Neighborhood effects on use of African-American Vernacular English

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African-American Vernacular English (AAVE) is systematic, rooted in history, and important as an identity marker and expressive resource for its speakers. In these respects, it resembles other vernacular or nonstandard varieties, like Cockney or Appalachian English. But like them, AAVE can trigger discrimination in the workplace, housing market, and schools. Understanding what shapes the relative use of AAVE vs. Standard American English (SAE) is important for policy and scientific reasons. This work presents, to our knowledge, the first experimental estimates of the effects of moving into lower-poverty neighborhoods on AAVE use. We use data on non-Hispanic African-American youth (n = 629) from a large-scale, randomized residential mobility experiment called Moving to Opportunity (MTO), which enrolled a sample of mostly minority families originally living in distressed public housing. Audio recordings of the youth were transcribed and coded for the use of five grammatical and five phonological AAVE features to construct a measure of the proportion of possible instances, or tokens, in which speakers use AAVE rather than SAE speech features. Random assignment to receive a housing voucher to move into a lower-poverty area (the intention-to-treat effect) led youth to live in neighborhoods (census tracts) with an 11 percentage point lower poverty rate on average over the next 10–15 y and reduced the share of AAVE tokens by ∼3 percentage points compared with the MTO control group youth. The MTO effect on AAVE use equals approximately half of the difference in AAVE frequency observed between youth whose parents have a high school diploma and those whose parents do not.

neighborhood effects | segregation | language | African-American Vernacular English | code switching

Language is in many respects a socially constructed behavior, jointly influenced by exposure, identity, and peer group influence (1). One's speech patterns are shaped not only by one's family, but also by one's broader regional and social environment. For example, people who immigrate from non-English-speaking countries to the United States at an early age wind up speaking English with nearly the same proficiency as those who were born in the United States, even though their older siblings and parents do not (2, 3). Less clear is whether different local social environments within a city, state, or country exert causal effects on the use of dialects such as African-American Language (4) or African-American Vernacular English (AAVE), which is the most vernacular variety of African-American English and is used across the country (5–7).

This work presents what, to our knowledge, is the first study of how much social environments—neighborhoods—exert a causal effect on the use of AAVE. Previous research in sociolinguistics has documented substantial variation in AAVE use by socioeconomic class, defined by using various combinations of occupational status, education, and income or residence quality (8, 9). There are theoretical reasons to believe any or all of these measures shape AAVE use by neighborhood (as discussed further below and in *SI Appendix*). However, this correlation may not reflect the causal effect of neighborhood environments on language and could instead be driven by the effects of unmeasured person- or family-level variables that jointly determine both residential location and speech patterns. Causal inference about the effects of neighborhoods on speech is more convincing if based on a study that uses a randomized experimental design to assign similar families to live in different types of neighborhood contexts.

Evidence for neighborhood effects on AAVE use is relevant for understanding the degree to which future changes in neighborhood economic and racial segregation may affect the vitality and use of this dialect (10, 11). This is a topic of importance to sociolinguists, because vernaculars have benefits as in-group markers and expressive resources (12). Such evidence is also relevant for understanding how changes in segregation will affect disparities in other life outcomes because previous studies suggest that AAVE use could affect children's school success—at least given the way schools currently operate—and that AAVE speakers are often victims of what Baugh calls "linguistic profiling" (13)—discrimination in the workplace, housing markets,

Significance

We provide, to our knowledge, the first experimental evidence of neighborhood effects on the use by low-income minority youth of African-American Vernacular English (AAVE). Rising U.S. residential economic segregation may be contributing to growing differences within the population in AAVE use, which has benefits to in-group solidarity and identity but is associated with discrimination in schools and workplaces and so may exacerbate the disadvantages of youth growing up in high-poverty areas. To the extent that the association between AAVE use and income represents a causal effect of AAVE use, our illustrative calculations suggest that neighborhood effects on speech could increase lifetime earnings by approximately $18,000 (∼3–4% of lifetime income).

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Data deposition: A restricted-access version of the data used in this paper will be provided to the U.S. Department of Housing and Urban Development (HUD). At the time of writing, HUD plans to make the data available to responsible researchers through a data license (details at www.huduser.org/portal/research/pdr_data-license.html). HUD expects to arrange for alternative archiving of the data in the future.

