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More than Just Neighbors: Immigrant Networks and Jobs in High-Paying Firms

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Abstract

We study whether immigrants' residential networks help provide them access to high-paying firms. Using Canadian longitudinal employer–employee tax data linked to immigration records, we measure employer quality with firm-specific earnings premiums and exploit variation across neighborhoods within larger locales. Neighborhood effects for immigrants operate almost entirely through immigrant-peers, not native-peers. And they are roughly twice as large when immigrant-peers share their country of birth or mother tongue. Effects arise at employer changes rather than through retention, consistent with referral and job-information channels. They nearly triple when co-resident family members are counted as peers, indicating that designs pooling household and neighborhood ties largely capture the former.

JEL classification

J15, J31, J61, R23, D8

Keywords

immigrants, neighborhood peer effects, social networks, referrals, firm pay premiums, linked employer–employee data

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1. INTRODUCTION

Shortfalls in immigrant employment at high-paying firms account for a large share of the immigrant–native pay gap (Barth et al., 2012; Dostie et al., 2023; Hermansen et al., 2025), and access to good employers is a key driver of immigrant economic assimilation (Arellano-Bover and San, 2026; Caiumi and Simonsen, 2026). How immigrants gain entry into high-paying firms is therefore a first-order question for their economic success. This paper asks whether residential networks provide such entry. Are immigrants more likely to work at high-paying firms when their residential neighbors do? If so, does peer influence depend on who those neighbors are: fellow-immigrants or natives and, among immigrants, neighbors who share the focal immigrant's country of birth or mother tongue? Do peer effects operate at the point of employer changes, or do they help immigrants remain at high-paying firms once matched? We address these three questions for a recent cohort of immigrants to Canada's three largest metropolitan areas, using linked employer-employee and immigration records covering the universe of Canadian tax filers.

Major barriers to employment in high-paying firms are search and information frictions, which are especially acute for immigrants who often face language barriers, have limited local knowledge, and possess few professional contacts (Borjas, 1995; Bertrand et al., 2000). Social networks can mitigate such barriers by transmitting vacancy information, facilitating referrals, and improving job-worker match quality (Granovetter, 1973; Montgomery, 1991; Topa and Zenou, 2015; Beaman, 2016). Indeed, in the population at large, social connections are an important determinant of how workers sort across high- and low-wage firms (Eliason et al., 2023). Whether they do the same for immigrants, and which connections are material, is less well understood.

Residential neighborhoods are pertinent in this context, because they provide a locus of social networks for immigrants whose labor market ties may be tenuous. Neighbors can share job leads, explain hiring practices, provide referrals, and offer job advice. Empirically, residential proximity predicts employment and employer allocation (Hellerstein et al., 2011, 2014; Schmutte, 2015), and referral-based search is an oft-emphasized channel through which peers transmit employer information and reduce screening frictions (Patacchini and Zenou, 2012; Dustmann et al., 2016; Kerr and Kerr, 2021). People also tend to be influenced by peers who resemble them (McPherson et al., 2001), and cultural proximity can make employer signals more scrutable and credible (e.g., Dustmann, 1996; Carillo et al., 2023). Our focus in this paper is thus not just *whether* neighborhood peers matter for immigrants' employer quality, but *who* those neighbors need to be.

We find that neighborhood peer effects on immigrants' employer quality are strongly identity-based.[1] Immigrants whose peers work for firms that pay higher premiums tend to work at higher pay-premium firms themselves, but these effects are driven by immigrant-peers; native-peers exert no effect. Among immigrant-peers, cultural commonality matters: effects are about twice as large when peers share (vs. do not share) the focal immigrant's country of birth or mother tongue. These positive peer effects arise at job switches rather than through job retention, pointing to a job-mobility margin rather than job-stability channel. These results are consistent with identity-based job information and referral channels rather than with undifferentiated neighborhood spillovers.

[1]Throughout, we use "identity" to refer to shared, observable markers of background–immigrant status, country of birth, mother tongue–that are known to govern homophily in social ties (McPherson et al., 2001), rather than in the belief- and norm-based sense of Akerlof and Kranton (2000).

Because we observe family ties directly, we can separate household members from neighborhood networks, which is something designs that pool all co-residents cannot do. When we count co-resident family members as peers, our overall estimate nearly triples and the immigrant-peer coefficient roughly quadruples, closing most of the gap with prior benchmarks. This suggests that a large share of what pooled designs attribute to "the neighborhood" may reflect the household, a strong-tie margin in the spirit of Kramarz and Skans (2014). Our baseline isolates the smaller, but distinct, extra-household component. This is arguably the margin most relevant for policy, since dispersal and placement policies typically shape who immigrants live near, not whom they live with.

We use the Canadian Employer–Employee Dynamics Database (CEEDD). CEEDD is a longitudinal linked employer–employee database covering the universe of Canadian tax filers, matched at the individual level to the Longitudinal Immigration Database (IMDB). Our data report annual earnings, employers, two nested residential identifiers, and demographics (age, sex, education) alongside detailed immigration records (arrival cohort, country of birth, mother tongue). Our analysis focuses on a recent cohort of immigrants who lived in Toronto, Montréal, or Vancouver between 2001 and 2017, having arrived there within the five years prior. These three metropolitan areas are Canada's largest and most diverse, containing immigrant populations at the neighborhood level that are large enough to support disaggregated analysis.

Employer quality is measured by firm-specific earnings premiums from a two-way fixed-effects model of log earnings following Abowd et al. (1999) (AKM), estimated on the full CEEDD universe of immigrant and native workers. Peer networks comprise neighbors in the same postal code, excluding co-resident family members. They are distinguished by immigrant versus native status and, when relevant, by shared country of birth or mother tongue. This allows us to examine whether immigrants benefit from better-placed peers, and whether it matters if these peers are immigrants or natives. Among immigrant-peers, we can further distinguish co-nationals and co-linguists from other immigrants.

Our goal is to identify the effect of neighborhood peers' employer quality on immigrants' employer quality. Since immigrants are not randomly assigned to neighborhoods, two broad threats to identification are relevant. The first is correlated effects, arising from neighborhood sorting or common local shocks. We address this by exploiting plausibly exogenous assignment to neighborhoods within larger residential locales, taking advantage of the Canadian postal structure whereby smaller six-character postal codes ("neighborhoods") are nested within three-character codes ("locales"), designed for mail routing rather than socioeconomic demarcation. This approach builds on Bayer et al. (2008) and Schmutte (2015). In our baseline, we condition on locale fixed effects, time fixed effects, and a rich set of covariates. To support this identification strategy we show that, conditional on locale, there is little evidence of sorting on observables; we do not observe higher rates of cross-neighborhood relocation around job switches; and movers do not appear to sort into neighborhoods within a locale on the basis of future neighbors' job characteristics.

A complementary source of identification comes from the structure of our comparisons. Confounds operating at the neighborhood level (including common shocks, or sorting on overall neighborhood quality) raise the measured quality of immigrant- and native-peers together, pushing the coefficients on both peer groups in the same direction. They cannot readily generate effects that differ with the match between an individual's identity and that of their peers. To do so, confounds would have to be group-specific, and we address these directly with controls for peer-network composition and with

country-of-birth×locale, country-of-birth×year, and locale×year fixed effects, all of which leave the estimates essentially unchanged.

The second threat to identification is the reflection problem: with contemporaneous peer outcomes, the effect of peers' outcomes cannot be separated from the effect of peers' characteristics, since they are determined together (Manski, 1993). This concern is mitigated here because we use lagged peer employer quality, and because firm-specific earnings premiums are estimated from complete work histories and are therefore predetermined relative to the focal immigrant's current outcome (e.g., Cornelissen et al., 2017).

In the fully saturated specification, which conditions on locale fixed effects, individual covariates, peer composition, and prior employer quality, higher-quality neighborhood peers are associated with higher employer quality for immigrants. Raising peers' average firm premium by one standard deviation (i.e., 0.34 log points), raises immigrants' own earnings by 2.3 percent. Positive peer effects are driven by immigrant-peers: when we split neighborhood networks into immigrants and natives, we find a positive immigrant-peer coefficient, and a near-zero native-peer coefficient. A back-of-envelope calculation suggests that equalizing peer network quality across groups would close under a tenth of the immigrant-native gap in firm premiums. The implication would be that immigrant networks are a real, but modest, path into high-paying firms.

Peer effects arise at employer changes; they do not increase the probability of staying in a job. Interpreted through a simple model of job leads, the estimate for job switchers implies that about 4% of immigrants' employer changes come through immigrant neighbors. Splitting immigrant-peers by country of birth and mother tongue we find that both same- and different-identity immigrant-peers matter, but coefficients are roughly twice as large for co-national and co-linguist neighbors. Native-peer effects on immigrants remain close to zero. Using natives as the focal group yields the mirror pattern: natives respond primarily to native-peers, with smaller effects from immigrant neighbors.

Heterogeneity analysis suggests that focal immigrants whose formal labor market ties are likely weaker tend to enjoy larger immigrant-peer effects, while those who seem better equipped for labor market integration ex-ante benefit from some native-peer effects. Specifically, immigrant-peer effects are larger for immigrants who are less educated, and among family and refugee admissions. By contrast, native-peer effects are more evident for women, for older as well as more highly educated immigrants, and those from economic-class admissions. While shared ethnic identity markers seem to matter, similarity along non-ethnic dimensions is mixed. For men, only male immigrant neighbors yield positive coefficients whereas for women, immigrant-peers of both genders do. And immigrant-peers who–like our focal immigrants–are also relatively recent arrivals exert a similar peer effect to those who have been in Canada for longer.

Our baseline results are robust to a range of checks. Estimates are stable when we impose more demanding fixed effects and when we drop very small-population neighborhoods. The immigrant-peer effect is, if anything, larger when we tighten minimum peer-count thresholds or restrict our sample to immigrants who never changed neighborhoods, although the immigrant-native difference becomes imprecise in these smaller samples. Results are also robust to alternative peer-network lag structures, and the same-identity result persists when we define shared identity more coarsely, by world area of birth.

This paper builds on three strands of literature: residential peer effects in labor markets, social networks and job search, and co-ethnic networks and immigrant labor market integration. Its contribution rests on both the data and context we are able to exploit. Specifically, longitudinal linked employer–employee administrative data covering the universe of tax filers in Canada over nearly 20 years and linked to immigration records permit the use of rich worker and firm fixed-effects models, precise measurement of worker–firm trajectories, and a fine-grained characterization of broader immigrant (rather than narrower refugee) populations based on detailed information on country of origin, language, and related characteristics.

First, this paper contributes to the literature on residential peer effects in labor markets by shifting attention from whether neighbors matter to which neighbors matter for immigrants' access to high-paying firms. Existing work shows that geographically proximate contacts shape employment and employer allocation (Topa, 2001; Topa and Zenou, 2015; Bayer et al., 2008; Schmutte, 2015).[2] Han et al. (2026) show for Norway that immigrants are more likely than natives to work in the same establishments as their immigrant neighbors, particularly co-nationals, and that these tend to be low-wage establishments. Their analysis is descriptive and concerns whether neighbors share a workplace; we ask whether the quality of neighbors' employers carries over to an immigrant's own. Closest to our empirical setup is Schmutte (2015), who shows that exposure to neighbors employed at higher-quality firms predicts subsequent employment at higher-paying employers. Our paper asks a distinct question of whether, for immigrants, this neighborhood transmission mechanism operates through immigrant status and, among immigrants, via shared cultural identity. Our contribution is to trace peer effects along identity lines, rather than infer them from group composition. We can do so because we observe, in population-wide data and within small residential neighborhoods, who each neighbor is–not just immigrant or native, but also co-national or co-linguist–jointly with the quality of their employer. We also separate co-resident family members, a strong-tie channel (Kramarz and Skans, 2014), from other neighbors, which is something designs that pool the two cannot.

Second, a foundational idea is that social ties transmit job information and facilitate matches (Granovetter, 1973; Montgomery, 1991). Empirical work documents referral and coworker effects on hiring, wages, and employer switching in linked administrative and experimental settings (e.g., Cornelissen et al., 2017; Glitz and Vejlin, 2021; Hensvik and Skans, 2016; Brouard et al., 2024; Hansch et al., 2025; Pallais and Sands, 2016; Beaman, 2012). We add to this literature by emphasizing the residential margin of network exposure rather than firm- or task-based links. Closest in spirit, Eliason et al. (2023) show that social connections, including parents, siblings, former coworkers, and neighbors, shape the sorting of workers to high-wage firms in Sweden. We complement their population-wide analysis by centering on immigrants, for whom formal channels are weakest, and by showing that the residential transmission of firm-premium access is organized along lines of shared identity. Our findings also complement evidence on co-ethnic hiring in new ventures from Kerr and Kerr (2021), who argue that patterns are consistent with within-group informational advantages, along with some role for within-group preferences. Employer-side homophily, whereby immigrant managers hire immigrants (Åslund et al., 2014), may further amplify the returns to same-identity referrals. We show that similar homophilous patterns arise at the neighborhood level, where immigrant peers appear to shape access to higher-premium employers. Although we do not attempt to distinguish among specific micro-mechanisms, our finding that peer effects are stronger within both

[2]More recent contributions along those lines include Tan (2022), who studies the propensity to work in the same industry as peers in the same area.

shared-nationality and shared-language networks is consistent with homophily in the transmission of job-relevant information.

Third, our paper contributes to the literature on co-ethnic networks and immigrant labor market integration. Much of this work studies how ethnic enclaves, co-ethnic density, neighborhood conditions, or refugee placement affect employment rates or earnings levels (e.g., Edin et al., 2003; Beaman, 2012; Damm, 2009, 2014; Martén et al., 2019; Stips and Kis-Katos, 2020). Closest to our paper is Caiumi and Simonsen (2026), who exploit the quasi-random placement of refugees under Denmark's dispersal policy to show that settling where co-nationals work at high-premium firms raises refugees' employment and earnings, in part through access to better employers. Their design cleanly establishes that the employers reachable through a network matter; we ask which neighbors provide that reach. We do so within a single building or block rather than a municipality, for immigrants of all admission classes rather than refugees alone, and with native and other immigrant neighbors as comparisons.

The paper proceeds as follows. Section 2 outlines our empirical framework and identification strategy. Section 3 describes the data, while Section 4 provides evidence in support of the identification strategy. Section 5 presents our main results, including whether peer effects arise at job switches. Section 6 extends the analysis to natives and family members, and explores heterogeneity. Section 7 reports robustness checks. Finally, Section 8 concludes.

2. Empirical Strategy

This section presents the empirical models used to estimate firm-specific earnings premiums and neighborhood peer effects. It outlines the key challenges to identification of peer effects, and describes how we address them.

2.1. **Firm-specific earnings premium.**

The outcome variable in our peer effects analysis is the firm-specific earnings premium. We begin by estimating these using a two-way fixed effects (AKM) model, following Dostie et al. (2023):

$$\ln y_{ijt} = \theta_i + \gamma' \boldsymbol{x}_{it} + \psi_{j(i,t)} + \tau_t + \eta_{ijt} \tag{1}$$

where y_{ijt} is annual real earnings of worker i from their main employer j in year t; θ_i is a worker fixed effect capturing time-invariant differences in earnings across workers; $\boldsymbol{x}_{it}$ is a vector of individual time-varying controls, which includes a normalized quadratic age term as well as marital status; $\psi_{j(i,t)}$ is the firm-specific earnings premium at firm j where i works in period t; τ_t is a year fixed effect; and η_{ijt} is an error term.

The AKM model in equation (1) can be conceptually grounded in a number of different theoretical models, including wage posting (e.g., Burdett and Mortensen, 1998), search and match (e.g., Mortensen, 2010), on-the-job search with wage bargaining (e.g., Cahuc et al., 2006), and monopsonistic wage setting (e.g., Card et al., 2018). We are agnostic about the precise theoretical mechanism at play, noting simply that there is a conceptual basis for the existence of a firm-specific earnings premium which can be retrieved by estimating the model.

Estimating equation (1) by OLS yields unbiased firm premiums under the assumption of exogenous mobility: conditional on worker and firm fixed effects, job transitions must be unrelated to the time-varying error η_{ijt}, and in particular to any idiosyncratic worker–firm match component. Although strict exogeneity is typically rejected in formal tests, Card et al. (2018) show that the wage-change patterns of job movers are broadly consistent with the additive specification, a finding confirmed in a range of settings (e.g., Macis and Schivardi, 2016; Song et al., 2019; Dostie et al., 2023).

