Moving to Opportunity: Why, How, and What Next?

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The opinions expressed in this article are those of the author and do not necessarily reflect those of the U.S. Department of Housing and Urban Development.

Abstract

We discuss the policy background for the Moving to Opportunity (MTO) for Fair Housing demonstration experiment, the innovations in its design and implementation, and a few of the implications for future policy. We explain why a full-blown randomized experiment was necessary, in what ways MTO was unique, and whether the issues posed by concentrated poverty are the same today as when Congress first authorized the experiment.

Introduction

In 2001, Shroder wrote the following in this journal (italics added):

> Moving to Opportunity (MTO) is a demonstration designed to ensure a *rigorous* evaluation of the impacts of helping very low-income families with children to move from public and assisted housing in high-poverty inner-city neighborhoods to *middle-class neighborhoods throughout a metropolitan area.*
>
> Poverty in the United States has become *increasingly* concentrated in high-poverty areas. These concentrated high-poverty, usually urban, and frequently segregated neighborhoods are widely thought to deny their residents opportunities by denying them access to good schools, safe streets, successful role models, and good places to work…
>
> We do not know the extent to which moving the poor out of concentrated poverty neighborhoods, in fact, increases their life chances. Poor people who live in concentrated poverty may differ from other poor people both in ways that can be observed, like race

> or age, *and in ways that may not be observed, like aspiration or persistence*. Any differences in people's outcomes that seem to be associated with the neighborhoods in which they reside might be caused by those neighborhoods—or might be caused by unobserved factors that also affect the sorting of people into different neighborhoods. Only an experiment in which *neighborhoods are allocated randomly* can answer this question. (Shroder, 2001: 57)

In this article, we discuss the policy background for the experiment, the innovations in its design and implementation, and a few of the policy implications, providing particular attention to the items italicized in the preceding passage from 2001.

The problem of concentrated poverty was much in the news in the early 1990s, when MTO began. The U.S. Department of Housing and Urban Development (HUD), in principle, could have conducted this experiment 10 years earlier or 10 years later than it did, but Congress would not have had quite the same motivation to authorize it. In the early 1990s, gangs ruled or contested certain neighborhoods in the inner city, obtaining large illicit revenues from crack cocaine. In a background of high poverty, family disintegration, and social isolation, either the illicit revenues or the effects of the drug itself drove a tenfold increase in the rate of homicide among young African-American men in the late 1980s (Cook, 2009). Journalists like Kotlowitz (1991) and Lehman (1991) produced affecting portraits of brutality, isolation, and hopelessness oppressing another generation of young people. The idea of an underclass barely under the control of the larger society grew markedly after the Los Angeles riots of 1992, associated with the beating of Rodney King.

The subsidized housing stock was in the center of the storm. The National Commission on Severely Distressed Public Housing (1992) estimated that 86,000 public housing units (6 percent of the stock) were severely distressed, based on high incidences of serious crime, physical deterioration, and a constellation of management issues—high vacancy rates, high move-out rates, high refusal rates from tenants offered units, and low rates of rent collection. Much of the distressed stock was located in the center-city areas of blighted "ghetto poverty" that urban planners had no idea how to address (Jargowsky and Bane, 1991).

In certain jurisdictions, federal courts had ordered mobility programs to remedy the racial segregation of much of the public housing stock. One of these court orders came in the case of *Dorothy Gautreaux et al.* vs. *Chicago*.[1] The remedy agreed upon involved a special program to locate rental units in White suburbs or generally improving city neighborhoods and to facilitate their leasing to low-income African-American families. Early followup studies with the clients of the Gautreaux initiative (for example, Kaufman and Rosenbaum, 1992; Rosenbaum, 1991; Rosenbaum et al., 1991) appeared to show striking evidence for neighborhood effects. Low-income African-American families had moved with housing assistance to largely White neighborhoods in both the city and the suburbs, but researchers later observed considerable differences in employment and education outcomes between those who had moved to the suburbs and those who had moved to the city.

[1] The Gautreaux case was first filed in 1966. It resulted in a U.S. Supreme Court decision in the plaintiffs' favor in 1976, and the first of several negotiated settlement agreements occurred in 1981.

The lead plaintiffs' attorney in the Gautreaux case, Alexander Polikoff, approached HUD and its congressional appropriators with this evidence and broached the idea of replicating Gautreaux in one or more other cities. The idea, in general, was acceptable to both Congress and the George Bush Administration when framed in terms of poverty concentration rather than racial segregation.

Indeed, the leading social science thinking of that time (Kain, 1968; Wilson, 1987) emphasized nonracial aspects of the isolation of high-poverty neighborhoods as critical to their apparent negative effects. Kain's spatial mismatch theory pointed to the physical distance between the residents of such neighborhoods and the jobs for which they were qualified. Wilson's theory stressed the departure of the African-American middle class, after the Fair Housing Act of 1968, from ghettos to which open discrimination had previously confined them. As beneficial as that departure might prove to those who had left, Wilson held that the loss of role models and community leaders had severely affected those who remained.

HUD[2] proposed a rigorous experiment testing differences in outcomes between two groups of very low-income families with children, drawn from high-poverty neighborhoods in large metropolitan areas: one group that was offered counseling intended to result in a low-poverty subsidized rental placement (later known as the experimental group, or the low-poverty voucher group), and one group simply received a rent subsidy voucher with no such assistance (later known as the Section 8 group, or the traditional voucher group). Congress approved the study in the Appropriations Acts funding HUD operations in fiscal years 1992 and 1993 and authorized it in Section 152 of the Housing and Community Development Act of 1992, deviating from the HUD language only in narrowing the target population to residents of public and assisted housing in high-poverty census tracts and in mandating both short- and long-run reporting on the results.[3] The change in targeting was significant: it required greater cooperation from public housing authorities (PHAs) and project owners; furthermore, unsubsidized tenants in high-poverty neighborhoods have somewhat different incentives and might have different characteristics from their assisted neighbors.[4] The Appropriations Acts provided $70 million in additional voucher funding to support the demonstration.

HUD implementation of the demonstration required further decisionmaking. First, high- and low-poverty areas had to be defined. The high-poverty criterion—40 percent or more of the census tract population—was readily available from Jargowsky and Bane (1991). HUD chose as a criterion for target tracts that less than 10 percent of the population lived below the poverty line. HUD derived that standard from U.S. Census maps, which showed that very large portions of the landmass in most metropolitan areas were located in tracts meeting that specification, a fact that,

[2] Shroder wrote most of the original legislative language.

[3] HUD Assistant Secretary John Weicher and Senate Appropriations staff member Bruce Katz negotiated the narrower targeting in a meeting in 1992, but neither remembers who proposed it. Both HUD Secretary Jack Kemp and Senator Barbara Mikulski of Maryland, Katz's principal, were committed to the orderly demolition of distressed public housing without reconcentration of poverty in other places.

[4] Giving a housing voucher to someone living in private-market housing is a fundamentally different "treatment" from giving a voucher to someone who is already living in public housing. The latter is already receiving a substantial housing subsidy, although the person has no choice about where to use it; the former is unsubsidized. For some consequences, see Jacob and Ludwig (2012) and Mills et al. (2006).

presumably, would enhance the possibility for successful placements.[5] In a competitive process requiring a joint application from a PHA and a nonprofit provider of counseling services, HUD selected five metropolitan areas as demonstration sites: Baltimore, Boston, Chicago, Los Angeles, and New York.[6]

HUD also had to determine whether the experimental group would face any special limitations in its use of vouchers. In general, geographic limitations on the use of vouchers would expressly violate the Housing Act of 1937. The authorization for the demonstration, however, could validly be interpreted as permitting deviation from that general rule. In 1993, HUD decided that, for purposes of testing the effect of neighborhood on families, it would limit the experimental group vouchers to low-poverty census tracts.[7] Without that decision, MTO would have been a test of the effect of counseling on achieving mobility to low-poverty neighborhoods. Without the constraints on use, however, many fewer low-poverty placements would have been in the sample.

