Does Growing Up in a High Crime Neighborhood Affect Youth Criminal Behavior?[*]

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Abstract: This paper investigates the effect of early exposure to neighborhood crime on subsequent criminal behavior of youth exploiting a unique natural experiment between 1986 and 1998 when refugee immigrants to Denmark were assigned to neighborhoods quasi-randomly. We find strong evidence that the share of young people convicted for crimes, in particular violent crimes, in the neighborhood increases convictions of male assignees later in life. No such effects are found for other measures of neighborhood crime including the rate of committed crimes. Our findings suggest social interaction as a key channel through which neighborhood crime is linked to individual criminal behavior.

Keywords: Neighborhood effects, criminal convictions, social interactions, random allocation

JEL codes: J0, H43

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1. INTRODUCTION

The question of whether lifelong disadvantage is related to the type of neighborhoods individuals are exposed to at a young age is not only of concern to social scientists but is a key question in the public policy debate (see e.g. early work by Brooks-Gunn et al., 1993, or studies using random assignment by Gould, Lavy, and Paserman, 2004 and 2011, and Oreopolous, 2003). While crime is an outcome of particular interest, existing studies on the effects of neighborhood characteristics on delinquent youth behavior point to overall mixed findings[1]. However, most studies in the literature are focused on the effect of *overall* neighborhood characteristics and are not intended to isolate the effect of neighborhood crime from other factors. A few exceptions are early non-experimental work by Case and Katz (1991), which finds a positive relationship between neighborhood crime and criminal behavior of youth, and a recent work by Ludwig and Kling (2007), which finds no evidence for higher violent crime arrest rates for the Moving to Opportunity (MTO) program participants in communities with higher crime rates. Overall, there is no conclusive evidence thus far on how early exposure to neighborhood crime might affect the longer term criminal behavior of individuals.

In this paper we present new evidence on the relationship between early exposure to neighborhood crime, and subsequent criminal behavior of youth. We exploit a unique

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[1] For instance, based on the Moving to Opportunity (MTO) program, Katz, Kling, and Liebman (2001) and Ludwig, Duncan, and Hirschfeld (2001) find that in the early years after reallocation, males in the treatment group had fewer behavioral problems and fewer arrests, while Kling, Ludwig, and Katz (2005) suggest that relocation often reduces arrests for violent crime in the short run but increases arrests for men in the long run. In a final evaluation of the program, Sanbonmatsu et al. (2011) summarize that overall there are no clearly significant effects of assignment to the MTO treatment groups on arrests or delinquent behavior. Other studies that investigate the association between economic conditions and crime rates include Fougère, Kramarz, and Pouget (2009), Grogger (1998), Gould, Weinberg, and Mustard (2002), and Machin and Meghir (2004).

natural experiment that occurred in Denmark between 1986 and 1998, when refugee immigrants were subjected to quasi-random spatial dispersal across municipalities. Specifically, our analysis focuses on the children of these individuals who underwent this random assignment before the age of 15, and whose crime convictions we observe in each year between the ages of 15 and 21. The possibility to generate complete histories of these individuals based on multiple administrative data sources allows us to provide rare evidence on the effect of early childhood environment on later crime behavior, based on a research design that addresses the problem of endogenous neighborhood selection.

One of our key departures from the literature is that thanks to the detailed administrative data, we are able to construct more precise measures of the criminal environment that might affect young people's behavior than used in previous work. In particular, we posit that rates of committed crimes in a neighborhood, a commonly used measure in the neighborhood literature, may not fully capture the criminal context that leads to criminal behavior. If, for instance, young people's criminal behavior is affected by social interactions with other criminal youth, the *share of criminals* in the neighborhood rather than the *crime rate*, might account for such effect more precisely.[2] We indeed find that it is the share of convicted youth criminals in the neighborhood that matters for inducing criminal behavior in young men. We find no such effect for other measures of neighborhood crime including the rate of committed crimes, consistent with findings in existing studies that use the same measure.

That youth criminal behavior responds to the presence of other criminals is also consistent with findings in previous studies that establish clear effects of delinquent peers

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[2] A strand of literature postulates "contagion" theories in which the likelihood of antisocial or criminal behavior increases with exposure to others who engage in similar activities (through e.g. exchanging information about the returns to crime, see Cook and Goos 1996 and Becker and Murphy 2000).

on one's own outcomes in more confined environments. For instance, Bayer, Hjalmarsson, and Pozen (2009), in an analysis of the influence that juvenile offenders serving time in the same correctional facility have on each other's subsequent criminal behavior, provide evidence that peer effects are aggravated if individuals have past experience in the same crime category. Also, Carrell and Hoekstra (2010) show that children from troubled families decrease their peers' test score outcomes and increase misbehavior, while Deming (2011), using data from public school choice lotteries, shows that peer effects are one explanation for a gain in school quality leading to a significant reduction in crime.[3] Our study adds to this literature, in showing that the share of convicted criminals in the neighborhood where young boys grow up affects their criminal behavior later in life.

A major challenge in neighborhood studies is the separation of the effect of neighborhood crime and other shared neighborhood effects, as the criminal context in which young people grow up may be correlated with institutions or local economic conditions. In our data, the large number of allocation neighborhoods allows us to condition on a wide variety of neighborhood characteristics. Moreover, the fact that individuals were assigned to different neighborhoods over more than a decade provides us with the rare opportunity to control for neighborhood fixed effects to eliminate all time-invariant neighborhood characteristics. Therefore, we can distinguish more forcefully than previous work between the effect of neighborhood crime on delinquent behavior and other associated neighborhood characteristics.

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[3] Additionally, Sacerdote (2001), using random assignment of Dartmouth College first-year roommates and dorm-mates, identifies peer effects on joining social groups, and Kremer and Levy (2003) report that being assigned to a roommate who drank prior to college has a sizeable effect on males' academic performance.

We find strong and systematic evidence that the share of convicted criminals living in the assignment neighborhood, and particularly the share of those convicted for violent crimes, affects later crime convictions of males, but not of females, who were assigned to these neighborhoods as children. Specifically, we find that a one standard deviation increase in the share of youth criminals living in the assignment neighborhood, and who committed a crime in the assignment year, increases the probability of a conviction for male assignees by between 5% and 9% later in life (when they are between 15 and 21 years old). We do not find any such evidence when we use other measures of crime, such as adult crime conviction rates or the rate of committed crimes.

Our results are robust to conditioning on a large set of neighborhood characteristics, including neighborhood fixed effects. On the other hand, none of the other neighborhood characteristics (such as neighborhood poverty, immigrant ratio, and policing) have any systematic effect on criminal convictions conditional or unconditional on our crime measure. It is primarily conviction rates for violent crime that trigger later criminal behavior and that induce not only violent criminal behavior but also convictions for property crime and drug crime. Most particularly, it is the share of young (<26 vs. >25) criminals living in the area that affects a young man's convictions later in life, and it is criminals from an individual's own ethnic group that matter most. Our evidence also suggests that young men are the most vulnerable to the effect of delinquent neighborhood influences in their early teens, when they are particularly receptive to role models and peer behavior (see e.g., Ingoldsby and Shaw 2002).

