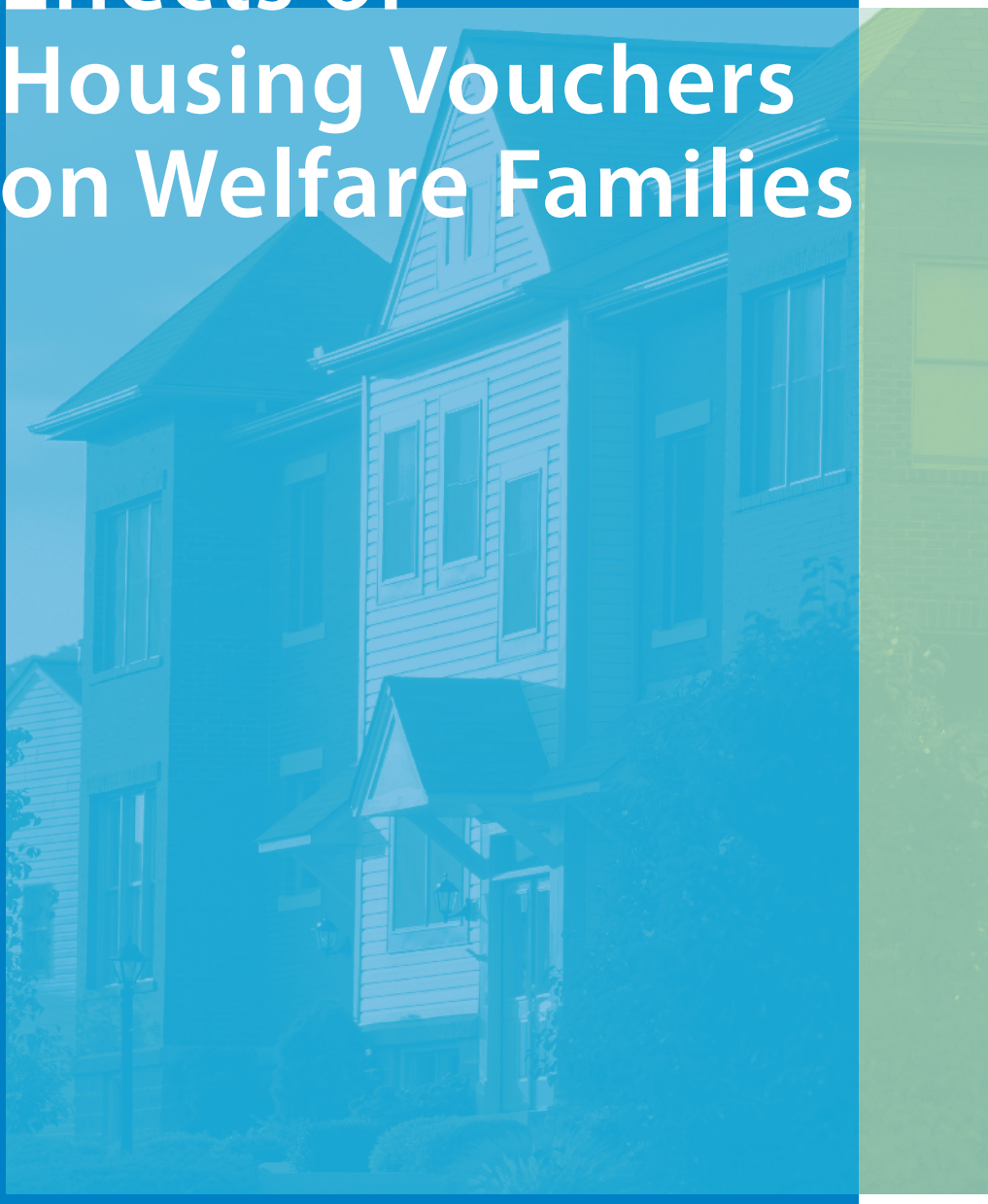
Effects of Housing Vouchers on Welfare Families



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EFFECTS OF HOUSING VOUCHERS ON WELFARE FAMILIES

Prepared for:
U.S. Department of Housing and Urban Development
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This report was a collaborative effort by the staff of Abt Associates and our small business partners, Amy Jones and Associates, Cloudburst Consulting, and The QED Group LLC. The principal authors of each chapter were as follows:

- Chapter One: Michelle Wood;
- Chapter Two: Michelle Wood;
- Chapter Three: Gregory Mills;
- Chapter Four: Daniel Gubits and Larry Orr;
- Chapter Five: David Long;
- Chapter Six: Judith Feins and Gregory Mills;
- Chapter Seven (and Executive Summary): Gregory Mills and Michelle Wood.

In addition to the lead authors listed above, Amy Jones and Janet Pershing of Amy Jones and Associates, who played a key role in conducting and analyzing the in-depth interviews contributed to the preparation of Chapters 3, 4, 5, and 6 by incorporating findings from the in-depth interviews.

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Foreword

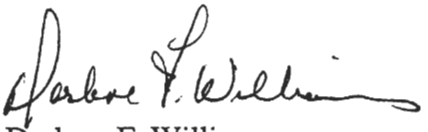
In the HUD Appropriations Act for Fiscal Year 1999, the Congress enacted the Welfare to Work Voucher Program. This demonstration program funded approximately 50,000 housing vouchers for families receiving or eligible to receive welfare. In the Act, the Congress specified that HUD would conduct an evaluation of the effect of providing this assistance. In 2004, HUD published *Evaluation of the Welfare to Work Voucher Program* to fulfill the Congressional mandate.

In setting up that evaluation, HUD contracted with Abt Associates to implement a rigorous research design in which HUD engaged seven public housing agencies, who randomly assigned 8,731 families into two groups. A treatment group received a housing voucher, and a control group did not. After the report to Congress was completed, HUD continued to track the families so that we could assess effects over a longer period of time and consider issues beyond the scope of the previous study.

This study, *Effects of Housing Choice Vouchers on Welfare Families*, examines the longer term impact on families some three and a half years after they received a housing voucher. It considers effects on such dimensions as employment, earnings, welfare receipt, adult education, housing quality, neighborhood conditions, child education, juvenile delinquency, and nutrition.

This report shows that for welfare families, vouchers are an effective housing program but not an effective anti-poverty program. The report finds that access to vouchers essentially eliminated homelessness, greatly reduced crowding and doubling up, and somewhat improved the neighborhoods in which extremely low-income families lived. Over the three and a half year study period, however, the voucher had no impact on employment, earnings, adult educational attainment, food security, marriage, or cohabitation.

We expect that these findings, grounded in the highest standards of American social science, will be of great interest to policy makers and the policy community, and it is my privilege to present them to the public.



Darlene F. Williams
Assistant Secretary for
Policy Development and Research

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Executive Summary

This report presents the final analysis of a study conducted over several years to measure the impacts of Housing Choice Vouchers on the housing mobility of low-income families, the characteristics of their neighborhoods, the composition of their households, their employment, earnings, participation in education and training, their receipt of public assistance, their poverty and material hardship, and the well-being of their children. The analysis, based on a six-site research sample of 8,731 families, uses an experimental design and makes use of outcome measures derived from tract-level Census data, person-level administrative data, and a follow-up survey. The impact estimates in this report encompass a follow-up period that is sixteen quarters in duration for all sites, and longer for some sites. Augmenting the experimental findings are insights from intensive interviews with a sample of 141 families.

This research was undertaken to evaluate the Welfare to Work Voucher (WtWV) program, initiated in Fiscal Year 1999 when Congress appropriated $283 million for tenant-based rental assistance to help families to make the transition from welfare to work. This appropriation funded 50,000 new rental assistance vouchers (P.L. 105-276). The U.S. Department of Housing and Urban Development (HUD) awarded these vouchers to local and state housing agencies (HAs) that presented reasonable plans for matching up eligible families with the available housing assistance and for coordinating these efforts with existing welfare reform and welfare-to-work efforts.

HUD renewed these vouchers annually to continue the demonstration program until FY2004 when the program was phased out. Participating housing agencies were notified by HUD in March 2004 that issuance of WtW vouchers that were not under lease at that time and those that became eligible through turnover after that time would no longer be subject to HUD's WtW voucher program requirements. After March 2004, WtW vouchers that became available through turnover were incorporated into an HA's regular Housing Choice Voucher (HCV) program.

When the WtWV program was phased out in 2004, the evaluation was renamed. The new name, Effects of Housing Choice Vouchers on Welfare Families, is used throughout the remainder of the report when referring to the evaluation. When referring to the demonstration program in operation between 2000 and 2004, we use the original program name, the WtWV program.

At the time the WtWV program was authorized, Congress mandated a comprehensive evaluation of the program to assess the results of rental assistance in promoting the self-sufficiency of the assisted families. HUD's Office of Policy Development and Research, as manager of the evaluation, designed the study as a social experiment, with random assignment of families to receive demonstration vouchers or to a control group that received no housing assistance from the demonstration. In implementing a rigorous evaluation of the effects of the WtWV program, HUD took a large step toward expanding what is known about the effects of tenant-based rental assistance on low-income families. The study offers policymakers and Congress evidence on the extent to which tenant-based rental assistance affects housing quality and location, mobility, housing crowding and homelessness,

employment and earnings, and other aspects of family well-being such as health, child well-being, and material hardship.

In 1999, HUD contracted with Abt Associates to design and implement the study and, through subsequent task order contracts, to collect and analyze data on the experiences of program participants. This report is the final report for the study. It presents the results of the assessment of net impacts of the WtWV program over the five-year follow-up period using data from a follow-up survey undertaken for a subset of the research sample as well as administrative records data and tract-level Census data collected for the entire study sample.

This research offers powerful new evidence concerning the effects of tenant-based rental assistance on self-sufficiency. The experimental design enables one to draw rigorous inferences about the effects of housing vouchers on family well-being, independent of all other factors affecting the lives of program participants. Random assignment serves to assure that the treatment and control groups are well matched on both observed and unobserved characteristics at the time of their entry into the study. It thus establishes the strongest possible foundation for understanding whether housing vouchers can assist welfare families in achieving greater financial independence or otherwise improving their lives.

Evaluation Sites and Program Implementation

This evaluation was conducted in six sites that were selected in early 2000. During 2000 and 2001 a total research sample of 8,731 families was randomly assigned. The sites (and their sample sizes, including both treatment and control groups) are as follows:

- Atlanta, Georgia (1,134)
- Augusta, Georgia (759)
- Fresno, California (2,621)
- Houston, Texas (2,021)
- Los Angeles, California (1,047)
- Spokane, Washington (1,149)

Random assignment began in April 2000 (in Fresno and Houston, the first-enrolled sites) and ended in May 2001 (in Los Angeles, the last-enrolled site).

The implementation of the WtWV program was monitored in each site to clearly establish the nature of the program intervention. Specific attention was given to whether (in addition to the voucher itself) the sites provided any services to treatment group members that were not also received by control group members. In most sites, any employment-related services offered in conjunction with the voucher were modest and similar to those available to the control group through the TANF agency. In Fresno, where specialized case management and employment services were developed for WtWV recipients, such services were not likely to have been provided to treatment group members during the first 18 months following random assignment. In Augusta, a case manager was added to the housing agency's staff in 2002 to provide specialized services to WtW voucher participants, but this occurred nearly two years after enrollment of the research sample, making it unlikely that they received these services.

We have concluded, therefore, that in assessing the effects of the program, the intervention being tested was the voucher itself. This conclusion was one of the reasons for changing the name of the evaluation when the WtWV program was phased out in 2004.

Data Sources

The following sources of data have been used to measure the impacts of vouchers on welfare families:

- ***Baseline survey***—To obtain basic descriptive, identifying, and locating information on the research sample upon entry into the demonstration, we administered a baseline survey to all sample members immediately prior to random assignment, covering employment status, satisfaction with the housing unit and neighborhood, receipt of public assistance, household composition, and information on contact persons.
- ***Follow-up survey—***Approximately 4½ to 5 years after random assignment, we conducted interviews with a subset of the research sample to collect information on outcomes that is not available from administrative data sources. The follow-up survey instrument collected information about housing assistance and services, housing mobility and neighborhood environment, adult employment, education and training, household income, public assistance, food security[1], and family and child well-being. The survey instrument consisted of a Core Module and a Parent-on-Child/Youth module. The Core Module was administered to the adult in each household who applied to the experimental housing voucher program. The Parent-on-Child/Youth module was also administered to the adult respondent for up to two children who were present in the household and age 15 or younger at the time of random assignment and who thus had reached the target age range of 4 to 19 years at the time of the survey. Follow-up survey data were collected for a total of 2,481 sample members.
- ***Unemployment insurance wage records***—To measure the effects of receiving a voucher on the employment and earnings of participants, we collected quarterly employer-reported earnings records from the employment security agencies of the four states participating in the evaluation for the period January-March 1999 (i.e., at least one year prior to random assignment) through December 2004.
- ***TANF data files***—To measure the effects of vouchers on public assistance, we collected information from state or local welfare agencies on the receipt of Temporary Assistance for Needy Families (TANF) and Food Stamp benefits, for a time period beginning at least one year prior to random assignment and extending through December 2004.
- ***PIC data files***—To monitor the receipt of housing assistance through the Housing Choice Voucher and public housing programs by sample members, data from HUD's

[1] As described in Chapter 5, the assessment of food insecurity is based on two Department of Agriculture's "short form" metrics, which are scores assigned to household based on answers to six survey questions. These questions were provided to us by the USDA and included in the follow-up survey.

Public Housing Information Center (PIC) System were collected in five extracts (May 2001, December 2001, September 2002, March 2004, and December 2004).

- ***Participant tracking***—To obtain current address information on sample members, we implemented a series of active tracking measures (i.e., periodic mail outs to sample members requesting updated address and telephone information on sample members and contact persons) and passive tracking measures (i.e., periodic extracts from administrative and commercial databases to obtain updated address and telephone information).
- ***2000 Census data***—To construct measures of neighborhood quality, data from the Census Bureau's Summary File 3 were assembled for the Census tracts in which participants resided during the follow-up period, by geocoding the addresses collected at the time of random assignment and the updated addresses gathered from the follow-up survey, PIC, TANF data, and from the participant tracking efforts.
- ***Local housing and employment data***—We obtained data for the cities and metropolitan areas in which the study sites are located from demographic profiles available from Bureau of the Census and Bureau of Labor Statistics on total population, incidence of poverty, median household incomes, housing vacancy rates, and labor market conditions.
- ***Interviews with program staff and service providers***—To monitor the implementation and operations of the WtWV programs in the research sites, we conducted on-site interviews with staff from the local HAs, TANF agencies, and other partner organizations in October-December 2001 and in February-March 2003, and periodic telephone interviews throughout the follow-up period.
- ***In-depth interviews.*** Using funding provided through the HUD contract and grants from the Annie E. Casey Foundation, the Rockefeller Foundation, and the Fannie Mae Foundation, we conducted in-depth interviews with 141 individuals in the treatment group who had completed an interview as part of the follow-up survey. These interviews were conducted in 2005 in the respondents' homes and collected information about the experiences of voucher recipients with respect to housing mobility and neighborhood location, sources of income, employment, education, health, and child well-being. The interviews were designed to solicit more detailed and nuanced information than was possible through the follow-up survey about how voucher recipients make decisions about housing, education, employment, child care, and use of the family's resources and the role the voucher plays in this decision making. Information from these interviews is integrated with the findings from the quantitative impact analysis in the subsequent chapters of this report. In addition, 75 in-depth interviews with both treatment and control group members were conducted in 2002, and information from those interviews is also integrated with the findings from the impact analysis.

Sample Characteristics, Lease-up Patterns, and Impact Measures

Baseline survey data were obtained for 8,573 of the 8,731 individuals randomly assigned across the six evaluation sites. The sample is predominantly female, never married, and between the ages of 19 and 44, with an average age of 31 years. Nearly half the sample members are non-Hispanic black, while 21 percent are Hispanic, and 20 percent are non-Hispanic white. More than one-half of sample members (nearly 57 percent) either graduated from high school or had a GED, and at the time of random assignment 16 percent were

enrolled in some type of school or training program. The average household included 4 persons. Comparison of the baseline characteristics between the treatment and control cases indicates that random assignment succeeded in providing two well matched groups.

Through the 42nd month after random assignment, 67 percent of treatment group members across all sites had leased with a voucher. Among all control group members, 41 percent had leased with a voucher. The latter was an expected result of the evaluation procedures under which individuals assigned to the control group were placed onto (or retained their position on) HCV program waiting lists and could potentially receive a voucher as time progressed. The 42-month interval is the longest over which all members of the research sample—including those in Los Angeles, the last-enrolled site—are observed in the PIC lease-up data available through December 2004 for this report.

The 42nd month lease-up rates among treatment group members were lowest in Los Angeles and Atlanta (51 percent and 56 percent) and highest in Augusta, Fresno, and Houston (84 percent, 74 percent, and 70 percent). Among control group members, lease-up rates at month 42 were lowest in Atlanta, Los Angeles, Fresno, and Spokane (29 percent, 36 percent, 37 percent, and 38 percent) and highest in Augusta (60 percent).

By the 48th month, across all sites except Los Angeles, 70 percent of treatment group members had successfully leased a unit, as had 42 percent of controls. Among treatment group members, the rates varied from a low of 57 percent in Atlanta to a high of 84 percent in Augusta. The lease-up rate among controls at month 48 ranged from 30 percent in Atlanta to 62 percent in Augusta.

The lease up percentages referred to above were taken from HUD's PIC data. At the time of the follow-up survey we also asked respondents whether they were receiving voucher assistance. Overall, in the survey respondent sample, 51 percent of treatments and 37 percent of controls reported receiving voucher assistance at the time of the follow-up survey.

In estimating the effects of receiving a voucher, it was important to take account of the fact that some treatment group members failed to lease up with their voucher (i.e., treatment-group nonparticipation), while some controls received, and leased up with, a voucher from the regular HCV program (i.e., control-group crossover). In all tables presenting impact estimates in this report, we show two sets of estimates, as follows:

- ***Intent-to-Treat (ITT) estimates.*** The ITT estimates measure the impact of the treatment on the entire treatment group, relative to the entire control group, adjusting only for a standard set of baseline participant characteristics. These estimates are called "Intent to Treat" estimates because they describe the impact of the treatment on the entire group, which the program *intended* to assist, regardless of whether individual members of the treatment group *actually* received the treatment (and whether control group members may have received the treatment).
- ***Treatment-on-Treated (TOT) estimates.*** The TOT estimates present the impact of the treatment on *those treatment group members who were actually treated* – those treatment group members who received a voucher and successfully leased up – relative to no voucher assistance. The TOT impacts thus adjust for treatment group member nonparticipation in the program. The TOT impacts also adjust for the fact that some control group members did, in fact, come off the HCV waiting list to receive a voucher

and did lease up. Thus, the TOT estimates control for both treatment group nonparticipation and control group crossover. In this report, we employ two different methods for calculating TOT impacts, one method for follow-up survey outcomes and a second for administrative and address history outcomes. For survey outcomes, we are unable to correct directly for the timing of lease-up because we know the status of survey outcomes at only a single point in time (i.e., the survey date). For administrative and address history outcomes, we do have continuous measures over time, and so we are able to directly correct for the differential timing of lease-up by treatment group participants and control group crossovers.

Impacts of Vouchers on Housing Location and Household Composition

Receipt of housing assistance in the form of a voucher should allow recipients to access housing in a wider range of neighborhoods than without the voucher. If a unit in a higher-quality neighborhood becomes affordable with the voucher, the family can potentially move to that unit (if it meets inspection standards and the landlord will accept the voucher). Additionally, the voucher may enable the family to "lease in place" (in their current unit), possibly avoiding a move to a neighborhood of lesser quality. An *impact on neighborhood quality* in this report refers to the net result of the pattern of "moves and stays" for treatment group members, versus the pattern for the control group. In this context, leasing in place may offer locational advantages as well as stability to a family's life.

Prior to estimating program impacts, we used the follow-up survey data to conduct an analysis of voucher retention among treatment cases (in the five sites excluding Los Angeles). We looked at the probability of still being leased-up with rental assistance at the time of the follow-up survey, among treatment group respondents who had leased-up with a voucher at *any* time after random assignment. The probability of still receiving assistance ranged from 61 percent in Spokane to 93 percent in Atlanta.

The voucher treatment was found to have the following significant impacts on outcomes regarding ***housing location***:

- An increased likelihood of residing outside the family's baseline Census tract by the end of the follow-up period.
- A reduction in the overall number of moves made during the follow-up period.
- A better residential location at the end of the follow-up period, as indicated by a lower poverty rate, a higher employment rate, and lower welfare concentration in the family's end-of-period Census tract.
- A different demographic composition of the family's end-of-period neighborhood, as indicated by lower minority concentration, lower black concentration, and lower female-headedness.
- A reduced extent to which graffiti and public drinking were problematic conditions in the end-of-period neighborhood.

No significant effects were found on the number of months lived at the end-period neighborhood, on other Census-measured neighborhood characteristics (educational attainment, youth idleness, and Hispanic concentration), on the family's neighborhood satisfaction, on additional indicators of neighborhood problems (litter or trash, abandoned buildings, people hanging out, or people using or selling drugs), or on crime victimization.

Locational effects by subgroup were varied, but generally showed a pattern of greater mobility (from the baseline Census tract) and higher locational quality in the end-of-period Census tract among the following categories: those residing at baseline in more stressful arrangements (e.g., in public or assisted housing or in shared or temporary housing), and those facing greater barriers to employment at baseline (e.g., less educated, with pre-school children, not employed, or never employed).

On ***household composition***, vouchers were found to have the following significant impacts:

- A reduction in the proportion of households that are multigenerational and a corresponding increase in the proportion of households that consist of a single parent with child(ren) with no other adults present.
- A reduction in household size, associated with a lower average number of elders, siblings, and other adult household members.
- No reduction in the likelihood of residing with a spouse or partner at the time of the survey.
- A reduced likelihood of residing with more than one spouse or partner since random assignment (compared with residing with none or one spouse or partner).

No effects were found on the number of children living in the household at the follow-up survey, either birth children or other children.

Some in-depth interview respondents reported that voucher assistance enabled them to stop living with partners in unsatisfactory or abusive circumstances, and others reported satisfaction with being able to leave a doubled-up housing arrangement and establish their own household without having to move in with a boyfriend or partner. However, other respondents reported that the voucher caused an unwanted family break-up or discouraged the formation of a two-adult household. In all five cities the belief was widespread that voucher program rules prohibited males who were unrelated to the leaseholder from living in a voucher-assisted unit. A few respondents thought this "rule" applied even to their husbands and adult sons.[2] In some cases, in-depth interview respondents said that the boyfriends and fathers of children established separate households. In other cases, respondents appeared to disregard what they thought to be the program rule and continued to live with the boyfriend, often not reporting the individual's presence (and income) to the housing authority. Respondents, even those who were quite candid in other sensitive areas, were reluctant to

[2] The source of this misunderstanding is unclear. One could speculate that this belief came from confusion between voucher and welfare program rules, although some respondents reported that housing authority staff had provided this information. It is also possible that the confusion arose based on voucher program rules that prohibit voucher assistance to persons involved in drug-related crimes and the fact that the income of additional household members reduces the amount of the voucher subsidy.

talk about the presence of boyfriends and children's fathers in the unit, presumably because of concerns about losing the voucher. This reluctance may have affected the survey responses and thus the impact findings.

Impacts were estimated by subgroup for major compositional outcomes. The significant treatment effects noted above on the proportion of households consisting of single parents with children (positive) and on household size (negative) were also both found for the following subgroups (as defined at baseline): those with at least a high school diploma, those not enrolled in school or training, those with any dependent children, those with children under the age of six, those ever employed, those not desiring to move for employment, those residing in public or assisted housing, and those receiving TANF.

Impacts on Employment, Means-tested Benefits, and Education

Housing Choice Vouchers are hypothesized to improve long-run labor market outcomes for participants and their families by providing voucher recipients additional resources with which to stabilize their families, help care for their children, and invest in education and training. Vouchers may also provide families an opportunity to relocate to neighborhoods that are closer to jobs or have community norms more supportive of work. The program may also create disincentives to work, however, at least in the short run. Economic theory predicts that income-conditioned subsidies such as housing vouchers, which simultaneously increase family resources and reduce the marginal returns to work through a higher implicit tax rate, will reduce work effort. In our earlier analysis of the impacts of vouchers on welfare families over the first 5-7 quarters after random assignment, we did in fact find small negative impacts on work effort and, consequently, increased reliance on public assistance.

The findings confirm that having and using a voucher reduced employment rates and earnings amounts in the first year or two after random assignment. However, the small negative impact of vouchers disappeared over time, and vouchers had no significant impact overall over 3.5 years of follow-up.

One of the ways in which vouchers may affect employment and earnings is through increased education and training, made possible by the additional household resources freed up by the voucher or by time freed up by any reduction in employment among voucher users. Although there was some evidence from in-depth interviews that voucher users did take advantage of this opportunity to upgrade their skills, the impact analysis shows no significant treatment-control difference in the amount or type of education and training received during the follow-up period.

Although we find significant negative impacts on employment and earnings only in the early part of the follow-up period, housing vouchers significantly increased total public assistance benefits received by treatment group participants throughout the entire follow-up period. The continued effect on receipt of public assistance appears to reflect the impact of the voucher on family composition: use of a voucher increased the proportion of single parent with children households (with no other adults present) versus all other household types at the time of the follow-up survey. This effect occurred because the voucher made it possible for single parents to live on their own.

Impacts on Poverty and Material Hardship

The voucher led to a reduction in the proportion of households whose combined cash and near-cash income was below the poverty threshold at the time of the survey. Receiving a voucher had this effect even though (1) the effects of the vouchers on earnings and welfare payments were insignificant at this juncture, and (2) the fraction of control group families using housing vouchers had risen nearly to the level for treatment group families.[3] Furthermore, the voucher treatment undoubtedly had an even greater effect on poverty earlier in the evaluation's observation period, when the treatment group's near-cash income from the vouchers far exceeded that of the control group.

On several indicators of material well-being, favorable impacts of the vouchers were statistically significant for virtually all types of households in the study:

- A substantial reduction in homelessness;
- An increase in independent housing and a corresponding reduction in doubling- up;
- An increase in the average number of rooms for household members and a corresponding reduction in crowding; and
- Increased household expenditures on food, which raised average family consumption but did not significantly reduce food insecurity.

The first three of these impacts demonstrate that the vouchers significantly increased the ***quantity*** of eligible families' housing—that is, whether they had any housing (were not homeless) and how much housing they had (the number of their rooms) at the time of the survey. The vouchers, however, did not significantly improve the ***quality*** of the housing. The treatment group's assessment of their housing, and the number of housing problems they reported (e.g., faulty plumbing, leaking roof), were not significantly different from those for controls.

With respect to the findings on homelessness, a quarter of the control group members who answered the pertinent survey questions reported that their household did not have a place to live at some point in the year before the follow-up interview. The fraction of the treatment group families that lacked a place to live was less than half this amount, and the difference between the groups was statistically significant.

The results for homelessness can be divided into two categories: (1) families living on the streets or in shelters at baseline, and (2) families who lived with or among friends, relatives, or others at baseline. More than a quarter of the control group members who reported being homeless were in the first group, while the remainder was in the second. The vouchers reduced homelessness in the first category from 7 to 5 percent and they cut homelessness in the second category from 18 to 12 percent. Both impacts were statistically significant.

[3] The "treatment on treated" (TOT) impact estimates correct for controls' use of vouchers over the entire course of the evaluation's observation period. However, for the reasons discussed later in this chapter, this correction does not fully capture the narrowing of the treatment-control difference in voucher use by the time of the survey, when the outcomes examined in Chapter 5 were measured.

The impacts of the vouchers were larger for some of the subgroups within the overall sample. One group includes welfare recipients who said at the time of random assignment that their eligibility for TANF would expire within six months. This group experienced a substantial reduction in poverty when near-cash income was included in the assessment. In addition, impacts on homelessness for this group, which was vulnerable to dislocation because of the impending loss of public assistance, were dramatic: homelessness in the year before the survey was cut in half and this result was statistically significant. It is noteworthy, too, that the vouchers significantly reduced other material hardships for this group. Food insecurity was substantially reduced, and a drop occurred in the incidence of families forgoing needed dental care.

Another important at-risk subgroup consists of households whose heads were unemployed at the time of random assignment. Again, the impacts of the vouchers on poverty (counting near-cash income), homelessness, and independent housing were very large. In addition, the impacts on several other hardship measures – notably the number of rooms in the family residence and the household's food expenditures – were impressive.

Finally, the vouchers appear to have substantially improved the well-being of families with children, particularly those with children less than six years old. For this group, the vouchers had significant impacts on virtually all outcomes considered in this analysis, including poverty and homelessness. Thus, the reductions in poverty and hardship generated by housing vouchers clearly reached young children.

Impacts on Child Well-Being

Treatment effects were found to be significant for only a small number of outcomes related to child well-being, as follows:

- ***Impacts on school performance and educational progress***—The voucher was found to reduce the likelihood that a child was out of school at follow-up because of health, financial, or disciplinary problems, for the youngest age group of children (those under 6 at baseline). The treatment was also found, however, to increase the likelihood that a child had repeated a grade since random assignment. No significant effects were found on any of the outcomes related to special classes or school services to address learning, behavior, or emotional problems. Nor did the voucher appear to affect the child's highest grade completed or whether he/she had enrolled in college.
- ***Impacts on behavior and time use***—Using the data for all children for whom surveys were completed, no significant treatment effects were found for any of these outcomes, including a "behavior problems index" that incorporated 11 forms of problem behavior into a composite measure. Treatment children under 6 years old at random assignment (under 11 years old at follow-up) were significantly less likely to be in afternoon school activities.
- ***Impacts on delinquency and risky behavior***—There were no significant effects of the voucher offer on these outcomes for the child sample as a whole.
- ***Impacts on parental involvement and family life***—For only one outcome in this domain was a significant treatment effect found, for estimates using all surveyed

children. This was a significant reduction in the likelihood of the parent working with a youth group or other activity outside of school. By subgroup, this effect was also found to be significantly negative for children in the 6-9 age group at baseline. In contrast, a favorable effect of vouchers on the latter subgroup (children 6-9 years old at baseline) was a significant increase in the number of days per week that the family eats together.

Concluding Assessment

The usefulness of this research for policy development lies importantly in the fact that the housing choices available to voucher users were expanded through greater affordability, with few constraints imposed by the program. To successfully lease-up with the voucher, a program participant needed to identify a housing unit that met inspection standards, with a landlord who was agreeable to voucher use. Treatment group members were able to use their voucher to lease in place. If they wished to move, their locational decision was not constrained by the characteristics of the new neighborhood. The program did impose deadlines for voucher lease-up, but otherwise generally allowed the participants to freely exercise housing choice.

The importance of this feature of the demonstration—expanded affordability with relatively few program constraints—is that the patterns of voucher use and the resulting impact estimates can be regarded as indicating the basic underlying preferences and priorities of voucher users. The issue now posed for policy makers by this research is whether the housing choices made by voucher users—i.e., their "revealed preferences"—are consistent with the program's intended goals. Did these families, largely unconstrained in their housing choices, make decisions that changed outcomes in desirable directions for themselves and their families? Answers will certainly differ as to whether the estimated impacts represent desirable or undesirable effects, or whether their magnitudes are large enough to be meaningful. What is most noteworthy here is that the debate over these questions can now proceed with well-developed empirical evidence in hand.

Chapter One
Introduction

The Welfare to Work Voucher (WtWV) program was initiated in Fiscal Year 1999 when Congress appropriated $283 million for tenant-based rental assistance to help families to make the transition from welfare to work. This appropriation funded 50,000 new rental assistance vouchers (P.L. 105-276). These vouchers were awarded to local and state housing agencies (HAs) that presented reasonable plans for matching up eligible families with the available housing assistance and for coordinating these efforts with existing welfare reform and welfare-to-work efforts. HUD renewed these vouchers annually to continue the demonstration program until FY2004 when the program was phased out. Participating housing agencies were notified by HUD in March 2004 that issuance of WtW vouchers that were not under lease at that time and those that became eligible through turnover after that time would no longer be subject to HUD's WtW voucher program requirements. After March 2004, WtW vouchers that became available through turnover were incorporated into an HA's regular Housing Choice Voucher (HCV) program.

At the time the program was authorized, Congress mandated a comprehensive evaluation of the WtWV program to assess the results of rental assistance in promoting self-sufficiency of families who received the assistance. The U.S. Department of Housing and Urban Development's (HUD) Office of Policy Development and Research, as manager of the evaluation, designed the study as a social experiment, with random assignment of families to receive demonstration vouchers or to a control group that received no housing assistance from the demonstration. In implementing a rigorous evaluation of the effects of the WtWV program, HUD took a large step toward expanding what is known about the effects of tenant-based rental assistance on the economic self-sufficiency and well-being of low-income families.[4] The study offers policymakers and Congress evidence on the extent to which providing tenant-based rental assistance affects the lives of low-income families, with respect to housing quality and location, mobility, employment and earnings, and other aspects of family well-being such as health, child well-being, material hardship, and adult family members' participation in education and training.

In 1999, HUD contracted with Abt Associates to design and implement the study and, through subsequent task order contracts, to collect and analyze data on the experiences of program participants. This report is the final report for the study. It presents the results of the assessment of net impacts of the WtWV program over the five-year follow-up period on the quality of family's residential location, housing mobility, housing quality, employment, earnings, receipt of public assistance, income and material hardship, health, and selected measures of child well-being using data from a follow-up survey with a sample of the research sample as well as administrative records data. This analysis provides rigorous and unbiased findings on the effects of housing assistance on a wide range of outcomes related to the well-being of low-income families.

[4] For a review of literature on the effects of housing assistance on self-sufficiency, see Shroder (2002).

1.1 Overview of the Welfare to Work Voucher Program

The rental assistance provided through a WtW voucher was essentially the same as that available through a regular voucher. Participants were free to use the voucher to rent a housing unit of their choice in the private rental market as long as it met HUD's Housing Quality Standards (HQS) and had a rent that was reasonable compared with the rents of unassisted units in the same housing market. The voucher assistance subsidized the monthly rent for the unit, and the value of the subsidy, as in the Housing Choice Voucher program, was the payment standard established by the HA (or the unit's actual rent, if lower) minus 30 percent of the family's adjusted monthly income.[5] An important exception was in the case of the Atlanta Housing Authority, which restricted the use of the WtW voucher outside the City of Atlanta and did not allow WtWV recipients to use the voucher to port out of the Atlanta Housing Authority jurisdiction. This type of policy was not adopted in the other evaluation sites.[6]

In addition to the special eligibility requirements for the WtW voucher program, there were two key operational differences that distinguished it from the HCV program as it is generally administered; but these differences did not have a substantial influence on program operations in the study sites. First, the final rule governing the operations of the regular HCV program (24 CFR Parts 888 and 982) requires that not less than 75 percent of new admissions to the program have incomes at or below 30 percent of the area median income. This requirement could be reduced for WtW voucher programs if the housing agency demonstrated that complying with the targeting rule for WtW voucher admissions would interfere with the objectives of the WtWV program.[7] Second, housing agencies that operated a WtW voucher program were allowed to terminate rental assistance if a family violated obligations established by the housing agency under the WtW voucher program, such as work requirements or requirements to participate in employment and training programs. Under regular HCV rules, a family can be terminated from rental assistance only for

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[5] Payment standards are adjusted for the number of bedrooms in the unit. The actual rent includes an estimate of the cost of utilities paid for by the tenant.

[6] Typically, in the HCV program, participants are allowed to move out of the jurisdiction of the housing authority that initially issued their voucher after residing for a year in the original jurisdiction. This is referred to as "porting-out", and each HA establishes policies and procedures for how this process is conducted. We expect that this policy restricting the portability of WtW vouchers in Atlanta would result in less mobility among voucher participants in that site as compared to the other evaluation programs. However, the analysis of mobility presented in Chapter 3 defines mobility in terms of having moved from one Census tract to another after receipt of the voucher in an effort to identify those voucher recipients that used the voucher to lease in place. This policy restricting portability out of AHA's jurisdiction would not be likely to influence the decision to lease in place with the voucher, but rather the total distance moved.

