# Do your neighbours matter? Evidence from Building Demolitions in Denmark

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**Abstract**

This paper provides evidence of the effect neighbours have on one's income and employment dynamics. We use the forced moves of households in the public housing sector as an external shock on exposure to neighbours. We find that being exposed to employed neighbours in the short-term significantly increases income and the likelihood of finding a job. On the other hand, exposure to unemployed peers increases the chances of remaining unemployed. These mechanisms are stronger among low-qualified and low-educated individuals. Long-term exposure to neighbours has a more substantial effect than short-term exposure but is not longer-lasting.

**JEL Classification:**

**Keywords:** Neighbourhood effects, Peer effects, Labour market.

# 1 Introduction

The importance of social interactions and networks on individuals' economic achievements has recently gained an increased interest in economics literature[1]. A significant share of the research in these areas, has mainly focused on measuring the effects of social interactions at a small residential level, such as neighbourhoods or blocks (Durlauf, 2004; Bayer et al., 2008; Algan et al., 2016; Haliassos et al., 2019). However, the estimation of such effects often suffers from numerous identification challenges, principally through the endogenous sorting of individuals into neighbourhoods, introducing correlated effects responsible for significant biases (Manski, 1993; Moffitt, 2001). This literature is also mainly characterised by definitions of the exposure to peers, which does not take into account the time component, i.e. the length of the social interactions.

This paper proposes a novel strategy for identifying the effects of the exposure to neighbours at the building level based on a measure of exposure defined as a function of both the number of peers and the amount of time spent with each of them. By using administrative register data, we have access to details on whole population of Denmark, including their location and the length of residency in a specific building. This allows us to compute how many days a resident lived together with each of their neighbours. To solve the identification challenges, we rely on the forced displacement of social housing tenants preceding the closure of the building they live in. According to the Danish regulation around social housing, public housing management agencies are required to provide displaced tenants with a housing similar in terms of cost, size and location to the one they are expelled from. The waiting time to be allocated a corresponding dwelling hinges on the availability of such housing in the social housing stock. Thus, the time people wait to be given a relocation offer

[1] This ranges from the effects of social interactions on education (Lin, 2010; De Giorgi et al., 2010, Boucher et al., 2014; ), crime (Bayer, Pintoff, and Pozen, 2008; Dustmann and Damm, 2014, Aliprantis and Hartley, 2015), and welfare programme participation (Bertrand, Luttmer, and Mullainathan 2000).

is independent from tenants' characteristics, conditional on the attributes of the housing (i.e size of the dwelling, specific neighbourhood).

We use this approach to investigate the impact of the exposure to different types of neighbours, on income and employment dynamics of displaced individuals after their rehousing. As our definition of exposure is a function of both the number of neighbours and the time spent with each of them, we use different fixed-effect specifications to disentangle the effect of each component. The exposure is first computed over the (conditionally exogenous) relocation period only, hence does not cover the full residency. This restriction on the exposure stems from the endogenous nature of the full residency, since displaced individuals chose to remain in the building until its closure. Later in the paper we use the short-term exposure as an instrument to estimate the effect of the exposure measured over the full residency, that we refer to as the "long-term" exposure.

Our results indicate that exposure to neighbours who are employed, has a positive and significant impact on the transition to employment for unemployed individuals. This effect is observed in the three years after displaced people were rehoused, although it reduces in magnitude over time. Conversely, exposure to unemployed neighbours reduces the odds of transition into employment and increases the probability of job loss. Both effects are mainly attributed to the population component of exposure (i.e. the number of peers, as opposed to the length of shared residency). Looking at the heterogeneity of the main effects across different subgroups, the overall impact of the exposure to employed neighbours, appears to be driven by neighbours occupying low and middle-skills jobs, and by those holding nothing higher than a secondary school certification. Results also significantly vary across genders, with only females being sensitive to the exposure to their employed neighbours.

In regard to income dynamics, exposure to employed neighbours leads to an upward shift in the position of individuals in the income distribution. On the other hand, exposure to poorer neighbours has the opposite effect. The impacts of the long-term exposure are similar to those of the baseline analysis, though of higher magnitude. These finding demonstrate the long-lasting effect of interactions in the construction and the maintenance of networks. To test the robustness of our findings, we examine the correlation between the length of the relocation period of displaced households and the characteristics of the building they are relocated in. We find no significant correlation, confirming that our results are not driven by the post-relocation environment of displaced individuals.

This paper relates to a vast literature attempting to identify the effect of social interactions at the residential level. In order to cope with the aforementioned identification challenges, numerous studies rely on experimental frameworks inducing the random displacement of households, to avoid biases caused by endogenous sorting. In particular, the investigation of the random allocation of housing vouchers to households residing in disadvantageous neighbourhoods as part of the Moving to Opportunity (MTO) programme yielded robust evidence of neighbourhood effects on health (Katz et al., 2008), crime (Ludwig et al., 2005), and children's long-term educational and economic outcomes (Chetty et al., 2016; Chyn, 2018). However, although they provide a clear identification design with a well-defined control group, experiments such as the MTO programme hinge on eligibility criteria that restrain the sample to a very specific population. Moreover, the neighbourhoods where treated households could settle in, were significantly less-disadvantaged than their previous neighbourhood of origin. One may thus doubt that such changes arise from real-life opportunities, hence questioning the external validity of the related findings.

Another strand of this literature focuses on displacements induced by natural experiments. This is the case of the random dispersal of refugees in the Netherlands (Kleinhans, 2003), Denmark (Damm, 2014; Dustmann and Damm, 2014) and Sweden (Haliassos et al., 2019) following immigration inflows in the eighties and the nineties. While the findings of these studies yield invaluable insight on the effect of allocation of individuals to particular environments, the population of interest, i.e. refugees, may not be representative of the main population in the host country. Furthermore, these studies mostly identify the "quality" of a neighbourhood through a binary-statistics (deprived or not deprived), a single economic indicators measured at the time people settle in, and do not take into account the potential changes in the environment occurring over time.

The research design used in this paper, combines a mobility shock induced by the closure of public housing buildings with partially overlapping groups of peers, as in de Giorgi et al. (2010), resulting from the variation in the timing of the relocation offers. While other studies rely on building demolitions as external shocks (Aliprantis and Hartley, 2015; Chyn, 2018; and to a lower extent Cingano and Rosolia, 2012, who look at random exposure of workers to their co-workers preceding firms' closure), we are not aware of any research using this approach to measure the length of exposure to a specific peer.

The remainder of this paper is organised as follows. Section II presents the data and the how the exposure is calculated. Section III describes the building closure process and the estimation method of the relocation period at the building level. Section IV introduces the empirical strategy and the main exogeneity assumptions and depicts the outcomes. Sections V and VI report the main empirical findings. The robustness checks are conducted in section VII. Section VIII concludes.

# 2 Data

## 2.1 Data Sources

We derive our data from two primary sources. First, we exploit administrative registers data from Statistics Denmark[2]. The administrative registers contain information on all

[2]Statistics Denmark website: http://www.dst.dk/en/OmDS

residents in Denmark, their labour status, income, schooling and education achievement, their immigration status, and their criminal record among others; and they have been updated on a yearly basis since 1980. Each resident is linked to a flat and a building with a corresponding move-in date. The move-out date of a dwelling is inferred from the move-in date to the next flat, which allows us to recover the full residential history of individuals.

The type of ownership, either public or private, is defined at the individual level. While the ownership type is usually homogenous at the building level, there are cases where both publicly and privately rented flats coexist in the same building. We treat as public (private) any building with mixed ownership with more than fifty per cent of publicly (privately)-owned flats at any time in the three years preceding the building closure. As our analysis focuses on the displacement of leasers in the public housing sector, we exclude privately-rented flats from our sample. Statistic Denmark does not provide any register that explicitly lists building demolitions or closures. However, the disappearance of a building from the register in a given year indicates that it was closed within the previous year (as most buildings that close end-up being demolished, in the rest of the paper we use closure and demolition interchangeably). We, therefore, treat any building that disappears from the register as closed. Over the period 1986-2012, 535 public housing buildings were closed. The size of closed buildings varies from single houses (one flat) to large housing blocks (up to 48 flats), with the average building consisting of 7.5 flats.

Second, we use predefined physically contiguous residential areas with at least 150 residents as neighbourhoods, as defined in Damm and Schultz-Nielsen (2008). These neighbourhoods were designed in 2004 and had to satisfy the following criteria: (i) each neighbourhood has to be conterminous with others, and its boundaries must either be natural demarcations, such as forests, rivers, and park areas; or non-housing infrastructures (e.g. industrial complexes, highway or railway area); (ii) they must belong to a unique geographic administrative unit such as ZIP codes or municipalities. These neighbourhoods contain on average 350 residents. In a recent Danish study, Hjorth and Dinesen (2016) asked a random sample of Danes to draw their self-perceived neighbourhoods on maps. By combining these data with the administrative register data, the authors found that the median-size neighbourhood drawn by residents consists of 479 residents, which is close to the average size of neighbourhoods designed by Damm and Schultz-Nielsen (2008). Thus, the definition of a neighbourhood we use approximately corresponds to the actual perception of a neighbourhood by Denmark residents. Note that these neighbourhoods were later incorporated in Statistics Denmark's registers; hence most of the registered buildings are attached to a neighbourhood.

Table A1 in the Appendix shows the comparative descriptive statistics of the working-age population (16-65 years old) living in demolished buildings to that of non-demolished public housings in the same neighbourhoods (i.e. only neighbourhoods with at least one building closure are included). Both populations are very similar on average. The share of immigrants and of individuals who followed a vocational educational track is slightly lower in demolished buildings (16 against 17 per cent and 21 against 22 per cent respectively). The employment rate is higher by one percentage point in demolished buildings (55 against 54 per cent), and the annual household income is larger in non-demolished buildings by DDK 3977 (USD 605). The household size is not statistically different across both samples, while households in demolished buildings have an extra 0.1 child. Finally, the average number of rooms per flat is almost identical in demolished and non-demolished buildings. The high level of similarity between both populations highlights that demolished buildings are representative of public housings conditional on a given neighbourhood.

