginning of each calendar

[1] As a result, specifications that incorporate property characteristics are restricted to the post-2008 period. Analyses that do not use these variables begin in 2005.

year. I use the state's business registration records to determine whether each individual has prior entrepreneurial experience. An individual is classified as an entrepreneur if their full name appears as the registered agent on a business filing, which I interpret as the founder of the business.

I identify nearby neighbors for each residential parcel using a procedure adapted from McCartney, Orellana-Li, and Zhang (2024). In particular, I define up to four neighbors per focal resident—two on each side—based on standardized address conventions in North Carolina. For each incumbent resident, I search for properties on the same street with house numbers two or four units above or below the focal address. For example, if the focal address is 6000, the immediate neighbors are 5998 and 6002 (next-door), and the two-next-door neighbors are 5996 and 6004, if they exist.

The dataset contains 4.7 million incumbent resident–year observations in which the focal individual receives a new nearby neighbor. Table 1 summarizes the sample.[2] The data cover 2,066 Census tracts—about 95 percent of all tracts in North Carolina—and roughly 75 percent of block groups, based on the 2010 Census. On average, a Census tract contributes 149 resident–year observations per year, and a Census block group contributes 63.

I define the variable *New Neighbor Entrepreneur* as a binary indicator equal to one if the incumbent resident has no prior entrepreneurial experience,[3] and the newly arrived neighbor has entrepreneurial experience. As defined, approximately 7% of incumbent residents in the sample receive a new neighbor with founding experience. Figure 1 plots the average treatment intensity at the Census tract level across North Carolina.

The dataset provides two key advantages. First, the hyperlocal definition of neighbors generates plausibly exogenous variation in social exposure, allowing me to implement a nearest-neighbor identification strategy and control for highly localized confounders. Second, the long panel structure allows me to trace both short- and long-run entrepreneurial responses.

3. Empirical Strategy: Identifying Entrepreneurial Spillovers

Estimating the causal effect of entrepreneurial neighbors on business formation requires overcoming two key identification challenges. First, neighbors are not randomly assigned. Individuals choose where to live, and households with entrepreneurial experience may disproportionately self-select into neighborhoods that are already conducive to entrepreneurship. As a result, any cross-sectional correlation between neighborhood composition and business formation risks confounding peer effects with endogenous sorting. Second, neighborhood characteristics—including demographics, amenities, and economic conditions—are tightly intertwined. People may appear to sort into neighborhoods with "like-minded" individuals, but this pattern may reflect shared preferences for local public goods such as school quality, safety, or commercial infrastructure, rather than interpersonal influence. In such cases, neighborhood composition is merely a proxy for latent amenity preferences, and any observed correlation may reflect contextual, rather than social, effects.

[2]Tables J.1 and J.2 in the Appendix summarize the assessment sample.

[3]This is captured by the variable Current Entrepreneurship, which equals 100 if the incumbent has prior entrepreneurial experience—measured before the arrival of the new neighbor—and zero otherwise.

Together, these challenges imply that a naive comparison across neighborhoods would likely yield biased estimates of peer effects. To address them, I exploit a natural experiment: the arrival of a new neighbor.

My identification strategy adapts the nearest-neighbor design of Bayer et al. (2022) to the setting of entrepreneurial exposure. Specifically, I compare incumbent homeowners who receive a new neighbor with entrepreneurial experience to similar incumbents who receive a non-entrepreneurial neighbor. The key insight is to focus only on incumbent-year observations in which a new neighbor moves in next door—generating exogenous variation in the immediate social environment, while holding fixed the incumbent's location and prior neighborhood composition.

Because households do not choose who moves in next door, and housing availability at the micro-geographic level is limited, the specific identity (and entrepreneurial background) of the arriving neighbor is plausibly idiosyncratic, conditional on block-level characteristics. This allows me to treat the arrival of an entrepreneurial neighbor as a quasi-random shock, conditional on fine-grained geographic and demographic controls. In this way, the design isolates variation in entrepreneurial exposure that is orthogonal to both incumbent characteristics and broader neighborhood-level selection.

The treatment variable, $\textit{New Neighbor Entrepreneur}_{i,t}$, equals one if the newly arriving neighbor has prior entrepreneurial experience—as measured by business registration records—and zero otherwise. I restrict the sample to incumbents without any entrepreneurial history prior to the arrival to ensure that treatment precedes potential outcomes. The outcome variable, $Y_{i,t+h}$, is an indicator equal to 100 if incumbent i initiates a new business within h years of the neighbor's arrival (where h ranges from 2 to 10, with $h = 5$ used in the baseline specification). Because every incumbent in the sample experiences the arrival of a new neighbor, identification derives from comparing residents exposed to entrepreneurial versus non-entrepreneurial entrants. This design thus conditions on the fact of a new arrival and isolates variation in the entrepreneurial background of the entrant. By holding fixed incumbent location and housing attributes, the strategy differences out stable characteristics of the property and local social environment, leaving only the quasi-random component of neighbor identity as the source of identifying variation. To test this, I estimate:

$$Y_{i,t+h} = \beta \cdot \textit{New Neighbor Entrepreneur}_{i,t} + \mathbf{X}_{i,t} \cdot \Theta + \eta_{g \times t} + \varepsilon_{i,t} \tag{1}$$

Here, $\textit{New Neighbor Entrepreneur}_{i,t}$ is the treatment indicator, and $\mathbf{X}_{i,t}$ is a vector of pre-treatment covariates including age, gender, political affiliation and property characteristics (e.g., assessed value, size, number of bedrooms and bathrooms). The specification includes Census block group-by-year fixed effects, $\eta_{g \times t}$, and the error term $\varepsilon_{i,t}$ is clustered at the Census tract level. The coefficient of interest, β, captures the causal effect of exposure to an entrepreneurial neighbor on the probability that the incumbent forms a new business.

This strategy compares treated and control incumbents within block group-by-year strata, ensuring that residents are evaluated relative to others living in the same micro-neighborhood and observed in the same calendar year. A Census block group is a highly localized unit of geography—containing, on average, about 587 households and 1,495 residents in the 2010 Census—so

this design effectively compares individuals living within a few city blocks of each other. The block group-by-year fixed effects absorb latent neighborhood amenities—such as school quality, business incentives, or safety—as well as contemporaneous local economic shocks. Remaining heterogeneity is addressed by covariates $\mathbf{X}_{i,t}$, which include incumbent demographics (age, race, gender, and political affiliation) and property characteristics. Robustness analyses go further by incorporating these demographics as fixed-effect interactions, thereby flexibly accounting for baseline differences in entrepreneurship across race (Fairlie 1999; Glaeser, Kerr, and Kerr 2015), gender (Field et al. 2016; Rocha and Van Praag 2020; Mertz, Ronchi, and Salvestrini 2024; Wallskog 2025), and political affiliation (Engelberg et al. 2022). Together, these controls ensure that, conditional on observables and fixed effects, variation in entrepreneurial exposure is plausibly orthogonal to unobserved determinants of entrepreneurship.

The identifying assumption is that, conditional on covariates and fixed effects, the entrepreneurial status of the incoming neighbor is as-good-as-random. This is credible for several reasons. First, while households choose where to live, they rarely control the identity of their immediate neighbors, and sellers have limited ability to target buyers based on nearby residents' unobservable traits. Within tight geographic and temporal windows, neighbor-incumbent matches are effectively idiosyncratic. Second, the richness of the data allows me to absorb fine-grained variation in neighborhood composition, leaving little scope for correlated sorting. Finally, empirical evidence supports the assumption: pre-treatment entrepreneurship trends are statistically indistinguishable between treated and control incumbents, event-study estimates show flat pre-trends, and observable characteristics are balanced across groups.

In sum, the design leverages plausibly exogenous shocks to the entrepreneurial composition of immediate neighbors, combined with granular fixed effects and rich controls, to identify peer effects in business formation. Under this framework, β is interpreted as the causal effect of gaining an entrepreneurial neighbor on the likelihood that an incumbent subsequently starts a business.

4. Main Results: Entrepreneurial Arrivals Inspire New Entrepreneurship

4.1. Baseline Results

Table 2 reports estimates of Equation 1 with a five-year horizon ($h = 5$). Each column introduces different configurations of fixed effects and control variables. Across all specifications, the estimated coefficient on *New Neighbor Entrepreneur*$_{i,t}$ is positive and statistically significant, indicating that incumbent residents are more likely to become entrepreneurs when a newly arriving neighbor has prior entrepreneurial experience.

The magnitude of the estimates is remarkably stable across specifications. Depending on the set of controls and fixed effects included, the effect ranges from 0.042 to 0.050 percentage points. Given that the mean of the dependent variable is approximately 0.99 percent, these results imply that exposure to an entrepreneurial neighbor increases the probability of business formation by about 4.2–5.0 percent relative to the baseline mean. This stability reinforces the idea that the effect is not driven by model specification or omitted-variable bias.

Although an effect of 0.042–0.050 percentage points may appear small in absolute terms, it is meaningful given the low baseline rate of entrepreneurship in the general population. To quantify the aggregate implications, consider column (8) of Table 2. Between 2005 and 2019, 4,672,279 incumbents entered the quasi-experimental sample—each living in a block group where at least one new nearby neighbor was an entrepreneur. Of this sample, 7.11 percent (332,199 incumbents) were treated. The baseline five-year business formation rate was 1.05 percent, and treated incumbents were 0.043 percentage points more likely to start a business than controls. Multiplying these figures implies that the arrival of entrepreneurial neighbors caused roughly 145 additional new businesses during this period.[4]

To examine persistence, Table 3 replicates the specifications of Table 2 but extends the outcome horizon to ten years ($h = 10$). The coefficients remain positive and statistically significant at the 1 percent level, and the magnitudes roughly double in absolute terms, ranging from 0.076 to 0.090 percentage points. Because the baseline mean of business formation over ten years is 1.96 percent, these effects translate into a 4.0–4.5 percent relative increase—remarkably similar to the medium-run estimates. This pattern suggests that the entrepreneurial spillover is not only immediate but also persistent, shaping incumbent behavior well into the long run.

Importantly, the results are consistent with the identifying assumption that, conditional on granular fixed effects, the assignment of entrepreneurial neighbors is as good as random. Under this assumption, the inclusion of additional covariates should not materially alter the estimates—a prediction borne out in both Table 2 and Table 3, where the coefficients remain virtually unchanged after accounting for age, race, gender, party affiliation, and various interactions with block-group fixed effects.

Another potential concern is that housing wealth and property characteristics may confound the estimated relationship, since home equity is a well-documented driver of entry into entrepreneurship (Adelino, Schoar, and Severino 2015; Schmalz, Sraer, and Thesmar 2017). To address this issue, Table 4 re-estimates the five-year specification using the assessment sample, which contains detailed property characteristics. Column (1) replicates column (8) of Table 2, but on the assessment sample. Subsequent columns sequentially add controls for home value, year built, lot size, square footage, and the number of bathrooms and bedrooms. Across specifications, the estimated effect is consistently around 0.062–0.068 percentage points, corresponding to a 6.2 percent relative increase over the baseline mean, and remains statistically significant at the 1 percent level. The exception is column (7), which includes the number of bedrooms and yields a larger coefficient (0.094 percentage points). As noted in the table, this variable has limited coverage and is concentrated in more urban areas, where treatment effects may plausibly be stronger.[5] Appendix Table J.6 shows that re-estimating the baseline specification on the assessment sample yields similarly stable effects that are slightly larger in magnitude than those in the main sample (Table 2).

[4]Calculation: $4,672,279 \times 0.0711 \times 0.000435$.

[5]Restricting the assessment sample to observations with non-missing bathroom counts yields larger estimates in the 0.08–0.09 percentage point range, as shown in Appendix Table J.7. These estimates are consistent with the stronger effects observed in column (7) of Table 4 and likely reflect the greater concentration of urban households in this restricted sample.

A final concern is that even Census block group fixed effects may be too coarse to fully absorb hyper-local neighborhood sorting or unobserved amenities. To address this concern, Appendix Tables J.8–J.10 replicate Tables 2–4, replacing Census block group fixed effects with Census block fixed effects and thereby comparing incumbent residents to neighbors on the same city block. Across all specifications—including the five- and ten-year outcomes in the main sample, as well as the housing-controlled assessment sample—the estimated peer effects remain statistically significant and are modestly larger in magnitude than in the baseline specifications. This pattern is consistent with attenuation bias arising from measurement error in coarser geographic fixed effects and indicates that the main results are not driven by unobserved micro-neighborhood heterogeneity.

Overall, these findings provide the first population-level evidence that entrepreneurial spillovers operate through direct neighborhood exposure. The robustness of the effect across time horizons, sample restrictions, and property-level controls underscores its substantive importance.

4.2. Spatial Scope of Local Exposure

The baseline design defines local entrepreneurial exposure using a deliberately conservative spatial criterion: for each arrival, treated incumbents are restricted to the four geographically closest neighbors located on the same side of the street. This choice prioritizes social proximity and limits heterogeneity in exposure intensity, but may exclude economically relevant interactions occurring at very short distances across the street. In this subsection, I assess whether the estimated effects depend on this restriction by expanding the spatial scope of local exposure to include the four closest neighbors regardless of street side, while holding the identifying structure, fixed effects, and outcome definitions fixed. This exercise evaluates the sensitivity of the main results to the spatial boundary used to define peer exposure, without altering the underlying estimand.

Tables J.11, J.12, and J.13 in the Appendix report estimates from the expanded spatial exposure definition, which includes the four geographically closest neighbors regardless of street side. Across all three tables, the estimated effect of exposure to an entrepreneurial neighbor remains positive, statistically significant, and stable across alternative configurations of fixed effects and controls. The magnitude of the estimates is slightly smaller than in the baseline specification that restricts exposure to neighbors on the same side of the street, but remains comparable in relative terms given similar baseline rates of business formation.

This attenuation in magnitude and precision is consistent with increased heterogeneity in exposure intensity when expanding the spatial boundary of treatment. Neighbors located across the street are plausibly less socially connected than those on the same side, leading to a noisier measure of effective peer exposure. Importantly, the persistence of statistically significant effects at both the five- and ten-year horizons, as well as in the housing-controlled assessment sample, indicates that the main findings are not sensitive to the precise spatial boundary used to define local exposure. If anything, this comparison suggests that the baseline design captures a more concentrated and socially salient form of entrepreneurial interaction.

4.3. Eliminating Local Contamination in the Control Group

The preceding analyses demonstrate that the estimated peer effects are robust to alternative spatial definitions of local exposure and to increasingly fine geographic fixed effects. A remaining concern, however, is that the control group itself may be indirectly exposed to entrepreneurial activity through very local interactions. In particular, incumbents classified as controls because their focal arrival is non-entrepreneurial may nonetheless be located near other entrepreneurial arrivals, potentially blurring the contrast between treated and control observations. Such local contamination would mechanically attenuate the estimated effects toward zero.

To address this concern, I refine the definition of the control group while preserving the original nearest-neighbor research design. I continue to define incumbents as the four geographically closest residents to each arrival on the same side of the street, and treatment status remains determined solely by whether the focal arrival has prior entrepreneurial experience. Among incumbents exposed to a non-entrepreneurial arrival, I additionally require that there be no entrepreneurial arrivals within a radius r meters—where $\in [25, 1000]$—during the year before, the year of, or the year after the focal arrival. Incumbents failing this condition are excluded from the control group. This restriction removes indirect entrepreneurial exposure among controls while leaving the underlying estimand and identification strategy unchanged.

Figures 2 and 3 plot the estimated effect of exposure to an entrepreneurial neighbor as a function of the exclusion radius r used to refine the control group, for the main and assessment samples respectively. In both samples, the estimated effect is remarkably stable for exclusion radii up to 250 meters, remaining close to the baseline estimate and statistically significant throughout. This stability suggests that very local contamination—within roughly a city block or its immediate surroundings—does not materially affect the baseline estimates.[6]

Beyond 250 meters, however, the estimated effects increase noticeably in both samples. This pattern is consistent with the presence of indirect entrepreneurial exposure at broader but still local spatial scales, which attenuates the baseline estimates when such observations are included in the control group. As the exclusion radius expands and potentially contaminated controls are removed, the estimated peer effects rise and then stabilize, albeit with wider confidence intervals reflecting the smaller effective sample size. Importantly, the qualitative pattern is highly similar across the main and assessment samples, reinforcing the interpretation that local contamination biases the baseline estimates toward zero rather than generating spurious effects.

5. Robustness: Addressing Peer Effects Challenges

Estimating peer effects is inherently challenging because the literature has highlighted several econometric obstacles to identification (Manski 1993; Sacerdote 2014). A first issue is the reflection problem, which arises when it is difficult to distinguish whether individuals' outcomes are driven by peers' behavior or by correlated responses to common shocks. In the present context, this concern

[6] Appendix Tables J.31 and J.32 report the corresponding regression estimates in detail.

is mitigated by the structure of the design. I define treatment as the arrival of a new entrepreneurial neighbor, ensuring that potential exposure occurs only after the incumbent has already chosen the residence. As in Wallskog (2025), this temporal ordering eliminates the concurrent nature of neighbor and incumbent entrepreneurship. Moreover, treatment status is based on predetermined characteristics—entrepreneurial activities undertaken by the neighbor prior to moving—rather than contemporaneous behavior. Consistent with this design, subsection 5.1 presents stacked event-study estimates showing no evidence of differential pre-trends prior to the neighbor's arrival, further supporting the causal interpretation.

A second concern relates to selection. Peer exposure may be correlated with an incumbent's unobserved entrepreneurial propensity if households sort non-randomly into neighborhoods. In this setting, however, treatment is generated by a quasi-random shock to neighborhood composition: the sale of a neighboring house. Because incumbents do not choose the identity of the buyer, the entrepreneurial background of the new neighbor is plausibly orthogonal to the incumbent's latent entrepreneurial potential. To further mitigate residual selection, I control for a rich set of demographic and housing characteristics, as well as incumbents' baseline traits, and I condition on finely defined geographic units. Moreover, in subsection 5.2 I present a series of balance tests using both individual-level and Census tract–level variables. These results show that treated and control groups are economically indistinguishable along observed characteristics, providing additional support for the identifying assumption.

A third issue is the possibility of confounding neighborhood effects with true peer spillovers. To address this, my preferred specification includes Census block group–by–year fixed effects, which absorb common local shocks at a very granular level. Identification thus comes from comparisons within the same neighborhood between incumbents exposed to an entrepreneurial neighbor and those whose new neighbor lacked entrepreneurial experience. In subsection 5.3 I provide evidence of the absence of pre-trends in housing prices within Census blocks and also disaggregate my main effect using the treatment interacted with the distance from the arrival of the new neighbor. Together, these results indicate that the estimated effects are not simply driven by correlated contextual changes at the neighborhood level, but instead reflect highly localized spillovers that decay with distance. This reinforces the interpretation of my findings as genuine peer effects rather than artifacts of broader neighborhood dynamics.

Finally, there is the concern of simultaneity and reverse causality. One might worry that entrepreneurial incumbents attract entrepreneurial neighbors, or that both respond to contemporaneous local opportunities. To address this, I restrict the sample to incumbents without prior entrepreneurial activity, thereby eliminating reverse causality from the incumbent side. In addition, I impose a lag structure by defining treatment only with respect to neighbors' past entrepreneurial activity, excluding current activity that could be jointly determined. Given this definition, the arrival of a new neighbor is determined independently of the incumbent's subsequent entrepreneurial decision. Importantly, incumbents have no influence over the identity or prior business history of the household purchasing the adjacent property, which further rules out strategic sorting on the part of treated residents. Moreover, the use of predetermined entrepreneurial histories ensures that treatment status is fixed prior to any potential interaction between incumbents and their new neighbors. As a result,

there is no remaining channel through which simultaneity could arise, and no additional adjustment is necessary within this empirical setting.

5.1. Entrepreneurship Pre-trends

Taking advantage of the granularity of the data, I implement a stacked event-study design, as used in recent work such as Cengiz et al. (2019). For each observation, I track outcomes from five years before to five years after the arrival of the new neighbor. Unlike the main analysis, which considers whether an incumbent starts a business within five years of exposure, here the outcome is defined as starting a business in the subsequent year.[7]

Formally, I estimate:

$$Y_{iae} = \sum_{h=-5, h\neq -1}^{h=5} \left[\delta_e \cdot \textit{New Neigh. Entrep}_{ia} \cdot \mathbb{1}\{e = h\}\right] + m_{g\times a\times e} + \gamma \cdot X_{iae} + U_{i,e} \tag{2}$$

The outcome variable Y_{iae} is an indicator equal to 100 if individual i starts a business in the year immediately following the arrival of a neighbor in year t. The specification takes the form of a staggered difference-in-differences design. The index a denotes the stack or sub-experiment defined by treatment timing (i.e., the calendar year in which observation i is first treated), and e denotes event time ($e = t - a$). The term $m_{g\times a\times e}$ captures Census block group (g) $\times$ stack (a) $\times$ event-time (e) fixed effects,[8] while X_{iae} is the vector of demographic and property controls. Standard errors, $U_{i,e}$, are clustered at the Census tract level.

This framework allows me to test for differential pre-trends prior to neighbor arrival and to trace the dynamic adjustment of incumbents' entrepreneurial activity following exposure.

Figure 4 and Figure I.2 report the stacked event-study estimates of incumbents' entrepreneurial entry relative to the arrival of a new entrepreneurial neighbor. Across both specifications, the coefficients on pre-treatment periods are small in magnitude and statistically indistinguishable from zero, providing no evidence of differential pre-trends. At the time of neighbor arrival ($e = 0$), there is a sharp and statistically significant increase in entrepreneurship, which persists in subsequent years at a stable level of roughly 0.12–0.16 percentage points. The similarity of results across specifications—with and without additional housing controls—suggests that the estimates are not driven by differences in observable property characteristics. Taken together, the evidence supports the validity of the identifying assumptions and indicates that exposure to a new entrepreneurial neighbor leads to a sustained increase in incumbents' likelihood of starting a business.

[7] The main outcome variable in the baseline specifications is an indicator equal to 100 if the incumbent starts a business within the following five years and 0 otherwise. In this section, I instead use a next-year indicator, also coded 100–0, to capture annual dynamics around treatment.

[8] For robustness, I also estimate versions of the model with separate fixed effects: $m_{g\times a}$ for group $\times$ stack and $v_{a\times e}$ for stack $\times$ event time. The results are virtually identical to those reported in the text.

5.2. Treated and Controls Groups are Economically Indistinguishable

Balance in Incumbent and Property Characteristics. To assess whether treatment is correlated with observable characteristics of incumbents or their properties, I conduct a series of balance tests. Specifically, I re-estimate the baseline specification but replace the dependent variable with demographic and property characteristics of the incumbent residents. Tables 5 and 6 present the main results, and Table J.33 in the Appendix reports additional specifications. All specifications include Census block group–by–year fixed effects, and standard errors are clustered at the Census tract level.

Table 5 presents estimates from the main sample using demographic characteristics as outcomes. Across age, gender, birthplace, and partisanship, the coefficients are small in magnitude and, although several are statistically significant, they are economically negligible relative to the sample means. For example, the coefficient on age is –0.088 years compared with an average of nearly 50 years, and differences in gender and political affiliation amount to only fractions of a percentage point. The only substantive difference is for current entrepreneurship: incumbents exposed to entrepreneurial neighbors are about 2.6 percentage points less likely to be entrepreneurs prior to treatment. The same pattern holds when I extend the specification to include housing property characteristics as controls, as shown in Table J.33 and 6.

Table 6 examines property characteristics as outcomes. Nearly all coefficients are statistically indistinguishable from zero. The few exceptions are homeownership, home age, and log home value. Treated incumbents are slightly less likely to be homeowners, but the effect is less than 2 percent of the mean. Their homes are about 0.49 years older, relative to an average of more than 30 years, and the coefficient on log home value is 0.013, corresponding to only a 1.3 percent difference at the mean. These magnitudes are economically trivial.

Taken together, the results indicate that observable demographic and property characteristics are largely uncorrelated with treatment assignment. The consistent negative coefficients on incumbents' baseline entrepreneurship suggest that entrepreneurial neighbors are somewhat more likely to locate in areas with fewer existing entrepreneurs. Rather than reflecting bias, this pattern strengthens the interpretation of the main results: entrepreneurial arrivals expand exposure among residents less accustomed to entrepreneurial peers, thereby amplifying the potential for spillovers.

Balance in Neighborhood Characteristics. To further assess the comparability of treated and control groups, I examine Census tract characteristics using data from the 2000 Decennial Census and the 2010 American Community Survey (ACS). Each individual is assigned the value of tract-level demographic and socioeconomic variables, measured either in levels (from the 2010 ACS) or as changes between the 2000 Census and the 2010 ACS. I then compute the average value of each characteristic separately for treated individuals—those exposed to an entrepreneurial new neighbor—and for control individuals—those exposed to a non-entrepreneurial neighbor—weighting by the number of individuals in each group.

I measure differences between groups using standardized (normalized) differences, defined as the difference in means divided by the pooled standard deviation of each characteristic. This scale-free

metric allows for consistent comparison across variables. Following standard practice, standardized differences near zero are interpreted as economically negligible, even when the corresponding t-statistics suggest statistical significance.

I implement this exercise in both levels and trends. Levels are calculated using ACS 2010 data, while trends are measured as changes between the 2000 Census and the 2010 ACS. The analysis is conducted separately for the main sample and the assessment sample.

Figures 5 and 6 for the main sample, and Figures I.3 and I.4 in the Appendix for the assessment sample, show that standardized differences—both in levels and in trends—are tightly centered around zero and consistently fall within the ± 0.1 benchmark. This indicates that treated and control groups are economically indistinguishable at the tract level, reinforcing the argument that the arrival of entrepreneurial neighbors is orthogonal to underlying neighborhood characteristics.

The balance tests at both the individual and neighborhood levels provide consistent evidence that treatment assignment is orthogonal to observable characteristics. At the micro level, incumbents exposed to entrepreneurial neighbors are economically indistinguishable from controls across demographic and property traits, with the only systematic difference being a slightly lower baseline rate of entrepreneurship among treated incumbents. At the tract level, comparisons of levels and trends in Census covariates show standardized differences well within conventional thresholds of economic significance. These results indicate that neither household sorting nor neighborhood composition is driving the observed exposure to entrepreneurial neighbors, lending further credibility to the identifying assumption underlying the empirical design.

5.3. Disentangling Peer Spillovers from Neighborhood Effects

House Price Pre-Trends and Entrepreneurial Exposure. While I previously showed that entrepreneurial entry exhibits no pre-trends among observed incumbents, that test necessarily conditions on residents who remain in place when a new neighbor arrives. Because households that moved out before the neighbor's arrival are not observed, a fully representative pre-trend analysis is not feasible. To further probe potential correlated contextual dynamics, I therefore examine block-level housing wealth—a well-documented determinant of entrepreneurial entry (Adelino, Schoar, and Severino 2015; Schmalz, Sraer, and Thesmar 2017). Differential trends in property values could bias the estimates if entrepreneurial neighbors systematically arrived in blocks already experiencing unusual appreciation or depreciation, conflating peer spillovers with wealth-driven entry. To rule out this concern, I compare the evolution of assessed housing values on treated and control blocks.

To implement this test, I follow the approach in McCartney, Orellana-Li, and Zhang (2024) and estimate event-study regressions of block-level assessed house prices. The specification regresses each block's mean assessed home value on a treatment indicator interacted with event-time dummies, while including block group–year fixed effects to absorb broader neighborhood shocks. Figure 7 presents the fitted values from this exercise with 95% confidence bands, allowing a visual assessment of whether treated blocks diverged from control blocks prior to the arrival of entrepreneurial neighbors.

Reassuringly, treated and control blocks display parallel trends in assessed prices during the three

years prior to the neighbor's arrival. This pattern is inconsistent with the concern that entrepreneurial neighbors disproportionately select into blocks that are already diverging from neighborhood trends. Rather, the evidence supports the interpretation that the subsequent entrepreneurial responses of incumbents reflect peer spillovers rather than contextual neighborhood effects.

Proximity and the Strength of Entrepreneurial Spillovers. Finally, I reestimate the main model allowing the effect of exposure to an entrepreneurial neighbor to vary by the physical distance between households. To do so, I calculate the distance between each incumbent and their new neighbor, and discretize it into three categories: 0–25 meters, 25–50 meters, and 50–120 meters. Columns (1) to (3) of Table 7 show the results for each subsample, while column (4) pools the full sample and interacts the main treatment indicator with the distance categories.

In the main sample (Table 7), the estimated peer effect is strongest when the entrepreneurial neighbor resides very close by—within 25 meters—and attenuates for neighbors living 25–50 meters or farther away. When I estimate the model on the full sample with interaction terms (column 4), the results confirm this pattern: the positive entrepreneurial spillover is concentrated in the most immediate neighbor range.

I replicate the same analysis in the assessment sample (Table J.34 in the Appendix), which additionally controls for housing characteristics. Again, the effect is largest when the entrepreneurial neighbor resides within 25 meters, and attenuates for neighbors located further away. To ensure that these patterns are not driven by the specific choice of distance cutoffs, Appendix Tables J.35 and J.36 replicate the analysis using distance terciles rather than fixed thresholds for the main and assessment samples, respectively. The results are qualitatively identical: spillover effects are strongest among the closest neighbors and decline with distance.

If correlated contextual effects were driving the results—for example, if all incumbents on certain blocks were simultaneously affected by broader local shocks—then the estimated effect of exposure to an entrepreneurial neighbor would be similar across distance bins. This is not what the data show. Instead, the effect is concentrated among the closest neighbors and attenuates sharply with distance, consistent with entrepreneurial spillovers operating through highly localized interactions rather than neighborhood-wide shocks.

Together with the absence of differential pre-trends in housing prices, these results indicate that correlated neighborhood dynamics are unlikely to explain the main findings. Housing wealth does not evolve differently prior to treatment, and entrepreneurial responses decay with physical proximity, supporting an interpretation of the estimated effects as genuine peer spillovers rather than artifacts of broader neighborhood context or sorting.

6. Mechanisms: The Role of Local Social Interaction

Having established the robustness of the baseline results and addressed key challenges to a peer-effects interpretation, this section investigates the mechanisms through which entrepreneurial spillovers operate. In particular, I examine whether the effect of exposure to an entrepreneurial

neighbor depends on the likelihood of direct, repeated social interaction. To do so, I exploit variation in whether arriving homeowners reside in their newly purchased properties or instead purchase without living there. While both types of arrivals generate identical housing transactions and neighborhood exposure, only resident arrivals plausibly engage in frequent, informal interactions with incumbent neighbors. If entrepreneurial spillovers are transmitted through local social contact—such as the exchange of information, observation of entrepreneurial activity, or informal advice—then the effect should be concentrated among neighborhoods receiving resident entrepreneurs rather than non-resident purchasers.

6.1. Resident and Non-Resident Homeowner Arrivals

Exploiting the granularity of the data, I distinguish between two types of homeowner arrivals: individuals who purchase a home and subsequently reside at that address ("resident arrivals") and those who purchase a home but do not live there ("non-resident arrivals"). To classify arrivals, I link the name and address of each new homeowner from CoreLogic property transaction records to individual voter registration files and track whether the homeowner is registered at the purchased address in the years following the transaction. An arrival is classified as resident if the homeowner's name and address match at any point between zero and five years after purchase; otherwise, the arrival is classified as non-resident. The results presented below use this baseline classification window.

Tables 8 and 9 report estimates separately for the full sample, resident arrivals only, and non-resident arrivals only. In both the main and assessment samples, the estimated effect of exposure to an entrepreneurial neighbor is substantially larger when the arriving homeowner resides in the purchased property. In the main sample, the effect nearly doubles when restricting attention to resident arrivals, while the estimate for non-resident arrivals is smaller in magnitude and less precisely estimated. A similar pattern emerges in the assessment sample, which additionally controls for housing characteristics. Together, these results indicate that entrepreneurial spillovers are strongest in settings where the arriving entrepreneur is likely to engage in repeated, direct interaction with incumbent neighbors, consistent with transmission through local social contact rather than purely passive neighborhood exposure. Appendix Tables J.37–J.40 show that the findings are robust to alternative definitions that restrict the matching window to two or five years.

Tables 10 and 11 present fully interacted specifications that allow the effect of exposure to an entrepreneurial neighbor to vary by the residency status of the arriving homeowner. In both the main and assessment samples, the estimated effect is substantially larger when the arriving entrepreneur resides in the purchased property. In contrast, the effect associated with non-resident arrivals is smaller in magnitude and statistically indistinguishable from zero in the main sample, and notably weaker in the assessment sample despite the inclusion of housing controls. The similarity of these patterns across samples indicates that the differential effects are not driven by housing characteristics or compositional differences across neighborhoods. Instead, the results reinforce the interpretation that entrepreneurial spillovers operate primarily through settings that facilitate repeated, direct social interaction between arriving entrepreneurs and incumbent residents.

While the results above show that entrepreneurial spillovers are substantially stronger when the arriving homeowner resides in the purchased property, some specifications continue to exhibit a positive effect associated with non-resident arrivals. Although smaller in magnitude, this pattern raises the possibility that spillovers may still operate through individuals who live in the neighborhood but do not own the property. To address this possibility, the next subsection focuses exclusively on non-resident homeowner arrivals and examines whether entrepreneurial exposure arises from individuals who occupy these properties as renters. Specifically, I identify likely renter arrivals residing in homes purchased by non-resident owners and replicate the main analysis using these renter occupants as the relevant source of entrepreneurial exposure, rather than the homeowner purchaser. This approach allows me to further isolate the role of direct residential presence in transmitting entrepreneurial spillovers.[9]

6.2. Non-Resident Homeowners and Renter Occupancy

This subsection focuses on non-resident homeowner arrivals and constructs a new sample that identifies individuals who occupy these properties as renters. Starting from the set of non-resident arrivals, I use the full property address from CoreLogic transaction records and examine voter registration files two years after the purchase to identify the individual(s) residing at the property. A property is classified as likely renter-occupied if the observed occupant's name differs from that of the homeowner and none of the occupants share the homeowner's last name. Using this classification, I define renter arrivals at the property level and assign entrepreneurial exposure based on whether any renter occupant has prior entrepreneurial experience. Incumbent neighbors surrounding these renter-occupied properties are then treated analogously to the baseline specification.

