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

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This study uses a new dataset 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 three years. The aim is to produce an excellent comparison group by matching on the same variables (measured in an earlier period) as the outcomes to be examined. The main findings are that housing subsidy programs reduce individual earnings by roughly 15% and household size by 5-10%. Although these programs are found to affect neighborhood choice, neighborhood poverty rates explain little of the impact on individual earnings.

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Introduction

This study uses a new dataset 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 three years. The outcomes include: income and poverty status; employment and earnings; receipt of welfare benefits and housing subsidies; 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 dataset 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 longitudinal nature of the SIPP. At the end of the panel, the study compares their outcomes, such as the receipt of food stamps and poverty status.

The hope is to produce an excellent 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, three 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 (e.g., 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 (i.e. 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 (since there is less need to work in order to pay the rent). Other economic effects are possible as well. Housing assistance is likely to cause many recipients to change neighborhoods. Subsidized housing units might be located either closer to or farther from employment sites than alternative unsubsidized residences. Finally, housing subsidies free up additional resources, which might be invested 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 demographics, specifically household composition. Ellen and O'Flaherty (2002) note that housing subsidy

[1] HUD allows certain deductions to earnings, but for most employed households, the marginal "tax" is 30 percent.

programs require recipients to live in units deemed large enough to accommodate their family. 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 a unit more quickly, and voucher recipients will have an easier time finding a unit in the private market. In addition, subsidized housing provides incentives to consume more housing, and one way to do this 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 household.

Subsidized housing can be expected to have a number of more sociological or psychological effects, either 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 (see Katz et al. 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 (see Katz et al. 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.

Perhaps because of these many possible mechanisms though which 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 2002)

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." 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] This study will be discussed in more detail below, after the results are presented. There is more consistent evidence of a subsidy-induced reduction in household size (again, see Ellen and O'Flaherty 2002 and the studies reviewed in Shroder 2002).

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.

Description of Data

The data set used in the project is the 1996 panel of the Survey of Income and Program Participation (SIPP) merged with HUD administrative data on housing subsidy receipt. The SIPP is a national panel data set that follows approximately 40,000 households for four years, covering the period from December 1995 through February 2000. Households in the SIPP are interviewed every four months, for a total of 12

[2] Another recent experimental evaluation is the series of Moving to Opportunity studies (e.g., Katz et al 2001). These 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.

"waves" of interviews. Households from areas with high poverty concentrations are over-sampled.[3]

The HUD administrative data identifies enrollment in the various housing subsidy programs administered by HUD, and the date of the most recent "transaction," as of December 1996. The programs covered include Public Housing, Section 8 Vouchers and Certificates, and a number of project-based subsidy programs, but does not include programs administered by the Rural Housing Service and units funded solely by Low-Income 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 prior to the start of the SIPP panel, usually sometime in the six months prior to the beginning of the panel. In general, the data identifies households that were subsidized at some point during the first year of the panel, most commonly towards the end of the first year, but sometimes as much as six months before the panel

Merging the Survey and Administrative Data

We could not make a match between the SIPP and the HUD data when the SSN (Social Security Number) was missing or invalid in either data set or when a subsidized

[3] The SIPP is described in detail at http://www.sipp.census.gov/sipp/intro.html.

[4] Transactions also occur when a household leaves subsidized housing, moves from one subsidized unit to another, and for some other administrative reasons.

[5] Five transactions are listed as occurring after December 1996, which is because income certifications can be done up to three months in advance.

household was not listed in the HUD data. Match rates (the probability that a household listed in the MTCS/TRACS 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 MTCS/TRACS 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 (i.e. 85 percent of 75 to 80 percent).

There is some evidence of non-random availability of Social Security numbers (and hence non-random matching failures), but the magnitude is modest. Hispanics appear to be underrepresented in the matched sample, by about two percentage points, compared to their true percentage in subsidized housing (13 percent). There is little evidence of any other important problems with non-random matching, although there may be a small tendency for recipients of social welfare programs 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 who are not covered in the administrative data.[6] To address this, 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

[6] Sample members who did not merge because they did not report an SSN to the SIPP cannot contaminate the comparison group, because they are excluded from both the subsidized and comparison samples.

(i.e., 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. 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, where a development's construction (or conversion) is subsidized by the government, but which are managed by private entities while receiving a continuing stream of subsidies. 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, and there are other need-based restrictions as well (e.g., the homeless have priority in some circumstances). Importantly, these programs are not entitlements, but are generally rationed using some type of waiting list. Hence there is a large pool of eligible but unsubsidized households, that are potentially available to serve as comparison group members.

[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 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 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 TRACS data; or as "Mod Rehab" (meaning Section 8 Moderate Rehabilitation) in the MTCS data.

The analysis sample was restricted to those who meet 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).[8] In all, 670 subsidized households that met these criteria.

Statistical Matching to Create Comparison Groups

In order 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 that were 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 (e.g., public housing) and those not listed as receiving subsidies in either the survey or administrative data. Next, the predicted probability of receiving a subsidy (the propensity score) was calculated for each case.

[8] As discussed below, statistical matching based on other pairs of waves was examined as well.

Finally, three comparison group members -- the three cases with the most similar propensity scores -- were chosen as matches for each subsidized household.[9]

Table 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.[10] 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 table, including four measures of bank savings accounts; the square of age; and a several measures of income, earnings, and public assistance measured at the end of the first year (wave three).[11]

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. However, the point here is not to estimate the coefficients precisely, 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 non-receipt 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-R^2s 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.

[9] The statistical match was implemented using PSMATCH2 software for the Stata statistical package (Sianesi 2001, Leuven and Sianesi 2003). I modified this software to facilitate the creation of a matched file for analysis with procedures other than PSMATCH2.

[10] Family income is shown in the table but was not used in the logit, because many the many similar measures included in the logit were deemed sufficient.

[11] Additional matching variables used in the propensity score logit but not shown in this table are presence of the following types of bank savings: a savings account, an interest-bearing checking account, a money market account, and a certificate of deposit; several variables measured at wave 3: person income, transfer income, family employment, family earnings, poverty status, and receipt of food stamps; and the square of age in wave 1.

Success of Statistical Matching

For all the comparisons in Table 1, there is no statistically significant difference between the subsidy and comparison groups. In addition, the differences are usually small as well. These results strongly support the success of the statistical match, since the aim was to create a comparison group that is similar at the beginning of the panel. This lack of significant difference is not a mechanical function of the fact that these variables entered the matching function. A priori, it is possible that there are no good matches for the subsidized cases, and that even those cases that are the closest in propensity scores will still show significant differences. 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 non-disabled person with a low income, since only the propensity score matters. The table shows, however, that neither of these problems seems to have occurred: the matching procedure has successfully produced a comparison group with characteristics similar to the subsidized groups.

Because the matching is done with replacement, it was possible for a single comparison group member to be matched to multiple subsidy group members. Table 2 shows the number of matches per comparison group member. This is an important statistic 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). Over 80 percent of cases were used only once as matches, and less than

five percent were used three or more times. There were about 2.4 unique comparison cases per subsidy group member, compared to the 3.0 there would have been if no case had matched twice. Overall, this table suggests that there are many unsubsidized cases similar to the subsidized group available as matches.

Methodology and Potential Biases

The goal of this study is to follow two groups, 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 prior. Figures 1A-1C provide some empirical evidence regarding self-reported subsidy rates over the life of the panel for those listed as subsidized in the HUD data. In the first year of the panel, self-reported subsidy rates were about 80 percent for public housing residents. The rates fall below 100% 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, and 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 who had moved out of subsidized housing, remain in the subsidized group. One reason for this was to avoid the obvious sample-selection problems that would result if only the presently-subsidized were

[12] The rates were below 100% 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 yet have moved in. Second, there was some under-reporting of subsidy status in the SIPP. About 10% of public housing residents reported "not assisted" to the SIPP, and about 20% of those in the other two programs (Susin, 2004).

included in the subsidized group. That is, those who remained longer in subsidized housing may have been more likely to have pre-existing disadvantages than the average member of the subsidized or comparison group. 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 in subsidized housing, these connections presumably would not be re-built immediately upon exit.