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and schools (7, 14–18). Of course, efforts to modify how social institutions interact with people using different dialects are independently important, regardless of any relationship between neighborhood segregation and AAVE use.

Our study capitalizes on a unique opportunity to understand neighborhood effects on speech (AAVE use) by incorporating sociolinguistic measures of speech and language patterns into the long-term follow-up of participants in a large-scale government residential-mobility experiment called Moving to Opportunity (MTO). We believe this type of language measurement has never before been incorporated into a large, randomized social experiment. We provide estimates of the causal effect of changes in neighborhood environments on speech patterns by using MTO's randomly assigned variation in opportunities for poor families to move to low-poverty areas.

MTO

The MTO experiment was designed and carried out by the U.S. Department of Housing and Urban Development (HUD). Eligibility was limited to families with children living in public housing projects located in selected distressed neighborhoods in five cities (Baltimore, Boston, Chicago, Los Angeles, and New York City). Approximately one-quarter of eligible families applied to MTO. From 1994 to 1998, MTO enrolled a total of 4,604 low-income, mostly minority families who were then randomly assigned by lottery to:

(*i*) A "low-poverty voucher (LPV) group" that was offered housing search assistance and housing vouchers that could only be used to relocate to a low-poverty census tract (one with a 1990 poverty rate <10%); or

(*ii*) A "control group" that received no special assistance, although some families can (and did) move by themselves.

(The MTO experiment also included a traditional voucher group that was offered housing vouchers that they could use to move to a new private-market apartment of their choice; for budgetary reasons, we did not collect speech data from that group.)

Not all households assigned to the LPV group relocated through the MTO program. In our analyses, we compare average speech patterns of all families assigned to the LPV group (regardless of whether they moved through MTO) vs. all controls. In the medical literature, this comparison is the basis of "intention-to-treat" (ITT) analysis and preserves the key strength of MTO's experimental design. As we show below, MTO generated large differences in the socioeconomic composition of neighborhoods experienced by otherwise similar groups of low-income families, and so helps overcome the self-selection problem that plagues nonexperimental studies of neighborhood effects on speech and other outcomes.

In earlier work examining various outcomes of youth in MTO, Ludwig et al. found no detectable effects on youth schooling and youth physical health of being in a family assigned to the LPV group (19). But they found gender-based differential effects of the LPV on other youth outcomes, with girls doing better on some measures than their control-group counterparts and boys doing worse relative to their controls (see also ref. 20). A more recent study of MTO youth by Chetty et al. that incorporates longer-term follow-up data shows sizable impacts of MTO moves to less-disadvantaged neighborhoods on earnings during adulthood of youth who were relatively young when their families moved in MTO (21).

The present work examines the degree to which this mobility experiment changes the speech patterns of MTO youth.

Results

In this study, we focus on MTO youth rather than adults, because propensity for second-language or dialect acquisition is more pronounced for youth than adults (22–24). Because our focus in this paper is on use of AAVE, we restrict attention to non-Hispanic African-American youth ($n = 629$). Table 1 presents the average baseline characteristics for the youth in our study sample by randomized group (LPV and control; see also *SI Appendix*, Table S3). The study sample is very economically disadvantaged; at baseline, four of five households were receiving cash welfare. The average poverty rate in the baseline neighborhoods (census tracts) was almost 60%, and >70% of residents were African-American. Statistical tests fail to reject the null hypothesis that the average baseline characteristics are the same for the LPV and control groups, confirming that MTO randomization was carried out correctly.

All families in the LPV group were offered the opportunity to move to a low-poverty neighborhood. However, of non-Hispanic African-American youth assigned to the LPV group who are in our speech analysis sample, only 52% of their families used an MTO voucher to relocate to a low-poverty census tract. Fig. 1 shows that random assignment to the LPV rather than control group generated larger differences in neighborhood disadvantage than in neighborhood racial composition. One year after random assignment, LPV group youth lived in neighborhoods with an average poverty rate 21 percentage points lower than the control group mean of 56%. This change equals 1.27 SDs of the tract poverty distribution within the control group (and 1.71 SDs in the national tract poverty distribution; *SI Appendix*, Table S6).

