

Longitudinal Outcomes of Subsidized Housing Recipients in Matched Survey and Administrative Data

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Abstract

This study uses a new data set combining survey and administrative data to investigate the longitudinal effects of subsidized housing on a broad range of outcomes relating to dependency. Given a household's assistance status in 1996, it examines outcomes over the subsequent 3 years. The aim is to produce a credible comparison group by matching on the same variables (measured in an earlier period) as the outcomes to be examined.

Both subsidized and comparison households made strong gains from 1996 to 1999, showing sharp increases in income, employment, and earnings and reductions in poverty and transfer program participation. The earnings of people in the comparison group increased more rapidly, however, suggesting that housing subsidy programs reduce individual earnings by roughly 15 percent. In two of the three programs, similar results were found for family earnings, much of which can be explained by reductions in household size of 5 to 10 percent. Impacts of subsidy programs on program participation were small and inconsistent, suggesting little effect. Although these programs are found to affect neighborhood choice, neighborhood poverty rates explain little of the impact on individual earnings.

Introduction

This study uses a new data set to investigate the longitudinal effects of public housing, vouchers, and project-based subsidized housing on a broad range of outcomes relating to dependency. Given a household's assistance status in 1996, it examines outcomes over the subsequent 3 years. The outcomes include income and poverty status; employment and earnings; receipt of welfare benefits and housing subsidies; and living arrangements such as household size and marital status. The main focus is on the role played by housing subsidies, neighborhood, and household composition in determining earnings outcomes.

This research uses a new data set created by merging the Survey of Income and Program Participation (SIPP) with administrative data on the receipt of the three major types of

housing subsidies. Administrative data is important because self-reports of housing assistance contain considerable error (Shroder and Martin, 1996). Subsidized households are compared to a sample of unsubsidized households, matched using propensity score methods. Subsidized cases are matched to unsubsidized cases that, at the beginning of the panel, have similar background characteristics (such as race) and measures of dependency and poverty (such as the receipt of food stamps). Both groups of households are then followed, taking advantage of the SIPP's longitudinal nature. At the end of the panel, the study compares the two groups' outcomes, such as the receipt of food stamps and poverty status.

The aim is to produce a credible comparison group by matching on the same variables (measured in an earlier period) as the outcomes to be examined. For example, it seems reasonable to expect that two households with the same earnings in one year are likely to have similar earnings, on average, 3 years later. This identification strategy, as will be seen, requires careful attention to the dynamics of earnings and other outcomes.

Background

The various possible effects of subsidized housing can be loosely classified into economic, demographic, and sociological effects, albeit with some degree of caricature. According to standard neoclassical consumer theory (for example, Varian, 1992), subsidized housing should have substitution and income effects, both operating to reduce work. Substitution effects arise because the tenant's contribution to rent is set at 30 percent of income. Since rent increases by 30 cents for each additional dollar of earnings, subsidized housing reduces labor supply (that is, work effort) just as would a 30-percent tax.¹ In addition, subsidized housing residence amounts to an increase in income, which should also reduce labor supply (because the rent will be paid whether the recipients work or not). Other economic effects are possible as well. Housing assistance is likely to cause many recipients to change neighborhoods. Subsidized housing units might be located closer to or farther from employment sites than alternative unsubsidized residences. Finally, housing subsidies free up additional resources, which might be invested in employment-enhancing ways such as in education or a car, and lead to more employment in the long run.

Subsidized housing might also cause a change in demographic factors, specifically household composition. Gould Ellen and O'Flaherty (2002) note that housing subsidy programs require recipients to live in units deemed large enough to accommodate their families. To the extent that the supply of larger housing units is limited, smaller households applying for public housing or project-based assistance will be offered units more quickly and voucher recipients will have an easier time finding units in the private market. In addition, subsidized housing provides incentives to consume more housing; one way to do so is to reduce household size. For example, receiving housing subsidies may (and is intended to) allow recipients living "doubled up" to move out and form their own households. There is fairly consistent evidence of a subsidy-induced reduction in household size (Gould Ellen and O'Flaherty, 2002; Shroder, 2002a).

Subsidized housing can be expected to have a number of more sociological or psychological effects; these effects may be positive or negative. Housing assistance might enable a parent to move away from a gang-infested area and reduce the time spent monitoring her children, possibly leading to new employment (Katz, Kling, and Liebman, 2004; Reed, 2004). In other cases, subsidized housing might induce moves to a high-crime neighborhood (such as a crime-ridden public housing project), which might reduce employment. The neighborhoods of assisted developments may be stressful and depressing in other ways as well, affecting motivation to search for a job (Katz, Kling, and Liebman, 2001). For others, housing subsidies may permit a move to less crowded conditions, where the reduction in background chaos may make job search easier. Finally, the neighbors of subsidized housing recipients may be less (or more) connected to the labor market, serving as weaker (or stronger) sources of employment leads and role models.

This study will not aim to untangle all these possible effects; but it will decompose the effects of housing subsidies into the impact due to household size, the impact due to neighborhood (as measured by census tract poverty rates), and a residual impact presumably due to economic incentives.

Perhaps because of the many ways subsidized housing can affect labor supply, researchers have found little in the way of consistent impacts despite a number of studies. A recent review of the literature (Shroder, 2002a: 410) concludes, “The literature to date fails to confirm the neoclassical hypothesis [of reduced labor supply]; the more sophisticated tests do not show stronger negative effects than the less sophisticated. The distribution of results from these 18 empirical studies is consistent with a true housing assistance/short term employment effect of zero.”

Of the studies with multivariate controls, Shroder praises those based on special local data, which generally have particularly rich sets of control variables. At the same time, studies based in a particular local area are necessarily of limited generalizability. Similarly, four out of these five studies are limited to welfare recipients. While this population is important, most subsidized housing recipients do not receive welfare. Shroder is more critical of the six multivariate studies that rely on national survey data. He argues that misreporting of housing assistance status is a very serious problem in survey data. The best of these studies make use of instrumental variables techniques, but Shroder argues that the instruments used are implausible. Finally, all these studies are mostly cross-sectional.

An important recent experimental study of the voucher program, however, finds that the program reduces employment and increases welfare receipt (Patterson et al., 2004).² Another important recent study combines administrative data on subsidized households with survey data on a comparison group of households, finding large reductions in earnings for the subsidized group (Olsen, 2004). These studies will be discussed in more detail below, after the results are presented.

The present study aims to duplicate some of the merits of the local studies—precise measure of assistance status and a rich set of longitudinal controls—while avoiding their limits due to focusing on special populations and locations.

Description of Data

The data set used in the project is the 1996 panel of the SIPP merged with U.S. Department of Housing and Urban Development (HUD) administrative data on housing subsidy receipt. The SIPP is a national panel data set that follows approximately 40,000 households for 4 years, covering the period from December 1995 through February 2000. Households in the SIPP are interviewed every 4 months, for a total of 12 “waves” of interviews. Households from areas with high-poverty concentrations are oversampled.³

The HUD administrative data identifies enrollment in the various HUD-administered housing subsidy programs and the date of the most recent “transaction” as of December 1996. Programs covered by the data include public housing, Section 8 vouchers and certificates, and a number of project-based subsidy programs. The data do not include programs administered by the Rural Housing Service and units funded solely by low-income housing tax credits. The “transaction” date most commonly refers to the date of the most recent income certification, which occurs when a household moves into subsidized housing and annually thereafter.⁴ Most transactions occurred sometime during 1996, with the modal month being November 1996.⁵ For 195 cases (16 percent of the data), the transaction occurred before the start of the SIPP panel, usually sometime in the 6 months before the beginning of the panel. In general, the data identify households that were subsidized at some point during the first year of the panel, most commonly toward the end of the first year, but sometimes as much as 6 months before the beginning of the panel.

Merging the Survey and Administrative Data

We could not make a match between the SIPP and HUD data when the Social Security number (SSN) was missing or invalid in either data set or when a subsidized household was not listed in the HUD data. Match rates (the probability that a household listed in the HUD administrative data and also interviewed in the SIPP will be matched) are estimated to be between 75 to 80 percent, depending on the subsidy program. Because the HUD data itself fails to list perhaps 15 percent of households in subsidized housing due to underreporting by local authorities, the probability that a truly subsidized SIPP household will be identified is about two-thirds (that is, 85 percent of 75 to 80 percent).

Some nonrandom availability of SSNs occurs (and hence, nonrandom matching failures), but the magnitude is modest. Hispanics appear to be underrepresented in the matched sample by about 2 percentage points, compared to their true percentage in subsidized housing (13 percent). There is little evidence of any other important problems with nonrandom matching, although there may be a small tendency for social welfare program recipients to be overrepresented in the matched data compared to their true proportion in subsidized housing.

Since there was little evidence of substantial and systematic matching failures, the main implication of the undercoverage is that the comparison group is potentially contaminated with subsidized households that are not covered in the administrative data.⁶ To keep the comparison group as free of subsidized tenants as possible, households are excluded from the comparison group if they are reported as subsidized in either the survey or administrative data sets. Because of these two sources of information, and because the number of uncovered subsidized households is fairly small relative to the pool of potential comparison group members (that is, disadvantaged unsubsidized households), “contamination” of the comparison group is likely to be a minor concern.

Analysis Sample

After creation of the merged SIPP/HUD file, the cases receiving housing subsidies (according to the administrative data) were grouped into three categories: public housing, vouchers, and project-based subsidies.⁷ Public housing consists of developments built by the government and managed by local public housing authorities (PHAs). Vouchers are tenant-based subsidies that allow recipients to rent in the private market, with HUD covering a portion of the rent. Project-based subsidies consist of multiple programs managed by private entities that receive a continuing stream of subsidies; the government also subsidizes the development construction (or conversion) for these programs. Tenants in all programs generally pay 30 percent of their income in rent, with government subsidies covering the rest. In all three types of programs, eligibility is restricted to those with low incomes; other need-based restrictions exist as well (for example, the homeless have priority in some circumstances). Importantly, these programs are not entitlements but are generally rationed using some type of waiting list. Hence, a large pool of eligible but unsubsidized households are potentially available to serve as comparison group members.