Overall, our findings show that how we measure the criminal context in which young people grow up is important for empirically identifying effects on later behavior.

We find that it is the presence of young criminals in the neighborhood, rather than crimes per se that affect criminal behavior later in life. Our results suggest that social interactions are one important mechanism in young men's engagement in criminal behavior. This speaks to Glaeser et al. (1996, 2003) in that contagion type transmission of crime propensity are likely to generate multiplier effects.

II. BACKGROUND, DATA, AND EMPIRICAL FRAMEWORK

II.A. The Danish Spatial Dispersal Policy

In 1986, the Danish Government, through the Danish Refugee Council, implemented a two-stage dispersal policy for asylum seekers whose applications had been approved (hereafter, refugees) with the primary objective of dispersing them across counties and municipalities based on the number of existing inhabitants.[4] Hence, the council first allocated refugees to counties proportional to the number of county inhabitants and then to municipalities within the counties proportional to the number of municipal inhabitants (Danish Refugee Council, CIU 1996, pp. 8–9).[5] Over the 13 years (1986–1998) during which the policy was in force, 76,673 individuals were granted refugee status (*Statistical Yearbook 1992*, *1997*, *2000*) and allocated across municipalities.[6]

Before being approved for refugee status, asylum seekers lived in Red Cross reception centers across Denmark, but within 10 days of receiving approval, the council

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[4] Following the usual convention, we use the term "asylum seeker" for a person seeking asylum and the term "refugee" for a person whose asylum status has been approved.

[5] In the policy period, Denmark was divided at the regional level into 15 counties with an average 323,788 inhabitants (in 1993), and at the local level into 275 municipalities with an average 18,838 inhabitants (in 1993).

[6] Edin et al. (2003) use a Swedish assignment policy similar to that studied here.

assigned them temporary housing in one of Denmark's 15 counties (Danish Refugee Council, CIU 1996, p. 9). After assignment to a county, the council's local office then assigned them to one of the municipalities within the county and helped them find permanent housing. To assist the council with its allocation decision, on receiving asylum, refugees filled in a questionnaire that asked for personal details like birth date, marital status, number of children, nationality - information that could have been used in allocation decisions. Thus, assignment was random conditional on these characteristics, which our analysis includes. By contrast, the council´s housing decision was not influenced by educational attainment, criminal record, or family income, as this information was not available to the council. Furthermore, and importantly, there were no face-to-face meetings between placement officers and refugees, and the only information available to the placement was that from the questionnaire.

Further, the council did not consider individual location wishes in the assignment process. Moreover, any reassignment request was considered *after* the individuals had first moved to the originally assigned municipality.[7] These points are important for our design because it is based on the randomness of the *first* assignment area and all variables refer to that initial municipality.

Once settled in the municipality of assignment, the refugees received social assistance for an introductory 18-month period while participating in Danish language

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[7] Interview on June 8, 2001, with former placement officers Bente Bondebjerg and Morten Iversen. When interviewed again on March 7, 2008, Bondebjerg, by then the Danish Refugee Council's chief consultant, did not recall that any refugee rejected the council's offer of housing assistance.

courses. Nevertheless, although the refugees were urged to stay in the assigned municipality during the entire introductory period, there were no relocation restrictions.[8]

This allocation policy was considered a success: according to the council's annual report for 1987, only two years after the introduction of the policy, refugees were living in 243 out of 275 municipalities (Danish Refugee Council 1987, pp. 30–31). The effect of the dispersal policy on the settlement pattern of refugees is clearly illustrated by the Danish Refugee Council's 1993 statistics on the geographical distribution of the overall population in Denmark and of refugee and non-refugee, non-Western immigrants. Of the overall population, 26%, 59%, and 15% lived in the capital and suburbs, towns, and rural areas, respectively, a dispersion that contrasts sharply with the 71%, 24%, and 5% of non-refugee, non-Western immigrants. Rather, the geographical distribution of refugees closely resembled that of the overall population, 33%, 56%, and 11% of whom lived in the capital and suburbs, towns, and rural areas, respectively. Figures A1a and A1b in the appendix, which outline the settlement of refugee immigrants in Demark in the 1980–1984 (pre-policy) and 1986–1998 (post-policy) periods, clearly show a strong concentration of refugees in the metropolitan areas of Copenhagen, Aarhus, Aalborg, and Odense in the pre-policy period but a fairly even distribution across areas in the post-policy period.

Given the way in which the dispersal policy was implemented, the allocation of families across municipalities should not have been responsive to youth crime or correlated with youth crime rates conditional on the information available from the

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[8]Appendix Table A1 reports the survival probabilities for the sample of refugee children we use for our analysis. Within the first year after assignment, nearly one in four households leave the assignment area; after that first year, however, movements are far less frequent. After eight years, one in two households still lives in the assignment area.

questionnaire, and that the council might have used for allocation purposes (e.g., household size). To test this assumption, we first define six measures of area crime in the municipality of assignment in the year of assignment: youth crime conviction rate, youth violent crime conviction rate, overall crime conviction rate, violent crime conviction rate for the entire population, number of reported crimes per capita, and number of reported violent crimes per 10,000 inhabitants (see Section II.C for a detailed explanation). We then regress these six measures on the individual characteristics of the refugee fathers observed by us and by the council at assignment (age, number of children, marital status), and on educational attainment, which is not available to the council but is available to us, conditioned on year of assignment and country of origin fixed effects. We perform these balancing tests for the final sample that we use for the analysis below.[9]

We report the results in Table 1, unconditional (odd columns) and conditional (even columns) on municipality fixed effects. We include the latter to validate the quasi-randomness of within-municipality variation in time of arrival, as we also estimate models that condition on municipality fixed effects. The *F*-test on the joint significance of the education variables (which are unobserved by the authorities) is rejected in each case. Only the number of children (which was known to the council from the questionnaire) is significant in three of the twelve regressions. The likely reason is that it was easier to find housing for larger families in rural areas, where crime rates are lower (see Table A5 for the correlations). We also estimate these same regressions for other area characteristics: poverty rate, immigrant share, log of inhabitants, teacher hours per pupil, crime detection rate, and number of police officers per 1000 inhabitants. Again, the

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[9] We have also performed these tests for the overall sample, including fathers of children whom we do not observe in each year, and therefore exclude from our analysis. Results are very similar than those for the estimation sample, which we report here.

educational variables are never statistically significant, with *p*-values for joint significance between 0.37 and 0.69. Finally, we compute the same regressions as in Table 1 separately for boys, and for girls. Only in one out of the 24 regressions could the null hypothesis of joint significance of the education variables not be rejected, for a test of size α=0.05. Thus, based on these tests, and the way the policy was implemented, we believe that the allocation of refugees to municipalities was quasi-random, conditional on the characteristics known to the council at assignment. [10]

II.B. Criminal Justice and Youth Crime in Denmark

Denmark, unlike many other countries, has no juvenile justice system: the minimum age of criminal responsibility is 15 years, above which young people are sentenced in the same courts as adult offenders and in accordance with the same criminal code.[11] For offenders below the age of 18, however, a number of sentencing options are available that do not exist for offenders 18 and older; for instance, the conditions for withdrawing a charge are more lenient, the most frequent sentence for those convicted is a monetary fine, and even when prison sentences are given, they are often suspended.

