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The Relationship Between Neighborhood Poverty and Alcohol Use: Estimation by Marginal Structural Models

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Abstract

Background—Previous studies on the relationship of neighborhood disadvantage with alcohol use or misuse have often controlled for individual characteristics on the causal pathway, such as income—thus potentially underestimating the relationship between disadvantage and alcohol consumption.

Methods—We used data from the Coronary Artery Risk Development in Young Adults study of 5115 adults aged 18–30 years at baseline and interviewed 7 times between 1985 and 2006. We estimated marginal structural models using inverse probability-of-treatment and censoring weights to assess the association between point-in-time/cumulative exposure to neighborhood poverty (proportion of census tract residents living in poverty) and alcohol use/binging, after accounting for time-dependent confounders including income, education, and occupation.

Results—The log-normal model was used to estimate treatment weights while accounting for highly-skewed continuous neighborhood poverty data. In the weighted model, a one-unit increase in neighborhood poverty at the prior examination was associated with a 86% increase in the odds of binging (OR = 1.86 [95% confidence interval = 1.14–3.03]); the estimate from a standard generalized-estimating-equations model controlling for baseline and time-varying covariates was 1.47 (0.96–2.25). The inverse probability-of-treatment and censoring weighted estimate of the relative increase in the number of weekly drinks in the past year associated with cumulative neighborhood poverty was 1.53 (1.02–2.27); the estimate from a standard model was 1.16 (0.83–1.62).

Conclusions—Cumulative and point-in-time measures of neighborhood poverty are important predictors of alcohol consumption. Estimators that more closely approximate a causal effect of

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neighborhood poverty on alcohol provided a stronger estimate than estimators from traditional regression models.

Several studies have reported that alcohol abuse and dependence, as well as other risk behaviors, cluster in contexts of poverty, residential instability, and social isolation.[1–5] Most of these studies are cross-sectional and do not account for the fact that neighborhoods change over time, or allow us to assess how such changes might affect alcohol misuse. The question remains whether such multilevel associations are actually due to the influence of neighborhood contextual characteristics on health outcomes such as alcohol abuse, or whether they merely reflect the selection of persons with similar socioeconomic characteristics and health problems into particular types of neighborhoods. Longitudinal studies that follow people and neighborhoods over time are needed to better estimate the nature of the association of neighborhood conditions with alcohol use.

Traditionally, longitudinal studies examining the association between neighborhood characteristics and risk behaviors have attempted to address individual selection into neighborhoods by using standard regression models or propensity-score analysis to control tightly for individual-level characteristics (such as socioeconomic position) that are causally related both to the type of neighborhood a person lives in and to the person's use of alcohol. A major concern with such methods, however, is that many of the time-varying potential confounders are also affected by prior neighborhood conditions, and are thus in the causal pathway between the exposure of interest and the outcome, at the same time that they affect the types of neighborhoods that persons move into.[6] Individual socioeconomic position, for example, not only contributes to the type of neighborhood a person can afford to live in and the level of alcohol consumed, but it is also a product of the types of income-generating opportunities afforded by the neighborhood socioeconomic environment.[5] By controlling for the individual-level composition of neighborhoods, to address individual selection into neighborhoods, traditional regression analytic techniques run the risk of also controlling for individual-level mediators of earlier neighborhood characteristics and thus underestimating the impact that long-term cumulative neighborhood exposure has on health outcomes. Unadjusted estimates are confounded by individual-level characteristics related to selection of persons into neighborhoods.[7]

Marginal structural models offer a particularly useful tool for research on neighborhoods and health, where there are often time-dependent covariates that act simultaneously as confounders and as intermediate variables in the causal pathway between the neighborhood exposure of interest and the outcome.[8] Marginal structural models describe the marginal causal relationship between a time-varying exposure such as neighborhood poverty and alcohol use, and therefore, allow us to control for time-varying confounders without conditioning on these variables. Formally, a marginal structural model for repeated measures is a parametric regression model relating any possible exposure history, up to time t, to the corresponding counterfactual outcome at time t. Marginal structural models are also useful in the case of loss-to-follow-up in longitudinal studies, because they allow us to account for differential loss to follow-up. Assuming ignorable treatment assignment and the absence of differential misclassification,[9] the parameters of a marginal structural model can be estimated in an unbiased manner with inverse probability-of-treatment and censoring weighting. This is a product of inverse probability-of-treatment weights and inverse probability-of-censoring weights. Such weighting makes it possible to obtain a comparable "pseudopopulation" in terms of stable and time-varying confounders across levels of the exposure, and thus estimate the unconfounded association between the exposure and outcome without conditioning on the covariate through its inclusion as a predictor in the outcome model.[8] A detailed example illustrating how weighting creates an unbiased "pseudopopulation" is provided in the eAppendix (http://links.lww.com/EDE/A397).

