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CHILDHOOD EXPOSURE TO SEGREGATION AND LONG-RUN CRIMINAL INVOLVEMENT

by

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Childhood Exposure to Segregation and Long-Run Criminal Involvement

Evidence from the "Whole of Sweden" Strategy[#]

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ABSTRACT

Crime is disproportionately concentrated among minority youths living in highly segregated neighborhoods. In this paper we present quasi-experimental evidence on how exposure to immigrant residential segregation during childhood affects male youths' criminal behavior. Our analysis exploits the "Whole of Sweden Strategy" in which all newly arrived refugees and their children during the years 1985–1994 were assigned to their initial neighborhood. We follow the children for a period up to 19 years using administrative records. We find evidence that exposure to a neighborhood with a large share of immigrants increases the probability of committing drug related crimes as well as raises the likelihood of being incarcerated later in life. Our results indicate that a one standard deviation decrease in segregation would shrink the immigrant-to-native crime gap for these specific types of offenses by about 20 percent. The impacts are mainly concentrated among youths with low educated parents. Our findings further lend some support to the idea that the relationship between segregation and crime may arise because of a weakened labor force attachment among individuals who grew up in segregated areas.

Keywords: Neighborhood effects, Criminal behavior, Residential segregation

JEL: J10, K42

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1. INTRODUCTION

Criminal offending is disproportionately concentrated among immigrant and minority males living in highly segregated neighborhoods (Glaeser, Sacerdote, and Scheinkman, 1996). Our own data, for instance, reveal that youths living in the most immigrant segregated areas in Sweden are five times as likely to be convicted as youths in the least segregated areas. Overall, immigrants are 80 percent more likely to be convicted for a crime relative to natives. Understanding whether these differences in criminal behavior are causally related to the type of neighborhood youths grow up in is fundamental when considering policies to improve equality of life chances.

In recent years, the economic literature has made progress in assessing the net effect of overall neighborhood attributes on criminal behavior (e.g. Ludwig, Duncan, and Hirschfeld 2001; Kling, Ludwig, and Katz, 2005).[1] There is still, however, limited knowledge of whether exposure to immigrant residential segregation causes criminal behavior or not. In this paper, we present quasi-experimental evidence on how exposure to segregation during childhood affects male youths' criminal behavior. In the absence of an experimental research design, identifying the effect of neighborhood segregation is challenging since youths that grow up in the same neighborhood not only are exposed to multiple attributes at the same time but are also self-selected by their parents into these areas.

[1] Ludwig, Duncan, and Hirschfeld (2001), study the effects of relocating families under the Moving to Opportunity (MTO) program from low- to high quality neighborhoods on juvenile crime. Their findings suggest that providing families with the opportunity to move to less disadvantaged neighborhoods reduces violent criminal behavior among youths. Analyses of the MTO program by Kling, Ludwig, and Katz (2005), indicates that moving to a less disadvantaged area has little effect on adult arrests, but reduces both violent and property crime arrests for female youths, and lowers violent crime arrests, while raising property crime arrest for male youths. Case and Katz (1991), Glaeser and Sacerdote (1999), and Glaeser, Sacerdote and Scheinkman (1996) represent other seminal contributions on the link between neighborhoods and crime. Past Swedish studies on the importance of general neighborhood effects on crime include Hederos Eriksson, Hjalmarsson, Lindquist and Sandberg (2013) and Sariasalan et al. (2013). The former study shows that family attributes is a much stronger predictor of criminal participation than neighborhood effects in the overall Swedish population.

We exploit a quasi-experiment that occurred in Sweden during the years 1985 to 1994 called the "Whole of Sweden Strategy".[2] The experiment implied that newly arrived refugee immigrants and their children could not choose themselves where to reside, instead the government assigned refugees to their initial location in a way we argue generates plausibly exogenous variation in the initial residential distribution. We take advantage of this policy to estimate the effect of childhood exposure to segregation on long-run criminal participation. A recent paper by Damm and Dustmann (2013) use a similar research design to analyze the causal effect of municipality level crime rates on criminal behavior in Denmark. They find that being assigned to a municipality with a large share of young people convicted for crimes increases future convictions of male refugee assignees. The paper provides perhaps the most compelling evidence to date about the importance of peer effects in criminal behavior.

Our analysis is made possible by access to rich administrative data. We draw on population wide data for the period 1985–2008, containing information on all convictions in criminal trials. These data have been linked to a broad set of standard individual characteristics taken from the income, educational and demographic registers. The detailed data, not only enable us to measure exposure to segregation during childhood, but also to follow the children as they get older, observing their encounters with the criminal justice system both before and after the peak of the age-crime profile. We study male youths, who were placed together with their parents in different localities at the ages 7–14, and follow them until they turn age 26, i.e. between 12 to 19 years after assignment. While other studies, due to data limitations, have been forced to study neighborhood effects at the municipality or even at the city level, our data include information on smaller geographical units providing us with detailed measures of segregation at the localized community level. Presumably, these

[2] The experiment has been used in several previous studies to investigate neighborhood effects among refugees (see e.g. Edin, Fredriksson and Åslund 2003; Åslund and Fredriksson 2009; Åslund et al. 2011; Grönqvist, Johansson and Niknami 2012.)

measures more accurately capture individuals' actual exposure to segregation, especially for children who are likely to spend most of their time close to their home.

We find evidence that being assigned to a neighborhood with a large share of immigrants increases male youths´ probabilities of committing drug related crimes and also raises the likelihood of being sentenced to imprisonment or youth custody. A one (within municipality-by-year) standard deviation increase in neighborhood segregation increases the probability of committing these types of crimes by between 11 to 13 percent. This corresponds to about one-fifth of the gap in crime between immigrants and natives for these types of offenses. We do not find significant effects for other types of crimes, such as violent and property crimes. The impacts are concentrated among youths with low educated parents.

The existing economic literature suggests at least three main channels through which segregation might affect criminal participation. Glaeser, Sacerdote and Scheinkman (1996) argue that the lack of positive role models and peer influences leads to crime in segregated areas. Segregation may also influence crime through a potential effect on education (e.g. Billing, Deming and Rockoff, 2014). Verdier and Zenou (2004) further suggest that segregation may increase crime by isolating immigrants away from jobs, thereby increasing the payoff to criminal activity relative to labor market activity. Our own analysis suggests some evidence in favor of the latter mechanism. For instance, the results show that a one (within municipality-by-year) standard deviation increase in childhood exposure to segregation increases the future probability of being unemployed or not enrolled in education by about 6 percent.

Our paper contributes to several branches of the literature. It is most closely related to the literature documenting the correlations between immigrant residential segregation and crime (e.g. Bell and Machin 2012; MacDonald, Hipp and Gill 2012; Stansfield 2014). This line of research seems to have been motivated out of concerns in many countries about the problems arising from failed integration linked to increasing levels of segregation among immigrants, which is commonly believed to have adverse consequences for immigrants and the host society as a whole. To the very best of our knowledge, Bell, Fasani and Machin (2013) is the only study presenting design based evidence on the relationship. They examine the contemporary link between immigrant concentration and reported regional crime rates using instruments based on past settlement coupled with a spatial dispersion policy that generated exogenous variation in the inflow of asylum seekers to different localities within England and Wales. They find that a larger inflow in the share of asylum seekers leads to a modest but significant increase in reported property crime rates, while a larger share of labor force migrants in fact reduces reported property crime rates. The authors argue that the findings are consistent with the notion that differences in labor market opportunities of different migrant groups shape its potential impact on crime.

Our paper is also related to studies that examine the relationship between *racial* segregation and crime using various sources of plausibly exogenous variation in segregation. Ludwig and Kling (2007) use the variation across MTO sites and groups to distinguish the effect of racial composition from that of neighborhood poverty and crime. Their results suggest that city-level racial composition is positively associated with crime and that the relationship is driven by drug market activity. Our paper also adds to recent studies investigating the link between school based racial segregation and crime (Billings, Deming and Rockoff 2014; Weiner, Lutz and Ludwig 2009). These studies find that increased school segregation is causally linked to higher crime rates.

Unlike previous work, we ask whether exposure to immigrant residential segregation during childhood is related to long-run criminal participation. Childhood and adolescence represent critical periods in life when many potentially life lasting investments are made: human capital accumulation is still in its formative stages and it is the period when most offenders make their criminal debut. This means that exposure to segregation during this period in life may have persisting consequences for criminal behavior. Following recent local outbursts of criminal riots in many Western countries, the question of whether minority residential segregation breads criminal activity has become a central topic in the public debate. We believe that answers to this question using a compelling research design therefore is especially urgent.

We also present evidence on the effects of long-term exposure to segregation during childhood. Since parents might respond to changes in the level of segregation simply by moving to another residential area, cross-sectional type of analyses risk to mask the effect of growing up in a segregated area. Our analysis is possible since our data contain information on the neighborhood of residence each year. We measure long-term exposure to segregation as the average exposure to segregation between age 7 and 14. Our research strategy is based on instrumenting for long-run exposure using segregation in the assigned residential area. The results suggest that growing up in a segregated area is strongly linked to future criminal activity. We are aware of no other study that has been able to address this potentially important question.

The rest of the paper is structured as follows. Section 2 explains the institutional background of the placement policy as well as the Swedish criminal justice system. Section 3 discusses the data and the empirical strategy. Section 4 presents the results from our empirical analysis and Section 5 concludes.

2. INSTITUTIONAL BACKGROUND

In this section we discuss institutional facts surrounding the “Whole of Sweden Strategy”. We also briefly describe crime in Sweden.

2.1 MIGRATION TO SWEDEN AND THE "WHOLE OF SWEDEN STRATEGY"[3]

Sweden has a rather large share of immigrants. In the early 1990s about 10 percent of its 9 million residents were foreign-born. Since the 1970s, labor migrants have been gradually replaced by refugees and family reunification migrants. Over the past decades, the relative economic performance of the immigrants has been trending downwards. Today, Sweden is one of the countries with the largest immigrant-native differentials in the labor market (OECD 2010).

The immigrant population is concentrated to certain regions and neighborhoods. The greater urban areas of Stockholm, Gothenburg and Malmö host about one third of the overall population but as much as half of the foreign-born. As a way of decreasing immigrant segregation the Swedish government implemented in 1985 a policy called "the Whole of Sweden Strategy" to assign newly arrived asylum seekers to an initial municipality of residence. Family reunification immigrants were exempted from the policy.

The assignment process can be summarized in the following way. Upon arrival, asylum seekers were placed in refugee centers, while waiting for the Immigration Board's ruling on whether or not to grant a residence permit. The refugee centers were distributed all over Sweden and there was no link between the port of entry to Sweden and the location of the center. On average, asylum seekers stayed in the centers between three and twelve months. After being granted refugee status, the placement officers at the Immigration Board immediately assigned refugees to their initial location. A family was in this process treated as a single unit. The original idea was to place people in locations with good opportunities for work and education. However, since the housing market was booming during this period it became very difficult to find housing. The placement officers therefore placed refugees in municipalities with available housing.

