Neighborhoods, Obesity, and Diabetes —
A Randomized Social Experiment

Jens Ludwig, Ph.D., Lisa Sanbonmatsu, Ph.D., Lisa Gennetian, Ph.D.,
Emma Adam, Ph.D., Greg J. Duncan, Ph.D., Lawrence F. Katz, Ph.D.,
Ronald C. Kessler, Ph.D., Jeffrey R. Kling, Ph.D., Stacy Tessler Lindau, M.D.,
Robert C. Whitaker, M.D., M.P.H., and Thomas W. McDade, Ph.D.

ABSTRACT

BACKGROUND
The question of whether neighborhood environment contributes directly to the development of obesity and diabetes remains unresolved. The study reported on here uses data from a social experiment to assess the association of randomly assigned variation in neighborhood conditions with obesity and diabetes.

METHODS
From 1994 through 1998, the Department of Housing and Urban Development (HUD) randomly assigned 4498 women with children living in public housing in high-poverty urban census tracts (in which ≥40% of residents had incomes below the federal poverty threshold) to one of three groups: 1788 were assigned to receive housing vouchers, which were redeemable only if they moved to a low-poverty census tract (where <10% of residents were poor), and counseling on moving; 1312 were assigned to receive unrestricted, traditional vouchers, with no special counseling on moving; and 1398 were assigned to a control group that was offered neither of these opportunities. From 2008 through 2010, as part of a long-term follow-up survey, we measured data indicating health outcomes, including height, weight, and level of glycated hemoglobin (HbA1c).

RESULTS
As part of our long-term survey, we obtained data on body-mass index (BMI, the weight in kilograms divided by the square of the height in meters) for 84.2% of participants and data on glycated hemoglobin level for 71.3% of participants. Response rates were similar across randomized groups. The prevalences of a BMI of 35 or more, a BMI of 40 or more, and a glycated hemoglobin level of 6.5% or more were lower in the group receiving the low-poverty vouchers than in the control group, with an absolute difference of 4.61 percentage points (95% confidence interval [CI], −8.54 to −0.69), 3.38 percentage points (95% CI, −6.39 to −0.36), and 4.31 percentage points (95% CI, −7.82 to −0.80), respectively. The differences between the group receiving traditional vouchers and the control group were not significant.

CONCLUSIONS
The opportunity to move from a neighborhood with a high level of poverty to one with a lower level of poverty was associated with modest but potentially important reductions in the prevalence of extreme obesity and diabetes. The mechanisms underlying these associations remain unclear but warrant further investigation, given their potential to guide the design of community-level interventions intended to improve health. (Funded by HUD and others.)
MANY OBSERVATIONAL STUDIES HAVE shown that neighborhood attributes such as poverty and racial segregation are associated with increased risks of obesity and diabetes, even after adjustment for observed individual and family-related factors. In response, the U.S. surgeon general has called for efforts to “create neighborhood communities that are focused on healthy nutrition and regular physical activity, where the healthiest choices are accessible for all citizens.”

Previous studies have suggested several pathways through which neighborhoods might influence health. Changes in the built environment (e.g., the addition of grocery stores or spaces where residents can exercise) might affect health-related behaviors and outcomes such as obesity. Proximity to health care providers might influence the detection or management of health problems. Neighborhood safety might influence exercise level, diet, or level of stress. Social norms for health-related behaviors may vary across neighborhoods.

It is unclear whether neighborhood environments directly contribute to the development of obesity and diabetes. People living in neighborhoods with high poverty rates differ in many ways from those living in neighborhoods with lower poverty rates, only some of which can be adequately measured in observational studies. These unmeasured individual characteristics may be responsible for variations in health among different neighborhoods. Inferences concerning the influence of neighborhood may be more credible if they are based on randomized studies in which otherwise similar people are encouraged to live in different types of neighborhoods. Using data from Moving to Opportunity (MTO), a large demonstration project intended to uncover the effects of neighborhood characteristics across a range of social and health outcomes in families, we examined the association of randomly assigned variations in neighborhood conditions with obesity and diabetes.

