Neighborhood Effects on Youth Crime: Natural Experimental Evidence on Magnitude, Duration, and Mechanism

Gabriel Pons Rotger[a,*], George Charles Galster[b]

[a] *The Danish National Centre for Social Research, Herluf Trolles Gade 11, Copenhagen K, DK-1052, Denmark*
[b] *Department of Urban Studies and Planning, Wayne State University, Detroit, MI 48202, USA*

ABSTRACT

We investigate the degree to which youth criminal offenses are influenced by neighbors, identifying causal effects with a natural experimental allocation of social housing in Copenhagen. We find that youth exposed to a one percentage point higher concentration of neighbors in their housing development with drug criminal records are 6.1% more likely to be charged for criminal offenses, and that this impact manifests itself after one year of exposure. We show that neighbors with drug criminal records are most likely to enhance youth crime by providing approbation of and accessibility to illegal drugs, not by engaging in criminal partnerships.

* Corresponding author. Fax: +45 33 48 08 33.

E-mail addresses: gpr@sfi.dk (G.P. Rotger); george.galster@wayne.edu (G. Galster).

1. Introduction

Urban youth crime is an issue of unassailable social salience, affecting as it does the quality of residential life, property values, neighborhood stability and the allocation of an immense amount of public resources to support a criminal justice system (Hipp, Tita, and Greenbaum 2009; Hipp 2010; Felson and Eckert 2015). Uncovering the roots of youth criminality has been of longstanding interest to social scientists and policy makers alike (Skogan 1990). Though individual and family characteristics undoubtedly play important roles, the influences of other individuals outside the family environment may also be critical.[2]

Residential proximity facilitates social contacts that may affect youth criminality by shaping their perceptions of the benefits and costs of crime (Becker and Murphy 2000). One mechanism may be that youth form criminal partnerships with neighbors, thereby enhancing their perceived benefits from engaging in illegal activity (Sah 1991; Murphy, Shleifer, and Vishny 1993; Rasmusen 1996). Another potential mechanism is that neighbors may share information, resources, and encouragement in ways that alter youths' perceptions about the prospective net benefits of crime (Becker and Murphy 2000; Chung 2000).[3]

Understanding the relevance and nature of neighbor influences on youth and young adult ("youth" hereafter) crime is crucial for policy makers in the domains of crime control strategies (Reiss 1988), public health (Hawkins et al. 1992, Krug et al. 2002) and housing and community development (Galster 2013). Unfortunately, there is very scarce causal evidence about the magnitude and mechanisms of neighborhood effects on youth criminal behavior (Sanbonmatsu et al. 2011: Ch. 6).[4] As far as we know, no causal study has provided evidence that *any* compositional aspect of a neighborhood's population affects a youth resident's criminal behavior, has measured the influence of the closest residential neighbors on youth crime, has investigated the required duration and mechanisms for neighbor exposure

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[2] There is a longstanding literature in criminology showing strong correlations between an individual's criminal or delinquent behavior and similar behaviors within their social circles; see the review in Warr (1996).

[3] Other social sciences have proposed theories of crime. For example, socialization suggests that youth will engage in criminal behaviors that they deem normative because they are commonly performed by neighboring peers. Collective social control posits that communities may enforce individual compliance with group values about inappropriate behaviors through collective efficacy (Sampson, Raudenbush, and Earls 1997). Neighborhood criminals may serve as role models for youth, thereby enhancing the self-respect and affirming the identity associated with deviance and criminality (Hurd, Zimmerman, and Xue 2009).

[4] For a thorough review of how neighborhood may affect youth criminality and the non-experimental research literature, see Sanbonmatsu et al. (2011: ch. 6).

to generate youth crime. The objective of this paper is to provide such evidence related to *the magnitude, duration, and mechanisms* of local neighbor influences on youth criminality.

Existing causal evidence shows that moving to low-poverty neighborhoods does not have a clear or consistent impact on youth crime (for evidence based on Moving to Opportunity (MTO) demonstration, see Katz, Kling, and Liebman (2001), Ludwig, Duncan, and Hirschfeld (2001), Kling, Ludwig and Katz (2005), and Sanbonmatsu et al. 2011: Ch. 6), although individuals who do so as children (and not as adolescents) are in the long-term better off in a range of youth crime risk factors (Chetty, Hendren, and Katz (2015)). Children exposed to peers both in school and close residential context exhibit more criminal behaviors and partner in crime with each other more (see Billings, Deming, and Ross 2016). Higher concentrations of youth with similar criminal backgrounds in prison increase youth recidivism (see Bayer, Hjalmarsson, and Pozen 2009). Boys exposed over a wide residential context to violent youth offenders are more likely to become criminals as youth (Damm and Dustmann (2014)). Thus, the existing research provides a more complete picture about how children's exposure to neighbors affects their subsequent criminality as youth, but paints an incomplete portrait about how youth's exposure to criminal neighbors impacts their criminality. Our paper helps to fill this gap in the literature by providing causal evidence on the large impact and speed of the influence of neighbors charged for illegal drug-related offenses on youth criminality.

Our paper overcomes the major challenge to causal inference of residential selection by exploiting the natural experimental variation in neighbors generated by waiting list allocation of social housing in the municipality of Copenhagen.[5] We explain how this system is impermeable to selections by either households or caseworkers, and demonstrate this empirically with balance tests. In addition, the Danish administrative data allows the identification of neighbor composition at two levels of residential proximity, the social housing development and the Copenhagen-defined social housing section.[6] We demonstrate that if one uses the latter, larger spatial unit, the estimated neighborhood effects cease to be significant because of the attenuation bias induced by including irrelevant neighbors from proximate developments in the reference group (see p. 621 of Katz, Kling, and Liebman 2001).

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[5] Prior studies employing natural experiments of social housing allocation in comparable fashion as we to quantify neighborhood effects on economic outcomes include Rosenbaum (1991), Edin, Fredricksson and Åslund (2003), Oreopoulos (2003), Jacob (2004), Damm (2009, 2014), and Weinhardt (2014) and Galster et al. (2015).

[6] See section 2.

2. Social Housing Allocation Process in Copenhagen

Non-profit housing associations own and manage the social housing stock in Denmark, which in Copenhagen comprises 20% of the total housing stock, and administer the allocation of apartment units to waited-listed families. Every third social housing unit that becomes vacant is reserved by the municipality for allocating households with "urgent needs:" those who cannot afford housing at private market rents or cannot afford standard waiting time for social housing. This third of units set aside for urgent need households is determined on a "rolling" basis as vacancies occur: after the 1st apartment is reserved for municipal urgent need allocation the 4th, 7th, 10th, etc. vacancies arising will also be so reserved. Copenhagen delineates 122 mutually exclusive geographic areas within the city called "housing sections" ("local areas," hereafter), each containing four social housing developments, on average.

It is important to note that any apartment located in a local area that exceeded a 40% rate of resident adult unemployment on January 1st of any given year is ineligible for assignment to urgent needs households for that year. This regulation implies that urgent need households are not allocated to the most disadvantaged residential sectors in Copenhagen. However, the wide definition of unemployment[7] used by the municipality, coupled with the variation of social housing neighborhoods, neighbors within developments over time, and neighbors within local areas, implies that one nevertheless can observe disadvantaged individuals assigned to a wide range of environments in terms of both geographic location (see Fig. 1 for a map showing the location of social housing developments in the municipality of Copenhagen) and composition of neighbors.[8]

Most youth in our study's age group applying to social housing in Copenhagen do so as independent youth or couples without children (82%) residing in the municipality who have been affected by a range of issues ranging from homelessness, addiction, handicap, divorce, etc.; all of whom have an urgent need for housing. The process of obtaining municipal social housing starts most often

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[7] The municipality considers a person non-employed if during the second quarter of the previous year s/he has received unemployment insurance, social assistance, reduced social assistance to new immigrants, or social assistance to individuals with reduced earning capacity during at least 9 weeks. Limitations imposed by this regulation meant that 25% of social housing developments (out of 358 total) were prohibited destinations for our analysis sample. In terms of dwellings, eligible developments included approximately 37,500 dwellings.

[8] See, in Fig. A.1 Panel A in the appendix, the distribution of social housing development neighbors with drug charges assigned to similar applicants over the period 2009-2012, and in Panel B, the distribution of social housing development neighbors with drug charges allocated to similar applicants over the period 2009-2012 and within the same local area.

with a citizen contacting the municipality regarding a housing problem. A social counselor screens the household's housing urgency and determines whether it requires counseling or social housing allocation. In the latter case, the social counselor is responsible for filling out the application. The relevant individual information on income and household size used in the allocation process is obtained directly by the social counselor from administrative income register and population register databases on the basis of applicant(s) identification number(s). A central task for the social counselor is to prepare a realistic budget that determines the household's maximum affordable house rent, given the administrative information gathered from registers. In addition, the applicant specifies in the application the degree of housing urgency by stating whether applicant currently has accommodation through an institution, family, friends, or otherwise is homeless. It is critical to emphasize that the allocation criteria employed by the social counselor do not take into account any characteristics of neighbors (including their criminal records) in the potential social housing development offered.

It is important to highlight that applicants have no ability to influence the dwellings or neighborhoods offered. Their preferences for particular social housing units are never taken into account in the allocation of first offers, as such preferences are deemed in contradiction to having an urgent need for housing. As the relevant application waiting list items to be used in the allocation of first offers are filled in by social counselors on the basis of administrative data, not personal testimony, it is very unlikely that applicants could manipulate affordable rent or household size. Finally, it is also unlikely that applicants could find empty properties themselves. As explained above, every third social housing dwelling that becomes vacant is allocated for urgent needs households, and there is no way for applicants or even neighbors to know which units these are. As comports with this logic, most social housing applicants are offered apartments far away from their last residences.[9]

After completing the waiting list application form, the social counselor sends it to the appropriate social housing department, a department that does not have contact with the applicant before the issue of first offer. The municipality must pay to the non-profit housing association rent and other expenses of vacant units up to the date a household moves in the apartment, and therefore the social housing department has strong incentives to offer vacant apartment as soon as possible after they are ready to be inhabited. In order to speed up the allocation of vacant units and reduce applicant waiting time, the caseworkers from the social housing department use allocation software to match every newly available

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[9] The average distance between applicant's address before social housing and the address of the first offered social housing apartment is 6 km.

unit to an applicant.[10] Each time a housing unit reserved by the municipality is ready to be inhabited, the allocation program generates a queue of relevant applicants from the waiting list that matches the characteristics of the apartment in terms of house rent, household size and composition, and selects the application with older referral date (e.g. the application that has waited the longest). In rare occasions, the caseworker may allocate the available apartment to an applicant with extreme urgency for housing, e.g., homeless people without any kind of shelter, despite the applicant not being first in the queue of relevant applicants. Our balance tests presented in the next section demonstrate that extreme urgency of an application only influences the position in the queue of similar applicants but not the characteristics of first offered neighborhoods.[11]

Unlike those on the generic social housing waiting lists who must wait years before being allocated a unit, urgent need waiting list households on average receive a first housing offer 139 days after application. Though no household can be forced to accept an offer, most of them (72%) accept their first one due to their precarious situations.[12] The first offer can be rejected because of officially acceptable reasons offered by applicants (close to rival gangs, close to violent ex-husband, too far away from children's school) or officially unacceptable reasons (geographic and apartment preferences). Whereas applicants providing acceptable reasons are eligible to receive at least one additional offer,[13] those providing unacceptable reasons were given during the 2009-2010 period one final offer but have been deemed ineligible for further offers since 2011.

As shown in the next section, the allocation of offers to youth in urgent need applications sorts youth with prior criminal records into developments housing more neighbors with criminal records because a chief allocation item is financial means and less-expensive apartments have more residents with criminal histories. However, most importantly for this paper, given we observe all application items used by waiting list allocation software of the municipality of Copenhagen we can control for the residential sorting generated by different types of applications. Conditional on type of waiting list, approximated by observable application items, which neighbors are first offered to youth is a matter of chance, because such offers depend only on the applicant's relative position in the queue with respect

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[10] The municipality must pay to the non-profit housing association rent and other expenses of vacant units up to the date a household moves in the apartment, and therefore have incentives to offer the apartment as soon as possible it is ready to be inhabited.

[11] See Table 3 and Table A.1 at the appendix.

[12] Despite the fact that those who do not accept the first-offered unit are slightly different than other applicants, the neighborhoods they end up after relocation are practically identical (see Table 1 and Table 2).

[13] On average, those who reject their first offer wait 218 days to receive their last offer.

similar applicants and on the particular neighbors linked to the first-offered apartment that had become available when the applicant rose to the top of the queue.

[Fig. 1]

3. Data

3.1 Data on Sample Youth

The population sample we will study consists of all individuals between ages 14-29 years old with urgent housing needs who were referred to social housing in the municipality of Copenhagen between January 2009 and December, 2012 (observations=2230). At time of social housing allocation these individuals could have been single youth, couples, or dependent youth. We analyze only those age 14 and older because they would be old enough to be charged with a crime under Danish law during the two-year period after they moved to a social housing apartment—our analysis frame.

Our study employs data assembled from several administrative sources: municipality of Copenhagen, Copenhagen social housing organizations, Danish Central Police Register and Danish Administrative Registers. The social housing department of Copenhagen municipality has maintained information on all social housing applications and offered apartments since a new digitalized allocation system was adopted during summer 2007. These data include: application items used by allocation software like affordable house rent and household size; date of apartment offer; social rental date, address, house rent, size and rooms of offered apartment. By merging these addresses and dates with information on the tenants of social housing apartments maintained by Statistics Denmark, we can identify the location of each applicant living in the Copenhagen social housing sector and the addresses and identification numbers of all individuals living in the same housing section for any given date. Within a specific social housing section we are able to identify the residents of each development at that time. This allows us, in turn, to calculate aggregate population characteristics of each social housing development and proximate developments in the section precisely in our study period, after individual residents are matched to their Danish population and income Administrative Register data using their unique IDs.

