

An MDRC Working Paper

Subsidized Housing and Employment

Building Evidence About What Works to Improve Self-Sufficiency

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Abstract

For many years, policymakers have agreed that low-income, working-age people who receive government rent subsidies ought to strive for self-sufficiency and that the housing subsidy system should play an actively supportive role — or at least not stand in the way. This intent is clear in the most recent major public housing reform legislation, the Quality Housing and Work Responsibility Act (QHWRA) of 1998, which makes promoting residents' self-sufficiency a core objective. A variety of self-sufficiency innovations and housing policy reforms have been tried over the past two decades, and new ones are being proposed all the time. Yet what is striking about innovation in this field is that so little of it is based on credible evidence of “what works.”

This paper, which was prepared for “Revisiting Rental Housing: A National Policy Summit,” a November 2006 symposium organized by the Harvard Joint Center for Housing Studies and supported by the MacArthur Foundation, argues for building a stronger base of evidence in the housing-employment policy arena through an expanded use of randomized controlled trials. Greater use of this methodology, which involves comparing outcomes of program and control groups to which eligible people have been assigned through a lottery-like process, is technically feasible and practical, and it could substantially improve the body of evidence on “what works.” With better knowledge in hand, policymakers and others who wish to improve self-sufficiency outcomes could rely less on hunches and hope in deciding how to achieve those aims. Indeed, many experts see more credible evidence as an urgent policy need.

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Introduction

For many years, policymakers have agreed that low-income, working-age people who receive government rent subsidies ought to strive for self-sufficiency and that the housing subsidy system should play an actively supportive role — or at least not stand in the way. This intent is clear in the most recent major public housing reform legislation, the Quality Housing and Work Responsibility Act (QHWRA) of 1998, which makes promoting residents’ self-sufficiency a core objective. At the same time, there is a widely shared belief that government needs to push further in this direction, as reflected in the 2002 recommendation of the bipartisan Millennial Housing Commission that “more should be done to link housing assistance with economic opportunity, self-sufficiency, and personal responsibility.”¹

Reflecting this longstanding interest, a variety of self-sufficiency innovations and housing policy reforms have been tried over the past two decades, and new ones are being proposed all the time. Yet what is striking about innovation in this field — proposed or actual — is that so little of it is based on credible evidence of “what works.” Rare are the kinds of rigorous impact evaluations based on randomized controlled trials that have become more common in the welfare-to-work and workforce development fields and that are increasingly used in criminal justice and education research.² This, of course, makes it difficult to know whether self-sufficiency strategies for assisted housing that *sound* promising are really a good bet or a bad investment. Given that many work-promoting services and supports exist outside subsidized housing and are already available to assisted residents, do interventions that are explicitly linked to housing subsidies really “add value”? A number of descriptive and nonexperimental studies suggest that they do, but most studies are limited by the lack of appropriate control groups or have other data or methodological limitations that make it impossible to know for sure. In addition, many such evaluations do not carefully explore in depth how the programs are administered on the ground, and many do not include even rudimentary benefit-cost assessments.

Also uncertain is whether basic housing assistance in and of itself promotes or impedes residents’ progress toward self-sufficiency. As discussed below, countervailing forces are at play, and theory can be mustered to support competing claims, thus contributing to a need for more convincing evidence.

¹Millennial Housing Commission (2002, p. 56).

²Shroder (2000) summarizes eight housing-related experiments funded by the U.S. Department of Housing and Urban Development (HUD) since the 1970s, starting with the Experimental Housing Allowance Program (EHAP). Most focus on housing-specific outcomes — such as take-up of rent subsidies, housing expenditures, and mortgage delinquency — rather than on self-sufficiency results. More recent experiments that do focus more on self-sufficiency outcomes are the Moving to Opportunity and Welfare to Work Voucher demonstrations, which are discussed later in this paper.

This paper argues for building a stronger base of evidence in the housing-employment policy arena through an expanded use of randomized controlled trials. Greater use of this methodology, which involves comparing outcomes of program and control groups to which eligible people have been assigned through a lottery-like process, is technically feasible and practical, and it could substantially improve the body of evidence on “what works.” With better knowledge in hand, policymakers and others who wish to improve self-sufficiency outcomes could rely less on hunches and hope in deciding how to achieve those aims. Indeed, many experts see more credible evidence as an urgent policy need.³

The paper begins by considering the rationale for attaching self-sufficiency initiatives to the subsidized housing system when the main purpose of that system is to provide access to decent affordable housing for low-income people. The paper then briefly reviews past work-focused policies in this area and considers what evidence of their effectiveness exists, if any. After outlining what a comprehensive evaluation based on a randomized control trial would entail, the paper offers specific ideas for an evaluation agenda that, if pursued, could help build a foundation for truly *evidence-based* policymaking in this field. It considers *what kinds* of interventions to evaluate (based on emerging innovations and lessons from other fields on helping low-wage people secure, sustain, and advance in work) and *how* to evaluate them. It gives particular attention to strategies for tenant-based vouchers, which today are the most common form of federal rental housing assistance for the lowest-income groups in the country. In lesser detail, the paper also considers possible employment-focused experiments for Project-Based Section 8 housing, public housing, and HOPE VI housing.

Why Intervene in the Subsidized Housing System to Promote Work?⁴

Three main types of programs make up the federal housing assistance system for very low-income families: (1) public housing, (2) tenant-based portable rent vouchers for families to use in seeking private rental housing (known formally as Housing Choice Vouchers),⁵ and (3) “project-based” vouchers, which are attached to privately owned housing units.⁶ This system is administered by HUD through local public housing authorities. With some variation and excep-

³For example, see Newman (1999); Sard and Bogdan (2003); Shroder (2000); and Solomon (2006).

⁴Substantial parts of this section were prepared with assistance and written contributions from Sandra Newman and Joseph Harkness of the Institute for Policy Studies at Johns Hopkins University.

⁵This program was previously called Section 8 tenant-based assistance.

⁶In addition to these Project-Based Section 8 subsidies attached to private multifamily housing, the Housing Choice Voucher program allows local public housing authorities to attach up to 20 percent of their tenant-based vouchers to specific housing units in privately owned developments if the landlord meets certain HUD provisions and housing-quality standards.

tions, each of the three main subsidy programs requires tenants to pay 30 percent of their income (after some adjustments) for housing. The Low-Income Housing Tax Credit (LIHTC) program is another important federal vehicle for supporting affordable housing, but it operates through the tax system, and its rent subsidies are less generous. Consequently, the lower-income population it serves is considerably less poor and has fewer labor market difficulties than the more deeply subsidized people served by the three HUD-administered programs that are the focus of this paper.⁷

The *tenant-based voucher* program is the largest of the three subsidy programs, serving at any given time about 1.8 million households out of a total of about 4.2 million (43 percent) of all HUD-assisted households. In this program, the government enters into a contract with the landlord of a unit that is identified by the tenant, once the unit passes a quality inspection. *Project-based vouchers* cover about 1.3 million households (31 percent of HUD-assisted households). In this program, the landlord is identified and funded directly by the housing authority to make units available to eligible residents at reduced rents pegged to residents' incomes. *Public housing* developments, which are owned by the local housing authorities are home to about 1.1 million households (26 percent of all that are HUD-assisted).⁸

It is also noteworthy that many local housing authorities are replacing severely distressed public housing developments with mixed-income housing complexes. Funded by the federal HOPE VI program, they are building HUD-subsidized units within housing developments that also include other rental units subsidized through low-income housing tax credits, market-rate rental units, and home ownership units. HOPE VI grants allocated through 2003 are expected eventually to provide 50,000 units for families who meet the normal income and other requirements for public housing.⁹

The purpose of all these different forms of housing subsidies is, first and foremost, to help poor people afford decent rental housing, and local authorities are, by and large, achieving that mission. So why bother with matters of work and self-sufficiency, which are traditionally

⁷The LIHTC program subsidizes private housing construction by offering tax breaks to developers who agree to set aside for lower-income families a number of units with below-market rents. Typically, however, these credits are not deep enough to make it feasible for the developer to reduce rents sufficiently far below market rates to make the units affordable for the very low-income renters served through the HUD-administered housing subsidy programs. Thus, the rent subsidies offered through this tax credit strategy are generally considered "shallower" than the "deep subsidies" offered by the three HUD programs that target the poorest tenants. Self-sufficiency among LIHTC tenants has consequently not been as significant a policy concern as it has been for people living in public housing or using vouchers.

⁸See U.S. Department of Housing and Urban Development (2006a, 2006b). The total number of funded vouchers and public housing units is higher, but some vouchers are not redeemed, and some housing units are vacant at any given time (because of rehabilitation or replacement, normal tenant turnover, and other reasons).

⁹Popkin et al. (2004).

the responsibility of other government systems? There are at least three reasons, examined below: (1) to reach many of the nation's work-capable poor in need of employment support, (2) to counter possible negative work influences associated with housing assistance, and (3) to increase access to a limited supply of housing assistance by cycling recipients through the system more rapidly.

1. To reach many of the nation's work-capable poor in need of employment support

Although housing subsidies are not an entitlement, they reach a very large number of very low-income people. Many of these residents are capable of gainful employment (and many are already working), but they commonly have difficulty securing steady employment or advancing in work, despite whatever other government employment assistance might be available.

Of the approximately 4.2 million low-income households receiving HUD housing assistance at any given time, 46 percent (over 1.9 million) are headed by working-age, nondisabled people, about 83 percent of whom (1.6 million) are raising children.¹⁰ Taking into consideration the natural turnover of residents, this part of the nation's "housing safety net" actually "touches" many times more low-income individuals over a period of time. By contrast, the number of families who have children and are receiving cash aid through the Temporary Assistance for Needy Families (TANF) program at any given time (many of whom are also receiving HUD-assisted housing) is only somewhat higher: about 1.8 million in June 2006.¹¹

How much do tenants with housing assistance actually work? According to HUD data from 2000, about 57 percent of nonelderly, nondisabled households across the three subsidy programs had some income from earnings at the time they were recertified as eligible for housing assistance that year.¹² Employment levels varied somewhat by subsidy program. Approximately 52 percent of residents of public housing, 57 percent of people with tenant-based vouchers, and 65 percent of those in Project-Based Section 8 housing reported earned income. Not surprisingly, substantial numbers also rely on cash welfare. For example, in 2000, about 27 percent of working-age, nondisabled HUD-assisted households received TANF (in some cases in combination with earnings) or state general assistance.¹³

¹⁰The percentage distribution is based on the author's calculations using data from Lubell, Shroder, and Steffen (2003, Exhibit 1).

¹¹U.S. Department of Health and Human Services (2006).

¹²Author's calculations using data from Lubell, Shroder, and Steffen (2003, Exhibit 2). There is likely to be some degree of underreporting of earnings by tenants or inaccuracy or lags in reporting by housing authorities.