METHODS

STUDY DESIGN

The MTO demonstration project was designed and implemented by the Department of Housing and Urban Development (HUD) with the primary purpose of better understanding the effects of residential location on “employment, income, education, and well-being.” Families with children (defined as family members younger than 18 years of age) living in Baltimore, Boston, Chicago, Los Angeles, or New York in selected public housing developments in census tracts with poverty rates of 40% or more in 1990 were eligible. From 1994 through 1998, families were invited by local housing authorities to participate in a randomized lottery to receive a rent-subsidy voucher. One quarter of eligible families applied.

The analysis reported here focuses on one woman from each family, usually the household head, who was interviewed between 2008 and 2010. This research was approved by the Office of Management and Budget and by the institutional review boards at HUD, the National Bureau of Economic Research, and relevant universities. HUD assisted with the design of the data-collection protocol for the long-term MTO study and reviewed the manuscript before submission to ensure that the confidentiality of MTO program participants was not violated; HUD did not screen the manuscript for other purposes.

INTERVENTIONS AND RANDOMIZATION

Participating families were randomly assigned to one of three groups. Families assigned to receive low-poverty vouchers were offered a standard rent-subsidy voucher but were required to use it in a census tract with a low poverty rate (<10% in 1990). Vouchers served as subsidies for private-market housing and were equal in value to the difference between a rent threshold minus the family contribution to the rent (30% of income, which is identical to the contribution required for public housing). Families remained eligible for vouchers as long as they met the income criteria and other requirements. Census tracts contain between 2500 and 8000 people and were defined by the Census Bureau as being “homogeneous with respect to population characteristics, economic status, and living conditions.” Families that received low-poverty vouchers also received short-term counseling to help with their housing search. After 1 year, these families could use the voucher to relocate to a different tract, regardless of the poverty rate in that tract. In the traditional-voucher group, families were given a standard voucher with no restrictions on where they could reside; they were not provided with counseling. This group was included to distinguish the effects of moving with...
a voucher from the effects of moving to a lower-poverty area. Families in the control group were offered no new assistance.

Randomization was conducted for HUD by Abt Associates with the use of a computerized random-number generator.16 HUD selected sample sizes for power to detect effects on the primary outcomes of the MTO study (i.e., employment, income, and education).17 During the study, Abt Associates adjusted the random-assignment rates of later entrants on the basis of acceptance rates among earlier entrants to equalize the statistical power of different cross-group comparisons.18

**DATA COLLECTION**

MTO applicants completed a baseline survey that contained questions concerning “the people who live with you, your housing, your neighborhood, and your work experiences.”9 Among the few baseline measures related to health was the receipt of Supplemental Security Income, a benefit provided for aged, blind, and disabled persons.

After randomization and completion of the baseline survey by participants, HUD engaged our team to follow the families in order to assess long-term outcomes, including some related to health. Data on outcomes were collected by the Survey Research Center at the University of Michigan from June 2008 through April 2010 — an average of 12.6 years after randomization (range, 10.0 to 15.4). The sample frame included one adult from each family in the group that received low-poverty vouchers and the control group and from a randomly selected two thirds of the families in the traditional-voucher group (this group was undersampled for budgetary reasons).

Candidates for study participation were offered $50 to complete our survey9 and another $25 to undergo height and weight assessments and provide a blood sample. Written informed consent was obtained before the interviews began; the interviews were usually conducted in the participant’s home and were completed in 2 hours. Interviewers were unaware of group assignments. The long-term survey design involved two-phase sampling. In phase 1, interviewers sought to interview everyone in the survey sample frame. Once a response rate of 75 to 80% was reached, the interviewers began phase 2, which involved trying to reach a probability subsample of 35% of the families that could not be surveyed in phase 1.20

**Obesity Assessment**

Height and weight were measured with the use of modified protocols from the University of Michigan Health and Retirement Study.21 Respondents removed heavy outer clothing and items from their pockets and stood with heels and shoulders against a wall. Height was marked on the wall with the use of a rafter angle square and measured to the nearest 0.6 cm (0.25 in.) with a metal tape measure. Weight was measured to the nearest 0.23 kg (0.5 lb) with a digital electronic floor scale (Health o meter [Pelstar], model 800KL), which had a maximum capacity of 180 kg (397 lb).22 When weight or height could not be measured, that reported by the participant was recorded.