In our analysis we measure individual criminal activity based on charges and convictions[12] for offenses against the criminal code, which are recorded from the age of

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[10] We also regressed the crime conviction rate in the initial settlement area on the same set of variables for pre-reform refugee fathers (who immigrated 1981-1984), as in column 1 of Table 1. In spite of the low number of observations (N=164), the F-test on joint insignificance of educational attainment dummies cannot be rejected at the 10 percent level, with a p-value of 0.0642. Similarly, the F-test on joint insignificance of educational attainment dummies and age, number of children, and marital status has a p-value of 0.0327.

[11] We draw here on an excellent overview of the youth crime justice system in Denmark by Britta Kyvsgaard (2004).

[12] Criminal behavior in the U.S. is most commonly measured by arrests, which in Denmark are not as common. In fact, according to Statistics Denmark 2005 (Table 6.04), there were only 42,137 arrests in Denmark in 2005, meaning that less than one in seven charges was accompanied by an arrest. Hence, even

15 onward. We define convictions as court rulings that the suspect is guilty of the charge, signaled by the awarding of a sentence (either a fine, conditional withdrawal of charge, or a suspended or unsuspended prison sentence). A suspect is considered "not guilty" if a "not guilty" verdict was recorded or the indictment was dropped (Statistics Denmark 2005, p. 39). Although charges are typically a predecessor to a conviction, in the sample of refugee children we use for analysis only 29% of charges (excluding Traffic Offenses) led to a conviction. This is similar to the ratio of convictions to charges for Danes: for a random 10% sample of Danes born in 1980 and whom we follow until age 21, we find that 28% of charges led to a conviction.

In the Central Police Register charges and convictions are categorized into eight different types of offenses: sexual assault, violent crime, crimes against property, other offenses against the penal code, offenses against the Traffic Act, offenses against the Drugs Act, offenses against the Arms Act, and offenses against the Tax Acts or other special acts.[13] Individuals convicted for violation of the penal code (e.g., sexual assault, violent crime, crimes against property) or the Drugs Act have a criminal record for 2–5 years after conviction or release from prison depending on the sentence. Throughout the analysis, we omit offenses against the Traffic Act and combine the remaining offenses into four categories: property crimes, violent crimes (including sexual assault), drug crimes, and other offenses (see appendix Table A2, for a more detailed explanation). For these categories, we consider all offenses committed between the ages of 15 and 21 but distinguish between those at ages 15–17 and those at ages 18–21.

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though an arrest almost always leads to a charge, a charge only implies an arrest if the delinquent act was very serious.

[13] Other special acts cover such areas as marketing, pharmacy, trade, and restaurant laws.

II.C. Data

Primary data sources and samples. We derive our data from three primary sources: the Central Police Register, which records individual crime charge and conviction records for the full Danish population (including refugees); the Administrative Registers, which provide individual demographic characteristics (age, current residence, parents' ID numbers, country of origin, immigrant status, and date of immigration), and the Educational Institution Register and Surveys, which contain data on educational attainment. Because all such information is available for the 1980–2006 period, we can link individual records from the three registers via a unique ID number (the detailed definitions and primary data sources for each variable are given in appendix Table A3).

The information on crime charges includes the date of the charge and the start and end date of committing the offense. The information on crime convictions includes the date of conviction, verdict, sentence, and type of offense, which can all be linked to the start date of committing the offense.

To construct a data set containing different measures of neighborhood crime in each calendar year between 1980 and 2006 for all municipalities in Denmark, we link individual records from the Central Police Register with the Administrative Registers.

To construct our sample, we link individual records from all three registers and extract observations for refugee children who arrived in Denmark together with at least one parent between 1986 and 1998 from one of the following eight source countries: Lebanon, Iran, Iraq, Somalia, Sri Lanka, Vietnam, Afghanistan, and Ethiopia.[14] Refugees

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[14] Refugees from the former Yugoslavia are excluded from our sample because in contrast to refugees from other refugee-sending countries, they were initially granted provisional asylum and therefore subject to a special refugee dispersal policy implemented in 1993 (the so-called Bosnian program). Refugees subject to the Bosnian program were accommodated in refugee-reception centers and "refugee villages."

from these countries accounted for more than 86% of the total number of permanent residence permits granted to refugees between 1985 and 1997.[15] This sample contains observations on the children (below age 15 at the time of immigration to Denmark) of adult refugees who were assigned to a municipality by the council under the terms of the 1986–1998 ordinary spatial dispersal policy. We define a refugee as an individual who (i) immigrated from one of these eight countries during the 1986–1998 period and (ii) at the time of immigration (i.e., the year of receiving a residence permit) was not married to either an individual from a non-refugee sending country or an immigrant from a refugee-sending country who had immigrated at least one year earlier. We impose the latter criterion in order to limit the refugee sample to refugees assigned to a location by the council after being granted asylum.

The children analyzed are the subgroup of refugees who were under 15 at the time of assignment, have at least one refugee parent residing in Denmark, immigrated at most one year after the refugee parent(s), and have records in the registers until the age of 21. We exclude refugee children who cannot be followed up to age 21 in the administrative registers, which comprises 21% of all individuals.[16] The final outcome is a sample of 4,425 children, 55% of them male, whose individual and family background characteristics and country of origin are reported in appendix Table A4. For this sample

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[15] For these groups, the number of non-refugee immigrants relative to the total number of immigrants (after exclusion of immigrants who were married to a resident in Denmark from a non-refugee sending country in the year of immigration) is less than 4.8%.

[16] Of the 5,615 refugee children who arrived in Denmark before age 15 together with at least one parent, we exclude 975 refugee children who had left Denmark before age 21 and 215 refugee children who were not observed in every year between arrival and age 21. To check whether this attrition is random with respect to our outcome variables, we regress the indicator variable for whether the individual leaves the sample on the youth crime conviction rate in the municipality of assignment, as well as on observed background characteristics. This regression yields a t-value for the crime variable of 0.25. If our regressor is the overall crime conviction rate in the municipality of assignment or the youth violent crime conviction rate, the t-value is 0.69 or 0.7, respectively.

of children, we observe all criminal convictions and charges between the ages of 15 and 21. We construct indicator variables for at least one conviction in the 15-21, 15–17 and 18–21 age range. We also construct a variable for the number of convictions and convictions for particular types of crime, as well as an indicator variable for at least one charge and the number of charges in these age ranges.