Our empirical strategy requires the measurement of individual and neighbor characteristics with respect two different dates. We measure the characteristics of individuals just before the date when the letter with the first offered apartment is issued, and we measure individual outcomes within the two

years after that date. We measure the characteristics of assigned neighbors just before the date when the letter with the first offered apartment is issued and we measure the characteristics of actual new social housing neighbors just before the letter with the last accepted offer is issued. By doing so, we ensure that neighbor characteristics are not affected by the new deprived neighbors and that individual outcomes are measured henceforth from those dates.

From Danish population and income Administrative Registers we obtain longitudinal data about each of these individual characteristics related to demography (age, gender, familial status, etc.), ethnic background and socioeconomic status (educational attainment, social assistance receipt, etc.), using unique personal IDs to merge records across registers. Descriptive statistics for our sample of youth (aged 14-29 at time of social housing application) with urgent needs who applied for social housing during the period 2009-2012 are presented in Table 1. They show that at time of first housing offer, 55% of our sample applicants were women and 39% were immigrants, with average age 22 years and 1.7 persons per household. Given that substantial proportions previously were homeless or lived in institutions, the urgency of their housing needs was palpable. Their deprived economic state was clear from the very low household affordable rent and the fact that the majority of individuals, about 75% of subsample of those at least 18 years old, received one or more kinds of social assistance that comprised a majority of their disposable income.

Table 1 presents information for four different subsamples: those who accepted the first offer, those who dropped out of the allocation process after first offer, those who accepted the last offer, and those who rejected the last offer. We present these disaggregated figures to demonstrate that in spite of the fact that these groups evince slightly different profiles, they end up in similar neighborhoods (see Table 2).

We use the Central Police Register from 2005-2013 to gain information about the types of crime with which individuals were charged and associated dates of offense, charge and resolution (whether convicted or not). Dates allow us to distinguish recorded criminal offenses (for which a charge was made) for the 2230 individuals analyzed either as occurring within four years before first offer or move into social housing and/or within two years after those dates.

A brief discussion of the Danish criminal justice system is appropriate since it differs from that in the U.S. in some important ways.[14] Denmark does not have a distinct juvenile justice system; all youths 15 years and older are tried and sentenced in the same courts as adults, though more lenient sentencing options are available for offenders under age 18. Charges are recorded when the police have determined

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[14] For details of the Danish system, see Kyvsgaard (2003).

that sufficient evidence exists for the individual to be tried in court. A conviction is recorded if the court rules that the suspect is guilty of the charge; this is indicated by the recording of a sentence (Statistics Denmark 2005). Many charges do not lead to convictions within the two-year span we used for measuring criminal outcomes. For example, 10% of our sample was charged for violent crimes after occupying social housing, whereas only 6% received a conviction for violent offenses. Similarly, the proportion of youth convicted of a property crime within two years after occupying social housing was 12%, whereas the proportion of those charged during this period was 16%.

We employ in our analysis criminal charges for offenses in violation of the criminal code.[15] We use the date of the alleged offense to determine whether it occurred before or after the individual was assigned to a first offered social housing apartment or after the individual moved in. We recognize that this indicator of criminality involves measurement error, the degree varying by type of offense. Some offenses may never be reported and some that are reported never produce an individual charge; these underreporting problems are especially acute in the instances of less serious crimes. Some individuals may be erroneously charged with crimes. Unfortunately, we have no way of assessing the magnitudes of these measurement errors and assume that they are not correlated with the treatment variable, the concentration of 15+ neighbors in housing development with charges for drug crime.

Charges and convictions are categorized by the Danish Central Police Register into eight types of offenses related to: violence, sexual assault, property, drugs, illegal weapons, taxes, other offenses against the penal code, and traffic violations. Following criminological convention (Damm and Dustmann 2014), we combine violence, sexual assault and possession of an illegal weapon into one measure of violent crime, and analyze violent, property and drug crimes separately throughout. We do not analyze the other violations because they are few, idiosyncratic and heterogeneous in character. Because this becomes crucial in our later analyses, we emphasize at the outset that the sale *and* possession of specified drugs is illegal in Denmark. In our main analyses we do not make these distinctions among types of drug-related offenses but later when analyzing mechanisms of effects we find it revealing to do so.

Table 1 shows that in the four years before sample youth applied for social housing, 36% of them were charged at least once for property (26%), violent (17%), or drug crime (17%); 14% were charged with at least one crime involving perpetration with a co-offender. In the two years after they were first offered social housing, one-quarter of the individuals in our sample were charged with one or more

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[15] As in Damm and Dustmann (2014) we do not use arrests as a measure of criminality in Denmark due to limitations on their use by the "Law on Administration of Justice".

offense; 16% were charged with a property crime, 10% with a violent crime and 13% with a drug-related crime; 6% were charged with at least one crime involving perpetration with a co-offender.

[**Table 1**]

3.2 Data on Social Housing Neighborhoods and Neighbors

We collected a wide range of characteristics of individual residents of social housing developments (i.e., the prospective neighbors of our sample youth) at dates specific to each sampled youth. We measured characteristics for individuals living in the same housing development as the apartment offered the day that social housing authority issued the offer to each of the 2230 sample youth applicants. Specifically, by merging data from the aforementioned housing association and register databases we can construct "bespoke" aggregate population profiles of the age 15+ residents of the first-offered *for the four-year period up to the day of the youth's first offer*.[16] We time these measures to guarantee that they are exogenous to the youth's own criminal offending behavior after move.

The letter with the first offer was issued on average 132 days after applicants were referred to social housing. The average period between an accepted offer and a residential move was about 1.5 months. Given their typically small household sizes, the social housing first offered to sample youth was modest in scale: 2.2 rooms comprising 60 square meters, on average (see Table 2).

The mean population aged 15 and older (i.e., who could have had a criminal record) in the 263 social housing developments under study was 427 (see Table 2). In addition, we also measured the shares of residents with different types of criminal records present at the neighboring social housing developments within the same geographic social housing section. Concretely, the 122 housing sections defined in Copenhagen include, on average, four developments housing a population aged 15 and older of 1206 (see Table 2). Hereafter we will refer to the youth's individual social housing development as the "neighborhood," the other social housing developments within the social housing section as the "proximate neighborhoods" and the entire social housing section as the "local area".

We identified all neighbors age 15+ living in the same development on the dates of a given sample youth's first offer and last offer. For these neighbors we measured their criminal charges for property crime, violent crime, and drug crime committed during the four-year period immediately before the particular bespoke date of reference (day of first offer or day of last offer). We specified neighbors as

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[16] In computing the share of the development's residents with charges we note whether each has a recorded charge for offense committed during the four years up to the day prior to first offer given to the sample youth.

criminals if they had been charged at least once for a particular type of crime committed within the last four years, and aggregated this percentage to the development level.[17] We recorded two variants of criminal context variables based on the presence of criminals in the neighboring developments of the local area, so that we could assess whether the source of neighbor influences is limited to the closest neighborhood or apply to the entire local area.

We employed a number of variables to describe comprehensively the ethnic (immigrant) and socioeconomic (poverty, joblessness, employment, education) context of the neighborhood and to analyze the robustness of the neighborhood effects to controlling for such neighborhood characteristics. Descriptive statistics for these neighborhood indicators are presented in Table 2. They show that our sample was first offered neighborhoods where 38% of residents were immigrants, 35% had only upper secondary education or vocational education, 28% were not employed and 10% of households were poor. Not surprisingly given our neighborhoods are defined by social housing developments, the neighbors of our analysis sample were, on average, economically deprived on a variety of indicators, but not as extremely so as our sample members due to the aforementioned policy of social mix that limited the concentration of deprivation. Notably, there were no substantial differences in mean characteristics of any neighborhood indicators among those in the sample who differed in which housing offer they accepted, rejected, or moved into, if any. Table 2 reports key statistics on the offered and accepted apartments. These figures show that whereas there is practically no difference in terms of the characteristics of neighbors, there are differences in the characteristics of the rejected and accepted apartments.

We believe that the temporal exposure of sample youth to their newly assigned neighbors was sufficient to generate neighborhood effects, inasmuch as they typically exhibited only modest out-mobility. Within the first six months of residence in the new neighborhood, only 7% left their apartments; 17% did so within one year; 24% did so within 1.5 years.[18]

[**Table 2**]

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[17] Point estimates are robust to measuring neighborhood criminality only with the more recent offenses like is done in Damm and Dustmann (2014) (see section 5.3).

[18] Unfortunately, we do not have information on residential location two years after initial move into social housing.

4. Empirical Approach

4.1 Identification

We investigate the relevance of criminal neighbors for criminal behavior of deprived youth in a regression framework. To do so we estimate the coefficient (γ) in a regression of a measure of youth criminal behavior (Y) on the share of neighbors with criminal charges (D):

$$Y = D\gamma + W\delta + \varepsilon \tag{1}$$

given waiting list application characteristics of youth (W) and the error term (ε). Concretely, the covariate set W includes the application items used by social housing agency software for allocating first offers: maximum affordable rent, household size, a dummy denoting an individual corresponding to independent youth (e.g. the youth applies for social housing), and the age of the individual. Household size, age of the individual and the independent youth dummy for the individual capture household composition, as the youth in our sample could have been heads of households, dependent youth in households, or independent youth. Model (1) implies a quasi-random exposure to criminal neighbors (D) for youth pertaining to the same waiting list type.

The estimation of γ in (1) is potentially affected by bias because 28% of applicants fail to comply with the first offered apartment and its associated neighbors. Thus, we estimate neighborhood effects directly from equation (2):[19]

$$Y = Z_1\pi_1 + W\delta_1 + \epsilon \tag{2}$$

where Z_1 is the share of criminal neighbors associated to the first offered housing unit, and ϵ is the error term. π_1 is the intent-to-treat neighborhood effect (ITT), measuring the marginal increase in youth criminal behavior due to a marginal increase in the share criminal neighbors associated to the first offered apartment. Given that sorting into the first offer is exclusively driven by the waiting list application items (W), π_1 can be estimated with ordinary least squares (OLS) conditional on W.

As a complement to ITT effects, we also estimate the neighborhood effect γ in equation (1) with two stage least squares (2SLS) using Z_1, the share of criminal neighbors associated to the first offer, as

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[19] The nocompliance rate among youth aged 10-20 to the experimental group of MTO vouchers was 52% (Sanbonmatsu et al. 2011).

an instrumental variable for D, exposure of criminal neighbors in new social housing development.[20] The 2SLS estimated γ identifies a causal effect under Angrist and Imbens (1995) conditions:[21] First, Z_1 has a causal effect on D. This assumption is fulfilled given the high compliance rate to the first offer, 72%, and the short elapsed time between offer and move.[22] Second, the characteristics of first-offered neighbors are independent of unobservable characteristics of sample youth who ultimately occupy social housing. We fulfill this assumption given the quasi-randomization of first-offered apartments to all applicants conditional on W (see subsection B below). Third, first-offered criminal neighbors only affect youth criminal behavior after they move into their social housing apartments. This is a realistic assumption because applicants have only one week to accept or reject the offered apartment.[23] Finally, exposure to criminal neighbors (D) must be monotonic with first-offered criminal neighbors (Z_1) ($E(D|z_1'') > E(D|z_1')$ for $z_1'' > z_1'$) This assumption is not verifiable but given the large proportion of applicants who comply with the first offer, we expect similar ITT and 2SLS estimates.

4.2 Balance evidence

The matching between first offers and applicants basically depends on the amount of similar applicants with longer waiting time and the amount of relevant social housing apartments that becomes vacant for urgent housing need allocation, and there should not be a correlation between other individual characteristics and neighborhood characteristics after conditioning on the allocation characteristics. This subsection investigates whether, conditional on application items W, the assignment of social housing apartments does in fact quasi-randomize youth to neighbors associated to the first apartment offered to the applicants.

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[20] Concretely, we define

$$D = \begin{cases} Z_1 & \text{if applicant accepts the first offer} \\ Z_2 & \text{if applicant rejects the first offer and accepts the last offer} \\ 0 & \text{if applicant does not accept any offer} \end{cases}$$

where Z_2 is the share of neighbors with criminal charges of the last offered housing unit accepted by youth, and $D = 0$ for applicants who reject the last offered social housing apartment.

[21] Concretely, the 2SLS estimated γ identifies a weighted-average causal effect attributable to each percentage-point change in the share of neighbors with drug criminal backgrounds, where the weights are proportional to the effect of Z on D (Angrist and Imbens 1995).

[22] There is a median time of one month between the date of the first offer and the moving date. Thus, it is unlikely within such short time span that the aggregate characteristics of neighbors like criminality will change substantially.

[23] In this period the applicant may visit the apartment, but we deem it implausible that during such a brief visit the potential neighbors could affect the applicant's criminal behavior.

To do so, we present correlation estimates and balance tests obtained from a large number of regressions between sample youths' characteristics and neighbors' criminal behaviors measured within the four years preceding the date of the first offer.[24] We test for zero correlation between individual and neighborhood characteristics with equation (3):

$$N_1 = X_{j-}\theta + W\eta + \epsilon \qquad (3)$$

where N_1 denotes a characteristic of neighborhood and X_{j-} denotes an individual characteristic.[25] We estimate (3) without controls; including waiting list controls on affordable rent household size, age of young individual, and a dummy denoting the individual is the applicant; (W), and finally including local area fixed effects with all prior controls.

