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Neighborhood Effects on the Long-Term Well-Being of Low-Income Adults

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Nearly 9 million Americans live in extreme-poverty neighborhoods, places that also tend to be racially segregated and dangerous. Yet, the effects on the well-being of residents of moving out of such communities into less distressed areas remain uncertain. Using data from Moving to Opportunity, a unique randomized housing mobility experiment, we found that moving from a high-poverty to lower-poverty neighborhood leads to long-term (10- to 15-year) improvements in adult physical and mental health and subjective well-being, despite not affecting economic self-sufficiency. A 1-standard deviation decline in neighborhood poverty (13 percentage points) increases subjective well-being by an amount equal to the gap in subjective well-being between people whose annual incomes differ by \$13,000—a large amount given that the average control group income is \$20,000. Subjective well-being is more strongly affected by changes in neighborhood economic disadvantage than racial segregation, which is important because racial segregation has been declining since 1970, but income segregation has been increasing.

Nearly 9 million people in the United States live in “extreme-poverty” neighborhoods in which at least 40% of residents have incomes below the federal poverty threshold, which for 2011 equaled about \$23,000 for a family of four (1, 2). Such neighborhoods also tend to be racially segregated, with high rates of crime and disorder and low-quality public services (3). Studies dating back as far as the 17th century have shown that people living in distressed neighborhoods have greater criminal involvement and fare worse on educational, economic, and health outcomes than do those living in less distressed areas (3–6). These patterns have generated a long-standing concern that distressed neighborhood environments might themselves adversely affect people’s lives and “doubly disadvantage” their low-income residents.

But much uncertainty remains about the degree to which variation across neighborhoods in people’s outcomes reflects the independent causal effects of neighborhood environments per se instead of the propensity of different types of people to live in different areas. Even the most detailed data collection effort may be unable to

measure adequately all of the individual- or family-level characteristics that influence both neighborhood selection and life outcomes. This type of “selection bias” can substantially distort nonexperimental estimates of “neighborhood effects” (7). Yet, determining the importance of changes in people’s neighborhood environments for their life outcomes is a central issue for the social and medical sciences and social policy.

An understanding of the mechanisms through which neighborhood environments affect people’s lives is a crucial issue for policy design. Much of the debate among researchers has focused on the relative importance of residential racial segregation versus economic segregation. Nearly 70 years ago, Gunnar Myrdal argued that racial segregation enabled policymakers to reduce the quality of public services to blacks without harming whites (8), a concern echoed by the 1968 National Advisory Commission on Civil Disorders (the “Kerner Report”) (9). Douglas Massey and Nancy Denton subsequently argued in their widely cited 1993 book *American Apartheid* that “residential segregation has been instrumental in creating a structural niche within which a deleterious set of attitudes and behaviors—a culture of segregation—has arisen and flourished” ([10], p. 8).

In contrast, William Julius Wilson’s landmark 1987 book *The Truly Disadvantaged* argued that the flight of black working- and middle-class families out of ghettos in the 1960s and 1970s was harmful to the families who remained behind not because of any increased racial segregation, but rather because this exodus removed “mainstream role models that help keep alive the perception that education is meaningful, that steady employment is a viable alternative to welfare, and that

family stability is the norm, not the exception” ([11], p. 49). Subsequent work has examined other pathways through which spatially concentrated disadvantage might affect people’s lives, such as declines in “collective efficacy”—the willingness and ability of community residents to work together to support shared norms (3, 5).

Distinguishing the effects of changes in racial versus income segregation also helps answer the question of whether the problem of harmful neighborhood effects on disadvantaged populations is getting better or worse over time, given opposing recent trends in U.S. residential segregation by race and income. Specifically, racial segregation in America peaked in 1970 and has been declining over the past 40 years, to levels not seen since 1910 (12), whereas income segregation has been increasing since 1970 (13, 14).

This paper examines the long-term effects of moving into a less distressed neighborhood environment on the well-being of low-income adults using new data from a unique, large-scale randomized social experiment: the U.S. Department of Housing and Urban Development’s (HUD) Moving to Opportunity (MTO) demonstration. Via random lottery, MTO offered some public housing families but not others the chance to move into a less distressed area (supplementary materials, section 1). MTO randomization generates large, persistent differences in neighborhood conditions across otherwise comparable groups of families and enables us to attribute differences in post-baseline outcomes across groups to the MTO-assisted moves.