HUD used an existing contract with Abt Associates Inc. to design and implement the experiment.[8] The first and most important issue for the contractor was how many random-assignment groups to create. Although $70 million for vouchers, in 1992 money, was a substantial support for a social science project, the number of vouchers available would be quite limited. The appropriations supported vouchers not for 1 year, which is current practice, but for 5; given inflation, that funding pattern effectively reduced the number of new families assisted by a factor of more than five. Moreover, families with children, in general, require larger units than the voucher program average unit size, and units in metropolitan areas, especially in better neighborhoods, require more subsidy because area rents are higher than the national average. Finally, the authorizing language required that some of the vouchers be used within the regular Section 8 program rules.

Given that HUD could not furnish large numbers of eligible families with vouchers in any case, Abt[9] argued that it was both ethical and scientifically necessary to have a control group that would receive neither a low-poverty nor a traditional voucher. The creation of a control group in addition to the two voucher treatment groups would allow for strong comparisons of the effect of neighborhood on low-income families with children.

Random assignment began in Baltimore in 1994 and concluded in Los Angeles in 1998. We leave the results of the experiment to other articles in this symposium. The remainder of this article concerns the following issues: (1) Was a full-blown randomized social experiment necessary, or

[5] During implementation of the demonstration, families in the experimental group with large numbers of children were allowed to use their vouchers in somewhat higher poverty tracts.

[6] Shroder and Bill Murphy were the authors of the Notice of Funding Availability for the site competition. The selection of the 40-percent poverty standard proved not to be innocuous, because some metropolitan areas did not have large numbers of such tracts or large amounts of public and assisted housing in them. This standard worked against the selection of otherwise strong proposals from Seattle and Fort Worth, among others.

[7] The principal policymaker was Assistant Secretary Michael Stegman.

[8] Shroder and John Goering were the authors of the initial Statement of Work.

[9] Orr was Abt's principal investigator on the project.

could "natural experimental" or "quasi-experimental" studies have produced equally valid results? (2) What was unique about MTO? (3) Where do we stand today regarding the concentration of poverty? Are the issues the same or are they different?

Was a Randomized Social Experiment Necessary?

The use of administrative records about families placed in different neighborhoods on a quasi-random basis has led to claims, both in the Gautreaux program and elsewhere, that the experience of those families constitutes a "quasi-experiment" or a "natural experiment."[10] Unless these phrases are stretched well beyond their intended meaning, this claim is mistaken. Housing agencies in every case used by researchers to date have not kept data on the families who refused the placement, and the families who refuse placement in a "good" neighborhood are probably different from the families who refuse placement in a "bad" one. Consequently, even if the offer of the unit was made randomly, the placement was not.

Mendenhall, DeLuca, and Duncan (2006) reported known characteristics of the Gautreaux households that actually leased up at the time the offer of a city or suburban unit occurred. Exhibit 1 displays the results.

We can, with considerable confidence, rule out the possibilities that the Gautreaux placements were effectively randomized and that the two groups in the exhibit differ only randomly in observable measures. Of eight baseline measures, the study detected significant differences between the two groups in five at the 10-percent level and in three at the 5-percent level in the Aid to Families with Dependent Children sample. It also found significant differences in five measures at the 10-percent level and in four measures at the 5-percent level in the employment sample. The difference in

Exhibit 1

Nonrandomness of Assignment to Treatment in Gautreaux

Baseline Measures	AFDC Sample (N = 793)			Employment Sample (N = 1,258)		
	City	Suburban	p-Value	City	Suburban	p-Value
Number of children	1.98	2.06	.319	1.71	1.90	.005***
On AFDC	0.74	0.74	.911	0.68	0.70	.391
Years since move in 1990	5.97	5.26	.000***	6.62	6.32	.093*
Age of youngest child in 1990	10.42	9.76	.017**	12.97	12.33	.109
Moved from public housing	0.41	0.35	.060*	0.41	0.35	.025**
Mean family income in origin tract ($1,000s)	26.50	28.21	.057*	27.78	30.64	.000***
Percent non-Hispanic African American in origin tract	85.61	84.24	.445	84.66	81.56	.039**
Violent crime per 1,000 people in origin tract	23.47	25.99	.021**	21.99	23.32	.110

AFDC = Aid to Families with Dependent Children.

**Significant difference at the .10 level. **Significant difference at the .05 level. ***Significant difference at the .01 level.*

Source: Mendenhall, DeLuca, and Duncan (2006)

[10] In addition to the Gautreaux studies previously cited, other examples are Oreopoulos (2003) and Schwartz (2010).

observables is itself of no great importance from an evaluation perspective, because one can control for observables in multivariate analysis. Necessarily, however, one cannot control for unobserved differences that might also bias the analysis.

Differences that would normally be unobserved (because information about them is generally not collected) turned out to be important in MTO. Shroder (2002) analyzed the probability of a family in the experimental group making use of the offer of a low-poverty voucher. Exhibit 2 shows that uncertainty about liking a new neighborhood, level of comfort with sending a child to a nearly all-White school, dissatisfaction with the current neighborhood, and preferred distance from that current unit that the family head would like to move are all strongly associated, either positively or negatively, with actual lease up. These factors associated with placement might also be associated with other outcomes, such as employment and education, and could bias studies based on observational data. These attitudinal indicators would not normally be collected and, therefore,

Exhibit 2

Probability of Moving From High- to Low-Poverty Census Tract if Assigned to the Experimental Group

Variable	Coefficient	Standard Error
Metropolitan area vacancy rate	0.247***	0.073
Household size	– 0.258***	0.082
Number of school-age children	0.108	0.081
Number of preschool children	0.084	0.095
Hispanic head	– 0.387***	0.143
Uncertainty about finding an apartment	– 0.056	0.058
Uncertainty about liking a new neighborhood	– 0.194***	0.062
Belongs to a church within 15 minutes of origin project	– 0.045	0.117
Has many friends in current neighborhood	– 0.066	0.162
Comfort with children in nearly all-White school	0.243**	0.113
Housing condition at baseline	0.091	0.074
Dissatisfaction with current neighborhood	0.176***	0.066
Feels very good about moving	0.140	0.145
Preferred distance from origin project	0.181***	0.061
Head attended school last week	0.407**	0.180
Previously applied (Boston only)	0.657***	0.224
Hourly wage	0.013	0.026
Weekly hours of work	– 0.008	0.006
SSI/SSDI/SS survivor benefits	– 0.322**	0.132
Car or license	0.161**	0.075
Years in metropolitan area	– 0.011**	0.005
Intensity of counseling services	0.030**	0.012
Baltimore	– 0.504*	0.265
Boston	– 0.162	0.236
Chicago	– 0.818**	0.350
Los Angeles	– 0.429	0.296
Constant	– 1.629**	0.646

MTO = Moving to Opportunity. SS = Social Security. SSDI = Social Security Disability Insurance. SSI = Supplemental Security Income.

**Significant difference at the .10 level. **Significant difference at the .05 level. ***Significant difference at the .01 level.*

Notes: N = 1,740. Logistic regression: dependent variable = 1 if family leases up. Reference group: New York MTO sample.

Source: Shroder (2002)

researchers could not control for them. Even with all the variables shown, however, the model in exhibit 2 correctly predicts only 65 percent of lease-up outcomes. We simply do not know all the factors that affect residential decisions for a given family, and asserting that what we don't know won't hurt us when we analyze other outcomes would be presumptuous. The value of a large-sample randomization is to ensure that these unmeasured or unmeasurable factors will balance out among the two treatment groups and the control group.