¹³Author's calculations using data from Lubell, Shroder, and Steffen (2003, Exhibit 1).

Importantly, however, the *amount* of earned income of households with workers was quite low (according to housing authority records), with about 30 percent earning less than \$8,300 per year, half earning less than \$12,000 per year, and about 80 percent earning \$18,000 per year or less.¹⁴ This puts most *employed* recipients of federal housing assistance firmly in the ranks of the working poor or near poor. Moreover, in reviewing data on both nonworking and working residents, HUD emphasizes in a 2005 report to Congress that “public housing and Housing Choice Vouchers continue to serve the poorest segments of the population. . . . By 2004, 77 percent of the families in public housing had incomes less than 30 percent of local median income; the percentage in Housing Choice Vouchers was 79 percent.”¹⁵

In terms of volume alone, then, the subsidized housing system is a good place to find large numbers of the very kinds of people that policymakers want to help succeed in work. But even if this system is a potentially valuable portal to the working poor or work-capable poor, it is reasonable to ask whether these residents are already being reached and helped through the mainstream jobs programs in their communities, especially those operated by the TANF welfare-to-work and Workforce Investment Act (WIA) programs. Certainly some are. Yet the majority (about three-quarters) of nondisabled, working-age assisted housing residents do *not* receive welfare and, consequently, do not participate in welfare-to-work programs (although many of them undoubtedly have in the past). In addition, those currently receiving welfare may very well exit welfare (and become ineligible for further employment assistance through that system) while still living in HUD-assisted housing because they get (usually low-wage) jobs, reach welfare time limits, or leave for other reasons. Still other working-age residents have older children or no children, or they do not otherwise qualify for welfare.

Evidence on the extent to which assisted housing residents use WIA-funded employment services is scant, as is evidence on their participation in other job placement programs or education and training, such as those offered by community colleges or nonprofit service organizations. However, it is reasonable to expect that many potentially eligible residents are not aware of such opportunities or do not use them for any number of other reasons. It is also important to recognize that both the welfare-to-work and the workforce development systems are focused primarily on helping unemployed people *find* jobs, not on helping the working poor sustain employment and advance.

¹⁴ Author’s calculations using data from Lubell, Shroder, and Steffen (2003, Exhibit 4).

¹⁵ U.S. Department of Housing and Urban Development (2005, pp. 8-9). The report explains: “The predominance in the Housing Choice Voucher program of extremely low-income households, those with incomes less than or equal to 30 percent of median income, is not surprising given the current income targeting requirements. In Housing Choice Vouchers, 75 percent of new households must have incomes less than or equal to 30 percent of median income. In public housing, 40 percent of new households must have incomes less than or equal to 30 percent of area median income” (pp. 9-10).

Even if these other systems are already helping to improve work outcomes for HUD-assisted tenants to some extent, the employment and earnings data cited above suggest that considerable room for improvement remains. Indeed, it is reasonable to speculate that, given proper assistance and incentives, many more assisted residents could enter work, sustain work, and advance in work than now do. Many such residents face substantial personal and situational barriers to employment, including low education and skill levels, health problems, and personal or family problems, and often they have poor knowledge about or skill in finding jobs or navigating their way to better opportunities. Special assistance tailored for assisted tenants that addresses these problems — and that could be supplied in collaboration with existing, nonhousing workforce programs (as is the case with some initiatives, such as the Family Self-Sufficiency and Jobs-Plus programs, discussed below) — might help improve assisted tenants’ success in the labor market.

In sum, these data show that many people with HUD-assisted housing who presumably have some potential to work are not working at a given point in time or, if they do work, that they often have difficulty getting or keeping anything other than low-paying jobs. And it appears that many are not served extensively through employment-related programs through other systems. Thus, assisted housing offers one important opportunity for government to “do better” in reaching substantial numbers of some of the lowest-income people in the nation and helping them succeed in work.

2. To counter possible negative work influences associated with housing assistance

Given that the main purpose of housing assistance is to provide access to affordable housing, the system’s effect on self-sufficiency should — at the very least — be neutral: It should neither penalize nor impede work. Yet, in its current incarnation, housing assistance may do just that; like any income transfer benefit program, it may deter many residents from trying to increase their earnings. According to this perspective, housing assistance recipients, on average, work and earn less than they would if they did not have a housing subsidy, because of the way the housing subsidy is structured. With the tenant’s rent contribution set at 30 percent of adjusted gross income, every dollar increase in income is effectively taxed at 30 percent. In other words, as residents’ earnings rise and generate higher income, their rent also goes up (and the housing subsidy is reduced), thus substantially offsetting the gain in income through work. Housing assistance benefits phase out entirely when income rises beyond a certain point. Under the voucher program, for example, when 30 percent of income has been greater than or equal to the rent for six months, the tenant is no longer eligible for a subsidy. In public housing, when 30 percent of income equals the “ceiling rent” attached to a particular housing unit — if the ceiling is pegged to the cost of operating the unit or to market rates — the housing subsidy essentially ends, although residents paying the ceiling rent are not required to move out of public housing.

Since this income-based-rent rule is equivalent to a tax on earnings, theory says — and many housing experts certainly believe — it will be a disincentive to increasing work effort, other things being equal. Theory also suggests that residents may feel less pressure to work to maintain a given lifestyle when their current living expenses are subsidized.

Of course, the real world is more complicated than this simple economic model assumes. Assisted tenants face a complex environment in which other public benefit and tax systems interact with rent subsidy rules in ways that could deepen the hypothesized negative effects of rent rules for some residents or, for others, could counteract the rules. A related problem is that the sheer complexity of this system makes the overall work incentive structure difficult for residents even to comprehend and to tailor their behavior to rationally. Consequently, how individual tenants view work in response to traditional rent rules may vary dramatically, depending on their personal ambitions, circumstances, and perceptions (right or wrong) about the added economic payoff of increased work effort in low-paying jobs.

The limited supply of housing assistance is another factor that might dampen work effort, especially among voucher holders and those on waiting lists for housing assistance. Unlike most federal safety net programs, housing assistance is not an entitlement; only about a fourth of eligible households receive it, and there are long waiting lists for assistance in most communities. As a consequence, families with vouchers may be more reluctant to increase their earnings to the point of becoming ineligible for housing assistance, because they are not assured of being able to obtain a voucher again if they lose their job or see their earnings fall for other reasons.¹⁶ This concern over job loss would seem to be particularly justified for those who could expect to participate in only the bottom rungs of the labor market, given that low-wage workers change jobs more than other workers and experience turnover that is more likely to be involuntary (such as because of layoffs) and to be followed by a period of nonemployment.¹⁷ One study estimated that only about 60 percent of low-wage workers in their midthirties held a job for at least a year.¹⁸

Beyond the structure of the subsidy itself are possible negative peer and neighborhood effects. One hypothesis is that the large concentration of very low-income tenants in dense public housing and other project-based assisted housing developments, which also tend to be disproportionately located in poorer neighborhoods, discourages work effort.¹⁹ Part of the negative effect, if there is one, might arise from the more limited information, experience, and contacts through social networks in such settings. Kasinitz and Rosenberg's qualitative study of the Red

¹⁶Harkness and Newman (2006); Newman (1999).

¹⁷Anderson, Holzer, and Lane (2005).

¹⁸Bernhardt, Morris, Handcock, and Scott (2001).

¹⁹Newman and Schnare (1994, 1997); Kingsley and Tatian (1999). Kingsley and Tatian also show that the proportion of recipients of HUD housing assistance — including recipients of tenant-based vouchers, who live in high-poverty census tracts — is much more extreme in some metropolitan areas than others.

Hook neighborhood in Brooklyn, New York, graphically illustrates the second hypothesis.²⁰ Industrial employers with vacancies in blue-collar jobs located in this neighborhood were much less likely to hire local public housing residents than other residents. Not being tied into the right networks, many public housing residents were unaware of job openings and did not apply.

An alternative perspective argues that housing assistance may function as a springboard into work.²¹ Because housing assistance provides a decent, stable, and affordable residence, recipients may be in a better position than they otherwise would to focus their energies on job-seeking rather than on housing problems. It may also be that physical adequacy, stability, and affordability might reduce their stress and improve their health, making it easier for them to focus on work. Finally, with regard to portable rent vouchers, it is possible that, by giving participants the ability to live in more advantageous locations relative to work opportunities, vouchers might improve success in finding work and, possibly, “better” jobs.

Taking account of these alternative perspectives and the numerous countervailing forces at play, it becomes quickly evident that, as some experts stress, theory itself is ambiguous when it comes to understanding the likely link between housing assistance and labor force behavior.²² Ultimately, the question is an empirical one, making carefully built evidence all the more important.

So what does the available evidence say? A growing body of *nonexperimental* research on this topic points to conflicting results. Some studies show positive effects of housing assistance on work outcomes; others show negative effects; and still others find little effect either way.²³ Overall, the weight of this evidence suggests that housing assistance may not matter consistently for labor force participation.

²⁰Kasinitz and Rosenberg (1996).

²¹See, for example, Millennial Housing Commission (2002); Newman (2005); Sard and Waller (2002). Also, citing research by Murray (1980) and Schone (1992), Shroder (2002) notes that housing assistance, because it is not fungible, could be viewed as a “price cut” to a commodity that, under certain assumptions, may increase work effort to consume other goods.

²²Good discussions of the complicated theoretical underpinnings of this issue are provided by Shroder (2002); Stoloff (2001); and Olsen, Tyler, King, and Carrillo (2005).

²³For a comprehensive review, see Shroder (2002). In a more recent review, Newman (2005) shows that among 11 nonexperimental studies that looked at the effects of housing subsidies — any housing subsidy or certain types — alone (that is, not as part of special employment services or welfare reform initiatives), four find negative effects (Schone, 1994; Murray, 1980; Fischer, 2000; Susin, 2005); one finds a positive effect (Ong, 1998); and six find no enduring statistically significant effects (Reingold, 1997; Ong, 1998; Painter, 2001; Reingold, Van Ryzin, and Ronda, 2001; Berger and Heintze, 2004; and Harkness and Newman’s analysis subsequently published in 2006). Another recent study, drawing heavily on HUD data on housing type and income, adds to the negative side of the ledger, finding that vouchers as well as place-based housing subsidies reduce earnings compared with earnings in the absence of housing assistance but that the effects of vouchers are smaller (Olsen, Tyler, King, and Carrillo, 2005).

