

Differential Mental Health Effects of Neighborhood Relocation Among Youth in Vulnerable Families

Results From a Randomized Trial

Theresa L. Osypuk, ScD, ScM; Eric J. Tchetgen Tchetgen, PhD; Dolores Acevedo-Garcia, PhD; Felton J. Earls, MD; Alisa Lincoln, PhD; Nicole M. Schmidt, PhD; M. Maria Glymour, ScD

Context: Extensive observational evidence indicates that youth in high-poverty neighborhoods exhibit poor mental health, although not all children may be affected similarly.

Objective: To use experimental evidence to assess whether gender and family health problems modify the mental health effects of moving from high- to low-poverty neighborhoods.

Design: Randomized controlled trial.

Setting: Volunteer low-income families in public housing in 5 US cities between 1994-1997.

Participants: We analyze 4- to 7-year outcomes in youth aged 12 to 19 years ($n=2829$, 89% effective response rate) in the Moving to Opportunity Study.

Intervention: Families were randomized to remain in public housing (control group) or to receive government-funded rental subsidies to move into private apartments (experimental group). Intention-to-treat analyses included intervention interactions by gender and health vulnerability (defined as prerandomization health/developmental limitations or disabilities in family members).

Main Outcome Measures: Past-year psychological distress (Kessler 6 scale [K6]) and the Behavioral Problems

Index (BPI). Supplemental analyses used past-year major depressive disorder (MDD).

Results: Male gender ($P=.02$) and family health vulnerability ($P=.002$) significantly adversely modified the intervention effect on K6 scores; male gender ($P=.01$), but not health vulnerability ($P=.17$), significantly adversely modified the intervention effect on the BPI. Girls without baseline health vulnerabilities were the only subgroup to benefit on any outcome (K6: $\beta=-0.21$; 95% CI, -0.34 to -0.07 ; $P=.003$; MDD: odds ratio=0.42; 95% CI, 0.20 to 0.85; $P=.02$). For boys with health vulnerabilities, intervention was associated with worse K6 ($\beta=0.26$; 95% CI, 0.09 to 0.44; $P=.003$) and BPI ($\beta=0.24$; 95% CI, 0.09 to 0.40; $P=.002$) values. Neither girls with health vulnerability nor boys without health vulnerability experienced intervention benefits. Adherence-adjusted instrumental variable analysis found intervention effects twice as large. Patterns were similar for MDD, but estimates were imprecise owing to low prevalence.

Conclusions: Although some girls benefited, boys and adolescents from families with baseline health problems did not experience mental health benefits from housing mobility policies and may need additional program supports.

Arch Gen Psychiatry. 2012;69(12):1284-1294.

Published online October 8, 2012.

doi:10.1001/archgenpsychiatry.2012.449

MENTAL DISORDERS AND high levels of internalizing and externalizing symptoms in adolescents have negative health and social consequences throughout the life course.¹⁻⁴ Racial/ethnic minority families are disproportionately more likely to live in impoverished neighborhoods,^{5,6} and many research studies suggest that adolescents who reside in high-poverty communities experience elevated

psychiatric morbidity.⁷⁻⁹ However, previous neighborhood-mental health studies have been observational, and, thus, effect estimates are potentially confounded.⁹ Only 1 randomized trial that could evaluate the effects of neighborhood context via housing mobility has been conducted: the Moving to Opportunity trial (MTO). The MTO randomly assigned families to receive offers to move out of public housing using a Section 8 voucher, which subsidizes rent in the private market, and these families

Author Affiliations are listed at the end of this article.

STUDY DESIGN

moved to different neighborhood contexts. This experimental design is advantageous for evaluating the causal effects on mental health related to moving out of public housing and changing neighborhood context.

Although housing policies are not usually considered part of national health services infrastructure, growing evidence documents that social and economic policies may influence health^{6,10-14} by changing social determinants of health, for example, income, housing, and neighborhood context. Reducing mental health disparities will likely require multilevel approaches beyond only individual and family-level interventions; indeed, the stress reduction accompanying a move to a low-poverty neighborhood may be of similar magnitude to that of pharmacologic treatment for a mental disorder.¹⁰

Previous analyses of the MTO documented puzzling evidence. Although neighborhood poverty was reduced in intervention group families compared with public housing controls,¹⁵ the effects of these moves were not uniformly beneficial. Four to 7 years after randomization, health effects for young children were not statistically significant,¹⁶ whereas adult women in the MTO experimental group experienced better mental health and lower obesity; indeed, recently released findings¹⁷ indicate that this obesity advantage persisted 10 to 15 years after randomization. The MTO benefited adolescent girls for psychological distress, lifetime major depressive disorder (MDD), and health behaviors (eg, smoking). Among adolescent boys, however, effects for psychological distress and lifetime MDD outcomes were not statistically significant, and externalizing behaviors were unexpectedly increased by the intervention.^{10,11,15,18}

Because gender has shown such strong qualitative effect modification for other mental health outcomes in the MTO, we hypothesized similar patterns for past-year MDD, which has not yet been examined in the MTO. However, no subsequent study has succeeded in explaining these opposite gender patterns or in extending findings to explain which children may benefit and which may be harmed by housing mobility. Indeed, previous evidence from psychosocial interventions suggests that frailer subgroups may not benefit from interventions to the same extent as healthier individuals and in some circumstances may even be harmed.¹⁹⁻²¹ We, therefore, hypothesized that family health-related vulnerability adversely modifies the effect of using rental subsidies to move out of public housing to lower-poverty neighborhoods. Intervention differences between more and less vulnerable groups may be due to either differential compliance rates (eg, in the MTO, if households with disabled family members were unable to find suitable private market rental units, even with voucher subsidies^{22,23}) or differential effects among compliers (eg, in vulnerable families, the benefit of moving may be offset by other chronic stressors²⁴). This study, therefore, extends previous MTO findings by testing whether the baseline health of families modifies the MTO intervention effect on adolescent mental health in addition to improving operationalization of mental health.

The Moving to Opportunity for Fair Housing Demonstration Program was a randomized controlled trial sponsored by the US Department of Housing and Urban Development^{25,26} in 5 US cities: Boston, Massachusetts; Baltimore, Maryland; Chicago, Illinois; Los Angeles, California; and New York, New York. Eligible families had children younger than 18 years and were recruited from public housing or project-based assisted housing in areas with high concentrations of poverty.²⁶ Volunteer applicants were obtained from the MTO waiting lists, signed enrollment agreements and informed consent forms, completed the baseline participant survey, and were evaluated for eligibility²⁷ by local public housing authorities. Eligible families were randomly assigned by computerized random-number generators to 1 of 3 intervention groups (**Figure 1**).¹⁵ The MTO was not registered with CONSORT (Consolidated Standards of Reporting Trials) because it was not a medical intervention.

Evaluation surveys were conducted by Abt Associates Inc at baseline and at the interim follow-up (4-7 years after baseline, December 2001 to September 2002) among household heads and their children. Ninety-eight percent of interviews were conducted in person via computer-assisted personal interviewing technology.^{15,27} Youth were interviewed predominantly in teen centers to improve privacy.²⁶ We focused on adolescents randomized between 1994 and 1997 in the MTO Tier 1 Restricted Access Data (n=3537; aged 12-19 years as of May 31, 2001 [aged 5-16 years at randomization]). This analysis includes 2829 adolescents who answered the interim survey. With a 2-stage follow-up to locate participants, the interim survey had an 89% effective response rate¹⁵ (Figure 1). Adults provided written informed consent before both surveys for themselves and their children.^{15,26,27} The present analyses were approved by the Northeastern University Institutional Review Board.

VARIABLES

The past-month psychological distress outcome was measured in 2002 by survey using the Kessler 6 scale (K6), a broad-gauged dimensional screening scale for mental illness.²⁸ The K6 scale includes 5-item Likert answers (none to all the time during the past 30 days) for 6 items: so depressed that nothing could cheer you up, nervous, restless or fidgety, hopeless, everything was an effort, and worthless. We scored the K6 scale using 2-parameter binary item response theory latent variable methods, recoding each item into a set of ordered dichotomies and outputting a standardized factor score.²⁸ For observations missing scale items (<3%), we calculated row-column imputed values (Cronbach $\alpha=0.80$, mean [SD]= -0.0395 [1.123]). Given standardization, intervention effect sizes correspond approximately to the proportion of an SD change in the outcome.

