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Original Contribution

Neighborhood Poverty and Injection Cessation in a Sample of Injection Drug Users

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Neighborhood socioeconomic environment may be a determinant of injection drug use cessation. The authors used data from a prospective cohort study of Baltimore City, Maryland, injection drug users assessed between 1990 and 2006. The study examined the relation between living in a poorer neighborhood and the probability of injection cessation among active injectors, independent of individual characteristics and while respecting the temporality of potential confounders, exposure, and outcome. Participants' residences were geocoded, and the crude, adjusted, and inverse probability of exposure weighted associations between neighborhood poverty and injection drug use cessation were estimated. Weighted models showed a strong association between neighborhood poverty and injection drug use cessation; living in a neighborhood with fewer than 10%, compared with more than 30%, of residents in poverty was associated with a 44% increased odds of not injecting in the prior 6 months (odds ratio = 1.44, 95% confidence interval: 1.14, 1.82). Results show that neighborhood environment may be an important determinant of drug injection behavior independent of individual-level characteristics.

drug users; epidemiologic methods; heroin; poverty; residence characteristics; social environment; substance-related disorders

Abbreviations: ALIVE, AIDS Link to Intravenous Experience; CI, confidence interval; HIV, human immunodeficiency virus; IDU, injection drug user; IPW, inverse probability weight; OR, odds ratio.

In 2003, an estimated 13.2 million adults worldwide injected drugs, with approximately 22% of injection drug users (IDUs) residing in developed countries (1). Injection drug use has been one of the 2 most common routes for the spread of human immunodeficiency virus (HIV) infection worldwide since the beginning of the pandemic, either directly through the sharing of contaminated syringes and injection equipment or indirectly through engaging in other risk behaviors associated with injection drug use, including sexual risk behaviors (2). In the United States, urban areas have been disproportionately affected by the dual epidemics of injection drug use and HIV (3). In Baltimore City, Maryland, for example, the per capita prevalence of injection drug use is 162 per 10,000 population, second among the largest metropolitan statistical areas in the United States (4), and the 14% prevalence of HIV among IDUs in 1998 ranked the city 11th among the largest metropolitan statistical areas in the United States (5).

Optimal prevention of the morbidity and mortality associated with injection drug use can best be achieved through injection cessation. The most commonly studied predictors of injection cessation have been individual-level characteristics relating to socioeconomic status, drug use behaviors, social and drug networks, and treatment access (6–11). However, a potential limitation of this approach is that the individualistic interventions it informs may not adequately address the underlying forces driving the prevalence and distribution of risk factors for injection drug use in the population. For example, a recent study of drug users discharged from community-based outpatient and residential treatment facilities in Los Angeles County, California, found that the location of treatment facilities may actually increase clients' exposures to potential neighborhood-level stimuli for relapse, including neighborhood disadvantage, violence, and drug activity (12).

Prominent sociologists, including William Julius Wilson, have proposed that the maladies plaguing urban areas of the United States, including the concentration of poverty and drug use, particularly among blacks, should be understood within a broader historical context of deindustrialization and declining job opportunities (13–16). Baltimore City, for example, has experienced a precipitous loss of its manufacturing base; accompanying these economic changes have been dramatic social and demographic shifts that have affected the neighborhood environment (17). The neighborhood environment, particularly levels of neighborhood poverty, may influence the likelihood of drug injection cessation.

A growing body of research shows that the primary determinant of cessation is the proportion of one's interactions with IDU subculture (10, 18, 19). Borrowing from symbolic interactionist thought (20), decreasing the proportion of interactions with drug use subculture and increasing the proportion of positive reference groups may produce the role strain necessary for inducing a shift out of IDU identity and promoting injection cessation. More affluent and socially cohesive neighborhoods may engender the positive social interactions and attendant financial and emotional resources that may decrease the proportion of one's interactions with IDU subculture and increase the likelihood of cessation. As such, the neighborhood environment may be an important determinant of injection cessation. A better understanding of how the urban environment influences patterns of injection drug use may facilitate the development of more effective interventions to mitigate the morbidity and mortality associated with injection drug use.

