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Author(s): Sandra Newman, C. Scott Holupka and Joseph Harkness

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The Long-Term Effects of Housing Assistance on Work and Welfare

*Sandra Newman
C. Scott Holupka
Joseph Harkness*

Abstract

This paper examines the long-term effect of project-based housing assistance—public housing and private assisted housing—on work, earnings, and welfare receipt. We use the Panel Study of Income Dynamics–Assisted Housing Database to identify women ever living in project-based assisted housing and to create comparison groups using propensity scores. Analyses show no evidence that moving into this type of assisted housing is associated with sustained reductions in employment rates, work hours, or earnings. Although welfare rates decline, they remain higher for assisted housing recipients compared with nonrecipients. Overall, these findings align with those of recent experimental and nonexperimental studies that include other forms of housing assistance and cover different time periods. © 2009 by the Association for Public Policy Analysis and Management.

INTRODUCTION

Expanding participation in social welfare programs between 1965 and 1985 spawned a large body of microeconomic research attempting to measure the effects of these programs on participants' labor supply and other employment-related outcomes (Moffitt, 2003). Much attention focused on the AFDC program, which seemed to have an especially perverse incentive structure. A principal concern then, and now, is that recipients may become overly reliant on public aid and fail to strive for greater economic productivity and self-sufficiency (Moffitt, 1992). Housing assistance was a late entrant to this genre, but it made up lost ground with a spate of studies in the late 1990s and early 2000s. Shroder's (2002) review of this research concludes that, unlike most other forms of federal assistance programs where at least modest effects have generally been detected, "Housing assistance is not persuasively associated with any effect on employment."

This paper uses panel data with validated measures of assisted housing receipt to examine the question of whether housing assistance has long-term repercussions on the work, earnings, and cash welfare receipt of women with children. It also compares these findings to the results of several newer, more rigorous studies that have appeared since Shroder's review. The imposition of time limits on TANF by the 1996 Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) legislation demonstrates that the main concern is whether participation in public assistance programs leads to dependency, that is, a permanent reduction in workforce participation and increased reliance on public assistance. This question has largely not been explored, with most studies of welfare program effects focusing on short-term impacts. Several studies have examined the tendency of former recipients of AFDC and of food stamps to return to assistance (Blank & Ruggles, 1994;

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Gleason, Schochet, & Moffitt, 1998; Harris, 1996; Hoynes, 2000), and some studies have examined the economic well-being of single mothers who left welfare (Cancian et al., 2002; Cancian & Meyer, 2004; Meyer & Cancian, 1998). But only one study compared the long-term social and economic outcomes of AFDC recipients and nonrecipients (Vartanian & McNamara, 2004), and no previous studies of housing assistance examine the long-term economic outcomes of women.¹

THEORIES OF HOUSING ASSISTANCE EFFECTS

The static labor supply model based on microeconomic theory has guided most empirical research on public assistance program effects.² This approach is somewhat limited for investigating long-term effects because, as the name suggests, time is ignored in these models. A fully developed model for estimating long-term effects would have to take both the individual's future expectations as well as past history into account, which exceeds the scope of available data. However, the static labor supply model provides some insights about how the effects of in-kind assistance—in this case, housing—might differ from those of cash or near cash programs.

Leonesio (1988) shows theoretically that, under two conditions, the effects of in-kind assistance programs on the labor supply of participants would be more positive than the effects of an equivalent cash grant. The first condition is that the program must induce participants to consume more (in terms of either quantity or quality) of the subsidized good than they would consume if given cash of equivalent value. Several studies confirm that this condition holds for housing assistance recipients, showing that recipients live in larger or better quality housing than they would if their housing subsidies were converted to a cash grant (for example, Olsen, 2003).

Leonesio's second condition is that the offered good must be, in economists' parlance, a "Hicks substitute" for leisure. This means that if the price of the good were reduced, participants would increase their labor supply. An example of a Hicks substitute is child care. If the cost of high-quality child care were lowered, it is likely that many stay-at-home mothers would enter the workforce. This same logic underlies arguments for why housing vouchers, which provide low-income families with the ability to move to neighborhoods with better access to job opportunities, may have positive effects on labor supply. In this case, the price of decent housing in more advantageous neighborhoods is being lowered. As another example, lower-priced housing might allow families living in dilapidated units or doubled up with relatives or friends to move into their own physically adequate dwellings, potentially reducing stress, improving health, and increasing labor supply. Similar effects might occur if housing assistance enables women to escape abusive partners, or if lower housing prices in safer neighborhoods allow mothers to feel more secure about going to work and leaving their older children at home alone.

On the other hand, lower housing prices might engender deleterious long-term effects. Because housing assistance is rationed, recipients who relinquish it have no assurance that they will be able to gain it again. This is in marked contrast to most other major safety net programs where eligibility guarantees participation. In the Housing Choice Voucher Program, which assists more households than any other single federal housing program, tenants become ineligible for assistance when

¹ Newman and Harkness (2002) examined the adult outcomes (for example, labor supply, welfare receipt) of children who lived in public housing between ages 11 and 17.

² Ellwood (1994) classifies ways of thinking about the long-term effects of participation in public assistance programs on social and economic outcomes into three paradigms: rational choice, expectancy, and cultural. The rational choice paradigm, the dominant approach, involves highly stylized mathematical models where individuals are conceptualized as rationally assessing a known array of choices and selecting the one that will yield the greatest satisfaction, given fixed tastes and preferences. Although it is possible, in principle, to incorporate insights from the expectancy and cultural paradigms into the rational choice framework, data are lacking to test such models empirically.

income reaches a point where the subsidy drops to zero and remains there for six months. As a form of insurance against future setbacks, therefore, housing assistance recipients may reduce work effort to remain eligible, as Newman (1999) reports based on interviews with housing practitioners.

HOUSING ASSISTANCE PROGRAMS

Unlike other safety net programs for the poor, such as food stamps and Medicaid, housing assistance is not an entitlement. Instead, housing assistance of all forms is available in very limited amounts such that only between one-quarter and one-third of income-eligible households are served (Braconi, 2001). This has led some to compare housing assistance receipt to a lottery.

Housing assistance is also not a single program, which further complicates the analysis of program effects. The three main types of federal housing assistance are public housing; privately-owned, publicly subsidized housing; and rent vouchers. Public housing is subsidized by HUD but owned and operated by local PHAs. Privately owned but publicly subsidized housing ("private assisted housing") is owned and operated by private owners (both for-profit and nonprofit) who receive concessionary financing in return for offering affordable rents to income-eligible households for an established period of time. Vouchers are used by income-eligible tenants to rent units in the private housing market. Housing units in all three types of programs must meet similar rent rules and housing quality standards. With some variations, all programs require tenants to pay 30 percent of their income (after some adjustments) for housing.