The behavior problems outcome was measured in 2002 by 11 self-reported survey items adapted from the Behavior Problems Index (BPI)²⁹ primarily assessing externalizing behaviors. Responses for items such as "I lie or cheat" and "I have a hot temper" ranged from 0 (not true) to 2 (often true). Standardized BPI scores were estimated using 2-parameter binary item response theory models ($\alpha=0.80$, mean [SD]= -0.0250 [1.086]).

In supplemental analyses, we report outcomes for past-12-month and lifetime DSM-IV MDD. Major depressive disorder was assessed in 2002 by survey questions adapted from the National Comorbidity Survey Replication Adolescent Supplement and implemented by trained lay interviewers. This measure displays good concordance with clinically administered interviews, such as the Schedule for Affective Disorders and Schizophrenia for

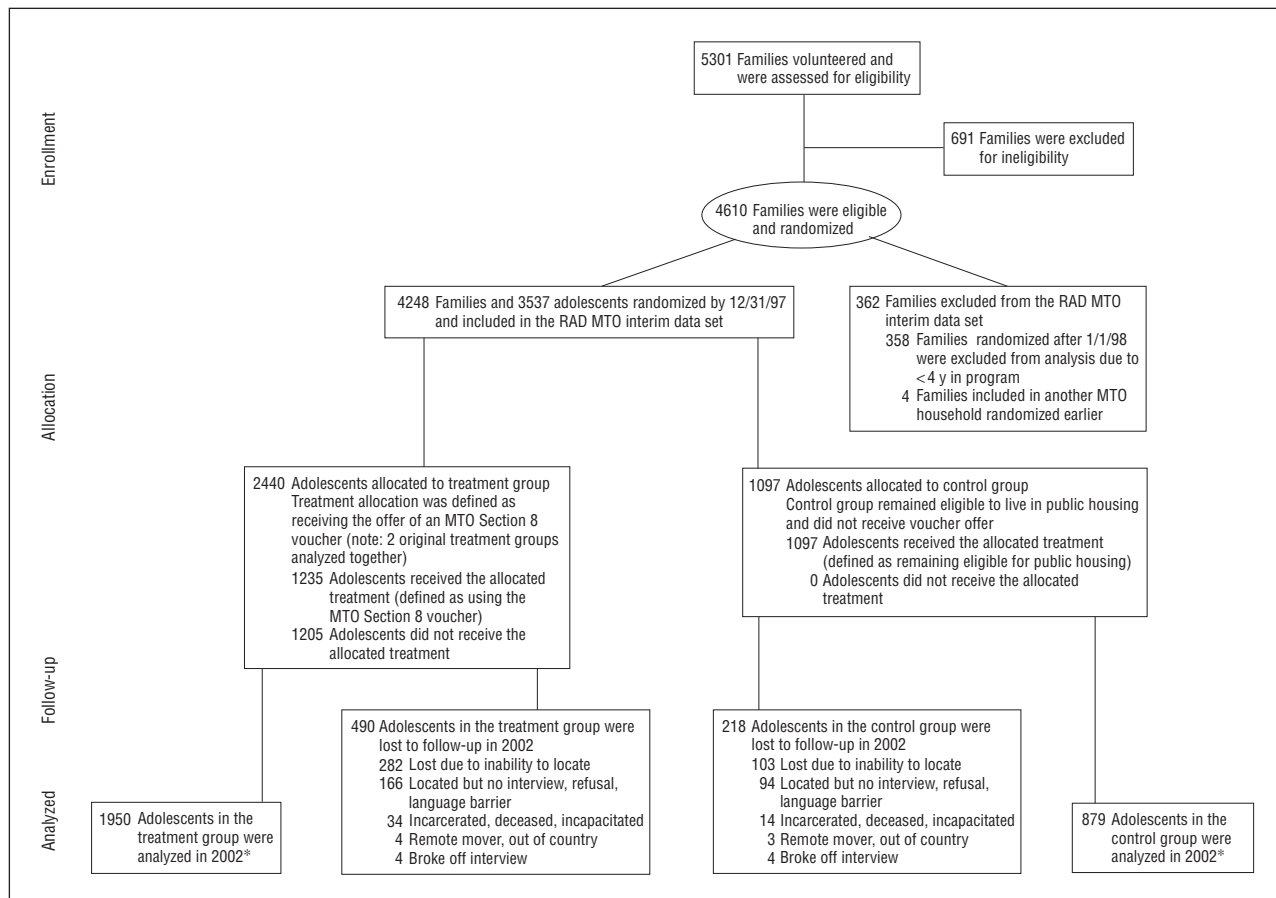


Figure 1. Moving to Opportunity trial (MTO) youth enrollment, intervention allocation, and attrition. *The 2002 interim survey yielded an 89% effective response rate (RR) using a 2-stage follow-up sampling strategy, calculated as $RR = MRR + SRR \times (1 - MRR)$, where MRR indicates the RR for the main sample (respondents initially responding to the 2002 survey interview request); SRR, the RR for the subsample (a second attempt to find every 3 in 10 hard-to-reach families initially nonresponsive in 2002).¹⁵ RAD indicates Restricted Access Data.

School-Age Children,³⁰ both of which seek to assess *DSM* diagnoses. The algorithm to derive MDD is described in the eAppendix (<http://www.archgenpsychiatry.com>). Because of the low prevalence of 12-month MDD (0.029) and lifetime MDD (0.046), we report detailed results in eFigure 1 and eFigure 2. We focus on past-year (vs lifetime) MDD because in the context of a trial, the past-year measure captures MDD during a period that follows random assignment, preserving temporal order between exposure and outcome.³¹ Moreover, results for past-year MDD have not been published in the MTO.

Randomly assigned treatment was modeled with 1 dichotomous variable: intervention vs control group. Although the study contained 2 experimental intervention groups, intervention effects on mental health were statistically indistinguishable across experimental arms (vs controls); therefore, we combined the experimental groups to facilitate the presentation. However, results retaining the original 3 intervention groups are presented in eFigures 3, 4, and 5.

Intervention adherence for families in the experimental groups was defined as using the rental subsidy voucher to lease an apartment^{10,15} (as opposed to random assignment, which provided an offer of a voucher). Many control and experimental families later moved away from public housing using other means besides the MTO voucher. Based on intention-to-treat (ITT) principles, these families are classified according to their original randomly assigned exposure.

Modification of the effect of intervention on mental health was evaluated using prerandomization covariates indicating gender (specified based on previous MTO results) and, separately, fam-

ily health-related vulnerability (specified a priori before the team had access to the data). Health vulnerability was operationalized using a family-level binary variable indicating any household member had a disability or any household that had a child who had any of 4 health or developmental problems: behavioral problems, learning problems, problems that made it difficult to get to school or to play active games, and problems that required special medicine or equipment. We additionally hypothesized that violent crime victimization and socioeconomic position modified the intervention effect, and these results are presented elsewhere. We also stratified the results herein by site.

COVARIATES

We adjusted regression models for site and several prerandomization covariates, including demographic variables and those significantly associated with the outcome, to increase precision³²: adolescent age, black race (parent reported in prespecified categories; collected to monitor racial disparities in fair housing), gifted student, and whether the school asked to discuss the child's schoolwork or behavior problems; and household head's marital status, employment status, education, neighborhood stability, relationships with neighbors, presence of family or friends in the neighborhood, and previous application to Section 8 (**Table 1**). The small proportion of missing baseline covariate data was imputed to site-specific means.¹⁰ Results were statistically comparable in models with vs without covariate adjustment.

