Published in final edited form as:
*Soc Sci Med*. 2011 March ; 72(5): 641–649. doi:10.1016/j.socscimed.2010.12.013.

# Neighborhood socioeconomic status predictors of physical activity through young to middle adulthood: The CARDIA Study

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## Abstract

Neighborhood socioeconomic status (SES) is related to a wide range of health outcomes, but existing research is dominated by cross-sectional study designs, which are particularly vulnerable to bias by unmeasured characteristics related to both residential location decisions and health-related outcomes. Further, little is known about the mechanisms by which neighborhood SES might influence health. Therefore, we estimated longitudinal relationships between neighborhood SES and physical activity (PA), a theorized mediator of the neighborhood SES-health association. We used data from four years of the Coronary Artery Risk Development in Young Adults (CARDIA) study (n=5,115, 18–30 years at baseline, 1985–86), a cohort of U.S. young adults followed over 15 years, and a time-varying geographic information system. Using two longitudinal modeling strategies, this is the first study to explicitly examine how the estimated association between neighborhood SES (deprivation) and PA is biased by (a) measured characteristics theorized to influence residential decisions (e.g., controlling for individual SES, marriage, and children in random effects models), and (b) time-invariant, unmeasured characteristics (e.g., controlling for unmeasured motivation to exercise that is constant over time using repeated measures regression modeling, conditioned on the individual). After controlling for sociodemographics (age, sex, race) and individual SES, associations between higher neighborhood deprivation and lower PA were strong and incremental in blacks, but less consistent in whites. Furthermore, adjustment for *measured* characteristics beyond sociodemographics and individual SES had little influence on the estimated associations; adjustment for *unmeasured* characteristics attenuated negative associations more strongly in whites than in blacks.

### Keywords

Environment design; socioeconomic status; physical activity; epidemiologic methods; confounding factors; USA; race

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There were no potential or real conflicts of financial or personal interest with the financial sponsors of the scientific project.




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## INTRODUCTION

Neighborhood socioeconomic status (SES) such as census-tract level poverty or composite measures are consistently associated with numerous health outcomes, including mortality (Subramanian, Chen, Rehkopf, Waterman, & Krieger, 2005), general health (Do, 2009), and cardiovascular disease (Diez Roux, Merkin, Arnett, Chambless, Massing, Nieto et al., 2001). Theorized mechanisms by which neighborhood SES influences health (Diez Roux, 2007; R.J. Sampson, Morenoff, & Gannon-Rowley, 2002) include mediation by health behaviors through inequitable access to physical activity (PA) opportunities, healthy foods, or health care (structural perspective) or through establishment of social norms (contagion perspective) (Ross, 2000), or direct, cumulative biological effects of chronic stress (Cox, Boyle, Davey, Feng, & Morris, 2007; Merkin, Basurto-Davila, Karlamangla, Bird, Lurie, Escarce et al., 2009). While there is an international literature on this topic (e.g, (Boyle, Norman, & Rees, 2002; Curtis, Setia, & Quesnel-Vallee, 2009)), we focus on the U.S., given our study population and the nature of the research question in a U.S. context.

Existing research largely focuses on the influence of neighborhood exposures on broader health outcomes (e.g., neighborhood poverty as a predictor of mortality), rather than on health behaviors (e.g., neighborhood poverty as a predictor of physical activity [PA]). Physical inactivity and obesity are key outcomes related to neighborhood SES (Do, Dubowitz, Bird, Lurie, Escarce, & Finch, 2007; Lee, Cubbin, & Winkleby, 2007; Wen & Zhang, 2009) and in countries like the U.S. exhibit dramatic racial and socioeconomic disparities (Gordon-Larsen, McMurray, & Popkin, 1999; Ogden, Carroll, Curtin, McDowell, Tabak, & Flegal, 2006) which may result in part from differences in structural (e.g., built environment) (Gordon-Larsen, Nelson, Page, & Popkin, 2006; Moore, Diez Roux, Evenson, McGinn, & Brines, 2008), contagion, or stress-related factors. While neighborhood SES and physical fitness at a single time point has been examined using the U.S.-based Coronary Artery Risk Development in Young Adults (CARDIA) Study (Shishehbor, Gordon-Larsen, Kiefe, & Litaker, 2008), PA is a modifiable behavior that is amenable to intervention, whereas fitness is influenced by physiological factors.

Additionally, a major limitation of existing research examining neighborhood influences on health and related behaviors is potential bias resulting from self-selection into neighborhoods (Boone-Heinonen, Gordon-Larsen, Guilkey, Jacobs, & Popkin; Diez Roux, 2004Oakes, 2004; van Lenthe, Martikainen, & Mackenbach, 2007). Briefly, factors such as financial resources and household structure (e.g., marital status, children) influence not only where people are able (through affordability or other constraints) or prefer to live (Clark & Ledwith, 2007; Geist & McManus, 2008; Lund, 2006), but also health behaviors (Bell & Lee, 2005; Yannakoulia, Panagiotakos, Pitsavos, Skoumas, & Stafanadis, 2008) and outcomes (Gordon-Larsen, Adair, & Popkin, 2003; Sobal, Rauschenbach, & Frongillo, 2003). Without accounting for factors which influence residential mobility or location decisions, neighborhood SES-health associations could be biased and incorrectly interpreted as neighborhood influence on health. Yet, studies investigating neighborhood influences on PA – which generally stem from the built environment literature (Papas, Alberg, Ewing, Helzlsouer, Gary, & Klassen, 2007), as opposed to demographic and geographic studies (e.g., (Curtis et al., 2009)) -- generally control for individual SES but not other observed characteristics related to residential selection such as marriage and children.