This study focuses on public housing and private assisted housing. As shown in Appendix Table A1,³ as of approximately 2006, there were about 1.2 million public housing units and more than 3 million private assisted housing units, for a total of more than 4 million "project-based" subsidized housing units (HUD 2007a–e). The work, earnings, and welfare participation outcomes examined here are most relevant for the more than 1.05 million able-bodied, working-age adults in these two types of project-based assisted housing.⁴

Public housing is much more likely to be found in high-poverty census tracts than either of the other two types of assisted housing (Newman & Schnare, 1997). Tenant composition also differs, with the lowest income families living in public housing (Newman & Schnare, 1994; Susin, 2005).⁵ The present paper also finds that those who moved into public housing are much worse off than those moving into private assisted housing. It is unclear whether we should expect public housing to have more, or less, favorable economic impacts than private assisted housing. If there are neighborhood or peer effects, then the better neighborhoods surrounding private assisted housing and its less disadvantaged tenant mix suggest better effects relative to public housing. On the other hand, if the less desirable environment of public housing is an incentive to work harder to get out, the opposite might be the case. Similar considerations apply to theoretical effects of project-based (whether public or private assisted) housing versus vouchers. Peer and neighborhood effects

³ All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at <http://www3.interscience.wiley.com/cgi-bin/jhome/34787>.

⁴ The 1.05 million estimate of able-bodied, working-age adults is based on calculations using HUD administrative data for 2004 (HUD, 2005). It is an underestimate because it excludes roughly 1.4 million units funded under the Low-Income Housing Tax Credit program, which is administered by the Department of the Treasury, not HUD.

⁵ Analyses disagree about voucher users. Newman and Schnare (1994) find they are more like public housing residents, whereas Susin (2005), using 1996 data, finds they are more comparable to those living in private assisted housing, and 1996 HUD administrative data suggest an intermediate position (authors' calculations). By 2001, however, voucher users experienced greater employment and earnings than those in private assisted housing.

associated with project-based housing might lead to more adverse economic outcomes. But because project-based housing restricts housing choice, the incentive to work oneself free of assistance may be greater. The two studies that have examined the issue find no difference in employment and earnings between the two types of assistance (Bania, Coulton, & Leete, 2001; Susin, 2005).

RECENT RESULTS

Several new studies have appeared since Shroder's review.⁶ Three of them track subjects over time and are therefore pertinent to the question of whether housing assistance has long-term effects on work and earnings. Susin (2005) used data from the 1996 Survey of Income and Program Participation (SIPP) matched to administrative records of federal housing programs to identify housing assistance recipients in the first wave of the survey.⁷ He applied propensity score matching to select a comparison group that was not receiving housing assistance in the initial waves, and analyzed outcomes over the 1996–1999 period, following the procedures developed by Rosenbaum and Rubin (1983, 1985). He found that employment increased for both the assisted housing and comparison groups over time, with no significant differences between them. Those in assisted housing experienced smaller increases in earnings (roughly \$250 less at the end of year 4). None of the three assisted housing programs was associated with an increase in AFDC or TANF.

Although this study has several strengths, one limitation is that the assisted group consists of those who were already receiving housing assistance, and it is unknown how long they had been doing so. Current recipients of housing assistance are a select group. As with other welfare programs, long-term recipients at any point in time are overrepresented in assisted housing because short-term recipients cycle in and out more quickly. Because long-term recipients arguably differ from short-term recipients—at the very least by being more reliant on the assistance in question—we cannot draw inferences from current recipients to the population of all housing assistance recipients. Nonetheless, even for this select group, there is little evidence of adverse long-term effects on various indicators of economic self-sufficiency. It is likely that effects for a representative sample of recipients would be even more favorable.

Two other recent studies correct this deficiency by tracking recipients from the time before they received housing assistance to several years after. The Welfare-to-Work Voucher (WtWV) Demonstration used an experimental design to test the effects of housing choice vouchers on a range of outcomes including employment, earnings, and utilization of public assistance receipt (Abt Associates, 2006; Gubits & Orr, 2006). Under WtWV, current or former welfare recipients on waiting lists to receive housing vouchers were randomly selected for an immediate award. Each experimental case was paired with a control case from the same waiting list.⁸ Study participants were tracked over a five-year period using both interviews and administrative data.

⁶ Of some relevance are analyses that have examined how housing assistance recipients have fared under welfare reform. Two are evaluations of four state-level welfare reform experiments examining the impacts of reform provisions on welfare recipients who were also receiving housing assistance compared to those receiving welfare alone (Lee et al., 2003; Verma, Riccio, & Azurdia, 2003). These studies generally find that impacts of welfare reform on some indicators of economic self-sufficiency—especially earnings and amount of cash welfare assistance received—are at least as favorable for housing assistance recipients as nonrecipients. Harkness and Newman (2006) find that employment gains for single mothers with housing assistance under welfare reform were as large as those of single mothers without housing assistance, although the drop in welfare participation was not as large for the former group as it was for the latter.

⁷ Susin matched SIPP and housing assistance records using social security numbers.

⁸ Because many in the control group subsequently went on to receive (and use) a voucher, the intervention is strictly a test of the impact of earlier versus later award of a voucher, not award versus denial. However, the WtWV evaluators developed and applied a series of methods to cope with this crossover problem.

WtWV found small but statistically significant differences in work and earnings 5–7 quarters after random assignment, with voucher users reporting both lower employment rates and earnings. But these effects became insignificant after about 3.5 years. Voucher users showed less of a decline in TANF and food stamp receipt over time, which the analysts partly attribute to an increase in the proportion of single-parent households with no other earners.

The second study (Jacob & Ludwig, 2006) uses a quasi-experimental design to examine the effects of vouchers. In 1995, HUD took over the Chicago Housing Authority and assigned management of the voucher program to a new nonprofit corporation. The corporation suspended the waiting list for vouchers, held an open registration, and created a new waiting list with each applicant assigned a random position. Because the number of applicants far exceeded the number of available vouchers, there is a natural control group for those who received vouchers. The study tracked individuals over roughly a seven-year period using several administrative data sources. The overall sample size is very large, exceeding 80,000 cases.

In contrast to the WtWV results, Jacob and Ludwig (2006) find evidence of early adverse effects of vouchers on employment, earnings, and welfare receipt that grow larger over time. For example, they estimate that after about four years, vouchers reduce earnings by about 10 percent and increase the use of TANF by about 14 percent, on average.

Because the question of the effects of housing assistance on work and welfare remains open, it is worth pursuing again with different data and methods. This study uses data from the 1968–2001 waves of the Panel Study of Income Dynamics (PSID)–Assisted Housing Database (AHD) to examine the effects of housing assistance on the welfare receipt, labor force participation, hours of work, and earnings of women with children under 18 years old.

This study extends recent work in at least two ways. First, it examines a different, but still very significant, type of housing assistance—project-based assisted housing (that is, public housing and private assisted housing)—rather than housing vouchers (also known as tenant-based assistance). Although HUD has increasingly emphasized tenant-based housing assistance, millions of project-based assisted housing units remain part of the subsidized housing stock, as noted earlier. For example, there are roughly 2 million project-based units under HUD's auspices that are serving low-income households at a cost of roughly \$13 billion, and accounting for 33 percent of the HUD budget for assisted housing in 2007 (Office of Management and Budget, 2007). Further, an estimated additional 2 million units of project-based subsidized housing were developed in approximately the first half of this decade under other programs administered by HUD (HOME; Community Development Block Grants) and by the Department of the Treasury (Low-Income Housing Tax Credit).⁹ Second, this study examines the effects of project-based assisted housing on work and welfare before implementation of TANF. If the findings of this study are consistent with the other studies reviewed, it would strengthen arguments that the effects are attributable to housing assistance, and would also help to disentangle the role of housing assistance from that of welfare reform.

