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ORIGINAL PAPER

The price of admission: does moving to a low-poverty neighborhood increase discriminatory experiences and influence mental health?

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Abstract

Purpose The Moving to Opportunity (MTO) study is typically interpreted as a trial of changes in neighborhood poverty. However, the program may have also increased exposure to housing discrimination. Few prior studies have tested whether interpersonal and institutional forms of discrimination may have offsetting effects on mental health, particularly using intervention designs.

Methods We evaluated the effects of MTO, which randomized public housing residents in 5 cities to rental vouchers, or to in-place controls ($N = 4248$, 1997–2002), which generated variation on neighborhood poverty (% of residents in poverty) and encounters with housing discrimination. Using instrumental variable analysis (IV), we derived two-stage least squares IV estimates of effects of neighborhood poverty and housing discrimination on adult psychological distress and major depressive disorder (MDD).

Results Randomization to voucher group vs. control simultaneously decreased neighborhood % poverty and increased exposure to housing discrimination. Higher neighborhood % poverty was associated with increased psychological distress [$B_{IV} = 0.36$, 95% confidence interval (CI) 0.03, 0.69] and MDD ($B_{IV} = 0.12$, 95% CI −0.005, 0.25). Effects of housing discrimination on mental health were harmful, but imprecise (distress $B_{IV} = 1.58$, 95% CI −0.83, 3.99; MDD $B_{IV} = 0.57$, 95% CI −0.43, 1.56). Because neighborhood poverty and housing discrimination had offsetting effects, omitting either mechanism from the IV model substantially biased the estimated effect of the other towards the null.

Conclusions Neighborhood poverty mediated MTO treatment on adult mental health, suggesting that greater neighborhood poverty contributes to mental health problems. Yet housing discrimination-mental health findings were inconclusive. Effects of neighborhood poverty on health may be underestimated when failing to account for discrimination.

Keywords Discrimination · Housing · Neighborhood poverty · Psychological distress · Randomized controlled trial

Abbreviations

2SLS	Two-stage least squares
CI	Confidence interval
ITT	Intent-to-treat
IV	Instrumental variable
MDD	Major depressive disorder
MTO	Moving to Opportunity

Introduction

The US is highly segregated along racial and socioeconomic lines, [1] and such segregation is thought to be a central cause of enduring social and health inequalities [2, 3]. However, efforts to reduce segregation may inadvertently increase encounters with interpersonal discrimination. A geography of opportunity perspective highlights the

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interrelationship among interpersonal racism, discrimination, and racial segregation, as contributing to an enduring "spatial racism". Discrimination behavior by white individuals in housing transactions excludes minorities from benefiting in housing markets [4–6] (e.g., housing discrimination). Black individuals living in lower proportion black neighborhoods encounter more discrimination than those living in higher proportion black neighborhoods [7] and such discrimination erodes mental health [8, 9]. Since minority racial composition and racial segregation are strongly positively associated with neighborhood poverty, [10] defined as the percent of residents in a census tract living below the poverty line, the benefits of moving to lower poverty neighborhoods (and therefore to lower proportion black areas) on mental health may be offset by an increase in discrimination and its adverse effect on mental health. However, interpersonal and structural forms of discrimination are rarely modeled simultaneously for their effects on health [6].

Although evidence comes predominately from observational studies of discrimination or neighborhood context on mental health [9, 11, 12], estimates may be biased by unmeasured confounders, particularly mobility-related selection, which has been deemed the most serious threat to causal inference in neighborhood studies [13]. To overcome such issues, we use the Moving to Opportunity (MTO) trial, a social experiment that randomized a Section 8 affordable housing rental subsidy to volunteer low-income families living in public housing. These families used the voucher to subsidize rent in private market apartments located in lower poverty neighborhoods than the public housing developments [14].