In addition, we test for the joint insignificance of all individual characteristics $\boldsymbol{X_-} = (X_{1-}, \dots, X_{J-})$:

$$N_1 = \boldsymbol{X_-}\boldsymbol{\theta} + W\eta + \epsilon \qquad (4)$$

We only report in Table 3 a subset of all balance tests. The complete set includes additional individual and neighborhood characteristics and does not change the results presented in Table 3; see Table A.1 in the Appendix. The coefficients in Table 3 Panel A are the estimated correlations without controlling for waiting list type or for local area fixed effects. Thirty-three out of a total of eighty-four estimated coefficients are statistically significant at the 5% level. Most correlations between neighbors and individual variables measuring criminal charges have a positive sign. These results show that, *unconditionally*, the allocation system in Copenhagen tends to assign youth with prior criminal records to those social housing developments exhibiting higher concentrations of different types of criminal neighbors.

The coefficients in Table 3 Panel B show a completely different picture. When we include waiting list controls there are no coefficients that are significant at the 5% level. Similarly, when we add local area fixed effects, only two coefficients are significant at the 5% level; see Panel C.

The last rows of Panels A, B and C of Table 3 report the p-values of the test for joint insignificance of individual characteristics. The balance tests confirm the results obtained before.

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[24] We only report these correlations for the neighborhood context used in the paper, social housing development, but similar results are obtained when local area neighbors are included.

[25] See Table A.1 for a complete list of individual and neighborhood characteristics.

Unconditionally, assignment of first offers tends to sort youth with previous charges among more criminal neighbors, especially those with prior drug offenses. However, when we control for waiting list type, all balance tests convincingly fail to reject the null hypothesis of no correlation between individual and neighbor characteristics.

The balance tests provide support for our previous conjectures that, conditional on waiting list controls, the process for allocating social housing in Copenhagen is effectively insulated from potential selections by urgent need households, counselors or caseworkers and operates in a way that quasi-randomly assigns neighbors to the youths we are studying.

[**Table 3**]

5. Neighborhood Effects on Youth Crime

5.1 Magnitude of the Neighborhood Effect

5.1.1 Reference group

One of the challenges in the estimation of social interaction effects is the measurement of the reference group (Manski 1993). The relevant neighbors, if any, who are affecting youth crime need to be identified if attenuation bias has to be avoided (Katz, Kling and Liebman 2001) and neighborhood social mix policies are to be designed efficaciously (Galster 2013). Unlike in the analysis of peer effects within a school (Billings, Deming, and Ross 2016) or correctional facility (Bayer, Hjalmarsson, and Pozen 2009), where the boundaries encompassing the most influential people are given, the appropriate geographic scale of neighborhood is unknown. In addition, the defining characteristic(s) of such a group is also unknown and inasmuch as endogenous and exogenous social interaction effects are not separately identified (Manski 1993), it is not clear which type of criminal neighbors should be selected. Bayer, Hjalmarsson, and Pozen (2009) and Damm and Dustmann (2014) demonstrate that co-location with only certain types of criminals will generate effects. Therefore the more plausible strategy is to estimate neighborhood effects for different groups of criminal neighbors residing in the smallest-scale, most-proximate residential context that allows measuring neighbor features accurately, observing the cases in which relationships prove strongest, and then assess whether any emerging neighbor influences extend to neighboring social housing developments.

To do so, we assess the effect of four different concentrations of neighbors (all age 15 or older) in the social housing development who differ in the type of crime with which they have been previously charged: any crime, violent crime, property crime, or drug crime. The development includes, on average, 420 residents age 15 and older (when criminal charges can be recorded). The scale of the development is reasonable on both conceptual and practical grounds, inasmuch it is small enough to include individuals to whom in-moving youth are most likely to be exposed within the residential context, yet large enough to allow measuring neighbor characteristics accurately in the aggregate. It is possible, of course, that neighborhood effect mechanisms may operate over a larger geographic scale than the social housing development.[26] To address such a possibility we also consider the effect of the percentage of residents from the neighboring social housing developments within the same local area with charges for the different types of criminal offenses. Social contact between youth at risk and offenders from other developments may be more difficult to detect by non-criminal neighbors, and therefore it is possible that sample youth co-offend or are influenced by others residing in the local area, not simply their own development.

We identify the appropriate reference group by estimating π_1 in (2) for percentages of neighboring individuals with any charges and the three types of charges living in the same social housing development, then we replicate the tests expanding the relevant pool of neighbors to include other social housing developments in the local area. Table 4 below, and tables A.2, A.3 and A.4 in the appendix report ITT estimates of these potential effects on two types of outcomes: probability that a sampled youth is charged with any crime within two years after the first offer of social housing and the number of charges within the same period, both in aggregate and then by crime type. All models include waiting list controls, and we assess the robustness of these results to local area fixed effects.

Columns (1) to (4) of Table 4 report ITT estimates of π_1 in (2) for percentages of neighboring individuals with any charges for different locations of neighbors. Results show a positive and significant effect of 0.007 on the probability of being charged when neighborhood is restricted to the social housing development. Columns (3) and (4) of Table 4 show that residents with criminal charges from proximate developments do not have any impact and the effects from development residents are positive but insignificant. Columns (1) to (5) of Table 5 report the ITT estimates when concentrations of neighbors with drug charges are used as reference groups. These estimates clearly demonstrate that the source of neighborhood effects on youth crime is the presence of neighbors with charges for drug crime. In

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[26] Damm and Dustmann (2014) identified an influence of aggregate violent criminality in the environs measured across the much larger scale of the municipality.

addition, columns (6) to (8) demonstrate that adding the more distant neighbors to the regression on the closest ones does not affect the influence of neighbors within the development, which remains strong and unchanged. Table A.3 confirms that when neighbors with drug charges residing in the given social housing development and neighboring developments within the same local area are aggregated as the reference group, all estimates are insignificant at 5% in spite of the fact that the reference group includes neighbors from the youth's own housing development. Our inductive analysis thus demonstrates that the geographical scale of the neighborhood is a key factor for the analysis of neighborhood effects.

Columns (5) to (8) of Tables A.3 and A.4 show that neither violent criminal neighbors living in the development nor the local area have any influence on the criminal behavior of youth. This result suggests that youth are less vulnerable in some respects when they are exposed at ages 15-29 to violent criminal neighbors, compared to when they are exposed at younger ages (Damm and Dustmann 2014).

Finally, we investigated the relevance of neighbors with charges for property crime at both the social housing development and local area spatial scales (Columns (1) to (4) of Tables A.3 and A.4) to ascertain whether they generated neighborhood effects. These tables show that neighbors in the development previously charged with a property crime have no influence, in contrast with some of the findings of Bayer, Hjalmarsson, and Pozen (2009).

The results are clear: the only reference group predictive of any sort of youth crime is drug criminals living within the same social housing development.[27] We therefore focus on neighbors with charges for drug crime residing in the development in the rest of the paper.

[**Table 4**]

5.1.2 Main results

Table 5 presents the estimated effects generated by neighbors with drug criminal records, where youth criminal behavior is measured in terms of the probability and number of charges incurred over two years, by type of charge. Table 5 columns (2) and (3) show ITT and 2SLS estimates. Estimated first stage coefficients are reported at the last row of Table 5. What is immediately obvious from columns (2) and (3) are the statistically and substantively strong neighborhood effects of drug criminal neighbors living in the development on the probability that youth will be charged with any type of crime. Both ITT and 2SLS estimates indicate that property crime primarily drives this aggregate pattern. Considering any type of criminality, a one percentage-point increase in neighbors with drug charges increases the

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[27] This differs pointedly from the findings of Bayer, Hjalmarsson, and Pozen (2009) and Damm and Dustmann (2014) who focused on property and violent crime offenders, respectively, as main sources of neighborhood effects.

2SLS-estimated probability of a youth being charged with any crime within two years after moving into social housing by 0.015 (representing a 6.1% increase from the mean probability), mainly because of the impact on the probability of a property crime charge of 0.010 (representing a 6.1% increase from the mean probability). These results contrast with Damm and Dustmann (2014) where the source of crime effects among children arose from the same type of criminal neighbors and some of the findings of Bayer, Hjalmarsson, and Pozen (2009), where the only source of peer effects among prisoners arose from the same type of criminals. Furthermore, we do not find a significant effect on youth violent crime, this contrasting with the evidence on neighborhood effects on later criminality of children (Damm and Dustmann 2014, Ludwig, Ducan and Hirschfield 2001), and suggesting the presence of a different neighborhood effect mechanism among youth.

Columns (5) and (7) of Table 5 show larger and statistically significant ITT impacts on the probability of being charged for male youth, and more moderate and statistically insignificant effects on the probability of being charged for female youth. This gender pattern in results is in line with the findings of Damm and Dustmann (2014). Yet, as shown in columns (9) and (11) of Table 5, the higher impact of neighbors on the probability of being charged for male youth can be explained by the much higher impact on youth with charges prior residential change.

As a complement to the effects on the number of youth ever being charged, we also examined the effect on the number of youth charges. These results are less precisely estimated, but are quite consistent with the effects on probability of being charged. A one percentage-point increase in neighbors with drug charges increases the ITT-estimated number of charges youth face within two years after moving into social housing by 0.059 (representing a 5.4% increase from the mean number). The 2SLS-estimated effect, less precisely estimated, is 0.076 (representing a 6.3% increase from the mean number). The results for youth charges display much pronounced gender differences than the results for probability of being charged. Columns (5) of Table 5 implies that a one percentage-point increase in drug criminal neighbors increases the ITT-estimated probability of males being charged by 3.17% and at the same time increases the number of male youth charges by 5.54%. The strong effect on the number of male charges is partly explained by drug-related crime charges (representing a 5.61% increase from the mean number). The evidence that greater concentration of neighbors with criminal charges for drug crimes has a large impact on the number of male youth drug crime charges is more in line with some other findings of Bayer, Hjalmarsson, and Pozen (2009),

[**Table 5**]

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5.1.3 Duration of Neighborhood Exposure and Neighborhood Effect

We explore the dynamics of the neighborhood effect in order to determine the speed at which crime effects become relevant. Table 6 shows the ITT estimates on youth crime over elapsed time since the date of the first offered housing unit. Comparisons across the columns of Table 6 show several patterns having strong intuitive appeal from the perspective of neighborhood effects. First, there is clear support for the notion of a minimum required duration of exposure. Neither probability of being charged nor the number of charges effects appear within months after youth occupy social housing, instead taking at least a year to manifest themselves. Second, the intertemporal dynamics are slightly distinctive across outcome types. The effect on the probability of being charged fully manifests itself after one a half year, though henceforth there seems to be no marginal effect from subsequent duration of exposure. In the case of criminality measured by number of charges, there is a consistent pattern of the effects growing monotonically over the first 18 months, and then the marginal effect of subsequent exposure still increases but at lower pace.

[**Table 6**]

To sum up our main results on the magnitude and dynamics of neighborhood effects, the concentration of neighbors with drug charges in the social housing development has an important impact on youth subsequent property and drug crime behaviors. The cumulative impact supports the presence of intra-neighborhood processes that take some modicum of time to become efficacious, but is not sufficient to support the claim of neighborhood effects being transmitted by contact with resident drug offenders, because correlated effects may generate similar results.

5.2 Mechanisms of the Neighborhood Effect

Our previous evidence is suggestive that drug criminals are involved in processes within neighborhoods with higher presence of drug criminal neighbors that have criminogenic consequences. In this subsection, we explore alternative mechanisms in an attempt to isolate, as much as possible, the source of these effects. Our data allow us to assess several potential mechanisms, rejecting those that do not compart with the evidence and forwarding those that are *most plausible*. We arrive at these deductions by investigating whether neighborhood effects are driven by three alternative mechanisms: (1) correlated effects; (2) interactions in which youths may engage with neighbors while committing crimes collaboratively; or (3) interactions with neighboring peer drug criminals of the same age.

A plausible mechanism to neighborhood effect is the correlated effect (Manski 1993), that originates from a multiplicity of unobservable non-neighbor forces correlated with the treatment variable. The presence of correlated effect can be partially addressed by controlling for fixed local area effects and exploiting within-local area variation in neighbor characteristics to identify the neighborhood effect:

$$Y = Z_1 \pi_1 + W \delta_1 + \theta + \epsilon \qquad (5)$$

where θ are fixed effects capturing all those features of the intra-city physical, demographic, socioeconomic and institutional/social service context that do not vary during the analysis period specified for the encompassing local area in which each of the social housing developments is located. We intend for these fixed effects to serve as proxies for the multiplicity of factors that will influence sample individuals' perceptions of the prospective expected benefits and costs of committing crime within that spatial context (Manski 1993, Glaeser, Sacerdote and Scheinkman 1996).

As vacant units of each local area set aside for urgent need households are determined on a "rolling" basis as vacancies occur, and rents of all social apartments located in the same local area are determined according to apartment size and financial situation of the area, the allocation of first opportunities quasi-randomizes different social housing development neighbors to applicants within the same local area (for balance evidence, see Table 3 and Table A.1 at the appendix). Column (5) of Table A.2 shows ITT estimates of the effect of concentrations of drug criminal neighbors residing in the social housing development based on equation (5) where we include a set of dummies for the housing section, parish and postcode. Results demonstrate that in spite of more reduced treatment variation (see Fig A.1), the effects of neighborhood composition variation within local areas are practically equivalent to the estimated effects of neighborhood effects across local areas.