Furthermore, key drivers of residential self-selection may be difficult or impossible to measure. For example, unmeasured characteristics of individuals who are more likely to select a neighborhood with high quality schools (in the U.S., generally in high SES areas) may also influence adoption of physically active lifestyles. Therefore, unmeasured characteristics may bias traditional, covariate-adjusted estimates of how neighborhood SES influences healthy behaviors. In contrast, with longitudinal data, unmeasured characteristics that are stable over time (time-invariant) can be addressed with within-person estimators (e.g., first difference and fixed effects models), which condition on the individual, thereby exploiting variation observed within person, over time (Boone-Heinonen et al.; Do & Finch, 2008; Eid, Overman, Puga, & Turner, 2008). While within-person estimators do not address dynamic feedback processes in which health may influence subsequent residential selection (Boyle et al., 2002; Curtis et al., 2009), they are uniquely suited to control for unmeasured confounders.

However, few neighborhood health studies have the longitudinal exposure and outcome data necessary to estimate within-person effects. The vast majority of neighborhood health research in the U.S. and elsewhere is cross-sectional, and the few existing longitudinal studies examine general health measures (Do & Finch, 2008) rather than behaviors that might mediate general health, such as PA. In addition, they do not always take advantage of the potentialities of a repeated measures design for addressing unmeasured confounders. Controlling for measured confounders related to residential self-selection and within-person estimation can provide insights into possible causal processes linking neighborhoods and health and into the sensitivity of studies to omission of these unmeasured confounders.

Therefore, we capitalized on longitudinal neighborhood and behavior data from four CARDIA study examinations to investigate how the estimated association between neighborhood SES and PA is influenced by controlling for the confounding effects of (a) measured characteristics related to residential selection in a large body of mobility research (e.g., individual SES, marriage, and children), and (b) unmeasured characteristics which are constant over time.

# METHODS

## Study Population and Data Sources

The CARDIA Study is a population-based prospective epidemiologic study of the determinants and evolution of cardiovascular risk factors among black and white young adults. At baseline (1985–6), 5,115 eligible subjects, aged 18–30 years, were enrolled with balance according to race (black, white), gender, education (≤ and >high school) and age (18–24 and 25–30 years) from four U.S. communities: Birmingham, Alabama; Chicago, Illinois; Minneapolis, Minnesota; and Oakland, California. Specific recruitment procedures were described elsewhere (Hughes, Cutter, Donahue, Friedman, Hulley, Hunkeler et al., 1987). Study data were collected under protocols approved by Institutional Review Boards at each study center and the University of North Carolina at Chapel Hill. Follow-up examinations conducted in 1987–1998 (Year 2), 1990–1991 (Year 5), 1992–1993 (year 7), 1995–1996 (year 10), and 2000–2001 (year 15) had retention rates of 90%, 86%, 81%, 79%, and 74% of the surviving cohort, respectively.

Using a Geographic Information System, we linked time-varying, community-level, U.S. census data to CARDIA respondent residential locations in exam years 0, 7, 10, and 15 from geocoded home addresses. 48.2, 68.8, and 33.0% of participants moved residential locations between years 0 and 7, 7 and 10, and 10 and 15, respectively.

Of the possible 20,460 observations for 5115 participants at baseline across 4 examinations, 4,400 observations were missing due to loss to follow-up (including mortality): 80, 77, and 72% of the initial participants were observed at years 7, 10, and 15, respectively. Of remaining observations, we excluded observations for women who were pregnant at the time of examination (n=114 observations), and with missing PA (n=126 observations), neighborhood SES variables (n=86 observations) or covariate data (n=274 additional observations). Those lost to follow-up or missing data were generally more likely black, male, younger, and of lower baseline education ($p<0.05$); however, attrition (except for year 7, $p=0.02$) and missing data were unrelated to baseline PA and, to the extent that attrition and missing data are related to unobserved fixed characteristics of the individuals, our fixed effects models may mitigate selection bias. The final analytical sample totaled 15,460 observations for 4,179 individuals.

### Neighborhood socioeconomic measures

Several commonly used neighborhood socioeconomic measures were approximately time-matched to each examination period (CARDIA year, Census: Year 0, 1980; Years 7 and 10, 1990; Year 15, 2000). Census tracts were used to define neighborhoods because they are consistent with prior research, block groups were not universally implemented until the 1990 census, and we theorized counties as too large to capture the neighborhood environment.

Measures of socioeconomic disadvantage included percent of persons with income less than 150% of federal poverty level [1.5 times federal poverty level (Krieger, Zierler, Hogan, Waterman, Chen, Lemieux et al., 2003; U.S. Census Bureau, 2009)] and percent of persons aged 25 years and over with less than high school level of education. Because nearly the entire study population (>96%) resided in metropolitan areas, we used the 150% poverty cutpoint to account for higher cost of living in urban areas; results using the 100% poverty cutpoint were similar but slightly weaker. Other SES measures included percent of persons ≥25 years with college degrees and median household income, which, for comparability across exam periods, was inflated to reflect the value of U.S. dollars in the year 2000, based on the Consumer Price Index.

Exploratory factor analysis was used to summarize neighborhood SES exposures and indicated that the four measures represented a single construct (factor) with similar factor loadings across exam years (electronic appendix 1, Table A1.1, available with the online version of paper). Therefore, factor analysis was performed in data pooled across years. Higher factor scores represent higher neighborhood deprivation, indicated by *higher* neighborhood poverty and proportion with <high school education and *lower* median household income and proportion with a college education. Each respondent's score on this factor was used as a composite neighborhood deprivation measure.