Origin firm FE	**Destination firm FE**				
	1st quintile	*2nd quintile*	*3rd quintile*	*4th quintile*	*5th quintile*
1st quintile	37%	23%	14%	13%	12%
2nd quintile	23%	31%	21%	13%	11%
3rd quintile	13%	20%	29%	24%	14%
4th quintile	9%	9%	18%	36%	28%
5th quintile	8%	6%	9%	23%	54%

TABLE 1. Descriptive statistics: Job ladder. *Notes.* This table presents the distribution of job transitions across quintiles of the firm fixed effects distribution, conditional on the quintile of the origin firm.

Table 1 provides a complementary diagnostic based on the job moves that identify the model. For job movers, it reports the distribution of destination-firm premium quintiles conditional on the origin-firm quintile. Three features support our use of AKM premiums. First, many moves go down the job ladder: depending on the origin quintile, between a fifth and a half of transitions are to lower-premium firms. If workers moved mainly to take up better matches, downward moves would be rare, so their prevalence is consistent with mobility being driven by factors such as layoffs and firm closures, which are plausibly unrelated to the match component of earnings. Second, the table is roughly symmetric: the share of moves from one quintile to another is similar in both directions (e.g., 23% from the first to the second and 23% from the second to the first). This mirrors the symmetric wage gains and losses of movers that the additive model implies and that Card et al. (2016) document for Portugal. Third, every pair of quintiles is linked by many moves, so the premiums, which are estimated from movers, rest on dense mobility rather than on sparse connections between a few firms.

2.2. **Linear social interactions model.** In our main analysis, we estimate a linear social interactions model to assess whether immigrants are more likely to work in high-paying firms when their residential neighbors do so. Our outcome of interest is the firm-specific earnings premium, $\psi_{it} \equiv \psi_{j(i,t)}$, estimated from model 1. Neighborhood networks are defined by co-residence in the same six-character postal code, excluding co-residing family members. For any variable z, we denote by $\overline{z}_{n(i,t-1),t-1}$ the exclusive mean of z in $t-1$ among i's peers, defined as individuals residing in the same neighborhood $n(i,t-1)$ as i in year $t-1$. This convention applies both to the firm premium ($\overline{\psi}_{n(i,t-1),t-1}$) and to peer characteristics ($\overline{\boldsymbol{x}}_{n(i,t-1),t-1}$).

We use once-lagged values ($t-1$) in our main specifications, since meaningful social interactions are unlikely to be formed contemporaneously, especially in the case of residential movers; Appendix Table B4 shows that our findings are robust to alternative lag structures, including twice-lagged and contemporaneous values. This lag structure helps address concerns related to simultaneity or reverse causality by making explicit the direction of the estimated peer effects, namely, from established peer networks to current behavior. It also means that peers are measured before any job switch observed

in t, so that a residential move coinciding with the switch cannot mechanically generate the estimated association.

2.2.1. *Preliminaries.* To set the stage, consider a linear social interactions model that captures peer effects for the full neighborhood network:

$$\psi_{it} = \gamma' \boldsymbol{x}_{it} + \nu \psi_{i,t^-} + \phi\, \overline{\psi}_{n(i,t-1),t-1} + \delta'\, \overline{\boldsymbol{x}}_{n(i,t-1),t-1} + \lambda_{\ell(n(i,t-1))} + \tau_t + \varepsilon_{it} \tag{2}$$

where the outcome variable is the pay premium of the firm in which individual i works at time t, estimated from the AKM model described in the previous section. The vector $\boldsymbol{x}_{it}$ contains individual covariates, including quadratic age, marital status, gender, country of birth, immigration category (family, refugee, economic, or other) at arrival, years of education at arrival, and official language knowledge (English or French) at arrival. The variable ψ_{i,t^-} denotes the firm pay premium at the individual's most recent prior distinct employer: t^- is the most recent year in which i's main employer differed from their employer in period t.

The vector $\overline{\boldsymbol{x}}_{n(i,t-1),t-1}$ contains so-called contextual effects: the averages of peers' characteristics, including average worker fixed effects $\overline{\theta}_{n(i,t-1)}$ estimated from equation (1), the proportion of females, the share by marital status (married, common law, separated, divorced, widowed and single), average age, and average age-squared. Hence, δ captures contextual effects. Including the first of these is central to our interpretation. Since the decomposition in model 1 separates each worker's earnings into a worker component capturing time-invariant earnings capacity, and a firm-specific premium, conditioning on $\overline{\theta}_{n(i,t-1)}$ allows us to distinguish the quality of the firms peers work for from the quality of peers themselves. The peer effect ϕ therefore captures the role of peers' employers, net of peers' skills and background.

The parameter $\lambda_{\ell(n(i,t-1))}$ is a fixed effect for the locale $\ell(n(i,t-1))$ containing neighborhood $n(i,t-1)$. It absorbs common shocks or sorting into larger residential areas. The parameter τ_t denotes year fixed effects, and ε_{it} is the error term. The coefficient ϕ measures the endogenous peer effect–the extent to which immigrants whose neighbors work at higher-paying firms themselves work at higher-paying firms in the subsequent year.

Model 2 is a reduced-form representation of several classes of social-interactions models.[3] In our context, the most natural interpretation is through job search, referrals, and employer-specific information transmission, which are more readily available through social contacts (e.g., Montgomery, 1991; Beaman, 2016). In environments with imperfect information about worker productivity or firm–worker match quality, referrals from well-placed neighbors can also reduce asymmetric information, increasing both hiring probabilities and match quality (e.g., Dustmann et al., 2016). Shared identity may strengthen these channels if information is easier to transmit, more credible, or more salient when it comes from socially similar peers.

Our empirical design is not intended to distinguish sharply between these channels. Instead, we interpret $\phi > 0$ as evidence that local peer exposure affects access to higher-premium firms through

[3]For example, Blume et al. (2015) show that it can be derived from a theoretical model in which individuals have a preference for conformity. In our application, this would imply that workers adjust their job search and application behavior toward the prevailing job opportunities or employer types observed among peers. Alternatively, models with strategic complementarities in effort or job search (e.g., Calvó-Armengol et al., 2009) predict that a higher incidence of high-quality jobs among peers increases the incentives or likelihood of securing such jobs oneself.

a combination of reduced search frictions and improved matching, while using peer identity, job retention, and job switches to shed light on the likely channels.

2.2.2. *Main specification.* To assess whether the strength of neighborhood peer effects depends on shared identity between an immigrant and their peers, we extend equation (2) by partitioning i's peers into groups indexed by $s \in \mathcal{S}$. Groups are defined relative to i: one group consists of the peers who share i's identity, and the set $\mathcal{S}$ depends on the identity marker:

$$\psi_{it} = \gamma' \boldsymbol{x}_{it} + \nu \psi_{i,t^-} + \sum_s \phi_s \overline{\psi}^s_{n(i,t-1),t-1} + \sum_s \delta'_s \overline{\boldsymbol{x}}^s_{n(i,t-1),t-1} + \lambda_{\ell(n(i,t-1))} + \tau_t + \varepsilon_{it} \tag{3}$$

where $\overline{\psi}^s_{n(i,t-1),t-1}$ and $\overline{\boldsymbol{x}}^s_{n(i,t-1),t-1}$, defined analogously to equation (2), are the average firm premium and average characteristics, respectively, among group-s peers. When the identity marker is immigrant status, $\mathcal{S} = \{I, N\}$: fellow immigrants, who share i's status, and natives, respectively. When it is country of birth, $\mathcal{S} = \{I^{\text{same}}, I^{\text{other}}, N\}$: immigrants born in the same country as i, immigrants born elsewhere, and natives, respectively. Similarly with mother tongue. For immigrant-peers, the contextual vector includes not only peer characteristics (including average worker fixed effects) from the model in equation (2), but also immigrant-specific composition and characteristics (proportions by immigration category at arrival and by country of birth, average years of education at arrival, and the proportions with official language knowledge at arrival and of recent immigrants). This accounts for differences in the overall mix of same- and different-identity peers that may be correlated with unobserved factors affecting employer quality, such as the probability of social contact between groups, or neighborhood institutions that facilitate employment referrals within certain communities. The peer effects coefficients, ϕ_s, therefore reflect the role of peers' employers net of peers' skills and background. In all cases, co-residing family members are excluded from peer sets.

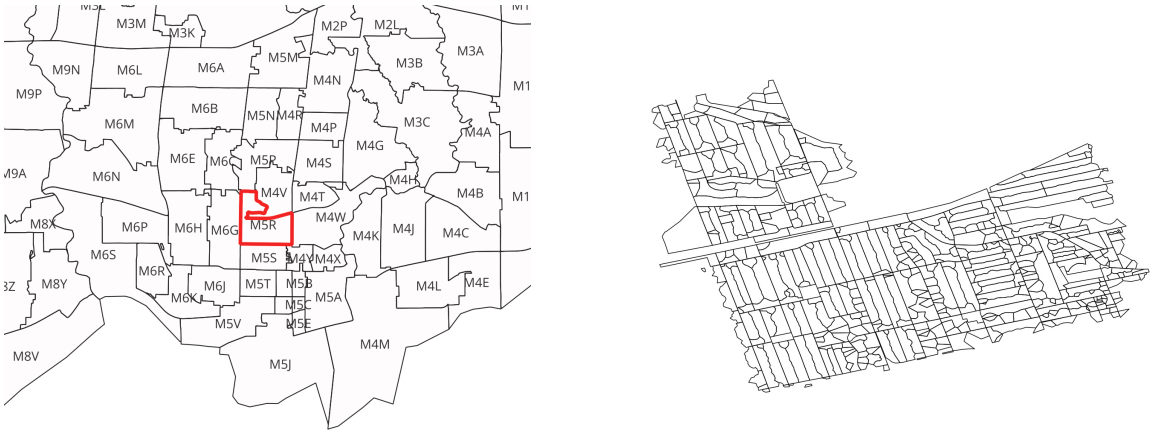
Our interest lies in whether ϕ_s is larger for the peer group that shares the focal worker's identity than for the other groups in $\mathcal{S}$, which would be consistent with homophily in job-related information flows.

2.3. **Identification.** There are two potential threats to identification of peer effects. The first, correlated effects, is our most pressing challenge so in this section, we describe how we tackle it in some detail. The second potential issue is the separate identification of endogenous peer effects and contextual effects. As we argue below, this is unlikely to be a major concern in our context.[4]

2.3.1. *Correlated effects.* The key identification challenge is correlated effects: the possibility that the correlation between an individual's outcomes and those of their peers is driven by endogenous choice of peers or by common shocks (Manski, 1993). In our context, the former may arise from neighborhood sorting. For example, people with high skills or a strong work ethic may choose to live near peers who work at high-paying firms, and would be more likely to find jobs at such firms in any case. The latter may be manifested in a common neighborhood-level shock. For instance, a bus line may make it easier to commute to work; or a daycare may facilitate longer work hours.

[4]An additional issue is that there can be no "isolated" individuals in neighborhood networks. In other words, it cannot be the case that there is only one member of a particular group in a neighborhood since such an individual wouldn't have a neighborhood network. We ensure this through our sample restrictions, discussed in Section 3.4.

To address correlated effects, we rely on quasi-exogenous variation in assignment to neighborhoods within larger residential locales. The basic idea is that people may choose to live in a particular locale based on individual characteristics or location characteristics. However, conditional on observable individual characteristics, exactly which neighborhood they live in within a given locale is plausibly exogenous.



(A) FSAs in downtown Toronto

(B) LDUs in the *M5R* FSA

FIGURE 1. Example of neighborhoods and locales: LDUs in one downtown Toronto FSA

We operationalize this by exploiting Canada's postal code structure, which we argue furnishes a natural way to address correlated effects while credibly capturing neighborhood-level social interactions. Specifically, postal codes–devised and maintained by the Canada Post Corporation–comprise 6 alphanumeric characters. The first three characters pertain to Forward Sortation Areas (FSAs); the last three characters, to Local Delivery Units (LDUs); and LDUs are nested in FSAs.[5] For example, the Toronto CMA has approximately 100,000 LDUs, which are nested within around 180 FSAs. Figure 1 presents an example of what this looks like in practice. Panel A presents a map of FSAs in downtown Toronto. Panel B zooms in on the (6-character) LDUs within the three-character FSA, *M5R*. This FSA, outlined in bold (red) in Panel (A) roughly coincides with what locals call "The Annex": a locale abutting the University of Toronto. Its population of 26,197 inhabitants according to the 2021 census is comparable to Toronto's median FSA population of 26,128. Its large LDU count and structure also mirror that of other FSAs in this area.

Boundaries of FSAs are constructed for ease of sorting and subsequent physical delivery of post. Within urban areas, the sorting feature means that each FSA is readily accessible by road, typically through a major artery. The physical delivery feature means that FSAs are geographically contiguous and contain addresses that are connected, often traversable by bicycle or foot. These features suggest that residents of an FSA have similar physical access to road and transportation networks; public goods and services such as schools, community centers and parks; and private goods and services

[5]The first character (a letter) denotes a large geographic area. For example, M denotes the City of Toronto. The second (numeric) character is always 0 for rural areas, and a positive integer for urban areas. The remaining characters have no discernible logic.

such as shops and restaurants. As such, FSAs–almost by construction–capture an important range of individual motivations for sorting as well as sources of common shocks, making them a natural measure of locale given our identification strategy.

LDUs within a given FSA are designed to facilitate hand-delivery of mail by postal workers. This means that, again almost by construction, each postal code contains a subset of blocks that are conducive to in-person interaction. This makes postal codes natural candidates for defining neighborhoods. At the same time, the housing market was tight during our observation period. According to Statistics Canada, the vacancy rates in Toronto, Vancouver, and Montréal between 2001 and 2017 were 2.3%, 1.2%, and 2.7%, respectively (Statistics Canada, 2025). This means that even if one could choose a locale to live in, finding housing in a particular neighborhood was a low-probability event. Additionally, gathering information on neighborhood characteristics at such a granular level is challenging for prospective buyers and renters (Bayer et al., 2008; Baum-Snow et al., 2024). This is apparent in the map reported in Panel (B) of Figure 1 above, where the single *M5R* FSA contains around 400 LDUs–which mostly correspond to single housing blocks with an average population of around 60, according to the 2021 census.

Our identification strategy rests on the claim that conditional on locale (i.e., FSA), neighborhood (i.e., LDU) assignment is plausibly exogenous. In Section 4, following Bayer et al. (2008) and Schmutte (2015), we furnish evidence to support this claim by showing that conditional on locale, there is little evidence that individuals are sorting based on observable characteristics. We also show that our baseline results go through when we include locale×time fixed effects, to allow for potential time-varying common shocks at the locale level. A final issue is the possibility that there may be identity-specific common shocks. For example, a regulatory change may allow for easier entry into high-paying firms whose employees tend to come from particular countries or speak particular languages. This would result in an upward bias of the peer effect estimate for immigrants' shared identity networks. We account for this possibility in Section 7 by showing that our results are robust to country-of-birth×year fixed effects.

The comparison across peer groups provides an additional check on correlated effects. Common shocks and sorting on overall neighborhood quality affect the measured employer quality of immigrant- and native-peers alike, and so cannot on their own generate differences in peer effects between the two groups. Such differences could instead arise from group-specific sorting, most plausibly, ethnic clustering in particular neighborhoods within a locale. Our specification and diagnostics address this: the contextual effects in model 3 control for the identity composition of the peer network; the sorting evidence in Section 4 extends to origin-specific characteristics; and in Section 7 we include country-of-birth×locale fixed effects, which absorb time-invariant clustering of origin groups within a locale.

2.3.2. *Reflection problem.* Even if correlated effects are addressed, the endogenous peer effect may not be separately identified from contextual effects. When own and peer outcomes are determined simultaneously, the average outcome of a group is a function of its average characteristics, so the two cannot be told apart. This is the reflection problem (Manski, 1993).

In our setting, this concern is limited for two reasons. First, we use lagged peer employer quality, relating own employer quality in year t to peer employer quality in year $t-1$. This timing helps to rule out simultaneity. Second, peer employer quality is measured using firm-specific earnings premiums

estimated from complete work histories, so the peer variable is predetermined relative to the focal immigrant's current outcome (Schmutte, 2015; Cornelissen et al., 2017).

3. DATA

This section describes the data, including data sources, sample construction, and descriptive statistics. Clarifying sample terminology upfront will be helpful. We refer to the sample used to estimate firm-specific earnings premiums from the AKM model as the *connected set* of workers and firms. Our core immigrant cohort, for whom we estimate peer effects using the linear social interactions model, is called the *analytic sample*; note that this sample consists exclusively of immigrants. For both the connected set and the analytic sample, we may be interested in *all* observations, or the subset of *job switchers*.