Possible Biases

The major threat to the validity of the matching procedure 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, or have been caring for sick relatives, neither of which are controlled for here. After all, disadvantages were presumably the reason they chose to move into subsidized housing in the first place. A particular concern is 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 a disabled child (or some other long-term factor) with comparison households that were suffering unemployment due to a temporary layoff. 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. [13]

[13] This setting is similar in some ways to Ashenfelter and Card (1985), except that those authors

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 will be monthly earnings. 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 is likely to be superior to matching on 12 months of earnings for two reasons. First, using two separate measures allows a trend to be captured by the matching procedure. Second, temporary shocks to earnings, for example, will cause a discrepancy between the two waves of data, and may cause matches to be rejected when they should be. For example, a high earner who is unemployed in wave 1 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. This wouldn't happen with annual earnings: we would just see a year of low earnings, and not realize that this person's earnings rebounded in the later months of the year. Using waves one and three means that a spell of unemployment has to be at least six months long before it can affect both waves, and even then it will only do so if it begins in month four. Below, results with various combinations of waves are explored further.

Illustration of Matching on Temporary Dips

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considered the case where workers 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.

Figure 2A illustrates the potential problem caused by matching on temporary shocks. In this figure, the comparison group was matched using only wave 1 variables and no employment history data. In the figure, both the vouchers recipients and their comparison group have falling poverty rates over time, but the poverty rates of the comparison group fall faster. Much of the difference is due to a rapid fall in the poverty rates of the comparison group in month five, between the first and second waves.[14] 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 member with temporary problems, experiencing a brief dip in income. Figure 2B shows the same outcome, but includes a comparison group matched using waves one and three and the employment history variables, the procedure of the rest of this study. In this figure, there is less evidence of a jump in the beginning of the panel. The poverty rate of the comparison group falls faster than that of the public housing residents, but the fall is more gradual. This figure 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 figures like these led to the decision to match on multiple waves of data.[15]

Baseline Results

Levels

[14] Transitions in the SIPP occur more frequently between waves, a phenomenon known as "seam bias."
[15] Poverty rates for voucher recipients were chosen as the case to graph because of its 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 figures 3A-3C).

Figures 3A-8A, 3B-8B, and 3C-8C show selected outcomes over the life of the panel, 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 AFDC/TANF[16]) show strong positive trends. Family earnings almost doubled by the end of the panel for the public housing group, 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. Figures 8A-8C display an important demographic outcome, the number of adults per household, which rises fairly substantially over time.

During this time period, 1996-1999, the economy achieved a strong recovery, with the unemployment rate falling from 5.4 to 4.2 percent.[17] Single mothers posted large employment gains (see, e.g., Lerman 2003), and the welfare rolls fell sharply as states implemented welfare reform. Grogger et al (2002) found that average earnings of single-mothers rose by 35% in real terms, and employment rates rose from 69 to 83 percent, from 1993-1999. Hence the strong gains by subsidized housing residents are not too surprising; they were able to take advantage of the 1990s economic boom.

Differences: Income

[16] AFDC is the former Aid to Families with Dependent Children program; TANF is the current Temporary Assistance for Needy Families program.
[17] Figures from the Bureau of Labor Statistics: ftp://ftp.bls.gov/pub/special.requests/lf/aat1.txt .

Table 3 shows the wave 12 (final wave) results for the three subsidy groups and their comparison groups. The underlying data consist of monthly averages over the four 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 poverty rates 8 percentage points higher. For recipients of vouchers and project-based subsidies, the differences were smaller, and none were statistically significant, although the point estimates are all in the same direction as those for public housing.[18]

Differences: Employment and Earnings

The patterns for the employment and earnings variables were similar to that for income, pointing towards 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, though the point estimates of reductions in earnings were similar to those for the other two programs.

We should be wary of interpreting any these results as suggesting that outcomes in one program are better than in the other. As discussed further below, statistical tests rarely reject the hypothesis that the differences are equal across programs

[18] Standard errors in all tables are based on the usual formula, which assumes a simple random sample. The standard errors for Table 3 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. Typically, the differences were 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 for validity on a "large" sample, while the sample here is relatively small. In particular, since the number of subsidized observations is less than the number of Primary Sampling Units (PSUs: 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 are presented in the tables, rather than the replicate SEs.

Differences: Transfer Programs

Most of the differences between the subsidized and comparison groups are fairly small, and only two of nine are statistically significant and point towards greater dependency. Public housing is estimated to increase food stamps receipt by 16 percentage points, which is 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 of effects are fairly small and statistically insignificant. Overall, these inconsistent and often statistically insignificant results weigh against concluding that there is much affect of housing subsidies on 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. There are statistically significant and somewhat sizeable reductions of 0.13 to 0.18 adults for the three programs. 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 the table. In most cases, the tests could not reject the null hypothesis that the differences across programs were all equal.[19] Although public housing, for example, shows more statistically significant differences than do vouchers, as indicated by the asterisks in table 5, the F-tests show that caution is warranted before concluding that the effects of the two programs are different.

Census Tract Poverty Rates

One goal of this study is to investigate the effects of neighborhoods on the residents of subsidized housing, separately from the other possible effects discussed above. Table 4 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 8.8 percentage points higher on average than do the comparison group, a substantial difference. Voucher recipients actually live in tracts with lower poverty rates than the comparison group, by 2 .3 percentage points. Since recipients of housing subsidies tend to be quite disadvantaged, the major concern was finding a comparison group that was as badly off

[19] The only substantive exceptions were food stamps, where equality can be rejected at the 10 percent level, and transfer payments (5% level). We can also reject that the impacts on the self-reported housing subsidy variables are equal, which is hardly surprising.

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 type of neighborhoods that subsidized households live in. Thus, any differences we observe between the subsidized and comparison groups may be partly due 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 table 4 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 unsurprising, since for many residents of public housing, their neighbors are also residents of the same large projects. Assuming that there are no important determinants of neighborhood choice omitted from the matching equation, these differences can be interpreted as the effect of subsidized housing on the neighborhood choices of residents.

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 (1998). In addition, many analysts have pointed to the tendency of public housing projects to spatially concentrate the poor. 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.[20] This was to be achieved either by building smaller "scattered-site" subsidized developments in higher income neighborhoods, or by allowing voucher recipients to choose their neighborhoods. The results in Table 4 suggest that the project-based programs have succeeded in increasing concentrated poverty by less than the public housing program, while the voucher program has been able to reduce the concentration of the poor by a modest amount. The current voucher program may do more to spatially disperse the poor than these results suggest, because in 1996, rent vouchers could only be used in the jurisdiction where they were issued (generally a city or county), while today vouchers are "portable."

Explaining the Subsidy Effects

Table 5 shows results that combine matching with regression. These results allow 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 12 on all the matching variables, measured in waves one and three, and an indicator for residence in public housing during the first few waves of the panel. The regression was estimated in the matched sample of public

[20] 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).

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 where 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 non-economic 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.[21]

Estimates without additional controls

The regression-adjusted results are broadly similar to the simple comparison of means for the matched sample in Table 3. For family earnings, there is little change, 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 householder's 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 becoming statistically insignificant for this group. This table also adds a fourth pair of columns, for all three programs combined. The results for the pooled sample are always statistically significant at the one percent level.

[21] 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, and suggested combining matching and regression in order to reduce the bias.

Tract Poverty Rates

In general, tract poverty rates have only modest effects. The point estimates for public housing and project-based subsidies are of moderate size, but 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, 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 is a much larger change than that induced by the program.[22] Tract poverty rates generally have very little effect on either person 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, which is a small effect, but does explain about a 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 over the four years of the panel. Still, the results do suggest that the effect of tract poverty rates is not very long lasting.

Adults in Household

The number of adults in the household has a substantial effect on family earnings. This is not surprising, since more adults, if they are related and have any earnings, will mechanically increase total family earnings. More adults have no

[22] In results not shown in the table, 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.

statistically significant effect on person earnings. A priori, more adults could either provide childcare, facilitating work by the householder, or provide extra income, reducing the need for the householder to work. However the results find no clear effect either positive or negative.

Explaining the Subsidy/Comparison Gap

Overall, the addition of tract poverty rates and the number of adults 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 programs individually, or 56 percent in when the three programs are pooled. The additional variables have very little effect on the householder's 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. This makes 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 pre-panel employment, in order to avoid matching subsidized members with more permanent disadvantages to comparison group members that are only experiencing temporary difficulties. At the same time, matching over too long a time period reduces the period of follow-up available. The results in Table 6 examine this issue in more detail.