## 2.2 Computation of the exposure

The core of this article is to investigate the impact of the exposure to other individuals one lives with. We define the exposure to neighbours (or peers) at the building level as the weighted population in the building that shares a given characteristic. The applied weight is based on the amount of time an individual spends with each of their neighbours. The exposure of individual $i$ to the characteristic $Z$ of their neighbours in building $b$ is computed as follows:

$$ExpZ_{ib} = \sum_{j}^{N_{ib}} \ell_{ij} \mathbb{1}_{\{Z_j=1\}}, \quad i \neq j \tag{1}$$

where $N_{ib}$ is the number of inhabitants in building $b$ during individual $i$'s residency[3], $\ell_{ij}$ is the number of days individuals $i$ and $j$ spent together in building $b$, and $\mathbb{1}_{\{Z_j=1\}}$ is equal to one if the variable $Z$ is equal to one for neighbour $j$. The fact that $ExpZ_{ib}$ is a function of both the number of neighbours and the length of residency spells $\ell$'s, implies that it is increasing in the population size of the building and in the tenure of individual $i$ in building $b$. Hence, people living in larger buildings will mechanically have a larger exposure than those living in smaller units. For instance, if someone stays for two years in a building with three other neighbours who all have jobs, their exposure to employed neighbours would be smaller than that of someone staying for the same amount of time with four employed neighbours in a hundred-person building. Similarly, the exposure of someone staying for one year with ten employed neighbours is equal to that of someone staying with one employed neighbour for ten years.

Our definition of exposure, measures an "absolute" exposure, as opposed to a standardised measure over the building population or the individual's tenure. The choice of this definition is motivated by the fact that both elements are important components of

[3] We exclude from $N_{ib}$ all the individuals who live in individual $i$'s flat, as household members are different from neighbours regarding how they interact with individual $i$.

networks. The population component, i.e. the number of different neighbours, accounts for the number of potential opportunities an individual can access to, whereas the tenure component accounts for the odd that two individuals in the same building interact (the longer people live together in the same building, the higher the probability that they meet and develop social ties). Standardising over the population size would result in defining the exposure as a weighted share of neighbours population. An individual living with five employed neighbours in a ten-person building would have the same value of exposure as someone living with fifty employed neighbours in a hundred-person building. However, these two situations are substantially different, in particular in regard to social interactions, since the second case involves significantly more potential encounters. This motivates our decision to take the building size into account in the definition of exposure. In the analysis that follows, we use different types of fixed-effects to attempt to disentangle the impact of both components.

# 3 Closure of Public Housing Blocks

## 3.1 Closure Process

Social housing in Denmark is managed by several public housing associations (PHAs), which all refer to a National Fund (Landsbyggefonden). PHAs are in charge of reviewing applications and allocating dwellings to claimants from the stock of available public housing units they manage, while ensuring a balanced budget. A PHA may decide to close a building for numerous reasons, including a low occupancy rate, or the deterioration of the building facilities or the surrounding environment, among others. To do so, he PHA first needs to assemble a file motivating the closure and submit it to the National Funds. The National Fund ensures the compliance of the closure request with the law and assesses the costs and benefits of the operation. If the National Fund validates the request, the case is passed onto the ministry in charge of housing, which carries out a second evaluation and renders a final decision. If the closure is allowed by the ministry, the PHA can start relocating the remaining tenants. According to the National Fund, the whole closure process can take up to three years but, on average, the relocation takes between one and two years.

The relocation of tenants is strictly regulated, and PHAs are required to provide leasers with a dwelling similar in size, price and location to the one they were occupying. However, relocation can also be made in a different type of dwelling if the situation of the household changed since they moved in (for instance, in the case of new children). Tenants are required to fill in a form indicating the actual characteristics of the household in order for the PHA to find a dwelling that best matches their needs. It is worth noting that only information relevant to the rehousing is asked, which does not include personal characteristics such as employment status and education. A PHA usually tries to rehouse households within their own housing stock, but it may also check availabilities in other PHAs' stocks if necessary. The length of the relocation period, therefore, hinges on the number of households to be rehoused and on the supply of corresponding flats. Thus, conditional on the household characteristics that determine the kind of flat they are entitled to, the time it takes for a household to be relocated is as random.

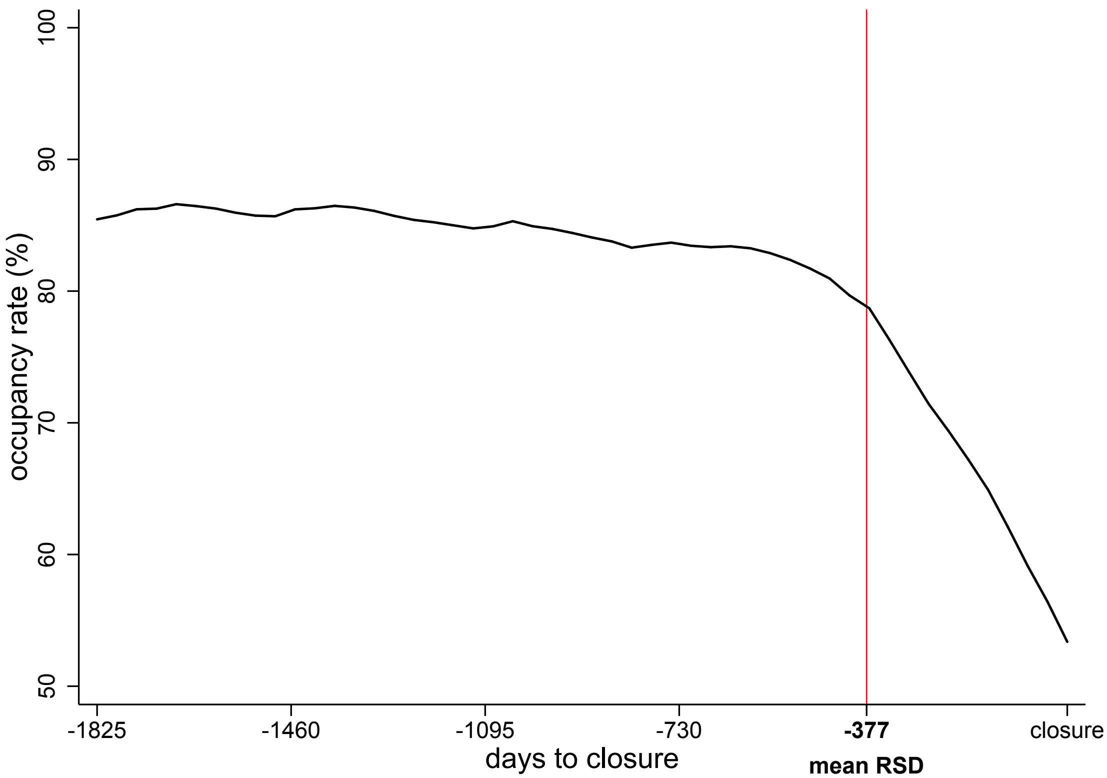
## 3.2 Identification of the Relocation Period

The date at which the PHA starts relocating tenants, from now on referred to as the relocation start date (RSD), is salient in our identification strategy, as it determines which households are forced to move out and how long they wait before being rehoused. The RSD is not available in Statistic Denmark's registers. However, it is possible to estimate it from the mobility pattern of tenants in demolished buildings following a similar approach to Jacob (2004). According to the Danish regulation of relocations in public housings, as soon as the PHA submits a closure request to the National Fund, the entry of new tenants in the building is frozen[4]. As a result, the occupancy rate of buildings that started a closure process remains constant before declining as relocations start. We first identify the closure date as the date of the last move out of the building. This approximation does not introduce any bias, as the emptiness of the building is a necessary condition for the closure. Once the closure date is identified, we work backward to find the date at which the occupancy rate starts dropping, i.e. the RSD of the building. More formally, the RSD is equal to the local maxima of the occupancy rate the closest to the closure date. Figure A1 in the Appendix illustrates how the RSD is estimated at the building level. The thick vertical black line represents the estimated RSD.

Figure 1 shows the kernel-weighted local polynomial regression of the occupancy rate on time over the five years preceding the closure for the closed buildings sample. The curve follows a slightly decreasing trend up to 420 days to the closure before sharply dropping. This kink in the occupancy rate is consistent with the beginning of the relocation of tenants. The vertical red line corresponds to the average RSD in our sample. On average, the RSD takes place slightly more than a year before closure (377 days), which is in line with the estimation of the National Fund (cf. section III.1). Note that buildings with less than five flats are excluded from the sample, as their occupancy rate is too sensitive to the move of a single household. We also exclude moves-in, which arose less than a year before the closure. Such moves correspond to temporary short-term tenancies of households who are waiting to be provided with a permanent housing solution, and who are not taken into account in the decision of the PHA to submit a closure request. There are no late in-movers in 57 per cent of buildings, and one and two late in-movers in 28 and 10 per cent of buildings respectively.

[4]Exceptions may apply and are mentioned later in this section.

Figure 1 – Occupancy Rate Over Time




Kernel-weighted local polynomial regression of demolished building occupancy rate on time to closure. The vertical red lines corresponds to the mean date at which the occupancy rate starts decreasing.

Figure A2 in the Appendix compares the change in occupancy rates over time of the full population to that where in-movers in the last year are excluded. Their inclusion shifts the kink in the occupancy rate to the right, i.e. reduces the average relocation period by about 90 days. The resulting relocation period is significantly smaller than that mentioned by the National Fund, which motivates the exclusion of such movers. In the rest of the paper, we refer to the sample that excludes late in-movers as the main sample.

In the analyses that follow, we restrict the sample to buildings for which the RSD lies between 2.5 years to 3 months to the closure date, which corresponds to 90 per cent of the sample of demolished housing blocks. This restriction is justified by the fact that RSDs outside of this window are unrealistic. Wrong estimations of RSDs may seriously bias the analysis. Underestimating the length of the relocation period would result in excluding some neighbours from the computation of the exposure. This could lead to downward estimates, as some variation in exposure would be unaccounted for, weakening the relationship with the dependent variable. On the other hand, overestimating the length of the relocation period would include in the analysis individuals whose relocation was not dependent on a rehousing offer. If such individuals have more resources and tend to relocate faster than other displaced households, a low post-RSD individual tenure, hence a low exposure, would be correlated with better performances later in life. This would bias estimated upward and overestimate the impact of exposure on labour market outcomes.

## 3.3 Exogeneity of Relocation

Our main identifying assumption relies on the exogeneity of the timing of relocations conditional on a set of observable characteristics. First, all individuals who are still in the building at the time of the RSD are considered as being displaced. It is worth noting that tenants are informed about the potential closure early in the process, as they are involved in the reflection around the relevance of shutting down the building. Thus, the sample of individuals who remain until they are rehoused is not random. However, within this group, the time individuals wait until they are given a relocation offer is exogenous conditional on the determinants of the availability of matching dwellings. Indeed, depending on the characteristics of a household, it may be more or less difficult for the PHA to find a flat that suits the household's needs.