The ITT-estimated neighborhood effect on the probability of being charged is 0.010 (slightly lower than 0.012 but not statistically lower), the impact on the probability of being charged for property crime is 0.008 (identical to the estimated effect without local area controls), the effect on the number of charges is 0.067 (slightly higher than 0.059) and the estimated effect on the number of drug charges is 0.020 (larger than 0.015, but not statistically larger). To sum up our main results on the magnitude of neighborhood effects do not change after controlling for a local area fixed effects. In any, the effect on drug charges detected among males is, after controlling for time invariant correlated effects, statistically significant at the 5% level among all youth.

The previous investigation explores the within-development variation over time in the share of drug criminal neighbors to control for time invariant correlated effects like the presence of illegal

activity opportunities in the local area. However, an important potential source of correlated effect is police surveillance. In order to test for police effect, we assume that neighbors with higher number of criminal charges will be kept under higher surveillance than neighbors with lower number of charges. Thus, the police effect will not be plausible if the neighborhood effect does not interact with the concentration of charges among charged neighbors, denoted Z_2. Columns (1) to (3) of Table 7 present estimates of equation (6):

$$Y = Z_1\pi_1 + Z_2\pi_2 + Z_1Z_2\pi_3 + W\delta_1 + \epsilon \tag{6}$$

The first row of column (1) presents the ITT-estimates of the neighborhood effect (π_1) of being charged. The estimated effect is 0.015, which is not statistically higher than 0.012, the estimated effect from equation (2). Most importantly, the interaction term is not significant and has the opposite sign than expected in case our neighborhood effects where driven by police surveillance.

[**Table 7**]

We now turn to the assessment of neighborhood effects on youth that yield co-offending with neighbors, inasmuch as sheds light on the degree to which neighboring drug criminals may "recruit" youth into their criminal networks. We measure co-offending activity with two outcomes: the probability of young being charged with a neighbor, and the number of neighbors charged with young. The notion here is that youths' increased exposure amid concentrations of drug criminal neighbors could increase their incentives to establish criminal partnerships that will manifest themselves as crimes committed with neighbors. These criminal partnerships could involve collective drug possession and/or sale, or the collaborative commission of property or violent crime that might be associated with drug possession and/or sale.

Table A.5 at the appendix reports no significant effect on co-offending activity at 5% level, though it is possible to appreciate small and insignificant positive effects for the subsample of youth with prior criminal charges. Concretely, for the small group of youth with prior drug charges the effect on the number of co-offending neighbors is significant at 10% level (see column 5 of Table A.5). These results are in line with the findings of Bayer, Hjalmarsson, and Pozen (2009) on recidivism among prison peers with previous drug charges, and suggests that co-offending requires spatial proximity among similar types of experienced criminals. However interesting these results suggesting a criminal recruitment mechanism of neighborhood effects, we hasten to note that they are highly selective, applying only to those with prior drug charges. The effect on neighbor co-offending activity becomes

statistically insignificant in the subsample of youth with prior experience in any type of criminality (column 2), and is highly insignificant in whole sample (column 1), suggesting that at different mechanism than partnerships in crime are at work for sampled youth.

The last type of potential intra-neighborhood youth interaction involves same-age peer criminals who might shape youths' perceptions of the normative and tangible benefits of "joining the crowd" in criminal behaviors. If neighboring peers with drug criminal records have strong impacts on youths' drug criminality, the prospect of "social multipliers" arises (Manski 1993). We probe this possibility by re-specifying the neighbor reference group to include only drug offenders ages 15 to 29 and re-estimating the ITT model; see column (1) of Appendix Table A.6. We find no effect of the concentration of neighboring peer drug criminals on either the probability or number of charges against youth for *any* type of crime. By contrast, the neighborhood effects generated by concentrations of drug offenders ages 30 years and older have a similar impact than the main effects (column (2) of Table A.6). These results stand in sharp contrast to other studies that emphasize the criminogenic role of peers in confined contexts like prisons (Bayer, Hjalmarsson, and Pozen 2009) and schools (Billings, Deming, and Ross 2016), or in wide residential context (Damm and Dustmann 2014).

In sum, our evidence suggests that neighborhood effects on youth crime are primarily manifested through the enhanced use of illegal drugs, rather than exposure to potential partners in crime or peers. This conclusion is most directly supported by the observed neighborhood effect on drug crime charges among males, which points to increased substance abuse by youth as the key causal link here.

We deem it highly plausible that two distinct, but not mutually exclusive, causal mechanisms are at work here. First, older neighbors (less so peers) with prior drug offenses may encourage youth to expand their use of drugs by (1) facilitating proximate, inexpensive and rapid access to drugs; (2) building youths' confidence that the neighbor can be trusted; (3) role-modeling that drug use is an appropriate behavior; and (4) increasing the privacy and safety of the drug use / sale transaction compared to alternative locales outside of the development. In all these ways, youths' perceived net benefits from the illegal action of consuming or purchasing drugs is likely greater when more neighbors in the closest residential context provide access to drugs. In turn, youth with more formidable drug habits increasingly expose themselves to drug possession charges and are pressed to commit property crimes to support their habit. Second, our evidence on selective co-offending suggests that another interaction mechanism may also be relevant among the subset of neighbors with similar criminal experience. Older neighbors with prior criminal records for drug offenses apparently recruit new, younger neighbors with *similar drug crime experience* to join in criminal partnerships through mentoring and/or proffering access to networks that increase the prospective benefits and reduce the

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costs of criminality.

These mechanisms operate most forcefully upon youth with prior criminal convictions; they seemingly have little impact on first-time criminality. From an economics of crime perspective, this result implies that neighborhood effects are only sufficiently powerful in shaping youths' perception of the net benefits from property and drug crime once they have previously decided for other reasons to begin such behavior.

5.4 Robustness of the Neighborhood Effect

Numerous additional checks than those provided in previous sections unanimously testify to the robustness of our aforementioned effects. First, we estimate the effect of the concentration of neighbors with drug charges controlled for the neighborhood socioeconomic characteristics. Tables A.7 columns (1) to (4) show that the estimates on probability of youth crime are robust to controlling for the percentage of non-employed adults, the percentage of poor households, the percentage of young adults with at least high school education or the percentage of immigrants. Estimates on number of charges are also robust to adding these neighborhood characteristics, although the estimated effect becomes only significant at 10% level when controlling for neighborhood employment or for neighborhood education, a result produced by the high correlation between these neighborhood characteristics with the percentage of neighbors with drug charges.

Second, Table A.7 column (5) shows that our results are robust to controlling for youths' previous observable criminal record in the three different types of offenses. Third, we assess the robustness of our results to controlling for the degree of housing urgency at the application date (Table A.7 column (6)). As discussed previously, applicants with extreme need for housing may be offered a vacant apartment before a similar applicant higher in the queue. Results show that controlling for housing urgency does not affect the results.

Fourth, we reformulated the treatment variable to reflect convictions for drug crime instead of charges, and to include neighbor drug criminal offenses committed within the 2 years before social housing.[28] Table A.6 columns (3) and (4) show that the estimated effects are robust to these different measures of drug criminal neighbors.

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[28] Unlike Damm and Dustmann (2014) we do not use convictions as outcomes in our main models since it is a poor measure for offenses committed during the second year of our analysis period, given the typical lag between offense and conviction.

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Finally, we examined the role of the size of the development by estimating an interaction model with the number of neighbors (see Table A.8). Neighborhood effects obtained from the interaction models are slightly higher and significant also for violent and drug crime, but the size of neighborhood does not influence the neighborhood effect.

6.   Conclusion

Our paper has found through the use of a natural experiment in Copenhagen that a particular aspect of the residential composition of the social housing development has a powerful effect on the criminal behavior of disadvantaged youth and young adults who move in: neighbors with a prior record of drug-related charges. The induced offending behavior is especially strong among youth with prior records, though there is no evidence of induced first-time offending or robust effects on violent crime. These neighborhood effects manifest themselves already after one year since youths moved into their social housing developments, further supporting the notion that effects are being transmitted via intra-neighborhood social interactions. Lack of general effects on co-offending behavior suggests that these transmission mechanisms are primarily through enhanced accessibility to and approbation of illegal drugs, though partnerships in crime seem to be operating upon youths with prior drug offenses. Our evidence indicates that the most plausible dominant mechanism is that drug offending neighbors encourage in a variety of ways drug consumption among youth with prior charges, thereby putting them at risk for possession charges and increasing the chances that they will also face charges for drug selling and property crime as they attempt to finance their expanded drug usage.

Though the foregoing results are robust to a variety of tests, in another sense they are fragile to several key methodological choices. We demonstrated that even this powerful neighborhood effect will not be observed if one selects a different category of neighbor defined by other criminal records or by socio-demographic characteristics as the reputed source of effect, or if one specifies a larger geographic scale for operationalizing neighborhood.

publication.

Declaration of conflicting interests

Acknowledgments

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Figures and tables
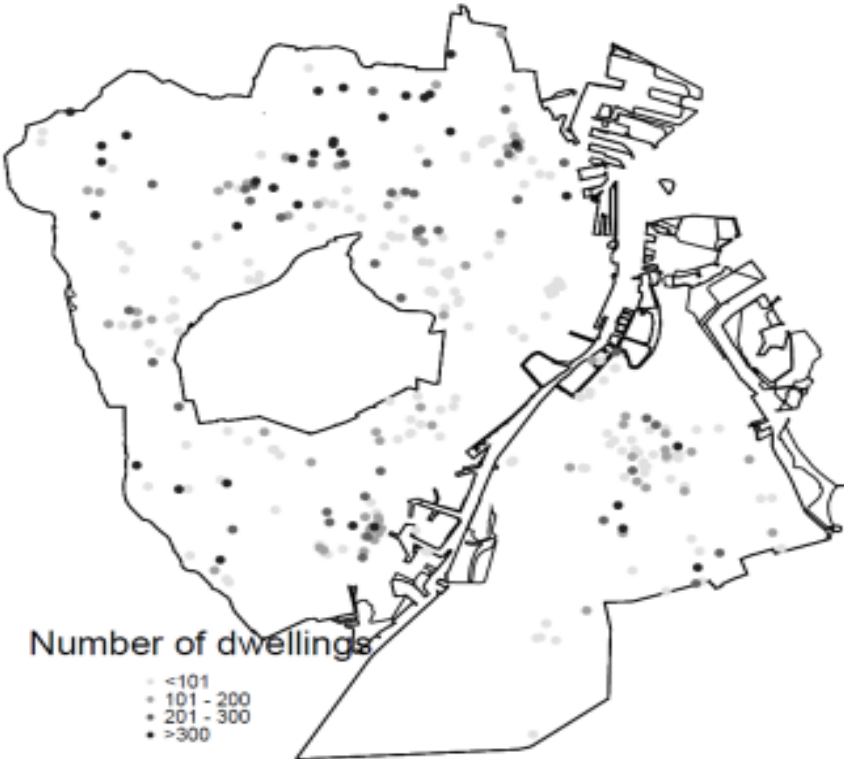
Fig. 1

Geographical dispersion of social housing developments in Copenhagen, by number of dwellings within development.

Table 1

Descriptive statistics of sample youth

	Applicants who accept the 1st offer	Applicants who drop out at 1st offer	Applicants who accept the last offer	Applicants who reject the last offer
Age	22.3(4.3)	22.6(3.6)	22.5(4.6)	24.5(3.9)
Female (%)	55(50)	53(50)	58(49)	55(50)
Immigrant (%)	39(49)	37(48)	45(50)	32(47)
Household size	1.7(1.2)	1.3(0.6)	2.0(1.2)	1.5(1.0)
Study support (%)[c]	8(26)	5(23)	13(31)	8(28)
Social assistance (active) (%)[d]	14(30)	10(27)	14(30)	8(25)
Social assistance (passive) (%)[d]	43(49)	48(50)	37(47)	53(50)
Sickness benefits (%)[c]	4(16)	3(17)	4(17)	4(18)
Disability support (%)[c]	2(13)	2(14)	1(9)	4(18)
Disability pension (%)[c]	2(13)	2(14)	3(18)	2(14)
Affordable rent (2014 DKK)	4321(1391)	3656(1078)	4731(1503)	4232(1429)
Not occupy private residence (%)	32(47)	30(46)	31(46)	32(47)
Occupies halfway house (%)	20(40)	18(38)	14(35)	13(34)
Occupies institution (%)	23(42)	22(42)	22(42)	13(34)
Charged before 1st offer (%) [a]	36(48)	38(49)	32(47)	30(46)
Property crime	26(44)	28(45)	22(42)	18(39)
Violent crime	17(37)	21(41)	15(36)	18(39)
Drug crime	17(37)	22(41)	15(36)	17(38)
Number of charges before 1st offer	2.2(7.3)	2.9(6.7)	1.5(4.8)	2.4(7.3)
Co-charged before 1st offer (%)	14(35)	16(37)	10(30)	7(25)
Charged after 1st offer (%) [b]	25(43)	27(44)	21(41)	22(42)
Property crime	16(36)	16(37)	14(34)	15(36)
Violent crime	10(30)	12(32)	7(25)	5(22)
Drug crime	13(34)	15(36)	10(30)	10(30)
Number of Charges after 1st offer	1.1(3.8)	1.3(4.1)	0.7(2.4)	0.7(2.9)
Property crime	1.3(6.1)	1.7(4.4)	0.9(3.3)	1.8(6.6)
Violent crime	0.4(1.3)	0.7(2.0)	0.3(1.1)	0.3(0.8)
Drug crime	0.5(1.9)	0.6(1.7)	0.4(1.1)	0.4(1.0)
Co-charged after 1st offer (%)	6(24)	7(25)	4(21)	5(22)
Observations	1603	318	249	60

Means and standard deviations (in parentheses) of the individual characteristics.

