

DEPRIVATION AND CRIMINALITY

The impact of neighbourhood deprivation on adolescent violent criminality and substance misuse: A longitudinal, quasi-experimental study of the total Swedish population

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Background A number of studies suggest associations between neighbourhood characteristics and criminality during adolescence and young adulthood. However, the causality of such neighbourhood effects remains uncertain.

Methods We followed all children born in Sweden from 1975–1989 who lived in its three largest cities by the age of 15 years and for whom complete information was available about individual and contextual factors ($N = 303\,465$). All biological siblings were identified in the sample ($N = 179\,099$). Generalized linear mixed-effects models were used to assess the effect of neighbourhood deprivation on violent criminality and substance misuse between the ages of 15 and 20 years, while taking into account the cross-classified data structure (i.e. siblings in the same families attending different schools and living in different neighbourhoods at age 15).

Results In the crude model, an increase of 1 SD in neighbourhood deprivation was associated with a 57% increase in the odds of being convicted of a violent crime (95% CI 52%–63%). The effect was greatly attenuated when adjustment was made for a number of observed confounders (OR 1.09, 95% CI 1.06–1.11). When we additionally adjusted for unobserved familial confounders, the effect was no longer present (OR 0.96, 95% CI 0.84–1.10). Similar results were observed for substance misuse. The results were not due to poor variability either between neighbourhoods or within families.

Conclusions We found that the adverse effect of neighbourhood deprivation on adolescent violent criminality and substance misuse in Sweden was not consistent with a causal inference. Instead, our findings highlight the need to control for familial confounding in multilevel studies of criminality and substance misuse.

Keywords Violence, substance-related disorders, residence characteristics, socio-economic factors, multi-level analysis, confounding factors

How did the study come about?

Almost two decades have passed since the World Health Assembly declared violence to be a major public health concern.¹ The numerous health-related consequences of violence have since been studied intensely.^{2–4} One mechanism suggested as being responsible for violence is the influence of an individual's neighbourhood of residence.

Research has consistently found criminal activity and substance misuse to be more common in deprived residential areas characterized by low socio-economic status (SES), large immigrant populations, and high rates of unemployment, divorce, and residential mobility.^{5–8} This raises the fundamental question of the extent to which neighbourhoods influence individuals, beyond their individual characteristics, to commit violent acts or engage in substance misuse.

Multilevel analyses performed in the US addressing individual and contextual effects simultaneously indicate sizeable effects of neighbourhood on criminality,⁹ whereas European studies have consistently found marginal effects of these factors.^{10–12} The only population-based neighbourhood study of substance misuse in Sweden suggested that children growing up in low income neighbourhoods experienced a 73 percent increased risk of engaging in such misuse as compared to their peers growing up in high income neighbourhoods.¹³

Causal inferences from neighbourhood studies should be drawn with caution,^{14–17} especially because of selection that is non-random in that a number of observed as well as unobserved individual, familial, and structural characteristics determine patterns of moving into and out of neighbourhoods.^{18,19} Standard multi-level models are particularly problematic because of their inherent limitation in adjusting only for observed characteristics, which generally gives rise to bias created by omitted variables.²⁰

Experimental studies have been inconclusive in determining links between neighbourhoods and criminality.²¹ The Moving to Opportunities project has found that females living in families that were randomly selected to move to more affluent areas had lower rates of offences involving property and violence than did their controls, whereas males had higher rates of crimes involving property and behavioural problems but lower rates of violent offences only in the short-run.^{22–24} In a similar study, the age of the study participants was identified as an effect modifier in that younger children who had moved to more affluent areas experienced fewer behavioural problems and less delinquency whereas older children experienced higher rates of both.²⁵

Another approach to dealing with issues of selection has been to adopt genetically informative research designs that compare differentially exposed family members to explain unobserved familial factors.^{26–28} In a recent study, mother-rated neighbourhood disadvantage was found to be an independent predictor of problems in the conduct of offspring, as reported by the study participants and their mothers, among differentially exposed cousins.²⁹

It has been proposed that schools mediate the association between neighbourhood disadvantage and outcomes of social behavior in adolescents and young adults.³⁰ Two recent studies concluded that observed neighbourhood effects tend to be explained by school-related effects.^{12,31}

We studied the general as well as the specific neighbourhood effects of neighbourhood deprivation on adolescent violent criminality and substance misuse. Although general neighbourhood effects refers to the neighbourhood-attributed variance in outcomes, specific neighbourhood effects is focused on the specific contextual associations between neighbourhood deprivation and outcomes.^{32,33} By using population-based longitudinal data and a sibling-comparison design, we conducted a study with the statistical power to detect small effects and which could take into account familial, neighbourhood, and school-related effects.

