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Are Neighborhood Health Associations Causal? A 10-Year Prospective Cohort Study With Repeated Measurements

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People who live in disadvantaged neighborhoods tend to have poor physical and mental health, but this might be due to selective residential mobility rather than causal neighborhood effects. As a test of social causation, I examined whether persons were less healthy when they were living in disadvantaged neighborhoods than at other times when they were living in more advantaged neighborhoods. Data were taken from the 10-year Household, Income and Labour Dynamics in Australia (HILDA) prospective cohort study, which had annual follow-up waves between 2001 and 2010 ($n = 112{,}503$ person-observations from 20,012 persons). Neighborhood disadvantage was associated with poorer self-rated health, mental health, and physical functioning, higher probability of smoking, and less frequent physical activity. However, these associations were almost completely due to between-person differences; the associations were not replicated in within-person analyses that compared the same persons living in different neighborhoods over time. Results were similar when using neighborhood remoteness as the exposure and when focusing only on long-term residence. In contrast, poor health predicted selective residential mobility to less advantaged neighborhoods, which provided evidence of social selection. These findings provide little support for social causation in neighborhood health associations and suggest that correlations between neighborhoods and health may develop via selective residential mobility.

depression; fixed-effects regression; longitudinal; neighborhood; panel study; self-rated health

Abbreviation: SLA, statistical local area.

Editor's note: *An invited commentary on this article appears on page 000, and the authors' response appears on page 000.*

Several studies on neighborhood effects have shown that people's mental and physical health correlate with neighborhood characteristics, such as socioeconomic status, crime rate, or ethnic composition of the residential area (1–6). Most of these studies are based on cross-sectional data (1, 2, 7), and the methodological problem of selection bias in identifying causal neighborhood effects has long been acknowledged (8–11). In terms of causal inference, people may not be exchangeable between neighborhoods (12), and neighborhood characteristics may not be exogenous exposure variables. People's health may therefore correlate with neighborhood characteristics because people with better health are more able to move to more affluent neighborhoods than are those with poorer health and not because neighborhood qualities have a causal impact on health (13). If this were the case, neighborhood associations could be considered as the "neighborhood consequences" of social inequalities that determine people's ability to move to desired locations.

Multilevel studies have attempted to identify the separate neighborhood and individual associations with health outcomes (14), but these studies can only demonstrate that a certain neighborhood characteristic is associated with individual health when taking into account specific individual traits. Causal interpretations are hampered by the difficulty of correctly adjusting for relevant person-level confounders while not overadjusting for individual characteristics that mediate the neighborhood associations (8, 11, 15, 16). Social experiments have provided some evidence of causal neighborhood effects (17). For example, in the Moving to Opportunity randomized community study, the subjective wellbeing of persons who moved away from poor neighborhoods remained modestly improved several years after moving compared with that of controls (18). The generalizability of these results to natural settings of the general population is unknown. Longitudinal data from observational studies can help to determine the temporal order between neighborhood exposure and health outcomes. However, the problem of selection bias is not directly addressed by longitudinal studies of disease incidence (19, 20) or by prospective follow-up studies with only 1 baseline measurement of neighborhood exposure (21, 22) because people may get selected to different neighborhoods on the basis of long-term latent disease incidence risk and health trajectories related to aging.

Evidence of a causal association between neighborhood and health could be strengthened considerably by demonstrating in observational studies that people's health changes as they move across different neighborhoods. However, very few studies have used longitudinal data with repeated measurements of both neighborhoods and health to examine such within-person changes. A 6-year longitudinal study with repeated measurements showed that urban sprawl was associated with a higher prevalence of obesity, but this association was not observed with first-difference regression of persons who moved to less or more densely populated areas, suggesting that the association was not causal (23). Similarly, data from a Swedish study in which siblings were compared did not support a causal interpretation of the association between neighborhood deprivation and the rate of violent crime (24).

In the present study, I examined social causation and social selection in neighborhood health associations in a large Australian prospective cohort study that had a 10-year follow-up with annual repeated measurements (25). Social causation was assessed using fixed-effect regression, which is based on within-person variation in the exposure and thereby removes confounding caused by stable differences between persons. This provides a strong test for causality in neighborhood associations in observational settings. Social selection was examined by using health status and behaviors to predict subsequent moves to more or less disadvantaged (or remote) neighborhoods.

METHODS

Participants

The Household, Income and Labour Dynamics in Australia (HILDA) Survey is an annual household-based panel study developed to collect information about economic and subjective wellbeing, labor market dynamics, and family dynamics (25). The survey began in 2001 with a large national probability sample of Australian households that occupied private dwellings ($n = 7{,}682$ households with 19,914 persons at baseline). All members of the households who provided at least 1 interview in wave 1 formed the basis of the longitudinal panel to be pursued in each subsequent wave. The sample has been gradually extended to include any new household members that resulted from changes in the composition of the original households. Through wave 10, which was carried out in 2010, a total of 28,547 persons had participated in at least 1 study wave.

The present study included all available person-observations from participants for whom data on all study variables in at least 1 study wave were available. The final sample included 112,503 person-observations from 20,012 unique persons across the 10-year follow-up period (an average of 4.4 (standard deviation, 1.7) person-observations per participant). There were no appropriate longitudinal sampling weights for the analysis used in the present study, so all models were fitted without sampling weights.

Measures

Neighborhood characteristics were determined at the level of statistical local areas (SLA), which is the general-purpose spatial unit used to collect and disseminate statistics ($n = 1{,}353$ SLAs in 2001). In years in which a census is not conducted, the SLA is the smallest unit defined in the Australian Standard Geographical Classification (see www.abs.gov.au for details of geographic hierarchy). The median population count of SLAs was 5,908 (interquartile range, 2,743–14,517), and the median area size was 74.5 km^2 (interquartile range, 7.5–1,944.0). Web Figure 1 (available at http://aje.oxfordjournals.org/) shows the map of SLA boundaries. Household addresses of participants were geocoded at each wave, and the participants' SLAs were determined from these data.

Two neighborhood indicators were derived from 2001 census data. Neighborhood disadvantage was determined based on the decile index of relative socioeconomic advantage/disadvantage as calculated using the Socio-Economic Indexes for Areas (26) indicators. The index is a continuum of advantage to disadvantage, and it takes into account variables such as the proportion of families with high incomes, people with a tertiary education, and people employed in a skilled occupation. For the present analysis, the scale was coded so that higher scores indicated higher neighborhood disadvantage. Web Figure 1 shows the distribution of neighborhood disadvantage across Australia. At the level of SLAs, the correlation between disadvantage deciles in 2001 and 2011 was 0.89 (calculated from census data available at www.abs.gov.au), which suggests a high rank-order stability of neighborhood disadvantage over the study period. Neighborhood remoteness was measured using Accessibility/Remoteness Index of Australia scores (27). Remoteness is determined on the basis of accessibility to various services, that is, a weighted score of road distances to "service centers" with smaller and larger populations. The scale ranges from 1 for a major city (indicating relatively unrestricted access to a wide range of goods and services and to opportunities for social interaction) to 5 for a very remote/migratory area (indicating very little accessibility of goods and services and few opportunities for social interaction).

