Does Housing Mobility Policy Improve Health?

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Abstract

This article summarizes the empirical evidence for the effect of housing mobility policies on health outcomes. Our focus derived from our interest in housing policies that might help reduce health disparities and our finding that, excluding policies concerned with the physical characteristics of housing (e.g., exposure to lead), only housing mobility has been evaluated for its effects on health.

We reviewed 13 articles covering five housing mobility studies and ranked them according to their methodological strength. Although health data have been collected in just a few studies, our review finds that this policy may potentially contribute to improving the health of both adults and children. Yet the empirical evidence is sparse, and only a handful of studies are methodologically sound. To date, the strongest evidence derives from the Moving to Opportunity (MTO) demonstration and from the Yonkers evaluation of scattered-site public housing.

Keywords: Low-income housing; Neighborhood; Urban policy

Introduction

The aim of this article is to summarize the research evidence that U.S. housing mobility policies may help improve health outcomes. First, we present a conceptual framework to distinguish various pathways between housing and health and briefly discuss several recent reviews that have examined some of these pathways. Second, we explain our focus on housing mobility policies by highlighting the role that such policies may play in reducing health disparities. Third, we review and assess the research on the health impact of housing mobility policies. Finally, we discuss the implications of our review for future research and data collection and call for expanded long-term follow-up of housing policy demonstrations to determine their health consequences and the mechanisms by which such housing policies could help improve health.
Housing and health

Historically, housing settlement patterns and conditions have been closely linked to the health of the population. The urbanization process that accompanied the Industrial Revolution in both the United States and Western Europe gave rise to an increase in mortality in urban areas. Infectious disease (e.g., tuberculosis) rates soared because of increased population density and overcrowding (Preston and Haines 1991). Until the 19th century, both housing policy and public health policy in Britain and other European countries were the responsibility of a single government entity: the Ministry of Health (Whitehead 2000). Although these functions were later separated, it is still common for public health practitioners to regard housing as an area for public health advocacy and action (Freeman 2002; Krieger and Higgins 2002; Srinivasan, O’Fallon, and Dearry 2003; Thiele 2002).

Although housing has long been hypothesized to affect health, documenting this relationship has been challenging. Some authors have argued that although the role of housing is part of the conventional wisdom on public health and is presented as a “factual reality that (by implication) precedes and/or transcends the epistemology of social science” (Allen 2000, 49), there is in fact limited research evidence that bad housing causes illness. In an editorial summarizing the findings of a special issue of Housing Studies dedicated to housing and health, Whitehead concluded that “[the articles fell] short of proof, either of the cause and effect or of the value of particular interventions, but in doing so [reflected] the current state of understanding of the relationship between health and housing” (2000, 339).

Pathways between housing and health

Several recent (2000–2002) reviews have examined the empirical evidence that housing has an effect on health. The conclusions are mixed. While some argued that the relationship between housing and health is clearly established (Krieger and Higgins 2002), others found a lack of evidence (Thomson, Petticrew, and Morrison 2001). Summarizing these reviews is difficult, however, because they focused on different pathways between housing and health.

The pathways proposed in the literature can be grouped into three categories:

This is only one possible framework. Alternatively, Dunn (2002) theorizes that housing affects health via three pathways: material (the role of housing affordability and
1. **Housing units as an immediate living environment.** Housing units may be the source of exposure to dangerous physical (poorly designed stairs), chemical (lead), and biological (cockroach allergens) conditions. Most of the literature on housing and health has focused on the effect of this material aspect of housing on specific illnesses (Allen 2000; Dunn 2000).

2. **Housing as an expression of socioeconomic status.** Housing is often examined as a socioeconomic factor, categorized as homelessness and housing tenure (homeownership status). As suggested by Dunn (2000), the research on homelessness generally has a disease focus and emphasizes the gap between the homeless and the rest of society, thus ignoring the possibility of a monotonic housing gradient in health comprising various aspects of housing, such as tenure, housing wealth, mortgage debt burden, and neighborhood quality. Like other indicators of socioeconomic status, homeownership displays an association with health: Namely, people with higher socioeconomic status (homeowners) may have better health outcomes than those with lower socioeconomic status (renters). Some studies have explored the robustness of predicting health with homeownership as a measure of individual socioeconomic advantage at different stages of life (Kuh et al. 2002; Robert and House 1996; Wadsworth, Montgomery, and Bartley 1999). The magnitude of the homeownership effect could be modest at the individual level, above and beyond other socioeconomic characteristics. In some health studies that controlled for the characteristics of the unit, housing tenure was no longer significant (Allen 2000), suggesting that homeownership may be better for health because of the better physical condition of the unit. Others have hypothesized that the psychosocial benefits of homeownership may be mediated by housing and neighborhood conditions (Kearns et al. 2000). Dunn’s (2000) criticism of the literature on homelessness and health can be applied to research on homeownership and health, since this body of work also tends to overlook the complexity of the housing gradient in health.

3. **Locational aspect of housing.** The location of housing may influence health through access to adequate physical and social environments (safe areas for children to play, social support networks) and public and private services (policing, transport) (Macintyre and Ellaway 2000). A growing literature is documenting the health effects of neighborhood environments (Ellen, Mijanovich, and Dillman 2001;
Ellen and Turner 1997; Kawachi and Berkman 2003b; Leventhal and Brooks-Gunn 2000) and segregated housing patterns (Acevedo-Garcia and Lochner 2003; Acevedo-Garcia et al. 2003; Ellen 2000; Williams and Collins 2001). In addition, some studies have tried to disentangle the effect of the quality of the housing unit from the quality of the neighborhood where the unit is located (Kearns et al. 2000).

Next, we summarize the findings of some recent reviews of the empirical evidence on the link between housing and health; we place our review in the context of other recent work that underscores a renewed interest in housing issues in the public health field. However, we do not aim to provide a comprehensive meta-analysis of this literature.

In the *British Medical Journal*, Thomson, Petticrew, and Morrison (2001) examined housing interventions aimed at rehousing and improving infrastructure, defined as heating, double-glazing, and general refurbishment. They found that although most studies demonstrated some health gains, small samples and inadequate control for confounders limited the ability to generalize the findings. In an issue of the *American Journal of Public Health* dedicated to the built environment and health, Saegert et al. (2003) reviewed U.S. interventions to improve health by modifying housing units from 1990 to 2001 and concluded (1) that most studies addressed a single condition (typically lead poisoning, injuries, or asthma) and (2) that while most evaluations reported statistically significant improvements, only 14 percent were regarded as “extremely successful” (1471).

In an issue of the *American Journal of Public Health* dedicated to housing, Krieger and Higgins (2002) concluded that “an increasing body of evidence” (758) has shown that housing as a determinant of health is associated with various conditions through material aspects of the housing unit (such as crowding or dampness), socioeconomic aspects (such as housing deprivation and the availability of subsidies), and locational aspects (such as air quality, noise, and fear of crime).

In a review of the North American and British literature in *Housing Studies*, Dunn (2000) identified three main areas of housing and health research: housing as a social determinant of health (which has been often examined in relation to homelessness), the pathological effects of inadequate housing, and the psychological distress associated with living in inadequate housing. There is sufficient evidence that homelessness is associated with poor physical and mental health and limited

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2 These authors excluded interventions to address lead, urea formaldehyde foam, poor air quality, allergens, and radon. No rationale was provided for these exclusions.
access to health care. The literature on the pathological effects of such housing attributes as high-rise housing, crowding, dampness, and cold is vast, although Dunn (2000) did not present an assessment of the evidence. Research on housing and stress is relatively recent, and the evidence is limited. Regarding housing policy, Dunn (2000) found that there is equivocal evidence on the effectiveness of British policies aimed at enhancing the health of individuals with medical problems by giving them priority for public housing.

In 2002, the Centers for Disease Control and Prevention (CDC) Task Force on Community Preventive Services published a series of literature reviews on policy interventions that could help improve community health (Anderson et al. 2002). Mixed-income housing and housing vouchers were examined. The authors did not find studies of mixed-income housing that met their design criteria for inclusion in their review, but did find sufficient evidence that housing voucher (mobility) policies reduce crime victimization and thus can be recommended. Our review differs from the CDC study in four main respects:

1. We examine housing mobility policies in the context of other housing and health research and other housing policies that might help reduce health disparities.

2. We examine various U.S. housing mobility policies, not just the Moving to Opportunity (MTO) demonstration.

3. We focus on health outcomes, while the CDC also included broad indicators that could have health implications, such as housing quality and neighborhood conditions.

4. In addition to summarizing the evidence, we discuss the design and findings of each study.  

Methodological challenges in housing and health research

Research on housing and health presents several methodological challenges. First, although there have been important advances in conceptualizing the relationship between housing and health—for example, Dunn’s (2002) work on the material, meaningful, and spatial dimensions of housing—for the most part, conceptual frameworks have yet

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3 Our review is also different from the first cross-site analysis of MTO by Goering, Feins, and Richardson (2002), who focused only on MTO and discussed health as part of a series of MTO outcomes, including crime, education, employment, and reliance on public assistance.
to be incorporated into the design of empirical studies. In a review of the literature, Allen (2000) concluded that research on housing and health has been largely atheoretical. Macintyre and Ellaway (2003) similarly assert that neighborhood and health research lacks theoretical development.

Second, it is difficult to establish whether bad housing leads to poor health or vice versa. Most research has focused on cross-sectional statistical associations between housing and health variables. The selection of individuals into housing and neighborhoods presents the largest potential source of bias in nonexperimental studies (which to date constitute the bulk of research in this area), since those with better health may be able to settle in better homes and neighborhoods. Researchers therefore need to construct rigorous research designs to simulate a plausible counterfactual scenario (what would have happened in the absence of the policy) (Orr 1999) in the absence of randomized designs (e.g., quasi-experimental designs, time-series designs). Longitudinal research designs are required to explore the direction of the link between housing and health, as well as to examine the temporal dimensions of these associations: that is, variation in housing/neighborhood environment and accumulation of deprivation across the life course, susceptibility to illness during social or physiological critical periods, and latency periods for the development of illness (Bartley, Blane, and Montgomery 1997). However, few longitudinal studies have shown an association between housing conditions in childhood and adult health (Marsh et al. 2000).