[a] Criminal behaviour measured before the first offer during the 4 years before

[b] Criminal behavior measured during 2 years after the date of first offer.

[c] Study support dummy is defined over the subsample of 17+ years old.

[d] Social assistance (passive worker) is defined for 16+ years old, and other public income transfers for 18+ years old.

Table 2

Descriptive statistics on apartments, by offer and response to offer

	On 1st offer, for applicants who accept the 1st offer	On 1st offer, for applicants who drop out at 1st offer	On 1st offer, for applicants who accept the last offer	On last offer, for applicants who accept the last offer	On 1st offer, for applicants who reject the last offer	On last offer, for applicants who reject the last offer
Delay from referral to offer (days)	132(116)	190(127)	124(108)	215(170)	131(115)	232(159)
Delay from offer to move (days)	47(48)	-	-	50(48)	-	-
House rent (2014 DKK)	3829(1199)	3204(1036)	4146(1339)	4059(1222)	3736(1071)	3699(1432)
Number of rooms	2.2(1.0)	1.7(0.7)	2.4(1.0)	2.5(0.9)	2.1(0.8)	2.1(1.0)
Size (m^2)	61(19)	50(17)	63(19)	66(19)	58(17)	56(19)
Moves out within 6 months (%)	7(25)			6(23)		
Moves out within 12 months (%)	17(38)			12(33)		
Moves out within 18 months (%)	24(43)			18(38)		
15+ years old in development	427(309)	422(276)	408(309)	412(296)	479(301)	429(54)
15+ years old residents in section	1206(707)	1198(666)	1097(659)	1212(682)	1224(660)	1079(120)
Social housing developments in section	3.9(3.2)	4.1(3.1)	3.4(2.7)	2.5(1.6)	3.8(3.0)	2.1(1.3)
Section residents charged (%)	8(2)	9(2)	8(2)	9(2)	9(2)	9(2)
Neighbors charged 4 years before offer	9(3)	10(3)	9(3)	8(3)	9(3)	10(4)
Property crime	5(2)	6(2)	6(2)	5(2)	6(2)	6(2)
Violent crime	3(2)	4(2)	3(1)	3(2)	4(2)	4(2)
Drug crime	4(2)	4(2)	4(2)	4(2)	4(2)	4(3)
Poor household neighbors (%)	11(7)	12(8)	9(5)	9(4)	11(9)	10(5)
Immigrant neighbors (%)	38(15)	41(17)	38(16)	35(15)	39(13)	39(17)
High school neighbors (%)	35(5)	34(5)	36(5)	36(5)	35(4)	35(25)
Higher educated neighbors (%)	19(9)	18(9)	17(8)	19(8)	17(7)	17(6)
Jobless neighbors (%)	28(7)	29(6)	27(7)	27(6)	28(6)	29(6)
Observations	1603	318	249		60	

The table reports the mean and standard deviation (in parentheses) of characteristics of offered apartments measured before the offer is issued.

Table 3

Balance tests

	Neighbors charged for property crime in develop.	Neighbors charged for violent crime in develop.	Neighbors charged for drug crime in develop.	Poor Neighbors charged for in develop.	Non-employed neighbors in develop.	Neighbors with upper secondary education in develop.	Immigrant neighbors in develop.
Panel A				No controls			
Probability of being charged before 1st offer	1.46*	1.74*	1.80*	0.42	0.53*	0.07	0.13
Number of charges before 1st offer	10.65	18.86*	14.00*	3.18	6.27*	0.33	1.34
Probability of being co-charged before 1st offer	0.42	0.42	0.33	0.14	0.19*	0.14	0.03
Persons charged before 1st offer	5.94	10.42*	8.12*	1.34	2.97*	0.88	0.60
Female	-0.95	-1.87*	-1.45*	-0.28	-0.43*	-0.06	-0.07
Immigrant	-0.94	-0.24	-0.43	-0.28	-0.30*	-0.17	-0.02
Occupies halfway house	-0.24	-0.00	-0.49	0.36*	0.07	0.23*	0.09
Homelessness	1.02*	0.92	1.36*	-0.23	0.34*	0.02	-0.05
Overall problem	-0.18	-0.46	-0.02	-0.10	-0.09	-0.00	-0.12*
Occupies public housing	-1.30*	-1.81*	-1.72*	-0.38*	-0.45*	-0.11	0.00
Lives with family or friends	0.23	0.49	0.53	0.15	0.19	-0.02	-0.03
Not occupy private residence	1.16*	1.72*	1.35*	-0.14	0.38*	-0.04	0.02
Occupies public institution	-0.07	0.13	-0.33	0.10	-0.07	0.08	0.02
Balance test (p-value)	0.21	0.04	0.00	0.03	0.00	0.22	0.42
Panel B				Waiting list controls			
Probability of being charged before 1st offer	0.40	0.14	0.67	0.03	0.22	0.02	0.13
Number of charges before 1st offer	1.01	5.02	3.54	-0.03	3.57*	-0.08	1.34
Probability of being co-charged before 1st offer	0.19	-0.02	0.04	-0.10	0.09	0.10	0.03
Persons charged before 1st offer	1.18	3.21	2.89	-1.05	1.49	0.54	0.60
Female	-0.04	-0.53	-0.55	-0.05	-0.18	-0.05	-0.07
Immigrant	-0.14	1.05	0.49	-0.04	-0.06	-0.15	-0.02
Occupies halfway house	-0.37	-0.28	-0.70	0.11	-0.00	0.18*	0.09
Homelessness	0.38	0.12	0.73	-0.12	0.21	0.06	-0.05
Overall problem	0.02	-0.21	0.21	-0.10	-0.04	-0.00	-0.12*
Occupies public housing	-0.16	-0.16	-0.49	-0.09	-0.12	-0.09	0.00
Lives with family or friends	-0.08	0.01	0.20	-0.03	0.10	-0.05	-0.03
Not occupy private residence	0.38	0.71	0.55	-0.16	0.19	-0.02	0.02
Occupies public institution	-0.20	-0.10	-0.53	-0.06	-0.14	0.05	0.02
Balance test (p-value)	1.00	0.91	0.34	0.35	0.33	0.28	0.42
Panel C			Waiting list controls and local area fixed effects				
Probability of being charged before 1st offer	0.24	-0.27	0.54	-0.24	0.15	-0.07	0.12
Number of charges before 1st offer	-3.03	1.70	2.01	-1.43	2.83	-0.26	2.15*
Probability of being co-charged before 1st offer	0.09	-0.25	-0.01	-0.22	0.10	0.03	0.03
Persons charged before 1st offer	-1.17	-0.17	1.04	-1.98*	1.20	-0.22	0.66
Female	-0.12	-0.12	-0.34	0.06	-0.24	-0.07	-0.09
Immigrant	-0.65	0.48	0.56	0.08	-0.24	-0.07	0.06
Occupies halfway house	-0.41	-0.58	-0.70	0.14	0.06	0.22*	0.12
Homelessness	0.02	0.15	0.72	-0.09	0.23	0.13	-0.09
Overall problem	0.31	0.24	0.16	-0.15	-0.08	-0.05	-0.19*
Occupies public housing	-0.44	-0.50	-0.82*	-0.04	-0.15	-0.06	0.05
Lives with family or friends	-0.05	0.28	-0.05	-0.13	0.11	-0.08	-0.04
Not occupy private residence	0.59	1.24	0.80	-0.22	0.20	-0.00	-0.03
Occupies public institution	-0.56	-0.73	-0.64	0.01	-0.22*	0.05	-0.04
Balance test (p-value)	0.91	0.75	0.43	0.65	0.17	0.19	0.16

The first thirteen rows of each panel show the estimated θ from equation (3) for each individual characteristic X_{j-} in the row and each neighborhood characteristic N_1 in the column. The rows corresponding to Balance test (p-value) show the p-value of the test $\theta=0$ from the equation (4) where the set of individual characteristics $\boldsymbol{X}_-$ include all individual characteristics listed in Table A.1 at the Appendix. The estimated models reported in Panel A do not control for waiting list characteristics W, the results shown in Panel B control for W, and the estimates in Panel C control for W and local area fixed effects. Robust standard errors are adjusted for development clustering of the first-offered social housing unit (259 cells). Sample size is 2230 14-29 years old individuals. * = p-value < 0.05.

Table 4

Effect of criminal neighbors and neighbors charged for drug crime on youth criminal charges, by location of neighbors

	Neighbors charged residing in development	Neighbors charged residing in section	Neighbors charged residing in development	Neighbors charged residing in proximate Developments	Neighbors charged for drug crime residing in development	Neighbors charged for drug crime residing in section	Neighbors charged for drug crime residing in development	Neighbors charged for drug crime residing in proximate developments
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Probability of being charged								
Any crime	0.007**	0.007	0.004	0.000	0.012**	0.011	0.009*	-0.003
	(0.002)	(0.004)	(0.003)	(0.006)	(0.004)	(0.007)	(0.004)	(0.008)
Property Crime	0.006**	0.007*	0.005*	0.001	0.008*	0.013*	0.008*	0.001
	(0.002)	(0.004)	(0.002)	(0.004)	(0.003)	(0.006)	(0.004)	(0.006)
Violent Crime	0.004*	0.004	0.003	0.002	0.006	0.001	0.003	-0.002
	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)	(0.004)	(0.003)	(0.005)
Drug Crime	0.002	0.002	0.001	-0.000	0.005	0.004	0.004	-0.003
	(0.002)	(0.003)	(0.002)	(0.004)	(0.003)	(0.006)	(0.003)	(0.006)
Number of charges								
Any crime	0.037	0.041	0.046*	-0.018	0.059*	0.068	0.065	-0.023
	(0.020)	(0.040)	(0.023)	(0.056)	(0.029)	(0.062)	(0.034)	(0.075)
Property Crime	0.018	0.020	0.026	-0.024	0.030	0.044	0.036	-0.013
	(0.013)	(0.030)	(0.014)	(0.049)	(0.022)	(0.051)	(0.024)	(0.056)
Violent Crime	0.009	0.011	0.008	0.001	0.014	0.016	0.010	-0.007
	(0.005)	(0.009)	(0.007)	(0.011)	(0.009)	(0.015)	(0.010)	(0.023)
Drug Crime	0.009	0.009	0.012	0.006	0.015	0.008	0.020*	-0.004
	(0.008)	(0.015)	(0.008)	(0.011)	(0.009)	(0.019)	(0.010)	(0.015)

Estimates are the intent-to-treat effects, from equation (2) where the characteristics of the assigned neighbors are measured in terms of shares of neighbors charged for criminal offenses residing in different locations. Column (1) reports estimates when Z_1 is the share of neighbors charged for criminal offenses residing in the assigned development. Column (2) reports the estimates when Z_1 is the share of neighbors charged for criminal offenses residing in the assigned section. Columns (3) and (4) report intent-to-treat estimates when Z_1 includes the share of neighbors charged for criminal offenses residing in the assigned development and the share of criminal neighbors residing in proximate developments within the assigned section. Columns (5) to (8) report similar estimates than (1) to (4) with the shares of neighbors charged for drug crime. The estimated equations all include waiting list controls. Robust standard errors adjusted for development clustering of the first-offered social housing unit (259 cells) are in parentheses. Sample size is 2230 14-29 years old individuals. * = p-value < 0.05, ** = p-value < 0.01.

Table 5

Effect of neighbors charged for drug crime on youth criminal charges, by gender and prior criminal behavior

	All			Males		Females		Without Prior Charges		With Prior Charges	
	SM	ITT	2SLS	SM	ITT	SM	ITT	SM	ITT	SM	ITT
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Probability of being charged											
Any crime	0.25	0.012*	0.015*	0.41	0.013*	0.12	0.007	0.11	0.005	0.50	0.016*
		(0.004)	(0.005)		(0.006)		(0.004)		(0.004)		(0.007)
Property Crime	0.16	0.008*	0.010*	0.24	0.008	0.08	0.005	0.06	0.002	0.33	0.012
		(0.003)	(0.004)		(0.005)		(0.003)		(0.003)		(0.007)
Violent Crime	0.10	0.006	0.007	0.18	0.008	0.03	0.002	0.03	0.003	0.21	0.007
		(0.003)	(0.004)		(0.005)		(0.002)		(0.003)		(0.006)
Drug Crime	0.13	0.005	0.006	0.25	0.010	0.03	-0.002	0.04	0.001	0.29	0.006
		(0.003)	(0.004)		(0.005)		(0.002)		(0.002)		(0.007)
Number of charges											
Any crime	1.09	0.059*	0.076	2.04	0.113*	0.30	-0.009	0.20	-0.012	2.69	0.136
		(0.029)	(0.040)		(0.056)		(0.018)		(0.014)		(0.071)
Property Crime	0.59	0.030	0.039	1.02	0.052	0.23	0.002	0.10	-0.012	1.47	0.075
		(0.022)	(0.030)		(0.043)		(0.016)		(0.011)		(0.053)
Violent Crime	0.22	0.014	0.018	0.44	0.028	0.03	-0.004	0.05	-0.000	0.53	0.029
		(0.009)	(0.012)		(0.019)		(0.004)		(0.005)		(0.023)
Drug Crime	0.28	0.015	0.019	0.57	0.032*	0.04	-0.007	0.05	0.000	0.69	0.031
		(0.009)	(0.012)		(0.016)		(0.005)		(0.005)		(0.021)
First stage D			0.773***								
			(0.029)								

SM, sample mean; ITT, intent-to-treat, from Equation (2), where Z_1 is the share of neighbors charged for drug offenses residing in the assigned development; 2SLS, treatment-on-treated, from Equation (1), where D is the share of neighbors charged for drug offenses residing in the residential development. The results reported by columns (5), (7), (9) and (11) are estimated π_{10} and π_{11} from $Y = (1 - G)(Z\pi_{10} + X\beta_{10}) + G(Z\pi_{11} + X\beta_{11}) + v$, where in columns (5) and (7) G is an indicator for female gender, and in columns (9) and (11) G is an indicator for the individual has been charged within the 4 years preceding the assignment of social housing offer. Robust standard errors adjusted for development clustering of the first-offered social housing unit (259 cells for ITT and 263 cells for 2SLS) are in parentheses. The estimated equations all include waiting list controls. Sample size is 2230 14-29 years old individuals, 1006 of them are males, 1224 are females, 1438 had no charges before social housing and 792 had at least a charge before social housing . * = p-value < 0.05, *** = p-value < 0.001.