One of the most important challenges to causal inference in observational studies of intra-urban, small-area (typically called "neighborhood") characteristics and health indicators is the nonrandom process through which individuals are selected into different neighborhoods based on attributes that may be associated with their health (21, 22). Most neighborhood effects studies attempt to address selection into neighborhoods and isolate the effect of a particular neighborhood exposure by using standard methods such as regression or propensity score matching to adjust for individual-level covariates that may be associated with subsequent neighborhood exposure and may be predictive of the outcome. However, this approach can be problematic because the potential confounders frequently adjusted for in neighborhood effects studies may be time-varying confounders affected by prior exposure. Controlling for such variables by using standard methods may yield biased estimates of the total causal effect of a neighborhood exposure by overcontrolling for covariates on the pathway between a neighborhood exposure and outcome or by inducing collider-stratification bias (23). One method for handling such variables is to fit models by using inverse probability weights (IPWs). Such models allow for the control of time-fixed and time-varying variables without conditioning on these variables (24–27).

Few studies have handled selection into neighborhoods in a meaningful way, and, to our knowledge, only one multilevel study previously used IPWs to address the issue of selection into neighborhoods (28). Here, we build on this work by using multilevel models fitted with stabilized IPWs to assess the relation between living in a poorer neighborhood and the probability of injection cessation in a longitudinal sample of Baltimore City IDUs with over 15 years of follow-up.

MATERIALS AND METHODS

Sample selection

The rationale, design, and methods of the AIDS Link to Intravenous Experience (ALIVE) cohort study have been described in detail previously (29). Briefly, beginning in 1988–1989, the ALIVE study recruited 2,946 IDUs through community outreach in Baltimore City. For this initial recruitment period, individuals were eligible to participate in the study if they reported injection drug use within the past 11 years; were at least 18 years of age; and, for HIV-positive participants, were free of acquired immunodeficiency syndrome at study entry. Since the initial recruitment period, there has been additional recruitment using similar eligibility criteria in 1994–1995, 1998, 2000, and 2005–2007. ALIVE participants are followed up semiannually, with annual retention rates of approximately 90%–95% over the last decade. The address of their residence, provided by respondents at each ALIVE study visit, is geocoded to the US Census tract level.

For these analyses, additional inclusion and exclusion criteria were applied. After excluding 1,307 visits for which there was invalid or no identifiable address information, we geocoded 41,378 of 42,083 (98.3%) visits between January 1, 1988, and December 31, 2005. We excluded 2,245 visits geocoded to census tracts outside of Baltimore City. Additionally, 4,375 visits that mapped to 15 of the 200 census tracts in Baltimore City with 1) more than one-quarter of their population living in group quarters (e.g., a correctional institution, nursing home, or college dormitory) or 2) unstable data (e.g., census tracts in which most of the area is zoned for a park or cemetery) were excluded. Of the remaining 34,758 visits, we restricted the sample to "active" injectors at baseline by excluding 2,325 visits from participants who reported at baseline that they had not injected within the past 6 months. A total of 6,714 visits were excluded to allow for covariates to be lagged up to 2 visits and to restrict the analysis to visits from the 1988–1989, 1994–1995, 1998, and 2000 ALIVE recruitment phases that occurred between the beginning of 1990 and the end of 2005. Regarding the remaining 25,719 visits, the final analysis was based on 19,054 visits for which there was complete information on covariates of interest.

Neighborhood-level measures

Census tracts were used as proxies for Baltimore City neighborhoods. We used the 1990 decennial US Census to obtain the percentage of residents living in poverty per census tract (30) and merged these data with each participant visit. Our analyses included 174 census tracts, and the median number of visits per tract was 88 (25th percentile = 35, 75th percentile = 201). The percentage of the population living in poverty has been used previously to describe social inequities in health (31, 32). We split our primary exposure, percentage of neighborhood poverty, into 4 categories with cutpoints at 10%, 20%, and 30%. These cutpoints were selected because approximately one-quarter of Baltimore City census tracts fall into each of the 4 categories. Additionally, selection of these categories facilitates comparison with other research on neighborhood poverty that uses 20%, the federal definition of a ‘‘poverty area,’’ as a cutpoint.

Individual-level measures

Information on a number of individual-level characteristics is collected from ALIVE participants at each semiannual visit. Data are gathered on sociodemographic characteristics (gender, age, race/ethnicity, educational attainment, employment in the formal economy, formal income), drug use characteristics (age at first injection, needle sharing, crack use, shooting gallery attendance), sexual behaviors, medical history (HIV status, presence of any sexually transmitted diseases), health care utilization (methadone treatment usage), and life events (homelessness, jail/incarceration), among other characteristics. The demographics and risk behavior portions of the questionnaire were interview administered initially and were converted to audio computer-assisted self-interview in 1998 because the latter was found to yield higher reports of sensitive behaviors, both in the ALIVE cohort and in other studies (33, 34).