Table 6 presents regression-adjusted matching results for three key variables, as in table 6. The first row of each panel presents results where the subsidy recipients were matched using waves one and four of the data (months 1-4 and 13-16). The next row presents results for waves one and three (the same as those in the previous table). Each successive row matches on a shorter period, with the fifth row matching on only wave one with no retrospective employment data.

In almost all cases, the estimated earnings impacts are reduced as the time period of the match is extended from wave one only to include retrospective data, and then wave two. This suggests that using additional waves does indeed exclude potential comparison group members with only short term earnings dips. However, extending the period to a third and then a fourth wave seems to reduce the earnings impacts much less consistently.

Comparing matches through wave three to matches through wave four provides some justification for the decision to emphasize matching on waves one and three in this study. For public housing and project-based subsidies, the effect on three of the four earnings results is reduced, but generally by modest amounts. The exception is family earnings for those in public housing, where the impact is actually estimated to be larger when the match is based on wave four. For person earnings in public housing, and family earnings with project-based subsidies, the reduction in impacts is less than 10 percent. A decrease of ten percent could easily explained by the reduced length of follow-up period, for example, if impacts cumulate linearly. The impact on person earnings with project-based assistance does decrease by a relatively large amount with the wave four match, by about 40 percent compared to wave three. However, the

impact with a wave three match is actually bigger than with a wave two match. Overall, the difference between time periods of three and four waves seems to be mostly due to the reduction in follow-up period, and there is relatively little evidence of a reduction in impacts due to finding comparison group members with more permanent disadvantages.

For vouchers we see smaller impacts on earnings and household size when the period is lengthened to two waves, bigger with three waves, and smaller impacts with four waves, with few of the results statistically significant. Overall, this table strengthens the conclusion that any impacts on voucher recipients are too small to be distinguished from zero.

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

Further Alternative Specifications

Probably the most relevant policy change during the period examined here was welfare reform and the sharp drop in welfare caseloads. To examine the impact of welfare reform, the models were re-estimated with the addition of David Ellwood's (2000) measure of non-economic caseload drops (caseload drops presumably due to changes in the 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 difference between

the two groups was always small and statistically significant. Adding the Ellwood variable to the matching function led to no qualitative change in the comparison of means in Table 3 (although it did have a modest effect on some individual estimates). When the Ellwood measure was added to the regressions in Table 5, it was never statistically significant and had little effect on the subsidy coefficients. The caseload change measure did have the expected effect on welfare participation in wave 12, indicating that the variable does provide a measure of welfare policies. Overall, this robustness check provides some reassurance that this important policy shifts are not driving the estimates of the subsidy effect.

The results presented thus far are unweighted, and there is little reason to do so. For example, matching and regression methods are an alternative. The strongest argument for weighting with the Census Bureau's sampling weights is that the SIPP oversamples high poverty areas, which may affect the estimates of the levels (though not the treatment effects). When Table 3 was reestimated using weights (specifically, applying subsidized group's weights to both the subsidized households and their comparison households), there was very little change in the results (both the differences and the levels).

Comparison to Experimental Research

A recent study by Abt Associates (Patterson et al 2003) is of great interest because it is the only experimental study of the effect of vouchers on employment, 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.[23] Compared to the SIPP sample used here, Abt Associates sample was somewhat more disadvantaged, but not extremely so.[24]

Patterson et al found a reduction in earnings of 13.9 percent over seven quarters, and an increase in the amount of welfare payments and food stamps received of 12.4 percent, and a decrease in tract poverty rates of 0.9 percentage points. However, several differences in study design mean that these figures are not strictly comparable to those presented here. In the Abt Associates data, before receiving a voucher, a sample member could have been living in their own household (57 percent), with friends or relatives (26 percent), in public or project-based subsidized housing (13 percent) or in a homeless shelter (2 percent).[25] In contrast, the SIPP sample used here restricts the subsidized and treatment samples to those living in their own household (called householders by the Census Bureau).[26]

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[23] The original intention was to provide enhanced job-search and employment services to the recipients, and subject them to enhanced sanctions. However, these program elements were not in place at the time of the study, and Patterson et al argue that the program was essentially identical to the usual rent voucher system. The cities were Atlanta, GA; Augusta, GA; Fresno, CA; Houston, TX; Los Angeles, CA; and Spokane Washington. Sample size was not proportional to city size. Los Angles had limited follow-up data and is not included in the figures cited here.

[24] The Abt Associates sample was 3-5 years younger on average, twice as likely to have never worked (19 percent), and much more likely to be receiving TANF benefits (over 50 percentage points more) or food stamps (roughly 25 percentage points more) but probably less likely to be disabled (11 percent received SSI).

[25] 1.5 percent were listed as "other," and 1 percent did not report any data.

[26] Another technical difference between this research and the Abt study is in the composition of the comparison group. This study compares those with vouchers to an unsubsidized group receiving no housing subsidies. Patterson et al estimate the difference between those with a voucher, and a comparison group without vouchers but that could have been living in public or project-based housing. By the end of follow-up, 21 percent of the Abt control group had lived in public or project-based housing at some point. Even the figures for those living in private housing before receiving a voucher include some control group members who were living in public or project-based housing, because 8 percent of the Abt sample moved into such housing after baseline. If we assume that vouchers have the same effect on earnings and welfare receipt as the other types of subsidized housing, the results in the Abt study will be about 8 percent smaller than the impacts on the unsubsidized population (i.e., the Abt impacts should be inflated by dividing by 0.92).

Restricting the Abt sample to those living in their own household before receiving a voucher (and excluding those in public or project-based housing) reduces the impacts to an earnings reduction of 11.0 percent, an increase in transfer payments of 10.0 percent, and switches the sign to an increase in tract poverty rates of 0.5 percentage points. Only the earnings impact remains statistically significant. By way of comparison, this study estimated a reduction in person earnings of 14.2 percent (an impact of -$141 from table 6 divided by a control mean of $995 in table 5), although smaller and statistically insignificant earnings reductions were found with slightly different matching periods. For the other two programs, we found reductions in person earnings of 17 percent (public housing) and 14 percent (project-based). These estimates are not significantly different from one another 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, then, is that the earnings reduction and, to a lesser extent, the fall in tract poverty rates, is of approximately the same magnitude.

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 residents of subsidized housing, 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 under-

represents Hispanics, and misses about a third of the truly subsidized, requiring the use of survey self-reports to screen out subsidized cases from the comparison group.

For almost every outcome, subsidized households shared in the gains of the economic boom of the 1990s, showing sharp increases in income and earnings, 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.

The statistical matching procedure also worked quite well, at least insofar as it successfully balanced the characteristics of the subsidized and comparison groups. 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 four years of the SIPP panel than did unsubsidized households that were similar at the beginning of the panel. Family earnings grew by 19 percent less than the comparison group for those in public housing, and person earnings by 17 percent less. For those with project-based subsidies, both earnings measures grew 13 percent less than the comparison group. The impacts on voucher recipients were less negative, and were never statistically significant. However, caution is warranted before concluding that the Voucher program is "better," because we are unable to statistically reject the hypothesis that all three programs have the same impact on these earnings outcomes.

In contrast to the reductions in on 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. These programs move recipients into neighborhoods with poverty rates 8.8 and 2.6 percentage points higher, respectively. Voucher recipients, by contrast, lived in neighborhoods with poverty rates 2.3 percentage points lower than unsubsidized households with similar individual characteristics. 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. Tract poverty and household size accounted for little of the reduction of person earnings, leaving the labor supply disincentives of subsidized housing as a prominent candidate for explaining the earnings effects of these programs

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Table 1: Means and Differences (Monthly Averages) for Wave 1[a]

	Public Housing	Comp-arison Group	Difference	Vouchers	Comp-arison Group	Difference	Project Based	Comp-arison Group	Difference
Family Income	919	907	12	1,100	1,093	7	945	900	45
Person 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
Person Earnings	384	345	39	444	401	42	469	451	18
Employed wave 1, 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 1, 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 1 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		0.283	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
Person 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 Female	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
Black	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	

Source: See table 2.