DATA

Begun in 1968, the PSID is an ongoing, nationally representative, longitudinal survey of a representative sample of U.S. households conducted by the University of Michigan Survey Research Center. Low-income families are oversampled to ensure sufficient observations for analyses of this subgroup. The survey was conducted

⁹ See Appendix Table A1 for numbers and sources. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at <http://www3.interscience.wiley.com/cgi-bin/jhome/34787>.

annually until 1997, and biennially thereafter. From the original 5000 families, the sample has nearly doubled over time through inclusion of new families formed by the children and other members of the original sample. The survey includes an extensive array of questions on earnings, work hours, and participation in safety net programs, along with demographics, educational attainment, and family structure. To construct the PSID-AHD, housing assistance recipients were identified by address-matching the PSID to addresses of assisted housing units, overcoming the unreliability of data on self-reported housing assistance receipt in surveys (Shroder & Martin, 1996).¹⁰ As previously noted, we focus on public housing and private assisted housing. Vouchers are excluded because historical address data on recipients were typically not retained by local PHAs during this time period. Exploiting the panel structure of these data, we follow housing assistance recipients from the two years before they moved into assisted housing up to six years after.¹¹

The assisted housing sample was restricted to households who lived in project-based assisted housing at some time between 1970 and 1995,¹² and who were classified by the PSID as “female-headed.” This includes households with no spouse or partner present, and households where the wife has the main financial responsibility for the household.¹³ The analysis sample was further limited to women who had their children living with them when they moved into assisted housing. This criterion eliminates a large fraction of the elderly and disabled from the analysis.¹⁴ Roughly 36 percent ($n = 116$) of the assisted households moved into public housing. Among the remaining households who moved into private assisted housing, the majority moved into HUD-assisted private housing, whereas a small fraction were assisted under non-HUD programs.¹⁵ We combined these two private assisted housing groups into one “private assisted” housing category ($n = 207$). Because the comparison group was created by matching cases with similar characteristics to the assisted housing sample, the restrictions applied to the assisted housing sample apply to the comparison group as well. Detailed analyses (shown in the appendix¹⁶) demonstrate that estimates are unlikely to be biased by the inclusion of voucher users in the unassisted comparison group.¹⁷

METHODS

We examine four outcomes in this paper: (1) whether the female household head worked; (2) average weekly hours worked; (3) average weekly earnings; and (4) whether the individual’s family received cash welfare (here measured by AFDC receipt).¹⁸ Average weekly work hours and earnings are the most direct indicators of incentive effects. For example, if women who move into assisted housing work less than they

¹⁰ A detailed description of the Assisted Housing Database (AHD) appears in Newman and Schnare (1997) and a description of the PSID-AHD appears in Newman and Harkness (1999).

¹¹ As noted, Susin (2005) used panel data and followed study participants over three years, but lacked observations of individuals from the period before they received housing assistance.

¹² The initial two years of the 1968–1995 address match were excluded because we required a two-year base period of observation preceding housing assistance receipt.

¹³ Marriage rates for this sample ranged from 37–49 percent over the 1970–1995 analysis period.

¹⁴ The restriction of the sample to households with children is one of the key differences between our sample and Susin’s (2005).

¹⁵ HUD-assisted housing includes units under such programs as the Section 8 New Construction and Substantial Rehabilitation programs, whereas non-HUD programs include the Low-Income Housing Tax Credit, Farmer’s Home, and state housing programs.

¹⁶ All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher’s Web site and use the search engine to locate the article at <http://www3.interscience.wiley.com/cgi-bin/jhome/34787>.

¹⁷ Estimates of voucher users during the study period range from 1.5 to 10 percent (HUD, 2000, 2003; Olsen, 2003).

¹⁸ We examined welfare receipt at the family, rather than the individual, level because the distinction is mostly irrelevant for heads of households (the exception being a nonparental relative or caretaker receiving welfare support for a child). Moreover, only family-level welfare receipt variables were available in our data.

did before, this provides fairly direct evidence of an incentive effect. But if those who move into assisted housing work just as much but earn less or rely on cash welfare more, it is unclear whether to attribute this result to incentive effects or some other explanation (for example, lower skills of assisted housing residents; poorer job options in assisted housing neighborhoods).

This research is structured as a matched comparison group design examining economic outcomes over a seven-year period. We examine how the four economic outcomes of women with children compare on a year-by-year basis to the outcomes of a group of unassisted women observed over the same period. We cannot pinpoint the date of move-in, and know only that, at some date, an individual was observed living in assisted housing and was not in assisted housing the year before. We denote the two-year “pre” period prior to this transition year as the “base period.” The six-year “post” period after the transition year, along with the transition year, make up the “follow-up period.” Although the analysis of outcomes covers a seven-year period, we collected information on women’s characteristics in the two years prior to their move into assisted housing. This information was used in matching.

Contrary to popular impressions, most households live in assisted housing for only a few years. The median length of residence for the public housing and private assisted housing samples for this analysis was three years and two years, respectively. These durations are comparable to the median of 3.2 years in public housing calculated with HUD administrative data on non-elderly, nondisabled households with children (Lubell, Shroder, & Steffen, 2003).¹⁹

Results are estimated separately for public housing and private assisted housing. The statistical significance of differences between average outcomes for the assisted and comparison groups for each year of the follow-up period was assessed using two-tailed *t*-tests of the null hypothesis of no difference.

Because of attrition, samples shrink slightly in the later years of the follow-up period. For example, the samples for follow-up year 6 outcomes are about 14 percent smaller than the samples for follow-up year 1 outcomes. For earlier follow-up years, we compared results obtained with samples that included and excluded subsequent attriters and found no substantive differences. The results presented below are for the sample of individuals who remained in the survey throughout the follow-up period.

There are no natural comparison groups in the PSID for the assisted housing groups. One option is to include *all* unassisted women with children in the analysis sample. A fundamental problem with this approach is that the comparison group is likely to vary on a number of dimensions from the small sample of women receiving housing assistance, making it difficult to distill the effect of housing assistance from other factors. Another approach is to select a subset of comparison cases that are “matched” to the women receiving housing assistance, thereby reducing systematic differences between the two groups (Rosenbaum & Rubin, 1983, 1985; D’Agostino, 1998).

For this paper, we matched three control group members to each member of the treatment (assisted housing resident) group. Matching was based on the difference in propensity scores.²⁰ In propensity score matching, a model of participation (here, housing assistance) is estimated, the probability of participation is predicted for each case based on these estimates, and the absolute difference between these predicted probabilities is used as the distance. *T*-tests of differences in means of base period characteristics indicate strong match quality. For example, in the base period, 87 percent of women who moved into public housing were black and their average family income was about \$21,280. These statistics are similar to the 90 percent of women in the comparison group who were black, with average family incomes of \$21,370.