[7] Such an exception was requested only by one evaluation site, Fresno, where the HA believed the income targeting rules severely impinged on the HA's ability to serve underemployed TANF recipients. The Fresno request was approved. In addition to income eligibility, the housing authorities also conduct criminal background checks. Some require that an applicant have no felony convictions within the previous five years, while others require that there be no drug-related convictions, regardless of timing. Finally, the housing authorities in the study also require that applicants not owe any back payments of rent if they lived in public housing in the past.

fraudulent or criminal behavior or after eviction by the landlord for a serious lease violation.[8] One of the HAs included in the evaluation, the Atlanta Housing Authority, imposed a requirement that WtW voucher recipients work a minimum of 25 hours per week or become involved in a training or education program within 60 days of using their WtW voucher to lease a housing unit and informed program participants that failure to comply with these requirements could result in termination from the voucher program. Atlanta Housing Authority staff reported that it was difficult to monitor compliance with the employment requirement over time, and in the end they did not terminate any WtW voucher participants for failure to adhere to the employment requirement.[9] None of the other HAs in the study imposed any termination procedures specific to their WtWV program.[10]

The program model envisioned by the Congress in the statute, by HUD in its implementing regulations, and by the sites in their funding applications, called for a two-part effort to provide housing assistance geared to promoting the self-sufficiency of welfare recipients. First, the program was to target housing vouchers to welfare recipients whose efforts to achieve self-sufficiency would benefit from housing assistance. Second, the program was to deliver housing- and employment-related program services to enhance the effectiveness of the voucher. Both components of this effort were to involve new partnering arrangements between housing authorities and TANF agencies, plus a coupling of housing- and employment-related program services with the WtW voucher.

However, HAs appear to have achieved only part of this vision. It appears that interagency partnering between the HA and TANF agency or the agency administering welfare-to-work employment and training grants was somewhat limited. This may be in part because of restrictions on the time allowed for HAs to lease their allotment of WtW vouchers. Participating HAs were required by HUD to lease their vouchers within one year of program start-up. Most agencies focused their energies on identifying eligible families and issuing vouchers as quickly as possible to the exclusion of developing interagency partnerships in the initial year of the program. As a result, although vouchers were targeted to eligible families, there was little effort made to select those families for which housing was particularly important for working or increasing earnings. Furthermore, for the most part, WtWV

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[8] 24 CFR Parts 888 and 982 "Section 8 Tenant-Based Assistance; Statutory Merger of Section 8 Certificate and Voucher Programs; Housing Choice Voucher Program; Final Rule". Federal Register, October 21, 1999. 24 CFR 982.552(c) (1) (x).

[9] As discussed later in the report, information from the in-depth interviews with program participants indicates that participants believed that their assistance would be terminated if they failed to comply with the employment requirement, and many pursued employment and/or training activities to avoid termination from the voucher program. As a result, even if the termination policy was not actually exercised by the housing authority, it still may have influenced participant behavior.

[10] Other housing authorities nationwide, however, appear to have terminated some participants for noncompliance with WtW-related work requirements or other family obligations. Note the following:

> "While Quadel cannot determine the number of terminations that have occurred due to failure to meet WtW family obligations, we can estimate that about 50 percent of the PHAs with WtW family obligations have terminated families for failure to meet these WtW family obligations."

See Quadel Consulting Corporation, "Welfare to Work Voucher Program: Final Report," September 30, 2002, pp. 2-11 and 2-12.

households did not receive specialized services beyond those available to TANF (or TANF-eligible) families that receive regular HCV assistance. An exception to this is the Fresno site, discussed in Chapter 2, in which specialized services were offered to program participants, but not until approximately 18 months after random assignment. As a result, we have concluded that the evaluation is essentially a test of the effects of receiving a housing assistance voucher, giving the study even broader policy applicability. Given this conclusion and the fact that the WtWV program was phased out in 2004, the evaluation was renamed in 2004. The new name, Effects of Housing Choice Vouchers on Welfare Families, is used throughout the remainder of the report when referring to the evaluation. When referring to the demonstration program in operation between 2000 and 2004, we use the original program name, WtWV program.

1.2 Design of the Evaluation

Research Questions

The fundamental goal of this evaluation was to assess the impacts of receiving a voucher on improving the housing quality and locations of families with children, on their obtaining and retaining employment, and on their levels of welfare dependency, material hardship, and family well-being. To assess these program impacts, a large body of data was collected from several administrative data sources and a follow-up survey of research sample members approximately 4 ½ years after random assignment. The study also includes qualitative research that gathered information from families that received a voucher about their experiences and decision making regarding housing, employment, education, health, family well-being, and their efforts to attain self-sufficiency.

The evaluation assesses impacts in four primary areas that may be affected by receiving a housing voucher:

- Housing mobility and neighborhood environment;
- Adult employment, education and training, and receipt of public assistance;
- Household income and material hardship (including rent burden and housing quality); and,
- Family and child well-being (household composition, parental involvement, child behavior, child time-use, and school performance).

Hypothesized Effects of Tenant-based Rental Assistance

There are three major operating hypotheses regarding the effect of housing vouchers on low-income families with children:

- Adults (and children of working age) in families that receive vouchers are more likely to obtain and retain employment than those in families that do not receive vouchers, and the average income of families that receive vouchers will increase. In addition, the quality of jobs obtained or retained by those who receive vouchers will be greater than those who do not receive vouchers.

- Families who receive vouchers are more likely to move to neighborhoods close to existing or prospective employment, employment training services, or public transportation than are families who do not receive vouchers.
- Over a longer term, the additional financial resources or the improved housing made available through vouchers could stabilize families' budgets, decrease stress, and improve family members' sense of control and ability to plan. Among voucher holders who decrease work hours, the increased time available may enable parents to reduce their reliance on out-of-home child care and to devote more time to direct supervision of their children's activities. Adults may also use the additional hours or the increased discretionary income to pursue additional education or training. Indirect benefits could include improved health of family members and improved school attendance and performance of children.

There are a number of possible mechanisms through which the housing voucher may affect the economic well-being of program participants.

- **Increased discretionary income.** The direct effect of the voucher is to increase the available discretionary income for the household by freeing up resources that would otherwise be used to pay for rent. Because the voucher limits the family's rent burden (the percentage of income devoted to rent) to about 30 percent, those with previous rent burdens greater than 30 percent now have more income available for other basic living needs.
- **Increased hours spent not working**. In the short run the voucher may serve to decrease work effort for some participants because the voucher permits the pre-existing level of income to be sustained with less work. In addition, the value of the housing subsidy declines as earnings increase, thereby reducing the returns to work. This feature of the voucher subsidy could also serve to decrease work effort. (Under conventional microeconomic theory, both the income effect and the substitution effect of the voucher tend to reduce labor supply.)[11] How the discretionary time is used under this scenario may have some benefits for low-income families.
- **Improved quality of neighborhood location and housing unit.** Other effects of the voucher could arise through choice of housing location and unit. Although voucher recipients can use the voucher to lease in place, vouchers often are used to move to a new location. This can improve the quality of the voucher holder's neighborhood location or the quality of the housing unit. Those who use the voucher to improve the quality of their housing may have more space for important activities (e.g., for children to do homework) or may reduce the stress associated with living in cramped quarters or with people with whom they are in conflict.
- **Proximity to jobs or training.** For those who use the voucher to move, the new location may be more convenient to jobs or training. Or the new location could be closer to childcare and transportation that would facilitate finding or retaining employment. A move to an area with higher employment rates and faster job growth may provide

[11] Two new studies, in a recent issue of the journal *Cityscape,* report findings on the effect of a housing voucher on earnings. Olsen *et al.*(2005) found that vouchers reduced earnings by at least 30 percent. Susin (2005) found a marginally significant reduction of about 10 percent in family earnings.

opportunities for jobs with higher wages. Finally, community norms in a new neighborhood may be more supportive of work and less supportive of public assistance, and these might increase job search and employment. It is also important to know whether vouchers promote mobility. To the extent that voucher holders lease in place, such tenant-based assistance will tend to have the same effects as an income transfer.[12]

- **Reduced stress and increased stability.** For those who use the voucher to stay in the same location, or for whom the voucher prevents unwanted later moves, the result may be reduced stress, greater ability to draw support from neighbors, or greater stability of children's schooling.

Site Selection and Sample Enrollment

The Effects of HCV on Welfare Families evaluation was conducted in six locations that were selected in early 2000. The study sites and number of WtW vouchers awarded were:

- Atlanta, Georgia (450 vouchers);
- Augusta, Georgia (700 vouchers);
- Fresno, California (City and County) (1,400 vouchers);
- Houston, Texas (700 vouchers);
- Los Angeles, California (700 vouchers); and
- Spokane, Washington (700 vouchers).

Site selection for the evaluation focused on choosing sites that were reasonably representative of the WtW voucher program and that offered a suitable environment in which to conduct the experimental evaluation. Selection was, necessarily, a judgmental process.

In October 1999, HUD awarded 121 voucher allocations to 129 local housing agencies, Indian tribes, and tribally designated housing entities (TDHEs) to implement the WtWV program. Among the 121 grants were eight joint applications in which two or more housing agencies partnered to submit one application. With the exception of eight sites for which set-asides were provided in the law, the awards were made competitively, based on the strength of program applications submitted by the HAs. Among the 121 allocations, 49 agencies received at least 450 vouchers, the threshold established for consideration for the evaluation. Of the 49 potential evaluation sites, 23 volunteered to participate in the evaluation as part of their application for program funds.

[12] Two recent studies using nonexperimental research methods explore factors that contribute to increased employment among welfare recipients. These studies help to illuminate key questions of interest about the relationship between housing assistance, housing location, and employment, but are unable to assess the impacts of housing assistance and housing location on employment as is possible through this experiment. Allard and Danziger (2003) explored the relationship between proximity to jobs and employment among recipients of welfare and found that greater proximity to employment opportunities is associated with a higher probability of working and of leaving welfare in the three-county Detroit metropolitan area. Bania *et al.* (2003) explored labor market outcomes for recipients of rental assistance vouchers, residents of public housing, and residents of project-based Section 8 properties. They found no difference in employment experiences among recipients of various types of housing assistance but did find differences depending on neighborhood characteristics.

An important consideration in selecting sites was to ensure, to the extent possible, that the experimental contrast established by random assignment could be preserved over time. Given that individuals assigned to the control group would remain on the HA's waiting list for regular voucher assistance, it was important to select sites where the expected likelihood of control group members receiving regular voucher assistance was low.[13] To assess this likelihood, we first considered each site's proposed strategy for recruiting WtW families. According to their applications, nearly all sites planned to draw most WtW families from the current HCV waiting list. This meant that families identified as eligible for WtW vouchers and assigned to the control group would retain their position on the HCV waiting list. We examined both the *size* of the existing waiting list and the *estimated proportion* of WtW-eligible families currently on the list, looking for sites in which less than half of the current waiting list was estimated to be eligible to receive a WtW voucher. This was meant to ensure that the majority of any new vouchers made available outside the WtW program would go to non-WtW eligible families—thereby reducing the chances of controls receiving such vouchers.

A description of the characteristics of each evaluation site is provided in Chapter 2.

Recruitment of Research Sample

To be eligible to receive a WtW voucher families had to be current or former TANF recipients or eligible for TANF benefits, as well as meeting the standard HCV eligibility requirements. The families could not be recipients of tenant-based assistance at the time of application (though they could have been receiving other forms of housing assistance), and the housing authority had to determine that the housing assistance provided through the WtW voucher program was critical to the families' ability to obtain or retain employment.

In some cases, the evaluation sites developed more targeted eligibility criteria within the framework of the TANF categories. For example, in Fresno and Los Angeles, the housing authorities and their partnering TANF agencies required that applicants work in order to be eligible for the program. (As described below, Fresno dropped this work requirement for the final cohort of WtW applicants.) In Augusta, applicants were required to be in compliance with their TANF work and services plans at the time of application or be expected to come into compliance with this plan within 30 days. In Spokane, all applicants were required to have completed an Individual Responsibility Plan with their TANF caseworker.

All of the evaluation sites except Augusta targeted only current and former TANF recipients, since these families could be readily identified in TANF caseload systems. The TANF-*eligible* population was considered more difficult to identify and was not included as a target group. In Augusta, by contrast, the TANF-eligible families *were* identified. The names of families on the HCV waiting list were submitted to the local TANF agency for eligibility certification. TANF staff completed a certification form for each family that indicated current or former TANF status or TANF eligibility. TANF eligibility was determined by whether families who were not current or former TANF recipients were receiving Medicaid.

[13] Although control group members were precluded from receiving a WtW voucher, it was deemed unethical to remove these families from the waiting list for regular vouchers or to prevent them from receiving such assistance. Our analytic treatment of controls who received regular vouchers ("crossovers") is discussed later in this chapter and in Appendix A.

Although HUD specified that WtWV housing assistance be determined to be critical to obtaining and retaining employment, the mechanisms for implementing this additional eligibility criterion were left to each housing authority. Houston and Los Angeles proposed specific procedures for assessing critical need on an individual applicant basis but did not actually implement them. Instead of conducting individual assessments of critical need, the sites asserted that low-income individuals currently or previously receiving TANF, by definition, have a critical need for stable, affordable housing to obtain or retain employment. Therefore, except in Augusta, the need was not evaluated on a case-by-case basis,[14] but was assumed for all applicants who met the current or former TANF criteria. This is consistent with findings from a study of the early implementation of the WtW voucher program and with the findings reported in a September 2002 report by WtWV program technical assistance providers. [15]

Random Assignment Procedures

Random assignment was initiated in April 2000 and was completed in May 2001. Start and end-dates for random assignment and sample sizes for each site are shown in Exhibit 1.1.

Exhibit 1.1
Random Assignment Period

Site	Random Assignment Period	Total Sample Size
Atlanta	June–September 2000	1,134
Augusta	June–November 2000	759
Fresno	April–June 2000	2,621
Houston	April–June 2000	2,021
Los Angeles	April–May 2001	1,047
Spokane	May–December 2000	1,149
Total		**8,731**

The random assignment procedures were tailored by site to avoid disruption of program operations and undue burden on HA staff. The WtWV study sites used four different sources for identifying potential participants:

- Some identified current or former TANF recipients on the HA's HCV waiting list;
- Some requested lists of current or former TANF recipients from the TANF agency, without regard to whether they were currently on the HA's HCV waiting list;
- Some took individual referrals from TANF staff or local nonprofit staff; and

[14] On the certification form completed by TANF staff for every applicant in Augusta, an item asked for information that "housing assistance is critical for obtaining or retaining employment." Augusta HA staff reported that this box was checked for all certified applicants.

[15] See Smith and Johnson (2000). That study reported that few of the 13 sites examined planned to assess critical need for housing on a case-by-case basis. See also Quadel Consulting Corporation, "Welfare to Work Voucher Program: Final Report," September 30, 2002.

- One conducted mass outreach to the community as a whole using public service announcements on the radio.

In each case, the HAs relied on the TANF agency to identify those individuals who met the HA's eligibility requirements for current or recent TANF receipt and for compliance with TANF requirements. If the TANF agency was not the original source of the list of potential participants, the HA sent the list to the TANF agency for review.

Once a list of potential participants was generated, the HA sent out a letter to all persons on the list informing them of the availability of WtW vouchers and inviting them to an orientation meeting to learn more about the program and find out if they qualified for a voucher.[16] At the WtWV orientation meeting, HA staff performed any eligibility checks that had not been done before the invitations went out; a small number of ineligibles were screened out at this point. The staff then described the WtW vouchers and explained the eligibility criteria for receiving them. They explained that they expected more applications than the available number of vouchers and that, among eligible applicants, those to receive vouchers would be chosen randomly. Those who wished to apply for a voucher signed a Participation Agreement, completed the HCV application materials, and furnished any additional information needed to determine their eligibility. In the Participation Agreement, the applicants acknowledged that they understood that vouchers would be awarded by lottery, agreed to complete a baseline survey, and gave permission to the researchers to access their administrative records at various government agencies.

During the orientation session, and prior to random assignment, the applicants also completed a baseline survey used to collect information on the applicant's characteristics and composition of the applicant's household. In Houston, Fresno, Los Angeles, and the large group sessions in Atlanta, the survey was completed as a group, with an evaluation staff member reading the questions and the applicants filling in their answers. In Atlanta, Augusta, and Spokane, the baseline form was self-administered, with evaluation staff available to assist applicants with questions or problems.

Random assignment was implemented toward the end of each orientation session, to ensure that only eligible applicants were included.[17] Two principal methods of random assignment were used, depending on the size of the orientation groups and other local considerations. Individual random assignment was used in Spokane, Augusta, and for the initial sessions in Atlanta. In Fresno, Houston, Los Angeles, and Atlanta's large-group sessions, the size of the orientation sessions made the use of individual random assignment impractical. To handle the volume of assignments in these sites, Abt Associates developed a random assignment approach based on the list of attendees at the orientation meetings.

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[16] In Los Angeles, the letter was sent by the TANF agency, which also hosted the orientation meetings and performed random assignment.

[17] In most evaluation sites, random assignment was conducted before criminal background checks of the applicants had been completed, because those checks take several days and the HA wanted to issue vouchers at the orientation meeting. Based on information from HA staff, we believe the proportion of applicants who failed these checks to have been small, ranging from less than 1 percent to perhaps 4 percent across sites. We expect that this had only a negligible effect on the results of the evaluation. In the analysis, the adjustment for families who failed to lease-up also removed any influence of these ineligibles on the impact estimates.

1.3 Data Sources

The remaining sections of this chapter provide technical details regarding the data and analytic techniques used to assess the impacts of receiving a rental assistance voucher. Several sources of data were used in this analysis. Appendix A provides additional details on the data sources and construction of datasets. This section gives an overview of each type of data.

Baseline Survey

A baseline survey was administered to all sample members immediately prior to random assignment. The information collected from this survey was used for several purposes:

- Participant demographic characteristics used to describe and stratify the sample;
- Baseline information used as part of the impact analysis;
- Contact information (for up to three friends or relatives) used for tracking sample members; and
- Participant identifiers used to extract administrative records.

The survey collected baseline information on employment status, satisfaction with the housing unit and neighborhood, receipt of public assistance, and household composition. Abt survey staff reviewed the baseline forms on-site for completeness and accuracy and then, at the end of each intake session, sent them to a central location for entry into data systems. Data files generated at the time of random assignment included a tracking file for logging in the receipt of each completed questionnaire and the participant agreement form. (The latter form provided the individual's consent to participate in the WtWV demonstration and explained that vouchers were to be distributed randomly to some, but not all, participants.)

Participant Follow-up Survey Data

From October 2004 through May 2005, we conducted interviews with a subset of the research sample to collect information on outcomes that is not available from administrative data sources. The follow-up survey instrument collected information about housing assistance and services, housing mobility and neighborhood environment, adult employment, education and training, household income, public assistance, food security, and family and child well-being. The interviews were conducted first by telephone, with in-person follow-up for those that could not be completed by telephone. The survey instrument consisted of a Core Module and a Parent-on-Child/Youth module. The Core Module was administered to the adult in each household who applied to the experimental housing voucher program. The Parent-on-Child/Youth module was also administered to the adult respondent for up to two children who were present in the household and age 15 or younger at the time of random assignment and who thus had reached the target age range of 4 to 19 years at the time of the survey. Follow-up survey data were collected for a total of 2,481 sample members. A discussion of the survey sampling procedures and response rate is provided in Appendix A. Tests for non-response bias are discussed in Appendix C.

In-depth Qualitative Interview Data

Using funding provided through the HUD contract and grants from the Annie E. Casey Foundation, Rockefeller Foundation, and the Fannie Mae Foundation, we conducted in depth

interviews with 141 individuals in the treatment group who had completed an interview as part of the follow-up survey. These interviews were conducted in 2005 in the respondents' homes and collected information about the experiences of voucher recipients with respect to housing mobility and neighborhood location, sources of income, employment, education, health, and child well-being. The interviews were designed to solicit more detailed and nuanced information than was possible through the follow-up survey about how voucher recipients make decisions about housing, education, employment, child care, and use of the family's resources and the role the voucher plays in this decision making. Information from these interviews is integrated with the findings from the quantitative impact analysis in the subsequent chapters of this report.

We also conducted in-depth interviews with 75 treatment and control group members in early 2002 to gather information about decision making regarding housing, employment, and pursuit of education and training in the first one to two years following random assignment. Information from these interviews is also incorporated into the analysis presented in this report.

Unemployment Insurance (UI) Wage Records

To measure quarterly earnings and employment rates, we collected employer-reported earnings records from the four states included in the WtWV evaluation. Quarterly wage records, provided by employers to each state's employment security agency under the requirements of the unemployment insurance system, were collected for members of the research sample, both treatment and controls, from one year prior to random assignment through the October-December 2004 quarter.

TANF Data Files

To measure the effects of the WtWV program on welfare participation, we collected information on the receipt of TANF and Food Stamp benefits from state or local welfare agencies. The data were obtained for a time period beginning 12 months prior to random assignment and extending through December 2004 in all sites except Los Angeles. In Los Angeles these data were obtained for the time period ending September 2002, because the TANF agency providing the data was unable to provide a final extract of data through December 2004.[18] Detailed monthly benefit data were collected that allowed us to construct a number of outcome measures for each of these two programs – e.g., average quarterly benefit, number of quarters of benefit receipt, total benefits received during the follow-up period, and number and duration of spells of welfare receipt during the follow-up period. They were also used to identify families who were TANF or food stamp recipients prior to random assignment.

[18] The Los Angeles Department of Public and Social Services (DPSS) established a data sharing agreement with the Abt team in 2001 and provided TANF and Food Stamp data through September 2002. However, in order to extend the time period of the data sharing agreement and provide data through the desired December 2004 time period, DPSS would have required that Abt obtain new participant agreement forms from the more than 1,000 study sample members in the Los Angeles site since the original participant agreements had been completed and signed by sample members in 2001. It was not feasible to obtain new agreements from all sample members in this study site.

HUD Data Files

Data from HUD's Public Housing Information Center (PIC) system (formerly Multifamily Tenant Characteristics System (MTCS)) were collected in five extracts: May and December 2001, September 2002, March 2004, and December 2004. These data were used to monitor the receipt of housing assistance through the Housing Choice Voucher and public housing programs for sample members in both the treatment and control groups. This information is recorded by HA staff on HUD Form 50058 and is transmitted periodically to HUD.

The PIC data were used for several purposes:

- To determine which treatment group members were successful in using their voucher to lease a housing unit or who received housing assistance through the public housing program;
- To determine if any control group members received a Housing Choice Voucher (or moved into public housing) after random assignment; and,
- To support locational tracking of the sample (for instance, for individuals who left TANF assistance and changed their residence after random assignment, but received housing assistance or services at their new location).

2000 Census Data

The analysis of former and current neighborhood quality for treatment and control group members was based on tract-level data from the 2000 Census. Census data, commonly used as a proxy to describe neighborhood-level characteristics, are particularly well suited for this analysis as the WtWV evaluation began in early 2000 and random assignment was completed in May 2001. Data from the Census Bureau's Summary File 3 were assembled for the census tracts in which participants resided during the follow-up period by geocoding the addresses collected at the time of random assignment and the updated addresses gathered from participant tracking efforts, PIC data, TANF data, and from the follow-up survey locating effort. Measures of neighborhood quality based on Census data included:

- Racial and ethnic composition;
- Percentage of persons living in poverty;
- Percentage of civilian labor force that is employed;
- Levels of adult educational attainment;
- Percentage of youths not in school and not in the labor force;
- Percentage of female-headed households; and,
- Percentage of households with public assistance.

Local Housing and Employment Data

In addition to the tract-level data from the 2000 Census, we also obtained data for the cities and metropolitan areas in which the study sites are located at baseline from demographic profiles available from the 2000 Census. These data included information on total population, incidence of poverty, median household incomes, and housing vacancy rates.

We also obtained local area labor market information from Bureau of Labor Statistics published reports.

Interviews with Program Staff and Service Providers

To monitor the implementation and operations of the WtWV programs in the research sites, we conducted interviews with staff from the local HAs, TANF agencies, and other partner organizations. We conducted interviews both in person (during site visits) and by telephone. This activity was concentrated in the first three years following random assignment. In the final years of the evaluation we conducted periodic telephone interviews with staff from the local HAs to discuss operations as the WtWV program was phased out.

Impact Estimates: Intent-to-Treat versus Treatment-on-Treated Estimates

With random assignment, simple differences in means between the treatment and control groups provide unbiased estimates of the impact of an intervention, provided that all treatment group members, and none of the controls, are exposed to the intervention. In practice, both of these conditions are usually violated to some extent. In this case, some treatment group members failed to lease up with the demonstration voucher, and some controls received, and leased up with, vouchers from the regular HCV program. In the literature, the former are known as "nonparticipants" and the latter are known as "crossovers."

In all of the tables presented in Chapters 3, 4, 5, and 6 we present two sets of estimates – the "intent to treat", or ITT, estimate, and the estimated impact of the "treatment on the treated," or TOT estimate. The ITT estimates measure the impact of the treatment on the entire treatment group, which the program *intended* to assist, regardless of whether individual members of the treatment group *actually* received the treatment and whether control group members may have received the treatment. Simply stated, the ITT estimates show the difference in outcomes between the entire treatment group and the entire control group, including those treatment group members who never used their voucher and those control group members who did manage to obtain and use a voucher.

The TOT impacts present the impact of the treatment on *those treatment group members who were actually treated*—those treatment group members who received a voucher and successfully leased up—relative to comparable control group members who received no voucher assistance. The TOT impacts thus adjust for treatment group member nonparticipation in the program. The TOT impacts also adjust for the fact that some control group members did, in fact, come off the HCV waiting list to receive a voucher and did lease up. Thus, the TOT estimates control for both treatment group nonparticipation and control group crossover. In this report, we employ two different methods for calculating TOT impacts, one method for follow-up survey outcomes and a second for administrative and address history outcomes. For survey outcomes, we are not able to correct directly for the timing of lease-up because we know the status of survey outcomes at only a single point in time (i.e. the survey date). For administrative and address history outcomes, we do have continuous measures over time, and so we are able to directly correct for the differential timing of lease-up by treatment group participants and control group crossovers.

Adjustment for Nonparticipants and Crossovers in the Analysis of Survey Outcomes

Comparison of outcomes for the entire treatment group with those of the entire control group provides an estimate of the average effect of the intervention on the entire treatment group, including nonparticipants (i.e., families that did not lease up). As discussed above, this is the so-called "intent to treat" (ITT) estimate. If not all members of the treatment group are exposed to the intervention, the average effect on the entire treatment group will be "diluted" by the presence of nonparticipants upon whom the intervention had little or no effect. The ITT estimate will therefore understate the effects on those who did participate — i.e., the effect of the "treatment on the treated" (TOT). Similarly, if some control group members are exposed to the intervention, the difference in outcomes between the overall treatment and control groups is less than it would otherwise have been, again reducing the estimated average effect on treatment group members.

Unfortunately, we cannot simply remove the nonparticipants and crossovers from the analysis sample. To do so would destroy the comparability of the two groups, because nonparticipants and crossovers are likely to be atypical of the overall group to which they were assigned. Fortunately, in some circumstances it is still possible to infer the TOT impact.

Bloom (1984 and 1993) has shown that under certain assumptions an unbiased estimate of the impact on treatment group members who were participants *and* who would not have been crossovers had they been controls (i.e., "non-crossover-like" participants) can be derived simply by dividing the estimated impact on the overall treatment group by p-c, where p is the participation rate (the proportion of the treatment group exposed to the intervention) and c is the crossover rate (the proportion of the control group exposed to the intervention). This procedure yields an unbiased estimate of the TOT impact under the following two assumptions:

1. The experimental treatment has no effect on nonparticipants (in the present case, those who did not lease up); and,
2. The effect of the treatment on crossovers is the same as it would have been had the same sample member been assigned to the treatment group.

This adjustment makes no assumptions about the similarity of participants and nonparticipants or of crossovers and uncontaminated controls. It simply attributes the entire impact on the overall treatment group to non-crossover-like participants, under the assumption that the treatment had no effect on nonparticipants and that the effects on crossovers in the control group are just offset by the effects on crossover-like participants in the treatment group. As noted above, however, the resulting estimate applies only to non-crossover-like participants; it is not possible to estimate impacts on nonparticipants and crossover-like participants.[19]

[19] Although we cannot identify the specific individuals who are "non-crossover-like participants" because we cannot identify which treatment group members are "crossover-like," this group *can* be described in terms of their observable characteristics by subtracting the distribution of characteristics of crossovers from the distribution of characteristics of participants in the treatment group.

The standard error of the TOT estimate is just the standard error of the ITT estimate times the same adjustment factor.[20] Because both the ITT estimate and its standard error are multiplied by the same factor in deriving the TOT estimate, the t-statistics of the two estimates are identical.

TOT estimates for survey outcomes are generated using a standard methodology.[21] We first obtain the ITT estimate from the multivariate model. In Equation 1.1, the estimated coefficient δ is the ITT estimate. Recognizing that some controls received vouchers, the ITT estimate can be viewed as the difference in the impact of voucher receipt on the treatment group as a whole (ITT_t) and the impact of voucher receipt on the control group as a whole (ITT_c):

Eq. 1.1 $ITT = ITT_t - ITT_c$

For both the treatment group and the control group, one can express the ITT estimate as a weighted average of the treatment effect on participants (the "treatment on treated" effect, represented by TOT_t and TOT_c) and the treatment effect on nonparticipants (represented by I_t and I_c). If p is the proportion of the treatment group who participated (the "participation rate"), and c is the proportion of the control group who participated (the "crossover rate"), then Equation 1.2 implies:

Eq. 1.2: $ITT = [p*TOT_t + (1-p)*I_t] - [c*TOT_c + (1-c)*I_c]$

If one assumes the treatment effect to be the same for those participating within either the treatment group or the control group ($TOT_t = TOT_c = TOT$), we then obtain:

Eq. 1.3: $TOT = [ITT - (1-p)*I_t + (1-c)*I_c] / (p-c)$

If one assumes that the treatment effect on nonparticipants is zero ($I_t = I_c = 0$), this reduces to:

Eq. 1.4: $TOT = ITT / (p-c)$

This derivation of the TOT estimate for survey outcomes is likely to overstate somewhat the true effect of the voucher, because voucher lease-up typically occurred later for control group crossovers (coming off of program waiting lists) than for treatment group participants (who received their vouchers very soon after random assignment). This would have allowed less time for the voucher's effect to materialize for the crossover cases, resulting in an inflation of the estimated TOT effect.[22] In the analysis of administrative data, in which we have continuous measures of the outcomes over the follow-up period, we can take account of the timing of lease-up in both groups (see the discussion below). For the point-in-time survey outcomes, this is not possible.

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[20] This statement treats p and c as fixed numbers. The standard error of the TOT estimate is somewhat larger if one takes the sampling error associated with p and c into account. However, in large samples (roughly those in excess of 1,000), the component of the standard error of estimate attributable to the sampling error of these rates is negligible (see Heckman, 1998).

[21] See Orr (1999).

[22] In this case, where control group crossovers lease up later in the follow-up period than treatment group participants, the 1/(p-c) TOT adjustment factor essentially *over-adjusts* for crossovers.

For each survey outcome, the TOT impact presented in this report is thus calculated by the "Bloom adjustment" methodology described above. The ITT point estimate and its standard error are both multiplied by the factor 1/(p-c). Separate adjustment factors have been computed for all survey respondents and for each specified subgroup of respondents, using the lease-up information through follow-up quarter 18, where the survey was conducted at follow-up intervals that ranged from 16 to 20 quarters.

Using data on the follow-up survey respondent sample for selected earnings outcomes and locational outcomes, we have conducted some analysis to assess the extent to which the above-described methodology indeed leads to an overstatement of the true TOT impact. For full respondent sample estimates at quarter 18, the adjustment factor of 1/(p-c) has a value of approximately 4.1. (For each survey outcome, the calculated TOT impact is thus 4.1 times the corresponding ITT impact.) Where both ITT and TOT impacts can be explicitly estimated from the available administrative data (including Census and address history data), TOT impacts are found to lie in the range of 3.3 to 3.9 times the corresponding ITT impact. The TOT estimates shown in this report should thus be regarded as provisional upper-bound approximations.

Adjustment for Nonparticipants and Crossovers in the Analysis of Administrative Outcomes

The standard adjustment for nonparticipation and crossover, although useful for constructing TOT impacts for the survey outcomes, thus does appear to overestimate those impacts because of the difference in timing of lease-up between the treatment and control groups. For administrative and address history outcomes, we can make use of additional information about the timing of lease-up to construct TOT estimates with greater precision.

The TOT impacts estimated in this report for nonsurvey outcomes can take advantage of the fact that we know exactly when each treatment or control group member leased up; we thus can make an econometric adjustment that controls for the amount of time any given control group member was, in fact, a crossover. Simply stated, the TOT estimates show the difference in outcomes between treatment group members who leased up and control group members who did not receive a voucher.

Assumption 2 of the standard Bloom adjustment, discussed above, states that the effect of the treatment on crossovers is the same as it would have been had the same sample member been assigned to the treatment group. In one sense, the intervention is virtually identical for participants in the treatment group and crossovers in the control group. Both received a voucher and leased up. Although the demonstration sites originally planned to provide services to treatment group members that would not be available to controls who received vouchers through the regular program, it appears that these services never materialized. However, treatment group members and controls received these vouchers at significantly different times. Thus, at any given number of months after random assignment, participants in the treatment group would have been leased up longer than crossovers. For example, if we were to estimate the impact of the treatment on some outcome 12 months after random assignment, nearly all of the participants in the treatment group would have leased up at least 6 months earlier, whereas only about a quarter of the crossovers would have been leased up that long. If, as one might expect, the effects of the voucher cumulate over time, one would not expect the voucher to have had the same effects *in month 12* on controls who had leased up with regular vouchers as on participants in the treatment group, who had leased up earlier.

Thus, the standard adjustment for crossovers can be improved upon in this case. Details regarding the timing of lease-up in the treatment and control groups and the improved strategy for adjusting for nonparticipation and crossover among outcomes that are measured at multiple points in time (the administrative outcomes) are provided in Appendix B.

Measuring Impacts

Measuring impacts involved estimating the following equation:

Eq. 1.5 $$O_t = \gamma_0 + \Sigma\gamma_k X_{kt} + \delta T_t + \nu_t \,,$$

where O_t is the outcome in period t, $\gamma_{0...k}$ are coefficients on the matrix X_t of covariates (including an intercept term), δ is the coefficient on the treatment term T_t, and ν_t is a random error term. The treatment term is a dichotomous variable taking the value 1 if a subject was randomly assigned to the treatment group, and 0 if she was not.[23] The ITT estimate in any equation, then, is just the coefficient δ.

We should note that in cases where the outcome of interest is a binary variable (with values of 0 or 1), the ITT impact is estimated using a probit model. Examples of binary outcomes include whether employed full-time; receipt of SSI; receipt of training in the follow-up period; and household income below the federal poverty threshold. More details about impact estimation with the probit model are provided in Appendix B.

The ITT estimates derived with Equation 1.1, along with information about lease-up rates, form the basis for the "treatment on the treated" (TOT) impact estimates that are also presented in the tables in Chapters 3, 4, 5, and 6.[24] The formulas for calculating the TOT estimates are presented in Appendix B.

The TOT estimates are based on the assumptions that: (a) the intervention had no effect on families who did not receive vouchers; and (b) in any given quarter after their receipt, vouchers had the same effect on controls that received them as on treatment group members who received them (see Appendix B for details). Estimation of the TOT impacts takes account of the fact that, in general, controls who received regular vouchers received them later than the treatment group members received WtW vouchers.[25] TOT estimates will generally be larger than the ITT estimate for the same outcome, because both nonparticipants and crossovers "dilute" the estimated impact – the former because the intervention can be expected to have little or no effect on those who do not receive it and the latter because

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[23] As we were using observation weighting to create a hypothetical 1:1 treatment-control ratio in the presence of changing random assignment ratios over time, the estimation process is more complicated than ordinary least-squares. Models were fit using Generalized Linear Modeling (GLM) and Huber's "sandwich" variance estimators, which produce consistent estimates of coefficient standard errors even with complex weighting.

[24] For a more detailed discussion, see Chapter 2 of Mills *et al*. (2003).

[25] The adjustment used to derive the TOT estimates is a generalization of the well-known "Bloom adjustment" (see Bloom (1984) and Bloom *et al*. (1993)), developed specifically for this study. The Bloom adjustment requires that crossovers receive the intervention at the same time as treatment group members; the method used here allows any time pattern of receipt of vouchers in both groups.

controls' receipt of the intervention reduces the treatment-control difference in outcomes. There is no predictable relationship between the statistical significance of the ITT estimates and that of the TOT estimates, however.