The size of the MTO effect on neighborhood poverty decreased over time, and the difference in poverty rates in the neighborhoods in which participants were living just before the beginning of the MTO long-term survey fielding period (10–15 y after random assignment) is only 4 percentage points (approximately a quarter of a control group SD). However, over the full course of the 10- to 15-y follow-up period, the average poverty rate difference is 11 percentage points (control mean 43%, $P < 0.05$). (See *SI Appendix*, Table S6 for additional results.)

MTO also changed other measures of neighborhood economic composition that are correlated with poverty and that could also be relevant for the language environment. For example, within the MTO control group, we find that AAVE use by youth is correlated with parent education (*SI Appendix*, Table S14), and MTO generated large changes in the share of adults in a family's neighborhood with a high school or college degree (*SI Appendix*, Table S6). Additionally, MTO had effects on the characteristics of the schools that these youth attended, although they are typically smaller in proportional terms than are MTO's effects on neighborhood characteristics (*SI Appendix*, Table S7).

All youth in the speech collection pool were asked to respond to an engaging open-ended question (a question about either the happiest or the scariest moment in their life) and to answer an open-ended question at the end of our interview about whether the respondent had anything else to say about their neighborhood or housing programs more generally. Written informed consent was obtained before beginning interviews. The intent was to elicit informal speech, but speech would still be relatively formal given the interviewer's status as a stranger recording an interview. These speech samples were transcribed by trained linguistic listeners (see *SI Appendix* for details). The unit of observation in our data is the "token"—that is, an occurrence of a selected speech variable or feature in which speakers have the option of using AAVE rather than Standard American English (SAE).

Tokens were coded for use of AAVE rather than SAE for 10 language features (five grammatical and five phonological) that have been shown in previous research to distinguish the two dialects (5, 8, 25). Of the 14,191 tokens we analyzed in our dataset, 1,492 (11%) represented grammatical features and 12,699 (89%) were phonological (pronunciation) features (*SI Appendix*, Table S2). Our key dependent variable is whether a given token is realized by a grammatical or phonological AAVE

Table 1. Baseline characteristics by treatment group

Characteristics	Control mean	LPV mean
Youth characteristics		
Male	0.554	0.494
Age in whole years as of December 31, 2007	16.615	16.643
Non-Hispanic African-American	1.000	1.000
Health problems that limited activity	0.047	0.053
Health problems that required special medicine or equipment	0.070	0.076
Household characteristics		
Adult was employed at baseline	0.245	0.258
Receiving Aid to Families with Dependent Children	0.818	0.853
Neighborhood characteristics		
Census tract poverty rate	0.588	0.584
Census tract share black	0.719	0.716
Primary move reason was to get away from drugs and gangs	0.535	0.535
Primary move reason was better schools for children	0.164	0.189
Randomization site		
Baltimore	0.202	0.167
Boston	0.128	0.087
Chicago	0.374	0.338
Los Angeles	0.148	0.239*
New York	0.148	0.169

All values represent shares (except age). Values are calculated by using sample weights to account for changes in random assignment ratios across randomization cohorts, survey sample selection, two-phase interviewing, and language sample selection. The sample is non-Hispanic African-American youth speakers (ages 13–20 as of December 2007) who were randomly selected for the linguistic component of the long-term survey from the LPV and control groups only whose speech samples included at least one analyzable language token ($n = 629$ youth). *$P < 0.05$ on an independent group t test of the difference between the LPV group and the control group.

variant rather than its SAE variant. To assess the reliability of our measure, we estimated that AAVE use rates have a correlation of 0.39 among siblings ($P < 0.001$) and that AAVE use among the first and second halves of each youth's tokens are significantly correlated with each other—see *SI Appendix* for details. The validity of our measure is suggested by the fact that AAVE use rates follow patterns reported in previous studies that measure AAVE use in other ways—for example, higher among African-Americans than Hispanics and higher among youth whose parents have less schooling.

The first row of Table 2 presents our key results for the effects of the MTO experiment on AAVE use, showing that assignment to the LPV rather than control group causes a decline in AAVE use of 2.8 percentage points (95% confidence interval + 0.1 to −5.7 percentage points; $P = 0.056$). The control mean is 48.5% of tokens using AAVE. This ITT estimate is based on a model that controls only for baseline characteristics measured before MTO random assignment. To provide some context for the size of this effect, within the control group, the difference in AAVE prevalence between youth whose head of household does vs. does not have a high school diploma equals 5.4 percentage points (see *SI Appendix*, Table S14 for details). So the MTO effect on AAVE use is approximately half the difference in AAVE use between children whose parents graduated high school and those who did not.