Note: *** = Statistically Significant at the 1% level; ** = 5% level; * = 10% 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.

Table 2: Number of Times Each Comparison Group Member was Matched

# of Matches	Public Housing	Vouchers	Project-Based
1	366	497	468
2	68	90	71
3	11	14	16
4	8	3	4
5	0	2	2
6	2	1	0
Total Comparison	455	607	561
Total Subsidized	193	249	228
Ratio	2.36	2.44	2.46

Note: Sample restricted to householders less than 55 years of age.
Source: Merged Census Bureau survey data and Department of Housing and Urban Development (HUD) administrative data from the 1996 Survey of Income and Program Participation (SIPP), the Multi-Family Tenant Characteristics System (MTCS), and the Tenant Rental Assistance Characteristics System (TRACS).

[psh100.xls]

Table 3: Means and Differences (Monthly Averages) for Wave 12[a]

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

Source: See table 2.

Note: *** = Statistically Significant at the 1% level; ** = 5% level; * = 10% 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.

[psh102.xls]

Table 4: Means and Differences of Census Tract Poverty Rates (1990) for Wave 1[a]

	Subsidize Group	Comp-arison 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 **

Source: See table 2.
Note: *** = Statistically Significant at the 1% level; ** = 5% level; * = 10% level. Sample restricted to householders less than 55 years of age.
[a] Interviews conducted April 1996 to July 1996.

Table 5: Regression-adjusted Matching Estimates:Effects of Poverty and Household Size on Wave 12[a] Outcomes

	Public Housing		Vouchers		Project-Based		Any Subsidy	
Family Earnings								
Public Housing	-240 **	-70	-158	-44	-195 *	-49	-213 ***	-94
	(116)	(112)	(133)	(123)	(106)	(95)	(75)	(67)
Wave 1 Tract Poverty Rate (%)		-6.08		1.67		-10.02 ***		-4.08
		(4.56)		(4.49)		(3.78)		(2.71)
Two adults in household, wave 12		806 ***		938 ***		946 ***		880 ***
		(161)		(141)		(151)		(99)
Three or more adults in household, wave 12		1857 ***		2132 ***		2003 ***		1991 ***
		(348)		(283)		(318)		(225)
R-squared	0.30	0.41	0.29	0.43	0.37	0.48	0.29	0.41
Person Earnings								
Public Housing	-129 *	-109	-141 *	-128	-131 *	-126 *	-144 ***	-138 ***
	(75)	(83)	(83)	(86)	(72)	(74)	(48)	(49)
Wave 1 Tract Poverty Rate (%)		-1.40		1.89		-3.76		-0.65
		(3.29)		(3.61)		(2.61)		(1.96)
Two adults in household, wave 12		26		-107		-48		-47
		(85)		(96)		(71)		(55)
Three or more adults in household, wave 12		178		113		176		158
		(253)		(166)		(214)		(146)
R-squared	0.34	0.34	0.37	0.37	0.48	0.47	0.37	0.37
Adults in Household								
Public Housing	-0.119 *	-0.064	-0.090	-0.087	-0.153 **	-0.148 **	-0.114 ***	-0.101 **
	(0.070)	(0.072)	(0.065)	(0.067)	(0.062)	(0.063)	(0.041)	(0.041)
Wave 1 Tract Poverty Rate (%)		-0.004 *		-0.002		0.001		-0.002
		(0.002)		(0.002)		(0.002)		(0.002)
R-squared	0.40	0.41	0.29	0.31	0.30	0.30	0.30	0.31
N	453	447	627	606	552	538	1,393	1,359

Source: See table 2.

Note: *** = Statistically Significant at the 1% level; ** = 5% level; * = 10% level. Sample restricted to householders less than 55 years of age.

Other explanatory variables in the regression are the same wave 1 and 3 variables as are in the matching function. See text and table 1.

Table entries are OLS regression coefficients, with heteroskedasticy-consistent standard errors in parenthesis. [psh103.xls]

[a] Interviews conducted December 1999 to March 2000.

Table 6: Regressions-Adjusted Matching Estimates for Selected Outcomes, Using Matching Periods of Different Lengths

	Public Housing		Vouchers		Project-Based		Any Subsidy	
Family Earnings								
Wave 1 & 4 match	-293	***	30		-177	*	-130	*
	(109)		(127)		(99)		(70)	
Wave 1 & 3 match	-240	**	-158		-195	*	-213	***
	(116)		(133)		(106)		(75)	
Wave 1 & 2 match	-435	***	-126		-205	*	-264	***
	(117)		(138)		(106)		(75)	
Wave 1 only match	-427	***	-270	*	-417	***	-374	***
	(140)		(160)		(122)		(87)	
Wave 1 only match, with no retrospective employment	-549	***	-274	**	-423	***	-407	***
	(152)		(129)		(116)		(81)	
Person Earnings								
Wave 1 & 4 match	-119		-66		-80		-87	*
	(76)		(79)		(72)		(46)	
Wave 1 & 3 match	-129	*	-141	*	-131	*	-144	***
	(75)		(83)		(72)		(48)	
Wave 1 & 2 match	-223	***	-58		-106		-134	***
	(79)		(82)		(68)		(47)	
Wave 1 only match	-223	***	-185		-139	*	-186	***
	(78)		(128)		(72)		(57)	
Wave 1 only match, with no retrospective employment	-316	***	-148	*	-186	**	-209	***
	(114)		(80)		(75)		(53)	
Adults in Household								
Wave 1 & 4 match	-0.096		-0.021		-0.107	*	-0.069	*
	(0.068)		(0.062)		(0.064)		(0.040)	
Wave 1 & 3 match	-0.119	*	-0.090		-0.153	**	-0.114	***
	(0.070)		(0.065)		(0.062)		(0.041)	
Wave 1 & 2 match	-0.204	***	-0.049		-0.092		-0.107	***
	(0.071)		(0.056)		(0.063)		(0.040)	
Wave 1 only match	-0.128	*	-0.116	*	-0.195	***	-0.144	***
	(0.071)		(0.061)		(0.067)		(0.043)	
Wave 1 only match, with no retrospective employment	-0.139	*	-0.123	**	-0.128	**	-0.132	***
	(0.075)		(0.059)		(0.063)		(0.040)	

Source: See table 2.

Note: *** = Statistically Significant at the 1% level; ** = 5% level; * = 10% level.

Sample restricted to householders less than 55 years of age. Other explanatory variables in the regression are the same wave 1 and 3 variables as are in the matching function. See text and table 1. Table entries are OLS regression coefficients, with heteroskedasticy-consistent standard errors in parenthesis.

[a] Interviews conducted December 1999 to March 2000.

[psh114comp.xls]

Figure 1. Proportion with Self-Reported Housing Subsidy: Subsidized vs. Comparison Group

