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Racial Inequalities in Adolescents' Exposure to Racial and Socioeconomic Segregation, Collective Efficacy, and Violence

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ABSTRACT In the United States, Black youth tend to grow up in remarkably less resourced neighborhoods than White youth. This study investigates whether and to what extent Black youth are moreover exposed to less resourced activity spaces beyond the home. We draw on GPS data from a large sample of urban youth in the Columbus, Ohio–based Adolescent Health and Development in Context study (2014–2016) to examine to what extent Black youth experience nontrivial, disproportionate levels of exposure to more disadvantaged and segregated contexts in their daily routines compared with similarly residentially situated White youth. Specifically, we estimate Black–White differences in nonhome exposure to concentrated disadvantage, racial segregation, collective efficacy, and violent crime. We find that Black youths' activity spaces have substantially higher rates of racial segregation and violent crime than those of White youth, and substantially lower levels of collective efficacy—even after accounting for a host of individual- and home neighborhood–level characteristics. We find more modest evidence of differences in exposure to socioeconomic disadvantage. These findings have important implications for neighborhood-centered interventions focused on youth well-being and the contextual effects and segregation literatures more generally.

KEYWORDS Activity space • Collective efficacy • Exposure to violence • GPS • Segregation

Introduction

Despite declines in racial segregation, Black youth remain disproportionately exposed to socioeconomic disadvantage in their neighborhoods, with significant implications for inequalities in well-being (Reardon and Bischoff 2011; Sharkey and Faber 2014). Indeed, highly disadvantaged neighborhoods tend to be further characterized by heightened levels of health-relevant risk factors, such as violent crime, as well as a reduced prevalence of protective factors, such as collective efficacy—all of which influence youth well-being (Sampson 2012; Sharkey 2018). An emerging literature additionally calls attention to potential racial inequalities in youths'

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activity spaces beyond the neighborhood, with recent research underscoring that youth spend relatively little time in the neighborhood outside their home (Browning, Calder et al. 2021; Zenk et al. 2019). Mounting evidence suggests that inequalities in activity space exposures have significant consequences for population health (Cagney et al. 2020), but little research has examined whether Black and White youth navigate racially segregated activity spaces. This omission is particularly important considering the mixed success of residential mobility interventions focused on Black adolescents' development and well-being (Chetty et al. 2016). To the extent that these youth are disproportionately drawn to more disadvantaged activity spaces beyond the neighborhood, residential neighborhood–centered interventions may be excessively optimistic regarding returns for Black adolescents (Clampet-Lundquist et al. 2011; Graif 2015).

This study examines Black–White differences in the composition of activity spaces beyond the home using data from the Adolescent Health and Development in Context study, a representative study of 1,405 youth aged 11–17 and their caregivers in Franklin County, Ohio. Five days of smartphone-based GPS data corroborated using a recall-aided, space–time budget methodology were used to construct individual-level measures of nonhome exposure compositions based on census block group aggregations. We focus on exposure to concentrated disadvantage, racial segregation, collective efficacy, and violent crime given the major relevance of these processes to youth well-being (Sharkey and Faber 2014).

Background

Mirroring pervasive patterns of racial residential segregation in the United States, a growing literature on the demography of everyday mobility points to potential racial inequalities in activity spaces. Some of this research aligns with expectations of the prominent "geographic" or "social isolation" perspective on urban segregation, anticipating that activity space compositions largely reflect compositions of residents' neighborhoods (Wang et al. 2018; Wilson 1987). Evidence is growing for a more dynamic "compelled mobility" perspective, however, demonstrating that urban residents—and particularly those residing in disadvantaged neighborhoods—experience far more heterogeneity in exposure to neighborhood characteristics than would be predicted by the social isolation approach (Browning et al. forthcoming). Although urban exposures are likely more complex and heterogeneous than previously acknowledged, mobility dynamics are nevertheless expected to be characterized by substantial disparity in everyday exposures by race, even for Black and White youth residing in similar neighborhoods.

A variety of factors are expected to account for these heterogeneous but racially disparate patterns of exposure, including those that *pull* individuals out of their neighborhoods toward settings with resources and network ties, as well as those that *push* them away from some areas, such as a reduced sense of safety. Pull factors are rooted in the segregation of people and resources characterizing most contemporary U.S. cities. On one hand, activity locations are increasingly clustered within nonresidential areas of cities, requiring residents of both disadvantaged and advantaged neighborhoods to leave their neighborhood to access resources

(Frumkin 2002; Tana et al. 2016). The outdated conception of neighborhoods as the center of urban life is likely least relevant for residents of disadvantaged neighborhoods and those with a high proportion of Black residents, however (Browning, Calder et al. 2021). Such neighborhoods tend to contain fewer health-related organizations (Anderson 2017), nonprofits (Crubaugh 2021), schools (Owens 2020), and businesses (Small and McDermott 2006; Small et al. 2021), forcing residents of disadvantaged neighborhoods, in particular, to travel beyond the neighborhood—often to more advantaged areas—to access these resources. In contrast, racial segregation in network ties and institutional affiliations likely contributes to racial segregation in activity space by driving youth to same-race-dominated areas of cities, regardless of home residence (Krysan and Crowder 2017; Small 2007; Small and Feldman 2012). Indeed, research has long acknowledged the tendency toward racial homophily in interpersonal network ties (McPherson et al. 2001; Small and Adler 2019). Krivo et al. (2013), for example, found that adult Black and Latino residents of Los Angeles tend to have routine activities in more disadvantaged census tracts relative to comparable White residents.

These pull factors suggest that while Black and White youth are exposed to more heterogeneous environments than previously thought, there will still be Black–White differences in activity space exposures even for youth residing in the same neighborhood. For instance, a Black youth residing in a disadvantaged neighborhood would be expected to spend a nontrivial amount of time in more advantaged areas as they seek organizational resources. However, a White youth residing in the same neighborhood likely spends more time in these advantaged contexts because of the pull of both organizational resources and network ties. Similarly, White youth residing in more advantaged contexts may satisfy most organizational and social needs in comparably advantaged neighborhoods, while Black youth living in advantaged neighborhoods would experience the pull of social ties and some institutional linkages in more disadvantaged neighborhoods.

A compelled mobility perspective further acknowledges that mobility patterns are a product not just of resource-seeking and network ties, but also of *push* factors that lead youth to avoid certain environments. For example, recent residential segregation research suggests that housing selection results from constrained access to information and perceptions of which neighborhoods may be less welcoming (Krysan and Crowder 2017). Networks and residential histories contribute to residents' heuristics of place, shaping where movers seek to relocate net of influences of socioeconomic resources and overt discrimination. Crucially, this research underscores the contribution of tacit *nonexclusionary discrimination*[1] and the *anticipation* of discrimination against minority home-seekers in shaping residential consideration sets (Krysan and Crowder 2017). The significance of anticipated discrimination likely extends to the activity patterns of minority youth, pushing them and their parents away from—or limiting their time in—some affluent areas in the course of their daily lives. For instance, anti-Black hate crimes are most numerous in low-proportion-minority neighborhoods with high rates of informal social control (Lyons 2007), and Black

[1] This is defined as "actions and practices that occur within an already established housing arrangement most often entailing racial harassment, differential treatment of tenants, or disparate application of contractual terms and conditions of residency" (Roscigno et al. 2009:52).

urbanites are keenly aware of the heightened scrutiny and mistrust they are likely to encounter in affluent areas (Anderson 2015; Feagin 1991; Krysan and Farley 2002; Lee 2000). Moreover, Black youth are frequently overpoliced by law enforcement and residents within more affluent communities (Anderson 2015; Feagin 2010; Plant and Peruche 2005;), with consequences for their well-being (DeAngelis 2022; Geller et al. 2014; Sewell et al. 2016; Young 2018).