When we split our analysis by language feature, we find a statistically significant effect on phonological tokens, but not grammatical tokens, although we cannot reject the null hypothesis that the two effect sizes are the same (*SI Appendix*, Table S11).

Also of interest is how gender moderates MTO's effects on AAVE use. Previous research in sociolinguistics finds a "conservative tendency" in speech patterns among women, who "show a lower rate of stigmatized variants and a higher rate of prestige variants than men," and who seem to adopt prestige forms at a higher rate than men (ref. 1, pp. 266–267 and 274; see also refs. 5 and 26–28). Even for young children, boys use AAVE variants during spontaneous discourse at approximately twice the rate of girls (29, 30). However, in the MTO data, we find that overall rates of AAVE use within the control group are similar for boys vs. girls (48.9% vs. 48.2%; Table 2). The estimated MTO effect on reducing AAVE use is larger for females compared with males (−4.5 vs. −1.1 percentage points), but we cannot reject the null that they are the same ($P = 0.245$; *SI Appendix*, Table S9).

Age patterns in vernacular use have also been of great interest to sociolinguists. In observational studies, younger children (e.g., age 13 or younger) seem more likely to change speech patterns after their families move to a new area than older youth (refs. 23; 24; and 31, p. 176). All else equal, that would lead us to predict that children who are relatively younger at baseline should exhibit more pronounced changes in AAVE use. Conversely, being relatively older at the time of our MTO long-term data collection may moderate MTO's effects, because previous research suggests that AAVE use increases during adolescence but begins to decline as youth enter college or the labor market (25, 31–34).

Table 2 highlights the challenge of testing for age effects in the MTO data: We see signs that older age (17+) at the time of data collection is associated with larger increases in AAVE use compared with those <17, but this difference could be due to the fact that youth of different ages at the time of our survey also had different average baseline ages. More generally, the MTO design does not make it possible to disentangle the independent effects of age at baseline, age at the time of our survey, and duration of exposure to the MTO experiment; the latter is just the difference between the first two factors, so we cannot vary one while holding the other two constant.

Subgroup analyses suggest that household heads' motivation for enrolling in MTO may be an important moderator of MTO effects on AAVE use. Table 2 shows that, for youth whose parents reported at baseline that they signed up for MTO to either get away from drugs and gangs in their current (baseline) neighborhoods or to access better schools, MTO treatment assignment reduces AAVE use by 5.2 percentage points (95% confidence interval −8.7 to −1.7 percentage points; $P = 0.003$). For the rest of the MTO youth sample, the effect of MTO treatment assignment on

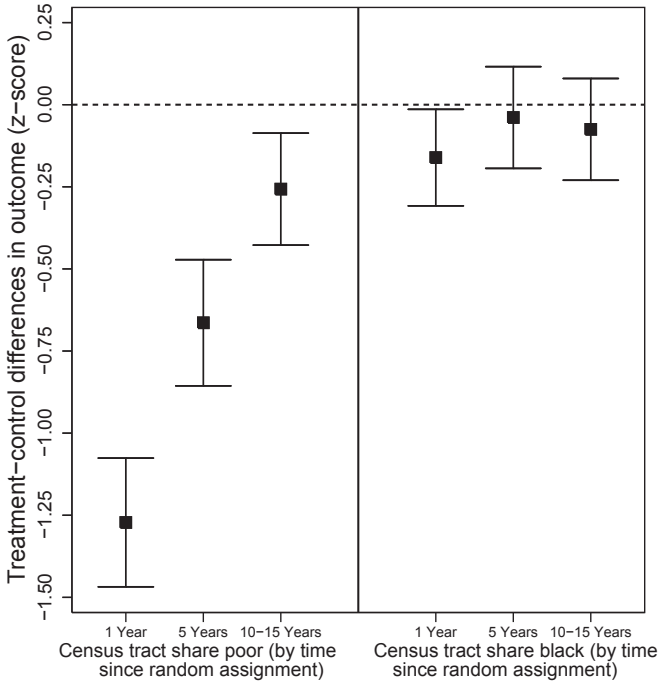

Fig. 1. MTO effects on neighborhood conditions. Impact on each outcome of assignment to the LPV group for non-Hispanic African-American youth whose language sample from the MTO long-term survey was analyzed. The squares represent the ITT estimate for the effect of being assigned to the LPV group, rather than control, for the outcomes listed on the *x* axis: neighborhood (census tract) share poor and share black at the address where the youth was living 1, 5, and 10–15 y after random assignment (the 10- to 15-y address is where the youth was living as of May 31, 2008, just before the beginning of the long-term survey fielding period). Share poor and black are z-scores, standardized by the control group mean and SD. The box whiskers represent the 95th percent confidence interval around the estimates. Census tract characteristics are based on interpolated data from the 1990 and 2000 decennial Censuses as well as the 2005–2009 American Community Survey.

