



Long-term effects of neighbourhood deprivation on diabetes risk: quasi-experimental evidence from a refugee dispersal policy in Sweden

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Summary

Background Although studies have shown associations between neighbourhood quality and chronic disease outcomes, such associations are potentially confounded by the selection of different types of people into different neighbourhood environments. We sought to identify the causal effects of neighbourhood deprivation on type 2 diabetes risk, by comparing refugees in Sweden who were actively dispersed by government policy to low-deprivation, moderate-deprivation, or high-deprivation neighbourhoods.

Methods In this quasi-experimental study, we analysed national register data for refugees who arrived in Sweden aged 25–50 years, at a time when the government policy involved quasi-random dispersal of refugees to neighbourhoods with different levels of poverty and unemployment, schooling, and social welfare participation. Individuals in our sample were assigned to a neighbourhood categorised as high deprivation (≥ 1 SD above the mean), moderate deprivation (within 1 SD of the mean), or low deprivation (≥ 1 SD below the mean). The primary outcome was new diagnosis of type 2 diabetes between Jan 1, 2002, and Dec 31, 2010. We used multivariate logistic and linear regressions to assess the effects of neighbourhood deprivation on diabetes risk, controlling for potential confounders affecting neighbourhood assignment and assessing effects of cumulative exposure to different neighbourhood conditions.

Findings We included data for 61386 refugees who arrived in Sweden during 1987–91 and who were assigned to one of 4833 neighbourhoods. Being assigned to an area deemed high deprivation versus low deprivation was associated with an increased risk of diabetes (odds ratio [OR] 1.22, 95% CI 1.07–1.38; $p=0.001$). In analyses that included fixed effects for assigned municipality, the increased diabetes risk was estimated to be 0.85 percentage points (95% CI –0.030 to 1.728; $p=0.058$). Neighbourhood effects grew over time such that 5 years of additional exposure to high-deprivation versus low-deprivation neighbourhoods was associated with a 9% increase in diabetes risk.

Interpretation This study makes use of a pre-existing governmental natural experiment to show that neighbourhood deprivation increased the risk of diabetes in refugees in Sweden. This finding has heightened importance in the context of the current refugee crisis in Europe.

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Introduction

Studies have shown that adverse neighbourhood socioeconomic characteristics are associated with worse chronic disease outcomes.¹ Of note, findings of several studies have shown a strong association between disadvantaged neighbourhood environments and the incidence of insulin resistance and type 2 diabetes.^{2–4} Potential mediating pathways include reduced employment and income opportunities resulting in the purchase of cheap, unhealthy food; reduced psychosocial resources resulting in high chronic stress; and poor food availability and walkability.⁵ Unfortunately, most studies of neighbourhoods and health have shown correlations and were unable to account for several methodological limitations such as selection bias (eg, less healthy individuals might move to more disadvantaged neighbourhoods), confounding by unobserved individual factors (eg, family socioeconomic status), and an inability to separate individual-level and area-level factors.^{6–8} The

only randomised trial done so far is the Moving to Opportunity experiment,⁹ in which individuals in five US cities were randomly assigned to a group offered a housing voucher that could only be used to move to a low-poverty neighbourhood, a group offered a traditional Section 8 housing voucher, or a control group. Investigators noted a small but statistically significant reduction in the prevalence of obesity and type 2 diabetes in the group who received a housing voucher to move to a low-poverty neighbourhood.⁹ Moving to Opportunity has been criticised on methodological grounds, such as residual selection bias and limited generalisability.^{10,11}

In this study, we overcome persistent challenges in the scientific literature on neighbourhood effects by means of a novel quasi-experiment. We took advantage of a policy in Sweden during 1985–94 that assigned incoming refugees in a quasi-random fashion to neighbourhoods throughout the country. The policy's goal was to ease labour market conditions in heavily settled areas and

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Research in context

Evidence before this study

We searched PubMed for studies of neighbourhood characteristics and type 2 diabetes with the following search terms: “diabetes” and “neighborhoods” or “neighbourhoods.” We restricted our search to articles published in English before Nov 31, 2015. We identified more than 12 observational studies that investigated the association between neighbourhood characteristics and the prevalence or incidence of type 2 diabetes. Most findings showed an association between increased diabetes risk and various measures of neighbourhood deprivation. However, we found only one randomised trial, the Moving to Opportunity experiment. In this trial, investigators noted that women whose family received a voucher to relocate to a low-deprivation neighbourhood had a 4 percentage point reduction in their likelihood of having an HbA_{1c} concentration of 6.5% or higher at 10–15 years of follow-up. We did not find any quasi-experimental evidence on this topic. In our search of the broader scientific literature on neighbourhood health effects, we found no other randomised trials and one quasi-experimental study, which noted no association between neighbourhood-level income inequality and risk of being admitted to hospital for any cause.