¹⁹ Length of residence for voucher users has a comparable median of 2.6 years (Lubell, Shroder, & Steffen, 2003).

²⁰ Details are available in the appendix to this paper in the online version accessible via JSTOR.

Table 1 lists the base-period variables used to construct the propensity score: age; whether the individual was black; whether the individual had a work disability; whether the individual was married; educational attainment; number of children in the household; whether a child younger than 5 years old lived in the household; family income; whether the family received cash welfare assistance; amount of cash welfare assistance as a proportion of family income; whether the individual received SSI income; whether the individual worked; average weekly earnings and hours worked; the population of the place of residence; and the median family income of the county of residence, both by itself and interacted with family income.²¹ Because the PSID data extend over 25 years, the propensity score model also included the first year in which the family received housing assistance as a “target” year for matching. Thus, a household that first received housing assistance in 1980 would be matched to other families in the PSID sample in 1980. In this example, 1980 is the first year in which residence in assisted housing is observed and is the third year of the nine-year time span of observation.

We restricted allowable matches to assisted and comparison group members with the same base and follow-up periods. For example, an individual who moved into assisted housing in 1975, whose base period would be 1973–1974 and follow-up period would be 1976–1981, could accept as matches individuals observed over the same time frame. Results were almost identical for matching with and without replacement, and we report only the results of matching with replacement.

The statistics presented in Table 1 indicate the quality of the match based on propensity scores. In neither the public housing nor private assisted housing samples were the differences significant at less than the 20 percent level. Computing a standardized difference score (*d*) for each measure and then averaging the results shows that the average absolute difference is 5.31 for the public housing sample and 2.61 for the private assisted housing sample (D’Agostino, 1998; Normand et al., 2001). A difference score (*d*) less than 10 indicates that the groups are relatively balanced (Cohen, 1977).

SAMPLE CHARACTERISTICS

Table 1 also shows the baseline characteristics over the two years prior to the move into assisted housing.²² The statistically significant ($p < 0.05$) results show that women who moved into public housing were more disadvantaged than those moving into private assisted housing. They had lower baseline family incomes (about \$21,300, or half the county median family income), 87 percent were black, 60 percent had not completed high school, and a fifth reported a work disability. They averaged slightly more than 2.5 children, but less than half were married.²³ Nearly 40 percent received AFDC, which comprised 22 percent of total family income, on average. More than 60 percent worked in the baseline period but averaged only 11 hours per week, with earnings averaging \$86 per week.

Women who moved into private assisted housing were marginally better off, with family incomes averaging about \$25,800, or almost 60 percent of the county median.

²¹ Heckman, Ichimura, and Todd (1997) note that labor market differences between samples are often a more important source of bias in nonexperimental studies examining work and income than selection bias. This data set contains too few cases per year or period in any one county or region to match an exact location, but both geographic (population size, median family income) and temporal factors are included in the propensity model to attempt to address these issues.

²² The specific time period for these baseline characteristics will vary depending on the year a woman was first observed in assisted housing. The largest proportion (41 percent) of women moving into public housing occurred between 1971 and 1975, with declines after that. For those who moved into private assisted housing, the proportion increases from 1971–1975 (15 percent) through 1981–1985 (25 percent) and declines thereafter.

²³ As noted earlier, the PSID classifies women with no spouse or partner as well as wives with financial responsibility for the family as female heads of households.

Table 1. Base-period means and *p*-values for difference in means for residents (1) and comparison group nonresidents (2) of assisted housing.

| | Women, Public Assisted | | | Women, Private Assisted | | |
|---|------------------------|-------|-----------------|-------------------------|--------|-----------------|
| | (1) | (2) | <i>p</i> (diff) | (1) | (2) | <i>p</i> (diff) |
| <i>N</i> | 116 | 348 | | 207 | 621 | |
| Black (0/1) | 0.87 | 0.90 | 0.44 | 0.72 | 0.71 | 0.86 |
| Age | 29.66 | 29.70 | 0.97 | 28.45 | 28.22 | 0.70 |
| Work disability (0/1) | 0.21 | 0.24 | 0.52 | 0.19 | 0.18 | 0.80 |
| Child < 5 years old (0/1) | 0.61 | 0.61 | 1.00 | 0.66 | 0.67 | 0.67 |
| # children in HH | 2.62 | 2.72 | 0.58 | 1.87 | 1.98 | 0.25 |
| Married (0/1) | 0.45 | 0.46 | 0.83 | 0.41 | 0.41 | 0.84 |
| Education | | | | | | |
| Grade school only (0/1) | 0.14 | 0.14 | 0.88 | 0.05 | 0.06 | 0.67 |
| Some high school (0/1) | 0.47 | 0.46 | 0.92 | 0.35 | 0.33 | 0.58 |
| High school grad (0/1) | 0.37 | 0.33 | 0.43 | 0.44 | 0.44 | 0.94 |
| Some college (0/1) | 0.02 | 0.04 | 0.24 | 0.15 | 0.15 | 0.87 |
| College grad (0/1) | 0.01 | 0.03 | 0.27 | 0.01 | 0.01 | 0.71 |
| Worked (0/1) | 0.63 | 0.67 | 0.43 | 0.79 | 0.80 | 0.81 |
| Weekly work hours | 10.88 | 11.54 | 0.65 | 16.34 | 16.58 | 0.84 |
| Average weekly earnings (2001\$) | 85.76 | 89.79 | 0.77 | 141.12 | 138.21 | 0.81 |
| Received SSI (0/1) | 0.01 | 0.01 | 1.00 | 0.07 | 0.07 | 0.94 |
| Total family income (\$10,000s in 2001\$) | 2.13 | 2.14 | 0.95 | 2.58 | 2.52 | 0.61 |
| County median family income (\$10,000s in 2001\$) | 4.36 | 4.43 | 0.43 | 4.56 | 4.51 | 0.48 |
| Ratio of family income to county median | 0.50 | 0.50 | 0.93 | 0.59 | 0.58 | 0.89 |
| Cash welfare recipient family (0/1) | 0.39 | 0.43 | 0.45 | 0.32 | 0.33 | 0.97 |
| Percent total family income from cash welfare | 22.45 | 25.64 | 0.41 | 10.82 | 11.28 | 0.81 |
| Population (1000s) | | | | | | |
| <50 (0/1) | 0.24 | 0.21 | 0.48 | 0.43 | 0.44 | 0.66 |
| 50-250 (0/1) | 0.23 | 0.27 | 0.46 | 0.21 | 0.20 | 0.65 |
| 250-500 (0/1) | 0.17 | 0.16 | 0.72 | 0.07 | 0.06 | 0.81 |
| >500 (0/1) | 0.35 | 0.37 | 0.82 | 0.30 | 0.30 | 0.97 |

Note: This table shows the means and results of two-tailed *t*-tests for differences in means for the four analysis groups. Comparison samples of assisted housing nonresidents were selected by matching on propensity scores. The three best matches were chosen (with replacement) for each assisted housing resident.