### Outcome: physical activity index

At each examination, frequency of participation in 13 categories of moderate and vigorous recreational sports, exercise, leisure, and occupational activities (electronic appendix 1, Table A1.2, available with the online version of paper) over the previous 12 months was ascertained by an interviewer-administered questionnaire designed for CARDIA. As described elsewhere (Jacobs, Hahn, Haskell, Pirie, & Sidney, 1989), PA scores were calculated in exercise units based on frequency and intensity of each activity. Reliability and validity of the instrument is comparable to other activity questionnaires (Jacobs Jr., Ainsworth, Hartman, & Leon, 1993; Jacobs Jr., Hahn, Haskell, Pirie, & Sidney, 1989). We excluded occupational and household PA from our PA score because they were not theorized to be influenced by the neighborhood environment.

### Individual-level covariates

Individual-level baseline characteristics included age (mean centered), race (white, black), and study center (Birmingham, Chicago, Minneapolis, Oakland). Education at Year 7, after most individuals attained their highest education level, was examined as a time invariant variable; Year 0 education was used if Year 7 education was missing. Time-varying individual-level characteristics included income, marital status (married, not married), and children or stepchildren 18 years or younger living in the household (any, none). Income was examined as a categorical variable (approximate tertiles: <$25,000, $25,000–49,900, > $49,900) because of a non-linear relationship with PA. Income was not collected in year 0 or 2, so the closest measurement (year 5) was analyzed. To avoid over-adjustment and induction of selection bias, we did not control for BMI, a theorized outcome of PA.

### Statistical analysis

Effects of neighborhood deprivation on PA score throughout young to middle adulthood were estimated in a series of longitudinal random and fixed effects linear models. Conditioned on the individual, fixed effect models account for time-invariant unmeasured variables (e.g., motivation to exercise that remains constant over time) which may be related to both PA and neighborhood deprivation (Boone-Heinonen et al.). By analyzing within-person variation observed in repeated measures over time, each individual in essence serves as his/her own control. In contrast, random effects models (random person-level intercept) (Rabe-Hesketh & Skrondal, 2008) analyze variation both within and between individuals; they do not control for possible correlation between observed and unmeasured characteristics and are therefore most comparable to cross-sectional associations reported in prior research. Random slopes were not estimated in order to maintain comparability with fixed effects estimates. See electronic appendix 2 (available with the online version of paper) for a detailed discussion of random and fixed effects models. All models were fit using the Stata 10.1 xtreg function, using the "fe" option for fixed effects models (StataCorp, 2005). The Hausman specification test formally compared fixed and random effects estimates.

In race-stratified random and fixed effect models, PA scores were modeled as a function of three cumulative sets of confounders which were theorized to influence residential selection: Model 1 included concurrent neighborhood deprivation, age and study center; Model 2 added individual-level education and time-varying income; and Model 3 further added time-varying marital status and children in household.

The built environment – such as PA facilities (Diez Roux, Evenson, McGinn, Brown, Moore, Brines et al., 2007; Gordon-Larsen et al., 2006), parks (Bedimo-Rung, Mowen, & Cohen, 2005), and pedestrian infrastructure (Krizek & Johnson, 2006) – and crime (Foster & Giles-Corti, 2008) were theorized to mediate the relationship between neighborhood deprivation and PA and thus not included in our models. Concurrent (versus time lagged) neighborhood deprivation was examined because the neighborhood socioeconomic environment was theorized to represent relatively immediate environmental PA influences. Coefficients for time-invariant individual-level variables (study center, education, sex) are estimated in random effects models but not fixed effects models.

PA scores were natural log-transformed to address skewness, so model coefficients were interpreted as the percent change in PA score expected from a 1-unit change in the corresponding independent variable; analysis of the continuous outcome variable allowed examination of changes across the full distribution of PA. To examine non-linear relationships between neighborhood deprivation and PA (assessed graphically and through testing of higher order terms), neighborhood deprivation was modeled as quartiles (in observations pooled over time). Quartiles were race-specific because, due to residential patterning by race in the U.S. (R. J. Sampson & Sharkey, 2008), there was limited overlap in neighborhood deprivation between whites and blacks. Sex interactions with each independent variable were tested using backward elimination; for comparability, interaction terms were retained if significant (likelihood ratio test, $p<0.10$) in Models 1, 2, or 3 in random or fixed effects models. Time interactions with neighborhood deprivation in Model 1 (using baseline instead of time varying age to avoid collinearity) were not significant and thus excluded; time main effects were not included because the sequencing of observations were not important for our study objective.

Because CARDIA respondents lived in different census tracts over time, our data were not nested across three levels (e.g., multiple people per census tract, multiple time periods per person) as required for multi-level models. Therefore, neighborhood deprivation was treated as an individual-level exposure. Our data were sparse (few individuals on average within census tracts) and unbalanced (variable numbers of individuals), with the following number of individuals per census tract, by study center [mean (range)]: Birmingham [10.1 (1, 46)], Chicago [6.3 (1, 47)], Minneapolis [6.3 (1, 173)], Oakland [5.1 (1, 28)] in year 0, declining to [2.7 (1, 17)], [1.4 (1, 13)], [1.8 (1,27)] and [1.6 (1, 11)] by Year 15, respectively. Intraclass correlations were relatively small (0.08 in pooled data).

## RESULTS

Compared to blacks, whites exhibited substantially higher PA and individual- and neighborhood-level education and income, were more likely to be married, and, except at Year 15, less likely to have children (Tables 1 and 2). Electronic appendix 1, Table A1.3, available with the online version of the paper, reports neighborhood characteristics within each race-specific neighborhood deprivation quartile.

Among blacks, neighborhood deprivation was related to incrementally lower PA, reaching 27% lower PA for those living in neighborhoods with the highest compared to the lowest deprivation (Table 3; random effects Model 1). This relationship did not differ by sex and was attenuated after controlling for individual income and education (Model 2), with no further attenuation after controlling for marriage and children (Model 3). After additionally controlling for time-invariant unmeasured characteristics using fixed effects models, estimates were slightly attenuated (Hausman $p \leq 0.0003$) compared to individual SES-adjusted random effects estimates (Table 3; fixed effects Model 1, random effects Model 2). There was little attenuation after controlling for additional time-varying covariates (Models 2 and 3).