Table 1. Baseline Variables for Moving to Opportunity Youth, Overall and by Intervention Group^a

Construct	Overall	Low-Poverty Neighborhood Section 8 Group	Geographically Unrestricted Section 8 Group	Controls
Participants in the interim survey in 2002, total No.	2829	1164	786	879
Tract poverty rate at baseline (1990 values), %	49.8	49.6	49.5	50.5
Family				
Health, %				
Household member had disability, health, or developmental problem	43.0	45.5	41.1	41.5
Household member had a disability	17.2	18.1	17.3	15.8
Site, %				
Baltimore	15.5	15.6	16.6	14.2
Boston	18.9	16.4	20.5	20.7
Chicago	22.4	23.7	22.7	20.4
Los Angeles	18.6	18.7	15.8	21.2
New York	24.6	25.7	24.3	23.5
Household size, %				
2	7.3	7.1	6.5	8.3
3	22.3	22.6	21.4	22.9
4	25.4	26.6	25.7	23.4
≥5 people	45.0	43.7	46.4	45.4
Youth				
Age, mean, y	9.94	9.98	9.95	9.88
Gender, %				
Male	49.9	48.0	51.5	51.0
Female	50.1	52.0	48.5	49.0
Race/ethnicity, %				
Black	62.8	63.5	62.7	62.1
Hispanic ethnicity, any race	30.0	30.1	30.5	29.5
White	1.1	0.9	1.2	1.2
Other race	2.2	2.3	2.4	1.9
Missing race	3.8	3.2	3.2	5.3
Gifted				
Special class for gifted students or did advanced work, %	15.4	14.0	15.8	16.8
Developmental problems, %				
Special school, class, or help for learning problems in past 2 y	16.6	17.4	15.8	16.3
Special school, class, or help for behavioral or emotional problems in past 2 y	7.7	8.3	9.3	5.3
Problems that made it difficult to get to school or to play active games	6.5	7.1	7.1	5.0
Problems that required special medicine or equipment	9.1	9.1	11.3	7.0
School asked to talk about problems child having with schoolwork or behavior in past 2 y	26.3	26.3	27.2	25.4
Household head, %				
Family structure				
Never married	55.9	55.2	55.1	57.5
Teen parent	25.9	26.6	26.1	25.0
Socioeconomic status, %				
Employed	25.8	28.3	23.1	25.3
On AFDC, welfare	76.0	75.9	75.1	76.9
Education, %				
<High school	47.1	49.0	44.9	46.7
High school diploma	36.2	36.0	37.5	35.3
GED	16.7	15.0	17.7	17.9
In school	13.9	13.0	16.4	12.6
Neighborhood/mobility variables, %				
Lived in neighborhood ≥5 y	65.7	62.9	69.8	65.5
No family living in neighborhood	64.1	65.3	60.2	66.3
No friends living in neighborhood	37.3	39.6	32.9	38.5
Had applied for Section 8 voucher before	44.3	46.0	40.3	45.8
Neighbor relationships, %				
Chats with neighbors at least once a week	51.9	51.0	51.5	53.2
Very likely to tell neighbor if he or she saw neighbor's child getting into trouble	56.7	57.1	56.5	56.4

Abbreviations: AFDC, Aid to Families with Dependent Children; GED, General Equivalency Diploma.

^aAll the variables range from 0 to 1 except baseline age (5-16 years). The analysis was weighted for varying intervention random assignment ratios across time and for attrition. Tests of intervention group differences were Wald statistics output from logistic regression for dichotomous baseline characteristics and multinomial logistic regression for categorical characteristics. *F* tests were used with linear regression for continuous variables. All the tests were adjusted for clustering at the family level; the null hypothesis was that none of the 3 intervention group proportions or means differed. Intervention group proportions differed significantly at *P* < .05 for special school/class/help for behavioral/emotional problems.

Table 2. Moving to Opportunity Intervention Effects at Interim (4- to 7-Year) Follow-up on Mental Health in Adolescents, Effect Modification by Gender^a

Variable	Psychological Distress						Behavior Problems					
	ITT			Adherence Adjusted ^b			ITT			Adherence Adjusted ^b		
	β (SE)	95% CI	P Value	β (SE)	95% CI	P Value	β (SE)	95% CI	P Value	β (SE)	95% CI	P Value
Regression coefficients												
Intervention	-.121 (.061)	-.241 to -.002	<.05	-.232 (.116)	-.460 to -.004	<.05	-.031 (.060)	-.148 to .085	.60	-.061 (.113)	-.283 to .161	.59
Male	-.403 (.069)	-.539 to -.267	<.001	-.400 (.068)	-.534 to -.266	.000	-.079 (.067)	-.211 to .053	.24	-.077 (.067)	-.207 to .054	.25
Intervention × male interaction	.265 (.086)	.097 to .433	.002	.527 (.170)	.193 to .861	.002	.210 (.082)	.050 to .371	.01	.427 (.163)	.107 to .746	.009
Calculated intervention effects												
Girls	-.121 (.061)	-.241 to -.002	<.05	-.232 (.116)	-.460 to -.004	<.05	-.031 (.060)	-.148 to .085	.60	-.061 (.113)	-.283 to .161	.59
Boys	.143 (.064)	.018 to .268	.03	.295 (.130)	.041 to .549	.02	.179 (.061)	.060 to .298	.003	.366 (.124)	.122 to .609	.003

Abbreviation: ITT, intention-to-treat models run with linear regression adjusted for survey design.

^aRegression models were adjusted for age, site, race (black vs Hispanic or non-Hispanic white), household head marital status (single vs not), currently working, education (certificate of General Equivalency Diploma and high school graduate vs less than high school), still in school, lived in neighborhood for 5 years or more, chats with neighbor at least once a week, no family in neighborhood, no friends in neighborhood, very likely to tell neighbor if he or she saw the neighbor's child getting into trouble, has applied for section 8 vouchers before, child is gifted, and child had problems with schoolwork or behavior plus male, intervention, and intervention × male interaction. Models were adjusted for family-level clustering, output with robust standard errors and were weighted. Item response theory methods were used to derive psychological distress and behavior problems. Calculated intervention effects were calculated from the regression coefficients from the interaction model.

^bAdherence-adjusted estimates are estimated using instrumental variable models adjusted for the survey design; second-stage estimates are reported herein.

ANALYTIC APPROACH

Primary analyses were based on ITT principles.³³ We regressed mental health outcomes on the intervention arm indicator variable using covariate-adjusted linear regression (for the K6 and the BPI) or logistic regression (for MDD). Approximately half of the families (51%) randomized to receive rental subsidy vouchers did not adhere to the assigned intervention (to use the voucher to move) and remained in public housing, so the ITT effect estimate is likely attenuated compared with the effect of using the voucher to move. When effect modification tests were statistically significant in ITT analyses, we also present adherence-adjusted effect estimates based on instrumental variable (IV) analysis, estimated using 2-stage least squares regression. The IV analysis is appropriate to correct for nonrandom adherence in randomized controlled trials because IV analysis avoids biases associated with directly comparing compliers with noncompliers.^{34,35}

Using ITT regression analysis, we first evaluated the intervention effect averaged over all the youth. Next, we assessed modification of the intervention effect on mental health by gender using a gender × intervention interaction (**Table 2**). We then assessed whether family health-related vulnerability modified the intervention effect by evaluating an intervention × vulnerability interaction. This interaction allowed us to formally test the primary novel hypothesis that the intervention effect on mental health was modified by family vulnerability. To assess the robustness of the findings to omitted higher-order interactions, a 3-way interaction involving the intervention, gender, and baseline health vulnerability variables was included but did not alter the results. For clarity, effect modification is displayed graphically as mean intervention effects on mental health (experimental – control differences on the absolute scale) and corresponding 95% CIs separately for each subgroup: girls from health-vulnerable families, girls from nonvulnerable fami-

lies, boys from vulnerable families, and boys from nonvulnerable families (**Figure 2**, and **Figure 3**). Negative values for intervention effect coefficients indicate beneficial intervention effects, and positive values indicate adverse effects. We exponentiated coefficients output from logistic regression into odds ratios (ORs) and corresponding 95% CIs; MDD was rare, so the OR estimates the risk ratio. We present the risk difference and its 95% CI for the intervention group vs controls based on marginal predicted probabilities output from logistic models. To confirm that the overall pattern of intervention effect heterogeneity is not merely driven by lower adherence rates in vulnerable families,^{22,23} we also estimated adherence-adjusted effects using IV analyses for distress and the BPI.

All the analyses were weighted to account for time-varying random assignment ratios and nonresponse and for household clustering (using the clustered sandwich estimator in STATA, version 11.0; StataCorp LP) since up to 2 children per household were randomly sampled at interim follow-up.¹⁰ We report robust standard errors with *P* values from 2-sided tests. We used Mplus 6.11 for item response theory analyses and STATA 11.0 for all other analyses.