Tables 12 and 13 report estimates of the effect of exposure to entrepreneurial neighbors who occupy nearby properties as renters. In both the main and assessment samples, exposure to a renter with prior entrepreneurial experience is associated with a statistically significant increase in incumbent business formation over a five-year horizon. These estimates should be interpreted as conditioning on cases in which non-resident homeowner purchases result in actual residential occupancy. Consistent with this interpretation, the presence of an entrepreneurial renter is associated with a stronger and more precisely estimated effect than the average effect of non-resident homeowner arrivals, which pools both occupied and unoccupied properties. The results are stable across alternative fixed-effects structures and control sets, including highly saturated specifications that flexibly account for local demographic composition.[10] Taken together, these findings indicate that the presence of an entrepreneurial neighbor—rather than homeownership status per se—is the key driver of spillover effects, reinforcing the interpretation that direct residential presence and local

[9] Homeowners constitute the majority of self-employed workers. Using ACS microdata, Han and Park (2025) shows that approximately 75% of self-employed individuals are homeowners. Appendix Figures I.5A and I.5B replicate this pattern using ACS microdata from 2001–2023 and show that this share has remained remarkably stable over time, both nationally and in North Carolina, and for both men and women.

[10] Appendix Tables J.41-J.44 show that the renter-based results are robust to using a one-year post-purchase window to identify occupancy and to relaxing the name-matching restriction used to classify renter occupants.

interaction are central to the transmission of entrepreneurial activity.[11]

7. Extensions: Entrepreneurial Quality and Spatial Scope of Spillovers

7.1. Treatment and Outcome Quality in Entrepreneurial Spillovers

A further step in the analysis is to move beyond the quantity of spillovers and examine their quality. In both treatment and outcome, quality can be captured by whether a business is incorporated. Incorporation is more than a legal formality: incorporated firms benefit from limited liability and a separate legal identity, features that enable larger and riskier investments (Levine and Rubinstein 2017). Prior work also shows that incorporated ventures are more likely to grow and persist (Guzman and Stern 2015; Andrews et al. 2022). I use this measure to distinguish between neighbors who bring higher-quality entrepreneurial experience and incumbents who go on to create higher-quality firms. The analysis therefore asks whether exposure to incorporated entrepreneurs shifts not only the likelihood of entry but also the likelihood that new ventures themselves are incorporated.

Table 14 examines whether exposure to higher-quality entrepreneurs—defined as new neighbors who previously founded incorporated businesses—affects subsequent entrepreneurial entry among incumbents. Across specifications, the estimated coefficients are positive and consistently significant at conventional levels, with magnitudes around 0.06, or roughly 6 percent of the baseline mean. Compared to the baseline estimates in the main analysis (Table 2), the coefficients in Table 14 are larger in magnitude and remain statistically significant, suggesting that the quality of entrepreneurial experience plays an important role in shaping spillovers. In other words, not all entrepreneurial neighbors exert equal influence: incumbents are more likely to start a business when exposed to neighbors who have already taken the more ambitious step of incorporation.

In contrast, Table 15 reports results for exposure to lower-quality entrepreneurs, defined as neighbors who previously founded non-incorporated firms. Here, the coefficients are uniformly smaller—on the order of 0.03—and statistically indistinguishable from zero across all specifications. The absence of meaningful effects suggests that mere exposure to business formation, without the institutional features and growth potential associated with incorporation, is insufficient to trigger entrepreneurial spillovers. This divergence between high- and low-quality treatments indicates that incumbents respond to signals of credible, higher-value entrepreneurship rather than simply to the presence of any entrepreneurial activity nearby.

Table 16 directly compares the effects of high- and low-quality entrepreneurial exposure within a unified specification. Consistent with the separate estimates, exposure to high-quality entrepreneurs yields a larger point estimate (about 0.06 percentage points) than exposure to low-quality entrepreneurs (about 0.03 percentage points), though the difference is not statistically significant ($p = 0.49$). Table J.49 repeats this analysis for the assessment sample, which includes housing

[11] In the assessment sample, statistical significance attenuates in the most saturated specifications that additionally control for detailed housing characteristics such as bedrooms and bathrooms, reflecting both reduced sample size and limited residual variation. Point estimates remain positive throughout.

characteristics as controls, and produces highly similar results. Taken together, the evidence indicates that while high-quality entrepreneurs appear to generate larger and economically meaningful spillovers, the estimates remain too imprecise to reject equality across the two types of exposure.

Having established that spillovers are stronger when the entrepreneurial neighbor is of higher quality, I next turn to the quality of outcomes. Here, I maintain the original treatment definition but distinguish whether incumbents themselves go on to start incorporated (high-quality) businesses.

Table 17 examines this outcome. The dependent variable is an indicator equal to 100 if the incumbent starts an incorporated firm. Across all specifications, the estimated effects are small and statistically indistinguishable from zero, indicating that exposure to entrepreneurial neighbors does not translate into higher-quality new ventures. In other words, while neighbors may spur entry, they do not appear to induce incumbents to take the more ambitious step of incorporation.

Table 18 turns to the complementary case—whether incumbents start non-incorporated businesses. Here, the estimated effects are consistently positive and statistically significant at the 5 percent level, with magnitudes of about 0.04. Given a baseline mean of 0.84 (less than one percentage point), the evidence suggests that spillovers are concentrated in the creation of relatively low-quality firms. Results are robust to using the assessment sample with housing controls (Table J.50), which yields nearly identical magnitudes and significance patterns. This pattern is consistent with the view that the main mechanism operates by lowering barriers to entry rather than transmitting high-quality entrepreneurial know-how. As emphasized by Guiso, Pistaferri, and Schivardi (2021), learning opportunities can reduce the setup costs of starting a firm, thereby facilitating entry. Yet precisely because the channel works through easing initial barriers, it tends to generate ventures of lower quality, with limited prospects for growth or persistence.

The next step is to combine both dimensions of quality, asking whether exposure to high-quality entrepreneurial neighbors increases not only the likelihood of entry but also the probability that incumbents themselves create high-quality ventures.

Figure 8 shows that none of the treatment–outcome combinations are statistically significant at conventional levels within five years.[12] Nevertheless, the largest positive effect arises when incumbents are exposed to high-quality entrepreneurs but subsequently start low-quality businesses, although the estimates remain imprecise. This pattern echoes earlier results: entrepreneurial neighbors appear to spur entry, but the effect is concentrated in lower-quality outcomes.

Figure 9 extends the analysis to a ten-year horizon. In this case, exposure to high-quality entrepreneurial neighbors yields statistically significant effects at conventional levels for both high- and low-quality outcomes. The strongest effect—nearly 0.1 percentage point—occurs when high-quality neighbors lead incumbents to start low-quality businesses, consistent with spillovers operating primarily through reduced entry barriers. At the same time, there is also a positive and significant effect of high-quality neighbors on high-quality outcomes. Results are robust to using

[12]The lack of significant effects in Figure 8 may appear inconsistent with the significant aggregate effect in Table 18, but the two results are not contradictory. Table 18 reports the average effect of entrepreneurial neighbors on low-quality outcomes, whereas Figure 8 disaggregates the analysis into four mutually exclusive treatment–outcome combinations. This finer split reduces statistical power and inflates standard errors, so that even when the aggregate coefficient is significant, the corresponding cell-level estimates may not be (i.e., the estimates remain imprecise).

the assessment sample with housing controls (Figures I.8 and I.9), which reproduce nearly identical treatment–outcome patterns over both five- and ten-year horizons. These findings suggest that high-quality neighbors do more than lower barriers to entry: they also facilitate the accumulation of entrepreneurial skills. As Guiso, Pistaferri, and Schivardi (2021) emphasize, observing entrepreneurs in action allows individuals to learn how to organize production, manage employees, and interact with suppliers and customers, thereby equipping them to undertake more ambitious ventures.

Taken together, the results highlight an asymmetry in entrepreneurial spillovers. Exposure to entrepreneurial neighbors increases entry, but the effects differ by the quality of the arriving entrepreneur. Point estimates indicate that high-quality neighbors exert the strongest influence, while low-quality neighbors have little effect, although the difference between them is not statistically significant. On the outcome side, most spillovers are concentrated in the formation of non-incorporated, lower-quality businesses, consistent with a mechanism of reduced entry barriers. Over longer horizons, however, high-quality neighbors also appear to raise the likelihood that incumbents start incorporated firms. This pattern suggests that beyond easing entry, high-quality neighbors may foster the accumulation of entrepreneurial skills—through observation and imitation—that enable more ambitious forms of entrepreneurship. In sum, entrepreneurial spillovers are not mechanical: both the treatment and the outcome dimensions of quality shape their strength, direction, and economic relevance.

7.2. Entrepreneurial Exposure in the Wider Urban Radius

While the baseline analysis focuses on immediate block-face neighbors (up to two houses on either side), entrepreneurial interactions may extend beyond direct adjacency. To investigate the spatial reach of peer effects, I construct an alternative dataset that includes all incumbent residents living within up to 40 next-door properties on each side of the focal arrival, restricting attention to residents on the same side of the street. This "ring method" sample increases coverage substantially, yielding over 27 million incumbent–year observations compared to 4.7 million in the baseline design. The advantage of this approach is twofold: it expands the scope of analysis to capture broader within-block spillovers, and it enables a systematic test of how treatment effects decay with ordinal neighbor distance.

The ring sample partitions incumbents into concentric distance bins of 25 meters each. For each bin, I compute demographic characteristics to assess comparability across space. Table J.51 summarizes the full ring sample (2005–2019). Demographic composition varies smoothly across bins: average age ranges from 48 to 49 years, the male share is stable at roughly 44–45 percent, and partisan affiliation changes gradually with distance. Racial composition exhibits a modest gradient, reflecting broader residential sorting patterns rather than sharp discontinuities at any particular cutoff. Overall, these patterns suggest that nearby bins form a plausibly comparable set of incumbents for estimating distance-specific treatment effects.[13]

To incorporate housing characteristics, I rely on an assessment subsample that begins in 2008

[13]The 225-meter radius corresponds roughly to one to two city blocks in North Carolina and is intended to capture the scale of highly localized neighborhood interactions. The final bin pools residents located beyond this threshold.

due to CoreLogic coverage of property data. Table J.52 reports demographic characteristics by distance bin for this assessment sample, while Table J.53 reports housing attributes for the same bins. As in the full ring sample, demographic composition varies smoothly with distance: average age remains close to 49 years, the male share is stable at approximately 44–45 percent, and partisan affiliation changes gradually across bins. Housing characteristics exhibit modest gradients, with average assessed values increasing slightly with distance, while lot size, year built, and square footage display no sharp discontinuities. Overall, the assessment sample mirrors the main sample in exhibiting smooth spatial variation, supporting its use for specifications that additionally control for housing characteristics.

To estimate treatment effects, I adapt the baseline specification to the ring setting. For each incumbent i at distance d from a newly arrived neighbor in year t, I estimate:

$$Y_{i,t+h} = \gamma \cdot T_i + \sum_{d \neq d_0} \alpha_d \cdot D_{id} + \sum_{d \neq d_0} \beta_d \cdot \left(T_i \times D_{id}\right) + \eta_{g \times t} + \lambda \cdot X_{i,t} + \epsilon_i \tag{3}$$

where $Y_{i,t+h}$ equals 100 if the incumbent starts a business within h years of arrival and 0 otherwise, T_i equals one if the new neighbor is an entrepreneur, and D_{id} are distance-bin indicators. One bin d_0 is omitted as the reference category, so all coefficients are interpreted relative to non-entrepreneurial arrivals in that bin. The main parameter of interest, γ, captures the treatment effect in the omitted bin, while the interaction terms β_d trace how effects vary across distance.

All specifications include highly granular location–time fixed effects. For each figure, I report estimates that alternatively absorb Census block group–by–year fixed effects or Census block–by–year fixed effects. These alternative structures address the possibility that a given physical distance corresponds to different numbers of nearby neighbors across settings with varying residential density. Standard errors are clustered at the Census tract level. I report two versions of the specification: one that controls only for incumbent demographics (age, gender, race, and political affiliation), and another that additionally includes housing characteristics (lot size, assessed value, and year built). The corresponding results are displayed in Figures 10 and 11, respectively.

Figures 10 and 11 plot five-year treatment effects by distance using the ring-method specification. Across both fixed-effect structures and in both the main and assessment samples, exposure to an entrepreneurial arrival is associated with positive effects at very short distances. However, the estimates do not exhibit a clear or monotonic pattern of attenuation with distance. While the largest point estimates are concentrated among the closest bins, effects remain positive at several intermediate distances, and the spatial gradient is considerably flatter than in the nearest-neighbor design. This contrast suggests that distance alone is an imperfect proxy for effective exposure in the broader ring sample.

A likely explanation is that the ring construction pools arrivals who ultimately reside in the purchased property together with those who do not. Physical proximity therefore conflates two distinct channels of exposure: direct residential presence, which facilitates repeated interaction, and ownership or transient occupancy, which may generate little local contact. Averaging across these heterogeneous channels attenuates the spatial decay pattern observed in the baseline analysis.

Motivated by this distinction, and using the definitions of resident and non-resident arrivals

introduced in Section 6.1, I divide the ring sample accordingly. Figures 12 and 13 report the resulting estimates for resident and non-resident arrivals in the main sample (controlling only for demographics) and in the assessment sample (which additionally controls for housing characteristics), respectively. Separating arrivals by residential status allows the spatial pattern of spillovers to be traced more directly.[14]

The figures reveal a sharp contrast between resident and non-resident arrivals in the spatial pattern of spillovers. When entrepreneurial neighbors reside in the purchased property, the estimated effects are concentrated at very short distances and attenuate rapidly as distance increases, closely mirroring the steep decay documented in the baseline nearest-neighbor design. By contrast, when arrivals do not reside in the property, the estimated effects exhibit little systematic variation with distance and do not display a clear spatial gradient. This divergence is present in both the main and assessment samples and is unchanged by the inclusion of housing controls.

Overall, the ring-method analysis reinforces the conclusion that entrepreneurial exposure is intensely local. Business formation responses are concentrated among incumbents living immediately adjacent to an entrepreneurial newcomer, with effects dissipating quickly beyond the same block face. These findings complement the baseline results and situate the mechanism within a broader urban context, showing that entrepreneurial spillovers operate at the scale of neighbors who are, quite literally, next door.

8. Conclusions: Local Interactions and the Spread of Opportunity

This paper provides the first causal evidence that entrepreneurial activity diffuses through the arrival of new neighbors. Using a novel, large-scale dataset that links business registrations, voter records, and housing transactions for the state of North Carolina, I show that entrepreneurial spillovers operate in everyday residential settings. By documenting these effects in a general population and at very fine spatial scales, the analysis contributes to the literatures on regional entrepreneurship and peer effects, highlighting the role of direct, face-to-face residential interaction in shaping economic decision-making.

The analysis leverages quasi-random variation in the entrepreneurial background of arriving neighbors within a highly granular spatial framework. By comparing incumbents in narrowly defined residential environments and subjecting the estimates to a comprehensive set of robustness checks, the paper addresses concerns related to endogenous sorting, shared local shocks, and contamination of the control group. The results are stable across alternative definitions of neighborhood exposure and sample restrictions, reinforcing the interpretation that the observed spillovers reflect a causal effect of local entrepreneurial presence on business formation.

The paper also sheds light on the mechanism underlying these spillovers. Entrepreneurial influence is concentrated among arrivals who actually reside in the neighborhood, rather than among non-resident homeowners. When properties purchased by non-resident owners are later occupied by entrepreneurial renters, the spillover effects re-emerge. These patterns indicate that residential

[14] Appendix Tables J.54 through J.59 report summary statistics for the samples split by residential status.

presence—rather than ownership or investment activity—is essential for transmission, consistent with a mechanism based on repeated, low-cost local interaction.

Extensions further illuminate the scope and localization of these spillovers. Most effects are concentrated in unincorporated, lower-cost businesses, suggesting that exposure primarily facilitates entry into simpler forms of self-employment. At the same time, exposure to high-quality entrepreneurs is associated with a greater likelihood of starting incorporated firms, and this relationship strengthens over longer horizons, indicating that some neighbors adopt more ambitious entrepreneurial forms. Spillovers also decline sharply with physical distance and are concentrated among immediate neighbors, fading beyond a small radius. Importantly, this spatial decay is observed only when entrepreneurial arrivals reside nearby; no comparable pattern emerges for non-resident arrivals. Together, these findings rule out explanations based on shared neighborhood shocks and instead point to direct social exposure as the primary channel.

Taken together, the results demonstrate that entrepreneurship is not solely the outcome of individual characteristics or broad local conditions, but also of highly localized peer effects. The arrival of a single entrepreneurial household can alter the economic trajectories of nearby residents. By documenting this mechanism in a large and representative population, the paper deepens our understanding of how entrepreneurial activity propagates across space and situates neighborhoods as active environments in which economic opportunity is formed.

Several questions remain for future research. One direction is to examine how the strength of neighborhood spillovers depends on institutional and policy environments—for example, whether credit access, zoning regulations, or residential mobility amplify or dampen the diffusion of entrepreneurship. Another is to study the longer-run consequences of localized spillovers for neighborhood trajectories, including property values, income distribution, and migration. More broadly, linking household-level exposure to entrepreneurial peers with firm performance, innovation, or job creation could clarify how micro-level social interactions propagate into aggregate economic outcomes. In this sense, the findings connect to the broader urban economics perspective in which neighborhoods function as engines of mobility, highlighting entrepreneurship as a distinctive pathway through which local social exposure shapes economic opportunity.

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Figures

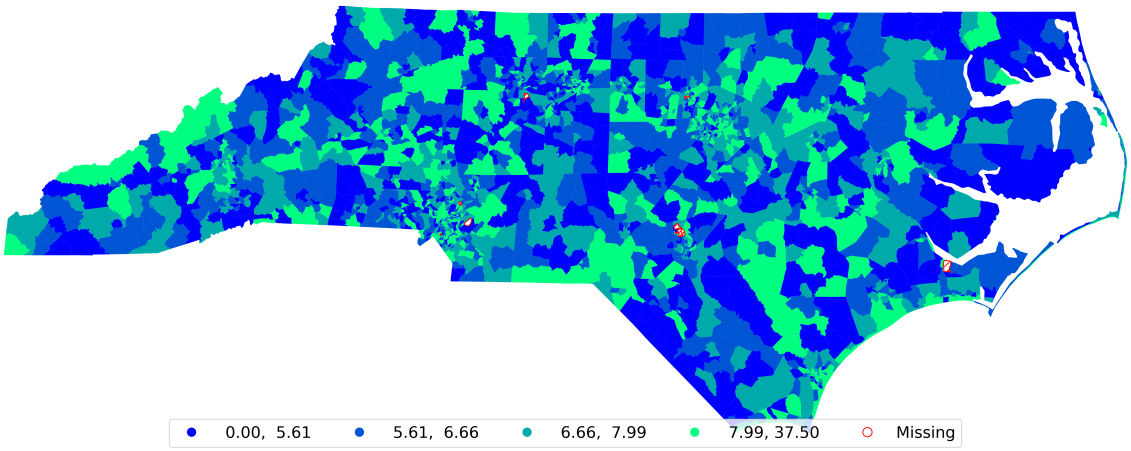


FIGURE 1. Average Entrepreneurial Neighbor Treatment by Census Tract in North Carolina

The figure shows the average treatment intensity—defined as the arrival of a new neighbor with prior entrepreneurial experience—in the estimation sample, aggregated to the census tract level across North Carolina.

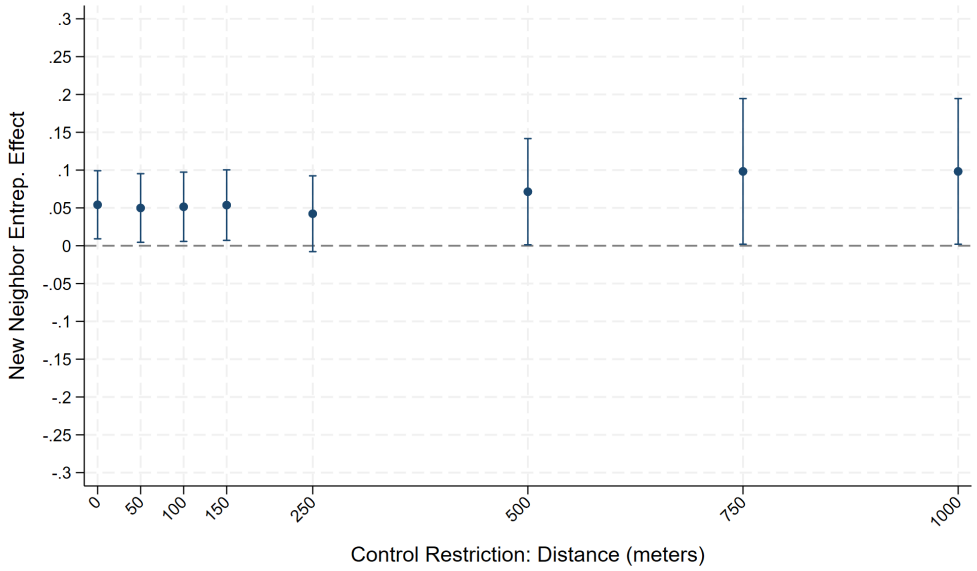


FIGURE 2. Effect of Entrepreneurial Neighbors by Control Exclusion Radius — Main Sample

The figure plots estimates of the effect of exposure to an entrepreneurial neighbor under alternative definitions of the control group. The specification is identical to Equation 1 with $h = 5$—including the treatment definition, Census block group–by–year fixed effects, and incumbent controls—except for the control group definition. Control incumbents are required to have no entrepreneurial arrivals within a radius r meters during the year before, the year of, or the year after the focal arrival. Each point corresponds to a separate estimation using a different value of r; vertical bars denote 95 percent confidence intervals.

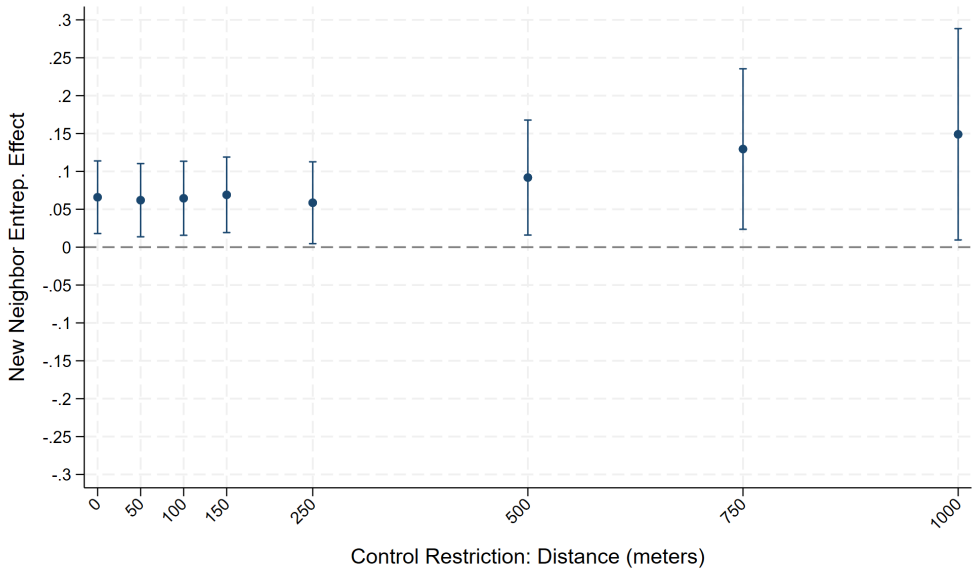


FIGURE 3. Effect of Entrepreneurial Neighbors by Control Exclusion Radius — Assessment Sample

This figure replicates the specification in Figure 2, but additionally controls for housing characteristics, including lot size, assessed value, and year built.

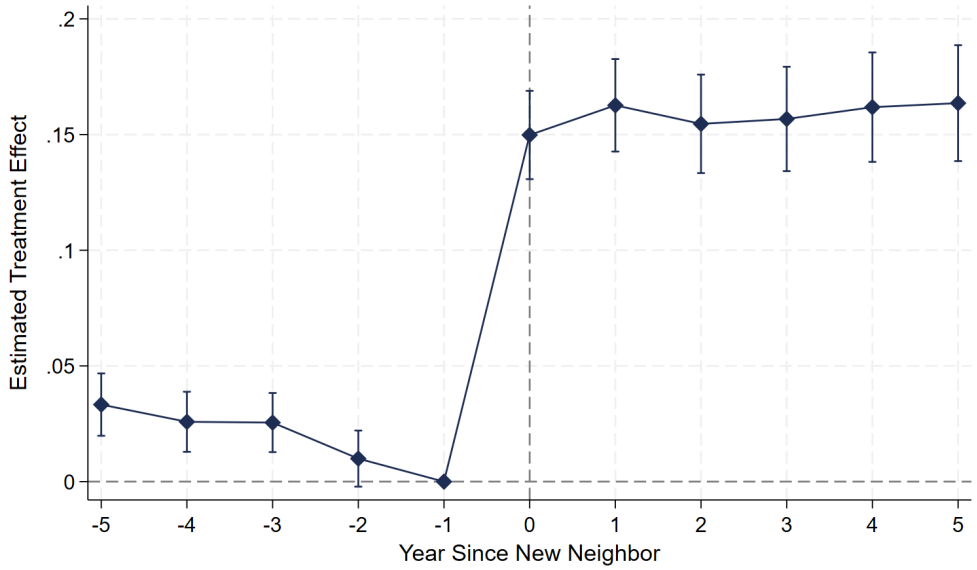


FIGURE 4. Event-Study Estimates of Incumbent Entrepreneurship Around the Arrival of a New Entrepreneurial Neighbor, Main Specification

This figure plots coefficients from the stacked event-study regression in Equation 2, where the dependent variable equals 100 if the incumbent starts a business in the year following event time e. Treatment is defined as the arrival of a new neighbor with prior entrepreneurial activity. The omitted period is $e = -1$. All specifications include Census block group–by–year fixed effects and demographic controls for age, gender, race, and party affiliation. Standard errors are clustered at the Census tract level.

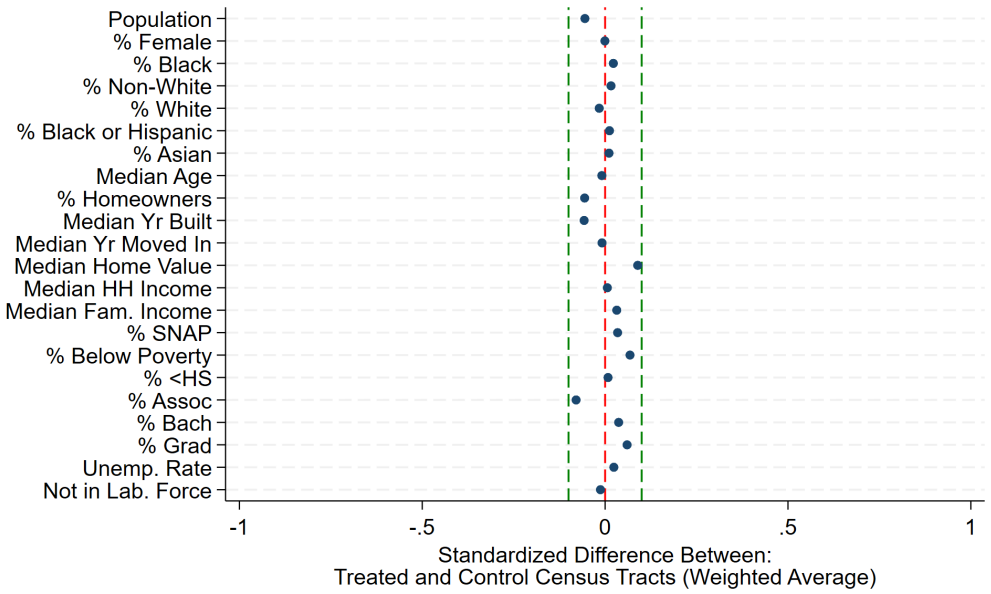


FIGURE 5. Balance of Census Tract Characteristics—Levels, Main Sample

This figure shows balance in covariates between Census tracts, averaging over treated tracts (receiving an entrepreneurial new neighbor) and control tracts (receiving a non-entrepreneurial new neighbor). Normalized differences are computed by subtracting the average of each characteristic by treatment status and dividing by the pooled standard deviation. Level estimates use ACS 2010 data. Green dashed lines mark ±0.1 of the standardized difference.

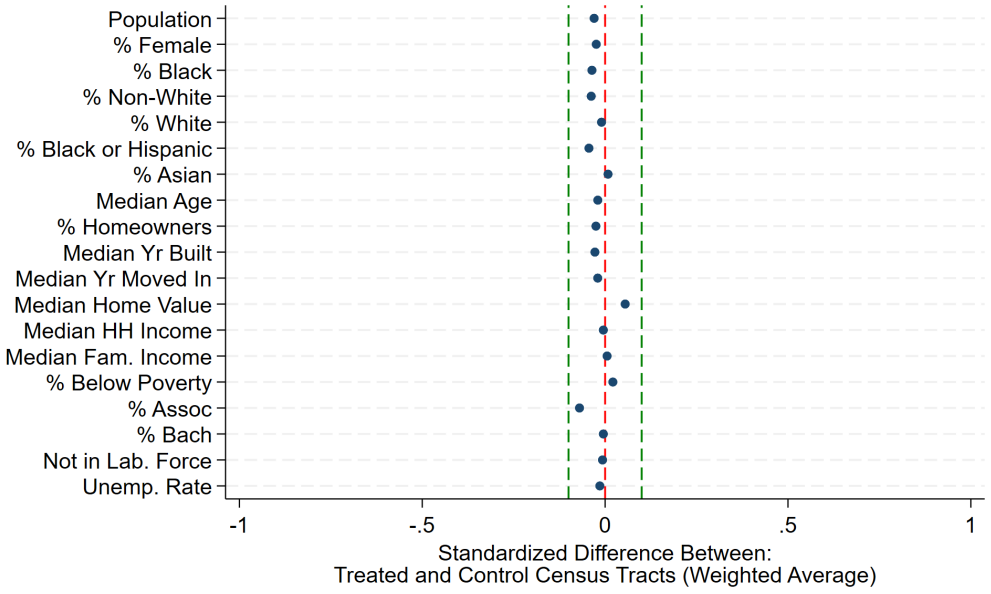


FIGURE 6. Balance of Census Tract Characteristics—Trends, Main Sample

This figure compares pre-treatment trends in Census tract covariates for treated and control tracts. Normalized differences are computed as in Figure 5. Trends are measured using changes between the 2000 Decennial Census and the 2010 ACS. Green dashed lines mark ±0.1 of the standardized difference.

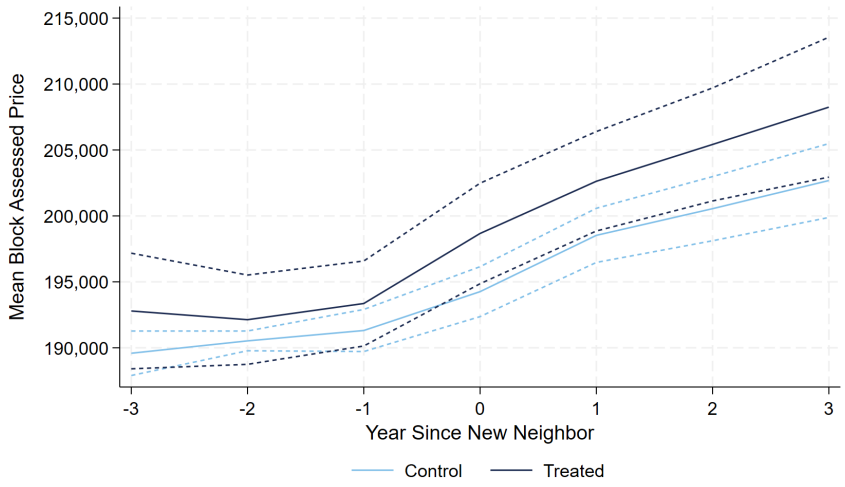


FIGURE 7. Block-level house prices: Treated versus Controls blocks, Assessment Sample

This figure plots average assessed home values on Census blocks where treated incumbents live and blocks where control incumbents live in the three years before and after the arrival of a new neighbor. We calculate each block's mean assessed home value and regress it on a treatment dummy interacted with an event-time dummy, including Census block group-year fixed effects. The fitted values are plotted separately for treated and control blocks, along with 95% confidence bands shown in dashed lines.

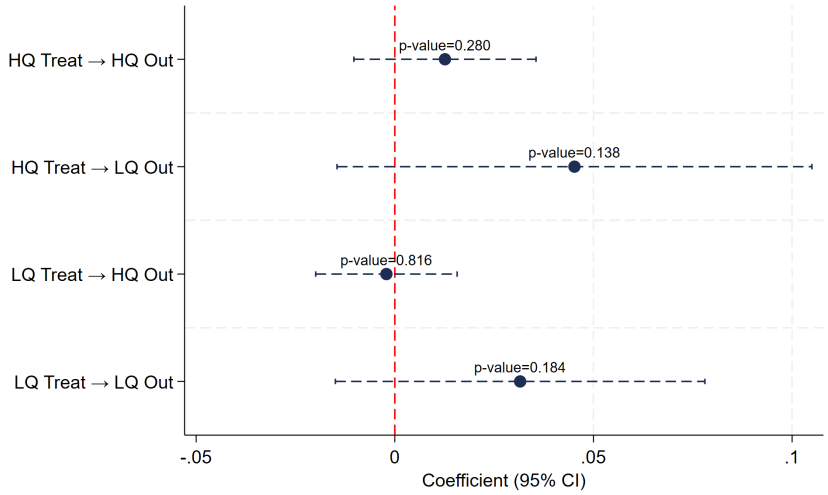


FIGURE 8. Effect of High- and Low-Quality Entrepreneurial Neighbors on High- and Low-Quality Outcomes, Five-Year Horizon

This figure plots coefficients from Equation 1 with $h = 5$ and their 95% confidence bands. The treatment indicators are defined as exposure to a new neighbor who previously founded either an incorporated firm (high-quality treatment, HQ Treat) or a non-incorporated firm (low-quality treatment, LQ Treat). The outcomes are defined as whether the incumbent subsequently starts an incorporated (HQ Out) or non-incorporated (LQ Out) business within five years of the neighbor's arrival. All models include Census block group–by–year fixed effects and controls for incumbents' age, race, gender, and political affiliation. Standard errors are clustered at the Census tract level.