Criminal offences, crime measures, and neighborhood characteristics: The numbers in Table 2 show that 38% of all refugees who arrived in Denmark as children had been charged (first row) and 31% convicted (second row) at least once for a criminal offense by the age of 21.[17] This compares to about 13% (and 11%) for a 10% random sample of Danes born in 1980. Interestingly, the charge rates for males (55%) and females (17%) are not dissimilar to the arrest rates reported by Kling, Ludwig, and Katz (2005) for the MTO sample, in which 53% of males and 19% of females had been arrested at least once.[18] Criminal convictions by the age of 21 are also much higher for males than for females, 46% versus 13%. Subdividing convictions by crime type further reveals that, perhaps not surprisingly, the largest contributor is property crime, followed by violent crime.[19]

Panel B of Table 2 shows the distribution of the number of convictions. The number of repeat offenders is considerable: Among men, 60% of those who were convicted at least once, had more than one conviction, and 23% had more than 4 convictions. In panels C and D of the Table, we break down overall charge and

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[17] The ratio of convictions to charges before age 21 is 28% (see numbers in panel C and D).

[18] These arrest rates, based on youth 15–21 at the end of 2001, capture criminal behavior for that group through the end of that year.

[19] Subdividing property crime further shows that of those convicted at least once for a property crime, 61% of males and 87% of females are convicted for theft. Males also have convictions for burglary (22%), fraud (11%), handling of stolen goods (13%), robbery (17%), and vandalism (10%), whereas the only other large categories for females are fraud (11%) and forgery (6%).

conviction probabilities by age ranges 15–17 and 18–21. These figures show for males that about 20% carry convictions for crimes committed in both age ranges. The overlap for females, in contrast, is far smaller.

Our measure of neighborhood is municipalities, which is the spatial unit across which random assignment took place. However, because a municipality has about 18,500 inhabitants on average, larger than the average 8,000 individuals in a census tract, one concern may be that this neighborhood definition is too large and could result in less precise estimates. On the other hand, defining neighborhoods too narrowly may lead to the omission of a considerable number of interactions. We investigate these concerns computing the residency of co-offenders for the first crime for which individuals have been convicted.[20] About one in four first convictions was for a crime for which at least one other offender was convicted. Of these co-offenders, 34% lived in the same smaller spatial area in the year of committing the crime (with an average of 2,300 inhabitants), but 75% resided in the same municipality of residence in the year of committing the crime, which is our neighborhood measure.

Our main measure of area youth criminality (C_{tr}) is the share of individuals aged 15-25 convicted of a crime committed in calendar year t (which we relate to the year of our sample individuals' assignment) and who lived in municipality r at that time. We refer to this share as the *youth crime conviction rate* of municipality r in year t. We further distinguish between the shares of individuals convicted for particular crimes, like violent crimes and property crimes, and compute crime measures for different age ranges. As alternative crime measures, we also compute more commonly used measures of

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[20] We compute an indicator variable using unique case numbers that identify individuals convicted for the same crime and same type of crime.

neighborhood crime, such as the number of reported crimes per capita and the number of reported violent crimes per 10,000 inhabitants for each municipality in each year.

We include three sets of municipality characteristics, the first of which comprises the log of the number of inhabitants, the relative poverty rate, and the share of immigrants in the neighborhood. Glaeser and Sacerdote (1999) hypothesize that the area's "population size" is likely to be related to criminal activity, as well as to opportunity, because it may directly impact returns to crime and arrest probabilities (see also Glaeser, Sacerdote, and Scheinkman 1996). We include poverty as it may be a good summary measure for neighborhood quality (see Kling, Liebman, and Katz, 2007).[21] We include the share of immigrants living in the area because it may relate to neighborhood segregation, thereby inducing social conflict as well as criminal behavior (e.g., Logan and Messner 1987).[22]

Our second set, designed to condition on local school resources, contains only one measure: the weekly number of teacher hours per pupil in the municipality.[23] We include this measure because school resources may affect the relative attractiveness of pro- versus antisocial behavior (Sanbonmatsu et al. 2011), including criminal activities. Lochner and Moretti (2004), exploiting changes in state compulsory school attendance laws over time to estimate the effects of schooling on crime, find that schooling significantly reduces the probability of incarceration and arrest. Likewise, Deming

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[21] We compute the poverty rate as the share of adults in the municipality who live in a household that meets the OECD definition of "relatively poor" (i.e., disposable household income below 50% of the national equivalence-scaled median disposable household income).

[22] Although the results reported here are based on the share of immigrants and their descendants, we have also experimented with other variables like the share of individuals coming from refugee countries. We discuss some of these alternative specifications in Section IV.B.

[23] Because this information is available for the 1981–1993 period only, we replace missing values for the subsample of individuals who immigrated after 1993 with the 1993 value for the municipality of assignment. We have also experimented with an alternative measure of local school resources: the average class size.

(2011), using data from public school choice lotteries, provides strong evidence that better schools lead to a reduction in crime seven years after the experiment.

Our third set, constructed to reflect the efficiency and presence of police services in the area, contains two measures of policing. The first is the number of police officers per 1,000 inhabitants in the municipality, and the second, the crime detection rate in the municipality, obtained from administrative police statistics and computed as the number of charges relative to the number of reported crimes (see appendix Table A3.B for further details).[24] The allocation of more police resources to areas with higher crime rates may create a correlation between area crime rates and individual criminal convictions. For example, Kling, Ludwig, and Katz (2005) suggest that better policing may increase arrest probabilities, but also deter criminal behavior. Levitt (1997), exploiting electoral cycles in police hiring to estimate the effect of police on crime, finds that the size of the police force reduces both violent and property crime (see also Levitt 1998).

We display the correlations between the various crime measures and municipality characteristics across municipalities during 1986–1998 in Table A5. Interestingly, the youth crime conviction rate – our primary measure of area crime – is only mildly correlated with alternative measures of neighborhood crime, like reported crimes per capita (0.5) or reported violent crimes/10,000 (0.43). Also, it appears not to be strongly correlated with other neighborhood characteristics, with the strongest correlation with the immigrant share (0.44).

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[24] Note that these variables (as all other area variables) are measured in the year of assignment, so that they cannot be affected by assignees' criminal behaviour.