For everyone in the connected set, we are able to construct neighborhood peer networks. We refer to these as *neighbors* or *peers*. When we disaggregate these by *immigrant-peers* and *native-peers*, we refer to them as such, or as *immigrant neighbors* and *native neighbors*. Our main focus will be on the analytic sample's neighbors. However, in this section, for the purpose of comparison, we will describe average neighbor characteristics and aggregate neighborhood characteristics of not just the analytic sample, but also the connected set. In some tables, we compare all the observations in the respective samples to the subsample of job switchers; and when describing these for the connected set, we disaggregate the (sub)sample by immigrants and natives.

3.1. **Data source.**

Our primary data source is the Canadian Employer-Employee Dynamics Database (CEEDD) for the years 2001-2017 (Statistics Canada, 2020). The CEEDD links administrative files to match employees and employers in the Canadian labor market, providing population-wide coverage of individuals and firms. Like most administrative data sets, the CEEDD contains limited demographic characteristics, but does include age, gender, marital status, and province of residence. The core of the CEEDD is the T1 (individual tax file), which provides information on annual income from employment. By matching the T1 with the job-level T4 Record of Employment, we can identify the individual's main employer.[6]

3.2. **Connected set.**

We impose four sample inclusion criteria on the core 2001-2017 CEEDD data. First, we only include Canada's three largest Census Metropolitan Areas (CMAs)–Toronto, Montréal and Vancouver–which, in 2024, accounted for 36% of the total Canadian population. CMAs are economically and socially integrated urban areas that closely resemble U.S. commuting zones. As stipulated by Statistics Canada, they comprise adjacent municipalities centered around a population core, with a total population of at least 100,000 of which at least 50,000 live in the main urban center. We focus on these CMAs because they are sizable, and have substantial immigrant populations. The 2001 census recorded a CMA population of 4.7 million in Toronto, 3.4 million in Montréal, and 2.0 million in Vancouver, with immigrant population shares of 43.8%, 17.9%, and 37.6%, respectively. Additionally, these CMAs are immigrant hubs. In 2001 around 70% of landed immigrants in the country resided in these three CMAs.

The second sample inclusion criterion pertains to worker ages and locations. We focus on workers between the ages of 25 and 60 who report working or residing in these three CMAs, in that (i) the

[6]As is often the case with firm-level tax data, information at the establishment level is unavailable.

headquarters of their main employer during the year was located in the CMA; and/or (ii) they reported a residential address within the CMA in their tax returns. Since commuting is not uncommon, this allows us to be as comprehensive as possible in our estimation of firm-specific earnings premiums.

The third criterion is aimed at approximating full-time status. Ideally, we would want to estimate the two-way fixed effect model of equation (1) on full-time workers only. Unfortunately, like other administrative data sets such as Social Security earnings records in the U.S., the CEEDD does not report hours worked or any indication for full- versus part-time status. We therefore only include workers whose earnings from the main employer during each year are above a threshold, which reflects the hypothetical earnings of an individual working full time at minimum wage (Dostie et al., 2023).[7]

Finally, since we are interested in the *firm* premium, we do not include public sector employers. With these restrictions in place we are left with a CEEDD dataset containing approximately 250,000 unique employers in 2001 and 350,000 in 2017, representing industries across the spectrum. We use this connected set of workers and firms to estimate the firm-specific earnings premium–i.e. the firm fixed effect–from the AKM model.[8] In this model, firm fixed effects are identified only within "connected sets", the networks of workers and firms linked through job moves. Since our focus is on CMAs exhibiting high worker mobility, the largest connected set in each CMA captures nearly the entire CEEDD population in Toronto, Montréal and Vancouver: 97% of workers and 95% of firms. This set comprises 17 million immigrant-year and 52 million native-year observations (Table 2).

3.3. **Analytic sample.** We construct the core immigrant sample of analysis from the connected set by taking advantage of the fact that the CEEDD has been matched to the Longitudinal Immigration Database (IMDB), which contains detailed information on all immigrants who entered Canada since 1980.[9] Following Picot and Piraino (2013) and Dostie et al. (2023), among others, we categorize individuals as "immigrants" if they appear in the IMDB; "natives" are individuals who are not in the IMDB. In addition to the basic demographics mentioned earlier, the IMDB has detailed information on immigrants at entry, including education, official language knowledge, immigration category (economic, family, refugee, other), country of birth, and mother tongue.

Our analytic sample comprises a subset of immigrants from the connected set, for whom we estimate peer effects. It satisfies five inclusion criteria, in addition to those described in Section 3.2. First, we restrict attention to immigrants who arrived in Canada between 1996 and 2001. This ensures that we study a cohort of recent immigrants, observed within five years of arrival, who are likely to

[7]Specifically, we construct the threshold in each year of observation by taking the product of the Ontario minimum wage in that year; the average number of weeks worked in Ontario in 2001; and the average number of hours worked per week in Ontario in 2001. The latter two are obtained from Census data. To put this threshold in perspective, in 2001, it amounted to CAD 13,962. The choice of Ontario avoids sample selection based on institutional differences in provincial minimum wages: Ontario is the largest labor market in our data, and its threshold is broadly representative of the inter-provincial range of minimum wages during the study period. Moreover, the share of employment spells near the cutoff is small. Our results are robust to the use of province-specific thresholds.

[8]Two additional criteria are that (i) information regarding sex be non-missing in both tax records and IMDB, and (ii) real value added per worker be larger than CAD 100. Very few observations do not satisfy these requirements.

[9]Technically, the IMDB has tracked immigrants since 1952. However, detailed information, including immigration category, language, and education was only made available from 1980 on. It has also tracked non-permanent residents (NPRs) since 1980. We exclude NPRs from our analysis because they typically hold study permits and are not in full-time employment, or hold work permits restricted to a specific employer. See Statistics Canada (2024).

have relatively weak pre-existing ties. Second, they must have lived in one of the three CMAs for at least one year between 2001 and 2017, which is necessary to construct neighborhood peer networks (Section 3.4). Third, they must be between ages 25 and 44 at the start of the observation period, so that the oldest in the sample are 60 by its end, allaying concerns about retirement effects.

Fourth, they must have at least two immigrant workers and one native worker in their neighborhood. This requirement, together with exclusive averaging, satisfies the "no isolation" condition and allows us to construct networks of immigrant and native neighbors for each focal immigrant. Finally, we require that each locale contains at least two neighborhoods. This restriction ensures within-locale variation, which is central to our identification strategy.

The final analytic sample, which satisfies these inclusion criteria, comprises roughly 900,000 individual-year observations, constituting about 5% of immigrant observations in the connected set for the Toronto, Montréal and Vancouver CMAs. Around 60% of these immigrants reside in Toronto, 15% in Montréal, and 25% in Vancouver.

	Analytic Sample		**Connected set**			
	All	**Job Switchers**	**All**		**Job Switchers**	
			Immigrants	**Natives**	**Immigrants**	**Natives**
Average firm pay premium	-0.007	-0.002	-0.026	0.009	-0.025	-0.003
Job switches	20%	100%	11%	11%	100%	100%
Female	43%	40%	45%	45%	41%	42%
Age in 2001	34	34	33	35	32	33
Married	81%	80%	72%	64%	70%	61%
Years of education	15	15	12		12	
Official language knowledge	74%	76%	66%		68%	
N.Obs.	900,000	180,000	17.3 million	52.4 million	1.5 million	4.4 million
N.Workers	120,000	90,000	2.7 million	7.6 million	890,000	2.5 million
N.Firms	60,000	40,000	490,000	650,000	180,000	300,000

TABLE 2. Descriptive statistics: Individual characteristics. *Notes.* This table presents descriptive statistics for the analytic sample and the connected set. For the analytic sample, we report summary statistics for all observations in the sample and for the subsample of observations following a job switch. Similarly, we report summary statistics for all observations in the connected set and for the corresponding subset of job switchers, separately by immigrant status. Reported statistics include the average firm pay premium ($\psi_{j(i,t)}$) from Eq. 1; the share of observations that follow a job switch; the share of women; average age in 2001; the share of married individuals; and average years of education and official language knowledge at arrival, for immigrants. All figures are rounded to comply with Statistics Canada's confidentiality vetting process; the same convention applies to all other tables.

Table 2 presents descriptive statistics for the analytic sample and, for comparison, all immigrants and all natives in the connected set. These and all subsequent summary statistics are rounded to comply with Statistics Canada's confidentiality vetting process. Immigrants in the analytic sample work at somewhat higher-premium employers than immigrants in the connected set (first row), though their firm premiums remain below those of natives. Job switching is more common in the analytic sample (20% of observations) than among immigrants or natives in the connected set (11%). The female share is slightly lower in the analytic sample, consistent with the full-time earnings threshold and the post-switch subset modestly reducing women's representation. Education and official language knowledge at arrival are reported only for immigrants and are higher in the analytic sample than in the

connected set. Marriage rates are also higher in the analytic sample although, in general, immigrants in the connected set are more likely to be married than natives.

3.4. **Neighbors and neighborhoods.** For each immigrant in the analytic sample, and for each year of observation, we construct neighborhood-level peer networks. We first identify peers as *all* individuals in the connected set who report a residential address in the same 6-character postal code as the focal immigrant in their annual tax return, and who do *not* belong to the same family as the immigrant as reported in the immigrant's tax records. These peer networks comprise 16.6 million observations.

	All			Job switchers		
	All	**Natives**	**Immigrants**	**All**	**Natives**	**Immigrants**
Average firm pay premium	-0.016	-0.006	-0.028	-0.015	-0.004	-0.028
Female	47%	48%	45%	47%	48%	45%
Age in 2001	32	32	32	34	34	34
Married	65%	58%	72%	64%	57%	71%
Years of education			12			12
Official language knowledge			68%			69%
N.Obs.	16.6 million	8.7 million	7.9 million	5.9 million	3.1 million	2.8 million

TABLE 3. Descriptive statistics: Neighborhood-peer characteristics of analytic sample. *Notes.* This table presents descriptive statistics for the peers of individuals in the analytic sample and for the peers of immigrants in the job switchers subsample. We report summary statistics for all peers, and separately by peer immigrant status. Reported statistics include the average firm pay premium ($\psi_{j(i,t)}$) from Eq.1; the share of women; average age in 2001; the share of married individuals; and average years of education and official language knowledge at arrival, for immigrants.

Table 3 provides the analog of the individual-level descriptives in Table 2, but for neighborhood peers of immigrants in the analytic sample. Since we disaggregate peer effects by immigrants and natives in our analysis later on, we do the same here for the descriptive statistics. Statistics are very similar for all observations and the subsample of job switchers, except that the latter are slightly older. Native-peers are employed in higher-quality firms than immigrant-peers, consistent with the immigrant–native gap in employer quality already seen in Table 2, and Dostie et al. (2023). Overall, the size distributions of immigrant-peer and native-peer networks are quite similar, characterized by continuous mass and common support (see Appendix Figure A1).

Table 4 provides neighborhood-level descriptives for the analytic sample and the connected set, the latter disaggregated by immigrant and native status. Relative to the neighborhoods of both immigrants and natives in the connected set, those of the analytic sample are more populous (98 versus 68 and 46 residents), more immigrant-dense (47% versus 32% and 14%), and somewhat less affluent, with average employment income about $2,000 lower. The number of neighborhoods per locale is similar across all groups, and the same contrasts hold in the job-switcher subsample.

To explore the effect of shared national identity, we disaggregate immigrant-peers in each neighborhood by country of birth. Specifically, for each immigrant in our analytic sample, we group other immigrants in their neighborhood according to whether they have the same country of birth as the focal immigrant, or come from a different country of birth. The challenge we face is that immigrants in these data originate from over one hundred different countries, many of which have small absolute

	Analytic sample		Connected set			
			All		Job Switchers	
	All	Job Switchers	Immigrants	Natives	Immigrants	Natives
# Neighborhood-year observations	488,000	141,000	3.2 million	8.1 million	800,000	2.3 million
# Neighborhoods	68,000	49,000	365,000	704,000	199,000	466,000
# Neighborhoods per locale (mean)	678	665	689	723	673	688
Neighborhood characteristics (average)						
Total population	98	121	68	46	94	80
Immigrant share	47%	49%	32%	14%	41%	18%
Average employment income	$44,000	$41,000	$46,000	$46,000	$44,000	$47,000
Average T4 income from main employer	$42,000	$39,000	$44,000	$44,000	$42,000	$45,000

TABLE 4. Descriptive statistics: Neighborhood & neighborhood-peer characteristics of analytic sample & connected set. *Notes.* This table presents descriptive statistics for the residential neighborhoods of the analytic sample and of the connected set from Eq.1. For the analytic sample, we report neighborhood-level summary statistics for all observations in the sample and for the subsample of observations following a job switch. Similarly, we report summary statistics for all observations in the connected set and for the corresponding subset of job switchers, separately by immigrant status. Reported statistics include the average total population, immigrant share, average employment income, and average income from the main employers, at the neighborhood level (both measured in Canadian dollars). The number of neighborhoods per locale is averaged across neighborhood-year observations.

numbers of immigrants. Hence, immigrants from "low intake" countries often fail to satisfy the no isolation requirement, or are characterized by very small fellow-national immigrant networks that lack meaningful variation necessary for identification.

For this part of our analysis we therefore impose a further sample restriction, in order to ensure that co-national neighborhood immigrant networks exist and are large enough to make meaningful statistical inference. Specifically, we restrict the analytic sample to immigrants who come from the ten most common origin countries in each CMA–a total of 18 countries between them–over our period of observation.[10] Immigrants from these countries represent around 66% of the full analytic sample. Requiring, in addition, at least one co-national and one other immigrant-peer in the neighborhood leaves roughly 400,000 worker-years, about 45% of the analytic sample (Appendix Table B6).

We conduct an analogous exercise for co-linguists by disaggregating immigrants in each neighborhood according to whether they share a mother tongue with the focal immigrant or not. Again, there are hundreds of different languages so we restrict our sample to immigrants with the ten most common mother tongues upon arrival in each CMA, with a grand total of 15 languages.[11] Immigrants with these mother tongues represent around 69% of the full analytic sample. The analogous no-isolation requirement again leaves roughly 400,000 worker-years.

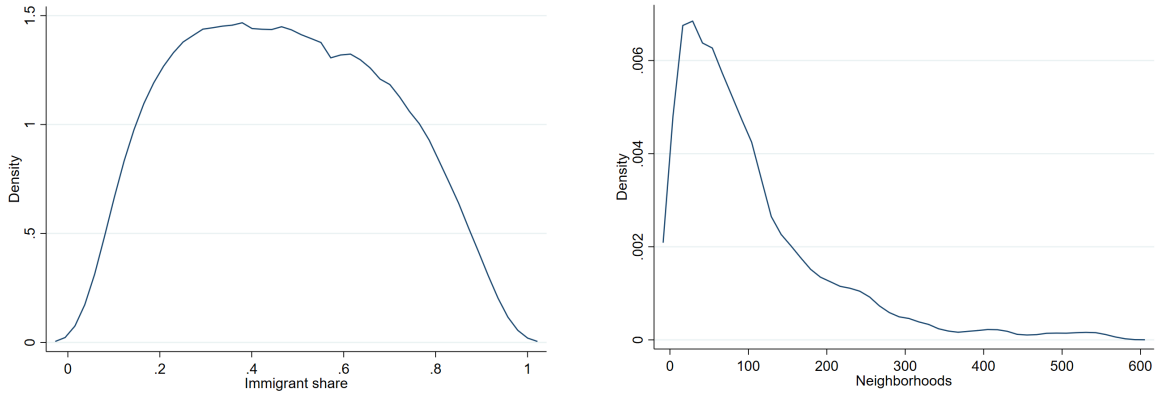
[10]From 2001-2017, these countries were India, China, Philippines, Pakistan, Hong Kong, Jamaica, Romania, Sri Lanka, Russia, and Iran in the Toronto CMA; France, Algeria, China, Romania, Morocco, Haiti, Philippines, India, Russia and Sri Lanka in the Montréal CMA; and China, Philippines, India, Hong Kong, United Kingdom, Iran, Taiwan, Japan, Romania and South Africa in the Vancouver CMA.

[11]These are English, Mandarin, Cantonese, Chinese, Punjabi, Urdu, Tamil, Tagalog, Russian, and Romanian in the Toronto CMA; French, Arabic, Spanish, Romanian, Russian, Creole, English, Mandarin, Tagalog and Chinese in the Montréal CMA; and Tagalog, Mandarin, English, Punjabi, Cantonese, Chinese, Russian, Spanish, Japanese and Romanian in the Vancouver CMA.

4. IDENTIFICATION: SUPPORTIVE EVIDENCE

In this section, we provide supporting evidence for identification of peer effects along several dimensions. We first show that immigrant- and native-peer effects can in principle be identified because most neighborhoods contain a mix of immigrants and natives, and locales contain multiple neighborhoods. Next–although neighborhoods of residence are not randomly assigned–we provide support for the identifying assumption that, conditional on locale, there is negligible sorting based on observables. Finally, we examine residential mobility and show that residential moves are not more likely in the years surrounding a job switch, nor do movers select neighborhoods based on the job characteristics of future neighbors.