We included the following covariates, measured in the baseline survey, in every regression:

- income earned in the past year (earnings), earnings squared, and earnings cubed[26];
- whether the respondent was working at baseline;
- the respondent's reservation wage per hour,[27] a variable asked only of persons who were not working at baseline (categories: $3 to $5.99; $6 to $8.99; $9 to $12.99; $13 to $15.99; not asked if person was working);
- education variables (whether respondent was in school; whether respondent had a high school diploma; whether respondent had a GED);
- training variables (respondent was enrolled in a job training program; respondent was enrolled in a job training program but had not yet started training; respondent was not enrolled in a training program);
- race/ethnicity (respondent was White non-Hispanic; Black non-Hispanic; Hispanic; Other non-Hispanic; or missing, in mutually exclusive categories);
- gender (male, female, missing);
- whether the respondent had, at baseline, a car that ran, and whether the respondent had a current driver's license;
- whether the respondent was on TANF at baseline;
- whether the respondent had ever been a recipient of TANF/AFDC;
- for respondents on TANF at baseline, the amount of time until TANF benefits were due to expire (categories: within 6 months; 6 to 12 months; 12 to 18 months; more than 18 months);
- whether anyone in the respondent's household received food stamps, Supplemental Security Income (SSI), or Medicaid at baseline;
- whether the respondent was ever married;
- whether the respondent had any dependent children;
- age of the youngest person in the household (age categories: less than 6 years; 6 years or more but less than 18; 18 years or older);
- household size (categories: 1 person; 2 people; 3 people; 4 people; 5 people; 6 people; 7 people; 8 or more people);
- respondent's age, age squared, and age cubed[28];

[26] Squared and cubed terms for a respondent's baseline earnings and age are included as covariates to control for possible non-linear effects of earnings and age on outcome measures.

[27] The "reservation wage" is the lowest wage rate at which an individual will accept a job. Sample members were asked their reservation wage in the baseline survey.

- the ratio of monthly household rent payment to monthly household income;
- whether the respondent desired to move for employment reasons;
- respondent's baseline housing situation (categories: respondent rents or owns his/her own apartment or house; respondent is in public or other assisted housing; respondent lives with friends or relatives or in a homeless shelter or transitional housing)
- whether the respondent was a frequent mover (had moved more than three times in the past five years);
- site in which the respondent lived (Atlanta, Augusta, Fresno, Houston, Los Angeles, or Spokane); and
- the monthly Metropolitan Statistical Area (MSA)-level unemployment rate for the site where the respondent lived, averaged over the twelve months prior to the respondent's random assignment date.

In Chapter 6, additional baseline covariates were included for age and gender of focus children. In addition to this set of common covariates, the regressions for which results are presented in Chapters 3, 4, 5, and 6 also controlled for baseline values of the outcome variable. For example, in Chapter 3, which presents impacts on locational characteristics, all regressions included the baseline value of the particular outcome variable in question. In Chapter 4, which presents impacts on employment, earnings, income, and receipt of public assistance, all regressions dealing with employment and earnings included control variables for the total amount of UI earnings and the number of quarters employed in the four quarters prior to random assignment. Those regressions measuring impacts on TANF receipt contained a control variable for the number of quarters the respondent had received TANF in the four quarters prior to random assignment, and those measuring impacts on food stamps receipt contained a control variable for the number of quarters the respondent had received food stamps in the four quarters prior to random assignment. Some of these baseline values of the administrative outcome variables were highly correlated with certain survey questions. Nevertheless, they were not perfectly collinear and provided valuable data from the same source as the outcome variables.

Subgroup Impacts

The treatment might be expected to affect some groups of participants differently, as barriers to relocation or lease-up, search costs, and other factors that might affect the outcomes of interest vary among individuals with different circumstances. For example, it is possible that participants with young children may be likely to lease in place and to use their increased income to spend more time at home with their families. In contrast, participants with older children may find it easier to use their voucher to move to a better neighborhood, and perhaps to seek improved employment opportunities. On the other hand, families with school age children might want to avoid the disruption associated with changing schools and thus could be less likely to move. Either way, it seems likely that the treatment might affect these two groups differently. As a result, it is helpful to examine the effects of program participation on these two groups

[28] Squared and cubed terms for a respondent's baseline earnings and age are included as covariates to control for possible non-linear effects of earnings and age on outcome measures.

separately. Groups defined by characteristics that we think might influence how participants respond to the program are called subgroups. Subgroups have been defined with respect to baseline characteristics such as ethnicity, the age of children, whether respondents indicated that they desired to move at baseline, and baseline levels of earnings, education, and reservation wages. Subgroup impacts are obtained simply by estimating the regression equations (Equation 1.1 and the TOT estimates) on data restricted to respondents who are in a particular subgroup. In Chapters 3, 4, 5, and 6, we present treatment impacts (both ITT and TOT) for a set of subgroups that were expected to have varying sensitivity to the treatment.

It should be noted that some of our subgroups are quite small; because statistical precision is a function in part of sample size, it is possible that a subgroup could experience genuine program impacts, yet the estimated impacts for that subgroup will not be statistically significant. Based on our assumptions about the expected size of treatment effects, the extent of non-participation among treatment group members, and the extent of cross-over among controls, we have made estimates of the Minimum Detectable Effects that could be identified for subgroups of different sizes, assuming standard levels of statistical power. These prior estimates indicated that, for a subgroup that contained at least one quarter of the sample, we could expect to detect a statistically significant impact on earnings (for example) if the true treatment impact was at least 20 percent. Smaller impacts for a subgroup of this size would not be detected as statistically significant. For a subgroup that was smaller than one quarter of the sample, impacts on earnings would have to be *even larger* than 20 percent in order to be detected as statistically significant. In fact, as can be seen below in Exhibit 1.2, several of our subgroups do make up fairly small fractions of the total sample.

Because lack of statistical significance may reflect small sample size rather than an absence of true impacts, we are careful throughout this report to interpret the subgroup impacts accordingly. While statistically significant impacts for a given subgroup can be taken as evidence that that subgroup *did* experience program impacts, the reverse is not necessarily true. When we find statistically *insignificant* impacts for particular subgroups—especially for the smaller subgroups—we do not take this finding as definitive evidence that the subgroup was *unaffected* by the treatment. Rather, we interpret this finding as simply indicating that this study sample provides no definitive evidence that the subgroup experienced program impacts.

1.4 Baseline Characteristics of the Research Sample

A total of 8,731 individuals were randomly assigned across the six evaluation sites. Complete baseline survey data were obtained for 8,573. The baseline characteristics of those randomly assigned are shown in Exhibit 1.2. The sample is predominantly female, never married, and between the ages of 18 and 44, with the average age 30.7. Nearly half of the sample is African American, while 21 percent are Hispanic and 20 percent are Caucasian. The majority of sample members either graduated from high school (40 percent) or had a GED (17 percent), and at the time of random assignment 16 percent were enrolled in some type of school or training program. The average household size was 4 persons.

Exhibit 1.2
Baseline Characteristics of Research Sample

Characteristic	All Sample Members Combined (N=8,573)
Study Site	
Atlanta	12.9%
Augusta	8.8
Fresno	29.9
Houston	23.0
Los Angeles	12.1
Spokane	13.3
Gender	
Male	7.7%
Female	91.8
Missing[29]	0.5
Marital Status	
Never married	54.0%
Married	16.5
Separated/Divorced	23.3
Widowed	1.3
Missing	5.0
Age at Random Assignment	
<18	0.3%
18-24	30.2
25-34	38.1
35-44	23.4
45-54	6.9
55+	1.1
Mean age	30.7
Race/ethnicity	
White, non-Hispanic	19.6%
Black, non-Hispanic	49.8
Hispanic	21.4
Other	8.2
Missing	1.0
Educational Attainment	
HS Graduate	39.7%
GED	17.2
Neither HS Diploma Nor GED	35.4
Missing	7.6
Enrolled in School at Baseline	
Yes	16.4%
No	79.1
Missing	4.5

[29] Items reported in this exhibit come from the baseline interview, which was a self-administered interview that individuals completed at the time of random assignment. Gender was left missing on the baseline interview forms for .5 percent of the study sample.

Exhibit 1.2 *(Continued)*
Baseline Characteristics of WtWV Research Sample

Characteristic	All Sample Members Combined (N=8,573)
Average size of household	4.0
Employment status at baseline	
Working for pay:	
Yes	44.5%
No	51.9
Missing	3.6
Not working, looking for work:	
Yes	54.4%
No	38.2
Missing	7.4
Working for TANF benefits:	
Yes	11.8%
No	80.3
Missing	8.0
Attending school:	
Yes	16.8%
No	75.2
Missing	8.0
Keeping house/caring for children:	
Yes	54.4%
No	38.2
Missing	7.4
Doing something else:	
Yes	7.8%
No	83.4
Missing	8.8
Average hourly wage[30]	$6.96
Ever worked for pay?	
Yes	80.3%
No	19.3
Missing	0.5

[30] A total of 3,375 survey respondents reported their wages on a per hour basis. Among the remaining respondents, 16 reported their earnings at their job on a per day basis, 85 reported their earnings on a per week basis, 102 reported their earnings every two weeks, 99 reported their earnings per month, and 3 reported their yearly earnings. Respondents were also asked to specify the number of hours that they usually work in a typical week so that their hourly wages could be determined. To compute the hourly wages of respondents who reported their earnings on a per day basis, it was assumed that they work 5 days per week. It was also assumed that respondents who reported their earnings on a yearly basis worked 52 weeks per year. Valid hourly wages were computed for 14 respondents who were paid on a per day basis, 80 respondents who reported their earnings on a per week basis, 97 respondents who were paid every two weeks, 88 respondents who reported monthly earnings, and 3 respondents who reported their earnings on a per year basis.

Exhibit 1.2 *(Continued)*
Baseline Characteristics of WtWV Research Sample

Characteristic	All Sample Members Combined (N=8,573)
Type of housing at baseline (self reported)	
Rent apartment or house	56.3%
Own apartment or house	0.7
Living with friends or relatives	25.8
Public housing	7.0
Assisted housing	5.7
Homeless shelter or transitional housing	1.9
Other	1.5
Don't know	0.0
Missing	1.0
Average monthly rent at baseline	$314.43
Desire to move at baseline?	
Yes	88.0%
No	11.9
Don't know	0.0
Missing	0.2
Satisfaction with neighborhood at baseline	
Very satisfied	16.8%
Somewhat satisfied	22.8
Neither satisfied nor dissatisfied	27.7
Somewhat dissatisfied	15.7
Very dissatisfied	16.4
Missing	0.7
Transportation	
Valid driver's license:	
Yes	60.2%
No	39.5
Don't know	0.0
Missing	0.3
Access to a car that runs:	
Yes	40.8
No	58.9
Missing	0.4
Childcare responsibilities	
Have responsibility for children in the home:	
Yes	90.1%
No[a]	9.5
Missing	0.4

Exhibit 1.2 *(Continued)*
Baseline Characteristics of WtWV Research Sample

Characteristic	All Sample Members Combined (N=8,573)
Receipt of TANF benefits at baseline	
Receiving TANF at baseline:	
Yes	80.3%
No	19.6
Missing	0.2
Other sources of household income at baseline	
Food stamps:	
Yes	85.7%
No	13.8
Missing	0.5
SSI:	
Yes	11.2
No	85.4
Missing	3.5
Child support:	
Yes	16.0
No	80.5
Missing	3.5
WIC:	
Yes	39.0
No	58.3
Missing	2.8
Unemployment compensation:	
Yes	2.5
No	93.4
Missing	4.1
Social Security disability or survivor's benefits:	
Yes	6.1
No	90.1
Missing	3.8

Source: Baseline Survey
Note: a. Either no children in the home or others have principal responsibility for children in the home.

With respect to employment experiences, 44 percent said that they were working for pay at baseline, and another 12 percent were working for TANF benefits. Among those working at the time of baseline, the average hourly wage was $6.96. Most individuals had some work experience in the past, with 80 percent reporting having worked at some time for pay.

The baseline survey also collected information about housing arrangements at baseline. Survey respondents reported spending $314 per month for rent on average, and a substantial majority (88 percent) said that they wanted to move at the time of random assignment. The most common type of housing arrangement was renting an unsubsidized apartment or house (56 percent), followed by living with friends or relatives (26 percent). Altogether nearly 13 percent reported receiving some type of housing assistance at baseline (public housing or project-based assisted housing), and 2 percent reported living in a homeless shelter or transitional housing. Approximately one-third of the sample members reported being somewhat or very dissatisfied with their neighborhoods at the time of random assignment,

most likely reflecting the large proportion who expressed a desire to move. Seventeen percent said they were very satisfied with their baseline neighborhood, 23 percent were somewhat satisfied, and 28 percent were neither satisfied nor dissatisfied.

The baseline survey also asked questions about transportation and childcare responsibilities, which are potential barriers to employment. Well over half of all respondents (60 percent) reported having a valid driver's license, but only 41 percent said that they had access to a car that runs, suggesting that transportation issues could limit employment opportunities for some sample members. A full 90 percent of sample members reported having responsibility for children in the home, an indication that access to childcare could be an important factor influencing employment experiences.

Given the targeting criteria for the WtWV program, it is not surprising that the overwhelming majority (80 percent) reported receiving TANF cash assistance at the time of baseline. Other sources of household income included food stamps (received by 85 percent), SSI (12 percent), child support (16 percent), and Supplemental Feeding Program for Women, Infants, and Children (WIC, 39 percent). Unemployment compensation (3 percent) and social security benefits (6 percent) were less common sources of household income.

1.5 Organization of the Report

The remainder of the report is organized as follows. Chapter 2 describes the implementation of the WtWV program in the evaluation sites and the extent to which sample members were successful in using vouchers to lease a housing unit over the follow-up period from random assignment through December 2004. Chapter 3 presents the results of the analysis of impacts of receiving a voucher on mobility, the characteristics of program participants' neighborhood, neighborhood satisfaction, street conditions, crime victimization, and also presents descriptive analysis of the reasons for leaving the voucher program for those who stopped receiving housing assistance at some point during the follow-up period. Chapter 3 also presents results on the effects of vouchers on household composition. Chapter 4 presents the findings of the analysis of impacts on employment, earnings, education and training, receipt of TANF and food stamps, using both administrative and survey data. Chapter 5 describes the analysis of survey-measured outcomes on household income, housing security (rent burden and housing unit quality), and health status of adults and children. In Chapter 6, we analyze impacts of receiving a voucher on child well-being using information from the follow-up survey and the parent on child/youth module of the follow-up survey on such topics as household composition, school performance and attendance, child behavior and time use, and parental involvement. Chapter 7 presents conclusions. The report is also supplemented with several technical appendices. Appendix A discusses data sources and construction of datasets, and Appendix B presents detailed information on the construction of the survey sample and analytic methods used to estimate impacts. In Appendix C we present an analysis of tests of survey nonresponse bias. Finally, Appendices D, E, F, and G present additional detailed estimates to accompany Chapters 3, 4, 5, and 6.

Chapter Two
Implementation of the Welfare to Work Voucher Program

This chapter presents background information about the sites participating in the Effects of Housing Choice Voucher Program on Welfare Families study at the time of participant enrollment. The chapter also provides details about the implementation of the WtW voucher program in each of the sites. This information was taken from the 2003 Report to Congress and updated to reflect new information. It provides a comprehensive review of the program as background to the empirical results presented in later chapters.

The information on program implementation is useful not only for understanding how the program was operated in the evaluation sites, but also to define the intervention that was tested in the evaluation. Based on our assessment of program operations in the evaluation sites, we have concluded that the intervention in place during the early period following random assignment and tested by the evaluation is essentially the receipt of the voucher itself, since specialized services were not systematically offered to WtWV participants. The chapter concludes with an assessment of the extent to which participants in the WtWV program were successful in using their voucher to lease a housing unit and also the extent to which individuals assigned to the control group received and used a rental assistance voucher over the course of the follow-up period.

2.1 Characteristics of the Study Sites

The six study sites represent a range of geographic locations and housing and labor market characteristics. To provide context for the impact analysis, Exhibit 2.1 shows select demographic, economic, and housing market data for each of the sites, as of the time the evaluation began in 2000, as well as annual unemployment rates during the follow-up period. The information is taken from the 2000 Census, Bureau of Labor Statistics, and TANF program data. The information is provided for the jurisdiction served by the WtWV program in each of the study sites. In all except one site (Fresno), the program jurisdiction is the corresponding city. In Fresno, the program serves the city and county of Fresno. In Spokane, the housing authority's jurisdiction includes the city of Spokane and three surrounding rural counties, but the agency focused its WtWV program on the city of Spokane.

The six study sites include two of the largest U.S. cities (Los Angeles and Houston), with 2000 populations ranging from 2.0 million (Houston) to nearly 3.7 million (Los Angeles). The other four sites are metropolitan areas, with the Fresno site serving an area with 922,516 persons and Atlanta with a 2000 population of 416,474. Augusta and Spokane are medium-sized cities with populations of 310,294 and 195,629. The exhibit also shows how the six sites compared to one another and to the country overall in terms of poverty rates and median incomes at baseline. All of the six sites had 2000 poverty rates greater than the national rate

Exhibit 2.1
Characteristics of Study Sites at Baseline

Characteristic	Atlanta	Augusta	Fresno	Houston	Los Angeles	Spokane	U.S.
City characteristic in 2000:							
Total population	416,474	310,294	922,516	1,953,631	3,694,820	195,629	281,421,906
Percent of pop. in poverty	24.4%	15.2%	22.7%	19.2%	22.1%	15.9%	12.4%
Median household income	$34,770	$38,436	$34,960	$36,616	$36,687	$32,273	$41,994
Homeownership rate	43.7%	65.6%	57.7%	45.8%	38.6%	58.8%	66.2%
Homeowner vacancy rate	4.1%	2.6%	1.6%	1.6%	1.8%	2.4%	1.7%
Rental vacancy rate	7.2%	10.4%	5.4%	8.7%	3.5%	9.4%	6.8%
Unemployment rate	9.0	NA	7.1	4.8	5.6	5.7	3.7
Unemployment rate for the Metropolitan Statistical Area (MSA)[a]							
2000	3.1	3.9	10.4	4.3	--	5.2	4.0
2001	3.6	4.6	10.7	4.7	5.3	6.6	4.7
2002	4.9	5.0	11.5	6.0	6.4	7.6	5.8
2003	4.8	5.1	11.7	6.8	6.5	7.5	6.0
2004	4.6	5.4	10.4	6.3	6.0	6.5	5.5
Maximum monthly TANF benefit for a family of 3 (adult and 2 children) in 2000	$280	$280	$626	$201	$626	$546	--

Source: 2000 Census for items other than MSA unemployment rates and monthly TANF benefit levels. Metropolitan Statistical Area (MSA) unemployment rates are annual rates from Bureau of Labor Statistics Local Area Information. Monthly TANF benefits levels are from the U.S. House of Representatives, Committee on Ways and Means, "Background Material and Data on Programs Within the Jurisdiction of the Committee on Ways and Means: The 2000 Greenbook."

[a]For all sites except Los Angeles, random assignment took place in 2000. In Los Angeles, random assignment was conducted in 2001.

of 12.4 percent. The highest poverty rate was in Atlanta (24.4 percent) and the lowest was in Augusta (15.2 percent). Median household income for the nation overall ($41,994) was higher in 2000 than in any of the study sites. Among the study sites, median household income ranged from $38,436 (Augusta) to $32,273 (Spokane). Similar median household incomes were observed in Atlanta and Fresno (nearly $35,000) and in Houston and Los Angeles (nearly $37,000). Homeownership rates varied substantially across the study sites, from a low of 38.6 percent in Los Angeles to a high of 65.6 percent in Augusta, all below the national rate of 66.2 percent.

An important housing market characteristic for a voucher study is the rental vacancy rate, because it is an indicator of how difficult it might be for families to use their vouchers to rent a housing unit. Areas with lower vacancy rates are considered more difficult housing markets for voucher recipients because there are fewer rental units available and landlords may be less inclined to consider leasing to voucher holders. Nationwide, the 2000 Census reported a rental vacancy rate of 6.8 percent. Among the evaluation sites, we found substantial variation. Los Angeles had the tightest rental market, with a rental vacancy rate of 3.5 percent according to the 2000 Census. In contrast, the looser rental markets in Augusta (10.4 percent vacancy), Spokane (9.4 percent), and Houston (8.7 percent) would appear to offer greater options for voucher recipients. Updates to the 2000 Census are not yet available for cities and metropolitan areas, but nationally the rental vacancy rate has risen over the period of the study, with third quarter 2005 rental vacancy rates estimated at 9.9 percent.[31] Local estimates for rental vacancy rates that are more recent than 2000 Census are available for some locations from economists working in local HUD offices who gather data on local housing market conditions. Where such information is available, it is discussed below.

Although the Census provides an important and uniform measure of rental vacancy across the sites, information about particular segments of the market can be even more illuminating.[32] In 2003 and again in 2006, we conducted interviews with local housing authority staff and HUD Field Office staff to obtain more detailed information about conditions in the local rental housing markets. This information confirmed the overall trends observed in the Census data—that is, Los Angeles had the most difficult or tightest rental market, while Augusta, Spokane, and Houston had the loosest markets. In Atlanta, a market study completed in early 2003 concluded that rental market conditions had softened substantially since the 2000 Census as a result of record rates of rental housing construction and slowing demand for housing. Local HUD staff estimated that rental vacancy rates were

[31] U.S. Census Bureau, Housing Vacancies and Homeownership (CPS/HVS). Third Quarter 2005 data.

[32] Local fair market rents (FMR) are set by HUD each year to approximate the average rent at the middle of the market. (In most markets, the FMR is set at the 40th percentile of local rents.) Housing authorities establish a voucher payment standard that is typically between 90 and 110 percent of the FMR. The voucher payment standard establishes the upper bound on the amount of rental subsidy that a program participant can receive. The rental subsidy is the lesser of the payment standard or the actual rent of the unit minus 30 percent of the family's adjusted monthly income. As a result, voucher recipient families can seek housing units at the middle of the housing market. Studies that provide details on vacancy rates in various segments of the rental market thus give more precise information about the tightness of the rental market faced by voucher recipients.

more than 10 percent in the early period of the decade.[33] Since then, new housing development has slowed and vacancy rates have decreased, with fourth quarter 2005 rental vacancy rates estimated at 6.2 percent. (This information for Atlanta is not specific to the affordable end of the rental market.)

A detailed analysis of the rental housing market in Houston in late 2002 revealed that vacancy rates among more affordable units were lower than for the rental market overall (5 percent compared with 8.7 percent at the time of the 2000 Census).[34] Overall, the rental market in Houston has remained soft over the follow-up period, with vacancy rates increasing in 2004. In 2005, many evacuees from Hurricane Katrina were relocated to Houston following the storm, and this resulted in a drop in rental vacancy rate from 12 percent in August 2005 to 4 percent in September 2005. By December, however, the rate had risen to 8 percent.

In Augusta, the rental market appears to have tightened over the follow-up period, with local HUD staff estimating a rental vacancy rate of 6 percent as of the fourth quarter 2005. In Spokane, rental vacancies are estimated to have dropped since 2000 based on an apartment survey conducted in spring 2005 by the Washington Center for Real Estate Research at Washington State University. Similarly, in Fresno, housing authority staff reported that the rental market tightened substantially over the course of the study period. In Los Angeles, rental market conditions remained difficult for voucher recipients, with low vacancy rates continuing over the course of the follow-up period.

Labor market characteristics of the study sites offer additional context for understanding the effects of the voucher program. Areas with higher rates of unemployment indicate more difficult labor markets with fewer job opportunities. Enrollment in the evaluation occurred in 2000 in all sites except Los Angeles, where enrollment took place in 2001. Most of the metropolitan areas covered by the evaluation sites (4 of the 6) experienced higher rates of unemployment than the nation during the year of program enrollment. The largest difference was in Fresno, which had an annual unemployment rate of 10.4 percent in 2000 compared with the national average of 4.0 percent. Atlanta and Augusta had lower unemployment rates in the year of enrollment than the nation overall. Over the course of the follow-up period, a period coinciding with national recession, unemployment rates rose in each of the metropolitan areas and nationwide. In general, though, both Atlanta and Augusta had unemployment rates lower than the national average, and the other sites experienced higher unemployment rates than for the nation as a whole over the period 2000 through 2005.

To be eligible to receive a voucher through the WtWV program, applicants had to be either current or former TANF recipients or currently eligible to receive TANF benefits. Exhibit 2.1 shows the maximum monthly TANF benefits for a family of 3 (1 adult and 2 children) in 2000. The highest TANF benefit levels were in Fresno and Los Angeles ($626), while the lowest benefits were in Houston ($201). As of 2004, maximum monthly benefit levels for a

[33] U.S. Department of Housing and Urban Development, Georgia State Office. January 16, 2003.

[34] Information was reported by HUD Field Office staff in Houston based on analysis conducted by O'Connor and Associates.

family of three had remained the same in all sites except Fresno and Los Angeles, where maximum monthly benefits for a family of three had increased to $679.

2.2 Implementation of the WtWV Program

The WtWV program envisioned by the Congress in the statute, by HUD in its implementing regulations, and by the sites in their funding applications, called for a two-part effort to provide housing assistance geared to promoting the self-sufficiency of welfare recipients. First, the program was to target housing vouchers to welfare recipients whose efforts to achieve self-sufficiency would benefit from housing assistance. Second, the program was to deliver housing- and employment-related program services to enhance the effectiveness of the voucher. Both components of this effort were to involve new partnering arrangements between housing authorities and TANF agencies, plus a coupling of housing- and employment-related program services with the WtW voucher.

In this section, we describe how the WtWV program was implemented at the study sites to assess the extent to which the original vision was achieved. The description helps us to understand the nature of the intervention at the evaluation sites and the extent to which the WtWV program provided services above and beyond those available to voucher recipients in the regular HCV program. The section focuses on: the degree to which WtWV program operations are conducted separately from the HCV program; organization and staffing of the program; partnerships established with the local TANF agency and other organizations; and services provided to voucher recipients to assist them in locating suitable housing and in obtaining and retaining employment. The information presented here is taken from interviews with WtWV staff in the six housing agencies, local partners, and local TANF agencies. Interviews were conducted by telephone in September 2000, in person during October-December 2001, and again in person in February-March 2003. The study design has not called for additional site visits to the evaluation sites since 2003. We have conducted periodic telephone conversations with representatives of the evaluation sites to update information on the program and to discuss the follow-up survey data collection activities that were conducted in the study sites.

Information gathered from discussions with program staff in the six evaluation sites indicates that the HAs have achieved only part of the original program vision. It appears that interagency partnering (between the PHA and TANF agency) was limited and, for the most part, focused mostly on enrollment of program participants rather than on services.[35] Local TANF agencies were involved in enrollment to ensure that applicants satisfied the TANF requirements of the WtWV program. As a result, although vouchers have been targeted to eligible welfare families, there was little effort to select those families for whom housing would be particularly important for working or increasing earnings. Furthermore, except in Fresno, at a later stage of the WtWV program, households did not receive program services

[35] As described in the sections that follow, the Fresno site is an exception to the general pattern.

beyond those available to TANF (or TANF-eligible) families that receive regular HCV assistance. [36]

In addition, in Atlanta, voucher recipients in the WtWV program were subject to requirements not in place in the regular voucher program. This program prohibited using the WtW voucher outside the Atlanta city jurisdiction and also required participants to maintain 25 hours per week of employment or to participate in training.

Separateness of the WtWV Program from the HCV Program

The six study sites reflect a variety of HA types and sizes. The Housing Choice Voucher programs ranged from about 3,800 vouchers in Augusta to more than 43,000 vouchers in Los Angeles, based on information collected during the 2003 site visits. As of January 2006, the voucher program in Augusta had decreased to 3,472 and was still the smallest voucher program among evaluation sites. The Los Angeles program remained the largest in the study, and the voucher program had increased to 44,731 (based on information available in the HA profile for this agency available on HUD's website).[37]

The WtWV programs ranged from 450 vouchers in Atlanta to 1,400 in Fresno.[38] During the 2003 site visits we gathered information about how the operations of the WtWV program compared to the regular HCV program during the time WtWV operated as a separate program (until 2004). This information is used to assess the extent to which the WtWV program was implemented as a program distinct from HCV and, in turn, whether the program experiences of WtWV participants were likely to vary from those in the regular HCV program. Exhibit 2.2 displays several aspects of program operation. When the WtWV program was phased out in 2004, any vouchers that became available through turnover were incorporated into the regular voucher program, so that over time the distinctions noted in the exhibit ceased to exist. We confirmed with PHA staff that, after HUD announced the phase out of the WtWV program as a separate program, they simply folded WtW vouchers that became available through turnover into the regular HCV program. They also discontinued eligibility determination based on the TANF participation requirements associated with the WtWV.

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[36] Our findings with respect to the limited specialized service provision associated with the WtWV program are consistent with information gathered from HUD's Technical Assistance (TA) providers for the WtWV program, who provided TA to HAs from 2000 through 2004. The TA providers completed a report in 2002 on the operations of the WtWV program overall. The evaluation sites appear to have operated their programs in a similar manner to the other WtWV sites. See Quadel Consulting Corporation (2002).

[37] HA profiles, available at www.hud.gov/pic/haprofiles, are a module of HUD's PIC system that can be accessed by the public and provide general information about a housing agency including fiscal year, and current size of the public housing and HCV programs.

[38] In Fresno, the program is being administered jointly by the Housing Authorities of the City and County of Fresno. The two agencies are staffed jointly, and each entity was awarded 700 WtW vouchers. The total voucher program for these agencies' jointly operated program was 11,873 as of January 2006.

Exhibit 2.2
Extent to which WtWV Program Operated Separately from HCV

	Atlanta	Augusta	Fresno	Houston	Los Angeles	Spokane
Used separate waiting lists for HCV and WtWV	X		X			X
Separate briefings held for WtWV participants			X		X	X
Different procedures used in recertification for WtWV program	X	X				X
Separate staffing unit established for WtWV program	X		X		X	

Source: Interviews with Housing Authority staff.

Exhibit 2.3
Role Played by Local TANF Agency in WtWV Program

	Atlanta	Augusta	Fresno	Houston	Los Angeles	Spokane
TANF agency's role limited to providing referrals	X	X		X	X	X
TANF agency's role diminished over time	X	X		X	X	X

Source: Interviews with Housing Authority staff

In selecting families to receive a WtW voucher, housing authorities were required to draw TANF and TANF-eligible program participants from their existing HCV waiting lists. If no WtW-eligible families were on the waiting list, the list could be opened to admit new families. Although not required, three of the evaluation sites (Atlanta, Fresno, and Spokane) maintained separate waiting lists for the WtWV and HCV programs. The WtWV waiting lists in these sites included only those families who satisfied the TANF eligibility requirements at the time they were placed on the waiting list. Maintaining separate waiting lists might have helped to ensure that the WtWV eligibility criteria were correctly applied when WtW vouchers became available for re-issuance, since the TANF criteria would have already been verified for these families. In Los Angeles, where a combined waiting list was maintained for HCV and WtWV, families that were referred to the housing authority's waiting list by the TANF agency were flagged in the waiting list and contacted when a WtW voucher became available. In Augusta, the agency also maintained a combined waiting list.

When WtW vouchers were available for reissuance, the housing authority sent lists of families from the waiting list (in order of the date/time of their application) to the TANF agency for verification of their TANF status.

As with the HCV program, housing authorities were required to give families who were issued a WtW voucher an oral briefing[39] with the following types of information: how the voucher program works; family and owner responsibilities; and where the family may lease a unit, including a dwelling unit outside the PHA jurisdiction (as noted earlier, in Atlanta program participants were restricted to using the voucher within the AHA jurisdiction). In addition, if a family currently lived in a high poverty census tract, the briefing was required to explain the advantages of moving to an area that does not have a high concentration of poverty. For WtW voucher recipients, the briefing also had to include a description of any additional obligations of a WtWV recipient imposed by the HA and an explanation that failure to meet these obligations would be grounds for denial of admission or termination of assistance. The work requirement imposed by the Atlanta program is an example of such a requirement. We collected information from the evaluation sites about how the briefings were conducted for the WtWV program. In Fresno, Los Angeles, Houston, and Spokane, housing authority staff held briefings for WtWV participants apart from HCV recipients.[40] In Fresno and Los Angeles, WtWV staff conducted the briefing, while in Spokane regular voucher program staff conducted the briefing. In Fresno, the briefing for WtWV participants was longer than for the regular program and included a more detailed discussion of the Family Self-Sufficiency (FSS) program. Participation in FSS was a requirement for WtWV program participants at that site.[41] The Fresno briefing also provided more focus on housing search and the kinds of assistance available to voucher recipients during the housing search process than in the briefing for the regular voucher program. In Los Angeles, similarly, the WtWV briefing contained a more lengthy discussion of the housing search process than the regular HCV briefing, as well as more details about the resources available to voucher recipients during housing search. In Spokane, WtWV recipients were required to complete an individual action plan that specified employment goals and planned actions towards reaching the goals. Participants could replace this with the individual responsibility plan that was required by the TANF agency. In Atlanta and Augusta, WtWV participants received their briefing along with voucher recipients in the HCV program. In Atlanta, a special programs counselor met separately with each participant to explain the special work requirements associated with the WtW voucher and the limits on portability. Participants in Atlanta signed a family obligations agreement to confirm their understanding of these requirements. The content of the briefings was the same for WtWV and HCV voucher recipients in these sites.

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[39] Requirements of the briefing are located at 24CFR 982.301.

[40] In Houston, families who were randomly assigned and are included in the evaluation received large group briefings at the time of random assignment. This briefing was held separately from the regular HCV briefing. Subsequently, new WtWV participants who joined the program after random assignment was complete received a briefing together with HCV voucher recipients.

[41] As described later, FSS helps participants in the voucher program and residents of public housing to become self-sufficient through education, training, case management, and other supportive services.

As in the regular HCV program, HAs were required to reexamine the income and family composition of WtWV families at least annually. The purpose of this annual reexamination was to ensure that the family continued to meet the eligibility requirements of the program and to update the income information that forms part of the basis for the rental subsidy calculation. In Fresno, Los Angeles, and Houston, the same procedures were used in the WtWV and HCV programs for conducting the recertifications. In the other sites, the annual reexaminations were more involved for WtWV participants than for other HCV participants. In Atlanta and Spokane, for example, housing authority staff reviewed the family's compliance with the local TANF program requirements if the family was receiving TANF benefits. Also, in both of these sites, the staff discussed the family's current employment situation and employment goals. In Augusta, a subset of the WtWV participants received a more extensive recertification from a case manager who worked only with 150 WtWV participants. The case manager reviewed the family's employment action plan and identified whether the family needed any specialized employment or supportive services to achieve the plan. The case manager contacted each WtWV participant every 60 days throughout the year, usually by phone, and also used the reexamination meeting as an opportunity to discuss these issues in person.

Organization and Staffing

We also collected information about the staffing arrangements used to operate the WtWV program. Operating the WtWV program required all the functions performed in the HCV program including participant outreach and intake, eligibility determination, voucher issuance, voucher briefings, Housing Quality Standard inspections, and annual recertifications. Beyond these basic tasks, however, some agencies developed special activities or functions associated with the WtWV program. In this section we explore the staffing arrangements in place at the six evaluation sites and the implications of these arrangements for providing employment-related services to participants.

We found that staffing arrangements varied according to the level of integration between WtWV staff and regular HCV program staff. In Spokane, staff from the regular voucher program performed functions both for the WtWV program and the HCV program, with little specialization of WtWV functions. The entire caseload, regardless of type of voucher, was allocated among approximately 10 staff persons who perform intake, briefings, issuance, and recertifications.

A second approach to staffing involved designation of one or more staff to work on some aspects of the WtWV program, with other functions performed by regular voucher staff. In Houston, the same staff assigned to the regular voucher program also conducted most WtWV activities. However, those WtWV participants who enrolled in the Family Self Sufficiency Program were assigned to a case manager who worked only with WtWV participants enrolled in FSS through a special programs division. FSS helps participants in the rental voucher program to become self-sufficient through employment-related services, case management, and the ability to build savings through an interest bearing escrow account that is funded based on increases in earned income. This staff person monitored the client's employment and education goals and made referrals as needed to outside service providers. This approach was also used in Atlanta and Augusta. In Augusta, a WtWV case manager

was responsible for providing case management services to a group of WtWV participants, while intake, eligibility determination, and recertification were conducted by staff from the regular voucher program. In Atlanta, a WtWV coordinator was assigned to supervise the initial intake and lease-up for the WtWV program and to develop procedures for conducting recertification and service provision. The WtWV coordinator monitored the progress of all WtWV clients and referred the clients to agency case managers who could then refer clients to appropriate services. In the two Georgia sites, although staff that also worked on the regular program performed some functions associated with operating the WtWV program, the designation of WtWV staff offered the opportunity to provide specialized services to voucher recipients.

In Los Angeles and Fresno, the WtWV program was staffed separately from the regular voucher program. Special units or divisions of staff were designated in these agencies to administer the WtWV program. These "special programs divisions" were responsible for administering other types of special vouchers. All functions associated with the WtWV program, including intake, eligibility determination, recertification, and service provision were conducted through the separate division. In Fresno, this arrangement offered a mechanism for enforcing the required participation in FSS that was unique to the WtWV program. In Los Angeles, despite the separate staffing unit, few specialized employment services were offered to WtWV recipients.