Table 6

Effect of neighbors charged for drug crime residing in development on youth criminal charges, by exposure

	After 6 months exposure	After 12 months exposure	After 18 months exposure	After 24 months exposure
Probability of being charged	0.003	0.009*	0.012*	0.012*
	(0.003)	(0.003)	(0.004)	(0.004)
Number of charges	0.020	0.036*	0.050*	0.059*
	(0.012)	(0.017)	(0.020)	(0.029)

Estimates are the intent-to-treat effects, from equation (2) where Z_1 is the share of neighbors charged for drug criminal offenses residing in the assigned development. Robust standard errors adjusted for development clustering of the first-offered social housing unit (259 cells) are in parentheses. The estimated equations all include waiting list controls. Sample size is 2230 14-29 years old individuals. * = p-value < 0.05.

Table 7

Effect of neighbors charged for drug crime interacted with number of charges in neighborhood

	Neighbors Charged for Drug Crime	Charges per Charged Neighbor	Neighbors Charged for Drug Crime × Charges per Charged Neighbor
	(1)	(2)	(3)
Probability of being property crime charged			
Any crime	0.015*	0.000	-0.000
	(0.006)	(0.000)	(0.000)
Property Crime	0.012*	0.000	-0.000
	(0.005)	(0.000)	(0.000)
Violent Crime	0.010*	0.000	-0.000
	(0.005)	(0.000)	(0.000)
Drug Crime	0.010*	0.000	-0.000
	(0.004)	(0.000)	(0.000)
Number of crime charges			
Any crime	0.053	-0.000	0.000
	(0.043)	(0.001)	(0.000)
Property Crime	0.025	-0.000	0.000
	(0.030)	(0.000)	(0.000)
Violent Crime	0.020	0.000	-0.000
	(0.013)	(0.000)	(0.000)
Drug Crime	0.008	-0.000	0.000
	(0.014)	(0.000)	(0.000)

Estimates in columns (1) to (3) are the intent-to-treat effects π_1, π_2 and π_2, from the equation (6) where Z_1 is the share of neighbors charged for drug criminal offenses residing in the assigned development, and Z_2 is the number of charges per charged neighbor. Robust standard errors adjusted for development clustering of the first-offered social housing unit (259 cells) are in parentheses. The estimated equations all include waiting list controls. Sample size is 2230 14-29 years old individuals. * = p-value < 0.05.

APPENDIX

Table A.1

Correlations between characteristics of youth applicants and neighbors in first-offered social housing apartments (A. No controls)

	Neighbors charged for property crime in develop.	Neighbors charged for violent crime in develop.	Neighbors charged for drug crime in develop.	Income of Neighbors in develop.	Poor Neighbors in develop.	Non-employed neighbors in develop.	Neighbors with upper secondary education in develop.	Neighbors with higher education in develop.	Immigrant Neighbors In develop.	Neighbors charged for Property crime in local area	Neighbors charged for violent crime in local area	Neighbors charged for drug crime in local area
Probability of being charged before 1st offer	1.46*	1.74*	1.80*	-0.00*	0.42	0.53*	0.07	-0.01	0.13	2.88*	2.35	2.98*
Property crime	0.48	1.32*	1.09*	-0.00*	0.37	0.45*	0.06	-0.00	0.13*	0.80	1.56	1.58*
Violent crime	0.86*	1.04*	0.72	-0.00	0.10	0.29*	-0.06	-0.05	0.06	1.62*	1.69*	1.40
Drug crime	1.33*	1.66*	1.44*	-0.00*	0.20	0.43*	0.08	0.09	-0.00	1.46*	1.42	2.08*
Number of charges before 1st offer	10.65	18.86*	14.00*	-0.00*	3.18	6.27*	0.33	0.95	1.34	10.71	9.65	13.04
Property crime	4.96	7.93	8.50*	-0.00*	3.09*	4.24*	0.77	1.34	1.03	3.40	-1.05	4.47
Violent crime	1.41	3.16	1.07	-0.00	0.34	0.81*	-0.13	-0.33	0.34	2.69	4.45	3.45
Drug crime	4.28*	7.78*	4.43*	-0.00	-0.25	1.22*	-0.30	-0.05	-0.03	4.62	6.25	5.12
Probability of being co-charged before 1st	0.42	0.42	0.33	-0.00*	0.14	0.19*	0.14	0.03	0.03	0.35	0.21	0.27
Property crime	0.20	0.21	0.26	-0.00*	0.15	0.16	0.09	-0.01	0.05	-0.07	0.01	0.32
Violent crime	0.27	0.28	0.01	-0.00	-0.02	0.05	0.05	0.02	-0.01	0.19	0.26	-0.18
Drug crime	-0.01	-0.08	-0.05	0.00	0.02	-0.01	0.04*	0.03	-0.01	-0.05	-0.24*	-0.22*
Persons charged before 1st offer	5.94	10.42*	8.12*	-0.00*	1.34	2.97*	0.88	-0.67	0.60	11.56*	14.22	13.05*
Property crime	1.98	3.48	3.99*	-0.00*	1.50	1.41*	0.72	-0.22	0.37	3.89	1.40	3.65
Violent crime	2.41	4.60	2.10	-0.00*	-0.13	0.89*	-0.07	-0.60	0.23	5.28	11.59	6.48
Drug crime	1.55	2.34	2.03*	-0.00	-0.02	0.67*	0.23	0.16	-0.00	2.40	1.23	2.92
Female	-0.95	-1.87*	-1.45*	0.00	-0.28	-0.43*	-0.06	-0.07	-0.07	-1.02	-0.95	-1.73
Immigrant	-0.94	-0.24	-0.43	0.00	-0.28	-0.30*	-0.17	-0.16	-0.02	-1.91*	-0.66	-1.23
Study support	0.61*	0.44	0.40	0.00	-0.02	0.05	0.00	0.05	0.02	0.64	0.76	0.68
Social assistance (active)	0.54	1.06*	0.34	-0.00	-0.11	0.09	-0.21*	-0.21*	-0.04	0.39	1.51*	0.20
Social assistance (passive)	1.81*	2.06*	2.46*	-0.00*	-0.01	0.45*	0.19	0.11	-0.11	1.60	1.39	3.46*
Sickness benefits	0.17	0.33	0.34*	-0.00	-0.04	0.07	-0.01	-0.01	-0.02	-0.03	0.42	0.33
Disability support	-0.07	0.01	-0.05	0.00	-0.02	-0.00	0.00	0.02	0.00	-0.13	-0.17	-0.09
Occupies halfway house	-0.24	-0.00	-0.49	-0.00*	0.36*	0.07	0.23*	-0.07	0.09	-0.16	0.70	-0.37
Homelessness	1.02*	0.92	1.36*	-0.00	-0.23	0.34*	0.02	-0.07	-0.05	1.72*	0.46	1.97*
Overall problem	-0.18	-0.46	-0.02	0.00	-0.10	-0.09	-0.00	0.27*	-0.12*	-1.08	-0.85	-1.16
Occupies public housing	-1.30*	-1.81*	-1.72*	0.00*	-0.38*	-0.45*	-0.11	-0.14	0.00	-1.58*	-2.13	-2.31*
Lives with family or friends	0.23	0.49	0.53	-0.00*	0.15	0.19	-0.02	0.09	-0.03	0.58	0.91	0.66
Not occupy private residence	1.16*	1.72*	1.35*	-0.00	-0.14	0.38*	-0.04	-0.04	0.02	1.43	0.90	1.58*
Occupies public institution	-0.07	0.13	-0.33	-0.00	0.10	-0.07	0.08	-0.15	0.02	0.38	1.07	-0.14
Balance test (p-value)	0.21	0.04	0.00	0.00	0.03	0.00	0.22	0.05	0.42	0.18	0.53	0.07

(B. Waiting list controls)

	Neighbors charged for property crime in develop.	Neighbors charged for violent crime in develop.	Neighbors charged for drug crime in develop.	Income of Neighbors in develop.	Poor Neighbors in develop.	Non-employed neighbors in develop.	Neighbors with upper secondary education in develop.	Neighbors with higher education in develop.	Immigrant Neighbors In develop.	Neighbors charged for Property crime in local area	Neighbors charged for violent crime in local area	Neighbors charged for drug crime in local area
Probability of being charged before 1st offer	0.40	0.14	0.67	-0.00	0.03	0.22	0.02	-0.01	0.13	1.75*	0.93	1.89*
Property crime	-0.17	0.32	0.40	-0.00	0.03	0.25*	0.01	-0.02	0.13*	0.06	0.70	1.01
Violent crime	0.39	0.32	0.18	0.00	-0.12	0.14	-0.09	-0.05	0.06	1.12	1.04	0.92
Drug crime	0.41	0.34	0.45	-0.00	-0.00	0.17	0.07	0.11	-0.00	0.51	0.20	0.98
Number of charges before 1st offer	1.01	5.02	3.54	-0.00	-0.03	3.57*	-0.08	0.99	1.34	0.58	-3.35	2.32
Property crime	-0.66	-0.46	2.46	-0.00	0.93	2.65*	0.46	1.33	1.03	-2.69	-8.71	-1.53
Violent crime	-0.12	0.97	-0.68	0.00	-0.26	0.36	-0.21	-0.34	0.34	1.12	2.43	1.88
Drug crime	1.79	4.52	1.76	0.00	-0.71	0.55	-0.32	-0.00	-0.03	2.15	2.94	1.96
Probability of being co-charged before 1st	0.19	-0.02	0.04	-0.00	-0.10	0.09	0.10	0.02	0.03	0.02	-0.15	0.11
Property crime	0.01	-0.15	0.04	-0.00	-0.07	0.09	0.05	-0.03	0.05	-0.34	-0.27	0.23
Violent crime	0.19	0.16	-0.09	0.00	-0.08	0.02	0.04	0.02	-0.01	0.09	0.15	-0.25
Drug crime	-0.04	-0.13*	-0.09	0.00	0.01	-0.02	0.04*	0.03	-0.01	-0.08	-0.28*	-0.26*
Persons charged before 1st offer	1.18	3.21	2.89	-0.00	-1.05	1.49	0.54	-0.72	0.60	6.44	7.72	8.52
Property crime	-0.10	0.03	1.70	-0.00	-0.05	0.69	0.46	-0.29	0.37	1.41	-1.56	2.02
Violent crime	1.31	3.01	0.83	-0.00	-0.70	0.55	-0.14	-0.62	0.23	4.17	10.11	5.47
Drug crime	-0.02	0.18	0.36	0.00	-0.31	0.26	0.22	0.18	-0.00	0.86	-0.83	1.04
Female	-0.04	-0.53	-0.55	-0.00	-0.05	-0.18	-0.05	-0.06	-0.07	0.04	0.19	-0.81
Immigrant	-0.14	1.05	0.49	-0.00	-0.04	-0.06	-0.15	-0.15	-0.02	-1.14	0.51	-0.44
Study support	0.56*	0.36	0.34	0.00	-0.06	0.03	-0.01	0.04	0.02	0.57	0.68	0.63
Social assistance (active)	0.31	0.83*	0.08	0.00	-0.10	0.04	-0.21*	-0.20*	-0.04	0.20	1.23	-0.17
Social assistance (passive)	0.20	-0.02	0.76	-0.00	-0.07	0.05	0.21*	0.18	-0.11	0.16	-0.72	1.24
Sickness benefits	0.06	0.21	0.23	-0.00	0.01	0.05	-0.00	-0.00	-0.02	-0.12	0.28	0.14
Disability support	-0.15	-0.10	-0.14	0.00	-0.02	-0.02	0.00	0.02	0.00	-0.21	-0.28	-0.19
Occupies halfway house	-0.37	-0.28	-0.70	-0.00	0.11	-0.00	0.18*	-0.09	0.09	-0.36	0.53	-0.34
Homelessness	0.38	0.12	0.73	-0.00	-0.12	0.21	0.06	-0.04	-0.05	1.16	-0.36	1.03
Overall problem	0.02	-0.21	0.21	0.00	-0.10	-0.04	-0.00	0.26*	-0.12*	-0.92	-0.60	-0.91
Occupies public housing	-0.16	-0.16	-0.49	0.00	-0.09	-0.12	-0.09	-0.14	0.00	-0.44	-0.57	-1.03
Lives with family or friends	-0.08	0.01	0.20	-0.00	-0.03	0.10	-0.05	0.08	-0.03	0.24	0.49	0.41
Not occupy private residence	0.38	0.71	0.55	-0.00	-0.16	0.19	-0.02	-0.01	0.02	0.69	-0.11	0.54
Occupies public institution	-0.20	-0.10	-0.53	0.00	-0.06	-0.14	0.05	-0.16	0.02	0.24	0.89	-0.21
Balance test (p-value)	1.00	0.91	0.34	0.61	0.35	0.33	0.28	0.05	0.42	0.64	0.87	0.53