Unlike many social experiments that follow people for short periods, we focused on long-term effects through in-person data collected 10 to 15 years after randomization. We have shown elsewhere that MTO moves have long-term beneficial effects on a narrow but important set of physical health measures, related to extreme obesity and diabetes (15). The implications for how neighborhoods affect the overall quality of the lives of participating families were not addressed in that work.

We used data from the MTO experiment to examine the long-term effects of moving to less distressed neighborhoods on broad measures of the well-being of low-income adults. We examined “objective” outcomes (economic self-sufficiency, physical health, and mental health) that have been the traditional focus of this literature. We also took a new approach in examining experimental neighborhood effects on a comprehensive measure of people’s quality of life as they perceive it, using adult self-reports of subjective well-being (SWB). And, we investigated the relative importance of racial segregation versus income segregation in affecting the SWB of low-income adults.

The MTO experiment. From 1994 to 1998, MTO enrolled 4604 low-income public housing families living in high-poverty neighborhoods within five U.S. cities: Baltimore, Boston, Chicago,

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Los Angeles, and New York. Families were randomized into three groups: (i) the Low-Poverty Voucher (LPV) group, which received housing vouchers that subsidize private-market rents but could only be used in census tracts with 1990 poverty rates below 10%; (ii) the Traditional Voucher (TRV) group, which received regular housing vouchers without any MTO relocation constraint; and (iii) a control group, which received no assistance through MTO. Some 48% of the adults assigned to the LPV group and 63% of those assigned to the TRV group managed to relocate using an MTO voucher (the MTO “compliance rate”). Because the effects of LPV and TRV assignment on neighborhood conditions converge over time, and to maximize statistical power, we initially present results that pool the two treatment groups together. (Separate estimates for the LPV and TRV groups are in tables S1 to S4.)

Data from baseline surveys collected from all MTO adults shows these families were quite economically disadvantaged when they applied for MTO (Table 1). Most household heads were African-American or Hispanic females; less than 40% had completed high school. By far the most common reason applicants reported signing up for MTO was to get away from gangs and drugs, with around three quarters reporting this as one of their top two reasons for wanting to move.

Being a random assignment experiment, the distribution of baseline characteristics was balanced between the treatment and control groups. Among the 21 baseline characteristics reported in Table 1, only two treatment-control differences are significant at $P < 0.10$, and none is significant at the $P < 0.05$ threshold. An F test fails to reject the null hypothesis that treatment-control differences in the baseline variables shown in Table 1 are jointly zero ($P = 0.462$).

Measures. To measure long-term effects of changing neighborhoods on adults in the MTO demonstration, the Institute for Social Research at the University of Michigan—under subcontract with our research team—collected in-person data from participants 10 to 15 years after random assignment (hereafter “long-term survey”) (supplementary materials, section 2). Interviewers were blinded to the MTO group assignments of participating families. The effective response rate for MTO adults was 90% and was similar across randomized MTO groups.

To measure the neighborhood conditions in which families were living during the follow-up study period, we linked address information for MTO adults to census tract-level data on population characteristics from the 1990 and 2000 decennial censuses and the 2005 to 2009 American Community Surveys. Our main results focus on duration-weighted tract characteristics averaged over the entire post-randomization study period because people’s life outcomes may depend on cumulative exposure to neighborhood environments, not just current neighborhood conditions (16). The long-term surveys also asked MTO participants to self-report on conditions of the neigh-

borhoods and housing units in which they were living at the time.

To measure neighborhood effects on traditional “objective” measures of well-being, we constructed summary indices of long-term adult outcomes in the domains of economic self-sufficiency, physical health, and mental health. We focused on adults in part because of our interest in well-being over the long term, which may not yet be evident for the MTO children. Our outcome indices are constructed from a set of individual outcomes that are rescaled so that higher values represent “better” outcomes and then converted to z scores by using the control group distribution. Aggregating outcomes improves statistical power to detect impacts and reduces risk of “false positives”

from examining numerous outcomes (7). To reduce the risk of false positives due to data mining, we examined outcome indices that were prespecified for the interim (5-year) MTO follow-up (7).