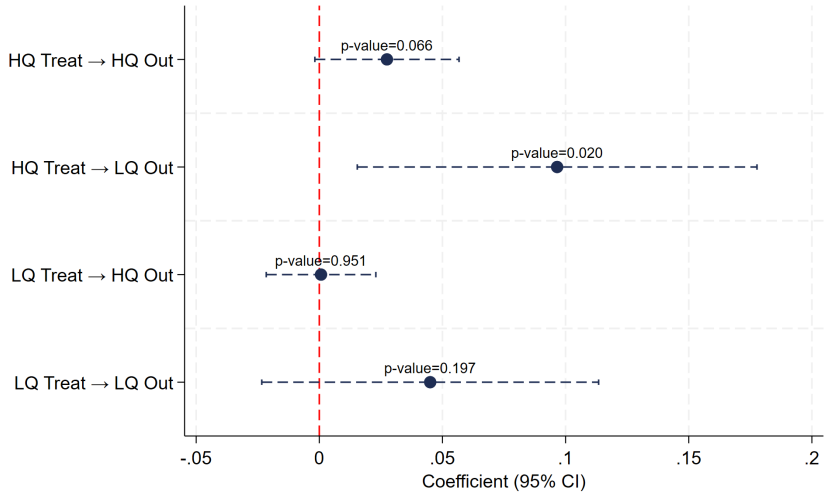


FIGURE 9. Effect of High- and Low-Quality Entrepreneurial Neighbors on High- and Low-Quality Outcomes, Ten-Year Horizon

This figure replicates Figure 8 using a ten-year horizon ($h = 10$). The remaining notes are the same as in Figure 8.

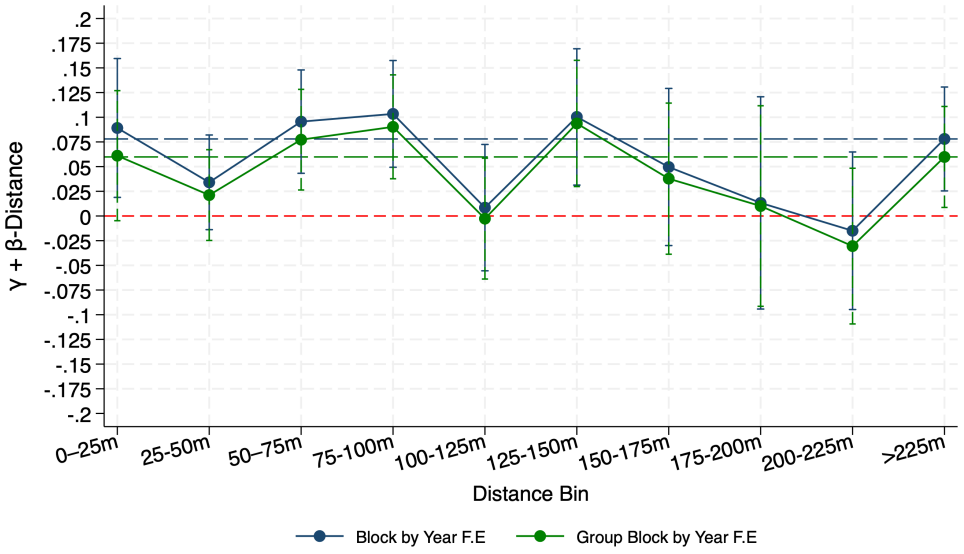


FIGURE 10. Five-Year Treatment Effects by Distance: Demographics-Only Specification

This figure plots treatment effects estimated from the ring-method specification (Equation 3), where the outcome equals one if the incumbent starts a business within five years of the neighbor's arrival. Distance bins are defined in 25-meter increments, with the > 225 meter bin omitted as the reference category. The figure reports estimates from specifications that alternatively include Census block group–by–year fixed effects and Census block–by–year fixed effects, along with controls for age, gender, race, and political affiliation. Standard errors are clustered at the Census tract level, and 95 percent confidence intervals are shown.

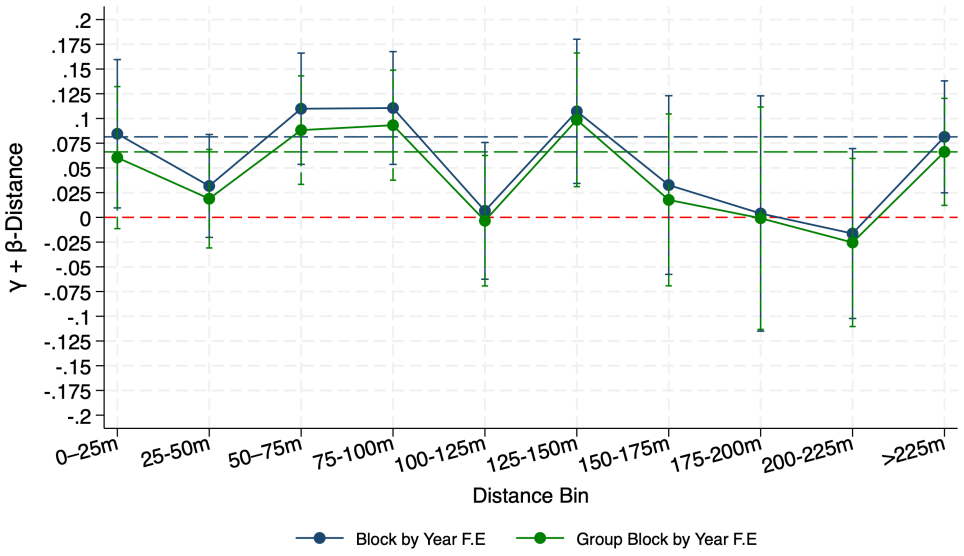


FIGURE 11. Five-Year Treatment Effects by Distance: Demographics and Housing Controls

This figure replicates Figure 10 using the assessment sample (2008–2019) and adds controls for housing characteristics (lot size, assessed value, and year built). The outcome, bin definitions, omitted category, and fixed effects are the same as in Figure 10. Standard errors are clustered at the Census tract level, and 95 percent confidence intervals are shown.

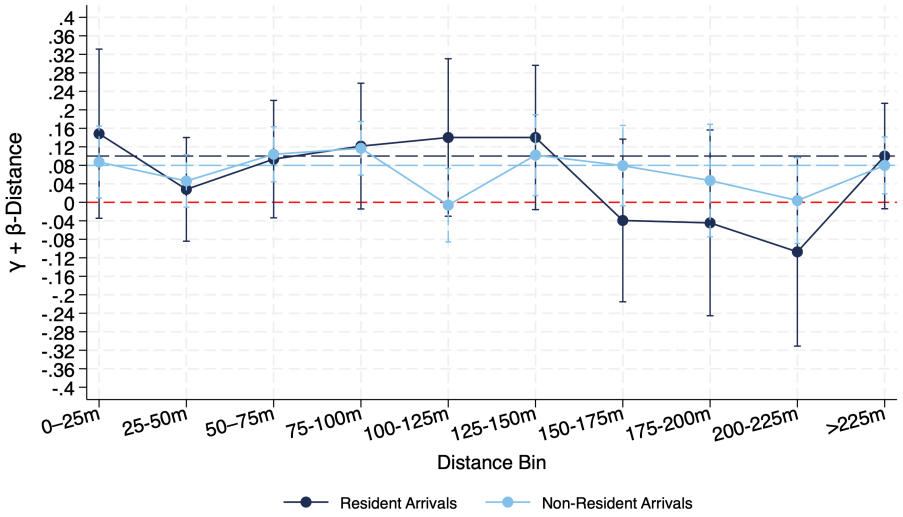


FIGURE 12. Five-Year Treatment Effects by Distance: Resident vs. Non-Resident Arrivals

This figure plots treatment effects estimated from the ring-method specification (Equation 3), where the outcome equals one if the incumbent starts a business within five years of the neighbor's arrival. Distance bins are defined in 25-meter increments, with the > 225 meter bin omitted as the reference category. All specifications include Census block group–by–year fixed effects and controls for age, gender, race, and political affiliation. Standard errors are clustered at the Census tract level, and 95 percent confidence intervals are shown.

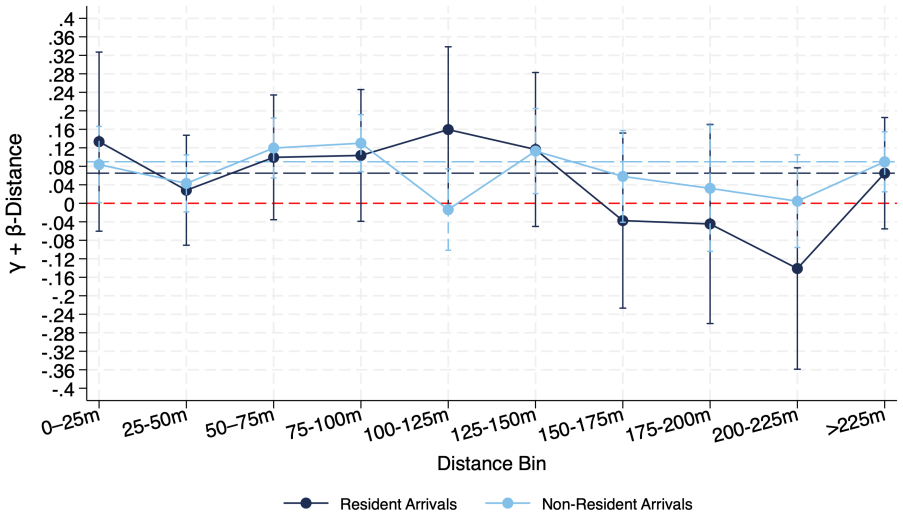


FIGURE 13. Five-Year Treatment Effects by Distance: Resident vs. Non-Resident Arrivals (with Housing Controls)

This figure replicates Figure 12 using the assessment sample (2008–2019) and adds controls for housing characteristics (lot size, assessed value, and year built). The outcome, bin definitions, omitted category, and fixed effects are the same as in Figure 12. Standard errors are clustered at the Census tract level, and 95 percent confidence intervals are shown.

Tables

TABLE 1. Describing the Sample of Incumbent Residents Who Got New Nearby Neighbors-Full Sample

	Count	Mean	Std Dev
Dependent Variable			
Star a Business within 2 Years (0-100)	4,769,797	0.35	5.88
Star a Business within 5 Years (0-100)	4,769,797	1.00	9.93
Star a Business within 10 Years (0-100)	4,769,797	1.97	13.89
Current Resident Politics			
Democrat	4,769,797	0.39	0.49
Republican	4,769,797	0.34	0.47
Unaffiliated	4,769,797	0.26	0.44
Current Resident Demographics			
Gender: Female	4,769,797	0.53	0.50
Gender: Male	4,769,797	0.46	0.50
Gender: Other	4,769,797	0.01	0.11
Race: White	4,769,797	0.78	0.41
Race: Black	4,769,797	0.15	0.36
Race: Other	4,769,797	0.06	0.24
Age	4,769,600	49.75	19.20
Born in NC	4,769,797	0.33	0.47
Property Characteristics			
Homeownership Arriver	4,769,797	0.54	0.50
Current Entrepreneurship			
Current Entrepreneur (0-100)	4,769,797	1.90	13.66
New Nearby Neighbor Entrepreneurship			
New Nbr Entrepreneur	4,769,797	0.07	0.26

This table summarizes the sample of resident–year observations in which a new nearby neighbor arrives, defined as individuals living up to two houses away on either side of the focal property. Both residents must appear in the merged dataset, which combines North Carolina business registrations, voter files, and CoreLogic property data. Property characteristics come from CoreLogic; political affiliation and demographics from voter files. Homeownership Arriver equals one if the new neighbor occupies the purchased property. Current Entrepreneurship equals 100 if the incumbent has prior entrepreneurial experience (measured before the neighbor's arrival) and zero otherwise. New Nbr Entrepreneur equals one if the incumbent lacks such experience but the new neighbor has it.

TABLE 2. Effect of Entrepreneurial Neighbors on Incumbent Business Formation after Five Years

	1	2	3	4	5	6	7	8
New Nbr Entrepreneur	0.04280**	0.04192**	0.04787**	0.04436**	0.05000**	0.04265**	0.04500**	0.04352**
	(0.02030)	(0.02031)	(0.02029)	(0.02029)	(0.02029)	(0.02030)	(0.02052)	(0.02030)
Fixed Effects:								
Year by Block Group	X	X		X		X		X
Year by Block Group by Race			X					
Year by Block Group by Gender					X			
Year by Block Group by Party							X	
Controls:								
Age	X	X	X	X	X	X	X	X
Race		X						X
Gender				X				X
Party						X		X
No. of Observations	4,672,279	4,672,279	4,672,167	4,672,279	4,672,107	4,672,279	4,672,028	4,672,279
Dependent Variable Mean	0.9917	0.9917	0.9917	0.9917	0.9918	0.9917	0.9917	0.9917
New Nbr. Entrep. Mean	0.0711	0.0711	0.0711	0.0711	0.0711	0.0711	0.0711	0.0711
FE Cells	67,064	67,064	152,764	67,064	138,576	67,064	177,104	67,064
R^2	0.0152	0.0157	0.0449	0.0162	0.0335	0.0152	0.0371	0.0167

This table reports estimates of Equation 1 with $h = 5$, where outcomes are measured over a five-year horizon following the arrival of a new neighbor. Specifically, $h = 5$ indicates that the dependent variable captures whether an incumbent resident starts a business within five years of the neighbor's arrival. The treatment indicator, *New Neighbor Entrepreneur*$_{i,t}$, equals one if the incoming neighbor has prior entrepreneurial experience (as measured by past business registration) and zero otherwise. The sample is restricted to incumbents with no entrepreneurial history prior to the neighbor's arrival. The regression compares treated and control incumbents residing within the same Census block group and observed in the same calendar year. Census block groups—the level at which fixed effects are defined—contain on average about 500 to 600 households in North Carolina, while entrepreneurial exposure is defined at the level of immediate next-door neighbors within those groups, ensuring that identification is based on highly localized variation. Standard errors, clustered at the Census tract level, are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE 3. Effect of Entrepreneurial Neighbors on Incumbent Business Formation after Ten Years

	1	2	3	4	5	6	7	8
New Nbr Entrepreneur	0.07785***	0.07624***	0.08074***	0.08079***	0.08956***	0.07755***	0.08089***	0.07921***
	(0.02917)	(0.02918)	(0.02922)	(0.02926)	(0.02916)	(0.02918)	(0.02950)	(0.02927)
Fixed Effects:								
Year by Group	X	X		X		X		X
Year by Group by Race			X					
Year by Group by Gender					X			
Year by Group by Party							X	
Controls:								
Age	X	X	X	X	X	X	X	X
Race		X						X
Gender				X				X
Party						X		X
No. of Observations	4,672,279	4,672,279	4,672,167	4,672,279	4,672,107	4,672,279	4,672,028	4,672,279
Dependent Variable Mean	1.9575	1.9575	1.9575	1.9575	1.9575	1.9575	1.9574	1.9575
New Nbr. Entrep. Mean	0.0711	0.0711	0.0711	0.0711	0.0711	0.0711	0.0711	0.0711
FE Cells	67,064	67,064	152,764	67,064	138,576	67,064	177,104	67,064
R^2	0.0172	0.0180	0.0469	0.0190	0.0371	0.0172	0.0399	0.0198

This table reports estimates of Equation 1 with $h = 10$, where outcomes are measured over a ten-year horizon following the arrival of a new neighbor. Specifically, $h = 10$ indicates that the dependent variable captures whether an incumbent resident starts a business within ten years of the neighbor's arrival. The remaining notes are the same as in Table 2, which presents the five-year ($h = 5$) specification.

TABLE 4. Effect of Entrepreneurial Neighbors on Incumbent Business Formation after Five Years-Assessment Sample

	1	2	3	4	5	6	7
New Nbr Entrepreneur	0.06604***	0.06243***	0.06518***	0.06592***	0.06658***	0.06785***	0.09392***
	(0.02411)	(0.02412)	(0.02442)	(0.02445)	(0.02445)	(0.02486)	(0.02813)
Fixed Effects:							
Year by Block Group	X	X	X	X	X	X	X
Controls:							
Home Value		X	X	X	X	X	X
Year Built			X	X	X	X	X
Lot Size (Acres)				X	X	X	X
Building Sq Ft					X	X	X
# Bathrooms						X	X
# Bedrooms							X
No. of Observations	3,378,890	3,378,307	3,312,731	3,296,001	3,282,750	3,172,876	2,374,822
Dependent Variable Mean	1.0485	1.0483	1.0529	1.0549	1.0552	1.0570	1.0087
New Nbr. Entrep. Mean	0.0758	0.0758	0.0759	0.0759	0.0760	0.0761	0.0759
FE Cells	54,393	54,390	52,856	52,558	52,198	50,638	41,854
R^2	0.0189	0.0190	0.0189	0.0189	0.0188	0.0190	0.0206

This table reports estimates of Equation (1) for the assessment sample with $h = 5$. All specifications include Census block group–by–year fixed effects and controls for the age, race, gender, and party affiliation of incumbent residents. The sample is restricted to incumbents with no entrepreneurial history prior to the neighbor's arrival and with non-missing housing characteristics. The dependent variable equals one if the incumbent initiates a new business within five years of the neighbor's arrival. Columns 2–7 sequentially add property-level controls. Standard errors are clustered at the Census tract level and reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE 5. Balance Test—Comparing Demographic Characteristics of the Treatment and Control Groups

Sample:	Current Residents Who Got New Nearby Neighbors							
Dependent Variable:	Age	Born in NC	Female	Male	Democrat	Republican	White	Black
New Nbr Entrepreneur	-0.08802*	0.00254**	0.00606***	-0.00642***	0.00165	-0.00069	-0.00001	0.00185**
	(0.04740)	(0.00101)	(0.00076)	(0.00076)	(0.00101)	(0.00109)	(0.00107)	(0.00093)
Fixed Effects:								
Year by Block Group	X	X	X	X	X	X	X	X
Demo. Controls:	X	X	X	X	X	X	X	X
No. of Observations	4,766,686	4,766,686	4,766,686	4,766,686	4,766,686	4,766,686	4,766,686	4,766,686
Dep. Variable Mean	49.7486	0.2598	0.5335	0.4551	0.3935	0.3397	0.7849	0.1544
Relative Effect	-0.0018	0.0098	0.0114	-0.0141	0.0042	-0.0020	-0.0000	0.0120
New Nbr Entrep. Mean	0.0710	0.0710	0.0710	0.0710	0.0710	0.0710	0.0710	0.0710
FE Cells	72,660	72,660	72,660	72,660	72,660	72,660	72,660	72,660
R^2	0.1699	0.2015	0.0182	0.0174	0.2087	0.1362	0.3186	0.3594

This table presents coefficient estimates of the "effect" of receiving a new entrepreneurial nearby neighbor on a number of outcomes related to demographic characteristics of incumbent residents. The sample is identical to that used in the main analysis. Control variables include all those in the baseline specification, except for the characteristic analyzed on the left-hand side. Variables are as defined in the text and Table 1. All specifications include Census block group–by–year fixed effects, and standard errors are clustered at the Census tract level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

TABLE 6. Balance Test—Comparing Property Characteristics of the Treatment and Control Groups

Sample:	Current Residents Who Got New Nearby Neighbors						
Dependent Variable:	Homeownership	Home Age (Yrs)	Log Home Value	Lot Size	Log Build. Sq Ft	# Bedrooms	# Bathrooms
New Nbr Entrepreneur	-0.00465***	0.49472***	0.01350***	-0.00418	0.00034	0.00099	0.00453
	(0.00114)	(0.08053)	(0.00210)	(0.01214)	(0.00125)	(0.00482)	(0.00385)
Fixed Effects:							
Year by Block Group	X	X	X	X	X	X	X
Demo. Controls	X	X	X	X	X	X	X
Housing Controls	X	X	X	X	X	X	X
No. of Observations	3,296,001	3,296,001	3,296,001	3,296,001	3,282,750	2,389,421	3,180,532
Dep. Variable Mean	0.3183	30.2356	12.0945	0.4937	7.5059	3.1878	2.5085
Relative Effect	-0.0146	0.0164	0.0011	-0.0085	0.0000	0.0003	0.0018
New Nbr Entrep. Mean	0.0759	0.0759	0.0759	0.0759	0.0760	0.0758	0.0760
FE Cells	52,558	52,558	52,558	52,558	52,198	42,377	50,901
R^2	0.0986	0.6893	0.7199	0.0561	0.7169	0.3927	0.5286

This table presents coefficient estimates of the "effect" of receiving a new entrepreneurial nearby neighbor on property characteristics of incumbent residents' homes. Outcomes include homeownership, home age, log lot size, log square footage, number of bedrooms, number of bathrooms, and log assessed value. The specification includes demographic and housing property controls, as described in the text. All specifications include Census block group–by–year fixed effects, and standard errors are clustered at the Census tract level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

TABLE 7. Heterogeneity over Distance to the New Nearby Entrepreneur, Main Sample

Subsample, Distance to Neighbor (Meters)	0-25	25-50	50-120	All
	(1)	(2)	(3)	(4)
New Nbr Entrepreneur	0.07705*	0.04914	0.04786	0.08108**
	(0.04288)	(0.03239)	(0.05038)	(0.03861)
Distance: 25-50m				0.03028**
				(0.01499)
Distance: 50-120m				0.05569***
				(0.01844)
New Nbr Entrepreneur x Distance: 25-50m				-0.04866
				(0.04866)
New Nbr Entrepreneur x Distance: 50-120m				-0.04324
				(0.05837)
Fixed Effects:				
Year by Block Group	X	X	X	X
Demographics Controls	X	X	X	X
No. of Observations	1,354,351	1,787,535	794,013	4,076,898
Dependent Variable Mean	1.1092	0.9791	0.9180	1.0035
New Nbr. Entrep. Mean	0.0689	0.0684	0.0719	0.0693
FE Cells	39,822	53,190	39,252	62,842
R^2	0.0294	0.0317	0.0508	0.0178

This table estimates the main specification separately for three subsamples defined by the distance between the incumbent's home and the new neighbor's home: 0–25 meters, 25–50 meters, and 50–120 meters. Column (4) pools all observations and interacts the treatment indicator with distance categories. All specifications include year-by-Census block group fixed effects and demographic controls. Standard errors, clustered at the tract-year level, are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE 8. Effect of Entrepreneurial Neighbors on Incumbent Business Formation, by Arriver Residency Status (Within 0-5 yrs After Arrival), Main Sample

	All Arrivals Full Sample	Resident Arrivals Only	Non-Resident Arrivals Only
	(1)	(2)	(3)
New Nbr Entrepreneur	0.04352**	0.07392*	0.04353*
	(0.02030)	(0.04276)	(0.02442)
Fixed Effects:			
Year by Block Group	X	X	X
Demo. Controls	X	X	X
No. of Observations	4,672,279	2,086,378	2,583,131
Dependent Variable Mean	0.9917	1.0007	0.9845
New Nbr Entrepreneur Mean	0.0711	0.0407	0.0956
FE Cells	67,064	54,247	62,232
R^2	0.0167	0.0280	0.0256

This table estimates the main specification separately for three samples defined by the residency status of arriving homeowners: all arrivals, resident arrivals only, and non-resident arrivals only. Arrivals are classified as resident if the new homeowner is observed living at the purchased address at any point within zero to five years after the transaction, based on a match between CoreLogic property records and voter registration files; otherwise, arrivals are classified as non-resident. All specifications include year-by–Census block group fixed effects and demographic controls. Standard errors, clustered at the Census tract level, are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE 9. Effect of Entrepreneurial Neighbors on Incumbent Business Formation, by Arriver Residency Status (Within 0-5 yrs After Arrival), Assessment Sample

	All Arrivals Full Sample (1)	Resident Arrivals Only (2)	Non-Resident Arrivals Only (3)
New Nbr Entrepreneur	0.06592***	0.08685*	0.07286**
	(0.02445)	(0.04839)	(0.02974)
Fixed Effects:			
Year by Block Group	X	X	X
Demo. Controls	X	X	X
Housing Controls	X	X	X
No. of Observations	3,296,001	1,543,004	1,751,548
Dependent Variable Mean	1.0549	1.0598	1.0505
New Nbr Entrepreneur Mean	0.0759	0.0442	0.1039
FE Cells	52,558	41,829	47,575
R^2	0.0189	0.0298	0.0288

This table replicates the analysis in Table 8 using the assessment sample, which additionally controls for housing characteristics. Samples are defined by the residency status of arriving homeowners as described above. All specifications include year-by–Census block group fixed effects and demographic and housing controls. Standard errors, clustered at the Census tract level, are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE 10. Differential Effects of Entrepreneurial Neighbors by Arriver Residency Status, Main Sample

	1	2
New Nbr Entrepreneur	0.04352**	
	(0.02030)	
New Nbr Entrepreneur × Resident Arrivals		0.07379*
		(0.04098)
New Nbr Entrepreneur × Non-Resident Arrivals		0.03725
		(0.02328)
Fixed Effects:		
Year by Block Group	X	X
Demo. Controls	X	X
No. of Observations	4,672,279	4,672,279
Dependent Variable Mean	0.9917	0.9917
New Nbr Entrepreneur Mean	0.0711	
New Nbr Entrepreneur × Resident Mean		0.0182
New Nbr Entrepreneur × Non-Resident Mean		0.0529
FE Cells	67,064	67,064
R^2	0.0167	0.0164

This table estimates the main specification allowing the effect of exposure to an entrepreneurial neighbor to vary by the residency status of the arriving homeowner. Resident arrivals are defined as homeowners observed living at the purchased address at any point within zero to five years after purchase; non-resident arrivals are defined analogously. Column (2) reports a fully interacted specification with separate effects for resident and non-resident arrivals. All specifications include year-by–Census block group fixed effects and demographic controls. Standard errors, clustered at the Census tract level, are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE 11. Differential Effects of Entrepreneurial Neighbors by Arriver Residency Status, Assessment Sample

	1	2
New Nbr Entrepreneur	0.06592***	
	(0.02445)	
New Nbr Entrepreneur × Resident Arrivals		0.09348**
		(0.04611)
New Nbr Entrepreneur × Non-Resident Arrivals		0.05946**
		(0.02829)
Fixed Effects:		
Year by Block Group	X	X
Demo. Controls	X	X
Housing Controls	X	X
No. of Observations	3,296,001	3,296,001
Dependent Variable Mean	1.0549	1.0549
New Nbr Entrepreneur Mean	0.0759	
New Nbr Entrepreneur × Resident Mean		0.0207
New Nbr Entrepreneur × Non-Resident Mean		0.0552
FE Cells	52,558	52,558
R^2	0.0189	0.0185

This table replicates the analysis of Table 10 using the assessment sample, which additionally controls for housing characteristics. All definitions, specifications, fixed effects, and inference procedures are identical to those in the main sample.

TABLE 12. Effect of Renter Entrepreneurial Neighbors on Incumbent Business Formation after Five Years, Main Sample

	1	2	3	4	5	6	7	8
New Nbr Entrepreneur	0.11144**	0.10792**	0.12382**	0.11898**	0.12734**	0.11086**	0.10957**	0.11545**
	(0.05258)	(0.05212)	(0.05344)	(0.05278)	(0.05514)	(0.05258)	(0.05310)	(0.05236)
Fixed Effects:								
Year by Block Group	X	X		X		X		X
Year by Block Group by Race			X					
Year by Block Group by Gender					X			
Year by Block Group by Party							X	
Controls:								
Age	X	X	X	X	X	X	X	X
Race		X						X
Gender				X				X
Party						X		X
No. of Observations	2,216,149	2,216,149	2,216,042	2,216,149	2,216,008	2,216,149	2,215,940	2,216,149
Dependent Variable Mean	1.0076	1.0076	1.0077	1.0076	1.0077	1.0076	1.0077	1.0076
New Nbr. Entrep. Mean	0.0780	0.0780	0.0780	0.0780	0.0780	0.0780	0.0780	0.0780
FE Cells	46,087	46,087	92,507	46,087	92,532	46,087	116,526	46,087
R^2	0.0247	0.0253	0.0641	0.0262	0.0521	0.0247	0.0589	0.0268

This table reports estimates of Equation 1 with $h = 5$, where outcomes are measured over a five-year horizon following the arrival of a new neighbor who is likely a renter. Specifically, $h = 5$ indicates that the dependent variable captures whether an incumbent resident starts a business within five years of the renter's arrival. Renter arrivals are identified based on occupancy of properties purchased by non-resident homeowners, as described in Section 6.2. The remaining notes are the same as in Table 2.

TABLE 13. Effect of Renter Entrepreneurial Neighbors on Incumbent Business Formation after Five Years, Assessment Sample

	1	2	3	4	5	6	7
New Nbr Entrepreneur	0.12188**	0.11988**	0.12175**	0.09205*	0.09161*	0.08083	0.04492
	(0.05295)	(0.05306)	(0.05454)	(0.05382)	(0.05374)	(0.05477)	(0.06139)
Fixed Effects:							
Year by Block Group	X	X	X	X	X	X	X
Controls:							
Demographics	X	X	X	X	X	X	X
Home Value		X	X	X	X	X	X
Year Built			X	X	X	X	X
Lot Size (Acres)				X	X	X	X
Building Sq Ft					X	X	X
# Bathrooms						X	X
# Bedrooms							X
No. of Observations	2,125,277	2,125,036	2,084,541	2,024,664	2,015,424	1,955,319	1,522,749
Dependent Variable Mean	1.0296	1.0296	1.0357	1.0307	1.0315	1.0353	0.9866
New Nbr. Entrep. Mean	0.0805	0.0805	0.0805	0.0748	0.0750	0.0751	0.0797
FE Cells	46,591	46,588	45,377	45,089	44,778	43,465	35,960
R^2	0.0277	0.0278	0.0274	0.0279	0.0279	0.0282	0.0302

This table replicates the analysis in the main sample (Table 12) using the assessment sample, which additionally controls for housing characteristics. The specification, variable definitions, and horizon $h = 5$ are identical to those in the main sample. The remaining notes are the same as in Table 12.