Figure 1 illustrates our hypothesized causal model, in which randomly assigned MTO treatment may affect mental health via either neighborhood poverty or housing discrimination, and both of these associations with mental health are potentially confounded by unobserved variables ("C"). In causal structures such as Fig. 1, instrumental variable (IV) estimates can be derived for the effects of both mediators on the outcome, by using multiple IVs [15]. The MTO study, because of its randomized treatment, offers a rare opportunity to leverage an exogenous factor that affected neighborhood mobility, neighborhood poverty, housing discrimination, and mental health. In this manuscript, we use these associations along with instrumental variable analysis to test our primary hypotheses of whether neighborhood poverty and housing discrimination both have causal effects on two adult mental health measures.

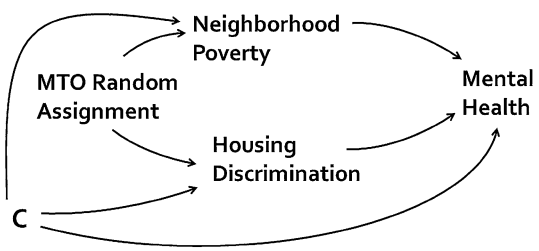

Fig. 1 Causal diagram (directed acyclic graph)

Methods

Data

The Moving to Opportunity (MTO) for Fair Housing Demonstration Project was initiated by the US Department of Housing and Urban Development [16] in five cities: Boston, Baltimore, Chicago, Los Angeles, and New York. Eligible families had children under 18 years old and lived in public housing or project-based assisted housing in high-poverty census tracts (over 40% poverty). Public Housing Authorities contacted eligible households; interested household heads applied and were placed on a waiting list. Applicants were drawn from waiting lists for intake, given an explanation of the program, signed enrollment agreements and informed consent forms, completed the baseline survey, and were evaluated for eligibility before random assignment. 5301 families volunteered, and 4610 families were eligible and randomly assigned [14].

Assessment Surveys among household heads were conducted at baseline (1994–1998) and at the interim evaluation (2001–2002, 4–7 years after random assignment). Most interviews were conducted in-person via computer-assisted personal interviewing technology [14, 17]. We focus on adult household heads ($n=4248$) randomized through 12/31/1997 in the MTO Tier 1 Restricted Access Data (90% effective response) [14]. Our institutions' Institutional Review Boards approved this study.

Measures

Treatment assignment Special software randomly assigned eligible MTO families to one of three conditions. The "regular Section 8" treatment group was offered a Section 8 housing voucher to move from public housing to a qualified, subsidized private market rental apartment in any neighborhood within 90 days (after which time the voucher offer expired). The "low-poverty-neighborhood" treatment group was also offered this Section 8 housing voucher, but the voucher was redeemable only for apartments in neighborhoods where $<10\%$ of tract residents were impoverished. Low-poverty neighborhood families also were offered housing counseling services to aid relocation. Finally, an untreated control group received no further assistance but could remain in public housing [17]. Treatment was modeled in 3 contrast-coded categories, as randomized, with controls as the referent.

Treatment adherence In sensitivity analyses we defined treatment adherence as *using* either offered experimental voucher (modeled separately) to lease an apartment within 90 days [14, 18]. By definition, no experimental voucher was available to the control group so there was full compliance among controls. Approximately half of families randomly assigned either offer of an experimental voucher took up the offer and moved using the voucher within 90 days.

Mental health (outcomes) Past-month psychological distress was measured at interim in 2002, by the Kessler-6 (K6) scale, a valid, reliable broad-gauged screen for non-specific psychological distress [19]. It includes five-item Likert responses, ranging from all of the time to none of the time, for 6 items: so sad nothing could cheer you up; nervous; restless or fidgety; hopeless; everything was an effort; worthless. We scored distress by calculating a mean across items [Cronbach's alpha = 0.86, mean (SD) = 1.96(0.95)]. Dimensional measures tap different constructs than diagnostic measures, so we conducted sensitivity analyses using *past-year major depressive disorder* (MDD) as a secondary diagnostic outcome (also measured in 2002). MDD was measured using the Composite International Diagnostic Interview Short Form (CIDI-SF), a short scale assessing major depressive episodes with high accuracy [20].

