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Neighborhood Effects on Youth Crime: Natural Experimental Evidence

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ABSTRACT

We investigate the degree to which youth (ages 14-29) 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 with drug criminal records are 6% more likely to be charged for criminal offenses (both drug and property crimes), and this impact manifests itself after six months of exposure. This neighborhood effect is stronger for previous offenders, and does not lead to criminal partnerships. Our exploration of alternative mechanisms suggests youth interaction in proximate residential context with older adults with drug crime experience as the most plausible source of neighborhood effects.

Keywords: Neighborhood effects; Natural experiments; Urban youth criminality

JEL classification: J13, K42, R23, Z13

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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]

The social interaction of youth with other offenders in the residential context may increase youth criminality through one or more economic mechanisms. Such social contact may encourage illegal activity by increasing youths' perceived benefits of crime (Sah 1991; Cook and Goss 1996, Becker and Murphy 2000, Chung 2000), by providing youth with information and criminogenic resources (Bayer, Hjalmarsson, and Pozen 2009), or by facilitating the formation of criminal partnerships that increase the net benefits of crime (Sah 1991; Murphy, Shleifer, and Vishny 1993; Rasmusen 1996; Billings, Deming, and Ross 2016).[3]

Understanding the relevance and nature of neighbor influences on youth and young adult 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 criminal behavior of youths, and none on young adults (Sanbonmatsu et al. 2011: Ch. 6).[4] As far as we know, no experimental or natural experimental study has provided evidence that any compositional aspect of a neighborhood's population affects a youth or young adult's resident's criminal behavior, measured the influence of the closest residential neighbors on their criminality, or investigated the required duration for neighbor exposure to generate crime. The objective of this paper is to provide such evidence related to the magnitude, speed, and mechanisms of neighbor influences on criminality of youth and young adults ages 14-29 years.

Experimental evidence shows that moving from public housing in deprived neighborhoods to private housing in low-poverty neighborhoods does not have a consistent impact on youth crime (for evidence based

[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).

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 move as children are better off in a range of crime risk factors as young adults (Chetty, Hendren, and Katz 2015). In contrast to this finding, Oreopoulos (2003) does not find evidence on such effects among Canadian adults who were quasi-randomly assigned to different qualities of neighborhoods as children residing in social housing. None of these studies directly measure the neighborhood criminogenic environment, however. Damm and Dustmann (2014) demonstrate that which neighborhood characteristics are measured is crucial for the analysis of neighborhood effects. They show in their Danish natural experimental study in immigrants that the concentration of young criminals living in the municipality, rather than the concentration of crimes committed there, affects boys' later criminal behavior.

The literature on peer effects suggests that the geographic scale over which neighbors affect youths' illegal activity may be smaller than the census tracts or municipalities used in MTO studies and Damm and Dustmann (2014), respectively. Billings, Deming, and Ross (2016) show that residential proximity reinforces school peers' effects on criminal behavior and generates partnerships in crime. Bayer, Hjalmarsson, and Pozen (2009) show that higher concentrations of youth with similar criminal backgrounds in correctional facilities increase youth recidivism.

In sum, prior studies about the criminogenic effects of context come to no consensual findings. But the results suggest that the identification of neighborhood effects on crime will be contingent on the age of the person exposed, the characteristics of neighborhood that are measured, and the geographic scale over which these characteristics are measured.

This paper contributes to this literature with the first investigation of the effect of the presence of criminal neighbors in youth's and young adults' ("youth" hereafter) residential context. Our paper overcomes residential selection by exploiting the natural experimental variation in neighbors generated by social housing allocation of households in urgent needs in the municipality of Copenhagen.[5] We describe in detail how this neighborhood assignment is impermeable to selections by either households or caseworkers, and we demonstrate this empirically with balance tests. Our paper fills an important gap in the literature with two main contributions. First, we provide robust causal evidence on the importance for youth criminality of residential proximity to older adult neighbors charged for illegal drug-related offenses. Second, our data allow us to identify neighbor composition precisely at two levels of residential proximity, the social housing development and the social housing section.[6] We demonstrate that if one uses the latter, larger spatial unit,

[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), Oreopoulos (2003), Jacob (2004), Weinhardt (2014) and Galster et al. (2016a, b).

[6] See next section.

the estimated neighborhood effects are affected by attenuation bias induced by including non-influential neighbors from proximate housing developments in the reference group.

2. Allocation of Households in Urgent Need for Housing 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 households (Scanlon and Vestergaard 2007). Every third social housing unit that becomes vacant is reserved by the municipality for allocating families with dependents, childless couples and independent individuals ("households" hereafter) with "urgent needs": those who cannot afford housing at private market rents or cannot afford standard waiting time for social housing (Social Housing Act 2010). 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", each containing four social housing developments, on average. It is important to note that any apartment located in a housing section that exceeded a 40% rate of resident adult unemployment on January 1st of any given year is ineligible for assignment to urgent need households for that year. This regulation implies that households with urgent needs are not allocated to the most disadvantaged social housing neighborhoods in Copenhagen. However, the wide definition of unemployment[7] used by the municipality implies that one nevertheless can observe such households assigned to a wide range of environments in terms of both composition of neighbors and geographic location (see Fig. 1 for a map showing the location of social housing developments in the municipality of Copenhagen).[8]

Most youth in our study's age group (14-29 years old) applying to social housing in Copenhagen do so as independent youth or couples without children (82%) residing in the municipality. They have been affected by a range of issues ranging from homelessness, addiction, handicap, and divorce, which have created an urgent need for housing. The process of obtaining municipal social housing starts most often with a citizen contacting the municipality regarding a housing problem. A social counselor screens the applicant's

[7] The municipality considers a person unemployed 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 in the appendix, the distribution of social housing development neighbors with drug charges assigned youth in social housing applications over the period 2009-2012.

housing urgency and determines whether the case requires counseling or social housing allocation. In the latter case, the social counselor is responsible for filling out the social housing application. The most relevant information for the allocation of apartments to applications, e.g. household size and household income, is obtained directly by the social counselor from administrative register databases on the basis of applicant(s) identification number(s). In addition, a central task for the social counselor is to prepare a realistic budget that determines the household's maximum affordable house rent. It is critical to emphasize that the application items filled in by the social counselor do not include any preferences for neighbors (including their criminal records) and for neighborhoods (including location) of the apartment.

After completing the application form, the social counselor sends it to the department of social housing of the municipality, a department that does not have any 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 family moves into the apartment, and therefore the social housing department has strong incentives to offer any vacant apartment as soon as possible after it is ready to be inhabited. In order to speed up the allocation of vacant units, the department of social housing assigns every newly available unit to an applicant with allocation software. Each time a housing unit reserved by the municipality is ready to be inhabited, the allocation program generates a list of applicants that matches the vacant apartment in terms of household size and house rent and selects the application with oldest referral date. In rare occasions, the caseworker may allocate the 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.[9] Unlike those on the generic social housing waiting lists who must wait years before being allocated a unit, urgent need waiting list households receive a first housing offer 139 days after application, on average.

This assignment of apartments through the urgent need waiting list implies that for youth with applications that are similar in terms of household size and affordable rent, which neighbors are first offered to them is a matter of chance. This follows because neighborhood assignment depends on the position of the application in the queue of similar applications and on the particular neighbors linked to the apartment that had become available when the application rose to the top of the queue.

Though no household can be forced to accept an offer, most of them (72%) accept their first one due to their precarious situations.[10] The first offer can be rejected because of officially acceptable reasons offered by applicants (e.g., 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

[9] The applicant specifies his/her current housing situation in the application by stating whether applicant currently has accommodation through an institution, family, friends, or otherwise is homeless.

[10] 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).

acceptable reasons are eligible to receive at least one additional offer,[11] those providing unacceptable reasons were given during the 2009-2010 period one final offer but have been deemed ineligible for further offers since 2011.

It is important to highlight that applicants have no ability to influence the dwellings or neighborhoods offered to them for first time. Their preferences for particular social housing units are not specified in the application and 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. Because the relevant application items for the assignment-- household size and affordable house rent-- are filled in by social counselors on the basis of administrative data, not personal testimony, applicants cannot influence their assigned apartment by manipulating affordable rent or household size. 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.[12]

[Fig. 1]

3. Data

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). 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 studied period. At time of social housing allocation these individuals could have been single youth, couples, or dependent youth.[13]

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

[12] Different pieces of evidence demonstrate that our sample youth were exposed to new social networks after the municipality assigned them a new residence. First, the average distance between applicant's address before social housing and the address of the first offered social housing apartment is 6 km. Second, 95% of youth changed parish after social housing. Fourth, no future assigned neighbor was charged with young before neighborhood change. Finally, distance between the new social housing apartment and the last residence is not correlated with application items, acceptance of the first offer, with applicant waiting time or with the elapsed time between a vacant apartment is reserved to the municipality and is offered for first time to a family with urgent needs.

[13] Independent youth can be referred to social housing several months before they become 18, but they can only move into social housing apartment if they are at least 18 years old.

Sample youth were referred to social housing because of transitions from halfway houses (19%), homelessness (32%), overall social problems (17%) or reasons non-specified in the application.[14]

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 department of social housing of the municipality of Copenhagen has maintained information on all applications and offered apartments since a new digitalized allocation system was adopted during summer 2007. These data include: application items used by the allocation software (household size and affordable house rent), application items not used by the allocation software (housing urgency), and detailed information on each offered apartment (address, date of offer; acceptance/rejection, rental date, house rent, size and number of rooms).

We merge the addresses and dates of first offered apartments with information on 100% addresses and tenants of social housing apartments maintained by Statistics Denmark, to identify the location of each offered apartment in the Copenhagen social housing sector and the identification numbers of all individuals living in the same the section when the apartment is offered to an applicant. 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 the assigned social housing development and proximate developments in the housing section precisely in our study period. We measure characteristics of neighbors before the date when the letter with the first offered apartment is issued. By doing so, we ensure that neighbor characteristics are not affected by the new youth neighbors.