Information on health status and health behaviors was collected from participants' self-reports. Mental health and physical functioning were assessed with the Short Form-36 mental health and physical functioning composite scores (28). Self-rated health was reported on a 5-point scale (1 = poor, 5 = excellent). Smoking was coded dichotomously

(0 = nonsmoker, 1 = current smoker). Physical activity level was assessed with a question about how often the person participated in physical activity (without specifying a difference between leisure-time or nonleisure activity), with the following response options: 1= not at all (11% of all person-observations); 2 = less than once per week (15%); 3 = 1–2 times per week (24%); 4 = 3 times per week (16%); 5 = more than 3 times per week (21%); 6 = every day (13%). Alcohol consumption was determined using the question "Do you drink alcohol?" with the following response options: 1 = I have never drunk alcohol (10%); 2 = I no longer drink (6%); 3 = yes, but rarely (24%); 4 = 2–3 days per month (12%); 5 = 1–2 days per week (19%); 6 = 3–4 days per week (13%); 7 = 5–6 days per week (8%); and 8 = everyday (8%). Age, sex, and country of birth (0 = Australia, 1 = United Kingdom, 2 = other) were included as sociodemographic covariates in all models. Additional covariates included educational level (highest educational degree), income (total household income), and marital status (married or cohabiting vs. not married or cohabiting). These were assessed at each study wave concurrently with the health outcome measures and were modeled as time-varying covariates. The results of the fully adjusted models remained unchanged when the covariates were used as time-lagged (i.e., covariate measured 1 year before the health outcome measure rather than concurrently with the outcome) time-varying variables (data not shown). Table 1 shows additional descriptive statistics of the sample.

Table 1. Descriptive Statistics of the 112,503 Person-Observations From 20,012 Persons Over 10 Annual Data Collection Waves in the Household, Income, and Labour Dynamics in Australia Survey, 2001–2010

Variable[a]	Total No.	No. of Persons	%	Mean (SD)	Within-Person SD
Sex					
Men	52,707	9,614	46.8		
Women	59,796	10,398	53.2		
Age, years				43.8 (18.0)	2.6
Country of birth					
Australia	88,871	15,742	79.0		
United Kingdom	7,572	1,190	6.7		
Other	16,060	3,080	14.3		
Self-rated health[b]				3.4 (0.97)	0.52
SF-36 mental health score[c]				4.7 (2.47)	1.44
SF-36 physical functioning score[d]				3.08 (1.14)	0.6
Physical activity level[e]				4.6 (1.55)	1.01
Alcohol consumption[f]				4.34 (2.00)	0.81
Smoking status					
Nonsmoker	89,501	16,851	79.6		
Current smoker	23,002	5,932	20.5		
Neighborhood disadvantage[g]				5.46 (2.90)	1.01
Remoteness					
Major city	69,158	13,262	61.5		
Inner regional	28,115	5,726	25.0		
Outer regional	13,023	2,868	11.6		
Remote	1,794	473	1.6		
Very remote/migratory	413	114	0.4		
Neighborhood dissatisfaction[c]				2.08 (1.75)	1.18
Neighborhood problems[b]				2.68 (0.59)	0.35

Abbreviations: SD, standard deviation; SF-36, Short Form-36.

[a] For categorical variables, the values are the number of total person-observations, number of unique persons, and percentages calculated from person-observations. For continuous variables, the values are means, overall standard deviations, and within-person standard deviations.

[b] Rated on a scale of 1–5.

[c] Rated on a scale of 0–10.

[d] Rated on a scale of 1–4.

[e] Rated on a scale of 1–6.

[f] Rated on a scale of 1–8.

[g] Rated on a scale of 1–10.

To empirically test the validity of the fixed-effect regression models in the present context, we included additional outcome measures of self-reported neighborhood dissatisfaction ("How satisfied are you with the neighborhood you live in?": 0 = totally satisfied, 10 = totally dissatisfied) and perceived neighborhood problems (9 items each rated on a 5-point scale, with higher values indicating more perceived problems, such as noise, vandalism, and hostile residents). If the fixed-effect regression models can accurately capture individual variations that accompany neighborhood changes, the within-person analysis should yield support for causal neighborhood associations at least for the measures of neighborhood satisfaction and neighborhood problems, because these are expected to be sensitive to within-person changes across locations.

Statistical analysis

Associations between neighborhood characteristics and health were assessed with random-intercept multilevel models to take into account the nonindependence of repeated measurements of the same persons over time (linear regression for continuous variables and logistic regression for dichotomously coded variable of smoking). The total regression coefficient is estimated as a weighted average of both between-person and within-person variations in the exposure associated with the outcome (29). With repeated measurements, these 2 components can be estimated separately with the linear regression model $y_{it} = \alpha + \beta_{0i} + \beta_W(x_{it} - \bar{x}_{i.}) + \beta_B\bar{x}_{i.} + \varepsilon_{it}$, where α is the overall intercept, β_{0i} is the participant-specific intercept, x_{it} is the exposure variable for the ith participant at the tth measurement time of the participant, $\bar{x}_{i.}$ is the mean value of the exposure variable averaged across all measurement times separately within each participant, and ε_{it} is the error term. Then the regression coefficient β_W gives the within-person (or fixed-effect) estimate and β_B gives the between-person estimate. The difference between the total and within-person regression coefficients was tested using the Wald test (30). Robust estimation with household clustering was used in all models to account for the nonindependence of household members.

Neighborhood associations may require long exposure periods to develop, in which case short residence times only add unnecessary noise in the data. To test this, the above analyses were repeated by including only person-observations of subjects who had the same level of neighborhood disadvantage (or remoteness) in at least 3 consecutive survey years. The neighborhood associations were thus assessed only when the participant had lived 3 or more years in both the old and the new location. For example, a person who had lived in a neighborhood with a disadvantage score of 5 for 3 years and then moved to a neighborhood with a disadvantage score of 7 for 4 years would contribute 1 person-observation from the first neighborhood (the last year in that neighborhood) and 2 person-observations from the second (the 2 last years in that neighborhood). All the regression models were recalculated with this reduced data set.

Social selection in neighborhood associations arises if persons who move to more affluent neighborhoods have better health than those who move to poorer neighborhoods. To test this, logistic regressions were fitted among persons for whom the level of neighborhood disadvantage changed between 2 consecutive follow-up waves, so that each participant could contribute more than 1 person-observation to the data set. The outcome was the direction of change in neighborhood disadvantage between follow-up waves coded dichotomously as 0 (move to less disadvantaged neighborhood) or 1 (move to more disadvantaged neighborhood). To establish correct temporal ordering for social selection hypothesis, health covariates were assessed at data-cycle baselines, that is, 1 study wave before the move. Each variable was assessed in a separate model that was adjusted for sex, age, and baseline neighborhood disadvantage. The corresponding analyses were fitted for remoteness. Because the present focus was not on the associations between health and overall residential mobility, persons who remained in the same location over time (or for whom the level of neighborhood disadvantage did not change when moving) were not included in this analysis.

The Short Form-36 scales were negatively skewed. The mental health scale was transformed using cubic transformation and then divided by 100,000 to have a range between 0 and 10. The physical functioning scale was too heavily skewed to be transformed to even close to a normal distribution, so the scale was recoded into 4 categories (0–60 = 1; 60–80 = 2; 80–90 = 3; and 90–100 = 4). Using the original scale, the mean was 83.5 and standard deviations were 23.0 across all person-observations, 21.5 between persons, and 12.0 within persons. The corresponding values for the recoded variable were 3.08, 1.14, 1.03, and 0.60, respectively, indicating that the ratio of within-person variation to total variation in the recoded scale (0.60:1.14) remained very similar to the original scale (12.0:23.0). The correlations of the original and recoded scales with other covariates remained largely unchanged (data not shown).