Neighborhood poverty was defined as the proportion of census tract residents living under the poverty line from census data, linked to the census tract of residence of the family's address history from baseline through 2002. We interpolated each percent poverty value linearly between 1990 and 2000 in the residential history, with values 2001–2002 interpolated to 2000 values. We then calculated the average tract poverty across the residential history (baseline to 2002) for these analyses. In IV analyses, we modeled poverty such that a one-unit change reflects a 30%-point change in poverty, since this is the average change in poverty experienced by low-poverty neighborhood group members immediately after moving with the voucher (i.e., the neighborhood poverty change targeted by MTO).

Housing discrimination was reported at the 2002 interim evaluation by the adult household head based on a two-part question. Part 1 asked "Since [year of random assignment] have you gone in person to rent a house or apartment you thought was available and been told by a landlord, real estate agent, or manager you could not rent it?" Those answering yes to Part 1 were asked Part 2: "For the most recent time this happened, what was the main reason they gave for not renting the house or apartment to you?" Respondents were considered to have encountered housing discrimination if they reported any of these explanations: "don't rent to Section 8"; "don't rent to people from public housing"; "don't rent to people with children or with too many children"; "don't rent to White/Black/Hispanic/Asian people". Although it was not explicitly noted on the survey and respondents may not have been aware, denying housing due to race/ethnicity or the presence of children is illegal per federal law, and 4 of the 5 MTO cities prohibit discrimination based on the source of rental income (e.g., Section 8). Six percent of the sample reported such housing discrimination.

Covariates Covariate adjustment in experimental designs is not strictly necessary for internal validity; however, it often improves efficiency without compromising type-1 error [21]. Therefore, we adjusted for pre-randomization covariates, including demographic, socioeconomic, and housing preference variables (see Table 1 for details). Covariate adjustment had little effect on results.

Analytic approach

To assess the consistency of our hypothesized causal structure (Fig. 1) with the MTO data, we first estimated the association between random assignment and experiences of housing discrimination and neighborhood percent poverty [22]. In table notes, we also report F tests from the first stage of the IV models to evaluate the strength of the instruments [23]. We use intention-to-treat (ITT) linear regression models, although logistic regression-based estimates of the effect of randomization on binary outcomes produced qualitatively similar findings. We then used 2-stage least squares (2SLS) instrumental variable (IV) models estimated using Stata's ivreg2, as discussed for experimental designs [15, 24–26] to estimate the joint effects of housing discrimination and neighborhood poverty on mental health.

Typical IV analyses use 2SLS, in which an endogenous variable ($\hat{E}$), assumed to fully mediate the effect of the instrument on the outcome, is the dependent variable in a linear regression model estimated for the first stage:

$$\hat{E} = \beta_0 + \beta_1(\text{Treatment\_Arm}) + \beta_k(\text{Other\_Covariates}). \quad (1)$$

The predicted value of the endogenous variable is then used as an independent variable in the second stage, to predict psychological distress (Y):

$$Y = \gamma_0 + \gamma_1\left(\hat{E}\right) + \gamma_k(\text{Other\_Covariates}) + \varepsilon. \quad (2)$$

If the effects of the endogenous variables on the outcome are homogeneous for all individuals in the population, the 2SLS coefficient provides a consistent estimate of the Population Average Treatment Effect [27]. When homogeneous treatment effects seem implausible, the monotonicity assumption is commonly invoked to equate the 2SLS estimate with the Local Average Treatment Effect [28]. In the current setting, monotonicity is harder to define precisely because there are two endogenous variables, one of which is continuous. A third option, which we adopt here, is to assume that the average effect

Table 1 Moving to Opportunity adults, baseline variables (measured 1994–1997), overall and by treatment group