As long as tenants can refuse a unit that is offered to them, and housing authorities do not track the families that refuse units, it is difficult to say whether differences in the realized outcomes of families who accept random placements reflect the effect of neighborhood or merely the differing unmeasured characteristics of the families themselves.[11]

Readers can find additional evidence on the critical importance of randomization in Kling, Liebman, and Katz (2007) and Ludwig and Kling (2007). Their evidence is entirely different from what we present here but is entirely supportive of this point. For example, Ludwig and Kling wrote that applying nonexperimental techniques to MTO data yields evidence that crime is contagious—that is, bad examples from peers in bad neighborhoods tend to cause young men to commit crimes—but that after making use of the experimental design of MTO, they find "no evidence that contagion is as important as much of the previous research would suggest…" (Ludwig and Kling, 2007: 511).

In short, a randomized social experiment was necessary to estimate the effects of neighborhood on very low-income families with children. Nonexperimental techniques cannot solve the fundamental obstacles to valid inference.

What Makes MTO Unique?

MTO was remarkable in (1) the questions it asked, (2) the depth and scope of the effects it analyzed, (3) the range and quality of data sources it tapped, and (4) the long period over which it tracked participating families. Taken together, these attributes make MTO pathbreaking social policy research. In this section, we explore each way that MTO distinguished itself from other social research.

The Research Question

The question that MTO set out to address—What is the effect of neighborhood on low-income families?—is absolutely fundamental to our understanding of the nature of poverty. At least since the 1960s, various social scientists have hypothesized that concentrated poverty engenders a "culture of poverty" that encourages shortsighted, self-defeating behavior that traps low-income families in the underclass (see Harrington, 1962; Lewis, 1959). Although the culture of poverty is not synonymous with spatial concentration of low-income residents, such concentration would at least facilitate the social isolation that is advanced as a hallmark of that culture. Although the concept of a culture of poverty is controversial among social scientists, it is at least plausible that

[11] If housing authorities both randomized placement offers and maintained good records on families who rejected them, the baseline situation would be equivalent to that of a randomized experiment.

the lack of community resources and successful role models in areas of concentrated poverty could undermine the motivation and aspirations of low-income families and limit their access to high-quality education, jobs, and other prerequisites of a healthy, prosperous life.

Nonetheless, the personal attributes of low-income people that may serve as barriers to success—for example, low education, lack of skills and job experience, poor health—would be present regardless of their residential location. The relative contributions of personal characteristics and residential environment to the poverty problem have been an unsolved riddle since the inception of the War on Poverty in the 1960s. Unlike most demonstrations, which seek only to test some specific approach to a problem, MTO set out to help us understand the problem in the only way we could hope to—with a rigorous experimental design capable of separating the effects of neighborhood from the effects of personal characteristics.

MTO did more than that, however. It also sought to measure the effectiveness of the two principal approaches that HUD had used throughout most of its history to address the poverty problem: public housing and rent vouchers. This ambitious combination of basic research and policy research objectives makes MTO unique among the major social experiments of the past 40 years.

The key feature of MTO that made it a test of the effects of neighborhood was restricting the use of the experimental group vouchers to low-poverty neighborhoods. As noted previously, without this restriction, MTO would have been simply a test of the addition of counseling to the traditional Section 8 voucher—a relatively unremarkable, if possibly useful, test much like many other experiments. With the locational restriction, MTO engineered a substantial difference in social environment for statistically matched groups of families—a circumstance that would never occur naturally.

The feature that allowed MTO to test both the effect of neighborhood and the relative effects of Section 8 rent vouchers and public housing was the random three-way assignment to the experimental group with locationally restricted vouchers, the Section 8 group with traditional vouchers, and a control group that remained in public housing. This design enabled the researchers to answer three distinct questions: (1) What is the effect of living in private housing in a low-poverty neighborhood relative to living in public housing in an area of concentrated poverty? (2) What is the effect of living in private housing in a low-poverty neighborhood relative to living in private housing in a substantially higher poverty neighborhood? (3) How effective are Section 8 vouchers, relative to public housing, in improving the lives of low-income people?

The Effects Analyzed and the Methods Used To Measure Them

The hypothesis that residential environment shapes the lives of low-income people implies that neighborhood effects may be felt across virtually every domain of life. A project that sets out to measure the effects of residential environment must therefore measure a very large number of potential effects of intervention. MTO measured the effects of neighborhood in six broad domains: mobility, housing, neighborhood, and social networks; physical health; mental health; economic self-sufficiency; risky and criminal behavior; and educational achievement.

To measure outcomes across all these domains, the study tapped a wide range of data sources: personal interviews with family members, interviewer observations, census data, audio recordings and

physical biomarkers (discussed further in the following sections), administrative data on earnings and arrest histories, data from several national databases on the characteristics of schools attended by MTO youth, and study-administered achievement tests in math and reading.

This problem of compliance with treatment is an issue in most random-assignment studies and, from the outset, was treated as a serious problem in this one. Most personal outcomes are measured with considerable noise—variation not systematically related to measurable factors—and small numbers of lease ups in the treatment groups would threaten the chances that analysts could detect any statistical effect of neighborhood. Abt allocated the families recruited into the sample among the two treatment groups and the control group to minimize the variance of the treatment-control comparisons, using initial lease-up assumptions that were periodically updated.

In all cases, the outcome measures used by the study were state of the art. Survey measures were taken from, or designed to be comparable with, those used in large-scale national surveys. For example, the MTO educational assessments were those used in the fifth- and eighth-grade followup waves of the Department of Education's Early Childhood Longitudinal Study, Kindergarten Cohort. MTO took survey-based measures of physical and mental health largely from the National Health Interview Survey, the Behavioral Risk Factor Surveillance System, the National Survey on Drug Use & Health, the World Health Organization's Composite International Diagnostic Interview, and other widely used survey batteries. The demonstration based measures of risky and criminal behavior on those used in the National Longitudinal Survey of Youth. Use of these established measures not only ensures that the MTO outcomes are based on well-tested interview scales, but it also allows for direct comparison of the MTO sample with national populations.

Perhaps the most innovative feature of the data collection was the use of biomarkers to assess physical health. As in the preceding interim analysis, survey interviewers measured respondents' height and weight during the home visit. In addition, for the long-term study, interviewers measured blood pressure and waist circumference (a better measure of obesity-related health risks than previous height- and weight-based measures) and collected blood spots from finger pricks. These blood samples enabled the researchers to detect the presence of uncontrolled diabetes and to measure high-sensitivity C-reactive protein levels, an important predictor of cardiovascular disease.

The MTO Followup Period

Given the large sample and broad scope of investigation, the MTO followup data collection was of unprecedented length.[12]

MTO enrolled families from 1994 to 1998. Abt conducted the interim impacts evaluation (Orr et al., 2003) followup survey in 2001, 4 to 7 years after random assignment.[13] Orr et al.'s (2003) analysis,

[12] For context, Greenberg and Shroder (2004) reported longer term followups in the following cases: National Supported Work Demonstration, 8 years, sample of 6,600, administrative data only; New York Nurse Home Visitation, 15 years, sample of 400, survey and administrative data; Perry Preschool, 27 years, sample of 123, survey only; Carolina Abecedarian, 18 years, sample of 111, survey only. David Greenberg found these examples. Grinstein-Weiss et al. (2011) reported on an Individual Development Accounts experiment, 10 years, sample of 1,100, survey only.

[13] Other federal agencies and private foundations contributed to the interim data collection and analysis, responding to proposals primarily written by Jeffrey Kling.

based on this survey and a wide array of other data sources, provided what in most demonstrations would have been considered a long-term followup; few experiments are able to follow sample members this long.