RESULTS

Of the MTO adolescents, 50% were male, 63% were black, 30% were Hispanic, and 43% lived in families with baseline health vulnerabilities (Table 1). Youth lived in neighborhoods (tracts) that were 49.8% poor at baseline, and in 2002, mean neighborhood poverty was significantly lower for intervention youth vs controls (mean neighborhood poverty: low-poverty neighborhood group, 31.4%; regular Section 8 group, 32.2%; and controls, 39.2%; *F* test, *P* < .001).

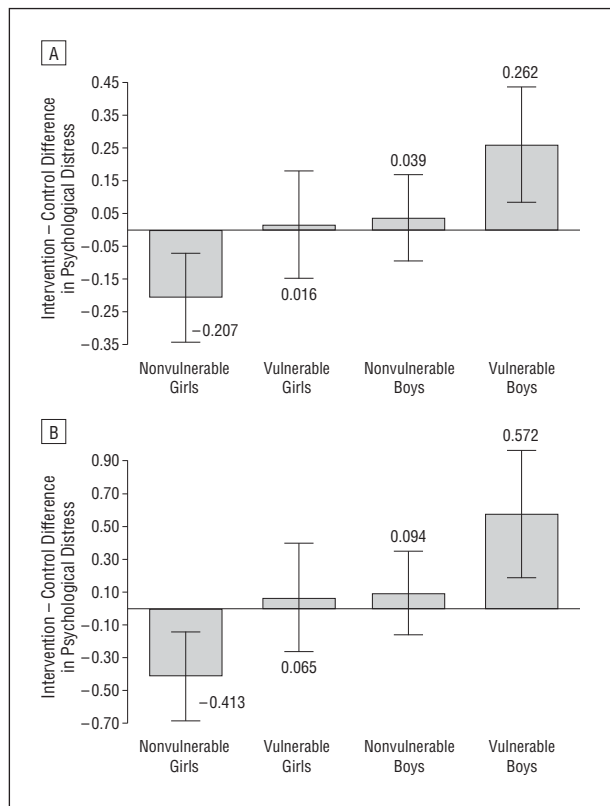


Figure 2. Moving to Opportunity trial intervention effects on mean difference in psychological distress 4 to 7 years after baseline modified by gender and health vulnerability. A, Linear regression intention-to-treat (ITT) estimates. B, Adherence-adjusted estimates of intervention effects from the second-stage instrumental variable (IV) analysis. The primary hypothesis tested whether the intervention \times health vulnerability interaction coefficient was significantly different from zero. Intervention \times health vulnerability interaction results for ITT were β (SE) = 0.223 (0.092); 95% CI, 0.042 to 0.404; P = .02 and for IV were β (SE) = 0.478 (0.192); 95% CI, 0.102 to 0.854; P = .01. Models were adjusted for the covariates listed in Table 2 plus the intervention \times baseline health vulnerability interaction. Subgroup sample size is 875 for nonvulnerable girls, 551 for vulnerable girls, 761 for nonvulnerable boys, and 642 for vulnerable boys. Error bars represent 95% CI for the mean.

The ITT estimates of the marginal effect of the MTO intervention on psychological distress were not significant (β = 0.012; 95% CI, -0.077 to 0.102) and on the BPI were marginally harmful (β = 0.075; 95% CI, -0.012 to 0.162). These mean effects masked qualitative effect modification by gender (intervention \times gender interaction for the K6: β = 0.265; 95% CI, 0.097 to 0.433; P = .002; for the BPI: β = 0.210; 95% CI, 0.050 to 0.371; P = .01). Randomization to the experimental intervention was significantly beneficial for girls' distress (β = -0.121; 95% CI, -0.241 to -0.002; P = .05), significantly harmful for boys' distress (β = 0.143; 95% CI, 0.018 to 0.268; P = .03), and significantly harmful for boys' BPI (β = 0.179; 95% CI, 0.060 to 0.298; P = .003). No statistically significant association was found relating the MTO intervention to girls' BPI (Table 2).

Family health-related vulnerability significantly adversely modified the effect of the MTO intervention on psychological distress in girls and boys in ITT models (intervention \times vulnerability interaction: β = 0.223; 95% CI, 0.042 to 0.404; P = .02) (Figure 2A). The modest average beneficial effect of intervention for girls reflected

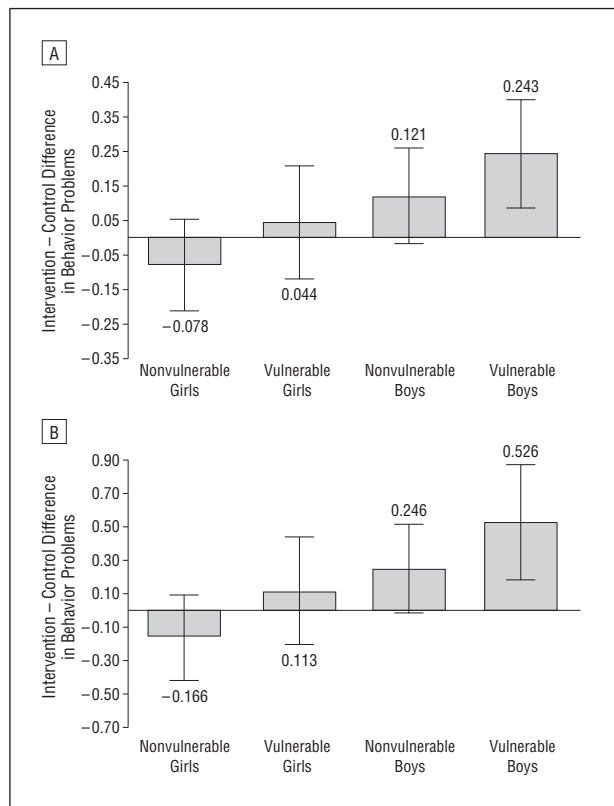


Figure 3. Moving to Opportunity trial intervention effects on mean difference in behavior problems 4 to 7 years after baseline modified by gender and health vulnerability. A, Linear regression intention-to-treat (ITT) estimates. B, Adherence-adjusted estimates of intervention effects from the second-stage instrumental variable (IV) analysis. The primary hypothesis test tested whether the intervention \times health vulnerability interaction coefficient was significantly different from zero. Intervention \times health vulnerability interaction results for ITT were β (SE) = 0.122 (0.090); 95% CI, -0.054 to 0.298; P = .17 and for IV were β (SE) = 0.279 (0.183); 95% CI, -0.079 to 0.638; P = .13. Models were adjusted for the covariates listed in Table 2 plus the intervention \times baseline health vulnerability interaction. Subgroup sample size is 875 for nonvulnerable girls, 551 for vulnerable girls, 761 for nonvulnerable boys, and 642 for vulnerable boys. Error bars represent 95% CI for the mean.

large benefits in girls from families without health-related vulnerabilities (β = -0.207; 95% CI, -0.342 to -0.071; P = .003) and small and nonsignificant effects in girls from families with health-related vulnerabilities (β = 0.016; 95% CI, -0.149 to 0.182). The overall harmful distress effect in boys reflected substantial harm in boys from health-vulnerable families (β = 0.262; 95% CI, 0.087 to 0.437; P = .003) and small and nonsignificant effects in boys not from health-vulnerable families (β = 0.039; 95% CI, -0.094 to 0.172). Baseline health vulnerability generated similar ITT effect modification patterns of the MTO on the BPI as for the K6, although the effect modification test results for the BPI were not significant (interaction β = 0.122; 95% CI, -0.054 to 0.298; P = .17) (Figure 3A).

Adherence-adjusted IV analysis results were similar to ITT analysis results for the K6 and the BPI, with a marked gradient across the 4 subgroups. The IV estimates for the K6 indicate significant benefits in girls from nonvulnerable families, harmful effects in boys from vulnerable families, and no significant effects for the other 2 groups. The IV estimates for the BPI indicate no sig-

nificant effects in girls and statistically significant adverse effects in boys, with larger adverse effects for boys from vulnerable families (Figures 2B and 3B). The IV effect estimates are about twice as large as ITT estimates.

For past-12-month MDD, patterns were similar but estimates were imprecise because of low prevalence in the experimental (2.7%) and control (3.4%) groups. The nonsignificant mean ITT effect (OR = 0.73; 95% CI, 0.43 to 1.23) masked a beneficial intervention effect for girls' MDD on relative scales (OR = 0.56; 95% CI, 0.31 to 1.01; $P = .05$) and absolute scales (risk difference = -0.024 ; 95% CI, -0.051 to 0.003 ; $P = .09$) and nonsignificant adverse intervention effects for boys. Effect modification by gender for 12-month MDD was not significant (interaction OR = 2.42; 95% CI, 0.63 to 9.35; $P = .20$) (eTable 1).