Construct	Variable	Overall	Means by treatment group			*P* value
			Low-poverty neighborhood Section 8	Geographically unrestricted Section 8	Controls	
Total in interim survey, 2002	*N*	3526	1453	993	1080	
Age	Years	33.5	33.4	33.7	33.3	0.618
Gender, %	Female	98.5%	99.0%	98.1%	98.2%	0.123
Race/ethnicity, %	African American	63.8%	64.4%	63.4%	63.4%	0.865
	Hispanic ethnicity, any race	28.9%	28.0%	30.0%	29.1%	0.657
	White	2.7%	2.9%	2.5%	2.8%	0.914
	Other race	3.8%	3.6%	3.8%	4.2%	0.107
	Missing race	0.7%	1.1%	0.2%	0.6%	0.809
Site, %	Baltimore	15.0%	15.0%	14.8%	15.2%	0.978
	Boston	21.7%	21.7%	22.2%	21.4%	0.932
	Chicago	22.7%	23.2%	22.9%	22.0%	0.826
	Los Angeles	15.6%	15.6%	15.0%	16.3%	0.704
	New York	24.9%	24.6%	25.1%	25.1%	0.959
Household size	2 people	21.5%	22.8%	20.7%	20.5%	0.425
	3 people	30.9%	30.3%	30.5%	32.0%	0.687
	4 people	22.7%	23.1%	22.6%	22.4%	0.917
	≥ 5 people	24.8%	23.7%	26.1%	25.1%	0.483
Family structure, %	Never married	62.1%	61.9%	62.2%	62.2%	0.993
	No teens (age 13–17) in home	60.8%	59.2%	61.2%	62.5%	0.327
Socioeconomic position, %	Employed	26.7%	28.6%	25.4%	25.2%	0.082
	On AFDC (welfare)	74.5%	74.0%	74.7%	75.1%	0.878
Education, %	High school diploma	37.3%	38.8%	37.9%	34.6%	0.137
	GED	17.9%	16.8%	17.9%	19.2%	0.423
	In school	15.7%	16.0%	15.6%	15.5%	0.628
Household characteristics, %	Household member with disability	16.4%	16.3%	16.7%	16.2%	0.895
	Household member victimized in past 6 months	41.9%	42.0%	42.6%	41.2%	0.851
Neighborhood/mobility variables, %	Moved > 3 times in past 5 years	9.1%	8.0%	9.0%	10.6%	0.258
	Very dissatisfied with neighborhood	46.4%	46.2%	46.9%	46.3%	0.983
	Streets near home very unsafe at night	48.8%	48.3%	49.1%	49.2%	0.951
	Very sure would find a new apartment in different part of the city	45.7%	44.8%	48.1%	44.6%	0.372

All variables range between 0 and 1 except baseline age (17–87), so means represent proportions. Analysis weighted for varying treatment random assignment ratios across time and for attrition. Missing baseline covariate data were imputed to site-specific means. *P* value for test of treatment group differences calculated from Wald Chi-squared tests output from logistic regression for dichotomous baseline characteristics. *F* tests were used with linear regression for continuous variables. The null hypothesis is that none of the treatment group proportions or means differ

AFDC aid to families with dependent children, *GED* general equivalency diploma

of each endogenous variable does not vary by levels of the unmeasured confounders. In other words, there may be unmeasured factors that influence both neighborhood poverty and psychological distress, but we assume that the *average causal effect* of neighborhood poverty on distress is similar regardless of the value of such confounders. With this assumption, we can interpret the 2SLS coefficients as the effect of each endogenous variable on the outcome *at the level of the endogenous variable actually experienced by the participant*.

To identify two endogenous variables, we require at least two IVs, and additional IVs are needed to implement overidentification tests to assess the validity of the IV assumptions. We created additional instruments by interacting the two MTO voucher treatment groups with pre-random assignment baseline disability, defined as household head report

that anyone in the household has a disability. We chose disability using empirical criteria consistent with prior research [15] since it satisfied the assumptions required for creating multiple instruments using baseline variables interacted with treatment. To confirm the validity of these treatment interactions as instruments, there must be no interaction between the baseline variable used as a 2nd instrument (e.g., disability) and the endogenous variables (neighborhood poverty and discrimination) on mental health [15] (homogeneity test), which we confirmed empirically. One advantage of over-identification of the IV equation is to facilitate evaluation of the validity of the instruments, as recommended in previous research, [29, 30] by testing for an association between the instruments and the second-stage error term, reported as the Hansen J statistic or the Sargan–Hansen test [15, 22, 31]. As reported in the Table 4 notes, we failed to find empirical evidence against the null. Results were comparable when using the original treatment groups as instruments (Appendix eTable 1).