A first concern is that immigrant- and native-peer effects cannot be separately identified if neighborhoods are too homogeneous or if locales contain only a single neighborhood. Figure 2 shows that neither problem arises. Panel A documents substantial interior mass in the distribution of immigrant shares across neighborhoods: most contain both immigrants and natives, with relatively few all-immigrant or all-native enclaves. Panel B shows wide variation in the number of neighborhoods per locale, ensuring that peer effects can be identified from within-locale contrasts rather than from across-locale differences.[12]



(A) Immigrant share of neighborhood population

(B) Number of neighborhoods by locale

FIGURE 2. Immigrant share and neighborhood size distributions. *Notes.* Panel A shows the distribution of neighborhood-level immigrant shares across the residential neighborhoods of immigrants in the analytic sample. Panel B shows the distribution of the number of neighborhoods per locale within the same sample; its right tail is truncated. Both distributions are smoothed, and the tail is truncated, to comply with Statistics Canada's confidentiality vetting process; the same convention applies to all other figures.

A second concern is sorting into neighborhoods. The identification strategy assumes that, within a given locale, an individual's neighborhood choice is plausibly exogenous. A testable implication is that, conditional on locale, immigrants should not sort into neighborhoods based on observables. We assess this following Bayer et al. (2008) and Schmutte (2015). Concretely, we randomly draw one person from the neighborhood of each individual in the analytic sample. We regress an

[12]A separate concern regarding sample size is that, at such a granular level, neighborhood networks may be small, and with small networks measurement of average characteristics may be noisy–possibly attenuating peer effects estimates. We address this in Section 7, by showing that our results are robust even when we restrict attention to immigrants with at least five immigrant neighbors and five native neighbors.

observed characteristic of the randomly drawn individual on the corresponding neighborhood average (excluding the selected individual and their family).

	Peer sample				**Immigrant-peer subsample**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Native	0.519	0.027	0.021	0.021				
Female	0.446	0.001	0.001	0.001	0.391	0.001	0.001	0.001
Age	0.951	0.016	0.007	0.007	0.954	0.014	0.002	0.002
Married	0.650	0.021	0.021	0.020	0.691	0.009	0.009	0.009
$\widehat{\theta_i}$ from Eq. 1	0.134	0.060	0.059	0.058	0.113	0.027	0.027	0.026
Residuals from Eq. 1	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Years of education					0.857	0.003	0.003	0.003
Recent immigrant					0.341	0.028	0.002	0.001
Official language knowledge at arrival					0.667	0.005	0.005	0.005
Locale FE	No	Yes	Yes	No	No	Yes	Yes	No
Year FE	No	No	Yes	No	No	No	Yes	No
Locale-Year FE	No	No	No	Yes	No	No	No	Yes

TABLE 5. Sorting within locales. *Notes.* Each row reports the R^2 from regressions of a randomly selected individual's characteristic on the neighborhood mean of that same characteristic (excluding the individual and co-residing family). We estimate these models in the full sample (natives and immigrants) and in the immigrant subsample. Columns 1 and 5 report univariate R^2 without fixed effects; Columns 2 and 6 report the *within* R^2 after adding locale fixed effects; Columns 3 and 7 add year fixed effects; Columns 4 and 8 add locale×year fixed effects.

Table 5 reports R^2's from this exercise. In Columns 1-4 we run this exercise on all peers; for this sample we have data on sex, age, marital status, immigrant status, as well as the worker fixed effect, $\widehat{\theta_i}$, and the residual from model 1. In Columns 5-8 we repeat this exercise for the subset of immigrant-peers, for whom we have additional measurements on years of education, official language knowledge at arrival, and an indicator for arrival within the past ten years. As expected, without fixed effects (Columns 1 and 5), neighbors' averages strongly predict the randomly selected individual's own characteristic–evidence of nonrandom residential choice at broad geographic scales.

Once we condition on locale (Columns 2 and 6), *within* R^2 values fall to 0.06 or below in the overall peer sample and 0.03 or below in the immigrant-peer subsample. Adding year fixed effects (Columns 3 and 7) and locale×year fixed effects (Columns 4 and 8) does not substantively change this pattern. In our main specifications, we therefore include (time-invariant) locale fixed effects to absorb sorting across locales, as well as year fixed effects. Tables B1, B2, and B3 extend this analysis to immigrant-specific characteristics: world area of origin, country of birth, and mother tongue. Immigrants sort strongly along these dimensions in the raw data, with R^2 values as high as 0.38. Conditioning on locale removes most of this: *within* R^2 values fall to 0.05 or below for every world area, and to 0.09 or below for every country of birth and mother tongue. Sorting along identity lines therefore occurs mostly across locales, not within them. Our estimates are also unchanged when we add country-of-birth×locale fixed effects (Section 7).

A third, related, concern is that workers may move into neighborhoods where their future colleagues already reside, rather than their neighbors subsequently becoming their colleagues. If this happens, then the observed correlation between a worker's outcomes and the characteristics of their neighbors may not reflect a causal neighborhood effect on employment outcomes. Instead, it may simply reflect sorting or selection based on job-related factors. While our social interactions model already

addresses potential reverse causality through its lag structure, we undertake two additional exercises to allay this concern.

First, we examine the timing of residential moves across neighborhoods around the time of a job switch, controlling for individual characteristics as well as year and individual fixed effects by estimating the following model:

$$moved_{it} = \alpha_i + \sum_{\substack{k=-3 \\ k\neq 0}}^{3} \beta_k D_{(t-t_i=k)} + \gamma' x_{it} + \tau_t + \varepsilon_{it} \tag{4}$$

where $moved_{it}$ is an indicator equal to one if individual i changed neighborhoods between years $t-1$ and t; α_i denotes individual fixed effects capturing time-invariant differences across workers; $D_{(t-t_i=k)}$ is a set of event-time dummies equal to 1 if calendar year t is k years away from the year of the job switch t_i, for $k \in \{-3, \ldots, 3\}$, $k \neq 0$; the switch year $k = 0$ is the omitted reference category, so all coefficients are measured relative to it; x_{it} is a vector of time-varying controls, including a quadratic in age and marital status; τ_t are calendar-year fixed effects; and ε_{it} is an error term.

The results, shown in Figure 3, suggest that focal immigrants are not more likely to relocate across neighborhoods in the three years before or after a job switch, than in the switch year itself.

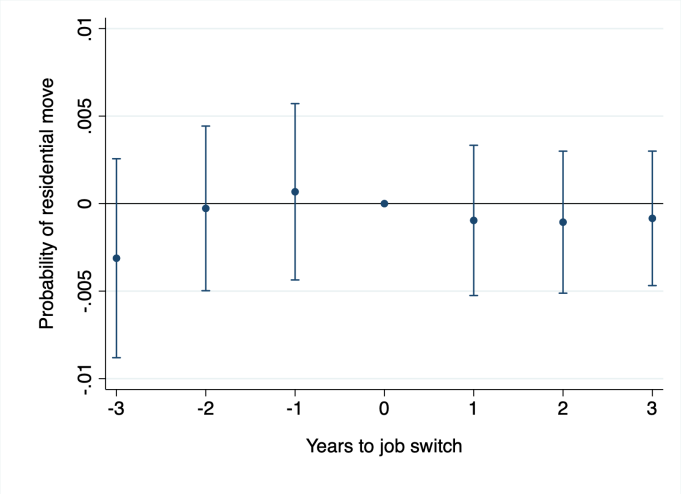


FIGURE 3. Probability of residential moves around a job switch. *Notes.* The figure plots estimates of β_k from Eq. 4–which capture the probability of changing neighborhoods around the time of a job switch–together with 95% confidence intervals. The regression includes individual fixed effects, year fixed effects, and controls for age and marital status. The switch year $k = 0$ is the omitted reference category; its coefficient is normalized to zero and shown for reference. Sample: immigrants in the analytic sample.

Second, we conduct an exercise analogous to that in Table 5, to provide supportive evidence that, conditional on locale and time FE, neighborhood choice is not based on the job characteristics of future neighbors. Specifically, we restrict attention to the peers of our analytic sample who switched neighborhoods. From this subsample of neighborhood movers, we randomly select one individual per neighborhood. Using this randomly selected subset, we estimate univariate regressions for the year prior to the residential move, where the dependent variable is a job characteristic, namely, industry,

	Peer sample				Immigrant-peer sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Agriculture, forestry, fishing, hunting	0.002	0.001	0.001	0.001	0.003	0.001	0.001	0.001
Mining, utilities, construction	0.026	0.001	0.001	0.000	0.008	0.001	0.001	0.001
Manufacturing	0.140	0.003	0.001	0.001	0.093	0.002	0.001	0.000
Wholesale and retail trade, transportation	0.139	0.000	0.000	0.000	0.056	0.000	0.000	0.000
Information, finance, insurance, etc.*	0.290	0.003	0.001	0.001	0.173	0.002	0.001	0.001
Education, healthcare, social assistance	0.110	0.002	0.001	0.001	0.028	0.001	0.000	0.000
Arts, entertainment, hospitality	0.019	0.000	0.000	0.000	0.009	0.001	0.001	0.000
Other services	0.012	0.000	0.000	0.000	0.005	0.000	0.000	0.000
Firm size	0.115	0.000	0.000	0.000	0.038	0.000	0.000	0.000
Commuting distance	0.107	0.000	0.000	0.000	0.039	0.000	0.000	0.000
Locale FE	No	Yes	Yes	No	No	Yes	Yes	No
Year FE	No	No	Yes	No	No	No	Yes	No
Locale-Year FE	No	No	No	Yes	No	No	No	Yes

TABLE 6. Sorting within locales: movers. *Notes.* This table presents the R^2 for a set of regressions with a characteristic for a randomly selected individual among the subset of residential movers on the left-hand side, and the current average of that variable for their future neighbors on the right-hand side. Columns 2-4 and 6-8 report the *within* R^2 after including the fixed effects. * Information, finance, insurance, real estate, professional, scientific and technical services, management of companies, administrative services

firm size and commuting distance of the individual. The independent variable is the average value of that same characteristic among their future neighbors. The results, presented in Table 6, mitigate concerns of sorting on this basis.

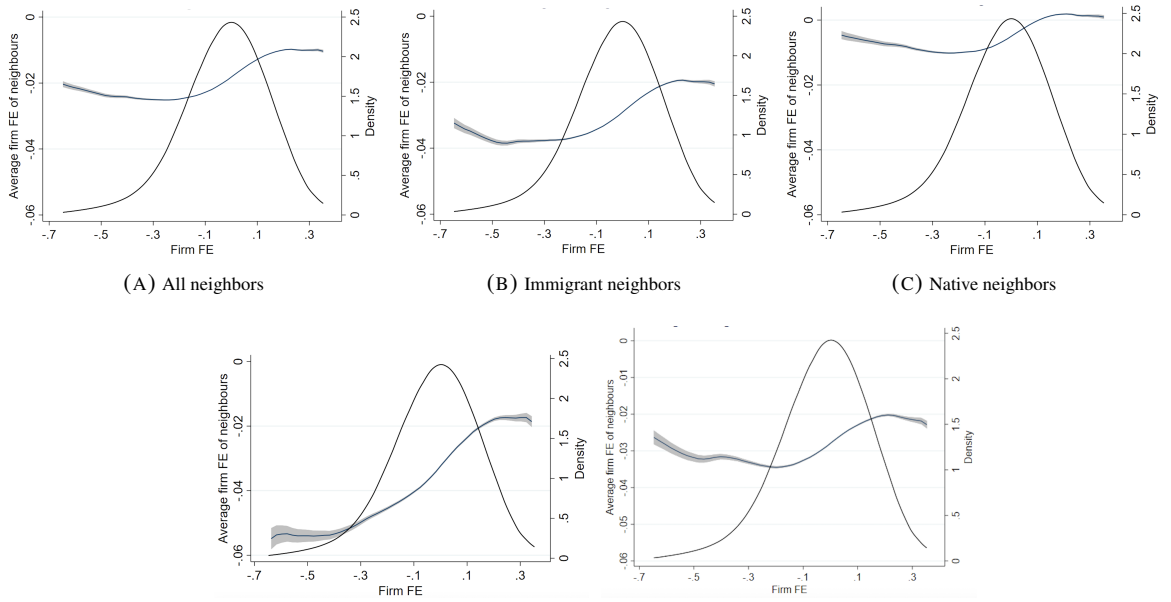
5. MAIN RESULTS

In this section, we report neighborhood peer effects estimates on the firm pay-premiums of immigrants (from the analytic sample). To set the stage, we first describe the nonparametric relationship between individual firm-specific earnings premiums and those of their neighbors. We then estimate the peer effect, ϕ, from the benchmark model 2 for the full neighborhood network, and move quickly to disaggregating this network by shared identity (model 3), beginning with fellow-immigrant vs. native neighbors. We then examine whether peer effects are at play in job retention, or whether they arise when immigrants switch employers. Finally, we disaggregate immigrant neighbors into finer dimensions of common national and linguistic identity.

5.1. **Descriptive evidence.**

Figure 4 traces the relationship between an individual's firm pay premium (x-axis) and the average premium of their neighborhood peers (y-axis), estimated using a local polynomial regression of the latter on the former. Panel A (all neighbors) shows a weak positive association, strongest around the middle of the premium distribution. Panels B and C split peers by immigrant status: both slopes are positive, but the association is clearly stronger for immigrant-peers than for native-peers.

The bottom row restricts the sample to immigrant-peers from the top countries of birth and separates co-nationals from other immigrants. Panel D (co-nationals) shows a steeper positive association



(A) All neighbors (B) Immigrant neighbors (C) Native neighbors

(D) Immigrants: same birth country (E) Immigrants: other birth countries

FIGURE 4. Firm pay premium of individuals and their neighbors. *Notes.* The figures show the nonparametric relationship between immigrants' firm pay premium and the average premium of their peers (measured on the left y-axis), estimated by local polynomial regression, for individuals in the 1st–99th percentiles of the firm pay premium distribution. Panels D and E are restricted to immigrants from the top ten countries of birth in each CMA. Shaded gray areas indicate 95% confidence intervals. Each figure also shows the distribution of individual firm pay premiums (right y-axis).

than Panel E (other immigrant origins), indicating stronger alignment in employer quality with co-nationals than with immigrants from other countries. Taken together, these correlations are consistent with positive neighborhood peer effects, especially among immigrant neighbors of shared identity.

5.2. **Full neighborhood, immigrant-peer, and native-peer effects.** This section presents peer effect estimates. We start by estimating the peer effect, ϕ, from the benchmark model 2 for the full neighborhood network, and move quickly to disaggregating this network by shared identity (model 3) beginning with fellow-immigrant vs. native neighbors.

Table 7 presents peer effect estimates for the full neighborhood network (Panel A) and the neighborhood network disaggregated by immigrant status (Panel B).[13] Column 1 of Panel A shows a coefficient of 0.275 on neighbors' average firm premium, controlling only for year fixed effects. When we account for residential sorting by including locale FE in Column 2, the coefficient falls by around half. Adding individual controls in Column 3 further lowers this coefficient.

Column 4 adds peer characteristics (average worker fixed effect, proportion of females, the share by marital status, average age, and average age-squared). The peer effects coefficient falls only slightly, indicating that the peer effect estimates are not simply picking up contextual composition. This suggests that immigrants benefit from neighbors who work at high-paying firms, over and above those

[13]Estimates for peer effects in this and all subsequent tables are for once-lagged peer averages. Appendix Table B4 shows that the results are qualitatively identical for contemporaneous, as well as twice-lagged peer averages.