We also examined a self-reported measure of comprehensive SWB—the first time the effect of neighborhoods on SWB has been assessed in an experimental analysis. Our primary measure of SWB is based on responses to the following question from the General Social Survey (GSS) that we included on our long-term follow-up survey of MTO adults: “Taken all together, how would you say things are these days—would you say that you are very happy, pretty happy, or not too happy?” (17). This type of happiness ques-

Table 1. Baseline characteristics (1994 to 1998) of adults interviewed as part of long-term survey ($n = 3273$ interviewees), by randomized MTO treatment status. Mean values represent shares, except for age and income; missing values have been imputed (except income). Values are weighted to account for changes over time in treatment assignment likelihood and for the follow-up survey sampling design (supplementary materials, section 1). *** $P < 0.01$, ** $P < 0.05$, * $P < 0.10$ on two-tailed t test of difference between MTO treatment and control groups.

	Control group mean	MTO treatment (voucher) groups mean
	$n = 1139$	$n = 2134$
Gender and age		
Female	0.978	0.984
Age as of 31 December 2007 (years)	44.5	44.6
Race and ethnicity		
African-American (any ethnicity)	0.660	0.640
Hispanic ethnicity (any race)	0.304	0.325
Other demographic characteristics		
Never married	0.637	0.623
Working	0.245	0.270
High school diploma	0.361	0.367
General Educational Development (GED) certificate	0.199	0.169*
Receiving Aid to Families with Dependent Children (AFDC)	0.763	0.752
Household characteristics		
Household income (2009 dollars)	\$12,438.64	\$12,833.64
Site		
Baltimore	0.135	0.136
Boston	0.205	0.203
Chicago	0.205	0.206
Los Angeles	0.226	0.225
New York	0.229	0.229
Neighborhood characteristics		
Household member was crime victim in past 6 months	0.416	0.425
Very dissatisfied with neighborhood	0.467	0.478
Primary or secondary reason for wanting to move		
To get away from gangs and drugs	0.779	0.770
Better schools for children	0.481	0.516*
To get a bigger or better apartment	0.457	0.440
To get a job	0.069	0.058

tion yields results similar to those from questions about general life satisfaction; both provide global retrospective assessments of how people think their lives are going and are increasingly used to assess public policy impacts (18). We used the same three-point response scale as the GSS to benchmark MTO against national samples; tradeoffs with this scaling are discussed in the supplementary materials. Another reason we focused on adults is because more is known about measuring SWB of adults than of youth (19). SWB was not included in the interim MTO survey but was added to the long-term survey to be one of the key summary measures of the net impacts on families from moving to a less distressed neighborhood. MTO controls are slightly happier than adults in national surveys with similar sociodemographic characteristics (table S2).

Methods. We conducted intention-to-treat (ITT) estimates that capture the effect of being

offered the chance to use an MTO voucher to move into a different neighborhood. These estimates were calculated as the difference in average outcomes for families assigned to treatment versus those assigned to the control condition. ITT estimation assumes that randomization was carried out correctly, that there is no selective attrition in measuring outcomes across groups, and that MTO's effect on a given family is independent of the treatment status of other families.

We also used the MTO experimental data to estimate the relationship between outcomes and some specific neighborhood attributes $\mathbf{W}$, as in Eq. 1. Ordinary least-squares estimation of Eq. 1 may yield biased estimates because of possible correlation of $\mathbf{W}$ with unmeasured individual characteristics (ϵ) that influence both neighborhood selection and outcomes, Y . We instead used two-stage least-squares to generate instrumental variables (IV) estimates, where in the first stage of the equation we used interactions

of MTO random assignment and indicators for which MTO site families live in at baseline as instrumental variables to generate predicted values of $\mathbf{W}$ that were then substituted for the actual value in the second stage Eq. 1 (7). The equation also includes a set of baseline characteristics, $\mathbf{X}$, including indicators for MTO demonstration site and numerous participant sociodemographic characteristics, to improve the precision of our estimates.

$$Y = \pi_0 + \mathbf{W}\pi_1 + \mathbf{X}\pi_2 + \epsilon \quad (1)$$

IV estimation of Eq. 1 essentially fits a “dose-response” model and asks whether those treatment groups and sites that experience relatively larger gains in specific elements of $\mathbf{W}$ as a result of treatment assignment also experience relatively larger gains in the outcome of interest. This estimation approach assumes that this is the only reason why the effect of treatment assignment on outcomes varies across randomized groups and demonstration sites. It also assumes that the only pathway through which the instruments affect the outcomes of interest is by affecting the neighborhood measures included in Eq. 1. Given the large number of neighborhood attributes affected by MTO moves, this approach cannot isolate the effect of a specific attribute. We instead view any single variable used in $\mathbf{W}$ to be a summary measure of neighborhood environment (for example, tract poverty captures the effects of moving to an area with a lower poverty rate and other aspects of neighborhood economic disadvantage that co-vary with tract poverty).