On average, they had fewer children than women who moved into public housing, but they were no more likely to be married. About 40 percent had not completed high school, and about a third received AFDC. Their average weekly work hours were 50 percent higher than those of women who moved into public housing, and their earnings were 65 percent higher, on average.

RESULTS

Table 2 lists the results of the analysis for women in public housing, and Table 3 lists the results for women in private assisted housing. Looking across both tables shows that there was a general decrease over time in welfare receipt, an increase in

Table 2. Differences in outcomes between residents and nonresidents of public housing: Matching with replacement, absolute propensity score distance.

| Years After Move-in | | Received Welfare | Employed | Weekly Hours Worked | Weekly Earnings* |
|---------------------|----------------|------------------|----------|---------------------|------------------|
| 0-1 | Mean—resident | 0.43 | 0.54 | 13.62 | 103.91 |
| | Mean—non-res | 0.34 | 0.60 | 15.48 | 130.03 |
| | Difference | 0.09 | -0.06 | -1.86 | -26.12 |
| | <i>p</i> | 0.08 | 0.26 | 0.32 | 0.16 |
| | Difference (%) | 28 | -10 | -12 | -20 |
| 1-2 | Mean—resident | 0.48 | 0.54 | 12.46 | 103.44 |
| | Mean—non-res | 0.33 | 0.60 | 15.99 | 145.73 |
| | Difference | 0.15 | -0.06 | -3.53 | -42.29 |
| | <i>p</i> | 0.01 | 0.26 | 0.06 | 0.04 |
| | Difference (%) | 45 | -10 | -22 | -29 |
| 2-3 | Mean—resident | 0.42 | 0.58 | 14.71 | 109.87 |
| | Mean—non-res | 0.36 | 0.59 | 16.79 | 153.93 |
| | Difference | 0.06 | -0.01 | -2.08 | -44.06 |
| | <i>p</i> | 0.26 | 0.90 | 0.28 | 0.04 |
| | Difference (%) | 17 | -1 | -12 | -29 |
| 3-4 | Mean—resident | 0.45 | 0.56 | 16.06 | 141.17 |
| | Mean—non-res | 0.32 | 0.61 | 17.40 | 160.48 |
| | Difference | 0.13 | -0.05 | -1.34 | -19.31 |
| | <i>p</i> | 0.01 | 0.34 | 0.51 | 0.39 |
| | Difference (%) | 42 | -9 | -8 | -12 |
| 4-5 | Mean—resident | 0.36 | 0.60 | 16.24 | 133.00 |
| | Mean—non-res | 0.28 | 0.60 | 16.46 | 152.63 |
| | Difference | 0.08 | 0.00 | -0.22 | -19.63 |
| | <i>p</i> | 0.15 | 0.98 | 0.91 | 0.37 |
| | Difference (%) | 26 | 0 | -1 | -13 |
| 5-6 | Mean—resident | 0.36 | 0.55 | 15.24 | 132.50 |
| | Mean—non-res | 0.23 | 0.59 | 16.66 | 167.17 |
| | Difference | 0.13 | -0.04 | -1.42 | -34.67 |
| | <i>p</i> | 0.02 | 0.52 | 0.47 | 0.14 |
| | Difference (%) | 53 | -6 | -9 | -21 |
| 6-7 | Mean—resident | 0.30 | 0.59 | 17.73 | 140.89 |
| | Mean—non-res | 0.24 | 0.59 | 17.23 | 180.81 |
| | Difference | 0.06 | 0.00 | 0.50 | -39.92 |
| | <i>p</i> | 0.19 | 0.97 | 0.80 | 0.10 |
| | Difference (%) | 27 | 0 | 3 | -22 |

* 2001 dollars.

Table 3. Differences in outcomes between residents and nonresidents of private assisted housing: Matching with replacement, absolute propensity score distance.

| Years After Move-in | | Received Welfare | Employed | Weekly Hours Worked | Weekly Earnings* |
|---------------------|-----------------|------------------|----------|---------------------|------------------|
| 0-1 | Mean—resident | 0.30 | 0.70 | 18.11 | 159.38 |
| | Mean—non-res | 0.23 | 0.73 | 19.08 | 172.26 |
| | Difference | 0.07 | -0.03 | -0.97 | -12.88 |
| | <i>p</i> | 0.08 | 0.50 | 0.50 | 0.43 |
| | Difference (%) | 30 | -4 | -5 | -7 |
| 1-2 | Mean—resident | 0.24 | 0.68 | 17.88 | 167.70 |
| | Mean—non-res | 0.20 | 0.69 | 20.23 | 182.27 |
| | Difference | 0.04 | -0.01 | -2.35 | -14.57 |
| | <i>p</i> | 0.29 | 0.85 | 0.12 | 0.39 |
| | Difference (%) | 19 | -1 | -12 | -8 |
| 2-3 | Mean—resident | 0.27 | 0.68 | 19.52 | 170.32 |
| | Mean—non-res | 0.19 | 0.73 | 20.49 | 185.07 |
| | Difference | 0.08 | -0.05 | -0.97 | -14.75 |
| | <i>p</i> | 0.04 | 0.21 | 0.53 | 0.41 |
| | Difference (%) | 40 | -7 | -5 | -8 |
| 3-4 | Mean—resident | 0.27 | 0.69 | 20.51 | 195.13 |
| | Mean—non-res | 0.19 | 0.72 | 21.05 | 192.80 |
| | Difference | 0.07 | -0.03 | -0.54 | 2.33 |
| | <i>p</i> | 0.06 | 0.49 | 0.74 | 0.90 |
| | Difference (%) | 37 | -4 | -3 | 1 |
| 4-5 | Mean—resident | 0.25 | 0.73 | 20.25 | 195.66 |
| | Mean—non-res | 0.17 | 0.70 | 21.21 | 198.73 |
| | Difference | 0.08 | 0.03 | -0.96 | -3.07 |
| | <i>p</i> | 0.03 | 0.51 | 0.54 | 0.87 |
| | Difference (%) | 43 | 4 | -5 | -2 |
| 5-6 | Mean - resident | 0.28 | 0.71 | 19.24 | 190.90 |
| | Mean - non-res | 0.13 | 0.70 | 20.81 | 187.46 |
| | Difference | 0.15 | 0.01 | -1.57 | 3.43 |
| | <i>p</i> | 0.00 | 0.76 | 0.33 | 0.86 |
| | Difference (%) | 111 | 2 | -8 | 2 |
| 6-7 | Mean—resident | 0.20 | 0.67 | 19.85 | 197.23 |
| | Mean—non-res | 0.14 | 0.70 | 21.48 | 201.25 |
| | Difference | 0.06 | -0.03 | -1.63 | -4.02 |
| | <i>p</i> | 0.07 | 0.54 | 0.31 | 0.83 |
| | Difference (%) | 42 | -4 | -8 | -2 |

* 2001 dollars.

hours worked and in weekly earnings, and little change in the percent employed. However, women in both types of assisted housing had higher welfare participation rates than their comparison groups that were statistically significant in the majority of follow-up years. Although women in public housing also had significantly lower weekly earnings in several of the early years of the follow-up period, these differences are not sustained over time.