Average firm pay premium	*Dependent variable: Firm pay premium* (1)	(2)	(3)	(4)	(5)
	Panel A: all peers				
All peers	0.275***	0.142***	0.097***	0.082***	0.066***
	(0.015)	(0.011)	(0.010)	(0.010)	(0.009)
N.Obs.	900,000	900,000	900,000	900,000	900,000
	Panel B: immigrant and native peers				
Immigrant peers	0.125***	0.066***	0.049***	0.041***	0.034***
	(0.009)	(0.007)	(0.006)	(0.006)	(0.006)
Native peers	0.080***	0.031***	0.016***	0.010	0.008
	(0.009)	(0.007)	(0.006)	(0.006)	(0.005)
Test: Immigrant peers = Native peers					
F	13.380	13.372	13.745	12.551	10.994
Prob > F	0.000	0.000	0.000	0.000	0.001
N.Obs.	900,000	900,000	900,000	900,000	900,000
Year FE	Yes	Yes	Yes	Yes	Yes
Locale FE	No	Yes	Yes	Yes	Yes
Individual controls	No	No	Yes	Yes	Yes
Contextual effects	No	No	No	Yes	Yes
Previous employer firm pay premium	No	No	No	No	Yes

TABLE 7. Full neighborhood, immigrant-peer, and native-peer effects. *Notes.* Panel A presents coefficient estimates for ϕ in model 2, while Panel B presents estimates for ϕ_I and ϕ_N in model 3 for the immigrant sample. The dependent variable is the firm pay premium estimate $\psi_{j(i,t)}$ from model 1. Individual controls include quadratic age, sex, marital status, years of education and official language knowledge at arrival, immigration category, and country of birth. Contextual effects include the average age and average square of age, the share of women, the share of peers by marital status, and the average individual fixed effects from model 1 among the individual's peers. In panel B, contextual effects also include average years of education, share by official language knowledge at arrival, share by immigration category, share of recent immigrants, and share by country of birth, among immigrant-peers. *F*-statistics and *p*-values for the test $\phi_I = \phi_N$ (equal immigrant- and native-peer effects) are reported below the estimates. The number of observations is rounded to comply with Statistics Canada's confidentiality vetting process; the same convention applies to all other results tables. Robust standard errors in parentheses are clustered at the locale level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

neighbors' own earning capacity. In Column 5, we include the immigrant's prior firm premium.[14] Consistent with job ladders, the coefficient on that variable (not shown) is positive, and the peer effect estimate falls further to 0.066.

Column 5 represents our fully saturated model. Qualitatively the estimate shows that, conditional on the full set of controls, immigrants whose peers work in higher-quality firms tend themselves to be employed in higher-quality firms. The point estimate in Column 5 suggests that raising neighborhood peer quality by 0.34 log points, one standard deviation of the firm-effect distribution, increases own firm quality by about 0.022, translating to an earnings increase of 2.3%.

[14]Controlling for prior firm premium also mitigates concerns that peer effects simply reflect lower average employer quality among immigrants and co-nationals.

Panel B reports peer effect estimates for model 3, where the neighborhood network is disaggregated by immigrant status.[15] Columns 1–4 (with additional controls for immigrant characteristics and neighborhood composition in Column 4) show the same attenuation pattern as Panel A. In Column 5, the coefficient on immigrant neighbors' average firm premium is 0.034, implying a 1.2% earnings increase following an increase of the same size in immigrant-peer quality. By contrast, the coefficient on native-peers is close to zero and statistically insignificant. Its standard error indicates that we can rule out native-peer effects larger than 0.018 at conventional levels, i.e., roughly half the immigrant-peer effect. As the F-test at the bottom of Panel B indicates, the difference between immigrant- and native-peer effects is statistically significant regardless of specification.[16]

To put these magnitudes in perspective, the immigrant-native difference in average firm pay premiums in our data is roughly 3.5 log points (Table 2). Two comparisons are useful. First, the 1.2% that comes with raising immigrant neighbors' average firm premium by one standard deviation of the firm-effect distribution amounts to a third of this gap, so peer network quality is a quantitatively meaningful margin. Second, immigrant-peers' average premium trails native-peers' by about 2.2 log points (Table 3), far less than a standard deviation of firm premiums. Hence, according to our estimates, equalizing average peer quality across immigrant and native networks would close well under a tenth of the gap.[17]

5.3. **Staying and switching jobs.** If neighbors pass on job information and referrals, their influence should manifest when immigrants change employers. Alternatively, neighbors may help immigrants who are already at good employers stay there. Table 8 examines both channels. Panel A relates immigrant- and native-peer quality to the probability that the immigrant stays with the same employer. The estimates are close to zero and statistically insignificant across specifications, so job stability is unlikely to account for the peer effects.[18]

Panel B explores the mobility channel by estimating model 3 on the full immigrant sample, with peer quality interacted with indicators for switcher-years (observations directly following a job switch) and stayer-years, so that both sets of coefficients come from one regression with common controls.[19]

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[15]These estimates are obtained from a regression that includes both variables jointly, as in model 3. They are therefore conditional effects and not a simple disaggregation of Panel A, which uses the average of all neighbors as a single regressor. Because immigrant and native neighbor quality are correlated, the coefficients in Panel B do not sum to the Panel A estimate.

[16]Appendix Table B5 reports fully saturated estimates separately for Toronto, Montréal, and Vancouver. While magnitudes vary somewhat across cities (they are similar in Toronto and Vancouver, and more muted in Montréal), the immigrant-peer coefficient is positive in each CMA, and the qualitative contrasts documented in Table 7 persist: immigrant-peers matter and native-peers matter little by comparison. Appendix Table B13 reports estimates for the same model with log earnings as the dependent variable, showing similar patterns of immigrant- and native-peer effects for this outcome.

[17]This estimate uses rounded means from Tables 2 and 3 and the immigrant-peer coefficient from Column 5 of Table 7. Immigrant-peers' average firm premium is -0.028; that of native-peers is -0.006, with an absolute difference of 0.022. Raising immigrant-peer quality by this amount, holding native-peer quality fixed, raises an immigrant's own premium by $0.034 \times 0.022 \approx 0.0007$ (0.07 log points), or about 2% of the 0.035 immigrant-native gap in the connected set (0.009 versus -0.026). Measured against the smaller gap between the analytic sample (-0.007) and natives, the share closed is about 5%. Using the pooled peer coefficient of 0.066 in place of the immigrant-peer coefficient gives about 4%. The exercise is partial equilibrium; the social multiplier $1/(1-\phi)$ implied by model 2 is negligible at these coefficient values.

[18]Appendix Table B7 shows that the null effect is not masking heterogeneity: peers do not seem to affect job stability in either high- or low-premium firms.

[19]Schmutte (2015) shows that results for switcher sub-samples are robust to controlling for endogenous selection into job-to-job mobility.

Average firm pay premium	(1)	(2)	(3)	(4)	(5)
	Panel A: probability of staying in job				
Immigrant peers	-0.011	0.001	0.002	0.006	0.007
	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)
Native peers	0.001	-0.010	-0.006	-0.002	-0.002
	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)
N.Obs.	800,000	800,000	800,000	800,000	800,000
	Panel B: firm pay premium, switcher- and stayer-years				
Immigrant peers × switcher	0.103***	0.047***	0.041***	0.033***	0.044***
	(0.009)	(0.007)	(0.007)	(0.006)	(0.006)
Native peers × switcher	0.092***	0.036***	0.023**	0.016	0.013
	(0.012)	(0.011)	(0.010)	(0.010)	(0.008)
Immigrant peers × stayer	0.130***	0.071***	0.051***	0.043***	0.032***
	(0.009)	(0.007)	(0.007)	(0.006)	(0.006)
Native peers × stayer	0.078***	0.030***	0.015**	0.009	0.007
	(0.009)	(0.007)	(0.006)	(0.006)	(0.005)
Test: Immigrant peers = Native peers, switcher-years					
F	0.558	0.641	2.018	1.881	7.685
Prob > F	0.456	0.424	0.156	0.171	0.006
Test: Immigrant peers = Native peers, stayer-years					
F	16.753	16.019	14.742	13.367	8.403
Prob > F	0.000	0.000	0.000	0.000	0.004
N.Obs.	900,000	900,000	900,000	900,000	900,000
Year FE	Yes	Yes	Yes	Yes	Yes
Locale FE	No	Yes	Yes	Yes	Yes
Individual controls	No	No	Yes	Yes	Yes
Contextual effects	No	No	No	Yes	Yes
Previous employer firm pay premium	No	No	No	No	Yes

TABLE 8. Peer effects: staying and switching jobs. *Notes.* Panel A presents coefficient estimates for ϕ_I and ϕ_N in model 3 for the immigrant sample. The dependent variable is an indicator for retention at the same employer in $t + 1$. Panel B presents estimates of model 3 on the full sample of immigrant worker-years, allowing the peer coefficients to differ between observations that directly follow a job switch (switcher-years) and observations at the same employer as in $t - 1$ (stayer-years); both sets of coefficients are estimated jointly in a single regression with common controls. The dependent variable is the firm pay premium estimate $\psi_{j(i,t)}$ from model 1. Individual controls and contextual effects are the same as in Panel B of Table 7. Because retention is defined on the basis of employment at the same employer in $t + 1$, Panel A is restricted to worker-years for which employment is observed in the subsequent period, resulting in sample loss. *F*-statistics and *p*-values for the test $\phi_I = \phi_N$ (equal immigrant- and native-peer effects separately for switcher- and stayer-years) are reported below the estimates. Robust standard errors in parentheses are clustered at the locale level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

In the fully saturated specification (Column 5), the immigrant-peer coefficient is 0.044 in switcher-years and 0.032 in stayer-years, while native-peer coefficients are small and statistically insignificant. The two coefficients answer different questions: because the firm pay premium is fixed within a job match, only a switcher-year can register the influence of peers on the employer a worker moves to.

The switcher-year coefficient can be interpreted through a simple conceptual lens. Suppose a worker's new job comes either through the formal market or through a lead from an immigrant or a native neighbor, and that such a lead points to the employer of a randomly chosen neighbor from that group. If the worker takes the job regardless of its premium, as is roughly the case after a layoff or firm closure, the coefficient on a group's average firm premium equals the share of employer changes that come through that group. This follows Schmutte (2015), who interprets the switcher coefficient as the share of job offers that arrive through the network; we state it in terms of employer changes, since that is what we observe. If so, just over 4% of immigrants' employer changes come through immigrant neighbors. The native-peer coefficient of 0.013 is not statistically different from zero.

This mapping is exact only in that case. When workers move voluntarily, accepting only offers that improve on their current job, or when well-placed neighbors are more likely to pass on leads, the coefficient also reflects how peer quality affects which offers are taken up. Conditioning on the previous employer's premium, as in Column 5, narrows this gap but does not close it. The large share of downward moves in Table 1 indicates that acceptance does not depend tightly on the premium, whether because moves are involuntary or because workers trade the firm premium off against other features of the job. Either way, the mapping is closer to exact than it would be if workers accepted only better-paying offers. Finally, because peer quality does not affect whether immigrants stay with their employer (Panel A), the composition of switchers is unlikely to vary with peer quality, so the coefficient does not simply reflect who switches. Appendix C sketches the model more formally.

A stayer-year inherits the premium of a job the worker switched to earlier, so its coefficient cannot reflect the influence of current neighbors on the current job. It captures an older correlation, between today's peers and a premium that was set when the worker last changed employer; to the extent that peer quality is serially correlated within a worker, this coefficient echoes that earlier move. Alternatively, better-connected immigrants may be more likely to keep high-premium jobs, so that stayer-years over-represent such jobs. Panel A rules this out: neighbors do not affect whether immigrants stay with their employer, at either high- or low-premium firms (Appendix Table B7).

A comparison of Columns 4 and 5 supports this interpretation. Adding the previous employer's pay premium absorbs recent job history, and the stayer-year coefficient falls (from 0.043 to 0.032). The switcher-year coefficient rises (from 0.033 to 0.044) because the control removes a mean-reversion drag: immigrants with higher-quality networks tend to be leaving higher-premium firms and, since premiums mean-revert across moves, have less room to climb.

5.4. **Co-nationals and co-linguists.** Figure 5 investigates peer effects among immigrants along two salient markers: nationality and language. It presents coefficient estimates for three separate regressions for the fully saturated model.[20] The top panel presents the same estimates as Table 7 Panel B (the immigrant vs. native-peer effects comparison) highlighting again that peer effects on immigrants run through fellow-immigrants rather than natives. The middle panel restricts the analytic sample to immigrants from the largest origin countries, disaggregating their neighborhood networks into co-national immigrants, non-co-national immigrants, and natives. The bottom panel repeats this exercise for the largest language groups, except by co-linguists rather than co-nationals. In both cases, positive, statistically significant peer effects are exerted by both same- and different-identity immigrant-peers. However, coefficient estimates are roughly twice as large for those who share

[20]Appendix Table B6 provides detailed results from which Figure 5 was constructed.

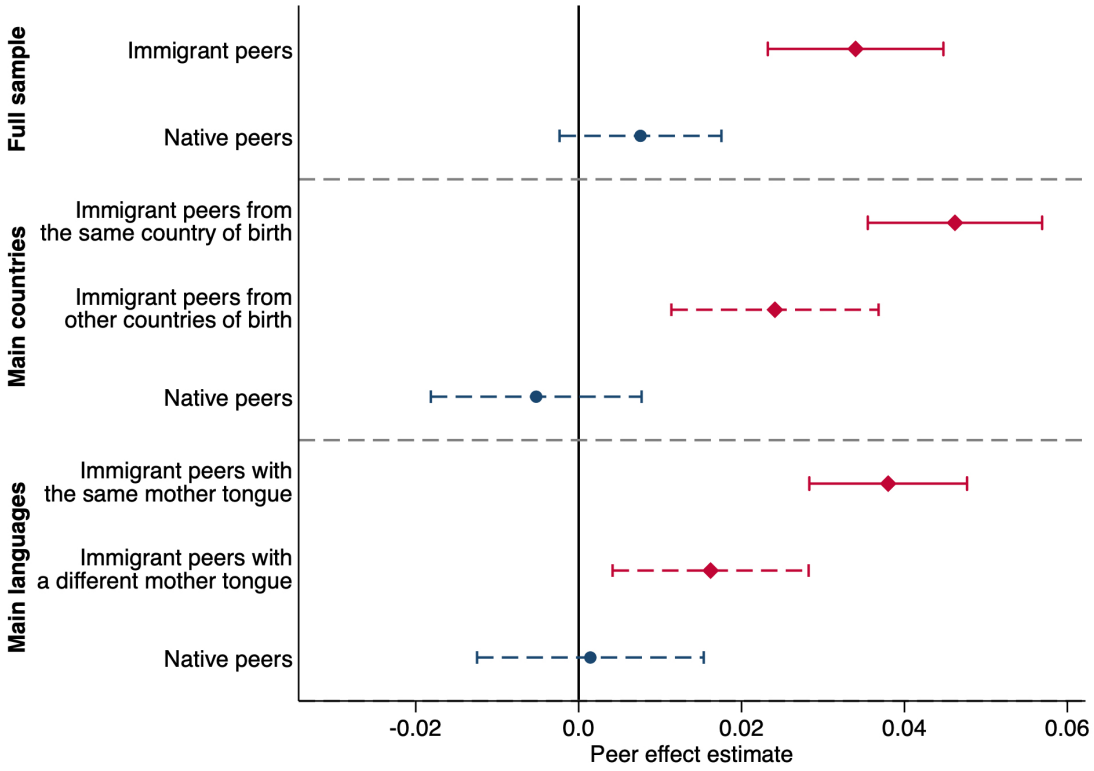


FIGURE 5. Peer effects: Co-nationals and co-linguists. *Notes.* Each set of plots in this figure shows peer effect coefficient estimates for the fully saturated model, along with their 95% confidence intervals. Estimates for different regressions are separated by a dashed line. The first set of estimates graphically depicts point estimates from Column 5 of Panel B in Table 7. The remaining sets are for co-nationals vs. others; and co-linguists vs. others. See Appendix Table B6.

the same country of birth or the same language as for those who do not, a statistically significant difference in both cases.

Native-peer effects are close to zero. This pattern is informative about the underlying mechanisms: if neighborhood effects simply reflected generic local spillovers, one would expect comparable effects from immigrant- and native-peers within the same locale. Instead, the concentration of effects among immigrant-peers, their amplification among co-nationals and co-linguists, and their presence among job-switchers are more consistent with identity-mediated transmission of job-relevant information and referrals: immigrants benefit most from peers with whom they share a cultural background and a language.

6. EXTENSIONS AND HETEROGENEITY

This section poses four related questions. First, are the patterns we document specific to immigrants, or do natives exhibit them too? Second, how would peer effects change if we included family members in neighborhood networks? Third, which immigrants benefit most from peer exposure?

Finally, does shared identity extend beyond nationality and language to other salient dimensions such as gender and recency of arrival?

6.1. **Natives.** We begin by asking whether the peer effects between immigrants reflect group-specific interactions or more general neighborhood spillovers. To address this, we re-estimate the analysis we conducted for immigrants, but this time with natives as the focal group. Estimates for the fully saturated model are presented in Table 9. Column 1 provides a reminder of the baseline results for immigrants from Section 5, and Column 2 provides analogous estimates for natives.[21] The overall neighbor coefficient for natives is 0.066 (Column 2), virtually identical to the estimate for immigrants (Column 1). What differs is who drives the effect. For natives, the coefficient on native-neighbors is a statistically significant 0.045, while immigrant-neighbors also exert a smaller, but positive and statistically significant, influence. As the F-test at the bottom of Panel B indicates, the difference between native- and immigrant-peer effects is statistically significant.