The disabled are an important population in subsidized housing. To capture the effect of disabilities on outcomes, variables indicating the (self-reported) presence of a disability that limits or prevents work were included as matching and control variables. The disabled are included in the sample for three reasons. First, some of the outcomes studied are not directly related to disabilities; for example, family earnings (which could be due to other family members) and the number of adults in the household. Second, the disabilities measured in the SIPP are not necessarily permanent, and partial disabilities do not preclude work. In particular, residents of subsidized housing with partial disabilities at the beginning of the panel are employed at about the same rate (59 percent) as their nondisabled counterparts (although their earnings are lower). Of the subsidized housing residents who initially reported full disability, 13 percent are working by the end of the panel as are 19

percent of their counterparts in the comparison group. At the same time, the percentage reporting work-preventing disability drops to 79 percent (72 percent for the comparison group) by the end of the panel.

The analysis sample was restricted to those who met four criteria: (1) they were SIPP householders (meaning a household member whose name is on the lease or deed), (2) they were less than 55 years old in the first month of the SIPP panel (because policy interest in dependency focuses on younger people), (3) they had valid SSNs in the SIPP (because only this group can be merged with the HUD data), and (4) they were present in the first three waves of the SIPP (because the statistical match is based on data from these waves). In all, 670 subsidized households met these criteria.

Statistical Matching To Create Comparison Groups

To create comparison groups, these three groups of subsidized cases were statistically matched to unsubsidized households that had similar characteristics in the first year of the SIPP panel. The goal was to choose comparison groups similar to the subsidized groups at the beginning of the SIPP panel and then compare their outcomes at the end of the panel.

Propensity score matching was used to select the comparison groups (Rosenbaum and Rubin, 1983). That is, an indicator for the receipt of housing subsidies was regressed on a number of variables likely to predict subsidy receipt, such as income, education, and marital status. This logit regression was run in a sample consisting of those receiving one type of subsidy (for example, public housing) and those not listed in either the survey or administrative data as receiving subsidies. Next, the predicted probability of receiving a subsidy (the propensity score) was calculated for each case. Finally, three comparison group members—the three cases with the most similar propensity scores—were chosen as matches for each subsidized household.⁸

Exhibit 1 shows the means of the main variables used in the propensity score logit, which include measures of income, earnings, employment history, public assistance, household composition, other demographics, and disabilities.⁹ These variables are all measured during the first wave of the panel. A number of additional variables were also included in the matching logit but are not shown in the exhibit, including four measures of bank savings accounts; the square of age; and several measures of income, earnings, and public assistance measured at the end of the first year (wave three).¹⁰

In general, the logit results are not especially interesting because many variables are highly collinear (such as earnings in the first and third waves) and, therefore, many coefficients are individually statistically insignificant. The point here is not to estimate the coefficients precisely, however, but to predict the probability of living in the subsidized housing. Several of the variables related to savings (such as possession of a money market account) perfectly predict the nonreceipt of subsidized housing. Naturally, cases with these types of savings will not appear in the matched sample. The logit models predict subsidy receipt reasonably well, with pseudo-R²s between 0.30 and 0.34. The real test is whether the comparison group is similar to the subsidized group. As discussed below, the match does very well by this criterion.

Success of Statistical Matching

For all the comparisons in exhibit 1, there is no statistically significant difference between the subsidy and comparison groups. In addition, the differences are usually small as well. This lack of significant difference is not a mechanical function of the fact that these variables entered the matching function. For example, it is possible that there are no good matches for the subsidized cases and that even those cases closest in propensity scores

Exhibit 1

Subsidized Households vs. Comparison Groups: Average Income, Earnings, Employment, Program Participation, Household Composition, and Demographics in Wave One^a

	Public Housing	Comparison Group	Difference	Vouchers	Comparison Group	Difference	Project Based	Comparison Group	Difference
Family income	919	907	12	1,100	1,093	7	945	900	45
Individual income	710	685	25	821	789	32	714	672	42
Poverty	0.718	0.741	-0.023	0.631	0.627	0.004	0.668	0.671	-0.004
Family earnings	537	498	40	665	625	39	662	632	30
Family employment	0.452	0.433	0.019	0.529	0.530	-0.001	0.535	0.544	-0.009
Individual earnings	384	345	39	444	401	42	469	451	18
Employed wave one, job started 1995 or later	0.275	0.257	0.017	0.261	0.276	-0.015	0.294	0.288	0.006
Employed wave one, job started before 1995	0.192	0.180	0.012	0.245	0.222	0.023	0.224	0.231	-0.007
New job in wave one or contingent worker	0.021	0.028	-0.007	0.012	0.009	0.003	0.018	0.016	0.001
Not employed, worked 6+ months in 1995	0.073	0.066	0.007	0.124	0.139	-0.015	0.101	0.115	-0.015
Not employed, worked 6+ months before 1995	0.352	0.373	-0.021	0.293	0.286	0.007	0.281	0.249	0.032
Never employed	0.088	0.097	-0.009	0.064	0.067	-0.003	0.083	0.101	-0.018
Transfer income	205	217	-12	214	191	23	138	138	0
Welfare	0.325	0.325	0	0.283	0.313	-0.030	0.255	0.239	0.016
Food stamps	0.615	0.623	-0.008	0.563	0.551	0.012	0.573	0.537	0.037
Persons in household	3.10	3.21	-0.11	3.31	3.31	0.00	2.85	2.86	-0.01
Adults in household	1.40	1.43	-0.03	1.42	1.46	-0.04	1.37	1.39	-0.02
Children in household	1.71	1.78	-0.08	1.89	1.85	0.04	1.48	1.48	0.00
Married	0.187	0.195	-0.009	0.221	0.220	0.001	0.171	0.183	-0.012
Single females	0.705	0.699	0.005	0.699	0.701	-0.003	0.719	0.721	-0.001
Age	35.3	36.3	-0.9	34.2	34.4	-0.2	33.7	34.0	-0.3
Partial disability	0.342	0.377	-0.035	0.305	0.335	-0.029	0.307	0.313	-0.006
Full disability	0.244	0.285	-0.041	0.205	0.245	-0.040	0.202	0.222	-0.020
African American	0.560	0.544	0.016	0.305	0.303	0.003	0.395	0.393	0.001
Hispanic	0.114	0.136	-0.022	0.141	0.147	-0.007	0.127	0.139	-0.012
Education (years)	11.2	11.0	0.2	11.7	11.7	0.0	11.5	11.4	0.1
N	193	455		249	607		228	561	

Notes: None of the differences in the exhibit were statistically significant at the 10-percent level. Sample restricted to householders less than 55 years of age. All variables are measured as monthly averages with the exception of the employment history variables.

^a Interviews conducted April 1996 to July 1996.

Source: See exhibit 10

will still show significant differences.¹¹ Overall, the results in exhibit 1 strongly support the success of the statistical match. The propensity score procedure appears to have successfully produced comparison groups with characteristics similar to the subsidized groups at the beginning of the panel.

Because the matching is done with replacement, it was possible for a single comparison group member to be matched to multiple subsidy group members. This is an important criterion for evaluating the success of a statistical match, since a high rate of multiple matches can indicate that the data contain few (or no) good matches (Dehejia and Wahba, 1999). Fortunately, there appear to be many unsubsidized cases available as matches that are similar to the subsidized group. About 2.4 unique comparison cases were chosen per subsidy group member (compared to the 3.0 there would have been if no case had matched twice). More than 80 percent of cases were used only once as matches, and less than 5 percent were used three or more times.

Methodology and Potential Biases

The goal of this study is to follow two groups of households, one that was subsidized at the beginning of the panel and one that was unsubsidized, and examine their outcomes later in the panel. As mentioned above, the administrative data identifies households that were subsidized at some point during the first year of the panel or a few months before. Exhibits 2A–2C provide some empirical evidence regarding self-reported subsidy rates over the life of the panel for those listed in the HUD data as subsidized. In the first year of the panel, self-reported subsidy rates were about 80 percent for public housing residents. The rates fall below 100 percent mainly due to underreporting in the SIPP, an important reason for using administrative data instead.¹² As time goes on, some households leave subsidized housing. By the end of the panel, subsidy rates fell to 61 percent for public housing residents. There is a similar pattern for the other two programs, with voucher recipients leaving the fastest.

All of these households, including those that had moved out of subsidized housing, remain in the subsidized group. One reason for this approach was to avoid the obvious sample selection problems that would result if only continuously subsidized households were included in the subsidized group. That is, those who remained longer in subsidized housing may have been more likely to have preexisting disadvantages than the average subsidized or comparison group member. In addition, this procedure allows for the possibility that the effects of subsidized housing may linger, even after households leave. For example, if connections with the labor market deteriorated during time spent in subsidized housing, these connections presumably would not be rebuilt immediately upon exit.

One potential concern is the possibility that subsidized households are more likely to underreport their income to SIPP interviewers for fear that their answers will be reported to the PHA. Three factors mitigate this concern. First, subsidized households report 10 to 20 percent more income to the survey than they do to HUD; this difference provides some evidence that subsidized households believe the Census Bureau's confidentiality promises. Second, many comparison group members receive other means-tested subsidies (especially food stamps) and have similar incentives to misreport income. Third, many in the initially subsidized group (perhaps a quarter) have left for private-market housing by the end of the panel and no longer have this incentive to underreport their income.