To some extent, the conflicting results may be driven by problems in measuring housing receipt through self-reported survey data.²⁴ But the three studies of this group that use more accurate measures of assisted housing receipt (from HUD administrative data) show more consistency, finding initial negative effects on work and earnings but also that these effects lessen over time.²⁵

The best evidence on this issue comes from the few unusual studies that use *experimental* (that is, random assignment) research designs and that collect administrative records data to measure receipt of housing assistance as well as employment outcomes. One of these is the evaluation of the Welfare to Work Voucher (WtWV) program,²⁶ which tested the effects of awarding tenant-based rent vouchers to recipients of cash welfare in six cities. (See Box 1.) To enroll in this study, a family had to be eligible to receive, be receiving, or have been a recipient of cash aid from federal or state welfare funds within the prior two years. In addition, the housing voucher had to be “critical” to the family’s success in obtaining or retaining employment (although this standard was applied only loosely and inconsistently).²⁷ The study randomly assigned over 8,700 qualifying applicants who were on voucher waiting lists in six cities either to a program group that was offered a voucher or to a control group that was not. Few additional services were provided to the program group, thus making the study a reasonably “pure” test of the vouchers themselves. The findings reveal no evidence that the program improved participants’ labor market outcomes. In a pattern generally consistent with the findings of the three nonexperimental studies highlighted above, the study found statistically significant *declines* in work and earnings for five to seven quarters after random assignment, but these declines faded over time. Over 3.5 years of follow-up, the differences in employment and earnings between the treatment and control groups were not statistically significant.²⁸

The second source of experimental evidence on this question comes from the Moving to Opportunity (MTO) demonstration. (See Box 2.) As part of that demonstration, interested residents of public or other project-based housing in high-poverty areas of large central cities were randomly assigned (1) to a group that was offered special tenant-based rent vouchers (along with relocation counseling) that could be used only in low-poverty areas (where less than 10 percent of the population were poor) or (2) to a group that was offered the regular Section 8 rental vouchers or (3) to a control group that was offered neither. Within the first four years of follow-up, neither the special MTO vouchers or the regular Section 8 vouchers produced statistically significant changes in employment or earnings outcomes, relative to what the control

²⁴Shroder (2002).

²⁵Harkness and Newman (2006); Susin (2005); Olsen, Tyler, King, and Carrillo (2005).

²⁶Patterson et al. (2004).

²⁷Smith and Johnson (2001).

²⁸Mills et al. (2006).

Box 1

Welfare to Work Vouchers Experiment

Study sponsor: U.S. Department of Housing and Urban Development.

Evaluators: Abt Associates Inc. and the QED Group LLC.

Target group: Current and former TANF (welfare) recipients, or people eligible to receive such assistance, and whose self-sufficiency efforts could benefit from a housing subsidy. At least 75 percent had to have incomes at or below 30 percent of the area's median income.

Intervention: Offer of tenant-based housing assistance (like Housing Choice Vouchers).

Study design: Random assignment experiment: half of eligible residents assigned to receive the Welfare to Work Vouchers, and half assigned to the control group.

Sample size and intake period: 8,730 people were randomly assigned from April 2000 through May 2001.

Six sites: Atlanta, Georgia; Augusta, Georgia; Fresno, California; Houston, Texas; Los Angeles, California; Spokane, Washington.

Lease-up rates: About 67 percent of the program group had leased an apartment using a Welfare to Work Voucher through Month 42 after random assignment. Among the control group, 42 percent had leased apartments with regular Housing Choice Vouchers.

Interim impact findings: The intervention reduced average employment rates and earnings for the program group relative to the control group. Over 15 months of follow-up, program group members who used their vouchers had spent 7 percent fewer quarters employed and earned 12 percent less than they would have in the absence of the intervention. Average earnings were also reduced. Longer-term data suggest that these early negative effects dissipated but that there was still no evidence of positive effects on employment rates or earnings after 3.5 years of follow-up. However, over this period, public assistance receipt, which declined for both groups, fell less for the treatment group. At the same time, the vouchers caused favorable reductions in homelessness, household crowding, and the doubling-up of families in housing units.

group had achieved. It is noteworthy, however, that although the results are not statistically significant, the outcomes for the two voucher groups were worse than those for the control group, but these differences became less negative in the third and fourth years of follow-up.

The third important experimental study in this area examines the effects of offering Housing Choice Vouchers to residents of unsubsidized private housing in Chicago who were on a voucher waiting list maintained by the city's public housing authority. (See Box 3.) The study takes advantage of the fact that the priority on the waiting list was determined through a randomized process, so that the people who were and those who were not offered rental vouchers

Box 2

Moving to Opportunity Experiment

Study sponsor: U.S. Department of Housing and Urban Development.

Evaluators: Abt Associates Inc., the National Bureau of Economic Research, and the Urban Institute.

Target group: Low-income families with children under age 18 living in public housing or HUD-subsidized private housing in high-poverty areas of large cities where at least 40 percent of the population were poor.

Intervention: Offer of special Section 8 tenant vouchers that could be used only for apartments in low-poverty neighborhoods where less than 10 percent of the population were poor; plus counseling to help find and lease qualifying housing.

Study design: Three research groups: The program group was offered special Section 8 vouchers and relocation counseling; the regular Section 8 group was offered normal vouchers that came with no geographical restrictions and no relocation counseling; the control group was offered neither vouchers nor counseling but could continue receiving project-based assistance. A follow-up period of 10 years is planned. Findings are currently available through four to seven years after random assignment (depending on the outcome variable.)

Sample size and intake period: For the interim study, 4,248 program applicants were randomly assigned from 1994 through December 1997.

Five sites: Baltimore, Maryland; Boston, Massachusetts; Chicago, Illinois; Los Angeles, California; and New York, New York.

Lease-up rates: About 47 percent of program group members leased apartments using their special vouchers; about 62 percent of the regular Section 8 group were able to use their vouchers.

Interim impact findings: Through four years after random assignment, there are no statistically significant differences in employment or earnings outcomes between the program group and the control group, nor between the Section 8 group and the control group, when estimated with unemployment insurance wage records. Though not statistically significant, the program and Section 8 groups had lower earnings than those of the control group during the first two years after random assignment. During Years 4 and 5, their earnings were still lower, though by a smaller margin. Assignment to the MTO program group was found to have noteworthy impacts on a number of important quality-of-life outcomes among adults, including increased perceived safety, reductions in obesity, and reductions in psychological distress. Among teenage girls, program impacts included reductions in certain risky behaviors. (The Section 8 group also saw a reduction in arrests for violent crimes among girls.) However, among boys in the program group, certain problem behaviors and arrests for property crimes increased somewhat.

Box 3

Chicago Housing Voucher Study

Study sponsor: An independent research study.

Evaluators: Brian Jacob and Jens Ludwig.

Target group: Low-income residents with no current housing assistance who were on a waiting list for housing vouchers.

Intervention: Housing Choice Vouchers offered to low-income residents of unsubsidized private housing.

Study design: Natural experiment taking advantage of a waiting list in which priority for vouchers was determined randomly.

Sample size and intake period: Of the 26,374 households on the randomly ordered waiting list, 11,457 received an offer of a voucher in 1997-1998 or 2000-2001, and 14,917 (the control group) received no voucher offer.

One site: Chicago, Illinois.

Lease-up rates: About 49 percent of the “treatment” group used a voucher offered through the wait-list strategy or another source. About 9 percent of the control group used a voucher from any source.

Impact findings: The proportion of follow-up quarters in which the household head was employed was 6 percentage points lower for the treatment group members who used their vouchers than would have been the case in the absence of the vouchers. Average quarterly earnings were \$367 lower, a reduction of 11 percent. The likelihood of receiving TANF increased by 17 percent.

within a given period of time represented, in effect, naturally occurring experimental and control groups with similar pre-voucher characteristics.

The study found that receipt of a housing voucher reduced employment by 6 percentage points, or nearly 11 percent, and decreased average quarterly earnings by approximately \$367, or 15 percent. The vouchers also increased the likelihood of receiving TANF, by 21 percent.²⁹

²⁹Jacob and Ludwig (2006).

3. To increase access to a limited supply of housing assistance

Leaving aside the question of whether housing assistance reduces recipients' efforts to become self-sufficient, a very different motivation for promoting greater employment and earnings among housing assistance recipients arises from the recognition that the supply of assistance is limited. As a consequence, it is unequally distributed, with some qualified families receiving housing assistance and other, equally qualified families not receiving it. Some obvious solutions would be to either increase the size of housing programs or tighten eligibility standards. A third approach would be to cycle participants through the program more rapidly. As it stands, many working-age, nondisabled residents do use housing assistance for only a short period of time. Although longitudinal data that would permit a true picture of the length of residents' spells in assisted housing are unavailable, some insights about tenant turnover can be gleaned from a study that measured tenancy duration for public housing residents and tenant-based voucher recipients as of late 2000. Among the households receiving assistance at this time that were headed by a resident who was neither elderly nor disabled, the median length of stay had *so far* been about 3.6 years for those in public housing and about 2.8 years among voucher holders. (Note, however, that neither group was at the *end* of its stay.) At the same time, about 20 percent of such tenants had already accumulated about 11.3 years in public housing and 7.0 years in the voucher program.³⁰

One controversial way to reduce time spent receiving housing assistance would be to impose time limits on that assistance itself. Another approach that would hold wider appeal would be to accelerate participants' progress toward self-sufficiency so they would have the means to leave assisted housing or could manage with a reduced amount of subsidy, freeing up resources to serve other eligible families. In that way, a given pot of housing assistance dollars could be used to assist more households within a given year.

The Quality Housing and Work Responsibility Act (QHWRA) of 1998 manifests this logic. The stated "purpose" in Section 502(b) of the legislation is to "promote homes that are affordable to low-income families in safe and healthy environments, and thereby contribute to the supply of affordable housing." As one of seven "strategies" for achieving this goal, the act lists "creating incentives and economic opportunities for residents of dwelling units assisted by public housing agencies to work, become self-sufficient, and transition out of public housing and federally assisted dwelling units." According to this perspective, the policy problem is not so much that housing assistance discourages work effort but that there is a shortage of affordable housing. Speeding the transition of housing assistance recipients to self-sufficiency so that more families can be assisted is one possible way of addressing this problem.

³⁰Author's calculations using data from Lubell, Shroder, and Steffen (2003, Exhibits 5 and 7).

Reforms and Interventions Already Tried

A number of housing-based interventions to promote work among HUD-assisted tenants have been tried over the past two decades, and they have generally involved collaboration between a local housing authority and other public and private agencies to supply employment-related services.

The Family Self-Sufficiency Program and Its Predecessors

The most important of these housing-employment programs have been part of a sequence of initiatives that many housing authorities across the nation have operated: Project Self-Sufficiency (PSS), launched in 1984; Operation Bootstrap, launched in 1989; and the Family Self-Sufficiency (FSS) program, which began in 1991 and continues today.³¹ The first two of these programs were available exclusively to low-income people who were applying for Section 8 assistance. In addition to receiving that assistance, they were offered help with education, training, job placement, and other assistance. The current FSS program — which, in addition to services, includes a financial incentive to work and save — is open to residents of public housing and recipients of Housing Choice Vouchers but not to residents of Project-Based Section 8 housing.