RESULTS

Of the 20,012 participants, 4,284 (21%) lived in 2 or more different neighborhoods with different levels of disadvantage, 1,679 (8%) lived in 3, and 774 (4%) lived in 4; 2,038 (11%) lived in 2 neighborhoods with different levels of remoteness and 191 (1%) lived in 3 or more. These were the participants who contributed data to estimate the within-person associations in the fixed-effect models. Residential stability was high, as indicated by intraclass correlations of 0.85 for neighborhood disadvantage and 0.89 for remoteness. Over the 10-year follow-up, there were 11,992 moves across levels of neighborhood disadvantage between consecutive follow-up waves (6,078 moves to more disadvantaged neighborhoods and 5,914 moves to less disadvantaged neighborhoods) and 3,032 moves across levels of remoteness (1,517 moves to more remote neighborhoods and 1,515 to less remote neighborhoods). At the level of person-observations, the correlation between neighborhood disadvantage and remoteness was 0.39.

Social causation

The magnitudes of the total, between-person, and within-person regression coefficients of the multilevel models are illustrated in Figure 1 (see Web Table 1 for numerical details).

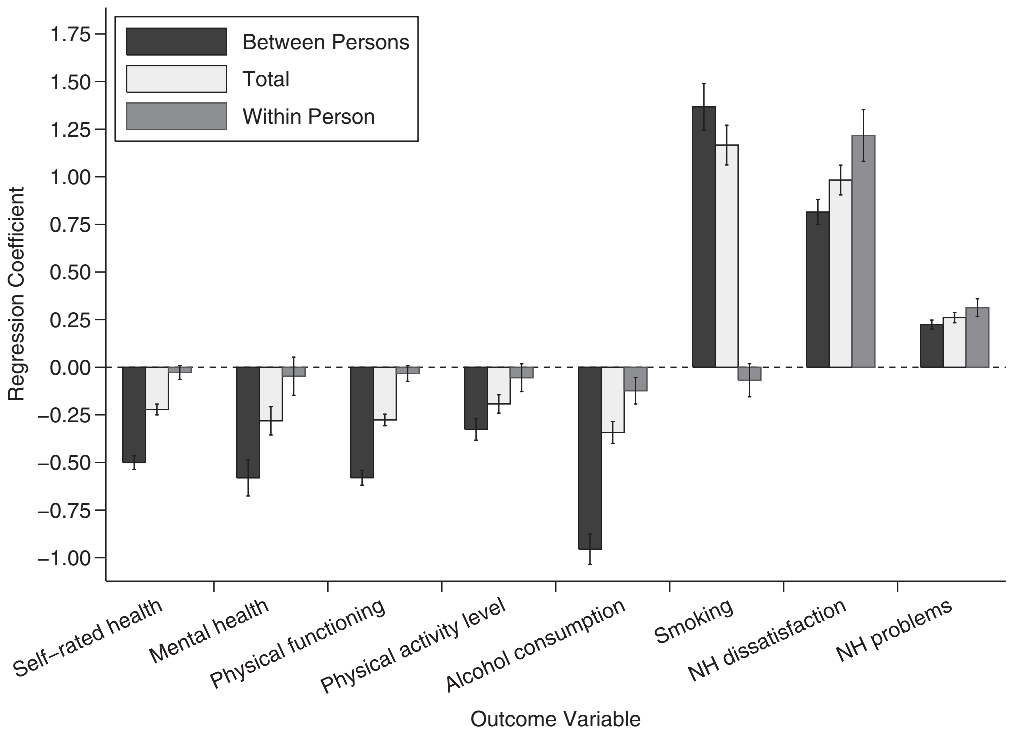

Figure 1. Associations between neighborhood (NH) disadvantage and outcome variables based on between-person (dark bars), total (light bars), and within-person (dark gray bars) regressions using 10 annual repeated measurements of neighborhood disadvantage and outcomes (112,503 person-observations from 20,012 unique persons), Household, Income, and Labour Dynamics in Australia Survey, 2001–2010. The shaded bars illustrate the magnitude of regression coefficients (linear regression coefficients for continuous outcomes and logit odds ratios for dichotomous outcomes). All differences between within-person and between-person regression coefficients were statistically significant ($P < 0.05$). See Web Table 1 for statistical details. Bars, 95% confidence intervals.

Neighborhood disadvantage was associated with poorer health and health behaviors, except for alcohol consumption, which was less frequent in neighborhoods with higher disadvantage. However, these associations were largely due to variation between persons; the within-person associations were substantially weaker and mostly statistically nonsignificant. Only the association between neighborhood disadvantage and lower alcohol consumption was observed in the within-person analysis. All the differences of within-person and between-person coefficients were statistically significant ($P < 0.05$). Including only person-observations from residences at which subjects had lived for 3 years or more strengthened some of the overall associations but did not change the conclusions on the dominating role of between-person associations over the within-person associations (Web Table 1).

Neighborhood remoteness was associated with poorer self-rated health, poorer physical functioning, and higher probability of smoking but also with better mental health and higher level of physical activity (Figure 2; Web Table 2). Again, these associations were largely between-person associations, and only the association between remoteness and higher physical activity level was replicated in the within-person analysis. For mental health, the within-person coefficient was not significantly different from the between-person coefficient ($P = 0.21$), which suggests that the total association should be taken as the most efficient estimate. Adjusting the associations of neighborhood disadvantage and remoteness for time-varying indicators of educational level, marital status, and household income attenuated most of the total and between-person associations to some degree but had negligible influence on the within-person associations (Web Tables 3 and 4).

To confirm that the lack of within-person associations for health outcomes was not due to methodological artifacts that would have precluded the demonstration of true within-person associations, the above models were fitted for neighborhood dissatisfaction and perceived neighborhood problems. Neighborhood disadvantage was associated with higher neighborhood dissatisfaction and neighborhood problems, and these associations were replicated in within-person analyses (Figure 1). The within-person associations were stronger than the total or between-person associations. Remoteness was associated with lower levels of neighborhood dissatisfaction and problems, and these associations were also replicated in the within-person analyses with stronger magnitudes compared with the total and between-person associations (Figure 2). These associations provided support for the validity of the within-person regression in the present context.