In a model that relates Y to a single neighborhood measure $\mathbf{W}$ with the only covariates ($\mathbf{X}$) being the indicators for the MTO cities, the IV estimation of Eq. 1 is equivalent to fitting a regression line through the 15 data points that correspond to the average values of Y and $\mathbf{W}$ for each of the three randomized MTO groups in the five demonstration sites relative to the site overall mean. Below, we present several visual instrumental variables graphs that show the data and logic behind our IV estimates.

Results. As shown in Table 2, MTO does indeed generate sizable and sustained differences in average neighborhood conditions of the individuals across randomly assigned groups, despite the fact that only around half the adults assigned to treatment used a MTO voucher to relocate. One year after random assignment, the average control group family is living in a census tract with a poverty rate of 50%, compared with 34% for the average family assigned to treatment (SE of the difference $\pm 0.7\%$). This difference in tract poverty across randomized groups narrowed over time, mostly because tract poverty rates declined for controls over time. This decline was driven by control families increasingly moving into lower-poverty neighborhoods on their own, as opposed to their baseline neighborhoods gentrifying around them. Averaged over the entire study period, assignment

Table 2. MTO effects on post-randomization housing and neighborhood conditions of adult participants interviewed in long-term survey. Table shows average outcomes for control group adults and ITT contrast of outcomes for adults assigned to treatment (pooling the low-poverty and traditional voucher groups) rather than control. Housing and neighborhood conditions were measured from long-term survey data and census tract-level data interpolated from the 1990 and 2000 decennial censuses and the 2005–2009 American Community Survey. ITT was calculated by using ordinary least-squares regression controlling for baseline covariates, using weights (Table 1 and supplementary materials, sections 1 and 5). *** $P < 0.01$, ** $P < 0.05$, * $P < 0.10$ on two-tailed t test.

	Control mean	MTO treatment (voucher) groups versus control			
		ITT		SE	<i>n</i>
Census tract characteristics					
Share poor at different points in time					
1 year after random assignment	0.499	−0.160	***	(0.007)	3224
5 years after random assignment	0.399	−0.089	***	(0.007)	3208
10 to 15 years after random assignment (May 2008)	0.311	−0.034	***	(0.007)	3206
Share poor for all addresses since random assignment (duration-weighted)					
Share poor	0.396	−0.082	***	(0.005)	3270
Share poor, z score using U.S. tract poverty distribution	2.082	−0.666	***	(0.041)	3270
Share poor, z score using MTO control group tract poverty distribution	0.000	−0.653	***	(0.040)	3270
Duration-weighted poverty rate is...					
Less than 20%	0.054	0.196	***	(0.013)	3270
Less than 30%	0.242	0.237	***	(0.018)	3270
Less than 40%	0.512	0.206	***	(0.018)	3270
Share minority					
10 to 15 years after random assignment (May 2008)	0.844	−0.024	**	(0.009)	3206
All addresses since random assignment (duration-weighted)	0.880	−0.046	***	(0.006)	3270
Residential mobility					
Number of moves after random assignment	2.165	0.584	***	(0.068)	3273
Self-reports on long-term (10- to 15-year) follow-up surveys about neighborhood and housing conditions					
Feel unsafe during day	0.196	−0.039	**	(0.015)	3262
Number of housing problems (0 to 7)	2.051	−0.380	***	(0.076)	3267
Likely or very likely to report kids spraying graffiti (collective efficacy)	0.589	0.064	***	(0.020)	3255
One or more friends with college degree	0.532	0.049	**	(0.020)	3203

to treatment reduced average tract poverty rates by 8.2 percentage points ($SE \pm 0.5\%$), or about one fifth of the control group average of 40%. This is equal to about two thirds of a SD reduction in tract poverty in the national tract-poverty distribution.