Third, only a few empirical studies have carefully examined specific pathways between housing and health. It has been difficult to isolate a given pathway from other possible mechanisms. For example, it may be hard to separate poor housing conditions from other facets of socioeconomic deprivation such as poverty. Similarly, it may be difficult to disentangle the effect of a housing unit’s condition from locational effects, specifically neighborhood conditions. Multilevel research designs are being used to disentangle these effects (Acevedo-Garcia et al. 2003; Ellen et al. 2001; O’Campo 2003; Subramanian, Jones, and Duncan 2003). Identifying mechanisms is especially important for tying any one specific housing policy to health, since policy often occurs far upstream of health problems in the causal chain of events: Specifically, Williams and Collins (2001) point to such mediators as improved physical or social neighborhood environment.

Fourth, plausible biological pathways between housing and health are well established for some exposure/outcome combinations (exposure to lead and cognitive function in children), but most research has not
addressed the biological links between housing and health. Biomarkers, which are objectively measured indicators of “normal biologic processes, pathogenic processes, or pharmacologic responses” (De Gruttola et al. 2001, 487), are commonly employed to gauge toxic exposure in occupational and environmental health (lead and carbon monoxide), to document smoking status and exposure to secondhand smoke (nicotine levels), and to evaluate the effectiveness of pharmacologic interventions (to reduce blood pressure or to treat AIDS) (Christiani 1996; De Gruttola et al. 2001). Yet the use of biomarkers in housing and health interventions, aside from gauging toxic exposure, is incipient (Loucks et al. 2002). Use of objective measures of physiologic responses to housing may provide unbiased measures for outcomes subject to bias from social desirability or recall or for outcomes that are imperceptible for self-report, such as blood pressure and immune response (Loucks et al. 2002).

**Housing and health disparities**

Understanding the source of health disparities and addressing them along racial/ethnic and socioeconomic lines has become an increasingly important topic in public health research and practice (Geronimus 2000; Williams 1997; Williams and Collins 2001; Williams and Harris-Reid 1999). Sociological research suggests that housing factors may underlie disparities in socioeconomic outcomes, which have been widely recognized as upstream determinants of health (Berkman and Kawachi 2000). For example, at the household level, limited access to homeownership contributes to substantial differences in wealth and its intergenerational accumulation across racial/ethnic groups. In turn, wealth disparities are linked to educational and labor market disparities (Conley 1999). There is also concern that housing segregation along racial lines and the spatial concentration of poverty separate racial/ethnic groups into vastly different neighborhood environments. In turn, the negative effects imposed on individuals living in highly disadvantaged neighborhoods are the source of disparities in socioeconomic outcomes (Altshuler et al. 1999; Iannotta, Ross, and the National Research Council 2002). Disparities in both individual-level and neighborhood-level socioeconomic outcomes may underlie health disparities along racial/ethnic lines (Williams 1996, 1997; Williams and Collins 2001).

Social epidemiology research is beginning to show the effect of neighborhood characteristics independent of individual-level socioeconomic factors on racial/ethnic disparities in health outcomes—for example, low birth weight (Buka et al. 2003). Research has also documented a
positive association between housing segregation and mortality and morbidity among blacks. Although most of these studies have some methodological weaknesses, the evidence to date consistently suggests that blacks in urban areas characterized by higher levels of housing segregation experience higher mortality and morbidity (Acevedo-Garcia and Lochner 2003; Acevedo-Garcia et al. 2003; Williams and Collins 2001). However, research on both neighborhoods and segregation in relation to health outcomes has been atheoretical, and the identification of causal mechanisms has only recently become an area of exploration (Acevedo-Garcia and Lochner 2003; Acevedo-Garcia et al. 2003; O’Campo 2003). As noted by O’Campo, “We still lack a clear picture of the intervention and policy implications of this body of work” (2003, 9).

In this article, we focus on the evidence that housing mobility policy may serve as a tool to improve health outcomes. Our previous work has focused on the locational aspects of housing—segregation and neighborhood environment—as a social determinant of health disparities. Therefore, we are particularly interested in whether housing policy interventions could help address health disparities. Most of the empirical evidence to date addresses the physical condition of units, and housing interventions and policies aimed at improving those conditions are widely recognized as tools to protect public health (Mood 1986). However, while policies that address the locational aspects of housing are just beginning to be recognized as public health tools, in the past three decades housing policy has been increasingly informed by concerns about racial and income segregation. We set out to examine the research evidence that public policies that have influenced or tried to correct the spatial and social isolation of low-income and racial/ethnic minority groups have also had an impact on their health outcomes. Recent social policy experiments have begun to document how, by enhancing people’s neighborhood environment, housing mobility policy might benefit health.

**Housing policy and health**

Housing is an important social policy domain. It represents one of the largest monthly costs for many Americans, and equity from

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4 Although public health research has recognized a possible link between segregation, disadvantaged neighborhood environments, and ill health, health research is only beginning to address the role of public policy in health inequalities. For example, Hart et al. (1998) showed that metropolitan areas characterized by metropolitan governance had lower black mortality rates than areas characterized by municipal fragmentation and that housing segregation mediated the effect of metropolitan governance on black male mortality.
 homeownership is the most common source of household wealth (U.S. Bureau of the Census and Economics and Statistics Administration 2001). The government, at all levels, intervenes in the housing market by providing, subsidizing, or constraining the purchase, location, or rental of housing in a variety of ways, including tax policy, zoning laws, and provision of housing for low-income residents.

In this literature review, we first sought to identify those federal housing policies that appear to have the greatest potential for improving the health of low-income individuals and, thus, for reducing health disparities. On the basis of empirical evidence for various aspects of housing as a social determinant and for racial/ethnic and socioeconomic disparities in housing outcomes, we identified three such policies: homeownership promotion, enforcement of antidiscrimination laws, and housing mobility. We then focused on housing mobility, since it appeared to be the only one that has been empirically evaluated for its effects on health. Additionally, this focus is warranted because such policies are embedded in rental assistance programs, which constitute the largest (in monetary terms) and the most politically viable vehicle for addressing the needs of the U.S. low-income population facing the most severe housing problems and concentration of poverty (Sard 2000).

Homeownership policy

There are several pathways through which homeownership may help improve health. First, homeownership is the main source of wealth for most American households. Access to home equity provides owning households with greater financial stability and enables them to benefit from other investment opportunities. In 2001, the median net wealth for U.S. homeowner households was about $171,800, compared with only about $4,810 for renter households (Joint Center for Housing Studies 2003). Potentially, promoting homeownership and thus accumulation of wealth could be an effective way to enhance health. A vast body of social epidemiology literature suggests an inverse gradient

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5 Our focus does not derive from an assessment that federal policy is more influential than state or local housing policy, but rather from an organizing principle: We decided to start our review work by focusing first on national policy. Future reviews may address state and local housing policy. However, our examination of the literature suggests that there may be only limited instances of state and local policies that have been evaluated for their effect on health with the criteria we established.

6 A related (and controversial) issue is whether redistribution of wealth could help reduce health inequalities (Deaton 2002).
between socioeconomic status (income, education, and occupation) and physical and mental health outcomes (Barker et al. 1993; Hattersley 1999; Kaplan and Salonen 1990; Lundberg 1993; Marmot et al. 1991; Power et al. 1997; Syme 1987; Syme and Berkman 1976). Although studies of the association between wealth, including homeownership, and health are relatively scarce because of lack of data on accumulation of wealth, the available evidence suggests a positive association between wealth and health (Deaton 2002) and between homeownership and health (Dunn 2000; Kuh et al. 2002). Some studies found that homeownership is associated with a lower probability of limiting long-term illness (Wadsworth, Montgomery, and Bartley 1999); others found that children living in houses owned by their parents experienced lower rates of behavioral, emotional, and cognitive problems and recommended promoting homeownership as a strategy for reducing such problems (Boyle 2002; Haurin, Parcel, and Haurin 2002).

Further, although the evidence is limited, homeownership may confer psychological benefits (Dunn and Hayes 2000; U.S. Department of Housing and Urban Development [HUD] 1995) and may contribute to better neighborhood physical and social environments (HUD 1995), which in turn may have health-promoting effects. However, the burden of a home mortgage can also detrimentally influence health. For instance, in a nationally representative British panel study, Nettleton and Burrows (1998) documented that when homeowners experienced problems paying their mortgage or fell into arrears, their subjective well-being declined compared with those who had no mortgage problems, even after adjusting for health problems, change in health status, income, and employment since baseline. Thus, the fear of losing one’s home results in worse health despite the fact that relatively few people actually experience repossession (Nettleton and Burrows 1998).

Although it would be important to incorporate health impact assessment into policy demonstrations aimed at promoting homeownership among racial/ethnic minority and low-income households, to our knowledge no such studies exist. Also, current policy goals assume that “homeownership is not an option for everyone”—that some low-income households may be better served by rental assistance (HUD 2002, 7).

**Fair housing policy**

Enforcing antidiscrimination laws is another key component of housing policy. Housing discrimination and antidiscrimination policy could also potentially affect health outcomes through at least two mechanisms. First, since discrimination limits homeownership among minorities
(Yinger 1995), antidiscrimination policy could enhance minority homeownership and accumulation of wealth and, as a result, minority health. Further, health research suggests that institutional housing discrimination (Gee 2002), as well as perceptions of racism and discrimination, could have detrimental effects on cardiovascular and mental health, for example (Harrell, Hall, and Taliaferro 2003; Jackson et al. 1996; Krieger and Sidney 1996; Williams and Neighbors 2001; Williams, Neighbors, and Jackson 2003). However, to our knowledge, housing antidiscrimination policy has not been evaluated for its effects on health.