III. EMPIRICAL METHODOLOGY AND INTERPRETATION

The main question posed in this paper is whether children assigned to a neighborhood with a higher crime rate are more likely to engage in criminal behavior later in life. To answer this question, we draw on data from a Danish refugee allocation experiment that provides a unique opportunity to address the problem of individuals sorting into residential locations, and to assess the causal effect of neighborhood crime on later criminal behavior. Our basic specification represents the criminal behavior of individual i assigned to neighborhood r in assignment year t:

$$(1) \qquad y_{itr} = a_1 + a_2{}^M G_i\, C_{itr} + a_2{}^F (1 - G_i)\, C_{itr} + a_3 G_i + X_{it} a + T_t + \varepsilon_{ijt}$$

where the variable y_{itr} is an indicator that takes the value 1 if individual i assigned to location r in year t is convicted of a crime committed in the age range 15–21, 15–17, or 18–21. Alternatively, we use the number of convictions. The variable C_{itr} is a measure for neighborhood crime. To account for differences between individuals' pre-assignment characteristics, including those known to the council at assignment (i.e., household size, country of origin, parental age and marital status) and those not known to the council (e.g., parental educational attainment),[25] the vector X_{it} contains individual background characteristics in the year of assignment. T_t is a year of assignment dummy, and ε_{ijt} is an error term. To enhance efficiency, we estimate Equation (1) for the pooled sample of males and females but allow both the level and the impact of area crime to differ between

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[25] These variables also account for the variation in socioeconomic background that may be associated with child neglect. For instance, Currie and Tekin (2012) demonstrate a strong association between child maltreatment and future crime.

genders by measuring the interaction between a gender dummy ($G = 1$ for males) and area crime rates.

In our research design, the quasi-randomization of refugees to neighborhoods ensures that C_{itr} is not correlated with ε_{ijt} in (1), conditional on the vector X_{it} variables that were known to the council when assigning individual families. Hence, the key parameter of interest is $a_2^{\,j}, (j = M, F)$, which, given our research design, is a causal parameter. However, it may not measure the effect that area crime itself has on criminal activity, but rather represent the effect of area crime through its correlation with neighborhood characteristics, which in turn affect criminal behavior. For instance, neighborhoods with higher crime conviction rates may also be neighborhoods with higher poverty, higher immigrant density, lower school quality, and/or more crime prevention measures, characteristics that may be correlated with both crime conviction rates at assignment and later individual convictions.

In our case, the large number of assignment neighborhoods with varying characteristics provides sufficient variation to allow identification of both the effects of neighborhood crime and all plausibly relevant neighborhood characteristics. A further unique characteristic of our experiment is its prolonged implementation over multiple cohorts of assigned refugees, which allows us to estimate the effects of neighborhood crime – identified by differences in crime rates upon assignment across cohorts within neighborhoods – by conditioning on neighborhood fixed effects. This should allow assessment of whether it is crime itself that affects criminal behavior. Thus, to isolate the effect of crime from other neighborhood and institutional characteristics at assignment,

we add a vector A_{tr} that includes the three sets of area characteristics discussed in Section II.C., and we condition on municipality of assignment fixed effects R_r to obtain:

$$(2) \quad y_{itr} = b_1 + b_2^M G_i\, C_{itr} + b_2^F (1 - G_i)\, C_{itr} + b_3 G_i + X_{it}b + A_{tr}d + T_t + R_r + e_{ijt}$$

The key variable in (2) is C_{itr} which we compute as the share of individuals aged 15-25 who were convicted for a crime committed in year t in which individual i was assigned to municipality r in which the offender lives (which we refer to as the *youth crime conviction rate*). Our unique administrative data provides us with the rare opportunity to compute this measure, which should best capture the criminal context to which young people are exposed. We compare and contrast this variable to alternative and more commonly used measures of crime, such as reported crime rates.

IV. RESULTS

IV.A. Main Estimation Results

Effect of youth crime conviction rate on criminal behavior. Table 3 reports our estimates for the coefficients on the neighborhood youth crime conviction rate at assignment, normalized by the standard deviation of that variable across all assignment areas and assignment years, for males (panel A) and females (panel B). Specification (1) conditions only on a gender dummy. Specification (2) conditions on year of assignment fixed effects, country of origin fixed effects, age at assignment dummies, family background characteristics (one dummy each for single parent, number of siblings, and father and mother's educational attainment and age), and the log number of individuals from the

same origin country in Denmark, all measured at the time of assignment. Specification (3) includes area characteristics that measure the neighborhood's socioeconomic context (log of inhabitants, poverty rate, and share of immigrants). Specification (4) additionally conditions on the weekly number of teacher wage hours per pupil, as well as the crime detection rate and number of police officers per 1,000 inhabitants as measures of police efficiency and presence. Finally, Specification (5) additionally conditions on municipality fixed effects. The standard errors take into account the clustering of the observations by municipality of assignment.

In the first block of each panel, we report the coefficients of the crime conviction rate in the assignment year in the assignment municipality when the dependent variable is a binary indicator for at least one conviction in the 15–21 (first row), 15–17 (row 2), or 18–21 (row 3) age range; in the second block, we report results when the dependent variable is the number of convictions. Results refer to our sample of youth assigned to an area before age 15, with the average age at assignment being 9. Convictions for crimes committed at age 15 or later are, on average, convictions for crimes committed at least six years post assignment.

The results for males, given in panel A, Table 3, point to a positive effect of the share of convicted criminals in the area of first assignment at assignment date on the probability of later conviction. Unconditional on background and neighborhood characteristics, the probability of later conviction in the 15–21 age range is about 2 percentage points higher in an area with a one standard deviation higher area crime conviction rate. The estimate is similar for convictions in the other two age ranges (18–21 and 15–17). Conditioning on background and neighborhood characteristics (see columns

(2-4)) changes the coefficient estimates only slightly. Overall, the point estimates for the 18–21 age range are slightly more precise and larger than those for the 15–17 age range. The estimates in the second block of panel A, in which the number of convictions is a dependent variable, are larger, and estimates remain similar across specifications.

Column (5) reports the results of additionally conditioning on all area characteristics that are fixed over time; for instance, neighborhood-specific "cultures" of attitudes toward crime that affect policing or reporting, or differences in institutions or social composition that are not captured by our neighborhood variables but are correlated with crime rates and individual convictions alike. For conviction probabilities, the estimates for crimes committed in the 18–21 age range increase only slightly, while those for the 15–17 and 15–21 age ranges nearly double in magnitude. A similar pattern emerges for the estimates on number of convictions. Overall, these findings provide solid evidence that the share of youth convicted for a crime committed in the assignment neighborhood during the assignment year leads to an increase in crime convictions of children assigned to that neighborhood in that year. Based on the estimates in the last two columns, and given the overall conviction rate (number of convictions) of 46% (1.48 convictions) for male refugee youths aged 15 to 21, a one standard deviation higher youth crime rate in the assignment area increases the probability of a crime conviction by between 5% and 9% and the number of convictions by between 7% and 11%.