[3] This section draws heavily on Åslund et al. (2011).

There were no face-to-face meetings between placement officers and refugees. The only information on the refugee that was available to the officer was age, education, gender, marital status, family size and country of origin. The officer may have used this information when deciding where to allocate refugees. However, since the administrative registers contain the same set of information we are able to control for potential selection of these observables. Once settled in the municipality of assignment, refugees were allowed to move if they found housing in another location, but they were still required to take part in an 18-month introduction program in their assigned municipality in order to qualify for social assistance during the introduction period. Eight years after arrival, about 50 percent were still living in their assigned municipality.[4]

Refugees were allowed to state their preferred municipality, but interviews with placement officers confirm that few, in practice, did and that preferences were given little weight (e.g. Edin, Fredriksson and Åslund 2003). There are several reasons for why preferences received little weight. Among refugees who stated preferences, most applied for residence in the three largest urban areas in Sweden: Stockholm, Gothenburg and Malmo. The main goal of the policy was however to reduce the inflow of immigrants to these areas. Importantly, there were few housing vacancies in these locations because the housing market boom (see e.g. Fredriksson and Åslund 2009). As placement occurred rapidly after having received the residence permit, the joint probability of receiving a permit at the same time as a housing vacancy in the preferred location opened was extremely low.

These arguments have been central for the research design in previous studies that have examined the impact of ethnic concentration and local labor market conditions on adult refugees' labor market outcomes (see e.g. Edin, Fredriksson and Åslund 2003; Åslund and

[4] Males and younger individuals were more likely to move. In general, those who moved tended to go to larger urban areas.

Rooth 2007; Grönqvist, Johansson and Niknami 2013). As will be clear, the identification strategy used in the present paper relaxes these assumptions.[5]

Note that the policy can also be used at the aggregate level. Dahlberg, Edmark and Lundqvist (2012), for instance, examine whether the *inflow* of refugees affect voters preferences for redistribution. In their study the subject of interest is not the outcomes of the individual refugee assignee, but on how the individuals already residing in the municipality reacts to changes in the aggregate inflow of refugees. Using the placement policy at the aggregate level is however complicated since, as already explained, it is important to adjust for individual characteristics that may govern the way placement officers matched individuals with localities.[6] This is also the essence of the critique of the policy by Nekby and Pettersson-Lidbom (2012) who argue that the aggregated inflow of refugees may be correlated with unobserved municipality trends. This issue is however not a concern in this study since we are able to adjust for all individual characteristics that governed the assignment process and also net out any trends by controlling for municipality-by-year fixed effects. Another benefit with conditioning on municipality-by-year fixed effects is that we are able to relax the identifying assumption made in previous studies of the policy that individual preferences for specific municipalities could not be realized.[7] As we have discussed, due of the design of the policy it is very unlikely that preferences were given weight in the assignment process. However, since we cannot completely rule out this possibility we choose a strategy that allows us to identify

[5] The present paper differs compared to past studies in the sense that it is the first to study the effect on criminal behavior. With the exception of Åslund et al. (2001), who examine how peers influences school performance, this is also the first paper to consider the effects on the children who were placed.

[6] For instance, Edin, Fredriksson and Aslund (2003) show that placement officers tried to send high educated individuals to more affluent neighborhoods.

[7] Although not providing any formal evidence, Nekby and Pettersson-Lidbom (2012) speculate that residential preferences in fact may have been given weight in the assignment process. Past studies have however provided substantial evidence that the policy actually created a geographic distribution that was independent of unobserved individual characteristics. For instance, Edin, Fredriksson and Åslund (2003) show that the residential area of those placed clearly differed from the location choices made by immigrants arriving from the same regions shortly before the reform. This can for instance be seen in Figure A1. If placement officers actually acted on residential preferences we would not observe these large changes in residential pattern.

the effect of interest by comparing individuals living in the same municipality; i.e. by contrasting the outcomes between individuals who had the same residential preferences (because they are living in the same municipality) at any given point in time. This approach also absorbs any potential changes in preferences that may occur over the time period the policy was in place.

As explained in past studies, it is difficult to test for random assignment as it requires information on some individual characteristic in the year of assignment that is observed by us and not by the placement officer (or at least unexploited). Instead, we provide results which illustrate the differences in how well individual characteristics predict properties of the local area in the year of arrival and then at age 26. During this period, individuals will have had time to change residential area. Consequently, one would expect the link between individual and neighborhood characteristics to grow stronger over time.

Tables A.1 and A.2 in the Appendix present estimates from regressions where the dependent variable is some feature of the neighborhood measured in the year of arrival and then at age 26. Since we are testing multiple hypotheses conventional critical values increases the probability of observing at least one significant result just due to chance. This may lead us to erroneously conclude that individual characteristics predict neighborhood properties. We therefore use Bonferroni-type corrections to the critical p-value.[8] Although placement officers may have taken these individual characteristics into consideration in the allocation process, we do not find any significant estimates when relating individual attributes to initial neighborhood conditions. However, when repeating this exercise for neighborhood characteristics measured at age 26 we find 30 significant estimates out of the 84 tests

[8] It is well-known that the Bonferroni correction is too conservative in the sense that while it reduce the number of false positives, it also reduce the number of true discoveries. This is however not a major concern in our context since our interest is mainly on contrasting the relative number of significant estimates in the regressions using initial versus subsequent neighborhood characteristics as dependent variables. A reduction in the number of true discoveries will influence both outcomes similarly and therefore not the relative number of significant estimates.

performed. This indicates that over time individuals tend to increasingly sort across municipalities based on their individual characteristics, which highlights the importance of accounting for selection bias when trying to uncover the causal effect of segregation on crime. The results from this exercise are also consistent with the view that placement officers likely did not act on unobserved individual characteristics, which should come as no surprise since they had access to the same information about the families as we are using. This suggests that the policy therefore can be used as a source of plausibly exogenous variation in segregation.

2.2 CRIME IN SWEDEN

Although the Swedish murder rate is substantially lower than in most Western countries, other crimes are more frequent. In the year 2006, the number of burglaries reported to the police per 100,000 persons was 1,094. The same numbers in the US and England was 714 and 1,157 (Harrendorf, Heiskanen and Malby, 2010). While the Swedish assault rate per 100,000 persons in 2006 amounted to 845, the equivalent figure for the US was 787. These figures, of course, partly reflect differences in the propensity to report crime, differences in crime severity, as well as inconsistencies in crime definition, and should be interpreted with caution. There is also a significant gap between immigrants' and natives' risk of getting involved with crime. For instance, our own estimations reveal that the overall immigrant-native crime gap is two thirds the size of the gender gap in crime and about half the size of the gap between individuals with high school diploma versus having completed compulsory school. On average, immigrants are 80 percent more likely than natives to be convicted for a crime, even after adjusting for standard individual background characteristics such as age, education and income.

The minimum age of criminal liability is 15 in Sweden. All individuals above this age are treated in the same judicial system. Some special rules do however apply for juveniles.

For instance, cases involving youths should be dealt with promptly and it is rare that individuals below age 18 are sentenced to prison. Instead of prison, juveniles are usually sentenced to youth custody in special facilities. As in most other countries, youths are overrepresented in the crime statistics. Independently of type of crime, crime peaks in the early twenties, and then falls. In Figure 1 we show the age-crime profile in our data for all convictions. This highlights the need to be able to follow the individuals for an extended period in order not to understate the potential social costs of segregation on crime.

3. DATA AND EMPIRICAL STRATEGY

3.1 DATA AND SAMPLE SELECTION

Our data originate from several administrative registers collected and maintained by Statistics Sweden. The registers contain information on the entire Swedish population aged 15 and older each year from 1985 to 2008. There is annual information on a wide range of educational and demographic characteristics as well as different income sources. The data include an exact link between children and their parents. There is hence information both on child and parental characteristics.

Information on individual crime for the same period was provided by the National Council for Crime Prevention (BRÅ). The data include information on type of offense, date of offense, date of conviction as well as sentence ruled by the court. Speeding tickets and other minor offenses are not included in the data. For all individuals in our data, we thus have a full record of their criminal convictions for a period up to 22 years. The crime categories we use in our analysis are: (i) violent crimes (e.g. homicide and assault); (ii) property crimes (e.g. thefts, burglary, motor-vehicle theft); (iii) drug offenses; (iv) crimes that have resulted in prison sentences or youth custody. The latter category is intended to capture more serious type of crimes.

Table A.3 in Appendix outlines the way we have constructed these variables. Since criminal behavior is inferred from conviction data, it provides an objective measure of criminal involvement that minimizes problems of misreporting and measurement error. It should, however, be noted that if the likelihood of being convicted for a crime, conditional on actually having committed it, is correlated with segregation, the data may result in a biased estimate of an individual's criminal behavior. As will be explained later, however, the specific sample we use, coupled with our research design, presumably accounts for this potential source of endogeneity and is therefore not likely to prejudice our results.

Our population of interest consists of children aged 7–14 whose parents arrived from a refugee sending country between 1985–1994**.** In Sweden, children typically start school at the age of 7. Starting school is likely to increase children's mobility in the local area, and thus implies higher exposure to neighborhood conditions.[9] The reason for the upper age restriction is that children arrived in different years and we want to observe criminal convictions of these youths using an identical span of the age-crime profile (between age 15 and 26).[10]

Refugee sending countries with only few observations have been aggregated by Statistics Sweden due to confidentiality rules. Table A.4 in Appendix lists the included source countries along with information about the number of observations from each country. We exclude children with parents that have a spouse, child or parent already living in Sweden at the time of immigration as family reunification immigrants were exempted from the placement policy. In our main analysis we focus on male youths, which constitute 12,148 individuals. Background characteristics for the sample are reported in Table A.5 in the Appendix.

Our data include geographical identifiers at four different levels: SAMS (Small Area Market Statistics), parish, municipality and county. There are about 9,000 SAMS areas, 2512

[9] Lowering the age restriction further would imply that we are not able to observe all individuals after the peak of the age crime profile.
[10] Since 15 is the age of criminal majority in Sweden no individuals below this age can be convicted.

parishes, 290 municipalities, and 21 counties in Sweden. We primarily measure segregation at the SAMS level. A SAMS area is a geographically localized community within the municipality and corresponds closely to the concept of a neighborhood. The average SAMS hosts slightly more than 1,000 individuals. Since individuals do not enter the data before age 15, we use the assignment location of the parent(s) who arrived together with the child to get information on their first SAMS area.

Several measures can be used to capture the degree of immigrant segregation within a neighborhood. In this paper we follow Bell, Fasani and Machin (2013) and measure segregation simply as the share of refugee immigrants living in the SAMS area.[11] [12] As a robustness check we also compute the dissimilarity and the isolation index used by e.g. Cutler and Glaeser (1998), and Cutler, Glaeser and Vigdor (1999). Figure 2 shows the distribution of segregation across SAMS in our sample. We can see that most individuals were placed in areas with low immigrant shares, but that there is quite a lot of variation in this measure.