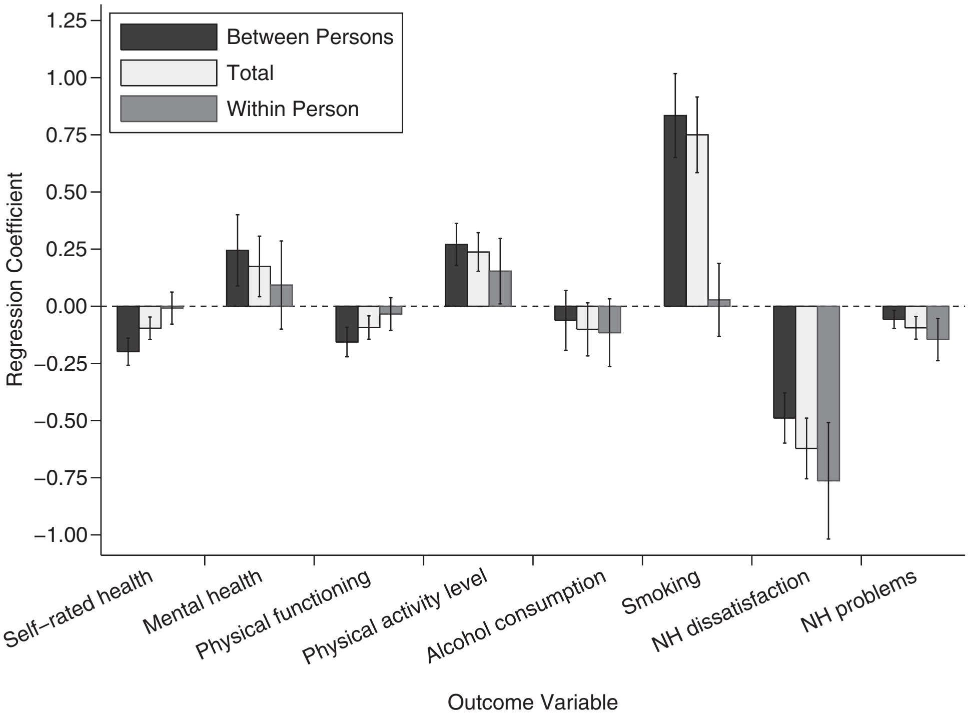

Figure 2. Associations between neighborhood (NH) remoteness and outcome variables based on between-person (dark bars), total (light bars), and within-person (dark gray bars) regressions using 10 annual repeated measurements of neighborhood disadvantage and outcomes (112,503 person-observations from 20,012 unique persons), Household, Income, and Labour Dynamics in Australia Survey, 2001–2010. The shaded bars illustrate the magnitude of the regression coefficients (linear regression coefficients for continuous outcomes and logit odds ratios for dichotomous outcomes). The difference between within-person and between-person regression coefficients was statistically significant for self-rated health, physical functioning, and smoking ($P < 0.05$). See Web Table 2 for statistical details. Bars, 95% confidence intervals.

Social selection

Compared with those who moved to less disadvantaged neighborhoods between follow-ups, persons who moved to more disadvantaged neighborhoods had poorer self-rated health, mental health, and physical functioning, had lower physical activity levels, were more likely to smoke, and were less likely to use alcohol in the study wave preceding the move (Table 2). Persons who moved to more remote neighborhoods had lower self-rated health than did those who moved to less remote neighborhoods, but no other associations with health or health behaviors were observed for remoteness.

DISCUSSION

Evidence from a 10-year prospective cohort study of more than 20,000 participants with annual repeated measurement data suggests that most of the associations between neighborhood disadvantage and health outcomes represent differences between persons rather than dynamic processes within persons. People living in disadvantaged neighborhoods of Australia had poorer mental and physical health than did those living in advantaged neighborhoods. However, a person was not markedly healthier when living in an advantaged neighborhood than when living in a disadvantaged neighborhood at a different time. The results were more heterogeneous for neighborhood remoteness, but between-person associations were nevertheless more important than within-person associations. These findings provide little support for social causation as the explanation for associations between neighborhood characteristics and health outcomes.

The large number of participants and person-observations from 10 measurement times afforded a large sample size to estimate the within-person regressions with sufficient precision, so their interpretation was not hampered by the wide confidence intervals that are often encountered in fixed-effects models. Neighborhood characteristics were assessed with 2 different variables based on objective measures of the person's residential location defined with the accuracy of SLAs (roughly comparable to census tracts in the United States), which provided relatively detailed data on participants' residential locations. Health status and health behaviors were assessed using multiple measures, with converging results. The lack of within-person associations in health outcomes was unlikely to be a methodological artifact, as the validity of fixed-effect regression analysis was supported by within-person associations observed for neighborhood dissatisfaction and perceived neighborhood problems. Despite these strengths, the results need to be interpreted taking into

Table 2. Associations Between Baseline Covariates and Subsequent Moves to Neighborhoods With Higher Disadvantage (Model 1) or Higher Remoteness (Model 2) in the Household, Income, and Labour Dynamics in Australia Survey, 2001–2010

Predictor Variable[a]	Outcome			
	Neighborhood Disadvantage[b]		Neighborhood Remoteness[c]	
	OR	95% CI	OR	95% CI
Self-rated health	0.85	0.82, 0.89	0.88	0.78, 0.99
Mental health	0.96	0.94, 0.97	0.98	0.94, 1.03
Physical functioning	0.84	0.81, 0.88	0.99	0.88, 1.11
Physical activity level	0.97	0.94, 0.99	1.02	0.95, 1.09
Alcohol consumption	0.95	0.93, 0.97	0.99	0.93, 1.05
Smoking	1.67	1.52, 1.82	1.17	0.92, 1.48
Neighborhood dissatisfaction	0.96	0.94, 0.98	1.05	1.00, 1.10
Neighborhood problems	0.90	0.83, 0.98	1.28	1.03, 1.61

Abbreviations: CI, confidence interval; OR, odds ratio.

[a] All associations are from logistic regression models fitted separately for each predictor variable and outcome and adjusted for sex, age, and baseline neighborhood disadvantage (or remoteness).

[b] Outcome was coded as 0 (move to neighborhood with lower disadvantage) or 1 (move to neighborhood with higher disadvantage); $n = 11{,}992$ person-observations from 6,737 participants who moved to different neighborhoods.

[c] Outcome was coded as 0 (move to less remote neighborhood) or 1 (move to more remote neighborhood); $n = 3{,}032$ person-observations from 2,229 participants who moved to different neighborhoods.

account some methodological limitations. The analysis was restricted only to one country, so it is uncertain how the findings from Australia generalize to other countries in which neighborhood influences and patterns of selective residential mobility may be different. All health information was based on self-reported data, so potential reporting biases related to neighborhood characteristics might have confounded the results.

In contrast to the limited evidence for within-person neighborhood associations, the alternative hypothesis of social selection received some support in the case of neighborhood disadvantage but not in the case of neighborhood remoteness. Compared with those who moved to more advantaged neighborhoods, people who moved to more disadvantaged neighborhoods had poorer mental and physical health, were more likely to smoke, and were less physically active. Thus, at least some of the neighborhood correlations with health may be the consequences of selective residential mobility (31, 32). This may be mediated by direct mechanisms related to health (e.g., poor health making it more difficult to move) and by indirect mechanisms of sociodemographic factors correlated with health (e.g., more educated persons having better health and being more likely to move to advantaged neighborhoods).

The drift of persons to neighborhoods that match their health status may influence the development of health differentials between residential areas—people create neighborhoods. Gentrification and "white flight" are examples of person-level selective processes that modify neighborhoods (33). Similarly, neighborhood health differentials may represent the downstream consequences of the more fundamental causes of health inequalities (34), that is, socioeconomic resources that determine people's ability to select residential locations. These processes may even extend to intergenerational continuities, as recent studies have suggested that there is moderate stability in residential characteristics between parents and their children (35, 36). The present results do not yet tell to what extent social selection may help to explain the emergence of health variations across neighborhoods (37). The overall impact of selective mobility depends not only on the magnitude of associations between health and residential mobility but also on specific migration patterns, such as total numbers of people migrating between areas. More detailed spatial modeling is needed to evaluate the plausible long-term associations of selective residential mobility on neighborhood health differences (38).