This mirror pattern answers the question we started with. A neighborhood-wide influence, such as proximity to good employers or a local demand shock, would show up in the quality of all neighbors' employers, and should then matter to immigrants and natives alike. Instead, the same neighbors matter differently depending on who is asking: immigrant neighbors for immigrants, native neighbors for natives. Peer effects thus seem to hinge on whether a worker and their neighbors share an identity, and not on the neighborhood as such.

Why do immigrants draw on their own group? One explanation is that they have little choice: firms reachable through native neighbors may not hire more recent arrivals. The results for natives suggest that this is not the whole story. Natives face no such barrier, and yet they too respond mainly to their own group. This suggests that job leads are shared mostly within groups, even where no such barrier exists. Some do cross group lines: cross-group coefficients are small but positive for both immigrants and natives, and statistically significant for natives.

6.2. **Family ties.** Next, we ask whether the relevant ties originate from family members inside the home, or neighbors outside the home. To gauge their relative importance, while aligning our measure with designs that aggregate all neighborhood residents, we broaden the network to include co-resident family members. Column 3 of Table 9 re-estimates the fully saturated model with family members counted among peers.[22] The coefficients rise markedly relative to Column 1. The overall peer effect nearly triples, from 0.066 to 0.179, and the coefficient on immigrant-peers roughly quadruples, from 0.034 to 0.127. This matters for how neighborhood effects are measured. Where the data do not identify households, studies of residential peer effects must count everyone at the same address as a neighbor. Such designs cannot separate the household from the rest of the neighborhood. In our data, roughly two-thirds of a pooled effect of this kind comes from inside the home.[23]

Family members are the closest tie we observe, and this result extends the ordering in Section 5: peer effects are smallest for natives, larger for other immigrants, larger still for co-nationals and co-linguists, and largest when the household is included. We are cautious about this last step. Family

[21]Also analogous to Tables 5 and 6 for immigrants, Appendix Tables B8 and B9 provide supportive evidence for the plausible exogeneity of sorting within locales for natives. Appendix Table B10 furnishes estimates for the full range of specifications.

[22]Appendix Table B11 furnishes estimates for the full range of specifications.

[23]Our estimate with family included is comparable to Schmutte's (2015) estimate for non-native workers (0.16).

Average firm pay premium	Immigrants	Natives	Immigrants incl. family peers
	(1)	(2)	(3)
	Panel A: all peers		
All peers	0.066***	0.066***	0.179***
	(0.009)	(0.010)	(0.010)
N.Obs.	900,000	1,400,000	900,000
	Panel B: immigrant and native peers		
Immigrant peers	0.034***	0.010**	0.127***
	(0.006)	(0.004)	(0.007)
Native peers	0.008	0.045***	0.015***
	(0.005)	(0.007)	(0.005)
Test: Immigrant peers = Native peers			
F	10.994	21.500	148.612
Prob > F	0.001	0.000	0.000
N.Obs.	900,000	1,400,000	900,000
Year FE	Yes	Yes	Yes
Locale FE	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes
Contextual effects	Yes	Yes	Yes
Previous employer firm pay premium	Yes	Yes	Yes

TABLE 9. Peer Effects: Immigrants, natives, and immigrant family members. *Notes.* This table presents estimates from the fully saturated model, where the dependent variable is the firm pay premium estimate $\psi_{j(i,t)}$ from model 1. In Column 1 we report our benchmark results for the immigrant sample, which correspond to Column 5 of Table 7. Column 2 reports the corresponding results for the native sample. Column 3 reports results for the immigrant sample, where we include household members among peers. Robust standard errors in parentheses are clustered at the locale level. *F*-statistics and *p*-values for the test $\phi_I = \phi_N$ (equal immigrant- and native-peer effects) are reported below the estimates. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

members may work at similar firms for reasons other than job leads, for instance because spouses resemble each other or because households move together. For the measurement point this does not matter: whatever the reason, a pooled design attributes it to the neighborhood. For interpretation it does. We interpret the increase as an upper bound on what family ties transmit, and our baseline as the part of the effect that operates outside the home.

6.3. **Heterogeneous responses.** The third exercise asks whether responses to peer quality vary with the focal immigrant's characteristics. Figure 6 plots interaction coefficients from separate regressions (marked by vertical dashed lines) that allow effects to differ by the focal immigrant's sex, age, education, the presence of children, and admission category.[24]

Immigrant-peer effects are present in every subgroup. They are similar for women and men, for younger (below age 34 in 2001) and older immigrants, and for those with and without children.

[24]See Appendix Table B12 for details.

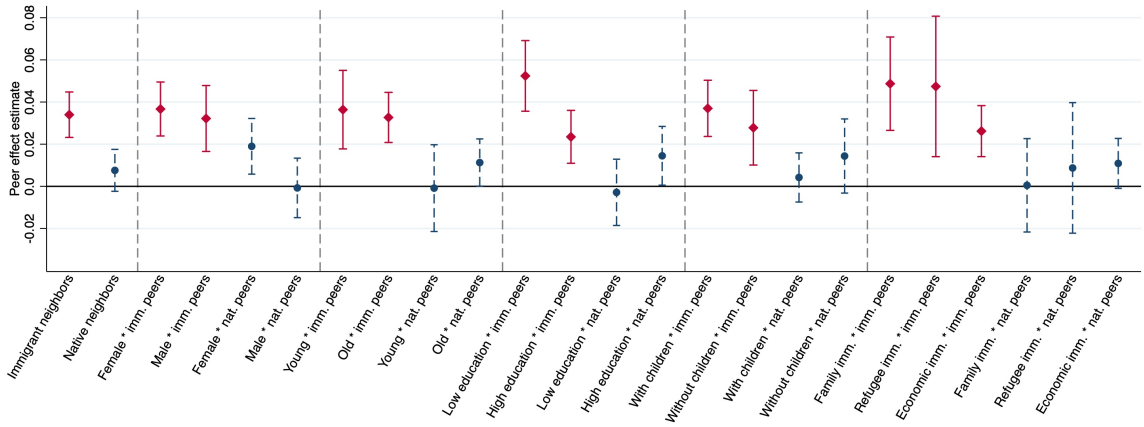


FIGURE 6. Heterogeneous responses. *Notes.* Each set of plots in this figure shows peer effect coefficient estimates for the fully saturated model, along with their 95% confidence intervals. Estimates for different regressions are separated by a dashed line. The first set of estimates graphically depicts point estimates from Column 5 of Panel B in Table 7. The remaining sets report effects by sex, age, education at arrival, the presence of children in the household in 2001, and admission category. See Appendix Table B12.

Where they differ is along two dimensions that plausibly capture access to formal hiring channels. The immigrant-peer coefficient is more than twice as large for immigrants with lower schooling (fewer than 15 years at arrival) as for those with higher education. It is also larger for family and refugee immigrants than for economic immigrants, who are selected on labor market criteria.

Native-peer effects are absent for most subgroups, but do manifest for the highly educated, for women, for older immigrants and, more weakly, for economic immigrants. Two of these, the highly educated and economic immigrants, are the groups for whom immigrant-peer effects are smallest.

One possible interpretation is that immigrant networks substitute for formal hiring channels where these are weak. Seen through the framework in Section 5, a larger share of job changes comes through immigrant neighbors for the less educated and for family and refugee immigrants, as one would expect if formal offers are scarcer for them. An open question is why two subgroups (women and older immigrants) also respond to native-peer effects.

6.4. **Gender and recency.** Finally, we extend the homophily analysis beyond nationality and language by splitting immigrant- and native-peers along two non-ethnic markers of identity: gender and recency of arrival.

Figure 7 presents coefficient estimates for peer groups disaggregated along these dimensions for the fully saturated model.[25] For gender, the pattern is asymmetric. Among men, only male immigrant neighbors matter. The coefficient on male immigrant-peers is statistically significant, while the coefficient on female immigrant-peers is small and imprecisely estimated; native-peer coefficients are near zero. Among women, immigrant-peers of both genders matter in similar magnitude, again with small and statistically insignificant native-peer coefficients. Thus, gendered homophily appears for men, whereas for women the immigrant-peer effect does not hinge on neighbor gender. Recall

[25]See Appendix Table B6 for more detail.

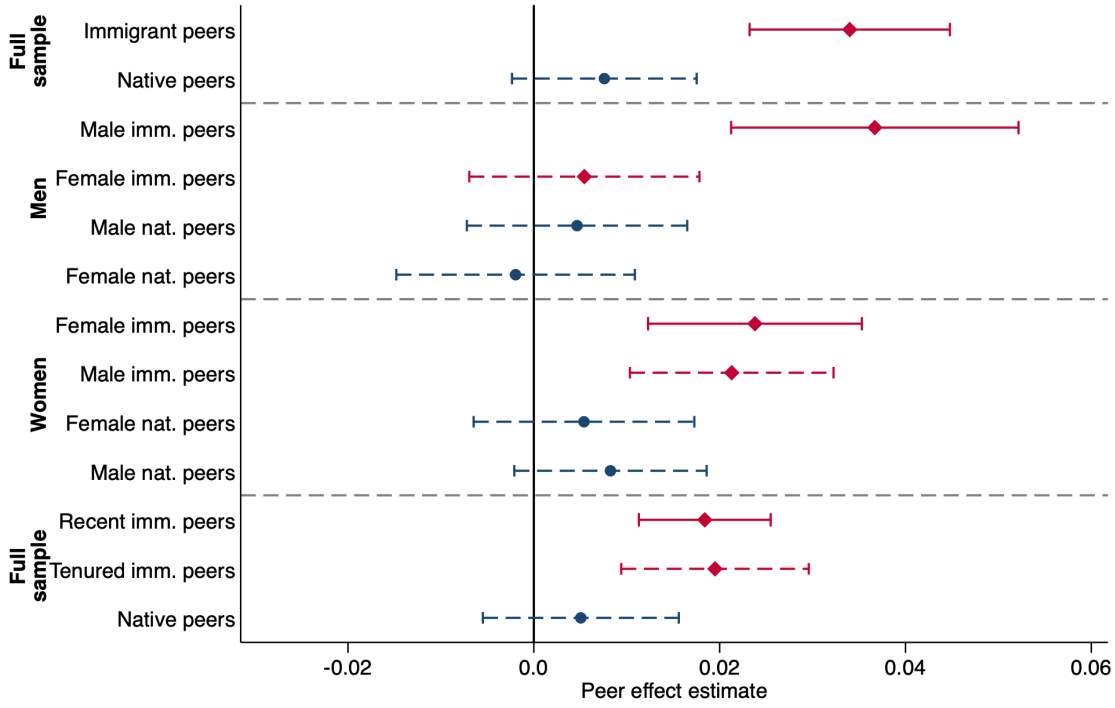


FIGURE 7. Homophily in neighborhood peer effects: Gender and Recency. *Notes.* Each set of plots in this figure shows peer effect coefficient estimates for the fully saturated model, along with their 95% confidence intervals. Estimates for different regressions are separated by a dashed line. The first set of estimates graphically depicts point estimates from Column 5 of Panel B in Table 7. The remaining sets are for immigrants and natives of the same sex vs. others; and recent vs. tenured immigrants. See Appendix Table B6.

that women, unlike men, also respond to native-peers (Figure 6). Women thus appear to draw on a wider set of neighbors than men do.

In terms of recency–defined as having arrived in the previous ten years–immigrant-peers who (like those in our analytic sample) arrived recently and those who are longer-settled have very similar effects, which are not statistically distinguishable. Native-peer coefficients remain small and insignificant.

These results highlight the limits of the similarity pattern. Nationality and language matter for everyone; gender matters only for men, and recency of arrival not at all. The gender result may reflect the kinds of jobs that leads point to: if men and women work in different occupations, a lead from a male neighbor is of more use to a man. This would not, however, explain why women respond equally to male and female neighbors, and we cannot take it further with the data at hand. The recency result speaks against one explanation. If what peers passed on was knowledge built up over years in Canada, longer-settled immigrants should matter more than recent arrivals. They do not.

7. ROBUSTNESS

In Table 10, we address several concerns that might call into question the robustness of our main results. The fully saturated specification from Column 5 of Table 7 is repeated in the "Baseline" Column. First, we argued earlier that locale FEs absorb common shocks. However, this does not hold true if the shocks are time-varying. To address this, Column 2 adds locale×year fixed effects. The results are virtually unchanged.

	Dependent variable: Firm pay premium							
	Baseline	Locale-Year FE	Country of birth-Year FE	Country of birth-Locale FE	At least 5 native and 5 immigrant peers	At least 10 native and 10 immigrant peers	At least 10 native and 10 immigrant peers; Never moved	Drop low population postal codes
Average firm pay premium	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Immigrant peers	0.034***	0.031***	0.034***	0.035***	0.051***	0.051***	0.054**	0.034***
	(0.006)	(0.005)	(0.006)	(0.006)	(0.010)	(0.015)	(0.023)	(0.006)
Native peers	0.008	0.006	0.007	0.006	0.017**	0.028*	0.022	0.007
	(0.005)	(0.005)	(0.005)	(0.005)	(0.008)	(0.014)	(0.022)	(0.005)
Test: Immigrant peers = Native peers								
F	10.994	9.739	11.915	13.426	6.812	1.361	1.036	11.604
Prob > F	0.001	0.002	0.001	0.000	0.009	0.244	0.309	0.001
N.Obs.	900,000	900,000	900,000	900,000	700,000	400,000	100,000	900,000

TABLE 10. Robustness. *Notes.* This table presents coefficient estimates for ϕ_I and ϕ_N in model 3 for the immigrant sample; the dependent variable is the firm pay premium estimate $\psi_{j(i,t)}$ from model 1. Column 1 presents estimates for our main specification, and corresponds to Column 5 in Table 7. In Columns 2, 3, and 4, we include locale×year, country×year, and country×locale fixed effects, respectively. Columns 5-8 present estimates for subsets of the main sample, after we impose different restrictions. Individual controls and contextual effects are the same as in Table 7. *F*-statistics and *p*-values for the test $\phi_I = \phi_N$ (equal immigrant- and native-peer effects) are reported below the estimates. Robust standard errors in parentheses are clustered at the locale level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Second, even with locale fixed effects, peer effects might pick up time-varying shocks specific to origin-country groups (on either the demand or supply side). Column 3 includes country-of-birth×year fixed effects to account for this. Again, the peer effect estimates are very similar.

Third, although the evidence in Section 4 suggests that sorting within locales is limited, one may still be concerned that immigrants from the same country of birth–or immigrants sharing the same mother tongue–cluster in particular neighborhoods within a locale. If co-ethnics share information about available housing or if immigrants with similar labor market aspirations tend to locate near one another, our estimates could reflect sorting on these origin-specific networks rather than neighborhood peer effects. This is precisely the group-specific sorting that, as discussed in Section 2.3, could generate differences in peer effects across peer groups where neighborhood-level confounds cannot. Column 4 therefore includes country-of-birth×locale fixed effects, which absorb all time-invariant factors common to immigrants from the same country of birth within a locale. The estimated peer effects barely budge, indicating that our findings are not driven by this form of residential sorting.

Fourth, in neighborhoods with a small number of immigrant-peers, estimates may be attenuated because the network quality measure is noisy in very small cells. Column 5 restricts the sample to neighborhoods where immigrants in our core sample have *at least* five peers in both immigrant

and native networks. This reduces the sample by roughly one fifth. Nevertheless, the results are qualitatively similar: peer effects are positive, and if anything larger, and remain significantly larger for immigrant- than for native-peers (Column 5). Column 6, which imposes a 10-peer minimum, halves the sample: point estimates are close to those in Column 5, but with the loss of precision the immigrant–native difference is no longer statistically significant.

Fifth, although Section 4 shows that neighborhood moves are not concentrated around job switches, there may still be concern that immigrants sort into neighborhoods closer to their workplaces. Such location sorting could bias upward the peer-effect estimates for neighborhood switchers (not to be confused with *job* switchers). This bias should not characterize individuals who remain in the same neighborhood throughout. Column 7 therefore adds the sample restriction that immigrants must not have moved neighborhoods between 2001 and 2017, along with the 10-peer minimum. For the roughly 100,000 person-year observations that meet these criteria, the immigrant-peer point estimate remains intact (if anything, slightly larger), although the small sample renders the immigrant–native comparison imprecise; this suggests that workplace-based location sorting is unlikely to drive our findings.[26]

Finally, Column 8 reports results for a subsample living in neighborhoods that are not *too small*: we drop neighborhoods in the bottom decile of total resident population, where peer interactions may be more scarce.[27] The results are again virtually identical.