WELFARE RECEIPT

As just noted, while welfare participation rates declined over time for all women, those who moved into assisted housing—whether public or privately owned—had

consistently higher welfare rates than their respective comparison group. There is no noticeable temporal pattern in these results, with differences of roughly the same magnitude occurring in both earlier and later follow-up years. Similarly, the smallest gaps were as likely to occur in the early years (for example, year 2 for private housing and year 3 for public housing) as in later years (year 7 for public housing). In the first year after moving into assisted housing, for example, 43 percent of women in public housing reported receiving welfare compared with 34 percent of the comparison group, a difference of 9 percentage points. Five years later, although the overall rates had dropped, the difference is even larger (36 percent for women in public housing versus 23 percent for the comparison group). These differences are statistically significant at the standard level ($p < 0.05$) in three of the seven years examined, and significant at $p < 0.10$ in a fourth year.

For private assisted housing, similar calculations show a 7–8 percentage point difference in welfare participation between the assisted housing and comparison groups. These differences are statistically significant ($p < 0.05$) in three of the seven years, and significant at $p < 0.10$ in three additional years.

Taken together, these results show that even though welfare participation rates declined over time for all women, those in assisted housing had consistently higher welfare rates than their unassisted counterparts. One possible explanation for the association between housing assistance and welfare receipt is a relationship between marital status and housing assistance. As shown in Table 4, although there are few significant differences in marriage rates between women who lived in assisted housing and their comparison groups in the years before moving into assisted housing, the differences are stark in the period after assisted housing receipt. During the base period, about 45 percent of women who moved into public housing and women in the public housing comparison group were married. After moving into public housing, women's marriage rate dropped, so that by follow-up year 5, only 30 percent of women in the public housing group were married, compared with 53 percent of women in the comparison group. Although there is a smaller but statistically significant marriage gap between women who moved into private assisted housing and their comparison group, the marriage rate of women in private assisted housing did not decline as it did for women moving into public housing.

Table 4. Marriage rates before and after moving into housing for residents (1) and comparison group nonresidents (2) of assisted housing.

| | Women, Public Housing | | | Women, Private Assisted | | |
|----------------------------------|--------------------------|-----|----------------|----------------------------|-----|----------------|
| | (1) | (2) | <i>P(diff)</i> | (1) | (2) | <i>P(diff)</i> |
| 2 years prior | 45 | 46 | 0.83 | 41 | 41 | 0.84 |
| 1 year prior | 43 | 50 | 0.22 | 41 | 48 | 0.05 |
| Year moved into assisted housing | 38 | 51 | 0.02 | 38 | 52 | 0.00 |
| 1 year after | 38 | 49 | 0.03 | 40 | 52 | 0.00 |
| 2 years after | 38 | 49 | 0.04 | 40 | 51 | 0.00 |
| 3 years after | 36 | 51 | 0.01 | 40 | 52 | 0.00 |
| 4 years after | 34 | 51 | 0.00 | 41 | 52 | 0.00 |
| 5 years after | 30 | 53 | 0.00 | 41 | 53 | 0.00 |
| 6 years after | 32 | 52 | 0.00 | 44 | 53 | 0.02 |
| 7 years after | 31 | 50 | 0.00 | 44 | 53 | 0.03 |

Note: This table shows the means and results of two-tailed t-tests for differences in means for the two analysis groups. Comparison samples of assisted housing nonresidents were selected by matching on propensity scores. The three best matches were chosen (with replacement) for each assisted housing resident.

Past research has consistently found that assisted housing households have fewer adults present compared with similar unassisted households (Nagle, 2003; Verma & Hendra, 2003; Mancuso et al., 2003; Swartz, 2003; Ellen & O'Flaherty, 2002; Zedlewski, 2002). In their analysis of New York City data for 1997 and 1999, Ellen and O'Flaherty, for example, concluded that Section 8 voucher participation and public housing tenancy "... make households smaller" (p. 2). Thus, the typical Section 8 voucher household contained 9 percent fewer adults than similar households without vouchers.

The more recent WtWV also found that voucher recipients and voucher users were less likely to be living with a spouse or partner than the control group after 4–5 years (Abt Associates, 2006). Although this difference was small and insignificant, the analysts note some reluctance by respondents in both the treatment and control groups to report accurately about spouses and partners. WtWV did find a significant decline in the total number of adult earners in voucher households. Because this reduced total household earnings, it could explain the Demonstration's finding of higher welfare participation rates by voucher households, particularly in the food stamp program.

In the present analysis of project-based assisted housing, the relationship between welfare participation rates and housing assistance receipt appears to be largely mediated by marital status. When current marital status is added as a control in the welfare receipt tests, the association between public housing and welfare receipt disappears and is substantially attenuated for the private assisted housing group. But this should not be interpreted as a causal effect of housing assistance on women's marital status because the causation could operate in reverse, as speculated by Shroder (2002). For example, women in more troubled relationships may be more likely to apply for housing assistance. If so, then marital difficulties may cause mothers to seek out housing assistance, rather than housing assistance causing marital disruption. Moreover, if housing assistance enabled some women to escape from difficult relationships, it could be beneficial for them and their children.

WORK AND EARNINGS

A majority of women in both types of assisted housing and their comparison groups reported working. These labor force participation rates did not change much over time, nor was there ever a statistically significant ($p < 0.05$) difference between either assisted housing group and its respective comparison group. For women in public housing, employment rates ranged from 54–60 percent compared with a 59–61 percent range for their comparison group. For women in private assisted housing, employment rates ranged from 67–73 percent, and from 69–73 percent for their comparison group.

Although employment rates did not change much over time, there were small increases in the number of hours worked per week over the time period examined. Among women in public housing, the average hours worked increased from 14 hours in the first year after receiving assistance to 18 hours in the last year examined. For women in private assisted housing, there was a somewhat smaller increase from 18 hours in the first year after moving in to 20 hours in the last observed year. Although the assisted housing groups almost always reported working fewer hours per week than the comparison groups, these differences were never statistically significant. The one near exception to this finding is in year 2, when women in public housing experienced a small decrease in work hours while women in the comparison group showed a small increase, creating a difference that was almost significant ($p = 0.06$).

Similarly, although earnings for women who moved into public housing also tended to be lower than in their comparison group, these differences rarely achieved statistical significance. On average over all years of the follow-up period,

women who moved into public housing earned \$121 compared with \$155 in earnings for women in the comparison group. However, the only statistically significant differences ($p < 0.05$) are in the second and third years after move-in. No statistically significant differences in earnings exist for women in private assisted housing, with only a 2 percent or less difference between them and their comparison group in most years.²⁴

SENSITIVITY ANALYSES

An unavoidable issue in using matching methods to compute treatment effects for subsidized housing tenants is the possibility that unobserved differences between those receiving housing subsidies and those in the comparison groups cause the differences in outcomes (Becker & Caliendo, 2007; Caliendo, Hujer, & Thomsen, 2005; Caliendo & Kopeinig, 2005; DiPrete & Gangl, 2004; Frank, 2000; Harding, 2003). Although it is impossible to estimate the magnitude of this potential “hidden bias,” we can gauge the sensitivity of results to possible unmeasured influences. We use two methods of measuring the sensitivity of our results to possible confounding variables. (Details on these methods appear in the appendix available in the online version of this paper.)