In order to formally identify the drivers of relocation offers, we look at the correlation between the post-RSD tenure, i.e. the time it takes to rehouse a tenant from the RSD, on a set of individual characteristics. Under our main assumption that relocation offers are random conditional on characteristics which are observed by PHAs, we should not observe any significant correlations between tenants' characteristics and the timing of relocations. More specifically, we estimate the following regression:

$$RT_{ib} = \alpha + X_i'\beta + \gamma RP_b + \mu_n + \mu_T + u_{ibn} \tag{2}$$

where $RT_{ib}$), is the post-RSD tenure of individual $i$ in building $b$, $X_i$ is a vector of individual characteristics, $RP_b$ is length of the relocation period at the building level, i.e. the time it took to relocate all the tenants of building $b$, $\mu_n$ and $\mu_T$ are neighbourhood and year fixed-effects, and $u_{ibn}$ is the error term. The vector $X_i$ includes age, gender, immigration status, two education dummies, whether the person is employed, the logarithm of the household income, the household size, the number of children in the household and the number of rooms in the occupied flat. We also include a set of building characteristics measured at the time individual $i$ moved in building $b$. Those are meant to account for self-selection of individuals into specific buildings.

Table 1 presents the estimation of equation (2). No coefficient is significant with the exception of that on the household size. An additional household member is associated with an extra 6.9 days to the post-RSD tenure. This correlation indicates that larger families take longer to relocate. This could stem from the smaller stock of houses that can accommodate large households, or from the presence of children. Indeed, when children are involved, PHAs are supposed to find a dwelling in an area such that children are not forced to change school, which further reduces the size of the pool of eligible houses. The household size is a piece of information known by PHAs, as it is a major determinant of the type of accommodation people will be rehoused in. The result in Table 1 suggests that it is the only characteristic that impacts the time people wait to be relocated. It is worth noting that no coefficient on the characteristics unknown of PHAs, such as the employment status and the educational level, are significant. This confirms our initial assumption that, conditional on a set of observable characteristics, the post-RSD tenure is as random.

# 4 Empirical Strategy

## 4.1 Research Design

The core of the identification strategy is based on the households' mobility shock provoked by the closure of demolished buildings. While they are waiting to be rehoused, individuals in demolished buildings are exposed to the characteristics of their neighbours for an exogenous amount of time conditional on relocation determinants (cf. section III.3). We use the variation in the length of exposure to different neighbours to estimate a causal impact of neighbours' characteristics on individuals' labour market outcomes. The diagram in Figure 2 illustrates the main identification mechanism. Consider three individuals in a given building, A, B and C, who all wait to be relocated. We start following the residents from the date at which only households to be displaced remain in the building. This corresponds to time $t_0$ in Figure 2. The timing of $t_0$ is fully exogenous since it is based on the mobility pattern of other residents. As a result, the tenure of tenants in the building from $t_0$ to their relocation is (conditionally) orthogonal to their characteristics. The reason why we do not start measuring the exposure at the RSD is not to lose the observation of the first tenant to move out (whose tenure from the relocation date is zero by definition). Individual A is rehoused at time $t_1$ and is exposed to individuals B and C's characteristics for the period $[t_0, t_1]$. Individual B is the second one to move out and is exposed to individual C's characteristics for the period $[t_1, t_2]$. At $t_2$ individual C is the only tenant remaining in the

Table 1 – Determinants of Relocation Periods and Individual Tenure

| | Post-RSD tenure<br>(1) |
|---|---|
| Age | -0.00979 |
| | (0.185) |
| Female | -1.202 |
| | (3.810) |
| Immigrant | -2.961 |
| | (5.526) |
| Vocational educ. | 2.329 |
| | (5.397) |
| Post-secondary educ. | 11.39 |
| | (9.852) |
| Employed | 3.246 |
| | (4.925) |
| Criminal record | -37.59 |
| | (39.39) |
| Household size | 6.868** |
| | (2.880) |
| Number of children | -4.013 |
| | (3.730) |
| Household income | -1.58e-05 |
| | (1.64e-05) |
| Number of rooms | 3.959 |
| | (4.415) |
| Building relocation period length | -0.0381 |
| | (0.0472) |
| Building char. | YES |
| Year FE | YES |
| Neighbourhood FE | YES |
| Observations | 1,320 |

Linear Probability Model. Building characteristics include: share of females, share of immigrants, share of tenants with secondary, vocational and post-secondary education, employment rate, average age, average household income and the average number of rooms of flats. Significance levels: * 10%, ** 5%, *** 1%.

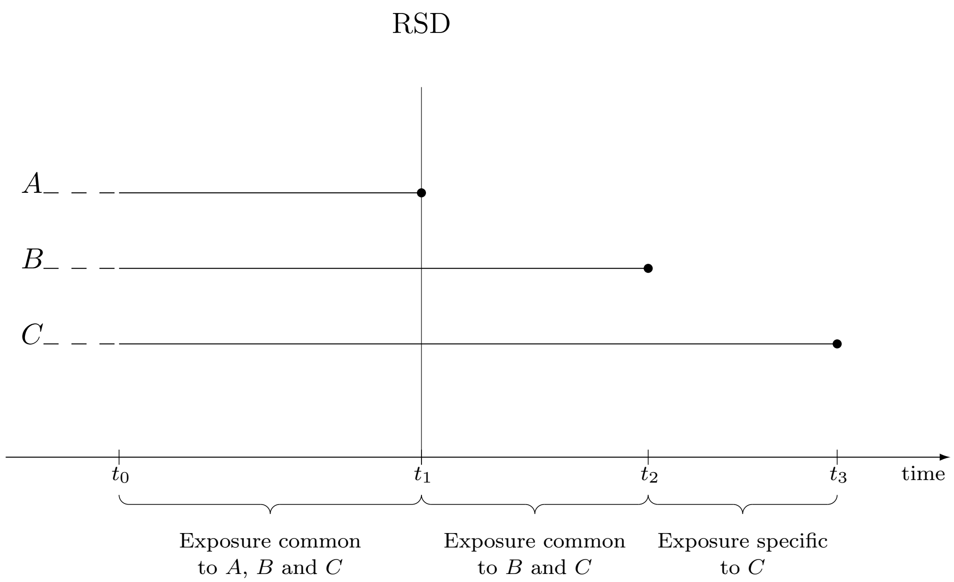
building and is no longer exposed to any neighbour. Thus, their exposure is similar to that of individual B.

This research design raises an important point. By starting to measure the exposure at $t_0$ we ignore the exposure to other neighbours before this date. This is due to the endogenous nature of the full tenure of individuals in their dwelling. The exposure measured from $t_0$, therefore, is a truncated version of the full exposure and might not capture the whole influence of neighbours on displaced individuals. The truncated exposure can be interpreted as a short-term exposure, as the average period over which it is measured is equal to 291 days compared to 6 years for the full tenure (Figures A3a and A3b in the Appendix show the distribution of the truncated and of the full tenures). Focusing on the truncated exposure on its own makes the implicit assumption that short-term and long-term exposures differ. In other words, the effect of someone one has been exposed to five years ago is different from the effect of someone one was exposed to six months earlier. It is similar to applying a time discount factor to the computation of exposure. It is noteworthy that the truncated tenure is fully exogenous from the full tenure, as it only depends on the post-RSD tenure, which is conditionally orthogonal to tenants' characteristics. In the analyses that follow, section V focuses on the impact of short-term exposure, while section VI uses an instrumental variable approach to estimate the effect of long-term exposure.

## 4.2 Specification

Our econometric specification explores the impact of individuals' exposure to their neighbours on their labour market performances after they were relocated to a different building. Exposure can be interpreted as the (potential) strength of a network of neighbours. Spending more time with neighbours with specific characteristics increases the chances to take advantage of those characteristics in the future. In the setting of Figure 2, individuals B

Figure 2 – Overlapping Exposure



and C benefit from a larger network than individual A, since they were exposed to other neighbours for a longer period of time. We restrict the sample to the working-age population (18 to 65), and we exclude individuals who moved less than a year before the building's closure for the reasons mentioned in section III.2. The main equation is defined as follows:

$$Y_{ibn} = \alpha + \beta ExpZ_{ib} + X'_{R,i}\gamma + X'_{MI,i}\eta + \lambda RP_b + \mu_n + \mu_T + \varepsilon_{ibn} \tag{3}$$

where $Y_{ibn}$ is a post-relocation outcome of individual $i$ in building $b$ in neighbourhood $n$, $ExpZ_{ib}$ is the exposure of individual $i$ to neighbours of type $Z$ in building $b$, $X_{R,i}$ is a vector of individual characteristics measured in the year of the RSD, $X_{MI,i}$ is a vector of individual and building characteristics measured at the time individual $i$ moved in building $b$, $\mu_n$ and $\mu_T$ are neighbourhood and year fixed-effects; and $\varepsilon_{ibn}$ is the error term. The vector $X_{R,i}$ aims to control for the determinants of relocation and includes age, age squared, gender, immigration status, two education dummies, the logarithm of the household income, the size of the household, the number of children in the household and the number of rooms in the flat. We also add the time individual $i$ spent in building $b$ before the exposure starts being measured (this corresponding to the period from the move-in date to $t_0$ in Figure 2).

Mobility has proven to be negatively associated with social capital at the neighbourhood level (Kan, 2007: David et al., 2010). Tenants with a longer pre-exposure tenure may have developed stronger links with their neighbours and be more sensitive to their exposure. If the drivers of mobility also affect labour market performance, failing to control for pre-RSD tenure would result in biased estimates of $\beta$. The inclusion of the length of the relocation period at the building level, $RP_b$, accounts for the differences in exposure that are due to the overall time needed to rehouse the whole building. A longer relocation period may reflect either a higher number of tenants to displace or a tied market on the supply side. In both cases, residents of such buildings may share common unobservable characteristics that also affect their labour market outcomes. That could be the case if inhabitants of large buildings live closer to dynamic economic areas and, therefore, have a higher average exposure and a higher propensity to find a job after displacement.

In the $X_{MI,i}$ vector we include the same variables as in $X_{R,i}$ and we add a set of building-level variables including the share of employed tenants, the share of tenants with basic, vocational and post-secondary education respectively, the share females, the share of immigrants, and the average household size, number of rooms and household income. The variables in vector $X_{MI,i}$ are meant to account for the potential self-selection of individuals into specific buildings (we further discuss this issue later in this section). All the coefficients in the next two sections are ordinary least square (OLS) estimates with standard errors clustered at the building level.