Using data from a population-based longitudinal study of young adults, we investigated the potentially causal association of neighborhood poverty with 2 important aspects of alcohol consumption: frequency of alcohol consumption and binging. These 2 types of alcohol-related behavior may present contrasting etiologies,[7] and neighborhood poverty may have a stronger impact on heavy alcohol consumption than on the consumption gradient. We used marginal structural models to estimate the relationship between cumulative and point-in-time neighborhood poverty and alcohol use behaviors, after appropriately accounting for time-dependent confounders and for loss to follow-up. A directed acyclic graph illustrating the relationship is found in eAppendix (http://links.lww.com/EDE/A397). Because neighborhood poverty is a continuous exposure, pooled logistic regression is not appropriate for estimating inverse probability-of-treatment and censoring weighting treatment weights. Instead, our approach makes use of an estimated log-normal exposure probability density function that correctly accounts for the highly skewed continuous nature of the exposure of interest.

METHODS

The Coronary Artery Risk Development in Young Adults (CARDIA) study is a cohort study of cardiovascular risk factors among young adults.[10] The sample consists of 5115 adults aged 18–30 years at baseline (1985–1986). Participants were recruited through telephone contact from community lists in Birmingham, AL, Chicago, IL, and Minneapolis, MN, as well as from membership in a prepaid health plan in Oakland, CA. Investigators aimed to recruit nearly equal numbers of black and white people, men and women, persons <25 and ≥25 years of age, and persons with high school education or less and with more than a high school education. Respondents were interviewed 7 times between 1985 and 2006: at baseline (1985–1986), year 2 (1987–1988), year 5 (1990–1991), year 7 (1992–1993), year 10 (1995– 1996), year 15 (2000–2001), and year 20 (2005–2006). Cohort retention at year 20 was 69% of the original sample and 72% of survivors.

The outcomes of interest included frequency of alcohol consumption (operationalized as the number of glasses of wine, beer, and liquor consumed per week in the past year) and binging (operationalized as having consumed 5 or more drinks as the largest number of drinks per day in the past month). Alcohol consumption and binging were measured at each follow-up visit (from baseline through year 20).

The main exposure of interest was neighborhood poverty, defined as the proportion of residents living in poverty in the neighborhood (census tract) of the participant. This measure of neighborhood poverty characterizes a person's exposure to poverty in the neighborhood of residence. The Census Bureau uses a set of money-income thresholds that vary by family size and age composition to determine who is in poverty. If a family's total pretax income is less than the threshold, then that family is considered in poverty. For example, the poverty threshold for one person in 2007 was $10,590 in income. The official poverty thresholds are updated for inflation using the Consumer Price Index. The neighborhood poverty measure is highly correlated with many aspects of a disadvantaged neighborhood,[11] It also offers advantages in terms of constructing variables, because it is easy to log-transform into a normally distributed measure, which is convenient for calculation of the inverse probability-of-treatment-weights necessary to fit marginal structural models. Census tracts were used as proxies for neighborhoods, and participant addresses were geocoded at years 0, 7, 10, and 15 to identify census tract of residence. Neighborhood poverty was appended to individual-level data at baseline and at each geocoded follow-up time, using the closest decennial US Census. For baseline we used the 1980 Census, for years 7 and 10 we used the 1990 Census, and for year 15 we used the 2000 Census. Data for years 2 and 5 were estimated by linear interpolation from Census data for years 1980 and 1990. Two forms of neighborhood poverty were investigated: a measure of cumulative exposure to poverty ($pov_{neigh,cum_t-1,i}$), defined as the mean poverty for all examinations prior to the one on which the outcome was assessed, and a measure of poverty at the time of the examination prior to the one at which the outcome was assessed ($pov_{neigh,t-1,i}$).