TABLE 14. Effect of Exposure to High-Quality Entrepreneurial Neighbors on Incumbent Business Formation after Five Years

	1	2	3	4	5	6	7	8
New Nbr High Quality Entrep.	0.05907*	0.06663**	0.06066*	0.06290*	0.05889*	0.06021*	0.05967*	0.05967*
	(0.03299)	(0.03296)	(0.03298)	(0.03303)	(0.03299)	(0.03338)	(0.03300)	(0.03300)
Fixed Effects:								
Year by Block Group	X	X		X		X		X
Year by Block Group by Race			X					
Year by Block Group by Gender					X			
Year by Block Group by Party							X	
Controls:								
Age	X	X	X	X	X	X	X	X
Race		X						X
Gender				X				X
Party						X		X
No. of Observations	4,672,279	4,672,167	4,672,279	4,672,107	4,672,279	4,672,028	4,672,279	4,672,279
Dependent Variable Mean	0.9917	0.9917	0.9917	0.9918	0.9917	0.9917	0.9917	0.9917
New Nbr. High Quality Entrep. Mean	0.0268	0.0268	0.0268	0.0268	0.0268	0.0268	0.0268	0.0268
FE Cells	67,064	152,764	67,064	138,576	67,064	177,104	67,064	67,064
R^2	0.0152	0.0449	0.0162	0.0335	0.0152	0.0371	0.0167	0.0167

This table reports estimates of Equation 1 with $h = 5$, restricting the treatment to high-quality entrepreneurial neighbors. The treatment indicator, *New Nbr High Quality Entrepreneur*$_{i,t}$, equals one if the incoming neighbor previously founded an incorporated business and zero otherwise. The sample is restricted to incumbents with no entrepreneurial history prior to the neighbor's arrival. The dependent variable is an indicator equal to 100 if the incumbent starts a new business within five years of the neighbor's arrival. Standard errors, clustered at the census tract level, are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE 15. Effect of Exposure to Low-Quality Entrepreneurial Neighbors on Incumbent Business Formation after Five Years

	1	2	3	4	5	6	7	8
New Nbr Low Quality Entrep.	0.03055	0.03366	0.03199	0.03935	0.03042	0.03333	0.03131	0.03131
	(0.02559)	(0.02559)	(0.02561)	(0.02572)	(0.02559)	(0.02612)	(0.02559)	(0.02559)
Fixed Effects:								
Year by Block Group	X	X		X		X		X
Year by Block Group by Race			X					
Year by Block Group by Gender					X			
Year by Block Group by Party							X	
Controls:								
Age	X	X	X	X	X	X	X	X
Race		X						X
Gender				X				X
Party						X		X
No. of Observations	4,672,279	4,672,167	4,672,279	4,672,107	4,672,279	4,672,028	4,672,279	4,672,279
Dependent Variable Mean	0.9917	0.9917	0.9917	0.9918	0.9917	0.9917	0.9917	0.9917
New Nbr. Low Quality Entrep. Mean	0.0443	0.0443	0.0443	0.0443	0.0443	0.0443	0.0443	0.0443
FE Cells	67,064	152,764	67,064	138,576	67,064	177,104	67,064	67,064
R^2	0.0152	0.0449	0.0162	0.0335	0.0152	0.0371	0.0167	0.0167

This table reports estimates of Equation 1 with $h = 5$, restricting the treatment to low-quality entrepreneurial neighbors. The treatment indicator, *New Nbr Low Quality Entrepreneur*$_{i,t}$, equals one if the incoming neighbor previously founded a non-incorporated business and zero otherwise. The sample is restricted to incumbents with no entrepreneurial history prior to the neighbor's arrival. The dependent variable is an indicator equal to 100 if the incumbent starts a new business within five years of the neighbor's arrival. Standard errors, clustered at the census tract level, are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE 16. Effect of Exposure to High- and Low-Quality Entrepreneurial Neighbors on Business Formation after Five Business

	1	2	3	4
New Nbr Entrepreneur	0.04352**			
	(0.02030)			
New Nbr Low Quality Entrep.		0.03131		0.03322
		(0.02559)		(0.02558)
New Nbr High Quality Entrep.			0.05967*	0.06132*
			(0.03300)	(0.03299)
Fixed Effects:				
Year by Block Group	X	X	X	X
Demo. Controls	X	X	X	X
$H_0 : \beta_{low} = \beta_{High}$				
F-stat [p-value]				0.46 [0.49]
No. of Observations	4,672,279	4,672,279	4,672,279	4,672,279
Dependent Variable Mean	0.9917	0.9917	0.9917	0.9917
New Nbr. Entrep. Mean	0.0711			
New Nbr. Low Quality Entrep. Mean		0.0443		0.0443
New Nbr. High Quality Entrep. Mean			0.0268	0.0268
FE Cells	67,064	67,064	67,064	67,064
R^2	0.0167	0.0167	0.0167	0.0167

This table examines whether the effect of entrepreneurial exposure varies with the quality of the arriving entrepreneur. The dependent variable is an indicator equal to 100 if the incumbent starts a business within five years of a new neighbor's arrival. High-quality entrepreneurs are those who previously founded an incorporated business; low-quality entrepreneurs are those who founded a non-incorporated business. Column (1) reports the baseline effect of exposure to any entrepreneurial neighbor. Columns (2) and (3) restrict the treatment to low- and high-quality entrepreneurs, respectively. Column (4) includes mutually exclusive indicators for low- and high-quality exposure and reports an F-test for equality of the two effects. All specifications include year-by-group fixed effects and the listed demographic controls. Standard errors, clustered at the Census tract level, are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE 17. Effect of Entrepreneurial Neighbors on the Probability that Incumbents Start Incorporated Businesses

	1	2	3	4	5	6	7	8
New Nbr Entrepreneur	0.00348	0.00445	0.00377	0.00316	0.00350	0.00150	0.00368	0.00368
	(0.00726)	(0.00731)	(0.00725)	(0.00723)	(0.00726)	(0.00735)	(0.00726)	(0.00726)
Fixed Effects:								
Year by Block Group	X	X		X		X		X
Year by Block Group by Race			X					
Year by Block Group by Gender					X			
Year by Block Group by Party							X	
Controls:								
Age	X	X	X	X	X	X	X	X
Race		X						X
Gender				X				X
Party						X		X
No. of Observations	4,672,279	4,672,167	4,672,279	4,672,107	4,672,279	4,672,028	4,672,279	4,672,279
Dependent Variable Mean	0.1513	0.1513	0.1513	0.1513	0.1513	0.1513	0.1513	0.1513
New Nbr. Entrep. Mean	0.0711	0.0711	0.0711	0.0711	0.0711	0.0711	0.0711	0.0711
FE Cells	67,064	152,764	67,064	138,576	67,064	177,104	67,064	67,064
R^2	0.0144	0.0475	0.0147	0.0319	0.0144	0.0375	0.0147	0.0147

This table reports estimates of Equation 1 with $h = 5$, focusing on outcome quality. The treatment indicator, *New Neighbor Entrepreneur*$_{i,t}$, equals one if the incoming neighbor has prior entrepreneurial experience and zero otherwise. The sample is restricted to incumbents with no entrepreneurial history prior to the neighbor's arrival. The dependent variable is an indicator equal to 100 if the incumbent starts an incorporated business within five years of the neighbor's arrival. Standard errors, clustered at the census tract level, are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE 18. Effect of Entrepreneurial Neighbors on the Probability that Incumbents Start Non-Incorporated Businesses

	1	2	3	4	5	6	7	8
New Nbr Entrepreneur	0.03738**	0.04142**	0.03866**	0.04491**	0.03722**	0.04159**	0.03791**	0.03791**
	(0.01877)	(0.01878)	(0.01877)	(0.01880)	(0.01877)	(0.01907)	(0.01877)	(0.01877)
Fixed Effects:								
Year by Block Group	X	X		X		X		X
Year by Block Group by Race			X					
Year by Block Group by Gender					X			
Year by Block Group by Party							X	
Controls:								
Age	X	X	X	X	X	X	X	X
Race		X						X
Gender				X				X
Party						X		X
No. of Observations	4,672,279	4,672,167	4,672,279	4,672,107	4,672,279	4,672,028	4,672,279	4,672,279
Dependent Variable Mean	0.8383	0.8382	0.8383	0.8383	0.8383	0.8382	0.8383	0.8383
New Nbr. Entrep. Mean	0.0711	0.0711	0.0711	0.0711	0.0711	0.0711	0.0711	0.0711
FE Cells	67,064	152,764	67,064	138,576	67,064	177,104	67,064	67,064
R^2	0.0154	0.0443	0.0162	0.0332	0.0154	0.0370	0.0166	0.0166

This table reports estimates of Equation 1 with $h = 5$, focusing on outcome quality. The treatment indicator, *New Neighbor Entrepreneur*$_{i,t}$, equals one if the incoming neighbor has prior entrepreneurial experience and zero otherwise. The sample is restricted to incumbents with no entrepreneurial history prior to the neighbor's arrival. The dependent variable is an indicator equal to 100 if the incumbent starts a non-incorporated business within five years of the neighbor's arrival. Standard errors, clustered at the census tract level, are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Online Appendix: When Entrepreneurs Move In: Evidence from North Carolina

A. Life-Cycle Robustness

This section examines whether the estimated peer effects are sensitive to how age and life-cycle dynamics are modeled. Because entrepreneurial entry follows a pronounced non-linear life-cycle pattern, controlling linearly for age may be insufficient. I therefore re-estimate the baseline specification using increasingly flexible age controls. Specifically, I replace the linear age term with age-bin indicators[15] and, in the most demanding specification, absorb fully saturated group-by-year-by-age-bin fixed effects, which allow baseline entrepreneurial propensities to vary arbitrarily across age groups within each location–year cell.

Across both the main and assessment samples, reported in Tables J.60 and J.61, the estimated effect of exposure to a new entrepreneurial neighbor is remarkably stable across all specifications. Allowing age to enter non-parametrically has virtually no effect on either the magnitude or the statistical significance of the estimates. Even under fully saturated group-by-year-by-age-bin fixed effects, which absorb any age-specific local shocks, the estimated peer effect remains positive and statistically significant. These results indicate that the baseline findings are not driven by age-related confounding or life-cycle composition effects.

B. Heterogeneity by Pre-Period Tract Characteristics

Motivation. A potential concern is that the estimated peer effects reflect broader differences across places rather than highly localized interpersonal exposure. Entrepreneurial activity may be more responsive in "growth places" characterized by favorable demographics, stronger housing markets, or more dynamic local labor conditions. If so, exposure to an entrepreneurial neighbor could matter primarily because it occurs in tracts that were already predisposed toward business formation. To address this concern, I examine whether the estimated spillovers vary systematically with predetermined tract-level characteristics measured prior to the study period.

Empirical Design. To implement this test, I interact exposure to a new entrepreneurial neighbor with census-tract characteristics measured in the 2000 Decennial Census. Because these variables are predetermined relative to treatment, they capture long-run place fundamentals rather than endogenous responses to entrepreneurial activity. I consider two sets of characteristics. The first includes demographic composition—log population, gender composition, racial composition, and educational attainment. The second includes economic conditions—homeownership rates, housing values, household income, poverty, and unemployment. All tract-level variables are centered at their sample means, so the coefficient on exposure represents the treatment effect evaluated at the mean of the interacting characteristic. Each characteristic is introduced separately. I estimate these specifications first in the main sample and then replicate them in the assessment sample, which additionally includes housing-level controls.

[15] Age bins are defined as: younger than 30, 30–39, 40–49, 50–59, and 60 or older.

Results. Tables J.62 and J.63 report the results for the main sample. Across both demographic and economic characteristics, the estimated effect of exposure to a new entrepreneurial neighbor remains remarkably stable. The interaction terms are small and statistically insignificant, and the baseline spillover effect changes little across specifications. These findings indicate that entrepreneurial peer effects do not differentially vary with tract size, demographic composition, housing market conditions, income, poverty, or local labor market slack. The absence of systematic heterogeneity suggests that the estimated spillovers are not concentrated in advantaged or distressed locations, but instead reflect highly localized interpersonal mechanisms.

Tables J.64 and J.65 replicate the analysis in the assessment sample. The results are qualitatively unchanged: the magnitude of the baseline effect remains similar, and none of the interaction terms indicate meaningful heterogeneity. Taken together, these findings reinforce the interpretation that entrepreneurial spillovers operate through direct residential exposure rather than through broader tract-level economic environments.

C. Heterogeneity Across Neighborhood Socioeconomic Contexts (Quintile Analysis)

Empirical Design. This section examines whether entrepreneurial spillovers vary systematically across neighborhood socioeconomic contexts. Using tract-level measures from the 2010 American Community Survey (ACS), I construct four indicators of local environment: (i) population density, (ii) median household income, (iii) the share of adults with a bachelor's degree, and (iv) median home value. For each variable, Census tracts are ranked statewide and divided into five equal-sized groups (quintiles). I then re-estimate the baseline specification (Equation 1) separately within each quintile. The empirical specification is otherwise identical to the main results: outcomes are measured over a five-year horizon; fixed effects are defined at the Census block group $\times$ year level; and standard errors are clustered at the Census tract level. The assessment sample additionally includes housing controls, as in the main assessment specification.

Results. Tables J.66 through J.73 report the results. Across all four contextual dimensions, the quintile analysis reveals no anomalous or unstable patterns. Estimated spillovers remain generally positive across subsamples, and no systematic sign reversals emerge when dividing tracts more finely.

Spillovers tend to be strongest in the highest-density quintile, consistent with denser environments facilitating interaction and visibility. Across household income quintiles, estimated effects are similar in magnitude and largely imprecise, indicating that entrepreneurial diffusion is not concentrated in either low- or high-income neighborhoods.

Across education levels, effects are largest in the lowest-education quintile, suggesting that spillovers may be particularly impactful where baseline entrepreneurial human capital is relatively scarce. In contrast, when dividing by median home value, spillovers are strongest in lower-value tracts and attenuate in the highest-value quintile, consistent with lower housing costs and entry

barriers increasing responsiveness to exposure.

These patterns are broadly similar in both the main and assessment samples, indicating that the heterogeneity results reflect systematic differences in neighborhood context rather than instability of the baseline specification. Overall, entrepreneurial spillovers are most pronounced in dense, less-educated, and lower-cost neighborhoods, while remaining present across the socioeconomic distribution.

D. Excluding Same-Side Neighbors: A Cross-Street Exposure Test

To further probe the spatial scope of entrepreneurial spillovers, I re-estimate the baseline specifications while excluding same-side neighbors from the exposure definition. The baseline design assigns treatment using up to four closest neighbors on the block face (two on each side of the focal dwelling), and one robustness specification expands this to the four closest neighbors irrespective of street side (mixing same- and across-street units). In Tables J.16-J.20 in the Appendix, I hold the same set of focal arrival events, fixed effects, and identifying variation constant but redefine treated incumbents to include only the closest neighbors located beyond the same side of the street. In short, the arrival shocks and neighborhood-year comparisons are unchanged; only the spatial mapping from arrivals to treated incumbents is altered.

This exercise asks whether spillovers require literal block-face adjacency or instead operate at a very short physical radius that crosses the street. Across Tables J.16–J.20, the estimated effects remain positive, statistically significant, and economically similar in magnitude to the baseline specifications. The cleanest comparison is provided by the assessment sample, which conditions directly on detailed housing characteristics and therefore mitigates potential discontinuities across streets. Relative to the housing-controlled baseline in Table 4, the estimated coefficients are slightly smaller, though of comparable magnitude. This pattern suggests that same-side neighbors may exert marginally stronger effects, but that entrepreneurial spillovers are not confined to block-face adjacency. Taken together with the distance-decay evidence, the results indicate that spillovers operate within a very short physical radius driven by close residential proximity rather than by an arbitrary street-side boundary.

E. Ruling Out a House-Price Appreciation–Collateral Mechanism

This appendix evaluates a housing-price appreciation (collateral) channel as an alternative explanation for the estimated entrepreneurial spillovers. Under this mechanism, entrepreneurial arrivals would raise local housing demand and transaction prices, increasing nearby homeowners' equity and relaxing collateral constraints that could, in turn, stimulate business formation. I assess this channel in three steps, moving from buyer willingness-to-pay to neighborhood-level spillovers and, finally, to price dynamics around arrival episodes.

Table J.45 addresses the first implication: if entrepreneurial arrivals shift housing demand, they should pay a price premium for comparable properties. Using transaction-level repeat-sales

variation with parcel fixed effects, I compare prices paid by entrepreneurial versus non-entrepreneur buyers for the same property across time. Across increasingly demanding specifications—including arms-length restrictions, granular local market-by-time fixed effects, and a switcher-parcel sample—entrepreneurial arrivals do not pay a premium, and the estimated coefficient is consistently negative.

To complement the parcel fixed-effects comparison, Tables J.46 and J.47 report specifications that instead control directly for observable housing characteristics while omitting parcel fixed effects. These regressions include detailed property controls—lot size, building area, and year built in Table J.46, with additional controls for bedrooms and bathrooms in Table J.47—along with increasingly granular location-by-time fixed effects. Across all specifications, entrepreneurial arrivals again do not pay a price premium. Instead, the estimated coefficients remain negative and statistically significant, implying that entrepreneurial buyers transact at prices roughly 2–3 percent lower than other buyers for comparable properties. These results reinforce the conclusion that entrepreneurial arrivals do not represent unusually high willingness-to-pay housing demand shocks.

I next examine whether entrepreneurial arrivals affect prices paid by other buyers through contemporaneous neighborhood demand pressure. Table J.48 restricts the sample to non-entrepreneur transactions and relates log sale price to the number of entrepreneurial arrivals purchasing in the same census block group × quarter, absorbing block group fixed effects and tract × year-quarter fixed effects and controlling for property characteristics. Estimated effects are small and negative, providing no evidence that quarters with higher entrepreneurial arrival intensity exhibit higher prices for non-entrepreneur buyers. I then turn to a stacked event-study design that traces neighborhood transaction prices before and after entrepreneurial arrival episodes, which directly tests for post-arrival appreciation in local housing markets.

Building on this intensity test, Figures I.6 and I.7 implement the complementary dynamic version: a stacked event-study design tailored to staggered adoption. Because block groups experience entrepreneurial arrivals at different times—and may experience multiple arrival episodes—I define an "event" as the start of an entrepreneurial-arrival spell (a quarter with one or more entrepreneurial-arrival purchases preceded by a quarter with zero). For each event, I trace the evolution of neighborhood prices in event time from eight quarters before to eight quarters after, comparing the treated block group to other block groups in the same tract observed in the same calendar quarters. The outcome is the mean log sale price among non-entrepreneur transactions only, weighted by the number of such transactions in the block group-quarter. The specification absorbs event-by-block-group fixed effects and tract-by-calendar-quarter fixed effects, and standard errors are clustered at the tract level. I report results under two control definitions: "nostart" controls exclude block groups that begin an arrival spell within the event window, while "noent" controls require zero entrepreneurial-arrival purchases throughout the window.

Across both panels, estimated price effects are small, show no systematic upward shift after entrepreneurial-arrival spells, and are broadly flat in both the pre- and post-periods. Taken together with the parcel-level evidence that entrepreneurial arrivals do not pay a housing price premium (Table J.45) and the intensity regressions showing no contemporaneous price pressure on other buyers (Table J.48), the combined results provide little support for a house-price appreciation–collateral

mechanism as an explanation for the main spillover estimates.

F. Residential Presence and Entrepreneurial Spillovers

Introduction. A central question in interpreting neighborhood peer effects is whether spillovers reflect social interaction and learning from a nearby person or instead capture ownership-based neighborhood change (e.g., compositional shifts in buyers, investor activity, or unobserved local demand). If entrepreneurial spillovers operate through day-to-day contact, they should be strongest when the arriving neighbor is physically present in the micro-neighborhood. This motivates a "presence-based" extension that conditions directly on whether the household associated with the recent purchase actually resides at the purchased home, aligning the treatment definition with the paper's social-interaction mechanism.

What I did. I construct a unified set of present arrivals by combining two conceptually parallel groups, measured consistently two years after purchase to harmonize timing across data sources. First, I identify resident owner-arrivals—purchases where the owner is observed occupying the home at $t+2$. Second, I identify likely renter arrivals among non-resident purchases by matching to an observed occupier at $t+2$ and assigning entrepreneurial status using the occupant's entrepreneurial history. I then re-estimate the baseline nearest-neighbor specifications using this presence-restricted arrival sample in both the main and assessment datasets (Tables J.21 and J.22). Next, I decompose the presence-based effect by estimating the model separately for homeowner vs renter arrivals (Tables J.23 and J.24). Finally, I formally test for differential spillovers by interacting entrepreneurial exposure with homeowner- and renter-arrival indicators (Tables J.25 and J.26). Throughout, the identifying variation and micro-neighborhood exposure mapping follow the baseline design; what changes is that the "arrival" is restricted to households that are verifiably present.

Results. Tables J.21 and J.22 restrict the analysis to arrivals who are verifiably present in the purchased home two years after the transaction—either as resident owners or as likely renters (with entrepreneurial status assigned to the occupant). In the main sample (Table J.21), exposure to a present entrepreneurial neighbor increases incumbent business formation by about 0.10 log points and is highly stable across specifications, including those with demanding fixed effects. The assessment sample (Table J.22) delivers the same qualitative conclusion: estimates are positive and of similar magnitude, with attenuation only in the most saturated housing-control specifications where precision falls. Overall, conditioning on presence does not weaken the baseline effect and instead sharpens its interpretation as a person-to-person spillover rather than an ownership-driven neighborhood shift.

Tables J.23 and J.24 then separate present arrivals by tenure status. In the main sample, spillovers are positive for both homeowner and renter arrivals, with point estimates that are close in magnitude. In the assessment sample, homeowner-based effects remain strong while renter-based estimates are noisier, consistent with less variation and greater measurement error in renter identification. The

key implication is that spillovers arise when the arriving household is locally resident, not because the arriving household owns the property.

Finally, Tables J.25 and J.26 test this directly by interacting entrepreneurial exposure with homeowner- and renter-arrival indicators. In the main sample, the fully interacted specification implies nearly identical spillover effects for homeowner and renter arrivals, ruling out economically meaningful mediation by tenure status once presence is held fixed. In the assessment sample, the homeowner interaction is precisely estimated while the renter interaction remains positive but imprecise. Taken together, these results point to residential presence as the first-order condition for entrepreneurial spillovers, with any homeowner–renter differences second-order and not robustly distinguishable.

Conclusion. Overall, restricting the analysis to arrivals who actually live next door strengthens the mechanism interpretation of the paper's baseline findings. Entrepreneurial spillovers persist—and remain economically meaningful—when treatment is defined by the entrepreneurial status of the occupying household, not by property ownership alone. Moreover, once presence is imposed, spillovers are similar across homeowner and renter arrivals in the main sample and remain directionally consistent in the assessment sample. The evidence therefore points to a presence-based channel: neighborhood peer effects arise primarily from exposure to a nearby entrepreneur as a resident member of the local social environment.

While the presence-based results indicate that spillovers arise when the arriving household is locally resident, a remaining question is whether these effects reflect exposure to entrepreneurs as nearby individuals or instead capture entrepreneurial ownership of housing assets in the micro-neighborhood. The next subsection separates the entrepreneurial status of the resident from that of the purchaser to distinguish these channels.

F.1. Separating Entrepreneurial Residence from Entrepreneurial Property Ownership

Introduction. The presence-based analysis establishes that spillovers arise when the arriving household is locally resident. A remaining question is whether the effect is tied specifically to entrepreneurs who live nearby or whether entrepreneurial ownership of housing assets could also matter—for instance, through investment activity, neighborhood signaling, or other ownership-based channels. To distinguish these mechanisms, I separate the entrepreneurial status of the resident living at the property from the entrepreneurial status of the purchaser.

What I did. I augment the presence-based specification with an indicator for whether the property was purchased by an entrepreneur and interact it with the indicator that the arriving resident is an entrepreneur. This specification allows the effect of entrepreneurial arrivals to differ depending on whether the entrepreneur both purchases and occupies the property, resides in the property without purchasing it (consistent with renter occupancy), or purchases the property but does not live there. Because the sample conditions on residential presence two years after purchase, the arriving resident is always observed living at the property; what varies is whether that resident is an entrepreneur and

whether the purchaser is an entrepreneur. Tables J.27 and J.29 report the regression results, and the implied two-by-two decomposition is summarized in Tables J.28 and J.30.

Results. The results show that entrepreneurial spillovers arise only when the nearby resident is an entrepreneur. In the main sample, when an entrepreneur both purchases and occupies the property, exposure increases incumbent business formation by roughly 0.13 percentage points. When the entrepreneur resides at the property but did not purchase it—consistent with renter occupancy—the effect remains positive at about 0.09 percentage points. By contrast, when a property is purchased by an entrepreneur but occupied by a non-entrepreneur, the estimated effect is close to zero. The assessment sample yields the same qualitative pattern. Entrepreneurial residents generate positive spillovers regardless of whether they are owners or renters, while entrepreneurial ownership alone does not produce measurable effects. Taken together, these estimates reinforce the interpretation that neighborhood spillovers operate through exposure to entrepreneurs as nearby residents rather than through entrepreneurial ownership of housing assets.

G. Alternative Measures of Entrepreneurial Quality

This section examines whether the estimated spillover effects are sensitive to alternative definitions of entrepreneurial "quality." While the baseline analysis focuses on organizational form and related indicators of venture sophistication, quality may also be captured by (i) the arriving entrepreneur's accumulated experience, (ii) whether the entrepreneur is actively operating a business at the time of arrival, and (iii) the post-entry performance of induced businesses. The first two measures explore whether spillovers vary with the stock or salience of entrepreneurial human capital. The third extends the analysis from the extensive margin of entry to the intensive margin of business survival. Across all three dimensions, the results reinforce the conclusion that exposure—rather than longevity or realized performance per se—drives entrepreneurial diffusion.

G.1. Prior Entrepreneurial Experience

Prior research shows that founders' prior entrepreneurial experience predicts startup performance (Hsu 2007; Lafontaine and Shaw 2016). If neighborhood spillovers operate through the transmission of accumulated human capital or business-specific know-how, more experienced arriving entrepreneurs should generate stronger effects.

To test this prediction, I measure entrepreneurial experience as the number of years between an individual's first business registration and the arrival date. Experience is centered at its sample mean (approximately seven years) and interacted with the treatment indicator. In this specification, the main treatment coefficient captures the spillover effect evaluated at mean experience, while the interaction term measures how the effect varies with each additional year of prior entrepreneurial experience.

Tables J.74 and J.75 report the results for the main and assessment samples, respectively. In both samples, the interaction coefficient is small and statistically indistinguishable from zero. The implied

slope indicates that the spillover effect changes only minimally with additional years of experience. These findings suggest a flat experience gradient: exposure to an entrepreneurial neighbor increases entry, but the magnitude of the spillover does not scale with accumulated entrepreneurial tenure. This pattern is more consistent with a mechanism based on local salience or social exposure than with the transmission of experience-specific human capital.

G.2. Active versus Non-Active Entrepreneurs at Arrival

As a second alternative definition of arriver quality, I distinguish entrepreneurs based on whether they are actively operating a business at the time of moving into the neighborhood.

An entrepreneur is classified as active if at least one prior venture remains active at the arrival date (or dissolves thereafter), and non-active if all prior ventures dissolved on or before the arrival date.

If spillovers operate through direct observation of ongoing business activity, exposure to active entrepreneurs should generate stronger effects. Tables J.76 and J.77 report the corresponding estimates for the main and assessment samples. In the main sample, exposure to both active and non-active entrepreneurs increases the probability of entry, and the estimated effects are similar in magnitude. Although exposure to non-active entrepreneurs is less common and therefore estimated less precisely, formal tests fail to reject equality of coefficients across the two groups.

In both samples—and especially in the assessment sample (Table J.77)—the point estimate for non-active arrivals is somewhat larger,[16] though the difference remains statistically indistinguishable from that for active entrepreneurs. Overall, the basic pattern mirrors the main sample: exposure to an entrepreneurial neighbor raises entry regardless of current operating status.

These results suggest that spillovers do not hinge on contemporaneous business activity. Instead, the presence of an individual with an entrepreneurial background—whether currently operating or not—appears sufficient to generate diffusion. This pattern is consistent with a mechanism operating through entrepreneurial identity, visibility, or perceived feasibility of business ownership, rather than through observation of active firm operations or the transmission of real-time technical knowledge.

G.3. Post-Entry Survival and the Intensive Margin of Spillovers

The preceding analyses focus on the extensive margin of entrepreneurship—whether exposure to an entrepreneurial neighbor increases the probability of entry. I next examine whether spillovers extend to the intensive margin by studying the survival of businesses started by treated incumbents.

Tables J.78 and J.79 restrict the sample to individuals who start a business and estimate the effect of prior exposure on subsequent firm survival, measured as the number of years the new business remains active. In the main sample, exposure to any entrepreneurial neighbor is associated with a small, marginally significant decline in survival. When distinguishing between low- and

[16] One possible explanation is that the non-active category may include individuals with substantial prior entrepreneurial experience whose ventures dissolved before the move but who remain socially identified as entrepreneurs. In addition, the use of annual business dissolution data may introduce measurement error in the timing of active status.

high-quality arrivals, the coefficients remain negative and statistically indistinguishable from zero, and formal tests fail to reject equality across quality types.

The assessment sample yields a similar pattern. Coefficients are again small, negative, and statistically insignificant, with no detectable difference between exposure to high- and low-quality entrepreneurs.

Taken together, these findings indicate that entrepreneurial spillovers primarily operate on the decision to enter rather than on post-entry performance. Conditional on starting a business, exposed incumbents do not exhibit longer survival—and, if anything, display slightly lower average duration. One interpretation is a composition effect: exposure induces entry among marginal entrepreneurs who might otherwise not have started a business, thereby modestly reducing average survival conditional on entry. More broadly, the results suggest that neighborhood spillovers function as an informational or motivational channel that increases participation, rather than as a mechanism that enhances firm durability or entrepreneurial capability.

Taken together, the results across experience, active status, and post-entry survival point to a consistent conclusion. Spillovers do not scale with accumulated entrepreneurial tenure, do not depend on whether the arriving entrepreneur is actively operating a business, and do not translate into longer firm survival among induced entrants. Instead, exposure primarily affects the extensive margin of entrepreneurship—the decision to start a business—without measurably improving post-entry durability. This pattern is difficult to reconcile with a mechanism based on the transmission of experience-specific human capital or ongoing operational knowledge. Rather, it is more consistent with entrepreneurial diffusion operating through social exposure, identity salience, or shifts in perceived feasibility that increase participation but leave underlying firm fundamentals largely unchanged.

H. Minorities and the Transmission of Spillovers

I next explore whether entrepreneurial spillovers differ systematically by gender and race. To do so, I augment the dataset with demographic information on the arriving neighbors. For incumbents, sex and race are observed directly in the voter files. For arrivers, I merge full names and addresses from CoreLogic property transactions to the voter registration rolls, from which I recover sex and race indicators. This procedure necessarily reduces the sample, since not all arrivers can be matched to the voter records. Tables J.80 and J.81 in the Appendix document exposure rates for the main and assessment samples.

Gender. Tables J.82 and J.84 show that entrepreneurial spillovers differ sharply by gender. Column (2) indicates that women are significantly less likely than men to become entrepreneurs after exposure to an entrepreneurial arriver in general. Column (3) shows that this negative differential is somewhat attenuated—but not reversed—when the new entrepreneurial arriver is also a woman. Table J.83 clarifies this pattern by decomposing the effects by incumbent–arriver gender pair. Exposure raises business formation among male incumbents by 0.53 percentage points when the

arriver is a man and by 0.35 percentage points when the arriver is a woman. In contrast, exposure reduces business formation among female incumbents by 0.39 percentage points when the arriver is a man and by 0.38 percentage points when the arriver is a woman. Thus, women incumbents experience negative spillovers regardless of the arriver's gender, although the penalty is modestly smaller when the arriving entrepreneur is also female. Men likewise benefit disproportionately from exposure to male entrepreneurial arrivers. Overall, these results indicate that entrepreneurial spillovers are strongly gender-differentiated and directionally gender-homophilous, but highly asymmetric: entrepreneurial learning operates primarily through male networks, while women do not experience comparable gains from local exposure. Crucially, however, such homophilous opportunities are limited—while roughly 7.5 percent of all new arrivals have recent entrepreneurial experience, only about 4 percent of these entrepreneurial arrivals are women.

One remaining concern is that the gender patterns documented above may partially reflect household composition rather than individual-level spillovers, since some property purchases involve couples rather than single arrivals. Using the full names of purchasers in the transaction records, I classify arriving neighbors as single or joint buyers and restrict the sample to properties purchased by a single individual. Table J.85 repeats the analysis in Table J.82 for this subsample and yields very similar results. In particular, spillovers remain strongly gender-differentiated: exposure continues to raise business formation among male incumbents while reducing it among female incumbents, and the asymmetry across incumbent–arriver gender pairings persists. These findings indicate that the observed gender patterns are not driven by couple households or joint purchasing behavior, but instead reflect differences in how entrepreneurial spillovers operate at the individual level.

Tables J.86 and J.87 examine heterogeneity in entrepreneurial spillovers by race. Column (2) of Table J.86 shows that Black incumbents respond substantially more strongly to entrepreneurial arrivals than White incumbents. Column (3) allows the effect to vary with the race of the arriving entrepreneur, but the corresponding interaction terms provide little evidence that same-race exposure amplifies spillovers. Table J.87 clarifies this pattern by decomposing the effects by incumbent–arriver race pair. Exposure to entrepreneurial arrivals has small and statistically insignificant effects for White incumbents regardless of the arriver's race, whereas Black incumbents exhibit large and positive responses to entrepreneurial arrivals of both races. In particular, Black incumbents are significantly more likely to start a business following the arrival of either White or Black entrepreneurial neighbors, with no meaningful difference between same-race and cross-race pairings. The assessment sample in Table J.88 yields a similar pattern: spillovers are concentrated among Black incumbents, while White incumbents show little responsiveness, and coefficients associated with racial matching remain small and statistically insignificant. Overall, these results indicate that entrepreneurial spillovers differ by incumbent race but do not exhibit systematic racial homophily.[17]

Overall, these results contrast sharply with the gender dimension. Whereas women incumbents experience markedly weaker—and in some cases negative—responses to entrepreneurial exposure,

[17]Table J.89 replicates this analysis using a subsample restricted to single (non-couple) arrivals. The results are qualitatively similar and lead to the same conclusions.

Black incumbents respond substantially more strongly than White incumbents. This contrast is particularly notable when considered alongside the descriptive evidence: as shown in Tables J.80 and J.81, Black residents are also slightly more likely than White residents to be exposed to entrepreneurial arrivals in the first place. Thus, unlike the gender dimension—where both exposure and responsiveness are limited for women—racial differences in spillovers reflect heightened responsiveness among Black incumbents rather than differential access to entrepreneurial exposure.

Taken together, the evidence suggests that entrepreneurial spillovers operate through fundamentally different channels across gender and race. Along the gender dimension, spillovers reinforce disparities: women incumbents exhibit substantially weaker, and often negative, responses to entrepreneurial exposure, particularly when the arriving entrepreneur is male, and experience only modest attenuation when the arrival is female. By contrast, along the racial dimension, spillovers amplify responsiveness among Black incumbents without exhibiting racial homophily: Black residents respond strongly to entrepreneurial arrivals of both races, while White incumbents show little responsiveness. These patterns indicate that entrepreneurial learning in neighborhoods is shaped by gendered barriers to transmission, whereas racial differences reflect heterogeneous responsiveness rather than exclusion from local entrepreneurial networks.

Figures in the Online Appendix

I. Figures in the Online Appendix

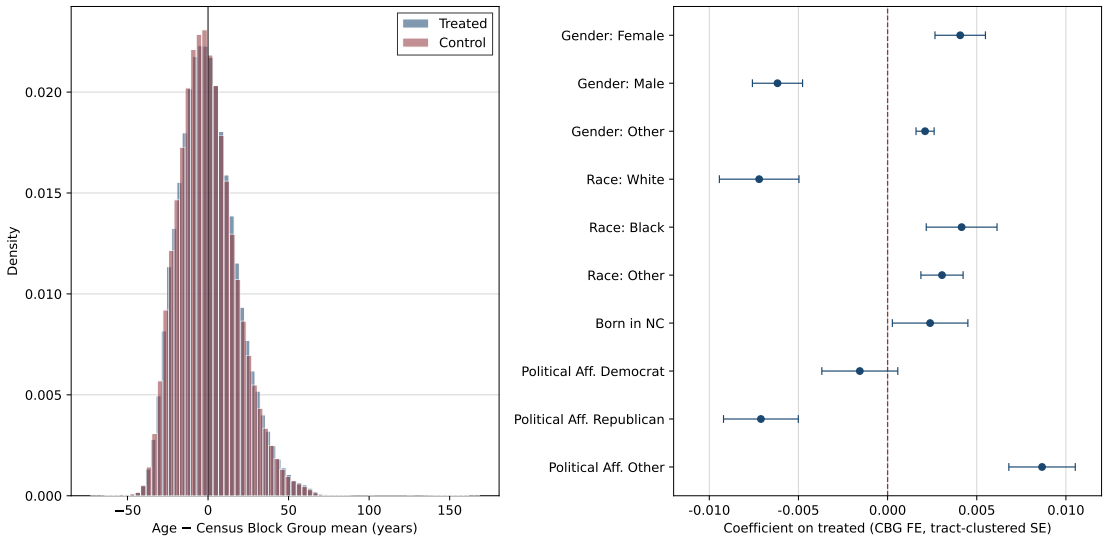


FIGURE I.1. Covariate Balance Within Census Block Groups, by Treatment Status

Covariate Balance Within Census Block Groups, by Treatment Status. The left panel plots the distribution of age after removing Census Block Group means, separately for treated and control units. The right panel reports, for each binary covariate, the treated−control difference within Census Block Groups (coefficient on *treated* from a regression with Census Block Group fixed effects) with 95% confidence intervals based on standard errors clustered at the census-tract level

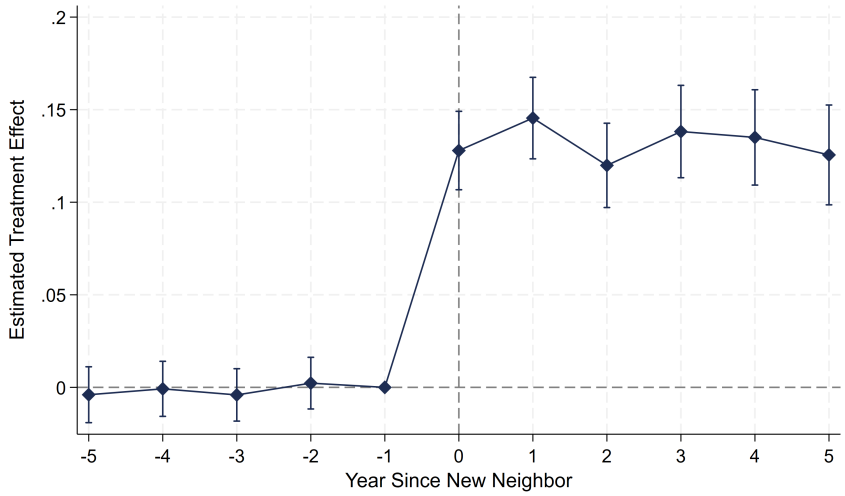


FIGURE I.2. Event-Study Estimates of Incumbent Entrepreneurship Around the Arrival of a New Entrepreneurial Neighbor with Housing Controls

This figure replicates the event-study analysis of Figure 4 but includes additional controls for housing characteristics (year built, lot size, and assessed value). The dependent variable and specification follow Equation 2. The omitted period is $e = -1$. All specifications include Census block group–by–year fixed effects, demographic controls, and housing controls. Standard errors are clustered at the Census tract level.