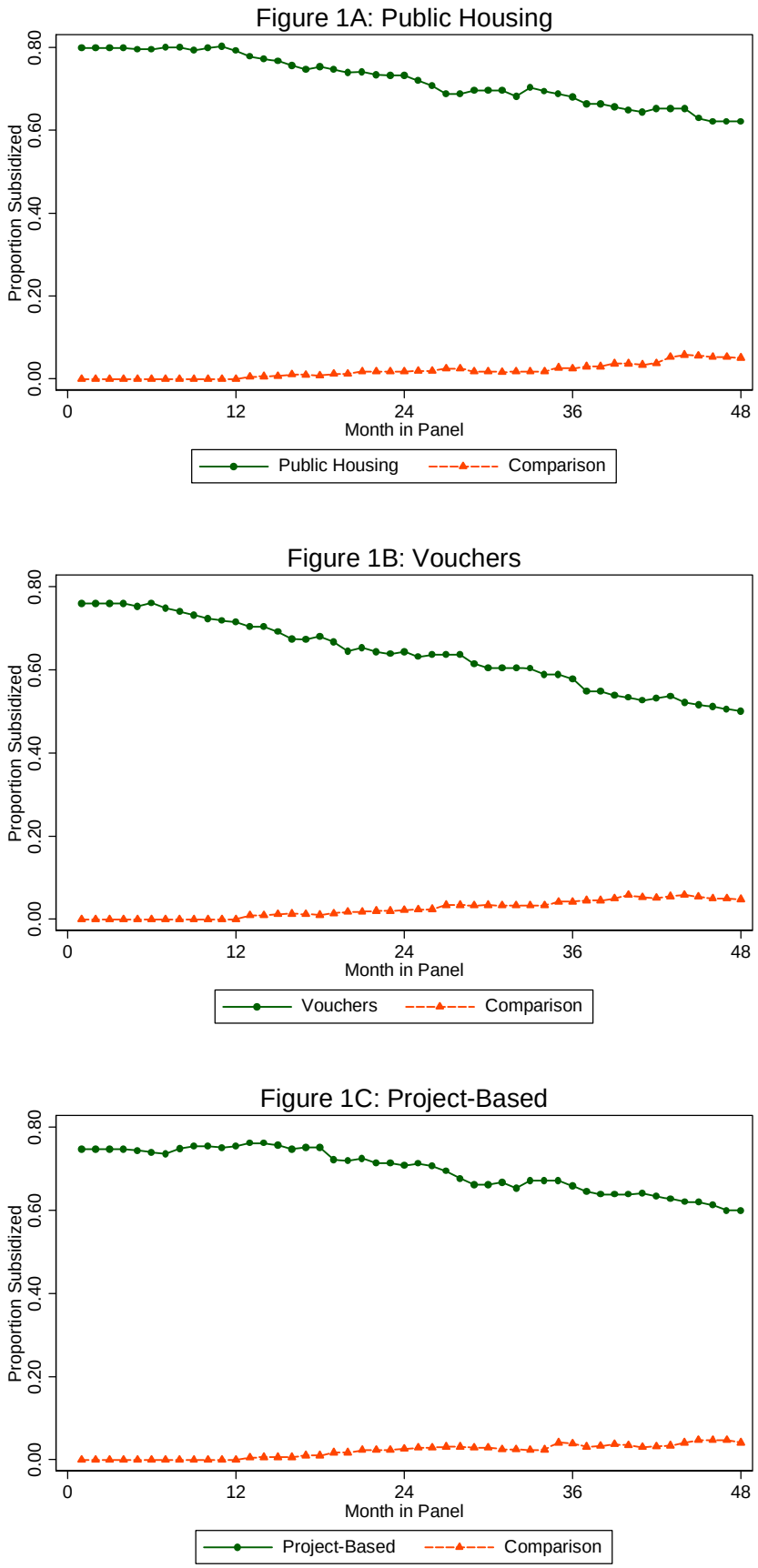
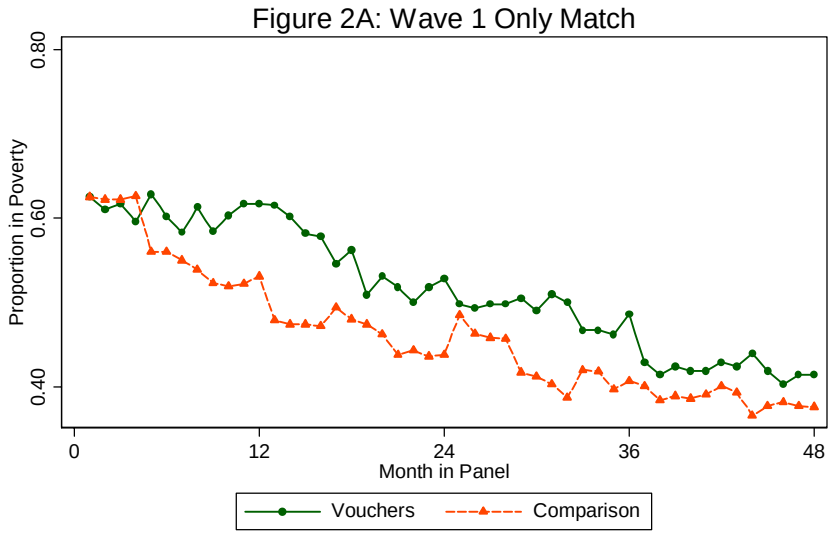


Figure 2. Illustration of Matching on Temporary Dips:
Wave 1 Match with No Employment History Variables
Compared with Matching on Waves 1, 3, and Employment History.



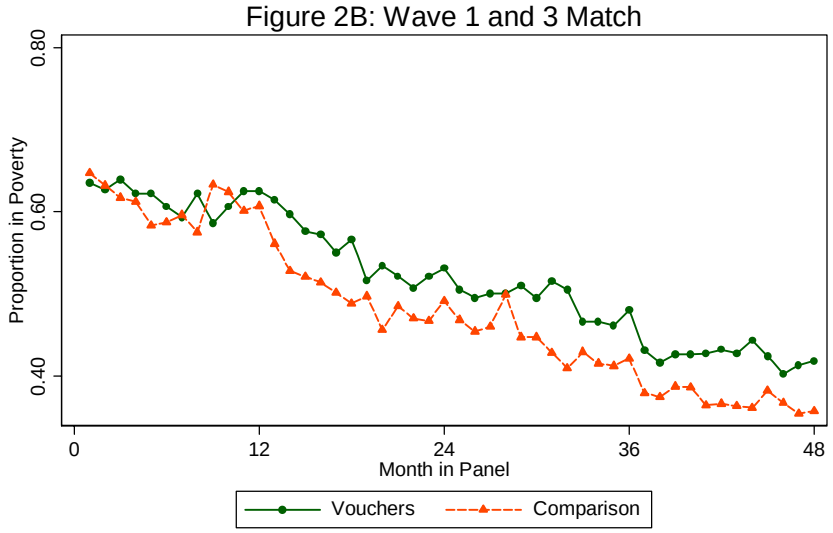
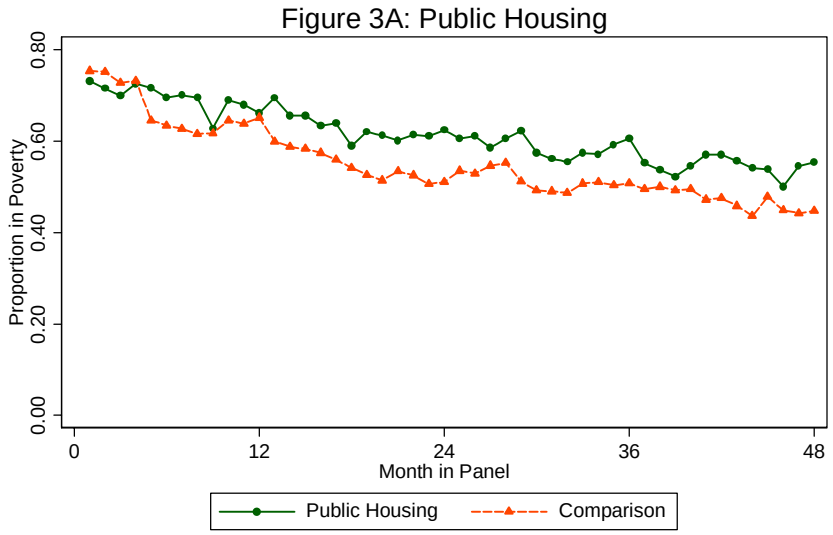
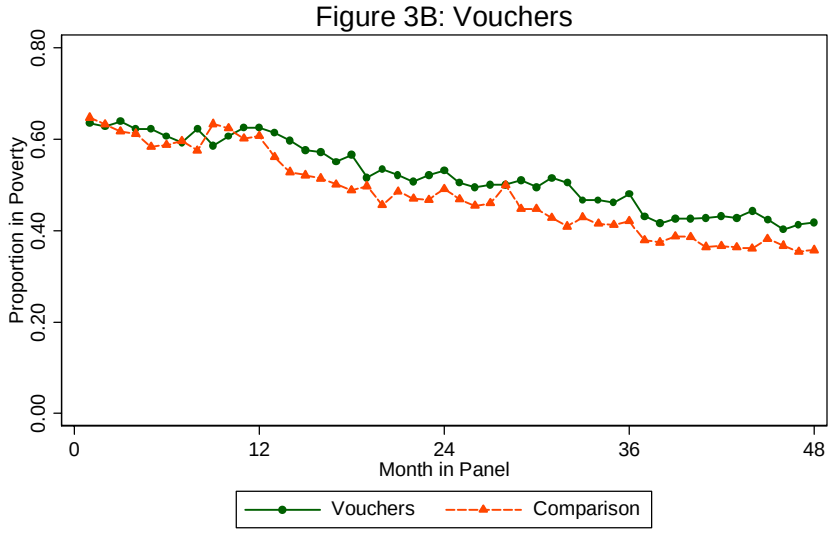