As a final check, we replace shared country of birth with shared world area of birth: America and Oceania, Europe, Africa, West Asia, East Asia, South Asia, and Southeast Asia (Table 11). World area is a coarser marker of identity than nationality, but it retains more of the sample. Requiring at least one immigrant-peer from the same world area and one from a different one leaves about three quarters of the analytic sample, compared with under half in the country and language exercises. Column 2 re-estimates the baseline on this subsample, where the immigrant-peer coefficient is 0.062, against 0.034 in the full sample. Column 3 splits immigrant neighbors by world area. The coefficient is 0.040 for neighbors from the same world area and 0.014 for those from a different one, a statistically significant difference. Native-peer effects remain close to zero. The gap between same- and different-origin neighbors is at least as large as for country of birth (Figure 5).

8. CONCLUSION

Access to high-paying firms is an important path to immigrants' economic integration. This paper examines whether residential peers shape that access, and whether peer effects depend on shared identity. Using linked Canadian employer-employee data combined with immigration records, we find that immigrants whose neighborhood peers work at higher-paying firms themselves work at higher-paying firms. The effect runs through immigrant-peers rather than native-peers. It arises when immigrants change jobs, not through retention: interpreted through a simple model of job leads, about 4% of immigrants' employer changes come through immigrant neighbors. It is roughly twice as strong when neighbors share a country of birth or mother tongue. Natives show the mirror image, responding mainly to native neighbors.

[26] We note that never-movers are a selected subset, so this is a conservative check on sorting rather than a new source of identification.

[27] Note that dropped neighborhoods do not differ materially on observables from those retained.

	Dependent variable: Firm pay premium		
Average firm pay premium	Baseline (1)	World area of birth sample (2)	World area of birth sample (3)
Immigrant peers	0.034***	0.062***	
	(0.006)	(0.009)	
Native peers	0.008	0.003	0.005
	(0.005)	(0.005)	(0.005)
Same world area of birth			0.040***
			(0.004)
Different world area of birth			0.014***
			(0.005)
Test: Immigrant peers = Native peers			
F	10.994	31.684	
Prob > F	0.001	0.000	
Test: Same world area of birth = Different world area of birth			
F			15.949
Prob > F			0.000
Test: Same world area of birth = Native peers			
F			26.049
Prob > F			0.000
N.Obs.	900,000	700,000	700,000

TABLE 11. Peer networks by world area of birth. *Notes.* This table presents coefficient estimates for ϕ_s in model 3 for the immigrant sample. The dependent variable is the firm pay premium estimate $\psi_{j(i,t)}$ from model 1. Column 1 reports our estimates of ϕ_I and ϕ_N for the main sample, and corresponds to Column 5 in Table 7. Column 2 reports the same estimates on the subsample of immigrants who have at least one immigrant-peer from the same world area of birth and one from a different world area of birth. In Column 3, we disaggregate the immigrant-peer network by whether neighbors share the focal immigrant's world area of birth, reporting $\phi_{I^{\text{same}}}$, $\phi_{I^{\text{other}}}$ and ϕ_N, on the same sample as Column 2. Individual controls and contextual effects are the same as in Table 7. *F*-statistics and *p*-values are reported below the estimates for the tests $\phi_I = \phi_N$ (Columns 1-2), and $\phi_{I^{\text{same}}} = \phi_{I^{\text{other}}}$ and $\phi_{I^{\text{same}}} = \phi_N$ (Column 3). Robust standard errors in parentheses are clustered at the locale level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Three further points bear noting. The first concerns what a neighborhood effect actually measures. Our estimates are smaller than those in studies that count everyone at the same address as a neighbor, and the family exercise in Section 6 indicates why. Estimates including co-resident household members in the network suggest that roughly two-thirds of the pooled effect comes from inside the home. This is not a criticism of those studies: after all, the household is part of the neighborhood. But it does mean the two should not be conflated when the policy question concerns who immigrants live near rather than whom they live with. The second point concerns interpretation. Immigrant-peer effects are larger for the less educated and for family- and refugee-class admissions, and native-peer effects show up mainly among women, older age groups, the better educated and the economic class. The simplest explanation is that, for some immigrant subgroups, networks substitute for formal labor market channels where these are thin. Our data do not permit us to say whether this is because job information from co-nationals is more credible, because co-nationals are more willing to refer, or because the firms that hire through immigrant networks are the only ones that will look at a recent arrival. We suspect all three play some role, though the last cannot be the whole story: natives face no such barrier, and they too draw mainly on their own group. Finally, similarity beyond nationality and language matters less: shared gender matters only for men, and recency of arrival not at all.

From a policy perspective, the findings suggest that proximity to well-placed peers can improve access to better employers, and primarily when identity is shared. The effect works in both directions: neighbors at low-paying firms draw immigrants toward low-paying firms. Policies that disperse immigrants widely may weaken these peer effects without necessarily creating alternative pathways into high-paying firms. At the same time, our findings highlight that networks alone cannot close immigrant–native gaps in employer access: by our estimates, equalizing peer network quality across groups would close under one-tenth of the firm-premium gap. Hence, complementary measures that strengthen immigrants' connections to mainstream labor market institutions remain important.

These conclusions should be interpreted with caution. The evidence comes from large Canadian metropolitan areas, where immigrant populations are dense, multi-ethnic, and long viewed as an asset rather than a liability. Peer dynamics may play out differently in contexts where immigrants are fewer, more isolated, or politically contested. Moreover, our estimates capture reduced-form exposure effects rather than specific mechanisms of job transmission.

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APPENDIX A. ADDITIONAL FIGURES

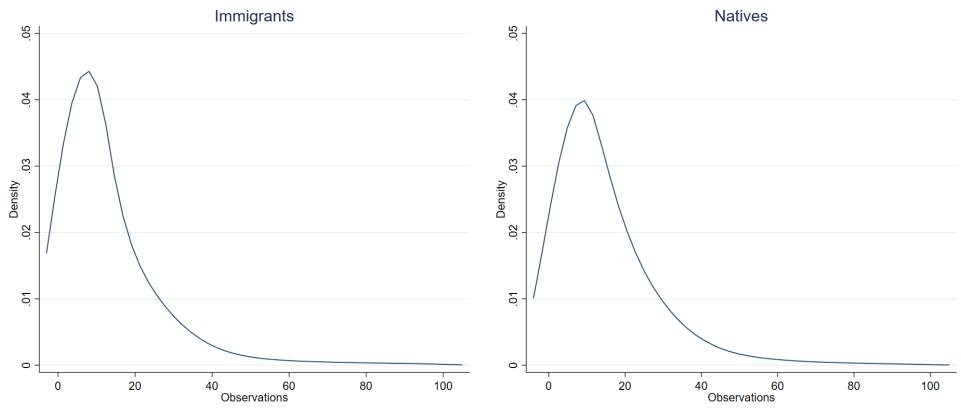


FIGURE A1. Distribution of neighborhood size by immigrant and native networks. *Notes.* The figures show the distribution of neighborhood size for immigrants in the analytic sample, by neighbors' immigrant status.

APPENDIX B. ADDITIONAL TABLES

	(1)	(2)	(3)	(4)
America and Oceania	0.156	0.012	0.012	0.012
Europe	0.220	0.021	0.021	0.020
Africa	0.092	0.011	0.011	0.010
West Asia	0.064	0.007	0.007	0.007
East Asia	0.319	0.039	0.039	0.038
Southeast Asia	0.193	0.048	0.048	0.048
South Asia	0.289	0.042	0.042	0.041
Locale FE	No	Yes	Yes	No
Year FE	No	No	Yes	No
Locale-Year FE	No	No	No	Yes

TABLE B1. Sorting within locales by world area of birth. *Notes.* Each row reports the R^2 from regressions of a randomly selected immigrant's world area of birth on the neighborhood share of immigrants from the same area (excluding the individual and co-residing family). We estimate these models in the immigrant subsample. Column 1 reports univariate R^2 without fixed effects; Column 2 reports the *within* R^2 after adding locale fixed effects; Column 3 adds year fixed effects; Column 4 adds locale×year fixed effects.

	(1)	(2)	(3)	(4)
Haiti	0.283	0.033	0.033	0.032
Jamaica	0.171	0.064	0.063	0.062
France	0.327	0.039	0.039	0.037
Romania	0.062	0.017	0.017	0.018
Russian Federation	0.044	0.009	0.009	0.009
United Kingdom	0.067	0.009	0.009	0.008
Algeria	0.102	0.011	0.011	0.010
Morocco	0.063	0.007	0.007	0.007
South Africa	0.017	0.003	0.003	0.004
Iran	0.091	0.014	0.014	0.014
China	0.218	0.034	0.033	0.030
Taiwan	0.040	0.012	0.012	0.013
Hong Kong	0.157	0.022	0.021	0.020
Japan	0.024	0.005	0.005	0.005
Philippines	0.280	0.078	0.078	0.077
Sri Lanka	0.118	0.032	0.031	0.031
India	0.343	0.047	0.047	0.046
Pakistan	0.066	0.017	0.017	0.017
Locale FE	No	Yes	Yes	No
Year FE	No	No	Yes	No
Locale-Year FE	No	No	No	Yes

TABLE B2. Sorting within locales by country of birth. *Notes.* Each row reports the R^2 from regressions of a randomly selected immigrant's country of birth on the neighborhood share of immigrants from the same country (excluding the individual and co-residing family). We estimate these models in the immigrant subsample. Column 1 reports univariate R^2 without fixed effects; Column 2 reports the *within* R^2 after adding locale fixed effects; Column 3 adds year fixed effects; Column 4 adds locale×year fixed effects.

	(1)	(2)	(3)	(4)
English	0.258	0.024	0.023	0.022
French	0.202	0.012	0.012	0.012
Russian	0.107	0.018	0.018	0.018
Romanian	0.038	0.011	0.010	0.011
Spanish	0.079	0.008	0.008	0.008
Arabic	0.190	0.009	0.008	0.008
Chinese	0.014	0.001	0.001	0.001
Cantonese	0.209	0.026	0.025	0.023
Mandarin	0.112	0.016	0.015	0.013
Japanese	0.016	0.004	0.004	0.004
Tagalog	0.215	0.064	0.063	0.063
Punjabi	0.382	0.090	0.089	0.088
Urdu	0.064	0.018	0.018	0.017
Tamil	0.115	0.030	0.030	0.029
Creole	0.168	0.023	0.023	0.023
Locale FE	No	Yes	Yes	No
Year FE	No	No	Yes	No
Locale-Year FE	No	No	No	Yes

TABLE B3. Sorting within locales by mother tongue. *Notes.* Each row reports the R^2 from regressions of a randomly selected immigrant's mother tongue on the neighborhood share of immigrants with the same mother tongue (excluding the individual and co-residing family). We estimate these models in the immigrant subsample. Column 1 reports univariate R^2 without fixed effects; Column 2 reports the *within* R^2 after adding locale fixed effects; Column 3 adds year fixed effects; Column 4 adds locale×year fixed effects.

	Dependent variable: Firm pay premium		
Average firm pay premium	Baseline (1)	0-year lag (2)	2-years lag (3)
	Panel A: all peers		
All peers	0.066***	0.067***	0.061***
	(0.009)	(0.009)	(0.009)
N.Obs.	900,000	1,000,000	800,000
	Panel B: immigrants and native peers		
Immigrant peers	0.034***	0.036***	0.033***
	(0.006)	(0.006)	(0.006)
Native peers	0.008	0.008*	0.005
	(0.005)	(0.005)	(0.005)
Test: Immigrant peers = Native peers			
F	10.994	11.769	12.324
Prob > F	0.001	0.001	0.001
N.Obs.	900,000	1,000,000	800,000

TABLE B4. Peer effect on immigrants: different lags. *Notes.* Panel A presents coefficient estimates for ϕ in model 2, while Panel B presents estimates for ϕ_I and ϕ_N in model 3 for the immigrant sample. Each column presents estimates for variations of our main models, where we vary the lag with which we construct immigrants' peer network. Column 1 presents estimates for the main sample, where the peer network is constructed with a 1-year lag, and corresponds to Column 5 in Table 7. Columns 2 and 3 present alternative estimates where the peer network is constructed with 0-year and 2-year lags, respectively. The dependent variable is the firm pay premium estimate $\psi_{j(i,t)}$ from model 1. Individual controls and contextual effects are the same as in Panel B of Table 7. *F*-statistics and *p*-values for the test $\phi_I = \phi_N$ (equal immigrant- and native-peer effects) are reported below the estimates. Robust standard errors in parentheses are clustered at the locale level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Dependent variable: Firm pay premium			
Average firm pay premium	Baseline (1)	Montréal (2)	Toronto (3)	Vancouver (4)
	Panel A: all peers			
All peers	0.066***	0.030	0.064***	0.064***
	(0.009)	(0.019)	(0.012)	(0.018)
N.Obs.	900,000	100,000	600,000	200,000
	Panel B: immigrants and native peers			
Immigrant peers	0.034***	0.018*	0.032***	0.039***
	(0.006)	(0.010)	(0.008)	(0.011)
Native peers	0.008	0.001	0.006	0.003
	(0.005)	(0.014)	(0.006)	(0.012)
Test: Immigrant peers = Native peers				
F	10.994	0.853	6.772	4.078
Prob > F	0.001	0.357	0.010	0.046
N.Obs.	900,000	100,000	600,000	200,000

TABLE B5. Peer effect on immigrants by CMA. *Notes.* Panel A presents coefficient estimates for ϕ in model 2, while Panel B presents estimates for ϕ_I and ϕ_N in model 3 for the immigrant sample. The dependent variable is the firm pay premium estimate $\psi_{j(i,t)}$ from model 1. Column 1 presents estimates for the main sample, which includes immigrants residing in the Montréal, Toronto and Vancouver CMAs, and corresponds to Column 5 in Table 7. Columns 2, 3, and 4 present estimates for the subsample of individuals residing in each CMA, separately. Individual controls and contextual effects are the same as in Panel B of Table 7. *F*-statistics and *p*-values for the test $\phi_I = \phi_N$ (equal immigrant- and native-peer effects) are reported below the estimates. Robust standard errors in parentheses are clustered at the locale level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Dependent variable: Firm pay premium					
Average firm pay premium	Baseline (1)	Top 10 countries of birth (2)	Top 10 mother tongues (3)	Men (4)	Women (5)	All (6)
Immigrant peers	0.034***					
	(0.006)					
Native peers	0.008	-0.005	0.001			0.005
	(0.005)	(0.007)	(0.007)			(0.005)
Same country of birth		0.046***				
		(0.006)				
Different country of birth		0.024***				
		(0.007)				
Same mother tongue			0.038***			
			(0.005)			
Different mother tongue			0.016***			
			(0.006)			
Female immigrant peers				0.005		
				(0.006)		
Male immigrant peers				0.037***		
				(0.008)		
Female native peers				-0.002		
				(0.007)		
Male native peers				0.005		
				(0.006)		
Female immigrant peers					0.024***	
					(0.006)	
Male immigrant peers					0.021***	
					(0.006)	
Female native peers					0.005	
					(0.006)	
Male native peers					0.008	
					(0.005)	
Tenured immigrant peers						0.020***
						(0.005)
Recent immigrant peers						0.018***
						(0.004)
N.Obs.	900,000	400,000	400,000	500,000	300,000	800,000

TABLE B6. Homophily. *Notes.* Column 1 presents coefficient estimates for ϕ_I and ϕ_N in model 3 for the immigrant sample. The dependent variable is the firm pay premium estimate $\psi_{j(i,t)}$ from model 1. The other columns present coefficient estimates of ϕ_s for variations of model 3 for the immigrant sample, where we disaggregate the peer network along different dimensions. In Columns 2 and 3, we split immigrant-peers by country of birth and mother tongue, respectively, restricting the sample to immigrants from the ten most common countries of birth or mother tongues in each CMA. In Columns 4 and 5, we split immigrant- and native-peers by gender, separately for men (Column 4) and women (Column 5). In Column 6, we split immigrant-peers by tenure in Canada. Tenured immigrants have lived for more than ten years in the country. In each column, the sample is restricted to immigrants who have at least one neighbor in each subgroup in which we disaggregate the peer network. Individual controls and contextual effects are the same as in Panel B of Table 7. Robust standard errors in parentheses are clustered at the locale level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Dependent variable: Retention		
Average firm pay premium	Baseline (1)	High initial firm pay premium (2)	Low initial firm pay premium (3)
Immigrant peers	0.007	-0.001	0.006
	(0.006)	(0.008)	(0.009)
Native peers	-0.002	-0.005	-0.003
	(0.007)	(0.009)	(0.010)
N.Obs.	800,000	400,000	400,000

TABLE B7. Retention by initial firm pay premium. *Notes.* The table presents coefficient estimates for ϕ_I and ϕ_N in model 3 for the immigrant sample. The dependent variable is an indicator for retention at the same employer in $t + 1$. Column 1 presents estimates for the main sample, and corresponds to Column 5 in Table 8. Columns 2 and 3 present the estimates when we split the sample by whether the firm FE preceding the job switch was above or below the median. Individual controls and contextual effects are the same as in Panel B of Table 7. Robust standard errors in parentheses are clustered at the locale level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Native sample			
	(1)	(2)	(3)	(4)
Female	0.426	0.001	0.001	0.001
Age	0.940	0.011	0.010	0.008
Married	0.545	0.024	0.024	0.022
$\widehat{\theta_i}$ from Eq. 1	0.107	0.043	0.042	0.042
Residuals from Eq. 1	0.000	0.000	0.000	0.000
Locale FE	No	Yes	Yes	No
Year FE	No	No	Yes	No
Locale-Year FE	No	No	No	Yes

TABLE B8. Sorting within locales: Natives. *Notes.* This table presents the R^2 for a set of regressions with a characteristic for a randomly selected individual on the left-hand side, and the average of that variable for all other residents in the same neighborhood on the right-hand side. We run separate regressions for the following individual characteristics: female, age, married, and the worker fixed effects $\widehat{\theta_i}$ and residuals from model 1. Column 1 presents the R^2 for univariate regressions, where we only include the average characteristic among neighbors on the right-hand side. We report the *within* R^2 after including locale fixed effects in Column 2, we add year fixed effects in Column 3, and a locale by year fixed effect in Column 4.

