Gender and Crime Victimization Modify Neighborhood Effects on Adolescent Mental Health

WHAT'S KNOWN ON THIS SUBJECT: Adolescents living in lower-poverty neighborhoods have better mental health than youth in high-poverty contexts, but it is unclear if associations are causal. Furthermore, it is unknown why some youth benefit more than others from moving to more advantaged neighborhoods.

WHAT THIS STUDY ADDS: Using an experimental study that randomly assigned families to receive vouchers to move to lower-poverty neighborhoods, we found that recent violent crime victimization adversely modified the mental health effects of moving to better neighborhoods.

AUTHORS: Theresa L. Osypuk, SD,[a] Nicole M. Schmidt, PhD,[b] Lisa M. Bates, SD,[c] Eric J. Tchetgen-Tchetgen, PhD,[d,e] Felton J. Earls, MD,[f,g] and M. Maria Glymour, SD[g]

[a]Department of Health Sciences and [b]Institute on Urban Health Research, Bouvé College of Health Sciences, Northeastern University, Boston, Massachusetts; [c]Department of Epidemiology, Mailman School of Public Health, Columbia University, New York, New York; and Departments of [d]Biostatistics, [e]Epidemiology, and [g]Society, Human Development, and Health, Harvard School of Public Health, and [f]Department of Global Health and Social Medicine, Harvard Medical School, Harvard University, Cambridge, Massachusetts

KEY WORDS

mental health, depression, adolescent behavior, randomized controlled trial, housing, public housing, adolescent, victimization, urban health

ABBREVIATIONS

BPI—Behavior Problems Index
CI—confidence interval
IRT—item response theory
ITT—intent-to-treat
IV—instrumental variable
MDD—major depressive disorder
MTO—Moving to Opportunity

abstract

OBJECTIVE: Leverage an experimental study to determine whether gender or recent crime victimization modify the mental health effects of moving to low-poverty neighborhoods.

METHODS: The Moving to Opportunity (MTO) study randomized low-income families in public housing to an intervention arm receiving vouchers to subsidize rental housing in lower-poverty neighborhoods or to controls receiving no voucher. We examined 3 outcomes 4 to 7 years after randomization, among youth aged 5 to 16 years at baseline (n = 2829): lifetime major depressive disorder (MDD), psychological distress (K6), and Behavior Problems Index (BPI). Treatment effect modification by gender and family's baseline report of recent violent crime victimization was tested via interactions in covariate-adjusted intent-to-treat and instrumental variable adherence-adjusted regression models.

RESULTS: Gender and crime victimization significantly modified treatment effects on distress and BPI ($P < .10$). Female adolescents in families without crime victimization benefited from MTO treatment, for all outcomes (Distress B = –0.19, $P = .008$; BPI B = –0.13, $P = .06$; MDD B = –0.036, $P = .03$). Male adolescents in intervention families experiencing crime victimization had worse distress (B = 0.24, $P = .004$), more behavior problems (B = 0.30, $P < .001$), and nonsignificantly higher MDD (B = 0.022, $P = .16$) versus controls. Other subgroups experienced no effect of MTO treatment. Instrumental variable estimates were similar but larger.

CONCLUSIONS: Girls from families experiencing recent violent crime victimization were significantly less likely to achieve mental health benefits, and boys were harmed, by MTO, suggesting need for cross-sectoral program supports to offset multiple stressors. *Pediatrics* 2012;130:472–481

Dr Osypuk had full access to all the data in the analysis and takes responsibility for the integrity of the data and the accuracy of the data analysis. Dr Osypuk conceived the hypotheses, obtained the data, conducted the majority of the data analysis, and wrote the majority of the manuscript. Dr Glymour aided in writing the article. Drs Glymour and Tchetgen-Tchetgen advised on the statistical analysis and interpretation of findings, in addition to writing and editing considerable portions of the Methods. Dr Schmidt conducted the item response theory analyses, aided with the literature review, and edited the manuscript. Drs Bates and Earls aided in the interpretation of findings and edited and helped to structure the manuscript. All authors are responsible for reported research, and all authors have participated in the concept and design, analysis and interpretation of data, and drafting or revising of the manuscript; all authors have approved the manuscript as submitted.

The National Institutes of Health had no role in the analysis or the preparation of this manuscript. The funders did not have any role in the conduct of the study or in the preparation, review, or approval of the manuscript. The US Department of Housing and Urban Development reviewed the manuscript to ensure respondent confidentiality was maintained in the presentation of results.

(Continued on last page)

Adolescents living in disadvantaged contexts, including neighborhood poverty, exhibit elevated emotional distress.[1,2] This association is hypothesized to be due to the higher prevalence of stressors and fewer stress-buffering resources in low-income neighborhoods.[1] However, the majority of neighborhood mental health studies are observational and therefore may be biased by unmeasured confounding.[3,4] Experimental designs alleviate these serious internal validity threats.[5] Moving to Opportunity (MTO) is the only study to date that has randomly assigned individuals to move to different neighborhood contexts, offering a robust test of whether changes in neighborhood and housing conditions cause mental health.

Not all youth respond uniformly to similar contexts.[6] For example, previous analyses of MTO showed mental health benefits for girls but either nonsignificant or null effects for boys.[7–9] These findings highlight the potential for subgroups to experience substantially divergent outcomes as a result of the same intervention. One possible source of heterogeneity is youth vulnerability. Many interventions provide the greatest benefit to individuals who are already relatively advantaged, with respect to health or social conditions. Children under stress before an intervention may not benefit to the same extent as more advantaged children; fully capitalizing on an intervention may require resources depleted in highly stressed or vulnerable youth.[10,11] Highly stressed or traumatized children may have difficulty transitioning to new communities (eg, feeling alienated, unable to establish social networks),[12–14] so relocating may be a more important stressor for these children.[15–17]

One of the most powerful and common sources of stress for youth in low-income neighborhoods is exposure to violence,[18,19] which is associated with poorer mental health,[20] school dropout, teen pregnancy, criminal involvement,[21] and fraternization with substance-using peers.[22] This suggests that youth exposed to violent victimization may be less able to benefit from moving out of disadvantaged neighborhoods because they are already at higher risk for poor outcomes, and the stress of violence exposure may be compounded by the stress of assimilating into a new neighborhood and social network.[12,13,15,17]

We hypothesize that the mental health of youth from families who have experienced recent violent crime victimization will not benefit from, and may even be harmed by, moving out of high-poverty, violent neighborhoods into better neighborhoods through a housing mobility intervention, unlike their counterparts from nonvictimized families. We aim in this study to (1) confirm previous differential gender effects of MTO by using improved measures of mental health outcomes and (2) test our novel hypothesis of effect modification by familial exposure to violent crime.