Table A.5 shows unadjusted summary statistics for neighborhood characteristics measured in the year of arrival and at age 26. We can clearly see that sorting seems to take place over time. Relative to neighborhood characteristics in the year of arrival, at age 26 assignees are living in areas with larger immigrant shares, larger populations, and higher poverty rates. This finding is consistent with the nature of the placement policy since we would not expect to find such pattern in the data if individuals were allowed to freely choose their initial residential area.

3.2 EMPIRICAL STRATEGY

Analyzing whether exposure to neighborhood segregation during childhood affects later criminal behavior is challenging. Although children have a limited role in choosing their

[11] Ludwig and Kling (2007) measure segregation using the share of Blacks in the population.
[12] The results are virtually identical when measuring segregation using the share of non-Nordic immigrants.

residential locations, neighborhood segregation is likely correlated with parental factors that directly influence the children's propensity to engage in crime. Identifying the causal effect of neighborhood segregation on crime, however, requires residential location to be uncorrelated with parental characteristics. Our strategy is to circumvent this methodological problem by taking advantage of the "Whole of Sweden Strategy", in which newly arrived refugee children and their parents were assigned to their first locality by government authorities. We run OLS regressions of the following form by type of crime

$$C_{ismt} = \beta_0 + \beta_1 Seg^{ass}_{\mathrm{smt}} + \beta_2 X_i + \beta_3 Z_{smt} + \theta_{mt} + \varepsilon_{ismt} \quad (1)$$

where the outcome variable C_{ismt} indicates whether individual i (aged 7–14 when arriving), assigned to neighborhood s (SAMS), in municipality m, at time t, has been convicted for at least one crime up to age 26. The key variable, Seg^{ass}_{smt}, is the (log) share of refugee immigrants in the assigned neighborhood in the year of arrival. To account for pre-assignment characteristics, we include a vector X_i that contains individual and family characteristics (i.e. age at immigration, parental age at immigration, parental educational attainment, number of siblings, and parental marital status) measured in the year of arrival.[13] We also include indicators for country of origin. This controls for the possibility that crime maybe more pronounced in different ethnic groups or that the police may target some groups more than others. These were the only attributes known to the placement officers upon assignment. The quasi-experimental research design therefore ensures that initial segregation is not correlated with unobserved individual characteristics after controlling for X_i.

Initial segregation may however still be correlated with other neighborhood characteristics. We therefore include a vector, Z_{smt}, controlling for (the log of) time-varying

[13] Educational attainment of parents arriving before 1990 is measured in 1990.

SAMS characteristics, such as population size, share of youths aged 15–26, share of university educated aged 30–64, share of the population that is in poverty[14], share of convicted youths[15] aged 15–26 and a dummy indicating whether there is a police station in the SAMS or not. By controlling for whether there is a police station in the SAMS, we wish to capture neighborhood police effort. If the police choose to target their activities mainly to segregated areas then there is a risk that our estimates not only reflect the effect of segregation on crime but also that of police effort.[16] Note however that since we measure exposure to segregation during childhood and observe criminal participation in adulthood, this is not likely to be a problem of first-order importance. Our data further allow us to include municipality-by-year fixed effects θ_{mt} to control for time varying municipality characteristics that may be correlated with neighborhood segregation. Our identification strategy thus is to compare children who were placed in different neighborhoods within the same municipality and year. Municipality-by-year fixed effects also absorb changes in police surveillance at the municipality level. Indeed, the municipality represents the lowest administrative level governing law enforcement effort and activities of the police and social authorities is primarily governed at the municipal level.

It is relevant to ask how to interpret the main parameter of interest. For our purposes, it is convenient to think of crime as being a function of segregation, σ, other neighborhood characteristics, ρ, and family background τ, i.e. $Crime = \mathrm{f}(\sigma, \rho, \tau)$. The economic literature suggests three main channels through which segregation might affect criminal participation. Glaeser, Sacerdote and Scheinkman (1996) argue that the lack of positive role models in

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[14] Measured as the share in the SAMS with family earnings adjusted for household size below 50 percent of the national median.
[15] Damm and Dustmann (2013) in fact find that it is the number of criminals rather than the number of crimes that matters for criminal behavior
[16] The direction of this potential source of bias is ambiguous. Increased police effort may lead to more criminals being apprehended, which would imply that our estimator is upward biased. It is also possible that more policemen on the streets deter individuals from engaging in crime. which would lead to a downward biased estimator.

segregated areas may lead to crime. Furthermore, segregation may be related to crime via education. For example, a large literature has shown that education is causally linked to criminal behavior (e.g. Meghir, Palme and Schnabel 2013, Hjalmarsson, Holmlund and Lindquist forthcoming, Lochner and Moretti 2004, Jacob and Lefgren 2003) and other studies have shown that segregation directly affects individual investments in education (Billing, Deming and Rockoff, 2014). Last, segregation may increase crime by keeping immigrants away from jobs, increasing the payoff to criminal activity relative to labor market activity (Verdier and Zenou 2004).

Alternative mechanisms have also been suggested. Segregation could alter perceptions of opportunities for minorities as well as self-esteem (Weiner, Lutz and Ludwig 2009). It is also possible that interacting mainly with other immigrants impairs language acquisition (e.g. Lazear 1999), which may hurt ones legal income prospects which, according to the Beckerian model of crime, will lower the opportunity costs of criminal behavior. Peterson, Krivo and Browning (2005) also propose that disadvantaged, segregated neighborhoods lack strong social control thereby encouraging criminogenic adaption.

While theoretically important, the distinction between these different mechanisms is a very complex task. In our main analysis we make no distinction between different channels, although we follow the convention in the literature and estimate minority exposure effects holding constant a set of standard area characteristics, such as population size, local poverty, unemployment or the share of criminals (e.g. Cutler and Glaeser 1997; Card and Rothstein 2006; Bell, Fasani and Machin 2013). Put differently, we will identify the effect of segregation net of other neighborhood characteristics, i.e. $\partial f(\sigma,\rho,\tau)/\partial\sigma$. One concern though is that controlling for potential neighborhood confounders may also partially absorb some of the mechanisms through which segregation may influence crime. Because of this we also estimate a bare bones model with a limited set of controls to which we successively add key neighborhood covariates. It turns out that the estimates from the bare bones model are similar to the one when expanding the set of controls.

4. EMPIRICAL ANALYSIS

This section provides the results from our empirical analysis of the effect of segregation on crime. We start by showing the main results and continue by probing the robustness of our estimates. After this we provide some additional analyses as well as an exercise where we try to unpack some of the potential mechanisms. We end with an examination of the consequences of long-term exposure to segregation during childhood for crime in adulthood. Throughout, estimates are reported for overall crime, violent crime, property crimes, drugs and imprisonment. Our baseline specification, given by equation (1), relates the probability of being convicted at least once between age 15 and 26 to segregation in the assigned neighborhood. We cluster the standard errors at the SAMS level.

4.1 MAIN RESULTS

Table 1 presents our main results. Numbers in brackets provide the mean of the dependent variable. It is clear from these statistics that young male refugee immigrants are highly overrepresented among criminal offenders.[17] For instance, when comparing these figures to the crime committed among young native males over the same age span we find that refugee immigrant males are more than twice as likely to be sentenced to prison. As already explained, our preferred specification follows the convention in the literature to include a number of standard local area controls. It is however relevant to ask how the estimates change when varying the set of covariates. We therefore first present results from a model with a restricted set of regional controls (population size, the share of male youths and whether there

[17] The overall conviction rate among native males over age 15-26 is just above 25 percent.

is a police station in the neighborhood). To this model we then add key neighborhood covariates separately.

The results from our bare bones model show significant estimates for drug related crime and imprisonment. We can see that the estimates do not change in any meaningful way compared to the restricted specification when enlarging the set of neighborhood controls. Adding the share of convicted criminals in the neighborhood produces slightly larger effect sizes for these types of crimes, but none of the changes are significantly different. Finally, adding all variables to the restricted model simultaneously does not lead to any significant changes in the coefficients. This suggests that these neighborhood attributes do not represent key channels through which segregation might matter for crime. We will return to investigating other potential mechanisms later in the paper.

The coefficients on the last row represent our preferred baseline specification. We can see that there is no statistically significant effect of segregation on the probability of committing overall crime, violent crime or property crime. The point estimates are also small in magnitude. For instance, a one (within municipality-by-year) standard deviation increase in neighborhood segregation (.72) increases the probability of violent crime by .7 percentage points (.001×.72×100). We do find, however, that being assigned to an area with a large share of immigrants increases the probability of committing drug related types of crimes. The estimate suggests that a one standard deviation increase in neighborhood segregation increases the probability of being convicted for drug crimes by .9 percentage points (.013×.72×100), which corresponds to a 10 percent increase compared to sample mean. Another way to interpret the coefficient is that doubling[18] the share of immigrants in the neighborhood increases the number of drug crimes by 1.3 percentage points. Put differently, this increase in segregation would lead to 1,300 more drug criminals per 100,000 persons.

[18] Doubling the share of immigrants imply a 170 percent increase in the share of immigrants since exp(1)-exp(0)≈1.7.

This finding is consistent with the results in Ludwig and Kling (2007) who show that segregation primarily affects crime through its influence on drug market activity. We also see that a standard deviation increase in neighborhood segregation raises the probability of being incarcerated by 1 percentage point (.014×.72), or 13 percent off the sample mean.

To assess the magnitude of these estimates, it is convenient to imagine how crime would change if moving an immigrant youth from the 10^{th} to the 90^{th} percentile of the segregation distribution. In our data, the 10^{th} percentile neighborhood has 1,813 immigrants per 100,000 individuals (-4.01 log points). The corresponding number for the 90^{th} percentile is 30,727 (-1.18 log points). Such residential change would thus raise the probability of being sentenced for drug crimes by 3.7 percentage points ((2.83×.013)×100) and increase crimes leading to prison sentences by 4 percentage points ((2.83×.014)×100). In relation to the sample mean, these numbers translate into a 43 percent increase in drug crimes and a 50 percent increase in prison.

It is also helpful to compare the estimates with the crime gap between immigrants and natives. Our own data show that native male youths of the same birth cohorts as our sample have a conviction rate of 4 percent for drug related crimes. Thus immigrant male youths are 118 percent more likely to be convicted for a drug type of crime relative to male youths born in Sweden. A one standard deviation reduction in segregation would therefore eliminate about 20 percent of the drug related crime gap between immigrants and natives ((.013×.72)/.047). The native incarcerations rate is 2.4 percent, this means that Immigrant youths are 233 percent more likely to be convicted for a more serious type of crime that results in the individual being sentenced to prison. A one standard deviation reduction in segregation would reduce the gap in serious crimes by 18 percent ((.014×.72)/.056).