HUD recognized, however, that the effects of neighborhood might not only be pervasive but also take some time to develop. Changing the behavior of families from impoverished areas might, for example, take prolonged exposure to a better residential environment. Teenagers who had lived most of their lives in lower poverty environments might also behave very differently than teenagers who had spent their early years in a high-poverty area before moving to a low-poverty neighborhood. For these reasons, one could not confidently conclude that the lack of a statistically significant effect on a given outcome 4 to 7 years after random assignment meant that the effect on that outcome would never be significant. On the other hand, strong interest persisted in determining how long some of the positive effects found in the interim impacts evaluation—for example, the improvements in adult mental health and obesity and the positive effects on girls' risky behavior and criminal activity—would persist. These considerations led HUD, with support from other agencies and foundations, to fund long-term followup data collection and analysis 10 to 15 years after random assignment. The National Bureau of Economic Research (NBER) conducted a final impacts evaluation (Sanbonmatsu et al., 2011) survey in 2009 and 2010, again supplemented by an array of other data sources.

Many low-income households are difficult to interview, either because of high mobility or for other reasons. Both the Abt and the NBER surveys employed a two-stage sampling process to ensure that the people actually reached were representative of the full sample. For example, in the main phase of the final survey, the Michigan Survey Research Center (the survey subcontractor) first obtained interviews with approximately 75 percent of the sample using standard respondent incentives and standard intensity of search by the staff. The researchers then randomly chose 35 percent of the remaining sample and substantially increased the respondent incentives and intensity of staff search for that subsample. Sanbonmatsu et al. (2011) then "weighted up" the respondents from the subsample to obtain the effective response rate; thus, if the first-stage rate was 0.75 and the second-stage rate was 0.6, the effective response rate would be 0.75 + (0.25 x 0.6) = 0.9. The effective response rates obtained in the final impacts evaluation survey were 90 percent for adults and 87 percent for youth (Groves et al., 2004; Sanbonmatsu et al., 2011).

If one considers the length of followup in association with the other elements enumerated and unenumerated previously—the rigor of the research design, the size of the sample (more than 15,000 individuals), the broad array of outcome measures collected, the remarkably high survey response rates, the difficulty and importance of the research and policy questions—MTO emerges as a landmark study.[14]

[14] The continuity of support for the experiment is the more remarkable because it occurred over six administrations. HUD Assistant Secretaries for Policy Development and Research John Weicher, Michael Stegman, Susan Wachter, Al Trevino, Dennis Shea, Darlene Williams, and Raphael Bostic all provided support for the project.

Has the Problem of Concentrated Poverty Changed?

According to the Census Bureau, in 1990, 13.5 percent of the U.S. population lived below the poverty line. In 2010, the figure was 15.1 percent. It is mathematically possible for the concentration of poverty to fall while the poverty rate rises, but the popular perception is that the nation became more, not less, economically segregated over those two decades. That perception has a strong factual basis. "After declining in the 1990s, the population in extreme-poverty neighborhoods—where at least 40 percent of individuals live below the poverty line—rose by one-third from 2000 to 2005–09" (Kneebone, Nadeau, and Berube, 2011: 1).

Certain changes, however, might affect the way that policymakers view the urgency of the problem and their policy levers for affecting it. The crack cocaine epidemic has passed and, with it, some of the surge in homicides, although the danger of violent death lingers in many low-income neighborhoods. As of September 30, 2011, the HOPE VI Program had effected the demolition of 96,797 public housing units, including nearly all of the most notoriously ungovernable and deteriorating projects.

Perhaps these projects contributed to the assumption that the concentration of poverty was anchored in place. Exhibit 3 maps the percentage of poverty by census tract in five metropolitan areas—Boston (3a), an MTO site, and Denver (3b), Houston (3c), Minneapolis (3d), and Nashville (3e), which were not MTO sites—both in 1990 and over a 5-year period from 2003 through 2007.[15] In this exhibit, a tract with less than 10 percent of the population living in poverty is shown as white, a tract with more than 40 percent of the population living in poverty is black, and tracts in between are in graduated shades of gray. The number of tracts in low poverty shrank in all five metropolitan areas. Many of the high-poverty areas became gray, but the total landmass of high-poverty tracts did not decline. The concentration of poverty has often shifted from one place to another. Boston exhibits a reconcentration of poverty in the near southwest. Concentrated poverty has moved with marked centrifugal force in Houston. Some of Denver's concentrations have leapfrogged other neighborhoods in the shift to the east and northeast of the city center. The deep poverty south of the center of Minneapolis has shifted to other locales. Only in Nashville does the pattern of neighborhood poverty appear stable.[16]

If the concentration of poverty is a moving target, lumbering federal policy based on rapidly obsolescing data will have a hard time dealing with it. Orr et al. (2003) repeatedly noted that the variability of the poverty rate over time tends to diminish the strength of the mobility treatment. For example, "Because many [of the experimental group movers] moved to neighborhoods where the poverty rate was increasing between 1990 and 2000, we estimate that only about half of their destinations had poverty rates below 10 percent at the time of the move…" (Orr et al., 2003: viii), whereas, even among members of the control group who stayed in their origin project, about 21 percent were no longer living in tracts with more than 40 percent poverty.

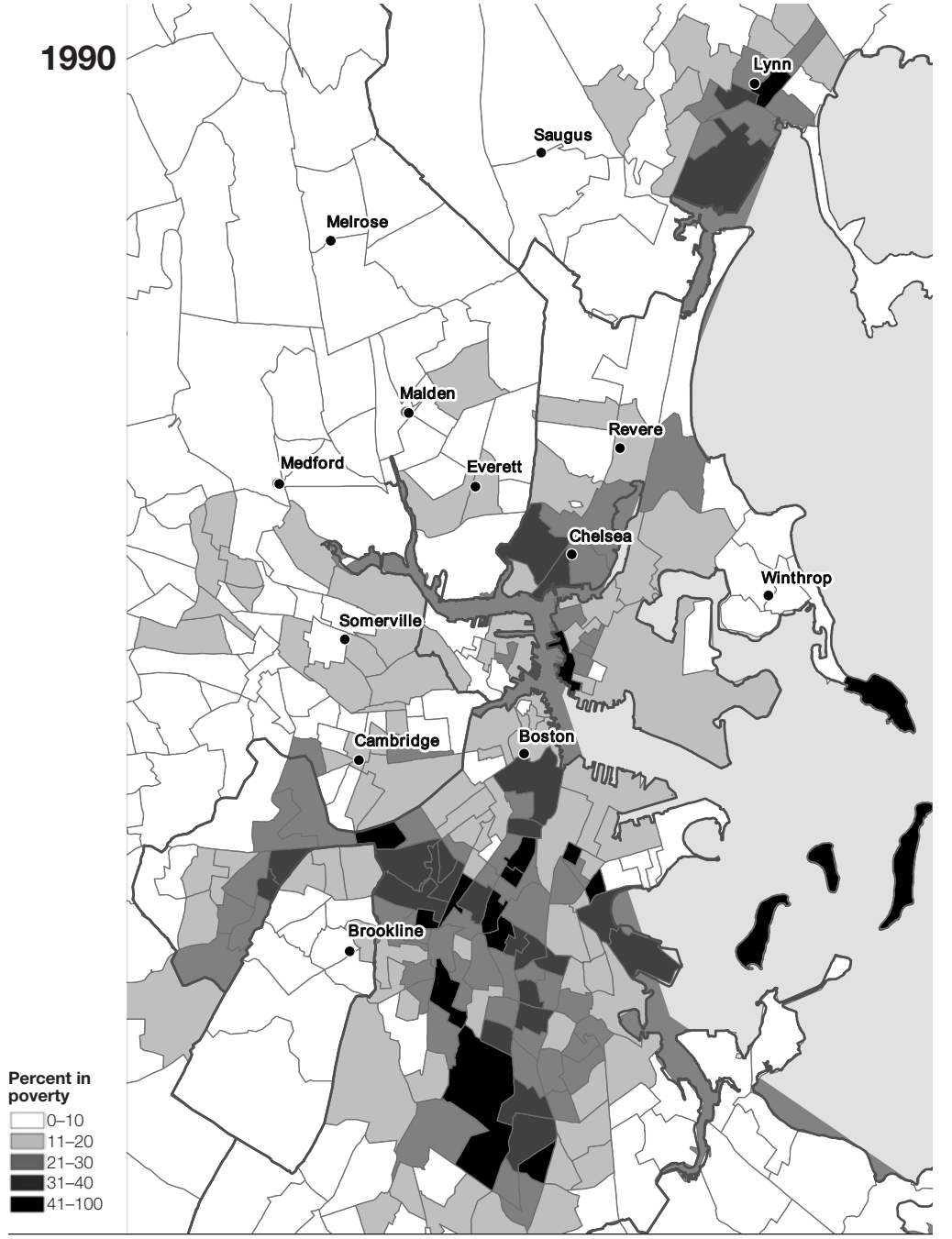
[15] Ron Wilson, a social science analyst in HUD PD&R, created these exhibits. With the elimination of the long-form decennial census, reliable poverty statistics at the census tract level depend on 5-year averages of American Community Survey data.