METHODS

The MTO for Fair Housing Demonstration Project was a randomized controlled trial sponsored by the US Department of Housing and Urban Development[23] in 5 sites (1994–1998): Boston, Baltimore, Chicago, Los Angeles, and New York. Eligible low-income families had children aged <18, qualified for rental assistance, and lived in public housing or project-based assisted housing in areas with high concentrations of poverty.[24] Applicants were drawn from waiting lists, signed enrollment agreements and informed consent, completed the Baseline Survey, and were evaluated for eligibility[25] by public housing authorities. Five thousand three hundred one families volunteered, and 4610 families were eligible and randomized[8] (Fig 1). MTO was not registered in Consolidated Standards of Reporting Trials (CONSORT) because it was not a medical intervention.

Treatment Assignment

Special software randomly assigned MTO families to 1 of 3 conditions. The "regular Section 8" treatment group was offered Section 8 housing vouchers to subsidize a private market rental apartment in any neighborhood. The "low-poverty-neighborhood" treatment group was also offered Section 8 housing vouchers, but they were redeemable only in low-poverty neighborhoods (where <10% of Census Tract households were impoverished). Families in this group were offered housing counseling services to aid relocation. The third group was an untreated control group, who received no additional assistance but could remain in public housing.[25]

Assessment

Surveys at baseline (1994–1998) and the interim follow-up (2001–2002) were conducted in person via computer-assisted personal interviewing technology with household heads and their children.[8,25] Youth were interviewed in teen centers to improve privacy.[24] We focus on adolescents (n = 3537 aged 12–19 as of May 31, 2001) randomized through December 31, 1997 in the MTO Tier 1 Restricted Access Data; the effective response was 89.3%.[8] Adults provided informed written consent for themselves and their children.[8,24,25] Northeastern University's Institutional Review Board approved this study.

Measures

Past-month nonspecific psychological distress was measured by the K6 scale,[26] with 5-item Likert frequency response options for 6 items: depressed; nervous; restless or fidgety; hopeless; everything was an effort; worthless. We scored the K6 with 2-parameter binary item response theory (IRT) methods to obtain

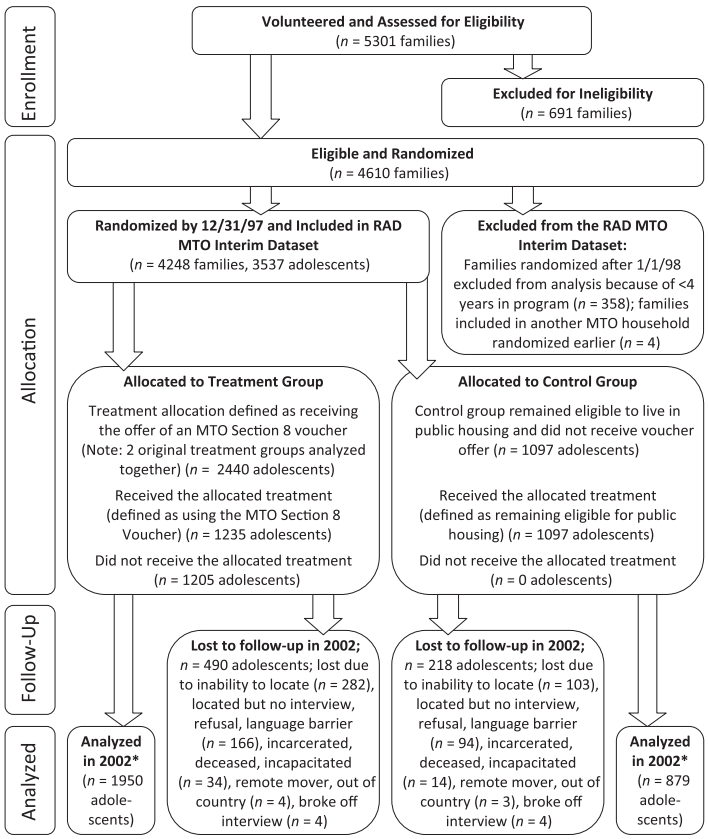

FIGURE 1

MTO youth enrollment, treatment allocation, and attrition. * 2002 Interim Survey yielded 89% effective response rate (RR) with a 2-stage follow-up sampling strategy, calculated as RR = MRR + SRR * (1 − MRR), where MRR = response rate for main sample (respondents initially responding to 2002 survey interview request) and SRR = response rate for subsample (a second attempt to find every 3 in 10 hard-to-reach families initially nonresponsive in 2002[8]; see p. A-8 of MTO Interim Evaluation [Orr et al, 2003].[8]).

distress factor scores with a standard normal distribution.[26] (Cronbach α = .80, mean [SD] = –0.0395 [1.123]). (Each unit change in the IRT-scaled K6 corresponds to approximately an SD; regression coefficients are interpreted approximately as proportions of an SD in effect size). IRT scoring is recommended because items with stronger relationships to the underlying distress construct are weighted heavier, increasing reliability and precision over simple summed scores.[26,27]

Behavior problems were measured by 11 self-reported items adapted from the Behavior Problems Index (BPI)[28] assessing primarily externalizing behaviors. Responses for items such as "I lie or cheat" and "I have a hot temper" range from 0 (not true) to 2 (often true). We used 2-parameter binary IRT methods to obtain BPI factor scores (α = .80, mean [SD] = −0.0250 [1.086]).