By interpreting $\beta$ as the causal effect of the exposure to the characteristic $Z$ of neighbours, we are making two key underlying assumptions about the exogeneity of the exposure. First, the post-RSD individual tenure needs to be exogenous. The plausibility of this assumption has been discussed in section III. This stems from the regulation of the relocation process in the public housing sector that requires displaced individuals to be provided with a dwelling similar to that they are expelled from. Thus, conditional on the determinants of relocation offers known by the PHA and by the researchers, the post-RSD tenure is as random. Therefore, the covariates in $X_{R,i}$ and $RP_b$ account for most of the endogeneity of the post-RSD tenure. Second, there is no self-selection in buildings once the neighbourhood and covariates in $X_{MI,i}$ have been controlled for. Since the exposure is a function of the post-RSD individual tenure and the characteristics of the neighbours, the selection of individuals into specific buildings based on unobservable characteristics that also affect future labour market outcomes would yield biased estimates of $\beta$. Indeed, if high-ability individuals are more likely to choose a flat in a building mainly populated with people who have jobs, a high exposure to employed neighbours may reflect ability, which is correlated with future performance on the labour market.

Two considerations motivate the plausibility of this second assumption. First, public housing buildings in Denmark - at least in large cities - are usually distributed over numerous neighbourhoods; thus, there are few areas with a very high concentration of public housing blocks. People willing to live in a specific neighbourhood, therefore, do not have much choice regarding the location of public housing blocks. Second, publicly available information on the tenants of a particular building is difficult to access. When looking for a place to settle in, individuals usually decide on a specific residential area to live in, and assume that their neighbours in their building are similar to the average inhabitants in the area. This assumption is further strengthened by the size of the neighbourhoods we are using, 150 households on average, which demarcates very specific areas. Bayer, Ross and Topa (2008) and Algan et al. (2016) rely on this key assumption to estimate causal effects at the block level. The former provides extensive evidence that individuals do not self-select into buildings once the neighbourhood they chose was accounted for. In order to test whether our second assumption holds in the Danish context, we study how individuals sort themselves into buildings. Table 2 shows the regression of building-level statistics on individuals' characteristics at the time new entrants move in the building. More specifically, we estimate the following equation:

$$\widehat{y}_{MI,b} = \alpha_0 + W_{MI,i}\beta_0 + \mu_n + \mu_T + \eta_{ibn} \tag{4}$$

where $\overline{y}_{MI,b}$ is the mean of characteristic $y$ in building $b$. To prevent any risk of overlap between the characteristics new entrants and that of the building's population, we measure $\overline{y}_{MI,b}$ in the year preceding the entry of individual $i$ in building $b$. $W_{MI,i}$ is a vector of individual characteristics measured at the time of moving in. Overall, individual characteristics only weakly explain residential sorting once neighbourhoods are taken into account. Being a female is positively associated with the share of females in the building (column 1) while the share of immigrants is correlated with gender, the household size and the number of children (column 2). There is no correlation between individual characteristics and the share of tenants with a vocational training education (column 3) and holding a post-secondary degree is positively associated with the share of post-secondary degree holders in the building (column 4). Finally, immigrants tend to be associated with buildings with a higher average household income (column 5) while noting is correlated to the building-level employment rate (column 6). As employment and income are is our main outcomes of interest, it is unlikely that our results are entirely driven by self-selection. Moreover, the inclusion of $X_{MI}$ in equation 2 should account for most of the potential endogeneity due to self-selection into buildings[5].

[5]Results in the following section are not sensitive to the inclusion of $X_{MI}$, which further confirm that the estimated coefficients are not driven by residential sorting.

Table 2 – Residential sorting of individuals at the building level

| | $\overline{Female}_b^{\star}$ | $\overline{Immigrant}_b^{\star}$ | $\overline{Vocational\ educ.}_b^{\star}$ | $\overline{Post\text{-}secondary\ Educ.}_b^{\star}$ | $\overline{Employed}_b^{\star}$ | $\overline{Household\ income}_b$ |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Age | 0.0275 | -0.0231 | 0.00335 | 0.0149 | -0.0408 | -54.00 |
| | (-0.021) | (-0.0312) | (-0.0185) | (-0.013) | (-0.0294) | (60.77) |
| Female | 1.270*** | 0.944* | 0.137 | -0.163 | -0.631 | -1,597 |
| | (-0.405) | (-0.549) | (-0.33) | (-0.204) | (-0.478) | (1,111) |
| Immigrant | 1.00 | 1.18 | 0.0214 | -0.416 | 0.587 | 3,487* |
| | (-0.726) | (-1.07) | (-0.707) | (-0.403) | (-0.833) | (2,019) |
| Vocational Educ. | -0.232 | -0.552 | 0.0532 | 0.37 | -0.268 | 1,323 |
| | (-0.574) | (-0.931) | (-0.58) | (-0.414) | (-0.958) | (1,929) |
| Post-secondary Educ. | 0.108 | -0.946 | 1.03 | 1.440* | 0.00829 | 3,020 |
| | (-0.762) | (-1.26) | (-0.859) | (-0.802) | (-1.49) | (3,463) |
| Employed | 0.303 | -0.0182 | 0.619 | -0.0897 | 0.212 | -1,589 |
| | (-0.429) | (-0.731) | (-0.43) | (-0.293) | (-0.629) | (1,763) |
| log(Household income) | 0.362 | 0.632 | -0.412 | -0.101 | -0.133 | -765.2 |
| | (-0.364) | (-0.578) | (-0.391) | (-0.213) | (-0.528) | (1,281) |
| Household size | 0.204 | 0.964** | -0.176 | 0.155 | 0.00606 | 203.6 |
| | (-0.368) | (-0.479) | (-0.331) | (-0.212) | (-0.508) | (1,503) |
| Number of children | -0.348 | -1.430** | -0.193 | 0.0971 | 0.121 | -324.4 |
| | (-0.399) | (-0.555) | (-0.399) | (-0.32) | (-0.68) | (1,711) |

$^{\star}$ coefficients multiplied by 100.

### 4.3 Outcomes

Our main outcomes model transition probabilities between employment status and are defined following Cappellari and Tatsiramos (2015). To keep the sample size as constant as possible, we investigate individuals' economic status in the first four years following their relocation. $Find\ Job_{i,y+j}$ is a binary variable that is equal to one if individual $i$ was unemployed when the relocation period started and employed in year $y+j$, where $y$ is the year they were rehoused. Hence, $Find\ Job_{i,y+j}$ represents the transition to employment of individual $i$ between the RSD and year $y+j$. It is noteworthy that this variable does not take into account the changes in employment status before time $y+j$. Hence, someone finding a job at $j=1$ and losing it at $j=3$ will be treated as not having transitioned to employment at $j=3$. Similarly, someone who finds a job at $y+2$ and maintains it over the years will be treated as having transitioned to employment for $j=2,3,4$. The descriptive statistics of $Find\ Job_{i,y+j}$ are shown in Table A2 in the Appendix. The share of individuals transitioning to employment increases from 10.3 to 12.6 per cent between the first and the fourth years after relocation[6]. This result indicates that the return to employment rate of displaced individuals increases over time.

Similarly, $Lose\ Job_{i,y+j}$ takes the value one if individual $i$ has a job when the relocation process starts and is unemployed in $j$ years after having been rehoused. The average rate of job loss increases from 7.8 to 9.1 per cent between the first and the fourth year after relocation. Note that this finding does not contradict an increasing rate of return to employment. Analysed together, these results highlight that the probability for someone to change their employment status increases over time. This could reflect a dynamic and flexible labour market, or underline the unstable nature of the population of interest (i.e.

[6] Note that $Find\ Job_{i,y+4}$ is equal to zero for someone who finds a job at $y+3$ and loses it at $y+4$. Thus $\widehat{FindJob}_{i,y+4}$ only accounts for individuals who are employed at time $y+4$, but it underestimates the population which was employed at some point since they relocated.

public housing beneficiaries).

Finally, *Change* $Income_{i,y+j}$ is defined as the change in the position of individual $i$ in the income distribution between the RSD and $j$ years after the relocation year $y$. The income distribution is expressed in 5-percentile units. Hence, *Change* $Income_{i,y+j} = 1$ indicates an upward shift in the income distribution by 5 percentiles. According to Table A2, displaced individuals experienced a decrease in their relative income (-0.57 unit or 2.85 percentiles) in the first year after their rehousing, before seeing their income increasing in the following years.

# 5 Short-Term Exposure and Labour Market Achievements Dynamics

## 5.1 Baseline Results

Exposure to neighbours may impact individuals' labour market outcomes through social interactions and networks. Using data from the 2006 Welfare Research Survey (SFI Survey in Denmark), Damm (2014) points out that 16.6 per cent of Danes rely on networks as their main channel to find a job, and 33.6 per cent of these networks are made of Danish acquaintances (as opposed to Danish relatives (19.1 per cent) and close friends (47.3 per cent)). Assuming that neighbours are identified as acquaintances, the exposure to one's neighbours could be interpreted as direct access to a specific network. Table 3 reports the results of the estimation of equation (3) for the three outcomes of interest. The first column shows the baseline effect of exposure, i.e. the combination of both the population and the time component (cf. section II.2). Columns 2 and 3 include number of neighbours and post-RSD individual tenure month-fixed-effects, respectively, to control for the different components of exposure. Controlling for the number of neighbours allows us to isolate the time dimension of the impact of exposure by reducing the population-related variations. Similarly, controlling for individual tenure isolates the population size component by attenuating the tenure-related variations.

Panel A reports the impact of the exposure on *Find* $Job_{y+t}$. The first row shows estimates of the exposure to employed neighbours[7]. The first three columns focus on individuals who found a job in the year following the relocation. The coefficient in column 3 is positive and statistically significant at the one per cent level, indicating that a higher exposure to employed neighbours increases the propensity to find a job in the year following the relocation. In terms of magnitude, a one-standard-deviation increase in exposure increases the odds of finding a job by 3.49 percentage points, which represents 11.4 per cent of the total standard deviation of the outcome. This result is driven by the population component of the exposure since the coefficients in the first two columns are not significant. This suggests that the quality of a network hinges more on the number of members than on the length of interactions with each of them. The effect persists two and three years after relocation (columns 6 and 9), although its magnitude slightly reduces over time. This indicates that the effect of exposure to employed neighbours fades away as time goes by after neighbours relocate in different buildings. A potential explanation is that neighbour-networks slowly collapse after people are no longer living together, which would suggest that neighbour networks are sensitive to the geographical distance between their members. This intuition is further strengthened by results for three and four years after relocation (columns 9 and 12), where the coefficient on exposure to employed neighbours further decreases before it disappears.

[7]We ignore self-employed neighbours in the computation of the exposure to employed peers.