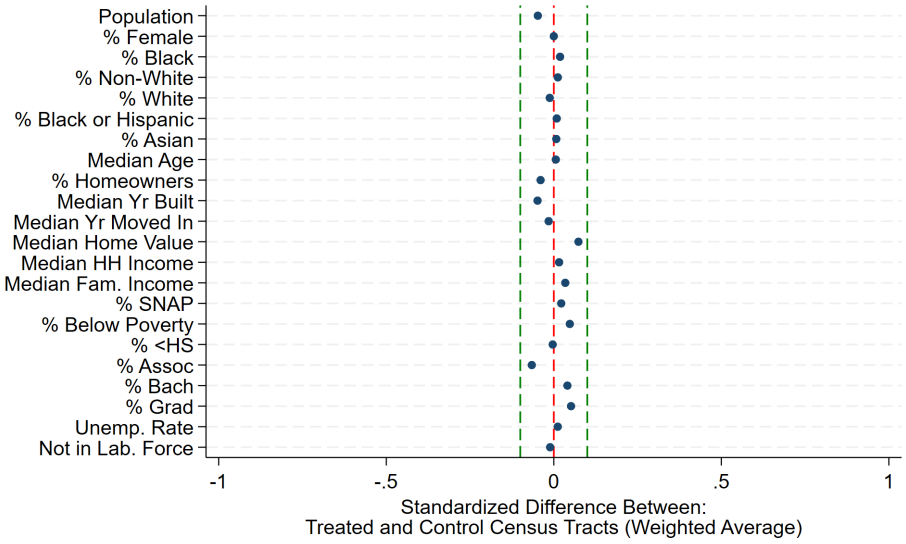


FIGURE I.3. Balance of Census Tract Characteristics—Levels, Assessment Sample

This figure shows balance in covariates between treated and control Census tracts for the assessment sample. Normalized differences are computed as in Figure 5. Level estimates use ACS 2010 data. Green dashed lines mark ±0.1 of the standardized difference.

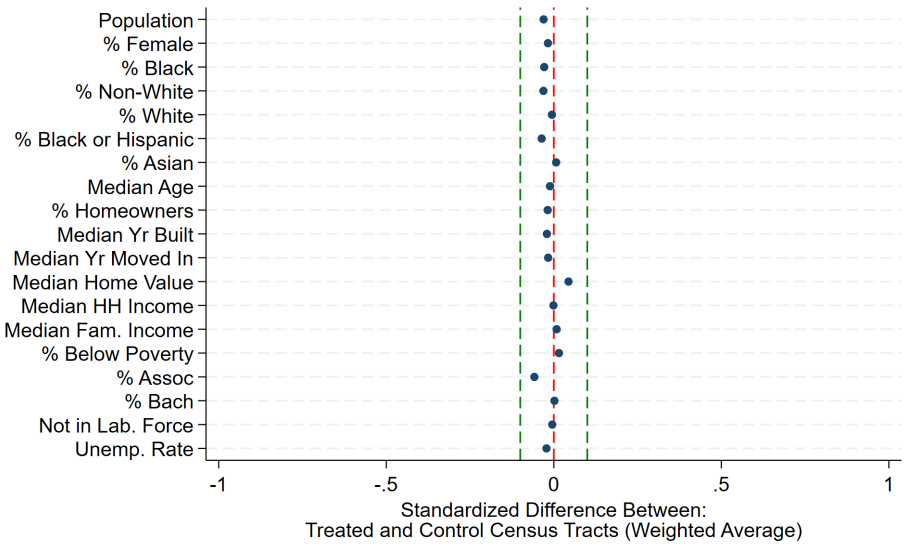
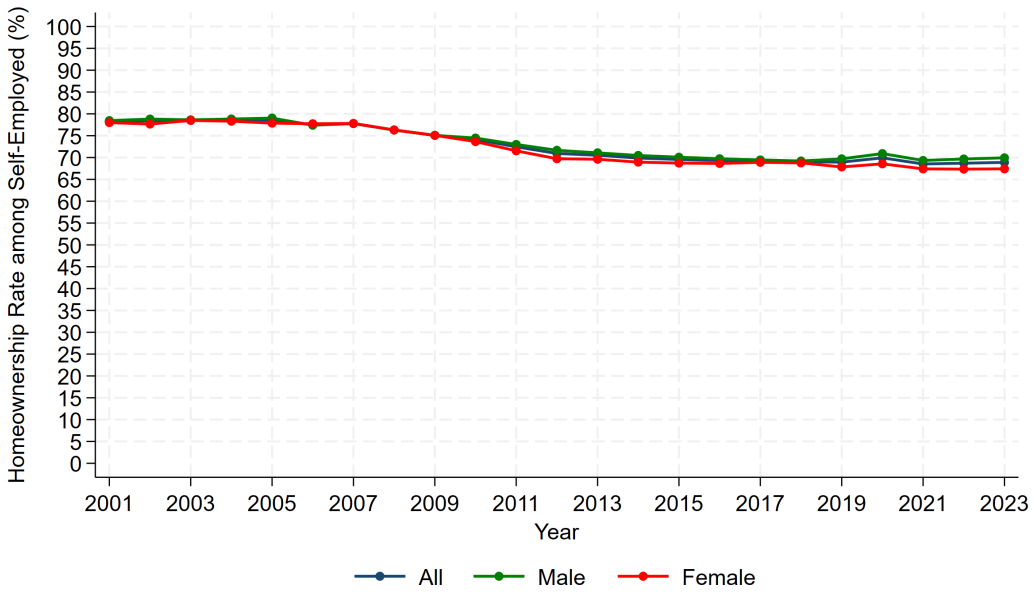
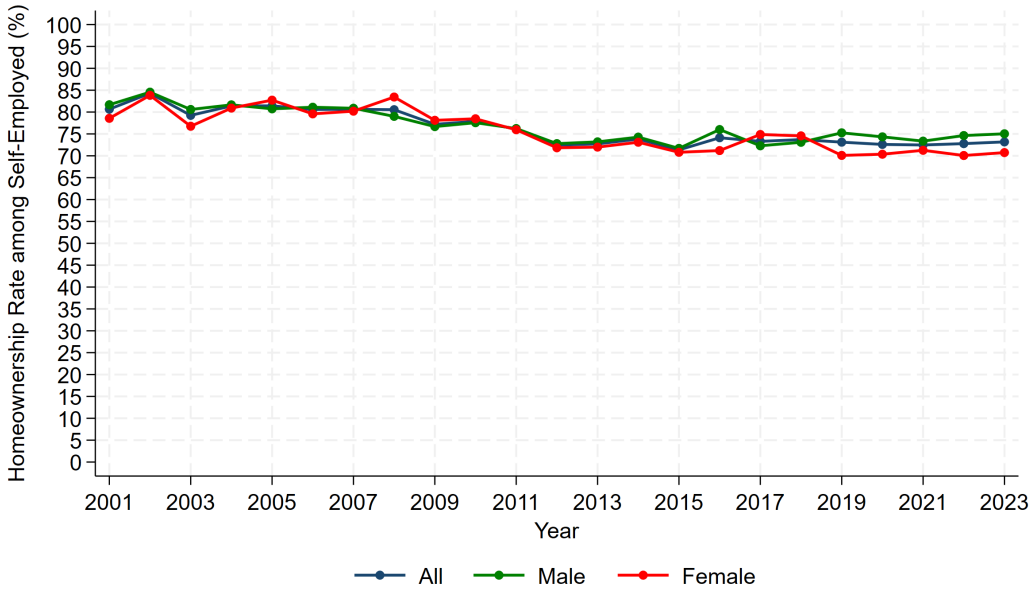


FIGURE I.4. Balance of Census Tract Characteristics—Trends, Assessment Sample

This figure compares pre-treatment trends in Census tract covariates for treated and control tracts in the assessment sample. Normalized differences are computed as in Figure 5. Trends are measured using changes between the 2000 Decennial Census and the 2010 ACS. Green dashed lines mark ±0.1 of the standardized difference.



A. United States



B. North Carolina

FIGURE I.5. Homeownership Rates among the Self-Employed, 2001–2023

This figure plots the share of self-employed individuals who are homeowners using ACS microdata from 2001 to 2023. Panel A reports national trends for the United States, while Panel B reports corresponding trends for North Carolina. In both panels, trends are shown for the full sample as well as separately by gender.

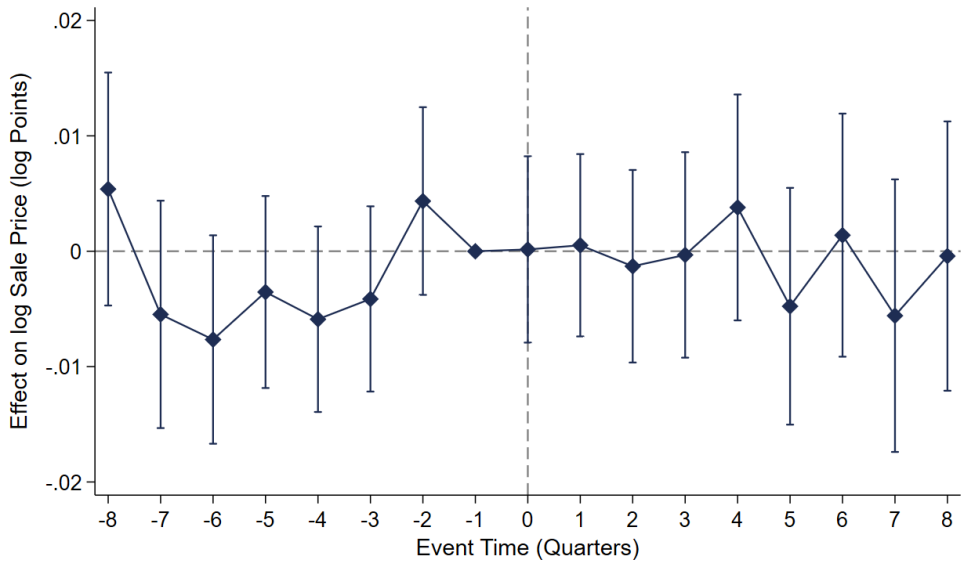


FIGURE I.6. Neighborhood House Prices Around Entrepreneur Arrival Spells: "No-Start" Controls

This figure plots coefficients from a stacked event-study estimating changes in neighborhood housing prices around the onset of an entrepreneur-arrival spell. The unit of observation is a census block group × calendar quarter, and the outcome is the mean log transaction sale price among non-entrepreneur purchases only, weighted by the number of non-entrepreneur transactions in the block group-quarter. An event occurs at $t = 0$ when the block group experiences entrepreneur-arrival purchases following a quarter with zero entrepreneur arrivals (i.e., the start of an arrival spell). The treated unit is the event block group. The control group consists of other block groups in the same census tract observed in the same event window, excluding control block groups that experience a spell start within the $[-8, +8]$ quarter window ("nostart" restriction). The regression includes event-by-block-group fixed effects and tract-by-calendar-quarter fixed effects; standard errors are clustered at the tract level. Coefficients are normalized to the omitted period $t = -1$. Vertical bars denote 95 percent confidence intervals.

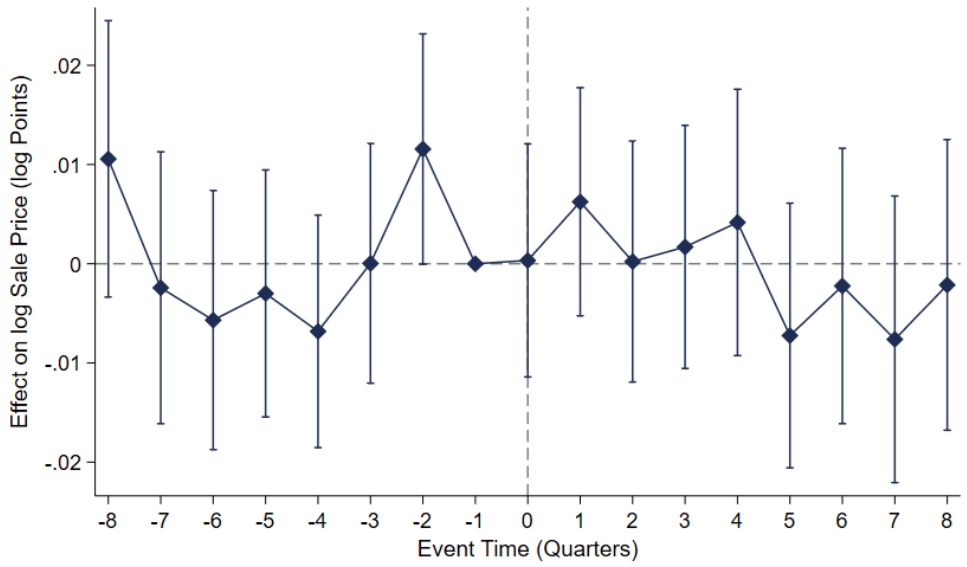


FIGURE I.7. Neighborhood House Prices Around Entrepreneur Arrival Spells: "No-Entrepreneur" Controls

Same specification as Figure I.6, except the control group is restricted to block groups within the same tract that experience no entrepreneur-arrival purchases in any quarter within the $[-8, +8]$ event window ("noent" restriction), rather than only excluding spell starts.

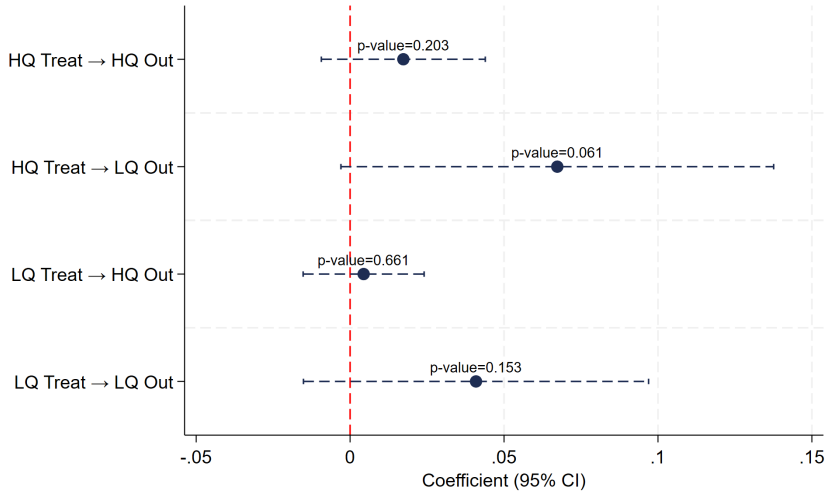


FIGURE I.8. Effect of High- and Low-Quality Entrepreneurial Neighbors on High- and Low-Quality Outcomes, Assessment Sample- Five-Year Horizon

This figure replicates Figure 8 using the assessment sample, which includes housing characteristics as additional controls. The remaining notes are the same as in Figure 8.

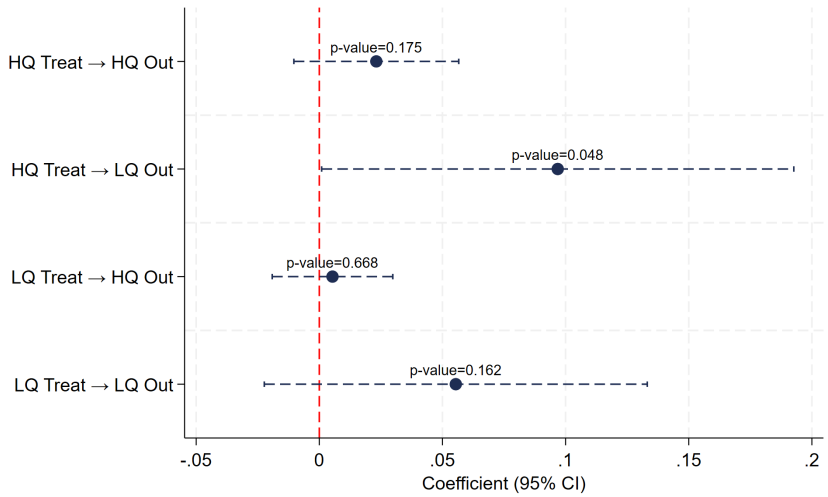


FIGURE I.9. Effect of High- and Low-Quality Entrepreneurial Neighbors on High- and Low-Quality Outcomes, Assessment Sample- Ten-Year Horizon

This figure replicates Figure 9 using the assessment sample, which includes housing characteristics as additional controls. The remaining notes are the same as in Figure 9.

Tables in the Online Appendix

J. Tables in the Online Appendix

TABLE J.1. Assessment Sample of Incumbent Residents Who Got New Nearby Neighbors-Part I

	Count	Mean	Std Dev
Dependent Variable			
Star a Business within 2 Years (0-100)	3,380,836	0.34	5.84
Star a Business within 5 Years (0-100)	3,380,836	1.05	10.19
Star a Business within 10 Years (0-100)	3,380,836	2.08	14.28
Current Resident Politics			
Democrat	3,380,836	0.38	0.49
Republican	3,380,836	0.34	0.47
Unaffiliated	3,380,836	0.28	0.45
Current Resident Demographics			
Gender: Female	3,380,836	0.53	0.50
Gender: Male	3,380,836	0.46	0.50
Gender: Other	3,380,836	0.01	0.11
Race: White	3,380,836	0.78	0.41
Race: Black	3,380,836	0.15	0.36
Race: Other	3,380,836	0.07	0.25
Age	3,380,836	50.59	19.12
Born in NC	3,380,836	0.32	0.47

This table combines North Carolina business registration records, voter registration files, and CoreLogic property data. For more details see Table 1.

TABLE J.2. Assessment Sample of Incumbent Residents Who Got New Nearby Neighbors-Part II

	Count	Mean	Std Dev
Property Characteristics			
Homeownership Arriver	3,380,836	0.52	0.50
Homeownership Incumbent	3,380,836	0.32	0.47
Home Value (USD)	3,380,252	213,014	155,099
Year Built	3,314,987	1984	22.75
Lot Size (Acres)	3,363,288	0.51	14.83
Building Sq Ft	3,318,943	1,971	865.26
# Bedrooms	2,410,310	3.19	0.77
# Bathrooms	3,204,024	2.51	0.95
Current Entrepreneurship			
Current Entrepreneur (0-100)	3,380,836	2.15	14.51
New Nearby Neighbor Entrepreneurship			
New Nbr Entrepreneur	3,380,836	0.08	0.26

This table combines North Carolina business registration records, voter registration files, and CoreLogic property data. For more details see Table 1.

TABLE J.3. External Validity and Treatment Balance: Comparison of Main Sample to North Carolina and U.S. Adult Populations

	NC Voter Data				Decennial Census 2000		Decennial Census 2010		Decennial Census 2020	
	Treated	Control	Main Sample	All Voters NC	U.S.	NC	U.S.	NC	U.S.	NC
Gender: Female	0.54	0.53	0.53	0.53	0.52	0.52	0.51	0.52	0.51	0.52
	(0.50)	(0.50)	(0.50)	(0.50)						
Gender: Male	0.45	0.46	0.46	0.46	0.48	0.48	0.49	0.48	0.49	0.48
	(0.50)	(0.50)	(0.50)	(0.50)						
Gender: Other	0.01	0.01	0.01	0.01						
	(0.12)	(0.11)	(0.11)	(0.10)						
Race: White	0.77	0.79	0.78	0.74	0.77	0.74	0.75	0.71	0.64	0.65
	(0.42)	(0.41)	(0.41)	(0.44)						
Race: Black	0.16	0.15	0.15	0.21	0.11	0.20	0.12	0.21	0.12	0.20
	(0.37)	(0.36)	(0.36)	(0.41)						
Race: Other	0.06	0.06	0.06	0.05	0.11	0.06	0.13	0.08	0.24	0.15
	(0.24)	(0.24)	(0.24)	(0.22)						
Age	50.06	49.73	49.75	51.22						
	(19.44)	(19.18)	(19.20)	(20.53)						
Born in NC	0.33	0.33	0.33	0.41						
	(0.47)	(0.47)	(0.47)	(0.49)						
Political Aff. Democrat	0.40	0.39	0.39	0.45						
	(0.49)	(0.49)	(0.49)	(0.50)						
Political Aff. Republican	0.33	0.34	0.34	0.32						
	(0.47)	(0.47)	(0.47)	(0.46)						
Political Aff. Other	0.27	0.27	0.26	0.23						
	(0.45)	(0.44)	(0.44)	(0.42)						

This table compares the demographic composition of the main sample, split by treatment status, to (i) all registered voters in North Carolina and (ii) the adult population (age 18 and older) in the United States and North Carolina using the Decennial Census (2000, 2010, and 2020). Columns (1)–(4) report summary statistics from North Carolina voter registration records. Columns (1) and (2) report means for treated and control incumbents, respectively; column (3) reports the pooled main sample; and column (4) reports all registered voters in North Carolina. Columns (5)–(10) report population shares from the Decennial Census for individuals age 18 or older. Standard deviations are reported in parentheses for individual-level data. To benchmark coverage, approximately 81% of North Carolina residents age 18 or older were registered to vote in 2005 (using the 2000 Census adult population as the denominator). The corresponding registration rates were 78% in 2010 and 72% in 2020.

TABLE J.4. Covariate Balance by Treatment Status (Main Sample): Raw and Within Census Block Group

	Treated	Control	Raw Diff.	Within-CBG Diff.	Norm. Diff.
	(1)	(2)	(3)	(4)	(5)
Gender: Female	0.54	0.53	+0.002**	+0.004***	0.00
	(0.50)	(0.50)	(0.001)	(0.001)	
Gender: Male	0.45	0.46	-0.005***	-0.006***	-0.01
	(0.50)	(0.50)	(0.001)	(0.001)	
Gender: Other	0.01	0.01	+0.003***	+0.002***	0.02
	(0.12)	(0.11)	(0.000)	(0.000)	
Race: White	0.77	0.79	-0.013***	-0.007***	-0.03
	(0.42)	(0.41)	(0.001)	(0.001)	
Race: Black	0.16	0.15	+0.010***	+0.004***	0.03
	(0.37)	(0.36)	(0.001)	(0.001)	
Race: Other	0.06	0.06	+0.003***	+0.003***	0.01
	(0.24)	(0.24)	(0.000)	(0.001)	
Age	50.06	49.73	+0.330***	+0.432***	0.02
	(19.44)	(19.18)	(0.035)	(0.053)	
Born in NC	0.33	0.33	+0.007***	+0.002**	0.01
	(0.47)	(0.47)	(0.001)	(0.001)	
Political Aff. Democrat	0.40	0.39	+0.006***	-0.002	0.01
	(0.49)	(0.49)	(0.001)	(0.001)	
Political Aff. Republican	0.33	0.34	-0.014***	-0.007***	-0.03
	(0.47)	(0.47)	(0.001)	(0.001)	
Political Aff. Other	0.27	0.27	+0.008***	+0.009***	0.02
	(0.45)	(0.44)	(0.001)	(0.001)	
Block Group FE				X	
Observations	338,565	4,431,232			

This table compares demographic characteristics between treated and control incumbents in the main analysis sample. Columns (1)–(2) report means by treatment status, with standard deviations in parentheses. Column (3) reports the raw difference in means (Treated − Control), with the standard error of the difference in parentheses. Column (4) reports the within–Census block group difference: the coefficient on treatment from a regression of each characteristic on the treatment indicator and Census block group fixed effects, with standard errors clustered at the Census tract level in parentheses. This within–group comparison mirrors the variation exploited in the main specification. Column (5) reports normalized differences, computed as $(\bar{X}_T - \bar{X}_C)/\sqrt{(s_T^2 + s_C^2)/2}$. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE J.5. Effect of Entrepreneurial Neighbors on Incumbent Business Formation after Ten Years — Assessment Sample

	1	2	3	4	5	6	7
New Nbr Entrepreneur	0.09183***	0.08551**	0.08723**	0.08947***	0.08795***	0.09012***	0.09581**
	(0.03359)	(0.03356)	(0.03388)	(0.03387)	(0.03401)	(0.03438)	(0.03886)
Fixed Effects:							
Year by Block Group	X	X	X	X	X	X	X
Controls:							
Home Value		X	X	X	X	X	X
Year Built			X	X	X	X	X
Lot Size (Acres)				X	X	X	X
Building Sq Ft					X	X	X
# Bathrooms						X	X
# Bedrooms							X
No. of Observations	3,378,890	3,378,307	3,312,731	3,296,001	3,282,750	3,172,876	2,374,822
Dependent Variable Mean	2.0835	2.0834	2.0907	2.0941	2.0950	2.0969	1.9999
New Nbr. Entrep. Mean	0.0758	0.0758	0.0759	0.0759	0.0760	0.0761	0.0759
FE Cells	54,393	54,390	52,856	52,558	52,198	50,638	41,854
R^2	0.0230	0.0231	0.0229	0.0229	0.0229	0.0230	0.0249

This table reports estimates of Equation 1 with $h = 10$, using the assessment sample. The remaining notes are the same as in Table 2, which presents the results using the main sample.

TABLE J.6. Effect of Entrepreneurial Neighbors on Incumbent Business Formation after Five Years — Assessment Sample

	1	2	3	4	5	6	7	8
New Nbr Entrepreneur	0.06118**	0.06132**	0.06877***	0.06590***	0.06738***	0.06106**	0.06203**	0.06604***
	(0.02412)	(0.02412)	(0.02416)	(0.02411)	(0.02434)	(0.02413)	(0.02451)	(0.02411)
Fixed Effects:								
Year by Block Group	X	X		X		X		X
Year by Block Group by Race			X					
Year by Block Group by Gender					X			
Year by Block Group by Party							X	
Controls:								
Age	X	X	X	X	X	X	X	X
Race		X						X
Gender				X				X
Party						X		X
No. of Observations	3,378,890	3,378,890	3,339,998	3,378,890	3,362,713	3,378,890	3,361,216	3,378,890
Dependent Variable Mean	1.0485	1.0485	1.0436	1.0485	1.0483	1.0485	1.0491	1.0485
New Nbr. Entrep. Mean	0.0758	0.0758	0.0758	0.0758	0.0758	0.0758	0.0758	0.0758
FE Cells	54,393	54,393	120,883	54,393	108,690	54,393	136,842	54,393
R^2	0.0169	0.0174	0.0498	0.0184	0.0360	0.0169	0.0401	0.0189

This table reports estimates of Equation 1 with $h = 5$, using the assessment sample. The remaining notes are the same as in Table 2, which presents the results using the main sample.

TABLE J.7. Effect of Entrepreneurial Neighbors on Incumbent Business Formation after Five Years — Restricted Assessment Sample

	1	2	3	4	5	6	7
New Nbr Entrepreneur	0.09070***	0.08778***	0.08921***	0.08921***	0.08894***	0.08887***	0.08888***
	(0.02816)	(0.02817)	(0.02818)	(0.02818)	(0.02816)	(0.02816)	(0.02816)
Fixed Effects:							
Year by Block Group	X	X	X	X	X	X	X
Controls:							
Age	X	X	X	X	X	X	X
Home Value		X	X	X	X	X	X
Year Built			X	X	X	X	X
Lot Size (Acres)				X	X	X	X
Building Sq Ft					X	X	X
# Bathrooms						X	X
# Bedrooms							X
No. of Observations	2,374,822	2,374,822	2,374,822	2,374,822	2,374,822	2,374,822	2,374,822
Dependent Variable Mean	1.0087	1.0087	1.0087	1.0087	1.0087	1.0087	1.0087
New Nbr. Entrep. Mean	0.0759	0.0759	0.0759	0.0759	0.0759	0.0759	0.0759
FE Cells	41,854	41,854	41,854	41,854	41,854	41,854	41,854
R^2	0.0186	0.0186	0.0186	0.0186	0.0187	0.0187	0.0187

This table reports estimates of Equation 1 with $h = 5$ using a restricted version of the assessment sample that excludes observations with missing housing property characteristics. The remaining notes are the same as in Table 2, which presents the results using the main sample.

TABLE J.8. Effect of Entrepreneurial Neighbors on Incumbent Business Formation after Five Years — Census Block Fixed Effects

	1	2	3	4	5	6	7	8
New Nbr Entrepreneur	0.08906***	0.09053***	0.09644***	0.09531***	0.09964***	0.08903***	0.08800***	0.09685***
	(0.02840)	(0.02837)	(0.02897)	(0.02848)	(0.02877)	(0.02840)	(0.02954)	(0.02846)
Fixed Effects:								
Year by Block	X	X		X		X		X
Year by Block by Race			X					
Year by Block by Gender					X			
Year by Block by Party							X	
Controls:								
Age	X	X	X	X	X	X	X	X
Race		X						X
Gender				X				X
Party						X		X
No. of Observations	4,193,210	4,193,210	4,191,599	4,193,210	4,190,581	4,193,210	4,189,580	4,193,210
Dependent Variable Mean	0.9763	0.9763	0.9762	0.9763	0.9762	0.9763	0.9764	0.9763
New Nbr. Entrep. Mean	0.0715	0.0715	0.0715	0.0715	0.0715	0.0715	0.0715	0.0715
FE Cells	314,909	314,909	440,922	314,909	568,611	314,909	650,012	314,909
R^2	0.0758	0.0762	0.1237	0.0769	0.1414	0.0758	0.1536	0.0773

This table replicates the specifications in Table 2, replacing Census block group fixed effects with Census block fixed effects. All specifications compare incumbent residents to neighbors residing on the same Census block.

TABLE J.9. Effect of Entrepreneurial Neighbors on Incumbent Business Formation after Ten Years — Census Block Fixed Effects

	1	2	3	4	5	6	7	8
New Nbr Entrepreneur	0.15232***	0.15500***	0.17056***	0.16417***	0.17406***	0.15226***	0.16021***	0.16696***
	(0.04015)	(0.04016)	(0.04088)	(0.04047)	(0.04062)	(0.04015)	(0.04146)	(0.04048)
Fixed Effects:								
Year by Block	X	X		X		X		X
Year by Block by Race			X					
Year by Block by Gender					X			
Year by Block by Party							X	
Controls:								
Age	X	X	X	X	X	X	X	X
Race		X						X
Gender				X				X
Party						X		X
No. of Observations	4,193,210	4,193,210	4,191,599	4,193,210	4,190,581	4,193,210	4,189,580	4,193,210
Dependent Variable Mean	1.9282	1.9282	1.9280	1.9282	1.9279	1.9282	1.9284	1.9282
New Nbr. Entrep. Mean	0.0715	0.0715	0.0715	0.0715	0.0715	0.0715	0.0715	0.0715
FE Cells	314,909	314,909	440,922	314,909	568,611	314,909	650,012	314,909
R^2	0.0794	0.0801	0.1260	0.0815	0.1464	0.0794	0.1583	0.0822

This table replicates the specifications in Table 3, replacing Census block group fixed effects with Census block fixed effects. All specifications compare incumbent residents to neighbors residing on the same Census block.

TABLE J.10. Effect of Entrepreneurial Neighbors on Incumbent Business Formation after Five Years: Assessment Sample with Census Block Fixed Effects

	1	2	3	4	5	6	7
New Nbr Entrepreneur	0.12058***	0.11861***	0.11830***	0.12036***	0.12169***	0.12169***	0.15856***
	(0.03303)	(0.03308)	(0.03349)	(0.03344)	(0.03340)	(0.03436)	(0.04019)
Fixed Effects:							
Year by Block	X	X	X	X	X	X	X
Controls:							
Age	X	X	X	X	X	X	X
Home Value		X	X	X	X	X	X
Year Built			X	X	X	X	X
Lot Size (Acres)				X	X	X	X
Building Sq Ft					X	X	X
# Bathrooms						X	X
# Bedrooms							X
No. of Observations	3,363,374	3,362,796	3,297,933	3,281,332	3,268,229	3,158,847	2,363,404
Dependent Variable Mean	1.0488	1.0487	1.0531	1.0550	1.0553	1.0571	1.0091
New Nbr. Entrep. Mean	0.0758	0.0758	0.0760	0.0760	0.0760	0.0761	0.0759
FE Cells	282,880	282,852	276,344	274,763	273,304	266,341	205,456
R^2	0.0850	0.0850	0.0848	0.0849	0.0847	0.0857	0.0879

This table replicates the specifications in Table 4, replacing Census block group fixed effects with Census block fixed effects. All specifications compare incumbent residents to neighbors residing on the same Census block.

TABLE J.11. Effect of Entrepreneurial Neighbors on Incumbent Business Formation after Five Years — Beyond the Same Side of the Street

	1	2	3	4	5	6	7	8
New Nbr Entrepreneur	0.03338**	0.03291**	0.04026**	0.03731**	0.03708**	0.03328**	0.03641**	0.03685**
	(0.01662)	(0.01661)	(0.01647)	(0.01658)	(0.01658)	(0.01662)	(0.01675)	(0.01656)
Fixed Effects:								
Year by Block Group	X	X		X		X		X
Year by Block Group by Race			X					
Year by Block Group by Gender					X			
Year by Block Group by Party							X	
Controls:								
Age	X	X	X	X	X	X	X	X
Race		X						X
Gender				X				X
Party						X		X
No. of Observations	7,559,266	7,559,266	7,559,266	7,559,266	7,559,266	7,559,266	7,559,266	7,559,266
Dependent Variable Mean	0.9755	0.9755	0.9755	0.9755	0.9755	0.9755	0.9755	0.9755
New Nbr. Entrep. Mean	0.0603	0.0603	0.0603	0.0603	0.0603	0.0603	0.0603	0.0603
FE Cells	91,887	91,887	181,963	91,887	186,887	91,887	244,115	91,887
R^2	0.0261	0.0264	0.0683	0.0279	0.0554	0.0261	0.0606	0.0283

This table replicates the specifications in Table 2 while expanding the definition of local exposure to include the four geographically closest neighbors regardless of street side. All specifications retain the same identifying structure, fixed effects, and control variables as in the baseline design.