Lifetime *Diagnostic and Statistical Manual of Mental Disorders, Fourth Edition* major depressive disorder (MDD) was adapted from the National Comorbidity Survey Replication–Adolescent Supplement, implemented by trained lay interviewers. This measure displays good concordance with *Diagnostic and Statistical Manual* diagnoses[29]; the algorithm to derive lifetime MDD is in Supplemental Information 1 (mean [SD] = 0.0458 [0.2538]). Although temporal ordering of treatment and lifetime MDD is not established in these analyses, given that this sample was young at baseline (before typical age of MDD onset[30]) and randomized to treatment, we feel confident that group differences in 2002 MDD can be attributed to incident differences since baseline.

Randomly assigned treatment of an offer of a housing voucher (versus not) was indicated with 1 binary variable: treatment versus control group. Although MTO contained 2 experimental treatment groups, effects on mental health were similar for both groups (versus controls), and formal statistical tests for each outcome could not reject ($P < .05$) effect homogeneity. We therefore combined experimental groups to improve statistical power. Results retaining the original 3 treatment groups are presented in Supplemental Figs 3–5. (Mean neighborhood poverty immediately after relocation for low-poverty and Section 8 treatment groups were 7.9% and 27.1%, respectively, compared with baseline control mean of 50.5%, indicating that experimental movers did move to substantially lower-poverty neighborhoods; 1990 census data).

Treatment adherence was defined as using the rental subsidy voucher offered to lease an apartment within 90 days (after which the voucher offer expired).[7,8] By definition, the experimental voucher was unavailable to the control group so control subjects were fully compliant. Approximately half of families randomly assigned to the experimental treatment of a voucher offer took up the offer and moved by using the voucher.

Victimization was based on household head report that a household member had been victimized by violent crime within 6 months before baseline.

Covariates

To improve efficiency, we adjusted for site, gender, and other prerandomization characteristics enumerated in Table 1. Covariate adjustment had little effect on results.

TABLE 1 MTO Youth, Baseline Variables, Overall and by Treatment Group

Construct	Variable	Treatment Group			
		Overall	Treatment Group	Control	*P*[a]
Total in interim survey in 2002	*N*	2829	1950	879	
Baseline mean poverty rate	Percent poverty rate in the 1990 census tract	49.8	49.5	50.5	
Family characteristics					
Victimization	Percent with household member victimized by crime during past 6 mo	43.0	43.8	41.3	
Site, %	Baltimore	15.5	16.0	14.2	
	Boston	18.9	18.1	20.7	
	Chicago	22.4	23.3	20.4	
	Los Angeles	18.6	17.5	21.2	
	New York	24.6	25.1	23.5	
Household size, %	2 people	7.3	6.9	8.3	
	3 people	22.3	22.1	22.9	
	4 people	25.4	26.2	23.4	
	≥5 people	45.0	44.8	45.4	
Youth characteristics					
Age, y		9.94	9.96	9.88	
Gender, %	Male	49.9	49.5	51.0	
	Female	50.1	50.5	49.0	
Race/ethnicity, %	African American	62.8	63.2	62.1	
	Hispanic ethnicity, any race	30.0	30.3	29.5	
	White	1.1	1.0	1.2	
	Other race	2.2	2.4	1.9	
	Missing race	3.8	3.2	5.3	
Gifted, %	Special class for gifted students or did advanced work	15.4	14.7	16.8	
Developmental problems, %	Special school, class, or help for learning problem in past 2 y	16.6	16.7	16.3	
	Special school, class, or help for behavioral or emotional problems in past 2 y	7.7	8.7	5.3	*
	Problems that made it difficult to get to school and/or to play active games	6.5	7.1	5.0	
	Problems that required special medicine and/or equipment	9.1	10.0	7.0	*
	School asked to talk about problems child having with schoolwork or behavior in past 2 y	26.3	26.7	25.4	
Household head characteristics					
Family structure, %	Never married	55.9	55.2	57.5	
	Teen parent	25.9	26.4	25.0	
Socioeconomic status, %	Employed	25.8	26.1	25.3	
	On AFDC (welfare)	76.0	75.5	76.9	
Education, %	Less than high school	47.1	47.2	46.7	
	High school diploma	36.2	36.6	35.3	
	GED	16.7	16.1	17.9	
	In school	13.9	14.4	12.6	
Neighborhood/mobility variables, %	Lived in neighborhood ≥5 y	65.7	65.8	65.5	
	No family living in neighborhood	64.1	63.1	66.3	
	No friends living in neighborhood	37.3	36.8	38.5	
	Had applied for Section 8 voucher before	44.3	43.6	45.8	
Neighbor relationships, %	Chats with neighbors at least once a week	51.9	51.3	53.2	
	Respondent very likely to tell neighbor if saw neighbor's child getting into trouble	56.7	56.8	56.4	

All variables range between 0 and 1 except baseline age (5–16) and mean poverty rate, so means represent proportions. Analysis weighted for varying treatment random assignment ratios across time and for attrition. All tests were adjusted for clustering at the family level. The following baseline variables were used as covariates in regression analyses: site; youth gender, race, giftedness, and schoolwork or behavior problems; household head marital status, employment, education, tenure in neighborhood, relationships with baseline neighbors, presence of family/friends in baseline neighborhood, and previous application for Section 8. Missing baseline covariate data were imputed to site-specific means (<5 missing)[8] or modeled with missing indicators (7 missing for youth giftedness and household head education). AFDC, Aid to Families With Dependent Children; GED, general equivalency diploma.

[a] *P* value from test of treatment group differences calculated from Wald χ^2 tests outputted from logistic regression for dichotomous baseline characteristics and multinomial logistic regression for categorical characteristics. *F* tests were used with linear regression for continuous variables. The null hypothesis is that the treatment and control group proportions or means did not differ.

* $P < .05$.