The first approach, proposed by Rosenbaum (2002), involves examining different levels of hidden bias in order to determine how large a bias is needed before statistically significant inferences become insignificant. The larger the degree of hidden bias needed to dissipate a statistically significant effect, the more unassailable the finding. Although showing that an effect is sensitive to relatively low levels of hidden bias does not mean the effect does not exist, it does suggest caution is required when interpreting findings (Caliendo, Hujer, & Thomsen, 2005).

Overall, the large degree of hidden bias required to nullify the observed effects supports the standard interpretation of the results shown in Tables 2 and 3. Outcomes that are statistically significant at 0.05 or less would generally require an unobserved bias of at least 20 percent or more before the effect would be expected to disappear. Conversely, outcomes where the statistical significance is above the standard 0.05 p -value would be sensitive to even a modest 5 percent unobserved bias.

A related approach to examine sensitivity, developed by Frank (2000), computes an index (called the “Impact Threshold for a Confounding Variable” or ITCV), which indicates the degree to which a confounding variable must affect the relationship between an independent and dependent variable in order for that relationship to no longer be statistically significant at some specified level (for example, p -value of 0.05). The ITCV can be further decomposed into partial correlations, showing the correlation that would be required between the confounding variable and both the independent and dependent variables in order to produce the ITCV.²⁵

Although most of the ITCV partial correlations are small, they are larger than most of the observed correlations. The average correlation between assisted housing and any of the outcomes examined is 0.049, and all but one of the ITCV partial correlations are above this threshold. In fact, only two of the ITCV partial correlations are below three-quarters of the observed correlations, whereas several are above the largest correlation observed (0.17). For most of the statistically significant relationships

²⁴ Additional analysis demonstrates that the decrease in earnings in the year before moving into public housing does not reflect an “Ashenfelter dip” (Ashenfelter, 1978). See the appendix in the online version of this paper. Go to the publisher’s Web site and use the search engine to locate the article at <http://www3.interscience.wiley.com/cgi-bin/jhome/34787>.

²⁵ In a multivariate analysis, ITCV can be compared to correlations observed for all of the other covariates. Pan and Frank (2003) show how the observed distribution of correlations can be used to assess the probability of observing a given ITCV. Because covariates were not used in this study, the ITCV can only be compared to the correlations observed between assisted housing and the various outcomes examined.

observed, therefore, a confounding variable would need to have an impact at least as strong as assisted housing to eliminate the observed effects, and in some cases (all involving welfare receipt) the confounding variable would need to be even more strongly related in order to alter the interpretation of the observed results.

DISCUSSION

This analysis provides no evidence that moving into private assisted housing is associated with a sustained reduction in women's labor force participation, work hours, or earnings. Moving into public housing is not associated with a significant change in labor force participation, nor, in general, were there significant differences in hours worked or total earnings compared to comparable women not in public housing. For those moving into either public housing or private assisted housing, the one difference that is sustained over time is a greater reliance on welfare by housing assistance recipients relative to nonrecipients.

As shown in Table 5, these findings, which pertain to public housing and private assisted housing examined over a 27-year period, align well with the other recent longitudinal studies that examine the relationship between assisted housing and economic outcomes, and particularly with the WtWV study, which used an experimental design to examine the effects of housing vouchers. Both this study and WtWV, for example, find some initial differences in employment activity for those receiving housing assistance—employment rates and earnings in WtWV, and earnings in this study (with an almost statistically significant difference in hours worked)—but neither finds statistically significant employment differences that persist over time. The one sustained difference in both studies is welfare use, with those receiving housing assistance showing less of a decline over time.

The labor supply results reported by Susin are also fairly consistent with the findings from the present study. For example, Susin finds no effects of any type of housing assistance on employment rates. He also finds that families in both public and private assisted housing have smaller increases in earnings (which we also find in the first two years in public housing). Unlike this study and WtWV, however, Susin's earnings differences (for project-based assisted housing) do not disappear. But because his study examined effects over a shorter time period, his results are not inconsistent with the short-term earnings results in our study or in WtWV.

Finally, like this study and WtWV, Susin finds that welfare rates decrease over time. But by contrast to these studies, Susin does not find greater reliance on welfare by housing assistance recipients relative to nonrecipients. Instead, welfare rates decrease even more for families in public housing than their comparison group, and there are no differences for families in private assisted housing or using vouchers. Although Susin's 1996–2000 study period, which coincides with the transformation of the welfare system, might initially seem to be a plausible explanation for these results, neither WtWV nor Jacob and Ludwig, whose studies cover a similar time period, report similar findings. The reason for Susin's unique welfare results, therefore, remains a mystery.

Jacob and Ludwig's study (2006) of families trying to enroll in Chicago's housing voucher program also yields a number of similar results to the other three studies. For example, the Chicago study reports similar effects of housing assistance on employment rates as WtWV over the initial 3–4 years of housing assistance receipt. Effects on welfare rates are also similar in Jacob and Ludwig and WtWV: They find a 9 percent difference from the control group mean, whereas WtWV finds a 5 percent difference. We find a much larger difference of 40 percent, on average, most likely because of the higher AFDC rates in the 1970s, when more than half of the PSID public housing sample and one-third of the private assisted housing sample began to be tracked. And although Jacob and Ludwig's analysis is the only one to produce statistically significant differences in earnings between housing assistance

Table 5. Summary of associations between assisted housing, employment, and welfare outcomes in key studies.

| Source (Data) | # of Years Observed (Years Studied) | Type of Housing Assistance Studied | Employment Rate | Work Hours | Earnings | Welfare Receipt |
|---|-------------------------------------|--|--|----------------|--|---|
| Newman, Holupka, and Harkness (PSID-AHD) | 7 (1968–1995) | Public housing; Private assisted | No differences | No differences | Public housing: smaller increases yrs. 2–3, no differences later Private assisted: no differences | Smaller decreases |
| Abt Associates (2006) (WtWV) | 5 (2000–2005) | Vouchers | Smaller increases yrs. 1–2, no differences later | No differences | Smaller increases yrs. 2–3, no differences later | Smaller decreases |
| Susin (2005) (SIPP) | 4 (1996–2000) | Public housing; Private assisted; Vouchers | No differences | N/A | Public and private assisted housing: smaller increases all years Voucher: no differences | Larger decreases in public housing; No differences in private assisted or voucher |
| Jacob and Ludwig (2006) (Chicago Housing Authority) | 7 (1997–2004) | Vouchers | Lower, difference grows over time | N/A | Lower | Higher, difference grows over time |

Notes: 1. In Newman, Holupka, and Harkness (study for this article) panel data were divided into separate 9-year spans, starting with 1970–1978 in order to insure data were available for at least two years prior to receiving housing assistance and at least seven years after receiving assistance.