Some evidence that these factors lead to segregation in youths' activity spaces comes from the Moving to Opportunity residential housing experiment (MTO), which randomized Black and Hispanic residents of high-poverty neighborhoods into treatment and control groups, the former of which involved relocation to lower poverty neighborhoods (Briggs et al. 2010). Though beneficial effects were found for participants who moved at younger ages, adolescent participants experienced slightly negative effects early on and later in adulthood (Chetty et al. 2016; Schmidt et al. 2018). An important insight has been that treatment group adolescents frequently faced adversities in their advantaged neighborhoods and schools, often leading to more time spent in (and relocation back to) disadvantaged neighborhoods (Briggs et al. 2008; Clampet-Lundquist et al. 2011; Sampson 2008). Treatment group males were particularly likely to struggle, reporting a heightened sense of scrutiny in their residential neighborhood, in addition to continued reliance on social ties to more disadvantaged neighborhoods (Boyd and Clampet-Lundquist 2019; Zuberi 2012). These observations suggest that, even when residing in more advantaged neighborhoods, Black youth may attempt to avoid these settings, leading to disproportionate exposure to more disadvantaged activity spaces compared with similar White youth.

The Present Study

This study examines Black–White differences in exposure to disadvantaged activity spaces. We hypothesize that, compared with White youth, Black youth will be exposed to higher levels of concentrated disadvantage and a higher proportion of Black residents in their activity spaces net of differences in home neighborhood conditions. We further expect Black youth will be disproportionately exposed to adverse health-related risk factors that typically cluster within more disadvantaged areas, focusing on lower collective efficacy and higher block group–level rates of violent crime (Sampson et al. 1997). This focus is consistent with research highlighting the relevance of these measures to delinquency and victimization (Wikström et al. 2012) and physical and mental health (Ahern and Galea 2011) among youth.

We additionally consider whether inequalities in nonhome exposures between Black and White adolescents vary by age, biological sex, levels of concentrated disadvantage in one's neighborhood, or having recently moved addresses. We do so given the evidence indicating that parents often allow older adolescent males more leeway to traverse neighborhoods than younger and female youth, and given the gender- and age-dependent findings from residential mobility interventions (Graif 2015; Spilsbury 2005). We assess racial disparities by neighborhood disadvantage and having recently moved to ensure that our findings are generalizable among residents of low-disadvantage areas and those more established in their neighborhood.

Data and Measures

The Adolescent Health and Development in Context (AHDC) study is a longitudinal data collection effort focused on the consequences of everyday contexts for health and well-being. The study was conducted in an urban and suburban area within Interstate 270—the Franklin County outer belt, including the majority of the city of Columbus and numerous inner suburbs. Wave 1 of AHDC is a representative sample of study-area permanent residences with youth aged 11–17 and an English-speaking caregiver collected between 2014 and 2016. The sampling frame was based on a combination of a vendor-provided list of potentially eligible households and data from public school districts representing households in the study area. The American Association for Public Opinion Research Response Rate 3—or the proportion of contacted households estimated to be eligible for inclusion that completed interviews—is 21.3%.[2] The AHDC sample is approximately representative of the population of youth in the study area with respect to race and household income (Boettner et al. 2019; Browning, Calder et al. 2021). The Columbus area is roughly average on key indicators of racial composition and segregation for large U.S. metropolitan areas: in a recent analysis of 51 major metropolitan areas, Columbus had a dissimilarity index score comparable to the overall average (62.2 vs. 59.0) and Black prevalence nearly equivalent to the mean (14.9% vs. 15.0%) (Frey 2018). For more information on the AHDC sampling design and study area, see Boettner et al. (2019) and Browning, Calder et al. (2021).[3]

Data were collected in weeklong periods with day of study entry varying across respondents. First, an *entrance survey* was administered to both caregivers and a focal youth covering demographic and socioeconomic background, household composition, family structure and marital status, employment and income, health, social support, behavior, mental and physical health, schooling, family conflict, and legal troubles. The questionnaires include separate modules on the geographic coordinates of places to which the caregiver and youth are regularly exposed (e.g., school, friends' houses). The entrance survey was followed by a seven-day smartphone-based Geographically Explicit Ecological Momentary Assessment (GEMA) (Kirchner and Shiffman 2016) period for the youth, combining GPS tracking and ecological momentary assessment to examine youth perceptions, behaviors, and activity space

[2] This response rate is consistent with recent survey response trends (Ghandour et al. 2018; National Research Council 2013). Research assessing the influence of response rates finds little evidence of an association between response rates and response bias (Czajka and Beyler 2016), and effects of response bias in multivariable models have been demonstrated to be limited when design variables are controlled for (Amaya and Presser 2017; Rindfuss et al. 2015).

[3] Table 1 in the online appendix displays racial-ethnic and income distributions for AHDC youth and 2009–2013 American Community Survey (ACS) youth aged 11–17 residing in the study area. The AHDC and ACS distributions are remarkably similar, although the proportion of Black residents is somewhat higher for the AHDC than for the population estimate (37.9% vs. 31.9%).

locations across the study week. During the in-home interview, the interviewer provided a GPS-enabled smartphone to the youth with instructions to carry the phone continuously for the seven-day period. The GPS feature of the study facilitated collection of in-the-moment data on locations at which youth spend time through continuous tracking (except during in-school hours). The phone app prioritizes spatial data from more accurate GPS satellites, logging location data every 30 seconds when connected. If no GPS satellite position has been saved in the last 10 minutes, location coordinates based on cell tower network position are collected every 60 seconds. If location services are turned off, the study application sends a prompt to remind the participant to turn services back on. The GPS data upload to secure servers every hour.

At the end of the seven-day period, the interviewer returned to the youth's home for a follow-up *exit survey*, during which the adolescent participant completed a recall-aided interactive space–time budget covering Friday, Saturday, Sunday, and the two most recent weekdays. Consistent with the broader time-use literature, this approach aims to adequately capture mobility occurring both during the school week and on the weekend when youth may have more spatial autonomy (Hofferth 2009). Prior to administering the space–time budget, the GPS data are processed using a convex, hull-based binning algorithm that summarizes data points into stationary and travel periods. The space–time budget application takes the output of the convex hull processing of the raw GPS data and displays estimated locations to the respondent. Each location is combined with labels from nearby routine location self-reports from the entrance survey along with Google Places search results; the respondent can then report whether each stable location was associated with a routine location or a Google Places result, write in other text, or change the location coordinates as needed for the corresponding five days of location data of the GEMA week. Our focus on five days of coverage aligns with recent research validating that as little as 1–6 days sufficiently captures between-person variability in activity spaces (Zenk et al. 2018).