Examining patterns by family health-related vulnerability, nonvulnerable girls were the only subgroup to benefit for past-year MDD (OR = 0.42; 95% CI, 0.20 to 0.85; $P = .02$; risk difference = -0.037 ; 95% CI, -0.076 to 0.001 ; $P = .06$) (eFigure 1). The other 3 subgroups displayed no significant intervention effects. The health vulnerability \times intervention interaction was in the harmful direction but was not significant ($\beta = 0.810$; 95% CI, -0.275 to 1.89 ; $P = .14$).

Although the pooled models for the BPI show patterns comparable with those for distress, BPI analyses stratified by site revealed considerable heterogeneity for boys (Figure 4), although CIs are wide. New York seemed to drive the pooled BPI results, Chicago and Boston showed no differential adverse effect of vulnerability for boys, Los Angeles demonstrated strikingly protective effects of the intervention on the BPI that were marginally significant for nonvulnerable boys. The patterns for distress were more consistent across sites (eFigure 6).

COMMENT

We found heterogeneity in the mental health effects of moving to low-poverty neighborhoods through the MTO program. Receipt of Section 8 housing vouchers had important mental health benefits for adolescent girls from families without health vulnerabilities. However, the MTO program also apparently harmed the mental health of some adolescents, particularly boys from families with health vulnerabilities. The vulnerability hypothesis was supported: the policy was more harmful or less beneficial for adolescents from more vulnerable families with health problems at baseline. The results for past-year MDD in the MTO are presented for the first time herein; although imprecise given the low MDD prevalence in this young population, the estimates from this diagnostic outcome suggest patterns similar to those of dimensional outcomes.

Results pooled across sites were similar using internalizing and externalizing measures, which is informative given gender differences in mental health manifestation and expression.^{36,37} However, there was considerable variation across sites, particularly for boys' BPI. Although site differences are not uncommon with social experiments (eg, see the study by Hamilton et al³⁸), we

did not resolve what drove this heterogeneity. Relevant factors could include differential program implementation (eg, variation in housing counseling services) or differential housing market conditions.²⁷

Notably, the previously published results of the MTO experiment on interim survey psychological distress outcomes reported nonsignificant effects for boys aged 12 to 19 years.¹⁵ However, we found statistically significant adverse intervention effects for this group. We believe that the discrepancy is due to the improved operationalization of the K6 using item response theory latent variable methods to reduce measurement error.³³

Child development researchers posit different pathways through which neighborhood environment affects youth mental health³⁹ based on ecologic theories emphasizing the influence of multiple contexts fostering youth development.^{40,41} Hypothesized mechanisms include norms and social processes, relationships, and institutional resources. Living in disadvantaged contexts (ie, poverty) may be associated with greater emotional distress for adolescents owing to the higher presence of stressors and fewer resources to buffer stress effects.^{24,42,43} Developmental psychology and psychiatric epidemiology are increasingly incorporating social context, specifically factors at the neighborhood level, along with individual and family-level explanations for youth mental health problems.⁴⁴ Focusing on context locates the sources of illness external to the child, implicating the toxic environments that youth encounter, which often overwhelm the abilities of families to counteract the risk.⁴⁵

The mental health benefit of social contexts may relate not only to social advantage of specific contexts but also to the "fit" between individual and context.⁴⁶ Such fit may differ by gender. For example, adolescent girls are more likely than boys to witness and experience community sexual violence,^{47,48} and sexual victimization and witnessing community violence are associated with depression.^{47,48} Improved neighborhood safety from sexual violence may, therefore, contribute to the beneficial mental health intervention effects in girls. This is consistent with qualitative research in MTO participants showing that girls in high-poverty neighborhoods experienced pervasive sexual harassment and risk of sexual assault.⁴⁹ This gender dimension of safety may have been less relevant for boys, whereas the adjustment to new social networks in the new neighborhoods may have been more difficult for boys than for girls, for example owing to the disruption of friendships or adult role models.⁵⁰ Other processes, such as family structure, parental educational level, the child's education needs, and parental mental health, may also be important moderators or mediators of the MTO intervention based on previous evidence that these influence youth functioning.^{44,51} Although these explanations suggest directions for future work, we did not test them in this analysis, and, therefore, we do not know what accounts for these different intervention effects.

Certain child developmental stages may be sensitive periods⁵² during which exposure to certain neighborhood environments may imprint expression of later-life mental health, and these sensitive periods may differ by gender. Indeed, leading researchers have hypothesized