**Diabetes Assessment**

Up to five drops of whole-blood capillary samples were collected on specimen-collection paper (Whatman no. 903) with an autoretractable lancet finger stick23 after it had been determined that the participant had no history of a bleeding disorder and was not taking medication that could affect coagulation. Samples were assayed for glycated hemoglobin (HbA1c) at a laboratory with Clinical Laboratory Improvement Amendments certification (FlexSite Diagnostics) with the use of a Roche COBAS Integra immunochemical analyzer that was validated for use with dried blood spots and certified by the National Glycohemoglobin Standardization Program. A single measurement of glycated hemoglobin provides an integrated assessment of a person’s average blood glucose levels over the preceding several months; fasting is not required before a sample is obtained.24

**RESPONSE RATES**

To account for two-phase sampling, we calculated effective response rates.20 For phases 1 and 2, the response rates were calculated as the number of participants with data from each phase, divided by the sum of the number of participants with data and the number with missing data (because the participant declined to provide the data, was incapacitated, had died, or was not contacted) from that phase. Response rates were calculated in accordance with definition RR1w from the American Association for Public Opinion Research.25 Thus, we calculated the overall response rate as $(P_1 \times R_1) + (P_2 \times R_2)$, where $P_1$ and $P_2$ are...
the share of the total sample from phase 1 and phase 2, respectively, and R1 and R2 are the response rates in phase 1 and phase 2, respectively.

OUTCOME MEASURES

We created dichotomous measures for obesity by applying commonly used criteria based on the body-mass index (BMI, the weight in kilograms divided by the square of the height in meters): 30 or more, 35 or more, and 40 or more.26 We defined diabetes as a glycated hemoglobin level of 6.5% or more, as recommended by the American Diabetes Association.27,28

HUD tracked participants’ addresses from baseline to the beginning of long-term follow-up. To illustrate the nature of the change in the neighborhoods where participants lived, we geocoded addresses and linked them to census-tract attributes. In addition, our long-term survey included questions on access to health care, neighborhood safety, and indicators of “collective efficacy” (the social cohesion of the neighborhood).29

STATISTICAL ANALYSIS

We first carried out an omnibus F-test to determine whether differences in baseline characteristics across groups were jointly zero.30 In our main analyses, we used the intention-to-treat principle, comparing differences in average outcomes for controls with those for all members of the two groups receiving vouchers, regardless of whether a family had moved as a result of study participation. The effects on continuous dependent variables were calculated with the use of linear regression, and the effects on dichotomous variables were calculated with the use of logistic regression and are presented as average marginal effects; adjustments were made for baseline covariates to improve precision. All estimates weighted individual participants according to the inverse of the probability of assignment to a particular group, with phase 2 participants also weighted according to the inverse of the likelihood of selection for phase 2 subsampling.30 We calculated Huber–White robust standard errors to adjust for heteroskedasticity.

We also used instrumental-variable methods to try to estimate the association between health and change in residence with the use of a voucher (the complier average causal effect, which in the MTO demonstration project equals the estimated effect of treatment on the treated)31 and to estimate a dose–response effect.32 (For details see Tables 1 through 9 in the Supplementary Appendix, available with the full text of this article at NEJM.org; these tables also provide data on selected means according to study group and compliance status.) For all end points, a two-sided P value of less than 0.05 was considered to indicate statistical significance, with no adjustment for multiple comparisons. Analyses were performed with the use of Stata software, version 11.0, special edition (StataCorp).33

RESULTS

STUDY POPULATION

A total of 4498 families underwent randomization to one of three study groups between 1994 and 1998 (Fig. 1). During the follow-up period, from 2008 through 2010, the effective response rates for data on BMI and glycated hemoglobin level were 84.7% and 70.1%, respectively, for the group that received low-poverty vouchers; 82.8% and 73.7%, respectively, for the group that received traditional vouchers; and 84.4% and 71.3%, respectively, for the control group.

Table 1 presents the baseline characteristics of respondents for whom valid data on BMI or glycated hemoglobin level were collected. (Information on additional baseline characteristics is provided in Table 1 in the Supplementary Appendix.) Most women in the study were unmarried and either black or Hispanic. There were no significant differences in the 57 baseline characteristics between the groups that received low-poverty vouchers or traditional vouchers and the control group (P=0.93 and P=0.35, respectively).