In contrast, among whites, we observed a weak association (or no association) between neighborhood deprivation and lower PA (Table 4; random effects Model 1). Neighborhood deprivation-PA relationships differed by sex in fixed effects models; for comparability, sex interactions were included in random effects models. Associations were attenuated after controlling for individual SES (Model 2), with no additional attenuation after controlling for marriage and children (Model 3). Fixed versus random effects estimates differed significantly (Hausman $p<0.0001$), with substantial attenuation to Model 1 estimates. Similar to blacks, controlling for time-varying covariates in fixed effects Models 2 and 3 resulted in minimal additional attenuation.

## DISCUSSION

In one of the first studies to examine the longitudinal relationship between neighborhood SES and PA, we found that in U.S. adults, high neighborhood deprivation was incrementally associated with lower PA in blacks – reaching 16% lower PA for the most versus least deprived neighborhoods in fully adjusted models – while associations were less consistent in whites. Furthermore, by comparing various strategies to adjust for confounders related to residential selection, we found that adjustment for *measured* characteristics beyond sociodemographics (age, sex, race) and individual SES had little influence on the estimated associations. Adjustment for *unmeasured* time-invariant characteristics via fixed effects models attenuated negative associations in blacks and whites, but associations remained significant and relatively strong in blacks.

Our findings suggest that PA may be one mechanism through which neighborhood deprivation might influence health, particularly in blacks. Our work also improves understanding of residential self-selection bias, a critical limitation to existing neighborhood health research, and raises questions concerning race differences and ongoing methodological limitations (e.g., adjustment for unmeasured time-variant characteristics) of neighborhood health research.

### Race differences

Prior U.S. studies examining neighborhood SES and various health outcomes have similarly found stronger associations in blacks than whites (Chichlowska, Rose, Diez-Roux, Golden, McNeill, & Heiss, 2008; Merkin et al., 2009; Subramanian et al., 2005), while race comparisons in other studies are inconsistent (e.g., (Auchincloss, Diez Roux, Brown, O'Meara, & Raghunathan, 2007; Do et al., 2007; Wen, Kandula, & Lauderdale, 2007)). However, race-specific associations in this study may not be comparable because, like others (Chichlowska et al., 2008; Diez-Roux, Kiefe, Jacobs, Haan, Jackson, Nieto et al., 2001), we examined race-specific neighborhood deprivation quantiles to address dramatic differences in neighborhood deprivation between whites and blacks. Racial disparities in neighborhood SES reflect residential segregation in the U.S. (R. J. Sampson & Sharkey, 2008) and have important implications for social justice. Furthermore, more pronounced associations for the 3$^{rd}$ and 4$^{th}$ quartiles in blacks suggest that the PA gradient with increasing deprivation may sharpen at the highest neighborhood deprivation levels more completely captured in blacks than whites.

Alternatively, stronger associations between neighborhood deprivation and lower PA in blacks than whites may result from mechanisms specific to health behaviors rather than health outcomes (e.g., cardiovascular disease) for which most race-specific estimates are reported. For example, whites may have better access to PA supports (e.g., higher quality PA facilities or stronger social norm) in employment or other nonresidential settings, thereby diluting residential neighborhood deprivation effects. Additionally, due to residential segregation by race, whites living in poor neighborhoods may be less isolated and geographically closer to PA supports in other neighborhoods. Indeed, Auchincloss and colleagues found that distance to a wealthy neighborhood was more strongly associated with insulin resistance than local neighborhood poverty (Auchincloss et al., 2007). Accounting for effects of deprivation in nearby neighborhoods (spatial lag) may be useful to further investigate this issue (Mears & Bhati, 2006).

Heterogeneity in estimated neighborhood deprivation effects by race may also reflect racial differences in unmeasured characteristics. In the U.S., knowledge, attitudes, and resources to engage in healthy lifestyles vary by race (Watters, Satia, & Galanko, 2007; Winkleby, Kraemer, Ahn, & Varady, 1998), and blacks are more likely to experience discrimination (Williams & Mohammed, 2009) and more prolonged periods of individual- and neighborhood-level poverty (Do, 2009). That is, the experience of living in any particular environment may vary by race due to cultural and historical differences not captured in this or similar studies. Such differences may also change over time, particularly throughout the time period (1985–2000) and life stages (young- to middle-adulthood) captured by this study.

## Residential self-selection

We investigated whether measured and unmeasured characteristics related to residential selection – either through differences in economic or cultural constraints or in residential preferences – were important sources of bias. In blacks and whites, the association between neighborhood deprivation and PA was sharply attenuated after controlling for individual-level education and income, which are, aside from race, the strongest predictors of residential selection in U.S. mobility and migration studies (Ioannides & Zabel, 2008;R. J. Sampson & Sharkey, 2008). Marriage and children were theorized to influence residential needs but did not bias the neighborhood deprivation-PA association, perhaps because they were largely captured by traditional sociodemographic control variables.

Comparison of random and fixed effects estimates suggest that confounding by unmeasured time-invariant characteristics was minimal in blacks. In whites, confounding was stronger, but associations generally remained weak and nonsignificant. Relevant unmeasured time-invariant confounders might include preferences for neighborhoods with PA-supportive amenities or other environment characteristics which do not directly support PA, but are correlated with low neighborhood deprivation and more frequently selected by those with high propensity for PA (e.g., upscale suburban neighborhoods). These findings suggest that longitudinal data and incorporation of specific, hypothesized neighborhood attributes are important in neighborhood health studies. Weaker confounding by unmeasured time-invariant characteristics in blacks may reflect the strong role of race in neighborhood selection in the U.S. (Clark & Ledwith, 2007;R. J. Sampson & Sharkey, 2008), thereby constraining the role of residential preferences in blacks.