Analytic Approach

We estimated additive effects of treatment assignment by regressing outcomes (K6, BPI, and MDD) on randomly assigned treatment group with covariate-adjusted linear regression, per intent-to-treat (ITT) principles.[31] Covariate adjustment was not necessary for internal validity given randomization; however, adjustment often improves efficiency without compromising type 1 error rate.[32,33] We first estimated adjusted models for treatment effects averaged over all youth and next assessed modification of treatment effects on mental health by gender with gender-by-treatment interactions (given previous evidence of gender effect modification[7,8]). We then assessed whether family violent crime victimization modified treatment effects by using treatment-by-victimization interactions, retaining the gender-treatment interactions. Effect modification is summarized graphically, displaying average treatment effects on mental health (experimental minus control means), for each gender-victimization subgroup. Negative values for treatment coefficients indicate beneficial effects of treatment.

When ITT effect modification tests were statistically significant, we also calculated adherence-adjusted effect estimates based on instrumental variable (IV) analysis, estimated with 2-stage least squares regression. We estimated IV effects because ITT effect estimates are probably attenuated compared with effects of using the voucher to move, given that many families randomized to receive vouchers did not use them. Direct comparisons of movers to nonmovers would potentially be biased. IV analyses are appropriate to correct for differential adherence in randomized controlled trials and avoid biases associated with directly comparing compliers to noncompliers.[34,35] Under the main assumption that treatment assignment can only affect mental health indirectly, via use of the voucher to move, treatment assignment is a valid IV to estimate the effects of using the vouchers to move out of public housing on mental health. The IV approach also reveals whether patterns of ITT treatment effect heterogeneity could be attributable to lower adherence rates among previously victimized families.[36] In MTO, IV effect estimates are typically interpreted as the effect of using the voucher among individuals who did in fact use it.[7,37]

All analyses accounted for household clustering, with weights accounting for random assignment ratio changes and attrition.[7] We report robust SEs. We imputed row-column values for missing outcome values (<1% for distress and BPI and 4% for lifetime MDD). We used M-Plus 6.11 for IRT analyses and Stata 11.0 (Stata Corp, College Station, TX) for all other analyses.

RESULTS

Table 1 presents sample descriptives.

ITT estimates of overall marginal effects of treatment were null for psychological distress (B = 0.015, 95% confidence interval [CI]: −0.075 to 0.105, P = .75), null for lifetime MDD (B = −0.008, 95% CI: −0.027 to 0.012, P = .44), and null for BPI (B = 0.066, 95% CI: −0.022 to 0.153, P = .141; results not shown). These average effects masked qualitative effect modification by gender (Table 2), significant for all 3 outcomes. Randomization to the MTO intervention significantly benefitted girls' psychological distress and MDD but significantly harmed boys' psychological distress and BPI.

Table 3 displays IV adherence-adjusted treatment effects by gender; patterns

TABLE 2 MTO ITT Treatment Effects at Interim (4- to 7-Year) Follow-up on Mental Health Among Adolescents, Effect Modification by Gender

	Psychological Distress					Behavior Problems					Lifetime MDD				
	β	SE	Lower CI[a]	Upper CI[a]	P	β	SE	Lower CI[a]	Upper CI[a]	P	β	SE	Lower CI[a]	Upper CI[a]	P
Regression coefficients															
Treatment	−0.123	0.061	−0.244	−0.003	*	−0.035	0.060	−0.153	0.083		−0.032	0.016	−0.064	−0.000	*
Male	−0.411	0.070	−0.547	−0.274	***	−0.063	0.068	−0.195	0.070		−0.074	0.016	−0.106	−0.042	***
Treatment by male interaction	0.274	0.086	0.104	0.443	**	0.199	0.082	0.037	0.360	*	0.049	0.020	0.011	0.087	*
Calculated treatment effects															
Girls	−0.123	0.061	−0.244	−0.003	*	−0.035	0.060	−0.153	0.083		−0.032	0.016	−0.064	−0.000	*
Boys	0.150	0.064	0.024	0.276	*	0.164	0.060	0.046	0.282	**	0.017	0.011	−0.005	0.038	

Treatment variable indicates assignment to active treatment group receiving a housing subsidy offer, compared with controls (omitted). Regression models adjusted for site; race; household head marital status, employment, education, lived in neighborhood for 5 y 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 neighbor's child getting into trouble, and has applied for Section 8 before; child is gifted; child had problems with schoolwork or behavior; plus male, treatment, and treatment by male interaction. Models adjusted for family-level clustering, output with robust SEs, and weighted. IRT methods were used to derive psychological distress and behavior problems.

[a] CI = 95% CI for the mean.

*** P < .001;

** P < .01;

* P < .05;

# P < .10.

are similar to ITT estimates but approximately twice as large.

Confirming our novel hypothesis, victimization adversely modified effects of the MTO intervention assignment on the K6 and BPI; youth in victimized families had worse average treatment effects than their counterparts from families who had not experienced victimization, overall and within gender group (Fig 2). For all 3 outcomes, girls from nonvictimized families were the only subgroup to significantly benefit from the intervention. Boys in victimized families were the only subgroup significantly harmed by the intervention (Fig 2 A and B).

Psychological distress illustrates these patterns. The modest average beneficial ITT effect of treatment of girls (Table 2, B = −0.123, P = .044) masked larger benefits for girls from nonvictimized families (B = −0.192, 95% CI: −0.334 to −0.050, P = .008, ie, a 0.17 SD decrease in distress [Cohen's D = −0.192/1.123[38]]) and an effect close to zero for girls from victimized families (B = −0.027, 95% CI: −0.187 to 0.133, P = .74; Fig 2A). Furthermore, the overall harmful ITT effect among boys (B = 0.150, P = .020) reflected substantial harm among boys from victimized families (B = 0.243, 95% CI: 0.076 to 0.410, P = .004) and null effects among boys from nonvictimized families (B = 0.078, 95% CI: −0.068 to 0.224, P = .29). Patterns for BPI (Fig 2A) and MDD (Fig 2B) were strikingly similar to those for distress. Effects were marginally stronger for (and concentrated in) the low-poverty versus Section 8 treatment group for distress and BPI (Supplemental Figs 3 and 4).

Victimization-treatment interaction coefficients were at least marginally significant for distress and BPI, indicating adverse heterogeneity of treatment effects by baseline victimization (respectively, B = 0.165, 95% CI: −0.017 to 0.347, P = .076 and B = 0.235, 95% CI: 0.059 to 0.410, P = .009). There was an adverse, but nonsignificant, effect modification of treatment by victimization for MDD.