AAVE use is a positive and statistically insignificant 3 percentage points. The difference between the estimated MTO effects for the two subgroups is 8.3 percentage points ($P = 0.007$; *SI Appendix*, Table S10). However, given the number of subgroups analyzed (*SI Appendix*, Tables S9 and S10), this result could be a "false positive" and thus should be viewed as only suggestive.

The *SI Appendix* shows that our results are qualitatively robust to a number of different decisions about how we define our sample and carry out our analyses. For example, the results are similar when we calculate average marginal effects using probit or logit models instead of linear regression, or limit the sample to youth whose language samples are above some threshold size, or collapse the data and carry out analysis at the person, rather than token, level.

Discussion

To our knowledge, ours is the first study to use data from a randomized experiment to examine whether moving into a more economically advantaged neighborhood causes a decline in the rate at which speakers use AAVE vs. SAE. On average, youth in the control group in our sample used AAVE in ∼49% of the speech tokens that we collected and analyzed 10–15 y after baseline. Random assignment to the LPV group, members of which were given the opportunity to use a housing voucher to move out of a high-poverty public housing project into a less-distressed neighborhood, reduced AAVE prevalence by ∼3 percentage points.

To take advantage of the key strengths of MTO (the randomized experimental design and large sample), we collected speech data from as large a sample as possible. However, given constraints on budget and how long we could spend with respondents for the multipurpose MTO study, the speech samples we collected were relatively short (*SI Appendix*). Relative to previous sociolinguistic research, the amount of speech recorded per person is lower, but the number of respondents is much greater. Because our focus is on comparing group averages (speech patterns of people who were, vs. were not, randomized to have the chance to move to a less-distressed neighborhood), the total volume of speech used in our main estimates is quite large ($n = 14{,}191$ tokens total).

It is important to keep in mind that the MTO study sample is not representative of all American households. Although the families that signed up for MTO are generally similar to other urban minority samples in high-poverty urban areas that have been studied in the "neighborhood effects" literature (35, 36), the families living in high-poverty urban areas are much more disadvantaged on average than other American families. Of course how to help these very disadvantaged families is of particular policy concern.

Given our research design, we cannot isolate what specific features of the neighborhood social environment are responsible for the observed effects on AAVE use, although we do see that MTO generated larger changes in the LPV group's neighborhood poverty rate than racial composition. MTO also led LPV youth to live in areas with more highly educated adults, who are more likely to work in occupations ranked higher on the "linguistic market" in requiring or rewarding standard rather than vernacular use (31, 37). In addition, LPV youth attended schools with somewhat fewer black and minority students, and so may have experienced less peer group pressure to retain the vernacular and avoid "acting white" (38). Potential effects of social class and network differences between the control and LPV neighborhoods on vernacular use are discussed at greater length in *SI Appendix*, taking the sociolinguistic literature into account.

Our MTO data are informative about the types of neighborhood changes induced by this intervention, which could be different from the impacts of even more dramatic mobility interventions—for example, moving low-income minority families like those in MTO out to very affluent, predominantly white suburbs. Our data also do not allow us to determine how much of the neighborhood effect on AAVE use is mediated by neighborhood effects on language use (or other things) in the home.

Although the youth in the LPV group do demonstrate an ability to use more SAE-like speech in a formal setting (an interview with a stranger), our experiment should not be interpreted as indicating that they have abandoned AAVE more generally. We have no evidence on these youths' informal vernacular use with family members and friends, and it is likely, if not certain, that their informal speech contains higher frequencies of AAVE features. At the same time, virtually all discussion in the linguistics literature of the issue of developing bidialectal competence in AAVE and other vernacular speakers has emphasized the feasibility and value of extending speakers' repertoire to include command of SAE when needed, rather than replacing competence in the vernacular with competence in the standard (for a summary, see ref. 39). That extension of bidialectal competence for MTO youth in the SAE domain is what the experiment discussed in this paper has demonstrated.