From Danish population and income Administrative Registers we obtain longitudinal data about each of these individual characteristics related to demography (age, gender, ethnic background, etc.), and socioeconomic status (income, employment, educational attainment, etc.), using unique personal IDs to merge records across registers. Descriptive statistics for our sample of youth are presented in Table 1. This table presents summary statistics for four different subsamples: those who accepted the first offer, those who dropped out of the allocation process after the first offer, those who accepted the last offer, and those who rejected the last offer. These disaggregated figures demonstrate that, in spite of the fact that these groups evince slightly different profiles, they end up in similar neighborhoods.

Table 1 shows 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

[14] There was 8.3% of sampled youth incarcerated within the four years before social housing application, and 6.4% were incarcerated within the two years before social housing application. 18% of sampled youth were in a halfway house at the application. For youth under age 22, 24% are referred because transition of halfway house, and 22% because of homelessness, whereas among young adults at least age 22, 14% are referred because of transition from halfway house, and 40% because of homelessness.

proportions previously were homeless or lived in institutions, the urgency of their housing needs was palpable. Their deprived economic state was clear from the much lower affordable rent (fixed expenses excluded) (4270 DKK=613 $ per month) than the average rent for a two-room apartment in Copenhagen metropolitan area (7174 DKK=1033 $ per month).[15]

We use the Central Police Register from 2005-2014 to gain information about the types of crime with which individuals were charged and associated dates of offense (whether the person charged was convicted or not). We distinguish recorded criminal offenses (for which a charge was made) for the 2230 individuals analyzed either as occurring within four years before the first offer or occurring after that date. The data available on individual crime allows us to measure criminal behavior 33 months after the first offer, on average.

A brief discussion of the Danish criminal justice system is appropriate since it differs from that in the U.S. in some important ways.[16] Denmark does not have a distinct juvenile justice system; all youths 15 (14 during July 2010-March 2012) 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.[17] Charges are recorded when the police have determined 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 time span we used for measuring criminal outcomes.[18]

We employ in our analysis criminal charges for offenses in violation of the criminal code.[19] 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. 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

[15] During 2009-2012, there were 49% females among Danish population aged 14-29, with an average age of 21.1, slightly lower than our sample youth, and 13% were immigrants, a much lower proportion than our sample youth.

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

[17] Criminal age was temporarily lowered from 15 to 14 years old during July 2010-March 2012.

[18] For example, 10% of our sample was charged for violent crimes committed within 2 years after social housing, whereas we only observe in our available data that 6% received a conviction for violent offenses committed during that period. Similarly, the observe 12% of youth convicted of a property crime committed within two years after social housing and 16% of youth charged for a property crime committed during that period. Below we perform a robustness test using convictions data.

[19] 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" (see Damm and Dustmann 2014).

they are not correlated with the treatment variable, the concentration of neighbors aged 15 and older in the housing development with charges for drug crime.[20]

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. We combine, as in Damm and Dustmann (2014), 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.

Table 1 shows that in the four years before sample youth applied for social housing, 36% of them were charged at least once, for 2.2 offenses on average. In the studied period after they were first offered social housing, almost one-third of the individuals in our sample were charged with one or more offense; and the number of average offenses was 1.9. Not only does the intensity of offenses increases among sample youth, drug offenses increase in relation to property and violent crime.[21]

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 first 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 residents aged 15 and older of the first-offered development for the four-year period up to the day of the youth's first offer.[22]

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 1).

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 1). In addition, we also measured the shares of residents with different types of criminal records present at the neighboring social housing developments

[20] We report neighborhood effects on charged offenses and we show that our results are robust to selecting only those charged offenses which end up with convictions (see Table A.4 Panel F at the appendix).

[21] 26% of youth were charged for property crime, 17% for violent crime, and 17% for drug crime before social housing. 14% were charged with at least one crime involving perpetration with a co-offender. After social housing, 20% of youth were charged with a property crime, 13% with a violent crime and 18% with a drug-related crime.

[22] 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.

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 1). 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".

We identified all neighbors age 15 and older living in the same neighborhood on the dates of a given sample youth's first 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 day of first offer. We specified neighbors as criminals if they had been charged at least once for such particular type of crime committed within the last four years, and aggregated this percentage to the development level.[23] We recorded two variants of residential context variables based on the presence of criminals in the proximate neighborhoods, so that we could assess whether the source of neighbor influences is limited to the closest neighborhood or apply to the entire housing section. Table 1 shows that in the four years before sample the share of those age 15 and older living in social housing with criminal charges for property crime, violent crime or drug crime was 9%, whereas the share of those 15-29 years old charged during the same period was 15%, much lower than the proportion of charged sample youth.

Table 1 reports key statistics on the offered and accepted apartments. The table shows minor differences in the characteristics of rejected and accepted apartments. The first offered apartment to youth in applications was on average 59 m^2 and with a monthly rent of 3773 DKK (excluding fixed expenses). The apartments offered to youth from applications that drop out from the urgent housing waiting list (column 2 of Table 1) are smaller and cheaper than the apartments offered to applicants that do not drop out at the first offer, this probably reflecting the lower availability of small apartments in the municipality.

We analyze, in Section 5.4, the robustness of the neighborhood effects to controlling for socio-economic characteristics of the neighborhood. Concretely, we construct measures of concentration of non-western immigrants, poverty, joblessness, and education of the social housing developments. Descriptive statistics for these neighborhood indicators are presented in Table 1. They show that our sample was first offered neighborhoods where 38% of residents were immigrants, 35% of those 25 years and older had only upper secondary education, 28% of those18 years and older were not employed and 10% of households were poor. Not surprisingly given our neighborhoods are defined by social housing developments, the "average" neighbors of our analysis sample were 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

[23] Point estimates are robust to measuring neighborhood criminality only with the more recent offenses (see Table A.3 Panel J at the appendix).

deprivation. Notably, there were practically no 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.

We construct similar measures of the neighborhood of the accepted apartments with respect to the day when the letter with the accepted offer was issued. The neighborhood of the accepted apartment better captures the context to which youth are actually exposed, but are endogenous given that applicants may reject the first offered apartment.[24]

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.[25]

[**Table 1**]

4. Empirical Approach

4.1 Identification

The effect of concentration of criminal neighbors in new neighborhood (D) for criminal behavior of youth (Y) can be represented using the reduced-form equation (1):

$$Y = D\gamma + A\delta + \varepsilon \quad (1)$$

where A are observable characteristics of youth and ε is an error term capturing unmeasured variables specific to the neighborhood and to the individual. In our residential context, we cannot distinguish the influence of characteristics of criminal neighbors on youth from the influence of future behavior of criminal neighbors (see Manski 1993). We assume, as in Bayer, Hjalmarsson, and Pozen (2009), that an eventual effect of neighborhood composition operates through the characteristics of criminal neighbors and not through neighbors' behavior, i.e., that we observe exogenous, not endogenous effects.

The identification of the neighborhood effect γ in (1) is complicated because the 28% of applicants who reject the first offered apartment may introduce correlation between composition of new neighborhood

[24] See section 4.

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

and the error term.[26] The allocation software of the municipality matches applications to first offered apartments on the basis of household size and affordable rent filled in by caseworkers before referring the application to the department of social housing. Thus, for similar youth in terms of these application items, it is a matter of chance which apartment is offered when a specific application reaches the top of the waiting list, and therefore, it is a matter of chance which neighborhoods they are assigned throughout the first offered housing unit. We fully observe these application items, and therefore we control for potential residential selection by estimating the neighborhood effect π from equation (2):

$$Y = Z_1\pi + A\vartheta + \epsilon \quad (2)$$

where Z_1 is the share of criminal neighbors associated with the first offered housing unit, A includes application household size and application affordable rent and dummies for the year of application, and ϵ is the error term. π is the intent-to-treat effect (ITT), measuring the marginal increase in youth criminal behavior during the studied period due to a marginal increase in the share of criminal neighbors associated with the first offered apartment.

As a complement to ITT estimates, we estimate the effect of actual exposure to neighborhood, γ, in equation (1) with two stage least squares (2SLS) using Z_1 as an instrument for D, where $D = Z_1$ if the household of youth accepts the first offer, $D = Z_2$ if the household of youth accepts the last offer, with Z_2 denoting the share of neighbors with criminal charges of the last offered housing, and $D = 0$ if the applicant does not accept any offer.

The 2SLS analysis of γ identifies a causal effect under additional assumptions than the ITT analysis (see Angrist and Imbens 1995). First, we assume that Z_1 has a causal effect on D, which is fulfilled given the high compliance rate to the first offer and the short elapsed time between offer and move.[27] Second, first-offered criminal neighbors only affect youth criminal behavior of those who move into the offered apartment. This is a realistic assumption because applicants have only five days to accept or reject the offered apartment.[28] Third, D is monotonic with Z_1. This assumption can break down for those who do not move into the first offer. However, given large compliance with the first offered housing unit, we do not expect that non-monotonicity in last offer can introduce significant bias in our 2SLS estimates. We show in Section 5, that the ITT and 2SLS estimates are not statistically different from each other for all outcomes (see Table 4).

[26] The compliance rate in our data set is much higher than the one in MTO experiment. For example, 52% of families with youth aged 10-20, who received a MTO vouchers rejected the opportunity to move to low-poverty neighborhood (Sanbonmatsu et al. 2011).

[27] 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.

[28] 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.