Importantly, in both blacks and whites, unmeasured characteristics that vary over time were not addressed by fixed effects models and may contribute additional bias. For example, proximity to work, school, or social networks may be critical drivers of residential selection which may change over time and is, in this and most other studies, unmeasured. To the extent that proximity to such resources is also related to neighborhood deprivation and PA, the neighborhood deprivation-PA association may be biased. To illustrate, one possible non-causal explanation for negative fixed effects neighborhood deprivation-PA associations might be that individuals who start new, more demanding jobs may have less leisure time for PA and move to neighborhoods closer to work (which may be closer to commercial centers and have low neighborhood SES). That is, employment factors may influence both residential selection and PA, is unmeasured, and changes over time, therefore resulting in bias.

In sum, individual SES, a typical control variable, was the most important measured confounder, but longitudinal analysis controlled for additional confounding. However, the role of time-varying unmeasured characteristics should be further investigated with instrumental variables or other simultaneous equation strategies which model residential selection factors that affect residential choice but not the outcome measure (Boone-Heinonen et al.).

## Implications and future research

Our study, which examined PA – a specific modifiable outcome known to strongly influence health (Haskell, Lee, Pate, Powell, Blair, Franklin et al., 2007) – using a longitudinal modeling strategy to address bias due to time-invariant, unmeasured residential self-selection factors, provides support that the neighborhood environment influences health and may therefore be a promising intervention focus. Differences in association by race further suggest that neighborhood-level interventions may benefit from being targeted to groups particularly sensitive to neighborhood environments.

However, specific policy implications require better understanding of mechanisms through which neighborhood deprivation influences PA and through which racial differences might arise. We hypothesize that inequitable distribution of built environment features such as PA facilities (Gordon-Larsen et al., 2006) is an important pathway. In this study, we did not adjust for potentially mediating neighborhood characteristics, but mediation analysis of such variables may help to develop policy strategies related to built or other modifiable neighborhood characteristics.

Mediation analysis can correspondingly reveal the importance of pathways other than the built environment. Neighborhood deprivation may capture multiple synergistic paths through which financial and social resources of a community might influence PA including not only PA opportunities (built environment) but also social cues and social support for PA. Indeed, neighborhood deprivation-PA associations in this study were stronger than typical built environment-PA associations (Wendel-Vos, Droomers, Kremers, Brug, & van Lenthe, 2007), perhaps because alternative mechanisms (e.g., social norms) influence the wide range of activities captured by our PA measure. Research clarifying the role of alternative pathways is needed to develop effective policy strategies.

Methodological implications of our findings include the importance of stratifying by race and the use of longitudinal data. Future research might include better analytical strategies to handle residential segregation, perhaps through stratified sampling (Oakes, Forsyth, & Schmitz, 2007) to obtain adequate sample size in various race-neighborhood deprivation classifications.

While we conceptualized PA as a mediating behavior in the well documented association between neighborhood SES and health, we did not address the potential role of health status on residential sorting (Boyle et al., 2002; Curtis et al., 2009). Those with declining health status are likely to have limited PA and more often relocate to areas of lower neighborhood SES, thereby magnifying associations between neighborhood SES and PA or health. However, such complex dynamic relationships involve their own set of selection and other biases which may be addressed using marginal structural models (Robins, Hernan, & Brumback, 2000), structural equation modeling (Kline, 2005), or similar strategies. Additionally, neighborhood SES may influence individual SES, so similar modeling may be required to avoid overadjustment by individual SES (Diez Roux, 2005; Do & Finch, 2008).

Finally, we assumed relatively immediate effects of the neighborhood environment on PA. Cumulative effects of neighborhood SES (Do, 2009) on PA or other behaviors, possibly through habits and attitudes established early in life, should also be investigated. Fixed effects models may be limited for outcomes with longer development duration because the time intervals examined may be too short to capture the corresponding lag time and may exhibit temporal mismatch with the relevant etiologic periods (Glymour, 2008).

## Strengths and Limitations

In addition to the methodological issues described above, limitations of our study include self-reported PA and individual characteristics. However, bias resulting from measurement error in our outcome and exposure is minimized because errors in self-reported PA and in objective neighborhood deprivation likely arise from different mechanisms and are therefore unlikely to be correlated. Additionally, the CARDIA PA score has been validated (Jacobs Jr. et al., 1993; Jacobs Jr. et al., 1989) and shown to be an important predictor of numerous health outcomes (e.g., (Carnethon, Jacobs, Sidney, Sternfeld, Gidding, Shoushtari et al., 2005; Schmitz, Jacobs, Leon, Schreiner, & Sternfeld, 2000)). Second, our analysis may have omitted important residential selection factors such as car ownership and employment type and location, which are not readily available for our sample. Third, there is slight temporal

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mismatch between the census and exam periods. However, neighborhood deprivation is unlikely to change rapidly enough to result in substantial exposure misclassification. Fourth, our results are vulnerable to selection bias, but fixed effects models may help to mitigate bias by controlling for time invariant unmeasured characteristics related to attrition and missing data. Fifth, our measure of neighborhood deprivation may be a poor proxy for neighborhood features relevant to PA such as social organization, but it is consistent with prior work examining obesity-related behaviors and outcomes (Auchincloss et al., 2007; Diez-Roux et al., 2001). Finally, log transformation of our outcome addressed skewness, but may have overemphasized changes for low PA levels.

However, our study is unique in using longitudinal data to estimate the effects of neighborhood deprivation on PA, a potential mediator between neighborhood SES and health. Our study population was large, sociodemographically and geographically diverse, and followed over 15 years. Lastly, we used several objective measures of neighborhood SES, which may better capture neighborhood SES than any single measure.