Table A.1 (Continued)

(C. Waiting list controls and local area effects)

	Neighbors charged for property crime in develop.	Neighbors charged for violent crime in develop.	Neighbors charged for drug crime in develop.	Income of Neighbors in develop.	Poor Neighbors in develop.	Non-employed neighbors in develop.	Neighbors with upper secondary education in develop.	Neighbors with higher education in develop.	Immigrant Neighbors In develop.	Neighbors charged for Property crime in local area	Neighbors charged for violent crime in local area	Neighbors charged for drug crime in local area
Probability of being charged before 1st offer	0.24	-0.27	0.54	0.00	-0.24	0.15	-0.07	0.03	0.12	1.62	-0.21	1.74
Property crime	-0.43	-0.03	0.29	-0.00	-0.15	0.19	-0.09	0.03	0.12	-0.49	-0.27	0.53
Violent crime	0.09	-0.31	-0.26	0.00	-0.24*	0.06	-0.12	-0.09	0.11	0.86	0.04	0.15
Drug crime	0.37	0.02	0.30	0.00	-0.20*	0.25*	0.02	0.06	0.02	0.56	-0.59	0.88
Number of charges before 1st offer	-3.03	1.70	2.01	-0.00	-1.43	2.83	-0.26	-0.73	2.15*	-11.39	-15.94	-9.34
Property crime	-3.19	-0.71	2.43	-0.00	0.25	1.82	0.51	-0.25	1.46*	-11.72	-14.17	-7.71
Violent crime	-0.75	-0.78	-1.94	0.00	-0.53	0.14	-0.37	-0.28	0.49*	-0.12	-1.12	-1.23
Drug crime	0.91	3.19	1.52	0.00	-1.15*	0.87*	-0.39	-0.20	0.20	0.45	-0.66	-0.41
Probability of being co-charged before 1st	0.09	-0.25	-0.01	0.00	-0.22	0.10	0.03	0.04	0.03	-0.12	-0.84	-0.42
Property crime	-0.07	-0.32	0.02	-0.00	-0.16	0.10	-0.01	-0.04	0.04	-0.42	-0.82	0.04
Violent crime	0.13	0.04	-0.21	0.00	-0.13*	0.01	0.01	0.03	-0.00	0.08	0.05	-0.72
Drug crime	0.01	-0.07	-0.05	0.00	0.00	-0.00	0.03	0.01	-0.01	-0.07	-0.22	-0.25
Persons charged before 1st offer	-1.17	-0.17	1.04	0.00	-1.98*	1.20	-0.22	-0.59	0.66	-0.97	-4.40	-4.71
Property crime	-1.02	-0.99	1.92	-0.00	-0.54	0.60	0.19	-0.40	0.22	-2.08	-5.57	-1.44
Violent crime	-0.38	0.62	-1.75	0.00	-0.86	0.16	-0.57	-0.31	0.43	0.15	2.60	-4.91*
Drug crime	0.24	0.20	0.86	0.00	-0.59*	0.43	0.16	0.12	0.01	0.97	-1.42	1.64
Female	-0.12	-0.12	-0.34	-0.00	0.06	-0.24	-0.07	-0.22	-0.09	0.39	2.27	-0.40
Immigrant	-0.65	0.48	0.56	-0.00	0.08	-0.24	-0.07	-0.23	0.06	-1.50	-0.11	-1.32
Study support	0.61*	0.25	0.28	0.00	-0.19*	-0.03	-0.06	-0.09	0.04	0.58	0.83	0.55
Social assistance (active)	0.25	0.81	0.10	0.00	-0.07	-0.02	-0.22*	-0.13	-0.10*	0.07	1.33	-0.52
Social assistance (passive)	0.21	-0.22	0.61	-0.00	0.03	0.11	0.33*	0.25	-0.06	0.31	-1.79	1.39
Sickness benefits	0.07	0.33	0.28*	-0.00	0.03	0.06	-0.01	-0.05	-0.01	-0.00	0.51	0.27
Disability support	-0.09	0.03	-0.11	0.00	-0.03	0.01	-0.01	0.01	-0.00	-0.20	-0.31	-0.24
Occupies halfway house	-0.41	-0.58	-0.70	-0.00	0.14	0.06	0.22*	-0.08	0.12	-0.59	-0.02	-0.45
Homelessness	0.02	0.15	0.72	-0.00	-0.09	0.23	0.13	-0.19	-0.09	1.05	-0.57	1.58
Overall problem	0.31	0.24	0.16	0.00	-0.15	-0.08	-0.05	0.30*	-0.19*	-0.54	0.74	-0.80
Occupies public housing	-0.44	-0.50	-0.82*	0.00	-0.04	-0.15	-0.06	0.03	0.05	-0.63	-1.12	-1.82*
Lives with family or friends	-0.05	0.28	-0.05	0.00	-0.13	0.11	-0.08	0.09	-0.04	0.09	0.93	0.10
Not occupy private residence	0.59	1.24	0.80	-0.00	-0.22	0.20	-0.00	-0.20	-0.03	1.24	0.24	1.56
Occupies public institution	-0.56	-0.73	-0.64	0.00	0.01	-0.22*	0.05	-0.09	-0.04	-0.35	-0.00	-0.63
Balance test (p-value)	0.91	0.75	0.43	0.77	0.65	0.17	0.19	0.54	0.16	0.73	0.95	0.59

The first thirty rows of each panel show the estimated θ from equation (3) for each individual characteristic X_{j-} in the row and each neighborhood characteristic N_1 in the column. The rows corresponding to Balance test (p-value) show the p-value of the test $\theta=0$ from the equation (4) where the set of individual characteristics X_- include all individual characteristics listed in Table A.1 at the Appendix. The estimated models reported in Panel A do not control for waiting list characteristics W, the results shown in Panel B control for W, and the estimates in Panel C control for W and local area fixed effects. Robust standard errors are adjusted for development clustering of the first-offered social housing unit (259 cells). Sample size is 2230 14-29 years old individuals. * = p-value < 0.05.

Table A.2

Effect of criminal neighbors and drug criminal neighbors given fixed local area effects, by location of neighbors

	Neighbors charged residing in development	Neighbors charged residing in section	Neighbors charged residing in development	Neighbors charged residing in proximate developments	Neighbors charged for drug crime residing in development	Neighbors charged for drug crime residing in section	Neighbors charged for drug crime residing in development	Neighbors charged for drug crime residing in proximate developments
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Probability of being charged								
Any crime	0.004	0.000	0.004	0.000	0.010*	0.006	0.009*	-0.003
	(0.003)	(0.005)	(0.003)	(0.006)	(0.004)	(0.010)	(0.004)	(0.008)
Property Crime	0.005*	0.003	0.005*	0.001	0.008*	0.011	0.008*	0.001
	(0.002)	(0.004)	(0.002)	(0.004)	(0.004)	(0.008)	(0.004)	(0.006)
Violent Crime	0.003	0.002	0.003	0.002	0.004	-0.005	0.003	-0.002
	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)	(0.006)	(0.003)	(0.005)
Drug Crime	0.001	-0.001	0.001	-0.000	0.004	0.000	0.004	-0.003
	(0.002)	(0.004)	(0.002)	(0.004)	(0.003)	(0.007)	(0.003)	(0.006)
Number of charges								
Any crime	0.045*	0.020	0.046*	-0.018	0.067*	0.032	0.065	-0.023
	(0.022)	(0.045)	(0.023)	(0.056)	(0.034)	(0.070)	(0.034)	(0.075)
Property Crime	0.024	0.007	0.026	-0.024	0.037	0.023	0.036	-0.013
	(0.014)	(0.032)	(0.014)	(0.049)	(0.023)	(0.054)	(0.024)	(0.056)
Violent Crime	0.008	0.003	0.008	0.001	0.011	-0.001	0.010	-0.007
	(0.006)	(0.012)	(0.007)	(0.011)	(0.010)	(0.019)	(0.010)	(0.023)
Drug Crime	0.013	0.009	0.012	0.006	0.020*	0.010	0.020*	-0.004
	(0.008)	(0.016)	(0.008)	(0.011)	(0.010)	(0.019)	(0.010)	(0.015)

Estimates are the intent-to-treat effects, from equation (2) where the characteristics of the assigned neighbors are measured in terms of shares of neighbors charged for criminal offenses residing in different locations. Column (1) reports estimates when Z_1 is the share of neighbors charged for criminal offenses residing in the assigned development. Column (2) reports the estimates when Z_1 is the share of neighbors charged for criminal offenses residing in the assigned section. Columns (3) and (4) report intent-to-treat estimates when Z_1 includes the share of neighbors charged for criminal offenses residing in the assigned development and the share of criminal neighbors residing in proximate developments within the assigned section. Columns (5) to (8) report similar estimates than (1) to (4) with the shares of neighbors charged for drug crime. The estimated equations all include waiting list controls and local area fixed effects. Robust standard errors adjusted for development clustering of the first-offered social housing unit (259 cells) are in parentheses. Sample size is 2230 14-29 years old individuals. * = p-value < 0.05, ** = p-value < 0.01.

Table A.3

Effect of property criminal neighbors and violent criminal neighbors, by location of neighbors

	Neighbors charged for property crime residing in development (1)	Neighbors charged for property crime residing in section (2)	Neighbors charged for property crime residing in development (3)	Neighbors charged for property crime residing in proximate developments (4)	Neighbors charged for violent crime residing in development (5)	Neighbors charged for violent crime residing in section (6)	Neighbors charged for violent crime residing in development (7)	Neighbors charged for violent crime residing in proximate developments (8)
Probability of being charged								
Any crime	0.003	0.008	-0.003	0.004	0.006	0.005	-0.000	-0.007
	(0.004)	(0.006)	(0.005)	(0.007)	(0.005)	(0.010)	(0.006)	(0.011)
Property Crime	0.004	0.006	0.000	0.001	0.004	0.005	-0.001	-0.002
	(0.003)	(0.005)	(0.004)	(0.006)	(0.004)	(0.008)	(0.005)	(0.008)
Violent Crime	0.003	0.008	0.000	0.005	0.002	0.001	-0.000	-0.004
	(0.003)	(0.004)	(0.003)	(0.004)	(0.003)	(0.006)	(0.004)	(0.005)
Drug Crime	0.001	0.004	-0.000	0.002	0.003	-0.004	0.001	-0.008
	(0.003)	(0.005)	(0.003)	(0.006)	(0.005)	(0.009)	(0.005)	(0.008)
Number of charges								
Any crime	0.030	0.065	0.040	-0.006	0.046	-0.023	0.052	-0.116
	(0.028)	(0.055)	(0.034)	(0.075)	(0.039)	(0.077)	(0.046)	(0.088)
Property Crime	0.010	0.027	0.022	-0.027	0.020	-0.009	0.027	-0.064
	(0.017)	(0.035)	(0.024)	(0.064)	(0.027)	(0.064)	(0.029)	(0.076)
Violent Crime	0.008	0.013	0.006	0.005	0.010	-0.002	0.007	-0.022
	(0.007)	(0.013)	(0.008)	(0.016)	(0.011)	(0.020)	(0.012)	(0.019)
Drug Crime	0.012	0.025	0.012	0.016	0.016	-0.013	0.018	-0.030
	(0.013)	(0.024)	(0.012)	(0.017)	(0.017)	(0.030)	(0.018)	(0.024)

Estimates are the intent-to-treat effects, from equation (2) where the characteristics of the assigned neighbors are measured in terms of shares of neighbors charged for criminal offenses residing in different locations. Column (1) reports estimates when Z_1 is the share of neighbors charged for property crime offenses residing in the assigned development. Column (2) reports the estimates when Z_1 is the share of neighbors charged for property crime offenses residing in the assigned section. Columns (3) and (4) report intent-to-treat estimates when Z_1 includes the share of neighbors charged for property crime offenses residing in the assigned development and the share of property crime neighbors residing in proximate developments within the assigned section. Columns (5) to (8) report similar estimates than (1) to (4) with the shares of neighbors charged for violent crime. The estimated equations all include waiting list controls. Robust standard errors adjusted for development clustering of the first-offered social housing unit (259 cells) are in parentheses. Sample size is 2230 14-29 years old individuals.