4.2 Balance test

We assess of our central identification assumption with the following balance test. In the case that household size and affordable house rent filled in by social counselors randomizes youth to heterogeneous neighborhood assignments through the first offer, then the conditioning on these two application items should eliminate any correlation between confounding characteristics of youth not used in the assignment and the shares of criminal neighbors associated to the first offered apartments. To implement the balance test, we run several regressions with measures of neighbors' criminal behaviors measured before the first offer (Z_1) on youth characteristics highly correlated with future crime but not used in the assignment of first offers (X). Two confounding characteristics (gender and whether the youth has any shelter) are observed by social housing caseworkers but are not inputs of the allocation software, and the remaining characteristics (three dummy variables indicating whether the youth has been charged for property crime, violent crime, or drug crime prior to first offer) are observed by us but not observed by social caseworkers.[29] We test for joint significance of θ in equation (3)

$$Z_1 = X\theta + A\eta + \epsilon \tag{3}$$

with different covariates A: 1) indicators for application year; 2) indicators for application year and application household size; 3) indicators for application year, application household size and application affordable rent; and 4) all prior controls and housing section fixed effects. Housing section fixed effects are approximated with indicators for housing section, and are intended to capture all kinds of non-neighbor forces operating in the local area of the housing development.

We present in Table 2 these balance tests. Table 2 Panel A reports the p-values of the test for joint significance of θ. Note that these results are obtained without controlling for any application item. For four out of eight regressions in this panel, we reject at the 5% level that individual characteristics are not correlated with the characteristic of first assigned social development. These results show that, *unconditionally*, the allocation system in Copenhagen tends to assign youth with higher potential for criminal behavior to social housing developments exhibiting higher concentrations of criminal neighbors. The results reported in Table 2 Panel B show a completely different picture. All balance tests obtained conditionally on the year of application and the size of household, cannot reject the absence of correlation between individual and neighbor characteristics at 5% level, though the test for no correlation between the share of neighbor poor households and individuals confounders is rejected at 10%. When we expand the covariate set with the other application item used by the allocation software, affordable rent, we do reject the presence of

[29] The joint test of no significance of these five covariates on the probability of being charged after social housing is F=259.53 with p-value=0.0000.

correlation between individual and criminal and socio-economic characteristics of neighbors (see Table 2 Panel C).

As vacant units of each housing section set aside for urgent need households are determined on a "rolling" basis as vacancies occur, and therefore are not selected by housing organizations or by the municipality, the allocation of first opportunities quasi-randomizes youth to heterogeneous housing development neighbors within the same local area. Table 2 Panel D demonstrates that the allocation of social housing achieves quasi-randomization of youth to neighborhoods within the same housing section. This result allows us to assess the robustness of neighborhood effects to controlling for time invariant housing section correlated effects, a crucial test to determining the social interaction nature of estimated effects (see Damm and Dustmann 2014). The balance tests provide support for our previous conjectures that 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.[30]

[**Table 2**]

5. Neighborhood Effects on Youth Crime

5.1 Reference group

One of the challenges in the estimation of social interaction effects is the measurement of the reference group (Manski 1993). The geographical scope, encompassing the potential relevant neighbors affecting youth crime, needs to be identified if attenuation bias has to be avoided (Katz, Kling and Liebman 2001). Unlike in the analysis of peer effects (Bayer, Hjalmarsson, and Pozen 2009; Billings, Deming, and Ross 2016) where group boundaries are narrow and determined by classrooms, schools or correctional facilities, the appropriate scale of neighborhood is unknown. In addition, the defining characteristic(s) of the reference group is also unknown. Bayer, Hjalmarsson, and Pozen (2009) and Damm and Dustmann (2014) demonstrate that co-location with only certain types of criminals generate effects.

[30] We have estimated the effect of false neighborhoods as an additional balance check. If urgent waiting list allocation quasi-randomizes neighbors to similar applicants, then the characteristics of neighbors offered to other similar applications should be orthogonal with the criminal behavior of youth. To do so, we assign the neighbors linked to a randomly chosen apartment offered to a youth with same household size, and very similar affordable rent (difference less than 12 US $, monthly) and then ascertain if any pseudo neighborhood effects emerge. None of the estimates turn out to be statistically significant. The results of this falsification test are available upon request.

Our data allow determining the boundaries of influential neighbors, by first estimating neighborhood effects for different groups of criminal neighbors residing in the smallest-scale, most-proximate residential context, and then by assessing whether any emerging neighbor influences extend to neighboring social housing developments.[31]

We first estimate the effect of three 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: 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 a reasonable notion of "neighborhood" 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.[32] To address such a possibility we also consider the effect of the percentage of residents from the neighboring social housing developments within the same housing section with charges for the different types of criminal offenses. The housing section includes, on average, 1206 residents age 15 and older. 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 are influenced by others residing in the housing section, not simply their own development.

We identify the appropriate reference group by estimating π in (2) for percentages of neighboring individuals with 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 housing section. Table 3 below, reports ITT estimates of these effects on the probability that a sampled youth is charged with any crime after the first offer of social housing, in aggregate and then by crime type. All models include application household size, application affordable rent and year of application controls.

Table 3 Panel A shows ITT effects of π in (2) for the share of neighbors living in the development charged for property crime, violent crime or drug crime within the four years before the day of the first offered apartment. These estimates show positive effect of 0.009, significant at 1% level, on the probability of being charged for any crime. By specific crime, the higher concentration of neighbors with criminal charges increases the probability that youth new neighbors are charged for property crime (estimate 0.007, significant at 5%) and drug crime (estimate 0.005, significant at 5%).

[31] We assess, in subsection 5.4, the sensitivity of the effect of criminal neighbors to controlling for the socio-economic characteristics of neighbors.

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

In an attempt to identify the neighbors with most criminogenic influence, Panels B, C, and D of Table 3 report ITT estimates of π in (2) for percentages of neighboring individuals with different type of charges living in the social housing development. Panel B shows 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). Panel C shows a positive and significant effect of 0.013 on the probability of being charged when neighborhood criminality is measured as the share of individuals in the development with charges for violent crime. This result suggests that youth are vulnerable to violent criminal neighbors in a smaller social context than children (Damm and Dustmann 2014). However, Panel D shows that the concentration of neighbors with charges for drug crime has a stronger impact on youth criminal behavior. The effect of this reference group on the probability of being charged for any crime is 0.015 and significant at 0.1% level. In addition, the estimates on the probability of being charged for property crime and for drug crime are statistically significant at 1% level and at 5% level, respectively.

We report in Panel E and F of Table 3 the effects of violence crime neighbors and of drug crime neighbors for a larger geographical scale of neighborhood, the housing section.[33] Both sets of results clearly show that more distant neighbors are less influential than the more proximate ones. The point estimates are lower and more imprecisely estimated in both regressions at the housing section scale. In order to disentangle the contribution of neighbors from proximate developments from that of the same development, we investigate in the same regression the separate impacts of similar criminal neighbors with residence in the housing development and with residence in the proximate developments within the section. Panel G of Table 3 shows that residents with violent criminal charges from proximate developments do not have any impact, whereas the effect from violent criminal neighbors within the development is identical to the estimate obtained without including neighbors from proximate developments. Table 3 Panel H reports the results of a similar regression where concentrations of neighbors with drug charges in the development and in proximate developments are included. Once again, the estimates demonstrate that neighbors charged for drug crime living in other developments in the housing section do not affect youth crime in the given development. Our inductive analysis thus demonstrates that the geographical scale of the neighborhood is a key factor for the analysis of neighborhood effects.

Finally, we include the concentration of neighbors in the development with charges for violent crime and drug crime as separate neighborhood characteristics. Both concentrations are highly correlated, probably because some neighbors have experience with both types of crime. This analysis is of key importance for the design of optimal housing policy and for criminal justice policy. The estimates presented in Panel I of Table

[33] The neighbors with property crime charges remain non influential for youth crime when measuring neighborhood crime at the housing section level (see Table A.1).

3 demonstrate that neighbors in the development with drug charges are the dominant source of youth crime, and that the previously detected effects of violent neighbors are driven by the high correlation between both concentrations of criminal neighbors.

To sum up, this section has provided strong evidence that the reference group of neighbors driving youth crime is composed of individuals living within the closest residential context, e.g. the social housing development, who have prior charges for drug crime. In addition, our analysis of reference group demonstrates that similar neighbors living in proximate developments do not affect youth criminality in the given development and, by implication, also suggests that more distant neighbors living outside the social housing section are even less likely to be a factor. We therefore focus in the rest of the paper on neighbors within the social housing development who have prior charges for drug crime.

[**Table 3**]

5.2 Main results

Table 4 presents the estimated effects generated by such neighbors, where youth criminal behavior is measured in terms of the probability and number of charges incurred after assignment over 33 months (on average), by type of charge. Table 4 shows ITT, 2SLS estimates, and first stage coefficients for a treatment dose analysis. What is immediately obvious from the estimates reported in Panel A 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 the effect on property crime and drug crime drive this effect. Considering any type of criminality, a one percentage-point increase in neighbors with drug charges increases the 2SLS-estimated probability of a youth being charged with any crime within the studied period after moving into social housing by 0.019 (representing a 6.03% increase from the mean probability), jointly because of the impact on the probability of a property crime charge (0.013 increase, representing a 6.44% increase from the mean probability) and the effect on the probability of a drug crime charge (0.010 increase, representing a 5.59% increase from the mean probability). 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 effects are less precisely estimated, and none of them are statistically significant at 5% level, but the magnitude of the estimates is quite consistent with the effects on probability of being charged.

[**Table 4**]

The impact of drug criminal neighbors on youth property crime contrasts with existing evidence where the source of crime effects among children and youth arises from neighbors or peers committing the same type of crime (Damm and Dustmann 2014; Bayer, Hjalmarsson, and Pozen 2009). 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). This suggests the presence of a youth-specific neighborhood effect mechanism distinguished both by the composition of influential neighbors and the geographical scale over which they exert their influence. We probe this mechanism further in section 5.3 below.