TABLE J.12. Effect of Entrepreneurial Neighbors on Incumbent Business Formation after Ten Years — Beyond the Same Side of the Street

	1	2	3	4	5	6	7	8
New Nbr Entrepreneur	0.06174***	0.06083***	0.06741***	0.06939***	0.06844***	0.06148***	0.05796**	0.06847***
	(0.02260)	(0.02258)	(0.02251)	(0.02256)	(0.02262)	(0.02260)	(0.02271)	(0.02253)
Fixed Effects:								
Year by B. Group	X	X		X		X		X
Year by B. Group by Race			X					
Year by B. Group by Gender					X			
Year by B. Group by Party							X	
Controls:								
Age	X	X	X	X	X	X	X	X
Race		X						X
Gender				X				X
Party						X		X
No. of Observations	7,559,266	7,559,266	7,559,266	7,559,266	7,559,266	7,559,266	7,559,266	7,559,266
Dependent Variable Mean	1.9461	1.9461	1.9461	1.9461	1.9461	1.9461	1.9461	1.9461
New Nbr. Entrep. Mean	0.0603	0.0603	0.0603	0.0603	0.0603	0.0603	0.0603	0.0603
FE Cells	91,887	91,887	181,963	91,887	186,887	91,887	244,115	91,887
R^2	0.0285	0.0292	0.0713	0.0320	0.0591	0.0285	0.0647	0.0328

This table replicates the specifications in Table 3 using the expanded spatial exposure definition that includes the four closest neighbors irrespective of street side. All other aspects of the specification are identical to those in Table 3.

TABLE J.13. Effect of Entrepreneurial Neighbors on Incumbent Business Formation after Five Years — Assessment Sample Beyond the Same Side of the Street

	1	2	3	4	5	6	7
New Nbr Entrepreneur	0.05126***	0.04680**	0.04898**	0.04845**	0.04867**	0.04251**	0.04116*
	(0.01915)	(0.01917)	(0.01957)	(0.01967)	(0.01981)	(0.02024)	(0.02281)
Fixed Effects:							
Year by Block Group	X	X	X	X	X	X	X
Controls:							
Home Value		X	X	X	X	X	X
Year Built			X	X	X	X	X
Lot Size (Acres)				X	X	X	X
Building Sq Ft					X	X	X
# Bathrooms						X	X
# Bedrooms							X
No. of Observations	5,471,164	5,469,407	5,219,350	5,183,658	5,103,547	4,903,284	3,828,641
Dependent Variable Mean	1.0274	1.0266	1.0331	1.0348	1.0362	1.0442	1.0032
New Nbr. Entrep. Mean	0.0679	0.0679	0.0681	0.0682	0.0686	0.0687	0.0688
FE Cells	71,830	71,826	69,241	68,865	68,000	65,430	55,240
R^2	0.0285	0.0286	0.0283	0.0283	0.0283	0.0288	0.0304

This table replicates the specifications in Table 4 using the expanded spatial exposure definition and the assessment sample. All specifications include Census block group-by-year fixed effects and sequentially add housing property characteristics as controls.

TABLE J.14. Effect of Entrepreneurial Neighbors on Incumbent Business Formation after Five Years — Original Specification, Assessment Sample Beyond the Same Side of the Street

	1	2	3	4	5	6	7	8
New Nbr Entrepreneur	0.04745**	0.04692**	0.05362***	0.05179***	0.05151***	0.04721**	0.05111***	0.05126***
	(0.01920)	(0.01918)	(0.01902)	(0.01917)	(0.01917)	(0.01919)	(0.01935)	(0.01915)
Fixed Effects:								
Year by Block Group	X	X		X		X		X
Year by Block Group by Race			X					
Year by Block Group by Gender					X			
Year by Block Group by Party							X	
Controls:								
Age	X	X	X	X	X	X	X	X
Race		X						X
Gender				X				X
Party						X		X
No. of Observations	5,471,164	5,471,164	5,471,164	5,471,164	5,471,164	5,471,164	5,471,164	5,471,164
Dependent Variable Mean	1.0274	1.0274	1.0274	1.0274	1.0274	1.0274	1.0274	1.0274
New Nbr. Entrep. Mean	0.0679	0.0679	0.0679	0.0679	0.0679	0.0679	0.0679	0.0679
FE Cells	71,830	71,830	143,407	71,830	147,443	71,830	191,615	71,830
R^2	0.0264	0.0269	0.0700	0.0280	0.0559	0.0264	0.0610	0.0285

This table replicates the specifications in Table J.13 using the expanded spatial exposure definition and the assessment sample. All specifications include Census block group-by-year fixed effects and sequentially add housing property characteristics as controls.

TABLE J.15. Effect of Entrepreneurial Neighbors on Incumbent Business Formation after Ten Years, Assessment Sample Beyond the Same Side of the Street

	1	2	3	4	5	6	7
New Nbr Entrepreneur	0.07312***	0.06549**	0.07532***	0.07495***	0.07576***	0.06727**	0.06289**
	(0.02605)	(0.02606)	(0.02676)	(0.02688)	(0.02707)	(0.02765)	(0.03148)
Fixed Effects:							
Year by Block Group	X	X	X	X	X	X	X
Controls:							
Home Value		X	X	X	X	X	X
Year Built			X	X	X	X	X
Lot Size (Acres)				X	X	X	X
Building Sq Ft					X	X	X
# Bathrooms						X	X
# Bedrooms							X
No. of Observations	5,471,164	5,469,407	5,219,350	5,183,658	5,103,547	4,903,284	3,828,641
Dependent Variable Mean	2.0784	2.0776	2.0920	2.0944	2.1000	2.1098	2.0248
New Nbr. Entrep. Mean	0.0679	0.0679	0.0681	0.0682	0.0686	0.0687	0.0688
FE Cells	71,830	71,826	69,241	68,865	68,000	65,430	55,240
R^2	0.0328	0.0329	0.0327	0.0327	0.0328	0.0329	0.0341

This table replicates the specifications in Table J.14 using the expanded spatial exposure definition and the assessment sample, and $h = 10$. All specifications include Census block group-by-year fixed effects and sequentially add housing property characteristics as controls.

TABLE J.16. Effect of Entrepreneurial Neighbors on Incumbent Business Formation after Five Years — Excluding Same-Side Neighbors

	1	2	3	4	5	6	7	8
New Nbr Entrepreneur	0.05174**	0.05263**	0.04768*	0.05802**	0.05783**	0.05161**	0.05397**	0.05896**
	(0.02434)	(0.02437)	(0.02489)	(0.02432)	(0.02399)	(0.02435)	(0.02467)	(0.02436)
Fixed Effects:								
Year by Block Group	X	X		X		X		X
Year by Block Group by Race			X					
Year by Block Group by Gender					X			
Year by Block Group by Party							X	
Controls:								
Age	X	X	X	X	X	X	X	X
Race		X						X
Gender				X				X
Party						X		X
No. of Observations	4,655,202	4,655,202	4,632,942	4,655,202	4,643,761	4,655,202	4,641,864	4,655,202
Dependent Variable Mean	1.0320	1.0320	1.0294	1.0320	1.0316	1.0320	1.0316	1.0320
New Nbr. Entrep. Mean	0.0684	0.0684	0.0684	0.0684	0.0684	0.0684	0.0684	0.0684
FE Cells	69,904	69,904	151,403	69,904	140,383	69,904	177,853	69,904
R^2	0.0187	0.0193	0.0536	0.0204	0.0411	0.0187	0.0444	0.0211

This table replicates Table 2 but redefines exposure to exclude same-side neighbors. The set of focal arrivals remains unchanged; only the spatial mapping of treated incumbents is modified.

TABLE J.17. Effect of Entrepreneurial Neighbors on Incumbent Business Formation after Ten Years — Excluding Same-Side Neighbors

	1	2	3	4	5	6	7	8
New Nbr Entrepreneur	0.12914***	0.13074***	0.12462***	0.14085***	0.13927***	0.12890***	0.13573***	0.14253***
	(0.03583)	(0.03595)	(0.03612)	(0.03589)	(0.03609)	(0.03587)	(0.03647)	(0.03605)
Fixed Effects:								
Year by Block Group	X	X		X		X		X
Year by Block Group by Race			X					
Year by Block Group by Gender					X			
Year by Block Group by Party							X	
Controls:								
Age	X	X	X	X	X	X	X	X
Race		X						X
Gender				X				X
Party						X		X
No. of Observations	4,655,202	4,655,202	4,632,942	4,655,202	4,643,761	4,655,202	4,641,864	4,655,202
Dependent Variable Mean	2.0182	2.0182	2.0129	2.0182	2.0175	2.0182	2.0179	2.0182
New Nbr. Entrep. Mean	0.0684	0.0684	0.0684	0.0684	0.0684	0.0684	0.0684	0.0684
FE Cells	69,904	69,904	151,403	69,904	140,383	69,904	177,853	69,904
R^2	0.0207	0.0217	0.0550	0.0238	0.0439	0.0207	0.0472	0.0249

This table replicates Table 3 but redefines exposure to exclude same-side neighbors. The set of focal arrivals remains unchanged; only the spatial mapping of treated incumbents is modified.

TABLE J.18. Effect of Entrepreneurial Neighbors on Incumbent Business Formation after Five Years — Assessment Sample Excluding Same-Side Neighbors

	1	2	3	4	5	6	7
New Nbr Entrepreneur	0.06598**	0.06335**	0.06245**	0.06404**	0.06437**	0.06532**	0.06257**
	(0.02625)	(0.02627)	(0.02656)	(0.02673)	(0.02678)	(0.02735)	(0.02795)
Fixed Effects:							
Year by Block Group	X	X	X	X	X	X	X
Controls:							
Demographics	X	X	X	X	X	X	X
Home Value		X	X	X	X	X	X
Year Built			X	X	X	X	X
Lot Size (Acres)				X	X	X	X
Building Sq Ft					X	X	X
# Bathrooms						X	X
# Bedrooms							X
No. of Observations	3,929,970	3,929,395	3,851,315	3,813,938	3,795,416	3,675,748	2,792,829
Dependent Variable Mean	1.0713	1.0712	1.0760	1.0827	1.0833	1.0873	1.0369
New Nbr. Entrep. Mean	0.0736	0.0736	0.0739	0.0740	0.0742	0.0744	0.0748
FE Cells	57,540	57,540	55,922	55,557	54,976	53,216	44,682
R^2	0.0209	0.0210	0.0207	0.0206	0.0206	0.0207	0.0224

This table replicates Table 4 but redefines exposure to exclude same-side neighbors. The set of focal arrivals remains unchanged; only the spatial mapping of treated incumbents is modified.

TABLE J.19. Effect of Entrepreneurial Neighbors on Incumbent Business Formation after Five Years — Original Specification, Assessment Sample Excluding Same-Side Neighbors

	1	2	3	4	5	6	7	8
New Nbr Entrepreneur	0.05798**	0.05946**	0.05018*	0.06442**	0.06235**	0.05791**	0.05584**	0.06598**
	(0.02625)	(0.02626)	(0.02685)	(0.02623)	(0.02586)	(0.02627)	(0.02665)	(0.02625)
Fixed Effects:								
Year by Block Group	X	X		X		X		X
Year by Block Group by Race			X					
Year by Block Group by Gender					X			
Year by Block Group by Party							X	
Controls:								
Age	X	X	X	X	X	X	X	X
Race		X						X
Gender				X				X
Party						X		X
No. of Observations	3,929,972	3,929,970	3,893,784	3,929,972	3,913,688	3,929,972	3,912,162	3,929,970
Dependent Variable Mean	1.0713	1.0713	1.0671	1.0713	1.0705	1.0713	1.0709	1.0713
New Nbr. Entrep. Mean	0.0736	0.0736	0.0736	0.0736	0.0736	0.0736	0.0736	0.0736
FE Cells	57,540	57,540	127,433	57,540	115,076	57,540	144,337	57,540
R^2	0.0186	0.0193	0.0548	0.0202	0.0406	0.0186	0.0436	0.0209

This table mirrors Table J.14 but redefines exposure to exclude same-side neighbors. The set of focal arrivals remains unchanged; only the spatial mapping of treated incumbents is modified.

TABLE J.20. Effect of Entrepreneurial Neighbors on Incumbent Business Formation after Ten Years — Assessment Sample Excluding Same-Side Neighbors

	1	2	3	4	5	6	7
New Nbr Entrepreneur	0.15530***	0.15090***	0.15107***	0.15256***	0.15407***	0.15764***	0.13118***
	(0.03862)	(0.03864)	(0.03904)	(0.03925)	(0.03927)	(0.04026)	(0.04023)
Fixed Effects:							
Year by Block Group	X	X	X	X	X	X	X
Controls:							
Demographics	X	X	X	X	X	X	X
Home Value		X	X	X	X	X	X
Year Built			X	X	X	X	X
Lot Size (Acres)				X	X	X	X
Building Sq Ft					X	X	X
# Bathrooms						X	X
# Bedrooms							X
No. of Observations	3,929,970	3,929,395	3,851,315	3,813,938	3,795,416	3,675,748	2,792,829
Dependent Variable Mean	2.1097	2.1096	2.1184	2.1263	2.1258	2.1314	2.0393
New Nbr. Entrep. Mean	0.0736	0.0736	0.0739	0.0740	0.0742	0.0744	0.0748
FE Cells	57,540	57,540	55,922	55,557	54,976	53,216	44,682
R^2	0.0248	0.0249	0.0246	0.0245	0.0244	0.0245	0.0263

This table replicates Table J.15 but redefines exposure to exclude same-side neighbors. The set of focal arrivals remains unchanged; only the spatial mapping of treated incumbents is modified.

TABLE J.21. Entrepreneurial Spillovers from Arrivals Who Reside Locally (Presence Measured at $t+2$), Main Sample

	1	2	3	4	5	6	7	8
New Nbr Entrepreneur	0.09951***	0.09835***	0.11064***	0.10347***	0.11289***	0.09926***	0.10144***	0.10236***
	(0.03851)	(0.03811)	(0.03929)	(0.03869)	(0.03999)	(0.03848)	(0.03849)	(0.03830)
Fixed Effects:								
Year by Block Group	X	X		X		X		X
Year by Block Group by Race			X					
Year by Block Group by Gender					X			
Year by Block Group by Party							X	
Controls:								
Age	X	X	X	X	X	X	X	X
Race		X						X
Gender				X				X
Party						X		X
No. of Observations	3,919,699	3,919,699	3,908,965	3,919,699	3,914,037	3,919,699	3,912,976	3,919,699
Dependent Variable Mean	1.0147	1.0147	1.0128	1.0147	1.0148	1.0147	1.0150	1.0147
New Nbr. Entrep. Mean	0.0613	0.0613	0.0613	0.0613	0.0613	0.0613	0.0613	0.0613
FE Cells	64,479	64,479	137,384	64,479	129,854	64,479	164,680	64,479
R^2	0.0188	0.0194	0.0531	0.0199	0.0402	0.0188	0.0457	0.0204

"Present entrepreneurial arrival" restricts the arrival events to cases where the household residing in the purchased home is observed two years after purchase. This pool combines (i) resident owner-arrivals (owner occupies at $t+2$) and (ii) likely renter-arrivals (non-resident purchases with an observed occupier at $t+2$; treatment is reassigned using the occupant's entrepreneurial history). All other definitions follow the baseline; the difference is the presence-based arrival construction (measured at $t+2$).

TABLE J.22. Entrepreneurial Spillovers from Arrivals Who Reside Locally (Presence Measured at $t+2$), Assessment Sample

	1	2	3	4	5	6	7
New Nbr Entrepreneur	0.11269***	0.10960***	0.11198***	0.09097**	0.08976**	0.08195**	0.05958
	(0.04049)	(0.04074)	(0.04172)	(0.04032)	(0.04016)	(0.04073)	(0.04683)
Fixed Effects:							
Year by Block Group	X	X	X	X	X	X	X
Controls:							
Demographics	X	X	X	X	X	X	X
Home Value		X	X	X	X	X	X
Year Built			X	X	X	X	X
Lot Size (Acres)				X	X	X	X
Building Sq Ft					X	X	X
# Bathrooms						X	X
# Bedrooms							X
No. of Observations	3,384,033	3,383,507	3,321,245	3,256,026	3,242,020	3,135,368	2,377,143
Dependent Variable Mean	1.0453	1.0453	1.0503	1.0482	1.0486	1.0502	1.0011
New Nbr. Entrep. Mean	0.0665	0.0665	0.0664	0.0626	0.0628	0.0629	0.0667
FE Cells	55,722	55,720	54,245	53,909	53,474	51,895	42,489
R^2	0.0211	0.0211	0.0209	0.0211	0.0211	0.0213	0.0230

Same presence-based arrival restriction as in Table J.21: arrivals are kept only if the residing household is observed at $t+2$ (resident owners + likely renters, with renter treatment reassigned using the occupant's entrepreneurial history). Relative to the baseline assessment specification, this table additionally conditions on housing characteristics;all other elements of the design remain unchanged.

TABLE J.23. Entrepreneurial Spillovers by Presence Type: Homeowner vs Renter Arrivals, Main Sample

	All Arrivals	Homeowner Arrivals	Renter Arrivals
	(1)	(2)	(3)
New Nbr Entrepreneur	0.10236***	0.10170**	0.11791**
	(0.03830)	(0.04916)	(0.05272)
Fixed Effects:			
Year by Block Group	X	X	X
Demo. Controls	X	X	X
No. of Observations	3,919,699	1,669,657	2,248,473
Dependent Variable Mean	1.0147	1.0173	1.0129
New Nbr Entrepreneur Mean	0.0613	0.0393	0.0776
FE Cells	64,479	49,892	48,341
R^2	0.0204	0.0317	0.0265

This table decomposes the "present arrivals" sample into homeowner arrivals (owner-occupied at $t+2$) and renter arrivals (non-resident purchases with an observed occupier at $t+2$, with treatment reassigned using the occupant's entrepreneurial history). Column (1) pools all present arrivals; columns (2)–(3) estimate the same specification separately by presence type. Relative to the pooled presence table, the change is the split into homeowner vs renter arrivals.

TABLE J.24. Entrepreneurial Spillovers by Presence Type: Homeowner vs Renter Arrivals, Assessment Sample

	All Arrivals (1)	Homeowner Arrivals (2)	Renter Arrivals (3)
New Nbr Entrepreneur	0.09097**	0.12670**	0.09214*
	(0.04032)	(0.05670)	(0.05421)
Fixed Effects:			
Year by Block Group	X	X	X
Demo. Controls	X	X	X
Housing Controls	X	X	X
No. of Observations	3,256,026	1,230,285	2,024,664
Dependent Variable Mean	1.0482	1.0775	1.0307
New Nbr Entrepreneur Mean	0.0626	0.0427	0.0748
FE Cells	53,909	38,575	45,089
R^2	0.0211	0.0334	0.0276

This table applies the same presence-type decomposition as in Table J.23 (pooling all present arrivals, then separating homeowner vs renter arrivals based on occupancy at $t+2$) to the assessment sample, additionally controlling for the standard set of housing characteristics used in the assessment specifications.

TABLE J.25. Entrepreneurial Spillovers and Presence Type: Interactions with Homeowner vs Renter Arrivals, Main Sample

	1	2	3	4
New Nbr Entrepreneur	0.10236***	0.10393***	0.10452**	
	(0.03830)	(0.03880)	(0.05122)	
Homeowner Arrivals		0.00918	0.00927	0.00927
		(0.01257)	(0.01279)	(0.01279)
New Nbr Entrepreneur × Homeowner Arrivals			-0.00181	0.10270**
			(0.06752)	(0.04755)
New Nbr Entrepreneur × Renter Arrivals				0.10452**
				(0.05122)
Fixed Effects:				
Year by Block Group	X	X	X	X
Demo. Controls	X	X	X	X
No. of Observations	3,919,699	3,919,699	3,919,699	3,919,699
Dependent Variable Mean	1.0147	1.0147	1.0147	1.0147
New Nbr Entrepreneur Mean	0.0613	0.0613	0.0613	
New Nbr Entrepreneur × Homeowner Mean				0.0168
New Nbr Entrepreneur × Renter Mean				0.0445
FE Cells	64,479	64,479	64,479	64,479
R^2	0.0204	0.0201	0.0201	0.0201

This table formalizes the homeowner–renter decomposition by interacting New Nbr Entrepreneur with indicators for Homeowner Arrivals and Renter Arrivals, using the same presence-at-$t+2$ definitions as above. Column (1) reports the pooled presence effect. Column (2) adds a level indicator for homeowner arrivals. Columns (3)–(4) include interaction terms that allow the spillover to differ by presence type; column (4) reports the fully interacted specification with separate effects for homeowner- and renter-arrival exposure.

TABLE J.26. Entrepreneurial Spillovers and Presence Type: Interactions with Homeowner vs Renter Arrivals, Assessment Sample

	1	2	3	4
New Nbr Entrepreneur	0.09097**	0.09155**	0.07917	
	(0.04032)	(0.04071)	(0.05196)	
Homeowner Arrivals		0.00472	0.00272	0.00272
		(0.01361)	(0.01392)	(0.01392)
New Nbr Entrepreneur × Homeowner Arrivals			0.04019	0.11936**
			(0.07212)	(0.05416)
New Nbr Entrepreneur × Renter Arrivals				0.07917
				(0.05196)
Fixed Effects:				
Year by Block Group	X	X	X	X
Demo. Controls	X	X	X	X
Housing Controls	X	X	X	X
No. of Observations	3,256,026	3,256,026	3,256,026	3,256,026
Dependent Variable Mean	1.0482	1.0482	1.0482	1.0482
New Nbr Entrepreneur Mean	0.0626	0.0626	0.0626	
New Nbr Entrepreneur × Homeowner Mean				0.0161
New Nbr Entrepreneur × Renter Mean				0.0465
FE Cells	53,909	53,909	53,909	53,909
R^2	0.0211	0.0208	0.0208	0.0208

Follows the same interaction design as in Table J.25 (presence type defined at $t+2$), estimated in the assessment sample and including the standard housing controls.

TABLE J.27. Decomposing Entrepreneurial Arrivals: Presence and Property Ownership-Main Sample

	1	2	3	4	5	6	7	8
New Neighbor Is Entrepreneur	0.08827*	0.08193*	0.09316*	0.09275*	0.10539**	0.08749*	0.08590*	0.08649*
	(0.04962)	(0.04849)	(0.05051)	(0.04952)	(0.05141)	(0.04958)	(0.04991)	(0.04845)
Property Purchased by Entrepeneur	-0.01475	-0.02424	-0.01142	-0.01747	-0.01445	-0.01552	-0.01905	-0.02698
	(0.04482)	(0.04496)	(0.04668)	(0.04536)	(0.04473)	(0.04479)	(0.04759)	(0.04552)
Neig. Is Entrep. × Purchased by Entrep.	0.04402	0.06670	0.05819	0.04503	0.03349	0.04617	0.05986	0.06766
	(0.07415)	(0.07260)	(0.07502)	(0.07384)	(0.07372)	(0.07408)	(0.07579)	(0.07243)
Fixed Effects:								
Year by Block Group	X	X		X		X		X
Year by Block Group by Race			X					
Year by Block Group by Gender					X			
Year by Block Group by Party							X	
Controls:								
Age	X	X	X	X	X	X	X	X
Race		X						X
Gender				X				X
Party						X		X
No. of Observations	3,919,699	3,919,699	3,908,965	3,919,699	3,914,037	3,919,699	3,912,976	3,919,699
Dependent Variable Mean	1.0147	1.0147	1.0128	1.0147	1.0148	1.0147	1.0150	1.0147
New Nbr. Entrep. Mean	0.0613	0.0613	0.0613	0.0613	0.0613	0.0613	0.0613	0.0613
FE Cells	64,479	64,479	137,384	64,479	129,854	64,479	164,680	64,479
R^2	0.0188	0.0194	0.0531	0.0199	0.0402	0.0188	0.0457	0.0204

The specification includes indicators for whether the arriving neighbor is an entrepreneur, whether the property was purchased by an entrepreneur, and their interaction. The omitted category is a non-entrepreneur resident in a property purchased by a non-entrepreneur. The sample restricts to arrivals observed living at the property, as in J.21.

TABLE J.28. Decomposition of Entrepreneurial Presence and Entrepreneurial Purchase, Main Sample

	Property Purchased by	
Resident Living at Property	Entrepreneur	Non-Entrepreneur
Entrepreneur	0.1272**	0.0865*
	(0.0499)	(0.0485)
Non-Entrepreneur	-0.0270	0
	(0.0455)	(omitted)

This table reports the implied 2×2 decomposition from the specification that includes an indicator for whether the new neighbor is an entrepreneur, an indicator for whether the property was purchased by an entrepreneur, and the interaction between the two (Table J.27). Rows classify the entrepreneurial status of the resident living at the property, and columns classify the entrepreneurial status of the purchaser. Entrepreneurs are always listed first by row and column. The omitted category is a property purchased by a non-entrepreneur and occupied by a non-entrepreneur. Entries are coefficients relative to that omitted category. The $(1,1)$ cell is the linear combination of the main effects and the interaction term. Standard errors are reported in parentheses.

TABLE J.29. Decomposing Entrepreneurial Arrivals: Presence and Property Ownership-Assessment Sample

	1	2	3	4	5	6	7
New Neighbor Is Entrepreneur	0.09298*	0.09619*	0.09831*	0.07715	0.07925	0.07459	0.04388
	(0.04988)	(0.04996)	(0.05083)	(0.05098)	(0.05089)	(0.05185)	(0.05870)
Property Purchased by Entrepreneur	-0.02099	-0.01878	-0.01152	0.02271	0.02283	0.02939	-0.02532
	(0.04763)	(0.04771)	(0.04850)	(0.03784)	(0.03775)	(0.03854)	(0.04508)
Neig. Entrep. × Purchased by Entrep.	0.07774	0.05688	0.05128	0.01923	0.00964	-0.00512	0.06954
	(0.07722)	(0.07697)	(0.07643)	(0.07497)	(0.07514)	(0.07680)	(0.08609)
Fixed Effects:							
Year by Block Group	X	X	X	X	X	X	X
Controls:							
Demographics	X	X	X	X	X	X	X
Home Value		X	X	X	X	X	X
Year Built			X	X	X	X	X
Lot Size (Acres)				X	X	X	X
Building Sq Ft					X	X	X
# Bathrooms						X	X
# Bedrooms							X
No. of Observations	3,384,033	3,383,507	3,321,245	3,256,026	3,242,020	3,135,368	2,377,143
Dependent Variable Mean	1.0453	1.0453	1.0503	1.0482	1.0486	1.0502	1.0011
New Nbr. Entrep. Mean	0.0665	0.0665	0.0664	0.0626	0.0628	0.0629	0.0667
FE Cells	55,722	55,720	54,245	53,909	53,474	51,895	42,489
R^2	0.0211	0.0211	0.0209	0.0211	0.0211	0.0213	0.0230

Same presence-based arrival restriction as in Table J.27. Relative to the baseline assessment specification, this table additionally conditions on housing characteristics; all other elements of the design remain unchanged.

TABLE J.30. Decomposition of Entrepreneurial Presence and Property Ownership (Assessment Sample)

	Property Purchased by	
Resident Living at Property	Entrepreneur	Non-Entrepreneur
Entrepreneur	0.1191**	0.0772
	(0.0511)	(0.0509)
Non-Entrepreneur	0.0227	0
	(0.0378)	(omitted)

Implied 2×2 decomposition from Table J.29. Same presence-based arrival restriction as Table J.27; housing characteristics are additionally controlled for relative to the baseline assessment specification.

TABLE J.31. Effect of Entrepreneurial Neighbors under Alternative Control Exclusion Radii — Main Sample

	No Rest.	No Rest.	25m	100m	250m	500m	750m	1000m
New Nbr Entrepreneur	0.05191**	0.05414**	0.04992**	0.05151**	0.04230*	0.07144**	0.09829**	0.09692
	(0.02223)	(0.02295)	(0.02308)	(0.02335)	(0.02555)	(0.03580)	(0.04910)	(0.06241)
Fixed Effects:								
Year by Block Group	X	X	X	X	X	X	X	X
Demo. Controls	X	X	X	X	X	X	X	X
Is 2005?	X							
No. of Observations	4,076,898	3,790,531	3,726,525	3,257,882	2,231,638	1,272,497	861,568	662,236
Dependent Variable Mean	1.0035	1.0152	1.0137	0.9939	0.9563	0.9208	0.9167	0.9426
New Nbr. Entrep. Mean	0.0693	0.0717	0.0729	0.0834	0.1217	0.2132	0.3145	0.4088
FE Cells	62,842	58,612	58,540	58,318	57,192	53,162	47,828	43,481
R^2	0.0178	0.0179	0.0181	0.0205	0.0288	0.0468	0.0614	0.0711

This table reports estimates of Equation 1 with $h = 5$ for the main sample under alternative definitions of the control group. All specifications are identical to the baseline design—including the treatment definition, Census block group-by-year fixed effects, and demographic controls—except that control incumbents are required to have no entrepreneurial arrivals within a radius r meters during the year before, the year of, or the year after the focal arrival. Each column corresponds to a different value of r, as indicated in the column header. Standard errors, clustered at the Census tract level, are reported in parentheses.

TABLE J.32. Effect of Entrepreneurial Neighbors under Alternative Control Exclusion Radii — Assessment Sample

	No Rest.	25m	100m	250m	500m	750m	1000m
New Nbr Entrepreneur	0.06590***	0.06073**	0.06454***	0.05865**	0.09187**	0.12952**	0.14896**
	(0.02445)	(0.02458)	(0.02492)	(0.02754)	(0.03870)	(0.05401)	(0.07113)
Fixed Effects:							
Year by Block Group	X	X	X	X	X	X	X
Demo. Controls	X	X	X	X	X	X	X
Housing Controls	X	X	X	X	X	X	X
No. of Observations	3,295,903	3,238,198	2,817,505	1,914,269	1,089,507	742,225	575,846
Dependent Variable Mean	1.0549	1.0535	1.0325	0.9970	0.9639	0.9632	0.9886
New Nbr. Entrep. Mean	0.0759	0.0773	0.0888	0.1307	0.2295	0.3365	0.4334
FE Cells	52,558	52,473	52,248	51,172	47,596	42,963	39,177
R^2	0.0189	0.0192	0.0217	0.0303	0.0485	0.0630	0.0728

This table reports estimates of Equation 1 with $h = 5$ for the assessment sample under alternative definitions of the control group. The table replicates the structure of Table J.31, but additionally controls for housing property characteristics, including year built, lot size, and assessed value.

TABLE J.33. Balance Test—Comparing Demographic Characteristics of the Treatment and Control Groups (with Housing Controls)

Sample:	Current Residents Who Got New Nearby Neighbors							
Dependent Variable:	Age	Born in NC	Female	Male	Democrat	Republican	White	Black
New Nbr Entrepreneur	-0.12084**	0.00302**	0.00698***	-0.00724***	0.00088	-0.00030	0.00134	0.00008
	(0.05129)	(0.00123)	(0.00086)	(0.00086)	(0.00118)	(0.00123)	(0.00102)	(0.00121)
Fixed Effects:								
Year by Block Group	X	X	X	X	X	X	X	X
Demo. Controls	X	X	X	X	X	X	X	X
Housing Controls	X	X	X	X	X	X	X	X
No. of Observations	3,296,001	3,296,001	3,296,001	3,296,001	3,296,001	3,296,001	3,296,001	3,296,001
Dep. Variable Mean	50.5540	0.3196	0.5302	0.4567	0.3808	0.3385	0.1478	0.7850
Relative Effect	-0.0024	0.0094	0.0132	-0.0159	0.0023	-0.0009	0.0091	0.0001
New Nbr Entrep. Mean	0.0759	0.0759	0.0759	0.0759	0.0759	0.0759	0.0759	0.0759
FE Cells	52,558	52,558	52,558	52,558	52,558	52,558	52,558	52,558
R^2	0.1737	0.1310	0.0183	0.0174	0.2104	0.1356	0.3526	0.3037

This table reports coefficient estimates of the "effect" of receiving a new entrepreneurial nearby neighbor on demographic characteristics of incumbent residents, using the assessment sample. Compared to Table 6, the specification additionally controls for housing property characteristics (year built, lot size, and assessed value). Variables are as defined in the text and Table 1. All specifications include Census block group–by–year fixed effects, and standard errors are clustered at the Census tract level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

TABLE J.34. Heterogeneity over Distance to the New Nearby Entrepreneur, Assessment Sample

Subsample, Distance to Neighbor (Meters)	0-25	25-50	50-120	All
	(1)	(2)	(3)	(4)
New Nbr Entrepreneur	0.09946**	0.06157*	0.06822	0.09908**
	(0.04789)	(0.03514)	(0.05496)	(0.04304)
Distance: 25-50m				0.01610
				(0.01715)
Distance: 50-120m				0.01755
				(0.02082)
New Nbr Entrepreneur x Distance: 25-50m				-0.06234
				(0.05401)
New Nbr Entrepreneur x Distance: 50-120m				-0.03253
				(0.06447)
Fixed Effects:				
Year by Block Group	X	X	X	X
Demographics Controls	X	X	X	X
Housing Controls	X	X	X	X
No. of Observations	1,113,807	1,424,922	634,479	3,295,903
Dependent Variable Mean	1.1632	1.0336	0.9550	1.0549
New Nbr. Entrep. Mean	0.0754	0.0754	0.0782	0.0759
FE Cells	32,128	43,013	31,667	52,558
R^2	0.0296	0.0326	0.0534	0.0189

This table repeats the analysis of Table 7 using the assessment sample, which additionally controls for housing characteristics. Subsamples are again defined as 0–25 meters, 25–50 meters, and 50–120 meters. Column (4) pools all observations and interacts the treatment indicator with distance categories. All specifications include year-by-group fixed effects, demographic and housing controls. Standard errors, clustered at the tract-year level, are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE J.35. Heterogeneity over Distance to the New Nearby Entrepreneur Using Distance Terciles, Main Sample

Subsample, Distance Tercile (Meters)	First Tercile	Second Tercile	Third Tercile	All
	(1)	(2)	(3)	(4)
New Nbr Entrepreneur	0.08090*	0.03548	0.03546	0.09077**
	(0.04280)	(0.03791)	(0.03747)	(0.04084)
Second Tercile				0.02055
				(0.01671)
Third Tercile				0.06147***
				(0.01759)
New Nbr Entrepreneur x Second Tercile				-0.07271
				(0.05352)
New Nbr Entrepreneur x Third Tercile				-0.04746
				(0.05269)
Fixed Effects:				
Year by Block Group	X	X	X	X
Demographics Controls	X	X	X	X
No. of Observations	1,357,274	1,356,997	1,357,202	4,076,898
Distance Mean (Meters)	17.54	31.56	154.87	68.03
Dependent Variable Mean	1.1091	0.9702	0.9324	1.0035
New Nbr. Entrep. Mean	0.0689	0.0677	0.0712	0.0693
FE Cells	39,855	49,476	49,059	62,842
R^2	0.0294	0.0380	0.0393	0.0178

This table reestimates the main specification allowing the effect of exposure to a new entrepreneurial neighbor to vary by physical distance, discretized into terciles of the distance distribution. Columns (1)–(3) report estimates for incumbents whose new neighbor falls within the first, second, and third distance terciles, respectively. Column (4) pools all observations and interacts the treatment indicator with distance tercile indicators. All specifications include year-by-group fixed effects and demographic controls. Standard errors, clustered at the tract-year level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE J.36. Heterogeneity over Distance to the New Nearby Entrepreneur Using Distance Terciles, Assessment Sample

Subsample, Distance Tercile (Meters)	First Tercile (1)	Second Tercile (2)	Third Tercile (3)	All (4)
New Nbr Entrepreneur	0.09599**	0.06428	0.03424	0.11050**
	(0.04799)	(0.04053)	(0.04173)	(0.04582)
Second Tercile				0.00396
				(0.01899)
Third Tercile				0.03598*
				(0.02066)
New Nbr Entrepreneur x Second Tercile				-0.07077
				(0.05894)
New Nbr Entrepreneur x Third Tercile				-0.06556
				(0.05952)
Fixed Effects:				
Year by Block Group	X	X	X	X
Demographics Controls	X	X	X	X
Housing Controls	X	X	X	X
No. of Observations	1,109,722	1,104,199	1,078,444	3,295,903
Distance Mean (Meters)	17.42	31.65	162.15	69.61
Dependent Variable Mean	1.1644	1.0223	0.9759	1.0549
New Nbr. Entrep. Mean	0.0754	0.0749	0.0775	0.0759
FE Cells	32,072	39,871	40,071	52,558
R^2	0.0296	0.0385	0.0409	0.0189

This table replicates Table J.35 using the assessment sample and additionally controls for housing characteristics. All other aspects of the specification, fixed effects, and inference follow Table J.35.