[16] "The population in extreme-poverty neighborhoods rose more than twice as fast in suburbs as in cities from 2000 to 2005–09" (Kneebone, Nadeau, and Berube, 2011:1).

Exhibit 3

Changes in Concentration of Poverty in Five Metropolitan Areas, 1990–2007

a. Boston, Massachusetts

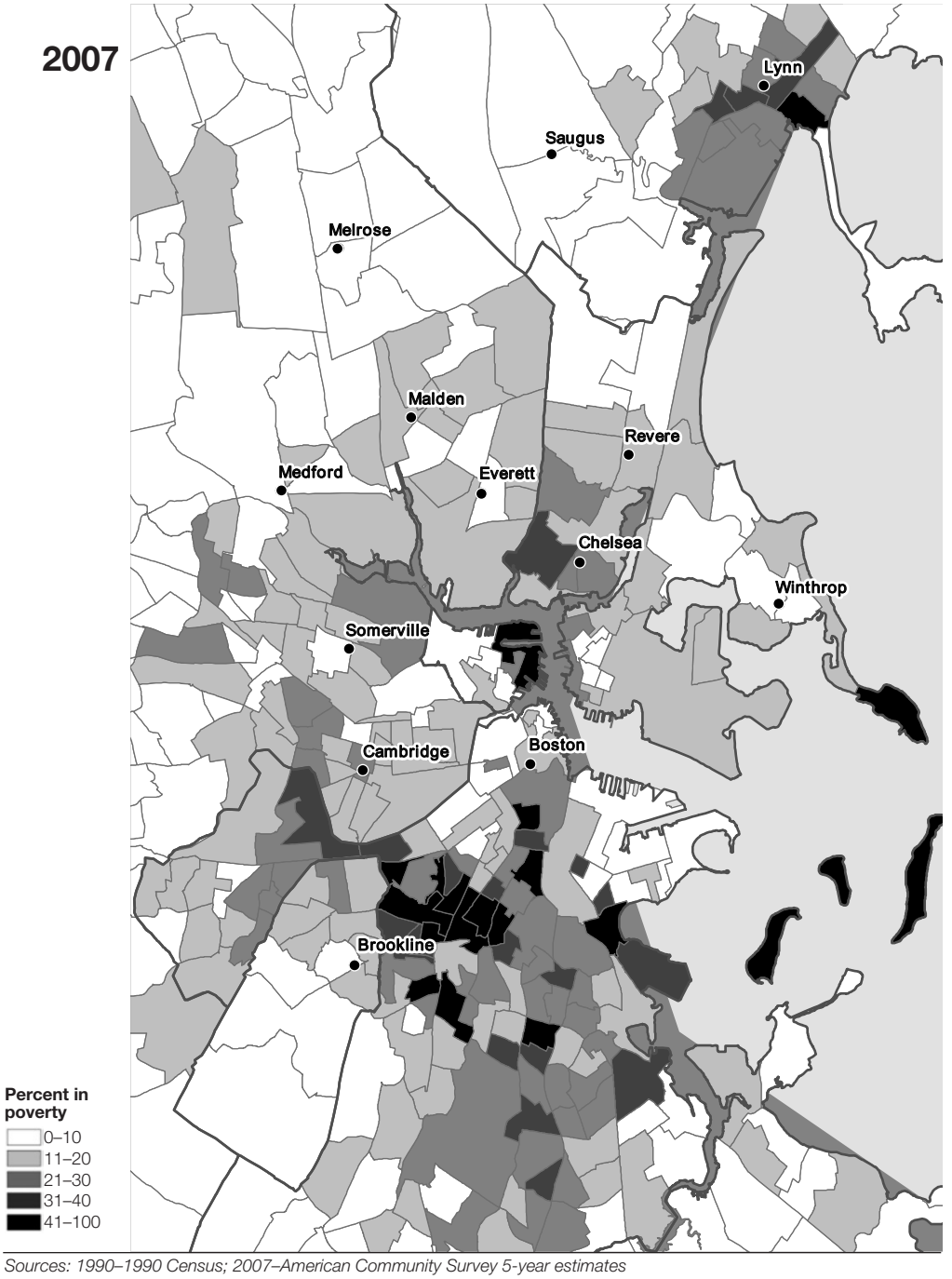


Sources: 1990–1990 Census; 2007–American Community Survey 5-year estimates

Exhibit 3

Changes in Concentration of Poverty in Five Metropolitan Areas, 1990–2007

a. Boston, Massachusetts



Sources: 1990–1990 Census; 2007–American Community Survey 5-year estimates

Exhibit 3

Changes in Concentration of Poverty in Five Metropolitan Areas, 1990–2007

b. Denver, Colorado

Sources: 1990–1990 Census; 2007–American Community Survey 5-year estimates

Exhibit 3

Changes in Concentration of Poverty in Five Metropolitan Areas, 1990–2007

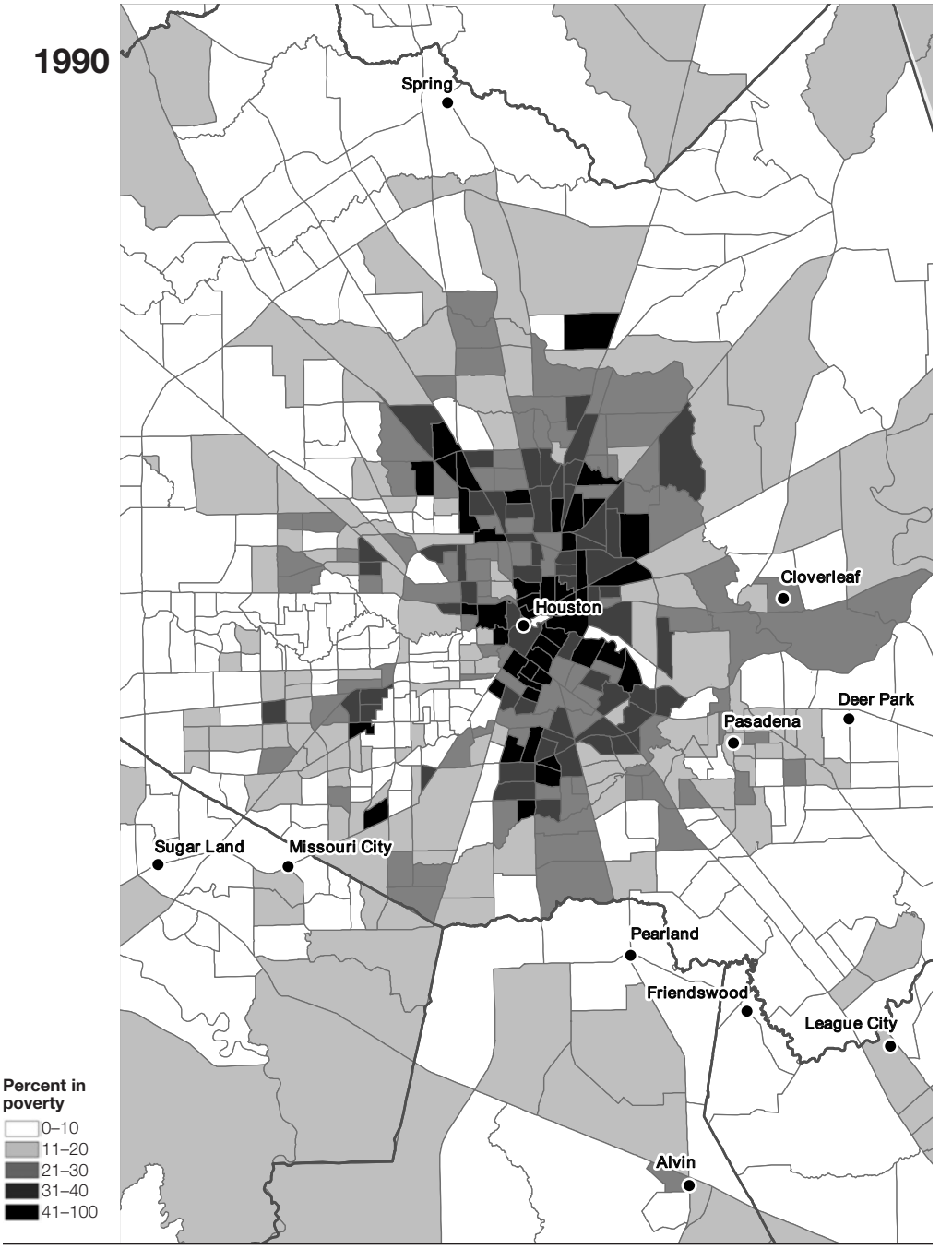
b. Denver, Colorado

Sources: 1990–1990 Census; 2007–American Community Survey 5-year estimates

Exhibit 3

Changes in Concentration of Poverty in Five Metropolitan Areas, 1990–2007

c. Houston, Texas

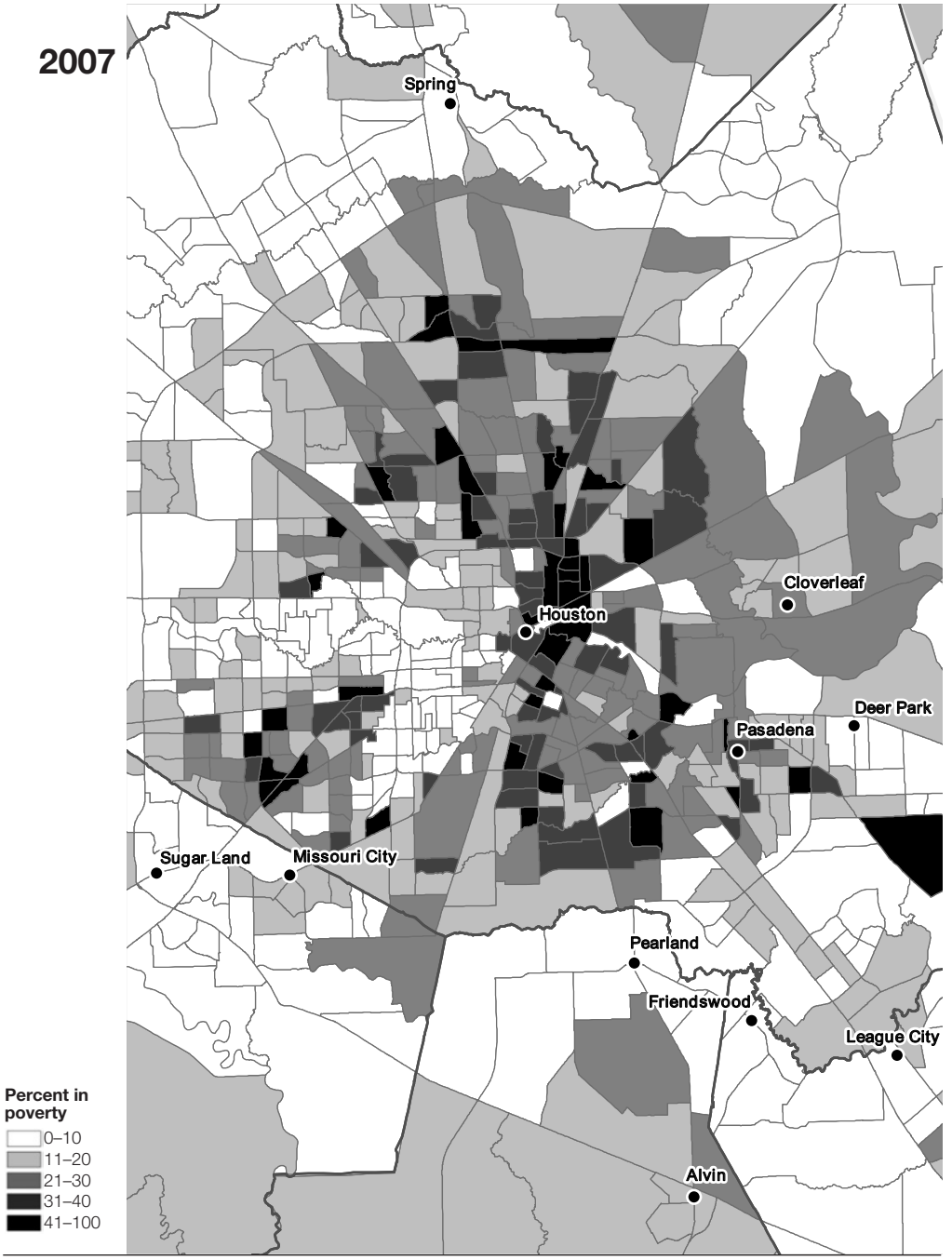


Sources: 1990–1990 Census; 2007–American Community Survey 5-year estimates

Exhibit 3

Changes in Concentration of Poverty in Five Metropolitan Areas, 1990–2007

c. Houston, Texas

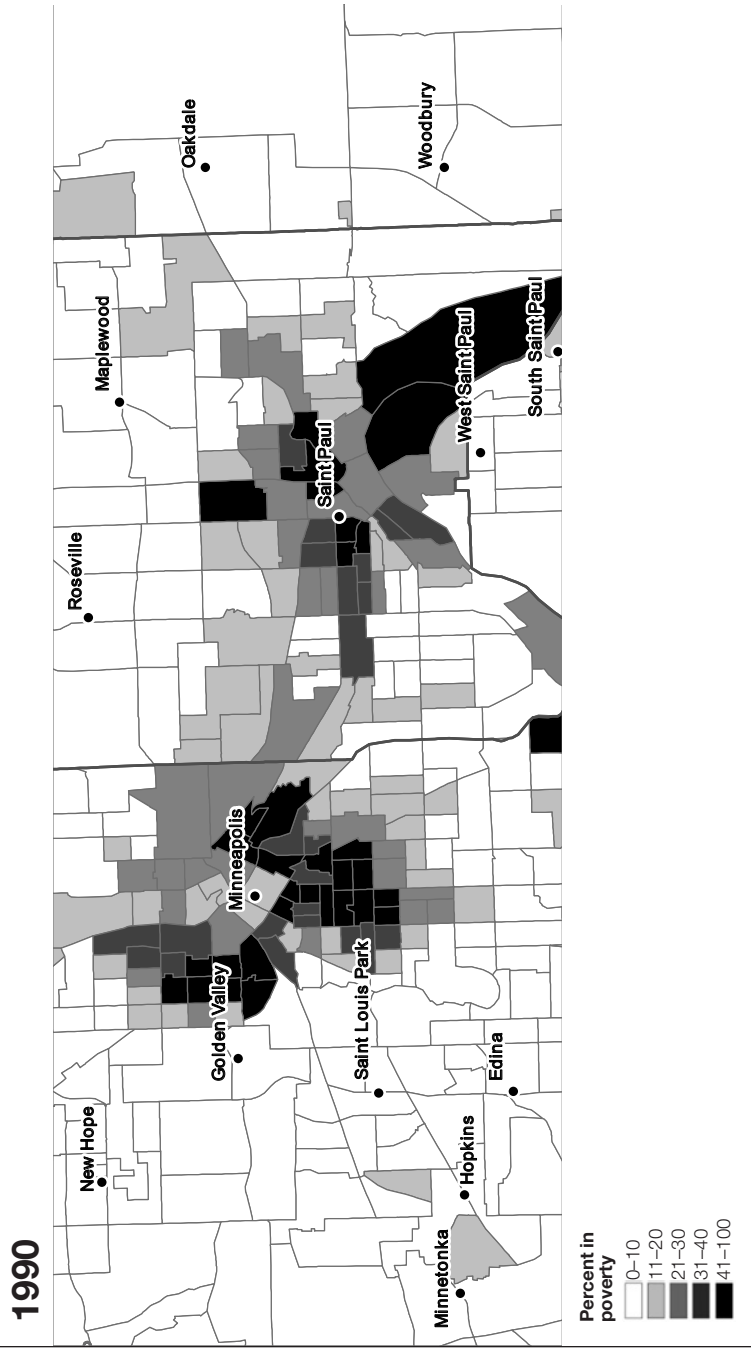


Sources: 1990–1990 Census; 2007–American Community Survey 5-year estimates

Exhibit 3

Changes in Concentration of Poverty in Five Metropolitan Areas, 1990–2007

d. Minneapolis, Minnesota



Sources: 1990–1990 Census; 2007–American Community Survey 5-year estimates

Exhibit 3

Changes in Concentration of Poverty in Five Metropolitan Areas, 1990–2007

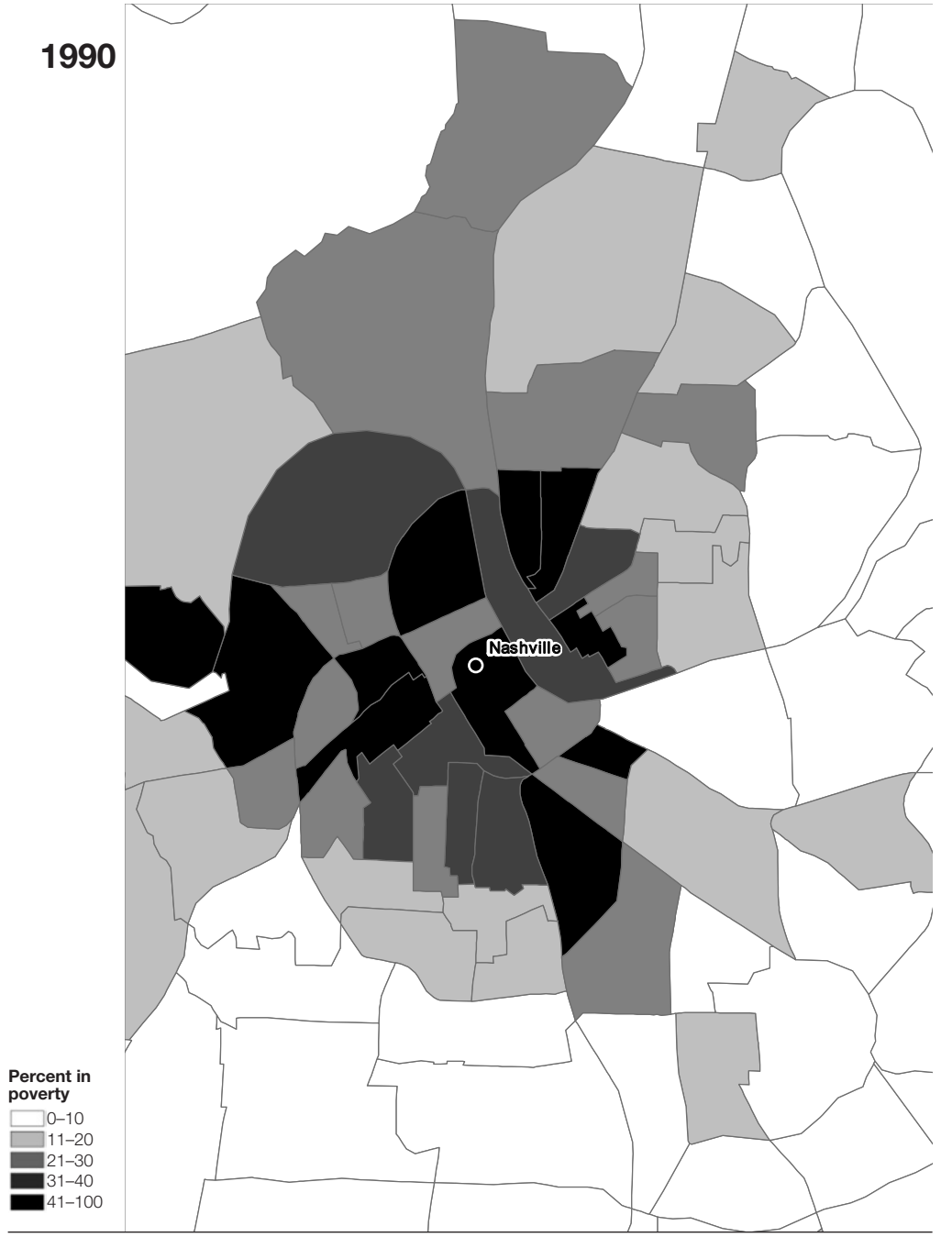
d. Minneapolis, Minnesota

Sources: 1990–1990 Census; 2007–American Community Survey 5-year estimates

Exhibit 3

Changes in Concentration of Poverty in Five Metropolitan Areas, 1990–2007

e. Nashville, Tennessee

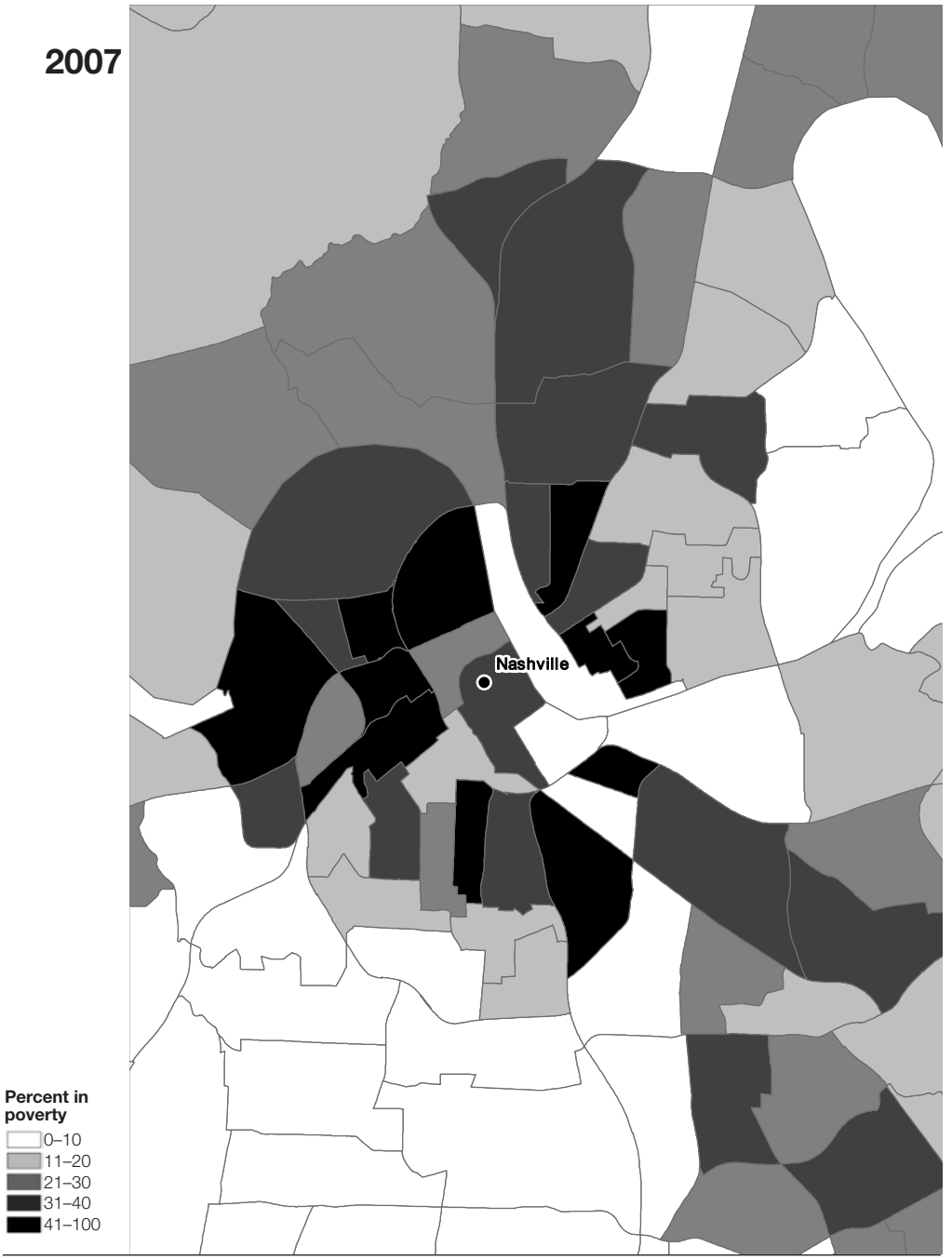


Sources: 1990–1990 Census; 2007–American Community Survey 5-year estimates

Exhibit 3

Changes in Concentration of Poverty in Five Metropolitan Areas, 1990–2007

e. Nashville, Tennessee



Sources: 1990–1990 Census; 2007–American Community Survey 5-year estimates