At each visit, participants were asked whether they had injected drugs in the past 6 months. Consistent with other research on cessation (6, 8, 10, 35), we defined our outcome measure, cessation, as not injecting drugs within the prior 6 months.

We included measures on a set of time-fixed and time-varying potentially confounding covariates. The selection of time-fixed and time-varying covariates was determined a priori based on previous analyses from this and other cohorts on drug injection cessation. For these analyses, gender, race/ethnicity (dichotomized as black or other), educational attainment (dichotomized as less than or greater than or equal to high school), and age at first injection (dichotomized as less than or greater than or equal to 20 years) were treated as time-fixed covariates. Current age (dichotomized at age 35 years), current employment status (dichotomized as not employed or employed), formal income in the past 6 months (dichotomized as less than or greater than or equal to $2,500), number of days incarcerated in the past 6 months (dichotomized as less than or greater than or equal to 1 week), homelessness in the past 6 months (dichotomized as no or yes), current HIV status (dichotomized as negative or positive), current infection with any sexually transmitted disease other than chlamydia (dichotomized as no or yes), crack use in the past 6 months (dichotomized as no or yes), needle sharing in the past 6 months (dichotomized as no or yes), shooting gallery attendance in the past 6 months (dichotomized as no or yes), and receipt of methadone treatment in the past 6 months (dichotomized as no or yes) were treated as time-varying covariates.

Statistical analyses

We ran 4 multilevel logistic regression models with nested random intercepts to account for within-person and within-neighborhood clustering, as described elsewhere (36). Consistent with the observation of frequent transitory patterns of drug use in the ALIVE cohort (37), which show that IDUs often shift between periods of injection and abstinence within 6 months, cessation was defined as a repeatable outcome and participants remained at risk even after their first report of cessation.

In the first model, we examined the crude or unadjusted relation between neighborhood poverty and cessation by regressing the odds of not injecting in the past 6 months measured at visit k on categories of neighborhood poverty measured at the prior visit, $k-1$. Second, we assessed the relation between neighborhood poverty and cessation after adjusting for time-fixed baseline covariates. Third, we assessed the fully adjusted relation by regressing the odds of not injecting in the past 6 months at visit k on categories of neighborhood poverty at $k-1$, while controlling for time-fixed covariates and time-varying covariates measured 2 visits prior to the outcome at $k-2$. Fourth, we removed the time-varying covariates from the logistic regression model but accounted for measured time-varying confounding and right-censoring due to losses to follow-up by weighting each participant visit using stabilized inverse probability of exposure and censoring weights (27, 38), as formally specified in the Appendix.

We assessed the distribution of the weights and truncated them at the 1st and 99th percentiles (mean = 1.00, median = 0.65, 1st percentile = 0.08, 99th percentile = 6.48) (38). The association between neighborhood poverty and the odds of not injecting in the past 6 months was assessed by regressing the odds of not injecting at time k on categories of neighborhood poverty measured at $k-1$ and the set of time-fixed covariates by using a stabilized inverse probability weighted multilevel logistic regression model. All analyses were conducted with the SAS statistical package, version 9.1.3 (SAS Institute, Inc., Cary, North Carolina).

RESULTS

The sample for this analysis consisted of 1,875 IDUs who contributed 19,054 study visits (median, 7; interquartile range, 3–12). The baseline (first visit) characteristics and balance of covariates across categories of neighborhood poverty are shown in Table 1. Among visits in each category of neighborhood poverty, participants reported not injecting drugs in the past 6 months at 4,887 of 11,807 (41%) visits in neighborhoods with greater than 30% of residents in poverty, 1,637 of 3,290 (50%) visits in neighborhoods with 20%–30% of residents in poverty, 1,312 of 2,417 (54%) of visits in neighborhoods with 10%–20% of residents in poverty, and 918 of 1,540 (60%) of visits in neighborhoods with less than 10% of residents in poverty (Table 2).