Added value of this study

In this study, we took advantage of a unique quasi-experimental policy to assess the causal association between neighbourhood

deprivation and type 2 diabetes risk. To our knowledge, this is the first quasi-experiment including neighbourhood deprivation and any health outcome, building on existing correlational and experimental evidence. Our follow-up of more than 20 years is longer than the Moving to Opportunity experiment and most observational studies of neighbourhoods and type 2 diabetes. Our results suggest that exposure to neighbourhood deprivation increased diabetes risk in our sample.

Implications of all the available evidence

Combined with existing evidence, our data suggest that the association between neighbourhood deprivation and type 2 diabetes risk is not driven solely by selection of families into neighbourhoods or other confounding factors. Neighbourhood environments have a causal effect on diabetes risk, which accumulates over time. Policy efforts to reduce area-level socioeconomic disparities might contribute to lowering the risk of type 2 diabetes. The focus of our study is the effects of neighbourhoods on refugees who arrived in Sweden 25–30 years ago. Although policy makers should be cautious about generalisability, these findings nevertheless have important implications for the unprecedented current movement of refugees and migrants to Europe.

promote better integration of refugees. The dispersal policy was most strictly applied from 1987–91,¹² creating a unique study group that enabled investigation of how neighbourhood environment affects the onset of type 2 diabetes. As a result of the dispersal policy, refugees were spread more evenly across the country during this period than before its implementation (figure 1). Several studies have assessed the effect of European refugee dispersal policies on labour markets, crime, and educational outcomes.^{12–14} Only one has assessed health outcomes, and found that neighbourhood income inequality was not associated with risk of being admitted to hospital.¹⁵ A significant contribution of the present study is the follow-up of more than 20 years, longer than existing experimental or quasi-experimental studies of neighbourhoods and health.

Neighbourhood effects are especially important for vulnerable populations, such as immigrants. Studies have consistently shown that immigrants to Sweden, as elsewhere, have an increased risk of diabetes and mortality compared with the native population, and higher incident diabetes after migration from a lower-income country.^{3,16,17} Moreover, incidence might be higher in refugees than in immigrants more generally.¹⁸ In the past 2 years, the European Union has experienced the largest inflow of asylum seekers since the end of World War 2.¹⁹ The current period of immigration includes mostly individuals from the Middle East and north Africa, where prevalence

of type 2 diabetes is the highest of any region worldwide: 11% in adults aged 20–79 years in 2013.²⁰ Although policy makers have focused on the economic, political, and cultural implications of integrating many immigrants into a country, mass migration also has ramifications for the health and welfare of the newly settled immigrants. Therefore, we aimed to test the hypothesis that neighbourhood deprivation affects the development of type 2 diabetes in order to gain crucial insight that could guide the design of community-level interventions to affect the long-term health of refugees in Europe.

Methods

Data sources

We drew on data from several national registers on the entire Swedish population, including immigrants (table 1). We linked the datasets by use of personal identification numbers assigned to all permanent residents in Sweden. Individual-level sociodemographic characteristics and aggregated statistics representing the neighbourhood-level variables were available in the total population register. Health outcome data, including the date of each health-care encounter, were available in the inpatient register (1987–2010), outpatient register (2001–10), and prescription drug register (2005–10). These data were collected from roughly 11.8 million individuals and were more than 99% complete for all Swedish residents.

Sample selection

Our analytic sample consisted of immigrants who obtained a Swedish residence permit during 1987–91, originated from a refugee-sending country (ie, a non-member country of the Organisation for Economic Co-operation and Development in 1985), and were aged 25–50 years at the time of entry into Sweden.¹³ Here, we refer to immigrants as any foreign-born person and refugees as the subset of immigrants who were forced to flee from persecution, and entitled to protections under international law.

Summary of Swedish dispersal policy

During 1985–94, the Swedish immigration board assigned refugees arriving in Sweden to neighbourhoods across the country. By 1989, 277 (98%) of 284 municipalities were participating in the programme. All refugees, except those reuniting with family members living in Sweden or those having the financial resources to support themselves, were subjected to the dispersal policy. Our study focuses on the period 1987–91, when the dispersal policy was strictly followed. From 1987–91, about 90% of incoming refugees were assigned to an initial municipality according to official reports, suggesting high participation in the placement programme.¹² During this time, Sweden experienced a large influx of refugees (figure 2). The Middle East was the most common region of origin for refugees in our sample (table 2), resembling the situation during the current period of immigration and making this study relevant to the present refugee crisis.²¹

The immigration board assigned refugees to one of the refugee centres distributed nationwide, where individuals awaited a response on whether a residential permit had been approved. The typical duration spent in the refugee centres was 3–12 months.²² Once a refugee received a residential permit, placement officers assigned the person to a place of residence. Placement officers did not have any direct interaction with refugees, meaning that any selection bias must have been the result of observed characteristics available to the officers in the application: refugees' language, length of formal schooling, and family size.¹² After refugees were placed in an initial residence, the immigration board offered Swedish language and training courses and social welfare support for about 18 months (depending on municipality and year). There were no restrictions on mobility if refugees found a residence independently, and receipt of welfare was not conditional on remaining in the assigned residence. Therefore, our study design is akin to a randomised encouragement design in which participants are randomly encouraged to be exposed to a given neighbourhood, and represents a lower bound on the health effects of neighbourhood deprivation. In this study, we estimated the effect of initial quasi-random neighbourhood placement. Therefore, we did not