MTO had more modest effects on neighborhood racial composition, as shown in Table 2. Assignment to treatment reduced the average neighborhood minority share experienced by participants over the study period by 4.6 percentage points ($SE \pm 0.6\%$), a small share of the control group's average of 88%, although there are larger treatment-control differences in this variable in some sites than others (this is the source of variation we used for our instrumental variables estimates; supplementary materials, section 3.3). Table 2 further indicates MTO generated sustained effects on neighborhood safety and other neighborhood social processes, such as collective efficacy that are thought to be important in changing behavior (3, 5).

Because moving itself is part of the MTO treatment and could have independent effects on people's life outcomes, it is important to keep in mind that the control group averaged 2.165 moves over the study period (Table 2). Treatment assignment increased the number of moves over 10 to 15 years by 0.584 ($SE \pm 0.068$).

As shown in Fig. 1, the opportunity to move through MTO had mixed (null to positive) long-term effects on objective measures of well-being of the type that have been the traditional focus of the neighborhood effects literature. ITT effects are not statistically significant on economic outcomes for adults in MTO households 10 to 15 years after random assignment. Effects on a broad index of physical health measures are in the direction of better health (ITT effect of $+0.060$ SDs, $SE \pm 0.039$) but are not quite statistically significant ($P = 0.12$; unless otherwise noted, all remaining statistical results come from *t* tests). Effects on mental health are marginally significant ($P = 0.084$) in the direction of better health (ITT effect of $+0.070$ SDs, $SE \pm 0.041$). However, ITT effects are more strongly beneficial for SWB (Fig. 1, far right bar), with the offer to move to a less disadvantaged area increasing SWB by $+0.098$ SDs ($SE \pm 0.039$, $P = 0.013$).

The basic intuition behind our instrumental variables estimates, which try to distinguish between the effects on SWB of neighborhood economic disadvantage (as represented by tract poverty rate) versus racial segregation (as measured by tract share minority), are shown in Fig. 2. The *x* axis of Fig. 2A represents the average tract poverty rate MTO adults experience over the study period, whereas the *y* axis represents SWB, both in standardized (*z* score) form. The data points are the average tract poverty and SWB for adults broken out by MTO randomized group and demonstration site. The slope of this line is essentially our IV estimate of the relationship between SWB and tract poverty. A 1-SD decrease in tract poverty (a 13-percentage-point change) is

associated with an increased SWB equal to 0.141 SDs ($SE \pm 0.054$, $P = 0.0009$) (table S5).

As suggested by Fig. 2, B to D, poverty concentration is more important than is racial segregation in affecting the SWB of MTO adults. SWB does not have a statistically significant relationship with the minority composition of the tracts in which MTO families reside ($P = 0.478$), as illustrated by the relatively flat line in Fig. 2B. The size of the increase in SWB from a 1-SD reduction in tract poverty nearly doubles once we control for tract minority share in the same model (from 0.141 to 0.261 SDs, $SE \pm 0.093$, $P = 0.005$) (table S9), as seen by comparing Fig. 2, A and C. In contrast, holding neighborhood poverty constant, a 1-SD decrease in neighborhood minority share makes MTO adults, if anything, worse off (-0.279 SDs, $SE \pm 0.169$, $P = 0.098$), as shown by the positive slope in Fig. 2D. The conclusion that a decline in neighborhood economic disadvantage has a more beneficial result for SWB than does a comparably sized decline in neighborhood minority composition comes from the fact that we can reject the null hypothesis that the slopes illustrated by Fig. 2, C and D, are equal ($P = 0.030$) (table S9).

Results are qualitatively similar if we estimate models that assume that outcomes are only affected by current neighborhood conditions, measured at the start of the survey period, May 2008 (tables S6 and S10 and figs. S4 to S7).