We hypothesize that both neighborhood poverty and housing discrimination mediate the MTO experimental effects on mental health, and, furthermore, they may have countervailing effects. Therefore, we included both endogenous variables simultaneously in our final models. However, we also estimate separate IV models for each hypothesized endogenous variable, recognizing that under our hypothesized causal structure, these IV estimates are potentially biased because of exclusion restriction violations via the other (omitted) endogenous variable. We present these models because most studies of discrimination or neighborhood do not consider (or model) the other for its association with mental health; [6] but doing so may yield biased results.

We evaluated how sensitive our results for MDD (a binary variable) were to the application of linear models (2SLS) using G-estimation, [32] which does not rely on correct specification of the outcome from linear regression, but only on randomization of the instrument. Results were almost identical (Appendix eTable 2) with few predicted probabilities of MDD reported outside the 0–1 range (Appendix eTable 3), so we present conventional 2SLS models.

We conducted a number of sensitivity models to test potential violations of IV assumptions, as guided by prior literature [22, 29, 30]. To help rule out the possibility that neighborhood characteristics aside from percent poverty and encounters with discrimination might create pathways linking treatment group and mental health (i.e., violations of the exclusion restriction), we replaced neighborhood poverty with an indicator for treatment adherence (i.e., whether treatment group families complied and used the housing voucher, a.k.a. "leaseup") to block other back-door paths. Leaseup is a mediator further upstream in the causal model than any neighborhood characteristic, so we hypothesize it would capture all neighborhood effects together. Results using leaseup in place of neighborhood poverty (both including discrimination) generated estimates in the same direction for housing discrimination, and negative estimates for leaseup (as expected, because leaseup is associated with lower % poverty) (see Appendix eTable 4). This provides some evidence that the exclusion criteria with respect to neighborhood quality variables are satisfied. We also conclude that neighborhood poverty is a proxy for the entire leaseup effect, as the so-called 'active treatment' within the MTO treatment. We then tested associations between our multiple instruments and baseline variables; as expected, our randomized instruments exhibited no associations with baseline variables (Appendix eTable 5).

All analyses were conducted in STATA 11.0 using robust standard errors [31], and weighted to account for random assignment ratio changes and attrition [18].

Results

Table 1 presents the MTO sample descriptives at baseline across the three treatment groups. There were no associations between any baseline variable and treatment group assignment.

ITT results

The ITT results (Table 2) demonstrate that random assignment to the low neighborhood poverty housing voucher group led to lower adult psychological distress ($B = -0.09$ 95% confidence interval (CI) $= -0.18$ to -0.01) and to marginally lower past-year MDD ($B = -0.03$, CI -0.06 to 0.00) compared to controls in public housing. Assignment to the regular Section 8 Treatment group was not associated with distress or MDD.

Random assignment to either housing voucher treatment led to *higher* housing discrimination in the treatment groups compared to controls (low poverty $B = 0.04$ CI 0.02–0.05; Section 8 $B = 0.06$ CI 0.04–0.09). Random assignment to either treatment group also led to *lower* % neighborhood poverty vs. controls (low poverty treatment: $B = -12.3$, CI -13.6 to -11.0; Section 8 treatment: $B = -10.1$ CI -11.4 to -8.9). There was imperfect adherence to the treatment; 59% of the Section 8 group, and 48% of the Low Poverty group, used the voucher to lease an apartment. MTO treatment group participants who reported housing discrimination also exhibited higher leaseup rates (Appendix Fig. 1).

IV results

The F test diagnostic from the first-stage IV estimation (see Table 3 notes) vastly exceeds recommended thresholds for neighborhood poverty, demonstrating that MTO treatment

Table 2 Intent-to-treat estimates of random assignment effects on psychological distress, major depressive disorder, subjective housing discrimination, and neighborhood poverty in the Moving to Opportunity experiment

Outcome	ITT: low-poverty neighborhood voucher treatment group vs. controls			ITT: Regular Section 8 voucher treatment group vs. controls		
	B	LCI	UCI	*B*	LCI	UCI
Distress	−0.09	−0.18	−0.01	−0.03	−0.13	0.07
Major depression	−0.03	−0.06	0.00	−0.01	−0.05	0.03
Subjective housing discrimination	0.04	0.02	0.05	0.06	0.04	0.09
Average tract % poverty	−12.30	−13.60	−11.00	−10.11	−11.35	−8.88