**Housing mobility policy**

In the United States, rental assistance is the main form of government housing assistance for low-income households (HUD 2002). Moreover, rental assistance, in the form of housing vouchers, is increasingly being examined as a tool for deconcentrating poverty (Sard 2000). Besides providing income assistance to low-income households, the Section 8 program by design could help reduce the concentration of poverty as it facilitates housing mobility. Participants are free to choose any housing that meets program requirements and are not limited to units in subsidized housing projects, which are generally located in distressed areas (Sard 2000; Turner, Popkin, and Cunningham 2000). Analyses of neighborhood characteristics of participants in different federal housing programs have found that Section 8 certificates and vouchers “reduce the probability that families live in the most economically and socially distressed areas” (Newman and Schnare 1997, 703). Only about 15 percent of Section 8 recipients live in neighborhoods with poverty rates in excess of 30 percent. By contrast, about 54 percent of public housing residents live in such neighborhoods (Newman and Schnare 1997; Turner, Popkin, and Cunningham 2000). Therefore, the Section 8 program may benefit participants both through an effect on household income and through an effect on neighborhood quality. Both are likely to have a beneficial effect on health outcomes.
Empirical evidence on housing mobility policy and health

**Literature search methods**

We began by casting a wide net for studies of any housing policies that might have been evaluated for health, but at the same time, we also imposed rigorous selection criteria. We searched eight medical, social science, policy, and housing databases for articles from 1974 through April 2002: MEDLINE, SocioFile, PsychINFO, the Social Sciences Citation Index, the HUD USER Bibliographic Database, ProQuest, PolicyFile, and JSTOR (which contains 35 journals for geography, population studies, and sociology). We used the following keywords in our search: public policy or policy, housing, and health. We also reviewed references within relevant articles, Internet sites, and housing journals: *Health & Place* (1995–2002); *Housing Studies* (1998–2002); *Housing Theory and Society* (October 1999–August 2000); *Urban Studies* (1995–2002); *Housing Policy Debate* (1993–2002); *Journal of Housing Research* (1993–2002); *Social Science and Medicine* (1995–2002); and the Princeton Moving to Opportunity, HUD, and Urban Institute Web sites.

To be included in our review, studies must have fulfilled the following conditions: (1) empirically evaluated a housing policy in the United States, (2) included at least one relevant health outcome, and (3) had a comparison group.

Relevant health outcomes were included as (1) direct health outcomes (individually measured mental or physical health, including experience of violence), (2) hazardous health behaviors (substance abuse), and (3) medical care. Although several studies included indicators that could be proxies for health (number of school absences, unsafe housing or neighborhood conditions), we have not included such indirect outcomes here.

We found three main bodies of research: housing mobility policy, housing policy for the homeless/mentally ill, and lead standards policy. We chose to focus on housing mobility as a potential tool for reducing health disparities. We did not find any studies on the health effects of homeownership promotion policy and antidiscrimination policy, the two

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7 Some of the earliest work on the effect of housing mobility and health focused on the severe distress that accompanied forced relocation because of urban renewal projects. For instance, 46 percent of women relocated from Boston’s West End experienced long-term grief reactions; 57 percent of these women reported that they continued to have symptoms of depression two years after relocation (Fried 1963).
other areas we had identified as possible vehicles for reducing health disparities. We excluded housing standards on lead, because, unlike housing mobility, they are already widely recognized as a tool for protecting health, and housing policy for the homeless or mentally ill population, because this research overlooks the possibility of a more complex housing gradient in health (Dunn 2000).8

Housing mobility studies that met our inclusion criteria were documented in an evaluation form developed at the CDC by Briss et al. (2000). Using the guidelines developed by these authors, we reviewed each of the studies and ranked them by the strength of their study designs. Our evaluation was based on a number of criteria, including study population and design, sampling methods, validity and reliability of exposure and outcome measures, data analysis, attrition, authors’ interpretation of results, and presence of other major threats to validity.

**Findings**

As shown in figure 1, our search yielded 479 citations, which were reviewed for relevance to our criteria. A total of 94 articles were kept for closer inspection, and of these, 13 articles qualified for inclusion. These articles discussed five residential mobility policies: the MTO experiment, the Gautreaux program, the Yonkers scattered-site public housing program, the Section 8 program, and the Cincinnati Special Mobility Program (table 1). We will first summarize the effects of each policy on health (table 2) and then summarize the evidence.

**Gautreaux program**

The Gautreaux Assisted Housing Program, the first and best known of the litigation-initiated residential mobility housing programs, resulted from a lawsuit that was filed by Dorothy Gautreaux and other public housing residents against HUD and alleged racial discrimination in the administration of the Chicago public housing program. In 1976, the Supreme Court ordered the city of Chicago to racially desegregate its public housing and offer placements to black families in private units located in other parts of the metropolitan area. Public housing residents and those on the waiting list received Section 8 certificates and housing counseling to move either within the city or to one of more than 115 predominantly white suburbs.

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8 Additionally, housing policy for the homeless/mentally ill population is a local policy area, while our focus here is on federal policies.
HUD evaluated the Gautreaux program in 1979, comparing Gautreaux participants with eligible nonparticipants and Section 8 participants (Peroff et al. 1979). Rosenbaum later used three surveys to evaluate the effects of the Gautreaux program: One was a cross-sectional survey of adults examining employment outcomes, and the other two were pre/post surveys of the same sample of household heads and their children, analyzing social and educational outcomes over seven years (1994, 1995). Suburban movers were significantly less satisfied with their medical care than city movers, and the latter experienced greater satisfaction at follow-up, compared with baseline (Rosenbaum and Popkin 1990). The 1979 HUD evaluation of Gautreaux reported that access to medical care was more difficult for participants than for eligible nonparticipants or Section 8 recipients; access was also more difficult for participants after the move than it had been before (Peroff et al. 1979).

The Gautreaux evaluation results are subject to selection bias, however. First, in the 1979 survey, those selecting into the program were different from eligible nonparticipants and Section 8 recipients for reasons relating to their participation as well as reasons affecting their health.
Table 1. Summary of Housing Mobility Studies with Health-Related Outcomes, Classified by Methodological Strength and Suitability of Study Design

<table>
<thead>
<tr>
<th>Source</th>
<th>Policy</th>
<th>Health-Related Outcomes Included in the Review</th>
<th>Classification of Outcome Type</th>
<th>Methodological Strength*</th>
<th>Suitability of Study Design*</th>
</tr>
</thead>
<tbody>
<tr>
<td>Katz, Kling, and Liebman 2001</td>
<td>MTO Boston</td>
<td>Crime victimization; injuries, accidents, or poisonings; asthma attack requiring medical attention; 7-item parent report of child behavior problems; adult self-reported health; mental health (reported major depressive symptom); calm/peaceful; been to medical care for regular checkup or immunization</td>
<td>Health, mental health, crime victimization, health care utilization</td>
<td>1 (good)</td>
<td>Greatest</td>
</tr>
<tr>
<td>Leventhal and Brooks-Gunn 2003b</td>
<td>MTO New York</td>
<td>Mental health for children (problem behavioral outcomes) and mothers</td>
<td>Mental health</td>
<td>2 (fair)</td>
<td>Greatest</td>
</tr>
<tr>
<td>Fauth, Leventhal, and Brooks- Gunn 2002</td>
<td>Yonkers</td>
<td>Adolescent exposure to violence and access to cigarettes and/or alcohol</td>
<td>Crime victimization and access to illegal health-harming substances</td>
<td>3 (fair)</td>
<td>Greatest</td>
</tr>
<tr>
<td>Rosenbaum and Popkin 1990</td>
<td>Gautreaux</td>
<td>Satisfaction with medical care</td>
<td>Health care</td>
<td>3 (fair)</td>
<td>Greatest</td>
</tr>
<tr>
<td>Leventhal and Brooks-Gunn 2003a</td>
<td>MTO New York</td>
<td>Adolescent use of cigarettes and alcohol; child mental health; child health; exposure to violence</td>
<td>Health, health behavior/substance abuse, mental health, crime victimization</td>
<td>3 (fair)</td>
<td>Greatest</td>
</tr>
</tbody>
</table>
**Table 1. Summary of Housing Mobility Studies with Health-Related Outcomes, Classified by Methodological Strength and Suitability of Study Design (continued)**

<table>
<thead>
<tr>
<th>Source</th>
<th>Policy</th>
<th>Health-Related Outcomes Included in the Review</th>
<th>Classification of Outcome Type</th>
<th>Methodological Strength*</th>
<th>Suitability of Study Design*</th>
</tr>
</thead>
<tbody>
<tr>
<td>Briggs 1997 and the Yonkers Family and Community Project</td>
<td>Yonkers Scattered-Site Public Housing</td>
<td>Mother’s reported mental health symptoms (depression, anxiety), substance abuse (alcohol, drugs), experience of violence during the past 12 months</td>
<td>Mental health, health behavior/substance abuse, crime victimization</td>
<td>4 (fair)</td>
<td>Greatest</td>
</tr>
<tr>
<td>Meyers et al. 1995</td>
<td>Section 8</td>
<td>Growth of child (height and weight for age), as a proxy for nutrition</td>
<td>Health/child development</td>
<td>5 (limited)</td>
<td>Least</td>
</tr>
<tr>
<td>Rosenbaum and Harris 2001</td>
<td>MTO Chicago</td>
<td>Experience of violence or crime</td>
<td>Crime victimization</td>
<td>5 (limited)</td>
<td>Greatest</td>
</tr>
<tr>
<td>Peroff et al. 1979</td>
<td>Gautreaux</td>
<td>Health care access easy or difficult</td>
<td>Health care</td>
<td>5 (limited)</td>
<td>Greatest</td>
</tr>
<tr>
<td>Hanretty, McLanahan, and Pettit 2003</td>
<td>MTO Los Angeles</td>
<td>Medical care access, regular place for care</td>
<td>Health care</td>
<td>5 (limited)</td>
<td>Greatest</td>
</tr>
<tr>
<td>Rosenbaum 1994</td>
<td>Gautreaux</td>
<td>Victimization: Whether youth were hurt by other students in school</td>
<td>Crime victimization</td>
<td>6 (limited)</td>
<td>Greatest</td>
</tr>
<tr>
<td>Rosenbaum 1995</td>
<td>Gautreaux</td>
<td>Victimization: Whether youth were hurt by other students in school</td>
<td>Crime victimization</td>
<td>6 (limited)</td>
<td>Greatest</td>
</tr>
<tr>
<td>Fischer 1991</td>
<td>Cincinnati Special Mobility Program</td>
<td>Being attacked/personal experience of violence or robbery for self, child, or neighbor; receipt of health care benefits; satisfaction with health care</td>
<td>Crime victimization, health care</td>
<td>8 (limited)</td>
<td>Least</td>
</tr>
</tbody>
</table>