Methods

National registers and the study population

We linked longitudinal data held by various governmental agencies in the Swedish *Total Population Register* (TPR). We were granted access to de-identified linked data from Statistics Sweden after approval from the Regional Research Ethics Committee at Karolinska Institutet.

We included all individuals born in Sweden from 1975–1989 who were residents in the three largest city regions of Sweden (Stockholm, Malmö, and Gothenburg) at age 15 years ($N=297\,752$). In addition, we identified all full siblings in the sample ($N=172\,525$). A flow chart of inclusion and exclusion criteria for the study samples is presented in [Figure 1](#).

The TPR contains basic information (e.g. sex, date, and country of birth) for all Swedish inhabitants since 1968.³⁴ We linked the TPR to the *Multi-Generation Register*, connecting index persons to their biological parents.³⁵ Data on biological parents were used in the study to identify full siblings.

The *Medical Birth Register* (National Board of Health and Welfare), includes pregnancy data with nearly full coverage (>99%) of all births in Sweden since 1973.³⁶

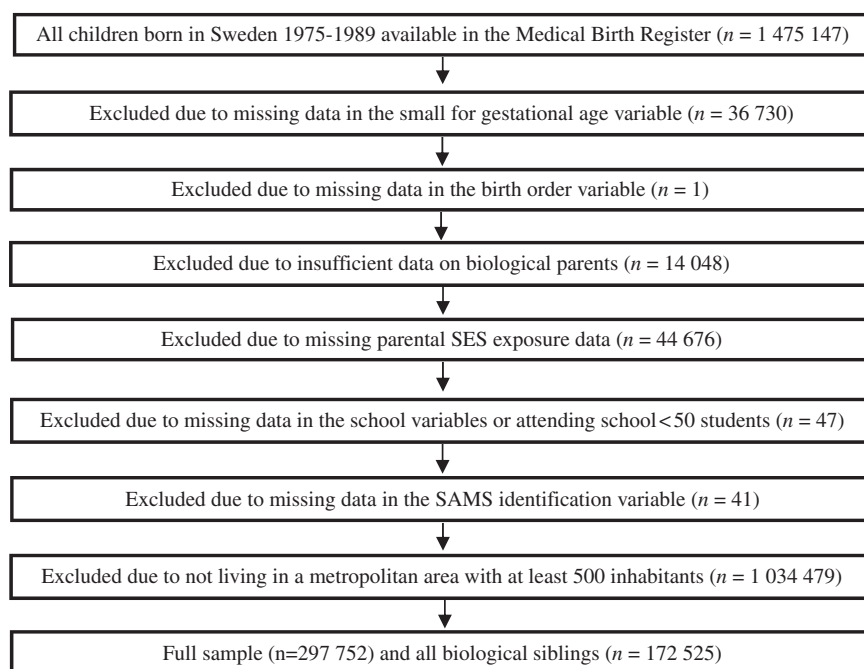


Figure 1 Flow chart of study samples

The *Primary School Register* (National Agency for Education) includes the final educational attainment scores for all students who finished compulsory schooling in Sweden from 1988–2008.

The *National Crime Register* (National Council for Crime Prevention) holds detailed information on all criminal convictions in lower general court in Sweden since 1973. The practice of plea bargaining, in which defendants agree to plead guilty to lesser charges to avoid trial or to seek sentence reductions, is not allowed, and conviction data include all persons who received custodial or non-custodial sentences, as well as cases in which the prosecutor decided to caution or fine the defendant. The age of legal responsibility in Sweden is 15 years, and we were therefore unable to study criminal offences by persons under the age of 15 years.

The *Patient Register* (National Board of Health and Welfare) contains diagnoses according to the International Classification of Diseases (ICD) for all individuals admitted to hospital (inpatient care) in Sweden since 1973 as well as diagnoses made in outpatient consultations with specialist physicians other than general practitioners since 2001.

The *LISA Register* (Statistics Sweden) is a longitudinal integrated data base for labor-market research that covers all citizens 16 years of age and older since 1990 and is updated annually.³⁷ The *LISA Register* includes a wide range of socio-economic and demographic variables.