EFFECTS OF THE INTERVENTION ON NEIGHBORHOOD CONDITIONS

Among the families assigned to receive low-poverty vouchers, 48% used the vouchers; among those assigned to receive traditional vouchers, 63% used
5301 Families volunteered

803 Were excluded
693 Were not eligible
4 Were duplicate families
106 Were not headed by women

4498 Underwent randomization

1486 Were included in phase 1 data collection
(P1=83%)
1383 Adults were interviewed
89 Died
14 Declined to participate or were incapacitated
302 Were included in phase 2 data collection
(P2=17%)
56 Were interviewed
7 Died
46 Declined to participate, were incapacitated, or were not contacted
193 Were randomly selected for exclusion

1312 Were assigned to receive traditional voucher
796 Used voucher to move
516 Did not use voucher

1398 Were assigned to control group

1129 Were included in phase 1 data collection
(P1=81%)
1068 Adults were interviewed
51 Died
10 Declined to participate or were incapacitated
269 Were included in phase 2 data collection
(P2=19%)
46 Were interviewed
2 Died
44 Declined to participate, were incapacitated, or were not contacted
177 Were randomly selected for exclusion

1788 Were assigned to receive low-poverty voucher
852 Used voucher to move
936 Did not use voucher

1168 Were included in glycated hemoglobin analysis
1120 Were included in phase 1 sample
(R1=75%)
48 Were included in phase 2 sample
(R2=44%)
620 Were not included in glycated hemoglobin analysis
193 Were randomly selected for exclusion
136 Were not excluded, but not interviewed
36 Were ineligible for glycated hemoglobin analysis
136 Were eligible, but data not collected
99 Had data collected, but not assayed
70.1% effective response rate

1092 Were included in BMI analysis
1046 Were included in phase 1 sample
(R1=93%)
1027 Had measured BMI data
19 Had self-reported BMI data
46 Were included in phase 2 sample
(R2=50%)
44 Had measured BMI data
2 Had self-reported BMI data
306 Were not included in BMI analysis
177 Were randomly selected for exclusion
107 Were not excluded, but not interviewed
22 Had missing BMI data
84.4% effective response rate

168.7% effective response rate 82.8% effective response rate 84.4% effective response rate
70.1% effective response rate 73.7% effective response rate 71.3% effective response rate

Copyright © 2011 Massachusetts Medical Society. All rights reserved.
The association between study-group assignment and neighborhood poverty rate was significant. One year after randomization, the census-tract poverty rate for the group that received low-poverty vouchers was 17.1 percentage points lower than that for the control group, for which the poverty rate was 50.0% (95% confidence interval [CI], −18.6 to −15.6) (Table 2), a change of 1.4 SD in the national census-tract poverty distribution (Table 2 in the Supplementary Appendix). This association between low-poverty vouchers and a reduced poverty rate attenuated over time, in part because families in the control group eventually moved to lower-poverty areas without assistance from the MTO program. Ten years after randomization, the mean poverty rate in the group that received low-poverty vouchers was 4.9 percentage points lower than the rate in the control group, which was 33.0%. Estimates of the effect of treatment on the treated were twice as large as the intention-to-treat estimates for the group that received low-poverty vouchers and were 1.5 times