The present findings call into question the causal interpretation of neighborhood effects in health outcomes. However, the results do not necessarily imply that all neighborhood associations identified in previous studies are not causal. First, the present sample included mostly adults, and neighborhood associations may have a different impact on children and adolescents (6). Some of the noncausal associations might reflect long-term intergenerational continuities in neighborhood disadvantage and poor health that originate in childhood, in which case time-varying associations in adulthood would not be expected (36). Second, neighborhood associations on other outcomes besides health (e.g., criminal behavior or school performance) may be causal even if associations with health and health behaviors are not (39). Third, the assessment of causality in neighborhood associations may depend on various methodological choices, such as the specific measures of neighborhood qualities, measures of health outcomes, geographical level of analysis, and country-specific factors (3, 40–43) that need to be examined in more detail.

Reviews of studies of neighborhoods and health (4, 10, 15) have repeatedly emphasized the problem of deriving causal inferences from cross-sectional studies that tend to dominate the neighborhood research literature (1, 2). Despite the crucial importance of this methodological problem, surprisingly few studies have used longitudinal data and appropriate panel-study methods to assess whether people's health varies as they move across different neighborhoods. The present findings from fixed-effects regressions suggest that neighborhood associations do not operate within persons but rather reflect stable differences between persons who live in different neighborhoods. This is in contrast to what one would expect if neighborhood disadvantage were causing poor individual health. Future studies of neighborhood associations need to consider more carefully the role of selective residential mobility as a potential mechanism causing geographic health inequalities.

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Invited Commentary

Invited Commentary: Repeated Measures, Selection Bias, and Effect Identification in Neighborhood Effect Studies

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Research on neighborhood effects faces enormous methodological challenges, with selection bias being near the top of the list. In this issue of the *Journal* (*Am J Epidemiol*. 2014;000(00):0000–0000), Professor Jokela addresses this issue with novel repeated measures data and models that decompose putative effects into those within and between persons. His contribution shows that within-person neighborhood effects are quite modest and that there is evidence of selection bias between persons. Like all research, the work rests on assumptions. Unfortunately, such assumptions are difficult to substantiate or validate in this context. A consequentialist epidemiologic perspective compels further innovation and a larger social epidemiologic imagination.

causal; counterfactual; dynamic; methodology

Professor Jokela's new article (1) is a thoughtful and important contribution to the social epidemiologic literature addressing neighborhood effects. The research uses rich repeated-measures data, defensible neighborhood quality measures, reasonable health measures, and an interesting set of analyses aimed at illuminating the problem of social selection, which has vexed researchers for many years.

Jokela's analyses are based on the idea that persons who move to different neighborhoods are exposed to new neighborhood environments, be they better or worse. Obviously there may be lateral moves, which is to say moves in which the new neighborhood environment is much like the original neighborhood environment. In fact, lateral moves are probably the norm. In any case, Jokela's is a within-person design; persons serve as their own counterfactuals when exposed to different neighborhood environments. The large number of people analyzed serve as replicates and thus increase precision of between-person averages. As usual, the questions are how different neighborhood environments impact health and to what extent better or worse health compels one to move to a better or worse neighborhood. To answer this, Jokela relies primarily on fixed-effect models of within-person change to decompose effect estimates into within-person and between-person associations.

In simplest terms, Jokela's analyses suggest that people's health influences their choice of neighborhood and that neighborhood correlations with health are likely due to between-person differences and related sorting by socioeconomic and health status, not necessarily neighborhood environment impact per se. In other words, Jokela's work implies that many prior estimates are biased and that neighborhoods may have less impact on health than previously thought. One might quibble with his data, measures, or model, but the results appear as robust as almost any.

We should not be surprised by Jokela's results. To the contrary, finding either a strong association of neighborhoods with health or no association of health with neighborhood selection in a within-person design would have been surprising. Here are some reasons why.

First, it seems that few people (to be more accurate, few families/households) make dramatic moves from one kind of neighborhood environment to another. Though no direct data are presented, I would be surprised to learn that many people moved to substantially more or less advantaged places in any given discrete move. Such moves often require an exogenous shock, like an unexpected infusion of resources from, say, an insurance settlement, or an unexpected illness without a sufficient safety net. Further, dramatic moves require imagination and a desire for a life-altering change (e.g., moving for new job). Ongoing research seems to show that it can be difficult for disadvantaged persons to imagine dramatic moves because they too often feel helpless in this regard and have too

		Time 1		
		Poor	Good	Excellent
Time 0	Poor	*N*	*N*	*N*
	Good	*N*	*N*	*N*
	Excellent	*N*	*N*	*N*

Figure 1. Example of simple transition proportion table by neighborhood time period and coarse gradients of desirable neighborhood environments. Persons on the diagonals are non-movers for the observed time period. N, sample size.

few reference examples upon which to draw, to say nothing of the many necessarily binding social relationships that are costly to alter. As a result, most moves appear lateral or nearly so. Accordingly, there is little "dose," and we should not expect large within-person associations with health.

To clarify some of these issues, it seems worth suggesting that researchers of neighborhood effects publish simple transition proportion/probability tables, such as in Figure 1. This simple cross-tabulation, with sample sizes of *N* in each cell, holds a great deal of meaningful information. The off-diagonal cells are of great interest, especially in the corners. How do people end up in such cells? Is it through divorce, a cancer diagnosis, or winning the lottery? What can be done to facilitate upward moves or mitigate such downward moves? Is there a linear dose-response relationship as we move off the diagonal? When people do move to better places, what becomes of those left behind?

Second, the persons in Jokela's data who moved did so more or less voluntarily. That is, they were presumably not forced to move at gunpoint or by some other disturbing threat. Obviously, getting sick or losing a job and having to relocate is not desired, but the choice as to where to relocate remains at least partially under a person's control. Thus, subtle if not latent characteristics or values of people who move help determine subsequent neighborhood environments. This is selection within a person/household, and it may not be time invariant. In fact, it is probably time and context dependent and thus violates assumptions in Jokela's model. Metaphorically, the problem is akin to people choosing their own diets to lose weight. If a repeated-measures study shows little impact of such diets on the dieters who chose them, should we discount the efficacy of such diets, or would it be better to know the results of an experimental study that randomized people to such diets?

Third, although it is a meaningful advance, the exposure timeframe in Jokela's data is just 10 years at maximum. Except for rare cases of a move to an acutely toxic or idyllic environment, it is hard to imagine that temporally short exposures would have large influences on health measures. My suspicion is that, save for the rare cases, neighborhood environments have subtle impacts on most people's outlook and health, and these take a long time to accumulate. An environmental change may be enjoyable or salubrious, but the corresponding difficulties of navigating a new area and social context may mute gains. On the other hand, self-reported health measures would probably be affected sooner rather than later. Additionally, Jokela creatively examined neighborhood satisfaction measures, which correlated as theory predicts.

What does Jokela's study mean for the problem of social selection in neighborhood effects research? Among the paper's contribution is that, given assumptions about sufficient change in neighborhood environment, control of time- and context-dependent effects, and sufficient exposure times (to name but a few variables), there is evidence to suggest that people are moving to different neighborhoods because of their health. In other words, the paper suggests selection bias is important and probably undermines many previously published parameter estimates. In fact, some might say that bias is so extensive as to undermine the notion that neighborhood contexts impact health more generally.

Yet, even though I appreciate Jokela's findings, I remain steadfast in believing that neighborhood contexts affect health above and beyond the characteristics of any given person. Imagine a newborn baby growing up with the same family in either a good or bad neighborhood. It seems to be common sense that exposure to the good neighborhood would be lead to better health outcomes, all else being equal.