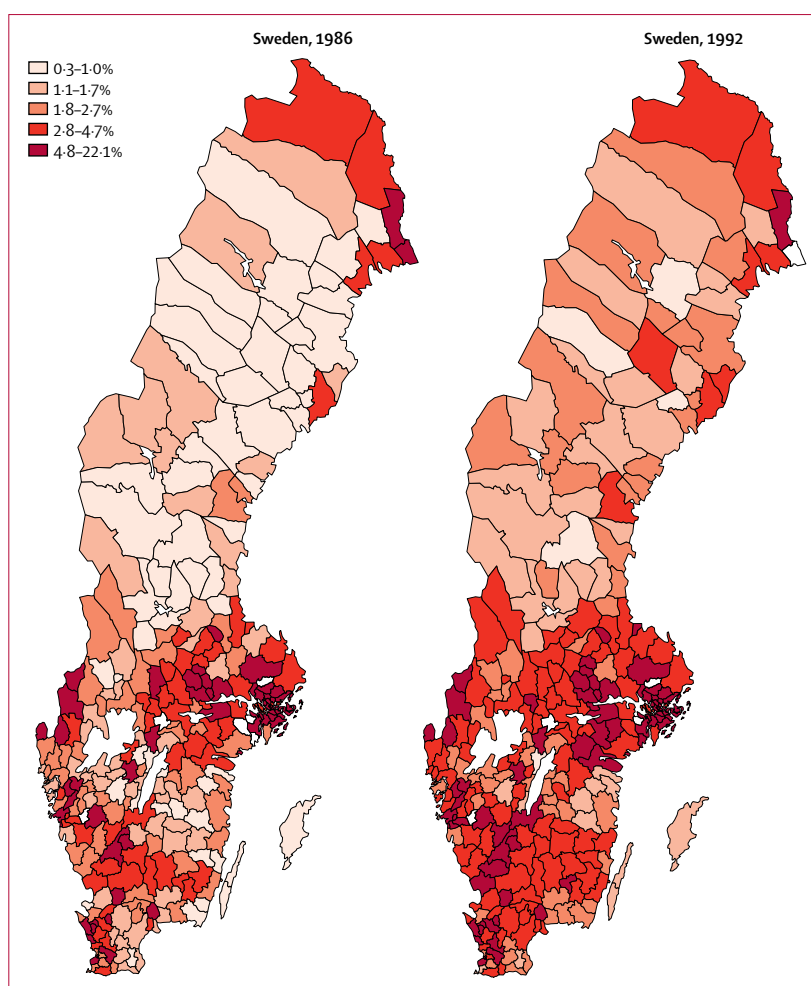


Figure 1: Proportion of immigrants by municipality in the years before and after the study period
Municipalities were classified into quintiles at baseline to create categories. The policy was strictly enforced during 1987–91. Data from Statistics Sweden.

estimate the subsequent effects of relocation because these are probably subject to selection bias.

From 1987–91, a thriving housing market in Sweden made it difficult for incoming refugees to find a residence in a desirable area.¹² Consequently, placement officers based their assignment even more strictly on housing vacancies. Refugees had limited ability to influence their initial place of residence during this period. Here, therefore, we assume that refugee placement during this narrower timeframe of 1987–91 was essentially random, conditional on the initially assigned municipality and recorded factors available to placement officers, all of which are available in Swedish register data. This is a strategy employed in, and supported by, previous research.^{12,13,23} In essence, there were no unobserved factors that could have affected neighbourhood placement, overcoming the challenge of confounding present in the existing literature on neighbourhood effects.

For the Statistics Sweden data see <http://www.statistikdatabasen.scb.se/pxweb/en/ssd/>

	Years	Variables
Total population register	1987–91	Age, sex, marital status, family size, region of residence, year of arrival, region of origin, and neighbourhood deprivation
Inpatient hospital register	1987–2010	Diagnostic codes and dates
Outpatient register	2001–10	Diagnostic codes and dates
Prescription drug register	2005–10	Prescriptions filled of insulin (analogues) and antidiabetic agents

Inpatient register data before 2001 were only used to exclude pre-existing cases of type 2 diabetes.

Table 1: Data sources

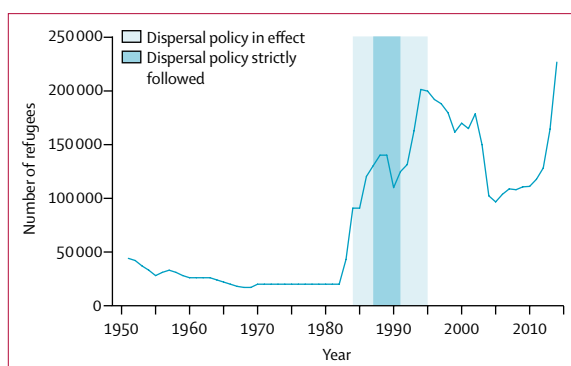


Figure 2: Number of new refugees in Sweden by year, 1951–2014
Includes refugees, asylum seekers, internally displaced people, and stateless people. Data from the United Nations High Commissioner for Refugees (UNHCR) Population Statistics Database.