Use of Partnerships to Operate the WtWV Program

The Notice of Funding Availability (NOFA) announcing the WtWV program required that housing authorities develop a program in consultation with the State or local entity administering the TANF program and the entity administering the Department of Labor's Welfare-to-Work grants.[42] The NOFA also stated that the rental assistance provided to WtWV participants should be coordinated with other welfare reform and welfare-to-work initiatives. Overall, we found that the role played by the TANF agency in the evaluation sites was limited to providing referrals to the WtWV program and assisting HA staff to determine whether families met the TANF eligibility criteria. As the initial lease-up period was completed, the role played by TANF diminished. By early 2003, housing authority and TANF staff in Los Angeles, Houston, and Spokane described the level of interaction between the two organizations as quite limited. In Augusta and Atlanta, TANF staff continued to provide referrals to the housing authority and help the housing authority verify the status of program applicants in the TANF program. Such referral activity diminished, however, as the number of WtW vouchers available for issuance declined over time. When the program was phased out in 2004, this referral was no longer needed at all.

In Fresno, the TANF agency played a key role in the WtWV program throughout its operation. The TANF agency was involved in the initial lease-up period, providing referrals of potential participants, assistance in verifying program eligibility, and automated matches between the voucher waiting list and current TANF caseload. In addition, after the initial lease-up was completed, the TANF agency continued to work closely with the housing

[42] The Notice of Funding Availability was published in the Federal Register January 28, 1999.

authority. In December 2001, FHA entered into a formal contract with the Fresno County Human Services System's Employment and Training Assistance Department (E&TA) that provided funding for five Family Self-Sufficiency staff. In addition, E&TA agreed to locate one of its own staff, a job specialist, in FHA's offices. This contract was renewed in December 2002, although the level of funding was reduced.

Housing Search Services Offered to WtWV Participants

Another indication of the extent to which WtWV was implemented distinctly from the regular HCV program was in the type of housing search assistance offered. In general, we found that little in the way of specialized housing search assistance was provided to the WtWV program participants included in the evaluation sample. Most of the sites offered WtWV participants the same services for finding housing that they offered regular HCV participants, and these services were minimal (e.g., listings of current landlords). Enhanced search assistance was provided in Fresno and Los Angeles, but these services appear not to have coincided with the period of housing search of the evaluation sample. Atlanta offered additional services for WtW voucher recipients, but only if they were at risk of having their vouchers expire. This included counseling from housing authority staff to identify barriers to finding housing and referrals to partner agencies to respond to those needs. Augusta also had additional services for voucher participants who were having difficulty leasing up, but these services were provided to both WtWV and regular HCV participants. They included referrals to social service agencies for assistance with security deposits and for assistance in locating available units.

In Los Angeles, enhanced housing search services were developed for WtWV participants, but these services were not available to all participants. Later enrollees, including all of the participants in the evaluation sample, received less intensive services. The Los Angeles Department of Public and Social Services (the local TANF agency) provided funding to the housing authority for housing search assistance services and mobility counseling. DPSS reimbursed the Housing Authority of the City of Los Angeles (HACLA) $2,500 for each household on the DPSS caseload that leased up with a WtW voucher. The housing authority used these funds to hire staff in-house to provide housing search assistance to WtW voucher clients.[43] The housing search services offered to WtW voucher clients in Los Angeles included a case worker to help voucher participants identify potential neighborhoods, assistance with landlord negotiations, and transportation to specific units. However, funding for these more intensive services was depleted prior to random assignment so that the intensity of the housing counseling services was greater for voucher recipients who received a WtW voucher *before* random assignment than for those in the research sample. In particular, housing authority staff reported that housing counseling services were offered on an individualized basis more often before random assignment. For members of the research sample, housing search assistance included housing search instructions during the briefings and access to a case worker who was available to provide counseling if requested by the participant. Since we did not conduct in-depth interviews with participants in the Los Angeles site, we do not have feedback from program participants about the housing search assistance provided.

[43] In the original application, HACLA had anticipated working with non-profits to provide housing counseling services, but in the end the agency decided to hire new in-house staff to provide these services.

Only Fresno established enhanced housing search services specifically for WtW voucher recipients and made those services available to *all* WtW voucher recipients. The Housing Authorities of the City and County of Fresno took a two-part approach to providing housing search assistance, and one part was available only to WtW participants. During the random assignment period (April-June 2000), the Housing Authority offered WtWV participants the same housing counseling services it offered all of its HCV participants. This included lists of landlords and vacant units, one-on-one counseling with housing authority staff if requested by the voucher recipient, credit counseling (in group sessions) led by a nonprofit organization, and referrals to United Way volunteers who assisted with moving furniture and belongings. Starting in July 2000 (after random assignment was completed), the city's housing authority provided special housing counseling in large group sessions exclusively for WtW participants who had not yet leased a unit with their voucher. Current landlords were present at these sessions to call prospective new landlords on behalf of the WtW voucher recipients. The participating landlords described the WtWV program and worked to persuade prospective landlords to agree to a payment plan for security deposits, when necessary. The HA also had a regular outreach program to landlords, and this was the primary vehicle for obtaining landlords for WtW participants. This program included monthly meetings with current HCV landlords and the Apartment Owners' Association, and the HA used these forums to advertise the WtW program. The housing authority also placed advertisements in local papers and in publications of the Apartment Owners' Association promoting the WtWV program as a safe and effective way to lease-up units. Housing search assistance for WtWV recipients was further enhanced in Fresno in late 2001, but this was approximately 18 months after the research sample had been enrolled.[44]

Employment-Related Services

Although housing assistance alone can change families' economic status by reducing their rent burdens or changing their locations (and access to employment), many families may also need assistance geared specifically towards obtaining and retaining employment. Housing authorities participating in the WtW voucher program were required to coordinate their efforts with the TANF agency and other local providers of employment and training services to create a comprehensive set of services that would help participants move toward the goal of economic self-sufficiency. However, HUD did not require specific employment services or dictate how the services were to be offered. In practice, we found that most HAs did not offer employment-related services to WtW voucher participants beyond what was already available to them through TANF and other services. In addition, the in-depth interviews with treatment group members also do not reflect receipt of special employment-related services. Some interview respondents reported having participated in FSS, but few indicated that they received

[44] Beginning in late 2001, the agency assigned a Housing Program Coordinator (HPC) from the special programs unit responsible for operating the WtWV program to each family who was issued a WtW voucher. The HPCs contacted families weekly until they submitted a Request for Tenancy Approval (RFTA). This contact was typically by telephone, but HPCs conducted home visits if necessary. Through the weekly contacts, the HPCs gave families encouragement and housing counseling as needed. The HPCs may also have contacted and negotiated with individual landlords to get families housed, particularly if the issue was the family's credit. These services were more extensive than those available to regular HCV families and were focused primarily on the initial lease-up rather than on subsequent moves. This is evidence of the effort by the Housing Authority of the City and County of Fresno to develop specialized services for the WtWV program.

employment training or services from the PHA as a result of having received the voucher. In this respect, the evaluation sites are similar to other WtWV programs, as reported by providers of technical assistance.[45]

Most of the evaluation sites referred WtW voucher recipients to existing employment-related services provided by the local TANF agencies or referred them to the HAs' FSS programs. Families who volunteered to participate in FSS signed a five-year contract with the PHA specifying the steps that both the family and the PHA would take to move them toward financial independence. Participants could also save money through FSS. An escrow credit, calculated by the PHA based on increases in earned income of the participating family, was deposited to an interest-bearing escrow account that the family could claim upon successful completion of the FSS contract.

All of the evaluation sites reported that they encouraged WtWV participants to enroll in FSS, but only Fresno required participation in FSS. This requirement in Fresno was not strictly enforced until late 2001, however, when the housing agency received funding from the TANF agency to fund case management for WtWV recipients.

We asked the housing authorities to estimate the number of WtWV participants enrolled in FSS in 2001 and again in early 2003. The results are shown in Exhibit 2.4. As the exhibit demonstrates, enrollment in FSS has been constant at all sites except Fresno, where approximately 50 percent of WtWV participants were enrolled in July 2001, but by February 2003 all WtWV participants (including members of the research sample) were enrolled in FSS.[46] At several sites, fewer than 10 percent of WtWV participants were enrolled in FSS. The largest rates of FSS participation were in Fresno (100 percent) and Augusta (44 percent).

Beyond FSS, most housing authorities referred WtWV participants to outside service providers for job search assistance, skills training, and supportive services. From our interviews it appeared that the most extensive array of employment-related services was offered in Fresno. Even at this site, the more intensive service provision began during 2002, well after enrollment of the evaluation sample. During the initial follow-up period, most of the research sample in Fresno did not receive any specialized employment-related services through the WtWV program. In Atlanta, program staff had intended to provide a 20-hour computer literacy training class to all WtWV participants. In practice, however, only about 10 families received the training in 2001, and the class was not provided after that time.

[45] See Quadel Consulting Corporation (2002).

[46] Beginning in late 2001, when the housing agency received funding from the TANF agency to provide enhanced case management services to WtWV participants, all WtWV participants were contacted and informed of the requirement to enroll in FSS. This contact was made to all participants, including members of the research sample who had entered the program in mid-2000.

Exhibit 2.4
Extent of FSS Participation by WtWV Participants

	Percent of WtWV Participants Enrolled in FSS: July 2001	Percent of WtWV Participants Enrolled in FSS: February 2003
Atlanta	44%	42%
Augusta	4%	4%
Fresno	50%	100%
Houston	4%	5%
Los Angeles	10%	11%
Spokane	4%	9%

Source: Housing Authority staff estimates of FSS participation rates.

If other services had been provided in combination with the WtW voucher, the difference between treatment and control group members in the receipt of services (as well as the rent subsidy itself) would be part of the intervention. Treatment-control differences in employment and other outcomes would reflect the effects of the *net* difference in services between the treatment and control groups, in addition to the impact of the rental assistance. As explained in Chapter 1, while some of the control group members received vouchers, it is possible to adjust for this in the analysis. If services provided to treatment group members by the housing authority were the same services they would have received anyway (i.e., if controls received the same services from other sources), this net difference would be small or nonexistent. If, however, a substantial proportion of the treatment group received additional services, these services should properly have been considered as part of the treatment.

Our conclusion is that any employment-related services offered in conjunction with the voucher were modest and similar to those available to controls. In Fresno, where specialized case management and employment services were developed for WtWV recipients, the timing of these services was such that they were not likely to have been provided to treatment group members during the first 12 to 18 months following random assignment. The more intensive employment services were implemented in early 2002, about 18 months after random assignment. In Augusta, a case manager was added to the housing agency's staff in 2002 to provide specialized services to WtW voucher participants, but this occurred nearly two years after enrollment of the research sample, making it unlikely that they received these services.

We have concluded, therefore, that in assessing the effects of the WtWV program, the intervention being tested is the voucher itself. This is an important conclusion because it means that it is not necessary to attempt to measure receipt of services by treatment and control group members to assess the impacts of the voucher program. It also means that information from this study can be used to make broader conclusions about the impacts of the voucher program overall for low-income families who receive welfare.

2.3 Lease-up Patterns

To set the stage for the assessment of impacts of the voucher program in the next four chapters, in this section we examine the incidence of lease-up among control group members who may have received voucher assistance through the regular HCV program. Using data from HUD's PIC system, we calculated the percentage of treatment and control group members who had leased up with a voucher during each month following random assignment. These results are presented for the same follow-up period that is used in estimating the program impacts presented in Chapters 3-6. For all sites, the lease-up rates are shown through the 14th quarter after random assignment (Month 42). For all sites except Los Angeles, the results are shown through the 16th quarter after random assignment (Month 48). For sites where data are available through Month 48, there is little difference between lease up rates at Month 42 and Month 48. The lease-up rates are displayed graphically in Exhibits 2.5 through 2.12.

The key findings from this analysis are as follows:

- Through the 42nd month after random assignment, 67 percent of treatment group members across all sites had leased with a voucher. Among all control group members, 41 percent had leased with a voucher. The 42-month interval is the longest over which all members of the research sample—including those in Los Angeles, the last-enrolled site—are observed in the PIC data available through December 2004 for this report.
- The 42nd month lease-up rates among treatment group members were lowest in Los Angeles and Atlanta (51 percent and 56 percent) and highest in Augusta, Fresno, and Houston (84 percent, 74 percent, and 70 percent). Among control group members, lease-up rates at Month 42 were lowest in Atlanta, Los Angeles, Fresno, and Spokane (29 percent, 36 percent, 37 percent, and 38 percent) and highest in Augusta (60 percent).
- By the 48th month, across all sites except Los Angeles, 70 percent of treatment group members had successfully leased a unit, as had 43 percent of controls. Among treatment group members, the rates varied from a low of 57 percent in Atlanta to a high of 84 percent in Augusta. The lease-up rate among controls at month 48 ranged from 30 percent in Atlanta to 62 percent in Augusta.

By the end of Month 42, the highest lease-up rates were observed in Augusta, where 84 percent of the treatment group and 61 percent of the control group had leased up. The lease-up rates in Los Angeles and Atlanta were substantially lower than those for the other sites as of the end of Month 42. In Los Angeles, 51 percent of treatment group members and 36 percent of controls had leased up by the end of Month 42. The corresponding lease up rates in Atlanta were 57 percent for treatments and 29 percent for controls.

Local rental market conditions could explain the low rates of leasing success in Los Angeles. Housing authority staff, as well as staff from the HUD Field Office, reported that the rental housing market in Los Angeles was extremely tight, especially during the latter half of 2001 when the voucher recipients included in the study were attempting to use their vouchers. Market conditions have reportedly remained difficult for voucher recipients over the follow-up period.

In Atlanta, the rental market was reportedly fairly soft at the outset of the study, with vacancy rates estimated at 9 percent. Over the follow-up period, the rental market is believed to have tightened substantially, making it more difficult for voucher recipients to use their vouchers. As discussed earlier, WtWV recipients in Atlanta were restricted in where they could use the vouchers, prohibiting leasing of WtW vouchers outside the city of Atlanta. This could help explain the lower leasing success among treatment group members in this site relative to the other study sites. However, since this portability restriction did not apply to controls, those who received vouchers under the regular program, it should not be a factor in the lower leasing rates among controls as compared to other sites.

Among the sites other than Los Angeles, there was little change in lease-up rates from Month 42 to Month 48. By the end of Month 48, Augusta still had the highest rate of lease-up among treatment group members, with 84 percent having leased by that time. Month 48 lease-up rates in the other sites ranged from 51 percent in Atlanta to 74 percent in Fresno. Among control group members, lease-up rates at Month 48 ranged from 30 percent in Atlanta to 62 percent in Augusta. Exhibits 2.5 through 2.12 display the lease-up rates over time in each of the evaluation sites. As the exhibits indicate, leasing among control group members increased over the follow-up period, as controls reached the top of the HCV waiting lists and as additional rental vouchers became available to the housing agencies through program turnover or through new allocations of vouchers. In Fresno, the local staff reported that as of February 2002, the waiting list for rental assistance had been depleted, so that no control group members would subsequently have been offered vouchers. This is borne out by the stable rate of control group lease-up observed in Fresno after Month 20 (see Exhibit 2.9).

In contrast, both Houston and Spokane experienced a steep increase in the rate of lease-ups among control group members between Months 20 and 24, corresponding to late 2001 through early 2002. These HAs received additional increments of vouchers through the regular voucher program at that time, allowing them to serve more families, including more control group members.

These lease-up patterns illustrate what we would expect, given that individuals randomly assigned to the control group remained on the HA's waiting list for regular voucher assistance: over time, the percentage of control group members who leased-up increased. Nevertheless, at the end of the follow-up period (42 months for all sites) there was still a fairly substantial differential between the percentage of treatments who had leased-up at some time during the follow-up period (67 percent) and the percentage of controls who had leased-up at some time during the follow-up period (41 percent).

These lease-up rates do not account for the fact that, over time, some individuals in both the treatment control groups who leased-up eventually left the voucher program (the lease-up rates instead are a measure of lease-up at any time during the follow-up period without regard to voucher retention). The follow-up survey provides information about the treatment/control differential in receipt of housing assistance at the time of the survey. At the time of the follow-up survey, we asked respondents whether they were receiving voucher assistance (whether a Welfare-to-Work Voucher or a Housing Choice Voucher). Overall,

37.2 percent of controls and 50.6 percent of treatments[47] reported that they were receiving some type voucher at the time of the survey. This substantially smaller treatment/control differential in the receipt of voucher assistance at the time of the follow-up survey illustrates the fact that some treatment group members who leased-up eventually left the voucher program during the follow-up period, just as more controls began leasing-up with vouchers. Controls also will leave the program over time, but, given the lag in initial lease-up among controls, by the time of the follow-up interview we would expect to find less incidence of controls who had leased-up having left the voucher program. This relatively small treatment/control differential in receipt of housing assistance at the time of the follow-up interview underscores the importance of adjusting for control group crossover when estimating impacts on outcomes derived from survey data. Such an adjustment has been made when estimating impacts and is described in the impact analysis chapters that follow.

In the next chapter, we begin presenting the results of the empirical analysis of voucher impacts. Chapter 3 presents results about the mobility of voucher recipients, characteristics of neighborhoods in which they reside, and other location-based impacts.

[47] The percentages reported here are weighted to reflect survey sampling probabilities and final sample weights.