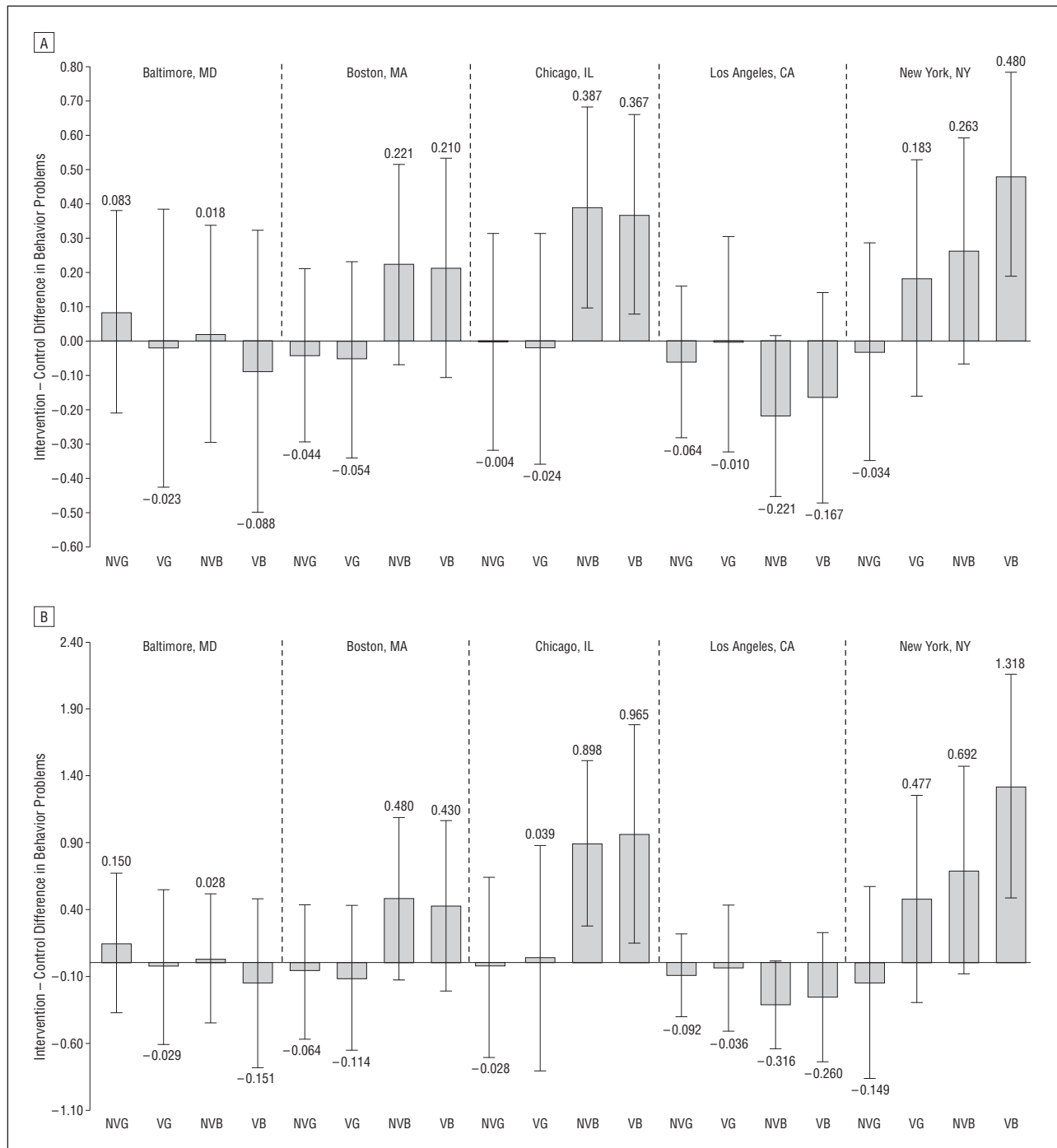


Figure 4. Moving to Opportunity trial intervention effects on mean difference in behavior problems 4 to 7 years after baseline modified by gender and health vulnerability, by site. A, Linear regression intention-to-treat estimates by site. B, Adherence-adjusted estimates of intervention effects from second-stage instrumental variable analysis by site. NVB indicates nonvulnerable boys; NVG, nonvulnerable girls; VB, vulnerable boys; and VG, vulnerable girls. The primary hypothesis tested whether the intervention \times health vulnerability interaction coefficient was significantly different from zero. Models were adjusted for the covariates listed in Table 2 plus the intervention \times baseline health vulnerability interaction. Subgroup sample sizes are as follows: nonvulnerable girls: 145 for Baltimore, 165 for Boston, 193 for Chicago, 193 for Los Angeles, and 179 for New York; vulnerable girls: 79 for Baltimore, 133 for Boston, 113 for Chicago, 56 for Los Angeles, and 170 for New York; nonvulnerable boys: 136 for Baltimore, 136 for Boston, 161 for Chicago, 191 for Los Angeles, and 137 for New York; and vulnerable boys: 88 for Baltimore, 141 for Boston, 134 for Chicago, 90 for Los Angeles, and 189 for New York. Error bars represent 95% CI for the mean.

that some experimental psychosocial interventions do not improve health because the intervention was delivered after the relevant etiologic period.⁵³ Therefore, future research should leverage the MTO and other longitudinal studies to test life course theories to inform future housing interventions.

Establishing that neighborhoods causally affect adolescent mental health is challenging because most such studies use observational, cross-sectional designs. Therefore, previous research may be biased owing to unmeasured confounding (by family characteristics such as socioeconomic status and mobility-related residential

selection), reverse causality, or violations of other causal inference assumptions.^{54,55} Experimental designs alleviate some of the most serious threats to internal validity by balancing confounders across experimental groups at baseline and establishing temporal sequencing between exposure and outcome.⁵⁰ The MTO is the only study measuring mental health to date that randomly assigned individuals to receive different neighborhood contexts via offers to move using housing vouchers; this experimental design is strong for assessing whether moves from public to private housing may cause mental health problems, especially since the MTO reduced exposure to neighborhood poverty.

Even then, experiments are not a panacea for understanding neighborhood effects.⁵⁷⁻⁵⁹ The MTO experiment resulted in a variety of bundled “treatments” that changed for families, including not only the housing voucher but also the move from public housing to private rental housing, the move from very poor to lower-poverty neighborhoods, and housing unit quality improvement.¹⁵ Analyses comparing the importance of these alternative mechanisms are potentially valuable for theoretical understanding and the design of future interventions. Future housing experiments would benefit from conceptualizing how to separate these relevant processes analytically or through study design to inform the most important mediators of the (bundled) intervention effect.

Policy experts recommend 2 strategies for improving neighborhood environments for individuals: people-based interventions (such as housing mobility vouchers), which help households acquire housing in better neighborhoods, and place-based interventions, which improve conditions in disadvantaged neighborhoods.^{5,6,60} The MTO used a people-based housing mobility intervention, modeled on Section 8 vouchers (now called Housing Choice Vouchers); these vouchers constitute the largest US federal affordable housing program,⁶¹ accounting for approximately 40% of US Department of Housing and Urban Development’s recent annual budgets.⁶² The MTO did not focus on place-based improvement of disadvantaged neighborhoods, so we cannot infer that interventions such as neighborhood improvement initiatives would achieve similar effects as those in the MTO. However, the MTO is policy relevant given its mechanism of vouchers and, therefore, broadens the scope of potential interventions that may improve access to better neighborhoods.⁵

The demographic group apparently harmed by the MTO intervention (low-income, predominantly racial/ethnic minority, adolescent boys) is among the most vulnerable populations in the United States, with poor long-term health outcomes.⁶³ These findings, therefore, merit special attention to understand why these adolescents fared worse in the intervention group, whether this heterogeneity extends to other subgroups, and how clinical, social, or policy interventions can remediate this harm. Families receiving federal rental assistance are typically in extreme need, so reducing Housing Choice Voucher housing support is not an appropriate response.

The MTO was designed in the housing sector, without awareness that it would affect health or that its ef-

fects would be modified by baseline health or gender. The present findings suggest that additional supportive services may be required to help vulnerable adolescents succeed in the context of residential moves, even when moving to seemingly better neighborhood environments. Children from health-vulnerable families may not be able to take advantage of the potential opportunities afforded by moves outside of public housing. These relationships between health and housing strongly suggest the need for more intersectoral collaboration, including, for example, incorporating the type of resident supportive services required under the HOPE VI (Housing Opportunities for People Everywhere) housing relocation program, including case management and linkage to health care access or educational services tailored to residents’ needs.^{64,65} Housing mobility programs may also benefit from integrating services with medical-legal partnerships, which serve the needs of low-income households in health care settings by combining medical care with other unmet service needs (eg, legal counseling, housing, and income support)⁶⁶; such a multipronged approach is promising for addressing prevention and treatment of mental health and housing problems. Moreover, future housing programs may benefit from measuring health, mental health, and symptoms at baseline and follow-up to identify and target health-vulnerable populations that may benefit from additional services.

The present results suggest that factors shaping mental health may differ for different groups since the MTO’s mental health effects differed by gender and baseline health. Potential mental health benefits of residential moves may be counteracted by the difficulties faced by adolescents in families with health vulnerabilities. Existing health or developmental problems may be more important chronic stressors for causing mental illness than neighborhood environment. However, in adolescents from families without these vulnerabilities, the relative importance of neighborhood context as a cause of mental illness may emerge.⁶⁷

This study has several limitations. The low prevalence of past-year MDD limited the power to test intervention effects on the disorder, although patterns were similar to those for the K6 and the BPI. Assessing the effects of context using dimensional measures of symptoms is appropriate for population or community assessment if the effects of social context are nonspecific.^{68,69}

This study population comprised very-low income minority adolescents living in extremely distressed high-poverty neighborhoods. The present findings, therefore, may not be generalizable to other populations. However, this population is a high priority for targeting health care services and improvements in population health because over their life course they experience high morbidity in many domains.^{64,70}

Although the 51% intervention adherence in this study could attenuate the ITT effect estimates compared with the actual effect of moving, we used IV analysis to model adherence-adjusted estimates. The IV analysis results mirrored the ITT analysis results but were twice as large, suggesting that differential take-up to use the Section 8 policy did not explain the effect modification patterns. The IV

interpretations rest on several assumptions, including that intervention assignment can affect mental health only indirectly, mediated by use of the voucher to move.⁷¹ This assumption is more credible in randomized trials, such as the MTO, than in quasi-experimental studies but cannot be confirmed empirically. The Stable Unit Treatment Variable Assumption is another assumption that generally holds when programs (such as the MTO) are small scale relative to the community.⁷¹ With additional assumptions, IV effect estimates are generally interpreted as referring to specific subpopulations, for example, those who used a voucher to move.⁷¹

Compliance was defined by the original investigators as using MTO Section 8 vouchers to move to private rental apartments. By design, control group members could not access MTO vouchers, so they could not take up the intervention.¹⁰ However, redefining take-up as moving away from public housing would reduce estimated adherence because some control group members moved without MTO vouchers.

In conclusion, this housing policy experiment benefited the mental health of some adolescents, particularly girls in families without health vulnerabilities, but had either nonsignificant or harmful effects on the mental health of adolescents from families with preexisting health-related vulnerabilities, particularly boys. The questions raised in this analysis should be explored in the 10- to 15-year MTO follow-up. The implications of the findings may guide future housing policy insofar as children from health-vulnerable families may require additional support or services during and after moves. These findings may, moreover, help improve our understanding of the etiology of adolescent mental illness. Clinically, it is important to understand which adolescents are at special risk for poor mental health outcomes in the context of residential moves.

Published Online: October 8, 2012. doi:10.1001/archgenpsychiatry.2012.449

Submitted for Publication: February 24, 2012; accepted March 26, 2012.