<table>
<thead>
<tr>
<th>Characteristic</th>
<th>Low-Poverty Voucher (N = 1425)</th>
<th>Traditional Voucher (N = 657)</th>
<th>Control (N = 1104)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age≤35 yr</td>
<td>196 (14.6)</td>
<td>94 (13.5)</td>
<td>163 (14.7)</td>
</tr>
<tr>
<td>36–40 yr</td>
<td>310 (21.5)</td>
<td>156 (23.9)</td>
<td>253 (23.3)</td>
</tr>
<tr>
<td>41–45 yr</td>
<td>347 (23.5)</td>
<td>143 (21.7)</td>
<td>257 (23.2)</td>
</tr>
<tr>
<td>46–50 yr</td>
<td>273 (18.6)</td>
<td>124 (20.5)</td>
<td>194 (17.1)</td>
</tr>
<tr>
<td>&gt;50 yr</td>
<td>299 (21.7)</td>
<td>140 (20.4)</td>
<td>237 (21.7)</td>
</tr>
<tr>
<td>Race or ethnic group‡</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Black</td>
<td>973 (65.0)</td>
<td>393 (63.9)</td>
<td>706 (66.1)</td>
</tr>
<tr>
<td>Other nonwhite</td>
<td>339 (28.1)</td>
<td>194 (27.6)</td>
<td>288 (26.8)</td>
</tr>
<tr>
<td>White</td>
<td>92 (8.5)</td>
<td>52 (7.1)</td>
<td>88 (6.9)</td>
</tr>
<tr>
<td>Hispanic</td>
<td>404 (31.5)</td>
<td>235 (33.0)</td>
<td>346 (30.3)</td>
</tr>
<tr>
<td>Never married</td>
<td>874 (62.6)</td>
<td>395 (63.5)</td>
<td>692 (64.3)</td>
</tr>
<tr>
<td>Age &lt;18 yr at birth of first child</td>
<td>347 (25.1)</td>
<td>163 (28.0)</td>
<td>265 (25.0)</td>
</tr>
<tr>
<td>Employed</td>
<td>368 (27.1)</td>
<td>176 (26.0)</td>
<td>258 (23.9)</td>
</tr>
<tr>
<td>Enrolled in school</td>
<td>216 (16.0)</td>
<td>113 (17.7)</td>
<td>172 (16.9)</td>
</tr>
<tr>
<td>Received high-school diploma</td>
<td>565 (38.3)</td>
<td>233 (34.3)</td>
<td>407 (35.9)</td>
</tr>
<tr>
<td>Received certificate of General Educational Development (GED)</td>
<td>235 (16.2)</td>
<td>124 (18.7)</td>
<td>204 (19.9)</td>
</tr>
<tr>
<td>Receives Supplemental Security Income§</td>
<td>221 (15.9)</td>
<td>107 (17.1)</td>
<td>171 (16.3)</td>
</tr>
</tbody>
</table>

* Numbers are raw, unweighted data. Percentages were calculated with the use of sample weights to account for changes in random-assignment ratios across randomized groups and for subsample interviews. Percentages include imputed values. The sample consisted of women for whom valid data on body-mass index or glycated hemoglobin level were available in the long-term follow-up study. An omnibus F-test failed to reject the null hypothesis that the baseline characteristics reported were the same across study groups. (P = 0.41 for the comparison of the characteristics of the low-poverty-voucher group with the control group; P = 0.77 for the comparison of the traditional-voucher group with the control group.) See Table 1 in the Supplementary Appendix for additional baseline characteristics and related P values.

† The age listed was that calculated as of December 31, 2007, just before the long-term follow-up began in June 2008.
‡ Race categories do not sum to the total number because of missing data (for 21 women in the low-poverty-voucher group, 18 in the traditional-voucher group, and 22 in the control group). A Hispanic person could be a member of any race.
§ Supplemental Security Income is a federal assistance program for aged, blind, and disabled people.
Table 2. Residential Mobility, Poverty Rate, and Census-Tract Characteristics, According to Study Group.*