Neighbourhood definition

A central issue in neighbourhood research is choosing theoretically meaningful and empirically informative

geographical units. Researchers have traditionally relied on administrative units too large to capture social homogeneity within neighbourhoods.³⁸ To overcome this, we used Statistics Sweden's SAMS (Small Area Marketing Statistics) classification for small geographical areas that are constructed to delineate socially homogenous small areas.³⁹ The selection criteria differ according to regional population density, leading to substantial heterogeneity in population sizes, which introduces statistical problems. Consequently, we included only SAMS areas in Sweden's three largest cities that had at least 500 inhabitants.

Outcome variables

Violent criminality was defined as any conviction for a violent offence from the age of 15 through the age of 20 years.⁴⁰

Substance misuse was defined as conviction for any drug-related crime (defined as any crime against the Narcotic Drugs Act (SFS 1968:64), driving under the influence of alcohol and/or illicit substances), or having had a diagnosis made of an alcohol- or drug-misuse-related disease in an inpatient or outpatient setting (ICD-8: 291, 303-4, 571, E853, E856.4, E859, E860, N980; ICD-9: 291, 303-5, 357.5, 425.5, 535.3, 571.0-571.3, E850, E854.1-2, E855.2, E860, N980; ICD-10: F10, G32.2, G62.1, G72.1, I42.6, K29.2, K70, K85, X41-2, X45, X61-2, X65, Y11 [with T43.6], Y12 [with T40], and Y15 [with T51]) from the age of 15 through the age of 20 years. Similar omnibus measures of substance misuse have been used previously in the literature.^{41,42}

Family and individual level exposures

Definition of the term ‘immigrant descendant’ was based on the parental country of birth. Participants who had at least one parent born outside a Nordic country were defined as immigrant descendants.

The term ‘birth order’ describes a dichotomous measure of whether the participant was the first-born offspring of his or her parents.

The terms ‘small for gestational age’ and ‘large for gestational age’ are defined as a body size that is either 2 SD below or 2 SD above the mean birth weight for gestational age, respectively.⁴³

The ‘primary school grade point average (GPA)’ was coded as a rank-transformed and standardized measure, since participants were examined under two separate grade systems.

‘Parental income’ addressed family disposable income, a measure of participants’ material resources at age 15. In cases in which the parents of a participant were separated, this measure corresponded to the average family disposable income across both parental households.

‘Parental education level’ referred to the highest education level (primary, secondary, or post-secondary school) achieved by either of the parents of a participant.

‘Parental welfare reciprocity’ reflected means-tested welfare benefits. Because of the comprehensive nature of the Swedish welfare state, receipt of means-tested benefits tends to reflect a wider range of psychosocial problems and not primarily lower (personal) economic status.⁴⁴

‘Single mother’ was coded dichotomously according to whether the mother’s participant offspring was living in a single-mother household at age 15.

‘Residential mobility’ was measured whether or not the mother of a participant had moved at least twice during the year in which the participant turned 15 years of age. Participants whose mothers fell into this category were classified as having high residential mobility.

The definition ‘parental conviction and substance misuse’ indicates any parental conviction for a criminal offence and/or for substance misuse. To assure the temporal order of any inter-generational effect, we considered only parental exposures that had occurred prior to the offspring’s outcomes. If the offspring had not been convicted of a criminal offence or engaged in substance misuse, parents were followed until the offspring’s 20th birthday.

Neighbourhood deprivation

We used the SAMS area of residence for the year in which each study participant turned 15 years of age.

‘Neighbourhood deprivation’ refers to the overall degree of socio-economic deprivation at the end of the year in which study participants reached the age of 15 years. First, we linked all individuals from the ages of 25 years to 64 years to their SAMS area of residence. We

subsequently derived indicator variables consisting of the proportions of welfare recipients, unemployed individuals, immigrants defined as individuals who were not born in Sweden, divorced individuals, and the proportion of individuals who had not completed secondary schooling, respectively. Residential mobility and crime rates, including median neighbourhood disposable income, were also calculated. These indicator variables were then subjected to a principal-components analysis that generated a single standardized neighbourhood deprivation score for each year and each residential area. We assigned a neighbourhood deprivation score to each study participant for the neighbourhood in which they resided in at the end of the year in which they turned 15 years of age. For descriptive purposes, we present deciles of this variable, but in all of our statistical models, neighbourhood deprivation is a standardized continuous measure.