Like its predecessors, FSS requires participants to agree to a personalized self-sufficiency plan (generally lasting up to five years) that identifies the steps they will take to become employed, improve their earnings, and no longer receive TANF or state general assistance. The program provides each participant with a case manager who helps arrange job counseling, education, job training, and support services (such as child care and transportation assistance), as called for by the plan. These services are provided by other agencies in the community, through a collaborative arrangement with the housing authority. The program also includes an important work-promoting asset-building component.³² Participants in good standing who increase their incomes by increasing their earnings have the *extra* rent that they would normally be obligated to pay (up to certain limits) deposited into an interest-bearing escrow account maintained by the housing authority. Those who complete their self-sufficiency plans get a full

³¹The Gateway Transitional Families Program—a small-scale initiative operated as a special demonstration project in the late 1980s to early 1990s by the Charlotte, North Carolina, Public Housing Authority — expanded on the Project Self-Sufficiency model by piloting an escrow savings account to promote home ownership, an idea later incorporated into the national Family Self-Sufficiency program. As a further work incentive, it obtained authorization to have participants' welfare and food stamp benefits frozen for a period of time so that earnings gains would not be offset by benefit reductions (Rohe and Kleit, 1997).

³²HUD provides housing authorities with funding to hire a case manager and to pay for the escrow savings. However, housing authorities may also use other local resources to hire additional staff or provide other service enrichments.

disbursement of the escrow accounts, as long as they are employed and are not receiving cash welfare at the time of disbursement.³³

The FSS program currently serves over 52,000 participants, of whom approximately 87 percent are voucher holders.³⁴ Despite the large number of participants, only about 5 percent of working-age, nondisabled Housing Choice Voucher recipients are enrolled in FSS at any given time, and roughly 1 percent of similar people living in public housing are enrolled.³⁵

Evidence of whether any of these programs actually improved participants' employment, earnings, and other self-sufficiency outcomes is weak. The existing evaluations are largely descriptive in nature, showing outcomes for program participants over time, but they were not able to produce reliable estimates of program effects, or "impacts," because they lacked control groups made up of truly equivalent types of people who did not have access to the program. (A proper control group is essential for establishing a benchmark showing what program participants *would* have achieved in the absence of the intervention.) Other limitations include the absence of outcome data on employment and transfer benefits receipt, other than the information reported to the housing authorities. This is a problem not simply as a matter of accurate reporting but also because these data cannot capture program effects on self-sufficiency outcomes that may grow larger or disappear after residents exit the program or leave housing assistance. The evaluations also do not provide any information on costs or cost-benefit results.³⁶

³³Interim disbursements are also possible if needed to help continue working (such as to pay for car repairs) or for education and training that can improve job opportunities.

³⁴The program was optional for housing authorities in 1991 and 1992, but it became mandatory in 1993. Public housing authorities were required to increase the number of FSS participants in accordance with any increase in the number of housing subsidies that they offered. Because the number of new tenant-based vouchers far exceeded the number of new public housing units, most FSS slots have been allocated to the voucher program. This rule was rescinded under QHWRA (Ficke and Piesse, 2004).

³⁵Author's calculations using data from Ficke and Piesse (2004) and from Lubell, Shroder, and Steffen (2003, Exhibit 1).

³⁶For a review and critique of the evaluations of these programs and a recommendation to use random assignment designs in future evaluations, see Bogdon (1999). A recent HUD-sponsored impact assessment of the FSS program for voucher holders is more comprehensive than past evaluations of FSS and uses a comparison group made up of non-FSS voucher recipients. The analysis is limited to single parents ages 25 to 44. Despite baseline comparability between the FSS and non-FSS groups on several important demographic variables, selection bias cannot be ruled out. As the report itself states: "Still, although the comparison group was similar to the FSS group in terms of age and family composition, it is possible that the groups differed in other characteristics that may have affected the outcomes discussed here. For example, because the comparison group members were not in the FSS program, they may be less motivated than were FSS participants" (Ficke and Piesse, 2004, p. 29).

The Moving to Work Demonstration

HUD's Moving to Work (MTW) demonstration, launched in 1997, has become a laboratory for trying a broader set of reforms intended to promote self-sufficiency among assisted tenants. As part of this initiative, 24 competitively selected public housing authorities have been granted far-reaching flexibility to combine their different streams of HUD subsidies and use them in any way that they think will encourage tenants to enter the workforce, free of many of the rules and regulations of typical housing programs. Variations in rent reforms that are being tried include delaying rent increases as income increases, reducing the rent-to-income ratio for a period of time, and setting flat rents or ceiling rents so that rents do not continue to increase with income. More recently, some public housing authorities have begun to institute time limits on housing assistance. While the Moving to Work demonstration has been an impressive source of creative reform, these efforts have not been assessed through rigorous evaluation. Thus, whether they have any substantial positive effects on residents' self-sufficiency remains unknown.³⁷

The Jobs-Plus Demonstration

An important exception to this lack of evidence can be found in the Jobs-Plus Community Revitalization Initiative for Public Housing Families (or "Jobs-Plus" for short), which was authorized under the Moving to Work legislation and involved an additional six public housing authorities.³⁸ The Jobs-Plus program was one of the most comprehensive employment interventions ever tried in public housing.³⁹ (See Box 4.) The program model included the following key elements:

- Assistance with **job placement and training**, from job coaches, job developers, and other staff conveniently located *on-site* at a small job center established within the housing development.
- **New rent rules** that helped make low-wage work pay by allowing working residents to keep more of their earnings. This usually took the form of a flat rent, with an income-based rent retained as a safety net option for those who lost jobs and would have had difficulty affording the flat rent.
- A **community support for work** component that involved residents in *neighbor-to-neighbor* information-sharing about work opportunities, rent incentives, and other program benefits.

³⁷Newman (1999). For a summary and assessment of this initiative, see Abravanel et al. (2004).

³⁸The 6 housing authorities operating Jobs-Plus, added to the 24 main MTW sites, brought the total number of demonstration sites to 30, the maximum number authorized by Congress. Jobs-Plus was jointly sponsored by HUD and the Rockefeller Foundation, with additional support from other public and private funders.

³⁹Findings on the impacts of this initiative are presented in Bloom, Riccio, and Verma (2005).

Box 4

Jobs-Plus Community Revitalization Initiative for Public Housing Families

Study sponsors: U.S. Department of Housing and Urban Development (HUD), the Rockefeller Foundation, and other public agencies and private philanthropies.

Evaluator: MDRC.

Target group: All working-age, nondisabled residents of selected low-work, high-welfare public housing developments.

Intervention: The program model had three main components: employment-related services, rent-based work incentives that allowed residents to keep more of their earnings than normal rules would have allowed, and activities to promote neighbor-to-neighbor support for work. It was implemented through a collaboration of local public housing authorities, local welfare and workforce development agencies, and residents' organizations.

Study design: Random allocation of eligible matched public housing developments to the program group or control group within each city, resulting in one Jobs-Plus and one or two comparison developments per city. Within this context, the study used a comparative interrupted time-series analysis to determine changes in employment and earnings trends for residents of the program and comparison developments before and after the start of Jobs-Plus.

Demonstration time period: 1998 to 2003.

Sample size: The main impact analysis sample included 4,774 residents aged 21 to 61.

Six sites: Baltimore, Maryland; Chattanooga, Tennessee; Dayton, Ohio; Los Angeles, California; St. Paul, Minnesota; and Seattle, Washington.

Impact findings: Three of the six study sites fully implemented and sustained the Jobs-Plus program for at least four years. In these sites, Jobs-Plus markedly increased the earnings of residents relative to the comparison group. These earnings gains averaged \$1,141 per year (a 14 percent improvement) over the study's four-year follow-up period. It is noteworthy that the size of the earnings gains *grew larger over time*, reaching \$1,543 (a 20 percent improvement) in the final year, with no sign of abating. Cumulatively over the four years, the earnings gains totaled almost \$4,600 per resident (including nonworkers) and nearly \$6,000 per *working* resident. Jobs-Plus's large positive earnings effects held for a wide range of residents: men and women, virtually all major ethnic and racial groups, people who had received and had not received welfare, and long-term and short-term public housing residents. Most striking were the especially large impacts for legal immigrant men. As a result of Jobs-Plus, some residents who were not working began to work, and many who were already working began to work more consistently, for more hours, or at better-paying jobs than they would have without the program.

A fourth site — Seattle — also fully implemented the program and generated positive effects on residents' earnings, but these effects were not sustained after the first year because the site began relocating residents as part of a federal HOPE VI project to tear down and rebuild the housing development.

Two other sites did not fully implement the program components, in part because of other priorities of the local housing authorities.

Jobs-Plus was not a limited-slot program; instead, it targeted *all* working-age, nondisabled residents of the housing development. It attempted to *saturate* the social environment within public housing with work-focused encouragement, information, incentives, and active assistance. Jobs-Plus was to be accomplished in each city through a local interagency partnership that involved the welfare department and the workforce development agency, along with the housing authority, which provided overall leadership. These partnerships also included resident representatives, whose voice and support were considered essential for such a significant intervention into their communities. Other social service, training, and employment agencies and schools also participated.

The evaluation of Jobs-Plus's effectiveness on residents' work outcomes combined a comparative interrupted time-series analysis with a development-level random assignment design. Within each of the six cities, two or three matched housing developments were placed in a pool, and one was then randomly selected to operate Jobs-Plus while the other one or two were allocated to the control group, which did not operate the program. The results of this study show conclusively that, when properly implemented, the program increased public housing residents' earnings, by an average of 14 percent over four years and by 20 percent in the last year. In dollar terms, residents in the Jobs-Plus developments, on average, earned over \$1,100 more per year than they would have earned without the program. For some groups, the earnings gains exceeded \$3,000 per year.⁴⁰

This success was brought about by a close collaboration among local welfare, workforce, and public housing agencies and is especially noteworthy because it was achieved in some of the poorest public housing developments in the nation, in very different cities, and for very different types of residents. These residents include black single parents in Dayton, Ohio; married Latino men in Los Angeles, California; and Southeast Asian refugee men and women in St. Paul, Minnesota.

The effectiveness of Jobs-Plus stands in marked contrast to the *absence* of labor market effects (at least in the short term) from an alternative approach tested by HUD's Moving to Opportunity demonstration.⁴¹ As described above, the MTO demonstration tested a strategy that sought to improve public housing residents' self-sufficiency, not through special services or incentives but, rather, through residential mobility. Residents were offered special tenant-based rent vouchers to subsidize their rent in the private housing market in low-poverty neighborhoods. The

⁴⁰These average earnings estimates are based on *all* working-age, nondisabled residents, not just those who were employed. Consequently, actual earnings gains accruing to those who worked were larger.