For females, as shown in panel B, the estimates are much smaller and in none of the specifications are they statistically significant, findings that stand in contrast to those for males. These estimates do not point to any systematic relation between area crime conviction rates at assignment and individual criminal behavior. They are in line with the

criminology literature that suggests males and females react differently to detrimental neighborhood conditions.[26] For example, Clampet-Lundquist et al. (2006) conclude, based on interviews with youth from the MTO experiment, that male youth have more negative peer exposure because they spend their free time in closer proximity to illegal activity than do females. Moreover, as also pointed out by Steffensmeier and Allan (1996), crime is stigmatizing for females and thus has a greater potential cost to life chances. These findings are also in line with an increasing number of experimentally based studies that point to gender differences in response to social context.[27] Our findings for any of the following estimations indicate no systematic relations between female criminal behavior and neighborhood characteristics, so for the remainder of the discussion, we focus on the results for males only.

Other neighborhood characteristics: In appendix Table A6.A, we report the estimates for the effect of neighborhood characteristics (measured in the year of assignment) other than crime conviction rates on criminal behavior in males aged 15 to 21. Here, the specifications include individual and background characteristics (uneven column numbers), and, in addition, neighborhood fixed effects (even column numbers). With few exceptions, these neighborhood characteristics have no effect on criminal behavior. One exception is perhaps the number of police officers, which decreases the

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[26] Mears , Ploeger, and Warr (1998) suggest that boys may be differently exposed to, and affected by, the same crimogenic conditions. Building on earlier work by Sutherland (1947) and Gilligan (1982), they argue that, given the same choices, males are more likely than females to have delinquent friends. Once exposed, this negative peer influence is reduced or even counteracted by females because of stronger negative moral evaluations. Moffitt, Caspi, Rutter, and Silva (2001) (among others) also provide empirical evidence for males being more likely to have delinquent peers, while Huesmann and Guerra (1997) show that females have more favorable normative beliefs than males about aggression and antisocial behavior.

[27] For instance, Kremer and Levy (2003) find that previous alcohol consumption by randomly assigned roommates has a strong effect on the academic performance of males but not females. Similarly, Katz, Kling, and Liebman (2001) find that MTO reallocation led to a decline in behavior problems among boys in both the experimental and Section 8 comparison groups but had no noticeable impact on girls. Kling, Liebman, and Katz (2007) conclude that female and male youth in the MTO treatment group responded to similar new neighborhood environments in different ways along various dimensions.

number of convictions if we condition on neighborhood fixed effects (thereby concentrating on the within-neighborhood *changes* in this variable) and the number of teacher hours, which is associated with an *increase* in the number of convictions unconditional on neighborhood fixed effects. In Table A6.B, we report the results of the same regressions conditioned on the crime conviction rate. The coefficient of the youth crime conviction remains stable and significant in most specifications. Again, the results give no indication that any of the other neighborhood characteristics is systematically related to later criminal conviction.[28]

Robustness checks. In Table 4, we report the results of various robustness checks in which we use specifications that correspond to Specifications (4) and (5) in Table 3 and focus on crimes committed in the 15–21 age range. In panel A, we report the results from Table 3 (males) as a reference; in panel B, we use criminal charges, rather than convictions, as an alternative measure for individuals' crime criminal behavior. These specifications produce estimates with large standard errors, suggesting that crime charges are a noisy measure for criminal propensity (remember that only 29% of charges lead to convictions, see Table 2). This supports our use of crime convictions as a measure of criminal behavior. In panels C and D, we re-estimate the model excluding Copenhagen and Aarhus (the two largest municipalities in Denmark) to address concerns that the estimates could be driven by these two municipalities. The results are very similar to those in panel A. To address the additional concern that our findings may be driven by

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[28] We also construct an index of neighborhood characteristics based on the weights from regressing Danish youth crime convictions in the 15–21 age range on all neighborhood characteristics in the neighborhood of residence at age 14. This index is never significant; for instance, for Specification (4) in panel A of Table 3, its estimate is -0.252, with a standard error of 0.279. We also run regressions using Specification (4) in Table 3 in which we additionally condition on all the interaction terms of the crime conviction rate and the neighborhood characteristics. Again, all these interaction terms are insignificant.

the shares of immigrants who belong to the same ethnic group, we also investigate the impact of ethnic group crime on criminal behavior, conditional not only on the overall share of immigrants and descendants in the assignment area in the assignment year but also on the share of co-nationals. These results, reported in panel E, again suggest that including this variable has almost no effect on the coefficient estimates. In panel F, we report the results once the unemployment rate in the assignment area at the assignment date is included as an additional regressor. This inclusion barely affects our estimates. Further estimations (not reported) using additional area characteristics like the share of lone mothers, the share of teenage mothers, or indices of inequality also do not change the estimates of the crime conviction rates.[29]

Crime-specific responses and crime-specific convictions: In Table 5, we report estimates when neighborhood crime conviction rates are broken down by crime categories, thereby distinguishing between conviction rates for violent crimes, property crimes, drug crimes, and other offenses. Here, we normalize every crime conviction rate by its standard deviation. The results indicate that it is mainly *violent* crime conviction rates that affect individual criminal behavior in each of the three age ranges, and coefficients for this crime category are precisely estimated. The effect of property crime conviction rates is somewhat smaller but not statistically significant.

We therefore wonder whether the type of neighborhood crime convictions has a different effect on the different types of crime for which young offenders are convicted. We explore this question in Table 6, in which the rows contain estimates for different

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[29] To further check whether our results are driven by one particular refugee group, we re-estimate our basic specification leaving out one minority group at a time. Again, the basic results remain unchanged (results not reported). For instance, for Specification (4), Table 3, our estimates for the probability of a conviction in the 15–21 age range vary between 1.7 and 2.7 percentage points.

neighborhood crimes and the columns, those for different types of convictions in the 15–21 age range. These results indicate that the youth crime conviction rate for violent crimes does affect conviction probabilities for violent crimes, property crimes, and drug crimes. More specifically, a one standard deviation increase in the youth violent crime conviction rate in the assignment area increases the probability of a conviction for a violent crime by 9%, the probability of a property crime offense conviction by 10%, and the probability of a drug offense by 1.6%. The effect of property crime conviction rates is smaller and only significantly different from zero in one case. Taken together, then, these results suggest that it is predominantly violent crime conviction rates that affect individual criminal behavior later on.

IV.B. Mechanisms

We now turn to an assessment of the possible mechanisms that link neighborhood crime at assignment, and later criminal behavior. Our results in the previous section suggest that higher crime conviction rates in the assignment area at assignment lead to criminal convictions of assignees later in life, *conditional* on a large array of observed neighborhood characteristics, as well as neighborhood fixed effects. Thus, it seems unlikely that institutions or neighborhood culture, correlated with both neighborhood crime rates and individual's criminal behavior later in life, drive our results. We now pursue this further. We argue that – if social interaction is a main channel that relates the two – then what should matter for later criminal behavior is not the crimes committed in the assignment neighborhood, but the criminals who actually live in that neighborhood. Also, if social interaction is an important transmission channel, it should be youth crime

conviction rates, and not overall or adult crime conviction rates, that affect later criminal behavior. We further assess whether convicted criminals in the assignment area who are possibly easier to interact with – such as those who are from the same ethnic group than the assignee– have a more pronounced effect on individuals' criminal behavior later in life. Finally, we investigate at which assignment age exposure to neighborhood crime matters most for later criminal delinquency.