<table>
<thead>
<tr>
<th>Variable</th>
<th>Control</th>
<th>Low-Poverty Voucher</th>
<th>Traditional Voucher</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean</td>
<td>Intention-to-Treat Estimate (95% CI)†</td>
<td>P Value</td>
</tr>
<tr>
<td>Mean no. of moves‡</td>
<td>2.1</td>
<td>0.57 (0.42 to 0.71)</td>
<td>&lt;0.001</td>
</tr>
<tr>
<td>Poverty rate in census tract (%)§</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Baseline</td>
<td>53.1</td>
<td>–0.37 (–1.23 to 0.50)</td>
<td>0.41</td>
</tr>
<tr>
<td>At 1 yr</td>
<td>50.0</td>
<td>–17.06 (–18.57 to –15.56)</td>
<td>&lt;0.001</td>
</tr>
<tr>
<td>At 5 yr</td>
<td>39.9</td>
<td>–9.78 (–11.25 to –8.31)</td>
<td>&lt;0.001</td>
</tr>
<tr>
<td>At 10 yr</td>
<td>33.0</td>
<td>–4.86 (–6.23 to –3.48)</td>
<td>&lt;0.001</td>
</tr>
<tr>
<td>Mean census-tract characteristics (%)¶</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Poor‖</td>
<td>39.6</td>
<td>–9.14 (–10.26 to –8.02)</td>
<td>&lt;0.001</td>
</tr>
<tr>
<td>Minorities</td>
<td>88.0</td>
<td>–6.23 (–7.58 to –4.89)</td>
<td>&lt;0.001</td>
</tr>
<tr>
<td>Household headed by a woman</td>
<td>54.3</td>
<td>–7.95 (–9.08 to –6.82)</td>
<td>&lt;0.001</td>
</tr>
<tr>
<td>College graduate</td>
<td>16.1</td>
<td>4.49 (3.68 to 5.30)</td>
<td>&lt;0.001</td>
</tr>
<tr>
<td>Respondents reporting collective efficacy (%)**</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>At 4–7 yr</td>
<td>54.0</td>
<td>10.61 (6.46 to 14.76)</td>
<td>&lt;0.001</td>
</tr>
<tr>
<td>At 10–15 yr</td>
<td>58.9</td>
<td>8.20 (4.20 to 12.21)</td>
<td>&lt;0.001</td>
</tr>
<tr>
<td>Respondents reporting feeling safe or very safe on streets near home during the day (%)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>At 4–7 yr</td>
<td>74.9</td>
<td>9.14 (5.77 to 12.52)</td>
<td>&lt;0.001</td>
</tr>
<tr>
<td>At 10–15 yr</td>
<td>80.7</td>
<td>3.70 (0.52 to 6.87)</td>
<td>0.02</td>
</tr>
<tr>
<td>Respondents reporting having at least one friend who graduated from college (%)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>At 4–7 yr</td>
<td>40.8</td>
<td>6.90 (2.63 to 11.17)</td>
<td>0.002</td>
</tr>
<tr>
<td>At 10–15 yr</td>
<td>53.4</td>
<td>6.90 (2.74 to 11.06)</td>
<td>0.001</td>
</tr>
<tr>
<td>Respondents reporting access to local health care services, excluding emergency room (%)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>At 4–7 yr</td>
<td>89.7</td>
<td>–1.35 (–4.13 to 1.43)</td>
<td>0.34</td>
</tr>
<tr>
<td>At 10–15 yr</td>
<td>93.4</td>
<td>–1.36 (–3.49 to 0.77)</td>
<td>0.21</td>
</tr>
</tbody>
</table>

* The analysis sample consisted of women with a valid BMI or glycated hemoglobin measurement. Analyses of number of moves and census-tract characteristics were further limited to participants with valid addresses at baseline and years 1, 5, and 10. The intention-to-treat estimates come from a regression that compares average outcomes across randomly assigned groups, with statistical control for baseline characteristics, which may differ slightly from the difference in raw group means presented here. See the Supplementary Appendix for the sample sizes used.

† Intention-to-treat estimates compare the average of the outcomes for everyone assigned to the intervention group with the average of the outcomes for controls, with adjustment for the set of baseline covariates shown in Table 1 and indicators for survey-sample release (families were randomly selected with regard to the time at which they would first be contacted about participation in the long-term follow-up study), site, and random-assignment periods. The effects on continuous dependent variables were calculated with the use of linear regression; the effects on dichotomous variables were calculated with the use of logistic regression and are presented as average marginal effects.

‡ The total number of moves is the number from the time of randomization (1994 through 1998) to the beginning of long-term follow-up (May 2008).

§ Census-tract characteristics were recorded as of the time when a family lived in the tract and were interpolated with the use of 1990 and 2000 decennial census data and data from the American Community Survey, 2005 to 2009.

¶ Average duration-weighted census-tract characteristics give more weight to tracts in which families spent relatively more time during the study period.

‖ The term “poor‖ is defined as having an annual income below the federal government’s poverty threshold.

** Collective efficacy is defined as the likelihood that adults will take action in response to youth spraying graffiti on local buildings. See Sampson et al. for more details on collective efficacy.²⁹
as large for the group that received traditional vouchers (see the Supplementary Appendix). In an analysis of the 25th percentile of each group’s census-tract poverty distribution (Fig. 2), the differences across groups were even larger.

Study-group assignment was also associated with other neighborhood attributes, including safety and collective efficacy. However, there was no significant association between study-group assignment and access to routine medical care.

**PRIMARY OUTCOMES**

At 10 to 15 years of follow-up, assignment to the low-poverty–voucher group was associated with a decreased risk of extreme obesity and diabetes. Among the women in the control group, 58.6% had a BMI of 30 or more, 35.5% had a BMI of 35 or more, 17.7% had a BMI of 40 or more, and 20.0% had a glycated hemoglobin level of 6.5% or more.