A series of measures were considered potential confounders and were thus used as both baseline and time-varying predictors of inverse probability-of-treatment and inverse probability-of-censoring weights. Variables available only at baseline included age cohort (25 years or older at baseline), sex, race/ethnicity, marital status, the number of stressors and traumatic events experienced in daily life (using the Life Events Form derived from the Psychiatric Epidemiology Rating Interview Life Events Scale[12]), and positive social support from family and friends (using the Social Support A form, based on a questionnaire developed by Seeman and Syme[13]). Variables measured at multiple interviews (and also included at baseline) included age; family income (defined as a continuous variable made up of the following categories: $0–4999; $5–11,999; $12,000–15,999; $16–24,999; $25–34,999; $35–49,999; $50,000+); less than secondary education (having <12 years of education); the mean proportion of years since baseline the subject had a nonmanagerial or professional occupational status (defined according to the Census occupation codes); the existence of any children or stepchildren of the respondent; home ownership (defined as owned vs. not owned); depressive symptoms as assessed by the Center for Epidemiological Studies depression scale (dichotomized at 16 or higher)[14]; and prior alcohol consumption levels or binging (depending on the outcome of interest). Given that the relationship of some of the individual measures with neighborhood poverty might vary by age, we also estimated the interaction between cohort and income, occupational status, the existence of children, and home ownership. Although we tested the interaction of education with cohort, the lack of an association in any of the models led us to remove it from the models reported here.

STATISTICAL METHODS

Missing Data

We used IVEWARE software to carry out the sequential regression imputation method,[15,16] for conducting multiple imputation of missing observations on covariates at each interview, using all available data on study variables. Imputation assumed that data were missing at random rather than completely-at-random.[17] Interpolation was used to predict covariate values in cases where a scale had, by design, not been measured at one time-point but had been measured at a time-point before and after; in cases where a variable had not been measured in the first 2 time-points of the study, the respondent was assigned the covariate value from the third examination.

Outcome Models

Once preconditions for the need for marginal structural models were evaluated (see eAppendices 4 – 6, http://links.lww.com/EDE/A397), marginal structural logistic regression models for repeated binary measures were used to model the odds of binging. Marginal structural mean regressions with a log link were used to model the repeated counts of drinks consumed per week in the past year. The intraclass correlation coefficient indicated that only 1–2% of the variation in the alcohol use outcomes occurred between neighborhoods and 53% of the tracts had only one person per tract by year 10. Our objective was not to estimate variance components,[18,19] and results from models that accounted for clustering between persons within neighborhoods were virtually identical to those from models that did not account for such clustering; therefore we used generalized estimating equations which accounted for only the correlation within persons over time, by robust estimation of the variances of the regression coefficients.[20,21]

We estimated 3 types of models: (1) traditional repeated-measures regression models estimating the association between neighborhood poverty and each of the alcohol risk behaviors, after adjusting for baseline values of all covariates; (2) traditional repeated measures regression models, further adjusting for a vector of time-varying covariates at $t - 2$ (to ensure they were measured prior to the measurement of neighborhood poverty at $t - 1$); and (3) marginal structural models for the counterfactual outcomes $Y_{it}\left(\overline{pov_{neigh}}\,(t-1)\right)$ and $\mu_{it}\left(\overline{pov_{neigh}}\,(t-1)\right)$, corresponding to person *i*'s binging (Y_{it}) or average consumption (μ_{it}) status at time *t*, given that, possibly contrary to fact, the person has been exposed to a history of poverty level $\overline{pov_{neigh}}\,(t-1)$ up to time $t - 1$. Odds ratios estimating the association between neighborhood poverty and alcohol consumption are obtained from exponentiated regression coefficients. The odds ratios from the traditional regression models are not directly comparable to the odds ratios from marginal structural models, because marginal structural models provide population (marginal) estimates and traditional multivariable regression models provide conditional estimates. However, we present both sets of estimates so the reader can consider both the marginal structural model results and the results obtained through the adjustment approach most common in the epidemiologic literature.