Exhibit 2.5 : Total Lease-up Rates, All Sites

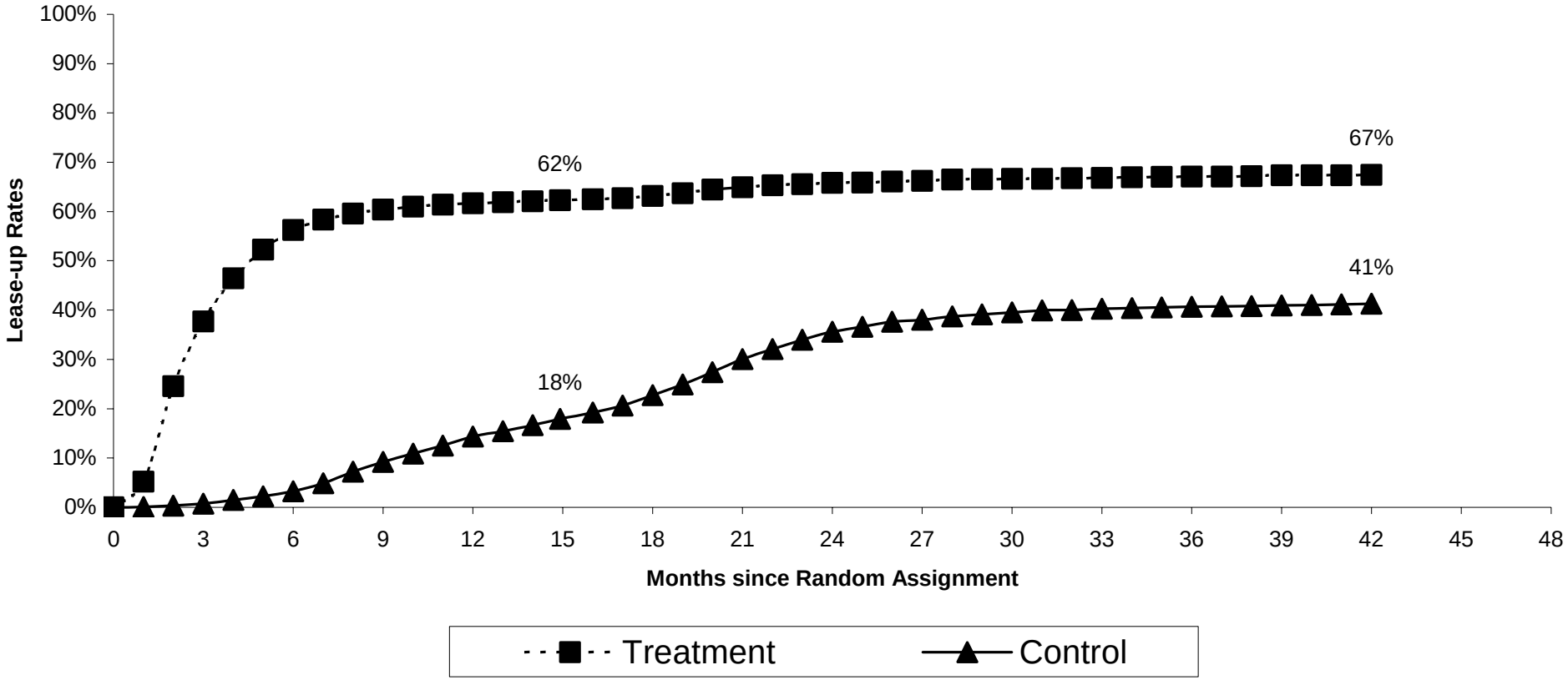


Exhibit 2.6 : Total Lease-up Rates, All Sites except Los Angeles

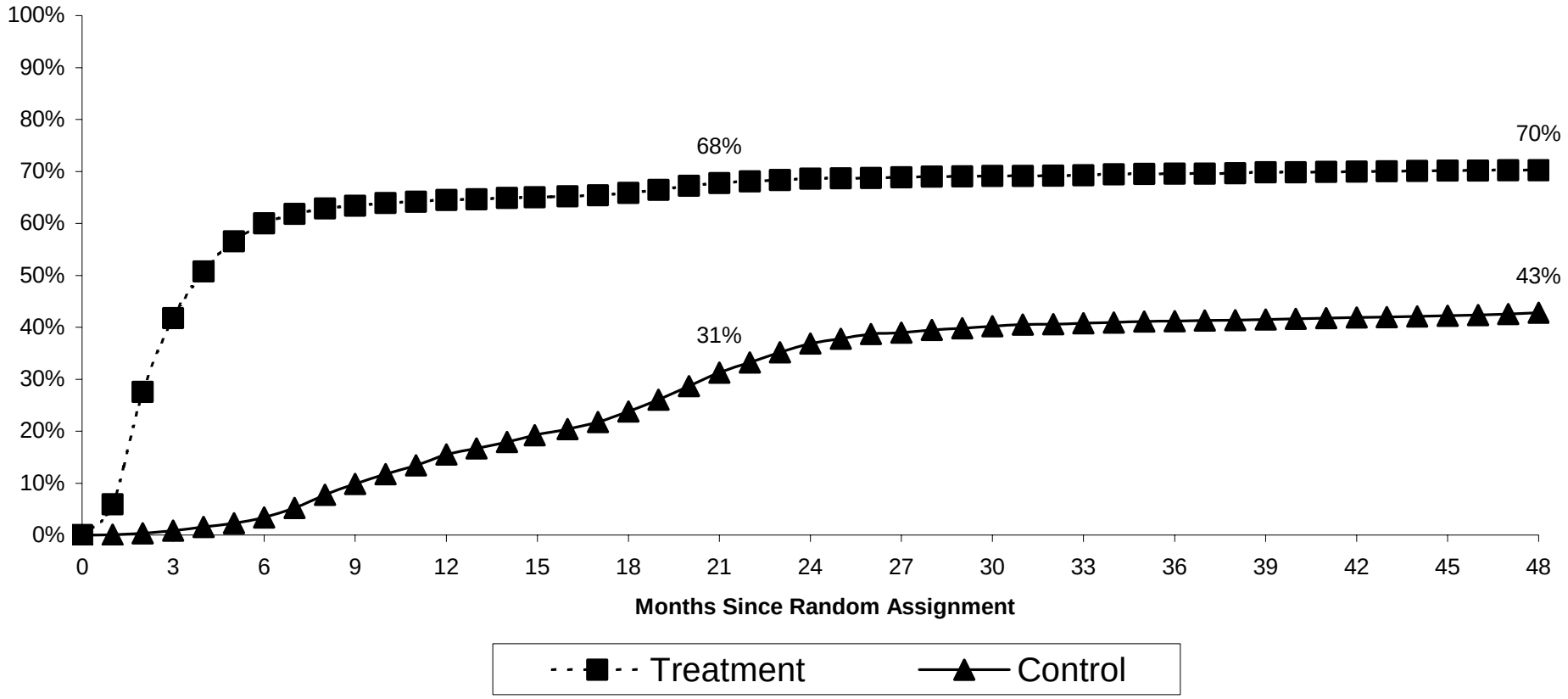


Exhibit 2.7: Total Lease-up Rates, Atlanta

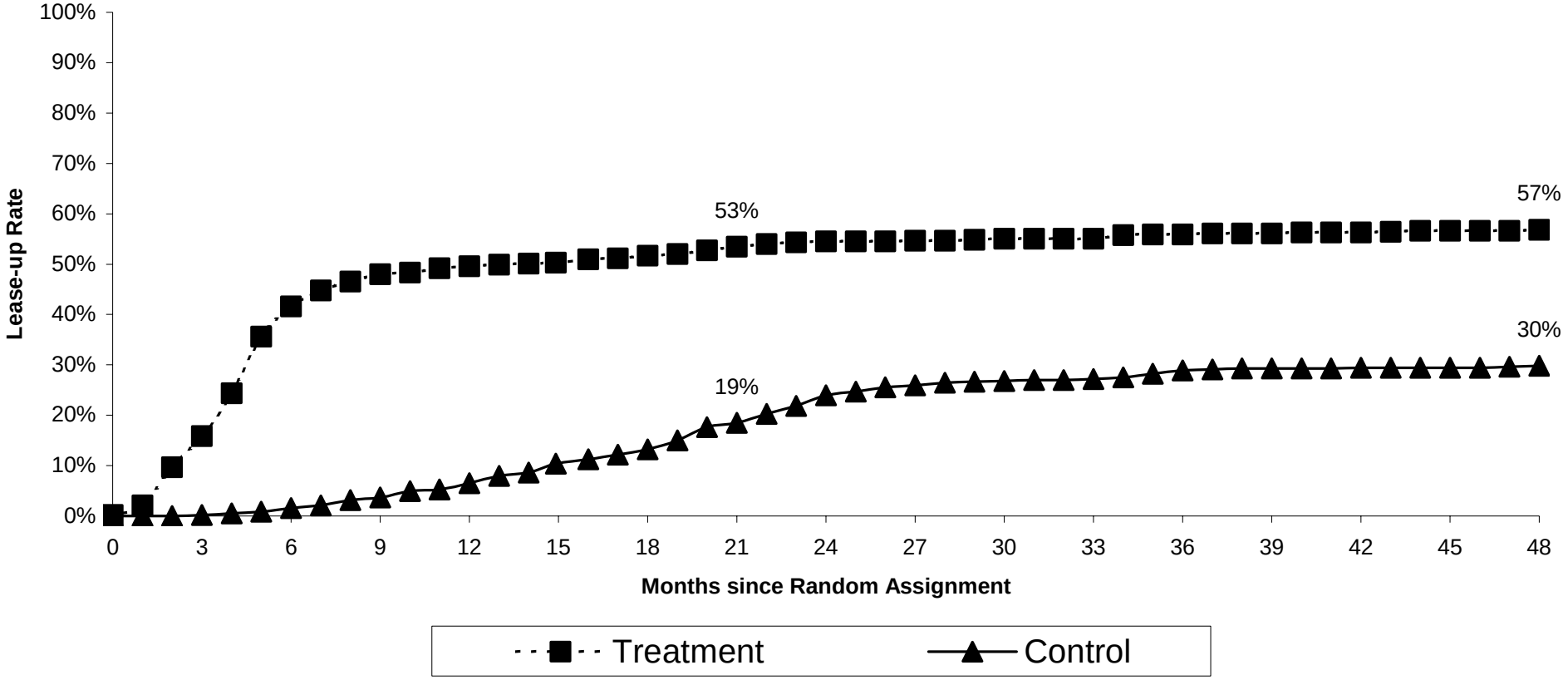


Exhibit 2.8: Total Lease-up Rates, Augusta

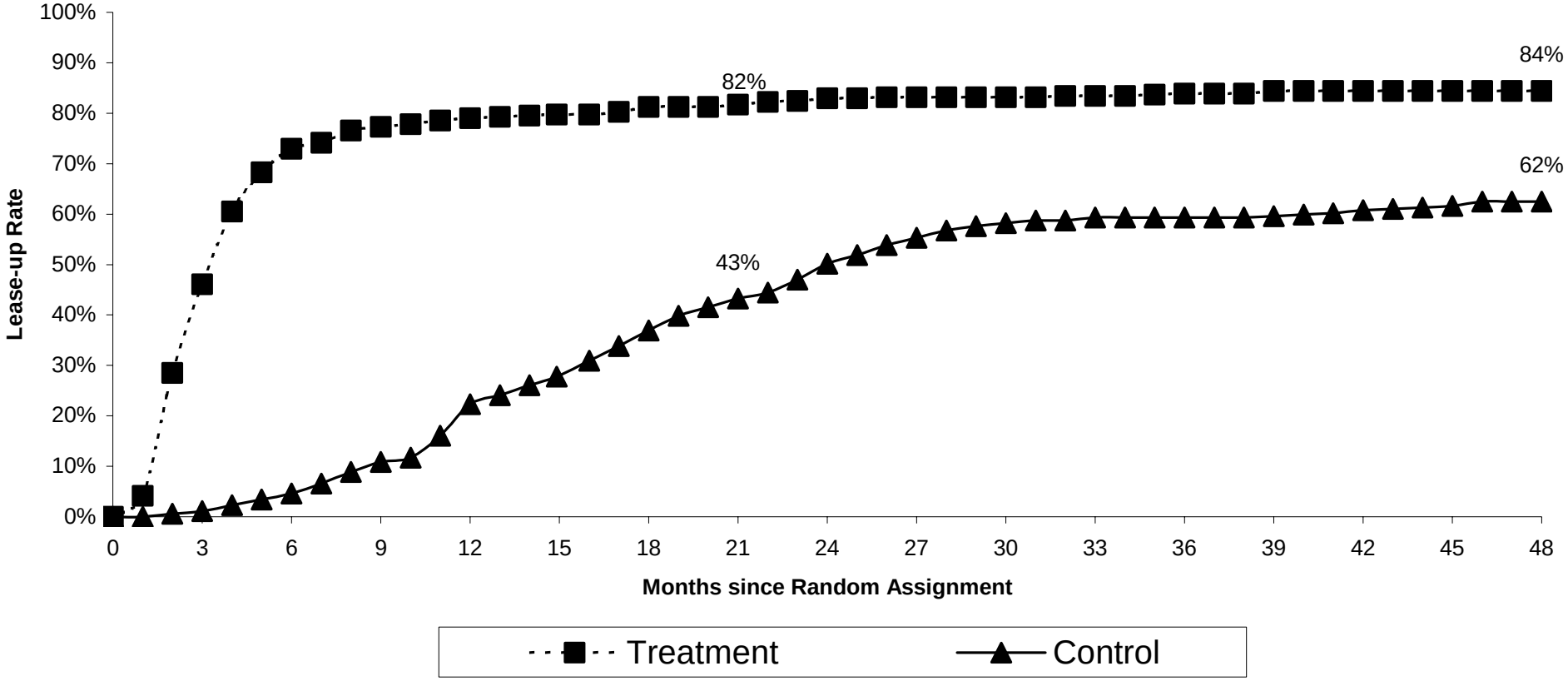


Exhibit 2.9 : Total Lease-up Rates, Fresno

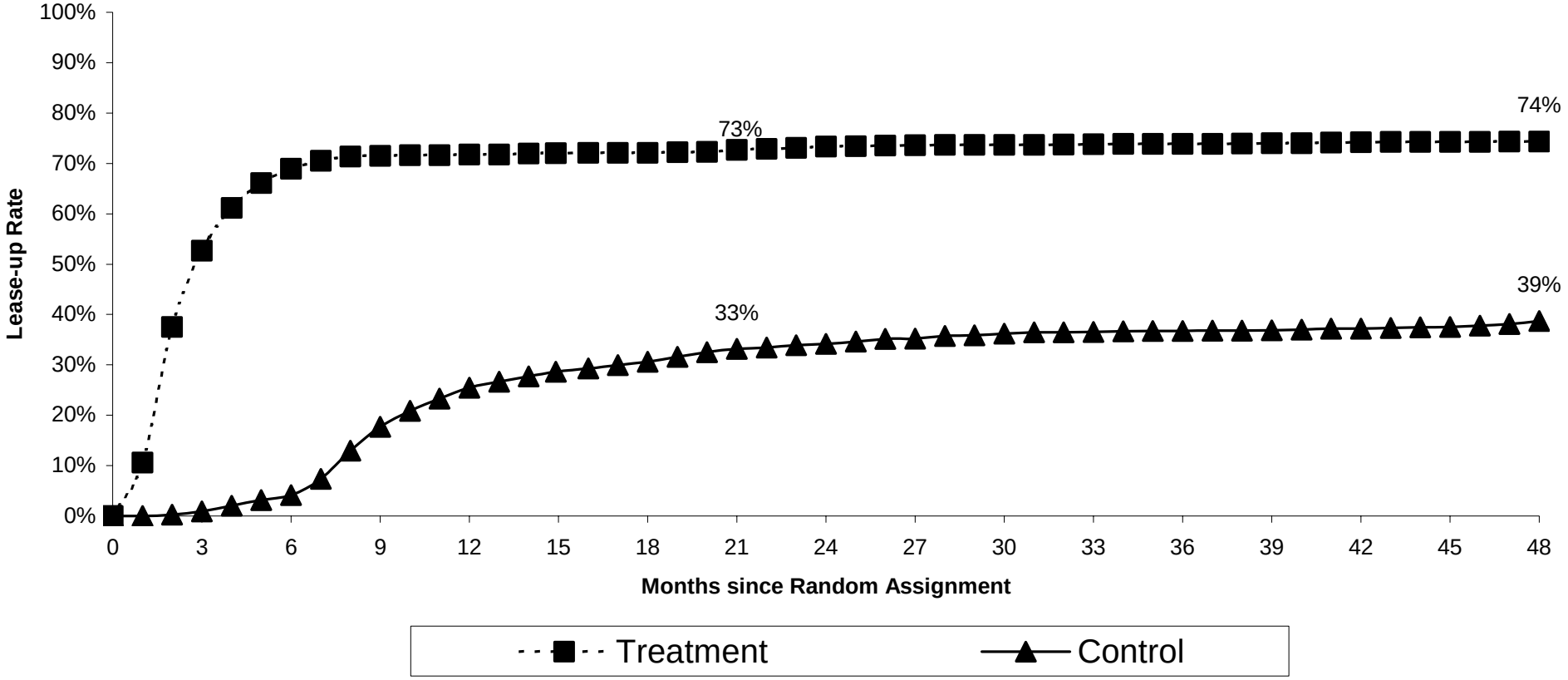


Exhibit 2.10: Total Lease-up Rates, Houston

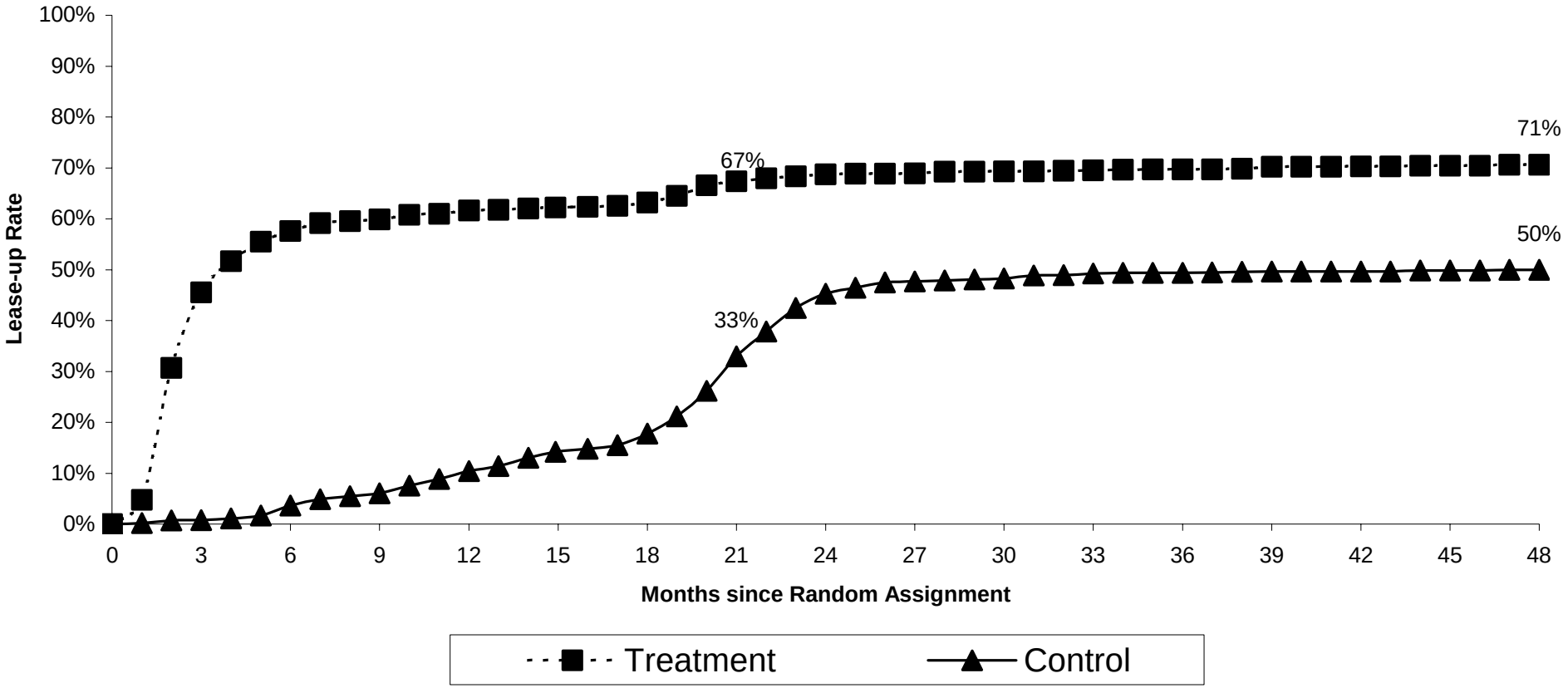


Exhibit 2.11 : Total Lease-up Rates, Los Angeles

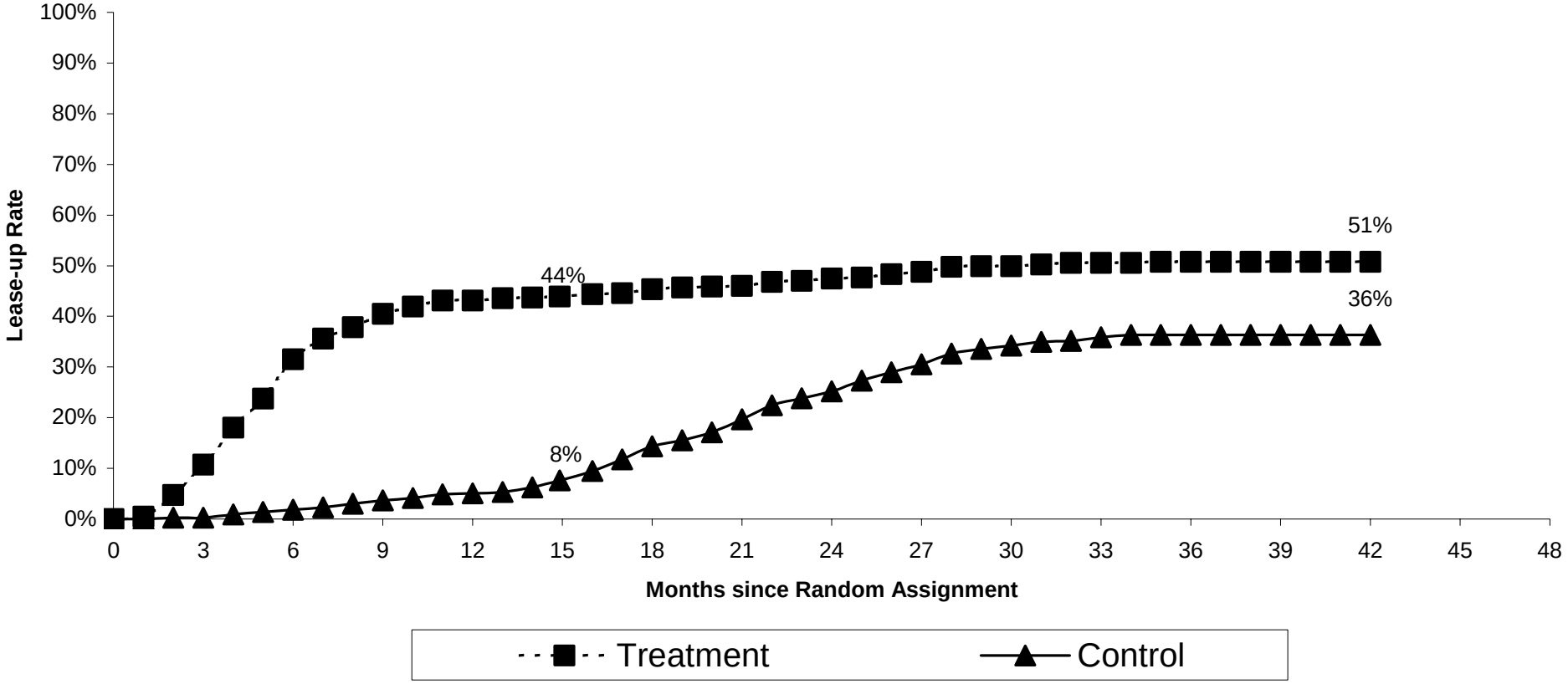
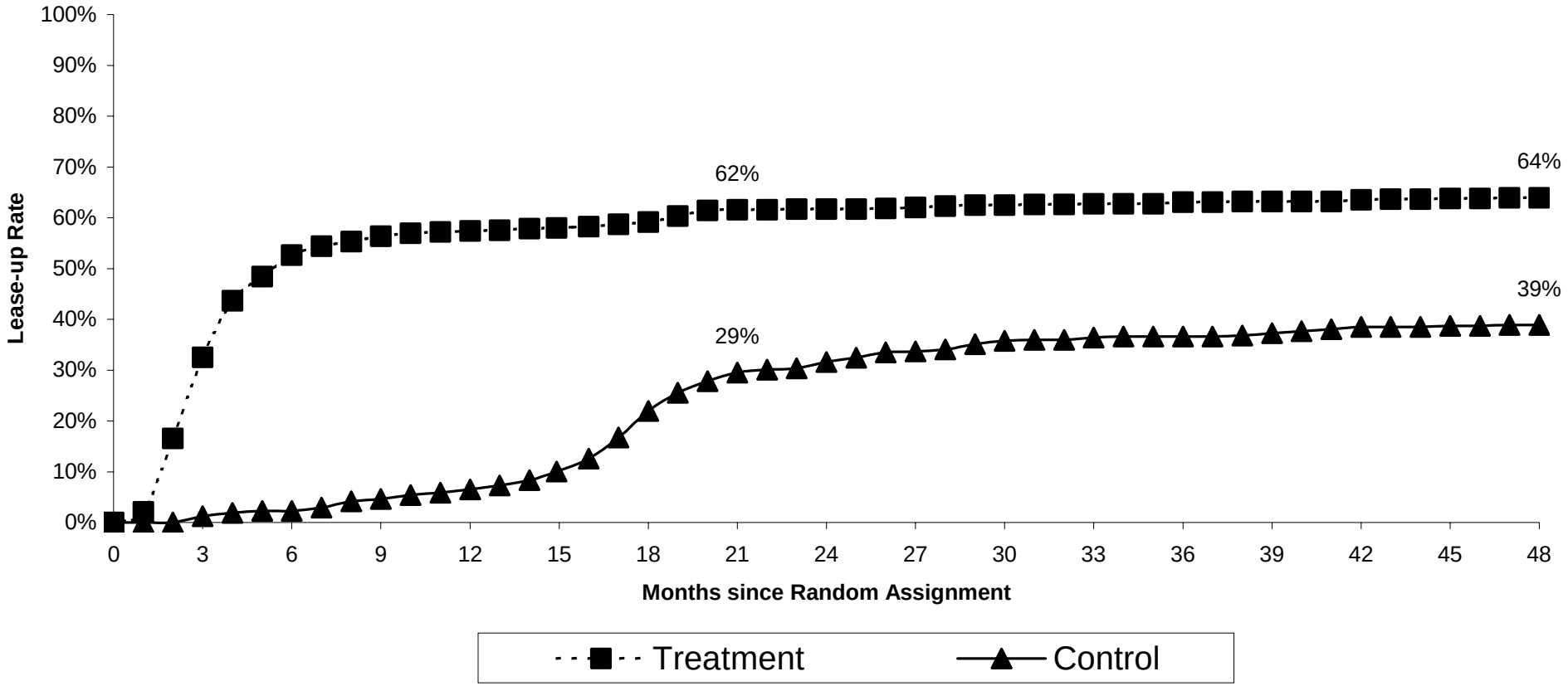


Exhibit 2.12: Total Lease-up Rates, Spokane



Chapter Three
Impacts on Housing Location and Household Composition

This chapter examines how families use their Housing Choice Vouchers. It describes how families issued vouchers use them to lease units (or, in some cases, fail to use them) and whether and how they use the voucher to move to different housing, to change neighborhoods, and to change the group of people with whom they live. Using the study's experimental design, the chapter presents impacts of the Housing Choice Voucher on the characteristics of the neighborhoods in which families live and on the composition of their households.

The primary outcomes regarding housing location relate to the economic profile of the neighborhood in which the family lives, based on tract-level Census indicators of income, employment, and education. Others pertain to the neighborhood's demographic composition: race, ethnicity, and family structure. Additional outcomes addressed in this chapter are based on the follow-up survey and reflect self-reported neighborhood satisfaction, observed neighborhood problems, and personal safety. The focus of this analysis is on the characteristics of the neighborhood rather than the quality of the housing unit, which is addressed in Chapter Five.

The outcomes regarding the composition of the household are all based on the follow-up survey data and include the presence in the household of a spouse or partner, the presence of children, household size, and household type (single-parent with children, two-parent with children, multigenerational, or other).

After a brief summary of the findings, the chapter discusses the hypotheses about the effects of vouchers on neighborhood characteristics, the qualitative evidence from in-depth interviews with voucher users, the data sources for this analysis, the sample's housing locations and household compositions at baseline, and then the impact estimates for all types of families in the study and for subgroups of families defined by characteristics such as race, age, and employment experience at baseline. In interpreting the impact estimates, we draw upon both quantitative and qualitative research.

3.1 Summary of Findings

Prior to estimating program impacts, we used the follow-up survey data to conduct an analysis of voucher retention among treatment cases (in the five sites excluding Los Angeles). Specifically, we looked at the probability of still being leased-up with rental assistance at the time of the follow-up survey, among treatment group respondents who had leased-up with a voucher at *any* time after random assignment. The probability of still receiving assistance ranged from 59 percent in Spokane to 93 percent in Atlanta.

The voucher treatment was found to have the following significant impacts regarding ***housing location***:

- An increased likelihood of residing outside the family's baseline Census tract by the end of the follow-up period.
- A reduction in the overall number of moves made during the follow-up period.
- A better residential location at the end of the follow-up period, as indicated by a lower poverty rate, a higher employment rate, and lower welfare concentration in the family's end-of-period Census tract.
- A different demographic composition of the family's end-of-period neighborhood, as indicated by lower minority concentration, lower black concentration, and lower female-headedness.
- A reduced extent to which graffiti and public drinking were problematic conditions in the end-of-period neighborhood.

No significant effects (at the 0.05 level or better) were found on the number of months lived at the end-period neighborhood, on other Census-measured neighborhood characteristics (educational attainment, youth idleness, and Hispanic concentration), on the family's neighborhood satisfaction, on additional indicators of neighborhood problems (litter or trash, abandoned buildings, people hanging out, or people using or selling drugs), or on crime victimization.

Locational effects by subgroup were varied, but generally showed a pattern of greater mobility (from the baseline Census tract) and higher locational quality in the end-of-period Census tract among the following categories: those residing at baseline in more stressful arrangements (e.g., in public or assisted housing or in shared or temporary housing), those facing greater barriers to employment at baseline (e.g., less educated, with pre-school children, not employed, never employed, or not enrolled in school or training).

On ***household composition***, vouchers were found to have the following significant impacts:

- A reduction in the proportion of multigenerational households and a corresponding increase in the proportion consisting of a single parent with children.
- A reduction in household size, associated with a lower average number of elders, siblings, and other adult household members.
- No reduction in the likelihood of residing with a spouse or partner at the time of the survey.

No effects were found on the number of children living in the household at the follow-up survey, either birth children or other children.

Impacts were estimated by subgroup for major outcomes related to household composition. The significant treatment effects noted above on the proportion of households consisting of a single parent with children (positive) and on household size (negative) were also found for the following subgroups (as defined at baseline): those with at least a high school diploma,

those not enrolled in school or training, those with any dependent children, those with children under the age of six, those ever employed, those not desiring to move for employment, those residing in public or assisted housing, and those receiving TANF.

3.2 Hypothesized Effects of Housing Vouchers

Housing Location

We expect families' locational decisions to be affected by the greater housing affordability enabled by the voucher. Although families in the Housing Choice Voucher program may use their vouchers to lease in place, an explicit rationale for the Welfare to Work Voucher program at the time it was enacted was to allow participants to relocate to neighborhoods with higher employment rates, lower crime rates, more amenities and conveniences, better schools, lower transportation costs, and more jobs and job training opportunities. We therefore hypothesized that families afforded the opportunity to move to (or remain in) housing in more advantaged neighborhoods would do so, and that this would ultimately result in treatment group members residing in systematically higher quality neighborhoods than control group members.

The treatment might be expected to affect some subgroups differently, as barriers to relocation or lease-up, and search costs, vary among individuals in different circumstances. For example, we might expect persons who are employed to be less likely to move, because of the higher opportunity costs associated with housing search and the possible disruption to established commuting patterns. (Offsetting this, however, is that employed persons are better able to afford the costs of relocating.) Responsibilities for pre-school children may also raise the costs of housing search and reduce the likelihood that a family will move to new housing. Similarly, those with school-aged children may resist any move that would require a change of schools. All other things equal, we might expect ethnic minorities to face higher barriers to mobility because of discrimination, constraining their choices and reducing their likelihood of moving.

Although vouchers could promote "moving out" of a family's baseline location, one also might expect greater "settling in" of voucher holders over the course of the demonstration, leading to fewer moves during the total follow-up period. In part, this might reflect the fact that voucher users generally sign annual leases and have their subsidy amounts determined annually through a process that requires documentation of income. Thus, voucher users may choose to move only at annual intervals. Control group members would be more likely to have either no lease or a shorter-term lease. In addition, voucher holders who act on their initial desire to move may tend *not* to make subsequent moves, as another landlord might not be willing to accept a voucher or a unit might not meet inspection standards. Finally, families with vouchers may be better able to afford their rent, making them less likely to be evicted for nonpayment--or less likely to move to avoid eviction.[48]

[48] For an example of recent research on vouchers and their effects on geographic mobility, see Judith F. Feins and Rhiannon Patterson, "Geographic Mobility in the Housing Choice Voucher Program: A Study of Families Entering the Program, 1995-2002," *Cityscape*, Volume 8, Number 2, 2005, pp. 21-47.

Household Composition

By expanding the range of affordable housing options, vouchers may have effects not only on where a person lives, but also with whom she chooses to live. The direction of the effect on marriage and cohabitation is not clear. Vouchers may increase the likelihood of single-parent households, to the extent that the voucher user may be better able financially to escape an unsatisfactory relationship with a spouse or partner. The voucher may also reduce the economic incentive to find a spouse or partner, both because of the financial resources provided and because the subsidy amount is income-conditioned. On the other hand, the additional financial resources could make the voucher user more attractive to potential partners and moving to a larger housing unit may increase the voucher user's willingness to continue a cohabiting arrangement.

With or without effects on marriage or cohabitation, the voucher subsidy might also be expected to influence the number of children born to voucher users. By making a housing unit with more living space affordable, a voucher might tend to increase the likelihood of having more children. In addition, the voucher might free up discretionary income that could be devoted to the expenses of an additional child, including childcare. An opposing view is that for those with children already (as with this welfare-eligible program population) additional financial resources will be directed at improving their quality of life, with no increase in the number of children.

The voucher might also affect whether a voucher user chooses to live with extended family members or nonrelatives. There are competing considerations. Shared living arrangements offer the advantages of companionship, economies of scale in living expenses, and in-kind services such as childcare. In contrast, independent living arrangements confer space and privacy. With vouchers users often occupying shared and overcrowded living quarters at baseline, the voucher's reduction of the financial incentive to share housing would result in a reduction in household size.[49]

3.3 Data Sources for Impact Estimates

The impact estimates presented in this chapter are of two types: those derived from a combination of address history data for sample members and corresponding Census data on tract-level characteristics, and those derived from follow-up survey data. We discuss these data sources in turn.

Address Histories and Census Data

The address-based data for this chapter are constructed from individual-level address histories compiled from the following sources: baseline and follow-up surveys, housing assistance program databases (HUD's PIH Information Center, or PIC, database), passive tracking efforts using National Change of Address (NCOA) data and commercially available third-party datasets, and responses to tracking letters sent to sample members (and, in some instances, to contact persons identified in the baseline survey). For every sample member, a

[49] For a useful summary of recent research on these topics, see Lance Freeman, "Household Composition and Housing Assistance: Examining the Link," *Cityscape*, Volume 8, Number 2, 2005, pp. 49-67.

chronological series of residential locations (the address history) was constructed. (Further detail is provided in Appendix B.) Each address was geocoded to the corresponding Census tract, a geographic unit of which there are more than 65,000 nationwide with a population typically between 1,500 and 8,000. The tract codes were then used to link tract-level data on selected measures of neighborhood quality from the Census Bureau's 2000 Summary File 3.[50] Where a sample member resided in multiple locations in a follow-up quarter, a weighted quarterly value was constructed for the quality indicator, based on the portion of the quarter spent at each location. The quarterly values served as dependent variables; baseline survey data and baseline values of the Census variables were included as covariates in the impact estimation.

The dependent variables from the Census Summary File 3 data are defined as follows:

- Percent below poverty level—percent of persons whose ratio of income to the poverty level (in 1999) was less than 1.00 ("poverty rate");
- Percent of civilians employed—percent of persons 16 years of age and over in the Census tract in the civilian labor force who were employed ("employment rate");
- Percent of adults with some high school education—percent of persons 25 years and over in the Census tract with at least a 9th-grade education ("educational attainment");
- Percent of youths not in school and not in the labor force—percent of persons 16 to 19 years of age in the Census tract who were not enrolled in school and not in the civilian labor force ("youth idleness");
- Percent of households with public assistance—percentage of households with income (in 1999) from public assistance ("welfare concentration").
- Percent minority—percent of persons in the Census tract categorized as minorities, including all non-white and all of Hispanic origin ("minority concentration");
- Percent black—percent of persons in the Census tract categorized as black ("black concentration");
- Percent Hispanic—person of persons in the Census tract categorized as of Hispanic or Latino origin ("Hispanic concentration"); and
- Percent of households with single female heads—percent of households that are family households headed by single females ("female-headedness").

Each of these nine variables was bounded between 0 and 100. The first five variables (poverty rate, employment rate, educational attainment, youth idleness, and welfare concentration) correspond to economic characteristics and can be regarded as indicators of neighborhood quality. The final four variables (the minority, black, and Hispanic concentrations and female-headedness) reflect the demographic composition of the neighborhood.

Although the individual-level addresses represent household locations at different points in time, the Census data reflect information only for the period in 2000 over which the Census was conducted. Given the timing of random assignment (April 2000 – May 2001), these

[50] We use "neighborhood" throughout to mean Census tract.

values are reasonably accurate initial characterizations of each tract. We did not attempt to account for whether a tract was undergoing any improvement or decline in its conditions. Nor did we examine locational data measured for areas below the Census tract level, such as data linked to or calculated for block groups. For this reason, changes in neighborhood characteristics resulting from relocation *within* a Census tract have not been measured.[51]

In addition to the tract-level neighborhood indicators, a dichotomous outcome variable indicating mobility was constructed on a quarterly basis from the address history data, as follows:

- Whether, at the end of the indicated quarter, the sample member resided in a Census tract different from the baseline Census tract.

A "mover" or "stayer" was thus defined according to whether the family left its baseline Census tract.

For the address-based outcomes, this chapter presents treatment impacts at the 16th and 18th quarters following random assignment. All sample members were observed through 16 follow-up quarters of address history. In all sites except Los Angeles, sample members were observed through 18 quarters of address history. Because the study sites enrolled their samples over differing multi-month periods, the follow-up quarters represent different calendar intervals for different sites and even for different individuals within the same site.

Survey Data

Using data from the follow-up survey, we constructed several additional locational outcomes pertaining to the mobility or stability of sample members, plus outcome variables pertaining to neighborhood satisfaction, observed neighborhood problems, and crime victimization. From the survey we also constructed all of the outcome variables relating to household composition.

The mobility/stability variables were as follows:

- number of moves during the follow-up period, as indicated by the answer to survey question (A21): "How many times have you moved since [month and year of random assignment]?" We deleted from the analysis those respondents (4) who reported having made more than 15 moves, as these observations appeared to be spurious outliers.
- number of months lived at the ending address, as indicated by the answer to survey question (A20): "How many months or years have you lived in your current neighborhood?"

For purposes of the survey, "current neighborhood" was explained as the place where you currently reside and the area around it.

[51] In the earlier Report to Congress (2004), moves within the baseline Census tract represented less than 10 percent of the total mobility that had occurred through quarter 5 (for all sites) or quarter 7 (for all sites except Los Angeles). See Exhibit 3.3 of that report.

One set of household composition variables pertained to the formation and stability of marriage or cohabitation arrangements ("unions") and to fertility for the primary adult member (the survey respondent). These variables indicated the presence in the household of a spouse/partner and birth/nonbirth children, as follows:

- Living with a spouse or partner at the time of the follow-up survey (binary);
- Have lived with the *same* spouse or partner since random assignment: a subset of the category immediately above (binary).
- Ever lived with a spouse or partner since random assignment (binary)
- Have lived with more than one spouse or partner since random assignment (binary): a subset of the category immediately above.
- Number of birth children in the household (regardless of age): biological children of the primary adult.
- Number of birth children in the household who were born since the time of random assignment: a subset of the category immediately above.

Other household composition variables pertained to household size (the number of household members, by their relationship to the primary adult) and household type at the time of the follow-up survey. The components of household size were as follows:

- Number of children (members less than 18 years of age as of 12/15/04), including birth children (biological children of the primary adult) and non-birth children (step-child, adopted child, foster child, child of partner, son-in-law, daughter-in-law, grandchild, or other related or nonrelated minor);
- Number of elders—those aged 65 or older (as of 12/15/2004);
- Number of siblings—brother, sister, step-brother, step-sister, half-brother, half-sister, brother-in-law, or sister-in-law;[52]
- Number of other relatives—foster child, child of partner, son-in-law, daughter-in-law, grandchild, mother, father, step-parent, mother-in-law, father-in-law, partner's parent, grandparent, sibling, or other relative;
- Number of other non-relatives—roommate, friend, or other non-relative; and
- Total number of household members.

Because the subcategories of household members are not mutually exclusive, their summed means do not equal the mean total number of household members.

Household types were identified by four mutually exclusive binary variables, as follows:

[52] In such analyses, we believe it is appropriate to classify brothers- and sisters-in-law as "siblings" rather than "other relatives."

- Single parent with children only—primary adult with one or more children (not restricted by age) who are biological children, step-children, adopted children, foster children, or partner's children, with no other members;
- Nuclear family (two parents with children)—primary adult with spouse or partner with children (not restricted by age) who are biological children, step-children, adopted children, foster children, or partner's children, with no other members;
- Multigenerational—primary adult with one or more of the following: mother, father, step-parent, mother-in-law, father-in-law, partner's parent, grandparent, or grandchild (and with no restrictions as to children); and
- All others—for example, a primary adult with or without a spouse or partner, but with no children and no members that would classify the household as multigenerational.

As described in Chapter 1, the follow-up survey was administered between October 2004 and May 2005. We refer to the survey-measured outcomes as "year 5" outcomes, as the interview was conducted during the fifth year after random assignment for 94 percent of the respondents.[53]

3.4 Baseline Neighborhoods, Household Composition, and Patterns of Lease-up and Mobility

Baseline Neighborhood Characteristics

An examination of the characteristics of the neighborhoods where the study's sample members lived at baseline shows that random assignment provided well-matched treatment and control groups with respect to their neighborhood characteristics (Exhibit 3.1). For both groups, their neighborhoods at baseline were comparable in their characteristics to the national average for metropolitan tracts with poverty rates over 20 percent.

Marital Status and Household Size

The baseline survey, administered to sample members prior to random assignment, provides information on the marital status of the primary adult member and on the size and composition of the household upon entry into the demonstration.[54] At baseline, 58 percent of the primary adults were single and never married and 17 percent were married. The remainder were either separated, or divorced, or widowed. There was substantial site-by-site variation in these patterns. For instance, the percent who were single and never married ranged from 38 percent in Los Angeles to 83 percent in Atlanta.

The average household size at baseline (including the respondent) was 4.0 persons. This included an average of 1.4 members living in the household who were adult relatives (including the spouse, if present).

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[53] Among the 2,481 survey respondents, the interview was conducted in the fifth year for 2,335, in the last quarter of the fourth year for 5, and in the first quarter of the sixth year for 141.

[54] See Larry Orr, et al., *Evaluation of the Welfare to Work Voucher Program: Interim Report on Quantitative Research*, Abt Associates Inc., November 13, 2002.

Exhibit 3.1
Baseline Neighborhood Characteristics

Tract characteristic	Baseline, All Sites: Control mean	Baseline, All Sites: Treatment mean	Higher-poverty metropolitan area mean[a]
Poverty rate	29.28%	29.01%	33.56%
Employment rate	86.72%	86.83%	85.84%
Educational attainment	82.60%	83.03%	83.16%
Youth idleness	9.66%	9.59%	11.77%
Welfare concentration	10.79%	10.70%	10.66%
Minority concentration	71.57%	71.21%	71.80%
Black concentration	33.45%	33.80%	36.02%
Hispanic concentration	29.33%	28.87%	30.40%
Female-headedness	23.15%	23.25%	23.50%

Notes:
[a] Unweighted mean over all Census tracts with poverty rate of 20 percent or more within Metropolitan Statistical Areas (MSAs) or Consolidated MetropolitanStatistical Areas (CMSAs).

Patterns of Mobility

Exhibit 3.2 provides descriptive information on lease-up rates and mobility rates (rates of moving outside the baseline tract) for treatment and control group members, expressed as percentages without any adjustment for baseline characteristics. Treatment cases were somewhat more likely than control cases to have moved at some time since random assignment. In both the 16-quarter and 18-quarter analyses, the treatment-control difference in this mobility rate was 3 percentage points (e.g., 62 percent versus 59 percent at quarter 18). This reflected the fact, among those who leased-up with a voucher, those who moved to a different tract outnumbered those who did not by roughly two-to-one. As indicated in Chapter 2, the rate of leasing up with a voucher was more than 25 percentage points higher for treatment cases than for control cases. In contrast, among those who never leased-up, stayers outnumbered movers.

The locational effects of vouchers are not necessarily limited to treatment group mobility; they could also result from a pattern of treatment group stability and control group mobility. For any given neighborhood indicator, a significant treatment effect indicates that treatment group members (treatment-movers and -stayers combined) resided in end-period locations that were systematically different from the end-period locations of control group members (control-movers and -stayers combined). For example, one can imagine a situation in which no treatment group members move, but some control group members move. If the control-movers tend to re-locate to lower quality neighborhoods than at baseline, we would estimate a favorable treatment effect. It would be related to mobility, but not the mobility of treatment cases.

Exhibit 3.2
Lease-Up Rates and Mobility Rates

Status at end-quarter	Quarter 16, All Sites		Quarter 18, All Sites Except Los Angeles	
	Control group	Treatment group	Control group	Treatment group
Ever leased up	41.7 %	68.1 %	43.7 %	70.5 %
Movers	28.1	44.2	31.5	48.8
Stayers	13.6	23.9	12.2	21.7
Never leased-up	58.3	31.9	56.3	29.5
Movers	25.3	11.9	27.9	12.8
Stayers	33.0	20.0	28.4	16.7
Total movers	53.4	56.1	59.4	61.6
Total stayers	46.6	43.9	40.6	38.4

Note: "Movers" are those residing at the end-quarter in a Census tract that is different from the baseline tract. All others are classified as "stayers", even though they may have resided at multiple locations within the baseline tract, or may have resided outside the baseline tract for some intervening period.

Patterns of Treatment Group Voucher Lease-up

The data collected for this research provides some insights into the patterns of voucher lease-up and voucher retention among treatment group members, as summarized by site in Exhibit 3.3.

The first column of the exhibit shows data by site on the percentage of the treatment group that was able to use their voucher by follow-up month 6. The six-month time window was adopted because 83 percent of the treatment group lease-ups occurred within six months of voucher issuance.[55] The treatment group lease-up rate at month 6 ranged from 32 percent in Los Angeles to 73 percent in Augusta.

A multivariate regression analysis was conducted to identify the baseline characteristics of treatment cases that were systematically related to the probability of lease-up at month 6. The results indicated a mix of factors positively related to lease-up, some that would conventionally be viewed as carrying economic advantages and some carrying economic disadvantages. Among the former were the following:[56]

[55] An earlier study of the Housing Choice Voucher program by Abt Associates found that 92 percent of voucher lease-ups in metropolitan areas occurred within 180 days of voucher issuance. See Finkel, Meryl and Larry Buron, *Study on Section 8 Voucher Success Rates, Volume I: Quantitative Study of Success Rates in Metropolitan Areas*. Washington, DC: Abt Associates Inc., 2001.

[56] Each of the listed characteristics was found to be related to the lease-up probability at the 0.10 level of significance or better.

- higher earnings amounts in the previous year (if previously employed);[57]
- enrolled in (but yet to start) job training (versus not enrolled in job training);
- with a current driver's license (versus without a license); and
- not receiving Supplemental Security Income (versus receiving SSI).

Exhibit 3.3
Lease-up Status of Treatment-Group Members, at Month 6 and at Year 5

Site	Leased-up at month 6 (PIC data) (%)[a]	Lease-up status at year 5 (survey data)[b]		
		Still leased-up (%)	No longer leased-up (%)	Total (%)
Atlanta	41.9	92.6	7.4	100.0
Augusta	72.9	78.5	21.5	100.0
Fresno	69.4	81.4	18.6	100.0
Houston	57.6	82.0	18.0	100.0
Los Angeles	31.7	na	na	na
Spokane	52.9	59.3	40.7	100.0
Total	56.7	79.8	20.2	100.0

Notes:
[a] Among all treatment-group members, based on PIC data.
[b] Tabulated among treatment-group respondents who received a Welfare to Work voucher or a Section 8 certificate or voucher after the random assignment date, based on survey data. "Survey month" is the month of the follow-up interview, which typically occurred in the fifth year (months 49-60) after random assignment.

Such individuals may have been more able to search for an apartment.

In contrast, other characteristics positively related to lease-up would normally be associated with economic disadvantage:

- never previously employed (versus ever previously employed);
- black non-Hispanic (versus white non-Hispanic, other non-Hispanic, and Hispanic);
- with dependent children (versus without dependent children);
- receiving TANF (versus those not receiving TANF); and
- higher MSA-level unemployment rate.

Such characteristics may correspond to persons who have greater needs for a voucher.

[57] Note that this result is consistent with the finding of the 2001 Abt study on successful voucher lease-up, whereby "having income above 30 percent of the local median increases the probability [of lease-up] by about 14 percentage points." (Finkel and Buron, 2001, p. 3-11).

Retention of Vouchers by Treatment Group Members

In a separate analysis of voucher retention using the survey data (from the five sites excluding Los Angeles), we looked at the probability of still being leased-up with rental assistance at the time of the follow-up survey, among treatment group respondents who had leased-up with a voucher at *any* time after random assignment (i.e., not necessarily within the first six months). As shown in the second column of Exhibit 3.3, the probability of still receiving assistance ranged from 59 percent in Spokane to 93 percent in Atlanta.

Those no longer receiving assistance at the time of the survey were asked to indicate "the main reason you stopped receiving housing assistance." The distribution of reasons among the 145 respondents who previously received tenant-based housing assistance is shown in Exhibit 3.4.

Exhibit 3.4
Reason for Leaving the Voucher Program, Treatment Group Respondents (Survey Data)

Reason	Number of respondents	Percentage of respondents[a]
My income was too high to qualify for assistance	36	24.5%
Told no longer eligible, for non-income reasons	34	22.5%
Landlord would not accept voucher	8	5.7%
Moved and could not use assistance in new place	28	21.8%
Moved in with family members	8	4.5%
Moved in with partner or spouse	7	3.7%
No longer wanted to be on assistance	5	5.2%
Other	11	6.8%
Refused or don't know	8	5.4%
Total	145	100.0%

Notes:
[a] Includes treatment-group respondents who had received a Welfare to Work voucher or a Section 8 certificate or voucher after the random assignment date, but were no longer receiving such assistance in the survey month.

The two most prevalent reasons were as follows:

- Income too high to qualify for assistance (25 percent); and
- Told no longer eligible for non-income reasons (23 percent)

These reasons accounted jointly for nearly one-half (48 percent) of those who exited. The first category appears to suggest policy-appropriate exits from the voucher program associated with changes in financial circumstances. Based upon the intensive interviews with 141 treatment group members, the second category is likely related to procedural non-

compliance on the part of the family, including failure to report changes in income or household composition or failure to recertify eligibility annually.

An additional two categories, representing more than a quarter (28 percent), reflect families who appear to have lost the voucher in the process of attempting a move.

- Moved and could not use assistance in new place (22 percent)
- Landlord would not accept voucher (6 percent)

The intensive interviews suggest that the opportunity to move comes with significant risks and challenges. Several respondents reported planning to move, giving notice to their current landlord, and then being terminated from the program because "their time ran out"—they were unable to find an acceptable unit before the voucher expired. The barriers to a successful move are numerous. A tenant must find a unit that passes inspection, save sufficient funds for security and utility deposits, and find a landlord that accepts the voucher.

Of the remaining categories, two are likely to represent positive situations.

- Moved in with spouse or partner (4 percent)
- No longer wanted to be on assistance (5 percent)

The interviewers talked with eight respondents who reported their assistance ended because they no longer needed it. These included five who reported that they no longer needed assistance because a spouse, fiancé, or boyfriend became a household member and began to contribute financially. The other three had completed college-level programs and had significant income increases. [58]

Based upon the intensive interviews, it would appear that the "other," "refused," and "moved in with family" categories (17 percent in total) are likely to represent families with serious problems. One of the dominant themes reported by families was how highly the voucher was valued as a means of establishing one's own household. Moving back in with family after receiving a voucher was generally perceived as a serious set back—generally not a choice, but a necessity. Among the intensive interview respondents who likely answered the survey questions this way were people with serious physical or psychiatric illness, others who lost voucher assistance when they entered drug rehabilitation programs, and those with limited education and coping skills.

[58] There is undoubtedly some overlap between these two categories and the “income too high to qualify for assistance” category, because respondents may not have been consistent on the fine point of whether the PHA told them or whether they knew they were over-income. Some interviewees reported that they had voluntarily relinquished their voucher before being terminated because they believed that "others needed it more." These comments seem to reflect an awareness that other families might be in even more difficult financial circumstances and an ethic about not wanting to use housing assistance resources themselves if not absolutely necessary.

3.5 Impacts Estimated on All Types of Families

The remainder of this chapter presents and interprets our estimates of the effects of vouchers on key participant outcomes relating to residential location and household composition. In this section, we discuss the impacts found across all types of families in the study for: residential mobility; neighborhood characteristics; neighborhood satisfaction, observed neighborhood problems, and personal safety; and household composition.

Impacts on Mobility

As indicated earlier, the initial descriptive analysis of out-of-tract mobility rates shows higher mobility among treatment cases than control cases. More accurate estimates of the treatment effect can be obtained in a multivariate context, using the estimation methodology described in Chapter 1. This methodology explicitly adjusts for treatment-control differences in baseline characteristics—differences that exist to some degree in any randomized experiment.

Vouchers were found to increase the probability of moving from one's baseline Census tract, based on the address history data. The effects were statistically significant and of similar magnitude at both quarter 16 (using address history data for all sites) and quarter 18 (using address history data for the five survey sites). For both quarter 16 and quarter 18, the intent-to-treat (ITT) effect was 3 percentage points (compared with a control mean of 53 percent at quarter 16). This is consistent with the differential observed in the descriptive analysis. At both intervals, the treatment-on-treated (TOT) effect was estimated at 11 percentage points . See the first row of Exhibit 3.5.

During the in-depth interviews, treatment-group respondents reported that the prospect of moving to another neighborhood was appealing but also fraught with challenges. Safety and better schools were the primary motivations for moving. Getting the children away from drugs, violence, and bad peer-group influences was a major decision factor. Finding a quiet, safe, and preferably single-family neighborhood was the ideal. Nearly all who made the move to a better location said that the move would not have been possible without the voucher.