It is important for drawing policy implications that we investigate whether the estimated neighborhood effects differ across types of youth. Concretely, it is of key importance for social housing design to determine whether higher concentrations of neighbors with drug charges have similar impacts on first-time youth offending and youth recidivism. Table 5 Panel A reports neighborhood effects for a fully interacted ITT analysis where we allow the neighborhood effect to differ depending on whether sample youth have previous charges ($G = 1$) or not ($G = 0$):

$$Y = (1 - G)(Z_1\pi_0 + A\vartheta_0) + G(Z_1\pi_1 + A\vartheta_1) + \epsilon \quad (4)$$

where π_0 can be interpreted as the neighborhood effect on first-time crime, and π_1 can be interpreted as the neighborhood effect on recidivism. The neighborhood effect on recidivism is 0.018, significant at 5% level, and statistically higher than the neighborhood effect on first-time youth crime (0.008), which is not statistically significant at 5% level. This heterogeneous effect is driven by the higher impact on property crime among youth with previous charges, whereas the estimates on drug crime are similar for both samples.

Table 5 Panel B reports quantile regression[34] estimates of ITT effects ($\pi(q)$) of drug criminal neighbors for the quantiles 0.65,…, 0.94 of the distribution of number of charges. The quantile regression results suggest some important differences across different points in the conditional distribution of the number of charges. At the lower and medium parts of the distribution, the estimates are not defined (approximately 35% of youth are not charged after social housing). However, the estimated effects are positive and significant for several quantiles between 0.65 and 0.94. In addition, it is possible to discern the emergence of two different effect patterns. For quantiles 0.65 - 0.80 the estimates are relatively stable with an estimated coefficient under 0.10, whereas from quantiles 0.80 and above the quantile neighborhood effect size increases linearly. These results suggest that the intensity of youth criminality is increased in a constant way for youth with moderate- to high-potential criminal behavior. However, youth with the highest crime potential are much more influenced by neighbors with drug crime experience. This result implies that youth

[34] For a description of this estimator, see Koenker and Bassett (1978).

who potentially will be charged for most number of offenses are also those individuals most susceptible to neighborhood influences.

The analysis of heterogeneous effects leads to a clear policy implication. Youth with previous charges, especially those who would seem most prone towards criminality in the future, should be assigned to those residential contexts with the lowest concentrations of neighbors with experience in drug crime.

[**Table 5**]

We now turn our focus to the minimum exposure necessary for the arising of neighborhood effects. 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. The effects on the probability of being charged do not appear within the first six months after youth occupy social housing, instead taking between six and twelve months to manifest themselves. Second, the intertemporal dynamics are slightly distinctive across crime types. The effect on the probability of being charged fully manifests itself after one and a half years, though henceforth there seems to be no marginal effect from subsequent duration of exposure. In the case of criminality measured by property crime, there is a consistent pattern of the effects growing monotonically over the first 18 months, but the marginal effect of subsequent exposure increases at lower pace. The neighborhood effect on drug crime grows at more constant pace and becomes significant first when outcomes are measured within the longest time horizon available in our sample, on average 33 months after the date of first offer. It is also possible to observe that neighborhood effects on youth violent crime reach their maximum between six and twelve months after occupation and are significant at 5% level, but such effects remain at the same level and are very precisely estimated. The cumulative impact of the neighborhood effect on youth crime clearly supports the presence of intra-neighborhood processes that take some modicum of time to become efficacious. We explore in the next section whether these neighborhood effects are being transmitted by contact with resident drug offenders, or because of uncontrolled correlated effects.

[**Table 6**]

5.3 Mechanisms

Our previous evidence is suggestive that youths with prior criminal records are involved in social

processes within neighborhoods with higher concentrations of drug criminal neighbors that have criminogenic consequences for these youth. 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 comport with the evidence and forwarding those that are *most plausible*. We arrive at these deductions by investigating whether neighborhood effects are driven by the following alternative mechanisms: (1) correlated effects; (2) partnerships in crime with neighbors; (3) peer effects; or (4) role models.

An alternative explanation to social interaction is the effect of non-neighbor forces correlated with the concentration of drug criminal neighbors (Manski 1993). The hypothesis of correlated effects can be partially assessed by controlling for fixed geographic effects at various nested scales, as many non-neighbor forces (like distance to low-poverty residential neighborhoods) are likely to be quite stable over the studied period. We expand the equation (2) with housing section indicators

$$Y = Z_1\pi + A\vartheta + \phi + \epsilon \quad (5)$$

where ϕ are fixed effects capturing all those features of the intra-city physical, demographic, socioeconomic and institutional/social service context that are stable during 2009-2014 (the analysis period) for the encompassing area in which each of the social housing developments is located. We intend for these area 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).

Equation (5) exploits within-area variation in neighbor characteristics to estimate the neighborhood effect. Table A.2 Panel A 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. Results for the probability of being charged for any crime (ITT estimate of 0.012 and 2SLS estimate of 0.015) are statistically significant at the 5% level and slightly lower than the main results (Table 4), but the estimated effects on property crime (ITT estimate of 0.011 and 2SLS estimate of 0.014) are slightly higher than the estimated effects without area fixed effects. The estimated effects on drug crime (ITT estimate of 0.006 and 2SLS estimate of 0.008) are lower and not statistically significant at 5%, but they are not statistically different from the estimates obtained without area fixed effects.

A high concentration of neighbors with drug charges may attract more intensive police surveillance, a potential correlated effect not captured by area fixed effects that could bias the estimated effects. Unfortunately, we do not have information on the time varying degree of police surveillance at the level of social housing development. Instead, we assess the plausibility of a correlated police effect by means of two

indirect checks. First, we investigate the role of neighbors with multiple charges, as they are more likely to attract police scrutiny than neighbors with few charges. A higher surveillance of multiple offenders should be manifested in terms of a neighborhood effect driven by the number of charges accumulated by these neighbors before the day of the first offer (denoted W_1) and in terms of a positive interaction between the share of neighbors with drug charges and the number of charges of these neighbors (π_3). Table A.3 Panel I presents estimates of π_1 obtained from equation (6):

$$Y = Z_1\pi_1 + W_1\pi_2 + Z_1W_1\pi_3 + A\vartheta + \epsilon \quad (6)$$

The ITT-estimates of the neighborhood effect (π_1) of the probability of being charged and the probability of being charged for property crime are equivalent to our main results and statistically significant at 5%, whereas the estimated π_2 is not statistically significant, and the estimate of π_3 is not statistically significant and has a negative sign, opposite than expected in the case where neighborhood effects were driven by heightened police surveillance of the "usual suspects." In addition, the estimated effects on youth violent crime and drug crime are now significant at 5%. Finally, estimates from (6) are robust to including area fixed effects.

We now investigate the neighborhood effect on the probability of youth being charged in conjunction with a new neighbor. The assessment of neighborhood effects on youth co-offending with neighbors sheds light on the degree to which neighboring drug criminals may "recruit" youth into their criminal networks. 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 crime that might be associated with drug possession and/or sale. This type of criminal activity is potentially more strongly correlated with police surveillance than our main results, which include the probability of being charged for any offense, crimes committed individually, in conjunction with non-neighbors or in conjunction with a new neighbor. Table A.4 Panel E reports no significant neighborhood effect on co-offending activity at 5% level. These results have two implications. First, it is unlikely that our main neighborhood effects (primarily capturing increased individual offending) are driven by correlated police surveillance effects, given the lack of observed neighborhood effect on co-offending with the neighbors that would more likely attract police attention. Second, the lack of effects on co-offending eliminates partnerships in crime as a potentially relevant mechanism.

In sum, we think it unlikely that our results are being driven by time-varying correlated effects related to police behavior. This conclusion is buttressed by the fact that households with urgent needs were allocated to many different locations (259) in the studied period, yet none of these destination developments

were located in the most deprived sections of the city (i.e., with at least 40% unemployment, for reasons we explained above), which are most likely to attract police surveillance due to their higher average crime rates.

We next explore the separate strength of peers and role models as potential mechanisms. We first investigate the role of social contact with 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 because new neighbors can also endogenously affect criminal behavior of existing neighbors (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 Panel A of Table A.3). We find no effect of the concentration of neighboring peer drug criminals on the probability of charges against youth for *any* type of crime. The estimated peer effect on drug crime is only 0.002, higher than for any other type of crime, but yet is still clearly insignificant. Finally, we investigate the effects generated by concentrations of drug offenders ages 30 years and older. These estimates are practically identical to the main results in terms of probability of being charged for any crime and for property crime, and are significant at 0.1% level. In addition, the estimates of role models on violent crime and drug crime are now significant at 5% level (Panel B of Table A.3).

Our results stand in sharp contrast to other studies that emphasize the criminogenic role of youth peers in confined contexts like prisons (Bayer, Hjalmarsson, and Pozen 2009) and schools (Billings, Deming, and Ross 2016). Our results have some similarity with Damm and Dustmann (2014), where criminals aged 15-25 increase the criminal behavior of children aged 10-14 living in the same municipality. However, our findings reveal the presence of a different mechanism, both in terms of the type of influential neighbors – adults age 30 and older with drug criminal experience – and the geographical scale of neighbor influences – only within the most proximate residential context within a social housing development.

In sum, our evidence suggests that neighborhood effects on youth crime are primarily manifested through the enhanced probability of individually committing property crime because of higher social contact in the residential context with adult neighbors who have been charged for drug crime. We deem it highly plausible that the following mechanism is at work here. The higher presence of adult neighbors 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 seller / consumer of drugs 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, it is implied that

youth with more formidable drug habits increasingly are pressed to commit property crimes, perhaps to support their habit.

This hypothesized mechanism operates more forcefully upon youth with prior criminal charges and it seemingly has 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 crime once they have previously decided for other reasons to begin such behavior.