Weights Estimation Methods

The marginal structural model approach involved fitting outcome models described above (#3), using inverse probability-of-treatment and censoring weights to account for time-dependent confounding and loss to follow-up. Following Robins et al[8] and Hernán et al,[22] weights for respondents were formed by the product of 2 factors—one corresponding to the ratio of conditional probability densities of receiving the exposure history the respondent did indeed receive, and the other corresponding to the ratio of conditional probabilities of remaining uncensored. A subject was censored for missing an interview or for failure to respond to the questions about the outcome of interest by time *t*. Probability densities were conditional on poverty exposure history, baseline, and time-varying values of the potential confounders, and remaining uncensored up to time *t*. Weights were stabilized to improve the precision of estimates. As these 2 sets of weights were unknown, we estimated them based on the observed data using simple parametric linear, log-linear, and logistic models, as described in eAppendix 3 (http://links.lww.com/EDE/A397). Each of the marginal structural outcome models was estimated using the “weight” statement in SAS PROC GENMOD,[23] which also provided “robust” estimates of standard errors, and thus conservative confidence intervals guaranteed to achieve at least 95% coverage rates.[22,24]

RESULTS

Table 1 presents descriptive data on all the study variables, for every year in which an interview was conducted. By examination 7 of the study, 47% of the sample was censored. Respondents were classified as “censored” the first time they skipped an examination or failed to respond to the alcohol outcome of interest. Study respondents were on average 25 years of age at baseline and 40 years old by examination 5 of the study. Slightly less than half (46%) were men; 52% were black and 48% white. The proportion of respondents who binged decreased from 25% to 15% throughout the study, while the mean number of drinks consumed per week in the past year remained constant throughout the study years. Moreover, the mean neighborhood poverty decreased from 24% to 11%. In parallel, the proportion who earned less than $25,000 a year decreased from 43% to 34% during the study.

Outcome Models: Traditional and Marginal Structural Models

Table 2 shows results of baseline-adjusted, traditional, and marginal structural model estimates of associations of neighborhood poverty with binging. Marginal structural models were used in this study after we found that certain assumptions were fulfilled: (1) time-varying confounding of the association between neighborhood poverty and alcohol use was indeed plausible in our data (eAppendix 4, http://links.lww.com/EDE/A397); (2) the weights accounted for the association between time-varying covariates and the exposure of interest (eAppendix 5, http://links.lww.com/EDE/A397); and (3) at each weight stratum, there was variation in observed neighborhood poverty, and those exposed to high versus low levels of poverty were comparable on the covariates of interest (eAppendix 6, http://links.lww.com/EDE/A397). The first set of columns presents results for cumulative neighborhood poverty up to $t - 1$. Results for the marginal structural model indicate that each unit increase in cumulative neighborhood poverty up to $t - 1$ (ie, an increase of 100% in mean exposure to poverty from baseline through $t - 1$) is associated with a 60% increase in the odds of binging, although confidence intervals were wide (OR = 1.60 [95% CI = 0.87–2.95]). This means that persons who, over the course of the study, resided in neighborhoods with an average of 20% more residents living in poverty, had a 10% higher odds of binging (CI = 0.97–1.24). The estimate from the standard (unweighted) generalized-estimating-equation regression model that included baseline and time-varying covariates as regressors was 1.26 (0.76–2.07). The second set of columns presents results for the statistical effect of neighborhood poverty at $t - 1$ on binging: in the marginal structural model a one-unit increase in the proportion of residents living in poverty was associated with an 86% increase in the odds of binging (OR = 1.86; CI = 1.14–3.03). This translates into a 13% increase in the odds of binging associated with a 20% increase in the proportion of residents living in poverty (1.03–1.25). In the standard regression model, a one-unit increase in the proportion of residents in poverty was associated with a 47% increase in the odds of binging (0.96–2.25).