If the movement of poverty concentrations can undercut the case for a demonstration, the attenuation would likely be larger in a program that was to scale. Over the course of 5 years, the demonstration moved 813 families in the experimental group to low-poverty neighborhoods. Polikoff (2004) describes what a national program to scale would look like:

> Suppose 50,000 housing choice vouchers were made available annually, were earmarked for use by black families living in urban ghettos, and could be used only in non-ghetto locations—say, census tracts with less than 10 percent poverty and not minority impacted. Suppose that the vouchers were allocated to our 125 largest metropolitan areas. Suppose also that to avoid "threatening" any receiving community, no more than a specified number of families (an arbitrary number—say, ten, or a small fraction of occupied housing units) could move into any city, town or village in a year.
>
> If an average of 40 municipalities in each metropolitan area served as "receiving communities," the result would be—using ten as the hypothetical annual move-in ceiling—that 50,000 families each year, or 500,000 in a decade, would move "in Gautreaux fashion." Notably, *the 500,000 moves would equal almost half the black families living in metropolitan ghetto tracts* [emphasis in original]. (Polikoff, 2004: 1)

Note that Polikoff specifies that the receiving neighborhood would have to be "not minority impacted" and low poverty, a combined threshold that most experimental group moves would not have met, and that even with the lower standard in the demonstration just 47 percent of families in the experimental group moved to neighborhoods meeting the target criteria for their treatment. If one assumes that reducing the number of eligible destination neighborhoods would reduce the lease-up rate to just 40 percent, the Polikoff proposal would involve offering 125,000 families per year a location-constrained voucher and appropriate housing counseling services, with 50,000 of those families actually making use of the vouchers. This number would be 307 times the average annual number of experimental group placements.