Statistical analyses

First, we fitted a linear mixed-effects model with random intercepts for each family, to estimate the sibling correlation in the neighbourhood deprivation exposure variable. To increase the accuracy of our results, we specifically studied neighbourhood-discordant siblings. The random effect was expressed as an intra-class correlation (ICC), which measures the degree to which individuals within clusters are similar in outcome.^{45,46} The measure of correlation ranges from 0 to 1, where a value of 1 implies that all individuals within a cluster had identical values.

To study the ICCs for different contexts (families, neighbourhoods, and schools) with the binary outcome variables, we fitted generalized linear mixed-effects models (GLMMs) for each combination of the outcome variables and contexts. We used a logit link function and calculated the ICCs using the latent variable method.^{32,47–49} The GLMMs were estimated through Gauss–Hermite quadrature, which is a computationally efficient and robust estimator of GLMMs with small cluster sizes.^{50,51} Adjustments for fixed-effects exposure variables were included in subsequent models, to investigate the extent of the variance for which they accounted.

The final models expanded the GLMMs to also include crossed random effects, hence accounting for the non-hierarchical, cross-classified data structure of siblings attending different schools and living in different neighbourhoods.⁵² A crude model including the random effects and the between- and within-neighbourhood deprivation estimates (bND and wND) can be written as:

$$\left. \begin{aligned} \Pr(y_i = 1) &= \text{logit}^{-1}(\alpha_{j[i]} + \alpha_{k[i]} + \alpha_{l[i]} + \beta^{bND} * bND + \beta^{wND} * wND) \\ \alpha_j &\sim N(\mu_\alpha, \sigma_{neighbourhood}^2), \alpha_k \sim N(\mu_\alpha, \sigma_{school}^2), \alpha_l \sim N(\mu_\alpha, \sigma_{family}^2) \end{aligned} \right\} \quad (1)$$

The model thus extends a generalized linear model with a logit link function, through including three

random intercepts for neighbourhoods (α_j), schools (α_k), and families (α_l) that are assumed to be normally distributed and independent. Familial confounding is taken into account through the within-family estimation of the neighbourhood-deprivation effect. The inclusion of the random effects provides more accurate standard errors for the fixed-effects estimates, which increases the possibility that an association found in a GLMM can be interpreted as causal.

The models were fitted in Stata version 12.1 IC⁵³ and the lme4-package⁵⁴ in R version 2.15.2.⁵⁵

Results

The 297 752 study participants were nested within 1049 neighbourhoods and 1121 schools. The 172 525 siblings were in turn nested within 78 570 families, 1028 neighbourhoods, and 788 schools. Overall, 23 096 (13.4%) of the siblings were discordant for neighbourhoods. Of these discordant siblings, 1408 and 2281 were further discordant for violent criminality and substance misuse, respectively. Approximately 83% of the SAMS-discordant siblings were discordant for neighbourhood-deprivation deciles.

Table 1 presents descriptive characteristics of the studied residential SAMS areas included in the study. Although the differences between the first and the fifth neighbourhood-deprivation deciles are rather marginal, there are stark differences between these two deciles and the tenth decile. Almost a third of the populations in the most deprived areas consisted of welfare recipients, whereas the corresponding figure in the least deprived areas was 1.17%.

Descriptive data are presented in Table 2. We found expected patterns of associations, with higher rates of violent crime and substance misuse in individuals of lower SES in both the unrelated and the sibling samples.

The sibling correlation for the neighbourhood-deprivation measure amounted to 0.46 (95% CI 0.45–0.48), implying that there was a fair amount of variability within families with which to continue our analyses.

General neighbourhood effects are presented in Table 3. The crude models suggested that 12.2% and 4.2% of the variance in violent criminality and substance misuse, respectively, were attributable to the neighbourhood context. The adjusted models markedly reduced these effects to 1.8% and 1.9%, respectively, indicating that substantial proportions of the attributed variances came from characteristics of the individuals living in the neighbourhood contexts rather than from context-specific factors. In stark contrast, the family context proved to be highly influential, accounting for 30.1% and 22.8%, respectively, of the variances in violent criminality and substance abuse in the adjusted models.