Table 1. Baseline (First Visit) Characteristics and Balance of Covariates Across Levels of Neighborhood Poverty[a] Among 1,875 Baltimore City, Maryland, Injection Drug Users Assessed Between 1990 and 2006

	Total (*N* = 1,875)		Poverty >30% (*n* = 1,256)		Poverty >20% but ≤30% (*n* = 297)		Poverty >10% but ≤20% (*n* = 214)		Poverty ≤10% (*n* = 108)	
	No.	%	No.	%	No.	%	No.	%	No.	%
Age ≤35 years	666	36	469	37	100	34	63	29	34	31
Female gender	477	25	329	26	63	21	62	29	23	21
Black race	1,780	95	1,221	97	279	94	190	89	90	83
<High school education	1,049	56	737	59	151	51	103	48	55	51
Employed[b]	440	23	278	22	72	24	58	27	32	30
Formal income <$2,500[b]	1,484	79	1,022	81	233	78	156	73	73	68
Incarcerated ≥1 week[b]	288	15	189	15	44	15	37	17	18	17
Homeless[b]	293	16	201	16	48	16	30	14	14	13
HIV positive	676	36	447	36	109	37	86	40	34	31
Any STD	102	5	67	5	18	6	13	6	4	4
Age at first injection <20 years	908	48	590	47	152	51	116	54	57	53
Needle sharing[b]	560	30	398	32	94	32	41	19	27	25
Crack use[b]	381	20	255	20	57	19	51	24	18	17
Shooting gallery attendance[b]	158	8	99	8	33	11	15	7	11	10
Methadone treatment[b]	186	10	114	9	37	12	24	11	11	10

Abbreviations: HIV, human immunodeficiency virus; STD, sexually transmitted disease.
[a] Neighborhood poverty based on percentage of residents living in poverty per US Census tract of residence.
[b] In the past 6 months.

Both unadjusted models and models adjusted for time-fixed baseline covariates showed a significant dose-response relation between neighborhood poverty and the probability of cessation (Table 2); compared with living in a neighborhood in the highest category of poverty, living in a neighborhood in the second, third, and fourth (lowest poverty) categories of poverty was associated with 11% (odds ratio (OR) = 1.11, 95% confidence interval (CI): 0.99, 1.26), 19% (OR = 1.19, 95% CI: 1.06, 1.33), and 44% (OR = 1.44, 95% CI: 1.22, 1.69) increased odds of not injecting in the prior 6 months, respectively, after adjusting for baseline covariates. However, these associations were substantially attenuated after adjusting for time-varying covariates. Fully adjusted models (Table 2) show that, relative to living in a neighborhood in the highest category of poverty, living in a neighborhood in the lowest category of poverty was associated with an attenuated odds of cessation (OR = 1.16, 95% CI: 0.97, 1.38).

In models fit by stabilized IPWs, we found that neighborhood poverty was a strong predictor of injection drug use

Table 2. Crude, Baseline Adjusted, Fully Adjusted, and Inverse Probability Weighted Odds of not Injecting in the Prior 6 Months, by Level of Neighborhood Poverty Reported at Prior Visit, for 1,875 Baltimore City, Maryland, Injection Drug Users With 19,054 Semiannual Study Visits Between 1990 and 2006

Neighborhood Poverty Level[a]	No. Not Using Drugs	No. of Visits	Crude[b]		Baseline Adjusted[b,c]		Fully Adjusted[b,c,d]		IPW[b,c,e]	
			OR	95% CI	OR	95% CI	OR	95% CI	OR	95% CI
Poverty >30% (reference)	4,887	11,807	1		1		1		1	
Poverty >20% but ≤30%	1,637	3,290	1.11	0.99, 1.24	1.11	0.99, 1.26	0.98	0.86, 1.11	1.20	1.03, 1.41
Poverty >10% but ≤20%	1,312	2,417	1.17	1.03, 1.33	1.19	1.06, 1.33	1.01	0.87, 1.16	1.35	1.12, 1.63
Poverty ≤10%	918	1,540	1.42	1.21, 1.67	1.44	1.22, 1.69	1.16	0.97, 1.38	1.44	1.14, 1.82

Abbreviations: CI, confidence interval; IPW, inverse probability weight; OR, odds ratio.
[a] Neighborhood poverty based on percentage of residents living in poverty per US Census tract of residence.
[b] Within-person and within-neighborhood clustering accounted for by using nested random intercepts.
[c] Adjusted for baseline covariates (gender, race, educational attainment, age at first injection).
[d] Adjusted for covariates measured one visit prior to neighborhood poverty (age, employment, formal income, jail, homelessness, human immunodeficiency virus status, any sexually transmitted disease, needle sharing, crack use, shooting gallery attendance, methadone treatment).
[e] Weighted for baseline covariates and covariates measured 6 months prior to neighborhood poverty: level of neighborhood poverty, outcome status, age, employment, formal income, jail, homelessness, human immunodeficiency virus status, any sexually transmitted disease, needle sharing, shooting gallery attendance, methadone treatment.