Although the point estimates for overall crime, violent crime and property crime are not statistically different from zero, they are precise enough for us to be able to rule out large effects. For instance, the upper bound of the 95 percent confidence interval implies that an increase of one standard deviation in segregation at most constitute 9 percent[19] of the gap in violent crime between immigrant and native male youths (((.001+1.96×.007)×.72) /.102)×100).

In Table A.6 we report the results from various robustness checks. We start by attempting to reproduce the relationship between segregation and crime employing two alternative measures of segregation: the dissimilarity index and the isolation index (see Cutler, Glaeser and Vigdor 1999). The dissimilarity index[20] is high when immigrants disproportionately reside in some neighborhoods in the municipality compared to natives. The isolation index[21] instead attempts to measure the extent to which immigrants will encounter natives within their own neighborhood. It is clear that the pattern is similar to baseline when using the alternative measures of segregation. A one standard deviation increase in the dissimilarity index (1.23) increases the probability of committing drug crimes by 8.4 percent (((.006×1.23)/.087)×100) and crimes leading to prison sentences by about 11 percent (((.007× 1.34)/.080)×100). The effect is similar for the isolation index (the within municipality-by-year standard deviation is 1.34). The results are thus not sensitive to how we define segregation.[22]

Although the Swedish refugee placement policy was in place from 1985 to 1994 it was most strictly enforced up until 1991. In 1992 the number of asylum seekers increased rapidly because of the war in Bosnia-Herzegovina. It therefore became more challenging to place the new arrivers in accordance to the policy. This is of course only a problem to the extent that any potential bias is not picked up by our quite extensive set of covariates. To address the

[19] The calculation is based on numbers that are rounded to three decimals. If we instead calculate the upper limit of the 95 percent confidence interval with non-rounded figures, the value becomes even lower (10 percent).

[20] The dissimilarity index is computed in the following way: $\frac{1}{2}\left|\frac{Imm_s}{Imm_m} - \frac{Nat_s}{Nat_m}\right|$, where Imm_s is the number of refugee immigrants in the SAMS, Imm_m is the number of refugee immigrants in the municipality, Nat_s is the number of natives in the SAMS and Nat_m is the number of natives in the municipality.

[21] The isolation index is computed in the following way: $\frac{Imm_s}{Imm_m} \times \frac{Imm_s}{N_s}$, where N_s is the number of people in SAMS.

[22] The results are also robust to using the share of non-Nordic immigrants in the neighborhood. Results are available upon request.

concern that our results may be driven by non-random sorting of immigrants arriving in the post 1991 period, we re-estimate the baseline model for the years 1985-1991. This reduces the sample by more than one half. Still, the results, reported in Panel D, are almost identical to those in Panel A.

4.2 SUBGROUP ANALYSIS

We continue the analysis by looking at results for different subgroups of the population. The estimates in Table 2 are shown for our preferred specification (Panel A) and then stratified for different segments of the population (Panels B to E). We focus on groups defined by the highest completed level of education of the parents, family type, and age at arrival.

In Panel B we stratify the sample by parental education. Low educated parents may have fewer opportunities to compensate for the potential adverse effects of living in a disadvantaged area. Also, segregation may affect skilled and unskilled individuals differently depending on whether it implies more or less mixing of skill within the minority population (Cutler and Glaeser 1997). Low educated parents is defined as both parents having completed at most shorter (vocational track) high school, while high educated parents is when at least one of the parents have attained longer (academic track) high school education or more. Starting with high educated parents, we can see that the point estimates are all small in magnitude and not statistically different from zero. Being assigned to an area with a large share of immigrants, however, increases both the probability of committing drug crimes and imprisonment among male youth with low educated parents. The point estimate in column (4) suggests that if the share of immigrants doubles the probability of engaging in drug types of crimes increases by 2.5 percentage points among male youths of poorly educated parents. In relation to the mean of the dependent variable this translates into an increase of 23 percent.

The effect is even larger for crimes resulting in prison sentences for which the point estimate suggests an increase of 30 percent.

Panel C shows results by family type. We can see that the magnitude of the point estimates is large for both drug crimes and imprisonment, and this independently of family type. However, we only find a statistically significant effect among male youths of married parents as the point estimates for male youths of single parents are imprecisely estimated.

The immigrant children in our sample were assigned to neighborhoods at different ages. As the potential years of exposure to neighborhood segregation differs, we split the sample by arrival age using 11 as a cut of. Previous studies using within-family variation in age at arrival have shown that a strong negative effect on school performance appears about this age (e.g. Böhlmark 2008). The results in Panel D indicate that the effect of segregation on crime is stronger among male youths who were placed before the age of 11. This finding can be rationalized by these children having experienced exposure to segregation for a longer period. However, since length of exposure and age at first exposure are perfectly collinear it could also be the case that these children were exposed at an age where they are especially vulnerable to neighborhood influences. We cannot tell which of these mechanisms is more important.

4.3 EXTENSIONS OF THE ANALYSIS

This section extends the analysis by exploring some additional specifications. The results are presented in Table 3. As before, the estimates are reported for overall crime and then broken down by crime categories. Panel A gives the baseline results and the numbers in brackets provide the mean of the dependent variable.

We have so far focused exclusively on male youths. We start this section by looking at the association between segregation and crime for female youths. The outcome means, given in Panel B of Table 3 (in brackets), show, not surprisingly, that the incidence of crime is much lower among female immigrant youths than among male immigrant youths. For example, while the probability of being convicted for any type of crime is 14 percent for females it is 44 percent for males. The gender gap in crime is even larger when comparing specific types of crimes. Being assigned to an area with a large share of immigrants does not statistically significantly increase the probability of committing crime among female youths. Segregation, however, actually seems to *lower* the risk of criminal involvement for more serious types of crimes that may result in the perpetrator being sentenced to prison. Yet, since only 28 females in the sample were ever sentenced to prison, this estimate should be interpreted with caution. The fact that we do not find that segregation increases crime among females could be interpreted as that female risky behavior manifest itself in other ways than through acts of crime, for instance by engaging in risky sexual behavior.

Our choice of using a binary dependent variable is motivated because we are primarily interested in learning about whether segregation leads to the creation of new criminals among youths rather than increasing the number of crimes committed by already existing criminals. Employing a binary dependent variable also eases concerns that a few individuals who have committed extremely many crimes may receive disproportionate weight in the regressions.[23] In Panel C, however, we use the number of crimes as an outcome instead of a dichotomous variable. Starting with columns 1-3 the results indicate that initial segregation has a positive effect both on the number of property crimes and on overall crime. Since the point estimate for property crime is small and statistically insignificant in baseline, it suggests that segregation increases the number of property crimes committed primarily of those who would have committed crimes anyway, rather than raising the propensity to engage in property crime in the general population. There is, however, still no effect of segregation on violent crime.

[23] For instance, while the average number of crimes committed in the sample is just above 2, the top 1 percentile have committed more than 100 crimes.

The coefficients of drugs and prison are statistically significant. In relation to outcome mean, these corresponds to a 21 percent increase in the number of drug crimes and a 15 percent increase in the number of crimes resulting in prison, when segregation doubles. The effect sizes in columns 4 to 5 are thus roughly similar to those in our baseline specification.

Existing studies have examined the association between segregation and crime at larger geographic units compared to this study. For instance, Ludwig and Kling (2007) measure segregation at the city level rather than at the neighborhood level. To examine whether the relationship varies depending on the unit of analysis considered we re-estimated our baseline model at the parish level. It is again worth mentioning that there are about 9,000 SAMS and 2,512 parishes in Sweden. The estimates in Panel D are generally smaller than in Panel A. One exception is the estimate for prison which is slightly larger at the parish level. The point estimate for drug crimes is not statistically significant at the parish level. In contrast to most other types of crimes, buying and selling drugs require face-to-face interactions between at least two individuals. One interpretation of the results is therefore that interactions could explain this finding as the probability of knowing others in the area is greater in smaller geographical units. To further explore this interpretation, we split the sample by the size of the assigned neighborhood using the median as cut-off. The results confirm that the relationship between segregation and crime is stronger in small neighborhoods (see Panel E). An alternative interpretation of this result is that smaller areas may more accurately capture actual exposure to segregation. This may be especially true for young children who most likely to spend most of their time in the neighborhood.

As already explained, in our main specifications we control for municipality-by-year fixed effects. The main benefit of doing so is that we can relax the identifying assumptions and also control for potential changes in police effort. It may however not be relevant to study segregation by only comparing neighborhoods within a municipality. Panel F therefore replicates our baseline without the inclusion of municipality-by-year fixed effects. As can be seen, the point estimates become slightly smaller, albeit the difference is not statistically significant from baseline.

4.4 MECHANISMS

We have already tried to tease out some of the potential mechanisms through which segregation might be liked to crime. We now provide some additional results on this theme in Table 4. Panel B replicates our baseline estimates including controls for immigrant quality. As argued by Cutler and Glaeser (1997), if the average human capital of the immigrants in the neighborhood is poor, then presumably, there will be fewer positive role models. We measure immigrant "quality" as the share of immigrants in the assigned neighborhood that have completed university. The point estimates are similar to those in the baseline, suggesting that the quality of the immigrants in the assigned neighborhood does not drive the observed relationship between segregation and crime. To further develop the idea that segregation affects crime through role models we report results where we condition on the compulsory school grades among all the peers residing in the neighborhood. Since we do not have information on grades for the entire sample, this analysis is done for a subsample. To ensure that the subsample is comparable to our main sample, we start by replicating the baseline in Panel C and get similar estimates. In Panel D we then add controls for the average compulsory school GPA of the peers. The coefficients are again similar to those in the baseline.

In Panel A of Table 5 we address whether our results are mediated through school performance as suggested by e.g. Billing, Deming and Rockoff (2014). We have three different measures of school performance: compulsory school grade point average (GPA), high school GPA and high school diploma. We do not find any statistically significant effects of segregation on educational performance. The point estimates are further precise enough to rule out large negative effects[24], suggesting that education does not seem to be an important mechanism underlying the segregation-crime relationship.

We last consider whether segregation is linked to labor force attachment as argued by e.g. Verdier and Zenou (2004) by analyzing the relationship between neighborhood segregation and the probability of not being in employment or enrolled in education at age 20. The standard economic model of crime suggests that labor market opportunity is a key determinant of criminal participation (e.g. Becker 1968, Ehrlish 1973). This notion is supported by ample empirical evidence (e.g. Gould, Weinberg and Mustard 2002, Grogger 1997, Grönqvist 2012, Machin and Meghir 2004, Niknami 2012).[25] Seminal work in other disciplines argues that reduced labor market opportunities may lead to frustration and anger, which in turn increases the likelihood of crime (e.g. Agnew 1992).