Figure 3. Proportion in Poverty: Subsidized vs. Comparison Group





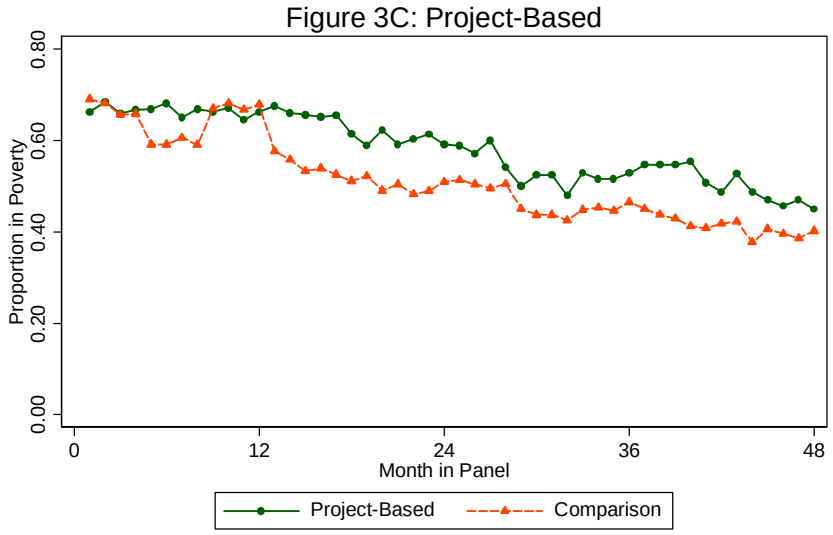
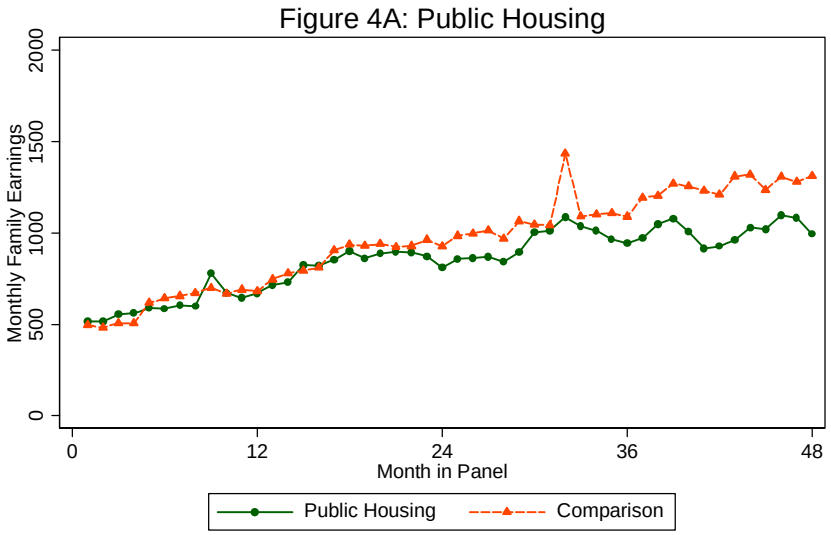
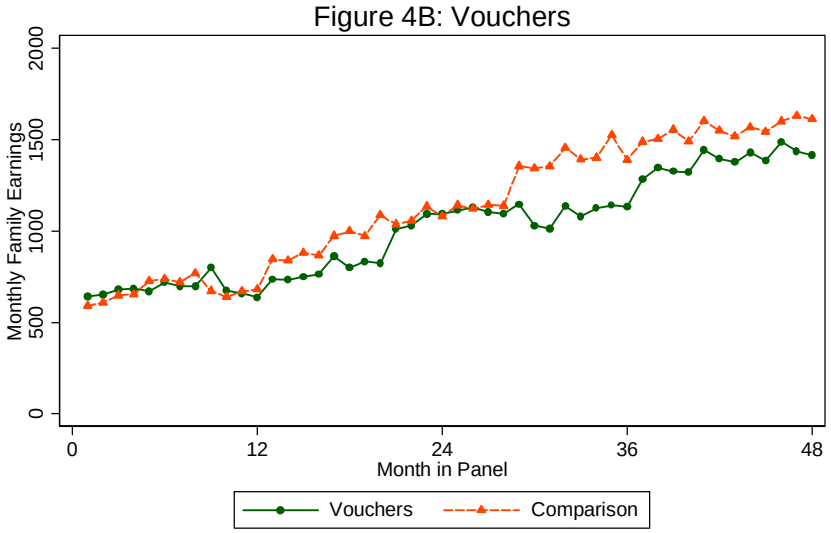


Figure 4. Monthly Family Earnings: Subsidized vs. Comparison Group





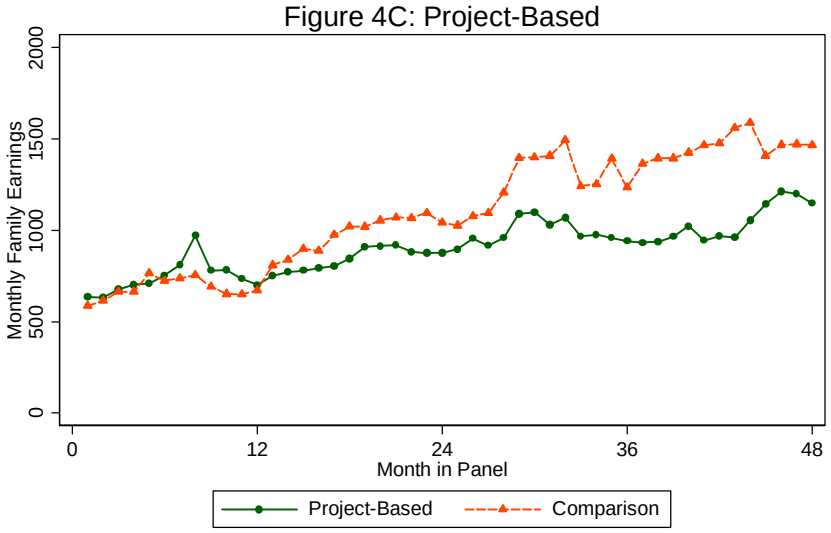
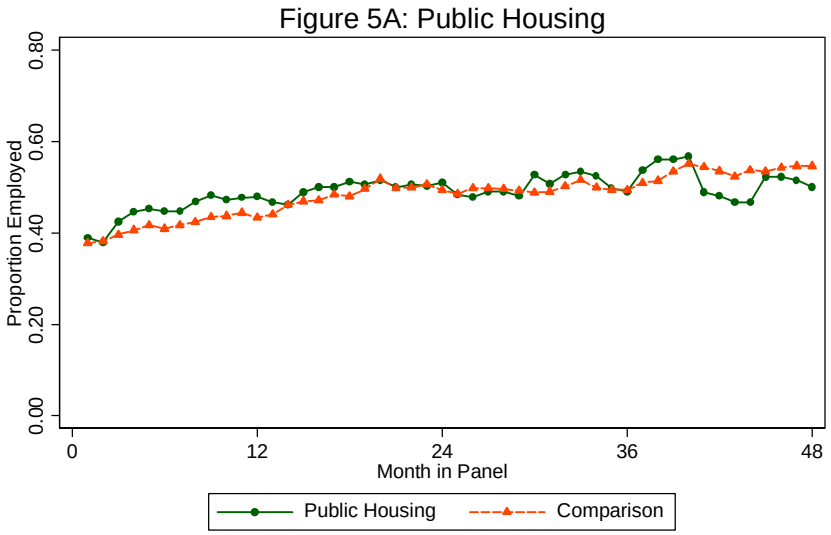
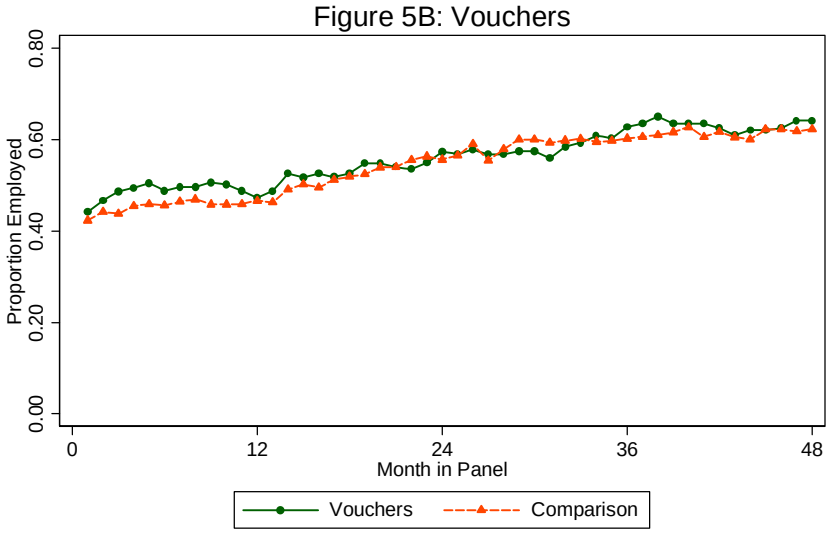