* Source for rating system: Briss et al. 2000
Table 2. Summary of Significant ($p < 0.05$) Short-Term Health, Health Care, and Health Behavior Outcomes Associated with Housing Mobility Policies

<table>
<thead>
<tr>
<th>Policy</th>
<th>Source</th>
<th>Age Group</th>
<th>Comparison Made</th>
<th>Health Indicator</th>
<th>Significant among which group? (Intent to Treat Results Reported)</th>
<th>Interpretation</th>
</tr>
</thead>
<tbody>
<tr>
<td>Boston MTO</td>
<td>Katz, Kling, and Liebman 2001</td>
<td>Children 6 to 15</td>
<td>MTO group (also known as the experimental group) and/or Section 8 group vs. control group, at follow-up</td>
<td>Any injuries, accidents, or poisonings over the past 6 months requiring medical attention</td>
<td>MTO</td>
<td>The MTO group had fewer incidents than the control group.</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Children 6 to 15</td>
<td>MTO group (also known as the experimental group) and/or Section 8 group vs. control group, at follow-up</td>
<td>Fraction of 7 behavior problems among boys</td>
<td>Both MTO and Section 8</td>
<td>Both experimental groups had fewer behavior problems than the control group.</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Adults</td>
<td>MTO group (also known as the experimental group) and/or Section 8 group vs. control group, at follow-up</td>
<td>Overall health good or better</td>
<td>Both MTO and Section 8</td>
<td>Both experimental groups were healthier than the control group.</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Adults</td>
<td>MTO group (also known as the experimental group) and/or Section 8 group vs. control group, at follow-up</td>
<td>Calm and peaceful “a good bit of the time” or more often in the past 4 weeks</td>
<td>Both MTO and Section 8</td>
<td>Both experimental groups were calmer than the control group.</td>
</tr>
<tr>
<td>New York MTO</td>
<td>Leventhal and Brooks-Gunn 2003b</td>
<td>Adults</td>
<td>MTO and/or Section 8 group vs. control group, at follow-up</td>
<td>Distress/anxiety symptoms in the past month</td>
<td>MTO</td>
<td>The MTO group had fewer symptoms than the control group.</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Children 8 to 18</td>
<td>MTO and/or Section 8 group vs. control group, at follow-up</td>
<td>Problem behavior: anxious/depressed</td>
<td>MTO</td>
<td>The MTO group had fewer symptoms than the control group.</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Boys 8 to 18</td>
<td>MTO and/or Section 8 group vs. control group, at follow-up</td>
<td>Problem behavior: anxious/depressed</td>
<td>MTO</td>
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<tr>
<td>New York MTO</td>
<td>Leventhal and Brooks-Gunn 2003b</td>
<td>Boys 8 to 18</td>
<td>MTO and/or Section 8 group vs. control group, at follow-up</td>
<td>Problem behavior: dependent</td>
<td>Both MTO and Section 8</td>
<td>Both experimental groups had fewer symptoms than the control group.</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Children 8 to 13</td>
<td>MTO and/or Section 8 group vs. control group, at follow-up</td>
<td>Problem behavior: headstrong</td>
<td>Section 8</td>
<td>The Section 8 group had fewer symptoms than the control group.</td>
</tr>
<tr>
<td></td>
<td>Leventhal and Brooks-Gunn, 2003a</td>
<td>Children 8 to 13</td>
<td>MTO and/or Section 8 group vs. control group, at follow-up</td>
<td>Problem behavior: unhappy, sad, or depressed in the past 6 months</td>
<td>Section 8</td>
<td>The Section 8 group reported lower levels of unhappiness/sadness than the control group.</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Children 8 to 13</td>
<td>MTO and/or Section 8 group vs. control group, at follow-up</td>
<td>Problem behavior: need to be near adults in the past 6 months</td>
<td>MTO group</td>
<td>The MTO group reported lower levels of problem behavior than the control group.</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Children 8 to 13</td>
<td>MTO and/or Section 8 group vs. control group, at follow-up</td>
<td>Problem behavior: too fearful or anxious in the past 6 months</td>
<td>MTO group</td>
<td>The MTO group reported lower levels of problem behavior than the control group.</td>
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<td></td>
<td></td>
<td>Boys 8 to 18</td>
<td>MTO and/or Section 8 group vs. control group, at follow-up</td>
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<td>Boys 8 to 18</td>
<td>MTO and/or Section 8 group vs. control group, at follow-up</td>
<td>Problem behavior: depend too much on others in the past 6 months</td>
<td>Section 8 group</td>
<td>The Section 8 group reported lower levels of problem behavior than the control group.</td>
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<tr>
<td>New York MTO</td>
<td>Leventhal and Brooks-Gunn, 2003a</td>
<td>Girls 11 to 18</td>
<td>MTO and/or Section 8 group vs. control group, at follow-up</td>
<td>Teen girl herself used alcohol in the past year</td>
<td>MTO group</td>
<td>The MTO group reported higher use of alcohol than the control group.</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Boys 11 to 18</td>
<td>MTO and/or Section 8 group vs. control group, at follow-up</td>
<td>Teen report that peers used cigarettes in the past year</td>
<td>MTO group</td>
<td>The MTO group reported peers used cigarettes less than the control group.</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Adults</td>
<td>MTO, Section 8, or control group, comparing baseline with the follow-up value</td>
<td>Exposure to violence (mugged, threatened, beaten, or stabbed/shot)</td>
<td>All three groups (MTO, Section 8, and controls)</td>
<td>Each group experienced a decline in exposure to violence from baseline to follow-up.</td>
</tr>
<tr>
<td>Los Angeles MTO</td>
<td>Hanratty, McLanahan, and Pettit 2003</td>
<td>Adults</td>
<td>MTO, Section 8, or control group, comparing baseline with the follow-up value</td>
<td>No significant results reported</td>
<td>—</td>
<td>—</td>
</tr>
<tr>
<td>Chicago MTO</td>
<td>Rosenbaum and Harris 2001</td>
<td>Any member of the household</td>
<td>Comparison of baseline versus follow-up value for either the MTO or Section 8 group (authors did not interview the control group at follow-up)</td>
<td>Being threatened with a knife or gun</td>
<td>Both MTO and Section 8 groups</td>
<td>There was a significant decline in the proportion of the group threatened with a knife or gun, within each group, since baseline.</td>
</tr>
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<td>Policy</td>
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<td>Chicago</td>
<td>Rosenbaum and Harris 2001</td>
<td>Any member of the household</td>
<td>Comparison of baseline versus follow-up value for either the MTO or Section 8 group (authors did not interview the control group at follow-up)</td>
<td>Being beaten or assaulted</td>
<td>Both MTO and Section 8 groups</td>
<td>There was a significant decline in the proportion of the group beaten or assaulted, within each group, since baseline.</td>
</tr>
<tr>
<td>Yonkers</td>
<td>Briggs and Yonkers Family and Community Project 1997</td>
<td>Adult mothers</td>
<td>Movers (those who participated in the policy) compared with stayers (control group of nonparticipants) at follow-up</td>
<td>Depression symptoms during the past 30 days</td>
<td>Movers</td>
<td>Movers reported fewer symptoms than stayers.</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Adult mothers</td>
<td>Movers (those who participated in the policy) compared with stayers (control group of nonparticipants) at follow-up</td>
<td>Problem drinking during the past 30 days</td>
<td>Movers</td>
<td>Movers reported less problem drinking than stayers.</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Adult mothers</td>
<td>Movers (those who participated in the policy) compared with stayers (control group of nonparticipants) at follow-up</td>
<td>Marijuana use during the past 12 months</td>
<td>Movers</td>
<td>Movers reported less drug use than stayers.</td>
</tr>
</tbody>
</table>
Table 2. Summary of Significant ($p < 0.05$) Short-Term Health, Health Care, and Health Behavior Outcomes Associated with Housing Mobility Policies (continued)