Adherence-adjusted IV estimates also indicate significant treatment benefits on distress and BPI for girls from nonvictimized families, harmful effects among boys from victimized families, and null effects for the other 2 groups (Fig 2C). Similar to ITT effects, IV results for MDD were null. IV effect estimates of using a voucher to move were twice as large as ITT estimates, as expected given 51% treatment adherence.

DISCUSSION

We found differential intervention effects on distress, behavior problems, and MDD 4 to 7 years after adolescents moved out of public housing into lower-poverty neighborhoods in the MTO experiment. Female adolescents experienced beneficial mental health effects of the MTO treatment, but male adolescents experienced harmful effects. Moreover, for both girls and boys, effects of the MTO treatment were worse for youth from families in which someone was a victim of violent crime before randomization. The MTO IV effect sizes ranged from a harmful 0.42 to 0.53 SDs for boys and from a beneficial 0.23 to 0.32 SDs for girls for the dimensional outcomes. These are moderate effect sizes[38]; indeed, the beneficial effect for girls' distress is nearly that obtained from psychotherapy treatment in youth[39] (especially notable because psychotherapy is administered to clinical populations, yet we see similar effect sizes here in a nonclinical sample).

TABLE 3 MTO IV Adherence-Adjusted Treatment Effects at Interim (4- to 7-Year) Follow-up on Mental Health Among Adolescents, Effect Modification by Gender

	Psychological Distress					Behavior Problems					Lifetime MDD				
	β	SE	Lower CI[a]	Upper CI[a]	P	β	SE	Lower CI[a]	Upper CI[a]	P	β	SE	Lower CI[a]	Upper CI[a]	P
Regression coefficients															
Treatment	−0.237	0.117	−0.467	−0.007	*	−0.068	0.115	−0.293	0.157		−0.062	0.031	−0.123	−0.001	*
Male	−0.409	0.069	−0.544	−0.274	***	−0.061	0.067	−0.192	0.070		−0.074	0.016	−0.106	−0.043	***
Treatment by male interaction	0.547	0.173	0.209	0.886	**	0.405	0.165	0.082	0.728	*	0.097	0.039	0.021	0.172	*
Calculated treatment effects															
Girls	−0.237	0.117	−0.467	−0.007	*	−0.068	0.115	−0.293	0.157		−0.062	0.031	−0.123	−0.001	*
Boys	0.310	0.132	0.052	0.569	*	0.337	0.125	0.093	0.581	**	0.035	0.023	−0.009	0.079	

Second-stage IV results presented here. The treatment variable represents treatment adherence, that is, whether the family used the housing subsidy to move. Two-staged least squares regression models adjusted for site; race; household head marital status, employment, education, lived in neighborhood for 5 y 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 neighbor's child getting into trouble, and has applied for Section 8 before; child is gifted; child had problems with schoolwork or behavior; plus male, treatment, and treatment by male interaction. Models adjusted for family-level clustering, output with robust SEs, and weighted. IRT methods were used to derive psychological distress and behavior problems. To assess robustness to misspecification of pre-randomization covariates in linear regression for binary outcome MDD, we implemented a version of G-estimation[64] which does not rely on correct specification of the outcome from linear regression but relies only on randomization of the instrument. Results were almost identical (available on request), so we present results from conventional IV analysis.

[a] CI = 95 CI for the mean

*** $P < .001$;

** $P < .01$;

* $P < .05$;