We found that youth in families that enrolled in MTO because they cared the most about crime or school quality in their baseline public housing projects experienced the largest reductions in AAVE speech. One possible explanation is suggested by the ethnographic work of Anderson (40), who notes that within high-poverty, inner-city neighborhoods, residents who identify as "street" are less able or willing to engage in "code switching" to SAE (pp. 35–36). A quarter century earlier, Labov (41) had noted that African-Americans who were not active participants

Table 2. MTO effects on youth AAVE use

Baseline characteristics used for subgroup analysis	AAVE variant used in token			
		LPV vs. control ITT effect		
	Control mean	Coefficient (SE)	*P* value	No. of tokens (no. of youth)
Overall	0.485	−0.028~ (0.015)	0.056	14,191 (629)
By gender				
Female	0.482	−0.045* (0.020)	0.030	7,347 (307)
Male	0.489	−0.011 (0.021)	0.589	6,844 (322)
By age in 2008				
Age <17 in 2008[†]	0.477	0.014 (0.026)	0.572	4,459 (210)
Age 17+ in 2008[‡]	0.489	−0.047* (0.017)	0.007	9,732 (419)
By household head's primary reason for wanting to move				
To get away from drugs and gangs or for better schools for the children	0.494	−0.052* (0.018)	0.003	9,754 (440)
Another reason	0.465	0.030 (0.025)	0.224	4,437 (189)

*$P < 0.05$, ~$P < 0.10$ on two-tailed *t* test. Robust SEs are shown in parentheses. LPV vs. control ITT effects were estimated by using an ordinary least squares regression model controlling for the baseline covariates in *SI Appendix*, Table S3, using person-level survey weights, and clustering by family ID. Subgroup analyses were run as an interaction with the treatment group indicator. Tokens represent each instance where the speaker used 1 of the 10 language features. Tokens were analyzed for whether the speaker used the AAVE or the SAE variant for that token. The sample is all tokens from the speech samples of non-Hispanic African-American youth (ages 13–20 as of December 2007) who were randomly selected for the linguistic component of the long-term survey from the LPV and control groups only (n = 14,191 tokens from n = 629 youth).
[†]Baseline age: mean 2.4, range 0–5; mean exposure, 12.5 y.
[‡]Baseline age: mean 6.1, range 2–11; mean exposure, 12.5 y.

in the street culture of adolescent Harlem gangs or peer groups, partly because of parental pressures, were also less consistent users of AAVE. It may be that youth in families most concerned about the potentially adverse effects of the "code of the street" participated less in vernacular peer groups and were most prepared to switch away from AAVE when moving to lower-poverty areas.

Our estimates have implications for how neighborhood effects on AAVE use may shape the long-term life outcomes of these youth. As noted above, given the current structure of American society, AAVE use is associated with adverse schooling, housing and labor market outcomes. Many researchers and policymakers are appropriately concerned about and are working to change how individuals and organizations interact with people who use vernaculars. However, in the meantime, changes in neighborhood economic and racial segregation may change school or economic outcomes by changing AAVE speakers' inclination and ability to code switch to SAE.

One challenge for assessing this hypothesis is that there are no plausibly causal estimates of the relationship between AAVE use and different key life outcomes. However, as described in *SI Appendix*, we used data from the MTO control group to regress different outcomes against AAVE use and some basic demographic controls. This exercise is necessarily speculative, because at least part of the association between AAVE use and different long-term life outcomes may be due to omitted variables. With this caveat in mind, our illustrative calculations suggest that the effects on AAVE use of spending 10–15 y in a neighborhood with a poverty rate ~11 percentage points lower than the youth would otherwise have (the MTO ITT effect) could increase annual earnings by approximately $350. Because only approximately half of the LPV group youth's families used their MTO vouchers to move to low-poverty areas, the effect on those who actually moved through MTO (the treatment-on-the-treated effect) could be as high as approximately $700 per year, or approximately $18,000 in present value over the youth's entire working career (3–4% of lifetime income). The results we present here thus may provide at least a partial explanation for the recent findings of long-term beneficial effects of approximately $3,500 per year on the adult earnings of MTO youth who were under age 13 at baseline (21).