For the Population Statistics Database see <http://popstats.unhcr.org>

Study variables

The primary outcome was diagnosis of type 2 diabetes between Jan 1, 2002, and Dec 31, 2010, the years in which outcome data were available. Cases were identified in the inpatient and outpatient registers from 2002–10 according to International Statistical Classification of Diseases (ICD)-10 codes E11–14 and in the prescription drug register from 2005–10 according to individuals who were prescribed or filled a prescription of insulin, insulin analogues, or oral antidiabetic drugs (anatomical therapeutic chemical codes A10B and A10X). The prescription drug register alone captures 90% of all diabetic cases in Sweden.^{24,25} Findings of validation studies have also shown that Sweden's clinical registers have broad coverage and are of high quality.²⁶

We excluded individuals diagnosed with other types of diabetes, including type 1 diabetes (ICD-10 codes E8–10). To capture only incident disease, we used inpatient register data dating back to 1987 to exclude individuals who were diagnosed with any form of diabetes (ICD-10 codes E8–14) within 5 years of arrival in Sweden because these individuals probably had pre-existing clinical or pre-clinical disease. For example, if a refugee arrived in 1987, he or she would not be considered an incident case if diagnosed with diabetes during 1987–92.

We defined neighbourhoods on the basis of small-area market statistics (SAMS) that use boundaries established by homogeneous building types (eg, high-rise buildings). SAMS have been a common neighbourhood definition in previous studies in Sweden.²⁷ The average population in each SAMS was about 2000 residents in Stockholm and 1000 residents elsewhere in Sweden.¹⁵ After excluding areas with fewer than 50 residents because of unstable statistical estimates, our analysis included individuals who were initially assigned to 4833 different SAMS.

We created a summary measure to characterise neighbourhood deprivation in 1987, following the approach adopted by previous Swedish studies.^{3,28} Specifically, we used a principal components analysis that included four variables for all residents aged 25–64 years in each SAMS, each measured in percentages in the year of initial placement: low education status (<10 years of formal schooling), low income (<50% of individual median income from all sources), unemployment (not employed [excluding full-time students, military, and retirees]), and social welfare assistance. We calculated a Z score for every SAMS, weighted by the coefficients on the eigenvectors. We then classified neighbourhoods into three categories, established in previous studies: low (≥ 1 SD below the mean), moderate (within 1 SD of the mean), and high (≥ 1 SD above the mean).²⁸ We also considered a second measure that trichotomises the component score into tertiles to achieve an equal number of individuals from the study population in each category.

Covariates

To improve the validity and precision of our estimates, our analyses adjusted for several individual characteristics measured in the year of initial placement. These were: 5-year age categories, sex, educational attainment, marital status, region of initial placement, family size, and region of origin. We also included indicator variables for year of arrival to adjust for secular trends.

Statistical analyses

We assumed that initial neighbourhood assignment within a municipality was quasi-random, conditional on all recorded factors available to placement officers: refugees' language, formal schooling, and family size. This assumption is not directly testable, although studies have shown increased dispersal of refugees during the policy period in comparison with before, with no evidence of sorting by ethnic group, and covariate balance by neighbourhood type.^{12,13,23} We also investigated the balance of covariates by neighbourhood deprivation level as a guide to the validity of this assumption. Next we did unadjusted logistic regressions to establish the association between neighbourhood deprivation and diabetes risk. We then adjusted for individual-level baseline characteristics, including the factors that might have affected the initial placement assignment by officials.

We then used linear probability models (ordinary least squares) to more precisely estimate the causal effect of neighbourhood deprivation on diabetes risk. To do so, we included fixed effects (ie, indicator variables) for assigned municipality, which adjusted for all time-invariant factors at the municipality level. Thus, we identified the neighbourhood effects using socioeconomic variation across neighbourhoods within a municipality. In other words, we considered the difference in outcomes for refugees placed in a given municipality who were assigned to a high-deprivation neighbourhood as opposed to a low-deprivation neighbourhood. We chose a fixed-effects approach over a multilevel modelling approach because we are substantively interested in identifying a causal effect, and multilevel models do not do as well as fixed-effects models in controlling for omitted variables bias.²⁹ Finally, to analyse how the effects of exposure to neighbourhood deprivation accumulated over time, we estimated the covariate-adjusted ordinary least squares models for outcomes reported in every year from 2002–10.

To account for correlated outcomes in individuals assigned to the same municipality, robust standard errors in all models were clustered by municipality (n=288). Analyses were intention to treat and included all refugees who arrived in Sweden from 1987–91. Individuals who emigrated from Sweden before follow-up were classified as non-cases.