5.4 Sensitivity analysis

We challenge in this section the robustness of our aforementioned effects with additional checks. First, we reformulated the treatment variable to reflect neighbor *convictions* for drug crime instead of charges, and to include neighbor drug criminal offenses committed only within the two years before social housing.[35] Table A.3 Panel C and Panel J show that the estimated effects on aggregated crime and on property crime are robust to these different measures of drug criminal neighbors, but the effect on drug crime is more imprecisely estimated when measuring neighborhood crime in terms of convicted offenses, and is insignificant when measuring neighborhood crime in terms of most recent offenses.[36]

Second, we estimate the effect of the concentration of neighbors with drug charges controlling for socioeconomic characteristics of neighbors highly correlated with neighborhood crime. Table A.3 Panels D to G show that the estimates on the 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 upper secondary education or the percentage of immigrants. Estimates on the probability of property crime are also robust to adding these neighborhood characteristics, although the estimated effect becomes only significant at 10% level when controlling for neighborhood non-employment. Estimates on the probability of drug crime are all significant at 5% level, with the exception of the regression where non-employment is controlled for. The sensitivity of crime-specific results to controlling for non-employment reflects the relatively higher correlation between this neighborhood socio-economic characteristic and the percentage of neighbors with drug charges. However, the impact of non-employed neighbors on youth criminality is statistically insignificant in all regressions. Therefore, we conclude that it is the concentration of drug criminal neighbors and not the lack of employment among neighbors that drives the observed neighborhood effect on youth crime.

[35] 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 later years of our analysis period, given the typical lag between offense and conviction.

[36] This result runs counter to that by Livingston et al., 2014, who found that only the concentration of neighbors that had committed crimes during the prior two years, not previously, were associated with first-time offending.

Third, Table A.4 Panels A to C show that our results on aggregated youth crime and property crime are robust to controlling for several individual characteristics with high predictive power for future crime: previous criminal record in the three different types of offenses (Panel A), the degree of housing urgency (Panel B), and gender (Panel C). 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, affordable house rent as shown on the housing application is also correlated with potential criminal behavior. Nevertheless, Table A.4 Panel D shows that the main results are robust to excluding affordable house rent, as well as year of application dummies.

Fifth, our results could be driven by large social housing developments with less contact with surrounding neighborhoods, including private housing. We do not find any evidence that our results are driven by the smallest or largest developments. Table A.3 Panel H reports the estimates of the interaction model (6) where W_1 is the number of neighbors at least 15 years old linked to the first offered aparment. The estimated neighborhood effects are slightly higher than the main results and now are also significant for violent and drug crime. We do not find any sign on influence of size of neighborhood on our results. These results demonstrate that the main estimates are representative for both small and big developments, and reinforce the interpretation of neighborhood effects as the effects of social interaction within the residential context.

Sixth, we present estimates on convicted charges for offenses committed within the first 24 months in Table A.4 Panel F. These results are comparable to the estimates of neighborhood effects on charges (with conviction or not) for offenses committed within the same time period (Table 6 Panel D). Estimates on convictions for any crime and for property crime are statistically significant at 5%, only slightly lower than main results on charges, but not statistically different. In addition, the estimated effect on convicted violent crime is significant at 5% level. Results on drug crime are not statistically significant but are comparable in magnitude to the main results.

Seventh, Table A.5 reports the estimates obtained with two binary treatment analyses. To do so, we split our sample into a treatment group (youth exposed to higher than "normal" concentrations of drug criminal neighbors) and a control group (the remaining youth).[37] The difference between average concentrations of drug criminals in the treatment group with respect the control group is 3.5%. Table A.5 Panel a shows ITT estimates of a magnitude consistent with the 6% marginal increase detected with the dose treatment analysis (representing a 6×3.5%=21% times increase from the mean probability of being charged

[37] The "normal" concentration of development neighbors with drug charges is set at the median value across all developments, 3.8%.

for any crime of the control group). However, the estimates on specific type of crimes are less precise and not significant at 5% level. Table A.5 Panel B reports the results of the second discrete treatment analysis that excludes youth exposed to "normal" criminal concentrations from treatment and control groups.[38] The greater contrast in terms of neighborhood quality leads to significant effects on property crime and drug crime, in spite of being obtained with a smaller sample.

Finally, the effect on the probabilities of violent crime and drug crime magnitude are estimated with less precision than the effect on the probabilities of any crime and property crime, given the fact that violent and drug crimes are less likely to occur in the same proportions. In order to increase the precision of our estimates, we estimate the neighborhood effect Π from a panel regression (7) in a similar fashion than Ludwig, Duncan and Hirschfield (2001). Concretely, we estimate a panel regression with half-yearly measured data. The panel approach, in addition to more precise estimates, allows us to control for common time trends in crime:

$$Y_t = Z_1\Pi + A\vartheta + \varphi_t + \xi_{t'} + \epsilon_t \qquad (7)$$

where φ_t includes indicators for the half-year since first offer assignment and $\xi_{t'}$ are indicators for calendar half-year of youth criminal behavior Y_t measured in half-year $t = 1,2,3,4$ after the day the letter with the first offered housing unit was issued. Table A.4 Panel G presents the estimated effect generated by neighbors with drug criminal charges, where youth criminal behavior is measured within a half-year, by type of charge. Note that the ITT parameter Π can be interpreted as the marginal increase in youth criminal behavior during a half-year due to a marginal increase in the share of criminal neighbors associated to the first offered apartment. Roughly the size of Π should be four times smaller than the estimated effect within 24 months after the first offer (see Table 6 Panel D). What is immediately obvious when comparing the panel estimated effects with the estimates on probability of being charged within the first two years (see Table 7 Panel D) is that the magnitude of our main results are robust to both the calendar period and the period with respect to assignment when offenses are comitted. In addition, the effects on crime-specific outcomes are more precisely estimated and show a similar pattern with previous estimations: property crime effects are driving the effect on aggregated crime, and we cannot discard the effect on drug crime and violent crime.

6. Conclusion

[38] Concretely, the treatment group includes youth assigned to developments with at least 4% neighbors with drug charges, whereas the control group includes youth assigned to developments with at most 3% of similar neighbors.

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 age 30 and older 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. These effects manifest themselves already after six months 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 and from peers suggests that these transmission mechanisms operate primarily through enhanced accessibility to illegal drugs. Our evidence indicates that the most plausible mechanism is that drug offending neighbors encourage 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 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 neighborhood effect will not be observed if one selects a different category of neighbor or by socio-demographic characteristics as the reputed source of effect, or if one specifies a larger geographic scale for operationalizing neighborhood.

Finally, we would point out that, though our results come from Copenhagen, we believe that they have broader generality and external validity. We see no reason why such a causal effect, transmitted through social interactions of a type clearly appertaining beyond Denmark, would not also be manifested in countries where housing allocations are accomplished primarily through market mechanisms.[39]

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[39] Evidence supporting this conclusion comes from Livingston et al.'s (2014) non-experimental study coming out of UK, which employed a panel analysis with individual and area fixed effects. It showed a strong positive relationship between the percentage of residents in the small-scale ("post-code") residential neighborhood who had committed a crime in the prior year and the probability that an individual would commit a crime for the first-time in the following quarter.

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Figures and tables

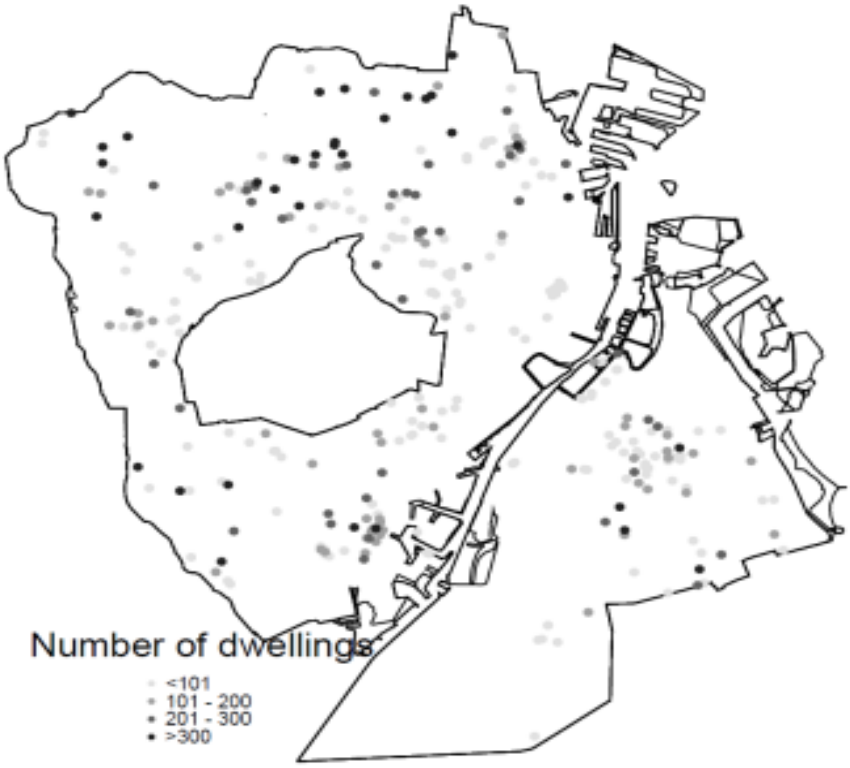

Fig. 1

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

Source:

Table 1

Descriptive statistics on sample youth, offered apartments, by response to offer and offer

	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	
Individual characteristics						
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)	
Affordable rent (2014 DKK)	4321(1391)	3656(1078)	4731(1503)		4232(1429)	
Referral because halfway house (%)	20(40)	18(38)	14(35)		13(34)	
Referral because homelessness (%)	32(47)	30(46)	31(46)		32(47)	
Referral because overall problem (%)	16(37)	19(40)	18(39)		20(40)	
Occupies institution (%)	23(42)	22(42)	22(42)		13(34)	
Charged before 1st offer (%) [a]	36(48)	38(49)	32(47)		30(46)	
Number of charges before 1st offer[a]	2.2(7.3)	2.9(6.7)	1.5(4.8)		2.4(7.3)	
Charged after 1st offer (%) [b]	32(47)	34(47)	27(45)		32(47)	
Number of Charges after 1st offer [b]	1.9(5.8)	2.1(6.0)	1.5(5.0)		1.0(3.3)	
Observations	1603	318	249		60	
	1st offered apartment	1st offered apartment	1st offered apartment	Last offered apartment	1st offered apartment	Last offered apartment
Apartment characteristics						
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)
Neighborhood characteristics						
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	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	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)
Jobless neighbors (%)	28(7)	29(6)	27(7)	27(6)	28(6)	29(6)
Observations	1603	318	249	249	60	60

Means and standard deviations (in parentheses) of the individual characteristics and apartment characteristics before the offer is issued.