Table 3 – Exposure and Employment and Income dynamics

| | $y+1$ | | | $y+2$ | | | $y+3$ | | | $y+4$ | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| **Panel A**: Dependent variable *Find job* | | | | | | | | | | | | |
| Exp. Employed | 0.0183 | 0.0145 | 0.0349*** | 0.0165 | 0.0171 | 0.0332** | 0.0279** | 0.0298 | 0.0325** | 0.0141 | 0.0123 | 0.0190 |
| | (0.0124) | (0.0204) | (0.0133) | (0.0118) | (0.0226) | (0.0144) | (0.0113) | (0.0225) | (0.0144) | (0.0127) | (0.0226) | (0.0168) |
| Exp. Unemployed | -0.0161 | -0.0212** | -0.0231 | -0.0369*** | -0.0309*** | -0.0444*** | -0.0297*** | -0.0264** | -0.0456*** | -0.0269** | -0.0251* | -0.0318** |
| | (0.0127) | (0.0102) | (0.0149) | (0.0118) | (0.0113) | (0.0138) | (0.0106) | (0.0113) | (0.0135) | (0.0106) | (0.0129) | (0.0152) |
| **Panel B**: Dependent variable *Lose job* | | | | | | | | | | | | |
| Exp. Employed | 0.00740 | -0.00753 | 0.00658 | -0.0163* | -0.0265 | -0.00939 | -0.00345 | -0.0123 | 0.0126 | -0.0242 | -0.0380 | -0.0184 |
| | (0.0115) | (0.0218) | (0.0128) | (0.00956) | (0.0169) | (0.0115) | (0.0126) | (0.0180) | (0.0145) | (0.0162) | (0.0236) | (0.0181) |
| Exp. Unemployed | 0.0133 | 0.0116 | 0.0282** | 0.0220* | 0.0248** | 0.0369*** | 0.0210 | 0.0132 | 0.0371** | 0.0235 | 0.0201 | 0.0368* |
| | (0.0125) | (0.00963) | (0.0114) | (0.0112) | (0.0104) | (0.0122) | (0.0135) | (0.0112) | (0.0145) | (0.0175) | (0.0144) | (0.0204) |
| **Panel C**: Dependent variable *Change income* | | | | | | | | | | | | |
| Exp. Employed | 0.283*** | 0.569*** | 0.426*** | 0.120 | 0.391* | 0.199 | 0.134 | 0.384** | 0.191 | 0.0562 | 0.173 | 0.242 |
| | (0.107) | (0.188) | (0.114) | (0.126) | (0.199) | (0.123) | (0.106) | (0.185) | (0.128) | (0.122) | (0.202) | (0.158) |
| Exp. Unemployed | -0.151 | -0.0560 | -0.241** | 0.0126 | 0.0911 | -0.0484 | -0.0423 | 0.0619 | -0.124 | 0.0502 | 0.183 | 0.113 |
| | (0.0982) | (0.0978) | (0.119) | (0.108) | (0.116) | (0.142) | (0.112) | (0.120) | (0.129) | (0.109) | (0.128) | (0.133) |
| Number of Neighbours FE | | X | | | X | | | X | | | X | |
| Post-RSD tenure month FE | | | X | | | X | | | X | | | X |
| Year FE | X | X | X | X | X | X | X | X | X | X | X | X |
| Neighbourhood FE | X | X | X | X | X | X | X | X | X | X | X | X |

$N_{y+1} = 1,326$, $N_{y+2} = 1,299$, $N_{y+3} = 1,283$, $N_{y+4} = 1,269$. Linear Probability Model. Standard errors clustered at the building level are in parentheses. Significance levels: * 10%, ** 5%, *** 1%.

The second row in Panel A shows the coefficients on the exposure to unemployed neighbours. The coefficients are negative and significant across all specifications, except in columns 1 and 3. This indicates that both the time and the population components of exposure matter. However, the magnitude of the coefficients is systematically higher in specifications that include tenure fixed-effects, suggesting that the number of neighbours remains the main driver of the observed effect. A one-standard-deviation increase in the exposure to unemployed peers reduces the chances to find a job by 7.9 to 17 per cent of the standard deviation of the outcome. The effect peaks two years after relocation before slowly decreasing. Exposure to unemployed neighbours can be interpreted as an irrelevant network, where members have few or no opportunities to share. Results in the second row of Table 3, therefore, insinuate that the lack of a proper network has a detrimental impact on successful job searches. This could stem both from a lack of opportunities and of motivation with members failing to encourage each other in their job search.

Panel B examines the impact of the exposure variables on the transition into unemployment. While the exposure to employed neighbours does not affect the probability to become unemployed (with the exception of column 4), exposure to unemployed neighbours significantly increases the chance of job loss. With the exception of $y + 2$, the parameter estimates are only significant in the tenure fixed-effect specification. Individuals who lose their job after having been rehoused appear to be less likely to go back into employment if they are exposed to unemployed neighbours. These findings corroborate those in Panel A, where exposure to unemployed peers has an adverse effect on finding a job.

Finally, Panel C reports the effects of exposure on income dynamics. A one-standard-deviation increase in the exposure to employed neighbours is associated with an increase in $\textit{Income Change}_{y+1}$ by 0.28 to 0.57 unit, which corresponds to an upward shift in the income distribution by 1.4 to 2.8 percentiles. The magnitude of the coefficients is highest for the number-of-neighbours-fixed-effect specification, revealing that the time component is more substantial in income growth. The time-component of exposure also appears to have a longer-lasting effect, as shown by the significant coefficients in columns 5 and 8. This finding suggests that the rise in income is not mainly driven by the return to employment observed in Panel A, since the latter was predominantly attributed to the population component of exposure. A potential explanation could be that the increase in income results from interactions with a subset of financially literate neighbours. We further investigate this channel in section 5.3. The weak correlation between employment and income might be explained by the generosity of the employment benefits in Denmark, which cover for a significant share of the lost salary.

## 5.2 Effects by Subgroups

In addition to spatial proximity, social interactions and networks are also formed along shared characteristics. Network effects have proven to be heterogeneous across gender (Bortnick and Ports, 1992; Marmarosa and Sacerdote, 2002) and occupations (Patel and Vella, 2013). In Denmark, Damm (2014) finds that immigrants' employment is positively associated with the skill level of other immigrants living in the same neighbourhood. In France, public housing blocks with a high level of ethnic heterogeneity have proven to be less efficient at preventing vandalism, due to the higher cost of social interactions among people from different ethnic and cultural groups (Algan et al., 2016). In this section, we explore the heterogeneous effect of exposure to employed neighbours across occupation, education and gender. It is noteworthy that, in the three sub-section that follow, the exposure to unemployed neighbours is included in all the regressions, although the coefficients are not reported in the tables (detailed results are available on request).

*Job Occupation Type*

Table 4 presents the impact of the exposure to neighbours with different level of occupation skills, namely low-skills, medium-skills and high-skills, on the probability to find a job [8]. Panel A shows the estimates of the exposure to low-skills employed neighbours. The coefficients are positive and significant for in the first three years after demolition for the time fixed-effect specification (columns 3, 6 and 9). Similar results are observed in Panel B with the exposure to medium-skills employees. However, in Panel C, only the coefficient on the exposure to high-skills employed neighbours is significant at the 10 per cent level in the first year after demolition (column 3), and is of lower magnitude than these on the two other occupation levels. These results indicate that low and medium-skills-networks are longer-lasting than high-skills-members-based networks. As most of our sample is made of low, medium and other-skills workers, our finding may reflect that networks are stronger among individuals sharing similar occupations. Another interpretation is that high-skills jobs are more scarce and subject to a more sophisticated hiring processes than for lower-skilled jobs, which gives less room for social interaction to make a difference.

[8] In addition to those three levels of occupation, Statistics Denmark also identifies a fourth category labelled as "other skills", which includes all occupations which are not identified as either low, medium or high skills. We excluded this category from Table 3 as it is very heterogeneous.

Table 4 – Exposure to Employed Neighbours by Skills

| | $y+1$ | | | $y+2$ | | | $y+3$ | | | $y+4$ | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Dependent variable: *Find job* | | | | | | | | | | | | |
| **Panel A** | | | | | | | | | | | | |
| Exp. Low-Skills Employee | 0.0188 | 0.0180 | 0.0297** | 0.0218* | 0.0269 | 0.0384*** | 0.0233** | 0.0223 | 0.0220* | 0.0164 | 0.0197 | 0.0215 |
| | (0.0114) | (0.0221) | (0.0119) | (0.0112) | (0.0221) | (0.0123) | (0.0107) | (0.0241) | (0.0131) | (0.0128) | (0.0228) | (0.0144) |
| **Panel B** | | | | | | | | | | | | |
| Exp. Middle-Skills Employee | 0.0243** | 0.0268 | 0.0357*** | 0.0135 | 0.0117 | 0.0258* | 0.0239** | 0.0302 | 0.0288** | 0.0171 | 0.0222 | 0.0212 |
| | (0.0116) | (0.0196) | (0.0128) | (0.0114) | (0.0194) | (0.0150) | (0.0102) | (0.0206) | (0.0136) | (0.0104) | (0.0189) | (0.0145) |
| **Panel C** | | | | | | | | | | | | |
| Exp. High-Skills Employee | 0.0177 | 0.0117 | 0.0250* | 0.0198 | 0.0236 | 0.0225 | 0.0141 | 0.0132 | 0.0228 | 0.00374 | 0.00472 | 0.00368 |
| | (0.0141) | (0.0186) | (0.0147) | (0.0153) | (0.0200) | (0.0161) | (0.0155) | (0.0210) | (0.0160) | (0.0139) | (0.0169) | (0.0159) |
| Number of Neighbours FE | | X | | | X | | | X | | | X | |
| Post-RSD tenure month FE | | | X | | | X | | | X | | | X |
| Year FE | X | X | X | X | X | X | X | X | X | X | X | X |
| Neighbourhood FE | X | X | X | X | X | X | X | X | X | X | X | X |

$N_{y+1} = 1,326$, $N_{y+2} = 1,299$, $N_{y+3} = 1,283$, $N_{y+4} = 1,269$. Linear Probability Model. The exposure to unemployed neighbours is not reported (available on request). Standard errors clustered at the building level are in parentheses. Significance levels: * 10%, ** 5%, *** 1%.

*Education*

Table 5 reports the effect of exposure to employed neighbours with different education levels on the transition into employment. Being exposed to employed neighbours with a basic education has a positive and significant impact on the probability to find a job over the three years following the relocation (columns 1 to 9 in Panel A). No effect is observed for the exposure to employed neighbours with vocational training or post-secondary education with the exception of column 3 in Panel C. These results are in line with those of Table 4 as workers with basic education are over-represented in low and medium-skills jobs.