Role of the funding source

The funders of the study had no role in study design, data collection, data analysis, data interpretation, or writing of the report. The corresponding author had final responsibility for the decision to submit for publication.

Results

In this quasi-experiment, 61386 individuals met the selection criteria (figure 3). Table 2 shows the characteristics of the sample at the time of arrival in Sweden. Many refugees in the sample were initially assigned to deprived neighbourhoods (45% to a moderate-deprivation area and 47% to a high-deprivation area), which shows that housing was most readily available in more deprived areas during the arrival period. About two-thirds of the sample was younger than 35 years. About three-quarters were married, and 83% had children. Most refugees were settled in the three largest cities. The refugees arrived from a diverse set of geographic regions, with almost half from Iran and the Middle East or north Africa.

Table 2 shows the balance of baseline characteristics by neighbourhood deprivation level. The placement officers only had information about refugees' language, length of formal schooling, and family size, so we expected these variables to be taken into account by placement officers and therefore adjusted for these recorded characteristics in our analyses. Most baseline characteristics did not vary

	Population	Percentage by deprivation		
		Low	Moderate	High
Age (years)				
25–29	23 925 (39%)	37.8	39.0	39.2
30–34	17 351 (28%)	28.8	28.2	28.2
35–39	10 724 (18%)	17.5	17.8	17.2
40–44	5721 (9%)	9.9	9.4	9.1
45–50	3665 (6%)	6.0	5.6	6.3
Men	28 686 (47%)	48.6	52.9	54.4
Educational attainment				
≤9 years	15 640 (26%)	17.9	25.2	27.0
10–12 years	5519 (9%)	8.0	9.7	8.5
>12 years	14 582 (24%)	29.5	24.6	21.9
Unknown	25 645 (42%)	44.6	40.5	42.6
Married or cohabitating	45 601 (74%)	72.2	73.5	75.4
Family size (number of children)				
0	10 347 (17%)	21.9	17.6	15.3
1	10 060 (16%)	20.3	16.3	15.8
2	18 553 (30%)	32.2	30.1	30.1
3	11 247 (18%)	13.4	18.3	19.1
≥4	11 179 (18%)	12.2	17.8	19.7
Region of residence				
Southern Sweden, large cities*	35 992 (59%)	73.5	53.1	61.4
Southern Sweden, other	19 130 (31%)	15.7	33.2	31.8
Northern Sweden	6264 (10%)	10.8	13.6	6.8
Year of arrival				
1987	9462 (15%)	21.9	17.6	15.3
1988	11 436 (19%)	20.3	16.3	15.8
1989	14 806 (24%)	32.2	30.1	30.1
1990	13 120 (21%)	13.4	18.3	19.1
1991	12 562 (21%)	12.2	17.8	19.7
Region of origin				
Iran	13 456 (22%)	18.0	22.3	22.2
Middle East or northern Africa	14 199 (23%)	16.1	20.3	27.1
Other Africa	5954 (10%)	9.7	9.9	9.5
Asia	7596 (12%)	20.1	12.4	11.0
Eastern Europe	11 783 (19%)	20.9	19.8	18.3
Latin America	8398 (14%)	15.2	15.2	11.9
Number of observations	61 386	4815	27 786	28 785
Cumulative diabetes events	4553	281	1994	2278
Cumulative diabetes incidence	7.4%	5.8%	7.2%	7.9%

*Large cities are Stockholm, Gothenburg, and Malmö.

Table 2: Baseline characteristics, overall and by level of neighbourhood deprivation

greatly by neighbourhood deprivation level. This supports our assumption that initial neighbourhood assignment within a municipality is quasi-random conditional on observed variables. Any other imbalance in unobserved factors should be the result of random chance, as in a randomised study. The cumulative incidence of type 2 diabetes at follow-up was 7.4% in our sample. Table 2 shows a deprivation–diabetes gradient, with cumulative incidence of diabetes increasing by deprivation level. By

comparison, overall diabetes prevalence in Sweden was estimated to be 4–6% during the same time period.^{30,31}

To further analyse the association between type 2 diabetes prevalence and neighbourhood deprivation, we estimated two sets of logistic regression models (figure 4). In unadjusted models, being assigned to a moderate-deprivation or high-deprivation SAMS increased the odds of type 2 diabetes diagnosis by 25% or 39%, relative to a low-deprivation SAMS. In adjusted models controlling for covariates, an increase remained although it was attenuated: 15% for moderate-deprivation and 22% for high-deprivation areas. We also stratified our adjusted model by region of origin, but did not detect significant differences (appendix).