The other indicator of mobility examined here was the reported number of moves made during the follow-up period, based on the survey responses to the question "How many times have you moved since [random assignment date]?" (This includes moves from one housing unit to another within the baseline tract, as well as moves to (or within) other tracts.) See the second row of Exhibit 3.5. ***Vouchers were found to reduce the number of residential moves over the follow-up period.*** Control group members on average had moved 1.98 times. The ITT effect was significant but small in magnitude (a reduction of 0.22, or proportionally about 10 percent.

The in-depth interviews suggested several possible reasons for fewer total moves by voucher holders. One is a lower incidence of eviction for nonpayment of rent. The second pertains to the procedural requirements, paperwork burdens, and financial expenses of moving. A voucher holder wishing to move faces the challenge of synchronizing three things: saving enough money for moving costs, complying with lease notice requirements, and obtaining "moving papers" from the PHA.

Exhibit 3.5
Impacts on Mobility

Outcome	Quarter 16, All Sites: Sample Size	Control Mean[a]	ITT Impact	TOT Impact	Quarter 18 (or Year 5), All Sites Except Los Angeles: Sample Size	Control Mean[a]	ITT Impact	TOT Impact
Moved out of baseline Census tract[b] (address history data)	8,657	0.534 0.499	0.029 *** (0.010)	0.110 *** (0.032)	7,614	0.594 0.491	0.026 ** (0.011)	0.109 *** (0.033)
Number of moves during follow-up period (survey data)	na	na	na	na	2,452	1.98 1.91	-0.22 *** (0.08)	-0.88 *** (0.30)
Number of months in end-period neighborhood (survey data)	na	na	na	na	2,432	42.76 67.04	0.55 (2.97)	2.17 (11.81)

Notes:
ITT = "Intent-to-Treat". TOT = "Treatment-on-Treated". Standard errors in parentheses.
*** indicates p < .01, ** indicates p < .05, * indicates p < .10
na = not applicable
[a] Standard deviations of control group outcomes are beneath control means.
[b] Impacts on this dichotomous outcome were estimated with the linear probability model to enable the calculation of TOT standard errors in the manner described in Appendix B.3.3.

There are other possible explanations for the reduced number of overall moves by the treatment group, together with their higher probability of residing outside the baseline Census tract. One is that control group members may have had a greater likelihood of an "out-and-back" combination of moves: a move out of the baseline tract followed by a move back. Another is that the control cases tended to experience more within-tract moves, never leaving the baseline tract. Yet another is that the treatment members, having improved their housing situation through an out-of-tract move that became affordable to them with the voucher, felt less need to move thereafter. The control cases, in contrast, made repeated moves in an attempt to improve their situation or avoid eviction without the benefit of the voucher subsidy.

In the in-depth interviews, those treatment group members who did not make moves to better locations often expressed concerns about childcare and other family support that depended upon staying in a particular area, sometimes combined with a sense of loyalty to the area where they were raised. A number of respondents also expressed concerns about whether landlords in "better" areas would accept the voucher and whether they could pass landlord screening, which was perceived to be more stringent in better areas, and afford upfront deposits, which in their experience were higher in the better areas.

Although voucher holders moved less often than control group members, many treatment group members did, nonetheless, make multiple moves. These voucher users reported a notable learning curve in the in-depth interviews. At voucher issuance, they often felt pressed for time, and many made expedient decisions that proved unsatisfactory. They reported

having learned from these experiences, making better decisions on subsequent moves. They cited specific techniques for sizing up a new neighborhood, including observing the area at various times of day. They also indicated that confidence in their abilities to negotiate with landlords increased in subsequent moves.

No full-sample treatment effect was found on the survey-based outcome variable that measured end-period residential stability: "how many months have you lived in your current neighborhood?" (third row of Exhibit 3.5). Among control group respondents, the average length of time in the neighborhood was 43 months. With the above-mentioned negative treatment effect on the number of moves, one might have expected a positive treatment effect on the length of residence in the end-period neighborhood. The ITT and TOT point estimates were indeed positive, but not statistically significant.

Impacts on Neighborhood Characteristics

By the end of the observed follow-up period, did treatment group members come to reside in neighborhoods of better quality than those where control group members resided? These are outcomes that can be measured for the full research sample (not just the survey sample), based on the constructed address histories covering 16 quarters in all sites and 18 quarters in all sites except Los Angeles. Moreover, both ITT and TOT estimates can be computed for these outcomes.

The voucher treatment was found to have a significant, favorable impact on the following indicators of neighborhood quality: poverty rate, employment rate, and welfare concentration. There were also significant treatment effects on measures of demographic composition: minority concentration, black concentration, and female-headedness. The estimated impact was significantly positive on the employment rate and significantly negative on all others, for ITT and TOT effects at quarters 16 and 18). See Exhibit 3.6. No significant effects were found on Hispanic concentration, educational attainment, and youth idleness. For those outcomes where treatment effects were significant, the estimated impacts were small in magnitude, in the range of 1 to 10 percent of the control mean.

It is of some interest to know the time pattern of the effects on those outcomes for which end-quarter estimates were significant. The effects may have emerged early or late in the follow-up period. To investigate this, we estimated on a quarter-by-quarter basis the ITT and TOT effects on the poverty rate, arguably the single most meaningful indicator of neighborhood quality. These estimates are shown in Exhibit 3.7.

In the analysis for all sites (through quarter 16) and all sites excluding Los Angeles (through quarter 18), the treatment effect on the poverty rate became significant by quarter 5 and then remained significant in all subsequent quarters. In magnitude, the effect measured in quarter 5 grew substantially over the remainder of the observation period: increasing proportionally by more than 50 percent in the ITT estimates (i.e., from 0.39 to 0.60 in the 16-quarter analysis and from 0.50 to 0.78 in the 18-quarter analysis). Over the same interval, the TOT estimates grew proportionally to an even greater extent, more than doubling from quarter 5 to the end-quarter.

Exhibit 3.6
Impacts on Characteristics of End-Period Census Tract
(Address History and Census Data)

Characteristic of end-period Census tract	Quarter 16, All Sites			Quarter 18, All Sites Except Los Angeles		
	Control Mean (%)[a]	ITT Impact	TOT Impact	Control Mean (%)[a]	ITT Impact	TOT Impact
Poverty rate	27.19	-0.60 ***	-1.85 ***	27.36	-0.78 ***	-2.36 ***
	14.25	(0.23)	(0.70)	14.64	(0.26)	(0.76)
Employment rate	87.51	0.25 **	0.80 **	87.42	0.28 **	0.93 ***
	6.65	(0.11)	(0.33)	6.84	(0.13)	(0.36)
Educational attainment	83.71	0.14	0.47	84.51	0.23	0.66
	12.86	(0.18)	(0.54)	12.95	(0.20)	(0.58)
Youth idleness	9.36	0.00	0.04	9.37	-0.04	-0.02
	7.19	(0.13)	(0.40)	7.19	(0.15)	(0.42)
Welfare concentration	10.05	-0.21 *	-0.68 **	9.84	-0.31 **	-0.97 ***
	7.58	(0.11)	(0.34)	7.72	(0.13)	(0.37)
Minority concentration	69.94	-0.54 *	-1.60 *	68.79	-1.05 ***	-2.78 ***
	28.56	(0.31)	(0.92)	29.24	(0.35)	(1.00)
Black concentration	32.56	-0.76 **	-2.35 **	34.24	-1.04 ***	-3.08 ***
	34.63	(0.35)	(1.05)	35.37	(0.40)	(1.15)
Hispanic concentration	28.55	0.25	0.81	26.39	0.08	0.52
	25.54	(0.29)	(0.85)	25.28	(0.31)	(0.89)
Female-headedness	22.27	-0.29 *	-0.88 *	22.64	-0.47 ***	-1.26 **
	10.33	(0.15)	(0.46)	10.57	(0.17)	(0.50)

Notes:
N = 8,657 for regressions run on all sites. N = 7,614 for regressions run on all sites except Los Angeles.
ITT = "Intent-to-Treat". TOT = "Treatment-on-Treated". Standard errors in parentheses.
*** indicates p < .01, ** indicates p < .05, * indicates p < .10
[a] Standard deviations of control group outcomes are beneath control means.

Exhibit 3.7
Quarter-by-Quarter Impacts on Poverty Rate of Census Tract (Address History and Census Data)

	Quarters 1-16, All Sites			Quarters 1-18, All Sites Except Los Angeles		
Follow-up quarter	Control Mean (%)[a]	ITT Impact	TOT Impact	Control Mean (%)[a]	ITT Impact	TOT Impact
1	29.31	0.04	0.11	30.03	0.02	0.04
	14.63	(0.05)	(0.13)	15.00	(0.05)	(0.13)
2	29.07	-0.11	-0.35	29.77	-0.17 *	-0.42 *
	14.55	(0.09)	(0.23)	14.92	(0.10)	(0.23)
3	28.74	-0.11	-0.13	29.42	-0.18	-0.28
	14.52	(0.13)	(0.28)	14.91	(0.15)	(0.29)
4	28.55	-0.24	-0.59 *	29.18	-0.34 *	-0.73 **
	14.46	(0.16)	(0.33)	14.85	(0.18)	(0.35)
5	28.30	-0.39 **	-0.83 **	28.89	-0.50 **	-1.00 **
	14.35	(0.18)	(0.39)	14.75	(0.20)	(0.41)
6	28.10	-0.46 **	-0.93 **	28.68	-0.58 ***	-1.14 ***
	14.40	(0.20)	(0.42)	14.80	(0.22)	(0.44)
7	27.92	-0.56 ***	-1.23 ***	28.45	-0.67 ***	-1.40 ***
	14.43	(0.21)	(0.45)	14.83	(0.23)	(0.48)
8	27.64	-0.50 **	-1.03 **	28.15	-0.62 ***	-1.29 **
	14.45	(0.22)	(0.49)	14.85	(0.24)	(0.52)
9	27.45	-0.50 **	-1.24 **	27.95	-0.65 ***	-1.54 ***
	14.47	(0.22)	(0.53)	14.88	(0.25)	(0.56)
10	27.29	-0.46 **	-1.14 **	27.76	-0.60 **	-1.44 **
	14.41	(0.23)	(0.56)	14.82	(0.25)	(0.59)
11	27.21	-0.50 **	-1.42 **	27.68	-0.66 ***	-1.76 ***
	14.38	(0.23)	(0.59)	14.78	(0.25)	(0.62)
12	27.13	-0.48 **	-1.34 **	27.59	-0.63 **	-1.68 **
	14.38	(0.23)	(0.62)	14.78	(0.26)	(0.65)
13	27.12	-0.50 **	-1.51 **	27.59	-0.67 ***	-1.89 ***
	14.35	(0.23)	(0.64)	14.75	(0.26)	(0.68)
14	27.13	-0.50 **	-1.50 **	27.60	-0.68 ***	-1.92 ***
	14.36	(0.23)	(0.66)	14.76	(0.26)	(0.70)
15	27.16	-0.50 **	-1.55 **	27.63	-0.69 ***	-1.99 ***
	14.33	(0.23)	(0.68)	14.72	(0.26)	(0.72)

Follow-up quarter	Quarters 1-16, All Sites: Control Mean (%)[a]	ITT Impact	TOT Impact	Quarters 1-18, All Sites Except Los Angeles: Control Mean (%)[a]	ITT Impact	TOT Impact
16	27.19	-0.60 ***	-1.85 ***	27.66	-0.81 ***	-2.32 ***
	14.25	(0.23)	(0.70)	14.63	(0.26)	(0.73)
17	na	na	na	27.54	-0.76 ***	-2.14 ***
				14.70	(0.26)	(0.75)
18	na	na	na	27.36	-0.78 ***	-2.36 ***
				14.64	(0.26)	(0.76)

Notes:
N = 8,657 for regressions run on all sites. N = 7,614 for regressions run on all sites except Los Angeles.
ITT = "Intent-to-Treat". TOT = "Treatment-on-Treated". Standard errors in parentheses.
*** indicates p < .01, ** indicates p < .05, * indicates p < .10
na = not applicable
[a] Standard deviations of control group outcomes are beneath control means.

Based on the time path of findings for this key outcome, it thus appears that the locational advantages afforded to the treatment group--through its pattern of mobility relative to that of the control group--became progressively larger as time passed.

The treatment group members who were interviewed in depth took into consideration the issues represented by these various Census indicators, but tended to use different descriptors. A few respondents expressed preference for "mixed" (racial/ethnic) neighborhoods, which were believed to provide stronger role models and external influences for themselves and their children. More hoped for a "quiet," "elderly," or "family" neighborhood, meaning one that was less dense, with yards or other open spaces where children could safely play. Most respondents were less concerned about poverty levels and income sources and more concerned with behavior, especially whether neighborhood children were adequately supervised. However, others who felt that they had found a good neighborhood for their family were subsequently disappointed when other low-income families also moved close by.

Impacts on Neighborhood Satisfaction, Observed Problems, and Personal Safety

The survey respondents were asked a series of questions regarding their experiences in and perceptions of their current neighborhood. As indicated in Exhibit 3.8, there was no significant sample-wide effect on general neighborhood satisfaction. Among control cases, 63 percent reported being “somewhat” or “very” satisfied with their neighborhood.
As for neighborhood conditions identified as either a “big” or a “small” problem (versus “no problem”), ***the treatment was found to have a favorable (negative) effect on the reported extent of graffiti and the extent of people drinking in public.*** There were no effects on the extent of litter or trash on the streets or sidewalk, abandoned buildings, or groups of people hanging out on the street. (On the last outcome, the effect was negative and marginally significant, at the 0.10 level.) Among control group cases, 29 percent reported a “big problem” with at least one of these issues. There was also no effect on the incidence of

seeing people using or selling drugs in the neighborhood. Such drug activities were seen at least once a week over the past 30 days by 17 percent of control cases.

There was no significant effect on the reported extent of crime victimization—that is, on the respondent (or someone in her household) having been recently robbed on the street, threatened with a knife or gun, beaten or assaulted, stabbed or shot, or having a home break-in. (On the last of these, the effect was negative and marginally significant.) Among control cases, 15 percent had experienced one or more of the above forms of crime within the previous six months.[59]

The in-depth interviews suggested that neighborhood satisfaction is a complex issue for many voucher holders. Respondents often had to make some trade-offs and rarely found all of their needs met by a single move. Sometimes respondents were single-minded in their decision-making. For example, respondents who were living in very poor quality housing often focused on improving their housing situation, even if this meant moving to a less desirable neighborhood or farther from family or work. Similarly, housing choice for respondents without personal transportation was driven by access to public transportation. In addition, satisfaction may sometimes be a relative term, meaning "I can deal with it for now," without regard to an objective standard.

As to personal safety, although several in-depth respondents reported having been victims of assault, mothers interviewed were concerned primarily about the safety of their children. In addition to moving from dangerous neighborhoods when they could, they employed a variety of strategies to keep their children safe. These included walking or driving the children to and from school, not letting children go outside unless with an adult family member, and personally not going outside after dark. This often severely curtailed the time they had available for other activities. For example, in order to work, such an individual needed an employment schedule that still enabled her to take the children to school and pick them up at the end of the day.

Impacts on Household Composition

Nearly one-fourth (23 percent) of control group members reported living with a spouse or partner at the time the survey was taken (Exhibit 3.9). Vouchers had no statistically significant effect on this choice. The intensive interviews suggested that many treatment group members thought that the rules of the voucher program—or the TANF program—prohibited a “man in the house.” Therefore, it is possible that living with a spouse or partner was under-reported to the survey by both treatment and control group members—and perhaps more so by treatment group members. Similarly, 38 percent of controls reported that they had lived with a spouse or partner at some point since random assignment, and there was no significant impact of the voucher on this.

[59] This incidence of victimization is noteworthy, as it indicates the appreciable degree of danger regularly faced by low-income urban families.

Exhibit 3.8
Impacts on Neighborhood Satisfaction, Observed Problems, and Personal Safety (Survey Data)

	Fifth Year, All Sites Except Los Angeles			
	Sample Size	Control Mean[a]	ITT Impact	TOT Impact
"Somewhat satisfied" or "very satisfied" with your neighborhood	2,469	0.634 0.482	0.003 0.022	0.012 0.088
There is a "big problem" or a "small problem" with:				
Litter or trash on the streets or sidewalk	2,469	0.498 0.500	-0.036 0.023	-0.144 0.092
Graffiti or writing on the walls	2,467	0.293 0.456	-0.040 ** 0.020	-0.158 ** 0.079
People drinking in public	2,461	0.358 0.480	-0.046 ** 0.021	-0.185 ** 0.085
Abandoned buildings	2,466	0.198 0.399	-0.024 0.017	-0.094 0.066
Groups of people just hanging out	2,465	0.409 0.492	-0.042 * 0.022	-0.166 * 0.089
There is a "big problem" with any of above conditions	2,460	0.286 0.452	0.003 0.020	0.013 0.080
Have seen people using or selling drugs in neighborhood at least once a week in past 30 days	2,431	0.173 0.378	-0.020 0.015	-0.079 0.059
During the past six months, you (or someone in your household):				
Had a purse, wallet, or jewelry snatched	2,385	0.045 0.206	0.001 0.004	0.003 0.017
Had a break-in (or attempted break-in) to your home	2,411	0.083 0.277	0.019 * 0.011	0.075 * 0.045
Were threatened with a knife or gun	2,337	0.036 0.187	-0.003 0.003	-0.011 0.013
Were beaten or assaulted	2,365	0.043 0.203	0.003 0.006	0.012 0.025
Were stabbed or shot	1,830	0.018 0.133	-0.007 0.039	-0.026 0.154
Experienced any of the above	2,446	0.150 0.357	0.016 0.015	0.065 0.060

Notes:
N = 2,472 for regressions run on all sites except Los Angeles.
ITT = "Intent-to-Treat". TOT = "Treatment-on-Treated". Standard errors in parentheses.
*** indicates p < .01, ** indicates p < .05, * indicates p < .10
[a] Standard deviations of control group outcomes are beneath control means.

Exhibit 3.9
Impacts on Presence in Household of Spouse/Partner and Children (Survey Data)

	Fifth Year, All Sites Except Los Angeles			
Status at follow-up survey	Sample Size	Control Mean[a]	ITT Impact	TOT Impact
Living with a spouse or partner	2,477	0.230	-0.017	-0.068
		0.421	(0.018)	(0.071)
Have lived with the same spouse or partner since random assignment (subset of above)	2,480	0.148	-0.015	-0.060
		0.355	(0.011)	(0.042)
Ever lived with a spouse or partner since random assignment	2,480	0.378	-0.023	-0.091
		0.485	(0.023)	(0.093)
Have lived with more than one spouse or partner since random assignment (subset of above)	2,083	0.020	0.0003 **	-0.001 **
		0.140	(0.0003)	(0.001)
Number of birth children in household	2,481	2.608	0.011	0.042
		1.471	(0.050)	(0.199)
Number of birth children in household born since random assignment (subset of above)	2,481	0.389	0.012	0.048
		0.692	(0.027)	(0.108)

Notes:
N = 2,481 survey respondents (all sites except Los Angeles).
ITT = "Intent-to-Treat". TOT = "Treatment-on-Treated". Standard errors in parentheses.
*** indicates p < .01, ** indicates p < .05, * indicates p < .10
[a] Standard deviations of control group outcomes are beneath control means.
Statistical significance is derived from underlying probit coefficient and robust standard error.
See Appendix B.3 for more details about impact estimation with the probit model.

A small percentage of control group members reported that they had lived with more than one spouse or partner since random assignment, and, as indicated in Exhibit 3.9, vouchers were found to significantly reduce this percentage. Stated otherwise, vouchers raised the likelihood of having lived with either one or no spouse or partner since random assignment, indicating some increase in the stability of marital status or cohabitation arrangements, but with no effects on the formation of such relationships.

Some intensive interview respondents reported that voucher assistance enabled them to stop living with partners in unsatisfactory or abusive circumstances, and others reported satisfaction with being able to leave a doubled-up housing arrangement and establish their own household without having to move in with a boyfriend or partner. However, other respondents reported that the voucher caused an unwanted family break-up or discouraged the formation of a two-adult household. In all five cities the belief was widespread that voucher program rules prohibited males who were unrelated to the leaseholder from living in a voucher-assisted unit.

A few respondents thought this "rule" applied even to their husbands.[60] In some cases this led to the break up of two-parent families, causing fathers of children and boyfriends to move back in with their own parents or establish separate households. In other cases, respondents appeared to disregard what they thought to be the program rule and continued to live with the boyfriend, often not reporting the individual's presence to the housing authority. Respondents to the intensive interviews, even those who were quite candid in other sensitive areas, were reluctant to talk about the presence of boyfriends and children's fathers in the unit, presumably because of concerns about losing the voucher. This reluctance may have affected the survey responses and thus the impact findings.

There was no significant effect on the number of children born to the primary adult since random assignment.

In contrast to the essentially non-existent effect of the voucher on marriage, other unions, or fertility, many of the primary adults in the study (almost all women) used their vouchers to leave multigenerational households. As indicated in Exhibit 3.10, significant treatment effects were estimated for both the size and the structure of the household. Vouchers were found to reduce the number of other adults—elders, siblings, and other relatives—living with the primary adult. There was no significant impact on the number of children living with her. Fifteen percent of the control households in the survey sample reported living in multigenerational households. The effect of the voucher was a significant reduction in this living arrangement.[61]

By reducing the number of adults living in the household, the voucher also reduced household size. Across all study households (including those not in multigenerational households), the treatment effect was to reduce the average size of the household by 0.18 members for those offered the voucher (the ITT estimate), in comparison to a control mean at the time of the follow-up survey of 4.3 members.

The absence of any positive effect on union formation, combined with the reduced likelihood of elders, siblings, or other relatives residing in the household with the primary adult, led to an increase by 6.1 percentage points in the incidence of households consisting solely of a single parent and children, the type observed among 64 percent of the control group. Only 19 percent of control group households were classified as a “nuclear families” (two parents with children), and the voucher had a very small and statistically insignificant effect on this category.[62]”

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60 The source of this misunderstanding is unclear. One could speculate that this belief came from confusion between voucher and welfare program rules, although some respondents reported that housing authority staff had provided this information.

61 One favorable byproduct of the reduction in multigenerational households may have been a decline in overcrowding, a significant finding discussed in Chapter Five.

62 There was a marginally significant negative effect on the “all others” household type, which included shared housing with an adult who was not a spouse or partner.

Exhibit 3.10
Impacts on Household Size and Type (Survey Data)

	Fifth Year, All Sites Except Los Angeles			
	Sample Size	Control Mean[a]	ITT Impact	TOT Impact
Household size (number of members, by relationship to adult respondent):				
Children	2,481	2.744	0.010	0.040
		1.437	(0.049)	(0.196)
Elders	2,481	0.035	-0.019 **	-0.078 **
		0.194	(0.008)	(0.031)
Siblings	2,481	0.076	-0.044 ***	-0.175 ***
		0.341	(0.013)	(0.051)
Other relatives	2,481	0.381	-0.134 ***	-0.534 ***
		0.912	(0.038)	(0.151)
Other non-relatives	2,481	0.046	-0.023	-0.090
		0.357	(0.014)	(0.056)
Total (including adult respondent)	2,481	4.343	-0.182 ***	-0.726 ***
		1.779	(0.061)	(0.241)
Household type				
Single parent with children only	2,480	0.635	0.061 ***	0.243 ***
		0.482	(0.022)	(0.089)
Nuclear family (two parents with children)	2,480	0.193	0.003	0.012
		0.394	(0.016)	(0.065)
Multigenerational	2,458	0.145	-0.051 ***	-0.203 ***
		0.352	(0.014)	(0.055)
All others	2,381	0.028	-0.003 *	-0.011 *
		0.165	(0.005)	(0.020)

Notes:
N = 2,481 survey respondents (all sites except Los Angeles).
ITT = "Intent-to-Treat". TOT = "Treatment-on-Treated". Standard errors in parentheses.
*** indicates p < .01, ** indicates p < .05, * indicates p < .10
[a] Standard deviations of control group outcomes are beneath control means.

The in-depth interviews provide important insights about the opinions and decision-making associated with smaller household sizes and moving out of intergenerational households. Key issues raised in the in-depth interviews on this topic were:

- Although the hardships of single parenting were acknowledged, those who had lived in multi-generational households generally viewed establishing a stable, single-parent household as an improvement.
- Even respondents who had not been doubled-up at baseline viewed the potential necessity of moving in with family members or friends as a step backward and credited voucher assistance with helping them to avoid this circumstance.
- Single-parent households were also a better solution for those attempting to escape a difficult or abusive domestic situation.
- Respondents attached important meanings to "having a home of one's own" including: a sense of increased stability and independence; improved ability to supervise and protect their children; and an enhanced acceptance of responsibility.
- The establishment of these separate households often addressed overcrowding.
- The in-depth interview respondents did not describe their moves away from multi-generational households as having an adverse effect on the amount of support provided by family members and friends. When it was available, support from the extended family did not seem to depend upon living in the same housing unit.

Refer to Chapter Five for the estimated effects of voucher use on housing security outcomes, including homelessness, crowding, and self-reported housing quality.

3.6 Impacts Estimated by Subgroup

This section presents impact estimates for the subgroups among the families in the study, as defined by baseline economic and demographic characteristics. The exhibits for this section appear in Appendix D.

Impacts on Mobility for Subgroups

Impacts were estimated by subgroup for whether the family moved out of the baseline Census tract (from the address history data), for the number of moves during the follow-up period (from the survey data), and for the number of months in the end-period neighborhood (from the survey data).

Out-of-tract mobility. As shown in Exhibit D.1, the treatment was found to increase the probability of moving out of the baseline tract for a number of subgroups. The treatment effects on this outcome were significant among those in potentially stressful housing conditions (those in public or assisted housing projects or in shared or temporary housing arrangements) and those with greater economic barriers or hardship (younger, less educated, with pre-school children, not employed, never employed, or receiving TANF). Others with significant treatment effects on out-of-tract mobility were subgroups that may have been less tied or connected to their baseline location (those not enrolled in school or training, those with no dependent children) or that did not face discrimination in prospective housing moves (those identifying themselves as white non-Hispanic).

The treatment effect on out-of-tract mobility was significantly positive among those who did *not* say that they wanted to move for employment reasons (comprising 88 percent of those surveyed). The positive treatment effect for those not wanting to move for job-related reasons is likely associated with other, more compelling, reasons to move, "push factors" related to personal safety and child well-being. The intensive interviews found that such non-job factors (e.g., the desire to escape an overcrowded, stressful housing arrangement) tended to be more urgent than job-related factors in making the decision whether and where to move.

Number of moves. Exhibit D.2 shows the estimates by subgroup for the survey-reported number of moves during the follow-up period. When the impact was significant, it was always negative (i.e., fewer moves), as it was for the sample-wide effect. The treatment resulted in fewer moves among some subgroups for whom relocation would have been particularly disruptive (youngest household member less than 6 at baseline). The mobility-reducing treatment effect was also pronounced among those not employed, perhaps because of health-related or child-related barriers that would make moves more difficult to accomplish. Consistent with the explanation offered above regarding the intention to move for job-related factors, the voucher was found to reduce the number of moves among those who desired to move for employment reasons, a group for whom strong push factors were not operating.

Number of months in end-period neighborhood. The subgroup estimates for this outcome are not shown, as only one subgroup had a significant (positive) treatment effect. This was the subgroup that desired to move for employment reasons. Again, consistent with the explanations above, the treatment cases in this subgroup may have been inclined to lease in place or settle in more quickly to a satisfactory relocation, whether or not this improved their employment situation (an issue raised in Chapter 4).

Impacts on Neighborhood Characteristics for Subgroups

Impacts by subgroup were estimated for the primary neighborhood quality indicator, the poverty rate of the end-period tract. These estimates are shown in Exhibit D.3. Wherever significant, the treatment effects were favorable (i.e., negative), consistent with the sample-wide effect.

Effects were significant for those with greater potential economic hardship or in more stressful arrangements: black non-Hispanic, young, less educated, those with dependent children, those with household members under 6, and those residing in public or assisted housing. For those subgroups, any move from their baseline housing location was likely to be an improvement. Significant effects were also found for some subgroups that were more able to move to better locations because they were not locationally constrained (not enrolled in school or training) and were perhaps more able to afford a move (employed, not receiving TANF).

Impacts on Neighborhood Satisfaction, Observed Problems, and Personal Safety for Subgroups

We estimated effects by subgroup for three outcomes: whether the survey respondent was "very" or "somewhat" satisfied with her neighborhood, whether she had a "big" problem with any several observed neighborhood conditions, and whether she had been victimized by

any of the crime types as previously shown. On these outcomes, the number of subgroups with significant treatment effects was very small--about as many as one would expect from chance alone, given the very large number of tests performed (39 subgroups for each outcome). For this reason, we do not show these estimates in exhibits.

Impacts on Household Composition for Subgroups

Impacts were estimated by subgroup for all of the outcomes related to household composition shown in Exhibits 3.9 and 3.10. For only a few of these variables did we find the number of significant subgroup effects to appreciably exceed the number that would have been expected by chance alone. For these variables, the subgroup estimates are shown in Appendix D (Exhibits D.4 and D.5).

For the following subgroups, voucher use was found to significantly increase the proportion of households consisting of a single parent with children and to significantly decrease household size: those with at least a high school diploma, those not enrolled in school or training, those with dependent children, those with children in the household under the age of six, those ever employed, those not desiring to move for employment, those in shared or temporary housing, and those receiving TANF.

3.7 Interpretation of Impact Estimates

By increasing the range of affordable housing choices, vouchers allowed families to locate themselves in neighborhoods of better quality. The time pattern of effects on the tract-level poverty rate suggests that treatment cases gained locational advantages by the start of the second year and that these effects grew substantially in the third and fourth years.

The estimated treatment effects indicate a pattern of residential outcomes for voucher users that combined greater out-of-tract mobility with greater overall housing stability. Participants were more likely to have moved out of their baseline tract, yet experienced fewer moves over the entire course of the demonstration period. One might have expected also to find a positive treatment effect on the length of time at the end-period neighborhood, but this was not evident in the data.

With treatment cases experiencing fewer moves than control cases, the result suggests that the voucher enabled treatment cases to make moves to neighborhoods of better quality than the neighborhoods into which control group cases were moving or, in some cases, to remain in better quality neighborhoods. (Some treatment group members may have leased in place or remained in the location to which they first moved with the voucher, while some number of their control group counterparts needed to move to lesser-quality neighborhoods.) In whichever way these residential dynamics were played out, the resulting pattern of locational advantages to the treatment group appears in the tract-level indicators of neighborhood quality, both across multiple Census indicators and across many subgroups of the sample (as evident in the pattern for the poverty rate outcome).

The size of the treatment effects on the neighborhood quality indicators is typically very small—less than 10 percent of the control group mean. This may be why we do not find any strong corresponding patterns of favorable effects in the survey-measured outcomes for

neighborhood satisfaction, observed neighborhood problems, and personal safety. The outcomes based on survey questions reflect some degree of subjective judgment on the part of respondents and thus are subject to greater measurement variability. For this reason, it is not altogether surprising to have found so few significant effects in these survey outcomes for the entire sample or for subgroups.

The effects on household composition indicate that treatment group members tended to use their vouchers to pursue independent living arrangements for themselves and their children. Most prominent among the findings was the decreased likelihood of the primary sample member and her children living in a multigenerational household. This is consistent with the expectation that vouchers provide the financial means by which a parent may exit from an overcrowded and stressful living arrangement with extended family members. At the same time, the absence of any effect on marriage or cohabitation suggests that vouchers do not provide a positive economic incentive to find a spouse or partner, but neither do they provide a negative incentive.

With no increase in household size from fertility, marriage, or cohabitation, and with a reduced likelihood of living with elders, siblings, or other relatives, the voucher caused a reduction in household size. This effect, although significant, was small in size—less than 5 percent of the control mean for the sample-wide ITT estimate (Exhibit 3.10) and at most 10 percent of the control mean for subgroups (Exhibit D.4). Nonetheless, as described in Chapters 4 and 5, these compositional effects may be partly responsible for the impacts found on household-level measures of economic well-being.

Chapter Four
Impacts on Employment, Means-Tested Benefits, and Education

This chapter presents the estimated impacts of the Housing Choice Voucher on employment rates and earnings amounts, receipt of means-tested public assistance benefits, and education and training of the heads of welfare families. The impact estimates are based primarily on administrative data from the Unemployment Insurance (UI) system and from state and local agencies administering TANF and the Food Stamp program. Additional outcomes reflect self-reported work histories from the follow-up surveys and responses to survey questions about employment, education, training, and barriers to work. Both for developing hypotheses and for interpreting findings, we draw on qualitative evidence from in-depth interviews with voucher users.

After a brief summary of the findings, the chapter discusses the hypothesized effects of the Voucher program on outcomes related to employment and receipt of public assistance, the data sources and measures used in the analysis, the sample members' baseline educational attainment, prior work and earnings, and welfare receipt. We then present impact estimates for all types of families in the study and for subgroups.

4.1 Summary of Findings

Housing Choice Vouchers may improve long-run labor market outcomes for participants and their families by providing voucher recipients additional resources with which to stabilize their families, help care for their children, and invest in education and training. Vouchers may also provide families an opportunity to relocate to neighborhoods that are closer to jobs or have community norms more supportive of work. The program may also create incentives to work less, however, at least in the short run. Economic theory predicts that income-conditioned subsidies such as housing vouchers, which simultaneously increase family resources and reduce the marginal returns to work through the benefit reduction rate, will reduce work effort. In our earlier analysis of the impacts of vouchers on welfare families over the first 5-7 quarters after random assignment, we did in fact find small negative impacts on work effort and, consequently, increased reliance on public assistance.

The findings presented in this chapter confirm that having and using a voucher *reduced* employment rates and earnings amounts in the first year or two after random assignment. However, the small negative impact of vouchers disappeared over time, and vouchers had no significant impact overall on employment and earnings over 3.5 years of follow-up.

One of the ways in which vouchers may affect employment and earnings is through increased education and training, made possible by the additional household resources freed up by the voucher or by time freed up by any reduction in employment made possible by the voucher. Although there was some evidence from in-depth interviews with voucher users that voucher recipients did take advantage of this opportunity to upgrade their skills, the impact analysis

shows no significant difference between treatment and control group members in the amount or type of education and training received during the follow-up period.

Although we find significant negative impacts on employment and earnings only in the early part of the follow-up period, housing vouchers significantly increased total public assistance benefits[63] received by treatment group participants throughout the entire follow-up period. The continued effect on receipt of public assistance appears to reflect the impact of the voucher on family composition: use of a voucher increased the proportion of single parent with children households (with no other adults present) versus all other household types at the time of the follow-up survey by 24 percentage points, or about a third of the control mean. It seems likely that this effect occurred because the voucher made it possible for single parents to live on their own. This interpretation is buttressed by a 20 percentage point reduction in the proportion of multigenerational households.[64]

4.2 Hypothesized Effects of Housing Vouchers

Standard economic theory predicts that housing vouchers will reduce the labor supply of those who receive them. Vouchers affect the household budget constraint, and therefore the labor supply decision, in two ways: they increase the amount of unearned income available to the family, and they reduce the marginal return to work (because the value of the voucher is reduced by additional earnings). In a standard static analysis of labor supply, both of these effects (known in the literature as "income" and "substitution" effects) reduce desired hours of work.

This theory relies on some important simplifying assumptions. First, it predicts only the short-run response to the subsidy. Any effects of housing stability, increased discretionary income, and moves to neighborhoods more supportive of work may occur only in the longer term. Second, it assumes that the household can freely choose its hours of work, both with and without the voucher—i.e., it assumes away involuntary un- or underemployment. Third, it treats the "tax" represented by the voucher benefit reduction rate as the *only* tax facing the household. Fourth, it treats the housing subsidy as an increase in income, rather than a reduction in the price of the subsidized commodity. Finally, it abstracts from any effects of the voucher on the residential location of the household that may affect labor supply decisions. Relaxing these assumptions calls into question the theoretical result that housing vouchers should unambiguously reduce labor supply.

Relaxing the short-term, static focus of the standard model opens up two possibilities for vouchers to *increase* labor supply:

1. Use of a housing voucher may increase the stability of the family, which may decrease stress and lead to an improved sense of control and ability to plan their lives. This may result in more active job search, greater likelihood of job retention and, therefore, increased employment and earnings. Such effects are likely to become evident only in the longer term.

[63] The term "total public assistance" refers to the combined total of TANF and Food Stamp benefits.

[64] These impacts on household composition are shown in Exhibit 3.10 and discussed in Chapter 3. The effects cited are the TOT impact estimates.