The trouble is one of effect identification, the teasing out or disentangling of unbiased effects in a system of dynamic feedback loops and dependent accumulative effects. As I wrote 10 years ago (2), it is hard to imagine any observational design-solving identification problems in neighborhood effects research. On the other hand, subsequent experimental designs entailing exogenous relocation, such as Move to Opportunity, clearly reveal practical obstacles of perturbing the social system's equilibrium. Efforts to exogenously change (i.e., improve) neighborhoods in some sort of community-randomized trial have faced similar political, cultural, and financial obstacles. However, such research difficulties do not mean that the impact of neighborhoods on health is negligible. Rather, they mean that the research question is difficult and that we may not ever get a precise unbiased estimate of a neighborhood's true impact. Some questions are just not answerable (3).

What should be done? A consequentialist perspective (4) compels us to redirect our collective energy and resources. Perhaps it is time to address the impact of larger phenomena, such as culture (5), religion, or the processed food industry; or, going the other way, we may need study the impact of the families/household or loving fathers on health. For those wishing to stay focused on neighborhood effects, (experimental) research into specific policy-relevant changes of neighborhood environments would be most helpful. In any case, it seems high time to expand the social epidemiologic imagination.

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Response to Invited Commentary

Jokela Responds to "Repeated Measures and Effect Identification"

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I thank Dr. Oakes for his insightful commentary (1) on my study of neighborhood effects and health (2). I can agree with almost all of his comments on the methodological strengths and limitations of the fixed-effects regression approach. The results of the present current study imply that many previous studies have probably overestimated the causal role of neighborhoods in adult mental and physical health. However, the fixed-effect analysis cannot refute the more general hypothesis that neighborhoods might affect people's health somehow. There may be critical periods (3), cumulative life-course effects (4), or specific environmental risk factors that influence only specific health conditions, such as respiratory diseases (5), although the causality of these effects may be difficult to demonstrate empirically.

Oakes correctly points out that people tend to move between similar neighborhoods, so the range of differential neighborhood exposure in the within-person analysis may not be sufficient to demonstrate neighborhood effects. Table 1 shows the number of moves by neighborhood socioeconomic status of the origin and destination neighborhoods in the present study. Although the number of moves decreased as the difference between neighborhoods' socioeconomic statuses increased, this decrease was not particularly dramatic within approximately 3 socioeconomic deciles of the origin neighborhood, especially for the average neighborhoods. Moreover, including only participants who moved across neighborhoods with a difference of larger than 1, 2, or 3 socioeconomic deciles (in 3 separate sets of analyses) did not change the

Table 1. Number of Moves Across Neighborhoods in Different Deciles of Socioeconomic Status[a] Over Consecutive Study Waves, Household, Income, and Labour Dynamics in Australia Survey, 2001–2010

Socioeconomic Status Decile in Wave 2	Socioeconomic Status Decile in Wave 1									
	1	2	3	4	5	6	7	8	9	10
1	8,916	372	167	109	76	46	40	37	24	26
2	377	8,852	224	139	88	85	84	80	42	33
3	191	232	8,065	221	150	138	90	101	76	51
4	130	195	197	7,808	168	178	117	106	114	75
5	78	142	145	171	6,883	178	198	159	109	97
6	74	84	120	153	153	7,945	164	231	186	137
7	54	75	105	117	154	161	7,641	225	185	139
8	55	53	75	100	144	177	222	8,581	323	204
9	28	53	58	83	108	137	169	312	8,652	286
10	19	45	56	75	80	115	141	241	260	7,156
Total no.	9,922	10,103	9,212	8,976	8,004	9,160	8,866	10,073	9,971	8,204
No. of moves[b]	1,006	1,251	1,147	1,168	1,121	1,215	1,225	1,492	1,319	1,048

[a] Decile 1 is the lowest and decile 10 is the highest.
[b] No. of moves to neighborhoods with different levels of disadvantage.

conclusions of the main analyses (data not shown). Thus, it seems that a "lack of dose" in neighborhood differences was unlikely to bias the analysis towards the null.

Regarding the second point that Oakes raised, most of the residential moves in the study were probably more or less voluntary, and this could have introduced confounding in the within-person associations. People might move to a wealthier area when, for example, they get promoted at work, and the increased socioeconomic status might influence their health and health behaviors as well, independently of the neighborhood change. In the hypothetical example of diet and body weight described by Oakes, I would assume that a fixed-effect analysis would indeed show a within-person association between these variables—suggesting causality—but this association could be confounded by time-varying individual behaviors, such as other lifestyle changes in the same healthy direction. However, in the present study, there were no within-individual neighborhood associations to begin with. Here, the time-varying confounders would have had to suppress the true causal effects of neighborhood changes. This seems less likely than time-varying confounders accounting for any initially observed within-person associations, because social factors determining health and neighborhood choices are likely to act in the same direction of healthy or unhealthy change.

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Are neighborhood health associations causal? A 10-year prospective cohort study with repeated measurements

Online Supplementary Material

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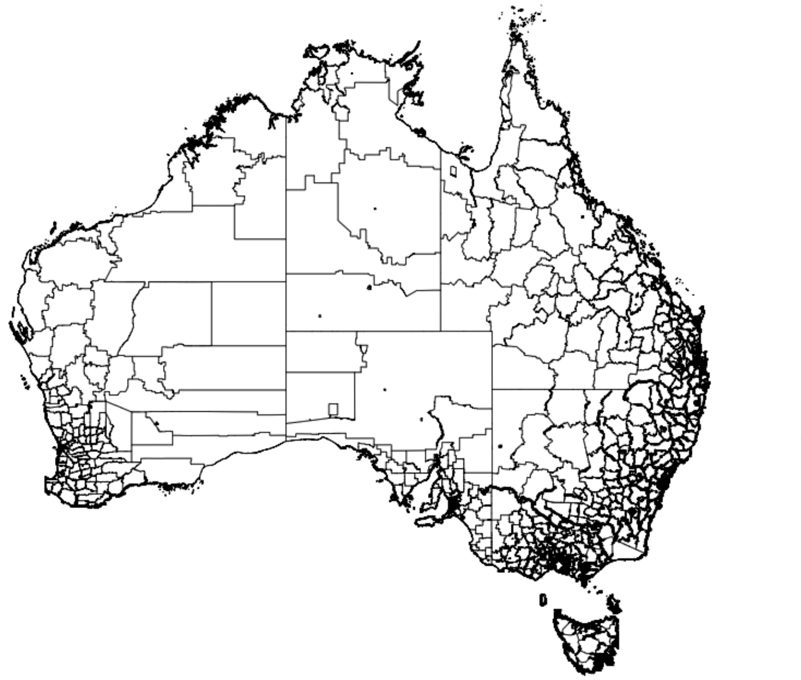