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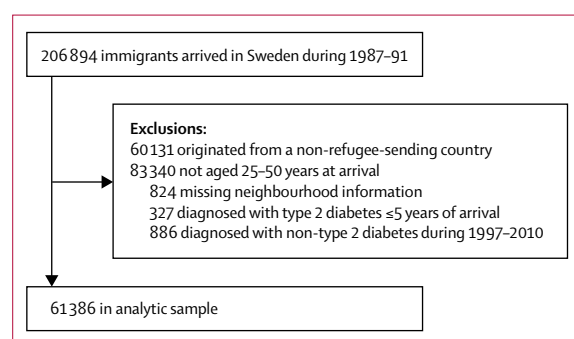


Figure 3: Flow chart of sample construction

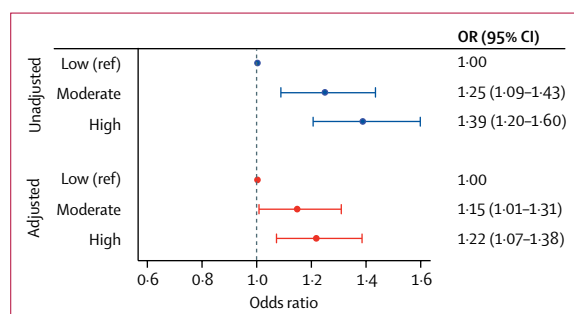


Figure 4: Odds ratios of diabetes risk by deprivation level

Odds ratios were derived from logistic regression models. The adjusted model includes 5-year age categories, sex, educational attainment, marital status, region of initial placement, family size, region of origin, and year of arrival as covariates. Robust standard errors are clustered by municipality.

We also tested a tertile measure of neighbourhood deprivation and excluded individuals who emigrated from Sweden before follow-up, each resulting in similar findings (data not shown).

In the second half of the analysis, we estimated linear probability models with fixed effects for initial municipality (table 3). As already described, the municipality fixed effects accounted for any non-random sorting that occurred in which refugees were assigned to certain municipalities or different municipal refugee centres while awaiting residential permits. Diabetes risk increased for refugees initially assigned to a high-deprivation versus low-deprivation area by 0.8–1.7 percentage points, depending on adjustment for covariates. This translates to a 15–30% increase in diabetes risk. Coefficients for moderate deprivation were not significant in these models, although they did suggest higher diabetes risk than in low-deprivation areas. The coefficients were less precisely estimated because of added parameters for the 288 municipality fixed effects.

Diabetes risk accumulated over time, rather than occurring immediately after arrival (figure 5). 5 years of additional exposure to high-deprivation versus low-deprivation neighbourhoods was associated with a 9% increase in diabetes risk.

Discussion

Our findings show that refugees initially assigned to highly deprived neighbourhoods had a 1.7 percentage point increased risk of developing type 2 diabetes. Covariate adjustment attenuated the magnitude of the association to 0.8 percentage points. This represents a relative increase of 15–30%. The accumulation of this effect over time is consistent with hypothesised mediating pathways, such as chronic stress, employment status, income opportunities, and food environment, all of which might take years to affect health. Moreover, the results suggest a gradient, such that high-deprivation areas have worse outcomes than moderate-deprivation areas. Without the policy of random placement, more refugees would have probably settled in areas with high proportions of immigrants, which have higher deprivation levels on average. Therefore, this policy probably reduced the prevalence of diabetes in refugees in this sample.

A strength of our study design is that we make use of an existing policy experiment. Our estimates are not subject to selection bias and unobserved confounding that have afflicted the neighbourhood effects literature. One previous study³ that used traditional correlational methods estimated the association of neighbourhoods with diabetes risk in the full population in Sweden (odds ratio [OR] 1.28, 95% CI 1.23–1.35) and in moderate-deprivation and high-deprivation areas (1.67, 1.57–1.77). The corresponding estimates in our study were 1.15 (95% CI 1.01–1.31) and 1.22 (1.07–1.38). Although the study populations differ, this comparison highlights the importance of gathering additional experimental and

	Model 1 (unadjusted)	Model 2 (adjusted)
Moderate	0.724 (−0.14 to 1.593)	0.437 (−0.420 to 1.295)
High	1.734* (0.846 to 2.622)	0.849† (−0.030 to 1.728)
Initial municipality fixed effects	Yes	Yes
Control variables	No	Yes

Data are linear probability models of neighborhood deprivation on diabetes with low deprivation neighbourhood as the reference and are percentage point changes in diabetes risk (95% CI). *p<0.01. †p<0.10. Model 2 adjusts for 5-year age categories, sex, educational attainment, marital status, region of initial placement, family size, region of origin, and year of arrival.

Table 3: Effect of neighbourhood deprivation on type 2 diabetes risk

quasi-experimental evidence on neighbourhood effects to reduce potential bias.