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<td>Yonkers</td>
<td>Briggs and Yonkers Family and Community Project 1997</td>
<td>Adult mothers</td>
<td>Movers (those who participated in the policy) compared with stayers (control group of nonparticipants) at follow-up</td>
<td>Experience of a violent or traumatic event during the past 12 months (victim of physical assault/abuse, of sexual assault/rape, of life-threatening mugging, of large fire or serious accident; witness to serious assault or violent death; child or spouse died)</td>
<td>Movers</td>
<td>Movers reported lower levels of violent or traumatic events than stayers.</td>
</tr>
<tr>
<td>Yonkers</td>
<td>Fauth, Leventhal, and Brooks-Gunn 2002</td>
<td>Youth 8 to 18</td>
<td>Movers (those who participated in the policy) compared with stayers (control group of nonparticipants) at follow-up</td>
<td>Access to cigarettes and alcohol</td>
<td>Movers</td>
<td>Movers have less access to cigarettes and alcohol than stayers do.</td>
</tr>
<tr>
<td>Gautreaux</td>
<td>Rosenbaum 1994</td>
<td>Youth</td>
<td>Those who moved to the suburbs compared with those who moved within the City of Chicago, at follow-up</td>
<td>No significant indicators reported</td>
<td>—</td>
<td>—</td>
</tr>
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<td></td>
<td>Rosenbaum 1995</td>
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<td>Gautreaux</td>
<td>Rosenbaum and Popkin 1990</td>
<td>Adults</td>
<td>Those who moved to the suburbs compared with those who moved within the City of Chicago, at follow-up</td>
<td>Satisfaction with medical care</td>
<td>City movers</td>
<td>City movers experienced higher satisfaction at follow-up than those who moved to the suburbs.</td>
</tr>
<tr>
<td>Adults</td>
<td>Comparison of follow-up with baseline, for each group (city and suburban movers)</td>
<td>Adults</td>
<td>Comparison of follow-up with baseline, for each group (city and suburban movers)</td>
<td>Satisfaction with medical care</td>
<td>City movers</td>
<td>City movers experienced higher satisfaction at follow-up, compared with baseline.</td>
</tr>
<tr>
<td>Peroff et al.</td>
<td>1979</td>
<td>Adults</td>
<td>Those who participated in the Gautreaux program (both city and suburban movers) compared with several control groups (including those using Section 8 and those in public housing)</td>
<td>Access to health clinic/hospital is easier/same/more difficult compared with the old neighborhood</td>
<td>Gautreaux participants</td>
<td>Access is more difficult for Gautreaux participants than for controls in Section 8 or eligible nonparticipants. (Note: No statistical test was reported, so it was not clear whether groups differed significantly.)</td>
</tr>
<tr>
<td>Adults</td>
<td>Those who participated in the Gautreaux program (both city and suburban movers) compared with several control groups (including those using Section 8 and those in public housing)</td>
<td>Adults</td>
<td>Those who participated in the Gautreaux program (both city and suburban movers) compared with several control groups (including those using Section 8 and those in public housing)</td>
<td>Health clinic/hospital services are better/same/worse compared with the old neighborhood</td>
<td>Gautreaux participants</td>
<td>Services are not the same (higher reports of “better” and “worse”) for Gautreaux participants after the move, compared with those in Section 8 or eligible nonparticipants. (Note: No statistical test was reported, so it was not clear whether groups differed significantly.)</td>
</tr>
</tbody>
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Table 2. Summary of Significant ($p < 0.05$) Short-Term Health, Health Care, and Health Behavior Outcomes Associated with Housing Mobility Policies (continued)

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<td>Gautreaux</td>
<td>Peroff et al. 1979</td>
<td>Adults</td>
<td>Those who participated in the Gautreaux program (both city and suburban movers) compared with several control groups (including those using Section 8 and those in public housing)</td>
<td>Getting to health clinic/hospital is easy/fairly easy/difficult</td>
<td>Gautreaux participants</td>
<td>Access is more difficult for Gautreaux participants than for those in Section 8 or eligible nonparticipants. (Note: No statistical test was reported, so it was not clear whether groups differed significantly.)</td>
</tr>
<tr>
<td>Section 8</td>
<td>Meyers et al. 1995</td>
<td>Children less than 3 years old</td>
<td>Those receiving Section 8 vouchers compared with those who were on the Section 8 waiting list, at one point in time (cross-sectional)</td>
<td>Low child growth (mean z-score of weight/age based on National Center for Health Statistics growth reference norms)</td>
<td>Section 8 participants</td>
<td>Children of adults receiving the Section 8 subsidy reported to have higher mean weight/age growth ratios (better growth) than children of adults on the waiting list for the subsidy.</td>
</tr>
<tr>
<td>Cincinnati Special Mobility Program (SMP)</td>
<td>Fischer 1991</td>
<td>Adults</td>
<td>Before versus after the move, among those in the SMP program</td>
<td>Being attacked/personal experience of violence or robbery for self, child, or neighbor</td>
<td>SMP participants</td>
<td>Movers report a reduction in violence since the move.</td>
</tr>
</tbody>
</table>
### Table 2. Summary of Significant (\( p < 0.05 \)) Short-Term Health, Health Care, and Health Behavior Outcomes Associated with Housing Mobility Policies (continued)

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<tr>
<td>Cincinnati Special Mobility Program (SMP)</td>
<td>Fischer 1991</td>
<td>Adults</td>
<td>Before versus after the move, among those in the SMP program</td>
<td>Job provides medical care benefits (among those employed)</td>
<td>SMP participants</td>
<td>Movers report that they are more likely to have benefits after they moved (compared with before). (Note: No statistical test was reported, so it was not clear whether groups differed significantly.)</td>
</tr>
<tr>
<td>Adults</td>
<td>Those in the SMP program compared with controls in public housing</td>
<td>Adults</td>
<td>Job provides medical care benefits (among those employed)</td>
<td>SMP participants</td>
<td>Those in the SMP program were more likely to have medical care benefits than the control group in public housing. (Note: No statistical test was reported, so it was not clear whether groups differed significantly.)</td>
<td></td>
</tr>
</tbody>
</table>

**Notes:** In this table, we applied the conventional significance value \( p < 0.05 \) for inclusion of health-related outcomes associated with housing mobility policy, so that we could be consistent across studies by applying the same cutoff value. Many studies reported several values more stringent than 0.05, although only a few reported significance values at \( p < 0.10 \). Housing mobility was associated with several health-related outcomes at a \( p \) value between 0.05 and 0.10 “marginally significant,” for all the outcomes that follow. In Boston, MTO treatment children (age 6 to 15) exhibited marginally fewer asthma attacks requiring medical attention in the past 6 months and marginally fewer personal crimes (personal threats, robberies, or attacks) (Katz, Kling, and Liebman 2001). Respondents in the Chicago MTO treatment group experienced a reduction in being stabbed or shot in their neighborhood from baseline to follow-up (Rosenbaum and Harris 2001). Youth movers in the Yonkers program experienced lower odds of being assaulted (Fauth, Leventhal, and Brooks-Gunn 2002). Young children whose family received the Section 8 voucher had higher standardized weight/height growth ratios than children whose families were on the Section 8 waiting list (Meyers et al. 1995). Parents in the New York MTO treatment group experienced lower levels of depressive symptoms than controls; MTO treatment children aged 8 to 13 experienced lower levels of anxious/depressed symptoms, and both treatment groups aged 8 to 13 displayed lower levels of dependent problems than controls; Section 8 youth aged 8 to 18 reported lower levels of dependency and headstrong problems than controls (Leventhal and Brooks-Gunn 2003b). There were marginally significant lower levels of problem behaviors/mental health for these New York groups: MTO treatment boys 8 to 18 “unhappy, sad, or depressed”; MTO treatment boys “depend too much on others”; Section 8 boys “need to be near adults”; Section 8 younger children (8 to 13) “need to be near adults”; Section 8 children (8 to 18) “depend too much on others” (Leventhal and Brooks-Gunn 2003a).
Does Housing Mobility Policy Improve Health?

Second, if relocation was not assigned randomly, agents’ perceptions or clients’ preferences introduced selection bias. Although researchers asserted that participants were assigned to their neighborhood regardless of their locational preference (Rosenbaum 1995), other evidence suggests that program managers may have screened families they believed were better suited for moving to the suburbs (Goering 2003). Third, the pre/post study of children experienced substantial loss to follow-up (40 percent attrition). In addition to limiting the power to detect meaningful effects (since the sample size was only 114 to begin with), the widely cited beneficial educational and social outcomes accruing to suburban children may have resulted from selection if those who dropped out of the study differed systematically from those who remained (Briggs 1997). Fourth, the Gautreaux program did not include a control group (both groups did move), so we cannot glean what might have happened to participants in the absence of the policy. We conclude therefore that the Gautreaux studies have methodological problems.

Section 8

The Section 8 program gives qualified low-income participants a rental subsidy for use in private apartment units. The program has been evaluated over the past 25 years in many comprehensive studies, including several federally funded policy experiments (Kennedy and Leger 1990; Peroff et al. 1979). Yet most of these studies did not include outcomes relevant to our literature review. We did, however, find one study that measured health. Meyers et al. (1995) found that children under age three from families on the Section 8 waiting list had decreased growth (weight for age) compared with children in families receiving vouchers, indicating that the Section 8 policy might prevent undernutrition. However, this study is subject to substantial validity threats because of its convenience sampling method and cross-sectional design.

Yonkers scattered-site public housing

In addition to tenant mobility programs, scattered-site public housing programs have been implemented as a result of court decrees to racially desegregate concentrated areas of poverty in central cities. In 1986, the District Court ordered Yonkers, N.Y., to racially desegregate its public housing, since 26 of the city’s 27 public housing units were located in the same high-poverty, racially isolated, southwest quadrant of the city. The court ordered the construction of 200 units of scattered-site public housing in low-poverty areas, 7 of which were built between 1990 and 1993 (Briggs and the Yonkers Family and Community Project 1997;
Peterson and Williams 1995). Tenants were selected by lottery once families met the qualification criteria.

The Yonkers research team plans a 15-year study, including measurement of physical and mental health effects. The team also created a comparison group through a snowball sampling method, beginning with referrals from movers—they gave the team the names of acquaintances who stayed behind in the public housing projects. Although the team found few demographic differences between the movers and the stayers (Briggs and the Yonkers Family and Community Project 1997), this does not rule out selection bias resulting from an invalid comparison, if the controls might not have qualified or might not have wanted to apply for scattered-site public housing for reasons that would make them not comparable to those who applied.

Briggs and the Yonkers evaluation team documented recent violent victimization, depression and anxiety symptoms, and substance abuse among mothers (Briggs, Darden, and Aidala 1999; Briggs and the Yonkers Family and Community Project 1997). Compared with stayers, the mover group reported a lower prevalence of depression symptoms, of problem drinking, of marijuana use, and of violent or traumatic events (Briggs and the Yonkers Family and Community Project 1997). Among youth, movers reported lower access to alcohol and cigarettes than stayers did (Fauth, Leventhal, and Brooks-Gunn 2002).

The strength of this study resides in its valid and reliable health measures and its prospective design with a concurrent comparison group. Yet the uncertain appropriateness of the control group (and the resulting selection bias introduced through creation of a snowball control group) remains the largest threat to the study’s validity.

Other litigation studies

Several cities throughout the country are conducting household mobility programs like the Gautreaux program or unit-based mobility programs like the Yonkers scattered-site public housing, most initiated by racial discrimination litigation. Although these studies are rarely evaluated, we did find that one qualified for inclusion in our review. The

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9 Originally, the Yonkers intervention would have exploited an experimental design, since a lottery was used, but there were too few lottery losers to create a large enough control group.