Figure 5. Proportion Employed: Subsidized vs. Comparison Group





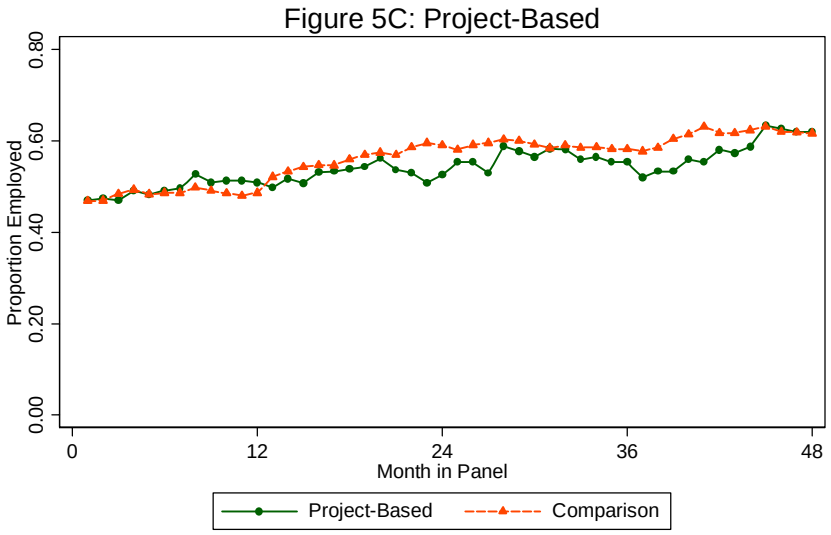
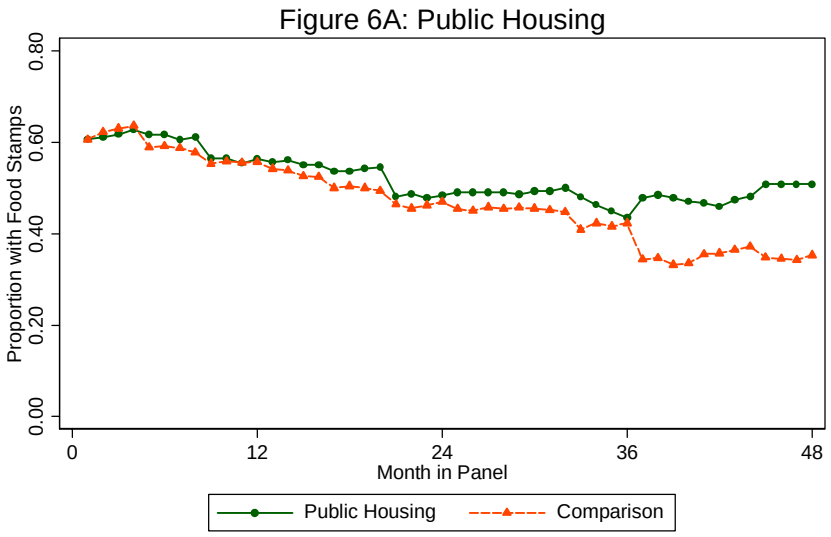
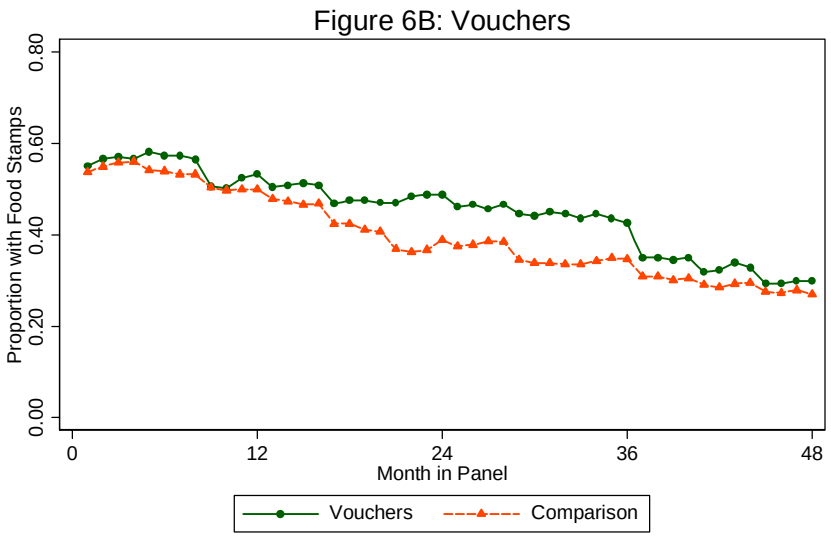


Figure 6. Proportion with Food Stamps: Subsidized vs. Comparison Group





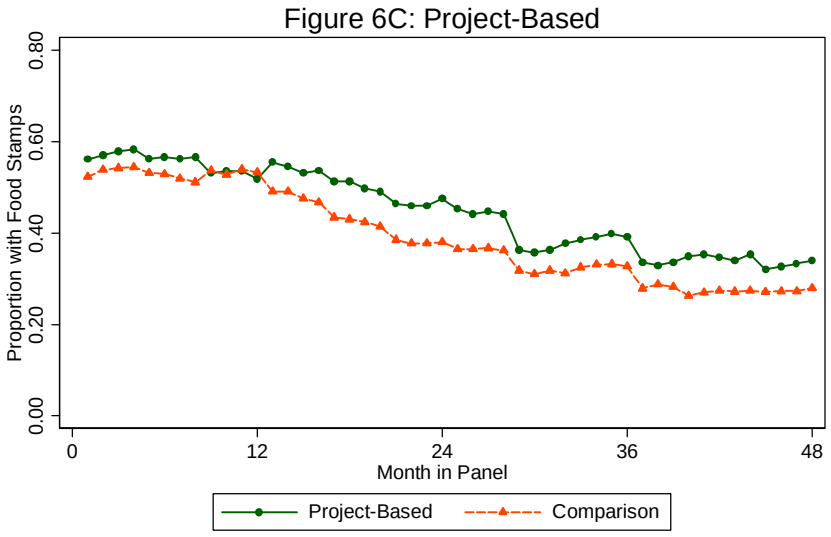
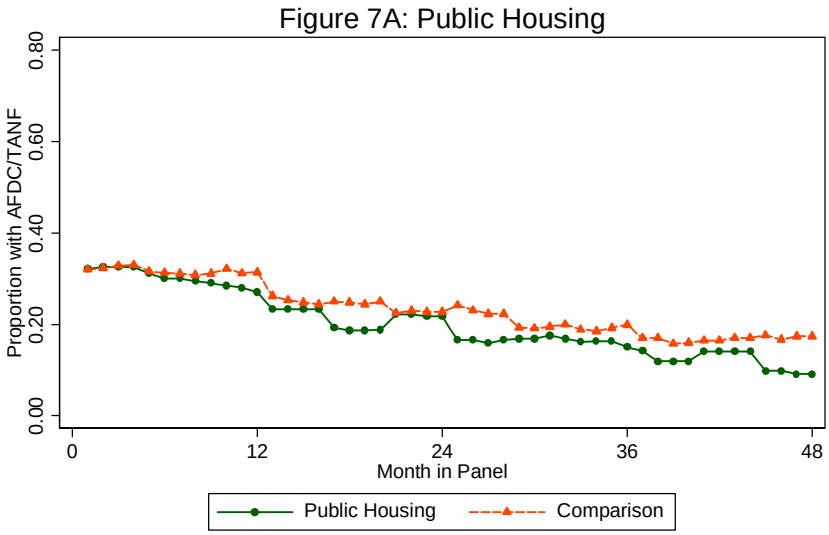
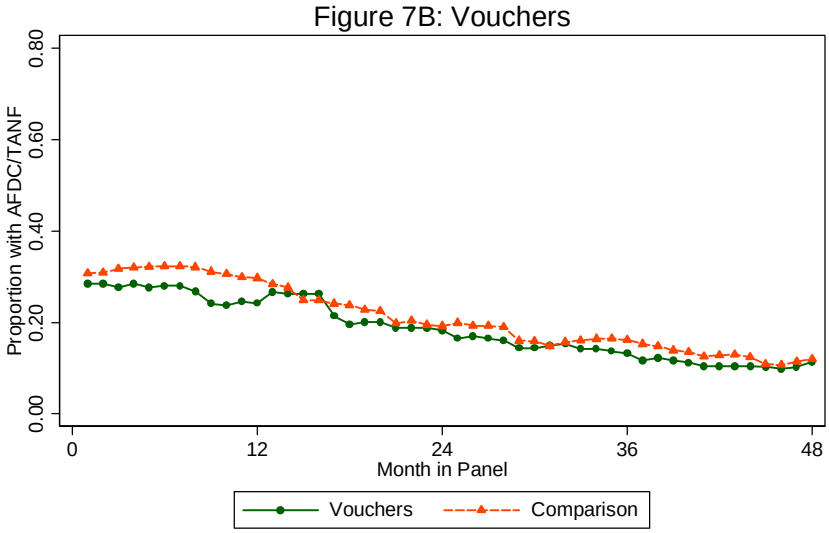