[a] Criminal behavior measured during the 4 years before the first offered apartment.

[b] Criminal behavior measured during on average 33 months after the date of the first offered apartment.

Table 2

Balance tests (p-value of joint insignificance test)

	Share of 15+ neighbors charged	Share of 15+ neighbors charged for property crime	Share of 15+ neighbors charged for violent crime	Share of 15+ neighbors charged for drug crime	Share of 18+ neighbors without employment	Share of 25+ neighbors with upper secondary education	Share of neighbor poor households	Share of non-western neighbors
Panel A								
A:=Year of application	0.005	0.022	0.016	0.004	0.004	0.185	0.049	0.181
Panel B								
$A \coloneqq \begin{cases} \text{Year of application} \\ \text{Household size} \end{cases}$	0.182	0.184	0.262	0.220	0.191	0.597	0.097	0.185
Panel C								
$A \coloneqq \begin{cases} \text{Year of application} \\ \text{Household size} \\ \text{Affordable house rent} \end{cases}$	0.169	0.162	0.275	0.253	0.216	0.648	0.269	0.157
Panel D								
$A \coloneqq \begin{cases} \text{Year of application} \\ \text{Household size} \\ \text{Affordable house rent} \\ \text{Area fixed effects} \end{cases}$	0.172	0.201	0.244	0.363	0.582	0.133	0.201	0.253
Observations	2230	2230	2230	2230	2230	2230	2230	2230

Each panel reports the p-value of the test for joint significance of individual characteristics X (dummy indicator for the individual has a shelter, gender, dummy indicator for the individual has been charged for property crime, dummy indicator for the individual has been charged for violent crime; dummy indicator for the individual has been charged for drug crime) in the equation (3) for different characteristics of social housing development Z_1 in the column. The estimated models reported in Panel A control for year of application. Those reported in Panel B control for year of application and application household size. The results shown in Panel C control for year of application, application household size and application affordable house rent. Finally, the results shown in Panel D control for all previous characteristics of youth applications and in addition for area fixed effects. Equation (3) is estimated in all cases with robust standard errors adjusted for development clustering of the first-offered social housing unit (259 cells).

Table 3

Dose treatment effect of the share of criminal neighbors on youth crime, by context and type of neighbors

	Probability of being charged for			
	Any crime	Property crime	Violent crime	Drug crime
Panel A				
Share of 15+ individuals in the development charged for any crime	0.009*** (0.003)	0.007* (0.002)	0.004 (0.002)	0.005* (0.003)
Panel B				
Share of 15+ individuals in the development charged for property crime	0.007 (0.004)	0.005 (0.004)	0.002 (0.003)	0.004 (0.004)
Panel C				
Share of 15+ individuals in the development charged for violent crime	0.013* (0.005)	0.006 (0.005)	0.004 (0.004)	0.009 (0.005)
Panel D				
Share of 15+ individuals in the development charged for drug crime	0.015*** (0.004)	0.010** (0.003)	0.006 (0.003)	0.008* (0.004)
Panel E				
Share of 15+ individuals in the section charged for violent crime	0.008 (0.010)	0.004 (0.008)	-0.001 (0.007)	0.004 (0.010)
Panel F				
Share of 15+ individuals in the section charged for drug crime	0.013* (0.007)	0.009 (0.006)	-0.000 (0.005)	0.007 (0.007)
Panel G				
Share of 15+ individuals in development charged for violent crime	0.013* (0.005)	0.006 (0.005)	0.004 (0.004)	0.009 (0.005)
Share of 15+ individuals in proximate developments charged for violent crime	-0.006 (0.009)	0.001 (0.007)	-0.004 (0.006)	-0.004 (0.008)
Panel H				
Share of 15+ individuals in the development charged for drug crime	0.015*** (0.004)	0.010** (0.003)	0.006 (0.003)	0.008* (0.004)
Share of 15+ individuals in proximate developments charged for drug crime	-0.001 (0.005)	0.001 (0.004)	-0.001 (0.004)	0.000 (0.005)
Panel I				
Share of 15+ individuals in the development charged for violent crime	0.001 (0.007)	-0.004 (0.006)	-0.001 (0.005)	0.003 (0.006)
Share of 15+ individuals in development charged for drug crime	0.014** (0.005)	0.012** (0.004)	0.006 (0.004)	0.006 (0.005)
Observations	2230	2230	2230	2230

Estimates are the ITT effects, from equation (2), where each panel reports a separate regression in terms of the residential context and type of neighbors used to define neighborhood composition (Z_1). Panel A shows ITT estimates for the case when Z_1 is the share of 15+ years old individuals charged for property, violent or drug crime residing in the development of the first offered housing unit. In Panel B, Z_1 is the share of 15+ years old individuals charged for property crime residing in the development of the first offered housing unit. In Panel C, Z_1 is the share of 15+ years old individuals charged for violent crime residing in the development of the first offered housing unit. In Panel D, Z_1 is the share of 15+ years old individuals charged for drug crime residing in the development of the first housing unit. In Panel E, Z_1 is the share of 15+ years old individuals charged for violent crime residing in the section of the first offered apartment. In Panel F, Z_1 is the share of 15+ years old individuals charged for drug crime residing in the section of the first housing unit. Results reported in Panel G are obtained by including two characteristics of neighborhood composition in Z_1, the share of 15+ years old individuals charged for violent crime residing in the development, and the share of 15+ years old individuals charged for violent crime residing in proximate developments within the section. In Panel H, Z_1 includes the share of 15+ years old individuals charged for drug crime residing in the assigned development, and the share of 15+ years old individuals charged for drug crime residing in proximate developments of the assigned development. Finally, in Panel I, Z_1 includes the share of 15+ years old individuals charged for violent crime residing in the assigned development, and the share of 15+ years old individuals charged for drug crime residing in the development. The estimated equations include year of application indicators, application household size, and application affordable house rent. Robust standard errors in parentheses 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, ** = p-value < 0.01, *** = p-value < 0.001.

Table 4

Dose treatment effect of the share of criminal neighbors on youth crime

	Probability of being charged for				Number of charges			
	Any crime	Property crime	Violent crime	Drug crime	Any crime	Property crime	Violent crime	Drug crime
ITT	0.015***	0.010*	0.006	0.008*	0.081	0.054	0.014	0.013
	(0.004)	(0.003)	(0.003)	(0.004)	(0.058)	(0.037)	(0.017)	(0.017)
First stage	0.772***	0.772***	0.772***	0.772***	0.772***	0.772***	0.772***	0.772***
	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)
	[287.05]	[287.05]	[287.05]	[287.05]	[287.05]	[287.05]	[287.05]	[287.05]
2SLS	0.019***	0.013*	0.007	0.010*	0.105	0.070	0.018	0.017
	(0.005)	(0.005)	(0.004)	(0.005)	(0.075)	(0.047)	(0.023)	(0.021)
Sample mean	0.315	0.202	0.134	0.179	1.868	0.961	0.400	0.508
Observations	2230	2230	2230	2230	2230	2230	2230	2230

The first row reports ITT estimates from equation (2), where Z_1 is the share of neighbors charged for drug offenses residing in the assigned development. The second row reports the first stage coefficient of the 2SLS estimator presented in row 3 from Equation (1), where D is the share of neighbors charged for drug offenses residing in the new residential development, and Z_1 is the excluded instrument. The F statistic for the exclusion of the instrument Z_1 from the first-stage equation is presented in brackets. The estimated equations all include year of application indicators, application household size, and application affordable house rent. 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. * = p-value < 0.05, ** = p-value < 0.01,*** = p-value < 0.001.

Table 5

Dose treatment effect of the share of criminal neighbors on youth crime, by youth criminal behavior

Panel A: Heterogeneity with youth prior charges

	Probability of being charged for			
	Any crime	Property crime	Violent crime	Drug crime
π_0	0.008	0.006	0.004	0.005
	(0.004)	(0.003)	(0.003)	(0.003)
π_1	0.018*	0.011*	0.004	0.006
	(0.007)	(0.006)	(0.007)	(0.008)
Sample Mean ($G = 0$)	0.155	0.090	0.045	0.063
Sample Mean ($G = 1$)	0.606	0.407	0.297	0.390

Panel B: Heterogeneity with youth future charges

q	Number of charges	q	Number of charges	q	Number of charges	q	Number of charges	q	Number of charges	q	Number of charges
0.65	0.000	0.70	0.046	0.75	0.065	0.80	0.125	0.85	0.233	0.90	0.451*
	(0.029)		(0.039)		(0.037)		(0.078)		(0.120)		(0.181)
0.66	0.087*	0.71	0.045	0.76	0.087*	0.81	0.145	0.86	0.308*	0.91	0.515*
	(0.030)		(0.044)		(0.041)		(0.080)		(0.131)		(0.197)
0.67	0.090*	0.72	0.047	0.77	0.104*	0.82	0.159	0.87	0.319*	0.92	0.470
	(0.031)		(0.030)		(0.046)		(0.085)		(0.137)		(0.244)
0.68	0.072*	0.73	0.048	0.78	0.096	0.83	0.190	0.88	0.350*	0.93	0.439
	(0.033)		(0.033)		(0.053)		(0.098)		(0.156)		(0.270)
0.69	0.054	0.74	0.051	0.79	0.102	0.84	0.213*	0.89	0.396*	0.94	0.391
	(0.036)		(0.035)		(0.068)		(0.105)		(0.170)		(0.328)

Panel A reports the ITT estimates from Equation (4), where Z_1 is the share of neighbors charged for drug offenses residing in the assigned development; and G is an indicator for the youth has been charged before social housing (Observations=792) or not (Observations=1438). Panel B reports the ITT estimates from a quantile regression (5) for quantiles 0.65 to 0.94. The estimated equations all include year of application indicators, application household size, and application affordable house rent. Robust standard errors ae adjusted for development clustering of the first-offered social housing unit (259 cells for ITT).