Author Affiliations: Department of Health Sciences (Drs Osypuk and Lincoln) and Institute for Urban Health Research (Drs Lincoln and Schmidt), Bouvé College of Health Sciences, and Department of Sociology, Northeastern University, Boston, Massachusetts (Dr Lincoln); Departments of Biostatistics and Epidemiology (Dr Tchetgen Tchetgen) and Society, Human Development, and Health (Drs Earls and Glymour), Harvard School of Public Health, Boston; Department of Global Health and Social Medicine, Harvard Medical School, Boston (Dr Earls); and the Heller School for Social Policy and Management, Institute for Children, Youth, and Family Policy, Brandeis University, Waltham, Massachusetts (Dr Acevedo-Garcia).

Correspondence: Theresa L. Osypuk, ScD, ScM, Department of Health Sciences, Bouvé College of Health Sciences, Northeastern University, 360 Huntington Ave, Robinson 316, Boston, MA 02115 (tosypuk@neu.edu).
Financial Disclosure: None reported.

Funding/Support: The authors and this work were supported by grants 1R01MD006064-01 and

1R21HD066312-01 from the National Institutes of Health (Dr Osypuk, principal investigator).

Previous Presentations: Preliminary findings in this manuscript were presented at the Third North American Congress of Epidemiology; June 21, 2011; Montreal, Quebec, Canada; and at the Population Association of America annual meeting; April 2, 2011; New Orleans, Louisiana.

Online-Only Material: The eAppendix, eTable, and eFigures are available at <http://www.archgenpsychiatry.com>.

Additional Contributions: Sandro Galea, MD, DrPh, MPH, provided helpful comments on a previous version of the manuscript.

REFERENCES

1. Kessler RC, Foster CL, Saunders WB, Stang PE. Social consequences of psychiatric disorders, I: educational attainment. *Am J Psychiatry*. 1995;152(7):1026-1032.
2. Kessler RC, Berglund P, Demler O, Jin R, Merikangas KR, Walters EE. Lifetime prevalence and age-of-onset distributions of DSM-IV disorders in the National Comorbidity Survey Replication. *Arch Gen Psychiatry*. 2005;62(6):593-602.
3. Brodbeck J, Vilén UL, Bachmann M, Znoj H, Alsaker FD. Sexual risk behavior in emerging adults: gender-specific effects of hedonism, psychosocial distress, and sociocognitive variables in a 5-year longitudinal study. *AIDS Educ Prev*. 2010;22(2):148-159.
4. Kaplan HB, Dampousse KR. Reciprocal relationships between life events and psychological distress. *Stress Health*. 1997;13(2):75-90.
5. Osypuk TL, Acevedo-Garcia D. Beyond individual neighborhoods: a geography of opportunity perspective for understanding racial/ethnic health disparities. *Health Place*. 2010;16(6):1113-1123.
6. Acevedo-Garcia D, Osypuk TL, McArdle N, Williams DR. Toward a policy-relevant analysis of geographic and racial/ethnic disparities in child health. *Health Aff (Millwood)*. 2008;27(2):321-333.
7. Leventhal T, Dupéré V, Gunn JB. Neighborhood influences on adolescent development. In: Lerner RM, Steinberg L, eds. *Handbook of Adolescent Psychology, Volume 2, Contextual Influences on Adolescent Development*. 3rd ed. Hoboken, NJ: John Wiley & Sons; 2009:411-443.
8. Aneshensel CS, Sucoff CA. The neighborhood context of adolescent mental health. *J Health Soc Behav*. 1996;37(4):293-310.
9. Mair C, Diez Roux AV, Galea S. Are neighbourhood characteristics associated with depressive symptoms? a review of evidence. *J Epidemiol Community Health*. 2008;62(11):940-946.
10. Kling JR, Liebman JB, Katz LF. Experimental analysis of neighborhood effects. *Econometrica*. 2007;75(1):83-119.
11. Leventhal T, Brooks-Gunn J. Moving to Opportunity: an experimental study of neighborhood effects on mental health. *Am J Public Health*. 2003;93(9):1576-1582.
12. Schoeni RF, House JS, Kaplan GA, Pollack H. *Making Americans Healthier: Social and Economic Policy as Health Policy*. New York, NY: Russell Sage; 2008.
13. Herd P, Schoeni RF, House JS. Upstream solutions: does the supplemental security income program reduce disability in the elderly? *Milbank Q*. 2008;86(1):5-45.
14. Strully KW, Rehkopf DH, Xuan Z. Effects of prenatal poverty on infant health: state-earned income tax credits and birth weight. *Am Sociol Rev*. 2010;75(4):534-562.
15. Orr L, Feins JD, Jacob R, Beecroft E, Sanbonmatsu L, Katz LF, Liebman JB, Kling JR. *Moving to Opportunity for Fair Housing Demonstration Program: Interim Impacts Evaluation*. Washington, DC: US Dept of Housing and Urban Development; 2003.
16. Fortson JG, Sanbonmatsu L. Child health and neighborhood conditions: results from a randomized housing voucher experiment. *J Hum Resour*. 2010;45(4):840-864.
17. Ludwig J, Sanbonmatsu L, Gennetian L, Adam E, Duncan GJ, Katz LF, Kessler RC, Kling JR, Lindau ST, Whitaker RC, McDade TW. Neighborhoods, obesity, and diabetes: a randomized social experiment. *N Engl J Med*. 2011;365(16):1509-1519.
18. Acevedo-Garcia D, Osypuk TL, Werbel RE, Meara ER, Cutler DM, Berkman LF. Does housing mobility policy improve health? *Housing Policy Debate*. 2004;15(1):49-98.