The results in Panel B suggest that segregation reduces labor force attachment. The point estimate is statistically significant at the 1 percent level and indicates that doubling segregation increases the probability of not working or being in education by 8 percent. The results thus lends some support to the view that labor force attachment may be one of the channels through which segregation affects criminal behavior later in life. Our findings echoes the results presented by Bell, Fasani and Machin (2013) who show that an increase in the share of immigrants with low employment probabilities is positively associated with crime while an increase in labor force migrants reduces crime. The magnitude of the estimate is however likely too small to imply that reduced labor market attachment explains a larger part of the segregation-crime relationship.

4.5 THE EFFECT OF LONG-TERM EXPOSURE TO SEGREGATION

[24] The lower limit of a 95 percent confidence interval for a 100 percent increase in segregation, lies between -4 to -7 percent dependently of how we measure educational performance.
[25] See Freeman (1999) for a comprehensive literature review.

So far, we have examined the consequences of being assigned to an area with a given level of segregation. Our approach arguably provides the causal effect of initial exposure to segregation on the probability of committing crime when aged 15–26. It is also relevant to ask what the effect is of growing up in a segregated neighborhood. To the extent that initial segregation is correlated with individuals' exposure over a longer period, our estimates also incorporate the impact of long term exposure to segregation. The reduced form approach, provided by our baseline model, however, produces conservative estimates of this relationship, since some of the families move out of the initial neighborhood over time. It is possible, for instance, that parents respond to increased levels of segregation by moving to another neighborhood. Our analysis could therefore be seen as providing a lower bound of the effect of growing up in a segregated area on future crime.

Our strategy to answer this question is to use initial segregation as an instrument for average exposure to segregation when aged 7–14.[26] This measure is possible to calculate since our administrative population data contain details on each individual's neighborhood of residence each year. This strategy produces estimates that are not deflated by movers. The instrumental variable (IV) approach requires that initial exposure to segregation have no direct effect on the propensity to commit crime other than through its influence on average segregation. If this assumption is violated, the IV estimator will be biased. To be more specific, we estimate the following model

$$C_{ismt} = \gamma_0 + \gamma_1 \overline{Seg}_{smt} + \gamma_2 X_i + \gamma_3 Z_{smt} + \delta_{mt} + v_{ismt} \quad (2)$$

where we instrument for average exposure to segregation, $\overline{Seg}_{smt}$ using assigned levels of segregation, Seg^{ass}_{smt}.

[26] Some children arrived later than age 7. In this case we calculate the average exposure to segregation from the year of arrival up until age 14.

Table 6 presents our OLS, first-stage and IV estimates. The control variables included in the regressions are the same as earlier. We can see that the OLS estimates are all positive and suggest a statistically significant association between average exposure to segregation and crime for all but violent crime. Other studies have also documented that immigrant residential segregation is correlated with some types of crime, e.g. drug related offenses, but not with violent crime (e.g. Stansfield 2014). It is evident from Table 6 that the first-stage is strong (F-value is 482), discarding any potential concerns of a weak instrument. We find that an increase in initial exposure to segregation by 10 percent raises average exposure to segregation by 3.9 percent.

Turning to the IV estimates, these indicate a strong relationship between long-term exposure to segregation and the probability of being convicted for both drug crimes and crimes resulting in prison. When long-term exposure to segregation increases by one standard deviation (.75), the probability of committing drug crimes increases by 28 percent ((.0327×.75)/.087)×100). Also the effect on imprisonment is large and of the same order: 28 percent ((.0356×.75)/.080)×100).

In the case that the response to average exposure to segregation with respect to crime varies between individuals, and that individuals take advantage of this heterogeneity when deciding on the level of segregation, i.e. $E(\gamma_{1i}\overline{Seg}_{st}) > 0$, the IV estimates identify the average effect of long-term exposure to segregation on crime for a nonrandom subpopulation in the data (e.g. Heckman and Vytlacil 1998). Under some additional assumptions (e.g. instrument monotonicity) outlined by e.g. Angrist and Imbens (1994) it is possible to interpret the IV estimates as the average causal effect among individuals who experiences increases in the level of segregation just because they were assigned to a high segregation area due to the policy and would otherwise not have experienced this. It is possible that the effect of segregation on crime is different for this group of individuals compared to the average effect in the population. We therefore present estimates from the linear control function approach developed by Garen (1984). A convenient feature of this model is that it not only provides an unbiased estimate of the average treatment effect for the population, but it also provides a test as to the relative importance of omitted variable bias as well as treatment effect heterogeneity for our estimates. In the economics of crime literature, the model has previously been employed by e.g. Lochner and Moretti (2004) to estimate the effect of schooling on crime. Rewriting the IV model as a random coefficients model[27] (surpressing the covariates) and combining it with some additional assumptions[28] gives us the model

$$(3) \qquad Crime_{ismt} = \omega_0 + \bar{\omega}_1 \overline{Seg}_{smt} + \varphi \hat{u}_{ismt} + \vartheta \overline{Seg}_{smt} \times \hat{u}_{ismt} + v_{ismt}$$

Where $\hat{u}_{ismt}$ is the residual from the first stage regression of the average segregation on assigned segregation. As shown by Wooldridge (1997), while φ provides a test of omitted variable bias in the least squares regression model, the sign and significance of the coefficient $\vartheta = Cov(\gamma_{1i}, u_{ismt})/V(u_{ismt})$ give us a test of treatment effect heterogeneity. While a common response to average exposure to segregation would imply that $\varphi \leq 0$, sorting based on idiosyncratic responses would lead to that $\varphi > 0$.

It is important to note that this generalization on the IV estimator rests on stronger assumptions, such that the conditional expectations of the individual specific error terms can be written as linear functions of the potential endogenous variable and the instrument. Since the model uses generated regressors from a firststage regression, we employ a block-bootstrap procedure to obtain our standard errors.

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[27] The random coefficients model is given by $C_{ismt} = \omega_0 + \bar{\omega}_1 \overline{Seg}_{smt} + (\omega_{i1} - \bar{\omega}_1)\overline{Seg}_{smt} + m_{ismt}$.

[28] The assumptions are (e.g. Garen 1984; Card 1999; Wooldridge 1997)

$E(m_{ismt} | Seg^{ass}_{smt}) = E(\gamma_{1i} | Seg^{ass}_{smt}) = 0$

$E(m_{ismt} | \overline{Seg}_{smt}, Seg^{ass}_{smt}) = \varphi_{ave} \overline{Seg}_{smt} + \varphi_{ass} Seg^{ass}_{smt}$

$E(\gamma_{1i} | \overline{Seg}_{smt}, Seg^{ass}_{smt}) = \sigma_{ave} \overline{Seg}_{smt} + \sigma_{ass} Seg^{ass}_{smt}$

As can be seen in Table 6, the estimates from the linear control function are very similar to the IV estimates. Moreover, ϑ is never positive. This suggests that heterogeneity across individuals does not appear to be important in the context of the present paper.

When comparing the magnitude of the OLS and IV estimates we find that the latter are larger for drug related offenses and for imprisonment, although not statistically significantly different. Note though that the estimates are smaller for the other outcomes. It is relevant to ask where this difference stems from. Since segregation is measured as an average over multiple years using administrative data we find it unlikely that measurement error in segregation could explain our findings. Another possibility could be variable treatment effect intensity. If the effect of segregation varies for different levels of segregation then the IV and OLS estimator identify different weighted averages of all per-unit effects (e.g. Lochner and Moretti forthcoming). We investigated this in the simplest possible way by including a third-order polynomial in average segregation, but found no evidence of a non-linear relationship. It is also conceivable that the difference in effect sizes could be due to treatment effect heterogeneity. However, as we already discussed, the IV estimates are very close to the estimates from our linear control function estimator. Moreover, ϑ is never positive. This suggests that treatment effect heterogeneity is not likely to explain the difference between the OLS and IV estimates. A final reason for the different estimates may be that the standard exclusion restriction needed for both the IV and linear control function models is violated. This would be the case if there is habit formation in crime. In this case exposure to segregation early in life may affect the accumulation of criminal capital over time. This would suggest a direct effect from our instrument on crime. Even though φ is insignificant, suggesting no omitted variable bias, we cannot rule out this possibility.

In summary, we believe that the main benefit of the IV approach is that it corrects for parental mobility over time, which allows us to take one important step in the direction towards answering the question what the effect is of growing up in a segregated area on crime. Yet, due to the reasons outlined above, some caution is warranted when interpreting the results. Indeed, some readers may prefer to view the IV estimates as a possible upper bound of the effect of growing up in a segregated area on future crime.

5. CONCLUDING REMARKS

Following local outbursts of criminal riots, the question of whether minority segregation breads criminal activity has become a central topic in the public debate in many countries. In this paper we are interested in the effect of exposure to segregation during childhood on future criminal participation. Exposure to segregation during childhood and adolescence may be especially harmful as this is a period when many key investments in human and potentially also criminal capital take place. Investigations of this question are complicated by the fact that individuals are not randomly allocated to neighborhoods. To account for this potential problem we study immigrant children who were plausibly randomly assigned to their first locality of residence by Swedish authorities. Our analysis is made possible by rich individual level data allowing us to link measures of childhood exposure to segregation to information on the subjects' future criminal convictions. To the best of our knowledge this paper provides the first pieces of evidence for this question using a compelling source of variation in residential segregation.

Our analysis reveals that being assigned to a neighborhood with a large share of immigrants increases both the probability of committing drug related offenses and being sentenced to prison later in life. The magnitudes of the estimates are large and they account for a substantial portion of the native-immigrant crime gap. The impacts are mainly driven by individuals with low educated parents. We also find some evidence in favor of a "spatial-mismatch" suggesting that one mechanism through which segregation may affect crime is by keeping immigrants away from employment. We also show that experiencing long-run exposure, i.e. to grow up in a segregated area, substantially increases the risk of crime.

The results in this paper suggest that childhood exposure to segregation may be one important reason for why young immigrants are overrepresented among criminal offenders. The question we examine also sheds light on the rationale for policies designed to alter the settlement of immigrants. These policies may come in the form of incentive programs, such as MTO program (see Kling, Liebman, and Katz 2007), or strategies to assign new immigrants to their initial place of residence. The latter kind of policies are (or have been) practiced by many European countries and also by the United States and Canada (see Edin, Fredriksson, and Aslund 2003). Our results suggest that such policies should aim to encourage immigrants to settle in low segregated neighborhoods. On a broader scale, our results may be interpreted as showing the implications of growing up in a disadvantaged area (see e.g. Oreopulos 2004).