Table 3 shows the relative difference in the average number of drinks consumed per week associated with neighborhood poverty. The marginal structural model indicates that one unit increase in cumulative neighborhood poverty up to $t - 1$ is associated with a 53% increase in drinks consumed per week (CI = 1.02–2.27), whereas the corresponding estimate from the standard generalized-estimating-equation model was 1.16 (CI = 0.83–1.62). The marginal structural model estimate indicates that a 20% increase in the proportion of residents living in poverty over the course of the study was associated with a 9% increase in the number of drinks consumed per week (1.00–1.18). Such an estimate would mean a shift from an average weekly consumption of 4.8 drinks to 5.3 drinks per week. For exposure 2 (neighborhood poverty at $t - 1$), the estimate of the ratio of weekly drinks per unit increase in neighborhood poverty at $t - 1$ from the marginal structural model was 1.29 (0.92–1.80), but the estimate from the standard generalized-estimating-equation model was 1.09 (0.81–1.47). The marginal structural model estimate translates into approximately 1 extra drink per week for every 20% increase in the proportion of residents living in poverty in the neighborhood (0.98–1.12).

DISCUSSION

Using marginal structural logistic and log-linear models, we found that greater cumulative and point-in-time neighborhood poverty exposures were both associated with increased odds of binging and an increased rate of weekly alcohol consumption, after adjustment for baseline and time-varying confounders, although the confidence intervals were wide. This study treated neighborhood poverty as a time-varying exposure, hence recognizing the fact that an individual's exposure to neighborhood conditions varies over time. Building on this assumption, we used rich data on neighborhoods and alcohol-use trajectories to more closely approximate the causal effect of neighborhood poverty on alcohol consumption, in a case when contextual exposures, outcomes, and confounders may all vary over time.

We estimated that a difference of 20% in the average proportion of neighborhood residents living in poverty over the course of the study was associated with a 10% higher odds of binging, and with a 9% increase in the number of drinks consumed per week; furthermore, a 20% shift in the proportion of residents in poverty in the prior interview was associated with a 13% increase in the odds of binging and approximately 1 extra drink. Neighborhood poverty could shape alcohol consumption through several mechanisms, including the limited availability of employment options in disadvantaged neighborhoods,[5] lower levels of social cohesion and social control over deviant behaviors such as excessive alcohol use,[2,3] higher alcohol outlet density,[25] or the disproportionate concentration of stressful life experiences, which leads to the use of alcohol as a form of “self-medication.”[1] Thus, interventions aimed at deconcentrating neighborhood poverty or addressing some of its consequences in the economic, built or social environment, could have a small impact on levels of alcohol consumption.

Three previous studies have used a longitudinal design to investigate the influence of neighborhood resources on alcohol use and abuse.[26–28] Of these, 2 found a positive relationship between neighborhood disadvantage and alcohol abuse.[26,27] The present work extends the findings of these previous studies by comparing the effects of long-term versus acute exposure to neighborhood poverty on trajectories of alcohol use and abuse in a population-based sample of young adults over 20 years.

The present work also makes a methodologic contribution to the literature on neighborhoods and alcohol use as it investigates a key limitation of many longitudinal neighborhood studies: the need to address confounding bias by appropriately controlling for time-dependent covariates that are simultaneously confounders and intermediate variables in the causal pathway between the exposure of interest and the outcome. With inverse probability-of-treatment and censoring weights, one can create a pseudopopulation in which there is no confounding by the measured covariates, thus more closely approaching a causal interpretation. Only one study has previously investigated the use of marginal structural models as a method to address this problem in longitudinal multilevel studies of neighborhood effects,[6] and none has focused on alcohol. By treating neighborhood poverty as a continuous exposure in the weight-estimation models, we were able to predict the inverse probability of exposure to poverty (and hence address time-varying confounding) with greater accuracy than if we had used categories of poverty as the exposure.

These results should be considered in the context of the following limitations. Marginal structural models do not, by themselves, address all issues of causal inference. First, marginal structural model estimates are valid to the extent that unobserved time-varying and static covariates that predict alcohol use are unrelated to the exposure assignment after controlling for the observed covariates.[29] The absence of a relationship between these covariates and poverty at baseline, as well as a concerted attempt to incorporate an extensive set of factors that may contribute to neighborhood selection, reduces this concern. Second, the absence of geocodes and certain measures at some study time-points necessitated interpolation, which may have led to exposure misclassification. However, a sensitivity analysis using data from only the third to the seventh time points, when these key time-varying covariates had been measured, did not produce substantively different results. Third, the analysis is based on the assumption that dropout was ignorable, conditional on observed covariates. Participants were censored at their first missing outcome measure. A sensitivity analysis conducted with respondents classified as “censored” after they missed an interview, rather than the first time they failed to respond to the outcome measure, provided similar estimates. Finally, the analysis is based on the assumption of random measurement error. The self-reported nature of both the exposure and outcome increases the risk of measurement errors that are correlated between the exposure and the outcome, which would lead to differential misclassification of the level of alcohol consumption by level of neighborhood poverty. However, concern about possible correlation of measurement errors is reduced by the fact that exposure and outcome data come from different sources; the exposure data are obtained from the US Census, and calculated as the proportion of all residents in the respondent's census tract who were below the poverty line, whereas alcohol use was reported by the respondent.