* = p-value < 0.05, ** = p-value < 0.01; *** = p-value < 0.001.

Table 6

Dose treatment effect of the share of criminal neighbors on youth crime, by exposure

	Probability of being charged			
	Any crime	Property crime	Violent crime	Drug crime
Panel A				
6 months after 1st offer	0.002 (0.003)	0.002 (0.002)	0.002 (0.002)	-0.001 (0.002)
Panel B				
12 months after 1st offer	0.010* (0.003)	0.005 (0.003)	0.005 (0.002)	0.002 (0.002)
Panel C				
18 months after 1st offer	0.012*** (0.004)	0.008* (0.003)	0.006* (0.003)	0.005 (0.003)
Panel D				
24 months after 1st offer	0.012* (0.004)	0.007* (0.003)	0.005 (0.003)	0.006 (0.003)
Panel E				
33 months (on average) after 1st offer	0.015*** (0.004)	0.010* (0.003)	0.006 (0.003)	0.008* (0.004)
Observations	2230	2230	2230	2230

The table reports reports ITT estimates from equation (2), where Z_1 is the share of neighbors charged for drug offenses residing in the assigned development, and the criminal behavior of youth is measured within different time intervals after the date of the first offered apartment. The estimated equations all include year of application indicators, application household size, and application affordable house rent. 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.

* = p-value < 0.05, ** = p-value < 0.01; *** = p-value < 0.001.

Table A.1

Dose treatment effect of the share of criminal neighbors on youth crime

	Probability of being charged				Number of charges			
	Any crime	Property crime	Violent crime	Drug crime	Any crime	Property crime	Violent crime	Drug crime
Panel A								
Share of 15+ individuals in the section charged for property crime	0.006 (0.007)	0.002 (0.006)	0.004 (0.005)	0.007 (0.006)	0.075 (0.111)	0.056 (0.070)	0.005 (0.022)	0.013 (0.038)
Panel B								
Share of 15+ individuals in the development charged for property crime	0.006 (0.004)	0.005 (0.004)	0.002 (0.003)	0.004 (0.004)	0.041 (0.054)	0.016 (0.033)	0.007 (0.017)	0.018 (0.018)
Share of 15+ individuals in proximate developments charged for property crime	0.001 (0.006)	0.001 (0.005)	0.003 (0.004)	0.002 (0.005)	0.040 (0.088)	0.031 (0.069)	0.006 (0.017)	0.003 (0.024)
Observations	2230	2230	2230	2230	2230	2230	2230	2230

Panel A reports ITT estimates from equation (2), where Z_1 is the share of neighbors charged for property crime residing in the assigned section of the first offered apartment. Panel B reports results from ITT estimates (2) where Z_1 includes the share of 15+ years old individuals charged for property crime residing in the assigned development, and the share of 15+ years old individuals charged for property crime residing in proximate developments within the section. The estimated equations all include year of application indicators, application household size, and application affordable house rent. 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.

* = p-value < 0.05, ** = p-value < 0.01,*** = p-value < 0.001.

Table A.2

Dose treatment effect of the share of criminal neighbors on youth crime given area fixed effects

	Probability of being charged for				Number of charges			
	Any crime	Property crime	Violent crime	Drug crime	Any crime	Property crime	Violent crime	Drug crime
ITT	0.012* (0.004)	0.011* (0.004)	0.006 (0.004)	0.006 (0.004)	0.118 (0.061)	0.076* (0.036)	0.018 (0.020)	0.024 (0.016)
First stage	0.768*** (0.035) [305.44]	0.768*** (0.035) [305.44]	0.768*** (0.035) [305.44]	0.768*** (0.035) [305.44]	0.768*** (0.035) [305.44]	0.768*** (0.035) [305.44]	0.768*** (0.035) [305.44]	0.768*** (0.035) [305.44]
2SLS	0.015* (0.006)	0.014* (0.005)	0.007 (0.005)	0.008 (0.005)	0.153* (0.076)	0.099* (0.045)	0.024 (0.026)	0.031 (0.020)
Sample Mean	0.315	0.202	0.134	0.179	1.868	0.961	0.400	0.508
Observations	2230	2230	2230	2230	2230	2230	2230	2230

The first row reports ITT estimates from equation (2), where Z_1 is the share of neighbors charged for drug offenses residing in the assigned development. The second row reports the first stage coefficient of the 2SLS estimator presented in row 3 from Equation (1), where D is the share of neighbors charged for drug offenses residing in the new residential development, and Z_1 is the excluded instrument. The F statistic for the exclusion of the instrument Z_1 from the first-stage equation is presented in brackets. The estimated equations all include year of application indicators, application household size, application affordable house rent and area fixed effects. 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.

* = p-value < 0.05, ** = p-value < 0.01; *** = p-value < 0.001.

Table A.3

Dose treatment effect of the share of criminal neighbors on youth crime, by neighborhood characteristics

	Probability of youth being charged for			
	Any crime	Property crime	Violent crime	Drug crime
Panel A				
$Z_1 :=$ Share of 15-29 years old neighbors charged for drug crime	0.001 (0.002)	0.001 (0.002)	-0.001 (0.002)	0.002 (0.003)
Panel B				
$Z_1 :=$ Share of 30+ years old neighbors charged for drug crime	0.014*** (0.003)	0.010*** (0.003)	0.006* (0.002)	0.006* (0.003)
Panel C				
$Z_1 :=$ Share of neighbors convicted for drug crime	0.015*** (0.004)	0.010* (0.004)	0.005 (0.003)	0.007 (0.004)
Panel D				
$Z_1 := \begin{cases} \text{Share of } 15+ \text{ years old neighbors charged for drug crime} \\ \text{Share of } 18+ \text{ years old neighbors without employment} \end{cases}$	0.013* (0.004)	0.007 (0.004)	0.005 (0.003)	0.005 (0.004)
Panel E				
$Z_1 := \begin{cases} \text{Share of } 15+ \text{ years old neighbors charged for drug crime} \\ \text{Share of } 25+ \text{ years old with upper secondary education} \end{cases}$	0.015*** (0.004)	0.010* (0.003)	0.005 (0.003)	0.008* (0.004)
Panel F				
$Z_1 := \begin{cases} \text{Share of } 15+ \text{ years old neighbors charged for drug crime} \\ \text{Share of poor households} \end{cases}$	0.015*** (0.004)	0.010* (0.003)	0.005 (0.003)	0.008* (0.004)
Panel G				
$Z_1 := \begin{cases} \text{Share of } 15+ \text{ years old neighbors charged for drug crime} \\ \text{Share of non} - \text{western immigrant neighbors} \end{cases}$	0.016*** (0.004)	0.012*** (0.003)	0.007* (0.003)	0.010* (0.004)
Panel H				
$Z_1 := \begin{cases} \text{Share of } 15+ \text{ years old neighbors charged for drug crime} \\ \text{Number of } 15+ \text{ years old neighbors} \\ \text{Share of } 15+ \text{ years old neighbors charged for drug crime} \\ \times \text{ Number of } 15+ \text{ years old neighbors} \end{cases}$	0.015* (0.006)	0.011* (0.005)	0.010* (0.005)	0.010* (0.004)
Panel I				
$Z_1 := \begin{cases} \text{Share of } 15+ \text{ years old neighbors charged for drug crime} \\ \text{Number of charges of drug criminal neighbors} \\ \text{Share of } 15+ \text{ years old neighbors charged for drug crime} \\ \times \text{ Number of charges of drug criminal neighbors} \end{cases}$	0.015* (0.006)	0.012* (0.005)	0.010* (0.005)	0.011* (0.004)
Panel J				
$Z_1 :=$ Share of $15+$ charged for drug crime the last 2 years	0.012* (0.004)	0.011* (0.004)	0.001 (0.004)	-0.001 (0.004)
Panel K				
$Z_1 :=$ Share of 30+ years old neighbors charged for drug crime area fixed effects included	0.014*** (0.003)	0.011*** (0.003)	0.007* (0.003)	0.005 (0.003)
Observations	2230	2230	2230	2230

The different Panels of this table report ITT estimates from equation (2), where Z_1 includes the share of neighbors charged for drug offenses residing in the assigned development measured in different ways or controlling for different neighborhood characteristics. The estimated equations all include year of application indicators, application household size, and application affordable house rent. The estimated equations in Panel K include prior controls and in addition area fixed effects. Robust standard errors adjusted for development clustering of the first-offered social housing unit (259 cells for ITT) is in parentheses.

* = p-value < 0.05, ** = p-value < 0.01; *** = p-value < 0.001.