2. While the reduction in housing costs associated with the voucher will tend to reduce work effort as the pre-existing level of consumption can be maintained for less work, program participants may use their increased discretionary income to pursue educational or training opportunities. This may improve earnings for the family *in the long run*.

Taking account of the fact that the housing voucher benefit reduction rate is not the only tax rate facing the household also tends to mitigate the effects predicted by the simple economic theory:

3. Households receiving TANF may face taxes on earnings close to 100 percent, even in the absence of the voucher.[65] Increased earnings in such households do little to increase household income. Since the voucher amount is based on household income *including TANF*, earnings have little effect on the value of the voucher. Thus, the voucher adds little to the existing (high) disincentive to work.[66] If we were to take into account the food stamp benefit reduction rate, the positive tax portion of the EITC, FICA, and state and Federal income taxes, the combined tax rate would be even higher, and the increase attributable to the housing voucher even less.

Those program participants who lease up and move may experience temporary reductions in employment that dissipate in the longer-run, for the following reasons:

4. Residential relocation may lead to temporary disruptions in earnings and employment for persons who were already working and need to take time off for housing search and relocation or change jobs entirely. Voucher recipients who were engaged in job search may suspend their job search in order to search for housing and/or to relocate.
5. Residential relocation may disrupt pre-existing social support networks that are important sources of informal childcare and labor market information and connections, with resultant negative effects on employment and earning. These disruptions could reduce employment and earnings until new social networks have been established.

Finally, several mechanisms may lead to higher employment and earnings among participants who use the reduction in the price of housing afforded by a voucher to move to (or stay in) better neighborhoods (i.e., neighborhoods with lower poverty rates and higher employment rates):

6. Residing in an area close to potential sources of employment may reduce job search costs and lead to a broader range of employment opportunities. Also, the expectation of lower commuting costs may reduce the reservation wage during job search. Once the

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[65] During the follow-up period, the three sites in Georgia and Texas had effective marginal tax rates of 100 percent on earnings for TANF recipients after the first four months of TANF receipt. (In the first four months, the effective marginal tax rate, or benefit reduction rate, was 66.7 percent in Georgia and 10 percent in Texas.) The three sites in California and Washington had effective marginal tax rates of 50 percent on earnings for TANF recipients throughout the follow-up period.

[66] Suppose, for example, that TANF benefits are reduced by $.80 for every $1.00 of earnings. If earnings increase by $1.00, household income increases by only $.20 (= $1.00 x (1-.80)), and the value of the voucher falls by only 30 percent of $.20, or $.06.

participant is employed, commuting costs may be lower, which may lead to increased hours of work and earnings.

7. Community norms in lower-poverty neighborhoods may be more supportive of work and less supportive of welfare. To the extent that recipients feel increased pressure to work and to leave welfare sooner, this might increase job search, employment, and earnings.
8. Residing in a safer neighborhood may decrease family stress and improve mental and physical health, enabling more active job search, longer job retention, and, therefore, increased employment and earnings.

Thus, to the extent that a high percentage of treatment group participants came to reside in better neighborhoods, we might expect increased levels of employment and earnings. As seen in Chapter 3, however, even for those treatment subgroups that did come to reside in somewhat better neighborhoods, the treatment-control differences in neighborhood quality by the end of the follow-up period were relatively small. In-depth interviews suggest that employment opportunities generally are not a high priority consideration when voucher holders consider moving and that some key barriers to employment (inadequate job skills and complicated family situations) are not overcome by living in better neighborhoods.

It is difficult to predict *a priori* the direction of the effect of vouchers on employment and earnings in the medium or long term. The income and substitution effects of standard economic theory may be offset by the more positive effects of improved neighborhood influences and employment opportunities or by uses of the increased discretionary income freed up by the voucher that enhance employability in the long run.

There is little reliable empirical evidence on the effects of housing vouchers on employment. The findings of our previous analysis of the impact of vouchers provided to welfare families show that program participation *reduced* employment rates and earnings amounts in the short-run (Patterson, et al., 2004). On balance, the negative effects of vouchers on work incentives outweighed the positive effects over the first seven quarters after random assignment. These negative impacts were, however, quite small: earnings were, on average, 12-14 percent lower among treatment group members who leased up than among their control counterparts, while the amount of time spent employed over the period was 7-8 percent less. The negative income tax experiments of the 1970s provide the most reliable estimates of income and substitution effects for low-income workers over a somewhat longer, 3-5 year period. For negative income tax plans that involved larger transfers of unearned income and higher tax rates than current housing vouchers, the experiments estimated labor supply reductions by female heads of household that averaged 15 percent of earnings across the experiments that included such households.[67] However, the cash transfer programs tested in these experiments did not have some of the locational effects that housing vouchers may have, as discussed above.

The available experimental evidence on locational effects on employment and earnings from the Moving to Opportunity Demonstration (Orr et al., 2003), casts doubt on the importance of those locational effects. Although the mobility program tested in that demonstration engineered a relatively large treatment-control difference in neighborhood characteristics

[67] See Burtless (1987), Table 2.

(e.g., a 21 percentage point difference in the neighborhood poverty rate between treatment group members who leased up and controls), there were no statistically significant effects on employment or earnings in the first four years after random assignment. Since, as shown in the previous chapter, the mobility effects of the present demonstration were much more modest, we might expect little or no locational effect on employment outcomes.

Shroder (2002) reviewed 18 nonexperimental studies of the impacts of housing vouchers on employment and earnings. He found that most of these studies were subject to relatively serious simultaneity or selection bias, but that, as a group, "the distribution of results...is consistent with a true housing assistance/short-term employment effect of zero." Moreover, the more methodologically sophisticated studies did not show greater negative effects than the less sophisticated.

Two more recent nonexperimental studies provide somewhat conflicting evidence. Olsen et al. (2005) found that vouchers reduced earnings by at least 30 percent, whereas Susin (2005) found a marginally significant reduction of about 10 percent of family earnings. Although both studies took a number of steps to eliminate selection bias, they drew comparison groups from data sources different from the data for the voucher group: the Panel Study of Income Dynamics (Olsen) and the Survey of Income and Program Participation (Susin). This opens the possibility of differences in unmeasured characteristics between assisted and unassisted families.[68]

The voucher's expected effect on the receipt of public assistance is determined primarily by its impacts on employment and earnings. An additional mechanism is the relationship of housing assistance to household composition. Receipt of housing assistance may permit multigenerational or extended family households to break into smaller units, most likely based on the nuclear family. In addition, housing assistance may permit recipients to exit stressed relationships, either breaking up nuclear family units (where a recipient separates from the father of her children) or breaking up relationships with "other" adults—e.g., a partner who is not the children's father. Smaller family units generally will be eligible for fewer welfare benefits, although per-person assistance may be unchanged. However, a participant who uses her voucher to become a single parent may well find herself eligible for increased benefits, particularly if she separates from a partner who was earning income.

4.3 Data Sources for Impact Estimates

The impact estimates presented in this chapter are of two types: those derived from administrative records and those derived from follow-up survey data.

Administrative Data

This chapter utilizes three types of administrative data that were gathered from state and local agencies: 1) unemployment insurance (UI) wage records, which were used to analyze

[68] Olsen et al. (2005) control for time-invariant unmeasured characteristics through a fixed-effects estimation procedure. Susin controls only for measured characteristics, including prior earnings, through a propensity score matching procedure.

employment and earnings impacts; 2) Temporary Assistance to Needy Families (TANF) records; and 3) food stamp benefits records.

UI Wage Records

In general, state UI wage records are an accurate source of follow-up data on participant earnings. Employer-reported quarterly earnings records are maintained by employment security agencies in all states for the purpose of calculating unemployment compensation benefits for insured workers who become unemployed. UI wage records have some acknowledged limitations, however: failure to cover certain industries and out-of-state jobs, some non-reporting by employers, and lack of detail on hours worked and the within-quarter timing of employment. Participation in the informal economy (i.e. self-employment or wage employment that is not reported to tax authorities) may provide a relatively large share of individual earnings for some sample members. It will be useful, therefore, to compare the measures of earnings derived from UI records with those reported by survey respondents.

Administrative data from UI records on the quarterly earnings of sample adults were collected from four states: Georgia, California, Texas, and Washington. These data made it possible to analyze employment and earnings outcomes for the six Voucher evaluation sites: Atlanta, Augusta, Fresno, Houston, Los Angeles, and Spokane. Earnings data were requested from all sites for a period beginning one year prior to random assignment and ending in December 2004.

As discussed in Chapter 1, delays in the implementation of the program in Los Angeles led to a much later period of random assignment at that site. Random assignment took place in April-May 2001 in Los Angeles, while in all other sites random assignment was completed by December 2000. As a result, UI records were available for two fewer quarters after random assignment in Los Angeles compared to the other sites. For all other sites, at least three and a half years[69] of follow-up data are available. Because of this, all impact estimates derived from UI records are presented in two sets of outcomes: through three years for all sites and through 3.5 years for five sites (all sites except Los Angeles).[70]

From UI records, we estimated impacts on the following measures of employment and earnings:

- For employment, half-yearly impacts were estimated for the number of quarters the sample member was employed during that half-year (0, 1, or 2 quarters), scaled by one-half. Employment status (yes / no) in any given quarter was based on whether UI

[69] The administrative data have been aggregated to the half-year level in order to provide robust treatment-on-treated (TOT) impact estimates. The TOT methodology (see Appendix B.3) is heavily dependent on the Treatment group members who lease up in the first period. Therefore, it is helpful to have a substantial number of Treatment group lease-ups in this first period. This is accomplished by using semiannual periods, rather than quarterly periods. The semiannual periods run from April 1 through September 30 and from October 1 through March 31. See Appendix A.1 for additional detail on the timing convention used for administrative data in this chapter.

[70] Note that all impact regressions presented in this report control for site effects through the inclusion of site dummies in the list of covariates. These dummies control for average differences in the outcome variables across sites.

earnings were positive in the quarter. We also constructed two measures of total employment over the entire follow-up period: total number of quarters employed through the sixth half-year (for all sites) and total number of quarters employed through the seventh half-year (for all sites except Los Angeles).

- For earnings, half-yearly impacts were estimated for dollar earnings during the half-year. Two measures of earnings over the entire follow-up period were also constructed: earnings summed over the first six follow-up half-years (for all sites) and total earnings summed over the first seven half-years (for all sites except Los Angeles).

TANF and Food Stamp Benefits Records

Administrative data on TANF receipt and benefit amounts were obtained for five sites from four TANF jurisdictions[71]: State of Georgia (Atlanta and Augusta TANF data), State of Texas (Houston TANF data), Fresno County (Fresno TANF data), and State of Washington (Spokane TANF data.) Administrative data on food stamp receipt and benefit amounts were obtained for four sites from three TANF agencies[72]: Georgia (Atlanta and Augusta Food stamp data), Texas (Houston Food stamp data), and Washington (Spokane Food stamp data). Therefore, analysis of TANF outcomes is restricted to five sites and analysis of outcomes related to food stamps is restricted to the four non-California sites.

State or county welfare agencies extracted the relevant TANF and food stamp records, identifying cases that matched the sample file by social security number (SSN). We requested data on benefit amounts for any case in which our sample member was a part of the case, regardless of whether our sample member was the payee.[73] Administrative data providing earnings information were requested for a period beginning one year prior to random assignment and continuing through December 2004, for all sites. For consistency with the analysis of employment and earnings, we have converted monthly TANF and food stamps data into half-yearly outcomes.

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[71] For the interim evaluation, Los Angeles County provided us with five quarters of TANF and FS records. We did not receive additional information on either TANF or Food Stamps receipt from Los Angeles. Therefore, we do not include Los Angeles in any of the impact estimates for public assistance outcomes. For impact estimates on TANF and Food Stamps over the first five quarters that include Los Angeles, see Patterson *et al*., 2004.

[72] Some Food Stamp benefits records were also obtained from Fresno County. These records, however, were only for months after the first seven half-years of the follow-up period. Therefore, Fresno was not included in the impact estimates for Food Stamp-related outcomes.

[73] Most of the matches were straightforward. However, in one site (Fresno) the data provided to Abt Associates included multiple records per case and issue-date, suggesting a failure to match the data properly by SSN. To process these data, we were obliged to resort to name matching to ensure that we had selected the correct records. One consequence of this process was that, for those SSNs associated with multiple cases on a given date, we selected only those observations where our sample member was the payee. This decision rule, which was necessitated by data limitations, meant that the Fresno sample was matched only to welfare data for which they were the payee – thus biasing receipt rates and benefit levels towards zero. Fortunately, this bias does not affect treatment and control group members differently. A second consequence of this process is that the Fresno data will be noisier than the other sites. Because name matching is inevitably more inaccurate than simple matching based on SSNs, the Fresno match is likely to have introduced greater error into this dataset.

For each half-year, we have three measures of public benefits receipt: number of quarters of TANF receipt; number of quarters of food stamp receipt; and number of quarters of receipt of public assistance (defined here as either TANF or food stamps).[74] Similarly, there are three value measures in each half-year: amount of TANF receipt; amount of food stamp receipt; and total amount of TANF and food stamps combined.

In addition to these six half-yearly measures, there are three aggregate receipt measures: the total number of quarters of TANF receipt over the follow-up period; the total number of quarters of food stamp receipt over the follow-up period; and the total number of quarters with any public assistance over the follow-up period. The aggregate value measures are: the total value of TANF received over the follow-up period; the total value of food stamps received over the follow-up period; and the total value of TANF plus food stamps received over the follow-up period.

Survey Data

We used responses from the follow-up survey (described in Chapter 1) to construct outcome measures relating to employment and earnings, receipt of means-tested benefits, and education and training.[75]

In the follow-up survey, respondents were asked to provide details about all the jobs that they held since random assignment (including start and end dates, weekly earnings, and weekly hours). From this job history data, we created period-by-period employment and earnings outcomes, which parallel the outcomes created from the UI data. We also created period-by-period outcomes for weekly hours. The weekly hours variable is defined as total hours worked in a half-year divided by 26 weeks. This is a composite measure of labor supply as it combines hours worked per week worked and number of weeks worked per half year.[76]

Survey respondents were also asked about the details of their current main job.[77] We constructed several outcome variables that pertain to the main job:

- whether working full-time at main job: equals one if weekly hours at main job are greater than or equal to 35 hours
- length of current main job in months
- whether employed one year or more at main job
- receipt of employer-provided health benefit
- receipt of employer-provided paid vacation benefit
- receipt of employer-provided sick leave benefit.

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[74] Each of these three measures are scaled by one-half, so that control means may be interpreted as average quarterly receipt rates and impacts as changes in those rates.

[75] See Appendix C for an analysis of non-response bias for outcomes derived from survey data.

[76] It is necessary to define weekly hours in this manner because the alternative definition of "hours worked per week worked" is undefined for those respondents who did not work in a particular half-year.

[77] If the respondent held more than one job at the time of the interview, the "main job" was defined as the job at which the respondent usually worked the most hours.

We also collected data on:

- number of employers since random assignment
- number of workers in household
- whether employed and found job through someone in neighborhood (a dummy variable equal to one if respondent is employed and the most important source of information used to find job was a friend, relative, or acquaintance who lived in the respondent's neighborhood at the time of hire).

In order to examine whether housing vouchers gave recipients easier access to work, we asked those respondents who were currently working about their journey to work. From the responses, we created variables for the time length of a worker's one-way commute, the usual means of transportation to work, possession of a driver's license, and access to a car that runs.

Respondents who were not working at the time of the survey were asked if they were looking for work, what job search actions they had taken in the past four weeks, how much time they had spent in the past four weeks looking for work, and their reservation wage (the lowest wage offer they would accept) for a job that came with health insurance. They were also asked whether they had a disability that prevented them from accepting a job. The question in our follow-up survey about job search actions was identical to the question included in the monthly Current Population Survey, administered by the U.S. Census Bureau. The U.S. Department of Labor's Bureau of Labor Statistics (BLS) categorizes each job search action as either an "active" method of job search or a "passive" method of job search.[78] Following the BLS definitions, we classified survey respondents as either 1) working, 2) actively job searching in the past four weeks, 3) not actively job searching and with a disability, or 4) not actively job searching and without a disability.[79]

The follow-up survey also asked respondents about receipt and amount of government assistance in the past month. From the responses, we created variables for TANF receipt and value, food stamps receipt and value, Supplemental Security Income (SSI) receipt and value (for all members of the household), and tax refund receipt and value for tax year 2003. The tax refund variables are intended to capture receipt of the Earned Income Tax Credit (EITC), since it is likely that most of the sample has little or no income tax liability.

We constructed the following education and training outcome variables:

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[78] The job search actions that the BLS considers "active" include: contacted employers, contacted public employment agency programs/courses, contacted private employment agency, contacted friends or relatives, interviewed for a job, contracted school/university employer center, sent out resumes/filled out applications, checked union/professional registers, and placed or answered ads. The job search actions that the BLS considers "passive" include: looked at ads and attended job training.

[79] We did not ask respondents who were looking for work if they could have started a job in the past week if one had been offered. This prevented us from matching the exact BLS definition for classifying respondents as either "unemployed" or "not in the labor force." Therefore, we use the categories "actively job searching" or "not actively job searching."

- years of schooling as of survey date
- whether a high school diploma or G.E.D. had been earned since random assignment
- whether any education or training had been received since random assignment
- hours of education and training received since random assignment.

In addition, any education and training received was categorized as one of four types:

- "academic," which included regular high school directed toward a high school diploma; preparation for a G.E.D. exam; 2-year college; 4-year college; or graduate courses
- "non-academic," which included vocational education outside a college such as business or technical schools, employer or union-provided training, or military training in vocational skills (not military skills)
- "adult education," which included non-vocational adult education (such as basic education, literacy training, or English as a second language) not directed toward a degree
- "job search," which included job search assistance, job finding, or orientation to the world of work.

4.4 Baseline Sample Characteristics

In this section, we present the baseline characteristics of the sample for the outcomes analyzed in this chapter: labor force status, public assistance receipt, and educational achievement. We also show the time paths of employment, earnings, and public assistance receipt for the control group. This background information provides a context for understanding the magnitudes of program impacts on the treatment group, which are presented in the next section.

Employment and Earnings at Baseline

At the time of random assignment, 45 percent of all sample members reported that they were working for pay (Chapter 1, Exhibit 1.2), and about 28 percent were not currently working but looking for work (not shown in table). There were no significant differences between treatment and control group members in these rates.

The overall employment rate in the quarter of random assignment (the "baseline quarter"), as measured by UI earnings data, is 52 percent. Because the baseline survey question asked about activities the sample member was engaged in "last week," it is to be expected that a measure that captures any earnings over an entire quarter is somewhat higher.

Sample member earnings in the baseline quarter averaged $1,076, including persons with no earnings. Among sample members with earnings, average earnings in the baseline quarter were $2,082. Earnings rates among sample members at work were similar to national averages for

welfare recipients, while employment rates in the sample were higher than the national average, reflecting the fact that not all of our sample members were current welfare beneficiaries.[80]

The follow-up survey allows us to examine additional employment-related characteristics beyond what is available from the UI wage records. Exhibit 4.1 contains the baseline values of these additional job characteristics for the 2,481 treatment and control group members in the survey respondent sample. In the week before random assignment, 43 percent of the respondent sample worked for pay. Of those who were employed, about half were working full-time (35 hours or more per week), and about one third of those working had employer-provided health benefits. Three-quarters of those who were working used private motor vehicles to commute, about half had both a driver's license and access to a car, and about half lived 15 to 30 minutes away from their workplace. Of those who were not working, 60 percent were willing to accept a job that paid $6.00 to $8.99 per hour if it provided health benefits.

Employment and Earnings by Quarter for the Control Group

Impacts are measured as the average outcomes for the treatment group minus the average outcomes for the control group.[81] The control group's experience over time represents the outcomes the treatment group would have experienced in the absence of the voucher. We present the trends over time for the control group in UI-derived employment and earnings outcomes, so that the reader has a context for interpreting the treatment group impacts.

The administrative data show a general upward trend in earnings for control group members that levels off after two years. The initial upward movement is not surprising, as current or prior TANF eligibility or receipt, which is a function of low earnings, was a requirement for program participation. As a result, persons applying for the program were likely to have earnings and income that were temporarily lower than average. Participants were likely trending back to their permanent income status over time. Indeed, we see (in Exhibit 4.2) that the upward trend in earnings for participants seems to level off in the second year after random assignment, consistent with the concept that participants have returned to their permanent income level.

Employment rates (defined here as the percentage employed at any time during the indicated quarter) show a somewhat different pattern. (See Exhibit 4.3.) Employment rates are roughly constant for the first five quarters after random assignment and then decline over the next 10 quarters. From a high of 54 percent (53 percent without Los Angeles) in the second quarter after random assignment, the employment rate drops to 47 percent in quarter 14 (to

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[80] For example, among all adults receiving TANF in Fiscal Year 2001, 24.3 percent had earned income, and those with earned income earned $686 per month, or $2,058 per quarter; thus, the average among working and non-working recipients was $500 of earned income per quarter. See Office of Family Assistance, "Fiscal Year 2001 Characteristics and Financial Circumstances of TANF Recipients." Website: http://www.acf.hhs.gov/programs/ofa/character/FY2001/characteristics.htm.

[81] Impacts presented are actually regression-adjusted differences between the treatment and control groups, not simple means, to control for chance variation in baseline characteristics between the groups.

Exhibit 4.1
Employment-Related Baseline Characteristics of Follow-up Survey Respondent Sample

	Entire Sample	Control Group	Treatment Group
Worked for pay at baseline	43.2%	40.6%	45.7%
Worked full time at baseline	23.1%	21.7%	24.5%
Length of tenure at job at baseline (in months)	5.2	5.1	5.3
Work hours per week at baseline	14.7	13.6	15.7
Received employer-provided benefits at baseline			
Health benefits	15.2%	13.1%	17.2%
Sick days with pay	11.7%	10.2%	13.2%
Paid vacation	17.7%	16.4%	19.0%
Transportation mode			
Car, truck or van	30.6%	29.2%	31.9%
Public transportation	11.3%	10.1%	12.5%
Walked	2.6%	2.1%	3.0%
Worked at home	1.2%	1.3%	1.0%
Other Method	0.5%	0.6%	0.5%
Not employed	53.9%	56.7%	51.1%
Total	**100.0%**	**100.0%**	**100.0%**
Travel time from home to work			
Less than 15 minutes	11.2%	10.3%	12.0%
15 to 30 minutes	21.1%	20.0%	22.2%
31 to 45 minutes	7.1%	6.4%	7.7%
46 minutes to one hour	3.6%	3.7%	3.5%
More than one hour	2.5%	2.1%	2.8%
Work at home	1.0%	1.2%	0.9%
Not employed	53.6%	56.4%	50.8%
Total	**100.0%**	**100.0%**	**100.0%**
Trans. Access			
Has access to a car that runs, but no license	2.1%	1.5%	2.6%
Have a license but no car	11.0%	11.0%	11.1%
Both license and car	22.6%	20.7%	24.6%
Neither license nor car	13.3%	13.0%	13.5%
Not employed	51.0%	53.8%	48.2%
Total	**100.0%**	**100.0%**	**100.0%**
Reservation wage:			
$3 to $5.99	7.7%	7.3%	8.1%
$6 to $8.99	59.5%	58.4%	60.7%
$9 to $12.99	25.9%	27.7%	23.7%
$13 to $15.99	7.0%	6.6%	7.5%
Total	**100.0%**	**100.0%**	**100.0%**

Exhibit 4.2
Earnings by Quarter for the Control Group

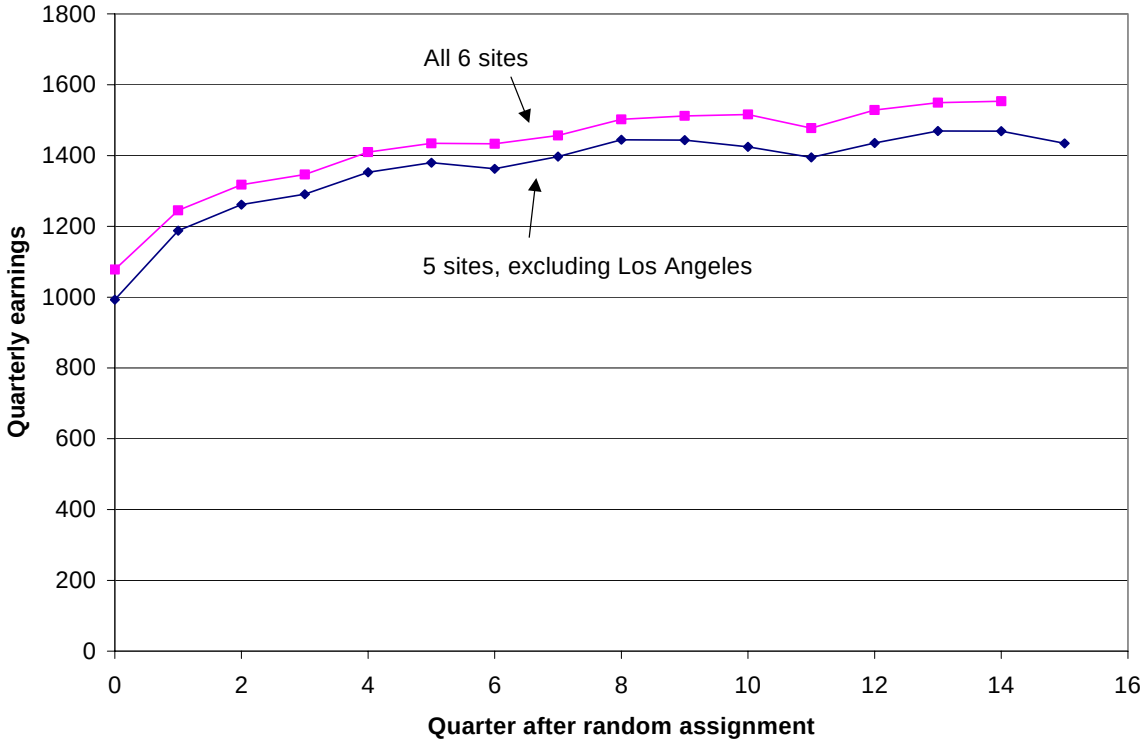
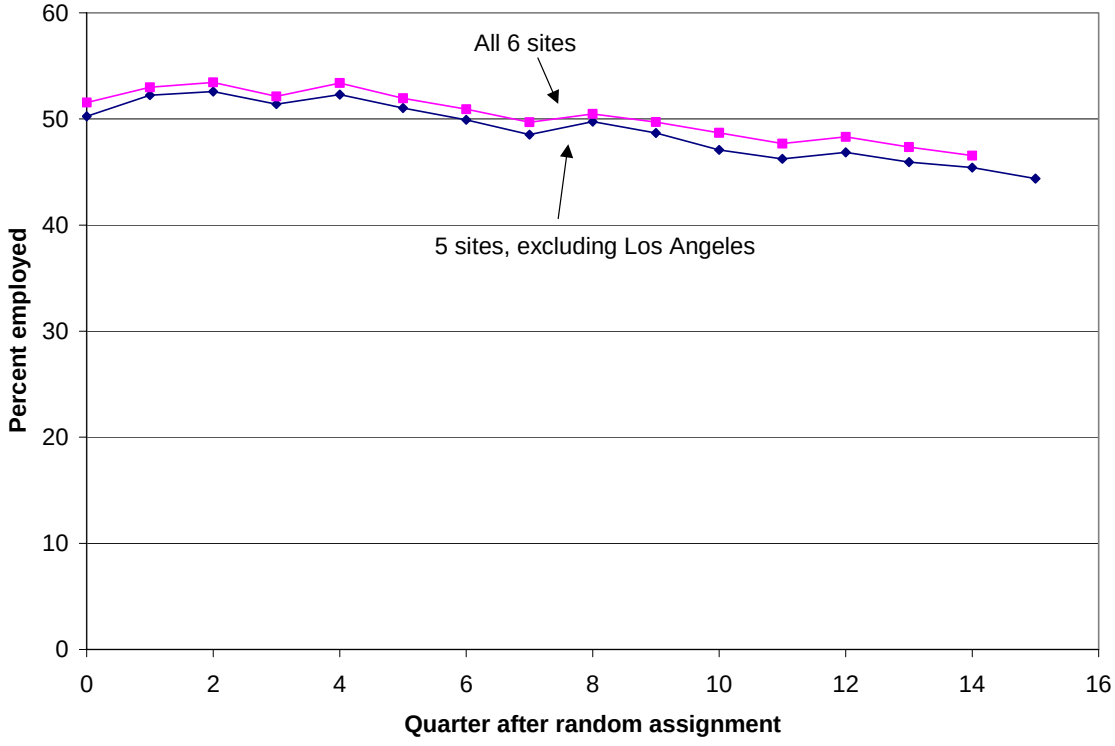


Exhibit 4.3
Employment Rates by Quarter for the Control Group



44 percent in quarter 15 without Los Angeles). Some of this decline may be explained by the fact that the two California sites required that applicants be working to receive a WtW voucher; therefore, the declining employment rate could be a reflection of program requirements, which were no longer binding after a voucher had been received and used.

Receipt of Public Assistance at Baseline

At the time of the baseline survey, 76 percent of the sample reported that they were currently receiving assistance from TANF. Eighty-four percent reported that they, or someone else in their household, were receiving food stamps. There were no significant baseline differences between treatment and control group members in these percentages.

According to the administrative records, 72 percent of sample members (excluding Los Angeles) were in households that received TANF during the quarter of random assignment, and 77 percent of sample members (excluding Fresno and Los Angeles) were in households that received food stamps during the quarter of random assignment (Exhibit 4.4). The lower receipt rates observed in the administrative data compared with the baseline interview responses are concentrated in the two Georgia sites. In particular, in Atlanta the administrative records show only 26 percent of sample members receiving TANF at baseline, while the survey self-report indicates 41 percent of sample members receiving TANF at baseline. In Augusta the difference is not as extreme: survey data indicates that 39 percent of sample members were on TANF at baseline, while administrative records indicate that just 35 percent were receiving welfare.[82] In all other sites, the overall TANF receipt rates are much higher as measured in both administrative and survey data; the two measures are much closer to each other; and the administrative records indicate slightly higher rates of receipt than the survey records. Very similar site-by-site patterns are found when we compare survey data and administrative data for food stamps receipt.

Exhibit 4.4
Receipt of Public Assistance in Quarter of Random Assignment
Administrative and Baseline Survey Data

	Cash Assistance Receipt		Food Stamp Receipt	
Site	**Administrative Records**	**Baseline Survey**	**Administrative Records**	**Baseline Survey**
Atlanta	26%	41%	45%	62%
Augusta	35	39	68	75
Fresno	93	94	na	93
Houston	82	76	90	84
Los Angeles	na	92	na	90
Spokane	82	78	91	86
Total	72	76	77	84

Note: na = not available

[82] The Atlanta and Augusta administrative data came from the same state data system; differences between the two sites, therefore, do not reflect differences in administrative systems.

The lower overall rate of welfare receipt in Georgia compared to other states reflects the fact that Georgia operationalized the requirements for voucher eligibility somewhat differently than the other sites. To recruit sufficient numbers of voucher-eligibles for the research sample, both Georgia sites made concerted efforts to establish whether applicants had been eligible to receive welfare within the past two years, not just whether they had actually received TANF benefits. As a result, Georgia applicants to the Welfare-to-Work voucher program had much lower welfare receipt rates at the time of random assignment.

For the overall sample (excluding Los Angeles), the average quarterly TANF benefit in the quarter of random assignment (including those who were not receiving benefits) was $757, according to administrative records. Among persons who were receiving TANF, the average TANF benefit was $1,045. The average value of food stamps received in the quarter of random assignment, over the entire sample (excluding Fresno and Los Angeles), was $598. Among persons who were receiving food stamps, the average value was $794.

The follow-up survey gives us a secondary data source with which to estimate impacts on TANF and food stamps receipt. It also allows us to analyze Supplemental Security Income (SSI) receipt. Exhibit 4.5 shows the baseline values of these measures for the follow-up survey respondent sample, a subset of the overall sample. At baseline, 76.8 percent of respondents received TANF, 86.5 percent of respondent households received food stamps, and 12.7 percent of respondent households received SSI.

Exhibit 4.5
Public Assistance Baseline Characteristics of Follow-up Survey Respondent Sample

	Entire Sample	Control Group	Treatment Group
Received TANF at baseline	76.6%	77.8%	75.3%
TANF benefits at baseline (includes those who reported not getting any benefits)	$275.16	$284.67	$265.64
Received Food Stamps at baseline	86.5%	88.3%	84.8%
Food Stamps value at baseline (includes those who reported not getting any benefits)	$254.84	$260.27	$249.47
Received SSI at baseline	12.7%	12.9%	12.4%
SSI value at baseline (includes those who reported not getting any benefits)	$65.14	$71.39	$58.81

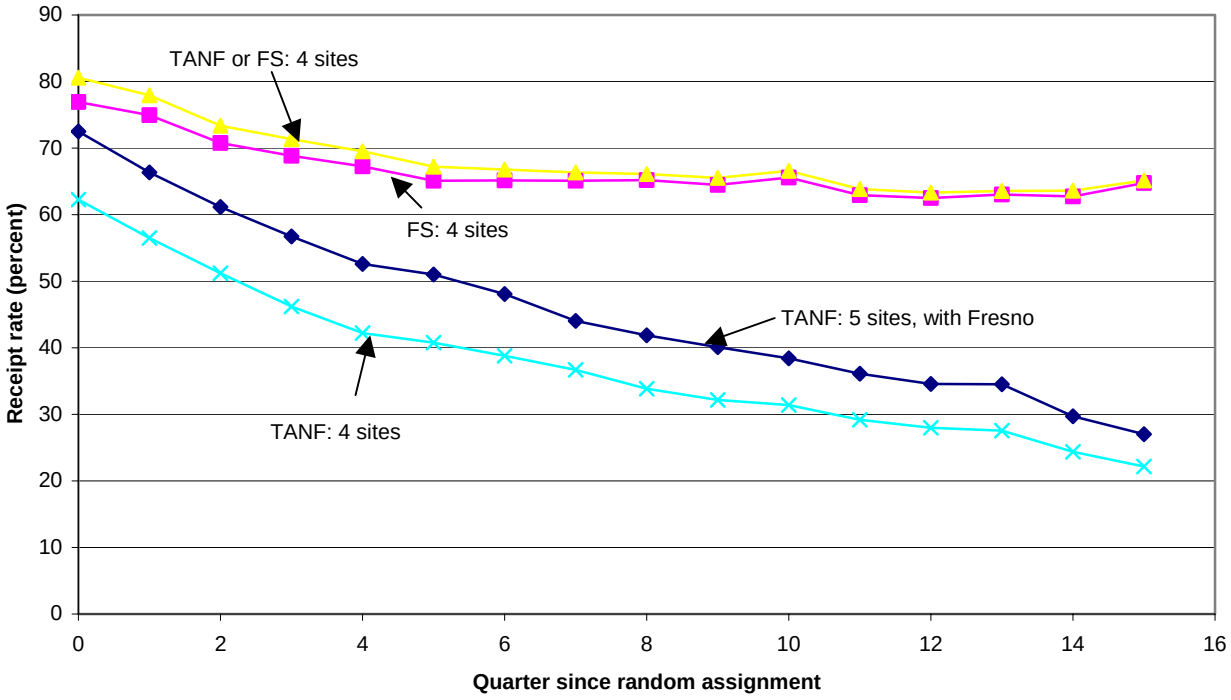
Receipt of Public Assistance by the Control Group

The administrative data for the control group show that the high initial rates of receipt for both TANF and food stamps declined steadily over time (Exhibit 4.6). TANF receipt rates fell more sharply than food stamp receipt rates, falling by 45 percentage points between the quarter of

random assignment and the fifteenth quarter of follow-up. This pattern of declining receipt is typical for any cohort of individuals who were all initially receiving benefits. Because current or prior TANF eligibility or receipt was a requirement for program participation, people applying for the program were likely to have income that was temporarily lower than average. Participants were likely trending back to their permanent income status over time. Time limits on TANF receipt instituted by welfare reform undoubtedly also served to reduce receipt rates over time for this fixed group of individuals. For the Housing Choice Voucher program to reduce welfare receipt, then, it would be necessary for the receipt rate of the treatment group not only to fall over time, but also to fall by more than the rate of the control group.

The decline of the food stamps receipt rate levels off in the second year after random assignment, and the receipt rate remains relatively constant through quarter 15. This steady rate of receipt may be explained by the fact that food stamp benefits have a higher income cutoff than TANF benefits and also are not subject to time limits.