The measure of neighbourhood deprivation was associated with the outcomes of both violent criminality

Table 1 Neighbourhood characteristics for the first, fifth, and tenth deciles of neighbourhood deprivation scores averaged over the study period (1990–2004)

	1 st decile	5 th decile	10 th decile
% Immigrants	10.07%	13.68%	52.97%
% Unemployed	4.35%	8.39%	17.11%
% Divorced	7.22%	12.06%	20.59%
% Welfare recipients	1.17%	3.55%	28.72%
% Primary school only	9.58%	16.43%	32.95%
Residential mobility rate	8.80	15.59	22.59
Crime rate	0.35	0.38	0.90
Median monthly income (in USD, 2012 values)	\$3776	\$2684	\$1801

and substance misuse in the total population sample (Table 4). An increase of 1 SD in the neighbourhood-deprivation score was associated with a 57% increase in the odds of being convicted of a violent offence. When we adjusted for observed confounders, the association was considerably attenuated (OR: 1.09; 95% CI: 1.06–1.12). In the final step, we adjusted for *unobserved* confounders within nuclear families and the association disappeared (OR 0.96; 95% CI 0.83–1.11). To obtain converging evidence about the validity of our results, we additionally studied the association within extended families among biological full cousins ($N = 169\,254$), and found that the results remained intact (OR 1.03; 95% CI 0.93–1.13).

An increase of 1 SD in the neighbourhood-deprivation score was associated with a 31% increase in the odds of engaging in substance misuse. The association disappeared, however, in the adjusted model (OR 0.98; 95% CI 0.96–1.01), indicating that the effect of the contextual exposure was confounded by family-level SES.

Discussion

In accord with the findings in previous studies,^{9,13} we found both general and the specific neighbourhood effects on violent criminal behavior and substance misuse; however, by using clustered data for both families and neighbourhoods, we were also able to show that the general neighbourhood effects and the specific effects of neighbourhood deprivation on these two outcomes in the study population were ultimately confounded by familial factors.

The divergent results of our study and those of prior research, primarily conducted in the United States, could be explained by at least two important factors. First, as previously argued by Brännström,¹⁰ the relatively modest neighbourhood differences found in Sweden could be due to the country's comprehensive welfare-state programs, which aim to actively

Table 2 Descriptive data for the full and siblings samples of residents of the three largest cities in Sweden born from 1975–1989 and followed through ages 15–20 years with respect to convictions for violent crimes and substance misuse

	Full sample <i>N</i> = 297 752				Siblings sample only <i>N</i> = 172 525			
	<i>N</i>	%	Violent crime %	Substance misuse %	<i>N</i>	%	Violent crime %	Substance misuse %
Total	297 752	100	2.37	4.04	172 525	100	2.07	3.59
Birth year								
1975–1979	93 892	31.53	2.51	3.10	46 535	26.97	2.18	2.69
1980–1984	92 411	31.04	2.39	3.90	64 097	37.15	2.04	3.40
1985–1989	111 449	37.43	2.24	4.96	61 893	35.87	2.01	4.48
Sex								
Female	145 280	48.79	0.66	2.86	83 779	48.56	0.56	2.52
Male	152 472	51.21	4.01	5.17	88 746	51.44	3.49	4.60
Immigrant descendant								
No	245 287	82.38	1.99	3.90	143 698	83.29	1.68	3.49
Yes	52 465	17.62	4.19	4.71	28 827	16.71	4.01	4.08
Birth order								
First born	132 488	44.50	2.18	3.84	65 390	37.90	1.68	2.71
Subsequent birth	165 264	55.50	2.53	4.20	107 135	62.10	2.30	4.13
Small for gestational age								
No	287 330	96.50	2.36	4.02	167 341	97.00	2.06	3.58
Yes	10 422	3.50	2.64	2.59	5184	3.00	2.28	4.09
Large for gestational age								
No	290 291	97.49	2.37	4.05	167 975	97.36	2.06	3.60
Yes	7461	2.51	2.40	3.71	4550	2.64	2.31	3.36
Compulsory school grade								
Q1 (low)	77 828	26.14	6.93	8.83	44 750	25.94	6.12	7.89
Q2	71 856	24.13	1.60	3.46	43 807	25.39	1.31	3.05
Q3	75 114	25.23	0.53	2.26	43 280	25.09	0.45	1.92
Q4 (high)	72 954	24.50	0.17	1.35	40 688	23.58	0.14	1.23
Parental disposable income								
Q1 (low)	74 440	25.00	4.49	6.36	43 135	25.00	4.04	5.46
Q2	74 441	25.00	2.67	4.11	43 133	25.00	2.15	3.47
Q3	74 435	25.00	1.49	2.97	43 127	25.00	1.30	2.76
Q4 (high)	74 436	25.00	0.85	2.73	43 130	25.00	0.78	2.68
Parental education								
Primary school	105 238	35.34	4.08	5.36	56 511	32.76	3.73	4.79
Secondary school	99 893	33.55	1.86	3.67	57 192	33.15	1.65	3.33
Post-secondary school	92 621	31.11	0.99	2.95	58 822	34.09	0.87	2.70
Parental welfare reciprocity								
No	270 880	90.98	1.87	3.55	160 006	92.74	1.68	3.25
Yes	26 872	9.02	7.41	8.98	12 519	7.26	7.04	7.96
Single mother								
No	213 359	71.66	1.68	3.06	133 963	77.65	1.55	2.89
Yes	84 393	28.34	4.13	6.52	38 562	22.35	3.87	6.05