Models are weighted but unadjusted for covariates. Average tract percent poverty represents the average neighborhood poverty across 1994–2002 (from 90 days after baseline through 2002) based on linearly interpolated tract measures from 1990 to 2000 census data, modeled as a 0–1 variable. $N = 3526$ for distress, housing discrimination, and tract % poverty models; 3520 for MDD models

Table 3 Instrumental variable analysis, second stage results, modeling two endogenous mediators simultaneously, for effects on adult mental health, in the Moving to Opportunity experiment

	Psychological distress			Major depressive disorder		
	B	LCI	UCI	*B*	LCI	UCI
Subjective housing discrimination	1.58	−0.83	3.99	0.57	−0.43	1.56
Average tract % poverty	0.36	0.03	0.69	0.12	−0.005	0.25

Distress models weighted and adjusted for baseline age, race, site, employment, welfare, education, in school status, disability, victimization, prior moves, neighborhood dissatisfaction, confidence in finding a new neighborhood, teens in household, household size, and marital status. MDD models weighted and adjusted for baseline race, site, welfare, in school status, disability, victimization, neighborhood dissatisfaction, confidence in finding a new neighborhood, and neighborhood safety at night. Average tract percent poverty represents the average neighborhood poverty across 1994–2002 (from 90 days after baseline through 2002) based on linearly interpolated tract measures from 1990 to 2000 census data. Neighborhood poverty modeled such that a one-unit change reflects a 30 percent point change in poverty. Two random assignment groups and treatment interactions with baseline household disability used as instruments. First stage *F* tests results for distress: housing discrimination 10.07 ($p < .001$); tract poverty 103.89 ($p < .001$). First stage *F* tests results for MDD: housing discrimination 9.53 ($p < .001$); tract poverty 102.26 ($p < .001$). $N = 3526$ for distress models; 3520 for MDD models. Hansen J statistic = 0.734, chi-sq *p* value = 0.69 for distress; = 0.971, chi-sq *p* value 0.62 for MDD

Table 4 Instrumental variable analysis, second stage results, modeling one endogenous mediator at a time, for effects on adult mental health, in the Moving to Opportunity experiment

	Psychological distress			Major depressive disorder		
	B	LCI	UCI	*B*	LCI	UCI
Subjective housing discrimination	−0.33	−1.76	1.09	−0.09	−0.70	0.52
Average tract % poverty	0.18	−0.01	0.38	0.06	−0.01	0.14

Distress models weighted and adjusted for baseline age, race, site, employment, welfare, education, in school status, disability, victimization, prior moves, neighborhood dissatisfaction, confidence in finding a new neighborhood, teens in household, household size, and marital status. MDD models weighted and adjusted for baseline race, site, welfare, in school status, disability, victimization, neighborhood dissatisfaction, confidence in finding a new neighborhood, and neighborhood safety at night. Average tract percent poverty represents the average neighborhood poverty across 1994–2002 (from 90 days after baseline through 2002) based on linearly interpolated tract measures from 1990 to 2000 census data. Neighborhood poverty modeled such that a one-unit change reflects a 30 percent point change in poverty. Two random assignment groups and treatment interactions with baseline household disability used as instruments. First stage *F* tests results for distress: housing discrimination 10.07 ($p < .001$); tract poverty 103.89 ($p < .001$). First stage *F* tests results for MDD: housing discrimination 9.53 ($p < .001$); tract poverty 102.26 ($p < .001$). $N = 3526$ for distress; 3520 for MDD models

and interactions provide strong instruments for neighborhood poverty. The instruments are weaker for discrimination, but still produce *F* tests at approximately the recommended threshold [23].

In second stage IV estimation, higher neighborhood poverty (e.g., a 30%-point increase) was associated with higher psychological distress ($B = 0.36$ CI 0.03 to 0.69) and marginally higher MDD (B = 0.12, CI −0.005 to 0.25) as hypothesized (Table 3). Although higher housing discrimination was associated with worse mental health, confidence intervals were wide (distress $B = 1.58$, CI −0.83 to 3.99; MDD $B = 0.57$, CI −0.43 to 1.56).