10 Some housing mobility programs, including those in Cleveland, Dallas, and Durham (N.C.), have been formally evaluated (Peterson and Williams 1995), but they are not
evaluation of the Cincinnati Special Mobility Program measured violent victimization and health care, although the study is methodologically very weak. Fischer (1991) compared the intervention group before and after the policy and also compared the intervention group with public housing controls at follow-up. He found that moving out of concentrated public housing was associated with fewer attacks or robberies and with the receipt of employer-supplied health care benefits. However, this was the weakest of all the studies included in our review. The control group was likely not comparable with the intervention group, and we could not assess the amount of bias, since the author did not report any multivariate regressions or other analyses to control for confounding. Moreover, he did not include any statistical tests of difference or table of numerical results.

**MTO**

The federal government has implemented a randomized housing mobility policy experiment in five U.S. metropolitan areas with the MTO program. Sponsored by HUD and begun in 1994, the experiment randomly assigned selected participants from central-city public housing to one of three groups:

1. The treatment group (also referred to as the experimental or MTO group) was offered both a Section 8 housing voucher that could be redeemed only in a low-poverty neighborhood and housing counseling.

2. The Section 8 group was offered a geographically unrestricted Section 8 housing voucher.

3. The in-place control group did not receive a voucher, but remained eligible for public housing.

All of the participants consisted of low-income families, and most were racial minorities (Goering et al. 1999). This review covers results from the first follow-up evaluation fielded in each city, which took place approximately two to three years after randomization and was released in 2002. The results from a more recent interim survey fielded in 2001–2002 were released while this article was under review.

considered natural experiments because of their design and implementation. As a result, conclusions drawn from favorable outcomes cannot be attributed to the policy itself unless a sound comparison group is found. Although we reviewed these studies for inclusion in this review, they did not qualify.
Two of the MTO sites have reported mental and physical health outcomes (Boston and New York), three have reported violent crime victimization (Boston, New York, and Chicago), two have reported medical care outcomes (Los Angeles and Boston), and one has reported health-behavior outcomes (New York) (Hanratty, McLanahan, and Pettit 2003; Katz, Kling, and Liebman 2001; Leventhal and Brooks-Gunn 2003a, 2003b; Rosenbaum and Harris 2001).

As a result of the Boston MTO policy, children in treatment group families have experienced fewer injuries or accidents, and boys in the treatment and Section 8 groups both displayed fewer reported behavior problems than controls. Investigators reported a marginal reduction in the treatment of children’s asthma attacks and in personal crime victimization (attacks, threats, or robberies). Boston treatment and Section 8 parents reported better overall health, as well as higher proportions of being calm and peaceful (Katz, Kling, and Liebman 2001).

Leventhal and Brooks-Gunn (2003a) provide evidence that moving to low-poverty neighborhoods in New York resulted in large improvements in mental health for boys, especially among the treatment group. As table 2 illustrates, compared with controls, the New York experimental and Section 8 children, as well as subgroups (usually boys or the younger children), reported significantly improved mental health, which manifested as fewer problem behaviors such as anxiety or depression, dependency, fear, and the need to be near adults. In some cases, Section 8 children experienced lower levels of problem behavior (being headstrong, unhappy, sad, or depressed and depending too much on others) than controls, when the experimental children did not (Leventhal and Brooks-Gunn 2003a, 2003b).

The New York MTO study found different significant effects for teen problem health behaviors (using cigarettes or alcohol) by gender. Unexpectedly, adolescent girls in the experimental group were significantly more likely to have used alcohol in the past year than in-place controls, but teen boys in the experimental group reported fewer peers using cigarettes. New York treatment group parents were significantly less likely to report distress symptoms than controls and marginally less likely to report depressive symptoms (Leventhal and Brooks-Gunn 2003a, 2003b).

In terms of medical care, neither of the two sites that measured medical care found an effect (Hanratty, McLanahan, and Pettit 2003; Katz, Kling, and Liebman 2001). None of the three sites that measured experience of violent crime found significant results between experimental and control groups ($p < 0.05$), although the New York and Chicago
sites both found a reduction in crime from baseline to follow-up (Leventhal and Brooks-Gunn 2003a; Rosenbaum and Harris 2001). This suggests that a secular decrease in crime may have occurred.

The design of the Chicago follow-up survey (investigators re-interviewed movers only, with no control group comparison) suggests that inferences from this site may be biased. The study may have experienced secular effects without control group measurement at follow-up; for instance, if the participants were affected by an external factor not derived from the MTO study, such as economic changes, the study would incorrectly infer an effect caused by MTO (Orr 1999).

While this article was under review, HUD released the MTO Interim Impacts Evaluation (Orr et al. 2003), which included detail of health outcomes resulting from relocation four to seven years after randomization. In that report, investigators did not report site-specific findings. Adults in the experimental and Section 8 groups experienced significantly lower obesity (measured as body mass index) than the control group, and adults in the experimental group had a lower prevalence of mental health problems (psychological distress and depression). No effects were observed among adults for asthma, self-reported health, high blood pressure, smoking, drinking alcohol, or activities of daily living. Among children, girls in both experimental groups reported improved mental health. Boys aged 12 to 19 in the Section 8 group reported more injuries requiring medical attention. Aside from injuries, no physical health effects were observed for children or teens, and there were no significant group differences in access to health care (Orr et al. 2003).

The MTO interim evaluation also measured risky health behaviors among adolescents. Teen girls in the experimental group reported lower lifetime use of marijuana and of smoking tobacco. Teen boys in both treatment groups unexpectedly reported significantly higher rates of smoking tobacco than controls (Orr et al. 2003).

The health findings of the 2003 MTO interim report are somewhat consistent with the first follow-up study, indicating mainly mental health benefits, in addition to the lower levels of adult obesity, which had not been previously measured, among the experimental groups. This policy may differentially affect children by gender, as suggested by Leventhal and Brooks-Gunn (2003a), where girls’ mental health may benefit, whereas boys’ health may be harmed by smoking uptake and injuries in adolescence. The interim findings are also consistent for health care access, since neither evaluation found group differences.
Although the randomized design of the MTO study strengthens the causal inferences drawn from the research, some threats to validity exist. If one analyzes only those who leased up (took advantage of the voucher), MTO individuals are highly selected. The low lease-up rates can be conceptualized as low treatment compliance and may reduce the study’s power to detect meaningful differences among groups.\textsuperscript{11} However, researchers can use and have used an intent-to-treat (ITT) analysis to keep randomized groups intact in the analysis. If conducted correctly, ITT analysis should treat noncompliant subjects, in this case those who were selected for the MTO experimental groups but did not use the voucher, as treatment failures, since noncompliance may be associated with “side effects” of the intervention. For instance, as suggested earlier, losing a regular source of medical care could be a significant side effect of moving to a new neighborhood. But if all original study members are identified and if the effects of the treatment can be assumed as positive, then when analyzed as ITT, the effects should be unbiased by selection for the average effect on the group regardless of compliance (Greenland 2000). But the effect estimated by ITT is the effect of being offered a housing voucher, not receiving one.

Furthermore, MTO researchers have conducted instrumental variable analysis (IVA) that strengthens the conclusions that the policy may improve health. The authors estimated the effect of the treatment-on-treated (TOT) by using this technique, which corrects for bias that would usually arise if one applied an ITT analysis or directly compared treated with untreated individuals in a randomized trial. The technique uses a variable, “the instrument,” which is associated with the predictor and has no other influence on the outcome except that mediated by the predictor, to estimate the causal effect of the treatment on those who were randomly assigned to treatment and actually received it. The TOT analyses found significant results for some health outcomes that were not significant in the ITT analyses, suggesting that, as expected,

\textsuperscript{11} The experimental group actually used the low-poverty neighborhood voucher at a low rate—48 percent across all five sites; see table 5 in Goering et al. (1999). Notably, the lease-up rate—hence selection into low-poverty neighborhoods—may be related to health. After conducting a multiple regression analysis predicting lease-up rates with baseline information across all five sites, Shroder (2002) reported that families receiving disability payments (supplemental security income/Social Security Disability Insurance/Social Security survivor benefits) were significantly less likely to lease up or take advantage of the housing voucher to move. Further, interviews with MTO mobility counselors revealed that many families that failed to use the voucher to move did so because they did not want to move farther away from the medical care that they or their children were receiving. These findings stimulated researchers to observe that “health care is perhaps the most important place-based service that these families rely on in their neighborhoods” (Kling, Liebman, and Katz 2001, 22). Thus, it is plausible that healthier individuals may have been selected into using the voucher.
the policy had stronger effects on those who actually relocated to low-poverty neighborhoods (Greenland 2000).  

The MTO research findings may also be biased by loss to follow-up. This threat presents itself especially in the New York study, where 31 percent of the participants were lost, and in Chicago, where only 51 percent—none of them controls—were interviewed at follow-up.

Although randomized trials eliminate many serious threats to internal validity, several remain. The most relevant for MTO are likely compensatory rivalry and compensatory equalization, both of which occur when the treatment is perceived as desirable (Cook and Campbell 1979). Compensatory rivalry, also called the John Henry effect, could have surfaced if MTO control group members felt cheated by not having received the treatment and worked harder than they would have in the absence of this policy to seek some sort of substitute for the treatment, such as neighborhood mobility or homeownership. With compensatory equalization, an administrative body that deems control status less desirable than treatment steps in to offer a comparable treatment as a sort of consolation. This could have happened in MTO, for instance, if housing authorities provided vouchers to controls in a more expeditious manner than they would have without knowledge of the control’s participation in the experiment. In both cases, the bias results in underestimating the treatment effect, because the controls are performing artificially better than they would have without the policy.