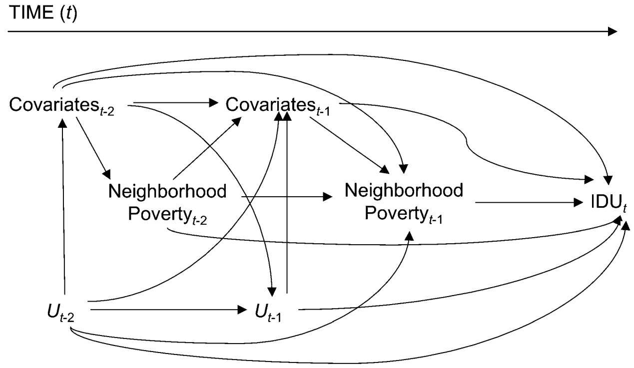

Figure 1. Causal diagram of the effect of neighborhood poverty on cessation of injection drug use (IDU). "Covariates" denote time-varying variables (i.e., age, employment, formal income, jail, homelessness, human immunodeficiency virus status, any sexually transmitted disease, sharing needles, crack use, shooting gallery attendance, methadone treatment) measured one visit prior to neighborhood poverty; "U" denotes unmeasured characteristics.

regardless of prior neighborhood (Table 2). Compared with living in a neighborhood in the highest category of poverty, living in a neighborhood in the second, third, and fourth (lowest poverty) categories of poverty was associated with 20% (OR = 1.20, 95% CI: 1.03, 1.41), 35% (OR = 1.35, 95% CI: 1.12, 1.63), and 44% (OR = 1.44, 95% CI: 1.14, 1.82) increased odds of not injecting in the prior 6 months, respectively.

DISCUSSION

We used data from a longitudinal cohort of 1,875 IDUs in Baltimore City to investigate the association between living in a poorer neighborhood and the probability of injection cessation. Inverse probability weighted models showed that IDUs living in more impoverished neighborhoods were less likely to stop injecting drugs, independent of individual-level covariates.

We examined the sensitivity of results to different modeling strategies for handling selection into neighborhoods. After finding an inverse dose-response association between levels of poverty and the probability of injection cessation in both crude unadjusted models and models adjusted for time-fixed baseline covariates, we used multivariable regression to adjust for an array of measured individual-level covariates hypothesized to influence our exposure and outcome; this approach is commonly utilized by neighborhood effects studies (refer, for example, to Nandi et al. (39), Williams and Latkin (40), Wu et al. (41), and Maas et al. (42)). We then compared these results with those from a stabilized inverse probability weighted regression model with random effects.

Overall, there was a significant inverse association between neighborhood poverty and the probability of cessation in models that used IPW to handle confounding but not when the traditional regression approach was used. These divergent results suggest that use of traditional regression to handle confounding in neighborhood effects studies may induce bias because the individual-level characteristics frequently adjusted for may be time-varying covariates affected by prior exposure. For example, as illustrated in Figure 1, time-varying covariates such as employment in the formal economy may predict whether a drug user lives in a relatively wealthier neighborhood. In turn, living in a wealthier neighborhood may influence future employment prospects and the probability of drug injection cessation. Similarly, drug use covariates related to a drug user's proclivity for injecting with others may predict whether a drug user lives in a poorer neighborhood with more shooting galleries versus a relatively richer neighborhood with fewer opportunities to share injection equipment, which may then influence injection behaviors during a subsequent time period.

Analyses of our data showed that sociodemographic characteristics, particularly employment status, were time-varying covariates affected by prior levels of exposure. By adjusting for individual-level factors related to socioeconomic status, studies on drug use may overcontrol for covariates on the pathway between a neighborhood exposure and outcome or induce selection bias due to collider stratification (24–27). Further neighborhood effects research should investigate the use of IPWs for handling confounding due to determinants of neighborhood selection.