Our analyses draw on these GEMA data to measure individual-level nonhome activity space compositions. We employ location data from the space–time budget, geocoding coordinates from locations encountered over the five-day period to census block groups. Home census tract and nonhome block group census characteristics are constructed using the American Community Survey 2009–2013 five-year file (Manson et al. 2021). *Individual-level "nonhome" activity space measures* are calculated by aggregating exposure data from the recall-aided space–time budget information provided by the youth over five days of the GEMA/GPS week (Boettner et al. 2019). We define "nonhome" locations as those the respondent identified as being separate from the home address or those at least 30 meters away from the home address in instances in which the respondent did not identify a location. We calculate nonhome individual-level mean exposure to block group characteristics across all locations within the study area of the I-270 Columbus outer belt boundary for the week, weighted by time spent in minutes at each location during waking hours.[4]

[4] Sensitivity analyses in which nonhome exposure to concentrated disadvantage, proportion Black, and violent crime include time spent beyond the I-270 study area boundary but within Franklin County yield conclusions identical to those discussed here.

Home neighborhood measures are based on the census tract characteristics for the respondent's self-reported primary address.[5,6]

Activity Space and Neighborhood Compositions

Concentrated disadvantage is a scale averaging together the following block group–level characteristics: poverty rate, unemployment rate, percentage of female-headed households, and percentage of households receiving cash assistance. These items are similarly combined at the census tract level to operationalize home neighborhood concentrated disadvantage. Both measures are *z*-score-standardized. *Proportion Black* is the proportion of block group or census tract residents who are Black.

Collective efficacy is based on caregivers' reports about their neighborhood and the areas surrounding their routine activity locations. Thus, in contrast to the conventional measures of collective efficacy that rely exclusively on residents' evaluations of their neighborhood, AHDC additionally draws on nonresident visitors engaged in routine activities as collective efficacy informants. This approach is consistent with mounting evidence finding that levels of collective efficacy vary by land use, highlighting the need for measurement strategies incorporating nonresidential areas (Corcoran et al. 2018; Wickes et al. 2019). The entrance survey of caregivers includes a "location generator" that prompts the caregiver to report on places they go during a typical week—including weekends—with the following list of possible location types: workplace, caregiver's school/training, library, place of worship, grocery store, relative's house, friend's house, park/recreation center, restaurant, store/business, civic organization, neighborhood organization, and other. After selecting all that apply, the interviewer assists the caregiver in geolocating each place using a Google Maps interface embedded in the survey software; the interviewer can search for names of establishments or drop a pin to indicate the correct location. The addresses are then geocoded using the Google Maps application programming interface, and the Google address, latitude, and longitude are saved. Caregivers can report more than one location per type. The most commonly reported location types are grocery stores (90% of caregivers report at least one), child's school (90%), workplace (67%), store/business (51%), restaurant (44%), and place of worship (43%). Location coordinates are then linked to census units using the R *sf: Simple Feature*s package (Pebesma et al. 2019).

For each reported routine location and neighborhood,[7] respondents were asked to report how much they agree with the following statements: (1) whether people on

[5] Respondents were also able to report additional places of residence, such as another parent or grandparent's house; 171 Black or White respondents reported a second residence. To ensure that time spent at a second home does not influence our results, we replicated all presented analyses when dropping multihomed respondents, yielding substantive conclusions identical to those discussed later.

[6] We operationalize home neighborhoods using census tracts to align with long-held convention in the neighborhood effects literature (Arcaya et al. 2016). Nonhome exposure compositions are operationalized using block groups to more precisely capture exposure to residential populations surrounding activity locations. Analyses in which both neighborhoods and nonhome exposure measures are based on census tracts are discussed later, however, and yield findings comparable with those presented here.

[7] Respondents were asked to provide four street intersections or landmarks they "think of as the boundaries of [their] neighborhood" (Pinchak et al. 2021), and to report on perceptions of trust, monitoring, and

the streets can be *trusted* ("trust"), (2) whether people are *watching what is happening* on the streets ("monitoring"), and (3) whether people would *come to the defense of others* being threatened ("norms toward intervention"). Response options ranged from 1 ("strongly disagree") to 5 ("strongly agree"), with items coded so that higher values signify higher levels of informal social control. Respondents were asked to report on their residential neighborhood both during the day and at night but provided summary evaluations for all other locations. We then estimate a block group–level aggregated measure combining reported trust (respondent $n=1{,}258$; report $n=5{,}974$; block group $n=565$), monitoring (respondent $n=1{,}308$; report $n=7{,}699$; block group $n=578$), and norms toward intervention (respondent $n=1{,}303$; report $n=7{,}738$; block group $n=577$) using a cross-classified linear model in which reports are clustered within respondents and within block groups (Raudenbush and Bryk 2002) using the *lme4* package in R (Bates et al. 2015:4).[8] A block group–level random effect is then recovered from this model for all block groups with at least one nonmissing report of monitoring, trust, or norms toward intervention (respondent $n=1{,}340$; report $n=21{,}411$; block group $n=580$). To obtain corresponding estimates for unobserved block groups, this block group–level random effect is spatially smoothed across the study area using a conditional autoregressive model proposed by Leroux (Leroux et al. 2000), implemented in R using the *CARBayes* package (Lee 2013, 2020). This process yields our measure of collective efficacy for all 615 Columbus block groups within the outer belt boundary. The correlation coefficient for the smoothed measure with the estimated random effects from the cross-classified multilevel models exceeds 0.99, indicating that our approach does not spatially smooth the estimated random effect, except in block groups without location reports. This feature is desired as we did not want to spatially smooth across block groups with reports and blur real discontinuities in the collective efficacy process. In areas with limited data, however, spatial smoothing allows the collective efficacy measure to be estimated. We replicate this procedure at the census tract level to generate a measure of home neighborhood collective efficacy, and the final measures are z-score-standardized.

Exposure to *violent crime* is based on reported crime incidents in the Ohio Incident-Based Reporting System between 2014 and 2016. Incidents were geocoded to block groups based on x–y coordinates. These were then used to create counts at the day level for each block group, resulting in a day–block group–level observation file that provided the total count of crimes that occurred at block group j on day t. Using this file, we then generated a 180-day rolling average for each block group. For example, the crime rate for block group j at day t would be equal to the sum of crimes that occurred at block group j between day t and day $t-180$, divided by the

intervention norms for this area. To ensure consistency with the larger collective efficacy literature, we geocode these reports of "neighborhood" perceptions to respondents' census tracts of residence.