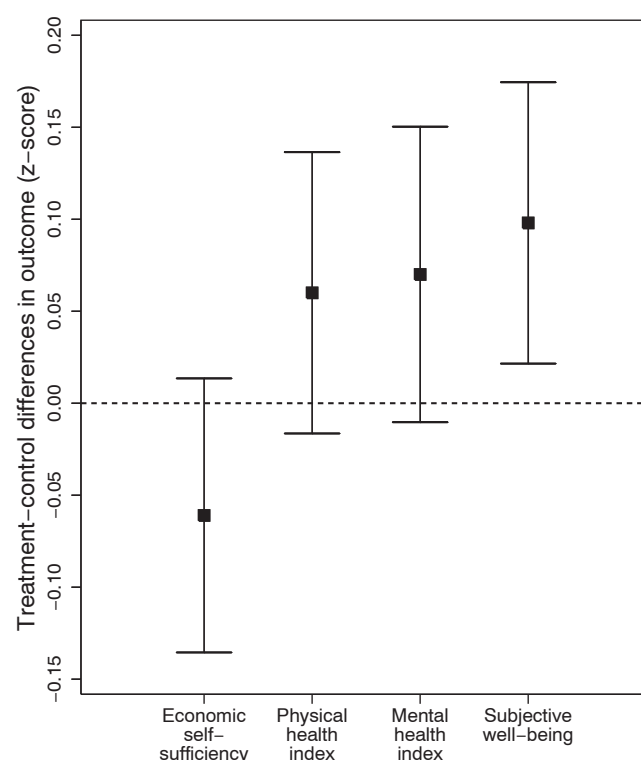
Discussion. To what extent does moving to a less distressed neighborhood environment affect people's well-being? In this Research Article, we present results from a large-scale randomized social experiment (MTO) designed to address this

question, which has been of long-standing concern to the social and medical sciences and to policy-makers. Random assignment in MTO overcomes concerns with selection bias by generating differences in the average neighborhood conditions experienced by otherwise comparable groups of people. MTO is unique in terms of the long duration of the follow-up data collection that has been carried out with participants spanning 10 to 15 years after randomization.

MTO has strong internal validity, but the MTO findings may not generalize to all U.S. families. Although the MTO sample is comparable with other urban minority samples in high-poverty urban areas that have been studied in this literature (20, 21), the sorts of families living in such extreme-poverty areas are very disadvantaged relative to other American adults. MTO was carried out during a time when concentrated poverty and crime rates were declining, and HUD's HOPE VI program was demolishing many public housing projects across the country. MTO's impacts also do not necessarily identify the effects of larger-scale mobility programs (22).

Keeping these caveats in mind, we find that over the long term (10 to 15 years) the chance to move to less distressed neighborhoods in MTO has no detectable long-term effects on adult economic self-sufficiency. In a previous paper, we showed that MTO had important long-term effects on two particularly important physical health measures that predict long-term disease risk; namely, extreme obesity and diabetes (15). We report here that MTO's impact on a broader index of physical health was in the same direction (toward improved health) but was not quite statistically

Fig. 1. Impact on each outcome of assignment to the MTO treatment (voucher) groups for adults interviewed in a long-term survey. The squares represent the ITT estimate for the effect of being assigned to MTO treatment (pooling low-poverty and traditional voucher groups), rather than control, for the outcomes listed on the *x* axis: economic self-sufficiency, physical health, mental health, and SWB (Table 2 and supplementary materials, sections 1, 4, and 5). The box whiskers represent the 95th percent confidence interval around the estimates.



significant, whereas we found a marginally significant beneficial impact of moving to a less distressed neighborhood on a broad index of mental health.

This mixed pattern of MTO impacts for traditional, objective measures of well-being echo

what was found in the interim (5-year) follow up of MTO families (7, 23). These mixed results have been disappointing to many observers, in part because the congressional legislation authorizing the MTO demonstration explicitly mentioned the goal of improving some outcomes that were

unaffected (such as adult earnings). Similar mixed findings are apparent in recent quasi-experimental studies of other housing mobility programs (24–26). These mixed results have led influential observers such as Yale Law School professor Robert Ellickson, who is generally sympathetic to the value of housing vouchers over project-based housing programs, to argue that “recently published studies have begun to destabilize the former consensus that a poor adult or child is significantly disadvantaged by residing among other poor people ... the case for dismantling an entire poor neighborhood ... is hardly so plain” [(27), p. 439].