Possible Biases

The major threat to the matching procedure validity is the possibility that those in subsidized housing might be more disadvantaged, in unobserved ways, than those in the comparison group. For example, they might have had lower motivation to find work or might have been

Exhibit 2

Proportion with Self-reported Housing Subsidy: Subsidized vs. Comparison Group

Exhibit 2A. Public Housing

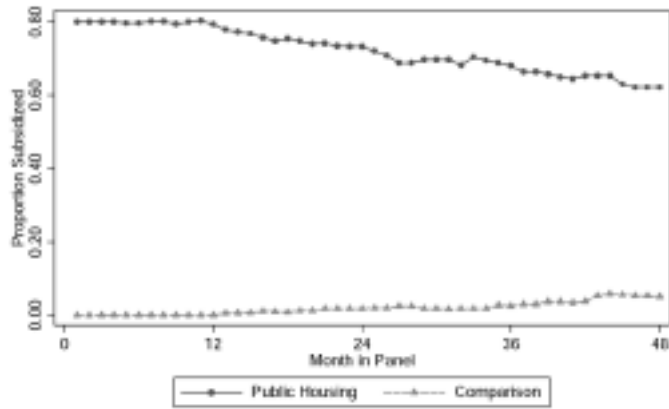


Exhibit 2B. Vouchers

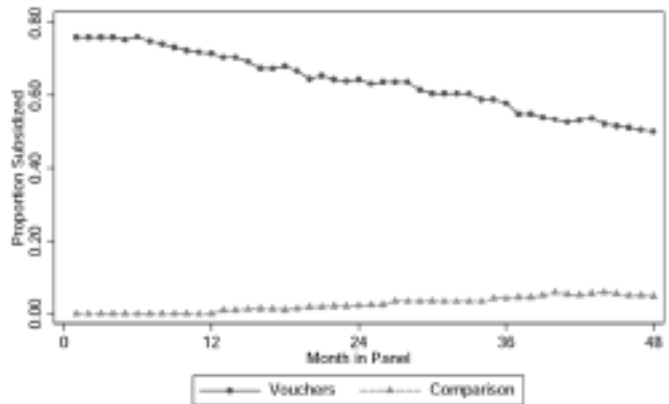
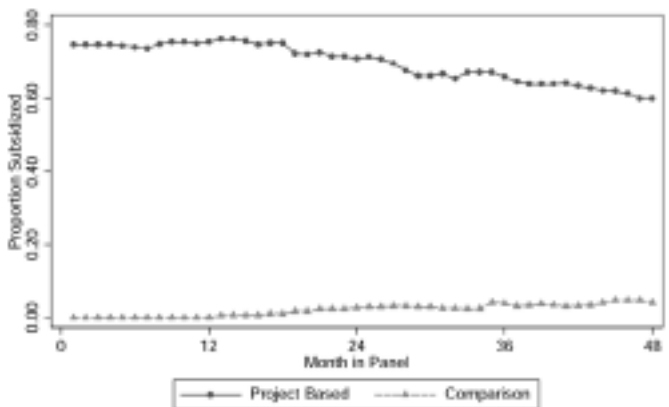


Exhibit 2C. Project Based



cares for sick relatives, neither of which is controlled for here. After all, disadvantages were presumably the reason they chose to move into subsidized housing in the first place. Indeed, during the period studied here, federal law required local PHAs to give preference to households that had high rent burdens, were displaced by federal action, were living in substandard housing, or were homeless.¹³

In general, selection for subsidized housing depends on PHA policies, applicants' actions, public and private landlords' actions, and the availability of suitable housing. In the end, this process probably selects for households with disadvantages not easily captured in survey data, but there are some factors that work to offset this selection. Public and private landlords have incentives to choose stable tenants; those likely to pay the rent and unlikely to cause property damage or other problems. For the voucher program in particular, approximately 20 to 30 percent of households offered rent vouchers failed to use them (Finkel and Buron, 2001). They were screened out by landlords or were otherwise unable to find qualifying housing. Shroder (2002b) finds some empirical evidence that voucher recipients with more disadvantages (those with disabilities or lacking cars) are less likely to find housing on which to spend their subsidies.

Minimizing Potential Biases

Concerns that housing subsidy programs systematically select those with unmeasured disadvantages can be interpreted econometrically as the possibility that we are matching those with permanently low incomes to those with only temporarily low incomes. Perhaps we are matching subsidized householders who were out of the labor force because of disabled children (or some other long-term factor) with comparison households that were suffering unemployment due to temporary layoffs. A similar potential problem is random measurement error. It could be that the comparison group simply had a negative error term in the beginning of the panel but rapidly reverted to the mean after the time of the match.¹⁴

This discussion suggests that a method for minimizing this problem is to match using variables measured over a longer period. For example, annual earnings will be closer to permanent earnings than monthly earnings will be. One way in which this idea is implemented is by matching on the employment history variables, which should provide some information about the more permanent components of earnings. In addition, the statistical match is based on variables from both wave one (months 1–4) and wave three (months 9–12). This choice of variables is likely to be superior to matching on 12 months of earnings for two reasons. First, using two separate measures allows the matching procedure to capture a trend. Second, those with temporary shocks to earnings will be screened out by this procedure, but might have spuriously matched if annual earnings had been used. For example, a high earner who is unemployed in wave one is likely to have found a job by wave three (because 9-month spells of unemployment are unusual). The higher earnings in wave three will then cause the match to be rejected. Annual earnings would not cause this bad match to be rejected; we would just see a year of low earnings and would not realize that this person's earnings rebounded later in the year. Using waves one and three means that an unemployment spell has to be at least 6 months long before it can affect both waves and, even then, it will do so only if it begins in month 4. Below, results with various combinations of waves are explored further.

Illustration of Matching on Temporary Dips

Exhibit 3A illustrates the potential problem caused by matching on temporary shocks. In this exhibit, the comparison group was matched using only wave one variables and no employment history data. In the exhibit, both the voucher recipients and their comparison group have falling poverty rates over time, but the comparison group's poverty rates fall faster. Much of the difference is due to a rapid fall in the comparison group's poverty

rates in month 5, between the first and second waves.¹⁵ This pattern casts doubt on the plausibility of interpreting the subsidy/comparison differences as causal and suggests that those with long-term troubles have been matched to comparison members experiencing a brief dip in income due to temporary problems. Exhibit 3B shows the same outcome but includes a comparison group matched using waves one and three and the employment history variables; the rest of this study uses this procedure. In this exhibit, there is less evidence of a jump in the beginning of the panel. The comparison group poverty rate falls faster than that of the public housing residents, but the fall is more gradual. This exhibit is more illustrative of what would be expected if the differences were truly causal, since we expect any effects of subsidized housing to build up slowly over time. In addition, there is nothing special about any particular month of the panel that should cause such a jump. Examining exhibits like these led to the decision to match on multiple waves of data.¹⁶

Exhibit 3

Illustration of Matching on Temporary Dips: Wave One Match With No Employment History Variables Compared With Matching on Waves One and Three and Employment History

Exhibit 3A. Wave One Only Match

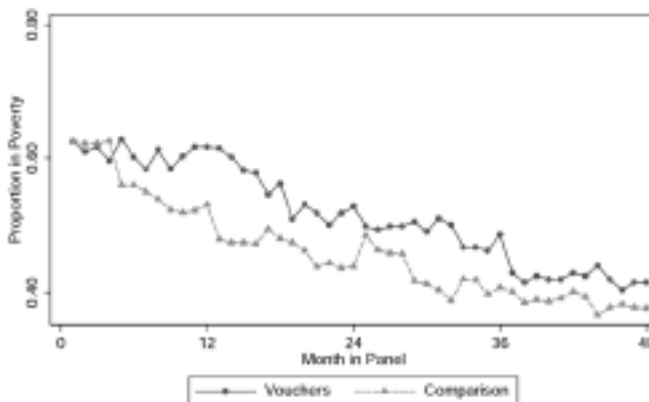
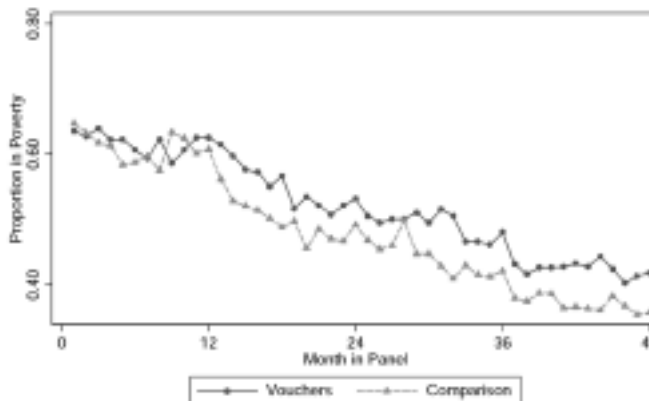


Exhibit 3B. Waves One and Three Match



Baseline Results

Levels

Exhibits 4A–9A, 4B–9B, and 4C–9C show selected outcomes over the panel’s life for the comparison group and for those with public housing, vouchers, or project-based subsidies, respectively. All the dependency-related outcomes (poverty, earnings, employment, food stamps, and Aid to Families with Dependent Children [AFDC]/Temporary Assistance for Needy Families [TANF]) show strong positive trends. Family earnings almost doubled by the end of the public housing group’s panel, while increasing by a factor of more than 2.5 for the comparison group. Employment rates rose by 11 percentage points for those in public housing and by 17 percentage points for the comparison group. Similarly, poverty and the receipt of food stamps and AFDC/TANF fell sharply for both groups. Those receiving vouchers or project-based subsidies experienced similar gains, with voucher recipients improving their situation most rapidly. Exhibits 9A–9C display an important demographic outcome, the number of adults per household, which rises fairly substantially over time.