[8] The Cronbach's alpha for responses to the three collective efficacy components is .67 when aggregated to the individual level and .66 when aggregated to the block group level. The mean number of reports given per block group is 31.0 (SD = 42.0). The mean number of respondents giving reports per block group is 9.6 (SD = 13.5). The block group–level intraclass correlation—or the ratio of the block group-level variance (.124, $p<.05$) to the sum of the variance components at the block group, respondent (.192), and report levels (.682)—indicates that 12.4% of the variance in collective efficacy is between block groups. This proportion is not unlike those from other studies of neighborhood social processes. For example, the between-neighborhood variance in informal social control in the Project on Human Development in Chicago Neighborhoods study is 13% (Raudenbush and Sampson 1999).

total population for block group j. We then used this 180-day rolling window to calculate time-weighted exposures to crime on a given day on the basis of the proportion of time a participant spent in each location block group on that day. The violent crime rate combines homicide, robbery, aggravated assault, and rape. Week-level measures of exposure to crime were then created for each respondent. Home neighborhood violent crime is measured using the 2014–2016 average violent crime rate, again combining incidents of homicide, robbery, aggravated assault, and rape.[9] Block group–level *population density* is based on ACS-linked census data.

Control Variables

Respondent and family control variables are based on self-reported survey data. For this study, *youth's race* is a binary indicator of non-Hispanic White (reference) and non-Hispanic Black. *Youth's foreign-born* status is a binary indicator for whether the respondent was born in (reference) or outside the United States. *Youth's* and *caregiver's biological sex* are binary measures for which female is the reference category. *Youth's* and *caregiver's age* are continuous measures of self-reported age. *Household size* is the caregiver's reported number of occupants in the household. *Household income* is based on caregiver self-reported data, with categories including less than \$30,000 (reference), \$30,001–\$60,000, and more than \$60,000. *Caregiver's marital status* includes four categories: married (reference), cohabiting, single, and other. *Caregiver's education* includes five self-reported categories: less than high school, high school/GED, some college, bachelor's degree, and graduate/professional degree. *Home ownership* is a binary indicator of whether the caregiver owns the place of residence. *Years in neighborhood* is a self-reported continuous measure of the number of years the caregiver has lived in their neighborhood (see footnote 8). *Moved in the past two years* is a binary caregiver-reported indicator. *Season* is a four-category measure of the season during which the youth participated in the study, with winter as the reference. In addition, we control for the total number of *minutes that a respondent spent outside the home* over the course of the study week. Lastly, we created a three-category variable capturing the number of weekend days covered in the GEMA data by each respondent (0, 1, or 2).[10]

Analytic Strategy

First, we use linear regression models to assess mean differences in nonhome exposure to concentrated disadvantage, proportion Black, collective efficacy, and violent crime between all Black and White AHDC youth ($n = 1{,}180$) net of controls only for respondent age and sex. For each dependent variable, the second model

[9] Crime rates reflect the true 180-day rate for more than 96% of respondents. For those who entered the study prior to July 2014, exposure to crime incidents occurs after the time of the initial interview.

[10] We considered control variables for transportation access, replicating all presented models when controlling for (1) a binary indicator of ever having car access to get to school (mean = .47) and (2) a continuous measure of the proportion of trips taken during the study week with a car (mean = .64; SD = .32). These analyses yielded conclusions identical to those drawn from our presented models.

adds all previously mentioned individual-level controls for demographic factors and census tract characteristics. The third models then assess mean differences among Black and White AHDC participant youth who live in census tracts with participants of the other race (i.e., Black or White, $n=674$), and the fourth models assess Black–White differences for youth living in the same census tract by controlling for home census tract fixed effects. We then turn to models assessing whether Black–White differences in nonhome exposures vary systematically by home neighborhood disadvantage, biological sex, age, or having recently moved (i.e., through statistical interaction). For each nonhome exposure composition, we control for home neighborhood concentrated disadvantage given the significance of this measure within the neighborhood effects literature, as well as the respective census tract–level measure of the outcome (i.e., controlling for home tract collective efficacy when predicting nonhome collective efficacy). We do not attempt to discern whether any observed Black–White differences in a given nonhome exposure measure are due to Black–White differences in another home tract or nonhome exposure measure, as these are all highly interrelated and likely to lead to problems of multicollinearity.[11] All presented regression models use cluster robust standard errors to account for clustering of respondents in census tracts of residence. An exception is made for models assessing cross-level interactions between respondent race and home neighborhood disadvantage, for which we use multilevel linear models with respondents clustered within census tracts to include a tract-level random slope for respondent race (Heisig and Schaeffer 2019).[12]

Results

Of the 1,405 youth in Wave I of AHDC, 1,258 self-identify as either non-Hispanic White or non-Hispanic Black. From this sample, we drop 70 respondents with no nonhome time for the study week and an additional eight with no nonhome time within the Columbus study area ($n=1{,}180$). We retain respondents missing data on control variables (driven mainly by missingness for household income; $n=81$) using multiple imputation by chained equations procedures with five imputed data sets in Stata 15, bringing our final analytic sample to 1,180 youth (StataCorp 2017; von Hippel 2020). In total, these youth have primary addresses in 178 of the 197 census tracts within the Columbus study area.

Table 1 displays descriptive statistics for study variables among the full sample of Black and White AHDC youth and those in the "fixed-effects sample," or respondents who live in a census tract with at least one respondent of the other race. Distributions of home neighborhood concentrated disadvantage by race for both the full and fixed-effects samples are displayed in online appendix Figures 1 and 2, respectively.

[11] Specifically, the Cronbach's alpha is .85 for all the nonhome exposure compositions (with collective efficacy being reverse-coded), .81 for all home tract compositions together, and .91 for all home tract and nonhome compositions together.

[12] To ensure robustness of our conclusions to choice of standard error, all analyses were replicated using HC2 and HC3 standard errors rather than clustered standard errors. Results from these analyses yielded substantive conclusions identical to those discussed here.

Table 1 Means and proportions for study variables, by analytic sample

Variable	Full Sample Mean/%	Full Sample SD	Fixed-Effects Sample Mean/%	Fixed-Effects Sample SD	Min.	Max.
Home Neighborhood Measures						
Tract concentrated disadvantage	0.00	1.00	0.02	0.79	–1.15	3.07
Tract proportion Black	.32	.30	.31	.26	0.00	0.91
Tract collective efficacy	0.00	1.00	–0.12	0.86	–2.51	1.73
Tract violent crime rate	18.88	18.90	20.43	18.48	0.00	94.72
Block group population density	5,241.90	2,933.05	5,523.58	2,940.54	323.58	17,947.28
Nonhome Exposure Measures						
Nonhome concentrated disadvantage	0.00	1.00	0.09	0.97	–1.58	4.49
Nonhome proportion Black	.25	.24	.26	.22	0.00	0.94
Nonhome collective efficacy	0.00	1.00	–0.14	0.93	–2.88	2.24
Nonhome violent crime rate	6.67	6.49	7.38	6.15	0.00	47.96
Adolescent Measures						
Total nonhome time (minutes)	1,758.91	930.42	1,707.94	940.52	2	5,318
Black (vs. White)	.48		.51		0	1
Foreign-born	.02		.02		0	1
Age	14.30	1.86	14.20	1.85	11	17
Male	.46		.46		0	1
Family Controls						
Household size	4.65	1.61	4.63	1.60	2	16
Parent age	45.43	8.55	44.84	8.86	18	81
Parent male	.12		.12		0	1
Household Income						
≤$30,000	.37		.40		0	1
$30,001–$60,000	.24		.26		0	1
≥$60,001	.40		.34		0	1
Parent Education						
<High school	.05		.06		0	1
High school/GED	.16		.17		0	1
Some college	.36		.42		0	1
College degree	.25		.21		0	1
Graduate/professional degree	.18		.15		0	1
Parent Marital Status						
Married	.53		.49		0	1
Cohabiting	.10		.12		0	1
Single	.20		.21		0	1
Other	.17		.18		0	1
Residence Owned (vs. rented)	.61		.57		0	1
Years Lived in Current Neighborhood	12.10	10.23	11.34	9.69	0	62
Moved in Last Two Years	.16		.16		0	1
Season						
Winter (December–February)	.23		.24		0	1
Spring (March–May)	.23		.22		0	1