In the intention-to-treat analysis, the women in the group that received low-poverty vouchers, as compared with the women in the control group, had lower prevalences of a BMI of 35 or more (−4.61 percentage points; 95% CI, −8.54 to −0.69; P=0.02, calculated without adjustment for multiple comparisons) and of a BMI of 40 or more (−3.38 points; 95% CI, −6.39 to −0.36; P=0.03), representing relative reductions of 13.0% and 19.1%, respectively (Table 3). The women in the group that received low-poverty vouchers also had a lower prevalence of glycated hemoglobin levels of 6.5% or more, as compared with the women in the control group (−4.31 percentage points; 95% CI, −7.82 to −0.80; P=0.02), a relative reduction of 21.6%.

The differences in outcomes for BMI and diabetes between the group that received traditional vouchers and the control group were not significant at the level of 0.05. The difference in outcomes between the two voucher groups was not significant for any BMI threshold, but there was a trend toward a significant difference in the prevalence of glycated hemoglobin levels of 6.5% or more (P=0.05).

We found no significant differences across subgroups defined by baseline characteristics in post hoc analyses, including baseline age or demonstration site (Tables 6 and 7 in the Supplementary Appendix).

Our dose–response model revealed that adults who spent more time in lower-poverty census tracts had greater improvements in diabetes and BMI outcomes (Table 9 in the Supplementary Appendix). We tested for the presence of nonlinear relationships between neighborhood attributes and these health outcomes, but these tests had low statistical power.

**DISCUSSION**

As compared with the control group, the group with a randomly assigned opportunity to use a voucher to move to a neighborhood with a lower poverty rate had lower prevalences of a BMI of 35 or more, a BMI of 40 or more, and a glycated hemoglobin level of 6.5% or more, representing...
relative reductions of 13.0%, 19.1%, and 21.6%, respectively. The magnitudes of the associations with health were larger still for participants who moved with a voucher that was restricted to use in a low-poverty area than they were for the intention-to-treat estimates for all participants who received the restricted voucher and are consistent with the effect sizes reported in previous observational studies. Because we generated estimates for several BMI cutoff points, our estimates for the associations between program participation and extreme obesity may be marginally significant.

Approximately half the participants randomly assigned to receive low-poverty vouchers used these vouchers, and many of the families in the control group subsequently moved to areas with lower poverty rates. Neither imperfect program compliance nor crossover compromises the internal validity of our intention-to-treat estimates, but these factors may reduce the statistical power of the analyses.

Although we could not reject the null hypothesis that the association of the traditional voucher with obesity is equal to zero or that the association is the same as that for the low-poverty voucher, the difference between the prevalence of a glycated hemoglobin level of 6.5% or more in the group that received low-poverty vouchers and the prevalence in the group that received traditional vouchers approached significance. This finding is consistent with that of previous MTO studies in which outcomes not involving health suggested that changes in the neighborhood environment, rather than the act of moving itself, are responsible for these effects. It is also consistent with our finding that low-poverty vouchers and traditional vouchers had different associations with neighborhood attributes that may affect health (Table 2).

An MTO study published in 2007, which measured self-reported outcomes 4 to 7 years after randomization, showed that the prevalence of obesity (defined as a BMI of 30 or more) among adults assigned to receive low-poverty vouchers was 42.0%, as compared with 46.8% for the control group. Use of self-reported measures raises concerns about the Hawthorne effect and the possibility that the neighborhood environment could affect self-reporting. The 2007 study was not informative with regard to long-term health effects because the problem of fade-out (attenuation in the differences in outcomes between treatment groups and control groups) is pervasive in social experiments, and the study did not show results for the most costly condition associated with obesity — diabetes.

The present study has several strengths, including the use of a large social experiment to overcome concerns about selection bias associated with epidemiologic studies and the collection of physical measurements for health outcomes 10 to
The results of this study, together with those of previous studies documenting the large social costs of obesity and diabetes, raise the possibility that clinical or public health interventions that ameliorate the effects of neighborhood environment on obesity and diabetes could generate substantial social benefits. The mechanisms accounting for these associations remain unclear, but further investigation is warranted to provide guidance in designing neighborhood-level interventions to improve health.

The views expressed in this article are those of the authors and should not be interpreted as those of the Congressional Budget Office, HUD, or any other federal agency or private foundation that provided support for the project.