From 1996 to 1999, the economy achieved a strong recovery, and the unemployment rate fell from 5.4 to 4.2 percent.¹⁷ Single mothers posted large employment gains (see, for example, Lerman, 2003) and welfare rolls fell sharply as states implemented welfare reform. Grogger, Karoly, and Klerman (2002) found that from 1993 to 1999 single mothers’ average earnings rose by 35 percent in real terms and employment rates rose from 69 to 83 percent. Hence, subsidized housing residents’ strong gains are not too surprising; they were able to take advantage of the 1990s’ economic boom.

Differences: Income

Exhibit 10 shows the wave twelve (final wave) results for the three subsidy groups and their comparison groups. The underlying data consist of monthly averages over the 4 months of the wave. Public housing residents had substantially lower family incomes than their comparison group, an average of \$1,502 per month compared to \$1,753, and 8 percent higher poverty rates. The differences were smaller for voucher and project-based subsidy recipients. None of the differences for vouchers or project-based subsidies were statistically significant, although the point estimates are all in the same direction as those for public housing.¹⁸

Differences: Employment and Earnings

The results for earnings were generally similar to those for income, pointing toward reductions in earnings. Public housing residents had family earnings \$235 lower than the comparison group, and those with project-based subsidies had family earnings \$277 lower. There were no statistically significant differences for voucher recipients, although the point estimates of reductions in earnings were similar to those for the other two programs.

There were no statistically significant effects on employment, implying that the earnings impacts are due to a reduction in work hours or wages among the employed. Regression-adjusted results not presented here find employment reductions in line with the earnings reductions for public housing residents but not for the other two programs.¹⁹ Decomposing the earnings impacts into the effects of employment, hours, and wages is not pursued further here; instead, the focus is on earnings.

Differences: Transfer Programs

Most of the differences between the subsidized and comparison groups are fairly small; only two of nine differences are statistically significant and point toward greater dependency.