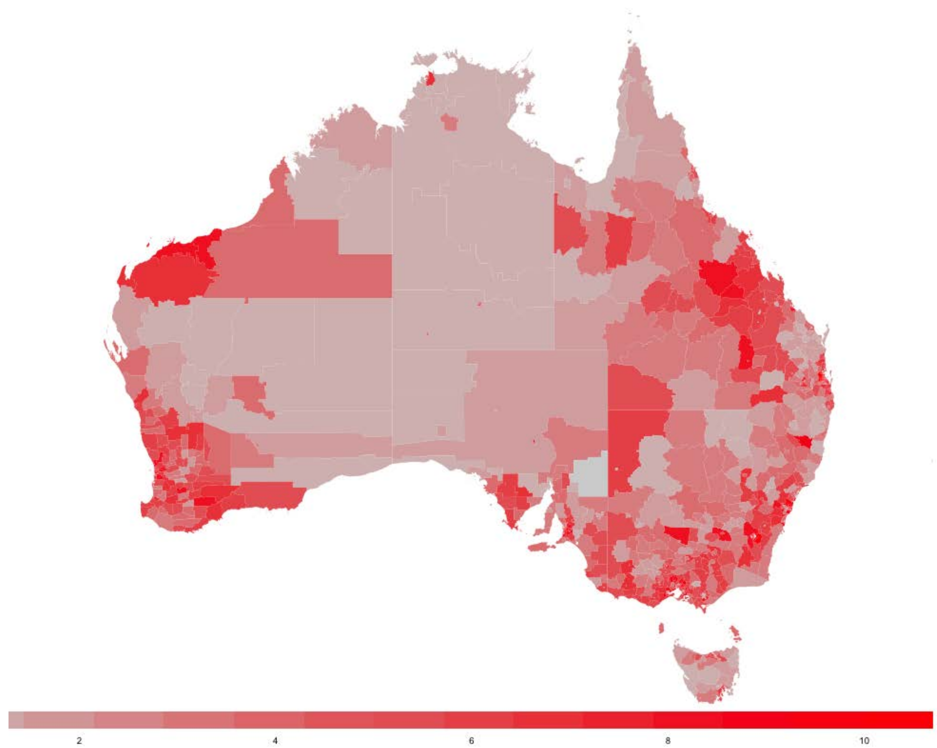
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Web Figure 1. Geographical borders of Statistical Local Areas (SLA) and the distribution of neighborhood advantage/disadvantage, with darker red indicating higher socioeconomic advantage of the SLA. Maps drawn by the author based on data derived from the Australian Bureau of Statistics (www.abs.gov.au).

Web Table 1. Associations between neighborhood disadvantage and health separately for within-individual and between-individuals components across 10 repeated measurement times.

	All person-observations[b]					
Outcome	Total effect		Within individual		Between individuals	
	B	95% CI	B	95% CI	B	95% CI
Self-rated health	-0.22	-0.25, -0.19	-0.03	-0.06, 0.01	-0.50	-0.54, -0.46
Mental Health	-2.81	-3.56, -2.07	-0.47	-1.47, 0.53	-5.81	-6.76, -4.86
Physical functioning	-0.28	-0.31, -0.25	-0.03	-0.07, 0.01	-0.58	-0.62, -0.54
Physical activity	-0.19	-0.24, -0.14	-0.06	-0.13, 0.02	-0.33	-0.38, -0.27
Alcohol consumption	-0.34	-0.4, -0.28	-0.12	-0.19, -0.05	-0.96	-1.03, -0.88
Smoking (no/yes)[a]	3.21	2.89, 3.57	0.93	0.86, 1.02	3.92	3.47, 4.43
Neighborhood dissatisfaction	0.98	1.06, 0.90	1.22	1.35, 1.08	0.82	0.88, 0.75
Neighborhood problems[d]	0.26	0.23, 0.29	0.31	0.27, 0.36	0.22	0.20, 0.25
	Long-term residence (≥3 years)[c]					
	Total effect		Within individual		Between individuals	
	B	95% CI	B	95% CI	B	95% CI
Self-rated health	-0.36	-0.41, -0.32	0.00	-0.09, 0.09	-0.48	-0.52, -0.44
Mental Health	-4.07	-5.19, -2.95	1.80	-0.55, 4.16	-5.93	-7.02, -4.83
Physical functioning	-0.39	-0.44, -0.35	0.03	-0.07, 0.13	-0.51	-0.56, -0.47
Physical activity	-0.27	-0.34, -0.20	-0.07	-0.27, 0.13	-0.31	-0.38, -0.25
Alcohol consumption	-0.66	-0.75, -0.56	-0.25	-0.42, -0.08	-0.95	-1.05, -0.86
Smoking (no/yes)[a]	3.37	2.92, 3.88	0.83	0.66, 1.03	3.55	3.06, 4.11
Neighborhood dissatisfaction	0.79	0.88, 0.69	0.93	1.24, 0.61	0.78	0.85, 0.70
Neighborhood problems[e]	0.24	0.20, 0.27	0.29	0.18, 0.40	0.23	0.20, 0.26

Values are B-coefficients (and 95% confidence intervals) of multilevel linear regressions for 10-point neighborhood disadvantage coded as a continuous variable ranging from 0=lowest decile to 1=highest decile. Except for the long-term residence associations with neighborhood satisfaction and neighborhood problems, all the differences of within-individual and between-individuals regression coefficients were statistically significant (p<0.05) based on the Wald test.

[a] Coefficients are odds ratios (and 95%confidence intervals). [b] n=112,503 person-observations of 20,012 unique individuals. [c] n=64,534 person-observations of 14,371 unique individuals. [d] n=75,526 person-observations of 18,980 unique individuals. [e] n=39,040 person-observations of 13,668 unique individuals

Web Table 2. Associations between neighborhood remoteness and health separately for within-individual and between-individuals components across 10 repeated measurement times.

	All person-observations[b]					
Outcome	Total effect		Within individual		Between individuals	
	B	95% CI	B	95% CI	B	95% CI
Self-rated health	-0.10†	-0.15, -0.05	-0.01	-0.08, 0.06	-0.20	-0.26, -0.14
Mental Health	1.74	0.42, 3.07	0.93	-1.00, 2.86	2.45	0.89, 4.00
Physical functioning	-0.09†	-0.14, -0.04	-0.03	-0.11, 0.04	-0.16	-0.22, -0.09
Physical activity	0.24	0.15, 0.32	0.15	0.01, 0.30	0.27	0.18, 0.36
Alcohol consumption	-0.10	-0.22, 0.02	-0.12	-0.26, 0.03	-0.06	-0.19, 0.07
Smoking (no/yes)[a]	2.12†	1.79, 2.50	1.03	0.88, 1.21	2.30	1.92, 2.77
Neighborhood dissatisfaction	-0.62	-0.49, -0.75	-0.76	-0.51, -1.02	-0.49	-0.38, -0.60
Neighborhood problems[d]	-0.09	-0.14, -0.04	-0.15	-0.24, -0.05	-0.06	-0.10, -0.02

	Long-term residence (≥3 years)[c]					
	Total effect		Within individual		Between individuals	
	B	95% CI	B	95% CI	B	95% CI
Self-rated health	-0.12†	-0.19, -0.05	0.10	-0.05, 0.25	-0.19	-0.26, -0.12
Mental Health	2.89	1.05, 4.74	2.83	-1.26, 6.92	2.84	1.01, 4.66
Physical functioning	-0.12	-0.2, -0.05	-0.15	-0.33, 0.03	-0.11	-0.19, -0.04
Physical activity	0.30	0.19, 0.41	0.24	-0.07, 0.54	0.32	0.21, 0.43
Alcohol consumption	-0.11	-0.29, 0.06	-0.17	-0.50, 0.15	-0.08	-0.23, 0.08
Smoking (no/yes)[a]	2.15†	1.73, 2.68	0.91	0.61, 1.36	2.22	1.77, 2.77
Neighborhood dissatisfaction	-0.56	-0.40, -0.72	-0.79	-0.27, -1.30	-0.49	-0.36, -0.61
Neighborhood problems[e]	-0.09	-0.15, -0.03	-0.11	-0.32, 0.09	-0.08	-0.12, -0.03

Values are B-coefficients (and 95% confidence intervals) of multilevel linear regressions for 5-point neighborhood remoteness coded as a continuous variable ranging from 0=major city to 1=very remote.