This is one of the first longitudinal studies to provide evidence about the effect of point-in-time and accumulated neighborhood poverty on alcohol use. The study highlights the need to consider the impact of short- versus long-term exposure to poverty on alcohol use and other associated behavioral out comes: while cumulative exposure to higher rates of poverty was associated with higher levels of drinking, short-term exposure to poverty was associated with only an extreme form of alcohol use (binging). The study also illustrates how analytic methods such as marginal structural models provide estimates of the relationship between neighborhood socioeconomic conditions and health that are consistent with a causal framework, in the context of an observational study with time-varying confounders that are affected by prior levels of the exposure of interest.

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

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TABLE 1

Sample Characteristics[a] by Study Examination, Coronary Artery Risk Development in Young Adults Study, 1985–2006

	1985–1986 (n = 5102)	1987–1988 (n = 4552)	1990–1991 (n = 4066)	1992–1993 (n = 3677)	1995–1999 (n = 3324)	2000–2001 (n = 2956)	2005–2006 (n = 2629)
Outcomes							
Binging	25	24	22	19	17	15	15
Frequency of consumption (number of glasses per week in past year); mean (SD)	4.8 (8.4)	4.8 (8.6)	4.4 (8.4)	4.5 (8.6)	4.3 (8.4)	4.4(10.3)	4.6 (9.4)
Main exposure							
Neighborhood poverty; mean (SD)							
Mean proportion in poverty	0.24 (0.13)	0.22[b] (0.12)	0.17[b] (0.12)	0.14 (0.14)	0.12 (0.12)	0.11 (0.11)	
Cumulative poverty[c]	0.23 (0.13)	0.23[b] (0.12)	0.21[b] (0.11)	0.19 (0.1)	0.18 (0.09)	0.17 (0.09)	
Measured only at baseline							
Cohort (aged 25 years or older at baseline)	55						
Sex							
Male	46						
Female	54						
Race/ethnicity							
Black	52						
White	48						
Marital status							
Married	22						
Divorced/separated	7						
Widowed	7						
Never married	64						
Life events score; mean (SD)	2386 (1415)						
Social support							
Instrumental support	1.43 (0.18)						
Emotional support	1.45 (0.22)						
Measured in multiple interviews							
Age (years); mean (SD)	24.8 (3.6)	26.9 (3.6)	30.0 (3.6)	32.0 (3.6)	40.1 (3.6)		
Income ($)							
Nonprofessional or nonmanagerial occupations	78	66	59	53	50		
Less than high school education	10	8	6	6	6		
Any children or stepchildren	32	34	50	56	64		
Own home	45	45	45	52	56		
Depression score (CES-D; % above depressive cutoff of 16)	20[d]	20[d]	20	20[b]	16		

[a]Percent, unless otherwise specified.

[b]Values interpolated from closest available measures.

[c]Mean value of poverty, based on measures of poverty at all the time points up to time t.

[d]Value taken from closest available measurement.