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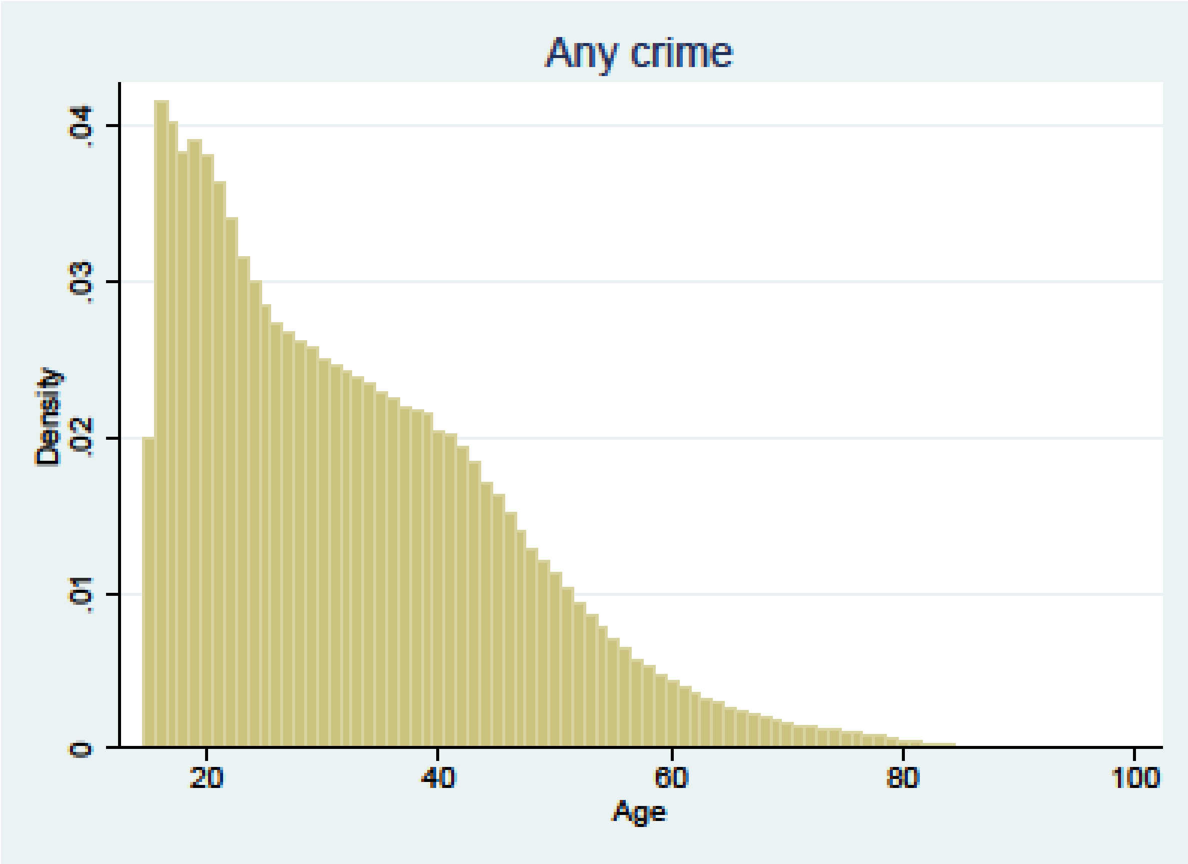

Figure 1. Distribution of convictions by age

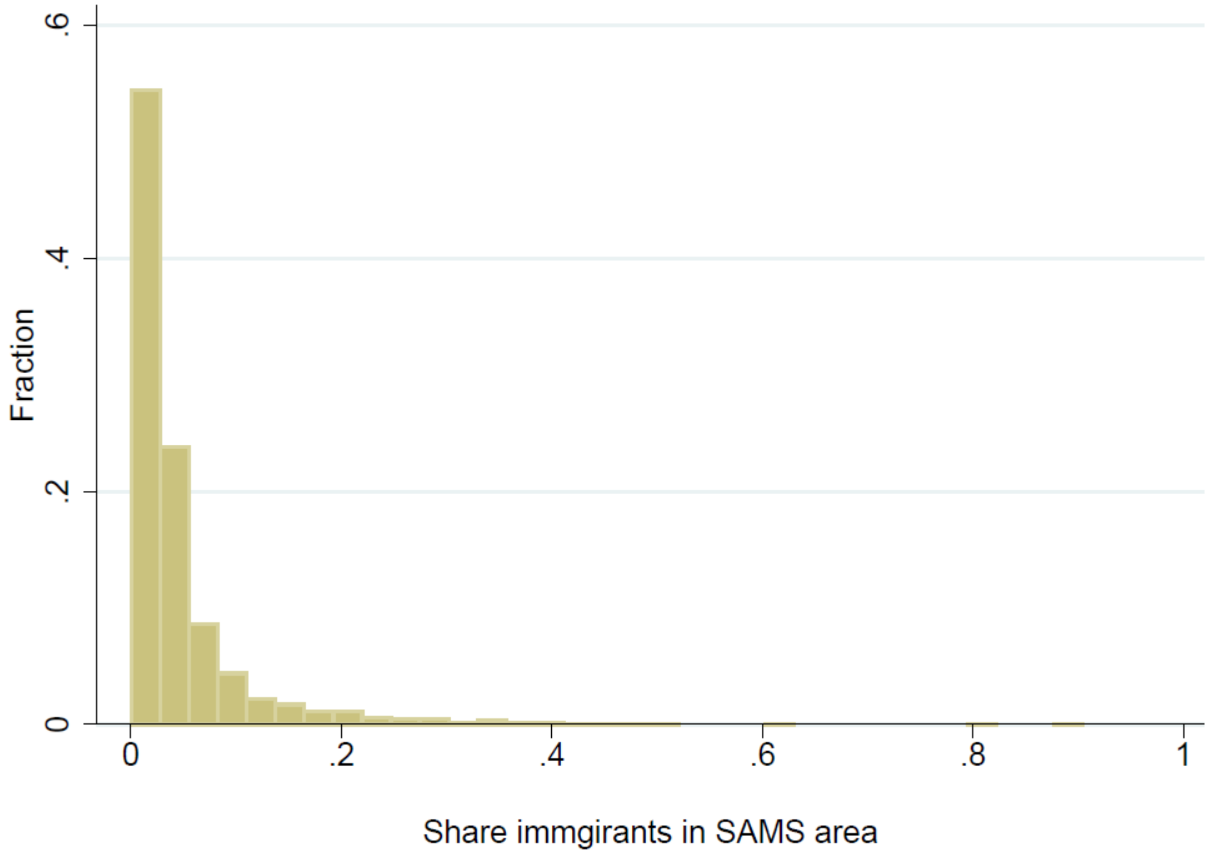

Figure 2. Distribution of refugee immigrants across neighborhood (SAMS)

Table 1. The effect of segregation on crime

Sample	Type of crime				
	Overall [.439] (1)	Violent [.145] (2)	Property [.204] (3)	Drugs [.087] (4)	Prison [.080] (5)
Restricted set of regional controls	-.001 (.008)	-.003 (.005)	.003 (.006)	.009** (.005)	.009** (.004)
Controlling for poverty rate	.007 (.009)	.004 (.007)	.005 (.007)	.012** (.005)	.011** (.005)
Controlling for share high educated	-.003 (.008)	-.002 (.006)	.004 (.007)	.008* (.005)	.009** (.005)
Controlling for share criminals	-.006 (.009)	-.003 (.007)	-.004 (.008)	.015*** (.005)	.011** (.005)
All regional controls	.003 (.010)	.001 (.007)	.005 (.008)	.013** (.005)	.014*** (.005)
Municipality×year FE:s	Yes	Yes	Yes	Yes	Yes
Country of origin FE:s	Yes	Yes	Yes	Yes	Yes

Notes: The table shows the coefficient on (log) refugee immigrant share. Each cell represents a separate regression. The baseline sample consists of male refugee immigrants who were aged 7-14 at arrival during the period 1985−1994 and who were still living in Sweden at age 26 (N=12,148). The dependent variable is the probability that the individual was convicted of a given type of crime or was sentenced to prison or youth custody between age 15 and 26. All regressions control linearly for the subject's and the mother's age at immigration, and include dummies for each parent's educational attainment, family size, and missing values. The restricted set of regional (SAMS) controls include (the log of) population size, the share male youths aged 15-26 and whether there is a police station in the SAMS. Numbers in brackets display mean of the dependent variable. Standard errors robust for clustering at the SAMS level in parentheses. ***= significant at 1 % level; ** = significant at 5 % level; * = significant at 10 % level.

Table 2. The effect of segregation on crime in different subgroups of the population

	Type of crime				
Sample	Overall	Violent	Property	Drugs	Prison
	(1)	(2)	(3)	(4)	(5)
A. Baseline	.003	.001	.005	.013***	.014***
(N=12,148)	(.010)	(.007)	(.008)	(.005)	(.005)
	[.439]	[.145]	[.204]	[.087]	[.080]
B. Parental Education					
High educated parents	.011	.002	-.002	-.002	-.001
(N=6,460)	(.015)	(.010)	(.012)	(.008)	(.007)
	[.373]	[.112]	[.165]	[.070]	[.057]
Low educated parents	-.003	-.002	.016	.025***	.032***
(N=5,050)	(.016)	(.012)	(.015)	(.011)	(.010)
	[.518]	[.182]	[.248]	[.107]	[.105]
C. Family type					
Married parents	.006	–.004	.009	.013**	.017***
(N=10,057)	(.011)	(.008)	(.010)	(.006)	(.006)
	[.432]	[.142]	[.196]	[.081]	[.081]
Single parents	-.035	-.030	-002	.024	.014
(N=2,093)	(.034)	(.024)	(.028)	(.022)	(.022)
	[.473]	[.160]	[.243]	[.117]	[.100]
D. Arrival age					
At least 11	–.005	–.004	.017	.011	.009
(N=5,198)	(.017)	(.012)	(.015)	(.009)	(.010)
	[.472]	[.156]	[.235]	[.081]	[.091]
Less than 11	.007	.002	.004	.014*	.013**
(N=6,952)	(.013)	(.009)	(.010)	(.008)	(.006)
	[.414]	[.137]	[.181]	[.092]	[.071]
Municipality×year FE:s	Yes	Yes	Yes	Yes	Yes
Country of origin FE:s	Yes	Yes	Yes	Yes	Yes
SAMS controls	Yes	Yes	Yes	Yes	Yes

Notes: The table shows the coefficient on (log) refugee immigrant share. Each cell represents a separate regression. The baseline sample consists of male refugee immigrants who were aged 7-14 at arrival during the period 1985–1994 and who were still living in Sweden at age 26. The dependent variable is the probability that the individual was convicted of a given type of crime or was sentenced to prison or youth custody between age 15 and 26. All regressions control (where appropriate) linearly for the subject's and the mother's age at immigration, and include dummies for each parent's educational attainment, family size, and missing values. SAMS controls include (the log of) population size, share aged 15-26, share university educated, share convicted individuals, poverty rate, whether there is a police station in the SAMS.. Numbers in brackets display mean of the dependent variable. Standard errors robust for clustering at the SAMS level in parentheses. ***= significant at 1 % level; ** = significant at 5 % level; * = significant at 10 % level.