(continued)

Table 2 Continued

	Full sample <i>N</i> = 297 752				Siblings sample only <i>N</i> = 172 525			
	<i>N</i>	%	Violent crime %	Substance misuse %	<i>N</i>	%	Violent crime %	Substance misuse %
Residential mobility								
Low	296 168	99.47	2.36	4.02	171 857	99.61	2.06	3.59
High	1584	0.53	5.87	7.39	668	0.39	4.49	5.54
Parental convictions								
No	170 947	57.41	1.31	2.81	104 341	60.48	1.17	2.70
Yes	126 805	42.59	3.81	5.70	68 184	39.52	3.43	4.96
Parental substance misuse								
No	252 859	84.92	1.79	3.39	151 604	87.87	1.63	3.16
Yes	44 893	15.08	5.66	7.73	20 921	12.13	5.25	6.75
Neighbourhood deprivation								
Q1 (low)	29 799	10.01	0.81	2.57	17 258	10.00	0.78	2.51
Q2	29 794	10.01	1.20	2.79	17 252	10.00	1.08	2.61
Q3	29 766	10.00	1.36	2.97	17 254	10.00	1.08	2.60
Q4	29 779	10.00	1.53	3.46	17 256	10.00	1.08	3.08
Q5	29 750	9.99	1.77	3.75	17 243	9.99	1.58	3.48
Q6	29 768	10.00	2.06	4.24	17 243	10.00	1.58	3.59
Q7	29 781	10.00	2.51	4.23	17 259	10.00	1.91	3.86
Q8	29 771	10.00	2.92	4.83	17 248	10.00	2.49	3.99
Q9	29 803	10.01	4.13	5.87	17 248	10.02	3.44	5.14
Q10 (high)	29 741	9.99	5.46	5.73	17 248	9.97	3.44	5.09

Table 3 Intra-class correlations for violent criminality and substance misuse according to neighbourhoods and schools in the full sample of children born in Sweden from 1975–1989 and followed through ages 15–20 years, and who lived in the three largest cities of Sweden at age 15 years (*N* = 297 752). Sibling intra-class correlations in the sibling sub-sample (*N* = 172 525)

	Violent crime ICC (95% CI)	Substance misuse ICC (95% CI)
Crude		
Neighbourhoods	0.122 (0.105–0.143)	0.042 (0.034–0.052)
Schools	0.130 (0.106–0.157)	0.048 (0.037–0.061)
Siblings	0.412 (0.384–0.442)	0.290 (0.264–0.318)
Adjusted^a		
Neighbourhoods	0.018 (0.011–0.029)	0.019 (0.014–0.027)
Schools	0.024 (0.017–0.036)	0.025 (0.019–0.034)
Siblings	0.301 (0.264–0.340)	0.228 (0.200–0.259)

^aConfounders were adjusted for sex, birth year, birth order, small or large for gestational age, immigrant descent, primary school grade-point average, parental income, welfare reciprocity, education level, single-parent household, residential mobility, and parental criminal convictions and parental substance misuse. ICC, intra-class correlation.

Table 4 Relative risks (odds ratios) for violent criminality and substance misuse among all children born in Sweden from 1975–1989 and followed through ages 15–20 years, and who lived in the three largest cities of Sweden at age 15 years according to neighbourhood deprivation (*N* = 297 752). Within-sibling analyses included all biological siblings in the sample (*N* = 172 525)

	Violent crime OR (95% CI)	Substance misuse OR (95% CI)
Neighbourhood deprivation		
Crude effect	1.57 (1.52–1.63)	1.31 (1.28–1.35)
Adjusted ^a	1.09 (1.06–1.12)	0.98 (0.96–1.01)
Within-sibling adjusted	0.96 (0.83–1.11)	1.05 (0.93–1.19)

Notes: The generalized linear mixed effects modeling took into account the cross-classified clustering of individuals in siblings, schools and neighbourhoods.