Table 1 (continued)

Variable	Full Sample Mean/%	Full Sample SD	Fixed-Effects Sample Mean/%	Fixed-Effects Sample SD	Min.	Max.
Summer (June–August)	.29		.28		0	1
Autumn (September–November)	.26		.26		0	1
Number of Weekend Days						
0	.02		.02		0	1
1	.03		.04		0	1
2	.95		.95		0	1
N: Individual Level	1,180		674			

Average Racial Inequalities

Table 2 displays results from linear regression models with tract-level cluster robust standard errors for the four nonhome exposure composition outcomes. We present reduced models focused on the Black (vs. White) coefficient of interest, but the full tables are displayed in online appendix Tables 2–9. Model 1 for *z*-score-standardized, nonhome concentrated disadvantage indicates that, net of respondent sex and age, Black youth have an expected 0.902 ($p<.001$) standard deviations higher concentrated disadvantage in their nonhome activity space than White youth. Model 2 adds all the individual-level control variables and home census tract concentrated disadvantage. This model indicates that Black youth have an expected 0.166 ($p<.05$) standard deviations higher concentrated disadvantage in their nonhome activity space than White youth. The third model reduces the analytic sample to White and Black respondents who live in a census tract with AHDC youth of the other race, with the coefficient for respondent Black (vs. White) race remaining positive ($b=0.117$), but statistically nonsignificant. Controlling for census tract fixed effects to compare youth who live in the same census tract in Model 4, the coefficient for respondent Black race remains statistically nonsignificant.

The second set of fitted models are for the nonhome proportion Black as the outcome (ranging from 0 to 1). Net of respondent age and sex, Model 1 indicates that respondent Black race is associated with a 0.305 increase ($p<.001$) in expected nonhome activity space proportion Black. Model 2 adds all individual-level demographic controls and home neighborhood–level concentrated disadvantage and proportion Black. Relative to White youth, Black youth are expected to have activity spaces that are 0.090 ($p<.001$) higher in proportion Black. Selecting on respondents who live in a census tract with a respondent of the other race in Model 3, respondent Black race is similarly associated with an expected 0.094 increase ($p<.001$) in activity space proportion Black. The fourth model including census tract fixed effects again indicates that, relative to residentially comparable White youth, Black youth have an expected higher exposure to proportion Black activity spaces ($b=0.091$; $p<.001$).

Table 2 Linear regression models for nonhome exposures, with cluster robust standard errors

	Concentrated Disadvantage				Proportion Black			
	Model 1	Model 2	Model 3	Model 4	Model 1	Model 2	Model 3	Model 4
A. Nonhome Concentrated Disadvantage and % Black								
Respondent Black (vs. White)	0.902***	0.166*	0.117	0.093	0.305***	0.090***	0.094***	0.091***
	(0.077)	(0.068)	(0.074)	(0.084)	(0.017)	(0.016)	(0.018)	(0.017)
Neighborhood-Level Measures								
Tract concentrated disadvantage		0.407***	0.499***			0.019†	–0.004	
		(0.047)	(0.066)			(0.012)	(0.016)	
Tract proportion Black						0.357***	0.411***	
						(0.040)	(0.047)	
Constant	–0.170	0.292	0.909	0.210	0.139**	0.082	–0.022	0.321*
	(0.226)	(0.443)	(0.587)	(0.743)	(0.048)	(0.086)	(0.131)	(0.136)
Age and Sex	✓	✓	✓	✓	✓	✓	✓	✓
Control Variables		✓	✓	✓		✓	✓	✓
Home Tract Fixed Effects				✓				✓
N: Neighborhood Level	178	89	89	89	178	178	89	89
N: Individual Level	1,180	674	674	674	1,180	1,180	674	674

Table 2 (continued)

	Collective Efficacy				ln(violent crime)			
	Model 1	Model 2	Model 3	Model 4	Model 1	Model 2	Model 3	Model 4
B. Nonhome Collective Efficacy and ln(violent crime)								
Respondent Black (vs. White)	–1.062***	–0.286***	–0.277***	–0.228**	1.435***	0.249***	0.226**	0.171*
	(0.086)	(0.063)	(0.074)	(0.072)	(0.123)	(0.061)	(0.072)	(0.076)
Neighborhood-Level Measures								
Tract concentrated disadvantage		–0.054	–0.025			0.135*	0.176*	
		(0.043)	(0.106)			(0.061)	(0.073)	
Tract collective efficacy		0.473***	0.503***					
		(0.059)	(0.119)					
ln(tract violent crime rate)						0.586***	0.654***	
						(0.062)	(0.053)	
Constant	0.419†	–0.390	–0.478	0.263	–0.201	–0.521	–0.423	1.159
	(0.231)	(0.402)	(0.564)	(0.653)	(0.293)	(0.448)	(0.593)	(0.893)
Age and Sex	✓	✓	✓	✓	✓	✓	✓	✓
Control Variables		✓	✓	✓		✓	✓	✓
Home Tract Fixed Effects				✓				✓
N: Neighborhood Level	178	178	89	89	178	178	89	89
N: Individual Level	1,180	1,180	674	674	1,180	1,180	674	674

Notes: The following variables are *z*-score-standardized: tract concentrated disadvantage, tract collective efficacy, nonhome concentrated disadvantage, and nonhome collective efficacy. Models control for all control variables described in the Data and Measures section. See the online appendix for full tables.

†$p<.10$; *$p<.05$; **$p<.01$; ***$p<.001$ (two-tailed tests)

The third set of models are for z-score-standardized nonhome exposure to collective efficacy. The first model indicates that Black respondents are expected to be exposed to 1.062 standard deviations lower collective efficacy ($p < .001$) in their activity spaces than White youth when controlling for respondent age and sex. The magnitude of this coefficient drops to −0.286 ($p < .001$) in Model 2, net of individual-level demographic controls and home neighborhood–level concentrated disadvantage and collective efficacy. Selecting on youth residing in census tracts with respondents of the other race in Model 3, the coefficient for Black race remains similarly pronounced and is statistically significant ($b = −0.277$; $p < .001$). The fourth model adds census tract fixed effects to control for all time-invariant differences between census tracts of residence, with the coefficient for Black race indicating that these youth are expected to be exposed to 0.228 standard deviations lower collective efficacy ($p < .01$) than are White youth.