⁴¹Bloom, Riccio, and Verma (2005). For a helpful comparison of Jobs-Plus with Moving to Opportunity and another HUD demonstration focusing on transportation to suburban jobs (Bridges to Work), see Turner and Rawlings (2005).

designers of this approach hoped that these special rent vouchers would allow residents to move to areas where more jobs and better jobs would be more accessible to them and where they could become part of community-based social networks that had a better flow of information about job opportunities and were more supportive of work. The interim results show that, within the first four years of follow-up, the intervention had not improved residents' employment or earnings. Of course, mobility strategies like these take more time to make a difference on employment, and perhaps the demonstration's longer-term findings will later reveal positive effects. Or it may be that mobility strategies *by themselves* are unlikely to produce effects on work outcomes for public housing residents. If so, perhaps they would be more likely to produce such effects if, like Jobs-Plus, they included explicit services to help residents find, keep, and advance in work, along with extra financial incentives to provide further encouragement to do so.⁴²

Welfare-to-Work Evaluations

A number of random assignment evaluations of welfare reform strategies of the 1990s speak to the question of whether employment-focused interventions — particularly, welfare-to-work services and participation requirements and, in some cases, welfare-based work incentives — can improve the self-sufficiency outcomes of welfare recipients who also receive housing subsidies. The findings are fairly consistent, showing that welfare reform typically produced larger earnings impacts for residents who were receiving HUD-administered housing assistance than it did for those without housing assistance. This is clear from Table 1, which summarizes the results of such analyses completed through 2003. (Each earnings impact shown in the table reflects the estimated difference in cumulative average earnings between the program and control group members of the specified housing subgroup.) In fact, of the 10 different analyses listed there (counting the two Minnesota studies as one), 8 clearly show larger and statistically significant earnings effects for welfare recipients who were receiving some form of housing assistance at the time of enrollment in the study, compared with recipients who had no housing assistance. The one main example that runs counter to the overall pattern is from the evaluation of Delaware's A Better Chance program, where the difference in impacts across the housing subgroups is small. However, this program was not very effective for *either* housing subgroup, and the full study found that, by the second year of follow-up, its overall earnings effects had faded completely.⁴³

⁴²Bloom, Riccio, and Verma (2005).

⁴³Verma and Riccio (2003). See also Riccio and Orenstein (2003). It may be noted that, for the Columbus NEWS Integrated Case Management Study, none of the subgroup earnings impacts shown in Table 1 are statistically significant. However, the impacts for the two subgroups with assisted housing are still much larger than those for the no-housing-assistance subgroup. Furthermore, other data show that the program impacts on employment rates and average earnings in the last quarter of the follow-up period are statistically significant for the public housing subgroup but not for the Section 8 or unassisted subgroups.

Table 1

Impacts of Welfare Reform Interventions on the Average Cumulative Earnings of Welfare Recipients, by Housing Subgroup

Welfare Reform Intervention / Sample	Follow-Up Period (Years)	Housing Subgroup <i>(Numbers reflect differences in earnings between program and control group members within each subgroup category)</i>				
		Public Housing (\$)	Vouchers (\$)	Combined Section 8 (\$) ^c	Any Housing Assistance (\$)	No Housing Assistance (\$)
Minnesota Family Investment Program / <i>interim findings; no HUD data^a</i>	1.5	NA	NA	NA	2,041***	429
Minnesota Family Investment Program / <i>long-term findings; with HUD data^b</i>	3	NA	NA	NA	5,473***	603
Indiana Manpower and Comprehensive Training Program (IMPACT) / <i>later cohort^f</i>	2	NA	NA	NA	1,461*	-202
Delaware's A Better Chance ^e	2	NA	NA	NA	591	318
Connecticut Jobs First						
First comparison ^b	4		NA		3,965**	1,658
Second comparison ^b	4	3,564	3,368			1,658
Indiana Manpower and Comprehensive Training Program (IMPACT) / <i>early cohort^c</i>	5	2,209	2,549	NA	NA	2,003***
Atlanta NEWWS / <i>Labor Force Attachment strategy^d</i>	3	2,115***	NA	1,801**	NA	1,585*
Atlanta NEWWS / <i>Human Capital Development strategy^d</i>	3	1,762***	NA	1,471*	NA	853
Columbus NEWWS / <i>traditional case management strategies^d</i>	3	2819*	NA	-20	NA	140
Columbus NEWWS / <i>integrated case management strategies^d</i>	3	2,239	NA	460	NA	992

(continued)

Table 1 (continued)

SOURCES: Adapted from Verma and Riccio (2003, Table 6.1).

^aMiller (1998). “HUD data” refers to HUD administrative records used to determine sample members’ housing status at the time of random assignment.

^bVerma and Riccio (2003).

^cLee, Beecroft, Khadduri, and Patterson (2003).

^dRiccio and Orenstein (2003). NEWWS refers to the National Evaluation of Welfare-to-Work Strategies.

NOTES: All results are based on a random assignment research design in which eligible people were randomly allocated across program and control groups for each study. The earnings impacts shown in each cell of this table were regression-adjusted using ordinary least squares, controlling for pre-random assignment characteristics of sample members.

NA = not applicable.

Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; and * = 10 percent.

^eThe Combined Section 8 category includes Project-Based Section 8 assistance and Section 8 vouchers.

These welfare reform findings lend credence to the idea that, at least for welfare recipients, adding an employment-focused intervention to housing assistance can improve earnings outcomes. And taken together with the Jobs-Plus results, they are a reason for encouragement that effective work-focused interventions for recipients of various forms of housing assistance can be designed and operated. Still, many questions remain about how to approach this challenge — and build reliable evidence of effectiveness — from within the housing assistance world.

Elements of a Comprehensive Random Assignment Evaluation

A randomized controlled trial, where feasible, is a highly credible way of determining whether a social intervention is successful in producing its intended effects. In a simple design, individuals (or groups) slated for an intervention are assigned through a lottery-like process to a program group that is subject to the intervention or a control group that is not. Because this assignment is made purely on the basis of chance, no systematic differences will distinguish the two groups at the beginning of the study (assuming sufficiently large sample sizes). For example, both the intervention group (the “program group”) and the control group will have similar proportions of people with measurable traits (for example, age, education, prior work experience) and unmeasurable background traits or circumstances (such as “ambition to succeed,” “people skills,” technical adeptness, supportive family environments) that might affect future employment, earnings, and other outcomes of interest. Thus, if subsequent average differences in outcomes do emerge between the groups, those differences can be attributed with confidence solely to the intervention.

One of the first responses to the idea of using random assignment to study a social program is that “it’s not ethical.” It is true that in many situations random assignment cannot be defended on ethical grounds. For example, it would not be ethical to use this methodology to assess the labor market effects of the current Earned Income Tax Credit (EITC), which qualifying individuals are already entitled to under current law. Denying access to this national benefit to a randomly selected subgroup of eligible taxpayers would be considered unfair. However, not all social programs are entitlements that are funded at levels permitting all who qualify to participate. That is true of assisted housing in general, for which, in many cities, long waiting lists exist, and it is true of many government-funded employment initiatives. In circumstances where the demand for a service or benefit outstrips the supply, random assignment can be viewed as a fair way of allocating a scarce resource, because all applicants have an equal chance of obtaining it. Indeed, some housing authorities have been known to randomly assign applicants to their placement on a waiting list to determine who has priority for assisted housing.

In many cases, randomly assigning individuals to a program or control group is simply not feasible. For example, because Jobs-Plus was designed as a saturation initiative that targeted all working-age, nondisabled residents and aimed to build a work-promoting social environment within public housing, it was not feasible for the evaluation to isolate from the intervention a subset of households within a public housing development to serve as a control group. Similarly, in schools, an education reform agenda may apply to the whole school, so that it is not feasible to isolate some students within the school to serve as a control group that would be “untouched” by the reforms. In these and other circumstances, however, it may be possible to make the institution or group the unit of randomization. Thus, in Jobs-Plus, entire housing developments within the selected cities were randomly allocated to the program or control groups. And in a number of education evaluations, whole schools have been randomly assigned to the program or control group. In some studies, whole communities have been randomly assigned.⁴⁴

Where feasible and implemented correctly, random assignment offers a highly credible means for determining the effectiveness of a social intervention. However, it is only a foundation. Maximizing the learning from such a trial calls for a comprehensive evaluation strategy with several strands of research, such as (1) an impact analysis, (2) an implementation and process analysis, and (3) a benefit-cost analysis.

- An **impact analysis** measures the differences made by the program on self-sufficiency and quality-of-life outcomes. In the best impact studies, a wide range of outcomes is measured using data collected from administrative records (such as unemployment insurance wage records and welfare agency records) and surveys of residents. An effect, or impact, is the difference in an

⁴⁴For a discussion of group-level randomization, see Bloom (2005).

outcome — for example, in average earnings after random assignment — between the program and control groups. Thus, an impact analysis would answer such questions as: To what extent does the intervention improve job entry, employment retention, career advancement, and total income for the program group relative to the control group? How much does the intervention reduce the need for welfare and improve various aspects of child and family well-being?⁴⁵ Which types of effects emerge in the short term, and which ones take longer to materialize? How long do the observed effects last? Which approaches are most successful in producing positive impacts? Through carefully specified subgroup analyses, an impact study can determine whether the effectiveness of an intervention is similar for different types of residents (for example, those with different types of barriers to work or different degrees of disadvantage) or whether the effects are much larger for some than for others. This is important, given the great heterogeneity of assisted housing families and the dynamics of their lives.⁴⁶

- An **implementation and process study** documents and assesses how — and how *well* — the intervention model was put into practice, how it functioned “on the ground,” and the challenges entailed in transforming the intervention concept into an operational program. This analysis answers such questions as: Was the treatment implemented as planned, permitting a “fair test” of the underlying concept? Which services were provided, how well were they delivered, and how widely and intensively were they used? And, recognizing that members of the control group might find their way to similar alternative services on their own, to what extent did the intended difference in service receipt between the program and control groups actually materialize? Given that the intervention in question may represent a “bundle” of attributes (say services of different kinds plus financial incentives), implementation and process analyses are often essential in getting behind the complexity of the model, showing how it functioned in the real world, and producing insights into what may or may not have contributed to the program’s effects — or

⁴⁵None of the past evaluations of FSS or other housing-based self-sufficiency programs have estimated effects on child outcomes. Moreover, according to Newman (2005), only two studies have examined the effects of assisted housing on children, and both focused on public housing: Currie and Yelowitz (2000), who studied grade retention; and Newman and Harkness (2002), who analyzed a range of long-term outcomes including work, earnings, and educational attainment after age 20. As Newman notes, both studies yielded the unexpected finding that public housing children experienced better outcomes than their unassisted counterparts. Work now under way is examining both short- and long-term effects for children.

⁴⁶Shroder (2002).

why it did not work. These analyses typically draw on a combination of qualitative field research and quantitative data on service receipt from agency records and surveys of residents.