Although work on the influence of the neighborhood environment on injection drug use cessation is sparse, our main finding of an inverse association between neighborhood poverty and the probability of cessation is consistent with prior observational work on illicit drug use (40, 43–49). For example, a study of 1,305 adults recruited from high-drug-risk areas in Baltimore City found that neighborhood poverty was significantly associated with current heroin and cocaine use even after accounting for network attributes (40); another analysis conducted among 835 adults from the same study population showed that IDUs in more disordered Baltimore neighborhoods had higher levels of depression and that depression was associated with greater injection frequency (47).

The level of socioeconomic deprivation in a neighborhood may influence the probability of cessation through a number of distinct pathways. For example, less impoverished neighborhoods may engender a social context more conducive to cessation through the enforcement of informal social control mechanisms and subsequent inhibition of dense drug use networks, as well as through the promotion of resources facilitating integration into mainstream society and the formal economy. The potential importance of the social environment in less socioeconomically deprived neighborhoods is supported by work showing associations between levels of disorder and the extent of drug use networks and between drug use networks and individual cessation (10, 50).

An alternative mechanism relates to the material environment of neighborhoods. For example, less impoverished neighborhoods may inhibit opportunities for purchasing drugs while promoting access to treatment resources. Clearly, a combination of these potential mechanisms, as well as others not discussed here, may explain the association we observed. Future work should focus on elucidating the pathways between socioeconomic deprivation and cessation of injection drug use.

There were a number of limitations to this study. First, the use of IPWs does not address unmeasured confounding. We attempted to minimize unmeasured confounding by including measures of as many recognized potential confounders as possible. Second, our weights were not multiplied across time and therefore do not account for cumulative effects of neighborhood poverty on cessation. However, longitudinal patterns of injection in the ALIVE study show that patterns of relapse are substantially more frequent than patterns of consistent abstinence of injection drug use (37), suggesting that the immediate 6-month interval preceding each study visit may be more important than long-term cumulative effects with respect to injection patterns. Third, we used only those variables measured in the prior 6 months as predictors in our weight models. To assess the sensitivity of our results to this decision, we conducted an analysis that incorporated the prior 2 visits in our weight models and found that our effect estimates did not change appreciably. Fourth, we excluded a number of visits, including those when a participant did not provide identifiable address information and those for which information on covariates of interest was missing. This exclusion may have introduced selection bias. However, the ALIVE study has standardized procedures for data collection and quality control and has minimized occurrence of missing data, particularly given its transient drug-using study population. Additionally, the distributions of key demographic factors were not significantly different in the study sample before and after missing visits were excluded. Fifth, we excluded residences outside of Baltimore City because they represented a limited number of contributions as well as visits during which the participant reported no address/homelessness. However, this occurrence was relatively uncommon, and we geocoded visits to shelter addresses if provided. Sixth, respondents were geocoded to their census tract of residence. It is plausible that respondents are influenced by multiple contexts outside of their own neighborhoods, and further work should attempt to quantify the impact of multiple environments on drug injection behaviors. Seventh, we relied on census tracts as proxies for neighborhoods. Although census tracts are a more practical unit of analysis because secondary data are more commonly aggregated to these units, they may be less sociologically valid to the extent that they represent the underlying processes theorized to be significant to the health of their residents. In this case, the availability of census tract data outweighed the potential benefits of using other boundaries, such as neighborhood statistical areas, which may have been more sociologically valid. Finally, we used 1990 US Census data to define our measure of neighborhood poverty. Although levels of poverty changed among Baltimore census tracts between 1990 and 2000, a sensitivity analysis using linearly interpolated poverty values produced qualitatively similar findings.

Our findings suggest that the neighborhood environment may be an important determinant of drug injection behaviors independent of individual-level characteristics. In contrast to adjusted models, models fit with inverse probability of exposure weights showed a robust association between neighborhood poverty and the probability of injection cessation. Future neighborhood effects research should consider the use of IPW as a method to address confounding by determinants of neighborhood selection, specifically for handling time-varying confounders affected by prior exposure. Some of the most important factors influencing persistent drug use among IDUs may be the environments in which risk is produced rather than individual-level factors that are frequently the emphasis of drug use research.

Our work has important implications for treatment delivery. Pairing drug treatment with job training or other programs that facilitate mobility into more economically stable neighborhood environments may increase the probability of injection drug cessation. Further work elucidating the pathways linking the neighborhood environment to drug use behaviors is warranted.