Assessment of the empirical evidence

The strongest studies had randomized designs using ITT analyses and/or valid and reliable measures of health. Our review methods gave the Boston MTO study (Katz, Kling, and Liebman 2001) the strongest methodological score because it was randomized and analyzed as ITT, used valid and reliable health measures, experienced very low attrition,

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12 In general, IVA effect estimates for a trial will be further from the null than ITT estimates because many of those assigned to the treatment may not actually receive it. These individuals presumably dilute the estimated effect of the treatment in an ITT analysis. The bias in the ITT effect estimates is proportional to the amount of noncompliance, so in experiments with substantial noncompliance, such as MTO, the difference between ITT and IVA estimates may be large. There is some criticism of using IVA estimates in this case, because people who did not adhere to the treatment/policy may be those who could somehow have been harmed by it. For example, in MTO, it is plausible that people who did not use the vouchers were those who were afraid to lose their regular source of medical care. In this case, the causal effect of the treatment on the treated is not the same as the average causal effect of the treatment on the whole population.
used instrumental analysis to examine effects on movers, and exhibited few remaining threats to validity (table 1). It was the only study to attain “good” methodological rigor according to the Briss method (Briss et al. 2000). The New York MTO mental health study (Leventhal and Brooks-Gunn 2003b) ranked next highest because of its randomized design, ITT and IVA, and valid and reliable health measures. Four other studies fall in this same fairly strong category: the other New York MTO study (Leventhal and Brooks-Gunn 2003a), the two Yonkers studies (Briggs and the Yonkers Family and Community Project 1997; Fauth, Leventhal, and Brooks-Gunn 2002), and one Gautreaux study (Rosenbaum and Popkin 1990). Despite its nonrandomized design, the Yonkers study scored high because of its very low attrition rate; its numerous valid and reliable health measures; and its thorough policy, population, and methodological discussions (Briggs and the Yonkers Family and Community Project 1997).

We evaluated the rest of the studies as being of limited methodological strength, because of five or more serious threats to validity. Falling in this category were the Chicago (Rosenbaum and Harris 2001) and Los Angeles (Hanratty, McLanahan, and Pettit 2003) MTO studies, the Section 8 study assessing child growth/undernutrition (Meyers et al. 1995), and the original Gautreaux evaluation assessing health care satisfaction (Peroff et al. 1979). The Chicago MTO study experienced substantial attrition (50 percent) and did not interview controls at follow-up, so the investigators could not conduct an ITT analysis. The Los Angeles study was ranked as limited because it was not clear how reliable or valid the health care outcome was, and the researchers were not explicit about their statistical and analytical methods. Two Gautreaux articles using the same study data (Rosenbaum 1994, 1995) and the Cincinnati litigation program (Fischer 1991) finished last in our analysis. These Gautreaux studies experienced substantial loss to follow-up (40 percent attrition), the author was not explicit about statistical methods used to account for confounding, and the validity and reliability of the victimization measure were not addressed. The Fischer (1991) study was methodologically weak because the author was not explicit about the statistical methods used to control for confounding, and he did not present tests of statistical significance, numeric results, or validity/reliability of measures.

Overall, the evidence suggests that both tenant- and unit-based housing mobility policies may contribute to improving child, adolescent, and adult health and health behaviors. However, this preliminary evidence comes from a small number of studies, many of which have methodological limitations associated with the lack of an experimental or quasi-experimental design.
Further, although there is some evidence that relocation to better neighborhoods could lead to improved health among public housing residents, none of the studies reviewed explicitly tested the mechanisms through which housing mobility policies could impact health. As Briggs notes, the literature on the neighborhood effects of housing mobility programs has not elucidated the mechanisms by which the observed outcomes came to be, or the “how” of the processes involved (1997). Some mechanisms are being tested in the Yonkers study, but according to our review, very few of these have been tested thus far in the MTO experiment. However, the potential exists to include such hypotheses in the data collection for the 10-year MTO follow-up.

Discussion

The housing mobility policies described in this article were conceived as racial and economic desegregation policies. Although they were not intended to affect health, it appears that they may have. Indeed, Leventhal and Brooks-Gunn claim that “the most significant benefits of the MTO program were noneconomic” (2003b, 1580). From a public health perspective, the most important implication of the evidence on housing mobility is that housing policy has the potential to improve individual health. Mobility and other housing policies should continue to be evaluated for their impact on health. If the long-term benefits are positive, as suggested thus far by this limited research on housing mobility, such policies may promise to improve individual health and quality of life, as well as the health of the population as a whole. Because housing mobility policies target very low-income families, these policies may also have the potential to reduce health disparities by improving the health of these disadvantaged groups.

Generalizability

The available evidence suggests that housing mobility programs could improve health. However, there is strong empirical evidence from only two MTO sites, Boston and perhaps New York, and from the Yonkers studies. These results may not be generalizable to other cities with different demographic and social characteristics. Moreover, as discussed earlier, MTO results could be generalizable by design only to families that volunteer to leave public housing. At least in the MTO demonstration, those families appear to be more disadvantaged than the general public housing population and may have had strong, specific motives to move. As summarized by Shroder (2002), “[W]ith this experiment we will have a chance to evaluate without bias the benefit of moving out of...
high poverty to those who want to move and are willing to keep searching until they succeed” (337, emphasis added). Accordingly, it is possible that MTO results overestimate the possible effects on health that the intervention could have on families that already are participating in the Section 8 program or families that could be transferred from public housing to Section 8 without volunteering for it.

Effects on health outcomes

Before MTO, much of the evidence on health and housing mobility had little scientific weight because of the lack of experimental designs and the associated threat of selection bias. The evidence from MTO is stronger because of its experimental design, but since this policy demonstration was not designed to measure the effects of housing mobility on health, the range of health outcomes is limited for the most part to a few self-reported measures. Since none of the sites at the first follow-up study collected any physiological health measures or blood samples, there is no evidence of change in biological markers. However, the apparently positive effect on self-reported outcomes suggests that, at a minimum, psychological well-being improved.

Additionally, although the research teams at the various sites have speculated about the possible mechanisms through which MTO may improve health, the demonstration was not designed to explore specific mechanisms. For example, Katz, Kling, and Liebman (2001) have proposed that the marginally significant reduction in the number of self-reported asthma attacks among children in experimental households may be linked to the better air quality and the decreased stress, crowding, lack of heat, or exposure to allergens from vermin that are associated with living in better neighborhoods. Other questions for exploration include whether the housing units in the new neighborhoods contain less mold caused by aging or leaky pipes or whether windows are less likely to be nailed or painted shut, thus preventing air flow that could disperse indoor allergens or toxins that trigger asthma. In other words, the marginal reduction in asthma rates could be due to a neighborhood factor (safety, air quality) or a household factor (mold, allergens), or both. Given the present MTO data, it is not possible to elucidate the relative weight of these mechanisms.

Segregation, neighborhoods, and health

Although the MTO results may provide some indirect support for social epidemiologic evidence that racial and economic residential segregation
could be positively associated with mortality and morbidity rates, the MTO’s scope is simply too small to alter racial segregation or poverty concentration patterns at the metropolitan level. For example, the size of the black population in the Boston metropolitan area in 1990 was approximately 198,000. In the same year, the dissimilarity index for blacks in this metropolitan area was 69.6, indicating that nearly 70 percent of the black population—more than 138,000 individuals—would have had to move to different neighborhoods to achieve an even distribution with respect to the non-Hispanic white population (Lewis Mumford Center for Comparative Urban and Regional Research 2001). By contrast, the MTO demonstration in Boston involved only about 135 (mostly black) families.

In relation to the epidemiologic evidence on neighborhood effects on health, the randomization of study participants to the control group, the regular Section 8 group, or the MTO group means that it is safe to assume that any observed effects on health are not due to individual-level characteristics. That is, there is no threat of selection bias. However, it is not clear that effects can be attributed to neighborhood environment.

**Future research and data**

*Conceptual frameworks.* As discussed earlier, epidemiologic research on housing and health has largely lacked a conceptual foundation. Housing policy research constitutes a unique opportunity to explore various neighborhood effects on health through experimental designs. Conceptual frameworks should outline plausible mechanisms through which neighborhood conditions could affect specific health outcomes. The development of such conceptual frameworks will require multidisciplinary research teams that include housing policy and public health experts (Srinivasan, O’Fallon, and Dearry 2003). Public health research can help inform the selection of health outcomes that are more sensitive to certain neighborhood conditions at certain times in the life course. For instance, Macintyre and Ellaway (2003) discuss the idea that fear of crime or violence may influence women and elders more than it does men or younger people, affecting mental but not physical health. Conversely, children may be more sensitive than adults to material conditions like damp, which affects respiratory disease (Macintyre and Ellaway 2003). In the latest MTO report (Orr et al. 2003), the investigators found it difficult to interpret the lower rates of obesity among experimental adults. However, public health research suggests that disadvantaged neighborhoods are associated with reduced access to healthful foods because fewer grocery stores are available (Morland
et al. 2002) and with unsafe or “low-walkability” conditions that may not be conducive to physical activity (Saelens et al. 2003). In turn, those factors may be linked to higher rates of obesity. Alternatively, the MTO interim finding that experimental families eat more meals together as a family may explain part of the obesity effect; for example, parents may be preparing healthier meals more frequently.

Since housing mobility programs have been introduced in part to help remedy racial segregation, which originates in both institutional and interpersonal racial discrimination, it is important to address discrimination in the conceptual framework and variables measured in future housing mobility studies. The MTO team has taken a first step toward this end by including an item in the interim report dealing with whether participants faced housing discrimination during their last housing search (Orr et al. 2003). Yet the authors have not presented this information within a racial context. The MTO sites varied substantially in the racial makeup of their population, and different metropolitan areas display markedly different rates of housing discrimination (Acevedo-Garcia, Osypuk, and Krimgold 2003; Gabriel 1996), as do neighborhoods with different racial composition (Gabriel 1996). Moreover, racial discrimination is experienced in many forms, across various life domains in addition to the housing domain, and many of these have been shown to affect health (Gee 2002; Krieger and Sidney 1996; Williams and Neighbors 2001; Williams, Neighbors, and Jackson 2003). Racial disparities in health are well-established (Williams 2001; Williams et al. 1997), but rarely do health investigators measure the social process of racism—that is, the mechanism by which race is most likely associated with health. Including a more comprehensive measure of racial discrimination, presenting racism experience by MTO site and by racial group, and analyzing data on discrimination by the racial composition of the census tract may be invaluable to better understanding both housing-specific and non-housing-specific discrimination and its effects on health. Also, the Section 8 program faces important challenges, such as the negative reactions that some communities have expressed against the influx of families on housing assistance (Briggs 1997, 1998; Rosenbaum 1995). Therefore, it would be important to assess the potentially negative effects—for example, stress and depression—on the health of families moving into hostile communities.