Supported by grants from HUD (C-CHI-00808), the National Science Foundation (SES-0527615), the National Institute of Child Health and Human Development (NICHD) (R01-HD040444 and R01-HD040444), the Centers for Disease Control and Prevention (R49-CE000906), the National Institute of Mental Health (R01-MH077026), the National Institute on Aging (R56-AG031259 and P01-AG005842-22S1), the National Institutes of Health (to Dr. Lindau) through NORC (SP30 AG012857) and the University of Chicago Center on Demography and Economics of Aging Core on Biomeasures in Population Based Aging Research (IK23AG032870-01A1), and the Institute of Education Sciences at the Department of Education (R305U070006) and by the Population Research Center at the National Opinion Research Center (through a grant [R24-HD051152-04] from the NICHD), the Center for Health Administration Studies at the University of Chicago, the John D. and Catherine T. MacArthur Foundation, the Smith Richardson Foundation, the Spencer Foundation, the Annie E. Casey Foundation, the Bill and Melinda Gates Foundation, and the Russell Sage Foundation.

Dr. Kessler reports receiving fees for board membership from Eli Lilly, Mindsite, and Wyeth-Ayerst; receiving consulting fees from Wellness and Prevention, GlaxoSmithKline, Sanofi-Aventis, Kaiser Permanente, Merck, Ortho-McNeil Janssen Scientific Affairs, Pfizer, Shire US, SRA International, Takeda Global Research and Development, Transcept Pharmaceuticals, Wyeth-Ayerst, and Plus One Health Management; and holding stock in DataStat. Dr. Kessler’s institution, Harvard Medical School, has received grant support from Analysis Group, Bristol-Myers Squibb, Eli Lilly, EPI-Q, Ortho-McNeil Janssen Scientific Affairs, Pfizer, Sanofi-Aventis, Shire US, and Walgreens. Drs. Lindau and Ludwig’s institution, the University of Chicago, has received grant support from PepsiCo. No other potential conflict of interest relevant to this article was reported.

Disclosure forms provided by the authors are available with the full text of this article at NEJM.org.

We thank the members of the research team at the National Bureau of Economic Research, Joe Amick, Ryan Gillette, Jyun Lai, Jordan Marvakov, Matt Sciandra, Fanghua Yang, Sabrina Yusuf, and Michael Zabek, for assisting with the data preparation and analysis; Nancy Gebler (working under subcontract to our research team) of the Survey Research Center at the University of Michigan for leading the data-collection effort for the survey; and Todd Richardson and Mark Shroder of HUD and Kathleen Cagney, Elbert Huang, and Harold Pollack of the University of Chicago for their helpful comments on an earlier version of this article.

15 years after randomization. The study also had the effect of causing a relatively homogeneous group of people to live in a wider range of neighborhoods than is usual for epidemiologic studies. Because the moves led to changes in neighborhoods as defined by the most commonly used markers of neighborhood areas (e.g., tracts and ZIP Codes), the study inherently addresses the potential for measurement error that can result when epidemiologic studies use the wrong geographic proxy for “neighborhood.”

Our study also has several limitations. First, it is possible that the participants for whom outcomes were not available in our long-term study would have differed systematically across the randomized groups in unobservable attributes. Second, our use of a glycated hemoglobin level of 6.5% or more does not account for people with successfully treated diabetes. Third, the baseline surveys conducted by HUD included little information about health. This restriction limits our ability to determine whether the association between a move to a lower-poverty neighborhood and reductions in the prevalence of obesity and diabetes reflects a change in onset or persistence, but it does not affect the internal validity of our intention-to-treat estimates.

A further limitation of the study is the fact that the participants volunteered. More than 90% of the households in the study were headed by a black or Hispanic woman and included children. Among the 1.2 million households in public housing nationwide, 50% are nonwhite and 38% headed by a black or Hispanic woman and included children. Among the 1.2 million households in public housing nationwide, 50% are nonwhite and 38% headed by a black or Hispanic woman and included children. Among the 1.2 million households in public housing nationwide, 50% are nonwhite and 38% headed by a black or Hispanic woman and included children. Among the 1.2 million households in public housing nationwide, 50% are nonwhite and 38% headed by a black or Hispanic woman and included children. Among the 1.2 million households in public housing nationwide, 50% are nonwhite and 38% headed by a black or Hispanic woman and included children.
REFERENCES

33. Stata 11.0 special edition. College Station, TX: Stata, 2009. (http://www stata.com.)