If we can project forward from the MTO results, however, the dynamics of neighborhood change would imply that perhaps only 25,000 of the 50,000 families would live in tracts that actually met the desired criteria. Further, perhaps 5,000 of the 25,000 would have moved from "metropolitan ghetto tracts" in which poverty had subsided to less than the target level, so that just 20,000 of the 125,000 families receiving the offer would feel the full, desired effect of the program.

The program as described here would likely frustrate or infuriate many of the people involved in it. The quantitative and qualitative evidence from MTO shows that concentrations of poverty are harmful in certain measurable ways and consistently damaging in certain subjective ways. The concentrations do not stay in one place, however, posing a huge and unsolved issue in policy design.

Conclusions

We return here to the questions we posed at the beginning of the article.

Was a full-blown randomized social experiment necessary, or could "natural experimental" or "quasi-experimental" studies have produced equally valid results?

Neither "natural experiments," at least those that have been analyzed so far, nor observational data are capable of answering fundamental scientific questions about the effect of neighborhood on individuals and families. Families assign themselves to neighborhoods, and they do not do so randomly. Even if a PHA randomly offers subsidized placement units in higher and lower income neighborhoods, the families receiving the offers will not randomly accept or reject them, and, to date, researchers have not captured the identities, baseline characteristics, or subsequent experiences of the families who refuse the placements. Differences in their neighborhood choices are correlated with a variety of observable individual characteristics and with a variety of not-usually observed characteristics as well. On the basis of both MTO and previous research, we have every reason to believe that these choices are also correlated with characteristics that have never been measured, which may result in biases that we cannot ordinarily observe, predict, or control for.

What was unique about MTO?

The rigor of the research design, the size of the sample (more than 15,000 individuals), the variety of sites, the length of followup, the broad array of outcome measures collected, the high effective survey response rate, and the difficulty and importance of the research questions both to national policy and to social science made MTO unique.