^aConfounders were adjusted for sex, birth year, birth order, small or large for gestational age, immigrant descent, primary school grade point average, parental income, welfare reciprocity, education level, single-parent household, residential mobility and parental criminal convictions and substance misuse.

diminish social inequalities; we found in the current study that Swedish urban neighbourhoods (as defined by SAMS areas) contribute to less than 2% of the variance in violent criminality and substance misuse. Second, previous (quasi-)experimental studies have been considerably smaller than our study in terms of sample size, as well as having been focused on less severe outcomes and lacking objective data with which to capture neighbourhood characteristics.²⁹

Several limitations of our study might need consideration when interpreting its results. Although conviction data are a comprehensive measure of more serious adolescent criminality, they do not capture less serious offending. We find it unlikely, however, that the relative importance of structural and family risk factors for minor offences would differ markedly from the results presented here. Second, one could argue that official statistics for crime partly reflect policing practices, with the targeting of individuals of lower SES resulting in a greater risk of conviction for individuals of lower SES than for those of higher SES.⁵⁶ We tried hard to counteract such bias by controlling for a wide range of indicators of SES on individual and contextual levels. Third, although we were able to adjust for school clustering, we had no access to indicators of school quality. Fourth, the multi-level models used in our study assume no correlation between the fixed and random effects that we included, nor between the random effects.⁵⁷ Therefore, we re-ran our models using the fixed-effects, 'multi-way', cluster-robust sandwich estimator approach, and obtained very similar results (Supplementary Table 1, available as Supplementary data at *IJE* online). Fifth, measuring neighbourhood membership and deprivation at a single point could lead to attenuation bias,^{28,58} but in most cases, neighbourhoods are stable. In a sub-sample of study participants born between 1980 and 1989, we found that the correlation between neighbourhood-deprivation scores at the age of 15 years and the same correlation between the ages of 10 and 15 years was 0.96 (95% CI 0.96–0.96). Moreover, a recent Swedish study examining the effects of residential mobility on mortality concluded that multiple cluster measurements added very little in terms of explained variance.⁵⁹ Given the large sample size in our study and the stability of our estimates over a number of model specifications, we consider the risks of these biases to be minimal. Sixth, endogeneity is a form of selection bias that arises in situations in which individuals can to some degree choose their exposures (i.e. what

neighbourhoods to live in).⁶⁰ Unlike standard multi-level studies, our extended quasi-experimental approach allowed us to explicitly take into account unobserved familial factors that explain this element of choice. Hence, we have minimized the risk of finding endogenous effects. Lastly, the sibling-comparison design of our study makes a number of important assumptions (e.g. that exposed siblings do not influence their unexposed siblings, that differentially exposed siblings are generalizable to the population and that siblings share their environment).^{26,61,62} Our results were similar when we studied differentially exposed full cousins suggesting that the results were not due to sibling effects.

This large, well-controlled, multi-level, population-based study highlights the peril of excluding important cross-nested contexts when attempting to study neighbourhood effects on human behavior. Future research might benefit from addressing the relative importance of familial and peer effects in adolescent criminality and substance misuse. In conclusion, our data suggest that the notorious heterogeneity in crime and substance misuse rates across different residential areas in Sweden is not caused by neighbourhood deprivation *per se*. Instead, it seems that there are selection processes at work that lead high-risk individuals into socio-economically deprived neighbourhoods as a factor predisposing to criminality and substance misuse. This implies that interventional efforts might be improved by addressing vulnerabilities shared within families rather than within neighbourhoods.

Supplementary Data

Supplementary data are available at *IJE* online.

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KEY MESSAGES

- The causal nature of neighbourhood effects on violent criminality and substance misuse has not been established.
- We found that the variation in adolescent violent criminality and substance misuse that is attributed to the neighbourhood context in Sweden is marginal.

- The effect of neighbourhood deprivation on violent criminality and substance misuse was found to be confounded by observed and unobserved family and individual level factors.
- Our findings indicate that there is a selection of high risk individuals into socioeconomically deprived areas.

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