- A **cost-benefit study** draws on both the impact and the process analyses to estimate the resources used by the program and the overall economic outcomes that it generates. It examines such issues as what the cost is to operate the program and which factors drive those costs, the added investment above the cost of any relevant alternative services received by the control group, and how the economic outcomes produced by the program stack up against the net investment made in the intervention. In comparing costs and benefits, it is necessary to distinguish “from whose perspective” the comparison is being made. It is common for such analysis to examine separately the “return on investment” to government budgets and taxpayers and also the gains and losses to the program’s participants. In the case of housing employment initiatives, it would be relevant to extend such an analysis to include the perspective of housing authorities as well.

The overall quality and usefulness of a random assignment study depend on a number of factors. First and foremost, care must be taken to ensure, as much as possible, that the interventions are implemented as designed and that the sites in which the experiment is carried out reflect common conditions. Key, of course, is implementing the random assignment process correctly in the field. This means ensuring not only that the allocation of sample members is truly random but also that the treatment differential between the program and control groups is maintained over time — for example, that the control group members do not inadvertently or deliberately get the treatment reserved for the program group for a specified period of time and that there is no differential attrition in the collection of follow-up data on the program and control groups that could introduce post-random assignment bias. This requires careful monitoring in the field. Also important is having an adequate-size sample to make it possible to determine whether observed program effects are statistically significant (that is, not chance outcomes) and to measure the program’s effects across a variety of important subgroups. Multisite studies are also valuable for assessing whether the intervention can work in a variety of settings and whether some types of practices or implementation conditions make the intervention more effective or less effective.⁴⁷

⁴⁷All these considerations can add appreciably to the cost of a randomized control trial. At the same time, it is important to recognize that often it is the cost of collecting and cleaning survey and administrative records data that accounts for a large portion of evaluation costs. But these are not costs that are peculiar to randomized trials; they would also be incurred for a nonexperimental evaluation that used comparison groups and required similar types of data. The added cost of the randomization process — and maintaining the integrity of the ex-
(continued)

Randomized Trials Within the Housing Choice Voucher Program

It is possible to envision a number of research demonstration projects for building evidence about ways to improve work outcomes for residents of assisted housing. This section discusses some possible interventions for Housing Choice Voucher recipients that could be evaluated through randomized controlled trials.⁴⁸

There are at least three reasons to focus work-promoting efforts on the voucher program. First, it is the best place to find working-age, able-bodied adults who receive HUD-administered housing assistance vouchers. In fact, able-bodied, working-age residents make up 61 percent of all voucher recipients, compared with 50 percent of public housing residents and 26 percent of residents of private project-based assisted housing (that is, Project-Based Section 8).⁴⁹ Overall, vouchers serve more families headed by an able-bodied, working-age resident than the other two programs combined.

Second, the voucher program is the only program among the three that has grown substantially over the last decade. Except for replacement of some aging units, largely through the HOPE VI program, there has been little new public housing construction. In fact, because of demolition, the total number of public housing units is shrinking. In the face of a strong rental market, Project-Based Section 8 is seeing increased expirations and nonrenewals of government contracts with private owners. Tenants who are displaced from the latter programs are typically transferred to vouchers.

Third, the Quality Housing and Work Responsibility Act (QHWRA) of 1998 requires that 75 percent of vouchers be reserved for families with incomes below 30 percent of the area's median income, which is roughly the poverty line. In contrast, at least 40 percent of public housing units must be reserved for families at this income level. For privately owned assisted housing subsidized under the Section 8 project-based program, income-targeting requirements are the same as for public housing. This raises at least the possibility that the tenant mix will

perimental design in the field — may, depending on the study, be a relatively small or modest share of the overall research costs in a comprehensive evaluation.

⁴⁸The types of experiments discussed here take the existence of housing assistance for granted. They ask: For those receiving housing assistance, can employment-focused interventions improve various work and life outcomes? In contrast to the Welfare to Work Vouchers (WtWV) experiment, it does *not* ask: What are the effects of vouchers alone on self-sufficiency outcomes, relative to the control condition of no (or a much lower likelihood of receiving) housing vouchers or other housing assistance? From a methodological standpoint, focusing on recipients of housing vouchers who have already gotten landlords to accept their vouchers would avoid the problem that both the WtWV and the MTO evaluations confronted — the fact that a substantial proportion of program group members in each study failed to find an apartment for which they could actually use the voucher.

⁴⁹Author's calculations using data from Lubell, Shroder, and Steffen (2003, Exhibit 1).

shift over time, with the most disadvantaged segments of the working-age population being increasingly concentrated in the voucher program.⁵⁰

Evaluating the Current FSS Program

As discussed above, a large federal program designed to increase the self-sufficiency of Housing Choice Voucher recipients already exists: the Family Self-Sufficiency (FSS) program. Although not a place-based intervention, FSS, like the successful Jobs-Plus program for public housing residents, combines employment-related services and financial work incentives. However, the effectiveness of this program, as well as its economic costs and benefits, remains largely unknown because of the very significant limitations of its prior evaluations. Subjecting FSS to a comprehensive, random assignment test could thus add substantially to the evidence on employment interventions in the assisted housing field.

The random assignment process could be set up in a number of ways, but any of these would require that there be substantially more people who are eligible for the program who want to join it than there are FSS slots available. With a surplus of applicants, random assignment would be a fair way to ration the limited supply of slots.

Assuming that an adequate supply of “vacant” FSS slots exists (which might require an overall expansion of the program capacity in evaluation sites), setting up random assignment would begin with the housing authority’s identifying all eligible working-age residents who are receiving vouchers but who are not currently enrolled in FSS (the majority of working-age voucher holders). A recruitment campaign would encourage eligible candidates to apply for a *chance* — as in a lottery — to secure one of the limited number of new FSS slots. Then, half of those who volunteer would be randomly assigned to the program group, and the remainder would be placed in the control group. Members of the control group would be restricted from participating in FSS during the study period. Because the allocation of people to the program and control groups would be determined randomly, the two groups would be similar, on average, in terms of measurable and unmeasurable background characteristics (assuming adequate sample sizes). In this way, the true effect of the intervention itself could be estimated with confidence by comparing the subsequent outcomes that the two groups achieve. This approach is especially important for tests of voluntary programs like FSS, because certain difficult-to-measure traits (for example, “an ambition to succeed”) that might lead some housing voucher

⁵⁰Joseph Harkness (personal communication).

recipients to volunteer for FSS in the first place are traits that might also affect their success in the labor market.⁵¹

To assess the effectiveness of FSS nationally, such an experiment would ideally be conducted in a representative and randomly selected subset of housing authorities that operate FSS programs. However, practical and other considerations may rule this out. If so, it would be essential to select at least a variety of locations that broadly capture the diversity of local conditions in which FSS programs operate and the diversity of the tenant populations that they serve. Of course, the greater the number of locations and conditions, the more challenging and expensive the study would be in terms of recruiting sites and households to participate, setting up and sustaining a proper trial, and collecting the necessary data. But, in general, the larger and more diverse the set of locations involved, the better would be the evaluation's ability to speak to the possible generalizability of the results beyond the study sites, and the more opportunity it would have to determine whether the intervention worked for very different types of people and places served or, in contrast, was effective only under certain conditions or for certain types of residents, but not overall.

Evaluating an Enhanced FSS Program

Despite the appeal of the FSS program in its current form, it is possible to envision a number of modifications and enhancements that have the potential to make it a more powerful intervention. These would include changes in the program's incentives structure that might broaden its appeal and strengthen its potential to induce bigger changes in residents' efforts and capacities to advance in work.

The current FSS program takes an escrow-based approach to rewarding work. Although clear evidence is lacking, it may be that the long delay in getting access to the escrow savings may limit the appeal of the program and its work incentives to a narrower — and more selective — slice of the potentially eligible population of residents than would other alternatives that offer more immediately accessible incentives. It is possible, for example, that residents who are most motivated to take advantage of the FSS program are also the ones who need the least assistance in order to work steadily — in other words, they may be just as likely to work (or to work consistently, full time, or to seek better employment) as they would without the program. Although the escrow strategy increases participants' assets — a positive accomplishment — it is not clear that it changes participants' *work behavior and opportunities* or that it is as effective as a behavior-changing intervention as alternative strategies might be that provide more income immedi-

⁵¹Precisely because they are so difficult to measure, nonexperimental statistical modeling methods cannot adequately control for the effects of these traits on future self-sufficiency outcomes. For a comparison of experimental and nonexperimental impact estimates for various initiatives, see Bloom (2005).

ately. It may be that the escrow strategy primarily rewards behavior that would have occurred anyway. (It is important to note that if that were the case, this should not be considered a “failure,” particularly if one goal is to transfer more resources to people who are working but who still have quite low incomes.)

Another reason for rethinking the FSS model is that housing authorities vary widely in the scope and intensity of services that they offer participants through their partnerships with other agencies and local service providers. But there is nothing inherent in the FSS model itself that calls for postemployment support, such as job coaching to promote advancement among working participants and assistance in accessing other financial work supports for which working residents may be eligible (such as food stamps, Medicaid, the State Children’s Health Insurance Program [SCHIP], child care subsidies, the EITC, and the Child Tax Credit). In addition, there are no immediate, direct incentives for skills-building. While escrow funds can be used to pay for extra education or training, it can take time for these to accumulate income gains through earnings.

With these limitations in mind, an enhanced FSS program — referred to here as “FSS-Plus” — could incorporate the following features as part of the core program model:

1. A **new rent-based work incentive** that would change how earnings are treated in calculating rent. The main objective would be to provide residents with a more immediate gain from increasing their labor supply and wages than is possible through the FSS escrow strategy. Such a rent policy could be structured in a number of different ways, as illustrated in Box 5.
2. **Assistance in accessing existing financial work supports** that help make low-wage work “pay.” As mentioned above, these work supports include the EITC, Child Tax Credit, food stamps, child care subsidies, and Medicaid/SCHIP. Educating participants on the potential value of these financial work supports when combined with earnings, assessing participants’ likely eligibility for them, and assisting participants in getting them would help working residents take full advantage of the current highly complicated patchwork of existing government make-work-pay benefits, which are thought to be substantially underused by eligible low-income workers. Increased use of such benefits could put more money *quickly* into the pockets of these workers.
3. **“Fill-the-gap” child care and transportation subsidies** where existing resources for such assistance for low-income workers is inadequate to meet the demand. This assistance would help reduce the “cost” of working and, in that way, would also help to make low-wage work “pay.”