[a] Coefficients are odds ratios (and 95%confidence intervals). [b] n=112,503 person-observations of 20,012 unique individuals. [c] n=64,534 person-observations of 14,371 unique individuals. [d] n=75,526 person-observations of 18,980 unique individuals. [e] n=39,040 person-observations of 13,668 unique individuals. † Difference of within-individual and between-individuals regression coefficient statistically significant ($p<0.05$) based on the Wald test.

Web Table 3. Associations between neighborhood disadvantage and health, adjusted for time-varying education, marital status, and household income, broken to within-individual and between-individuals components across 10 repeated measurement times.

	All person-observations[b]					
Outcome	Total effect		Within individual		Between individuals	
	B	95% CI	B	95% CI	B	95% CI
Self-rated health	-0.19†	-0.22, -0.16	-0.03	-0.06, 0.01	-0.37	-0.41, -0.33
Mental Health	-2.35†	-3.11, -1.60	-0.45	-1.45, 0.55	-4.44	-5.45, -3.43
Physical functioning	-0.20†	-0.23, -0.17	-0.03	-0.07, 0.01	-0.36	-0.40, -0.32
Physical activity	-0.20†	-0.25, -0.15	-0.06	-0.13, 0.01	-0.33	-0.39, -0.27
Alcohol consumption	-0.20†	-0.26, -0.14	-0.08	-0.14, -0.01	-0.53	-0.62, -0.45
Smoking (no/yes)[a]	2.68†	2.40, 2.99	0.93	0.85, 1.02	3.22	2.84, 3.66
Neighborhood satisfaction	1.06†	0.98, 1.14	1.22	1.09, 1.36	0.93	0.86, 1.00
Neighborhood problems[d]	0.28†	0.25, 0.31	0.32	0.27, 0.36	0.25	0.23, 0.28
	Long-term residence (≥3 years)[c]					
	Total effect		Within individual		Between individuals	
	B	95% CI	B	95% CI	B	95% CI
Self-rated health	-0.30†	-0.34, -0.26	0.00	-0.09, 0.09	-0.37	-0.41, -0.33
Mental Health	-3.26†	-4.41, -2.11	1.89	-0.47, 4.25	-4.64	-5.79, -3.48
Physical functioning	-0.27†	-0.31, -0.22	0.04	-0.06, 0.14	-0.30	-0.35, -0.26
Physical activity	-0.28†	-0.35, -0.21	-0.08	-0.27, 0.12	-0.33	-0.40, -0.26
Alcohol consumption	-0.42†	-0.51, -0.32	-0.21	-0.37, -0.04	-0.53	-0.63, -0.44
Smoking (no/yes)[a]	2.92†	2.52, 3.38	0.84	0.66, 1.06	3.07	2.63, 3.57
Neighborhood satisfaction	0.86	0.76, 0.96	0.93	0.62, 1.24	0.88	0.80, 0.96
Neighborhood problems[e]	0.26	0.22, 0.29	0.29	0.19, 0.40	0.26	0.23, 0.29

Values are B-coefficients (and 95% confidence intervals) of multilevel linear regressions for 10-point neighborhood disadvantage coded as a continuous variable ranging from 0=lowest decile to 1=highest decile. Except for the long-term residence associations with neighborhood satisfaction and neighborhood problems, all the differences of within-individual and between-individuals regression coefficients were statistically significant ($p<0.05$) based on the Wald test.

[a] Coefficients are odds ratios (and 95%confidence intervals). [b] n=112,503 person-observations of 20,012 unique individuals. c n=64,534 person-observations of 14,371 unique individuals. [d] n=75,526 person-observations of 18,980 unique individuals. [e] n=39,040 person-observations of 13,668 unique individuals. † Difference of within-individual and between-individuals regression coefficient statistically significant ($p<0.05$) based on the Wald test.

Web Table 4. Associations between neighborhood remoteness and health, adjusted for time-varying education, marital status, and household income, broken to within-individual and between-individuals components across 10 repeated measurement times.

	All person-observations[b]					
Outcome	Total effect		Within individual		Between individuals	
	B	95% CI	B	95% CI	B	95% CI
Self-rated health	-0.07†	-0.11, -0.02	-0.01	-0.08, 0.06	-0.08	-0.13, -0.02
Mental Health	2.05	0.72, 3.38	0.84	-1.09, 2.77	3.65	2.08, 5.22
Physical functioning	-0.02	-0.07, 0.03	-0.02	-0.09, 0.05	0.02	-0.04, 0.08
Physical activity	0.25†	0.16, 0.33	0.13	-0.01, 0.28	0.31	0.22, 0.41
Alcohol consumption	0.09†	-0.02, 0.20	0.02	-0.12, 0.16	0.28	0.15, 0.40
Smoking (no/yes)[a]	1.83†	1.54, 2.16	1.02	0.86, 1.21	1.95	1.62, 2.35
Neighborhood satisfaction	-0.59	-0.72, -0.46	-0.74	-0.99, -0.48	-0.49	-0.6, -0.37
Neighborhood problems[d]	-0.09	-0.14, -0.04	-0.14	-0.23, -0.05	-0.06	-0.1, -0.02
	Long-term residence (≥3 years)[c]					
	Total effect		Within individual		Between individuals	
	B	95% CI	B	95% CI	B	95% CI
Self-rated health	-0.06†	-0.13, 0.01	0.10	-0.05, 0.25	-0.07	-0.14, 0.00
Mental Health	3.46	1.60, 5.31	2.68	-1.42, 6.78	4.13	2.29, 5.97
Physical functioning	-0.02	-0.09, 0.06	-0.14	-0.32, 0.04	0.06	-0.01, 0.13
Physical activity	0.33	0.21, 0.44	0.22	-0.08, 0.53	0.35	0.24, 0.46
Alcohol consumption	0.11	-0.06, 0.28	-0.02	-0.33, 0.29	0.26	0.11, 0.41
Smoking (no/yes)[a]	1.92†	1.54, 2.41	0.95	0.62, 1.44	1.97	1.56, 2.48
Neighborhood satisfaction	-0.54	-0.7, -0.38	-0.76	-1.28, -0.24	-0.48	-0.61, -0.35
Neighborhood problems[e]	-0.09	-0.15, -0.02	-0.11	-0.32, 0.09	-0.08	-0.13, -0.03

Values are B-coefficients (and 95% confidence intervals) of multilevel linear regressions for 5-point neighborhood remoteness coded as a continuous variable ranging from 0=major city to 1=very remote. [a] Coefficients are odds ratios (and 95%confidence intervals). [b] n=112,503 person-observations of 20,012 unique individuals. c n=64,534 person-observations of 14,371 unique individuals. [d] n=75,526 person-observations of 18,980 unique individuals. [e] n=39,040 person-observations of 13,668 unique individuals. † Difference of within-individual and between-individuals regression coefficient statistically significant ($p<0.05$) based on the Wald test.