### Conclusion

After accounting for several potential sources of residential self-selection bias, we found strong estimated longitudinal effects of neighborhood deprivation on lower PA in U.S. blacks but not whites, although racial stratification makes race comparisons problematic. Typical adjustment variables (e.g., individual SES) were the most influential confounders, but time-invariant unmeasured characteristics also appear to contribute bias. Our examination of PA is a first step toward understanding mechanisms of neighborhood influences on health, but identification of policy strategies requires more research examining the role of the built environment and other environment factors in the relationship between neighborhood SES and PA.

## Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

## Acknowledgments

Funding for this study comes from the National Institutes of Health: (R01 HL104580, R01-CA109831, R01-CA121152). Additional funding has come from NIH (R01-AA12162 & DK056350), the UNC-CH Center for Environmental Health and Susceptibility (CEHS) (NIH P30-ES10126), the UNC-CH Clinic Nutrition Research Center (NIH DK56350), and the Carolina Population Center; and from contracts with the University of Alabama at Birmingham, Coordinating Center, N01-HC-95095; University of Alabama at Birmingham, Field Center, N01-HC-48047; University of Minnesota, Field Center, N01-HC-48048; Northwestern University, Field Center, N01-HC-48049; Kaiser Foundation Research Institute, N01-HC-48050 from the National Heart, Lung and Blood Institute.

Analysis was supported by the Interdisciplinary Obesity Training postdoctoral fellowship (T32MH075854-04). The authors would like to thank Drs. Barbara Sternfeld and Joshua S. Richman for their comments on the manuscript, Brian Frizzelle, Marc Peterson, Chris Mankoff, James D. Stewart, Phil Bardsley, and Diane Kaczor of the University of North Carolina, Carolina Population Center (CPC) and the CPC Spatial Analysis Unit for creation of the environmental variables, and Ms. Frances Dancy for her helpful administrative assistance.

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## Table 1

Individual-level sample characteristics by exam year[a] and race[b] [mean/% (standard error)]: the Coronary Artery Risk Development in Young Adults (CARDIA) Study, 1985–2000

| | Blacks | | | | Whites | | | |
|---|---|---|---|---|---|---|---|---|
| | Year 0 | Year 7 | Year 10 | Year 15 | Year 0 | Year 7 | Year 10 | Year 15 |
| Count | 2,037 | 1,861 | 1,864 | 1,665 | 2,142 | 2,022 | 1,975 | 1,894 |
| Female (%) | 57.4 | | | | 53.0 | | | |
| Education (%) | | | | | | | | |
| >=College degree | 12.1 | 20.6 | 22.5 | 25.6 | 41.4 | 55.8 | 57.6 | 60.6 |
| Some college | 7.5 | 14.2 | 15.1 | 20.6 | 5.5 | 6.6 | 9.3 | 11.0 |
| ≤High School | 80.5 | 65.2 | 62.4 | 53.8 | 53.1 | 37.5 | 33.1 | 28.4 |
| Income (%) | | | | | | | | |
| >$50k | 15.3 | 18.2 | 24.1 | 42.7 | 32.2 | 40.8 | 51.6 | 72.4 |
| $25–49.9k | 34.1 | 36.3 | 36.5 | 31.4 | 40.8 | 37.1 | 32.7 | 20.0 |
| <$25k | 50.6 | 45.5 | 39.4 | 25.9 | 27.0 | 22.1 | 15.7 | 7.6 |
| Married (%) | 20.4 | 34.6 | 38.2 | 43.2 | 25.7 | 51.9 | 58.2 | 62.4 |
| Children[c] in HH (%) | 38.4 | 56.0 | 58.5 | 58.0 | 16.3 | 41.0 | 49.5 | 56.9 |
| PA Index (mean) | 338.6 (278.9) | 261.8 (257.0) | 246.2 (248.3) | 250.5 (258.6) | 392.4 (260.1) | 290.0 (227.0) | 281.0 (228.3) | 293.6 (238.6) |
| Age (mean) | 24.4 (3.8) | 31.5 (3.8) | 34.5 (3.8) | 39.6 (3.8) | 25.5 (3.4) | 32.5 (3.4) | 35.6 (3.4) | 40.7 (3.4) |

[a] All variables significantly different by race, within year ($p<0.0001$) except PA index at year 7 ($p=0.0003$) and children in household at year 15 ($p>0.05$)

[b] All variables significantly different at each year compared to year 0, within race ($p<0.0001$, except income between Year 7 and 0 for blacks, $p=0.004$)

[c] Children or stepchildren living in household

## TABLE 2

Neighborhood socioeconomic measures, by exam year[a] and race[b] [median, (interquartile range)]: the Coronary Artery Risk Development in Young Adults (CARDIA) Study, 1985–2000