Box 5

Rent Reforms to Promote Increased Work and Earnings

One approach to work-promoting rent policies would be to model rent rules after the flat rents that were used successfully in Jobs-Plus or like the rules adopted by a number of housing authorities in the Moving to Work (MTW) demonstration. Under such a policy, residents could work more hours, obtain higher wages, and have other working-age household members work without having their rent go up — just as in the private rental market. (Periodic rent increases could be made to account for inflation, or rents steps could be introduced that gradually reduced the generosity of the subsidy.) For residents who lose jobs, the option of reverting back to a rent that is based on a percentage of income (perhaps a simplified version, relative to current rules, as suggested below) could be retained as a safety net feature.

Other options for rent-based work incentives might include a broadly targeted earnings disregard, according to which a certain proportion of earnings would not be counted when calculating a person's rent under the 30-percent-of-income rule, as proposed by a recent congressional housing reform bill (the Section 8 Voucher Reform Act). Another proposal would establish a simplified and limited set of tiered rents modeled after the Low-Income Housing Tax Credit's rent structure, which classifies residents into five income categories based on the same criteria used to determine initial eligibility for subsidized housing (a percentage of the area's median income) and tied to the apartment-unit size. Changes in income within each broad tier would not affect a family's rent (Public Housing Authorities Directors Association, 2005). An approach that seeks to be cost-neutral for housing authorities would set target earnings levels for residents to aim for, above which any additional earnings gains would be diverted into Family Self-Sufficiency-style escrow accounts (Lubell, 2006). This strategy might get residents to "aim higher" in terms of their earnings goals, though it would not address the issue of delayed reward inherent in the escrow strategy. Each of these alternative approaches carries with it advantages and disadvantages, including differences in administrative burden for the housing authorities, appeal to residents, and overall cost.

It would also be necessary to decide whether the rent-based work incentives should come with a time limit attached. For example, a flat-rent option could remain available to each participant for up to five years — the same amount of time that the escrow option is available under the current FSS program. After that point, rent determination would revert back to the percentage-of-income formula. Some residents who have gotten onto a path of sustained higher earnings would at that point no longer be eligible for a housing voucher, freeing that subsidy up for other low-income people. A goal of the FSS-Plus program would be to maximize the number of participants in this category by the end of the five-year period. A risk, of course, is that a certain number of residents would reduce their "work effort" after the incentive expired, in order to preserve a rent subsidy. However, without an expiration date for the flat-rent incentive, those lucky enough to obtain a housing voucher would never earn their way off, no matter how much their earnings grew.

This might be addressed by following the time-limited rent incentive with a modified FSS approach. After the time limit, residents would again have their rents based on a percentage of their income, but any earnings-based rent increases above the flat rent could be diverted into an escrow account. However, the escrow funds might be made 100 percent redeemable only on moving out of assisted housing, and they would be only partially redeemable otherwise. The objective here would be to create an incentive for residents to move out of subsidized housing eventually, freeing up resources for other people in need, and to leave them with extra resources at the time of that transition.

4. **An expanded scope of job coaching with a strong focus on employment retention and advancement** (such as those being tested in demonstration projects under way in the United States and the United Kingdom).⁵² These services would aim to help working residents stay employed and also acquire occupational skills, knowledge of the labor market, capacity for identifying better job opportunities, and a vision and confidence to move to better opportunities. The job search guidance would be informed by an understanding of how a particular job’s earnings would affect the worker’s *net income*, taking into account the interaction between earnings and the financial work supports described above (which can be aided with a special computerized “income calculator”).⁵³
5. **Financial incentives** to encourage participants to take up job-related **education and training while employed**. This could help them prepare for better-paying jobs in occupations with strong labor market demand. The incentives could take the form, for example, of tuition assistance and a bonus paid for completing approved courses while also holding a part-time or full-time job.⁵⁴
6. A new **asset-building component that is not tied to the rent structure**. This could help workers accumulate resources and provide a cushion against possible future spells of unemployment or poverty and a foundation for continued improvement in their economic security. It might be used, for example, to help residents save for a “rainy day,” prepare for a transition into unsubsidized housing or home ownership, pay for future education or training or self-employment opportunities, or even build resources for retirement. FSS-style escrow accounts are one type of asset-building strategy, of course, and they were the centerpiece of the FSS program. However, if other types of work-based rent reforms are adopted as an alternative, a program could still encourage residents to save for the future through other mechanisms. For ex-

⁵²For information on large-scale random assignment tests of the Employment Retention and Advancement (ERA) demonstration, the United Kingdom Employment Retention and Advancement (UK ERA) project, and the Work Advancement and Support Center (WASC) demonstration — all of which target low-income people — see www.mdrc.org.

⁵³For examples on the design and use of such a tool, see Anderson, Kato, and Riccio (2006) and Gardenhire-Crooks (2004). Providing job coaching and case management through a housing-based employment initiative raises the important practical question concerning how to deal with the fact that many residents may already have case managers in other programs in which they are involved, such as welfare-to-work programs. Resolving potentially overlapping services requires careful coordination across agencies. For examples of how this was addressed in Jobs-Plus, see Kato and Riccio (2001).

⁵⁴This strategy is being tested in the UK ERA project. See www.mdrc.org.

ample, asset-building strategies could include Individual Development Accounts (IDAs) or other savings accounts in which savings are rewarded with a small matching contribution. The matched portion of these accounts could be made payable on exiting from the housing assistance program or payable for expenses relating to education, training, or work. The program could also offer banking services (for example, free checking accounts and check cashing), debt counseling, and other financial planning services.

Taken together, this varied list of features of an enhanced FSS program would seek to address a broad range of conditions that discourage or make it difficult for many residents to become more self-sufficient.⁵⁵ As such, it is likely to be a more intensive intervention than the typical FSS program implemented across the country. It might also be more costly. Therefore, it is important to consider how to contain those costs. One way, as noted above, might be to shorten the time that participants have access to the rent-based work incentives — perhaps to three years rather than the current FSS program’s five-year maximum. It may also be worth considering shortening the length of eligibility for other services. A shorter period of eligibility per resident would allow a given number of staff to serve a larger number of people over time.

Evaluating the Individual *Components* of an FSS or FSS-Plus Program

One evaluation strategy is to evaluate FSS or FSS-Plus in its entirety. This makes sense if it is hypothesized that the program model contains a variety of features that work together to produce positive earnings and other effects. Thus, the impact evaluation question would be: Did the program, as a whole, succeed? However, there may also be reason to measure how much, if at all, certain core features of the program contribute to its success. For example, could the program (if it does succeed) be just as effective if it offered only the financial incentives, with no additional services? Or if it offered only the work-related services, with no incentives? A streamlined program would be less costly to operate, so knowing whether the added components of the program actually add value to its effectiveness (and not just to its costs) would be important.

Constructing an evaluation to provide this type of evidence is more complicated because it requires the creation of multiple treatment groups as well as a control group. Ideally, a multiple-group random assignment design would be implemented within each of several sites. For example, eligible applicants could be assigned at random to one of the following four groups: Group 1, which receives only the financial incentives; Group 2, which receives only the work-related services; Group 3, which receives the comprehensive program offering the full

⁵⁵Not all eligible residents would need or benefit from all these features, but having a comprehensive set of features increases the chances that, in a broad group of residents, everyone would be helped by at least something the program has to offer.

package of services plus incentives; and Group 4, which would be the control group that is offered no special services or incentives.

This type of test would allow a comparison of each alternative relative to the others. However, practical considerations — such as difficulty generating a large enough sample for all four groups or limited local capacity to operate more than one form of intervention at a time — might rule out this design in many locations. An alternative would be to test each of the three treatment group options separately against the control group in different sets of sites. Comparing the advantages or disadvantages of any given intervention strategy relative to the others would be more complicated and leave more uncertainty, but it could still be highly informative.

Evaluating Alternative Incentives Strategies Within FSS or FSS-Plus

Another way to build evidence on the effectiveness of FSS or related strategies is to conduct a more limited randomized control trial that focuses only on alternative rent-based work incentive strategies within the current FSS program. For example, one such test would compare a group of Housing Choice Voucher recipients that is offered the current FSS program with the escrow-based incentive plan and a group that is offered FSS with an alternative incentive plan, such as a type of flat-rent strategy, as described above, or some other alternative. This would show whether or not alternative approaches would convince more residents to enroll in FSS and would improve their employment, earnings, savings, and other outcomes.

Evaluating an “MTO-Plus” Strategy

As discussed above, evidence available so far from the Moving to Opportunity demonstration suggests that simply relocating public housing residents to low-poverty communities with the help of special rent vouchers does not improve their self-sufficiency outcomes. However, this leaves open the question of whether some version of such vouchers *combined with* employment-focused services could be effective (an approach that might be dubbed “MTO-Plus”).⁵⁶ With these considerations in mind, one can easily envision adapting one of the FSS-related tests outlined above to incorporate such a strategy. For example, eligible public housing or Project-Based Section 8 tenants would be randomly assigned to a program group that received MTO-Plus (the special rent voucher and relocation assistance combined with special self-sufficiency services) or to a control group that received neither. This would make it possible to learn whether the package of such assistance can boost self-sufficiency in ways that the

⁵⁶Another consideration is whether, to ease the challenge of accessing jobs, special vouchers should be targeted toward neighborhoods that have good access to the kinds of jobs that assisted tenants could fill (not all low-poverty communities have such jobs). See Goering (2005).

original program strategy did not and, down the road, could also generate other positive outcomes for families and children.⁵⁷

An Evaluation Using a “No Voucher” Control Group

The above ideas all focus on people who receive housing subsidies. The intent is to determine whether certain kinds of additional interventions can improve the self-sufficiency outcomes of voucher holders. Another approach would be to test more explicitly for whether — and, if so, by how much — a self-sufficiency intervention packaged with a voucher program would counteract any negative effects on employment outcomes that vouchers alone might have. To answer this question, a random assignment study would focus on low-income people who are not receiving any housing subsidies but who would be eligible for Housing Choice Vouchers. These voucher applicants would be assigned randomly to one of three research groups: (1) a group that is offered the full package of a voucher plus the self-sufficiency assistance, (2) a group that is offered only a voucher, and (3) a control group that is offered neither. Such a study may show whether, in order to promote self-sufficiency, a voucher *must* come with other services and incentives designed specifically with employment-related outcomes in mind.

Randomized Trials for Place-Based Housing Employment Initiatives

Under certain conditions, the effectiveness of employment interventions that are based in and targeted toward residents of assisted housing developments can also be evaluated through randomized controlled trials. These would include interventions for residents of traditional public housing, Project-Based Section 8 developments, and HOPE VI developments (where subsidized units are mixed together with unsubsidized rental units and privately owned units). Depending on the type of intervention and receptiveness of local housing officials and residents, either individual-level or group-level randomization could be used. For some types of interventions, however, only group-level randomization would be appropriate.