Our finding that MTO moves change the frequency of AAVE use by youth is consistent with previous observational research in sociolinguistics showing that lower- and working-class status is correlated with the highest frequency of AAVE use within the African-American community (see, for example, ref. 8). Various studies have shown that the level of income segregation in American neighborhoods has been increasing since 1970 and that concentrated poverty (the share of poor families living in census tracts with poverty rates of 40% or more) has increased as well, including during the 2000s (42–44). Our findings raise the possibility that rising U.S. residential economic segregation may be contributing to growing differences within the population in AAVE use in a manner that could further exacerbate the economic disadvantages of youth growing up in high-poverty areas. Although efforts to eliminate discrimination in schools, labor markets, and criminal justice settings are of critical importance, policies to reverse the trend toward increased economic segregation may also play a role in shaping black-white inequality in language and hence in life outcomes.

Materials and Methods

Our research team subcontracted with the Institute for Social Research at the University of Michigan to collect in-person data on outcomes for the MTO study sample in 2008–2010, or 12 y after baseline on average (range 10–15 y). The effective response rates for these long-term follow-up surveys were 90% for the adult household heads in MTO and 89% for all youth who were selected for the survey (who were between the ages of 10 and 20 at the end of 2007). Among the non-Hispanic African-American youth on whom we focus in this paper (ages 13–20 only), response rates were similar across randomized MTO groups (*SI Appendix*, Table S1), as were the characteristics of the interviewers working with each group (*SI Appendix*, Table S4). We successfully collected and transcribed speech data from 71% of the youth who were eligible for language data collection (68% for the LPV group and 73% for the control group), which leaves us with a final sample size of n = 629. (See *SI Appendix* for additional details). Our study was reviewed and approved by the federal Office of Management and Budget and the Institutional Review Boards at HUD, the National Bureau of Economic Research, the University of Chicago, the University of Michigan, and Northwestern University.

The five grammatical language features that were coded as AAVE or SAE are as follows: (*i*) use of "ain't" rather than standard negators like "aren't," "isn't," and "hasn't"; (*ii*) multiple negation, involving the use of negative indefinites like "never," "nothing," or "no one" in addition to a negated auxiliary verb like "shouldn't"; (*iii*) absence of third singular present tense –"s"

(as in "He walkØ" for "He walks"); (*iv*) absence of copula or auxiliary "is" or "are" (as in "They Ø happy" for "They are happy"); and (*v*) "was"-leveling (as in "They was nice" for "They were nice"). The five phonological language features were as follows: (*i*) consonant cluster reduction (as in "fas" for "fast"); (*ii*) r-deletion or vocalization after a vowel (as in "mothuh" for "mother"); (*iii*) DH-stopping (as in "dis" for "this"); (*iv*) TH-stopping (as in "wit" for "with" or "mout" for "mouth"); and (*v*) "ai" monophthongization (as in "rad" for "ride" or "ah" for "I"). A token can be a single phoneme or pronunciation segment (e.g., pronouncing "them" with either an initial "th" or with a "d") or a grammatical form (e.g., using the full, contracted, or deleted form of "is" in "She is~'s~Ø cold"). Note that a single phrase like "She should do nothing" can be examined for multiple examples of AAVE use—for example, both double negation ("She 'shouldn't' do 'nothing'" and use of "th" vs. "t" in "nothing"), so contributing two tokens to the analysis sample. Coding of both r-deletion and DH-stopping was generally capped at a maximum of 10 tokens per speaker.

To estimate the effects of the offer to use an LPV voucher, known as the ITT effect, we regress AAVE use (Y) on an indicator (Z) for whether youths' families were assigned to the LPV group instead of the control group and a set of baseline covariates (X) to improve statistical precision:

$$Y = Z\beta_1 + X\beta_2 + \varepsilon \quad [1]$$

We use linear regression to estimate Eq. **1**. We cluster SEs at the family level to account for the nonindependence of tokens taken from the same individual and from youth in the same families. The results are not sensitive to dropping the baseline covariates.

To approximate the effects of actually moving with an MTO voucher, under the assumption that assignment to the LPV group in MTO only affects the language use of those who move through MTO, we also report results dividing the ITT effect by the share of those assigned to the LPV group who relocate using an MTO voucher (*SI Appendix*, Table S12).

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