Finally, the fourth model set displays results for nonhome exposure to violent crime on its natural log scale because of heavy positive skew in this measure. Net of respondent race, sex, and age, the first model indicates that Black youth have an expected exposure of 320% ($\exp(1.435) = (4.20 - 1) \times 100 = 320$; $p < .001$) higher violent crime in their nonhome activity space than White youth. Model 2 adds all the sociodemographic variables and natural log of violent crime in respondents' home census tracts, indicating that Black youth are exposed to 28.3% more violent crime than White youth, net of these controls. Model 3 reduces the analytic sample to respondents living in census tracts with AHDC youth of the other race, and the coefficient for Black (vs. White) race indicates that these youth, on average, are exposed to 25.4% more violent crime in their activity space. Model 4 adds census tract fixed effects to explicitly compare youth who live in the same census tract, with the coefficient for Black race indicating that Black youth are expected to be exposed to 18.7% more violent crime than White youth.

Figure 1 provides a visual summary of the magnitude of coefficients for Black (vs. White) race from the foregoing Models 2–4 for nonhome exposures, now with each outcome having been z-score-standardized (mean = 0, SD = 1). For each outcome, the first bar corresponds to the full sample of Black and White AHDC youth in Model 2. The second bar corresponds to models selecting on the "fixed effects" sample of youth, and the third bar corresponds to the models controlling for census tract fixed effects. For example, racial inequalities in nonhome exposure to proportion Black are particularly sizable, with the second panel indicating that Black youth are exposed to about a 0.4 standard deviation higher level of proportion Black than White youth, on average. Replications of figures for nonhome proportion Black and the natural logarithm of the violent crime rate in their original, nonstandardized metrics are presented in Figure 3 of the online appendix.

Racial Inequality Interactions

We next examine whether racial inequalities in activity space exposures vary across the distribution of home neighborhood concentrated disadvantage, biological sex, age, and whether the respondent moved in the past two years given the importance of these moderators to residential mobility interventions and neighborhood effects

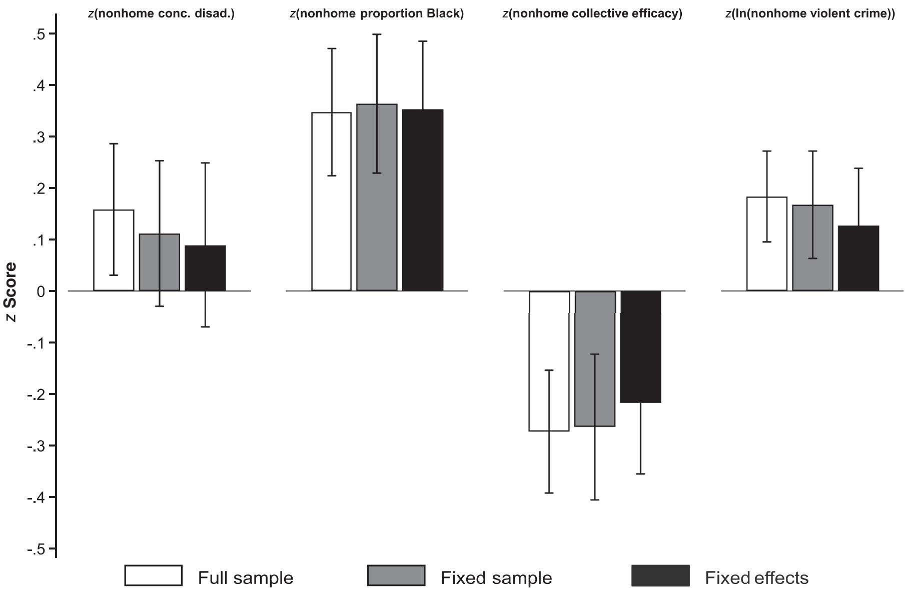

Fig. 1 The effect of respondent Black (vs. White) race on z-score-standardized nonhome exposure outcomes across models. The first bar corresponds to the full sample of AHDC youth, the second bar corresponds to models selecting on the "fixed effects" sample, and the third bar corresponds to the models controlling for census tract fixed effects. Whisker boxes represent 95% confidence intervals.

research more generally. Table 3 displays results from linear models for the full sample of Black and White youth, as well as youth in the fixed-effects sample, now with each model including a respective interaction between respondent Black race and neighborhood disadvantage, biological sex, age, or having recently moved. For each outcome, fitted models for the latter three interactions are linear regression models with cluster robust standard errors, while models for the interaction between Black and neighborhood disadvantage are two-level models including a random slope for Black race (Heisig and Schaeffer 2019). Across these models, the only statistically significant interaction term is between respondent Black race and age with nonhome collective efficacy ($b=0.043$; $p<.05$) as an outcome, indicating that expected racial inequalities in this outcome are lower for older adolescents.

Supplemental Analyses

Nonschool Exposures

Sensitivity analyses were conducted to ensure the robustness of these results. First, all analyses were replicated with nonhome exposure dependent variables that exclude time spent at school. We conducted these analyses because of the tendency for adolescents' nonhome time to be dominated by time spent at school or engaged in school-based activities (Hofferth 2009; Hofferth and Sandberg 2001), and thus the potential for this time to also dominate our nonhome exposure measures. Indeed,

Table 3 Linear regression models for nonhome exposures, with cluster robust standard errors and interactions

	Concentrated Disadvantage							Proportion Black						
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
A. Nonhome Concentrated Disadvantage and Proportion Black														
Moved in Past Two Years	0.091	0.079	0.160	0.079	0.164†	–0.015	0.097	0.036*	0.035*	0.045*	0.034*	0.046*	0.015	0.041
	(0.066)	(0.075)	(0.098)	(0.075)	(0.098)	(0.097)	(0.132)	(0.014)	(0.014)	(0.018)	(0.014)	(0.018)	(0.019)	(0.028)
Age	–0.004	–0.003	–0.017	0.013	0.001	–0.004	–0.017	0.002	0.000	0.002	0.004	0.006	0.000	0.002
	(0.013)	(0.013)	(0.015)	(0.016)	(0.021)	(0.013)	(0.015)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)
Male	–0.095*	–0.068	–0.119	–0.081†	–0.158**	–0.082†	–0.158**	–0.017†	–0.005	–0.015	–0.017†	–0.033**	–0.017†	–0.033*
	(0.044)	(0.057)	(0.080)	(0.045)	(0.057)	(0.045)	(0.057)	(0.010)	(0.012)	(0.016)	(0.009)	(0.012)	(0.009)	(0.013)
Respondent Black (vs. White)	0.176*	0.179*	0.132	0.656†	0.609	0.140†	0.074	0.089***	0.101***	0.109***	0.202**	0.205†	0.084***	0.090***
	(0.069)	(0.078)	(0.102)	(0.365)	(0.513)	(0.073)	(0.094)	(0.015)	(0.018)	(0.019)	(0.077)	(0.114)	(0.017)	(0.019)
Interaction Terms														
Black × tract disadvantage	–0.119†							–0.001						
	(0.067)							(0.014)						
Black × male		–0.027	–0.080						–0.023	–0.036				
		(0.086)	(0.115)						(0.018)	(0.023)				
Black × age				–0.034	–0.036						–0.008	–0.008		
				(0.025)	(0.035)						(0.005)	(0.008)		
Black × moved						0.147	0.104						0.030	0.007
						(0.131)	(0.170)						(0.026)	(0.036)
Constant	0.177	0.282	0.174	0.054	–0.028	0.320	0.222	0.089	0.074	0.305*	0.028	0.268†	0.088	0.322*
	(0.408)	(0.446)	(0.750)	(0.482)	(0.763)	(0.440)	(0.736)	(0.087)	(0.086)	(0.138)	(0.089)	(0.144)	(0.085)	(0.135)
Control Variables	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Fixed Effects			✓		✓		✓			✓		✓		✓
N: Neighborhood Level	178	178	89	178	89	178	89	178	178	89	178	89	178	89
N: Individual Level	1,180	1,180	674	1,180	674	1,180	674	1,180	1,180	674	1,180	674	1,180	674