| | | Year 0 | Year 7 | Year 10 | Year 15 |
|---|---|---|---|---|---|
| BLACKS | | | | | |
| %<150% poverty level | | 35.1 (25.8, 46.4) | 33.9 (19.8, 45.6) | 27.1 (13.6, 41.9) | 26.7 (13.3, 39.5) |
| | difference [t−(t−1)] | | 0.1 (−11.1, 7.1) | 0.0 (−15.5, 0.3) | 0.0 (−5.8, 4.4) |
| %<High School | | 38.1 (27.2, 47.3) | 29.8 (17.8, 39.2) | 26.9 (15.6, 37.1) | 21.0 (12.1, 32.0) |
| | difference [t−(t−1)] | | −7.0 (−15.0, −0.4) | 0.0 (−9.5, 2.2) | −3.8 (−9.7, 1.5) |
| %≥college degree | | 10.0 (5.7, 20.8) | 13.8 (6.6, 23.9) | 15.1 (8.3, 25.3) | 16.5 (9.6, 32.1) |
| | difference [t−(t−1)] | | 1.9 (−1.0, 7.9) | 0.0 (−2.1, 5.9) | 2.2 (−1.4, 8.2) |
| median income | | 25,811 (20,771, 31,878) | 30,880 (23,050, 40,595) | 33,911 (25,150, 44,636) | 37,813 (27,930, 50,718) |
| | difference [t−(t−1)] | | 3,562 (−1,565, 11,479) | 0 (−1,110, 10,565) | 2,509 (−1,984, 8,770) |
| Neighborhood deprivation factor score[c] | | 0.89 (0.27, 1.28) | 0.56 (−0.17, 1.06) | 0.29 (−0.36, 0.93) | 0.10 (−0.56, 0.76) |
| | difference [t−(t−1)] | | −0.24 (−0.68, 0.08) | 0.00 (−0.64, 0.09) | −0.16 (−0.47, 0.10) |
| WHITES | | | | | |
| %<150% poverty level | | 17.8 (10.5, 30.9) | 12.7 (8.0, 23.0) | 10.5 (5.9, 18.8) | 10.3 (5.7, 18.2) |
| | difference [t−(t−1)] | | −2.1 (−11.0, 2.8) | 0.0 (−6.7, 0.0) | −0.2 (−2.3, 1.8) |
| %<High School | | 21.6 (12.0, 29.4) | 12.7 (6.5, 20.2) | 12.1 (6.4, 20.2) | 9.1 (4.3, 15.9) |
| | difference [t − (t−1)] | | −6.9 (−13.7, −1.4) | 0.0 (−3.8, 2.8) | −2.5 (−6.2, 0.2) |
| %≥college degree | | 26.2 (17.2, 42.8) | 34.5 (18.1, 51.5) | 32.5 (18.3, 48.6) | 39.0 (22.9, 57.1) |
| | difference [t−(t−1)] | | 6.3 (−0.9, 14.6) | 0.0 (−8.6, 3.7) | 6.2 (1.2, 11.5) |
| median income | | 32,039 (24,979, 42,983) | 44,865 (35,036, 58,026) | 48,336 (37,826, 63,711) | 54,232 (41,750, 71,172) |
| | difference [t−(t−1)] | | 9,499 (2,524, 21,579) | 0 (−1,291, 13,134) | 4,670 (−337, 11,536) |
| Neighborhood deprivation factor score[c] | | −0.02 (−0.54, 0.49) | −0.53 (−1.05, −0.02) | −0.63 (−1.09, −0.09) | −0.83 (−1.37, −0.31) |
| | difference [t − (t−1)] | | −0.41 (−0.85, −0.12) | 0.00 (−0.40, 0.13) | −0.21 (−0.42, 0.00) |

[a] All variables significantly different by race, within year ($p<0.0001$)

[b] All differences variables significantly different from 0, within race ($p<0.0001$, except %≥college degree between year 7 & 10 for blacks, $p=0.0034$; %<High School between years 7 and 10 for whites, $p=0.0118$; and %<150% poverty level between years 10 and 15 for whites, $p=0.0061$).

[c] Higher scores represent higher neighborhood deprivation.

## TABLE 3

Longitudinal relationship between composite neighborhood deprivation factor and ln(physical activity) [coefficient (95% CI)] – Blacks: the Coronary Artery Risk Development in Young Adults (CARDIA) Study, 1985–2000[a]

| | Model 1[b] | Model 2[c] | | Model 3[d] | |
|---|---|---|---|---|---|
| | | Males | Females | Males | Females |
| **Random effects** | | | | | |
| Neighborhood deprivation[e] quartiles (vs. 1st quartile) | | | | | |
| 2 | **−0.13 (−0.21, −0.04)** | −0.08 (−0.17, 0.00) | | **−0.09 (−0.17, 0.00)** | |
| 3 | **−0.24 (−0.33, −0.16)** | **−0.17 (−0.26, −0.08)** | | **−0.18 (−0.27, −0.09)** | |
| 4 | **−0.27 (−0.37, −0.17)** | **−0.18 (−0.28, −0.08)** | | **−0.19 (−0.29, −0.09)** | |
| Education (vs. ≥college degree) | | | | | |
| some college | | **0.27 (0.04, 0.50)** | −0.15 (−0.33, 0.04) | **−0.39 (−0.69, −0.10)** | −0.12 (−0.31, 0.06) |
| ≤High School | | −0.08 (−0.24, 0.09) | **−0.31 (−0.45, −0.17)** | −0.19 (−0.41, 0.02) | **−0.27 (−0.41, −0.13)** |
| Income (vs.≥$50k) | | | | | |
| $25–49.9k | | **−0.17 (−0.25, −0.08)** | | **−0.22 (−0.30, −0.13)** | |
| <$25k | | **−0.32 (−0.41, −0.22)** | | **−0.39 (−0.49, −0.30)** | |
| Married (vs. not married) | | | | **−0.21 (−0.29, −0.13)** | |
| Children <18 years (vs. none) | | | | 0.09 (−0.02, 0.20) | **−0.10 (−0.20, −0.01)** |
| **Fixed effects[f]** | | | | | |
| Neighborhood deprivation[e] quartiles (vs. 1st quartile) | | | | | |
| 2 | −0.04 (−0.14, 0.06) | −0.03 (−0.13, 0.07) | | −0.04 (−0.14, 0.06) | |
| 3 | **−0.12 (−0.22, −0.01)** | −0.10 (−0.21, 0.01) | | **−0.12 (−0.22, −0.01)** | |
| 4 | **−0.16 (−0.28, −0.04)** | **−0.14 (−0.26, −0.02)** | | **−0.16 (−0.28, −0.04)** | |
| Income (vs. ≥$50k) | | | | | |
| $25–49.9k | | **−0.14 (−0.24, −0.04)** | | **−0.19 (−0.29, −0.09)** | |
| <$25k | | **−0.20 (−0.32, −0.08)** | | **−0.27 (−0.40, −0.15)** | |
| Married (vs. not married) | | | | **−0.23 (−0.32, −0.13)** | |
| Children <18 years (vs. none) | | | | −0.02 (−0.15, 0.12) | **−0.15 (−0.27, −0.02)** |