TABLE J.37. Effect of Entrepreneurial Neighbors on Incumbent Business Formation, by Arriver Residency Status (2 yrs After Arrival), Main Sample

	All Arrivals Full Sample	Resident Arrivals Only	Non-Resident Arrivals Only
	(1)	(2)	(3)
New Nbr Entrepreneur	0.04352**	0.09169*	0.04487*
	(0.02030)	(0.04891)	(0.02360)
Fixed Effects:			
Year by Block Group	X	X	X
Demo. Controls	X	X	X
No. of Observations	4,672,279	1,672,407	2,997,288
Dependent Variable Mean	0.9917	1.0147	0.9791
New Nbr Entrepreneur Mean	0.0711	0.0392	0.0889
FE Cells	67,064	50,447	63,818
R^2	0.0167	0.0316	0.0231

Note: This table replicates the analysis in Table 8 using an alternative definition of resident arrivals that restricts the homeowner–address match to the first two years following the purchase. All other specifications, fixed effects, and controls are identical to the baseline.

TABLE J.38. Effect of Entrepreneurial Neighbors on Incumbent Business Formation, by Arriver Residency Status (5 yrs After Arrival), Main Sample

	All Arrivals Full Sample	Resident Arrivals Only	Non-Resident Arrivals Only
	(1)	(2)	(3)
New Nbr Entrepreneur	0.04352**	0.07532*	0.04614*
	(0.02030)	(0.04455)	(0.02395)
Fixed Effects:			
Year by Block Group	X	X	X
Demo. Controls	X	X	X
No. of Observations	4,672,279	1,913,894	2,755,681
Dependent Variable Mean	0.9917	0.9975	0.9876
New Nbr Entrepreneur Mean	0.0711	0.0408	0.0921
FE Cells	67,064	53,328	62,768
R^2	0.0167	0.0301	0.0241

This table replicates the analysis in Table 8 using an alternative definition of resident arrivals based on a homeowner–address match observed five years after the purchase. All other specifications, fixed effects, and controls are identical to the baseline.

TABLE J.39. Effect of Entrepreneurial Neighbors on Incumbent Business Formation, by Arriver Residency Status (2 yrs After Arrival), Assessment Sample

	All Arrivals Full Sample	Resident Arrivals Only	Non-Resident Arrivals Only
	(1)	(2)	(3)
New Nbr Entrepreneur	0.06592***	0.12670**	0.06589**
	(0.02445)	(0.05670)	(0.02855)
Fixed Effects:			
Year by Block Group	X	X	X
Demo. Controls	X	X	X
Housing Controls	X	X	X
No. of Observations	3,296,001	1,230,285	2,064,331
Dependent Variable Mean	1.0549	1.0775	1.0415
New Nbr Entrepreneur Mean	0.0759	0.0427	0.0957
FE Cells	52,558	38,575	49,206
R^2	0.0189	0.0334	0.0261

Note: This table replicates the analysis in Table 9 using an alternative definition of resident arrivals that restricts the homeowner–address match to the first two years following the purchase. All other specifications, fixed effects, and controls are identical to the baseline.

TABLE J.40. Effect of Entrepreneurial Neighbors on Incumbent Business Formation, by Arriver Residency Status (5 yrs After Arrival), Assessment Sample

	All Arrivals Full Sample (1)	Resident Arrivals Only (2)	Non-Resident Arrivals Only (3)
New Nbr Entrepreneur	0.06592***	0.09862*	0.07115**
	(0.02445)	(0.05099)	(0.02907)
Fixed Effects:			
Year by Block Group	X	X	X
Demo. Controls	X	X	X
Housing Controls	X	X	X
No. of Observations	3,296,001	1,407,761	1,886,837
Dependent Variable Mean	1.0549	1.0579	1.0525
New Nbr Entrepreneur Mean	0.0759	0.0445	0.0994
FE Cells	52,558	41,008	48,144
R^2	0.0189	0.0320	0.0270

This table replicates the analysis in Table 9 using an alternative definition of resident arrivals based on a homeowner–address match observed five years after the purchase. All other specifications, fixed effects, and controls are identical to the baseline.

TABLE J.41. Effect of Renter Entrepreneurial Neighbors on Incumbent Business Formation (One-Year Occupancy Window), Main Sample

	1	2	3	4	5	6	7	8
New Nbr Entrepreneur	0.06932*	0.06513*	0.06774*	0.07590**	0.07504**	0.06904*	0.07283*	0.07170**
	(0.03677)	(0.03621)	(0.03780)	(0.03670)	(0.03826)	(0.03673)	(0.03736)	(0.03615)
Fixed Effects:								
Year by Block Group	X	X		X		X		X
Year by Block Group by Race			X					
Year by Block Group by Gender					X			
Year by Block Group by Party							X	
Controls:								
Age	X	X	X	X	X	X	X	X
Race		X						X
Gender				X				X
Party						X		X
No. of Observations	2,818,170	2,818,170	2,818,052	2,818,170	2,818,038	2,818,170	2,817,932	2,818,170
Dependent Variable Mean	1.0141	1.0141	1.0141	1.0141	1.0141	1.0141	1.0142	1.0141
New Nbr. Entrep. Mean	0.0678	0.0678	0.0678	0.0678	0.0678	0.0678	0.0678	0.0678
FE Cells	51,659	51,659	107,062	51,659	104,590	51,659	132,103	51,659
R^2	0.0217	0.0224	0.0604	0.0232	0.0464	0.0217	0.0524	0.0238

This table replicates the analysis in Table 12 using a one-year post-purchase window to identify renter occupancy. All other definitions, specifications, and inference procedures are identical to those in the main sample.

TABLE J.42. Effect of Renter Entrepreneurial Neighbors on Incumbent Business Formation (Relaxed Name-Matching Restriction), Main Sample

	1	2	3	4	5	6	7	8
New Nbr Entrepreneur	0.06253*	0.05941*	0.06876*	0.06928*	0.07624**	0.06212*	0.06553*	0.06616*
	(0.03659)	(0.03582)	(0.03721)	(0.03646)	(0.03734)	(0.03655)	(0.03671)	(0.03570)
Fixed Effects:								
Year by Block Group	X	X		X		X		X
Year by Block Group by Race			X					
Year by Block Group by Gender					X			
Year by Block Group by Party							X	
Controls:								
Age	X	X	X	X	X	X	X	X
Race		X						X
Gender				X				X
Party						X		X
No. of Observations	3,994,503	3,994,503	3,994,404	3,994,503	3,994,373	3,994,503	3,994,296	3,994,503
Dependent Variable Mean	1.0283	1.0283	1.0284	1.0283	1.0284	1.0283	1.0284	1.0283
New Nbr. Entrep. Mean	0.0784	0.0784	0.0784	0.0784	0.0784	0.0784	0.0784	0.0784
FE Cells	51,717	51,717	117,990	51,717	107,362	51,717	135,617	51,717
R^2	0.0164	0.0170	0.0491	0.0180	0.0363	0.0165	0.0400	0.0186

: This table replicates the analysis in Table 12 while relaxing the restriction that renter occupants must not share a last name with the homeowner. All other definitions, specifications, and inference procedures are identical to those in the main sample.

TABLE J.43. Effect of Renter Entrepreneurial Neighbors on Incumbent Business Formation (One-Year Occupancy Window), Assessment Sample

	1	2	3	4	5	6	7
New Nbr Entrepreneur	0.06488*	0.06279*	0.06586*	0.05065	0.04818	0.03814	0.01147
	(0.03776)	(0.03795)	(0.03835)	(0.03827)	(0.03826)	(0.03880)	(0.04558)
Fixed Effects:							
Year by Block Group	X	X	X	X	X	X	X
Controls:							
Demographics	X	X	X	X	X	X	X
Home Value		X	X	X	X	X	X
Year Built			X	X	X	X	X
Lot Size (Acres)				X	X	X	X
Building Sq Ft					X	X	X
# Bathrooms						X	X
# Bedrooms							X
No. of Observations	2,538,946	2,538,530	2,489,378	2,438,664	2,429,248	2,354,142	1,818,174
Dependent Variable Mean	1.0484	1.0486	1.0539	1.0570	1.0566	1.0599	1.0185
New Nbr. Entrep. Mean	0.0726	0.0726	0.0722	0.0687	0.0688	0.0689	0.0700
FE Cells	48,167	48,162	46,893	46,585	46,255	44,895	37,233
R^2	0.0246	0.0246	0.0244	0.0246	0.0245	0.0247	0.0266

This table replicates the analysis in Table 13 using a one-year post-purchase window to identify renter occupancy. All other definitions, specifications, controls, and inference procedures are identical to those in the assessment sample.

TABLE J.44. Effect of Renter Entrepreneurial Neighbors on Incumbent Business Formation (Relaxed Name-Matching Restriction), Assessment Sample

	1	2	3	4	5	6	7
New Nbr Entrepreneur	0.07144**	0.06967*	0.07166*	0.05388	0.05186	0.03757	0.03832
	(0.03639)	(0.03662)	(0.03733)	(0.03633)	(0.03636)	(0.03716)	(0.04404)
Fixed Effects:							
Year by Block Group	X	X	X	X	X	X	X
Controls:							
Demographics	X	X	X	X	X	X	X
Home Value		X	X	X	X	X	X
Year Built			X	X	X	X	X
Lot Size (Acres)				X	X	X	X
Building Sq Ft					X	X	X
# Bathrooms						X	X
# Bedrooms							X
No. of Observations	3,824,220	3,823,741	3,752,438	3,664,235	3,639,022	3,515,232	2,644,308
Dependent Variable Mean	1.0454	1.0454	1.0509	1.0495	1.0503	1.0532	0.9986
New Nbr. Entrep. Mean	0.0809	0.0809	0.0804	0.0748	0.0751	0.0755	0.0800
FE Cells	51,651	51,648	50,242	49,953	49,560	48,049	39,983
R^2	0.0191	0.0192	0.0190	0.0191	0.0192	0.0194	0.0213

This table replicates the analysis in Table 13 while relaxing the restriction that renter occupants must not share a last name with the homeowner. All other definitions, specifications, controls, and inference procedures are identical to those in the assessment sample.

TABLE J.45. Do Entrepreneurial Arrivals Pay a Housing Price Premium? Evidence from Parcel Fixed Effects

	1	2	3	4	5
New Nbr Entrepreneur	-0.03723***	-0.03891***	-0.03438***	-0.02975***	-0.03896***
	(0.00200)	(0.00211)	(0.00201)	(0.00208)	(0.00205)
Fixed Effects:					
Year-Quarter	X	X			X
Year-Quarter by Tract			X		
Year-Quarter by Block Group				X	
Parcel	X	X	X	X	X
Controls:					
Cash Purchase Indicator	X	X	X	X	X
No. of Observations	988,174	894,610	858,360	737,575	133,528
Dependent Variable Mean	12.14	12.15	12.17	12.23	12.17
New Nbr. Entrep. Mean	0.0613	0.0614	0.0612	0.0607	0.4281
FE Cells	60	60	95,425	133,522	60
FE Parcel Cells	436,061	399,234	383,208	329,991	55,665
R^2	0.9009	0.9044	0.9386	0.9516	0.8943

The dependent variable is log transaction sale price. New Nbr Entrepreneur is an indicator equal to one if the buyer is an entrepreneurial arrival (as defined in the main text). All specifications include parcel fixed effects, so coefficients are identified from within-parcel price changes across repeat sales. Column (1) includes year-by-quarter fixed effects and controls for cash purchases. Column (2) excludes short sales and related-party transfers. Column (3) replaces year-by-quarter fixed effects with tract-by-year-by-quarter fixed effects. Column (4) replaces year-by-quarter fixed effects with block-group-by-year-by-quarter fixed effects. Column (5) restricts the sample to "switcher" parcels—properties that transact to both entrepreneurial and non-entrepreneur buyers—using the same specification as Column (1). Standard errors, clustered at the census tract level, are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE J.46. Do Entrepreneurial Arrivals Pay a Housing Price Premium? Evidence Controlling for Housing Characteristics

	1	2	3	4
New Nbr Entrepreneur	-0.02181***	-0.02272***	-0.03280***	-0.02929***
	(0.00280)	(0.00285)	(0.00171)	(0.00162)
Fixed Effects:				
Year-Quarter	X	X		
Year-Quarter by Tract			X	
Year-Quarter by Block Group				X
Controls:				
Cash Purchase Indicator	X	X	X	X
Restricted Housing Controls	X	X	X	X
No. of Observations	1,869,068	1,778,553	1,768,400	1,709,799
Dependent Variable Mean	12.10	12.12	12.12	12.14
New Nbr. Entrep. Mean	0.0614	0.0612	0.0611	0.0610
FE Cells	60	60	120,999	239,738
R^2	0.4915	0.4942	0.7218	0.7686

The dependent variable is log transaction sale price. New Nbr Entrepreneur is an indicator equal to one if the buyer is an entrepreneurial arrival (as defined in the main text). Column (1) includes year-by-quarter fixed effects and controls for cash purchases. Column (2) excludes short sales and related-party transfers. Column (3) replaces year-by-quarter fixed effects with tract-by-year-by-quarter fixed effects. Column (4) replaces year-by-quarter fixed effects with block-group-by-year-by-quarter fixed effects. Restricted housing controls include lot size (acres), year built, and building square feet. Standard errors clustered at the census-tract level are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

TABLE J.47. Do Entrepreneurial Arrivals Pay a Housing Price Premium? Evidence with Full Housing Controls

	1	2	3	4
New Nbr Entrepreneur	-0.02335***	-0.02440***	-0.03347***	-0.03005***
	(0.00321)	(0.00326)	(0.00189)	(0.00179)
Fixed Effects:				
Year-Quarter	X	X		
Year-Quarter by Tract			X	
Year-Quarter by Block Group				X
Controls:				
Cash Purchase Indicator	X	X	X	X
All Housing Controls	X	X	X	X
No. of Observations	1,422,336	1,354,145	1,344,467	1,296,208
Dependent Variable Mean	12.07	12.08	12.09	12.11
New Nbr. Entrep. Mean	0.0611	0.0608	0.0608	0.0606
FE Cells	60	60	97,899	190,548
R^2	0.4935	0.4962	0.7184	0.7655

This table replicates the specifications of Table J.46 but augments the housing controls by additionally including the number of bedrooms and bathrooms. All other controls, fixed effects, and clustering follow the specifications described in J.46.

TABLE J.48. Entrepreneur Arrival Intensity and Housing Prices Paid by Non-Entrepreneur Buyers

	1	2
Count of entrepreneurial buyers in Block Group × Quarter	-0.00122**	-0.00133**
	(0.00051)	(0.00061)
Fixed Effects:		
Block Group	X	X
Year-Quarter by Tract	X	X
Controls:		
Cash Purchase Indicator	X	X
Lot Size (Acres)	X	X
Year Built	X	X
Building Sq Ft	X	X
# Bathrooms		X
# Bedrooms		X
No. of Observations	1,658,968	1,261,553
Dependent Variable Mean	12.12	12.09
FE Cells Block Group	6,780	5,574
FE Cells Year-Quarter by Tract	119,737	96,756
R^2	0.7415	0.7375

The dependent variable is log transaction sale price. The sample includes non-entrepreneur transactions only. The key regressor is the count of entrepreneurial arrivals in the same census block group × calendar quarter as the transaction. All specifications include block group fixed effects and tract × year-quarter fixed effects. Standard errors, clustered at the census tract level, are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE J.49. Effect of Exposure to High- and Low-Quality Entrepreneurial Neighbors on Business Formation after Five Business-Assessment Sample

	1	2	3	4
New Nbr Entrepreneur	0.06592***			
	(0.02445)			
New Nbr Low Quality Entrep.		0.05129*		0.05420*
		(0.03099)		(0.03100)
New Nbr High Quality Entrep.			0.08897**	0.09180**
			(0.03874)	(0.03875)
Fixed Effects:				
Year by Block Group	X	X	X	X
Demo. Controls	X	X	X	X
Housing Controls	X	X	X	X
$H_0 : \beta_{low} = \beta_{High}$				
F-stat [p-value]				0.59 [0.44]
No. of Observations	3,296,001	3,296,001	3,296,001	3,296,001
Dependent Variable Mean	1.0549	1.0549	1.0549	1.0549
New Nbr. Entrep. Mean	0.0759			
New Nbr. Low Quality Entrep. Mean		0.0476		0.0476
New Nbr. High Quality Entrep. Mean			0.0281	0.0281
FE Cells	52,558	52,558	52,558	52,558
R^2	0.0189	0.0189	0.0189	0.0189

This table replicates Table 16 using the assessment sample, which includes housing characteristics as additional controls. The remaining notes are the same as in Table 16.

TABLE J.50. Effect of Entrepreneurial Exposure on Business Formation by Outcome Quality, Assessment Sample

	1	2	3
New Nbr Entrepreneur	0.06592***	0.00878	0.05066**
	(0.02445)	(0.00807)	(0.02244)
Fixed Effects:			
Year by Block Group	X	X	X
Demo. Controls	X	X	X
Housing Controls	X	X	X
No. of Observations	3,296,001	3,296,001	3,296,001
Dep. Variable Mean	1.0549		
Dep. Variable Mean High Quality		0.1283	
Dep. Variable Mean Low Quality			0.9145
New Nbr. Entrep. Mean	0.0759	0.0759	0.0759
FE Cells	52,558	52,558	52,558
R^2	0.0189	0.0160	0.0185

This table replicates Tables 17–18 using the assessment sample, which includes housing characteristics as additional controls. Column (1) reports the baseline effect of exposure to any entrepreneurial neighbor on business formation. Columns (2) and (3) distinguish between incorporated (high-quality) and non-incorporated (low-quality) business outcomes. All specifications include year-by-group fixed effects, demographic controls, and housing controls. Standard errors, clustered at the Census tract level, are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE J.51. Summary Statistics by Distance Bin: Main Ring Sample (2005–2019)

Dist.-bin	# of Obs. (M)	Avg. Age (yrs)	Avg. White (%)	Avg. Rep. (%)	Avg. Male (%)
0-25m	1.91	47.89	59.74	29.55	43.57
25-50m	4.46	49.32	63.85	33.64	45.11
50-75m	3.97	48.69	64.04	32.65	44.71
75-100m	3.3	48.67	64.17	32.31	44.6
100-125m	2.72	48.31	64.66	31.68	44.58
125-150m	2.12	48.22	64.71	31.53	44.48
150-175m	1.66	48.61	65.52	32.28	44.81
175-200m	1.37	48.4	65.95	31.76	44.79
200-225m	1.08	48.45	66.27	32.06	44.78
>225m	4.97	49.21	69.55	33.71	45.15

This table reports demographic characteristics of incumbents in the main ring sample (2005–2019). Distance bins are defined in 25-meter increments relative to the focal arrival, with the final category pooling residents located more than 225 meters away. The table shows that demographic characteristics evolve smoothly with distance, with no sharp discontinuities across adjacent bins.

TABLE J.52. Summary Statistics by Distance Bin: Assessment Sample (2008–2019), Part I

Dist.-bin	# of Obs. (M)	Avg. Age (yrs)	Avg. White (%)	Avg. Rep. (%)	Avg. Male (%)
0-25m	1.59	48.19	71.44	28.96	43.51
25-50m	3.72	49.64	76.48	33.0	45.04
50-75m	3.38	48.84	75.31	31.85	44.6
75-100m	2.83	48.78	74.86	31.48	44.43
100-125m	2.35	48.36	74.68	30.77	44.42
125-150m	1.84	48.23	74.35	30.53	44.27
150-175m	1.44	48.63	75.41	31.31	44.61
175-200m	1.2	48.38	75.31	30.74	44.59
200-225m	0.95	48.43	75.42	31.07	44.59
>225m	4.4	49.25	78.59	32.87	44.96

This table reports demographic characteristics of incumbents in the assessment sample, which begins in 2008 due to CoreLogic coverage of property data. Distance bins are defined in 25-meter increments relative to the focal arrival, with the final category pooling residents located more than 225 meters away.

TABLE J.53. Summary Statistics by Distance Bin: Assessment Sample (2008–2019), Part II

Dist.-bin	Avg. Home Value ($)	Avg. Acres	Avg. Year Built	Avg. Square Feet
0-25m	184,767	0.2	1,988.12	1,754.56
25-50m	205,186	0.94	1,986.65	1,934.35
50-75m	200,042	0.68	1,988.69	1,902.81
75-100m	198,862	0.61	1,989.45	1,896.57
100-125m	199,233	0.46	1,990.47	1,885.70
125-150m	200,415	0.49	1,991.25	1,888.41
150-175m	200,818	0.56	1,990.63	1,902.20
175-200m	201,454	0.55	1,991.09	1,903.34
200-225m	202,234	0.54	1,991.20	1,914.36
>225m	204,042	0.92	1,989.90	1,895.03

This table reports housing characteristics of incumbents in the assessment sample, corresponding to the distance bins defined in Table J.52.

TABLE J.54. Summary Statistics by Distance Bin: Main Ring Sample, Resident Arrivals (2005–2019)

Dist.-bin	# of Obs. (M)	Avg. Age (yrs)	Avg. White (%)	Avg. Rep. (%)	Avg. Male (%)
0-25m	0.64	47.88	64.14	30.78	43.71
25-50m	1.6	49.37	67.87	35.24	45.47
50-75m	1.4	48.84	67.47	34.08	45.13
75-100m	1.17	48.56	67.24	33.32	44.82
100-125m	0.96	48.19	67.7	32.66	44.67
125-150m	0.76	47.84	67.3	32.02	44.44
150-175m	0.57	48.26	68.24	33.24	44.87
175-200m	0.48	48.08	68.13	32.52	44.77
200-225m	0.38	47.98	68.16	32.61	44.78
>225m	1.66	49.02	70.47	34.02	45.2

This table reports demographic characteristics of incumbents in the main ring sample for cases in which the arriving buyer resides in the purchased property (resident arrivals). Distance bins correspond to those defined in Table J.51.

TABLE J.55. Summary Statistics by Distance Bin: Assessment Sample, Resident Arrivals (2008–2019), Part I

Dist.-bin	# of Obs. (M)	Avg. Age (yrs)	Avg. White (%)	Avg. Rep. (%)	Avg. Male (%)
0-25m	0.56	48.18	74.03	30.05	43.63
25-50m	1.38	49.68	78.98	34.5	45.38
50-75m	1.22	49.01	77.5	33.19	45.0
75-100m	1.03	48.65	76.55	32.39	44.66
100-125m	0.85	48.24	76.36	31.71	44.47
125-150m	0.68	47.82	75.38	30.93	44.18
150-175m	0.51	48.28	76.84	32.23	44.67
175-200m	0.43	48.07	76.22	31.47	44.55
200-225m	0.34	47.96	75.98	31.56	44.58
>225m	1.49	49.08	78.37	33.06	45.0

This table reports demographic characteristics of incumbents in the assessment sample for cases in which the arriving buyer resides in the purchased property (resident arrivals). The assessment sample begins in 2008 due to CoreLogic coverage. Distance bins correspond to those defined in Table J.52.

TABLE J.56. Summary Statistics by Distance Bin: Assessment Sample, Resident Arrivals (2008–2019), Part II

Dist.-bin	Avg. Home Value ($)	Avg. Acres	Avg. Year Built	Avg. Square Feet
0-25m	202,148	0.18	1,989.24	1,846.16
25-50m	226,439	0.36	1,987.51	2,061.53
50-75m	219,884	0.41	1,989.31	2,029.32
75-100m	216,975	0.42	1,990.41	2,007.84
100-125m	215,599	0.47	1,991.34	1,986.93
125-150m	214,685	0.5	1,992.72	1,962.54
150-175m	216,908	0.52	1,991.91	2,006.0
175-200m	216,130	0.6	1,992.4	2,004.32
200-225m	214,900	0.57	1,992.79	2,010.68
>225m	216,374	0.98	1,991.28	2,022.73

This table reports housing characteristics of incumbents in the assessment sample for cases in which the arriving buyer resides in the purchased property (resident arrivals). Distance bins correspond to those defined in Table J.52.

TABLE J.57. Summary Statistics by Distance Bin: Main Ring Sample, Non-Resident Arrivals (2005–2019)

Dist.-bin	# of Obs. (M)	Avg. Age (yrs)	Avg. White (%)	Avg. Rep. (%)	Avg. Male (%)
0-25m	1.26	47.9	57.48	28.93	43.5
25-50m	2.85	49.29	61.59	32.73	44.91
50-75m	2.57	48.62	62.17	31.87	44.49
75-100m	2.13	48.74	62.47	31.76	44.47
100-125m	1.76	48.37	63.0	31.14	44.53
125-150m	1.36	48.44	63.25	31.26	44.5
150-175m	1.09	48.79	64.08	31.78	44.77
175-200m	0.89	48.57	64.78	31.35	44.8
200-225m	0.7	48.7	65.26	31.77	44.78
>225m	3.31	49.31	69.1	33.55	45.12

This table reports demographic characteristics of incumbents in the main ring sample for cases in which the arriving buyer does not reside in the purchased property (non-resident arrivals). Distance bins correspond to those defined in Table J.51.

TABLE J.58. Summary Statistics by Distance Bin: Assessment Sample, Non-Resident Arrivals (2008–2019), Part I

Dist.-bin	# of Obs. (M)	Avg. Age (yrs)	Avg. White (%)	Avg. Rep. (%)	Avg. Male (%)
0-25m	1.04	48.19	70.05	28.37	43.44
25-50m	2.34	49.62	75.02	32.11	44.84
50-75m	2.16	48.74	74.07	31.09	44.37
75-100m	1.8	48.85	73.89	30.96	44.3
100-125m	1.5	48.44	73.74	30.24	44.4
125-150m	1.17	48.46	73.74	30.3	44.32
150-175m	0.93	48.83	74.63	30.8	44.57
175-200m	0.77	48.56	74.8	30.34	44.61
200-225m	0.61	48.7	75.11	30.8	44.6
>225m	2.91	49.34	78.7	32.78	44.94

This table reports demographic characteristics of incumbents in the assessment sample for cases in which the arriving buyer does not reside in the purchased property (non-resident arrivals). The assessment sample begins in 2008 due to CoreLogic coverage. Distance bins correspond to those defined in Table J.52.

TABLE J.59. Summary Statistics by Distance Bin: Assessment Sample, Non-Resident Arrivals (2008–2019), Part II

Dist.-bin	Avg. Home Value ($)	Avg. Acres	Avg. Year Built	Avg. Square Feet
0-25m	175,402	0.21	1,987.52	1,705.06
25-50m	192,680	1.29	1,986.14	1,859.0
50-75m	188,840	0.84	1,988.33	1,831.02
75-100m	188,525	0.72	1,988.91	1,832.79
100-125m	190,015	0.46	1,989.97	1,828.38
125-150m	192,093	0.48	1,990.39	1,844.96
150-175m	192,017	0.58	1,989.93	1,845.19
175-200m	193,321	0.52	1,990.36	1,847.07
200-225m	195,243	0.52	1,990.32	1,860.89
>225m	197,730	0.89	1,989.19	1,829.1

This table reports housing characteristics of incumbents in the assessment sample for cases in which the arriving buyer does not reside in the purchased property (non-resident arrivals). Distance bins correspond to those defined in Table J.52

TABLE J.60. Robustness to Flexible Life-Cycle Controls, Main Sample

	1	2	3	4
New Nbr Entrepreneur	0.04352**	0.04470**	0.04468**	0.04413**
	(0.02030)	(0.02031)	(0.02031)	(0.02088)
Fixed Effects:				
Year by Block Group	X	X	X	
Year by Block Group by Age bin				X
Demo. Controls	X	X	X	X
Age	X		X	
Age bin		X	X	
No. of Observations	4,672,279	4,672,279	4,672,279	4,639,057
Dependent Variable Mean	0.9917	0.9917	0.9917	0.9927
New Nbr. Entrep. Mean	0.0711	0.0711	0.0711	0.0711
FE Cells	67,064	67,064	67,064	263,308
R^2	0.0167	0.0170	0.0170	0.0588

This table reports robustness checks allowing for flexible life-cycle controls in the main sample. Columns (1)–(3) include year-by-block-group fixed effects and progressively replace a linear age control with age-bin indicators. Column (4) absorbs fully saturated year-by-block-group-by-age-bin fixed effects. All specifications include demographic controls. Standard errors are clustered at the census-tract level.

TABLE J.61. Robustness to Flexible Life-Cycle Controls, Assessment Sample

	1	2	3	4
New Nbr Entrepreneur	0.06592***	0.06698***	0.06696***	0.07080***
	(0.02445)	(0.02447)	(0.02447)	(0.02511)
Fixed Effects:				
Year by Block Group	X	X	X	
Year by Block Group by Age bin				X
Demo. Controls	X	X	X	X
Housing Controls	X	X	X	X
Age	X		X	
Age bin		X	X	
No. of Observations	3,296,001	3,296,001	3,296,001	3,268,725
Dependent Variable Mean	1.0549	1.0549	1.0549	1.0560
New Nbr. Entrep. Mean	0.0759	0.0759	0.0759	0.0760
FE Cells	52,558	52,558	52,558	197,904
R^2	0.0189	0.0192	0.0192	0.0638

This table replicates the life-cycle robustness checks in the assessment sample. Columns (1)–(3) include year-by-block-group fixed effects and progressively replace a linear age control with age-bin indicators. Column (4) absorbs fully saturated year-by-block-group-by-age-bin fixed effects. All specifications include demographic and housing controls. Standard errors are clustered at the census-tract level.

TABLE J.62. Heterogeneity by Pre-Period Tract Demographic Characteristics (Census 2000), Main Sample

	1	2	3	4	5	6	7
New Entrepreneurial Neighbor	0.04498**	0.04592**	0.04553**	0.04566**	0.04581**	0.04559**	0.04607**
	(0.02051)	(0.02046)	(0.02050)	(0.02047)	(0.02047)	(0.02047)	(0.02055)
New Entrep. Neigh. × Ln Population		0.00661					
		(0.03794)					
New Entrep. Neigh. × Female Share			-0.08098				
			(0.11909)				
New Entrep. Neigh. × Black Share				0.18135			
				(0.11301)			
New Entrep. Neigh. × Asian Share					-1.01900		
					(1.24167)		
New Entrep. Neigh. × Associate Degree Share						-0.14902	
						(0.76247)	
New Entrep. Neigh. × Bachelor Degree Share							-0.07704
							(0.19429)
Fixed Effects:							
Year by Block Group	X	X	X	X	X	X	X
Demo. Controls	X	X	X	X	X	X	X
No. of Observations	4,631,184	4,631,184	4,631,184	4,631,184	4,631,184	4,631,184	4,631,184
Dependent Variable Mean	0.9922	0.9922	0.9922	0.9922	0.9922	0.9922	0.9922
New Nbr. Entrep. Mean	0.0704	0.0704	0.0704	0.0704	0.0704	0.0704	0.0704
FE Cells	65,499	65,499	65,499	65,499	65,499	65,499	65,499
R^2	0.0165	0.0163	0.0163	0.0163	0.0163	0.0163	0.0163

his table reports estimates of Equation 1 interacting exposure to a new entrepreneurial neighbor with census-tract demographic characteristics measured in the 2000 Decennial Census. In columns (2)–(7), each specification includes the centered tract-level characteristic and its interaction with exposure to a new entrepreneurial neighbor. Tract characteristics include log population, female share, racial composition, and educational attainment. All tract-level variables are centered at their sample means. Because these characteristics are time-invariant at the tract level, their main effects are absorbed by year-by-block-group fixed effects and are not separately identified. The coefficient on New Entrepreneurial Neighbor represents the treatment effect evaluated at the sample mean of the tract characteristic included in that column. All specifications include individual-level demographic controls and year-by-block-group fixed effects. Standard errors are clustered at the census tract level.