When housing discrimination was modeled as the sole endogenous variable (i.e., without adjusting for neighborhood poverty), the IV estimates of the effect of discrimination on mental health had the opposite signs as in models accounting for the mechanism via neighborhood poverty. Similarly, when neighborhood poverty was modeled as the sole endogenous variable (i.e., without adjusting for discrimination), the effect of neighborhood poverty was underestimated by half, although in the same direction as when it was simultaneously modeled with discrimination (Table 4) (Distress $B = 0.18$, CI −0.01 to 0.38; MDD $B = 0.06$, CI −0.01 to 0.14).

Discussion

Using data from a randomized experiment, our results suggest that mental health effects of a rental housing voucher may have been simultaneously mediated by beneficial effects of moves to lower poverty neighborhoods, and adverse effects of encountering housing discrimination during efforts to locate private market apartments. Although point estimates for the effect of discrimination were large, the estimates were imprecise. Estimated effects of low-poverty neighborhoods on mental health are substantially larger when accounting for encounters with discrimination.

Until recently, analyses of MTO data have defined leaseup as the relevant mediator of MTO treatment effects on health, [14, 18] although recent studies have tested whether other variables, such as housing quality, mediate MTO treatment on health effects [33–35]. Our analysis suggests that focusing exclusively on leaseup may be misleading; it is important to integrate other constructs, like discrimination and neighborhood quality, into evaluations of MTO effects on adult mental health.

There are several forms of racism, and they may operate independently or jointly to influence health. For example, institutional racism is the "differential access to goods, services, or opportunities of society by race" [36] (p. 1212), while interpersonal or personally mediated racism is defined as "prejudice and discrimination where prejudice means differential assumptions about the abilities, motives, and intentions of others according to their race, and discrimination means differential actions towards others according to their race", intentional or not [36] (pp. 1212–3). We tested these two forms of discrimination as mediators of the effects of MTO on adult mental health: institutional racism, measured as tract percent poverty, and interpersonal racism, measured as subjective housing discrimination. Operationalizing both interpersonal and institutional forms of discrimination is rarely done in the health literature [6]. We document, in a directly randomized setting, that low-income minorities who seek private market housing using a housing voucher are systematically exposed to more illegal housing discrimination. Institutional and interpersonal racism are conceptually related, but it is important to distinguish between them because the very activities necessary to reduce exposure to institutional racism may lead to increases in exposure to interpersonal racism.

Although the public health literature privileges interpersonal discrimination over institutional discrimination [6, 37], our results, and the larger social determinants of mental health literature [38, 39] suggest that structural context (neighborhood environment) is an important contributor to mental health problems. However, our results for the consequences of interpersonal discrimination on mental health were inconclusive; the association estimated from our IV models is in the same (harmful) direction as the literature (when simultaneously modeling neighborhood poverty as a mediator, although the coefficient was reversed when we did not), but the effect estimates are imprecise. We used a conservative definition of discrimination based on the real estate manager informing the victim that s/he had denied a housing request based on illegal reasons. Only 6% of respondents reported they were provided a reason for housing denial that was illegal and discriminatory. Presumably, most real estate managers would be sufficiently sophisticated to lie about their motivations rather than report an illegal activity to the victim of that action; indeed, 18% of the sample reported discrimination for any reason, regardless of legality. We therefore expect the measure we used captures only a fraction of all housing discrimination. Further, we are not addressing encounters with discrimination in any setting other than housing [9]. But the same in-group/out-group dynamics that increase housing discrimination may trigger discriminatory encounters in many other domains in a new neighborhood [7]. Thus, we are not able to evaluate conclusively whether housing discrimination is causally associated with mental health.