Table 3. Additional results

Sample	Overall (1)	Violent (2)	Property (3)	Drugs (4)	Prison (5)
	Type of crime				
A. Baseline	.003	.001	.005	.013***	.014***
(N=12,148)	(.010)	(.007)	(.008)	(.005)	(.005)
	[.439]	[.145]	[.204]	[.087]	[.080]
B. Female youths	.005	.001	.008	.001	-.002**
(N=10,857)	(.008)	(.003)	(.007)	(.001)	(.001)
	[.439]	[.010]	[.104]	[.006]	[.003]
C. Total crime	.227**	-.015	.065***	.049***	.058*
(N=12,148)	(.089)	(.014)	(.025)	(.018)	(.032)
	[2.041]	[.226]	[.434]	[.231]	[.390]
D. Parish	.000	.005	-.008	.006	.020**
(N=12,172)	(.015)	(.011)	(.011)	(.008)	(.008)
	[.439]	[.145]	[.205]	[.087]	[.080]
E. Sams size					
Large sams	.007	.006	.008	.004	.013
(6,074)	(.021)	(.014)	(.017)	(.011)	(.011)
	[.439]	[.145]	[.210]	[.091]	[.083]
Small sams	.007	.014	.008	.020**	.024***
(6,074)	(.014)	(.010)	(.012)	(.008)	(.007)
	[.439]	[.145]	[.199]	[.084]	[.076]
*F. Dropping municipality*year FEs*	.002	-.001	-.003	.010***	.007*
	(.006)	(.004)	(.005)	(.004)	(.004)
	[.439]	[.145]	[.204]	[.087]	[.080]
Municipality×year FE:s	Yes	Yes	Yes	Yes	Yes
Country of origin FE:s	Yes	Yes	Yes	Yes	Yes
SAMS controls	Yes	Yes	Yes	Yes	Yes

Notes: The table shows the coefficient on (log) refugee immigrant share. Each cell represents a separate regression. The baseline sample consists of male refugee immigrants who were aged 7-14 at arrival during the period 1985–1994 and who were still living in Sweden at age 26. The dependent variable in baseline is the probability that the individual was convicted of a given type of crime or was sentenced to prison or youth custody between age 15 and 26. All regressions control linearly for the subject's and the mother's age at immigration, and include dummies for each parent's educational attainment, family size, and missing values. SAMS controls include (the log of) population size, share aged 15-26, share university educated, share convicted individuals, poverty rate, whether there is a police station in the SAMS.. Numbers in brackets display mean of the dependent variable. Standard errors robust for clustering at the SAMS level in parentheses. ***= significant at 1 % level; ** = significant at 5 % level; * = significant at 10 % level.

Table 4. Mechanisms: The importance of role models and peer influences

	Type of crime				
Sample	Overall (1)	Violent (2)	Property (3)	Drugs (4)	Prison (5)
A. Baseline	.003	.001	.005	.013***	.014***
(N=12,148)	(.010)	(.007)	(.008)	(.005)	(.005)
	[.439]	[.145]	[.204]	[.087]	[.080]
B. Controlling for Immigrant quality	-.002	.001	.003	.012**	.013**
(N=12,148)	(.010)	(.007)	(.008)	(.006)	(.005)
	[.439]	[.145]	[.204]	[.087]	[.080]
C. Peer GPA sample					
a. No control for peer GPA	-.001	.000	.004	.015**	.012**
	(.011)	(.008)	(.009)	(.006)	(.006)
(N=10,607)	[.427]	[.143]	[.196]	[.087]	[.076]
b. Controlling for peer GPA	.002	.000	.005	.014**	.012**
	(.011)	(.008)	(.009)	(.006)	(.006)
(N=10,607)	[.427]	[.143]	[.196]	[.087]	[.076]
Municipality×year FE:s	Yes	Yes	Yes	Yes	Yes
Country of origin FE:s	Yes	Yes	Yes	Yes	Yes
SAMS controls	Yes	Yes	Yes	Yes	Yes

Notes: The table shows the coefficient on (log) refugee immigrant share. Each cell represents a separate regression. The baseline sample consists of male refugee immigrants who were aged 7-14 at arrival during the period 1985‒1994 and who were still living in Sweden at age 26. The dependent variable is the probability that the individual was convicted of a given type of crime or was sentenced to prison or youth custody between age 15 and 26. All regressions control linearly for the subject's and the mother's age at immigration, and include dummies for each parent's educational attainment, family size, and missing values. SAMS controls include (the log of) population size, share aged 15-26, share university educated, share convicted individuals, poverty rate, whether there is a police station in the SAMS.. Numbers in brackets display mean of the dependent variable. Standard errors robust for clustering at the SAMS level in parentheses. ***= significant at 1 % level; ** = significant at 5 % level; * = significant at 10 % level.

Table 5. The effect of segregation on school performance and labor market attachment

Outcome:	
A. Schooling	
Compulsory school GPA	-.481
(N= 11,479)	(.526)
	[34.71]
High school GPA	-.727
(N=7,215)	(.747)
	[33.11]
Pr(High school droput)	-.001
(N=12,148)	(.010)
	[.406]
B. Probability of not being in employment or education when aged 20	.024***
	(.009)
	[.296]
Municipality×year FE:s	Yes
Country of origin FE:s	Yes
SAMS controls	Yes

Notes: The table shows the coefficient on (log) refugee immigrant share. Each cell represents a separate regression. The baseline sample consists of male refugee immigrants who were aged 7-14 at arrival during the period 1985–1994 and who were still living in Sweden at age 26. All regressions control linearly for the subject's and the mother's age at immigration, and include dummies for each parent's educational attainment, family size, and missing values. SAMS controls include (the log of) population size, share aged 15-26, share university educated, share convicted individuals, poverty rate, whether there is a police station in the SAMS.. Numbers in brackets display mean of the dependent variable. Standard errors robust for clustering at the SAMS level in parentheses. ***= significant at 1 % level; ** = significant at 5 % level; * = significant at 10 % level.

Table 6. The effect of long-run exposure to segregation on crime

Sample	Type of crime				
	Overall [.439] (1)	Violent [.145] (2)	Property [.204] (3)	Drugs [.087] (4)	Prison [.080] (5)
A. Least squares	.037***	.002	.023***	.009**	.018***
	(.008)	(.006)	(.006)	(.004)	(.005)
B. First-stage	.388***				
	(.017)				
C. IV	.006	.001	.012	.036***	.033**
	(.024)	(.017)	(.020)	(.013)	(.013)
D. Control function					
Average segregation ($\bar{\omega}_1$)	-.003	-.004	.007	.037***	.032**
	(.026)	(.019)	(.023)	(.013)	(.015)
Residual (φ)	-.011	-.019	-.010	-.025	-.019
	(.033)	(.021)	(.029)	(.019)	(.020)
Average segregation×Residual (ϑ)	-.024***	-.011*	-.012	.002	-.001
	(.009)	(.006)	(.008)	(.005)	(.006)
Municipality×year FE:s	Yes	Yes	Yes	Yes	Yes
Country of origin FE:s	Yes	Yes	Yes	Yes	Yes
SAMS controls	Yes	Yes	Yes	Yes	Yes

Notes: The table shows the coefficient on (log) refugee immigrant share or on (log) refugee immigrant share averaged between the year of arrival and age 14. The baseline sample consists of male refugee immigrants who were aged 7-14 at arrival during the period 1985−1994 and who were still living in Sweden at age 26 (N=12,148). Crime is measured as the probability that the individual was convicted of a given type of crime or was sentenced to prison or youth custody between age 15 and 26. All regressions control linearly for the subject's and the mother's age at immigration, and include dummies for each parent's educational attainment, family size, and missing values. SAMS controls include (the log of) population size, share aged 15-26, share university educated, share convicted individuals, poverty rate, whether there is a police station in the SAMS. Numbers in brackets display mean of the dependent variable. Standard errors robust for clustering at the SAMS level in parentheses. Standard errors in Panel D are obtained using block-bootstrapping. ***= significant at 1 % level; ** = significant at 5 % level; * = significant at 10 % level.

APPENDIX

Table A.1 The association between assigned neighborhood conditions and individual/family attributes

	Dependent variable:						
	Mean earnings	Non-empl. rate	p50/p10	Share university educated	Conviction rate	Mean age	Share immigrants
Age at immigration	.0010	-.0029	.0027	.0007	-.0068	.0011	-.0022
	(.0015)	(.0016)	(.0058)	(.0016)	(.0026)	(.0006)	(.0037)
Female	-.0020	-.0058	.0145	.0003	-.0138	.0029	-.0276
	(.0079)	(.0066)	(.0291)	(.0064)	(.0102)	(.0019)	(.0140)
Mother's characteristics							
Age at immigration	.0004	-.0007	-.0022	.0008	.0029	-.0011	.0049
	(.0008)	(.0010)	(.0040)	(.0010)	(.0016)	(.0003)	(.0024)
Short high school	.0230	-.0272	.0190	.0041	-.0377	.0085	-.0884
	(.0115)	(.0140)	(.0587)	(.0137)	(.0208)	(.0044)	(.0321)
Long high school	.0192	-.0057	-.0327	-.0003	-.0213	.0042	-.0133
	(.0148)	(.0128)	(.0559)	(.0123)	(.0177)	(.0037)	(.0260)
Short university	.0396	-.0307	.0217	.0186	-.0687	.0062	-.0832
	(.0161)	(.0148)	(.0668)	(.0149)	(.0228)	(.0046)	(.0321)
Long university	.0573	-.0377	.0470	.0125	-.0521	.0095	-.0929
	(.0195)	(.0185)	(.0746)	(.0182)	(.0251)	(.0054)	(.0371)
Father's characteristics							
Age at immigration	.0004	.0003	.0032	-.0003	-.0039	.0008	-.0033
	(.0007)	(.0009)	(.0034)	(.0009)	(.0014)	(.0003)	(.0021)
Short high school	-.0032	-.0031	-.0481	.0145	-.0216	.0026	-.0109
	(.0117)	(.0142)	(.0631)	(.0142)	(.0209)	(.0043)	(.0303)
Long high school	.0060	-.0051	-.103	.0262	-.0073	-.0009	-.0240
	(.0130)	(.0142)	(.0600)	(.0137)	(.0214)	(.0041)	(.0322)
Short university	-.0083	-.0115	-.0480	.0438	.0051	-.0039	-.0084
	(.0226)	(.0162)	(.0737)	(.0180)	(.0250)	(.0047)	(.0334)
Long university	.0179	-.0255	-.0095	.0448	-.0225	.0016	-.0541
	(.0139)	(.0156)	(.0626)	(.0170)	(.0247)	(.0048)	(.0363)

Notes: The sample consists of refugee immigrants aged 7-14 at arrival who arrived during the period 1985–1994 and who were still living in Sweden at age 26 (N=23,000). The dependent variables are measured for the assigned SAMS. All regressions control for (the log of) population size, country of birth, and municipality-by-year Fes. Standard errors robust for clustering at the SAMS level in parentheses. Bold numbers indicate significance according to Bonferroni-type corrections to the critical p-value of 0.05 when testing 84 hypotheses.