Yet, the results reported here might lead to quite a different conclusion, in that we find sizable positive effects of moving from a more distressed to a less distressed neighborhood on SWB, a measure that represents a comprehensive assessment by the participants themselves of the extent to which their lives have been affected. Our results suggest that living in distressed neighborhoods has more important adverse impacts, and escaping from such neighborhoods has more important positive effects, on the well-being of low-income adults than was revealed by previous experimental and quasi-experimental studies of neighborhood effects that focused on traditional measures of socioeconomic and health outcomes. Whether or not the MTO vouchers imposed additional locational constraints on families does not appear to matter much for the positive effects of such moves on well-being (table S4).

Although “happiness” has no natural metric, one can still interpret the magnitude of our results by noting that a 1-SD reduction in neighborhood poverty (about 13 percentage points) is associated with an increase in SWB that is about two-thirds of the gap in SWB between U.S. blacks and whites [which is around one quarter of a SD in favor of whites (28)] and about equal to the remaining gap in SWB between families with annual incomes that differ by \$13,000 after conditioning on a standard set of control variables that differ by income and affect happiness (supplementary materials, section 3.3). This is a large amount, equal to about two thirds of the average income of MTO control group families in our long-term survey (\$20,000).

Subject self-reports of SWB have the potential to provide an informative summary measure of the overall impact of neighborhood conditions on people's lives. Although SWB measures are being used with increased frequency in the social sciences and policy analysis, SWB has not been the focus of much previous “neighborhood effects” research. The proper interpretation of self-reports about SWB remains the topic of some debate. Previous studies show different measures of self-reported SWB to be correlated in expected ways with objective indicators of well-being such as life events, biological indicators (such as smiling frequency and brain activity), and reports from significant others about the person's happiness at