Evaluating Rent Reform for Place-Based Assisted Housing

The Jobs-Plus evaluation showed that a comprehensive and saturation-based employment intervention in public housing that includes a change in rent rules to promote work can succeed. That evidence provides an empirical justification for a wider replication of the program, especially in high-need public housing developments. However, the cost of Jobs-Plus

⁵⁷Considering that less than half the MTO program group were able to use the vouchers that were offered to them, any subsequent test of an MTO-type intervention should also carefully consider how to improve lease-up rates.

makes it unlikely that the federal government would implement the program on more than a limited scale in the current federal fiscal environment. This makes it important to consider whether, in many public housing developments, a less comprehensive intervention can also produce substantial earnings effects and achieve other positive outcomes.

Of particular interest is whether rent reform — a critical component of Jobs-Plus — could, *by itself*, promote greater employment and earnings among residents than occurs in the absence of such reform (beyond those reforms already enacted under QHWRA).^{58,59} Many experts believe that it may. Indeed, rent reform continues to be the most widely advocated self-sufficiency reform measure for public housing. As mentioned above, it is a core feature of the housing reforms implemented at the Moving to Work sites; it is strongly urged by tenant advocates and industry trade groups (although they may differ substantially on the *types* of reforms they recommend); and it is an important feature of new legislative proposals for housing assistance reforms.⁶⁰

Yet, there is no solid evidence that rent reform by itself would improve residents' employment and earnings. The patterns of results in the Jobs-Plus evaluations suggest that that program's rent-based work incentives were certainly a major contributor to its overall success, but other features of the program appear to have mattered as well. Unfortunately, that evaluation was not able to disentangle definitively the effects of the rent-based work incentives from the program's other components. Thus, the potential *independent* influence of rent reform on self-sufficiency and other outcomes remains a critical open question. For this reason, it would be valuable to make it the centerpiece of a future, carefully evaluated employment initiative in public housing.

The question of whether rent reform could improve residents' self-sufficiency is also an important one for Project-Based Section 8 and HOPE VI developments. In both these types of settings, income-based rents, like those used in traditional public housing, are still the norm for tenants who live in subsidized units. Because in many cities the social environment within Sec-

⁵⁸Among other things, QHWRA's rent reform provisions require housing authorities, when computing existing residents' rent, to disregard — for certain types of residents — any increase in earnings resulting from employment for 12 months. In such situations, the rent is held constant for a year, despite the increased income. In the next 12 months, the rent increase is to be limited to half the amount it would normally be under traditional rent rules. Qualifying residents include those who had been receiving TANF in the prior six months, those who have been unemployed for at least a year, and those who increase their earnings while participating in a qualifying employment training or self-sufficiency program. Many experts believe that these rent-based work incentives are complex to administer and are unevenly promoted and implemented across housing authorities. Public housing authorities must also establish flat-rent options, which are generally set at market rates.

⁵⁹For an in-depth discussion of the implementation of Jobs-Plus rent-based work incentives and residents' use of them, see Gardenhire-Crooks (2004).

⁶⁰See, for example, the U.S. House of Representatives bill called "the Section 8 Voucher Reform Act" (SERVA) (H.R. 5443); the House and Senate bill called "the State and Local Housing Flexibility Act" (S.711, H.R. 1999); Public Housing Authorities Directors Association (2005); and Solomon (2006).

tion 8 and HOPE VI developments may differ substantially from what is found in many traditional public housing developments, the “added value” of rent reform in promoting self-sufficiency — if there is any — may not be the same across all three forms of place-based housing assistance. Thus, evidence about the effectiveness of such reforms should be gathered separately by type of housing development.

It would be challenging, but not impossible, to evaluate such rent reform for place-based housing assistance using a randomized controlled trial. Such an evaluation could be done through either a group-level or an individual-level random assignment design. A *group-level* design (as used in the Jobs-Plus evaluation) would involve identifying a pool of eligible developments in a given city or a set of cities and then randomly allocating some to a program group and some a control group. All eligible residents of the complexes assigned to the program group would be entitled to have their rents determined according to the new policy being tested (and promotional efforts would be initiated to “get the word out” about the advantages of the new policy), while those in the comparison developments would continue to be subject to existing rules. A more ambitious study (requiring a larger sample of developments) could adopt a multiple-group design to test the relative merits of alternative rent policies. For example, one group of randomly selected developments might implement a flat-rent strategy (with income-based rents as a safety net feature); a second group might implement a new earnings disregard policy; and the third group could continue under current rules.

In principle, the selection of housing developments for such trials could be made from a pool of qualifying apartment complexes affiliated with a nationally representative sample of public housing authorities. However, this may not be feasible or practical, for the same reasons noted above in regard to evaluations of FSS. In that case, it would be important to pick a set of housing developments that reflects at least to some degree the diversity of local settings and tenant populations found across the country.

A different kind of evaluation strategy would be to use an *individual-level* random assignment research design to test an alternative rent policy for a limited number of households *within* a single housing development. Some eligible households would be randomly selected to have their rent determined by the new policy, and the remaining households would continue to be subject to current rules. (Again, though, such a study would ideally include a variety of housing developments operating a similar test but under different local conditions.)

Individual-level random assignment within a given housing development would present special challenges. Indeed, convincing housing authorities and residents to participate in a study that has neighbors who are in the same complexes being subjected to different rent policies might seem especially daunting. However, this may not be an insurmountable obstacle. Cooperation might be forthcoming if administrators and residents understand that the number of households

that could be offered the new and presumably more “favorable” rent options is limited due to limited HUD funding but that all qualifying households have an equal chance of accessing them. Also, those not selected for the program group would be no worse off, because they would continue to be subject to the existing rules. The fact that some residents would be allowed to pay rent under a different set of rules than their neighbors are subject to might also not be as problematic as it might first appear. As it is, different residents in the same kinds of housing units already pay very different rents in accordance with their very different personal income levels.

Evaluating a Services-Plus-Incentives Strategy for Place-Based Assisted Housing

It may be that rent reform, by itself, would not have as large an effect on self-sufficiency outcomes as it would if packaged with employment-related services. As discussed above, this is part of the theory behind the FSS model. Also, incentives and services make up two of the three core components of the successful Jobs-Plus model. Thus, random assignment trials could be usefully deployed to test whether an incentives-plus-services strategy would be effective in the context of Project-Based Section 8 and HOPE VI developments, or in a broader range of public housing developments than were included in the Jobs-Plus trial.

Place-based housing assistance through Section 8 and HOPE VI also creates an opportunity to incorporate some of the place-based elements of a Jobs-Plus program into an employment intervention. For example, this could include a community support for work component, in which residents reach out to and support their neighbors’ efforts to join and improve their standing in the labor market. Moreover, following the Jobs-Plus example, the initiative could adopt a “saturation” targeting strategy in which *all* eligible residents would become the focus of the intervention. It should be noted that, for this type of intervention, individual-level random assignment would be impossible. Because a saturation intervention involving community support for work strategies aims to change the whole environment within public housing and to tap residents’ social networks to help promote work, it would be impossible to isolate a control group of residents within the targeted development who would be left “untouched” by the intervention. In this case, randomly assigning entire developments — instituting the intervention in some and not in others — would be the only appropriate method for a randomized controlled trial.⁶¹

⁶¹If only a relatively small number of developments and cities were available for randomization, the Jobs-Plus evaluation strategy, which combined group randomization with a comparative interrupted time-series analysis, may be a reasonable alternative.

Assessing Development-Level Effects

In evaluations involving random assignment of whole developments, it becomes possible to assess program effects not only on individual residents — which requires following individuals even if they move away from the place — but also on the places themselves. For example, from the perspective of the place, the evaluation could test (as the Jobs-Plus evaluation did) whether the proportion of working people or the average earnings among tenants increases from year to year, contributing to making the place a more mixed-income development. This result is more likely to occur, of course, when residents who are helped by the employment intervention do not quickly move out of the development, so that their units are not filled by incoming residents with lower employment rates or earnings.⁶²

Conclusion

This paper has shown that although the primary mission of government-subsidized housing is to increase access to safe, decent, affordable housing, it may also be an important platform for reaching and helping many low-income people improve their economic self-sufficiency. But whether it really can serve this purpose is far from certain. There is no solid evidence that housing assistance alone improves the labor market outcomes of those who are fortunate enough to obtain it. There is also a dearth of reliable evidence on whether housing assistance that is packaged with work-promoting interventions makes a difference. Importantly, the Jobs-Plus evaluation shows convincingly that at least one approach to offering self-sufficiency services in combination with rent-based work incentives *can* substantially improve earnings among public housing residents. However, there is no comparably convincing evidence that other models can succeed and no evidence on the effectiveness of self-sufficiency interventions that are aimed at recipients of housing vouchers or at people receiving private, project-based assistance or at those living in HOPE VI developments.

Despite the vast resources invested in housing vouchers and other subsidies, and even in existing self-sufficiency programs, the gap in reliable evidence of “what works” leaves policymakers and housing experts having to guess about what new directions to take or to advocate. Fortunately, it is technically feasible to build much better evidence through a strategic use of random assignment methods as part of comprehensive implementation, impact, and benefit-cost evaluations. Doing so will cost money. But making policy in the absence of credible evidence can also be quite costly.

⁶²In Jobs-Plus, individual-level earnings impacts yielded larger development-level impacts in those locations where tenant turnover was lower, a condition that appeared to be related to the tightness of the housing market (Bloom, Riccio, and Verma, 2005).

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MDRC is a nonprofit, nonpartisan social and education policy research organization dedicated to learning what works to improve the well-being of low-income people. Through its research and the active communication of its findings, MDRC seeks to enhance the effectiveness of social and education policies and programs.

Founded in 1974 and located in New York City and Oakland, California, MDRC is best known for mounting rigorous, large-scale, real-world tests of new and existing policies and programs. Its projects are a mix of demonstrations (field tests of promising new program approaches) and evaluations of ongoing government and community initiatives. MDRC's staff bring an unusual combination of research and organizational experience to their work, providing expertise on the latest in qualitative and quantitative methods and on program design, development, implementation, and management. MDRC seeks to learn not just whether a program is effective but also how and why the program's effects occur. In addition, it tries to place each project's findings in the broader context of related research — in order to build knowledge about what works across the social and education policy fields. MDRC's findings, lessons, and best practices are proactively shared with a broad audience in the policy and practitioner community as well as with the general public and the media.

Over the years, MDRC has brought its unique approach to an ever-growing range of policy areas and target populations. Once known primarily for evaluations of state welfare-to-work programs, today MDRC is also studying public school reforms, employment programs for ex-offenders and people with disabilities, and programs to help low-income students succeed in college. MDRC's projects are organized into five areas:

- Promoting Family Well-Being and Child Development
- Improving Public Education
- Raising Academic Achievement and Persistence in College
- Supporting Low-Wage Workers and Communities
- Overcoming Barriers to Employment

Working in almost every state, all of the nation's largest cities, and Canada and the United Kingdom, MDRC conducts its projects in partnership with national, state, and local governments, public school systems, community organizations, and numerous private philanthropies.