Alternative local area crime measures. Table 7 reports estimation results for different measures of neighborhood crime, all standardized by the standard deviation of the respective crime measure. Specifically, the table reports the outcomes for Specifications (4) and (5) from Table 3 for the age range 15–21. The estimates in panels A and B replicate those in Tables 3 and 5 (for overall and violent crime conviction rates). In panels C and D, we condition on two additional crime measures frequently used in the literature: the number of reported crimes per capita and the number of reported violent crimes per 10,000 inhabitants. In our case, the coefficient estimates for reported crimes are small with large standard errors, and the estimates for reported violent crimes are likewise small and only significant in one specification. These estimates therefore provide little evidence that children allocated to areas with higher crime rates are more likely to be convicted for a crime later in life. Thus, it is the share of criminals who live in the neighborhood, rather than the crimes committed, that seems to matter for later criminal behavior.

The importance for later criminal behavior of criminals living in the area, rather than crimes committed in the area, provides support for social interaction as a primary conduit between neighborhood crime and own criminal behavior. To investigate this

further, in panels E and F, we report the results conditioned on both crime conviction rates and reported crimes, reported first for all crimes and then separately for violent crimes. Conditional on the share of individuals convicted for a crime, the rates of committed crimes do not affect criminal behavior: the estimated coefficients on rates of crimes committed are small, with varying signs, and never close to statistically significant, while the estimated parameters on the share of convicted criminals are very similar to those in panels A and B. [30]

In panels G and H, we report the findings conditioned on youth conviction rates and overall conviction rates (including convictions of individuals over 25) for all crimes (panel G) and for violent crimes only (panel H). The coefficients on the youth crime rates are again similar to those in panels A and B, while the coefficients on the overall crime conviction rates (which pick up the effect of the conviction rates for individuals 25+) remain mostly insignificant. It is thus young criminals living in the neighborhood at assignment that matter for later criminal convictions, which is compatible with social interaction being an important channel of transmission. [31]

Overall, these results suggest that when studying the effect of neighborhood crime on criminal behavior, it matters how criminal context is measured. One reason why measures of reported crime per capita may only incompletely capture the criminal

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[30] These specifications also address another important issue: resource swamping, which may lead to a correlation between crime rates and individual criminal behavior if an increase in crime reduces the resources available for crime investigation. Although our policing measures address this issue, the specifications in panels E and F should address any remaining concern.

[31] We have also estimated the effect of neighborhood crime on criminal behavior (measured as convictions) of the parents of our sample individuals, seven years after assignment (21.8% of fathers, and 14.9% of mothers carry a conviction at that point). None of the crime measures has any effect. For instance, the coefficient of the youth crime conviction rate on the probability that fathers have a conviction seven years after assignment is 0.005 (0.012) and 0.01 (0.020) (for specifications like 4 and 5 in Table 2 respectively). This excludes intergenerational transmission as a possible mechanism linking neighborhood youth crime, and later criminal behavior (see Hjalmarsson and Lindquist,2012). It also shows that it is young people who are affected by youth crime in the neighborhood.

environment to which young people are exposed is that they do not allow distinction between neighborhood crime intensity by the age range of offenders; the results in Table 7 show that the share of adult criminals seem not to affect later youth criminal behavior. Further, measures of reported crimes per capita may be a noisy proxy for the criminal environment to which young people are exposed, due to e.g. different degrees of repeat offending across neighborhoods (see Table 2), or criminals travelling to commit crime.[32]

Crime conviction rates by origin group. If social interaction is a main channel through which neighborhood crime affects criminal behavior later in life, then it is reasonable to expect that young men will be more affected by criminals from their own ethnic group with whom they have more communication and interaction opportunities. In Table 8, we report the results of additionally conditioning on conviction rates of different origin groups. Specifically, we condition in addition on the crime conviction rates of young offenders who are immigrants or descendents of immigrants (columns (3) and (4)), belong to a minority group from one of the eight refugee origin countries (columns (5) and (6)), belong to the same origin country group as the respondent (columns (7–10)), and are from a minority group that belongs to one of the eight refugee origin countries but not to the respondent's group (columns (9) and (10)). Each group-specific conviction rate is normalized by the group-specific standard deviation. The coefficient estimates on these additional variables measure the impact of the youth crime conviction rate for any of the respective groups, over and above the effect of the overall youth crime conviction

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[32] Some evidence for travels to crime in Denmark is given by Sorensen (2007) who reports that the average journey of crime for burglary was 14 km, with a median of about 5 km, using detailed administrative data for 2002/2003. He also reports evidence that criminals "travel" to affluent areas. This is compatible with our data, where the correlation between municipality crime conviction rates and mean log income is 0.003, but increases to 0.11 between the latter variable and reported crimes per 10,000 inhabitants.

rate. We report results for the conviction probabilities and the number of convictions in panels A and B, respectively, for the 15–21 age group.

The inclusion of these additional variables barely changes the coefficient estimates of the overall youth crime conviction rates, and the youth crime conviction rate of other minority or immigrant groups does not additionally explain both conviction probabilities and the number of convictions. On the other hand, the youth crime conviction rates of co-nationals clearly affect later criminal behavior, over and above the effect of the overall share of convicted criminals. Whereas the coefficients on the overall share remain basically unchanged, a one standard deviation increase in the share of criminals from the same ethnic group increases conviction probabilities by another 2 percentage points (or about 4%) and the number of committed crimes by 0.1 (about 7%). These figures suggest that the effect of the share of criminals living in the assignment area who were convicted for a crime committed in the assignment year is larger when the criminals belong to the same ethnic group. This finding is further evidence that social interaction may be an important transmission channel.

Crime and age at assignment. Despite a variety of theoretical explanations for the effect of peers on children's behavior throughout childhood and the teenage years (see Corsaro and Eder 1990 for an overview), more recent literature does agree that peers may be as important as adults for children's development of social skills (e.g., Youniss 1980) through the sharing of and belonging to particular cultures. This process is more intensive at ages when children interact with others – for instance, through school leisure activities – and at ages when they are particularly susceptible to peer influence. Indeed, Ingoldsby and Shaw (2002) argue that, whereas at a young age and before primary school, children

may be shielded by their parents from outside influences, the contact with the neighborhood expands rapidly at the onset of middle childhood, and peer delinquency may particularly affect the development of individual anti-social pathways in that age range.