[a]CARDIA Study data from years 0, 7, 10, and 15. Physical activity was natural log transformed to address skewness. A single estimate indicates no significant sex interaction in any of the three models. Bold font indicates statistical significance ($p<0.05$)

[b]Model 1 adjusts for study center, age, $age^2$, $age^3$, sex, interaction between sex and age terms, and interaction between sex and study center

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[c] Model 2 adjusts for Model 1 variables, education, income, and interaction between sex and education

[d] Model 3 adjusts for Model 1 and 2 variables, marital status, any children <18 living in the household, and interaction between sex and children in household

[e] Higher scores represent higher neighborhood deprivation.

[f] Fixed effects models do not include time invariant characteristics, including education, which showed small variability over time.

**TABLE 4**

Longitudinal relationship between composite neighborhood deprivation factor and ln(physical activity) [coefficient (95% CI)] – Whites: Coronary Artery Risk Development in Young Adults (CARDIA) Study, 1985–2000[a]

| | Model 1[b] | | Model 2[c] | | Model 3[d] | |
|---|---|---|---|---|---|---|
| | Males | Females | Males | Females | Males | Females |
| **Random effects** | | | | | | |
| Neighborhood deprivation[e] quartiles (vs. 1st quartile) | | | | | | |
| 2 | −0.09 (−0.18, 0.00) | −0.05 (−0.13, 0.04) | −0.06 (−0.15, 0.03) | −0.02 (−0.11, 0.06) | −0.07 (−0.16, 0.02) | −0.05 (−0.13, 0.04) |
| 3 | **−0.12 (−0.22, −0.03)** | **−0.16 (−0.25, −0.07)** | −0.08 (−0.18, 0.01) | −0.12 (−0.21, −0.03) | −0.09 (−0.18, 0.01) | **−0.15 (−0.24, −0.06)** |
| 4 | **−0.16 (−0.26, −0.05)** | −0.09 (−0.19, 0.01) | −0.09 (−0.20, 0.02) | −0.03 (−0.13, 0.07) | −0.10 (−0.21, 0.00) | −0.04 (−0.14, 0.06) |
| Education (vs. ≥college degree) | | | | | | |
| some college | | | **−0.28 (−0.42, −0.14)** | | **−0.26 (−0.40, −0.12)** | |
| ≤HS | | | **−0.32 (−0.39, −0.24)** | | **−0.29 (−0.36, −0.22)** | |
| Income (vs. ≥$50k) | | | | | | |
| $25–49.9k | | | **−0.07 (−0.12, −0.01)** | | **−0.10 (−0.16, −0.05)** | |
| <$25k | | | **−0.15 (−0.22, −0.07)** | | **−0.19 (−0.27, −0.12)** | |
| Married (vs. not married) | | | | | **−0.13 (−0.19, −0.08)** | |
| Children <18 years (vs. none) | | | | | −0.02 (−0.10, 0.07) | **−0.23 (−0.30, −0.15)** |
| | | | | | −0.02 (−0.10, 0.07) | **−0.23 (−0.30, −0.15)** |
| **Fixed effects**[f] | | | | | | |
| Neighborhood deprivation[e] quartiles (vs. 1st quartile) | | | | | | |
| 2 | −0.02 (−0.12, 0.09) | 0.00 (−0.09, 0.10) | −0.02 (−0.12, 0.09) | 0.00 (−0.09, 0.10) | −0.03 (−0.13, 0.07) | −0.02 (−0.12, 0.07) |
| 3 | −0.02 (−0.13, 0.09) | −0.04 (−0.14, 0.07) | −0.01 (−0.13, 0.10) | −0.02 (−0.13, 0.08) | −0.03 (−0.14, 0.08) | −0.07 (−0.17, 0.03) |
| 4 | −0.04 (−0.17, 0.08) | 0.10 (−0.02, 0.21) | −0.04 (−0.16, 0.09) | 0.10 (−0.01, 0.22) | −0.05 (−0.18, 0.07) | 0.08 (−0.03, 0.19) |
| Income (vs. ≥$50k) | | | | | | |
| $25–49.9k | | | −0.01 (−0.07, 0.05) | | −0.04 (−0.10, 0.02) | |
| <$25k | | | −0.05 (−0.13, 0.04) | | −0.07 (−0.16, 0.01) | |
| Married (vs. not married) | | | | | **−0.15 (−0.21, −0.08)** | |
| Children <18 years (vs. none) | | | | | −0.06 (−0.15, 0.04) | **−0.23 (−0.33, −0.14)** |

[a]CARDIA Study data from years 0, 7, 10, and 15. Physical activity was natural log transformed to address skewness. A single estimate indicates no significant sex interaction in any of the three models. Bold font indicates statistical significance (p<0.05)

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[b] Model 1 adjusts for study center, age, age$^2$, age$^3$, sex, interaction between sex and age terms, and interaction between sex and study center

[c] Model 2 adjusts for Model 1 variables, education, income, and interaction between sex and education

[d] Model 3 adjusts for Model 1 and 2 variables, marital status, any children <18 living in the household, and interaction between sex and children in household

[e] Higher scores represent higher neighborhood deprivation.

[f] Fixed effects models do not include time invariant characteristics, including education, which showed small variability over time.