Exhibit 4

Proportion in Poverty: Subsidized vs. Comparison Group

Exhibit 4A. Public Housing

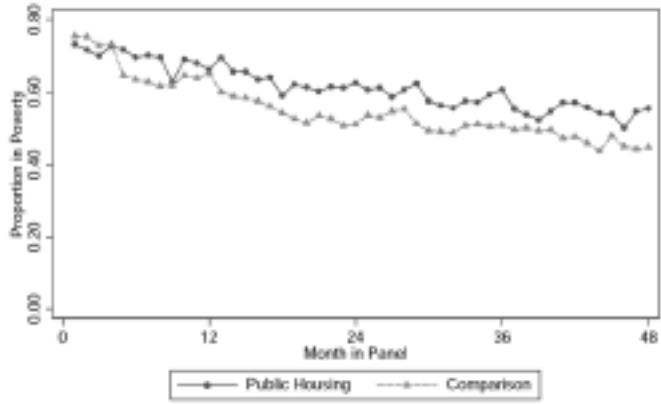


Exhibit 4B. Vouchers

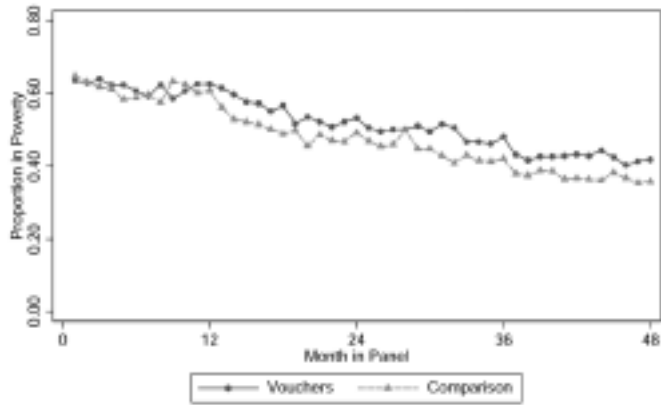


Exhibit 4C. Project Based

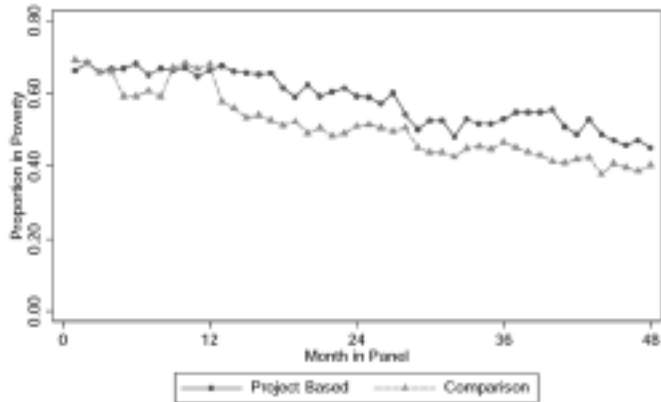


Exhibit 5

Monthly Family Earnings: Subsidized vs. Comparison Group

Exhibit 5A. Public Housing

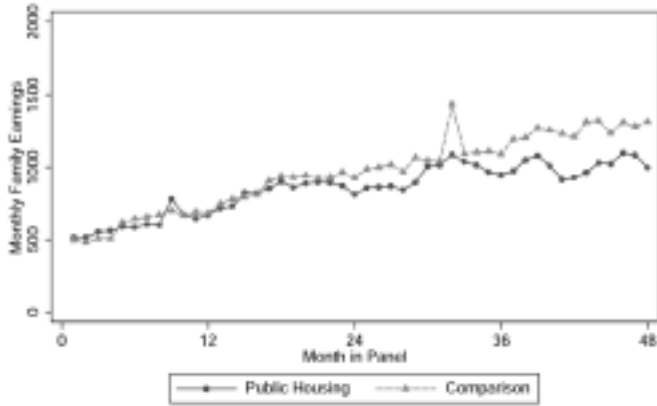


Exhibit 5B. Vouchers

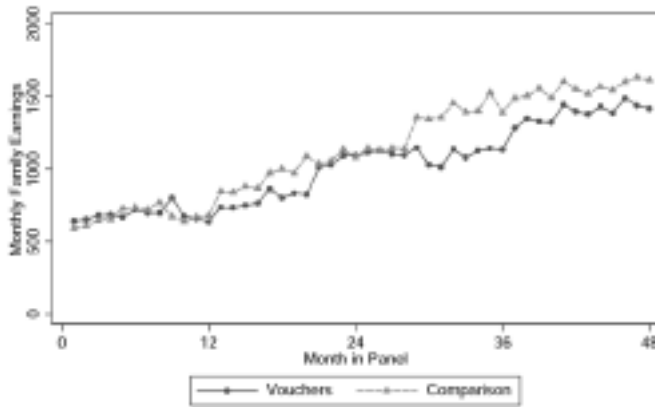


Exhibit 5C. Project Based

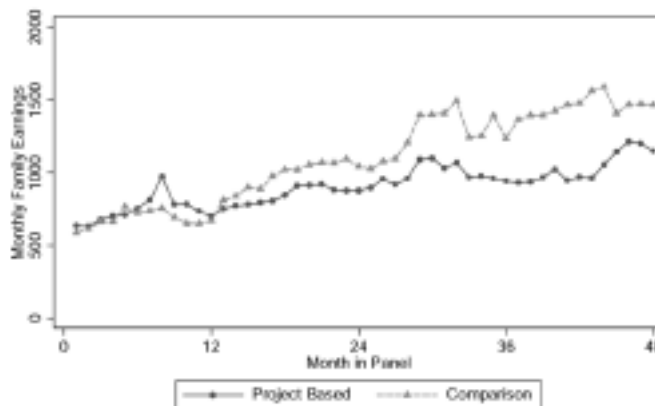


Exhibit 6

Proportion Employed: Subsidized vs. Comparison Group

Exhibit 6A. Public Housing

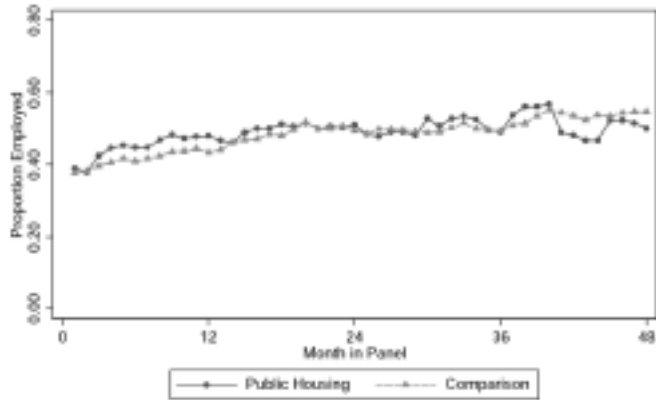


Exhibit 6B. Vouchers

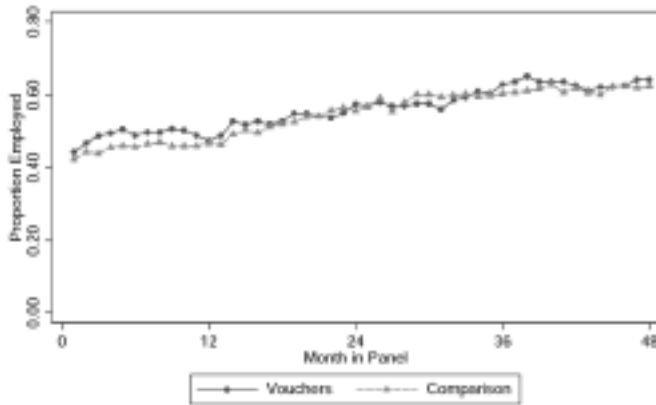


Exhibit 6C. Project Based

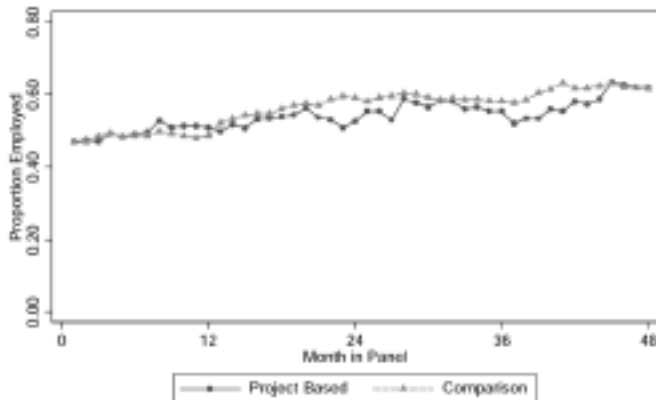


Exhibit 7

Proportion With Food Stamps: Subsidized vs. Comparison Group

Exhibit 7A. Public Housing

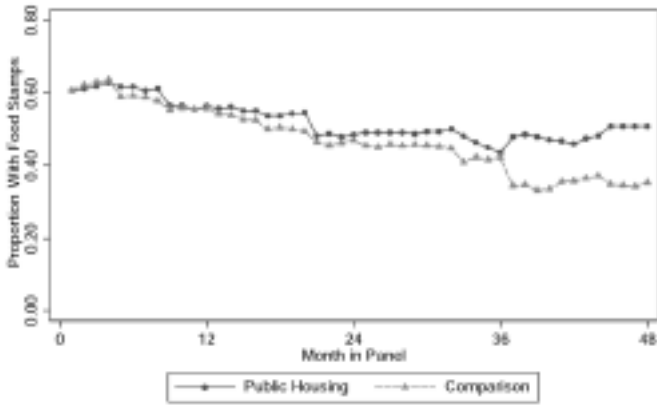


Exhibit 7B. Vouchers

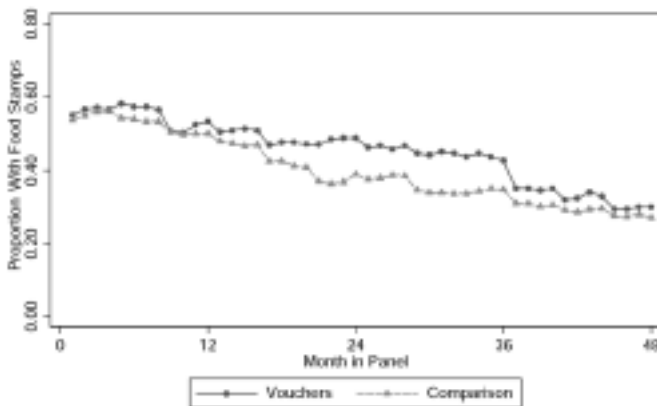


Exhibit 7C. Project Based

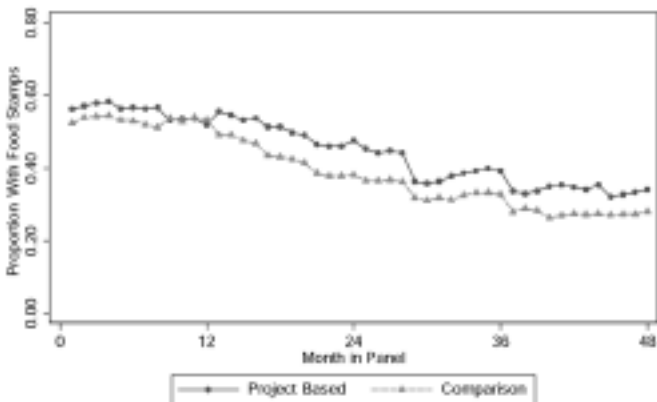


Exhibit 8

Proportion With AFDC/TANF: Subsidized vs. Comparison Group

Exhibit 8A. Public Housing

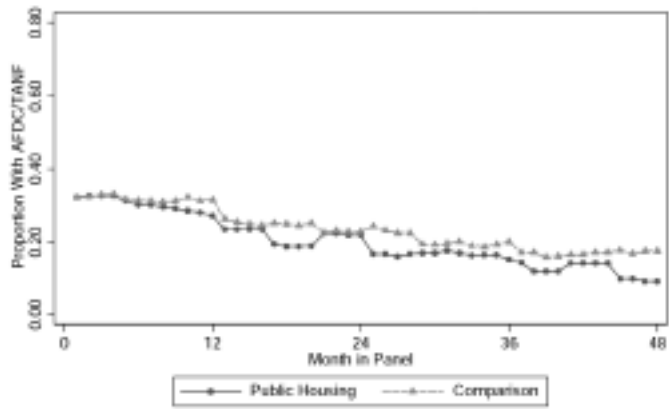


Exhibit 8B. Vouchers

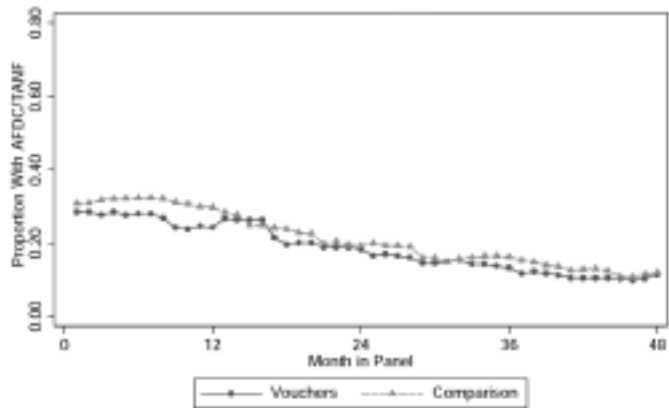


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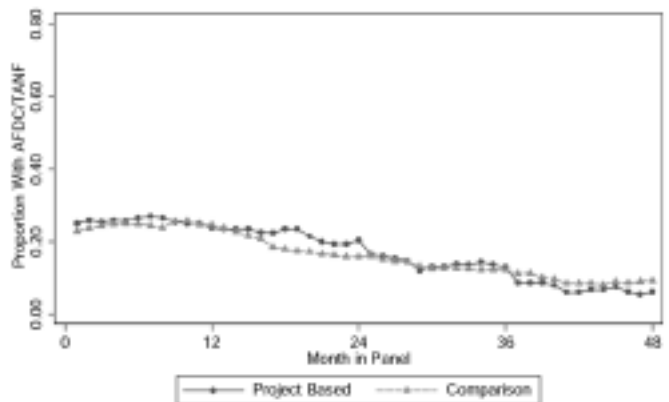


Exhibit 9

Adults per Household: Subsidized vs. Comparison Group

Exhibit 9A. Public Housing

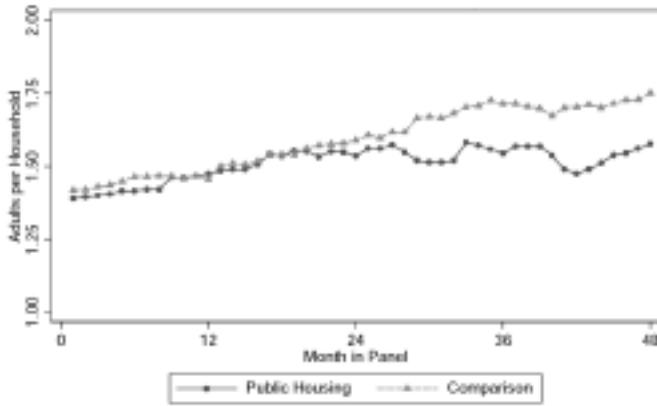


Exhibit 9B. Vouchers

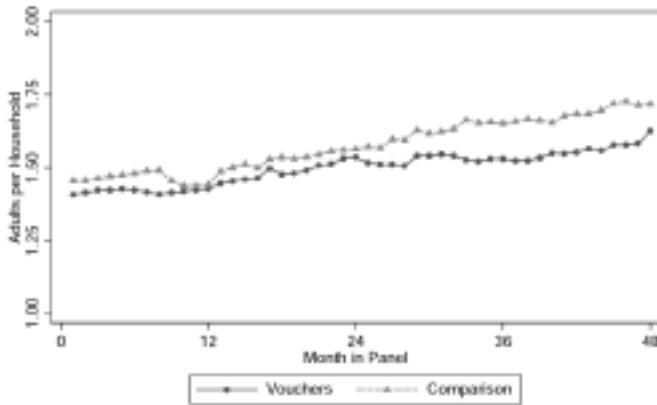


Exhibit 9C. Project Based

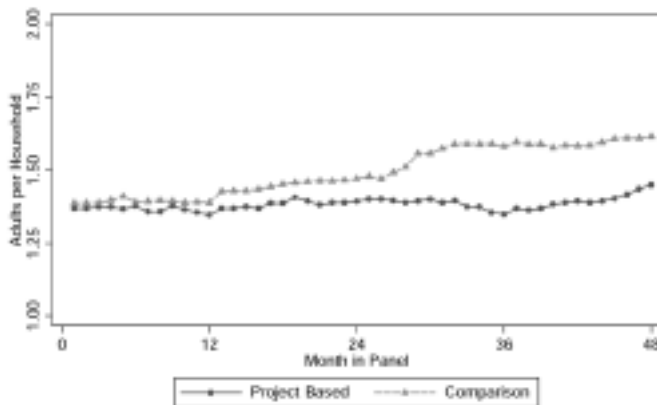


Exhibit 10

Subsidized Households vs. Comparison Groups: Average Income, Earnings, Program Participation, Household Composition, and Housing Subsidy Receipt in Wave Twelve^a