Table A.4

Effect of property criminal neighbors and violent criminal neighbors given fixed local area effects, by location of neighbors

	Neighbors charged for property crime residing in development (1)	Neighbors charged for property crime residing in section (2)	Neighbors charged for property crime residing in development (3)	Neighbors charged for property crime residing in proximate developments (4)	Neighbors charged for violent crime residing in development (5)	Neighbors charged for violent crime residing in section (6)	Neighbors charged for violent crime residing in development (7)	Neighbors charged for violent crime residing in proximate developments (8)
Probability of being charged								
Any crime	-0.002	-0.001	-0.003	0.004	-0.000	-0.013	-0.000	-0.007
	(0.004)	(0.007)	(0.005)	(0.007)	(0.006)	(0.013)	(0.006)	(0.011)
Property Crime	0.000	-0.002	0.000	0.001	-0.001	-0.007	-0.001	-0.002
	(0.004)	(0.006)	(0.004)	(0.006)	(0.005)	(0.010)	(0.005)	(0.008)
Violent Crime	0.001	0.004	0.000	0.005	-0.000	-0.005	-0.000	-0.004
	(0.003)	(0.005)	(0.003)	(0.004)	(0.004)	(0.007)	(0.004)	(0.005)
Drug Crime	0.000	0.001	-0.000	0.002	0.001	-0.014	0.001	-0.008
	(0.003)	(0.005)	(0.003)	(0.006)	(0.005)	(0.010)	(0.005)	(0.008)
Number of charges								
Any crime	0.039	0.032	0.040	-0.006	0.046	-0.103	0.052	-0.116
	(0.031)	(0.059)	(0.034)	(0.075)	(0.046)	(0.084)	(0.046)	(0.088)
Property Crime	0.017	0.010	0.022	-0.027	0.024	-0.047	0.027	-0.064
	(0.020)	(0.037)	(0.024)	(0.064)	(0.029)	(0.060)	(0.029)	(0.076)
Violent Crime	0.007	0.005	0.006	0.005	0.005	-0.027	0.007	-0.022
	(0.008)	(0.015)	(0.008)	(0.016)	(0.012)	(0.022)	(0.012)	(0.019)
Drug Crime	0.015	0.018	0.012	0.016	0.017	-0.028	0.018	-0.030
	(0.012)	(0.023)	(0.012)	(0.017)	(0.019)	(0.036)	(0.018)	(0.024)

Estimates are the intent-to-treat effects, from equation (2) where the characteristics of the assigned neighbors are measured in terms of shares of neighbors charged for criminal offenses residing in different locations. Column (1) reports estimates when Z_1 is the share of neighbors charged for property crime offenses residing in the assigned development. Column (2) reports the estimates when Z_1 is the share of neighbors charged for property crime offenses residing in the assigned section. Columns (3) and (4) report intent-to-treat estimates when Z_1 includes the share of neighbors charged for property crime offenses residing in the assigned development and the share of property crime neighbors residing in proximate developments within the assigned section. Columns (5) to (8) report similar estimates than (1) to (4) with the shares of neighbors charged for violent crime. The estimated equations all include waiting list controls and local area fixed effects. Robust standard errors adjusted for development clustering of the first-offered social housing unit (259 cells) are in parentheses. Sample size is 2230 14-29 years old individuals.

Table A.5

Effect of drug criminal neighbors on neighbor co-offending, by characteristics of youth

	All	Males	Prior criminal charges	Prior property criminal charges	Prior drug criminal charges
	(1)	(2)	(3)	(4)	(5)
Probability of youth being charged with new neighbor					
Any crime	0.001	0.004	0.005	0.006	0.011
	(0.002)	(0.004)	(0.004)	(0.006)	(0.007)
Property Crime	-0.000	0.001	0.001	0.001	0.002
	(0.001)	(0.003)	(0.003)	(0.004)	(0.004)
Violent Crime	-0.000	0.000	0.000	0.001	0.003
	(0.001)	(0.002)	(0.002)	(0.003)	(0.005)
Drug Crime	-0.000	-0.001	-0.000	-0.000	0.002
	(0.001)	(0.002)	(0.002)	(0.002)	(0.004)
Number of neighbors charged with youth					
Any crime	0.003	0.008	0.009	0.012	0.020
	(0.003)	(0.006)	(0.007)	(0.010)	(0.012)
Property Crime	0.000	0.001	0.002	0.003	0.003
	(0.002)	(0.004)	(0.004)	(0.006)	(0.007)
Violent Crime	0.001	0.003	0.004	0.005	0.011
	(0.002)	(0.004)	(0.005)	(0.006)	(0.009)
Drug Crime	0.000	0.000	0.001	0.002	0.006
	(0.001)	(0.002)	(0.002)	(0.003)	(0.005)

Estimates in column (1) are the intent-to-treat effects, from equation (2) where the characteristics of the assigned neighbors are measured in terms of shares of neighbors charged for drug crime offenses residing in development. The results reported in columns (2) to (5) are estimated intent-to-treat effects π_{11} from equation (6) π_{11} where in column (2) G is an indicator for male gender, in the in column (3) G is an indicator for prior charges, in column (4) G is an indicator for prior property crime charges, and in column (5) G is an indicator for prior drug crime charges. Robust standard errors adjusted for development clustering of the first-offered social housing unit (259 cells) are in parentheses. The estimated equations all include baseline application covariates. Sample size is 2230 14-29 years old individuals, 1006 of them are males, 792 had at least a charge before social housing, 579 had at least a charge for property crime, and 382 had at least a charge for drug crime .

Table A.6

Robustness of the neighborhood effect to the measurement of drug criminal neighbors

	15-29 years old neighbors charged for drug crime	30+ years old neighbors charged for drug crime	Neighbors convicted for drug crime	Neighbors charged for drug crime committed the last 2 years
	(1)	(2)	(3)	(4)
Probability of being charged				
Any crime	0.002	0.010***	0.012*	0.012*
	(0.002)	(0.003)	(0.004)	(0.005)
Property Crime	0.001	0.006*	0.007*	0.009*
	(0.002)	(0.003)	(0.003)	(0.004)
Violent Crime	0.000	0.005*	0.005	0.003
	(0.002)	(0.002)	(0.003)	(0.003)
Drug Crime	0.001	0.004	0.005	0.001
	(0.002)	(0.002)	(0.003)	(0.003)
Number of charges				
Any crime	0.005	0.052*	0.051	0.050
	(0.018)	(0.023)	(0.035)	(0.035)
Property Crime	0.001	0.029	0.030	0.025
	(0.013)	(0.017)	(0.027)	(0.026)
Violent Crime	0.002	0.009	0.011	0.012
	(0.005)	(0.006)	(0.009)	(0.009)
Drug Crime	0.002	0.014	0.010	0.013
	(0.005)	(0.008)	(0.010)	(0.011)

Estimates are the intent-to-treat effects, from equation (2), where in column (1) Z_1 is the share of 15-29 neighbors charged for drug offenses residing in the assigned development; in column (2) Z_1 is the share of 30+ neighbors charged for drug offenses residing in the assigned development, in column (3) Z_1 is the share of neighbors convicted for drug offenses residing in the assigned development, and in (4) Z_1 is the share of assigned development neighbors charged for drug offenses within the 2 years before social housing. Robust standard errors adjusted for development clustering of the (259 cells for first-offered social housing units) are in parentheses. The estimated equations all include waiting list controls. Sample size is 2230 14-29 years old individuals. * = p-value < 0.05, *** = p-value < 0.001.

Table A.7

Robustness of the neighborhood effect to characteristics of neighborhoods and youth

	Non-employed neighbors	Neighbors with upper secondary education	Poor neighbor households	Immigrant neighbors	Prior criminal behavior	Housing need urgency
	(1)	(2)	(3)	(4)	(5)	(6)
Probability of being charged						
Any crime	0.010*	0.012*	0.012*	0.013***	0.011*	0.011*
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)
Property Crime	0.005	0.008*	0.008*	0.009*	0.007*	0.008*
	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Violent Crime	0.005	0.005	0.005	0.006*	0.005	0.005
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Drug Crime	0.003	0.005	0.005	0.007*	0.004	0.005
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Number of charges						
Any crime	0.046	0.055	0.059*	0.075*	0.052	0.058*
	(0.032)	(0.029)	(0.030)	(0.028)	(0.027)	(0.029)
Property Crime	0.027	0.028	0.029	0.044*	0.026	0.031
	(0.024)	(0.021)	(0.022)	(0.020)	(0.021)	(0.022)
Violent Crime	0.009	0.013	0.014	0.015	0.012	0.013
	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Drug Crime	0.009	0.014	0.016	0.016	0.014	0.014
	(0.009)	(0.009)	(0.009)	(0.010)	(0.009)	(0.009)

Estimates are the intent-to-treat effects, from equation (2), where Z_1 is the share of neighbors charged for drug offenses residing in the assigned development. The estimates reported in column (2) control for waiting list controls and for the share of non-employed neighbors residing in the assigned development. The estimates reported in column (3) control for waiting list controls and for the share of assigned development neighbors with at least upper secondary education as highest achieved education. The estimates reported in column (4) control for waiting list controls and for the share of assigned development poor neighbors according with the poverty threshold in Denmark 2014. The estimates reported in column (5) control for waiting list controls and for individual previous property, violent, and drug criminal charges. The estimates reported in column (6) control for waiting list controls and housing need urgency specified in the social housing application. Robust standard errors adjusted for development clustering of the (259 cells for first-offered social housing units) are in parentheses.. Sample size is 2230 14-29 years old individuals. * = p-value < 0.05, *** = p-value < 0.001.

Table A.8

Effect of neighbors charged for drug crime interacted with number of neighbors

	Neighbors Charged for Drug Crime	Number of Neighbors	Neighbors Charged for Drug Crime × Number of Neighbors
	(1)	(2)	(3)
Probability of being charged			
Any crime	0.014*	0.000	-0.000
	(0.006)	(0.000)	(0.000)
Property Crime	0.011*	0.000	-0.000
	(0.005)	(0.000)	(0.000)
Violent Crime	0.010*	0.000	-0.000
	(0.005)	(0.000)	(0.000)
Drug Crime	0.009*	0.000	-0.000
	(0.004)	(0.000)	(0.000)
Number of crime charges			
Any crime	0.053	-0.000	0.000
	(0.043)	(0.000)	(0.000)
Property Crime	0.026	-0.000	0.000
	(0.030)	(0.000)	(0.000)
Violent Crime	0.022	0.000	-0.000
	(0.013)	(0.000)	(0.000)
Drug Crime	0.005	-0.000	0.000
	(0.014)	(0.000)	(0.000)

Estimates in columns (1) to (3) are the intent-to-treat effects π_1, π_2 and π_2, from the equation (6) where Z_1 is the share of neighbors charged for drug criminal offenses residing in the assigned development, and Z_2 is the number of neighbors of the development. Robust standard errors adjusted for development clustering of the first-offered social housing unit (259 cells) are in parentheses. The estimated equations all include waiting list controls. Sample size is 2230 14-29 years old individuals. * = p-value < 0.05.

a. Histogram of concentration of neighbors with drug charges within application type
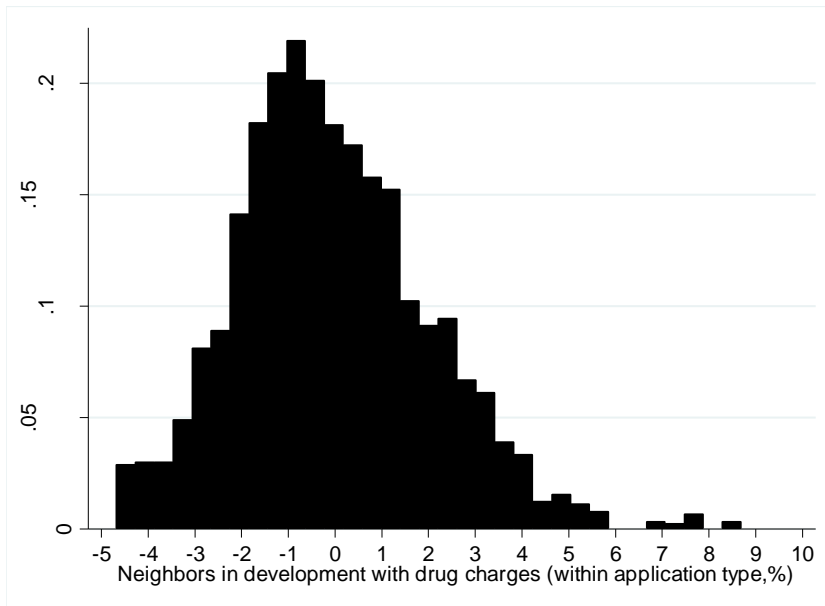
b. Histogram of concentration of neighbors with drug charges within application type and housing section
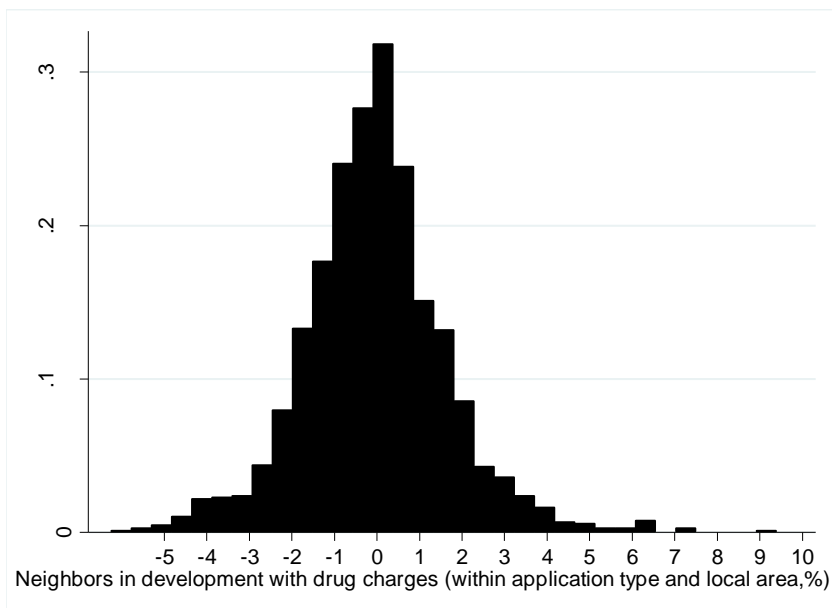

Fig. A.1

Distributions of neighbors with prior drug offenses given application type across social housing developments and within sections (local areas) in Copenhagen