Because the children in our sample are assigned to neighborhoods at different ages between 0 and 14 years, it is likely that the influence of neighborhood crime on young people's criminal behavior depends on how receptive they are to outside influences at assignment, which in turn may depend on their age at that point. To investigate this, we break the assignment age down into three categories: individuals assigned between 0 and 5 years (i.e., before starting primary school at age 6), individuals assigned between 6 and 9 years, and individuals assigned between 10 and 14 years of age,[33] which produces three groups of 1,014, 1,466, and 1,945 individuals, respectively (see Table A7).

Table 9 reports the results for convictions in these three age ranges, again based on Specifications (4) and (5) from Table 3. Panel A reports conviction probabilities and panel B, the number of convictions. Overall, the results suggest that those 10 to 14 years old at assignment are most affected by the neighborhood conviction rate: a one standard deviation increase in the crime conviction rate in the assignment area increases the probability of being convicted in the 15–21 age range by about 4-6 percentage points (or 9–13%). For individuals assigned before age 6, the estimates are generally small and have large standard errors. The point estimates for individuals assigned between age 6

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[33] Our data restrict the three age groups to assignment years 1986–1991, 1986–1994, and 1986–1998. Note that differences in assignment age may also change the potential exposure to neighborhood crime, a variable only partly captured by the age of individual assignment on which we condition. The estimates we present are thus a combination of group differences in exposure and age at assignment.

and 9 are similar to the overall estimates in Table 3 for crimes committed in the 15–21 and 18–21 age ranges.

In panel B, we report the results for the number of crimes for which an individual has been convicted. Again, the largest and more precise estimated effects are for the assignment age range between 10 and 14, with a one standard deviation increase in the youth crime conviction rate leading to about an 11–14% increase in the number of crimes committed between ages 15 and 21. The point estimates for the assignment age range between 10 to 14 are smaller and less precise. Overall, these findings indicate that the age at which boys are exposed to neighborhood delinquency matters for their later behavior.

IV.C. Neighborhood Crime and Longer Term Outcomes

The final question we address is the effect of assignment to a high crime area on longer term outcomes, like employment, or educational achievement.[34] Given the age structure of the assignees in our data and the period of the policy, this assessment is at this time only possible for a subset of our sample, and at a relatively young age. We are able to follow approximately one-third of our sample up to age 25 and monitor whether they have completed an upper-secondary or tertiary education, have joined the labor market, or are inactive. In Table 10, we investigate three outcomes: Whether the individual has obtained an upper-secondary or tertiary education (panel A), whether the individual is active (i.e. enrolled in education or employed) (panel B), and – considering only those who are not in education – whether the individual is employed (panel C). As this sample

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[34] In an early paper, Grogger (1995) estimates the effects of arrests on employment and earnings in a non-experimental setting, and concludes that effects are moderate and short-lived.

is only a subset of the sample we used before, we also report in the first row of each panel the effect of the youth crime conviction rate in the assignment area on the conviction probability at age 15-21. In the first two columns, we report regressions when individuals are 25 years old. In the next two columns, we pool observations for each individual for ages 23-25. For this specification, we allow for individual random effects. As before, we report results with, and without neighborhood fixed effects.

The effects of the youth violent crime conviction rate on conviction probabilities are similar (and possibly slightly larger) than those we report for the overall sample, as the estimates in the first row of each panel show. The youth violent crime conviction rate in the assignment area has no effect on the probability to have obtained an upper-secondary or tertiary education by age 23-25 (panel A). However, estimates point at those assigned to areas with higher violent youth crime conviction rates being less likely to be active (i.e. enrolled in education, or working) (panel B), and – among those who are not in education – less likely to be employed (panel C).[35] Although most of the coefficients in panels B and C are not precisely estimated, they are all similar in magnitude. Taking the estimates at face value, results in panel B suggests that an increase in the area crime conviction rate by one standard deviation decreases the probability to be active in the age range 23-25 by 1.8 percentage points (or 2.5 percent, given that 70% of this subsample is active). Thus, exposure to a high share of criminals living in one's neighborhood, in particular in the early teenage years, may have important consequences for other long-term outcomes of young men, either through their own criminal behavior, their contact

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[35] One reason for the employment effects may be that individuals who are convicted for a crime between age 15 and 21 are still incarcerated. This is unlikely to affect employment rates after age 23 (for convictions received before age 22), however, as prison sentences administered to individuals in that age range are very short in Denmark: Based on data from Statistics Denmark (2005, p. 79), we calculated the average length of unsuspended prison sentences for this age range to be 5 months.

with delinquent youth in general, or other mechanisms. This possibility raises interesting avenues for future research.

V. CONCLUSION

To answer the question of whether exposure to neighborhood crime during childhood affects later criminal behavior, this paper draws on an exceptional spatial allocation experiment with refugee families in Denmark, whose quasi-randomness offers a unique solution to the fundamental methodological problem of endogenous neighborhood selection. We find strong evidence that the share of convicted criminals in the area at assignment affects later crime conviction probabilities, as well as the number of crimes for which a young man is convicted. We find no such effects, however, for females. It is the share of offenders convicted for crimes committed in the neighborhood that affects later crime conviction rates, in particular conviction rates for violent crimes among those in the under-26 age group. We detect no effects of other crime measures such as the rate of reported crimes. This emphasizes that it is the share of criminals living in the area, and not the rate of committed crimes, that affects later criminal engagement, which speaks in favor of social interaction as a key factor linking neighborhood crime with later criminal behavior.

Our findings provide additional evidence in support of this hypothesis: First, it is the crime conviction rate of youth that affect criminal behavior later in life. Crime conviction rates of older individuals have no effect. Second, the crime conviction rates of individuals from the same ethnic group, with whom contact and interaction is likely to be easier and more frequent, matter more for individual convictions. Third, the age range in

which assignees are most susceptible to neighborhood crime is between 10 and 14, an age at which young men are particularly vulnerable to delinquent peer influence. All this suggests that social interaction between children assigned to different neighborhoods and convicted criminals who live in these neighborhoods is a principle channel through which criminal behavior is transmitted. This finding has important implications for crime-related policies not only because it emphasizes crime prevention as a prime measure to avoid further contagion through social interaction-related feedback mechanisms but also because it suggests the existence of a social multiplier (see Glaeser, Sacerdote, and Scheinkman 2003).

We further find some evidence that youth crime conviction rates in the assignment area at assignment reduce the probability to be active in the labor market or in education by age 25. Unfortunately, the young age of our sample does not allow us to explore more conclusively additional and longer-term outcomes at present.

We should finally note that the findings we present in this paper refer to a group of young men whose crime rates are higher than those of the overall population. Nevertheless, we believe not only that understanding criminal behavior and its sources matters most in groups like those studied here or investigated in the MTO experiments, but that the basic mechanisms that link neighborhood crime to criminal behavior, explored in this paper, are likely to be relevant for other population groups as well.

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