Table 3 (continued)

	Collective Efficacy							ln(violent crime)						
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
B. Nonhome Collective Efficacy and ln(violent crime)														
Moved in Past Two Years	–0.052	–0.045	–0.070	–0.045	–0.070	0.079	–0.119	0.012	0.002	0.034	0.002	0.032	0.070	0.201
	(0.059)	(0.059)	(0.078)	(0.058)	(0.078)	(0.097)	(0.124)	(0.065)	(0.072)	(0.097)	(0.072)	(0.098)	(0.129)	(0.185)
Age	–0.009	–0.010	–0.010	–0.030*	–0.004	–0.009	–0.011	0.034**	0.033**	0.014	0.041*	0.017	0.034**	0.015
	(0.011)	(0.012)	(0.012)	(0.015)	(0.017)	(0.012)	(0.012)	(0.012)	(0.012)	(0.015)	(0.017)	(0.021)	(0.012)	(0.015)
Male	0.114**	0.090	0.079	0.127**	0.125*	0.128**	0.125*	–0.034	–0.042	–0.088	–0.029	–0.013	–0.029	–0.017
	(0.040)	(0.061)	(0.089)	(0.040)	(0.057)	(0.041)	(0.057)	(0.044)	(0.071)	(0.092)	(0.049)	(0.062)	(0.049)	(0.062)
Respondent Black (vs. White)	–0.273***	–0.323***	–0.272**	–0.899**	–0.050	–0.252***	–0.242**	0.238***	0.237**	0.099	0.486	0.262	0.267***	0.221*
	(0.060)	(0.076)	(0.093)	(0.312)	(0.395)	(0.068)	(0.080)	(0.066)	(0.074)	(0.089)	(0.295)	(0.396)	(0.065)	(0.085)
Interaction Terms														
Black × tract disadvantage	0.053							–0.034						
	(0.062)							(0.069)						
Black × male		0.077	0.089						0.025	0.148				
		(0.080)	(0.115)						(0.087)	(0.104)				
Black × age				0.043*	–0.013						–0.017	–0.006		
				(0.021)	(0.027)						(0.021)	(0.028)		
Black × moved						–0.194	0.078						–0.106	–0.276
						(0.121)	(0.154)						(0.147)	(0.203)

Table 3 (continued)

	Collective Efficacy							ln(violent crime)						
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Constant	–0.133	–0.360	0.303	–0.092	0.180	–0.426	0.272	–0.534	–0.512	1.226	–0.638	1.117	–0.541	1.126
	(0.371)	(0.404)	(0.658)	(0.430)	(0.671)	(0.403)	(0.652)	(0.407)	(0.454)	(0.896)	(0.470)	(0.930)	(0.450)	(0.878)
Control Variables	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Fixed Effects			✓		✓		✓			✓		✓		✓
N: Neighborhood Level	178	178	89	178	89	178	89	178	178	89	178	89	178	89
N: Individual Level	1,180	1,180	674	1,180	674	1,180	674	1,180	1,180	674	1,180	674	1,180	674

Notes: Models including an interaction for Black × tract disadvantage are two-level linear multilevel models clustering on census tracts, and include a random slope term for Black (vs. White) respondent. All other models are single-level linear regression models with cluster robust standard errors. The following variables are z-score-standardized: tract concentrated disadvantage, tract collective efficacy, nonhome collective efficacy, and nonhome concentrated disadvantage. See the online appendix for full tables.

† $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests)

AHDC youth spent an average of 44% of their nonhome waking time at school (SD =0.35). Results from replicated Models 2–4 of Table 2 for each nonhome exposure measure excluding school time are displayed in Table 10 of the online appendix. Similarly, results from replicated interaction models in Table 3 for nonhome exposure measures excluding school time are displayed in online appendix Tables 11–14. Two findings that contrast with the presented results are evident. First, there is consistent evidence that Black youth are exposed to more concentrated disadvantage in their nonhome/nonschool time than White youth. The most conservative estimate of this difference comes from the model controlling for home tract fixed effects, indicating that Black youth are exposed to 0.218 standard deviations ($p < .01$) higher concentrated disadvantage. Second, there is some evidence that the Black–White difference in nonhome/nonschool exposure to concentrated disadvantage is largest among youth residing in neighborhoods with lower concentrated disadvantage (see online appendix Table 11). Specifically, the interaction between respondent Black race and home tract concentrated disadvantage is negative and statistically significant ($p < .05$) when predicting nonhome/school exposure to concentrated disadvantage.

Census Tract Exposures

Our second set of sensitivity analyses replicates presented models for nonhome exposure to concentrated disadvantage, proportion Black, and collective efficacy based on exposure to census tract–level features, rather than census block group–level features.[13] Results from replications of the analyses presented in Table 2 are presented in online appendix Table 15. Results from replications of the interaction analyses in Table 3 are presented in online appendix Tables 16–18. Two findings emerge from these analyses that contrast with results presented here. First, there is again evidence of Black–White inequalities in nonhome exposure to concentrated disadvantage, with the most conservative estimate of this difference indicating that Black youth are exposed to 0.182 standard deviations higher concentrated disadvantage ($p < .05$). Second, there is again some evidence that Black–White differences in nonhome exposures are most evident among youth residing in neighborhoods lower in concentrated disadvantage. Specifically, the interaction between Black race and tract concentrated disadvantage is negative and marginally significant ($p < .10$) when predicting nonhome exposure to concentrated disadvantage, and positive and statistically significant ($p < .001$) when predicting nonhome exposure to collective efficacy.

Resident-Based Collective Efficacy

Our final set of sensitivity analyses replicate the presented models for nonhome collective efficacy, but now measuring collective efficacy only with respondents' reports

[13] Census tract–based replications were not conducted for nonhome exposure to violent crime because of the well-documented tendency for crime to concentrate in highly specific areas of neighborhoods, making aggregations beyond the block group level less informative for the purposes of our study (Weisburd et al. 2016).

about their home neighborhoods, and not about their routine activity locations. The correlation between collective efficacy measures based on all reports and only home neighborhoods is 0.79 at the block group level and 0.86 at the individual level, however, indicating notable consistency. Results from these analyses are displayed in online appendix Table 19. Consistent with the models discussed here, these results indicate that Black youth are exposed to lower levels of collective efficacy in their activity spaces than White youth. There is additionally evidence that this Black–White difference is larger among younger youth.