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APPENDIX

We fitted a hierarchical logistic regression model for the repeated measurement of drug injection cessation of the form

$$Y_{ijk} = \beta_0 + \beta_1 X_{ijk-1} + \beta_2 \mathbf{V_{ij0}} + \gamma_{ij} + \gamma_i$$

$$\log \frac{\Pr(Y_{ijk} = 1)}{1 - \Pr(Y_{ijk} = 1)} = \beta_0 + \beta_1 X_{ijk-1} + \beta_2 \mathbf{V_{ij0}} + \gamma_{ij} + \gamma_i,$$

where 1) $Y_{ijk} = 1$ when participant *j* living in neighborhood *i* reported not injecting drugs within the prior 6 months at semiannual visit *k*; 2) X_{ijk} represents the category of poverty in the neighborhood that participant *j* lived in at visit *k*, such that $X_{ijk} = 1$ indicates a neighborhood with greater than 30% of residents in poverty, $X_{ijk} = 2$ indicates a neighborhood with 20%–30% poverty, $X_{ijk} = 3$ indicates a neighborhood with 10%–20% poverty, and $X_{ijk} = 4$ indicates a neighborhood with 0%–10% poverty; 3) $\mathbf{V_{ij}}$ is a vector of measured baseline covariates, including gender, race, educational attainment, and age at first injection; and 4) γ_{ij} and γ_i are random intercepts that account for the clustering of visits within persons and persons within neighborhoods, respectively.

We accounted for measured time-varying confounding and right censoring due to losses to follow-up by variables in $\bar{\mathbf{L}}_{\mathbf{ijk}}$ by fitting the hierarchical logistic regression model specified above with stabilized IPWs of the form $SW_{ijk} = SW_{ijk}^X \times SW_{ijk}^C$, where

$$SW_{ijk}^X = \frac{f\left[X_{ijk-1} \mid \bar{X}_{ijk-2}, \mathbf{V_{ij0}}, \bar{C}_{ijk-2} = 0\right]}{f\left[X_{ijk-1} \mid \bar{X}_{ijk-2}, \bar{\mathbf{L}}_{\mathbf{ijk}-2}, \bar{C}_{ijk-2} = 0\right]}$$

and

$$SW_{ijk}^C = \frac{\Pr\left[C_{ijk-1} = 0 \mid \bar{C}_{ijk-2} = 0, \bar{X}_{ijk-1}, \mathbf{V_{ij0}}\right]}{\Pr\left[C_{ijk-1} = 0 \mid \bar{C}_{ijk-2} = 0, \bar{X}_{ijk-1}, \bar{\mathbf{L}}_{\mathbf{ijk}-2}\right]},$$

where the weight SW_{ijk}^X accounts for measured confounding and the weight SW_{ijk}^C accounts for selection bias due to measured determinants of loss to follow-up. The conditional density function evaluated at the observed covariate values for a given participant is represented by $f[\bullet \mid \bullet]$, $\bar{\mathbf{L}}_{\mathbf{ijk}-2}$ is a vector of time-varying covariates measured at visit $k-2$, including the baseline covariates $\mathbf{V_{ij0}}$, and $C_{ijk-2} = 0$ if participant *j* is censored by visit $k-1$. For this study, a participant was censored if he or she was lost to follow-up but not if the outcome was present. Specifically, a visit was considered lost to follow-up if 1) there was a greater than 2-year interval between visits or 2) the visit was the last visit for a respondent and it occurred more than 2 years prior to the end of follow-up in 2005. We estimated SW_{ijk}^X using pooled multinomial logistic regression. Predicted probabilities for the numerator and denominator were assigned based on the category of actual exposure to neighborhood poverty at each visit and were then divided to obtain the stabilized IPW. SW_{ijk}^C was obtained by using pooled logistic regression, as described elsewhere (51).

To estimate causal effects, it is important to maintain the temporal sequence between potential confounders, exposure, and outcome. As such, we lagged the exposure X_{ijk} by one visit and lagged time-varying covariates $\bar{\mathbf{L}}_{\mathbf{ijk}}$ by 2 visits. We decided not to include covariates in $\bar{\mathbf{L}}_{\mathbf{ijk}}$ measured at the same visit as the outcome, X_{ijk}, in weight models that accounted for time-varying confounding. Although doing so may have introduced residual confounding, the addition of nonconfounding variables, including mediators, in weight models may induce selection bias due to collider stratification (23, 52), induce finite sampling bias (38), and decrease the statistical efficiency of the effect estimate (53).