TABLE 2

Odds Ratios of Binging (Dependent Variable) per Unit Increase in Neighborhood Proportion Below Poverty, as Estimated From Standard Models and Marginal Structural Models

	Exposure 1: Cumulative Poverty up to *t* – 1			**Exposure 2: Poverty at *t* – 1**		
	Crude Model[a] OR (95% CI)	**Model With Traditional Adjustment for Time-Varying Covariates[b] OR (95% CI)**	**Marginal Structural Model[c] OR (95% CI)**	**Crude Model[a] OR (95% CI)**	**Model With Traditional Adjustment for Time-Varying Covariates[b] OR (95% CI)**	**Marginal Structural Model[c] OR (95% CI)**
Time	0.87 (0.67–1.12)	0.96 (0.88–1.04)	0.86 (0.68–1.10)	0.88 (0.67–1.14)	0.97 (0.89–1.05)	0.87 (0.69–1.11)
Cumulative poverty up to t – 1	1.54 (0.87–2.76)	1.26 (0.76–2.07)	1.60 (0.87–2.95)			
Poverty at *t* – 1				1.88 (1.17–3.01)	1.47 (0.96–2.25)	1.86 (1.14–3.03)

[a]This model was unweighted and adjusted for baseline covariate values, just as they were used in the linear model for the numerator of the stabilized weight (age, cohort, sex, race/ethnicity, marital status, life events, instrumental support, emotional support, income, income × cohort, education, cumulative exposure to non-professional/managerial employment, employment × cohort, home ownership, home ownership × cohort, presence of children in the home, children × cohort, depression, and binging at baseline).

[b]This model was unweighted and adjusted for time-varying covariate values, just as they were used in the linear model for the denominator of the stabilized weight (age, cohort, sex, race/ethnicity, marital status, life events, instrumental support, emotional support, income, income × cohort, education, cumulative exposure to non-professional/managerial employment, employment × cohort, home ownership, home ownership × cohort, presence of children in the home, children × cohort, depression, and binging at *t* – 1).

[c]This model used inverse-probability-of-treament-and-censoring weights and also adjusted for baseline covariate values (age, cohort, sex, race/ethnicity, marital status, life events, instrumental support, emotional support, income, income × cohort, education, cumulative exposure to non-professional/managerial employment, employment × cohort, home ownership, home ownership × cohort, presence of children in the home, children × cohort, depression, and binging at baseline).

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TABLE 3

Relative Rates of Weekly Drinks Consumed (Dependent Variable) Associated With a Unit Increase in Neighborhood Proportion Below Poverty, as Estimated From Standard Models and Marginal Structural Models

	Exposure 1: Cumulative Poverty up to *t* – 1			Exposure 2: Poverty at *t* – 1		
	Crude Model[a] RR (95% CI)	**Model With Traditional Adjustment for Time-Varying Covariates[b] RR (95% CI)**	**Marginal Structural Model[c] RR (95% CI)**	**Crude Model[a] RR (95% CI)**	**Model With Traditional Adjustment for Time-Varying Covariates[b] RR (95% CI)**	**Marginal Structural Model[c] RR (95% CI)**
Time	0.98 (0.85–1.14)	0.99 (0.93–1.04)	0.98 (0.82–1.16)	0.99 (0.85–1.14)	0.99 (0.93–1.04)	0.98 (0.83–1.16)
Cumulative poverty up to *t* – 1	1.51 (1.04–2.20)	1.16 (0.83–1.62)	1.53 (1.02–2.27)			
Poverty at *t* – 1				1.37 (1.02–1.86)	1.09 (0.81–1.47)	1.29 (0.92–1.80)

[a]This model was unweighted and adjusted for baseline covariate values, just as they were used in the linear model for the numerator of the stabilized weight (age, cohort, sex, race/ethnicity, marital status, life events, instrumental support, emotional support, income, income × cohort, education, cumulative exposure to non-professional/managerial employment, employment × cohort, home ownership, home ownership × cohort, presence of children in the home, children × cohort, depression, and binging at baseline).

[b]This model was unweighted and adjusted for time-varying covariate values, just as they were used in the linear model for the denominator of the stabilized weight (age, cohort, sex, race/ethnicity, marital status, life events, instrumental support, emotional support, income, income × cohort, education, cumulative exposure to non-professional/managerial employment, employment × cohort, home ownership, home ownership × cohort, presence of children in the home, children × cohort, depression, and binging at *t* – 1).

[c]This model used inverse-probability-of-treatment-and-censoring weights and also adjusted for baseline covariate values (age, cohort, sex, race/ethnicity, marital status, life events, instrumental support, emotional support, income, income × cohort, education, cumulative exposure to non-professional/managerial employment, employment × cohort, home ownership, home ownership × cohort, presence of children in the home, children × cohort, depression, and binging at baseline).