2. PSID-AHD = Panel Study of Income Dynamics–Assisted Housing Database.

3. SIPP = Survey of Income Program Participants.

4. WtWV = Welfare-to-Work Voucher Demonstration.

recipients and nonrecipients, the differential between the two groups, on average, in their study (10 percent) is of similar magnitude to our study (12 percent for public and private assisted housing) and Susin (15 percent for all three assisted housing programs).

Where Jacob and Ludwig's results differ from both WtWV and our work is in the persistence of the effects of housing assistance on employment rates. As already noted, we did not detect employment rate effects, whereas WtWV found an early effect that faded over time.²⁶ By contrast, Jacob and Ludwig report that the effect of housing assistance on employment rates grew over time. They note that if they truncate their analyses to the same time frame as WtWV, their results provide "suggestive evidence for some dissipation" of an effect on employment. But employment differences increase over a time period that is comparable in duration to our study period.

We can think of three possible explanations for the disparity in results between their Chicago analysis and the multisite WtWV. First, Jacob and Ludwig point to their much larger sample ($n > 82,000$) as one possibility. But though this might explain the persistence of statistically significant differences in employment, it cannot explain why these differences grow over time, particularly post-welfare reform. A second possibility, also noted by Jacob and Ludwig, is that the WtWV sample consists almost entirely of TANF users, whereas less than half of their Chicago sample were receiving TANF. Beyond TANF receipt, the demographic composition of the two samples differs; for example, the Chicago sample includes more than double the proportion of disabled persons (more than 25 percent compared with 11 percent, respectively). Differences in sample composition could have a direct effect on employment or could interact with features of the voucher program to produce different employment profiles. Even more speculative are possible differences in the administration of the voucher programs in Chicago versus the WtWV study sites.

Overall, the many consistent findings in these four studies over time and across the three main forms of housing assistance is an important contribution in and of itself because of ongoing debates about the relative merits of supply-side versus demand-side subsidies (for example, Olsen, 2003). Although a major focus of these debates centers on the relative costs of these two housing assistance options, another issue is whether vouchers are more "employment friendly" both because they are more heavily concentrated in suburbs, which are experiencing more job growth than central cities, and because they are portable at least within the metropolitan area, thereby allowing the user to take advantage of new job opportunities. By contrast, project-based assistance constrains the recipient to the particular location of the housing development. These most recent studies provide no support for a distinction in employment friendliness across types of assisted housing. On the particular and persistent question of the disincentive effects of housing assistance, these studies move us in the direction of closure, but two important puzzles remain.

The first concerns the effects of housing assistance on TANF (or AFDC) receipt. Although there is no evidence that housing assistance is associated with an absolute increase in welfare use, the most consistent, sustained effect in three of the four studies is higher welfare use among housing assistance recipients relative to nonrecipients. That is, although welfare use declined among all groups, the decrease was smaller among housing assistance recipients. It is noteworthy that this more modest decline among recipients is evident in both the pre-welfare reform period covered by our study and the post-welfare reform era of the other three studies.

Both this study and WtWV examined household composition in an effort to interpret this welfare result. We find that women moving into public housing experienced a reduction in marriage rates, and this reduction is related to higher welfare use. The WtWV experiment also found that higher welfare rates among voucher

²⁶ Susin (2005) also found no effects on employment rates.

users might be attributed to the decreased number of adults in voucher households (Abt Associates, 2006). Although the WtWV did not find that voucher users were significantly less likely to have a partner or spouse, qualitative interviews with voucher users revealed a widespread belief that voucher rules prohibited unrelated adult males from living in the household, and that “a few respondents thought this ‘rule’ applied even to their husbands” (p. 75). So, though housing assistance may not be directly tied to welfare use, housing rules and regulations regarding family composition may be having an effect. The family and household composition changes that may be prompted or facilitated by housing assistance is an important topic for future research.

The second puzzle concerns earnings and employment rates. As noted earlier, if women worked less after they moved into assisted housing, this would provide the most direct evidence of the disincentive effect of housing assistance on employment. Jacob and Ludwig’s Chicago study is the only one among the four that observes a statistically significant difference in employment rates, which is not only sustained but actually increases over time.²⁷ They also are unique in finding a statistically significant and persistent earnings differential between housing assistance recipients and nonrecipients. Jacob and Ludwig argue that the lack of statistical power in the other studies is a major reason for the discrepancy in statistical significance, but the evidence suggests that the answer is more complex.

Although this study and Susin’s find earnings effects of approximately the same size as Jacob and Ludwig’s, neither we nor Susin nor WtWV detect differences in employment rates between housing assistance recipients and nonrecipients.²⁸ As noted earlier, finding that women in assisted housing are as likely to be in the labor force as nonrecipients but earn less is not necessarily evidence of a disincentive effect, and could be explained by other factors such as lower skills or lower-paying jobs near assisted housing. Further, although it is true, as Jacob and Ludwig note, that WtWV does not have a large enough sample to detect the 10 percent difference in earnings they observe in their analysis, the effect sizes reported in WtWV are much smaller (2–3 percent) with a narrow confidence interval that does not reach 10 percent. Jacob and Ludwig also argue that because the WtWV sample consists entirely of TANF recipients, the disincentive effects of TANF should reduce employment and earnings rates, again making it difficult to detect significant differences. But if this interpretation is correct, we should see parallel declines in employment and earnings rates, with a gap between housing assistance recipients and nonrecipients. Instead, WtWV finds that employment rates and earnings converge over time. Finally, neither we nor WtWV find sustained differences in work hours between recipients and nonrecipients. Again, finding lower earnings in the absence of lower labor force participation rates and fewer work hours is not necessarily consistent with a disincentive effect interpretation.

As with the first puzzle concerning the household composition changes that may be affected by assisted housing, this second puzzle regarding employment effects is well worth solving. Top candidates for exploration are the effects of differences in sample characteristics and voucher implementation between Jacob and Ludwig’s Chicago study compared with the WtWV sites, and an extension of the PSID-AHD analysis from its current end date of 1995 to the present.

²⁷ There are differences in how earnings data were obtained in each study. This study and Susin’s relied on self-reported interview data, whereas WtWV and Jacob and Ludwig utilized administrative records. Because the WtWV results are generally consistent with the results from the two studies based on interview data, differences in data sources shouldn’t explain the differences in earnings and employment reported by Jacob and Ludwig unless there is something different about the administrative data on Chicago during this time period.

²⁸ In fact, employment rates generally rise in Susin’s samples, though none of the differences between housing assistance recipients and nonrecipients is statistically significant.

SANDRA NEWMAN is Professor of Policy Studies at Johns Hopkins University and Director of the Hopkins Institute for Policy Studies.

C. SCOTT HOLUPKA is Senior Research Associate at the Johns Hopkins University Institute for Policy Studies.

JOSEPH HARKNESS is Coordinator of Projects and Models for the Baltimore City Public School System.

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