# $P < .10$.

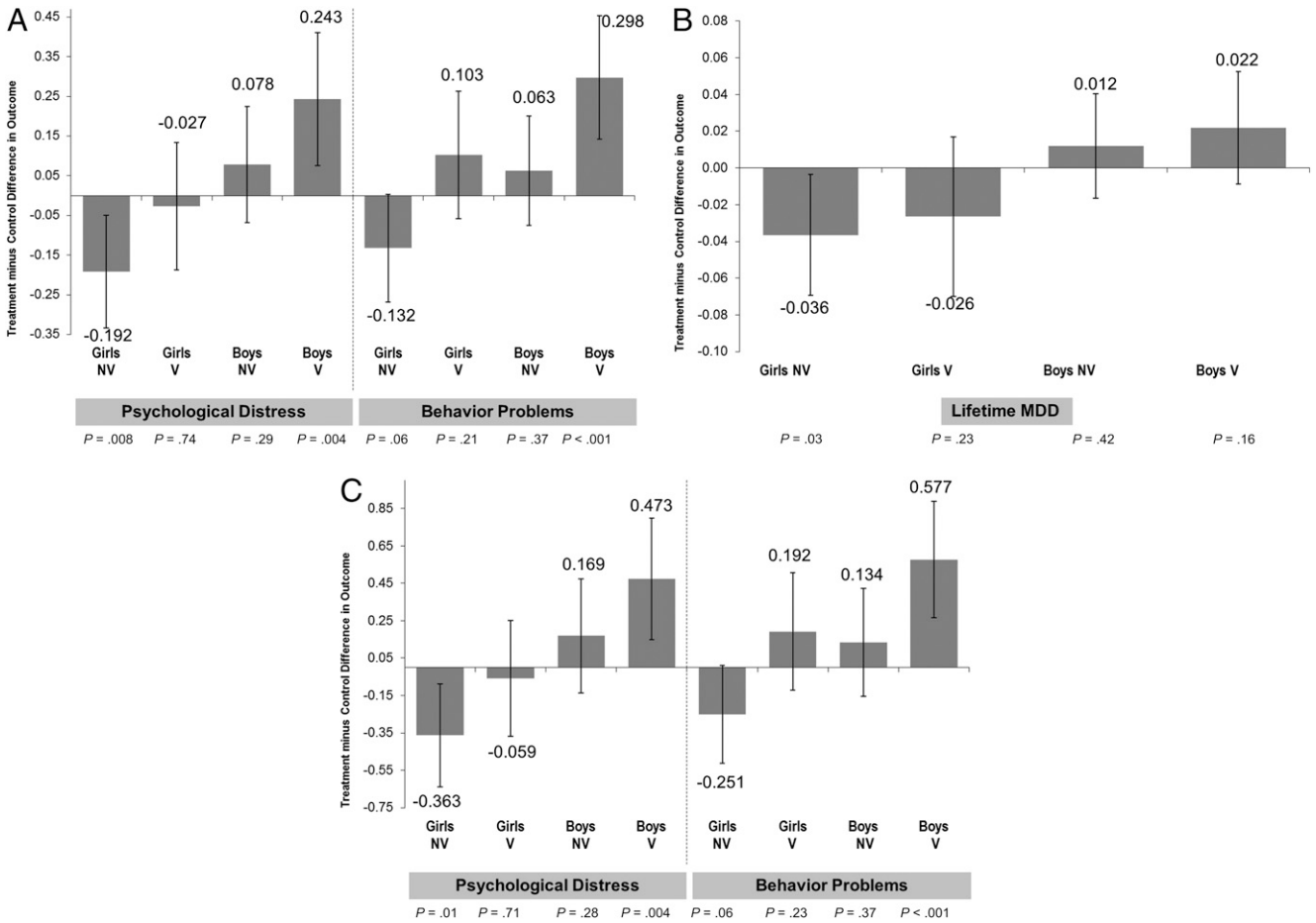

FIGURE 2

MTO treatment effects on mean difference in adolescent mental health 4–7 years after baseline, modified by gender and recent violent crime victimization. A, The linear regression ITT estimates for adolescent distress and behavior outcomes. B, Linear regression ITT estimates for lifetime MDD. C, Adherence-adjusted estimates of treatment effects from second-stage IV analysis for distress and behavior problems. Treatment-victimization interaction results for ITT: A, Distress: B (SE) = 0.165 (0.093), $P = .076$; Behavior Problems: B (SE) = 0.235 (0.089), $P = .009$; B, MDD: B (SE) = 0.010 (0.020), $P = .626$. C, For IV: Distress: B (SE) = 0.304 (0.183), $P = .096$; for Behavior Problems: B (SE) = 0.443 (0.178), $P = .013$. *P* values reported beneath each bar near figure bottom test each subgroup effect against a null hypothesis of zero. Models adjusted for covariates listed in Table 2, plus treatment-baseline victimization interaction. See Supplemental Information 1 for definition of lifetime MDD. NV, nonvictimized; V, victimized.

Few previous publications evaluate health effects of MTO at 4 to 7 years' follow-up.[7,8,40,41] Previous research reported adverse effects of MTO on overall youth self-rated health and asthma; beneficial effects of MTO on distress, lifetime MDD, and smoking for adolescent girls; and, for boys, null associations with mental health outcomes and, unexpectedly, adverse behavioral outcomes.[7,8,41] The Department of Housing and Urban Development recently released outcomes for a younger cohort of children, 12 years after baseline, showing similar gender patterns.[42] Statistical tests of gender interactions were not reported however, so no formal replication is available.

Our findings add to previous literature in 2 ways. We tested a specific explanation for treatment effect heterogeneity: vulnerability due to previous crime victimization. Furthermore, we operationalized health outcomes by using validated latent variable methods, improving precision in measuring distress, which substantively altered conclusions from previous work.[31] Specifically, we found that MTO significantly elevated distress for boys, in contrast to previous findings, which were null.[7,8]

The null effects we found for behavior problems among girls and MDD among boys may reflect gendered expression of mental health. Girls more commonly manifest distress as mood disorders/symptomatology, and boys more typically express externalizing disorders/symptomatology.[43,44] Our findings present consistent patterns; adolescent boys and youth from families with a history of violent crime victimization did not benefit from moves into lower-poverty neighborhoods and in some cases were harmed.

Qualitative evidence from MTO suggests that girls may have benefited because

they escaped neighborhood environments with pervasive sexual harassment and threats of sexual violence.[45] In contrast, adolescent boys may have had difficulty adjusting to changes in social networks and relationships in the new neighborhoods.[46] Boys in high-crime areas may cope by adhering to context-specific oppositional "street" cultures[47] that are less advantageous in lower-poverty neighborhoods. We did not test these potential explanations, although research on mechanisms is clearly needed. A child's age or developmental stage may also modify treatment,[39] but we did not test that hypothesis in this study, although other MTO analyses have.[39,48,49] This remains important to investigate.

Our findings suggest that gender is one of several social determinants that modifies children's capacity to benefit from new resources and neighborhood contexts, consistent with previous research on child development.[6] We found harm specific to 1 group: boys from families with a history of recent crime victimization. This adds to emerging evidence emphasizing that low-income children are often blocked from achieving healthy, successful adulthoods by an interlocking system of adversity in which individual and neighborhood socioeconomic disadvantage compromise health via multiple pathways.[6,50–52] Ameliorating these barriers requires meaningful intersectoral collaboration, in particular, coordinating housing and health services.

Housing Choice Vouchers (formerly known as Section 8), on which MTO was based, are central to US rental housing assistance policy.[53,54] Large-scale social interventions of this kind, therefore, should be designed and monitored with an eye to health impacts and the possibility of unintended negative consequences for vulnerable populations. Determining which children accrue health benefits and which are harmed may help enhance programs to improve outcomes for all children. Recently, innovative intersectoral programs (eg, medical legal partnerships,[55] volunteer programs such as Health Leads),[56] which integrate social services into health care settings, have demonstrated promise for improving health outcomes for low-income families. Social service delivery programs are also increasingly integrating health care; for example, HOPE VI, a major federal housing relocation program, requires tailored supportive services for its residents, including health care.[57,58]

An important question concerns whether the neighborhood context, and/or the move itself, influenced child mental health. Although there is some research on this question elsewhere,[15,16,59,60] MTO cannot tease these apart because of its bundled treatment. Our results documented marginally stronger treatment effects in the low-poverty group, rather than the section 8 group, providing 1 clue that the type of neighborhood to which families moved may have mattered more for adolescent mental health than the move itself. This important question for future research may implicate other potential interventions such as place-based neighborhood revitalization efforts to modify neighborhoods directly.[61]

MTO's original aims focused on economic self-sufficiency, not health,[62] so mental health was not assessed at baseline. Additional data would have improved statistical power and facilitated investigating differential treatment effects by baseline health. Although we had limited power given low prevalence of MDD, our consistent pattern of results was triangulated from different mental health outcomes, including diagnostic, symptomatology, internalizing, and externalizing measures, which may jointly better represent a broader universe of mental health outcomes. We operationalized violent crime victimization at the family level, assuming it affects the whole family,[63] because no individual-level variable was available. Lastly, these findings may not generalize to all adolescents because MTO families were exceptionally disadvantaged at baseline. Yet this population is of particular social relevance as the target of means-tested policies.