Figure7. Proportion with AFDC/TANF: Subsidized vs. Comparison Group





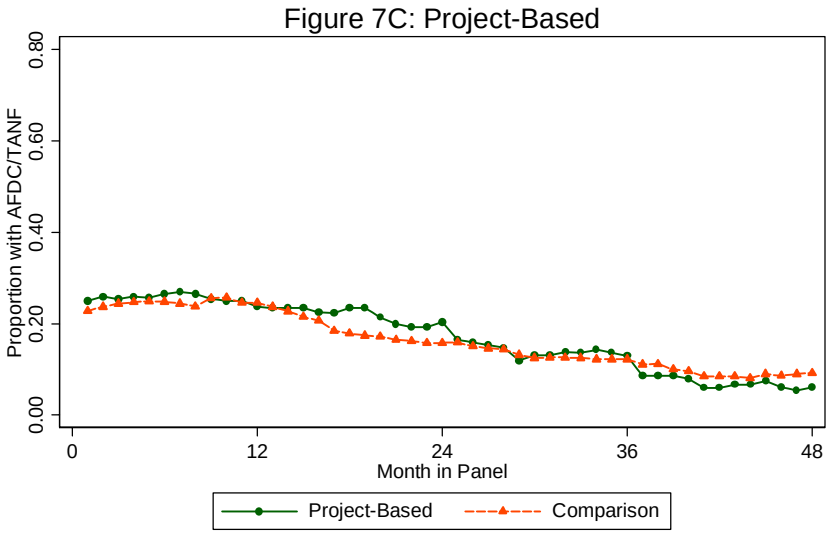
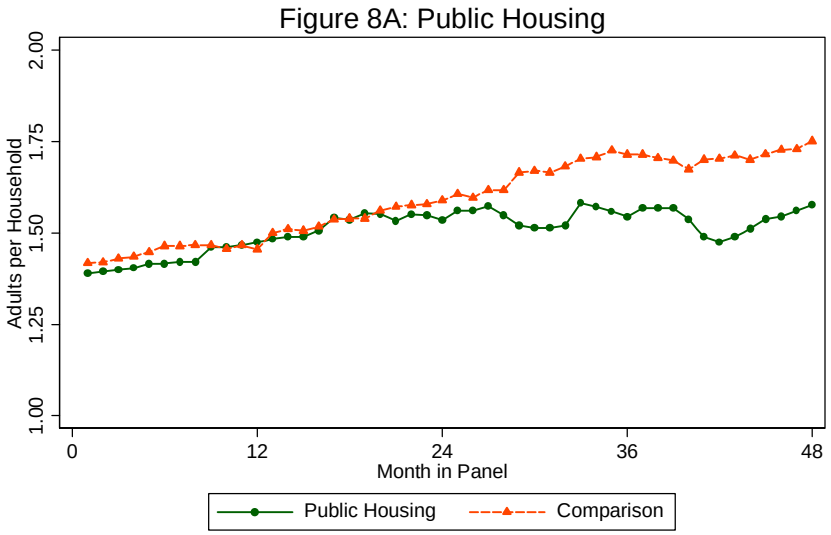
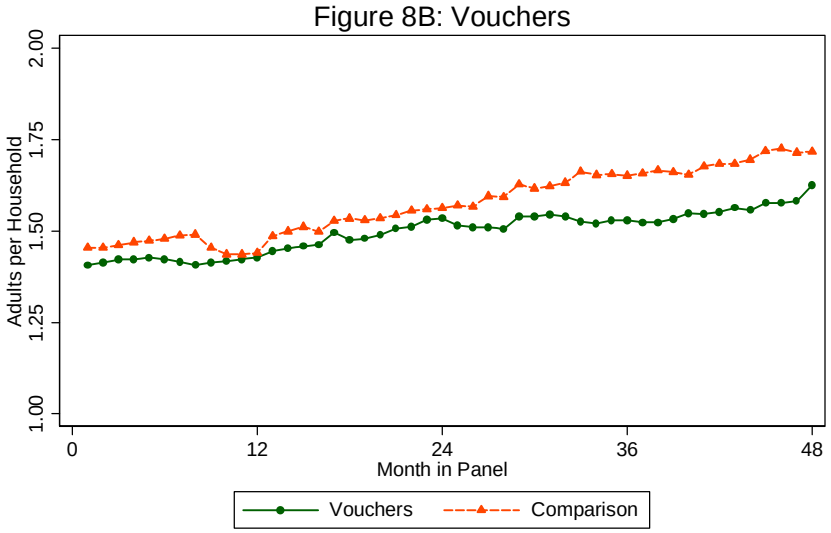
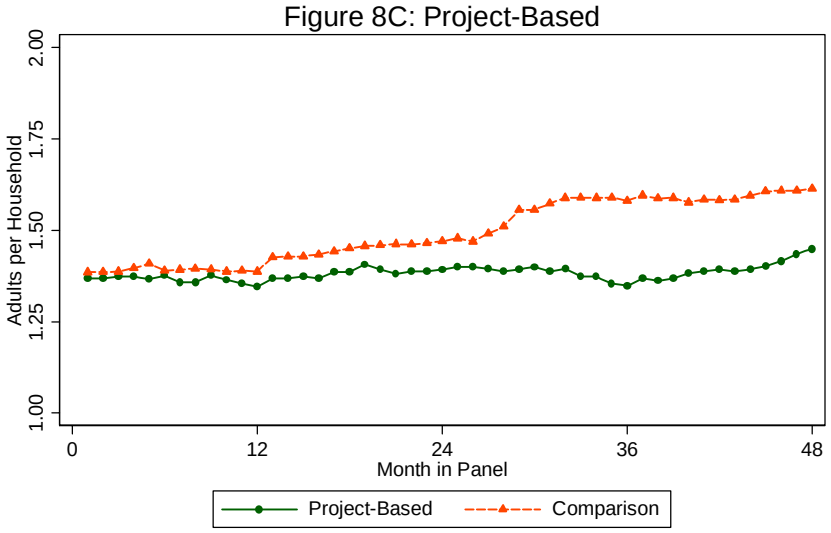



Figure 8. Adults per Household: Subsidized vs. Comparison Group