	Public		Comparison		Difference		Vouchers		Comparison		Difference		Project	
	Household	Group	Household	Group	Household	Group	Household	Group	Household	Group	Household	Group	Based	Comparison
Family income	1,502	1,753	1,825	1,972	-252 *	-147	1,825	1,972	1,567	1,763	-195	1,567	1,763	-195
Individual income	987	1,103	1,192	1,263	-116	-71	1,192	1,263	1,070	1,129	-59	1,070	1,129	-59
Poverty	0.534	0.454	0.414	0.365	0.08 *	0.049	0.414	0.365	0.461	0.397	0.063	0.461	0.397	0.063
Family earnings	1,048	1,283	1,430	1,594	-235 *	-164	1,430	1,594	1,175	1,452	-277 **	1,175	1,452	-277 **
Family employment	0.604	0.652	0.723	0.708	-0.047	0.015	0.723	0.708	0.682	0.713	-0.031	0.682	0.713	-0.031
Individual earnings	659	759	880	995	-99	-115	880	995	778	903	-126	778	903	-126
Employed (person)	0.515	0.542	0.632	0.620	-0.027	0.011	0.632	0.620	0.624	0.621	0.003	0.624	0.621	0.003
Transfer income	149	146	175	101	2	74 **	175	101	89	101	-12	89	101	-12
Welfare	0.095	0.173	0.105	0.113	-0.078 **	-0.008	0.105	0.113	0.063	0.089	-0.026	0.063	0.089	-0.026
Food stamps	0.508	0.347	0.296	0.275	0.160 ***	0.022	0.296	0.275	0.330	0.274	0.055	0.330	0.274	0.055
Adults in household	1.56	1.73	1.59	1.72	-0.18 **	-0.13 *	1.59	1.72	1.43	1.61	-0.18 ***	1.43	1.61	-0.18 ***
Children in household	1.77	1.72	1.73	1.63	0.04	0.10	1.73	1.63	1.37	1.38	0.00	1.37	1.38	0.00
Married	0.229	0.292	0.281	0.308	-0.063	-0.027	0.281	0.308	0.206	0.280	-0.074 *	0.206	0.280	-0.074 *
Single female	0.652	0.631	0.622	0.616	0.020	0.006	0.622	0.616	0.670	0.637	0.033	0.670	0.637	0.033
Housing project (self-report)	0.555	0.024	0.143	0.028	0.531 ***	0.115 ***	0.143	0.028	0.410	0.029	0.381 ***	0.410	0.029	0.381 ***
Other housing subsidy (self-report)	0.068	0.029	0.365	0.022	0.039 *	0.343 ***	0.365	0.022	0.197	0.016	0.181 ***	0.197	0.016	0.181 ***
N	132	321	184	443			184	443	147	405		147	405	

Notes: *** = Statistically significant at the 1-percent level; ** = 5-percent level; * = 10-percent level. Sample restricted to householders less than 55 years of age. All variables are measured as monthly averages with the exception of the employment history variables.

^a Interviews conducted December 1999 to March 2000.

Sources: Merged Census Bureau survey data and Department of Housing and Urban Development administrative data from the 1996 Survey of Income and Program Participation, Multifamily Tenant Characteristics System, and Tenant Rental Assistance Certification System

Public housing is estimated to increase food stamp receipt by 16 percentage points, a substantial effect, and vouchers are estimated to increase total transfer payments by \$74 per month, which is also substantial relative to the comparison group's mean of \$101. None of the programs increased welfare (AFDC/TANF) participation, and public housing is actually estimated to have reduced participation by 7.8 percentage points. The other six effects are fairly small and statistically insignificant. Overall, these inconsistent and often statistically insignificant results weigh against the conclusion that housing subsidies substantially affect program receipt.

Differences: Household Composition

None of the programs have much effect on the number of children in the household. Point estimates of the effects on marriage are moderately large, although only the estimate for project-based subsidies is statistically significant. Residents of public housing and other subsidized projects did have smaller increases in the number of adults in the household than did the comparison group. Reductions of 0.13 to 0.18 adults for the three programs are somewhat sizeable and statistically significant. An important topic for future research is to decompose this effect into the change due to marriage, cohabitation, and other types of living arrangements. Below, we examine to what extent the reduction in household size is responsible for the observed differences in family earnings.

Comparisons Across Programs

F-tests of the null hypothesis that the three programs had identical impacts (subsidized/comparison differences) were estimated for all the outcomes in exhibit 10. In most cases, the tests could not reject the null hypothesis that the differences across programs were all equal.²⁰ Although public housing, for example, shows more statistically significant differences than do vouchers, as indicated by the asterisks in exhibit 10, the F-tests show that caution is warranted before concluding that the effects of the two programs are different. In other words, we should be wary of interpreting any of these results as suggesting that outcomes in one program are better or worse than in another.

Census Tract Poverty Rates

One goal of this study is to investigate the effects of neighborhoods on subsidized housing residents separately from the other possible effects discussed above. Exhibit 11 reports statistics on one measure of neighborhood quality—the census tract poverty rate—measured in 1990 for the tract households occupied in early 1996. Public housing residents live in census tracts with poverty rates that are 8.8 percentage points higher on average than those of the comparison group, a substantial difference. Voucher recipients actually live in tracts with poverty rates that are lower by 2.3 percentage points than the comparison group. Since housing subsidy recipients tend to be quite disadvantaged, the major concern was finding a comparison group that was as disadvantaged as the subsidized groups. Although the difference in tract poverty rates was statistically significant, the sign of the difference alleviates concerns that the comparison group was too advantaged. Finally, those receiving project-based subsidies were located in tracts with poverty rates 2.6 percentage points higher than their unsubsidized counterparts. Overall, the matching does not control for differences in the types of neighborhoods that subsidized households live in. Thus, any differences we observe between the subsidized and comparison groups may be due partly to neighborhood effects. We could control for this difference in tract poverty rates simply by including this measure in the matching equation. Instead, tract poverty rates are left out at this stage and the effect of neighborhood on subsidized households is investigated further below.

The differences in exhibit 11 are also estimates of the effect of subsidized housing on the neighborhoods where the disadvantaged population chooses to live. We are comparing

the neighborhoods of subsidized households to those of a matched sample chosen to be similar on the basis of individual characteristics. It turns out that public housing residents live in much poorer neighborhoods than do other households with similarly low incomes, low likelihood of marriage, and so on. This is not surprising, since the neighbors of many residents of public housing are also residents of the same large projects. Assuming that no important determinants of neighborhood choice are omitted from the matching equation, these differences can be interpreted as subsidized housing’s effect on residents’ neighborhood choices.

Overall, these results line up well with expectations. The tract poverty rates for the different subsidy programs are fairly similar (within a few percentage points) to those reported by HUD (Burke, 1998). In addition, many analysts have pointed to the tendency of public housing projects to spatially concentrate low-income people. An important goal of the project-based and voucher subsidy programs was to deconcentrate poverty in response to the perceived troubles of the older public housing program.²¹ This goal was to be achieved either by building smaller, “scattered site” subsidized developments in higher income neighborhoods or allowing voucher recipients to choose their neighborhoods. The results in exhibit 11 suggest that the project-based programs have succeeded in increasing concentrated poverty by less than the public housing program has, while the voucher program has been able to modestly reduce the concentration of low-income people. The current voucher program may do more to spatially disperse low-income people than these results suggest. In 1996, rent vouchers could be used only in the jurisdiction where they were issued (generally, a city or county); today, vouchers are “portable.”

Exhibit 11

Subsidized Households vs. Comparison Groups: Census Tract Poverty Rates (1990) in Wave One^a

	Subsidized Group	Comparison Group	Difference
Public housing	32.8	23.9	8.8***
Vouchers	19.3	21.6	-2.3**
Project based	24.2	21.6	2.6**

Notes: *** = Statistically significant at the 1-percent level; ** = 5-percent level; * = 10-percent level. Sample restricted to householders less than 55 years of age.

^a Interviews conducted April 1996 to July 1996.

Source: See exhibit 10

Explaining the Subsidy Effects

Exhibit 12 shows results that combine matching with regression. These results enable us to examine how much of the subsidy effect is due to tract poverty rates and the presence of “extra” adults. For example, results in the upper left derive from a regression of family earnings in wave twelve on all the matching variables, measured in waves one and three, and an indicator for residence in public housing during the panel’s first few waves. The regression was estimated in the matched sample of public housing residents and unsubsidized households. The first column in each pair reports the coefficient on subsidy status. The second column reports results from a model in which tract poverty rates (measured in wave one) and indicators for the number of adults in the household (measured in wave twelve) are included. The matching/regression approach allows us to control for some simple noneconomic factors (or at least some factors not included in a simple neoclassical model). In addition, this “belt and suspenders” approach of using regression to control for any remaining differences in the matched samples also has some technical advantages. For example, it reduces the standard errors of the estimates and can reduce bias as well.²²

Exhibit 12

Regression-adjusted Matching Estimates
 Effect of Housing Subsidy Receipt, Tract Poverty Rates, and Household Size on Family Earnings, Individual Earnings, and Number of Adults in Household in Wave Twelve^a

	Public Housing		Vouchers		Project Based		Any Subsidy
Family Earnings							
Public housing	-240 ** (116)	-70 (112)	-158 (133)	-44 (123)	-195 * (106)	-49 (95)	-213 *** (75)
Wave one tract poverty rate (%)		-6.08 (4.56)		1.67 (4.49)		-10.02 *** (3.78)	-4.08 (2.71)
Two adults in household, wave twelve		806 *** (161)		938 *** (141)		946 *** (151)	880 *** (99)
Three or more adults in household, wave twelve		1857 *** (348)		2132 *** (283)		2003 *** (318)	1991 *** (225)
R-squared	0.30	0.41	0.29	0.43	0.37	0.48	0.29
Individual Earnings							
Public housing	-129 * (75)	-109 (83)	-141 * (83)	-128 (86)	-131 * (72)	-126 * (74)	-144 *** (48)
Wave one tract poverty rate (%)		-1.40 (3.29)		1.89 (3.61)		-3.76 (2.61)	-0.65 (1.96)
Two adults in household, wave twelve		26 (85)		-107 (96)		-48 (71)	-47 (55)
Three or more adults in household, wave twelve		178 (253)		113 (166)		176 (214)	158 (146)
R-squared	0.34	0.34	0.37	0.37	0.48	0.47	0.37
Adults in Household							
Public housing	-0.119 * (0.070)	-0.064 (0.072)	-0.090 (0.065)	-0.087 (0.067)	-0.153 ** (0.062)	-0.148 ** (0.063)	-0.114 *** (0.041)
Wave one tract poverty rate (%)		-0.004 * (0.002)		-0.002 (0.002)		0.001 (0.002)	-0.002 (0.002)
R-squared	0.40	0.41	0.29	0.31	0.30	0.30	0.31
N	453	447	627	606	552	538	1,359

Notes: *** = Statistically significant at the 1-percent level; ** = 5-percent level; * = 10-percent level. Sample restricted to householders less than 55 years of age. Other explanatory variables in the regression are the same wave one and three variables as are in the matching function. See text and exhibit 1. Table entries are OLS regression coefficients with heteroskedasticity-consistent standard errors in parentheses.