Where do we stand today regarding the concentration of poverty? Are the issues the same or are they different?

Poverty has increased in the past 20 years, and so has the population living in concentrated poverty. It appears, however, that the demolition of the notorious projects has meant that the locations of concentration are driven less by the locations of project-based assistance than they used to be. It turns out that an implicit and unexamined assumption of the demonstration was that low- and high-poverty tracts would largely retain their low- and high-poverty status over time. Contrary to that unstated expectation, it appears that concentrated poverty often moves from one tract to another, while tracts that initially meet the criteria of neighborhoods of opportunity lose those criteria with surprising speed.

These apparent trends pose a perplexing problem in policy design. The mobility of low-income families who were not part of the demonstration weakened the treatment effect on the families in the treatment groups relative to the control group. There can be little question that the same mobility would have similar consequences for any replication of the experiment or any policy attempting to bring the experiment to scale.

MTO has provided invaluable insight into the ways in which neighborhoods do and do not affect individual outcomes. In the ongoing debate between place-based versus person-based mobility initiatives, however, MTO has yielded no final conclusion. The readers of this symposium are likely to come to widely disparate conclusions about what the logical next steps should be, in either policy or research. HUD welcomes those suggestions.[17]

[17] This sentence is the only one in this article that is a statement of HUD policy.

We have noted some of our own doubts about the value of simple large-scale replication. Neither HUD nor most other government agencies can commit to research demonstrations of this scale and scope on a regular basis. In some ways, MTO is the kind of project that occurs not more than once in a generation. The authors and, we believe, nearly everyone else who has had a hand in MTO feel honored to have had some role in it.

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