	Native-peer sample			
	(1)	(2)	(3)	(4)
Agriculture, forestry, fishing, hunting	0.001	0.001	0.001	0.001
Mining, utilities, construction	0.008	0.000	0.000	0.000
Manufacturing	0.030	0.000	0.000	0.000
Wholesale and retail trade, transportation	0.064	0.000	0.000	0.000
Information, finance, insurance, etc.*	0.177	0.001	0.000	0.000
Education, healthcare, social assistance	0.049	0.001	0.000	0.000
Arts, entertainment, hospitality	0.003	0.000	0.000	0.000
Other services	0.003	0.000	0.000	0.000
Firm size	0.055	0.000	0.000	0.000
Commuting distance	0.055	0.000	0.000	0.000
Locale FE	No	Yes	Yes	No
Year FE	No	No	Yes	No
Locale-Year FE	No	No	No	Yes

TABLE B9. Sorting within locales: Native movers. *Notes.* This table presents the R^2 for a set of regressions with a characteristic for a randomly selected individual among the subset of native residential movers on the left-hand side, and the current average of that variable for their future neighbors on the right-hand side. Columns 2–4 report the within R^2 after including the fixed effects.

* Information, finance, insurance, real estate, professional, scientific and technical services, management of companies, administrative services

Average firm pay premium	*Dependent variable: Firm pay premium* (1)	(2)	(3)	(4)	(5)
	Panel A: all peers				
All peers	0.317***	0.153***	0.130***	0.099***	0.066***
	(0.026)	(0.011)	(0.011)	(0.011)	(0.010)
N.Obs.	1,400,000	1,400,000	1,400,000	1,400,000	1,400,000
	Panel B: immigrants and native peers				
Immigrant peers	0.078***	0.037***	0.031***	0.020***	0.010**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)
Native peers	0.202***	0.090***	0.078***	0.062***	0.045***
	(0.021)	(0.008)	(0.008)	(0.008)	(0.007)
Test: Immigrant peers = Native peers					
F	36.261	34.536	29.830	24.586	21.497
Prob > F	0.000	0.000	0.000	0.000	0.000
N.Obs.	1,400,000	1,400,000	1,400,000	1,400,000	1,400,000
Year FE	Yes	Yes	Yes	Yes	Yes
Locale FE	No	Yes	Yes	Yes	Yes
Individual controls	No	No	Yes	Yes	Yes
Contextual effects	No	No	No	Yes	Yes
Previous employer firm pay premium	No	No	No	No	Yes

TABLE B10. Peer effect on natives. *Notes.* Panel A presents coefficient estimates for ϕ in model 2, while Panel B presents estimates for ϕ_I and ϕ_N in model 3 for the native sample. The dependent variable is the firm pay premium estimate $\psi_{j(i,t)}$ from model 1. Individual controls include quadratic age, sex, and marital status. Contextual effects include the average age and average square of age, the share of women, the share of peers by marital status, and the average individual fixed effects from model 1 among the individual's peers. In panel B, contextual effects also include average years of education, share by official language knowledge at arrival, share by immigration category, share of recent immigrants, and share by country of birth, among immigrant-peers. *F*-statistics and *p*-values for the test $\phi_I = \phi_N$ (equal immigrant- and native-peer effects) are reported below the estimates. Robust standard errors in parentheses are clustered at the locale level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Average firm pay premium	*Dependent variable: Firm pay premium* (1)	(2)	(3)	(4)	(5)
	Panel A: all peers				
All peers	0.392***	0.274***	0.222***	0.214***	0.179***
	(0.013)	(0.011)	(0.010)	(0.011)	(0.010)
N.Obs.	900,000	900,000	900,000	900,000	900,000
	Panel B: immigrants and native peers				
Immigrant peers	0.234***	0.177***	0.154***	0.150***	0.127***
	(0.009)	(0.008)	(0.007)	(0.007)	(0.007)
Native peers	0.079***	0.037***	0.022***	0.018***	0.015***
	(0.008)	(0.007)	(0.006)	(0.006)	(0.005)
Test: Immigrant peers = Native peers					
F	142.505	163.159	166.050	166.890	148.612
Prob > F	0.000	0.000	0.000	0.000	0.000
N.Obs.	900,000	900,000	900,000	900,000	900,000
Year FE	Yes	Yes	Yes	Yes	Yes
Locale FE	No	Yes	Yes	Yes	Yes
Individual controls	No	No	Yes	Yes	Yes
Contextual effects	No	No	No	Yes	Yes
Previous employer firm pay premium	No	No	No	No	Yes

TABLE B11. Peer effect on immigrants: Including family peers. *Notes.* This table presents peer effect estimates for the immigrant sample, when we include family members among neighborhood peers. Panel A presents coefficient estimates for ϕ in model 2, while Panel B presents estimates for ϕ_I and ϕ_N in model 3 for the immigrant sample. The dependent variable is the firm pay premium estimate $\psi_{j(i,t)}$ from model 1. Individual controls and contextual effects are the same as in Table 7. *F*-statistics and *p*-values for the test $\phi_I = \phi_N$ (equal immigrant- and native-peer effects) are reported below the estimates. Robust standard errors in parentheses are clustered at the locale level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Average firm pay premium	*Dependent variable: Firm pay premium*					
	(1)	(2)	(3)	(4)	(5)	(6)
Immigrant peers	0.034***					
	(0.006)					
Native peers	0.008					
	(0.005)					
Female * Immigrant peers		0.037***				
		(0.007)				
Male * Immigrant peers		0.032***				
		(0.008)				
Female * Native peers		0.019***				
		(0.007)				
Male * Native peers		-0.001				
		(0.007)				
Young * Immigrant peers			0.036***			
			(0.010)			
Old * Immigrant peers			0.033***			
			(0.006)			
Young * Native peers			-0.001			
			(0.011)			
Old * Native peers			0.011**			
			(0.006)			
Low education * Immigrant peers				0.052***		
				(0.009)		
High education * Immigrant peers				0.024***		
				(0.006)		
Low education * Native peers				-0.003		
				(0.008)		
High education * Native peers				0.015**		
				(0.007)		
With children * Immigrant peers					0.037***	
					(0.007)	
Without children * Immigrant peers					0.028***	
					(0.009)	
With children * Native peers					0.004	
					(0.006)	
Without children * Native peers					0.014	
					(0.009)	
Economic immigrants * Immigrant peers						0.026***
						(0.006)
Family immigrants * Immigrant peers						0.049***
						(0.011)
Refugee immigrants * Immigrant peers						0.047***
						(0.017)
Other immigrants * Immigrant peers						0.109**
						(0.051)
Economic immigrants * Native peers						0.011*
						(0.006)
Family immigrants * Native peers						0.001
						(0.011)
Refugee immigrants * Native peers						0.009
						(0.016)
Other immigrants * Native peers						-0.088**
						(0.040)
N.Obs.	900,000	900,000	900,000	900,000	900,000	900,000

TABLE B12. Heterogeneity. *Notes.* Column 1 presents coefficient estimates for ϕ_I and ϕ_N in model 3 for the immigrant sample. The dependent variable is the firm pay premium estimate $\psi_{j(i,t)}$ from model 1. The other columns present coefficient estimates for ϕ_I and ϕ_N for model 3 for the immigrant sample, where we interact the immigrant- and native-peer averages with indicators for subgroups of immigrants. In Columns 2 and 3, we split immigrants by sex and age, respectively. In Column 4, we split the sample by educational attainment at arrival and in Column 5 by parental status. In Column 6, we split immigrants by immigration category. Individual controls and contextual effects are the same as in Panel B of Table 7. Robust standard errors in parentheses are clustered at the locale level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Average firm pay premium	*Dependent variable: Log real earnings* (1)	(2)	(3)	(4)	(5)
	Panel A: all peers				
All peers	1.555***	0.742***	0.110***	0.080***	0.063***
	(0.082)	(0.049)	(0.014)	(0.014)	(0.014)
N. Obs.	900,000	900,000	900,000	900,000	900,000
	Panel B: immigrant and native peers				
Immigrant peers	0.617***	0.265***	0.050***	0.038***	0.031***
	(0.036)	(0.021)	(0.009)	(0.009)	(0.008)
Native peers	0.520***	0.242***	0.018**	0.007	0.004
	(0.051)	(0.027)	(0.008)	(0.008)	(0.007)
Test: Immigrant peers = Native peers					
F	4.247	0.649	7.125	6.463	4.943
Prob > F	0.040	0.421	0.008	0.011	0.027
N. Obs.	900,000	900,000	900,000	900,000	900,000
Year FE	Yes	Yes	Yes	Yes	Yes
Locale FE	No	Yes	Yes	Yes	Yes
Individual controls	No	No	Yes	Yes	Yes
Contextual effects	No	No	No	Yes	Yes
Previous employer firm pay premium	No	No	No	No	Yes

TABLE B13. Peer effects on immigrants: Earnings. *Notes.* Panel A presents coefficient estimates for ϕ in model 2, while Panel B presents estimates for ϕ_I and ϕ_N in model 3 for the immigrant sample. The dependent variable is the log of real earnings. Individual controls and contextual effects are the same as in Table 7. *F*-statistics and *p*-values for the test $\phi_I = \phi_N$ (equal immigrant- and native-peer effects) are reported below the estimates. Robust standard errors in parentheses are clustered at the locale level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

APPENDIX C. INTERPRETING PEER EFFECTS AS SHARES OF MOVES

This appendix shows when the switcher coefficient equals the share of employer changes that come through a peer group, and why this is only approximately so when moves are voluntary.

Setup.

Consider a worker i who lives in neighborhood n within locale ℓ and is employed at a firm with premium ψ_{i,t^-}. A worker who changes employer in year t does so on the basis of a single offer. Let F denote the formal market and $\mathcal{S}$ the set of peer types. The offer comes through a channel $c \in \{F\} \cup \mathcal{S}$, and we denote by q_c the probability that it comes through channel c, with $q_F = 1 - \sum_s q_s$.

Assume that (i) a formal offer has premium $\psi \sim G_\ell$, with mean μ_ℓ; (ii) a type-s offer is at the employer of a type-s neighbor drawn at random from that peer group, so that its premium is given by $\psi = \overline{\psi}^s_{i,t-1} + e_s$, where $\overline{\psi}^s_{i,t-1} \equiv \overline{\psi}^s_{n(i,t-1),t-1}$ is the group average in the year before the move, and e_s has mean zero and a distribution that does not depend on $\overline{\psi}^s_{i,t-1}$; and (iii) the probabilities q_c do not vary with peer quality.[28]

Moves that do not depend on the offer.

Suppose first that the worker takes whichever offer arrives. The share of offers and the share of moves through each channel are then the same, so that

$$E[\psi_{it} \mid \text{switch}, \mathcal{I}_{it}] = q_F \mu_\ell + \sum_s q_s \overline{\psi}^s_{i,t-1} \tag{C1}$$

where $\mathcal{I}_{it}$ is the conditioning information in the empirical specification. Suppose further that locale fixed effects absorb μ_ℓ, and that the other identifying assumptions of model 3 hold. It follows that

$$\frac{\partial E[\psi_{it} \mid \text{switch}, \mathcal{I}_{it}]}{\partial \overline{\psi}^s_{i,t-1}} = q_s$$

That is, the switcher coefficient equals the share of employer changes that come through type-s peers. If (iii) fails, so that q_s varies with peer quality, the derivative has an additional term, $(\partial q_s / \partial \overline{\psi}^s_{i,t-1})(\overline{\psi}^s_{i,t-1} - \mu_\ell)$, and the coefficient no longer equals the share.

Notice that the expectation in (C1) conditions on a switch. If peer quality changed the probability of switching, then the composition of switchers would vary with $\overline{\psi}^s_{i,t-1}$, and the coefficient would also reflect who switches. Panel A of Table 8 shows that peer quality does not affect the probability of staying with the same employer, which suggests that this is not a concern. The immigrant-peer estimate of 0.044 then corresponds to about 4% of moves. Because the estimate comes from observed moves, this is a statement about employer changes, and not about all the offers a worker receives.

Voluntary moves.

Now suppose that the worker accepts an offer only if $\psi \geq \psi_{i,t^-}$. For channel c, denote by A_c the acceptance probability and by m_c the mean premium of accepted offers:

$$A_c = \Pr(\psi \geq \psi_{i,t^-} \mid c), \qquad m_c = E[\psi \mid c, \psi \geq \psi_{i,t^-}]$$

[28] Notice that (ii) and (iii) are assumptions, not normalizations. If well-placed neighbors are more likely to pass on leads, or if their leads point to other firms, then peer quality affects both the probability and the premium of a network offer.

The share of accepted moves that come through channel c is given by

$$h_c = \frac{q_c A_c}{\sum_{c'} q_{c'} A_{c'}}$$

and the mean premium of accepted moves by $\bar{m} = \sum_c h_c m_c$. Only A_s and m_s depend on $\overline{\psi}^s_{i,t-1}$. Under (iii), and using $\sum_c h_c = 1$, we obtain

$$\frac{\partial \bar{m}}{\partial \overline{\psi}^s_{i,t-1}} = h_s \left[\frac{\partial m_s}{\partial \overline{\psi}^s_{i,t-1}} + (m_s - \bar{m}) \frac{\partial \ln A_s}{\partial \overline{\psi}^s_{i,t-1}} \right] \equiv h_s \Gamma_s \tag{C2}$$

Consider the two terms in brackets. The first is at most one. The mean of a variable above a cutoff never falls when the cutoff rises, so when the distribution shifts up against a fixed cutoff, the mean above the cutoff rises no more than one-for-one. The second term is positive when network offers are better than the average accepted offer, because a better peer group then raises the share of accepted moves that come through the network. Hence, Γ_s can lie above or below one, and the switcher coefficient equals the share of accepted moves h_s only when $\Gamma_s = 1$. As the acceptance threshold binds less often, $A_s \to 1$, $m_s \to \overline{\psi}^s_{i,t-1}$ and $\Gamma_s \to 1$, which is the case above.

Conditioning on ψ_{i,t^-} matters here for two reasons. First, it holds the acceptance threshold fixed. Second, it removes the correlation between prior employer quality and peer quality that arises if the same neighbors helped the worker reach the previous employer. This is why Column 5 of Table 8 is the relevant switcher estimate. Even so, without further restrictions the control does not identify h_s.

If some moves do not depend on the offer and others are voluntary, then the coefficient is a weighted average of q_s and $h_s \Gamma_s$. The many downward moves in Table 1 show that acceptance does not depend tightly on the premium. Some of these moves are likely involuntary. Others may be voluntary moves in which workers accept a lower premium in return for other features of the job, such as a shorter commute. If those features are unrelated to the channel through which the offer arrives, they blur the acceptance threshold in the premium, which tends to move Γ_s toward one. However, downward moves alone do not tell us how close Γ_s is to one.

Stayer-years.

A stayer's current firm premium was set when the worker last changed employer. Hence, current peer quality can be associated with it simply because current peers resemble the peers the worker had at the time of that move. The absence of any detectable effect on one-year retention (Panel A of Table 8) makes a retention explanation unlikely. It does not, however, turn the ratio of the stayer and switcher coefficients into a measure of how persistent networks are. That would also require stable channel shares, no relationship between peer quality and the length of job spells, and a model of how immigrant- and native-peer quality evolve together. We therefore interpret the smaller stayer coefficient as consistent with the persistence of earlier network exposure, and not as an estimate of a persistence parameter.