^a Interviews conducted December 1999 to March 2000.

Sources: Merged Census Bureau survey data and U.S. Department of Housing and Urban Development administrative data from the 1996 Survey of Income and Program Participation, Multifamily Tenant Characteristics System, and Tenant Rental Assistance Characteristics System

Estimates Without Additional Controls

The regression-adjusted results are broadly similar to the simple comparison of means for the matched sample in exhibit 10. There is little change for family earnings except that the reduction in earnings for project-based recipients falls from \$277 to \$195 a month, remaining statistically significant at the 10-percent level. The reduction in individuals' earnings increases slightly and becomes statistically significant at the 10-percent level for all three programs. The reduction in the number of adults in the household decreases somewhat, falling from -0.13 to -0.09 for the voucher recipients, and becomes statistically insignificant for this group. This exhibit also adds a fourth pair of columns for all three programs combined. The results for the pooled sample are always statistically significant at the 1-percent level.

Tract Poverty Rates

In general, tract poverty rates have only modest effects. The point estimates for public housing and project-based subsidies are moderate, and only the estimate for project-based assistance is statistically significant. A coefficient of -6 implies that a 10-point increase in tract poverty rates reduces family earnings by \$60, which would explain a quarter of the baseline \$240 reduction in family earnings. A 10-point increase is a fairly large change; it is more than the estimated difference in poverty rates between public housing residents and their unsubsidized comparison group. A similar increase could explain more than half of the baseline project-based effect of \$195, but this change is much larger than that induced by the program.²³ Tract poverty rates generally have very little effect on either individual earnings or the number of adults in the household. The one exception is for public housing, where a 10-point increase in tract poverty rates is estimated to reduce the average number of adults in the household by 0.04. Although this effect is small, it does explain about one-third of the baseline reduction of -0.12 . One reason why poverty rates may have little effect is that they are measured in wave one, and many households have moved during the panel's 4 years. Still, the results do suggest that the effect of tract poverty rates is not very long lasting.

The relative unimportance of tract poverty rates is consistent with the generally disappointing findings of the Moving to Opportunity (MTO) experiments, which successfully moved public housing residents to low-poverty neighborhoods by providing vouchers but were not particularly successful in boosting the recipients' employment rates (Orr et al., 2003). The modest effect of neighborhood poverty found here is, at least, not inconsistent with the extensive nonexperimental literature on the spatial mismatch hypothesis. In a recent survey, O'Regan and Quigley (1999: 460–461) concluded, "Job access does play a role in gaining employment, at least for youth, but none of the research suggests it is the primary determinant. Individual characteristics (education, job skills) and labor market conditions (unemployment, industry mix) clearly dominate." That is, the spatial mismatch literature has found that accessibility plays a relatively modest role despite the fact that this research focuses on youth, whose employment rates are likely to be especially responsive to job access.

Adults in Household

The number of adults in the household has a substantial effect on family earnings. This finding is not surprising, since more adults, if they are related and have any earnings, will mechanically increase total family earnings. The presence of additional adults in a household has no statistically significant effect on individual earnings. A priori, more adults could either provide childcare, facilitating the householder's opportunity to work, or provide extra income, reducing the need for the householder to work. The results, however, find no clear effect—positive or negative.

Explaining the Subsidy/Comparison Gap

Overall, adding tract poverty rates and the number of adults in the household to the model sharply reduces the estimated negative effects on family earnings. These two measures “explain” about three-quarters of the estimated reduction in family earnings for the three individual programs, or 56 percent when the three programs are pooled. The additional variables have very little effect on the householders’ earnings. Tract poverty rates explain about half of the reduction in the number of adults per household for the public housing sample but explain little for the other two programs. These differing results make sense, since public housing residents live in much poorer (by 9 percentage points) tracts than do others with similar individual characteristics.

Alternative Estimates

The methodological discussion above emphasizes the importance of matching over as long a time period as possible and using retrospective data on prepanel employment to avoid matching subsidized members with more permanent disadvantages to comparison group members who are experiencing only temporary difficulties. At the same time, matching over too long a time period reduces the period of followup available.

To explore this issue, the key results were reestimated using alternative matching periods.²⁴ In general, the earnings impacts were reduced as the length of the matching period increased. For example, the impact on family earnings for public housing is \$549 when the match includes only wave one and no employment history variables, \$427 with the employment history variables added to the matching equation, and \$293 when the match includes waves one and four. In general, adding wave two results in large reductions in the earnings impact, but replacing wave two with wave three or four has a relatively small effect and sometimes increases the impact. It appears, then, that adding the retrospective employment data and at least a second wave makes an important difference, presumably leading to fewer matches based on temporary earnings shortfalls. There is no strong reason, however, to choose wave two, three, or four as the second wave in the match.

The impacts for the number of adults in the household are affected much less by the length of the matching period, especially for public housing and project-based assistance, which is consistent with the fact that living arrangements last longer than jobs do.

Probably the most relevant changes in the economic environment facing low-income households during the period examined here were welfare reform and the associated sharp drop in welfare caseloads. To examine the impact of welfare reform, the models were reestimated with the addition of Ellwood’s (2000) measure of noneconomic caseload drops (caseload drops due to changes in welfare rules rather than economic factors). This proved to have little effect on the results, probably because the subsidized and control groups were well balanced with respect to the Ellwood caseload drop measure.

The results presented herein are not weighted and there is little reason to do so; matching and regression methods are alternatives to weighting. The strongest argument for weighting with the Census Bureau’s sampling weights is that the Survey of Income and Program Participation oversamples high-poverty areas, which may affect the levels (but not the treatment effects). When exhibit 10 was reestimated using weights (specifically, applying the subsidized group’s weights to both the subsidized households and their comparison households), there was very little change in the differences or the levels.

Comparison to Other Research

A recent study by Abt Associates Inc. (Patterson et al., 2004) is of great interest because it is the only experimental study of the effect of vouchers on earnings, welfare receipt, and neighborhood poverty rates. Patterson et al. examined a pilot program that gave vouchers to randomly chosen current and former TANF recipients in six cities; the researchers analyzed outcomes over a seven-quarter followup period.²⁵ Compared to the SIPP sample used here, Abt Associates' sample was somewhat more disadvantaged, but not extremely so.²⁶

The most comparable findings from the Patterson et al. study are for those living in their own unsubsidized households before receiving vouchers. As with the SIPP sample, which is limited to householders, these results from Patterson et al. exclude those living with friends or relatives and those living in homeless shelters, who may have experienced different impacts. In addition, results for this subgroup exclude those living in public or project-based subsidized housing before receiving vouchers, in line with the SIPP comparison group, which is limited to unsubsidized households.

For this subgroup, the Abt study found an earnings reduction of 11.0 percent, an increase in transfer payments of 10.0 percent, and an increase in tract poverty rates of 0.5 percentage points. Only the earnings impact was statistically significant. In comparison, this study estimated a reduction in individual earnings of 14.2 percent (an impact of \$141 from exhibit 12 divided by a control mean of \$995 in exhibit 10). An important caveat is that slightly different matching periods resulted in estimating smaller and statistically insignificant earnings reductions. For the other two programs, we found reductions in individual earnings of 17 percent (public housing) and 14 percent (project based). These estimates are not significantly different from each other or from the voucher impact. This study also found inconsistent impacts on welfare receipt and a reduction in tract poverty rates of 2.3 percentage points. The major finding from a comparison of the SIPP-based and Abt Associates studies is that the earnings reductions and, to a lesser extent, the fall in tract poverty rates are of approximately the same magnitude.

Olsen (2004), who performed a nonexperimental study based on panel data, found earnings reductions of the subsidy programs roughly twice the size of those found here: an increase in monthly earnings of \$300 to \$500 relative to the comparison group over 2 years. These findings may be biased because of the relatively limited set of available control variables and because of measurement issues; the subsidy group's earnings were measured with administrative data and the comparison group's earnings were measured with survey data.²⁷ Nonetheless, the Olsen study is of special interest since it is based on methods somewhat similar to those used here. In particular, it is one of the few other studies that makes use of longitudinal data.

Conclusion

This study has examined the effects of subsidized housing on various outcomes related to dependency using a new data set created by an exact match between the SIPP and HUD administrative data. The match to administrative data allows for much more accurate identification of subsidized housing residents and allows the three major classes of subsidized housing to be distinguished, which would not be possible with the SIPP alone. At the same time, the match creates a sample that somewhat underrepresents Hispanics and misses about one-third of truly subsidized residents, requiring the use of survey self-reports to screen out subsidized cases from the comparison group. The statistical matching procedure also worked quite well, at least insofar as it successfully balanced the characteristics of the subsidized and comparison groups.

For almost every outcome, subsidized households shared in the gains of the 1990s' economic boom, showing sharp income and earnings increases and reductions in poverty and transfer program participation. Welfare reform (the introduction of TANF in 1996 and 1997) may have been another factor driving these trends, although this study presents no direct evidence on the reasons for the gains.