Table A.4

Dose treatment effect of the share of criminal neighbors on youth crime, by covariate set and outcome

	Probability of youth being charged for			
	Any crime	Property crime	Violent crime	Drug crime
Panel A				
$A \coloneqq \begin{cases} \text{Year of application} \\ \text{Application Household size} \\ \text{Application affordable house rent} \\ \text{Youth prior criminal behavior} \end{cases}$	0.012* (0.004)	0.008* (0.003)	0.004 (0.003)	0.006 (0.004)
Panel B				
$A \coloneqq \begin{cases} \text{Year of application} \\ \text{Application Household size} \\ \text{Application affordable house rent} \\ \text{Housing need urgency} \end{cases}$	0.014*** (0.004)	0.010* (0.003)	0.005 (0.003)	0.007 (0.004)
Panel C				
$A \coloneqq \begin{cases} \text{Year of application} \\ \text{Application Household size} \\ \text{Application affordable house rent} \\ \text{Gender} \end{cases}$	0.013** (0.004)	0.009* (0.003)	0.004 (0.003)	0.006 (0.003)
Panel D				
$A \coloneqq$ Application Household size	0.012*** (0.004)	0.009* (0.003)	0.004 (0.003)	0.006 (0.003)
Panel E				
$Y =$ Charged for offenses committed with a new neighbor	0.001 (0.002)	-0.000 (0.001)	-0.000 (0.001)	-0.000 (0.001)
Panel F				
$Y =$ Convicted for offenses committed within 24 months after 1st offer	0.010* (0.004)	0.008* (0.003)	0.006* (0.003)	0.004 (0.003)
Panel G				
$Y =$ Charged for offenses committed within a half-year after 1st offer	0.003*** (0.001)	0.002* (0.001)	0.001* (0.001)	0.001* (0.001)
Observations	2230	2230	2230	2230

Panels A to C present ITT estimates from equation (2) where the covariate set A include in addition to year of application indicators, application household size, and application affordable house rent , different individual characteristics of youth highly correlated with youth criminal behavior. Panel D presents the ITT estimates from equation (2) for a covariate set A including only application household size. Panels E to F present ITT estimates from equation (2) for different measures of youth criminal behavior. Concretely, Panel E reports the estimates on the probability of being charged with a new neighbor for different types of crime. Panel F reports estimates for the probability of being charged and convicted within 24 months after the first offered apartment. Finally, Panel G shows the estimates from panel equation (7). The estimated equations reported in Panels E to G include year of application indicators, application household size, and application affordable house rent. Robust standard errors adjusted for development clustering of the first-offered social housing unit (259 cells for ITT) is in parentheses.

* = p-value < 0.05, ** = p-value < 0.01; *** = p-value < 0.001.

Table A.5

Treatment effect of the share of criminal neighbors on youth crime

	Probability of being charged for			
	Any crime	Property crime	Violent crime	Drug crime
Panel A: Z_1 =1 {Share of 15+ individuals charged for drug crime residing in the assigned development>3.8%}				
Z_1 =0 {Share of 15+ individuals charged for drug crime residing in the assigned development<=3.8%}				
ITT	0.036*	0.020	-0.002	0.020
	(0.017)	(0.015)	(0.014)	(0.016)
First stage	0.737***	0.737***	0.737***	0.737***
	(0.016)	(0.016)	(0.016)	(0.016)
	[1992.07]	[1992.07]	[1992.07]	[1992.07]
TOT	0.059*	0.034	0.003	0.033
	(0.027)	(0.023)	(0.020)	(0.024)
Sample mean (Z_1=0)	0.285	0.187	0.126	0.157
Observations	2230	2230	2230	2230
Panel B: Z_1 =1 {Share of 15+ individuals charged for drug crime residing in the assigned development>4.0%}				
Z_1 =0 {Share of 15+ individuals charged for drug crime residing in the assigned development<=3.0%}				
ITT	0.060*	0.037*	0.021	0.046*
	(0.021)	(0.018)	(0.014)	(0.017)
First stage	0.748***	0.748***	0.748***	0.748***
	(0.016)	(0.016)	(0.016)	(0.016)
	[2042.01]	[2042.01]	[2042.01]	[2042.01]
TOT	0.087*	0.054*	0.032	0.065*
	(0.030)	(0.026)	(0.021)	(0.025)
Sample mean (Z_1=0)	0.263	0.175	0.108	0.131
Observations	1770	1770	1770	1770

Estimates are the ITT and TOT effects of two binary treatment analyses. Panel A reports the analysis where the treatment group includes all youth assigned to social housing developments with a concentration of neighbors with drug criminal charges under 3.8% (the median value over all sample), and the control group includes all other youth. Panel B reports the treatment effect analysis excluding youth assigned to developments with concentrations of neighbors with drug charges within the open interval (3.0%;4.0%). The first rows of both panels presents ITT estimates obtained with equation (2), the second rows of Panel A and Panel B reports first stage coefficients of the TOT estimates reported in the third rows of both panels. In these TOT analyses the endogenous treatment variable D is defined in the same way than the instrumental variable Z_1, but in terms of the concentrations of new neighbors youth are actually exposed to. Sample Means reported correspond to the outcome sample mean within the control group. The F statistic for the exclusion of the instrument Z_1 from the first-stage equation is presented in brackets. The estimated equations all include year of application indicators, application household size, and application affordable house rent. Robust standard errors ae adjusted for development clustering of the first-offered social housing unit (259 cells for ITT and 263 cells for TOT). * = p-value < 0.05, ** = p-value < 0.01; *** = p-value < 0.001.

Table A.6

Balance tests (p-value of joint insignificance test)

	Share of 15+ individuals in the section charged before the 1st offer			
	Any crime	Property crime	Violent crime	Drug crime
Panel A				
A:=Year of application	0.082	0.065	0.525	0.187
Panel B				
$A \coloneqq \begin{cases} \text{Year of application} \\ \text{Application Household size} \end{cases}$	0.268	0.403	0.912	0.545
Panel C				
$A \coloneqq \begin{cases} \text{Year of application} \\ \text{Application Household size} \\ \text{Application affordable house rent} \end{cases}$	0.265	0.412	0.899	0.534
Panel D				
$A \coloneqq \begin{cases} \text{Year of application} \\ \text{Application Household size} \\ \text{Application affordable house rent} \end{cases}$ Area fixed effects included	0.135	0.192	0.711	0.603
Observations	2230	2230	2230	2230

Each panel reports the p-value of the test for joint significance of individual characteristics X (dummy indicator for the individual has a shelter, gender, dummy indicator for the individual has been charged for property crime, dummy indicator for the individual has been charged for violent crime; dummy indicator for the individual has been charged for drug crime) in the equation (3) for different characteristics of social housing section Z_1 in the column. The estimated models reported in Panel A control for year of application. Those reported in Panel B control for year of application and application household size. The results shown in Panel C control for year of application, application household size and application affordable house rent. Finally, the results shown in Panel C control for all previous characteristics of youth applications and in addition for area fixed effects. Equation (3) is estimated in all cases with robust standard errors adjusted for development clustering of the first-offered social housing unit (259 cells). Sample size is 2230 14-29 years old individuals.

Table A.7

Sample moments of share of 15+ years old individuals living in the development with charges for drug crime before and after the date of the first offer

	Share of 15+ individuals living in the development the day of the first offered apartment or 1 year after that day charged for drug crime	Share of 15+ individuals living in the development the day of the first offered apartment charged for drug crime	Share of 15+ individuals living in the development 1 year after the day of the first offered apartment charged for drug crime
Sample Mean	0.0408	0.0410	0.0406
Standard Deviation	0.0223	0.0223	0.0224
Minimum	0.0000	0.0000	0.0000
Maximum	0.2000	0.16667	0.2000
Correlation		0.8060	
Observations	4130	2065	

Table A.8

Age distribution of sample youth

Youth Age, the day of the first offered apartment	Percent
14	3.32
15	3.59
16	3.50
17	5.96
18	11.66
19	5.96
20	6.77
21	6.55
22	7.04
23	6.46
24	6.14
25	8.03
26	7.31
27	5.70
28	5.70
29	6.32
Total	100.00

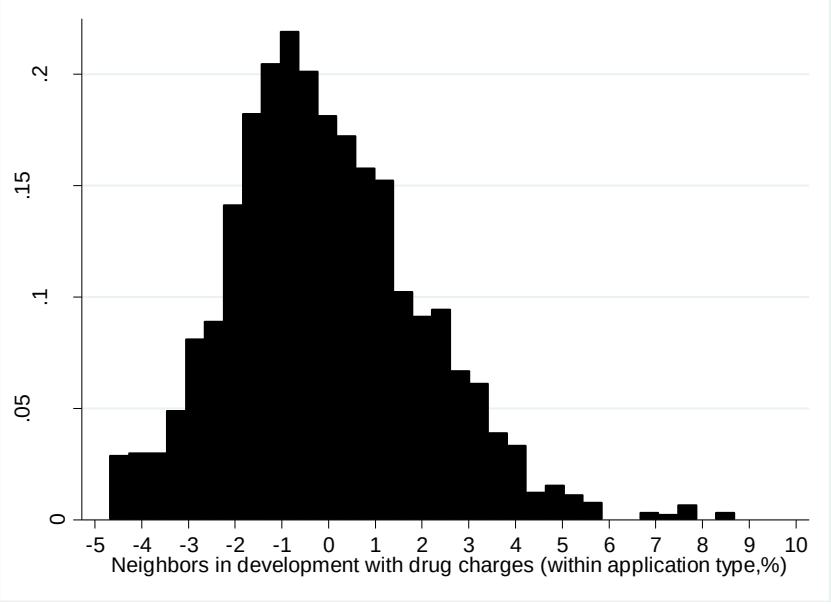

Fig. A.1

Histogram of share of 15+ years old individuals living in the development the day of the first offered apartment with prior drug offenses

The histogram is constructed with the residualized neighborhood composition given application household size, application affordable house rent and year of application