Our estimates are likely to be a lower bound on the true effects of neighbourhoods on diabetes risk. The dispersal policy did not restrict mobility away from initial neighbourhood assignment. If refugees moved to wealthier areas, it could undo some or all of the negative effects of being assigned to a deprived neighbourhood. In this respect, the dispersal policy is analogous to a randomised encouragement design in which participants are randomly encouraged or not encouraged to be exposed to a deprived neighbourhood, a situation in which non-adherence can have a role.³² In our study, about half of the initial sample left their assigned municipality after 10 years, which probably weakened the estimated effect size. However, even in the presence of high relocation rates, our findings showed a long-term effect on refugee health. In our sample, initial neighbourhood deprivation level was highly correlated with the deprivation level in the neighbourhood of residence after 5, 10, and 15 years (appendix), suggesting that initial assignment had long-term consequences on residential decisions and might be a mediating pathway through which initial neighbourhood placement affected diabetes risk.

This study has several limitations. First, register data do not suggest which immigrants were refugees who would have been subject to this policy. Instead, we followed previous research in which the sample was identified based on the year and region of origin.^{2,13} Government statistics show that most immigrants to Sweden during the study period were refugees (47%) or those moving to rejoin family members (ie, tied movers, 48%), compared with labour immigrants (1%), guest students (2%), or adopted children (3%). Adopted children and many students did not meet our age inclusion criteria, and tied movers did not generally arrive from refugee-sending countries. Consequently, most of our sample consisted of refugees, and misclassification was probably rare. Second, Sweden provided quite comprehensive social welfare assistance to refugees; neighbourhoods possibly exert a stronger effect in settings when social support is more limited. It is striking that even in a context renowned for its strong public safety net, we still recorded large neighbourhood effects. However, as a result, our analyses might not generalise to other periods of immigration or to other countries. Third, we excluded areas with fewer than 50 residents from our sample because of unstable estimates, so our results might not be generalisable to small communities. Fourth, we do not have data for the length of time that refugees spent in refugee centres before being assigned housing by a placement officer; those with longer wait times might have been more likely to find housing independently, which could bias the results. However, compliance with the policy in the whole sample was above 90%. Fifth, we did not adjust

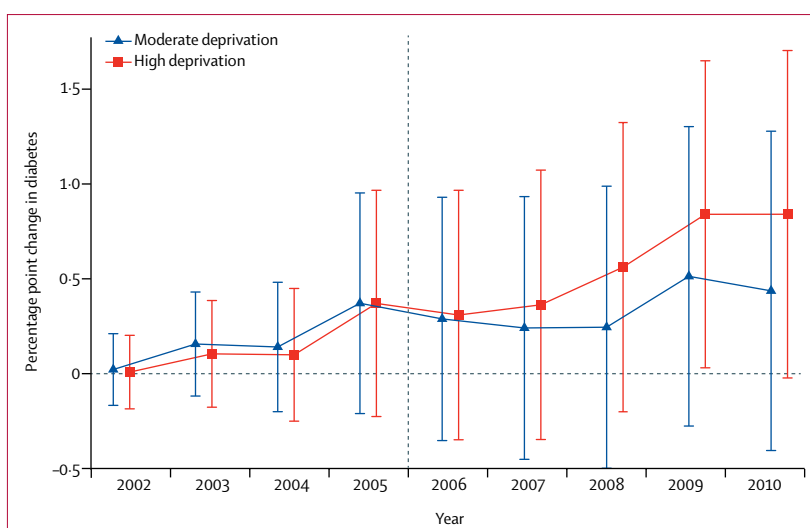


Figure 5: Cumulative effect of neighbourhood deprivation on diabetes risk over time

Marginal effects on diabetes risk of neighbourhood deprivation relative to that in low-deprivation areas, derived from separate yearly regression models corresponding to model 2 in table 3. The dotted line indicates the point at which the prescription drug register became available for identifying people with diabetes.

for neighbourhood-level differences in access to care, the presence of which would bias our estimates toward the null if health-care utilisation is worse in more deprived neighbourhoods. Still, this bias is probably negligible because findings have shown that access to care in Sweden was no worse for immigrants from refugee-sending countries during the study period.³³ Sixth, we relied solely on the inpatient register to exclude pre-existing cases of type 2 diabetes during the first years after immigration because the outpatient register was not established until 2001. This method could have caused us to miss less severe cases. Nonetheless, as long as these cases are randomly distributed across neighbourhoods, as we would expect, this would not bias our estimates.

Our study has direct relevance to the ongoing period of immigration to Europe. Because of the historically high numbers of incoming refugees, combined with already high unemployment rates, the new entrants are encountering less hospitable political and social environments. Our data suggest that decisions affecting the settlement and integration of immigrants can have long-term consequences for the health of the new arrivals. Refugees are among the most vulnerable populations in any society, and as such deserve special attention from governments in creating policies that protect and promote their health. Further investigation of the pathways through which neighbourhoods affect the risk of diabetes and other diseases might shed light on how best to buffer immigrants against the consequences of neighbourhood deprivation. Future studies should also consider the effects of these factors on other outcomes, such as mental health, which might be adversely affected by dispersal policies of this nature.

Contributors

JSW, RH, and SB conceived the study. JSW, RH, SB, JS, and KS designed the study. XL did the data analysis. JSW, RH, SB, JS, and KS interpreted the data. JSW wrote the initial manuscript, and RH, SB, HO, JS, and KS provided comments on the draft.