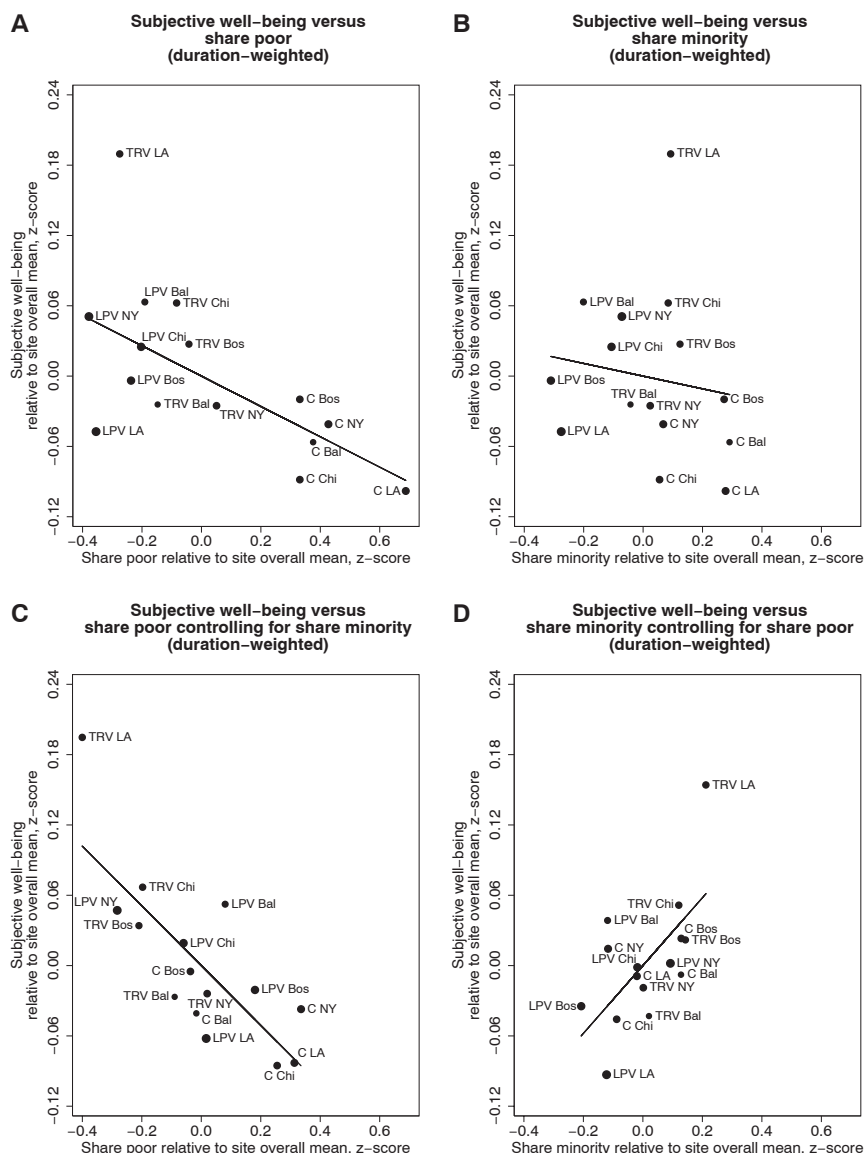


Fig. 2. Instrumental variable estimation of the relationship between SWB and average (duration-weighted) (A) tract poverty rate, (B) tract share minority, (C) tract poverty controlling for minority share, and (D) tract minority share controlling for tract poverty. The y axis is a three-point happiness scale (1 = not too happy, 2 = pretty happy, 3 = very happy) expressed in SD units relative to the control group. Share poor is the fraction of census tract residents living below the poverty threshold. Share minority is the fraction of census tract residents who are members of racial or ethnic minority groups. Tract shares are linearly interpolated from the 1990 and 2000 decennial census and 2005 to 2009 American Community Survey and are weighted by the time respondents lived at each of their addresses from random assignment through May 2008. Share poor and minority are z scores, standardized by the control group mean and SD. The points represent the site (Bal, Baltimore; Bos, Boston; Chi, Chicago; LA, Los Angeles; NY, New York City) and treatment group (LPV, low-poverty voucher; TRV, traditional voucher; C, control group). The slope of the line is equivalent to a two-stage least-squares estimate of the relationship between SWB and the mediator shown in each panel, using interactions of indicators for MTO treatment group assignment and demonstration site as instruments for the mediator (controlling for site indicator main effects).

both the individual and group levels (29, 30) (supplementary materials, section 2.3). We also corroborate our findings for SWB by examining the effects of MTO moves on related measures of psychological distress (table S4).

As noted in the introduction, it is also important for both science and policy to understand why changes in neighborhood environments affect the well-being of low-income adults. Isolating mechanisms with the MTO data are challenging, and our statistical power to do so is somewhat limited. We focused on distinguishing the effects of residential income segregation versus racial segregation because this is a key scientific question, because different policies may be required to address segregation by income versus race, and because racial segregation has declined the past 40 years whereas income segregation has substantially increased.

Our results suggest that changes in neighborhood poverty are more important than racial segregation in affecting the SWB of low-income adults in MTO. (We interpret neighborhood poverty as a marker for a collection of correlated neighborhood characteristics across the neighborhoods in which the MTO families reside.) The same qualitative pattern holds for adult physical and mental health outcomes as well (supplementary materials).

The rise in U.S. residential income segregation since 1970 raises the possibility that the problem of harmful neighborhood effects on people's well-being may be getting worse rather than better over time. Increased poverty concentration in America does not seem to be simply due to increases in overall income inequality (31). The average tract poverty rate for families in the bottom quintile of the U.S. income distribution increased over the past 40 years by about 2.4 percentage points (from 17.6 to 20.0%). If the results from our MTO sample generalize to other very low-income families, the increase in poverty concentration over the past 40 years reduced the well-being of the bottom quintile of the income distribution by an amount that may be equivalent to a decline in annual household income of about \$1400 (~8%). If our estimates are correct, the \$1400 dollar-equivalent for the decline in well-being for families in the bottom quintile caused by increased poverty concentration from 1970 to 2007 is about equal in size to the total gain in real annual family income of \$1300 that the bottom quintile has experienced over roughly the past 40 years from \$15,336 in 1969 to \$16,622 in 2007 [(32), converted to 2009 dollars; supplementary materials, section 3.3].

Our findings are also germane to debates about the proper objectives for public policy. For example, one recent review of U.S. antipoverty programs notes that their effectiveness depends "at least in part, on whether the programs do, in fact, reduce poverty" [(33), p. 12]. By that standard, MTO-type policy efforts to improve the neighborhood conditions of poor families would not be part of an effective antipoverty strategy

because the program failed to produce detectable impacts on family income (7, 23). But if the goal is the broader one of improving the well-being of poor families, then policies that seek to ameliorate the adverse effects of dangerous, distressed neighborhoods on poor families are worthy of careful consideration.

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Supplementary Materials

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