Additionally, future research should explore whether improvements in health outcomes might eventually lead to long-term improvements in economic outcomes. For example, parents who receive public assistance are more likely to have children with such chronic health conditions as asthma, which in turn limits their ability to work (Heymann and Earle 1999). If housing mobility policies can effectively improve children’s health, parents may be more likely to succeed in the labor force.

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Multilevel data and analytic methods. Future research on housing mobility should include a larger sample of individuals, neighborhoods of origin, receiving neighborhoods, and metropolitan areas, as well as more comprehensive data on unit characteristics. Many of the research questions that future studies should address are inherently multilevel and thus will require multilevel analytic methods (Subramanian, Jones, and Duncan 2003). For instance, given that improvements in neighborhood quality that result from mobility policies are conceivably accompanied by improvements in the quality of housing units, it is important to determine whether mobility policies result in improved health outcomes because of housing unit effects, neighborhood effects, or both. Similarly, improvements in housing and neighborhood quality could have differential effects on individuals with various demographic and socioeconomic characteristics. The health effects of improving the neighborhood environment may be greater for individuals/households with fewer coping resources, such as those with low socioeconomic status or single-headed households.

Multilevel methods may also allow us to assess whether there is significant variation in health outcomes across neighborhoods, including across experimental neighborhoods, and, if so, which factors could explain this variation. For instance, health effects may be greater if the receiving neighborhoods have a low poverty rate (below 10 percent), as opposed to being simply less disadvantaged than the neighborhoods of origin.

Further, to the extent that housing mobility demonstrations are conducted in a large cross-section of metropolitan areas, multilevel methods may allow us to determine whether health outcomes are affected by metropolitan area characteristics after we control for neighborhood or household characteristics. For example, are health effects greater if mobility policies are implemented in metropolitan areas with higher levels of racial or economic segregation, since movers may have more to gain than their counterparts in areas with lower levels of segregation?

Qualitative research methods. Our review focused on studies with an experimental design, which social epidemiologists regard as a valuable yet often not feasible method of evaluating neighborhood effects (Oakes 2004). The 2001 qualitative study of Boston’s MTO (Kling, Liebman, and Katz 2001), which brought the importance of health to the attention of investigators, and the 2002 qualitative study of MTO (Popkin, Harris, and Cunningham 2002), as well as the qualitative neighborhood and health studies of Boston by Gans and of Chicago by Raudenbush and Sampson (Kawachi and Berkman 2003a) suggest the potential of such research methods for understanding the pathways through which
neighborhood conditions might affect health. Qualitative investigation provides a historical context and also communicates the reciprocal and dynamic process of how people shape their neighborhoods (Macintyre and Ellaway 2003). Qualitative research methods could also be used to inform the design of the quantitative data collection and help explain the results from quantitative analyses (Popkin, Harris, and Cunningham 2002).

**Better health, housing, and neighborhood data.** Future studies should also include better measurement of health outcomes, including baseline and follow-up measurements and triangulation of methods—biological measures, self-reported health measures, and validated scales (for example, depression). Given proper ethical and confidentiality protections, it would also be important to link participant data to health insurance claims or medical records, especially for diagnostic-specific information. Such linking may be feasible for policies such as MTO if substantial proportions of participants were enrolled in Medicaid or state children’s health insurance programs. Linking to administrative health data seems feasible, since the MTO follow-up study plans to link to other administrative data systems, such as welfare, arrest records, and schools (Orr et al. 2003). In addition to being uniform across all the study sites (the original survey instruments were not), the 2001–2002 follow-up MTO household survey prepared by Abt Associates, Inc., partially addressed some of these issues. Namely, it included detailed sections on injuries, asthma symptoms, body mass index, and blood pressure measurement. Yet additional measures may include other health outcomes that could be associated with neighborhood conditions, for example, intimate partner violence and infectious diseases such as tuberculosis, HIV, and sexually transmitted diseases (Acevedo-Garcia 2000; Fullilove 2003; Kawachi and Berkman 2003b; O’Campo et al. 1995).

Biomarkers can be employed to test specific physiologic mechanisms of housing or neighborhood effects on health. For instance, the physiologic stress response can be tested by measuring the stress hormone cortisol in saliva samples. Several biomarkers are available for asthma (Christiani 1996; Loucks 2003) (i.e., inflammatory markers), as well as for immune function (Kiecolt-Glaser et al. 2002), both of which can be affected by stress. If we hypothesize that housing mobility affects nutritional intake in the form of increased access to higher-quality supermarkets in low-poverty neighborhoods or reduced access to fast food in high-poverty neighborhoods, we can measure serum fasting glucose or serum cholesterol. Biomarkers are useful for valid measurement of outcomes that may be influenced by cognitive status or by social desirability (such as smoking, intake of fruits and vegetables, or weight) or for...
intermediate outcomes for chronic diseases that take a long time to
develop (such as blood pressure or cholesterol for heart disease) (De
Gruttola et al. 2001; Loucks 2003).

To date, few social epidemiologic studies have progressed beyond
demonstrating that certain structural characteristics of neighborhoods,
such as poverty or disadvantage, appear to exert a contextual influence
on health after taking into account compositional characteristics such
as the socioeconomic status of residents. Few attempts have been made
to characterize and measure the social, physical, and service aspects of
neighborhood environments and to test the causal mechanisms under-
lying the influences of these aspects of neighborhood environments on
health outcomes. Social epidemiologists concur that future research on
neighborhood effects should incorporate the development of valid, reli-
able measures and scales of various aspects of neighborhood environ-
ments (Morland et al. 2002; O’Campo 2003; Saelens et al. 2003). For
example, assessing whether a neighborhood is conducive to physical
activity could involve gathering data on sidewalks, bicycle lanes,
shared-used paths, work sites, green ways, or recreational facilities
(Librett, Yore, and Schmid 2003). Recent work by the Urban Institute
under the National Neighborhood Indicator Partnership project (Pettit
et al. 2003) constitutes an example of the types of specific neighborhood
indicators that could be useful in future health research on neighbor-
hood effects.

Moreover, subjective and objective neighborhood data may tap different
health mechanisms. For instance, subjective rating of crime may be
more inhibiting for exercise than objective crime rates; alternatively,
subjective reports may introduce reporting bias. For instance, someone
with respiratory problems reports that his or her home is damp
because this person believes it contributes to his or her illness. Further,
subjective reports may empirically demonstrate less variation than
objective assessments (Kawachi and Berkman 2003a; Macintyre and
Ellaway 2003).

*Evaluation of other housing policies.* Given that they target low-income
and very low-income families, housing vouchers and mobility initiatives
should continue to be evaluated for their impact on the health of disad-
vantaged groups and their contribution to reducing health disparities.
Housing vouchers are becoming the dominant strategy for providing
housing assistance, with the potential for reaching approximately
5.4 million households—the estimated worst-case housing needs popu-
lation (HUD, Office of Policy Development and Research 2000). As a
policy demonstration, MTO has been very limited in scope (about 1,800
experimental families), and its political viability as a large-scale pro-
gram is not certain. Therefore, it will be important to further evaluate

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the health impact of Section 8 and some of its politically viable variations—such as regional, as opposed to local, administration of the program, which has been proposed as a way to enhance its housing mobility potential (Katz and Turner 2001; Turner 1998).

Although the evidence from the housing mobility studies is not definitive, it will be important to put the magnitude of these results into perspective. How big are the health effects of housing policy compared with the effects measured for explicit health interventions? For instance, are the effects on asthma from housing mobility large compared with the effects from health education programs specifically targeting asthma? A closely related issue is the approximate cost of housing mobility programs. We should take the relative effects of housing interventions and explicit health interventions on health outcomes in the context of their relative costs. A crude comparison shows that the total annual HUD budget for rental assistance is approximately $17.5 billion, compared with more than $140 billion annually for Medicaid. Incorporating more specific cost-benefit considerations into our analyses could inform considerations of the specific health outcomes of housing mobility policies.

During the 1990s, under the HOPE (Home Ownership for People Everywhere) VI program, the federal government changed its housing policy toward low-income households and moved away from project-based assistance (public housing projects) toward an increased use of housing vouchers and mixed-income housing developments (Buron et al. 2002; Sard 2000). The nature and scale of such policy changes warrant an examination of their possible health effects, especially on families that have been displaced and unable to find affordable housing. In a tracking study of HOPE VI, the Urban Institute has identified health problems as a major issue for former residents of distressed public housing (Buron et al. 2002).

Earlier, we identified promotion of homeownership among low-income households and antidiscrimination policy as two strategies that, like housing mobility policy, could potentially mitigate health disparities. In the future, health research may identify quasi-experimental or experimental policy demonstrations in those two areas, where health outcomes could be incorporated into the evaluation. Further, other important housing policies, such as federal tax deductions to promote homeownership, policies to promote homeownership among low-income households, and fair housing policies, should be evaluated for their effect on health outcomes. For instance, in terms of health impact among low-income groups, does homeownership have a larger positive health influence than housing mobility?
The evidence from Yonkers and MTO suggests that housing mobility policy could have positive and, ironically, largely unintended effects on health. HUD has a strong, long-standing tradition aimed at conducting housing policy demonstrations with an experimental design (Shroder 2000). Therefore, there is already a good infrastructure for carrying out housing policy demonstrations with attention to health outcomes. In their vision for housing policy in the new millennium, policy makers believe that “housing policy must be linked to other social policies” (Wachter 2000, 1). Integrating better health as an explicitly desirable outcome of housing policy could help fulfill this vision.

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