Table 5 – Exposure to Employed Neighbours by Education Levels

| | $y+1$ | | | $y+2$ | | | $y+3$ | | | $y+4$ | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Dependent variable: *Find job* | | | | | | | | | | | | |
| **Panel A** | | | | | | | | | | | | |
| Exp. Basic Educ. Employee | 0.0268** | 0.0310* | 0.0341*** | 0.0221* | 0.0283 | 0.0349*** | 0.0400*** | 0.0457** | 0.0411*** | 0.0170 | 0.0109 | 0.0152 |
| | (0.0116) | (0.0182) | (0.0124) | (0.0115) | (0.0195) | (0.0127) | (0.0110) | (0.0187) | (0.0123) | (0.0125) | (0.0198) | (0.0149) |
| **Panel B** | | | | | | | | | | | | |
| Exp. Vocational Educ. Employee | 0.00442 | -0.00122 | 0.0156 | 0.00292 | -0.00314 | 0.0169 | 0.00931 | 0.00823 | 0.00646 | 0.00706 | 0.00631 | 0.0119 |
| | (0.0101) | (0.0142) | (0.0120) | (0.0102) | (0.0153) | (0.0124) | (0.00923) | (0.0142) | (0.0137) | (0.0115) | (0.0139) | (0.0148) |
| **Panel C** | | | | | | | | | | | | |
| Exp. Post-Secondar. Educ. Employee | 0.0116 | -0.00485 | 0.0249* | 0.0162 | 0.0133 | 0.0253 | 0.00520 | -0.00281 | 0.0125 | -0.00677 | -0.0141 | 0.00198 |
| | (0.0117) | (0.0154) | (0.0132) | (0.0115) | (0.0166) | (0.0155) | (0.0104) | (0.0164) | (0.0123) | (0.0110) | (0.0146) | (0.0136) |
| Number of Neighbours FE | | X | | | X | | | X | | | X | |
| Post-RSD tenure month FE | | | X | | | X | | | X | | | X |
| Year FE | X | X | X | X | X | X | X | X | X | X | X | X |
| Neighbourhood FE | X | X | X | X | X | X | X | X | X | X | X | X |

$N_{y+1} = 1,326$, $N_{y+2} = 1,299$, $N_{y+3} = 1,283$, $N_{y+4} = 1,269$. Linear Probability Model. The exposure to unemployed neighbours is not reported (available on request). Standard errors clustered at the building level are in parentheses. Significance levels: * 10%, ** 5%, *** 1%.

*Gender*

Finally, Table 6 examines the heterogeneous impact of the exposure to employed neighbours across genders. Panel A regresses the probability to find a job on the exposure to female employees and its interaction with a *Female* dummy. The parameter estimates in the first row show the effect of the exposure to female employees on male (i.e. on the excluded category). With the exception of the coefficient in column 9 that is significant at the 10 per cent level, the transition to employment of men appears to be unaffected by the female-side of their network. When it comes to women, the coefficient on the interaction term is positive and significant over the two years following the relocation (columns 1 to 6), with a slightly decreasing magnitude between $y + 1$ and $y + 2$. This highlights a gender-bias in the functioning of networks, with only women benefiting from their exposure to female employees. This could reflect a higher level of social interactions among female neighbours. Another channel could be that occupations are highly gender-segregated in Denmark, preventing women to provide male workers with attractive employment opportunities.

Panel B shows the effects of the exposure to male employed neighbours. Baseline estimates in the first row represent the impact on females, while the interaction terms in the second row accounts for the impact on males. Exposure to male employed neighbours seems to only benefit women, as no significant coefficients are observed in the second row. These results suggest that only women are using their networks as a job-finding tool.

Table 6 – Exposure to Employed Neighbours by Gender

| | $y+1$ | | | $y+2$ | | | $y+3$ | | | $y+4$ | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Dependent variable: *Find job* | | | | | | | | | | | | |
| **Panel A** | | | | | | | | | | | | |
| Exp. Female Employee | -0.000310 | -0.0139 | 0.0155 | 0.00253 | -0.0101 | 0.0211 | 0.0211 | 0.0181 | 0.0277* | 0.00581 | 0.00562 | 0.00987 |
| | (0.0127) | (0.0174) | (0.0153) | (0.0138) | (0.0213) | (0.0173) | (0.0129) | (0.0200) | (0.0157) | (0.0136) | (0.0205) | (0.0178) |
| Exp. Female Employee × Female | 0.0251*** | 0.0234** | 0.0266*** | 0.0187* | 0.0232** | 0.0192* | 0.0140 | 0.0174 | 0.0171 | 0.0134 | 0.0127 | 0.0148 |
| | (0.00927) | (0.0108) | (0.00909) | (0.0110) | (0.0117) | (0.0108) | (0.0120) | (0.0120) | (0.0122) | (0.0125) | (0.0119) | (0.0127) |
| **Panel B** | | | | | | | | | | | | |
| Exp. Male Employee | 0.0237* | 0.0283 | 0.0370*** | 0.0183 | 0.0278 | 0.0295** | 0.0259** | 0.0238 | 0.0277* | 0.0152 | 0.00954 | 0.0196 |
| | (0.0134) | (0.0205) | (0.0132) | (0.0126) | (0.0214) | (0.0146) | (0.0122) | (0.0221) | (0.0149) | (0.0137) | (0.0216) | (0.0171) |
| Exp. Male Employee × Male | -0.0100 | -0.0125 | -0.0119 | -0.00369 | -0.00992 | -0.00374 | -0.00836 | -0.0147 | -0.0105 | -0.00609 | -0.00943 | -0.00716 |
| | (0.0110) | (0.0126) | (0.0107) | (0.0121) | (0.0127) | (0.0123) | (0.0124) | (0.0125) | (0.0126) | (0.0139) | (0.0140) | (0.0142) |
| Number of Neighbours FE | | X | | | X | | | X | | | X | |
| Post-RSD tenure month FE | | | X | | | X | | | X | | | X |
| Year FE | X | X | X | X | X | X | X | X | X | X | X | X |
| Neighbourhood FE | X | X | X | X | X | X | X | X | X | X | X | X |

$N_{y+1} = 1,326$, $N_{y+2} = 1,299$, $N_{y+3} = 1,283$, $N_{y+4} = 1,269$. Linear Probability Model. The exposure to unemployed neighbours is not reported (available on request). Standard errors clustered at the building level are in parentheses. Significance levels: * 10%, ** 5%, *** 1%.

### 5.3 Exposure to income levels

In this section, we explore the effects of the exposure to neighbours with different levels of incomes. As shown by Haliassos et al. (2019), exposure to financially literate neighbours increases financial knowledge and spurs investment in private retirement accounts and stockholding. Assuming that financially literate individuals are more represented in the upper tail of the income distribution, exposure to wealthier neighbours could affect the financial behaviour of displaced individuals. Table 7 reports the regressions of the change in income on the exposure to neighbours lying in the bottom (top) 50 per cent of the income distribution. A one-standard-deviation increase in the exposure to neighbours in the lower tail of the income distribution is associated with a reduction in the change in income by 0.223 to 0.363 unit, which corresponds to a downward shift in the income distribution by 1.1 to 1.8 percentile. The effect seems to persist over time, even though no impact is observed at $y + 2$. The impact of the exposure to neighbours of the first half of the income distribution is shown in the second row. The coefficients are positive and significant in all but one specification (column 4). The magnitude of the coefficients is relatively constant over the four years following the relocation, which underlines a long-lasting effect. The impact of exposure is associated with an upward shift in the income distribution ranging from 1.7 to 4 percentiles. The higher magnitude of the coefficients in the population-fixed-effect specifications (columns 2, 5, 8, 11) suggests that the impact is mostly driven by the length of the interactions with neighbours rather than by their number. This is in line with Haliassos et al. (2019) findings, as the transmission of financial knowledge probably is a long-time process.

One could suspect that these results are driven by the effect of the exposure to income on employment. Table A4 in the Appendix regresses the probability to find a job on the exposure to income. Exposure to poorer neighbours weakly reduces the chances to find a job. Remaining unemployed could affect income from transitioning from unemployment benefits to social assistance [9], which would explain the results in Table 6. However, no relationship between exposure to high-income neighbours and the increased probability to find a job is observed.

[9] Until 2010, unemployment benefits were awarded for four years to eligible applicants. It was then reduced to 2 years.

Table 7 – Exposure to Neighbours' Incomes

| | $y+1$ | | | $y+2$ | | | $y+3$ | | | $y+4$ | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Dependent variable: *Change income* | | | | | | | | | | | | |
| Exp. Income bottom 50% | -0.223* | -0.193 | -0.259* | -0.0835 | -0.0239 | -0.271 | -0.215* | -0.109 | -0.363** | -0.250** | -0.152 | -0.277 |
| | (0.121) | (0.133) | (0.146) | (0.128) | (0.136) | (0.181) | (0.125) | (0.135) | (0.160) | (0.120) | (0.156) | (0.175) |
| Exp. Income top 50% | 0.407*** | 0.787*** | 0.485*** | 0.223 | 0.548** | 0.382** | 0.350** | 0.716*** | 0.436*** | 0.387** | 0.561** | 0.521*** |
| | (0.153) | (0.231) | (0.151) | (0.157) | (0.246) | (0.154) | (0.148) | (0.231) | (0.151) | (0.152) | (0.258) | (0.176) |
| Number of Neighbours FE | | X | | | X | | | X | | | X | |
| Post-RSD tenure month FE | | | X | | | X | | | X | | | X |
| Year FE | X | X | X | X | X | X | X | X | X | X | X | X |
| Neighbourhood FE | X | X | X | X | X | X | X | X | X | X | X | X |

$N_{y+1} = 1,326$, $N_{y+2} = 1,299$, $N_{y+3} = 1,283$, $N_{y+4} = 1,269$. Linear Probability Model. Standard errors clustered at the building level are in parentheses. Significance levels: * 10%, ** 5%, *** 1%.