CONCLUSIONS

The $70 million MTO demonstration program sought to assess the potential effects of moving low-income families from high- to low-poverty neighborhoods.[24] Its experimental design places MTO among the strongest existing studies of how changes in neighborhood context affect children's mental health. Our results show that the MTO program benefited a robust group of adolescent girls and harmed a particularly vulnerable group of adolescent boys. Incorporating additional services across sectors may be important for facilitating successful outcomes among all families receiving housing mobility assistance.

REFERENCES

1. Aneshensel CS. Social stress: theory and research. *Annu Rev Sociol.* 1992;18:15–38
2. 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: Wiley; 2009:411–443
3. Oakes JM. The (mis)estimation of neighborhood effects: causal inference for a practicable social epidemiology. *Soc Sci Med.* 2004;58(10):1929–1952

4. 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, 948
5. Pebley AR, Sastry N. *Neighborhoods, Poverty and Children's Well-being: A Review*. Report CCPR-035-04. Los Angeles, CA: California Center for Population Research, University of California–Los Angeles; 2003
6. Boyce WT. A biology of misfortune: stress reactivity, social context, and the ontogeny of psychopathology in early life. In: Masten Ann S, ed. *Multilevel Dynamics in Developmental Psychopathology: Pathways to the Future*. Mahwah, NJ: Lawrence Erlbaum Associates; 2007:45–82
7. Kling JR, Liebman JB, Katz LF. Experimental analysis of neighborhood effects. *Econometrica*. 2007;75(1):83–119
8. Orr L, Feins JD, Jacob R, et al. *Moving to Opportunity for Fair Housing Demonstration Program: Interim Impacts Evaluation*. Washington, DC: US Department of Housing and Urban Development; 2003
9. Osypuk TL, Tchetgen Tchetgen E, Acevedo-Garcia D, et al. Differential mental health effects of neighborhood relocation among youth in vulnerable families: results from a randomized trial. *Arch Gen Psychiatry*. In press.
10. 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
11. McCormick MC, Brooks-Gunn J, Buka SL, et al. 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
12. O'Donnell DA, Schwab-Stone ME, Ruchkin V. The mediating role of alienation in the development of maladjustment in youth exposed to community violence. *Dev Psychopathol*. 2006;18(1):215–232
13. Garbarino J, Dubrow N, Kostelny K, Pardo C. *Children in Danger: Coping With the Consequences of Community Violence*. San Francisco, CA: Jossey-Bass; 1992
14. Sanders-Phillips K. The ecology of urban violence: its relationship to health promotion behaviors in low-income black and Latino communities. *Am J Health Promot*. 1996;10(4):308–317
15. Simmons RG, Burgeson R, Carlton-Ford S, Blyth DA. The impact of cumulative change in early adolescence. *Child Dev*. 1987;58(5):1220–1234
16. Adam EK, Chase-Lansdale PL. Home sweet home(s): parental separations, residential moves, and adjustment problems in low-income adolescent girls. *Dev Psychol*. 2002;38(5):792–805
17. Pribesh S, Downey DB. Why are residential and school moves associated with poor school performance? *Demography*. 1999;36(4):521–534
18. Sampson RJ, Raudenbush SW, Earls F. Neighborhoods and violent crime: a multilevel study of collective efficacy. *Science*. 1997;277(5328):918–924
19. Cutrona CE, Wallace G, Wesner KA. Neighborhood characteristics and depression: an examination of stress processes. *Curr Dir Psychol Sci*. 2006;15(4):188–192
20. Fowler PJ, Tompsett CJ, Braciszewski JM, Jacques-Tiura AJ, Baltes BB. Community violence: a meta-analysis on the effect of exposure and mental health outcomes of children and adolescents. *Dev Psychopathol*. 2009;21(1):227–259
21. Haynie DL, Petts RJ, Maimon D, Piquero AR. Exposure to violence in adolescence and precocious role exits. *J Youth Adolesc*. 2009;38(3):269–286
22. Joseph NP, Augustyn M, Cabral H, Frank DA. Preadolescents' report of exposure to violence: association with friends' and own substance use. *J Adolesc Health*. 2006;38(6):669–674
23. Goering J. *Expanding Housing Choices for HUD-Assisted Families: Moving to Opportunity*. First Biennial Report to Congress, Moving to Opportunity for Fair Housing Demonstration Program. Washington, DC: Department of Housing and Urban Development; April 1996
24. Feins JD, McInnis D. The Interim Impact Evaluation for the Moving to Opportunity Demonstration (C-OPC-21484). Cambridge, MA: Abt Associates; August 14, 2001
25. 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 Department of Housing and Urban Development; 1999
26. Kessler RC, Andrews G, Colpe LJ, et al. Short screening scales to monitor population prevalences and trends in non-specific psychological distress. *Psychol Med*. 2002;32(6):959–976
27. Hambleton RK, Swaminathan H. *Item Response Theory: Principles and Applications*. Boston, MA: Kluwer-Nijhoff Publishing; 1985
28. Zill N. *Behavior Problems Index Based on Parent-Report*. Washington, DC: Child Trends; 1990
29. Kessler RC, Avenevoli S, Green JG, et al; Concordance of DSM-IV/CIDI Diagnoses with Clinical Reassessments. 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
30. 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 [published correction appears in *Arch Gen Psychiatry*. 2005;62(7)768]. *Arch Gen Psychiatry*. 2005;62(6):593–602
31. Kleinbaum DG, Sullivan KM, Barker ND. *A Pocket Guide to Epidemiology*. New York, NY: Springer; 2007
32. Gail M, Wieand S, Piantadosi S. Biased estimates of treatment effect in randomized experiments with nonlinear regressions and omitted covariates. *Biometrika*. 1984;71(3):431–444
33. Rosenblum M, van der Laan MJ. Using regression models to analyze randomized trials: asymptotically valid hypothesis tests despite incorrectly specified models. *Biometrics*. 2009;65(3):937–945
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. 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
37. Gennetian LA, Bos JM, Morris PA. *Using Instrumental Variables Analysis to Learn More From Social Policy Experiments*. New York, NY: Manpower Demonstration Research Corporation; 2002
38. Cohen J. *Statistical Power Analysis for the Behavioral Sciences*. New York, NY: Academic Press; 1969
39. Weisz JR, Weiss B, Han SS, Granger DA, Morton T. Effects of psychotherapy with children and adolescents revisited: a meta-analysis of treatment outcome studies. *Psychol Bull*. 1995;117(3):450–468
40. Fortson JG, Sanbonmatsu L. Child health and neighborhood conditions: results from a randomized housing voucher experiment. *J Hum Resour*. 2010;45(4):840–864
41. Wiehe SE, Aalsma MC, Liu GC, Fortenberry JD. Gender differences in the association between perceived discrimination and adolescent smoking. *Am J Public Health*. 2010;100(3):510–516
42. Sanbonmatsu L, Ludwig J, Katz LF, et al. Moving to Opportunity for Fair Housing