The finding that the experiment caused an increase in reported housing discrimination within the MTO study, linked with prior evidence that discrimination affects mental health [8, 9, 40] calls for an important reinterpretation of MTO-based IV analyses that focus only on the beneficial

effects of the new neighborhoods. For example, adjusting for housing discrimination in IV models doubled the estimated causal effect of neighborhood poverty on both mental health variables. Therefore, neighborhood health effects may be severely underestimated if subjective discrimination is disregarded [6]. Indeed, our results here show that MTO households with children, who were 97% racial minorities, experienced initially higher housing discrimination when they searched for housing in lower poverty neighborhoods, even though they ultimately ended up in better neighborhoods by moving out of high-poverty areas, suggesting these two factors exert countervailing causal effects on health. Therefore, by ignoring racism processes when estimating neighborhood effects, or by ignoring institutional racism when modeling interpersonal racism, we may be misestimating how racism and/or neighborhoods contribute to health disparities. A better understanding of how neighborhoods and place contribute to health disparities may emerge by integrating institutional with interpersonal racism in health research.

Our results from this MTO experiment align with other evidence that minorities report *higher* interpersonal racism if they live in nonblack (vs. black) neighborhoods,[7] and findings in MTO that the older cohort of adolescent boys who moved to new neighborhoods encountered additional police attention [41, 42]. These results suggest that predominantly black neighborhoods shield minorities from the negative health effects of interpersonal discrimination. One act of housing discrimination excludes a household from a certain housing unit, but at the population level, such discrimination prolongs the time and expense involved in a housing search. Housing discrimination is also pinpointed as a major cause of asymmetric racial housing settlement, such as persistently high racial segregation [1, 43].

Limitations

Just as our findings provide an important caveat in the interpretation of previous MTO analyses, our results rest on our hypothesized causal structure, which entails only two endogenous variables linking MTO random assignment to mental health. In fact, a bundle of changes were induced by the MTO treatment, and there may be important endogenous variables that we have not considered [44]. We cannot rule out that interpersonal housing discrimination acts through neighborhood environment (indirect effects). Such a causal model is not implausible; minorities may initially experience subjective discrimination when they seek to rent an apartment in low poverty or in white neighborhoods, but the structural effects of the new neighborhood, once attained, may outweigh the adverse effects of the housing discrimination that they encounter [6].

Even though it was retrospective, housing discrimination was assessed in 2002, at the same time as the mental health outcomes. Therefore, the temporal sequence of discrimination preceding mental health may not have been strictly maintained. We also captured a multidimensional measure of housing discrimination based not only on race, but also on family structure and source of income. These different forms of discrimination may operate differently, but we do not have power to break them out separately. The majority of housing discrimination presumably is not described as such to the target, but rather occurs covertly, for example, when a housing applicant is falsely told the unit is already rented. Although such discrimination is detectable statistically with audit studies, [45] the design of MTO (based on self-report encounters of housing discrimination) could not detect whether this occurred. Similar measures of self-reported retrospective encounters with discrimination in housing searches have been used by the US Department of Housing and Urban Development (e.g., in the Fair Housing Survey [46]) although our measure of housing discrimination may have low sensitivity.

Conclusion

Our results add to a small but increasing body of rigorous evidence that social and economic policies influence health [47]. Section 8 policy, now called the Housing Choice Voucher program, is the primary federal affordable housing policy, used by over 2 million low-income households in America [48]. The treatment delivered in the Housing Choice Voucher program corresponds closely with the treatment delivered in the Section 8 arm of the MTO experiment, and MTO therefore provides a unique opportunity to understand why and how voucher programs may affect mental health. We find evidence that the program may have beneficial effects on mental health by offering opportunities to live in lower poverty communities, but these benefits may be offset by increasing encounters with discrimination. It is important to incorporate multidimensional forms of discrimination and racism into health research, including processes occurring in housing market transactions, to draw attention to the fundamental structure of racism and discrimination for perpetuating racial health disparities.

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Author contributions TLO conceived the hypotheses, obtained the data, conducted the majority of the data analysis, and wrote the majority of the manuscript. MMG aided in writing the paper. MMG and EJTT advised on the statistical analysis and interpretation of findings, in addition to editing the methods. NMS and RDK analyzed the data, created tables, and edited the manuscript.

Compliance with ethical standards

Conflict of interest On behalf of all authors, the corresponding author states that there is no conflict of interest.

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