TABLE J.63. Heterogeneity by Pre-Period Tract Economic Characteristics (Census 2000), Main Sample

	1	2	3	4	5	6
New Entrepreneurial Neighbor	0.04498**	0.04511**	0.04590**	0.04565**	0.04571**	0.04578**
	(0.02051)	(0.02050)	(0.02057)	(0.02051)	(0.02052)	(0.02051)
New Entrep. Neigh. × Homeowner		-0.10996				
		(0.08815)				
New Entrep. Neigh. × Ln Median Home Value			-0.01135			
			(0.02911)			
New Entrep. Neigh. × Ln Median HH Income				-0.01487		
				(0.03123)		
New Entrep. Neigh. × Share Below Poverty					0.08578	
					(0.24547)	
New Entrep. Neigh. × Unemployment Rate						0.35388
						(0.53385)
Fixed Effects:						
Year by Block Group	X	X	X	X	X	X
Demo. Controls	X	X	X	X	X	X
No. of Observations	4,631,184	4,631,184	4,631,184	4,631,184	4,631,184	4,631,184
Dependent Variable Mean	0.9922	0.9922	0.9922	0.9922	0.9922	0.9922
New Nbr. Entrep. Mean	0.0704	0.0704	0.0704	0.0704	0.0704	0.0704
FE Cells	65,499	65,499	65,499	65,499	65,499	65,499
R^2	0.0165	0.0163	0.0163	0.0163	0.0163	0.0163

This table mirrors the specification in Table J.62 but interacts exposure to a new entrepreneurial neighbor with tract-level economic characteristics measured in the 2000 Decennial Census, including homeownership rate, log median home value, log median household income, poverty share, and unemployment rate. In columns (2)–(6), each specification includes the centered tract-level characteristic and its interaction with treatment.

TABLE J.64. Heterogeneity by Pre-Period Tract Demographic Characteristics (Census 2000), Assessment Sample

	1	2	3	4	5	6	7
New Entrepreneurial Neighbor	0.06570***	0.06739***	0.06696***	0.06791***	0.06733***	0.06754***	0.06709***
	(0.02447)	(0.02445)	(0.02447)	(0.02447)	(0.02443)	(0.02443)	(0.02451)
New Entrep. Neigh. × Ln Population		0.01076					
		(0.04196)					
New Entrep. Neigh. × Female Share			-0.03247				
			(0.13773)				
New Entrep. Neigh. × Black Share				0.22874			
				(0.15231)			
New Entrep. Neigh. × Asian Share					-0.89296		
					(1.47599)		
New Entrep. Neigh. × Assoc. Degree Share						0.42964	
						(0.91589)	
New Entrep. Neigh. × Bach. Degree Share							0.00374
							(0.23382)
Fixed Effects:							
Year by Block Group	X	X	X	X	X	X	X
Demo. Controls	X	X	X	X	X	X	X
Housing Controls	X	X	X	X	X	X	X
No. of Observations	3,285,090	3,285,090	3,285,090	3,285,090	3,285,090	3,285,090	3,285,090
Dependent Variable Mean	1.0546	1.0546	1.0546	1.0546	1.0546	1.0546	1.0546
New Nbr. Entrep. Mean	0.0759	0.0759	0.0759	0.0759	0.0759	0.0759	0.0759
FE Cells	51,308	51,308	51,308	51,308	51,308	51,308	51,308
R^2	0.0185	0.0182	0.0182	0.0182	0.0182	0.0182	0.0182

This table replicates Table J.62 using the assessment sample, which additionally includes housing-level controls in each specification.

TABLE J.65. Heterogeneity by Economics Characteristic at Tract Level, Assessment Sample

	1	2	3	4	5	6
New Entrepreneurial Neighbor	0.06529***	0.06553***	0.06590***	0.06592***	0.06591***	0.06584***
	(0.02413)	(0.02415)	(0.02417)	(0.02415)	(0.02415)	(0.02416)
New Entrep. Neigh. × Homeowner		-0.07170				
		(0.10131)				
New Entrep. Neigh. × Ln Median Home Value			0.00120			
			(0.03322)			
New Entrep. Neigh. × Ln Median HH Income				0.00039		
				(0.03450)		
New Entrep. Neigh. × Share Below Poverty					-0.00770	
					(0.30501)	
New Entrep. Neigh. × Unemployment Rate						-0.14668
						(0.62925)
Fixed Effects:						
Year by Block Group	X	X	X	X	X	X
Demo. Controls	X	X	X	X	X	X
Housing Controls	X	X	X	X	X	X
No. of Observations	3,366,363	3,366,363	3,366,363	3,366,363	3,366,363	3,366,363
Dependent Variable Mean	1.0483	1.0483	1.0483	1.0483	1.0483	1.0483
New Nbr. Entrep. Mean	0.0757	0.0757	0.0757	0.0757	0.0757	0.0757
FE Cells	53,049	53,049	53,049	53,049	53,049	53,049
R^2	0.0186	0.0182	0.0182	0.0182	0.0182	0.0182

This table replicates Table J.63 in the assessment sample, additionally including housing-level controls in each specification

TABLE J.66. Heterogeneity by Population Density—Quintiles

	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
New Nbr Entrepreneur	0.07650	0.05398	0.00228	0.00470	0.09046*
	(0.04753)	(0.04224)	(0.04171)	(0.04869)	(0.04788)
Fixed Effects:					
Year by Block Group	X	X	X	X	X
Demo. Controls	X	X	X	X	X
No. of Observations	927,635	928,420	922,948	930,644	921,537
Dependent Variable Mean	0.90	0.96	0.99	1.04	1.06
New Nbr Entrepreneur Mean	0.0659	0.0687	0.0701	0.0709	0.0765
Density Mean [Pop/Km^2]	243	682	1,401	2,224	3,627
FE Cells	22,356	12,019	11,169	9,658	10,297
R^2	0.0254	0.0155	0.0150	0.0136	0.0141

This table estimates the main five-year specification separately for Census tracts grouped into quintiles of population density measured in the 2010 ACS. The dependent variable is an indicator for whether the incumbent resident starts a first business within five years after the new neighbor's arrival. The key regressor, New Nbr Entrepreneur, equals 1 when the nearest new neighbor has prior entrepreneurial experience; coefficients are reported in percentage points. All columns include Census block-group × year fixed effects and the same demographic controls as in the baseline results. Standard errors are clustered at the Census tract level. Quintiles are defined using the statewide 2010 distribution and are mutually exclusive and exhaustive. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE J.67. Heterogeneity by Population Density—Quintiles, Assessment Sample

	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
New Nbr Entrepreneur	0.05169	0.01616	0.02351	0.05715	0.10073
	(0.06167)	(0.05519)	(0.05758)	(0.06175)	(0.06876)
Fixed Effects:					
Year by Block Group	X	X	X	X	X
Demo. Controls	X	X	X	X	X
Housing Controls	X	X	X	X	X
No. of Observations	486,392	510,068	511,321	514,845	512,410
Dependent Variable Mean	0.98	1.03	1.04	1.09	1.10
New Nbr Entrepreneur Mean	0.0775	0.0801	0.0797	0.0814	0.0819
Density Mean [Pop/Km^2]	237	662	1,358	2,183	3,539
FE Cells	15,739	8,763	8,340	7,145	8,140
R^2	0.0332	0.0201	0.0196	0.0173	0.0191

This table estimates the main five-year specification separately for Census tracts grouped into quintiles of population density measured in the 2010 ACS. The dependent variable is an indicator for whether the incumbent resident starts a first business within five years after the new neighbor's arrival. The key regressor, New Nbr Entrepreneur, equals 1 when the nearest new neighbor has prior entrepreneurial experience; coefficients are reported in percentage points. All columns include Census block-group × year fixed effects and the same demographic controls as in the baseline results. Standard errors are clustered at the Census tract level. Quintiles are defined using the statewide 2010 distribution and are mutually exclusive and exhaustive. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE J.68. Heterogeneity by Household Income—Quintiles

	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
New Nbr Entrepreneur	0.03454	0.07108	0.04578	0.04488	0.03248
	(0.04747)	(0.04430)	(0.04246)	(0.04618)	(0.04816)
Fixed Effects:					
Year by Block Group	X	X	X	X	X
Demo. Controls	X	X	X	X	X
No. of Observations	927,947	924,610	937,067	922,249	919,311
Dependent Variable Mean	0.94	0.93	0.97	1.02	1.12
New Nbr Entrepreneur Mean	0.0756	0.0668	0.0677	0.0688	0.0732
Household Income Mean	32,068	44,692	54,310	67,375	97,846
FE Cells	23,959	15,005	11,013	8,463	7,059
R^2	0.0269	0.0185	0.0154	0.0130	0.0102

This table estimates the main five-year specification separately for Census tracts grouped into quintiles of median household income (2010 ACS). The outcome, treatment, fixed effects, controls, reporting units (percentage points), and clustering are identical to Table J.66. Quintiles use the statewide 2010 distribution. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE J.69. Heterogeneity by Household Income—Quintiles, Assessment Sample

	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
New Nbr Entrepreneur	0.06870	0.04137	0.08280	0.03333	0.02382
	(0.06372)	(0.06088)	(0.06513)	(0.05693)	(0.05890)
Fixed Effects:					
Year by Block Group	X	X	X	X	X
Demo. Controls	X	X	X	X	X
Housing Controls	X	X	X	X	X
No. of Observations	496,164	507,407	503,639	518,978	508,848
Dependent Variable Mean	0.99	1.01	1.04	1.05	1.15
New Nbr Entrepreneur Mean	0.0843	0.0792	0.0784	0.0766	0.0824
Household Income Mean	33,523	46,224	56,147	69,832	100,714
FE Cells	17,820	11,462	7,953	5,865	5,027
R^2	0.0359	0.0260	0.0198	0.0154	0.0130

This table estimates the main five-year specification separately for Census tracts grouped into quintiles of median household income (2010 ACS). The outcome, treatment, fixed effects, controls, reporting units (percentage points), and clustering are identical to Table J.67. Quintiles use the statewide 2010 distribution. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE J.70. Heterogeneity by Education (Bachelor's Degree Share)—Quintiles

	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
New Nbr Entrepreneur	0.10102**	0.01312	0.03389	0.03280	0.05025
	(0.04982)	(0.04020)	(0.04381)	(0.04592)	(0.04840)
Fixed Effects:					
Year by Block Group	X	X	X	X	X
Demo. Controls	X	X	X	X	X
No. of Observations	926,825	927,258	927,587	923,371	926,143
Dependent Variable Mean	0.88	0.98	0.98	1.00	1.13
New Nbr Entrepreneur Mean	0.0673	0.0680	0.0714	0.0708	0.0746
Bachelor's Degree Share Mean	0.09	0.17	0.24	0.31	0.41
FE Cells	26,332	14,113	9,970	7,528	7,556
R^2	0.0305	0.0176	0.0141	0.0113	0.0113

This table estimates the main five-year specification separately for Census tracts grouped into quintiles of the share of adults with a bachelor's degree or higher (2010 ACS). The outcome, treatment, fixed effects, controls, reporting units (percentage points), and clustering are identical to Table J.66. Quintiles use the statewide 2010 distribution. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE J.71. Heterogeneity by Education (Bachelor's Degree Share)—Quintiles, Assessment Sample

	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
New Nbr Entrepreneur	0.08403	-0.00653	0.07863	0.06494	0.03073
	(0.06497)	(0.05937)	(0.05504)	(0.05868)	(0.06637)
Fixed Effects:					
Year by Block Group	X	X	X	X	X
Demo. Controls	X	X	X	X	X
Housing Controls	X	X	X	X	X
No. of Observations	486,657	504,204	512,450	520,386	511,339
Dependent Variable Mean	0.96	1.03	1.05	1.06	1.14
New Nbr Entrepreneur Mean	0.0800	0.0810	0.0790	0.0795	0.0812
Bachelor's Degree Share Mean	0.09	0.17	0.25	0.32	0.42
FE Cells	19,543	10,639	6,916	5,728	5,301
R^2	0.0407	0.0235	0.0181	0.0148	0.0138

This table estimates the main five-year specification separately for Census tracts grouped into quintiles of the share of adults with a bachelor's degree or higher (2010 ACS). The outcome, treatment, fixed effects, controls, reporting units (percentage points), and clustering are identical to Table J.67. Quintiles use the statewide 2010 distribution. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE J.72. Heterogeneity by Median Home Value—Quintiles

	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
New Nbr Entrepreneur	0.08194*	0.07026	0.03751	0.03754	0.00887
	(0.04782)	(0.04749)	(0.04522)	(0.04725)	(0.04187)
Fixed Effects:					
Year by Block Group	X	X	X	X	X
Demo. Controls	X	X	X	X	X
No. of Observations	936,398	924,246	919,334	925,135	926,010
Dependent Variable Mean	0.94	0.99	0.99	0.98	1.06
New Nbr Entrepreneur Mean	0.0687	0.0678	0.0661	0.0676	0.0818
Median Home Value Mean	105,289	145,162	177,014	226,428	371,059
FE Cells	26,059	13,719	9,048	8,236	8,434
R^2	0.0288	0.0180	0.0128	0.0115	0.0122

This table estimates the main five-year specification separately for Census tracts grouped into quintiles of median owner-occupied home value (2010 ACS). The outcome, treatment, fixed effects, controls, reporting units (percentage points), and clustering are identical to Table J.66. Quintiles use the statewide 2010 distribution. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE J.73. Heterogeneity by Median Home Value—Quintiles, Assessment Sample

	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
New Nbr Entrepreneur	0.07209	0.04471	0.02899	0.09811	0.01204
	(0.06325)	(0.06168)	(0.06152)	(0.06033)	(0.05864)
Fixed Effects:					
Year by Block Group	X	X	X	X	X
Demo. Controls	X	X	X	X	X
Housing Controls	X	X	X	X	X
No. of Observations	504,886	499,613	503,238	513,175	514,121
Dependent Variable Mean	1.01	1.09	1.02	1.02	1.10
New Nbr Entrepreneur Mean	0.0821	0.0790	0.0751	0.0759	0.0886
Median Home Value Mean	109,625	150,306	182,160	231,885	369,553
FE Cells	19,717	9,887	6,315	5,902	6,305
R^2	0.0393	0.0227	0.0164	0.0148	0.0153

This table estimates the main five-year specification separately for Census tracts grouped into quintiles of median owner-occupied home value (2010 ACS). The outcome, treatment, fixed effects, controls, reporting units (percentage points), and clustering are identical to Table J.67. Quintiles use the statewide 2010 distribution. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE J.74. Spillovers and Arriver Experience (Centered), Main Sample

	1	2	3
New Nbr Entrepreneur	0.04352**	0.09114***	0.04350**
	(0.02030)	(0.03183)	(0.02030)
Experience		-0.00687*	
		(0.00373)	
New Nbr Entrepreneur x Experience			0.00286
			(0.00401)
Fixed Effects:			
Year by Block Group	X	X	X
Demo. Controls	X	X	X
No. of Observations	4,672,279	4,672,279	4,672,279
Dependent Variable Mean	0.99	0.99	0.99
New Nbr Entrepreneur Mean	0.0711	0.0711	0.0711
FE Cells	67,064	67,064	67,064
R^2	0.0167	0.0167	0.0167

This table examines whether spillover effects vary with the arriving entrepreneur's prior entrepreneurial experience. Experience is measured as the number of years between the entrepreneur's first business registration and the arrival date, and is centered at its sample mean. Column (1) reports the baseline specification. Column (2) includes experience linearly. Column (3) interacts the treatment indicator with centered experience. The coefficient on New Nbr Entrepreneur in column (3) represents the spillover effect evaluated at mean experience, while the interaction term captures the marginal change in the spillover effect per additional year of prior experience. All specifications include year-by–block group fixed effects and individual demographic controls. Standard errors are clustered at the Census tract level.

TABLE J.75. Spillovers and Arriver Experience (Centered), Assessment Sample

	1	2	3
New Nbr Entrepreneur	0.06592***	0.11084***	0.06579***
	(0.02445)	(0.03949)	(0.02444)
Experience		-0.00593	
		(0.00421)	
New Nbr Entrepreneur x Experience			0.00568
			(0.00457)
Fixed Effects:			
Year by Block Group	X	X	X
Demo. Controls	X	X	X
Housing Controls	X	X	X
No. of Observations	3,296,000	3,296,000	3,296,000
Dependent Variable Mean	1.05	1.05	1.05
New Nbr Entrepreneur Mean	0.0759	0.0759	0.0759
FE Cells	52,558	52,558	52,558
R^2	0.0189	0.0189	0.0189

This table replicates Table J.74 using the assessment sample and additionally includes housing characteristics as controls. All other variable definitions and specifications follow Table J.74.

TABLE J.76. Spillovers by Active Status of the Arriving Entrepreneur (Main Sample)

	1	2	3	4
New Nbr Entrepreneur	0.04352**			
	(0.02030)			
New Nbr Non-Active Entrep.		0.05520		0.05797
		(0.04905)		(0.04905)
New Nbr Active Entrep.			0.03977*	0.04062*
			(0.02206)	(0.02207)
Fixed Effects:				
Year by Block Group	X	X	X	X
Demo. Controls	X	X	X	X
$H_0 : \beta_{Non-Active} = \beta_{Active}$				
F-stat [p-value]				0.11 [0.74]
No. of Observations	4,672,279	4,672,279	4,672,279	4,672,279
Dependent Variable Mean	0.9917	0.9917	0.9917	0.9917
New Nbr. Entrep. Mean	0.0711			
New Nbr. Non-Active Entrep. Mean		0.0118		0.0118
New Nbr. Active Entrep. Mean			0.0593	0.0593
FE Cells	67,064	67,064	67,064	67,064
R^2	0.0167	0.0167	0.0167	0.0167

This table examines whether spillover effects vary with the arriving entrepreneur's operating status at the time of arrival. An entrepreneur is classified as active if at least one prior venture remains active at the arrival date (or dissolves thereafter), and non-active if all prior ventures dissolved on or before the arrival date. The reported coefficients allow the spillover effect to differ across these two groups. All specifications include year-by–block group fixed effects and demographic controls. Standard errors are clustered at the Census tract level.

TABLE J.77. Spillovers by Active Status of the Arriving Entrepreneur (Assessment Sample)

	1	2	3	4
New Nbr Entrepreneur	0.06592***			
	(0.02445)			
New Nbr Non-Active Entrep.		0.11810**		0.12174**
		(0.05518)		(0.05523)
New Nbr Active Entrep.			0.05168*	0.05360**
			(0.02667)	(0.02669)
Fixed Effects:				
Year by Block Group	X	X	X	X
$H_0 : \beta_{Non-Active} = \beta_{Active}$				
F-stat [p-value]				1.28 [0.26]
Demo. Controls	X	X	X	X
Housing Controls	X	X	X	X
No. of Observations	3,296,000	3,296,000	3,296,000	3,296,000
Dependent Variable Mean	1.0549	1.0549	1.0549	1.0549
New Nbr. Entrep. Mean	0.0759			
New Nbr. Non-Active Entrep. Mean		0.0136		0.0136
New Nbr. Active Entrep. Mean			0.0623	0.0623
FE Cells	52,558	52,558	52,558	52,558
R^2	0.0189	0.0189	0.0189	0.0189

This table replicates Table J.76 using the assessment sample and additionally includes housing characteristics as controls. All other definitions and specifications follow Table J.76.

TABLE J.78. Exposure to Entrepreneurial Neighbors and Subsequent Business Survival (Conditional on Entry), Main Sample

	1	2	3	4
New Nbr Entrepreneur	-0.07679*			
	(0.04665)			
New Nbr Low Quality Entrep.		-0.06215		-0.06543
		(0.05946)		(0.05933)
New Nbr High Quality Entrep.			-0.09043	-0.09413
			(0.07761)	(0.07748)
Fixed Effects:				
Year by Block Group	X	X	X	X
Demo. Controls	X	X	X	X
$H_0 : \beta_{low} = \beta_{High}$				
F-stat [p-value]				0.08 [0.77]
No. of Observations	77,801	77,801	77,801	77,801
Dependent Variable Mean	5.8062	5.8062	5.8062	5.8062
New Nbr. Entrep. Mean	0.0790			
New Nbr. Low Quality Entrep. Mean		0.0492		0.0492
New Nbr. High Quality Entrep. Mean			0.0298	0.0298
FE Cells	19,083	19,083	19,083	19,083
R^2	0.6230	0.6230	0.6230	0.6230

This table examines whether exposure to entrepreneurial neighbors affects the survival of businesses started by incumbent residents. The sample is restricted to individuals who start a business during the observation window. The dependent variable is the number of years the new business remains active. Column (1) estimates the effect of exposure to any entrepreneurial neighbor. Columns (2) and (3) separately estimate exposure to low- and high-quality entrepreneurs. Column (4) includes both simultaneously and reports an F-test of equality of coefficients. All specifications include year-by–block group fixed effects and demographic controls. Standard errors are clustered at the Census tract level.

TABLE J.79. Exposure to Entrepreneurial Neighbors and Subsequent Business Survival (Conditional on Entry), Assessment Sample

	1	2	3	4
New Nbr Entrepreneur	-0.07011			
	(0.04549)			
New Nbr Low Quality Entrep.		-0.05215		-0.05477
		(0.05601)		(0.05601)
New Nbr High Quality Entrep.			-0.07077	-0.07402
			(0.07601)	(0.07601)
Fixed Effects:				
Year by Block Group	X	X	X	X
Demo. Controls	X	X	X	X
Housing Controls	X	X	X	X
$H_0 : \beta_{low} = \beta_{High}$				
F-stat [p-value]				0.04 [0.84]
No. of Observations	58,639	58,639	58,639	58,639
Dependent Variable Mean	4.6686	4.6686	4.6686	4.6686
New Nbr. Entrep. Mean	0.0821			
New Nbr. Low Quality Entrep. Mean		0.0517		0.0517
New Nbr. High Quality Entrep. Mean			0.0302	0.0302
FE Cells	14,344	14,344	14,344	14,344
R^2	0.5266	0.5266	0.5266	0.5266

This table replicates Table J.78 using the assessment sample and additionally includes housing characteristics as controls. All other definitions and specifications follow Table J.78.

TABLE J.80. Exposure to Entrepreneurial Neighbors by Gender and Race, Main Sample

	Sex		Race	
	Men	Women	White	Black
	(1)	(2)	(3)	(4)
Mean share	0.074	0.076	0.074	0.080
Gap vs majority	.	0.001	.	0.006
No. of Observations	1,548,182	1,807,003	2,709,949	481,850

This table reports mean exposure rates to new neighbors with prior entrepreneurial experience in the main sample, separately by incumbent gender and race. Exposure is defined as the share of an incumbent's new neighbors in a given year who have prior entrepreneurial activity. Gender gaps are computed as the difference in mean exposure between female and male incumbents, and racial gaps as the difference between Black and White incumbents.

TABLE J.81. Exposure to Entrepreneurial Neighbors by Gender and Race, Assessment Sample

	Sex		Race	
	Men	Women	White	Black
	(1)	(2)	(3)	(4)
Mean share	0.079	0.081	0.079	0.085
Gap vs majority	.	0.002	.	0.006
No. of Observations	1,129,174	1,305,589	1,968,712	337,967

This table replicates the exposure analysis in Table J.80 using the assessment sample, which is restricted to incumbents with complete property characteristics. Exposure is defined as the share of an incumbent's new neighbors in a given year who have prior entrepreneurial activity. Gender gaps are computed as the difference in mean exposure between female and male incumbents, and racial gaps as the difference between Black and White incumbents.

TABLE J.82. Gender Heterogeneity in Entrepreneurial Spillovers, Main Sample

	1	2	3
New Nbr Entrep	0.02452	0.50080***	0.52844***
	(0.02444)	(0.03878)	(0.04155)
New Nbr Entrep x $\mathbb{1}\{Women\}$		-0.88868***	-0.91854***
		(0.04316)	(0.04604)
New Nbr Entrep x $\mathbb{1}\{Women\ Arrived\}$			-0.17649*
			(0.09749)
New Nbr Entrep x $\mathbb{1}\{Women\}$ x $\mathbb{1}\{Women\ Arrived\}$			0.18990*
			(0.11113)
Fixed Effects:			
Year by Block Group	X	X	X
Demo. Controls	X	X	X
No. of Observations	3,390,539	3,390,539	3,390,539
Dependent Variable Mean (Men)	1.45	1.45	1.45
Dependent Variable Mean (Women)	0.61	0.61	0.61
New Nbr Entrepreneur Mean	0.0753	0.0753	0.0753
FE Cells	65,874	65,874	65,874
R^2	0.0203	0.0204	0.0204

This table estimates whether entrepreneurial spillovers differ by gender using the main sample. The dependent variable is an indicator equal to 100 if the incumbent starts a business within five years of a new neighbor's arrival. Column (1) interacts exposure to entrepreneurial neighbors with a female incumbent indicator. Column (2) further distinguishes whether the new entrepreneurial neighbor is a woman. All regressions include demographic controls (age, race, and political affiliation of incumbents) and year-by–Census block group fixed effects. Standard errors, clustered at the tract level, are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. The mean (standard deviation) of the share of new neighbors with recent entrepreneurial experience is 0.075 (0.264), and the mean (standard deviation) of the share of new entrepreneurial neighbors who are women is 0.040 (0.197). The mean outcome is 1.

TABLE J.83. Effect of Exposure by Incumbent–Arriver Gender Pair

	Arriver: Man	**Arriver: Woman**
Incumbent: Man	0.528^{***}	0.352^{***}
	(0.042)	(0.091)
Incumbent: Woman	-0.390^{***}	-0.377^{***}
	(0.028)	(0.062)

Entries report the estimated marginal effect of exposure to entrepreneurial arrivers on the probability that an incumbent starts a business (percentage points, 0–100 scale), separately by incumbent–arriver gender pairing. Estimates are computed as linear combinations of the coefficients in column (2) of Table J.82. Standard errors, clustered at the tract level, are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE J.84. Gender Heterogeneity in Entrepreneurial Spillovers, Assessment Sample

	1	2	3
New Nbr Entrep	0.04146	0.50781***	0.53966***
	(0.02862)	(0.04533)	(0.04860)
New Nbr Entrep x $\mathbb{1}\{Women\}$		-0.87344***	-0.91271***
		(0.05042)	(0.05385)
New Nbr Entrep x $\mathbb{1}\{Women\ Arrived\}$			-0.19741*
			(0.11400)
New Nbr Entrep x $\mathbb{1}\{Women\}$ x $\mathbb{1}\{Women\ Arrived\}$			0.24202*
			(0.13272)
Fixed Effects:			
Year by Block Group	X	X	X
Demo. Controls	X	X	X
Housing Controls	X	X	X
No. of Observations	2,408,581	2,408,581	2,408,581
Dependent Variable Mean (Men)	1.49	1.49	1.49
Dependent Variable Mean (Women)	0.66	0.66	0.66
New Nbr Entrepreneur Mean	0.0805	0.0805	0.0805
FE Cells	47,991	47,991	47,991
R^2	0.0209	0.0210	0.0210

This table replicates the analysis in Table J.82 using the restricted assessment sample. All regressions include demographic controls (age, race, and political affiliation of incumbents), housing controls (year built, lot size, and assessed value) and year-by–Census block group fixed effects. Standard errors, clustered at the tract level, are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. The mean (standard deviation) of the share of new neighbors with recent entrepreneurial experience is 0.080 (0.272), and the mean (standard deviation) of the share of new entrepreneurial neighbors who are women is 0.043 (0.203). The mean outcome is 1.06.

TABLE J.85. Gender Heterogeneity in Entrepreneurial Spillovers, Main Sample-Non Couples

	1	2	3
New Nbr Entrep	0.05070	0.52949***	0.61037***
	(0.03728)	(0.05749)	(0.06677)
New Nbr Entrep x $\mathbb{1}\{Women\}$		-0.88592***	-0.96436***
		(0.06171)	(0.07174)
New Nbr Entrep x $\mathbb{1}\{Women\ Arrived\}$			-0.30644***
			(0.11648)
New Nbr Entrep x $\mathbb{1}\{Women\}$ x $\mathbb{1}\{Women\ Arrived\}$			0.29877**
			(0.12933)
Fixed Effects:			
Year by Block Group	X	X	X
Demo. Controls	X	X	X
No. of Observations	1,849,505	1,849,505	1,849,505
Dependent Variable Mean (Men)	1.46	1.46	1.46
Dependent Variable Mean (Women)	0.64	0.64	0.64
New Nbr Entrepreneur Mean	0.0706	0.0706	0.0706
FE Cells	57,165	57,165	57,165
R^2	0.0309	0.0310	0.0310

This table replicates the analysis in Table J.82 using the main sample, restricting the sample to properties purchased by single (non-couple) arrivals. All regressions include demographic controls (age, race, and political affiliation of incumbents) and year-by–Census block group fixed effects. Standard errors, clustered at the tract level, are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. The mean (standard deviation) of the share of new neighbors with recent entrepreneurial experience is 0.071 (0.256), and the mean (standard deviation) of the share of new entrepreneurial neighbors who are women is 0.038 (0.191). The mean outcome is 1.02.

TABLE J.86. Race Heterogeneity in Entrepreneurial Spillovers, Main Sample

	1	2	3
New Nbr Entrep	0.02786	-0.05190**	-0.04171
	(0.02442)	(0.02511)	(0.02654)
New Nbr Entrep x $\mathbb{1}\{Black\}$		0.55148***	0.57047***
		(0.07456)	(0.08366)
New Nbr Entrep x $\mathbb{1}\{Black\ Arrived\}$			-0.08567
			(0.07173)
New Nbr Entrep x $\mathbb{1}\{Black\}$ x $\mathbb{1}\{Black\ Arrived\}$			-0.02895
			(0.17254)
Fixed Effects:			
Year by Block Group	X	X	X
Demo. Controls	X	X	X
No. of Observations	3,390,539	3,390,539	3,390,539
Dependent Variable Mean (White)	0.84	0.84	0.84
Dependent Variable Mean (Black)	1.56	1.56	1.56
New Nbr Entrepreneur Mean	0.0753	0.0753	0.0753
FE Cells	65,874	65,874	65,874
R^2	0.0209	0.0209	0.0209

This table estimates whether entrepreneurial spillovers differ by race using the main sample. The dependent variable is an indicator equal to 100 if the incumbent starts a business within five years of a new neighbor's arrival. Column (1) interacts exposure to entrepreneurial neighbors with a Black incumbent indicator. Column (2) further distinguishes whether the new entrepreneurial neighbor is Black. All regressions include demographic controls (age, gender, and political affiliation of incumbents) and year-by–Census block group fixed effects. Standard errors, clustered at the tract level, are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. The mean (standard deviation) of the share of new neighbors with recent entrepreneurial experience is 0.075 (0.264), and the mean (standard deviation) of the share of new entrepreneurial neighbors who are Black is 0.011 (0.102). The mean outcome is 1.

TABLE J.87. Effect of Exposure by Incumbent–Arriver Race Pair

	Arriver: White	**Arriver: Black**
Incumbent: White	−0.042	−0.127*
	(0.027)	(0.068)
Incumbent: Black	0.529***	0.414***
	(0.081)	(0.139)

Entries report the estimated marginal effect of exposure to entrepreneurial arrivers on the probability that an incumbent starts a business (percentage points, 0–100 scale), separately by incumbent–arriver race pairing. Estimates are computed as linear combinations of the coefficients in column (2) of Table J.86. Standard errors, clustered at the tract level, are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE J.88. Race Heterogeneity in Entrepreneurial Spillovers, Assessment Sample

	1	2	3
New Nbr Entrep	0.04777*	-0.02616	-0.01526
	(0.02862)	(0.02948)	(0.03098)
New Nbr Entrep x $\mathbb{1}\{Black\}$		0.53954***	0.53654***
		(0.09344)	(0.10227)
New Nbr Entrep x $\mathbb{1}\{Black\ Arrived\}$			-0.09189
			(0.08295)
New Nbr Entrep x $\mathbb{1}\{Black\}$ x $\mathbb{1}\{Black\ Arrived\}$			0.05768
			(0.21542)
Fixed Effects:			
Year by Block Group	X	X	X
Demo. Controls	X	X	X
Housing Controls	X	X	X
No. of Observations	2,408,581	2,408,581	2,408,581
Dependent Variable Mean (White)	0.88	0.88	0.88
Dependent Variable Mean (Black)	1.66	1.66	1.66
New Nbr Entrepreneur Mean	0.0805	0.0805	0.0805
FE Cells	47,991	47,991	47,991
R^2	0.0219	0.0219	0.0219

This table replicates the analysis in Table J.86 using the restricted assessment sample. All regressions include demographic controls (age, race, and political affiliation of incumbents), housing controls (year built, lot size, and assessed value) and year-by–Census block group fixed effects. Standard errors, clustered at the tract level, are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. The mean (standard deviation) of the share of new neighbors with recent entrepreneurial experience is 0.080 (0.272), and the mean (standard deviation) of the share of new entrepreneurial neighbors who are Black is 0.011 (0.106). The mean outcome is 1.06.

TABLE J.89. Race Heterogeneity in Entrepreneurial Spillovers, Main Sample-Non Couples

	1	2	3
New Nbr Entrep	0.05407	-0.02760	-0.00692
	(0.03729)	(0.03779)	(0.04107)
New Nbr Entrep x $\mathbb{1}\{Black\}$		0.46145***	0.51824***
		(0.09817)	(0.11815)
New Nbr Entrep x $\mathbb{1}\{Black\ Arrived\}$			-0.15333
			(0.10132)
New Nbr Entrep x $\mathbb{1}\{Black\}$ x $\mathbb{1}\{Black\ Arrived\}$			-0.12941
			(0.21852)
Fixed Effects:			
Year by Block Group	X	X	X
Demo. Controls	X	X	X
No. of Observations	1,849,505	1,849,505	1,849,505
Dependent Variable Mean (White)	0.83	0.83	0.83
Dependent Variable Mean (Black)	1.55	1.55	1.55
New Nbr Entrepreneur Mean	0.0706	0.0706	0.0706
FE Cells	57,165	57,165	57,165
R^2	0.0313	0.0314	0.0314

This table replicates the analysis in Table J.86 using the main sample, restricting the sample to properties purchased by single (non-couple) arrivals. All regressions include demographic controls (age, race, and political affiliation of incumbents) and year-by–Census block group fixed effects. Standard errors, clustered at the tract level, are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. The mean (standard deviation) of the share of new neighbors with recent entrepreneurial experience is 0.071 (0.256), and the mean (standard deviation) of the share of new entrepreneurial neighbors who are Black is 0.011 (0.105). The mean outcome is 1.02.