Demonstration Program: Final Impacts Evaluation. Washington, DC: US Department of Housing and Urban Development Office of Policy Development & Research; 2011. Available at: www.huduser.org/portal/publications/pubasst/MTOFHD.html. Accessed November 3, 2011

43. Rosenfield S. Gender and mental health, do women have more psychopathology, men more, or both the same (and why)? In: Horwitz Allan V, Scheid Teresa L, eds. *A Handbook for the Study of Mental Health: Social Contexts, Theories, and Systems*. Cambridge, UK: Cambridge University Press; 1999:348–360
44. 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
45. Popkin SJ, Leventhal T, Weismann G. *Girls in the 'Hood: The Importance of Feeling Safe*. Washington, DC: The Urban Institute; 2008
46. 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
47. Anderson E. *Code of the Street: Decency, Violence, and the Moral Life of the Inner City*. New York, NY: W.W. Norton & Company; 2000
48. 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
49. Sanbonmatsu L, Kling JR, Duncan GJ, Brooks-Gunn J. Neighborhoods and academic achievement: results from the Moving to Opportunity experiment. *J Hum Resour*. 2006;41(4):649–691
50. Evans GW. The environment of childhood poverty. *Am Psychol*. 2004;59(2):77–92
51. Bronfenbrenner U. Reality and research in the ecology of human development. *Proc Am Philos Soc*. 1975;119(6):439–469
52. 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
53. Kruckenberg K. Poverty & Race Research Action Council. *Two Simple Changes to Improve Health Outcomes in the Section 8 Voucher Program* [Policy Brief]. Washington DC: Poverty & Race Research Action Council; 2011. Available at: www.prrac.org/pdf/HQS-SEMAPhealthpolicybriefApril2011.pdf. Accessed April 28, 2011
54. National Low Income Housing Coalition. Federal Budget and Appropriations, HUD Budget Charts, FY11 and FY12 Budget Chart for Selected HUD Programs, updated April 27, 2011. Washington, DC: National Low Income Housing Coalition; 2011
55. Cohen E, Fullerton DF, Retkin R, et al. Medical-legal partnership: collaborating with lawyers to identify and address health disparities. *J Gen Intern Med*. 2010;25(suppl 2):S136–S139
56. Bornstein D. Treating the cause, not the illness. *New York Times* July 28, 2011. Available at: http://opinionator.blogs.nytimes.com/2011/07/28/treating-the-cause-not-the-illness. Accessed August 9, 2011
57. Popkin SJ, Levy D, Harris LE, Comey J, Cunningham MK, Buron L. *HOPE VI Panel Study: Baseline Report*. Washington, DC: The Urban Institute; 2002
58. Engdahl L. *New Homes, New Neighborhoods, New Schools: A Progress Report on the Baltimore Housing Mobility Program*. Baltimore, MD; Washington DC: Baltimore Regional Housing Campaign, and Poverty & Race Research Action Council; 2009
59. DeLuca S, Dayton E. Switching social contexts: the effects of housing mobility and school choice programs on youth outcomes. *Annu Rev Sociol*. 2009;35:457–491
60. Adam EK. Beyond quality: parental and residential stability and children's adjustment. *Curr Dir Psychol Sci*. 2004;13(5):210–213
61. 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
62. Goering J, Feins J. *Choosing a Better Life? Evaluating the Moving to Opportunity Social Experiment*. Washington, DC: The Urban Institute Press; 2003
63. Klebanov PK, Brooks-Gunn J, Duncan GJ. Does neighborhood and family poverty affect mothers' parenting, mental health, and social support? *J Marriage Fam*. 1994;56:441–455
64. Robins JM. Correcting for non-compliance in randomized trials using structural nested mean models. *Commun Statist Theory Methods* 1994;23:2379–2412

(Continued from first page)

www.pediatrics.org/cgi/doi/10.1542/peds.2011-2535

doi:10.1542/peds.2011-2535

Accepted for publication May 11, 2012

Address correspondence to Theresa L. Osypuk, SD, Department of Health Sciences, Bouvé College of Health Sciences, Northeastern University, 360 Huntington Ave, Robinson 316, Boston, MA 02115. E-mail: tosypuk@neu.edu

PEDIATRICS (ISSN Numbers: Print, 0031-4005; Online, 1098-4275).

FINANCIAL DISCLOSURE: *The authors have indicated they have no financial relationships relevant to this article to disclose.*

FUNDING: Funding to support this research project was provided by National Institutes of Health grants 1R01MD006064-01 and 1R21HD066312-01 (Dr Osypuk, PI). Funded by the National Institutes of Health (NIH).