# 6 Long-Term Exposure and Labour Market Achievements Dynamics

The previous analyses have focused on short-term exposure. However, some individuals lived in the building for a much larger amount of time; thus, they were exposed to more neighbours over a longer period. Looking at the exposure during the whole period of residency would give us a picture of the whole network of neighbours of displaced individuals. As it was previously explained, the whole period of residency is endogenous, since individuals chose to stay in the building until they were relocated. As mobility is usually the consequence of a change in the socio-economic conditions or the preferences of a household (Knapp et al., 2001; Ferreira et al., 2010), there are reasons to believe that what drives the decision to stay in a specific house, hence the average tenure in a dwelling, also affects labour market outcomes. People with longer residencies at the same place could do so either because their household situation is stable, or because they cannot afford to move out. In both cases, such situations are directly linked to their employment status and their ability to find a lose a job. To overcome this identification issue, we use an instrumental variable approach where we the short-term exposure is used as an instrument for the long-term exposure (i.e. the exposure over the whole period of residency in the demolished building). More specifically, we estimate the following two-stage least squares system (2SLS):

$$LTExpZ_{ib} = a_0 + b_0 STExpZ_{ib} + X'_{R,i}c_0 + d_0 RP_b + \theta_T + u_{ib} \quad (5)$$

$$Y_{ibn} = a_1 + b_1 LTExpZ_{ib} + X'_{R,i}c_1 + d_1 RP_b + \theta_T + v_{ib} \quad (6)$$

where $LTExpZ_{ib}$ is the long-term exposure of individual $i$ in building $b$ as defined in section IV.1. To qualify as an instrument, the short-term exposure needs to be correlated with the long-term exposure but uncorrelated with any individual characteristics that could also affect labour market performance. The "first-stage" condition is likely to hold, as the instrument is a linear combination of the endogenous variable[10]. In regard to the exogeneity assumption, we proved in section IV that the short-term exposure was orthogonal to individuals' characteristics conditional on the information PHAs use to rehouse tenants. Hence, $STExpZ_{ib}|X_{R,i} \perp\!\!\!\perp v_{ib}$, which satisfies the exogeneity assumption. We could not include population or tenure fixed-effects in this specification as they were taking too much power from $STExpZ_{ib}$ in the first stage. Thus, the estimates in the following table represent the impact of the combination of the population and of the tenure component of exposure.

The estimations of equation 6 are reported in Table 8. Panel A shows the impact of the long-term exposure on the probability to find a job. The coefficient on the exposure to employed neighbours is positive and significant over the four years following relocation, indicating a longer-lasting effect than that of short-term exposure (cf Table 3). The magnitude of the coefficients is two to three times larger than in the short-term exposure specification, which reveals that previous exposure, i.e. exposure to neighbours who no longer live in the building, still affects the success of job search. A similar pattern is observed on the effect of the exposure to unemployed neighbours. Decomposing the exposure between ancient and current neighbours would inform us of the contribution of each group to the global effect, but it is not possible with our current identification strategy. It remains a lead worth exploring in future research.

Panel B shows the impact of exposure on the probability to lose one's job. The coefficients are only significant two years after relocation (column 2). Similarly, in Panel C, the effect of the exposure to employed and unemployed neighbours on income is only significant in the first year after the relocation. Conversely to the findings in Panel A, these two results

[10] $LTExpZ_{ib} = h_{ib} + STExpZ_{ib}$, where $h_{ib}$ is the exposure of individual $i$ between the day they moved in building $b$ and the RSD.

Table 8 – Long-Term Exposure

| | $y+1$ | $y+2$ | $y+3$ | $y+4$ |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| **Panel A**: Dependent variable *Find Job* | | | | |
| LT Exp. Employed | 0.0624* | 0.0908** | 0.103*** | 0.0693** |
| | -0.037 | -0.0354 | -0.0339 | -0.0332 |
| LT Exp. Unemployed | -0.0581 | -0.126*** | -0.105*** | -0.0896** |
| | -0.0415 | -0.041 | -0.0373 | -0.0352 |
| Kleibergen-Paap F-test | 67.42 | 65.84 | 69.85 | 66.37 |
| **Panel B**: Dependent variable *Lose Job* | | | | |
| LT Exp. Employed | -0.00647 | -0.0674* | -0.0399 | -0.0855 |
| | -0.0383 | -0.0355 | -0.0433 | -0.0551 |
| LT Exp. Unemployed | 0.0429 | 0.0769* | 0.07 | 0.0831 |
| | -0.0434 | -0.0397 | -0.0482 | -0.0621 |
| Kleibergen-Paap F-test | 67.42 | 65.84 | 69.85 | 66.37 |
| **Panel C**: Dependent variable *Change income* | | | | |
| LT Exp. Employed | 0.804** | 0.224 | 0.339 | 0.042 |
| | -0.336 | -0.373 | -0.344 | -0.346 |
| LT Exp. Unemployed | -0.564* | 0.0101 | -0.179 | 0.136 |
| | -0.327 | -0.364 | -0.372 | -0.354 |
| Kleibergen-Paap F-test | 67.36 | 66.19 | 69.85 | 66.84 |
| Observations | 1320 | 1295 | 1278 | 1266 |

All specifications include neighbourhood and year fixed effects. Standard errors clustered at the building level are in parentheses. Significance levels: * 10%, ** 5%, *** 1%.

suggest that exposure depreciates over time as the short-term effect seems diluted by the inclusion the exposure to former neighbours. A potential explanation could be that the mechanisms behind the relationship between exposure and the transition into employment are different from those behind job loss and income. These are areas for future research.

In summary, looking at the effect of long-term exposure on labour and income dynamics revealed that the quality of a network is based on the accumulation of social interactions with different neighbours and the maintenance of these relationships over time. Spending more time with the same individuals increases the chances of interactions, which can then lead to employment opportunities.

# 7 Robustness Checks

The above analysis is grounded on the crucial assumption that the post-RSD individual tenure is orthogonal to individuals' characteristics once the drivers of relocation known by the PHA have been controlled for. To further test the veracity of this hypothesis, we look at the relationship between the post-RSD individual tenure and the characteristics of the buildings people relocate to. If dwellings are solely allocated based on the housing market availabilities, there should be no correlation between the post-RSD individual tenure and the type of building displaced individuals are rehoused in. Conversely, a correlation between the post-RSD tenure and the characteristics of the relocation building would suggest that some tenants with specific characteristics are more likely to end up in particular buildings. If it is the case, one could not rule out that the observed results in the previous sections are partly driven by the post-relocation environment displaced individuals live in. For instance, if people who wait longer before being rehoused are provided with a dwelling in a building with a higher share of employed tenants, the positive association between exposure to employed neighbours and employment could reflect the better opportunities provided by the post-relocation environment. This would result in upward biases in the estimates of tables 3 to 8.

To do so, we estimate the following equation.

$$\overline{y}_{REL,ib} = a + bRT_{ib} + X'_{R,i}c + \theta_n + \theta_T + e_{ibn} \tag{7}$$

where $\overline{y}_{REL,b}$ is the mean of characteristic $y$ in building $b$ where individual $i$ was relocated to, and $\theta_n$ is a fixed-effect of the neighbourhood individual $i$ was displaced from. Table 9 reports the estimates of $b$ for five building characteristics: the share of female tenants, the share of tenants who did vocational training, the share of tenants who have a post-secondary education degree, the employment rate, and the average household income. No coefficient is significant throughout the five columns, indicating that there is no correlation between the amount of time people wait to be rehoused, and the type of neighbours they are relocated with. This result confirms the initial hypothesis that the post-RSD tenure is as random, conditional on the characteristics in vector $X_{R,i}$, thus the unbiasedness of the estimates of the first eight tables.

Table 9 – Robustness Check: Post-RSD tenure and relocation building's characteristics

| | Females | Vocational Educ. | Post-Secondary Educ. | Employment | Household Income |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| POSTRSDTEN | -0.00101 | 0.00509 | 0.00979 | -0.00420 | -2,718 |
| | (0.0162) | (0.0125) | (0.00756) | (0.0189) | (5,185) |
| Observations | 1,287 | 1,287 | 1,287 | 1,287 | 1,287 |

*Notes*: coefficient multiplied by 100. All specifications include neighbourhood and year fixed effects. Standard errors clustered at the building level are in parentheses. Significance levels: * 10%, ** 5%, *** 1%.

# 8 Conclusion

This paper aims to estimate the impact of exposure to neighbours on employment and income dynamics among public housing beneficiaries in Denmark. Using administrative register data, we design a novel definition of exposure as a function of both the number and the characteristics of building neighbours, as well as of the time spent with each of them. Our identification strategy is based on the conditional exogeneity of the timing of relocation offers to displaced households. This helps us address the bias related to endogenous sorting and tenure of individuals in specific buildings.

We find that short-term exposure to employed neighbours increases the chance for unemployed tenants to find a job in the three years following their relocation by 3 percentage point in average. On the other hand, exposure to unemployed neighbours is found to have a detrimental effect on the return to employment. In terms of income, exposure to employed neighbours increases individuals' position in the income distribution by 2 percentiles in average. We show that these effects are mainly driven by the number of neighbours one is exposed to, rather than by the amount of time spent with them. Refining the analysis to sub-categories of neighbours, we provide evidence that the effect of the exposure to employed neighbours is mainly driven by peers with basic education and low and medium-skills jobs. This suggests that building-level networks are stronger among people with similar social characteristics. In terms of genders, no network effect is found for men. In regard to income, exposure to neighbours lying in the top 50 per cent of the income distribution significantly increases future income.

The second part of the paper extends the analysis to the impact of the long-term exposure, i.e. the exposure measured over the whole residency spell in demolished buildings. We follow an instrumental variable approach, using the short-term exposure as an instrument for the long-term exposure in order to estimate a causal effect. We find that long-term exposure has a more substantial impact than short-term exposure, with estimates of greater magnitude. This suggests that the strength of neighbours-based networks hinges on the accumulation of interactions and their maintenance over time.

As the individuals in our sample are representative of the average dwellers in social housings in Denmark, this allows us to generalise our results to the whole population living in public housing buildings in those specific areas. In terms of policy implications, this paper demonstrates that social-segregation of individuals may have adverse spill-overs, as the exposure to unemployed neighbours further impedes the chance to find a job. Taking job status into account when allocating public dwelling to ensure a proper social mix at the building level, could therefore significantly benefit Danish tenants in the public housing sector, and the Danish society in general.

Our findings call for future research on the channels of network formation and development at the building level. Extending this analysis with survey data about individuals' attitude towards their neighbours and their social interaction at the block level would be a potential way to further exploring the mechanisms that drive the exposure effects. Including the exposure to the close surrounding would also be informative, as it would shed light on the size of local networks.