Table A.2 The association between neighborhood conditions measured at age 26 and individual/family attributes

	Dependent variable:						
	Mean earnings	Non-empl. rate	p50/p10	Share university educated	Conviction rate	Mean age	Share immigrants
Age at immigration	-.0228	.0016	**-.0221**	-.0344	**.0308**	**-.0020**	**-.0246**
	(.0017)	(.0018)	(.0045)	(.0014)	(.0021)	(.0005)	(.0034)
Female	-.0071	.0154	-.0254	.0044	.0061	-.0002	.0282
	(.0060)	(.0062)	(.0179)	(.0047)	(.0082)	(.0017)	(.0124)
Mother's characteristics							
Age at immigration	-.0007	.0003	.0006	-.0007	.0019	-.0005	.0020
	(.0000)	(.0009)	(.0022)	(.0007)	(.0011)	(.0002)	(.0018)
Short high school	**.0561**	**-.0506**	.0888	**.0433**	**-.0510**	.0032	**-.0985**
	(.0104)	(.0122)	(.0305)	(.0089)	(.0143)	(.0032)	(.0232)
Long high school	**.0512**	-.0437	.104	**.0479**	**-.0496**	-.0017	**-.0832**
	(.0101)	(.0107)	(.0292)	(.0079)	(.0135)	(.0029)	(.0211)
Short university	**.0731**	**-.0704**	**.147**	**.0886**	**-.0860**	-.0080	**-.110**
	(.0131)	(.0146)	(.0323)	(.0108)	(.0176)	(.0039)	(.0274)
Long university	**.120***	**-.112**	**.192**	**.128**	**-.169**	-.0013	**-.208**
	(.0156)	(.0169)	(.0387)	(.0123)	(.0205)	(.0046)	(.0311)
Father's characteristics							
Age at immigration	-.0012	**.0017**	-.0040	.0006	.0009	.0000	.0020
	(.0008)	(.0008)	(.0021)	(.0006)	(.0010)	(.0002)	(.0016)
Short high school	.0040	-.0158	.0062	.0089	-.0231	.0034	-.0048
	(.0135)	(.0141)	(.0359)	(.0097)	(.0174)	(.0038)	(.0278)
Long high school	.0117	-.0114	.0194	.0174	-.0234	-.0006	.0031
	(.0130)	(.0135)	(.0360)	(.0099)	(.0163)	(.0036)	(.0258)
Short university	.0383	-.0412	.102	.0431	**-.0605**	-.0027	-.0460
	(.0156)	(.0157)	(.0370)	(.0113)	(.0190)	(.0040)	(.0298)
Long university	**.0585**	-.0499	.116	**.0685**	**-.0858**	-.0015	-.0697
	(.0147)	(.0157)	(.0371)	(.0115)	(.0190)	(.0043)	(.0305)

Notes: The sample consists of male refugee immigrants aged 7-14 at arrival who arrived during the period 1985–1994 and who were still living in Sweden at age 26 (N=23,000). The dependent variables are measured for the SAMS where the individual lived at age 26 (all entered in logs). All regressions control for (the log of) population size, country of birth, and municipality-by-year FEs. Standard errors robust for clustering at the SAMS level in parentheses. Bold numbers indicate significance according to Bonferroni-type corrections to the critical p-value of 0.05 when testing 84 hypotheses.

Table A.3. Definitions of crime categories

Crime type	Explanation	Penal code
Violent crime	The full spectrum of assaults from less severe violence to murder.	BRB Chapter 3
Property crime	The full spectrum of thefts from shop-lifting to burglary. Robbery is also included.	BRB Chapter 8
Drugs	The full spectrum of drug related crime from possession to selling.	SFS 1968:64
Prison	Individual sentenced to prison or youth custody	

Table A.4. Region of birth

	freq	pct
Bosnia	2620	21.57
Ex Yugoslavia	2575	21.20
Poland	253	2.08
the Baltic region	29	0.24
Eastern Europe	494	4.07
Czechoslovakia	106	0.87
Central America	129	1.06
Chile	609	5.01
South America	168	1.38
Horn of Africa	329	2.71
Middle East	1691	13.92
Sub-Saharan Africa	92	0.76
Iran	1793	14.76
Iraq	556	4.58
Turkey	501	4.12
South Asia	203	1.67
Total	12148	100.00

Table A.5 Descriptive statistics

	Mean	Standard deviaton
Female	.461	.498
Age at immigration	11.053	2.200
Mother's characteristics		
No. children	2.855	1.230
Age at immigration	36.208	5.639
Arrived without partner	.122	.327
Compulsory school	.508	.500
Short high school	.132	.338
Long high school	.178	.383
Short university	.098	.297
Long university	.085	.278
Father's characteristics		
Age at immigration	38.959	8.117
Compulsory school	.293	.455
Short high school	.132	.338
Long high school	.155	.362
Short university	.112	.315
Long university	.138	.345
Compulsory school	.170	.376
SAMS characteristics at assignment		
Population size	2165	2067
Mean earnings (1000 SEK)	1377	372
p50/p10 income ratio	950	525
Non-employment rate	.277	.141
Share of poor	.086	.079
Share university educated	.275	.122
Conviction rate	.025	.016
Mean age	43.433	5.136
Share refugee immigrants	.151	.160
SAMS characteristics at age 26		
Population size	2777	2765
Mean earnings	1789	641
p50/p10 income ratio	1140	695
Non-employment rate	.330	.139
Share of poor	.139	.092
Share university educated	.466	.146
Conviction rate	.019	.010
Mean age	44.664	5.033
Share refugee immigrants	.281	.201

Notes: The sample consists of refugee immigrants aged 7-14 whose parents arrived during the period 1985–1994 and who were still living in Sweden at age 26 (N=23,000).

Table A.6 Robustness checks

	Type of crime				
Sample	Overall	Violent	Property	Drugs	Prison
	(1)	(2)	(3)	(4)	(5)
A. Baseline	.003	.001	.005	.013***	.014***
(N=12,148)	(.010)	(.007)	(.008)	(.005)	(.005)
	[.439]	[.145]	[.204]	[.087]	[.080]
B. Dissimilarity index	.003	-.000	.000	.006*	.007**
(N=12,148)	(.005)	(.004)	(.004)	(.003)	(.003)
	[.439]	[.145]	[.204]	[.087]	[.080]
C. Isolation index	.001	-.001	.002	.006**	.007**
(N=12,148)	(.006)	(.004)	(.005)	(.003)	(.003)
	[.439]	[.145]	[.204]	[.087]	[.080]
D. Arrival year <1992	.008	-.005	.001	.014*	.017**
(N=5,434)	(.014)	(.011)	(.012)	(.008)	(.008)
	[.510]	[.167]	[.246]	[.099]	[.094]
Municipality×year FE:s	Yes	Yes	Yes	Yes	Yes
Country of origin FE:s	Yes	Yes	Yes	Yes	Yes
SAMS controls	Yes	Yes	Yes	Yes	Yes

Notes: The table shows the coefficient on (log) segregation for different measures of segregation. Each cell represents a separate regression. The baseline sample consists of male refugee immigrants who were aged 7-14 at arrival during the period 1985−1994 and who were still living in Sweden at age 26. The dependent variable is the probability that the individual was convicted of a given type of crime or was sentenced to prison or youth custody between age 15 and 26. All regressions control linearly for the subject's and the mother's age at immigration, and include dummies for each parent's educational attainment, family size, and missing values. SAMS controls include (the log of) population size, share aged 15-26, share university educated, share convicted individuals, poverty rate, whether there is a police station in the SAMS.. Numbers in brackets display mean of the dependent variable. Standard errors robust for clustering at the SAMS level in parentheses. ***= significant at 1 % level; ** = significant at 5 % level; * = significant at 10 % level.

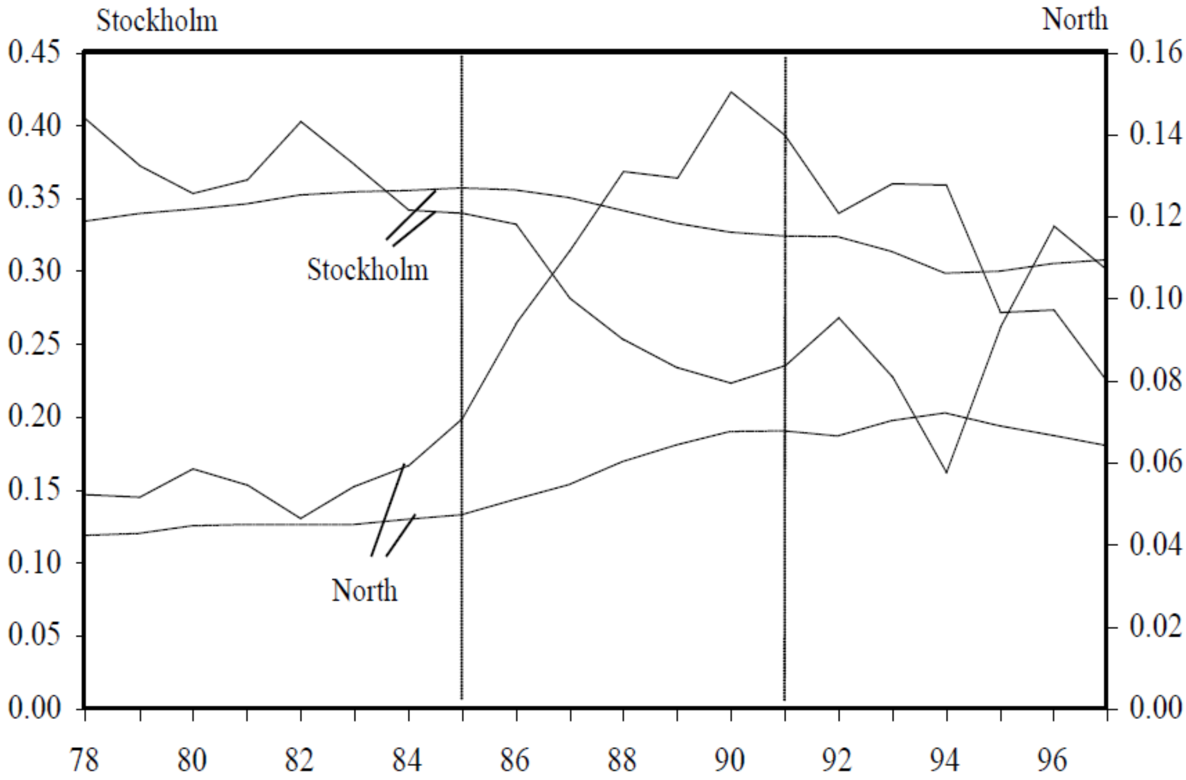

Figure A1 Share of non-OECD immigrant inflow (solid) and stock (dashed) located in Stockholm county and in the North counties of Sweden, respectively, 1978-1997. From Edin, Fredriksson and Åslund (2003).