Discussion

Although research on immigration and residential mobility are cornerstones of demographic inquiry, the demography of everyday mobility remains in its infancy (Browning, Pinchak, and Calder 2021; Cagney et al. 2020). A growing literature finds evidence of racial segregation in activity spaces beyond the home among urban adults, but little research has examined whether or to what extent these patterns are apparent among adolescents (Jones and Pebley 2014; Krivo et al. 2013). Research suggests that segregation in activity space resources is consequential for population health inequalities above and beyond effects of one's neighborhood (Cagney et al. 2020; Sharp and Kimbro 2021) and may moreover shed light on why Black youth relocating from high- to low-poverty neighborhoods experience some adverse outcomes (Graif 2015; Schmidt et al. 2018). Drawing on extensive smartphone-based GPS data from a large sample of urban youth, the present study examined demographic and resource compositions in naturally occurring mobility patterns. We found robust evidence that, relative to comparable White adolescents, Black adolescents are disproportionately exposed to activity spaces with lower levels of collective efficacy and higher levels of racial segregation, violent crime, and—to a less consistent degree—concentrated socioeconomic disadvantage. We additionally considered whether racial inequalities in activity space compositions vary by adolescent age, biological sex, home neighborhood concentrated disadvantage, or having recently moved, but found little evidence of this systematic variation.

The present study corroborates emerging evidence indicating that reliance on the residential census tract to approximate urban adolescents' nonhome exposures will lead to biased estimates, especially in studies focused on Black youth (Browning, Calder et al. 2021; Kwan 2009). Our results suggest that this critique may extend to housing mobility programs focused solely on the composition of the immediate neighborhood area (Clampet-Lundquist and Massey 2008). Though neighborhood-centered interventions are no doubt important to reducing racial disparities in well-being, the anticipated returns to these programs may be inflated without attention to the daily mobility patterns and accessibility of resources experienced by disadvantaged racial minority residents (Boyd and Clampet-Lundquist 2019; Clampet-Lundquist et al. 2011). For example, extensive research documents heightened rates of adverse experiences faced by Black youth when navigating more affluent neighborhoods, pushing them away from these areas (Feagin 1991; Lyons 2007; Sewell et al. 2016). Spatial patterns of racially segregated interpersonal and organizational network ties likely also disproportionately pull Black youth toward more disadvantaged neighborhoods (Krysan and Crowder 2017; Small and Adler 2019; Small and McDermott 2006).

These segregation mechanisms motivated our expectation of racial segregation in activity space, but an investigation of how they comparatively explain our findings is beyond the scope of this study. To this end, we urge researchers to consider how the spatial distribution of organizational resources, network ties, and discriminatory processes may differentially shape racial segregation in exposure to specific location types (e.g., schools, workplaces) or specific sections of cities (e.g., shopping districts). For example, racial segregation in exposure to commercial areas may be driven by personal or network-mediated reports of experiences with discrimination (Krysan and Crowder 2017), while racial segregation in school attendance may be more attributable to inequalities in available schooling options (Burdick-Will et al. 2020).[14] The influence of these processes may furthermore vary across cities, motivating additional research considering how city-level social processes affect segregation in activity space compositions and everyday patterns of mobility (Fenelon and Boudreaux 2019; Massey and Denton 1993). Lastly, though we found limited evidence that racial differences in activity space exposures vary by age, sex, home neighborhood disadvantage, or having recently moved, these interactions may yet be important for investigations of other everyday mobility processes—such as how Black youths' strategies for safely navigating advantaged neighborhoods may depend on gender—and deserve continued investigation (Browning, Pinchak, and Calder 2021; Clampet-Lundquist et al. 2011).

This study draws on extensive GPS-derived mobility data in a large contemporary sample of urban youth but is not without methodological limitations. Importantly, while the present study is motivated in part by findings from the MTO experiment, our data are not capturing activity space disparities in the context of experimentally induced moves. Even so, examining these patterns as they naturally occur sheds light on neighborhood effects research findings and has implications for understanding and implementing interventions focused on adolescents' residential neighborhood environment. In addition, though GPS data make possible unprecedented examinations of youths' everyday exposures, these data are still subject to error and ambiguity in the precise determination of location (Boettner et al. 2019; Browning, Pinchak, and Calder 2021). Our focus on GPS coverage spanning the weekend and three weekdays is bolstered by research finding that between-person variability in activity spaces can be adequately captured with 1–6 days of GPS data (Zenk et al. 2018). Nevertheless, five days of coverage are evidently less suited to capture *within*-person variability in activity spaces and, thus, mobility data collected over longer periods of time may be necessary to make claims regarding racial inequalities in mobility at more fine-grained levels (Zenk et al. 2018). The data analyzed here are also cross-sectional, rather than longitudinal, and thus we urge future studies to consider how inequalities in activity space resources may change over time.

[14] For example, residents of Columbus are afforded numerous school choice options through charter schools and a Columbus City Schools school choice lottery. Student participation in these programs may influence activity space compositional measures even when excluding time spent at school, especially for youth whose chosen school is farther from home than their default assigned school (Rich et al. 2021).

It is also important to note that our measures of nonhome exposure to concentrated disadvantage and proportion Black are capturing exposure to *residential* rather than *ambient* populations, such as to the composition of residents and visitors in an area simultaneously (Hall et al. 2019; Vallée 2018). This focus is consistent with a voluminous literature finding that exposure to neighborhoods of differing residential compositions is consequential to well-being, particularly among Black youth (Anderson 2015; Lyons 2007; Winkler 2012). Nonhome exposures based on ambient populations may nevertheless contrast with the present findings and, thus, we urge researchers to assess this, as well as how ambient and residential population processes may differentially shape youth well-being. Inequalities in public and personal transportation access or in routine distance traveled beyond the home—such as to workplaces or schools—were not considered in this study, but may illuminate the observed results and warrant more attention in the everyday mobility literature (Anderson and Galaskiewicz 2021; Tana et al. 2016).[15] Finally, although recent studies indicate that the Columbus study area is representative of U.S. metro areas with respect to racial segregation (Frey 2018; Hess et al. 2019), we acknowledge that our results may not be reflective of youth residing in other cities or among youth facing housing instability. Thus, mobility data collection efforts spanning multiple cities and countries and targeting more residentially mobile respondents remain necessary to fully inform the generalizability of this study.

This study has implications for the growing literature on activity spaces and the study of "contextual effects" more generally. Our results suggest that disproportionately "disadvantaged" activity space exposures among Black youth may contribute to racial disparities in life course well-being. However, the literature underscores that not all ecological resources necessarily benefit Black youth, in particular. Indeed, neighborhood rates of informal social control can be positively associated with racial hate crimes (Lyons 2007). Some research additionally finds evidence for ecological relative deprivation processes (DeAngelis 2022; Pinchak and Swisher 2022; Sharp et al. 2015), suggesting that heightened exposure to "advantaged" areas may confer some adverse consequences for disadvantaged youth. These findings urge researchers to consider how neighborhood and activity space resources additively and multiplicatively work together to shape youth well-being. ■

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[15] In calculations not shown, we found that both Black and White youth spend comparatively little time in either their home census tract or the first-order surrounding census tracts (in total, 18% of nonhome waking time for White youth and 11% for Black youth). This suggests that both Black and White Columbus youth routinely travel considerably far beyond the areas near their home neighborhood.

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