19. Ertel KA, Glymour MM, Glass TA, Berkman LF. Frailty modifies effectiveness of psychosocial intervention in recovery from stroke. *Clin Rehabil*. 2007;21(6): 511-522.
20. McCormick MC, Brooks-Gunn J, Buka SL, Goldman J, Yu J, Salganik M, Scott DT, Bennett FC, Kay LL, Bernbaum JC, Bauer CR, Martin C, Woods ER, Martin A, Casey PH. Early intervention in low birth weight premature infants: results at 18 years of age for the Infant Health and Development Program. *Pediatrics*. 2006;117(3): 771-780.
21. McCormick MC, McCarton C, Brooks-Gunn J, Belt P, Gross RT. The Infant Health and Development Program: interim summary. *J Dev Behav Pediatr*. 1998;19 (5):359-370.
22. Snell EK, Duncan GJ. Child characteristics and successful use of housing vouchers: estimates from the Moving to Opportunity demonstration. *Housing Policy Debate*. 2006;17(4):727-754.
23. Shroder M. Locational constraint, housing counseling, and successful lease-up in a randomized housing voucher experiment. *J Urban Econ*. 2002;51:315-338.
24. Pearlin LI. The sociological study of stress. *J Health Soc Behav*. 1989;30(3):241-256.
25. US Department of Housing and Urban Development. *Expanding Housing Choices for HUD-Assisted Families: Moving to Opportunity for Fair Housing Demonstration Program*. Washington, DC: US Dept of Housing and Urban Development; April 1996.
26. Feins JD, McInnis D. *The Interim Impact Evaluation for the Moving to Opportunity Demonstration, C-OPC-21484*. Cambridge, MA: Abt Associates Inc; 2001.
27. Goering J, Kraft J, Feins J, McInnis D, Holin MJ, Elhassan H. *Moving to Opportunity for Fair Housing Demonstration Program: Current Status and Initial Findings*. Washington, DC: Office of Policy Development and Research, US Dept of Housing and Urban Development; 1999.
28. Kessler RC, Andrews G, Colpe LJ, Hiripi E, Mroczek DK, Normand SL, Walters EE, Zaslavsky AM. Short screening scales to monitor population prevalences and trends in non-specific psychological distress. *Psychol Med*. 2002;32(6):959-976.
29. Zill N. *Behavior Problems Index Based on Parent Report*. Washington, DC: Child Trends; 1990.
30. Kessler RC, Avenevoli S, Green JG, Gruber MJ, Guyer M, He Y, Jin R, Kaufman J, Sampson NA, Zaslavsky AM. National Comorbidity Survey Replication Adolescent Supplement (NCS-A), III: concordance of DSM-IV/CIDI diagnoses with clinical reassessments. *J Am Acad Child Adolesc Psychiatry*. 2009;48(4):386-399.
31. Kramer M, von Korff M, Kessler L. The lifetime prevalence of mental disorders: estimation, uses and limitations. *Psychol Med*. 1980;10(3):429-435.
32. Tsiatis AA, Davidian M, Zhang M, Lu X. Covariate adjustment for two-sample treatment comparisons in randomized clinical trials: a principled yet flexible approach. *Stat Med*. 2008;27(23):4658-4677.
33. Kleinbaum DG, Sullivan KM, Barker ND. *A Pocket Guide to Epidemiology*. New York, NY: Springer; 2007.
34. Newhouse JP, McClellan M. Econometrics in outcomes research: the use of instrumental variables. *Annu Rev Public Health*. 1998;19:17-34.
35. Angrist JD, Imbens GW, Rubin DB. Identification of causal effects using instrumental variables. *J Am Stat Assoc*. 1996;91(434):444-455.
36. Rosenfield S. Gender and mental health: do women have more psychopathology, men more, or both the same (and why)? In: Horwitz AV, Scheid TL, eds. *A Handbook for the Study of Mental Health: Social Contexts, Theories, and Systems*. Cambridge, UK: Cambridge University Press; 1999:348-360.
37. Schwartz S, Meyer IH. Mental health disparities research: the impact of within- and between-group analyses on tests of social stress hypotheses. *Soc Sci Med*. 2010;70(8):1111-1118.
38. Hamilton G, Freedman S, Gennetian L, Michalopoulos C, Walter J, Adams-Ciardullo D, Gassman-Pines A, McGroder S, Zaslow M, Ahluwalia S, Brooks J, Small E, Ricchetti B. *How Effective Are Different Welfare-to-Work Approaches? Five-Year Adult and Child Impacts for Eleven Programs*. New York, NY: Manpower Demonstration Research Corp and Child Trends; 2001.
39. Leventhal T, Brooks-Gunn J. The neighborhoods they live in: the effects of neighborhood residence on child and adolescent outcomes. *Psychol Bull*. 2000; 126(2):309-337.
40. Bronfenbrenner U. Reality and research in the ecology of human development. *Proc Am Philos Soc*. 1975;119(6):439-469.
41. Boyce WT, Frank E, Jensen PS, Kessler RC, Nelson CA, Steinberg L, MacArthur Foundation Research Network on Psychopathology and Development. Social context in developmental psychopathology: recommendations for future research from the MacArthur Network on Psychopathology and Development. *Dev Psychopathol*. 1998;10(2):143-164.
42. Aneshensel CS. Social stress: theory and research. *Annu Rev Sociol*. 1992;18: 15-38.
43. Brooks-Gunn J, Duncan GJ, Aber JL. *Neighborhood Poverty: Context and Consequences for Children*. Vol I. New York, NY: Russell Sage; 1997.
44. Merikangas KR, Nakamura EF, Kessler RC. Epidemiology of mental disorders in children and adolescents. *Dialogues Clin Neurosci*. 2009;11(1):7-20.
45. Roosa MW, Jones S, Tein J-Y, Cree W. Prevention science and neighborhood influences on low-income children's development: theoretical and methodological issues. *Am J Community Psychol*. 2003;31(1-2):55-72.
46. Veiling W, Susser E, van Os J, Mackenbach JP, Selten J-P, Hoek HW. Ethnic density of neighborhoods and incidence of psychotic disorders among immigrants. *Am J Psychiatry*. 2008;165(1):66-73.
47. Foster JD, Kuperminc GP, Price AW. Gender differences in posttraumatic stress and related symptoms among inner-city minority youth exposed to community violence. *J Youth Adolesc*. 2004;33(1):59-69.
48. Singer MI, Anglin TM, Song LY, Lunghofer L. Adolescents' exposure to violence and associated symptoms of psychological trauma. *JAMA*. 1995;273(6):477-482.
49. Popkin SJ, Leventhal T, Weismann G. *Girls in the Hood: The Importance of Feeling Safe*. Washington, DC: The Urban Institute; 2008.
50. Caldwell CH, Rafferty J, Reischl TM, De Loney EH, Brooks CL. Enhancing parenting skills among nonresident African American fathers as a strategy for preventing youth risky behaviors. *Am J Community Psychol*. 2010;45(1-2):17-35.
51. Chapman MV. Neighborhood quality and somatic complaints among American youth. *J Adolesc Health*. 2005;36(3):244-252.
52. Kuh D, Ben-Shlomo Y, Lynch J, Hallqvist J, Power C. Life course epidemiology. *J Epidemiol Community Health*. 2003;57(10):778-783.
53. Berkman LF. Social epidemiology: social determinants of health in the United States: are we losing ground? *Annu Rev Public Health*. 2009;30(1):27-41.
54. Oakes JM. The (mis)estimation of neighborhood effects: causal inference for a practicable social epidemiology. *Soc Sci Med*. 2004;58(10):1929-1952.
55. Sobel ME. What do randomized studies of housing mobility demonstrate? causal inference in the face of interference. *J Am Stat Assoc*. 2006;101(476):1398-1407.
56. Pebley AR, Sastry N. *Neighborhoods, Poverty and Children's Well-being: A Review*. Los Angeles: California Center for Population Research, UCLA; 2003. Report CCPR-035-04.
57. Ludwig J, Liebman JB, Kling JR, Duncan GJ, Katz LF, Kessler RC, Sanbonmatsu L. What can we learn about neighborhood effects from the Moving to Opportunity experiment? a comment on Clampet-Lundquist and Massey. *Am J Sociol*. 2008;114(1):144-188.
58. Clampet-Lundquist S, Massey DS. Neighborhood effects on economic self-sufficiency: a reconsideration of the Moving to Opportunity experiment. *Am J Sociol*. 2008;114(1):107-143.
59. Sampson RJ. Moving to inequality: neighborhood effects and experiments meet social structure. *Am J Sociol*. 2008;114(1):189-231.
60. Katz B. *Neighborhoods of Choice and Connection: The Evolution of American Neighbourhood Policy and What It Means for the United Kingdom*. York, UK: Joseph Rowntree Foundation; 2004.
61. Kruckenberg K; Poverty & Race Research Action Council. Two simple changes to improve health outcomes in the Section 8 voucher program. Published April 2011. <http://www.pprac.org/pdf/HQS-SEMAPhealthpolicybriefApril2011.pdf>. Accessed October 26, 2011.
62. National Low Income Housing Coalition. Federal budget & approps: HUD budget charts, FY11 and FY12 budget chart for selected HUD programs. Updated April 27, 2011. <https://www2398.sslldomain.com/nlihc/template/page.cfm?id=28>. Accessed April 29, 2011.
63. Arias E. *United States Life Tables, 2006*. Hyattsville, MD: National Center for Health Statistics; 2010.
64. Popkin SJ, Levy D, Harris LE, Comey J, Cunningham MK, Buron L. *HOPE VI Panel Study: Baseline Report*. Washington, DC: The Urban Institute; 2002.
65. Engdahl L. *New Homes, New Neighborhoods, New Schools: A Progress Report on the Baltimore Housing Mobility Program*. Baltimore, MD: Baltimore Regional Housing Campaign and Poverty & Race Research Action Council; 2009.
66. Cohen E, Fullerton DF, Retkin R, Weintraub D, Tames P, Brandfield J, Sandel M. Medical-legal partnership: collaborating with lawyers to identify and address health disparities. *J Gen Intern Med*. 2010;25(suppl 2):S136-S139.
67. VanderWeele TJ, Hernández-Díaz S. Is there a direct effect of pre-eclampsia on cerebral palsy not through preterm birth? *Paediatr Perinat Epidemiol*. 2011; 25(2):111-115.
68. Aneshensel CS. Research in mental health: social etiology versus social consequences. *J Health Soc Behav*. 2005;46(3):221-228.
69. McMahon SD, Grant KE, Compas BE, Thurm AE, Ey S. Stress and psychopathology in children and adolescents: is there evidence of specificity? *J Child Psychol Psychiatry*. 2003;44(1):107-133.
70. Manjarrez CA, Popkin SJ, Guernsey E. *Poor Health: Adding Insult to Injury for HOPE VI Families*. Washington, DC: The Urban Institute; 2007.
71. Gennetian LA, Bos JM, Morris PA. *Using Instrumental Variables Analysis to Learn More From Social Policy Experiments*. New York, NY: Manpower Demonstration Research Corp; 2002.