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# Appendix

Figure A1 – Estimation of the Relocation Start Date (RSD)

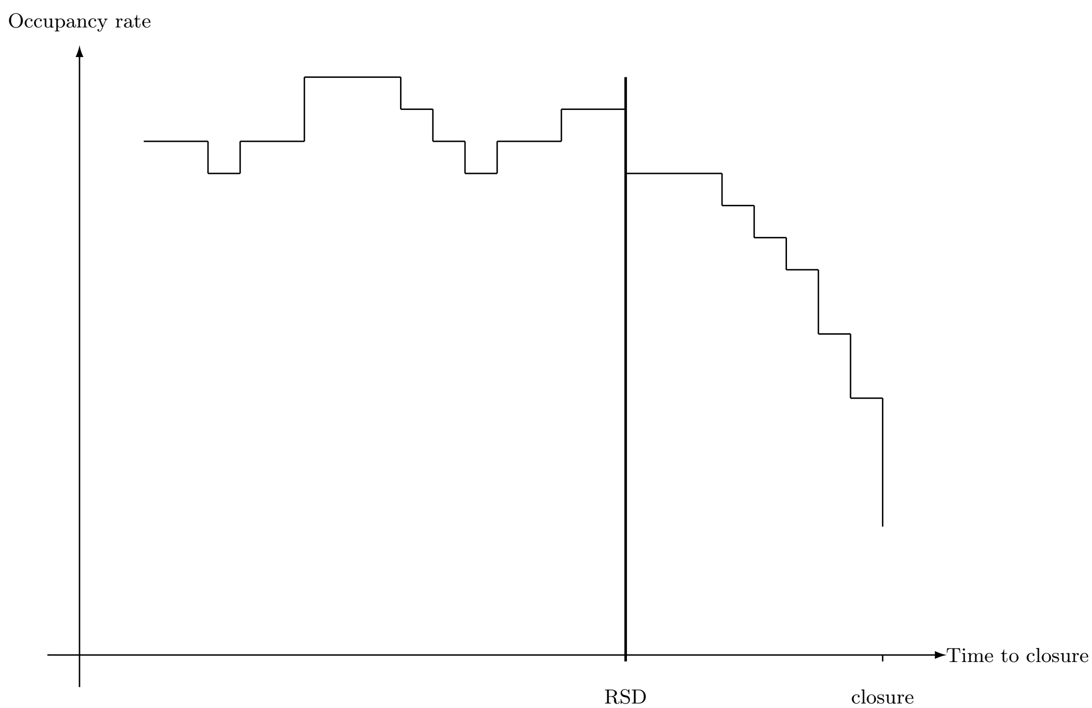
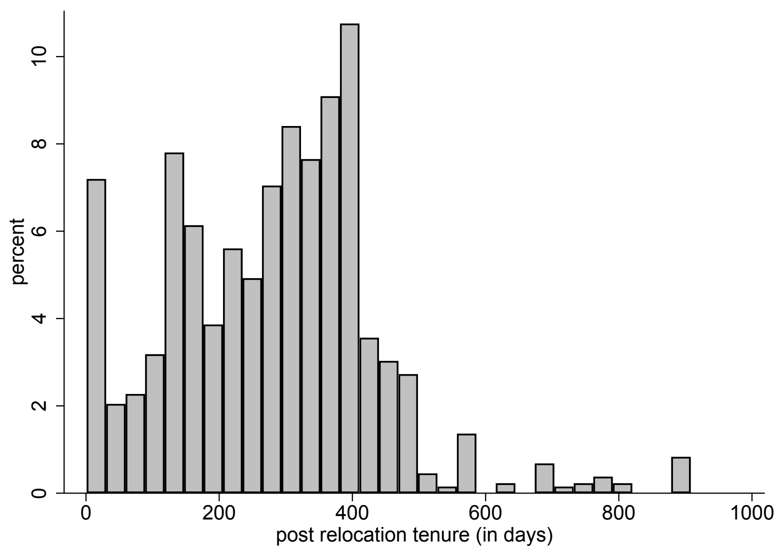


Figure A2 – Occupancy Rate Over Time

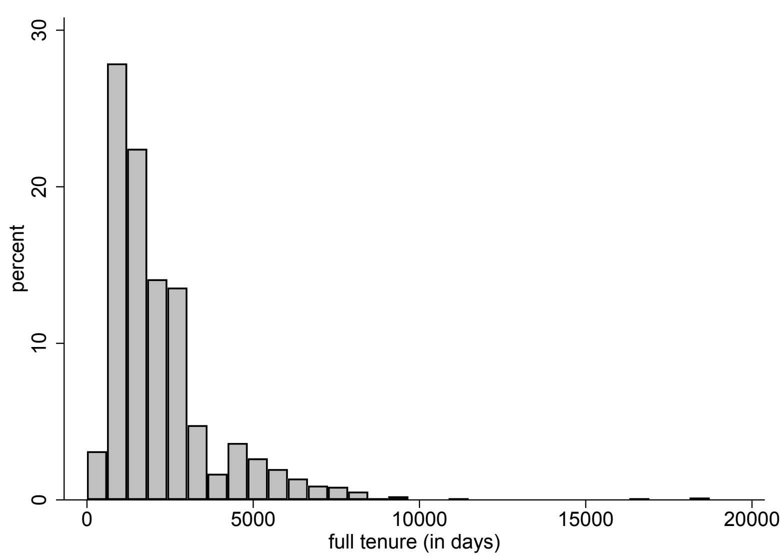

Kernel-weighted local polynomial regressions of demolished building occupancy rate on time to closure. The vertical black and grey lines correspond to the mean date at which the occupancy rate starts decreasing for the main sample and the sample including late movers respectively.

Figure A3 – Distribution of Tenure

(a) Truncated Tenure




(b) Full Tenure




*Source*: authors' own computation

Table A1 – Demolished vs. Non-Demolished Buildings Descriptive Statistics

| | Demolished Buildings[1] | Other Buildings[2] | |
|---|---|---|---|
| | Mean | Mean | Difference* |
| Age | 34.38 | 34.74 | 0.359*** |
| | | | (-0.077) |
| Female | 0.50 | 0.50 | 0 |
| | | | (-0.003) |
| Immigrant | 0.17 | 0.16 | -0.011*** |
| | | | (-0.002) |
| Educ. Vocational | 0.23 | 0.22 | -0.011*** |
| | | | (-0.003) |
| Educ. Post-secondary | 0.09 | 0.09 | 0.001 |
| | | | (-0.002) |
| Employed | 0.54 | 0.55 | 0.012*** |
| | | | (-0.003) |
| Household income | 371694.9 | 375672.4 | 3977.425** |
| | | | (-1394.8) |
| Household size | 2.74 | 2.73 | -0.009 |
| | | | (-0.011) |
| Number of children | 0.87 | 0.78 | -0.089*** |
| | | | (-0.007) |
| Number of rooms | 2.99 | 3.17 | 0.182*** |
| | | | (-0.013) |

[1] $N = 43048$
[2] $N = 76615$
* t-test

Table A2 – Descriptive Statistics

| | Mean | Std. Dev. | Min | Max | N |
|---|---|---|---|---|---|
| **At the time of moving in the demolished building** | | | | | |
| *Outcomes* | | | | | |
| Find $\text{Job}_{y+1}$ | 0.103 | 0.304 | 0 | 1 | 1327 |
| Find $\text{Job}_{y+2}$ | 0.119 | 0.324 | 0 | 1 | 1300 |
| Find $\text{Job}_{y+3}$ | 0.122 | 0.328 | 0 | 1 | 1284 |
| Find $\text{Job}_{y+4}$ | 0.126 | 0.332 | 0 | 1 | 1270 |
| Lose $\text{Job}_{y+1}$ | 0.078 | 0.268 | 0 | 1 | 1327 |
| Lose $\text{Job}_{y+2}$ | 0.075 | 0.263 | 0 | 1 | 1300 |
| Lose $\text{Job}_{y+3}$ | 0.077 | 0.267 | 0 | 1 | 1284 |
| Lose $\text{Job}_{y+4}$ | 0.091 | 0.287 | 0 | 1 | 1270 |
| Change $\text{Income}_{y+1}$ | -0.057 | 2.703 | -18 | 15 | 1325 |
| Change $\text{Income}_{y+2}$ | 0.251 | 3.121 | -18 | 13 | 1299 |
| Change $\text{Income}_{y+3}$ | 0.448 | 3.4 | -13 | 15 | 1282 |
| Change $\text{Income}_{y+4}$ | 0.717 | 3.624 | -14 | 15 | 1269 |
| | | | | | |
| *Individuals characteristics* | | | | | |
| Age | 36.705 | 12.885 | 16 | 63 | 1327 |
| Female | 0.489 | 0.5 | 0 | 1 | 1327 |
| Immigrant | 0.275 | 0.446 | 0 | 1 | 1326 |
| Vocational Educ. | 0.217 | 0.412 | 0 | 1 | 1327 |
| Post-Secondary Educ. | 0.105 | 0.306 | 0 | 1 | 1327 |
| Number of Children | 0.883 | 1.084 | 0 | 5 | 1327 |
| Household Income | 12.646 | 0.653 | 7.658 | 14.825 | 1327 |
| Household Size | 2.687 | 1.509 | 1 | 10 | 1327 |
| Number of Rooms | 2.811 | 1.09 | 1 | 6 | 1327 |
| Relocation Period at the Building Level | -324.787 | 147.845 | -908 | -89 | 1327 |

Table A3 – Exposure to Neighbours' Incomes on Finding a Job

| | $y+1$ | | | $y+2$ | | | $y+3$ | | | $y+4$ | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Dependent variable: *Find Job* | | | | | | | | | | | | |
| Exp. Income bottom 50% | -0.0139 | -0.0149 | -0.0234 | -0.0246* | -0.00987 | -0.0296* | -0.00553 | 0.00227 | -0.0280* | -0.0210* | -0.0129 | -0.0250 |
| | (0.0155) | (0.0139) | (0.0166) | (0.0148) | (0.0148) | (0.0159) | (0.0129) | (0.0149) | (0.0157) | (0.0120) | (0.0160) | (0.0167) |
| Exp. Income top 50% | 0.0176 | 0.00911 | 0.0344** | 0.00997 | 0.00132 | 0.0290 | 0.00748 | -0.00135 | 0.0245 | 0.00973 | -0.00733 | 0.0133 |
| | (0.0160) | (0.0232) | (0.0152) | (0.0162) | (0.0240) | (0.0183) | (0.0149) | (0.0248) | (0.0176) | (0.0157) | (0.0234) | (0.0209) |
| Number of Neighbours FE | | X | | | X | | | X | | | X | |
| Post-RSD tenure month FE | | | X | | | X | | | X | | | X |
| Year FE | X | X | X | X | X | X | X | X | X | X | X | X |
| Neighbourhood FE | X | X | X | X | X | X | X | X | X | X | X | X |

$N_{y+1} = 1,326$, $N_{y+2} = 1,299$, $N_{y+3} = 1,283$, $N_{y+4} = 1,269$. Linear Probability Model. Standard errors clustered at the building level are in parentheses. Significance levels: * 10%, ** 5%, *** 1%.