Declaration of interests

We declare no competing interests.

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Neighbourhood of residence and the risk of type 2 diabetes

In high-income countries, people living in deprived neighbourhoods are at increased risk of type 2 diabetes and related chronic diseases at both population¹ and individual levels.² However, there remain challenges to fully understanding the basis and mechanisms of the increased risk at a neighbourhood level. How to exclude selection bias as a contributor to the association—with the possibility of less healthy individuals moving to more deprived areas—is one such challenge. This type of bias can be notoriously difficult to identify and control for in observational studies,³ and a randomised study design is rarely possible for studying the effect of neighbourhoods on health. However, one relevant randomised study does exist, the Move to Opportunity study,⁴ in which women with children living in deprived areas in the USA were randomly assigned an opportunity to move to a less deprived area. Follow-up 10 to 15 years later found that those assigned to a less deprived area had a lower risk of obesity ($\text{BMI} \geq 35 \text{ kg/m}^2$) and type 2 diabetes ($\text{HbA}_{1c} \geq 6.5\%$).

In *The Lancet Diabetes & Endocrinology*, Justin White and colleagues⁵ provide further evidence for a causal relationship between the socioeconomic characteristics of neighbourhoods of residence and the subsequent risk of developing type 2 diabetes. The researchers studied the incidence of type 2 diabetes in refugees arriving in Sweden between 1987 and 1992. At this time, the national dispersal policy dictated that arriving refugees were allocated in a pseudo-random way to neighbourhoods of all deprivation levels. This policy enabled the investigators to undertake a quasi-experimental study⁶ of the association between allocation to neighbourhoods with different levels of deprivation and the risk of developing type 2 diabetes during the next 20 years (follow-up was until 2010). New diagnoses of type 2 diabetes were detected through linkage to inpatient, outpatient, and prescription drug registers. Principal components analysis was used to classify almost 5000 neighbourhoods into low, medium, and high levels of deprivation. More than 61 000 adults aged 25–50 years were dispersed across these three levels. To ensure that only new cases of type 2 diabetes were captured, any refugee identified as having diabetes within 5 years of arrival was excluded. Over the follow-up, 7.4% (4553 new cases) of the population developed

type 2 diabetes. Using a logistic regression adjusted for differences in baseline characteristics and for clustering of refugees within municipality, the researchers reported that individuals allocated to the most deprived areas had an odds ratio for developing type 2 diabetes of 1.22 (95% CI 1.07–1.38) compared with the least deprived. In a fixed-effects regression model, which better controls for unmeasured confounders than random effects,⁷ the researchers estimated that those in the most deprived areas had a 0.85 percent point (95% CI –0.03 to 1.73) higher cumulative incidence of type 2 diabetes compared with the least deprived, and difference in risk between the least and most deprived areas increased over time.

The interpretation of these findings is dependent on the comparability of the individuals allocated to the three different levels of deprivation. The placement officers had no direct interaction with the refugees, and only had three pieces of information about them: language, formal schooling, and family size. It seems that these characteristics might have affected the placement of refugees, with those allocated to the most deprived neighbourhoods more likely to have had a larger family size, fewer years of formal education, and been from the middle east and North Africa than those allocated to neighbourhoods with low levels of deprivation. The validity of the pseudo-randomisation process is conditional on adjustment, undertaken by the investigators, for the characteristics known to the placement officers. Additional adjustment for known baseline characteristics, including municipality in which they were placed (adjusted for age, sex, educational attainment, marital status, and family size), add to the plausibility that differences in the cumulative incidence of diabetes are causally related to where the individuals were initially placed. Assuming a causal relationship, this study probably underestimates its true size, because the analysis is based on where participants were initially assigned and does not take into account subsequent movement. About half of refugees had moved from their assigned municipality after 10 years. The overall effect of this movement was that about half remained in neighbourhoods of the same deprivation level, and roughly a quarter moved to more deprived, and a quarter to less deprived areas.



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The findings from this study suggest that where people live affects their risk of developing type 2 diabetes, and that by extension this affects the development of related chronic diseases. These results are consistent with those from the Move to Opportunity⁴ study done in the USA, which included people from a different population using a different welfare system. These two studies were both done in adults; the long-term effects of neighbourhood deprivation in children might be even greater.⁸ Although White and colleagues' study clarifies little about the mechanisms of the increased risk associated with moving to a deprived area, it emphasises the need to understand them to inform preventive interventions. The findings also support the notion that the most effective approaches to prevention will entail addressing both neighbourhood and individual level factors.⁹

In discussing the findings of this study, it is too easy to forget the extreme suffering and loss experienced by so many of the participants. Martin Luther King once wrote, "a great nation is a compassionate nation". Today, there is too little evidence of greatness in most of Europe, as it stutters a response to the challenge of the largest movement of refugees since World War 2.¹⁰ This study is a reminder that where people are allowed or forced to settle will have consequences for their health and wellbeing for years to come.

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