Compared to the matched cases, residents of public housing and other types of subsidized projects had substantially less income and earnings growth (by various measures) over the 4 years of the SIPP panel than did unsubsidized households that were similar at the beginning of the panel. Family earnings of those in public housing grew more slowly than those of similar families, ending up 19 percent below the comparison group's level. Those with project-based subsidies received 13 percent less earnings and those with vouchers also had lower earnings at the end of the panel than the comparison group had, but the difference was not statistically significant. Caution is warranted before concluding that the voucher program is "better," however, because we are unable to statistically reject the hypothesis that all three programs have the same impact on family earnings. Indeed, equal impacts cannot be ruled out for almost any outcome examined here.

All three programs had similar effects on individual earnings. Subsidized households ended up with 17 percent (public housing), 14 percent (vouchers), and 15 percent (project-based) lower earnings than the comparison group had. It is possible, of course, that this reduction in earnings may have been offset by increased time spent on childcare or other nonmarket labor, an issue not addressed here.

In contrast to the reductions in earnings, none of the programs increased welfare (AFDC/TANF) receipt, and effects on food stamps or total transfer payments were each found for only one of the three programs.

Public housing and other project-based subsidy programs were found to lower the number of adults in the household by 6.9 and 9.5 percent, respectively. The effects for vouchers were smaller and not statistically significant. The desirability of this effect is less than clear; it may reflect reduced crowding and an increased ability to leave abusive situations or it may reflect a move to a thinner "marriage market" with fewer opportunities for marriage and cohabitation.

Public housing and project-based subsidies move recipients into neighborhoods with poverty rates that are 8.8 and 2.6 percentage points higher, respectively. Voucher recipients, by contrast, live in neighborhoods with poverty rates that are 2.3 percentage points lower than unsubsidized households with similar individual characteristics.

Of course, all these conclusions may potentially be driven by the selection into subsidized housing of those with greater preexisting (unmeasured) disadvantages. To address these issues, the matching models used characteristics that are as permanent as possible (such as several years of employment history) and used the same variables (measured in an earlier period) as the outcomes to be examined. The hope is that year one earnings will capture many of the unobserved characteristics driving earnings in year four. In addition, the use of poverty rates as controls in some models lessens concerns about sample selection bias, especially for the voucher and project-based subsidy groups. Neighborhood poverty rates are likely to be a powerful measure of long-term disadvantage (or advantage), since location decisions are typically based on long-term factors.

At the same time, neighborhood poverty rates did little to explain the impacts on either family or individual earnings, which is consistent with the MTO studies. Combined, the reduction in the number of adults in the household and the move to neighborhoods with higher poverty rates could explain half to three-quarters of the reduction in family earnings

for public housing and project-based subsidies, with household size accounting for the bulk of the decrease. In other words, subsidized housing recipients have lower family earnings because they have less family (that is, they have smaller households).

Tract poverty rates and household size accounted for little of the reduction of individual earnings, casting some doubt on theories of neighborhood effects such as those based on labor market networks, crime, or psychological factors. By process of elimination, we are left with mechanisms that operate directly on individuals (perhaps stigma) or neighborhood effects not closely tied to poverty (such as commuting time). Especially prominent among the remaining explanations are neoclassical income and substitution effects such as the implicit “tax” on work due to rent increasing with income.

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This article reports the results of research and analysis undertaken by U.S. Census Bureau staff. It has undergone a Census Bureau review more limited in scope than that given to official Census Bureau publications. This article is released to inform interested parties of research and to encourage discussion.

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Notes

1. The U.S. Department of Housing and Urban Development allows certain earnings deductions, but for most employed households the marginal “tax” is 30 percent.
2. Another recent experimental evaluation is the series of Moving to Opportunity (MTO) studies (for example, Katz, Kling, and Liebman, 2001). These studies are of less relevance here because they compare voucher recipients to housing project residents. The MTO studies generally found little difference in earnings or welfare receipt between the two housing programs.
3. The Survey of Income and Program Participation is described in detail at <http://www.sipp.census.gov/sipp/intro.html>.
4. Transactions also occur when a household leaves subsidized housing or moves from one subsidized unit to another and for some other administrative reasons.
5. Five transactions are listed as occurring after December 1996; income certifications can be done up to 3 months in advance.
6. Sample members who did not merge because they did not report Social Security numbers to the Survey of Income and Program Participation cannot contaminate the comparison group because they are excluded from both the subsidized and comparison samples.

7. Specifically, cases were coded as living in public housing if they were identified as “Public Housing,” “Indian Housing,” and “Others (Public Housing)” in the HUD Multifamily Tenant Characteristics System (MTCS) data. Cases were coded as receiving vouchers if they were listed as Section 8 Certificates or Section 8 Vouchers in the MTCS data. Cases were coded as project-based subsidies if they were listed as living in “Section 8” (meaning Section 8 new construction or substantial rehabilitation), “Rent Supplement,” “RAP,” “Section 236,” “BMIR,” “Section 202 PRAC” “Section 811 PRAC,” or “Section 202/162 PRAC,” in the HUD Tenant Rental Assistance Certification System data; or as “Mod Rehab” (meaning Section 8 moderate rehabilitation) in the MTCS data.
8. The statistical match was implemented using PSMATCH2 software for the Stata statistical package (Leuven and Sianesi, 2003; Sianesi, 2001). I modified this software to facilitate the creation of a matched file for analysis with procedures other than PSMATCH2.
9. Family income is shown in the exhibit but was not used in the logit because many of the many similar measures included in the logit were deemed sufficient.
10. Additional matching variables used in the propensity score logit but not shown in this exhibit are the presence of the following types of bank savings: savings account, interest-bearing checking account, money market account, and certificate of deposit; several variables measured at wave three, including individual income, transfer income, family employment, family earnings, poverty status, and receipt of food stamps; and the square of age in wave one.
11. In addition, the propensity score is a single index that attempts to summarize a long list of variables. It is possible that, say, a disabled person (raising the chance of subsidy) with a relatively high income (lowering the chance of subsidy) might be considered a good match for a nondisabled person with a low income, since only the propensity score matters.
12. The rates were below 100 percent for two reasons. First, in any given month, a household reported as subsidized by HUD at some time during 1996 may have moved out of subsidized housing or may not have moved in yet. Second, there was some underreporting of subsidy status in the Survey of Income and Program Participation (SIPP). About 10 percent of public housing residents and about 20 percent of those in the other two programs reported “not assisted” to the SIPP (Susin, 2004).
13. Federal preferences were eliminated in 1998 but were in effect during the time when sample members entered subsidized housing.
14. This setting is similar in some ways to Ashenfelter and Card (1985) except that those authors considered the case in which workers were selected into a training program because their earnings were temporarily low, while in the present case households are assumed to select into subsidized housing because the permanent component of the incomes is low.
15. Transitions in the Survey of Income and Program Participation occur more frequently between waves, a phenomenon known as “seam bias.”
16. Voucher recipients’ poverty rates were chosen as the case to graph because of their illustrative value. When matching with only one wave, poverty rates showed the most worrisome pattern of second-wave jumps. When matching over a longer period, the voucher group showed the “nicest” time pattern of the three programs (see exhibits 4A–4C).

17. Figures from the Bureau of Labor Statistics can be found at: <ftp://ftp.bls.gov/pub/special.requests/lf/aat1.txt>.
18. Standard errors in all exhibits are based on the usual formula, which assumes a simple random sample. The standard errors for exhibit 9 were also recalculated using replicate weights (a type of bootstrap procedure) that in principle can account for the stratification and clustering in the sample design. I resampled from the data, conditional on the statistical match, and used replicate weights corresponding to the unweighted data. The differences were typically quite small, as the replicate SEs were about 5 percent larger than the usual SEs, and no statistical test was affected. The replicate SEs may have problems of their own since they rely on a “large” sample for validity, while the sample here is relatively small. In particular, since the number of subsidized observations is fewer than the number of primary sampling units (PSUs are counties or groups of counties), it is questionable whether the replicate weight procedure can correctly account for any within-PSU correlation. Hence, the usual SEs, rather than the replicate SEs, are presented in the exhibits.
19. That is, regression-adjusted estimates analogous to those in exhibit 11.
20. The only substantive exceptions were food stamps (where equality can be rejected at the 10-percent level) and transfer payments (5 percent level). We can also reject that the impacts on the self-reported housing subsidy variables are equal, which is hardly surprising.
21. The goal of spatially deconcentrating poverty was cited in the 1974 law authorizing the voucher program and several of the project-based subsidy programs (Schill, 1993). Another useful history of U.S. subsidized housing programs is Quigley (2000).
22. Combining matching and regression was suggested by Rubin (1973). Recently, Abadie and Imbens (2002) have shown that matching estimators, even though consistent, can be biased in small samples. Abadie and Imbens have suggested combining matching and regression to reduce the bias.
23. In results not shown in the exhibit, when tract poverty rates are entered into the family earnings model without the indicators for number of adults, the poverty coefficients are -8.6 ($t=2.1$), 0.46 ($t=0.10$), and -8.3 ($t=2.15$) for public housing, vouchers, and project-based subsidies, respectively.
24. These results are available from the author upon request.
25. The cities were Atlanta, Georgia; Augusta, Georgia; Fresno, California; Houston, Texas; Los Angeles, California; and Spokane, Washington. Los Angeles had limited followup data and is not included in the figures cited here.
26. Recipients in the Abt Associates’ sample were 3 to 5 years younger on average, twice as likely to have never worked (19 percent), and much more likely to be receiving Temporary Assistance to Needy Families benefits (more than 50 percentage points more) or food stamps (roughly 25 percentage points more), but probably less likely to be disabled (11 percent received Supplemental Security Income).
27. In contrast to the Moving to Opportunity results, Olsen finds that voucher recipients experienced faster earnings increases than participants in the other two programs.

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