Exhibit 4.6
Quarterly Receipt of Public Assistance for the Control Group



Education and Training at Baseline

Exhibit 4.7 shows the educational achievement and participation in job training at baseline of the survey respondent sample.[83] The median education level of sample members was completion of 12th grade. Slightly over one-third (38 percent) of the respondent sample had neither a high school diploma nor a G.E.D. At the time of random assignment, 17 percent of sample members were

[83] Baseline characteristics of the follow-up survey respondent sample, rather than the entire sample, are presented here because this is the group upon which education and training impacts as of the fifth year after random assignment are estimated.

enrolled in school and about one-third of this group (32 percent, not shown in table) were also working for pay. About one-fifth of the sample were enrolled in a job training program at baseline.

Exhibit 4.7
Education and Training Baseline Characteristics of Follow-up Survey Respondent Sample

	Entire Sample	Control Group	Treatment Group
Years of schooling completed at baseline			
8th grade or less	8.5%	7.4%	9.5%
9th-11th grade	42.0%	43.5%	40.6%
12th grade	32.6%	32.6%	32.7%
1-3 yrs of college	15.4%	15.0%	15.8%
4 yrs of college or more	1.5%	1.6%	1.4%
Total	**100.0%**	**100.0%**	**100.0%**
In school at baseline	17.6%	18.0%	17.2%
H.S. diploma or G.E.D. at baseline			
High school diploma	40.6%	40.3%	41.0%
G.E.D.	20.6%	21.3%	19.9%
Neither High School Diploma nor G.E.D.	38.8%	38.5%	39.1%
Total	**100.0%**	**100.0%**	**100.0%**
In job training program at baseline			
Enrolled	13.8%	14.4%	13.1%
Enrolled, but not yet started	7.0%	7.7%	6.4%
Not in job training program	79.2%	77.9%	80.5%
Total	**100.0%**	**100.0%**	**100.0%**

4.5 Estimated Impacts

This section presents the estimated impacts of the Housing Choice Vouchers on employment and earnings, public assistance, and education and training. Primary employment and earnings impacts are estimated for the full sample from all six sites (sample size = 8,664),[84] primary TANF impacts are estimated excluding Los Angeles sample members (sample size = 7,622), and primary food stamps impacts are estimated excluding both Fresno and Los Angeles (sample size = 5,056). Education and training impacts, secondary employment and earnings impacts, and secondary public assistance impacts are estimated for the survey respondent sample (sample size = 2,481).

As discussed in Chapter 1, we present two types of impact estimates, Intent-to-Treat (ITT) estimates and Treatment-on-Treated (TOT) estimates. ITT estimates reflect the impact of treatment on the entire treatment group, compared with the entire control group. TOT estimates reflect the impact of the treatment on those treatment group members who leased up, controlling for the fact that some control group members (crossovers) also leased up and some treatment group members (nonparticipants) did not. Thus, the TOT results are

[84] For 67 sample members, social security numbers could not be verified. Excluding these sample members reduces the total sample size from 8,731 to 8,664.

estimates of the impact of the vouchers on those treatment group members who used them, relative to control group members who received no vouchers.[85] Both ITT and TOT estimates are regression-adjusted for baseline characteristics to take account of chance differences between the treatment and control groups.

Employment and Earnings Impacts

We used the UI wage records to analyze the overall effect of the voucher on employment and earnings. Exhibit 4.8 presents two sets of estimated impacts on employment: impacts over three years of follow-up for all sites, and impacts over 3.5 years of follow-up for all sites except Los Angeles. The employment outcome analyzed here is the number of quarters employed in a half-year, scaled by one-half. In each half-year, the sample member can be employed in 0, 1, or 2 quarters[86]—therefore, the outcome can take on values of 0, 0.5, or 1. This definition allows us to interpret each control mean as the average quarterly employment rate within a half-year and an impact as the change in that employment rate. The control mean column shows that the employment rate declines slightly for the control group during the follow-up period. The standard deviations shown in the control mean column indicate that there was large variation in employment across the control group.

Impacts for all sites reveal small (5-8 percent[87]), marginally significant negative TOT effects on employment in the first year (i.e., the first two half-years), insignificant effects in the next two years, and an insignificant overall effect on employment during the three years. Over all three years, control group members averaged about 6.1 quarters of employment, and treatment group members averaged .06 fewer quarters, or 1 percent less time employed. Taking into account treatment group non-participation and control group crossover, the TOT estimates show that treatment group members who in fact used their voucher averaged 0.185 fewer quarters, or 3 percent less. Thus, over all three years treatment group members averaged slightly less time employed than control group members. However, the results are not statistically significant: we cannot determine whether this difference results from the voucher or from chance alone.

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[85] The TOT estimate is derived from the ITT estimate assuming that (a) the intervention had no effect on a family that did not use a voucher and (b) in any given quarter after lease-up, the voucher had the same effect on a control group member who leased up as on a treatment group member who had leased up for the same length of time. (See Appendix B.3 for details.)

[86] We analyzed employment in half-yearly periods because analysis of quarterly employment and earnings yielded unstable TOT estimates. Use of half-yearly employment (0/1) as the outcome variable (i.e., whether the individual had any earnings in the six-month period) would, however, mask much of the short-term variability of employment in this sample. Therefore, we retained an outcome variable that measures any employment within three-month periods.

[87] TOT impacts are not strictly comparable to overall control group means. The TOT estimates represent the effect of the voucher on treatment group members who were participants *and* who would not have been crossovers had they been controls (i.e., "non-crossover-like" participants). Therefore, the appropriate control means for the TOT impacts are the means for a subset of the control group–those controls who did not crossover but would have participated had they been treatment group members. Given the complexity of the TOT estimation process used here, it is not possible to calculate the appropriate control means for the TOT impacts. Where comparisons of TOT impacts to overall control group means are given in this chapter, they are intended to illustrate the orders of magnitudes involved.

Exhibit 4.8
Impacts on Quarterly and Total Employment (UI Data)

	Average Quarterly Employment Rate All Sites			Average Quarterly Employment Rate All Sites Except Los Angeles		
	Control Mean[a]	ITT Impact	TOT Impact	Control Mean[a]	ITT Impact	TOT Impact
Half-year 1	0.529	-0.015 *	-0.038 *	0.521	-0.019 **	-0.045 **
	0.459	(0.008)	(0.020)	0.458	(0.009)	(0.021)
Half-year 2	0.532	-0.014 *	-0.026	0.522	-0.018 **	-0.034 *
	0.459	(0.009)	(0.019)	0.459	(0.009)	(0.019)
Half-year 3	0.507	-0.009	-0.023	0.497	-0.015	-0.034
	0.464	(0.009)	(0.021)	0.463	(0.010)	(0.022)
Half-year 4	0.503	0.003	0.000	0.494	-0.003	-0.016
	0.465	(0.009)	(0.023)	0.463	(0.010)	(0.023)
Half-year 5	0.487	0.004	-0.005	0.472	0.003	-0.008
	0.470	(0.009)	(0.024)	0.467	(0.010)	(0.024)
Half-year 6	0.477	0.004	0.000	0.462	0.003	-0.007
	0.470	(0.009)	(0.025)	0.467	(0.010)	(0.025)
Half-year 7	-	-	-	0.450	0.017 *	0.031
				0.467	(0.010)	(0.026)
Total number of quarters employed over follow-up period	6.068	-0.057	-0.185	6.837	-0.064	-0.223
	4.521	(0.080)	(0.205)	5.114	(0.097)	(0.249)

Notes:
N = 8,664 for regressions run on all sites. N = 7,662 for regressions run on all sites except Los Angeles.
ITT = "Intent-to-Treat." TOT = "Treatment-on-Treated." Standard errors in parentheses.
*** indicates p < .01, ** indicates p < .05, * indicates p < .10
[a]Standard deviations of control group outcomes are beneath control means.

Impacts over 3.5 years (including all sites except Los Angeles) were similar, with small (7-10 percent) significant negative TOT effects in the first year, followed by two years of insignificant effects. It appears that slightly negative ITT effects become slightly positive over time. In the seventh half-year, the positive ITT effect of the voucher on employment is larger (about 4 percent) and significant at the 0.10 level, but the TOT effect is insignificant. In order to determine whether the employment effect does indeed become positive late in the follow-up period, we examined a subset of 7,153 sample members[88] whose random assignment occurred early enough that we have four years of data for them. The eighth half-year ITT for this group is 0.019, insignificant and about 1 standard error away from zero. The eighth half-year TOT estimate is also insignificant (with a point estimate of 0.018 and a standard error of 0.058). The positive effect in the seventh half year, therefore, may be anomalous. It appears that the voucher lowers labor supply immediately after receipt but that over time this effect disappears.

[88] This subset includes all the sample members from Atlanta, Fresno, and Houston, and some of the sample members from Augusta and Spokane.

Exhibit 4.9
Impacts on Biannual and Total Earnings (UI Data)

	All Sites			All Sites Except Los Angeles		
	Control Mean[a]	ITT Impact	TOT Impact	Control Mean[a]	ITT Impact	TOT Impact
Half-year 1	$2,651	-$124**	-$306**	$2,536	-$154**	-$362**
	$3,434	(59)	(146)	$3,339	(63)	(148)
Half-year 2	$2,837	-$100	-$174	$2,724	-$144**	-$260*
	$3,705	(67)	(144)	$3,642	(71)	(147)
Half-year 3	$2,889	-$76	-$195	$2,758	-$145*	-$339**
	$3,868	(73)	(169)	$3,775	(76)	(169)
Half-year 4	$3,007	$16	-$20	$2,880	-$65	-$211
	$4,091	(80)	(202)	$4,008	(83)	(200)
Half-year 5	$3,029	$30	-$32	$2,860	-$17	-$148
	$4,225	(83)	(213)	$4,078	(85)	(211)
Half-year 6	$3,046	$72	$103	$2,868	$18	-$80
	$4,268	(86)	(231)	$4,131	(88)	(225)
Half-year 7	-	-	-	$2,906	$121	$182
				$4,390	(94)	(246)
Total, all half-years	$17,458	-$182	-$624	$19,532	-$387	-$1,218
	$20,359	(365)	(930)	$22,870	(442)	(1120)

Notes:
N = 8,664 for regressions run on all sites. N = 7,662 for regressions run on all sites except Los Angeles.
ITT = "Intent-to-Treat". TOT = "Treatment-on-Treated". Standard errors in parentheses.
*** indicates p < .01, ** indicates p < .05, * indicates p < .10
[a] Standard deviations of control group outcomes are beneath control means.

The control group means in Exhibit 4.8 show a slight decline in employment for the control group. This contrasts with the increase in earnings seen in the control group means in Exhibit 4.9. Some of the earnings increase is due to inflation, but attributing the entire increase to inflation would imply compound annual inflation rates of 8.6 percent for all sites and 7.0 percent for all sites except Los Angeles.[89] These rates of inflation are substantially higher than the national inflation rate during this period. Therefore, those who were employed must have either increased their total hours worked or received wage increases or some combination of the two.

Exhibit 4.9 presents estimates of the impacts on earnings. Over three years, the all-sites estimates show a significant negative earnings impact only in the first of the six half-years. The TOT impact in this period is -$306, or about 12 percent of the control group mean

[89] These compound inflation rates are calculated by first computing a quarterly growth rate for each pair of consecutive quarters for the fixed sample who works in both quarters. The product of the quarterly growth rates is then matched by the total change generated by a compound inflation rate taken over the entire time period.

earnings. After the first half-year, impacts are insignificant. The point estimates of the impacts steadily decline and then become positive later in the follow-up period.

The 3.5 year estimates obtained when Los Angeles is excluded show a similar pattern. Control mean earnings are 4-5 percent lower when Los Angeles is excluded, and negative earnings impacts are of greater magnitude.[90] Without Los Angeles sample members, the significant negative earnings impacts persist through the third half-year, before shrinking to insignificance. The TOT impacts represent reductions of 10-15 percent of earnings. The TOT impact for the whole follow-up period represents a reduction of about 6 percent in earnings, although this is not a statistically significant finding. The bulk of this reduction occurs in the early part of the follow-up period.

In addition to estimates derived from the UI administrative data, we also estimated impacts on employment and earnings based on responses to the follow-up survey. More details about respondents' current employment situations were available from the follow-up survey than from the UI wage records. The follow-up survey also asked respondents to provide details about all the jobs they had held since random assignment. From these responses, we constructed a work history for each person and were able to estimate period-by-period impacts on employment and earnings (Exhibit 4.10). These impacts may be compared to those in Exhibits 4.8 and 4.9, although the reader should keep in mind the two ways in which the underlying data differ: 1) these impacts are estimated for the respondent sample rather than the full sample and 2) these impacts use respondents' recollected job histories rather than employer-reported UI data.

Employment levels reported by survey respondents are much lower than employer-reported levels, especially in the beginning of the follow-up period, and they rise substantially (by 12 percentage points) over time rather than declining (by 7 percentage points). This suggests that respondents were better able to remember jobs in the recent past than jobs several years ago. From the job histories, we found no significant employment impacts. This contrasts with the significant negative employment impacts in the first year we found using the UI data.

Similarly, earnings reported by survey respondents (Exhibit 4.10) are lower than UI earnings (Exhibit 4.9) over the first two years but very similar in the last 1.5 years. This also suggests that respondents recollected their job histories more completely in later periods. We found no significant earnings impacts using the survey data, although the point estimates for the impacts were in the same range over the first three half-years as those shown in Exhibit 4.9. The smaller sample size of the survey respondent sample leads to larger standard error estimates, raising the bar for a finding of statistical significance. Although insignificant, the TOT point estimates using the survey data imply a negative earnings impact of about 12 percent over the 3.5 years for those treatment group members who leased up. Negative earnings impacts appear to taper off a year later in the survey data than in the UI data.[91] The voucher had no impact on current earnings reported by the survey respondents.

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[90] This implies that the treatment vs. control group earnings comparison in Los Angeles is more in favor of the treatment group than in the remainder of the sample.

[91] In order to disentangle what causes the difference in these earnings impacts, we estimated impacts on the survey respondent sample using the UI data. Compared to the full sample UI data results, the survey sample has larger, more persistent negative impacts on UI earnings. Therefore, we note that there is less difference between

In Exhibit 4.11, we show impact estimates for several other employment-related outcomes based on survey data.[92] At the time of the follow-up survey (conducted in the fifth year after random assignment), 47.2 percent of control group members were working. Treatment group members had a current employment rate statistically indistinguishable from that of control group members. There was also no difference between treatment and control groups in the proportion that was employed full-time. We found no significant impact of the voucher on number of months in the current job, whether employed more than one year at the current job, receipt of employer-provided health benefits, or number of employers since random assignment. Our sample size limits our ability to detect TOT impacts to only those impacts of about 12 percentage points or greater for the binary measures. Any true TOT impacts, if present, are below 12 percentage points.

The survey asked about the employment of all adults in the household, not just the primary sample member on whose employment and earnings the estimates reported so far have been based. We found a significant negative impact of the voucher on the number of workers in the household. This impact can be decomposed into an impact on the number of adults in the household and an impact on the likelihood of adults in the household being employed. It appears that most of the significant impact we found resulted from fewer adults in the household, rather than from a lesser probability of being employed.[93]

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treatment and control group members in the survey job history data than in the UI data. This is consistent with the hypothesis that measurement error in the survey data biases impact estimates toward zero.

[92] In Exhibit 4.11 and the exhibits that follow, all ITT impacts for dichotomous outcomes are estimated using a probit model. See Appendix B.3.2 for additional details about impact estimation with the probit model.

[93] The impact on number of workers in the household = $(\Delta AH)\times(\Pr W)+(AH)\times(\Delta \Pr W)$,
where:

AH: Number of adults in the household

Pr*W*: Probability that an adult in the household is working

Δ: Impact of the voucher on a quantity

If we substitute the probability that the *sample member* is working for the probability that *an adult* in the household is working (Pr*W*), we can calculate the impact on number of workers in the household using our impact estimates on the probability of working for the sample member, the number of children in the household, and household size. In Exhibit 4.11, we see that 47.2 percent of the control group is working and that the ITT impact on the probability of working is .017. In Exhibit 3.10, we see that control group means for number of children in the household and household size are 2.74 and 4.34, respectively. The estimated ITT impacts on these two measures are .0101 and -.1824. Substituting these means and impact estimates into the equation for impact on the number of workers in the household, we find

$$(-.1824-.0101)\times(.472)+(4.3431-2.7437)\times(.017)=-.0813+.0272\approx .-054$$

This implies that the bulk of the impact is a result of the impact on the number of adults in the household.

Exhibit 4.10
Impacts on Quarterly Employment and Earnings (Follow-Up Survey)

	Average Quarterly Employment Rate All Sites Except Los Angeles			Earnings All Sites Except Los Angeles		
	Control Mean[a]	ITT Impact	TOT Impact	Control Mean[a]	ITT Impact	TOT Impact
Half-year 1	0.225	0.002	0.004	$1,863	-$139	-$312
	0.409	(0.016)	(0.037)	$4,120	(160)	(359)
Half-year 2	0.252	-0.004	-0.009	$2,148	-$210	-$413
	0.425	(0.017)	(0.033)	$4,374	(168)	(319)
Half-year 3	0.271	0.007	0.017	$2,372	-$158	-$322
	0.434	(0.017)	(0.038)	$4,580	(176)	(381)
Half-year 4	0.309	0.001	-0.001	$2,603	-$132	-$377
	0.451	(0.018)	(0.044)	$4,690	(180)	(445)
Half-year 5	0.321	0.006	0.013	$2,785	-$192	-$548
	0.459	(0.018)	(0.046)	$4,801	(181)	(465)
Half-year 6	0.332	0.006	0.015	$2,795	$9	-$70
	0.459	(0.018)	(0.049)	$4,670	(197)	(519)
Half-year 7	0.346	0.008	0.018	$2,836	$37	-$133
	0.463	(0.019)	(0.051)	$4,556	(184)	(503)
Total number of quarters employe over follow-up period	4.112	0.051	0.115			
	5.406	(0.212)	(0.536)			
				-	-	-
Total earnings, all half-years	-	-	-	$17,403	-$786	-$2,174
				$28,244	(1076)	(2721)
Current main job monthly earnings[b]				$543	$20	$78
				$794	(35)	(138)

Notes:
N = 2,267.
ITT = "Intent-to-Treat". TOT = "Treatment-on-Treated". Standard errors in parentheses.
*** indicates p < .01, ** indicates p < .05, * indicates p < .10
Data from job history response in follow-up survey.
[a] Standard deviations of control group outcomes are beneath control means.
[b] N=2,455; the TOT estimate for these measure uses the Survey TOT methodology.

Exhibit 4.11
Impacts on Employment Outcomes

Outcome	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean[a]	ITT Impact	TOT Impact
Working	2,478	0.472	0.017	0.068
		0.499	(0.024)	(0.095)
Working full-time at main job	2,459	0.312	-0.012	-0.048
		0.463	(0.022)	(0.086)
Length of current main job in months	2,312	11.01	1.19	4.73
		22.38	(1.02)	(4.07)
Employed >1 year at main job	2,311	0.238	0.023	0.093
		0.426	(0.020)	(0.078)
Receipt of employer-provided benefits:				
Health benefit	2,465	0.208	0.015	0.061
		0.406	(0.018)	(0.070)
Paid vacation	2,457	0.219	0.018	0.072
		0.414	(0.018)	(0.073)
Sick leave	2,438	0.182	0.016	0.065
		0.386	(0.016)	(0.064)
Number of employers since random assignment	2,442	0.927	-0.068	-0.269
		1.340	(0.056)	(0.225)
Number of workers in household	2,417	0.736	-0.059*	-0.233*
		0.732	(0.031)	(0.123)

Notes:
These impacts are estimated for the follow-up survey respondent sample.
ITT = "Intent-to-Treat". TOT = "Treatment-on-Treated". Standard errors in parentheses.
*** indicates p < .01, ** indicates p < .05, * indicates p < .10
Each outcome and control mean include those who are not working.
[a] Standard deviations of control group outcomes are beneath control means.

In-depth interviews with 141 treatment group voucher users shed additional light on the effect of the voucher on employment decisions. Nearly all of the interviewees had changed jobs since random assignment. A few had held stable positions that lasted through much of the five-year follow-up period. It was more common, however, to find women who moved from job to job, with periods of unemployment between positions. As a group, voucher holders encountered a number of barriers to employment that were often quite difficult to overcome. Lack of affordable childcare and reliable transportation were the most commonly cited barriers to employment. Other issues that limited or prevented work were health problems, lack of employment skills, lack of job search skills, and past incarceration.

The voucher made it possible for some respondents to leave jobs or not to work when they had other obligations. The nature of the upheavals respondents faced varied widely: a mother with cancer; a hospitalized child; a bread-winner with an injury; a child with severe behavior problems; recovery from a traumatic experience (rape, stabbing, murdered relatives).

In most cases, interviewees did not articulate a link between their receipt of housing assistance and employment decisions. However, some reported that the voucher allowed them to work less than they would if they had not received housing assistance. Typically, they reported that the hours saved by work reduction were spent either going to school or spending more time with children. For some this meant cutting back from working more than 40 hours per week or moving from multiple jobs to a single full- or part-time job. We evaluated whether the voucher allowed treatment group members to cut back from over 40 hours per week by using the follow-up survey data to compare treatment and control group members. The impacts (not shown) on whether survey respondents worked more than 44 hours and more than 49 hours per week were not statistically significant.

In the in-depth interviews, voucher users were asked about their decisions on how many hours per week to work. Although many interviewees said that having a voucher allowed them to reduce the number of hours they spent working, some respondents reported they were working more after receiving the voucher than before receiving housing assistance. One woman, for example, was doubled up with her grandmother and not working prior to receiving the voucher. Once she received it, she saw the possibility of having her own household and found employment in order to pay for her rental deposit.

Job flexibility was a major employment issue for interviewees. Because many were solely responsible for their families, they needed jobs that would allow them deal with emergencies, as well as the time pressures of every day life. Many reported that the kinds of work they were able to find did not permit that sort of flexibility.

The job histories constructed from follow-up survey responses allowed us to examine the time path of weekly hours worked over the follow-up period. Exhibit 4.12 shows the impacts of the voucher on weekly work hours for each half year period since random assignment. The control mean, which begins at under 8 hours per week in the first half-year, includes those who were not working at all. The increase in control mean work hours over time likely reflects more complete recollection of recent work history. We find no significant impact of the voucher on weekly hours of work in any of the first seven half-year periods. We also find no impact on the average weekly hours over the entire period since random assignment. Finally, we tested for the effect of the voucher on the number of weekly hours currently worked as reported to the survey. The voucher had no effect on this outcome. Our sample size limited our ability to detect impacts to those of about 1.4 hours per week (roughly 15 percent) or greater.[94]

[94] The impact on earnings can be decomposed into an impact on weekly hours worked and an impact on earnings per hour. The earnings TOT impact estimate was -12.5 percent (though insignificant) over the first 7 half-years of follow-up (see Exhibit 4.10). The TOT impact on weekly hours over this period was a point estimate of –3.6 percent, implying a TOT impact on hourly earnings of treatment group members who leased up of –8.9 percent. These impacts are imprecisely estimated and therefore should be viewed with caution.

Exhibit 4.12
Impacts on Average Weekly Hours

	Average Weekly Hours[b] All Sites except Los Angeles			
	Sample Size	Control Mean[a]	ITT Impact	TOT Impact
Half-year 1	2,265	7.58	-0.09	-0.20
		15.15	(0.61)	(1.38)
Half-year 2	2,265	8.78	-0.47	-1.02
		16.41	(0.65)	(1.23)
Half-year 3	2,265	9.55	-0.23	-0.36
		16.86	(0.67)	(1.44)
Half-year 4	2,265	10.56	-0.12	-0.39
		17.15	(0.67)	(1.67)
Half-year 5	2,265	11.26	-0.27	-0.87
		17.73	(0.69)	(1.77)
Half-year 6	2,265	11.39	0.11	0.14
		17.68	(0.70)	(1.87)
Half-year 7	2,265	11.69	0.23	0.19
		17.60	(0.71)	(1.95)
Average since random assignment[c]	2,267	10.41	0.11	0.42
		14.35	(0.63)	(2.49)
Value at main job (at survey date)[c]	2,451	16.29	0.11	0.42
		18.91	(0.80)	(3.19)

Notes:
These impacts are estimated for the follow-up survey respondent sample.
ITT = "Intent-to-Treat". TOT = "Treatment-on-Treated". Standard errors in parentheses.
*** indicates p < .01, ** indicates p < .05, * indicates p < .10
[a] Standard deviations of control group outcomes are beneath control means.
[b] Average weekly hours in a quarter equals total hours worked in a quarter divided by 13 weeks. Sample includes those who were not working.
[c] The TOT estimates for these measures use the Survey TOT methodology.

In-depth interviewees described their means of transportation to get to work. It was not uncommon for respondents who did not own their own cars to rely on others for transportation to work. However, this system often broke down because of factors beyond the respondent's control. Many interview respondents used public transportation to get to work. Those who lived in areas with good bus or train service and who worked daytime hours were able to use public transportation without serious difficulties. But public transportation presented major hurdles for many. For example, one woman, a minimum wage home health care provider, had little control over her work schedule or the location of her work assignments. At the time we spoke with her, she reported that her commute took two hours and required three bus changes each way. She would spend four hours commuting in order to work three hours and earn less than $20 per day.

Despite the frequency with which in-depth interviewees cited transportation as a challenge to employment, few used the voucher assistance to move closer to their jobs or job opportunities. Respondents expressed mixed feelings about the idea of moving for a job. For some, location was not an issue because they could find work wherever they ended up. Conversely, respondents with inadequate work skills doubted a change in location would make any difference. For others, the significant obstacles to moving (upfront costs, uncertainty about owner screening, compliance with PHA policies and lease notice requirements) discouraged consideration of moves for this purpose. Also, voucher holders faced the possibility of losing the voucher if they did not find a place within the specified time limits (usually 90-120 days for initial voucher use and 30-60 days for subsequent moves).[95]

We tested whether receipt of the voucher had an impact on travel time from home to work or on transportation mode used for the commuting journey (see Exhibit 4.13). We did not find any significant effect on either of these outcomes.

There has been a great deal of research in the past 35 years on the hypothesis that some unemployment of African-American workers results from employment opportunities being spatially "mismatched" from the residences of workers. Ihlanfeldt and Sjoquist's 1998 review article draws the conclusion that "…the lack of geographical access to employment is an important factor in explaining labor market outcomes…"[96] The negative effect on employment could be ameliorated somewhat by access to private transportation. As cities have grown increasingly polycentric, access to private transportation has become more important to securing employment, because public transportation systems have been designed mainly to bring people to downtown areas. In the in-depth interviews, lack of transportation was widely cited as a barrier to obtaining and keeping employment. By far the preferred method for getting to work or a job interview was by car. All of the cities had some public transportation, and in several cities the public transportation systems were quite extensive. Nonetheless, cars offered the flexibility respondents needed to get to work on time and efficiently. Cars also allowed respondents to drop children off with care providers and pick them up quickly in the event of illness or other emergency.

However, cars presented challenges for respondents as well as advantages. The biggest problem was finding the money to purchase and maintain the car. Some of the voucher users interviewed in depth reported that the voucher made a difference here, since it gave respondents more disposable income with which to cover the on-going expenses of owning a car. We tested whether the voucher had a quantitative impact on two components of access to private transportation, having a valid driver's license and having access to a car that runs (Exhibit 4.14). We found no impact of the voucher on either component.

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[95] Some interviewees were uncertain about the actual length of the time limits, which contributed to a fear that they would not be able to find a new apartment fast enough. Also, many did not seem to understand that they could look for new housing without giving notice to the landlord and remain where they were if they were unable to find a better alternative.

[96] Keith Ihlanfeldt and David Sjoquist, "The Spatial Mismatch Hypothesis: A Review of Recent Studies and Their Implications for Welfare Reform," Housing Policy Debate, Vol. 9, Issue 4, p. 881.

Exhibit 4.13
Journey to Work

	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean[a]	ITT Impact	TOT Impact
Travel time from home to work				
0 minutes	1,739	0.016	0.000	0.000
		0.126	(0.000)	(0.000)
1-14 minutes	2,378	0.109	0.013	0.052
		0.311	(0.014)	(0.054)
15-30 minutes	2,468	0.235	0.012	0.048
		0.424	(0.019)	(0.077)
31-45 minutes	2,333	0.051	-0.007	-0.026
		0.221	(0.007)	(0.027)
46-60 minutes	2,371	0.038	-0.004	-0.014
		0.191	(0.003)	(0.014)
61-120 minutes	1,910	0.019	0.000	0.000
		0.137	(0.000)	(0.000)
Not employed	2,468	0.531	-0.017	-0.068
		0.499	(0.024)	(0.095)
Transportation mode				
Car, truck, or van	2,476	0.341	0.024	0.097
		0.474	(0.023)	(0.091)
Public transportation	2,422	0.078	-0.003	-0.010
		0.268	(0.009)	(0.036)
Walked	2,141	0.021	-0.002	-0.006
		0.145	(0.004)	(0.014)
Worked at home	1,733	0.015	0.000	0.000
		0.121	(0.000)	(0.000)
Other method	2,242	0.017	-0.001	-0.002
		0.129	(0.001)	(0.002)
Not employed	2,476	0.528	-0.016	-0.064
		0.499	(0.024)	(0.095)

Notes:
These impacts are estimated for the follow-up survey respondent sample.
ITT = "Intent-to-Treat". TOT = "Treatment-on-Treated".
Standard errors in parentheses.
*** indicates p < .01, ** indicates p < .05, * indicates p < .10
[a] Standard deviations of control group outcomes are beneath control means.

Exhibit 4.14
Access to Private Transportation

	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean[a]	ITT Impact	TOT Impact
Has valid driver's license, but no access to car	2,348	0.041	0.006	0.025
		0.200	(0.006)	(0.024)
Has access to a car that runs, but no license	2,343	0.030	-0.001	-0.002
		0.171	(0.003)	(0.014)
Both license and car	2,472	0.331	0.014	0.057
		0.471	(0.023)	(0.090)
Neither license nor car	2,426	0.069	-0.001	-0.004
		0.254	(0.003)	(0.013)
Not employed	2,472	0.529	-0.015	-0.058
		0.499	(0.024)	(0.095)

Notes:
These impacts are estimated for the follow-up survey respondent sample.
Only employed respondents were asked whether they possessed a valid driver's license and whether they had access to a car that runs.
[a] Standard deviations of control group outcomes are beneath control means.

The in-depth interviews with treatment group voucher holders reveal other obstacles to work or to steady, full-time work.

The availability and cost of childcare was a significant barrier to working for many interviewees. At $6.50 an hour, a typical wage, respondents who could get a full 40 hours in a week could earn $260. For a family with several children, childcare costs might eat up a third to a half of that income. To help pay for childcare, respondents frequently relied on state subsidies. Even when cost was not an issue, finding childcare that covered the work hours offered to many job seekers was problematic. Many of the available service and retail positions required the newest workers to staff the evening and night shifts when childcare is less available or to work split shifts. Bureaucratic barriers could be considerable as well. For one subsidy program, a respondent reported that, although the subsidy was easy to obtain for those on TANF, for those not on TANF, there was a long waiting list.

Other barriers to participating in the labor force frequently cited by interviewees were poor health and disabilities. Approximately one-quarter of the respondents we interviewed reported that they had disabilities or health problems that prevented them from working.[97] In addition to their own problems, several interviewees attributed their inability to work to the health problems of a child or other family member.

[97] The proportion of the 141 in-depth interviewees with disabilities or health problems that prevented them from working was roughly double the proportion with a disability found among the 2,481 respondents to the follow-up survey.

Using follow-up survey responses, we categorized respondents who were not working according to whether they were actively searching for a job and whether they had a disability that prevented them from accepting a job. About 12 percent of the control group reported that they were not looking for a job and that they had such a disability. We found no significant impacts of the voucher on the likelihood of being in any of these categories (see Exhibit 4.15). We also found no impact of the voucher on the number of hours looking for work in the past four weeks.

One hypothesized effect of the voucher is that it would allow treatment group members to move to neighborhoods where more of the neighbors were working, and that this would help voucher holders in learning about job opportunities. We tested whether treatment group members were more likely to have found a job through a neighbor, but found no significant effect.

The follow-up survey asked respondents who were not working and who wanted a job how much an employer would need to offer to make it worthwhile to accept a job, if the job had benefits like health insurance. Economic theory predicts that an increase in non-labor income, such as that represented by the housing voucher, will increase a non-working person's reservation wage (the lowest wage at which it is worthwhile to work). Consistent with this economic theory, we find a marginally significant, but small, effect of the housing voucher on having a reservation wage in the $3 to $5.99 range. About 1.8 percent of the control group is not currently working, wants a job, and is willing to take a job with benefits only if it pays no less than $3 to $5.99 per hour. A slightly smaller percent of the treatment group is willing to accept a job in this wage range.[98] There is no difference between treatment and control group members in the likelihood of being in any other reservation wage category. The small control mean in the $16.00 or more category indicates that most of those who are not working but who want a job would be willing to accept a wage of less than $16.

Public Assistance Impacts

Exhibit 4.16 shows the impacts of the vouchers on the *average quarterly receipt rate* within each follow-up half-year. In the first half-year, an average of 60 percent of the control group received TANF in each quarter. By the seventh half-year, only about 29 percent of the control group was receiving TANF in a quarter. Over these 3.5 years, treatment group members persistently received TANF at a significantly higher rate than control group members. Even though treatment group members were also leaving TANF during this period, their TANF receipt declined at a slower rate than the TANF receipt of the control group. Out of a possible 14 quarters of TANF receipt, control group members averaged 6.0 quarters of receipt during the first 3.5 years of follow-up. The impact of the housing voucher on treatment group members who used their voucher was an additional 0.8 quarters of TANF receipt over this time period—that is, an additional 13 percent more quarters of receipt.

[98] Although the standard errors shown in the table for the impact on being unemployed with a reservation wage in the $3.00-$5.99 range are larger than the ITT and TOT impacts, statistical significance is derived from the raw coefficient in the probit model. See Appendix B.3.2 for additional details about impact estimation with the probit model.

Exhibit 4.15
Impacts on Labor Force Status from Follow-up Survey

Outcome	Fifth Year, All Sites except Los Angeles: Sample Size	Control Mean[a]	ITT Impact	TOT Impact
Working	2,478	0.472	0.017	0.068
		0.499	(0.024)	(0.095)
Not Working:				
Actively job searching in past four weeks	2,434	0.184	-0.015	-0.059
		0.387	(0.016)	(0.063)
Not actively searching, has disability that prevents accepting a job	2,446	0.115	-0.006	-0.023
		0.320	(0.009)	(0.036)
Not actively searching, without disability that prevents accepting a job	2,446	0.222	0.010	0.039
		0.416	(0.018)	(0.073)
Hours spent looking for work in past four weeks (includes those who are not job searching)	2,438	5.92	-0.35	-1.38
		19.05	(0.85)	(3.38)
Employed and found job through someone in neighborhood	2,421	0.062	0.002	0.008
		0.240	(0.009)	(0.036)
Reservation wage:				
$3 to $5.99	1,863	0.018	-1.2×10^{-6}***	-4.7×10^{-5}***
		0.132	(1.4×10^{-5})	(5.6×10^{-5})
$6 to $8.99	2,406	0.157	0.004	0.018
		0.364	(0.014)	(0.055)
$9 to $12.99	2,406	0.154	-0.003	-0.010
		0.361	(0.015)	(0.059)
$13 to $15.99	2,299	0.028	-0.002	-0.009
		0.164	(0.004)	(0.015)
$16.00 or more	1,899	0.016	0.000	0.000
		0.127	(0.001)	(0.004)

Notes:
These impacts are estimated for the follow-up survey respondent sample.
ITT = "Intent-to-Treat". TOT = "Treatment-on-Treated". Standard errors in parentheses.
*** indicates p < .01, ** indicates p < .05, * indicates p < .10
[a] Standard deviations of control group outcomes are beneath control means.

The steeply declining control means for TANF amount received parallel the declining receipt rates (see the right-hand panel of Exhibit 4.16). In five of the first seven half-years of follow-up, treatment group members received statistically significantly more in TANF benefits than control group members. The monthly impacts on the benefits of treatment group members who leased up were modestly sized, about $16-24 more per month. Over the

3.5 years, the treatment effect for treatment group members who leased up was an additional $739 in TANF benefits, representing an increase of about 11 percent.

Exhibit 4.16
Impacts on TANF Cash Benefits

	Average Quarterly Receipt Rate All Sites Except Los Angeles			Amount Received All Sites Except Los Angeles		
	Control Mean[a]	ITT Impact	TOT Impact	Control Mean[a]	ITT Impact	TOT Impact
Half-year 1	0.598	0.019**	0.044**	$1,325	$40*	$93*
	0.464	(0.008)	(0.019)	$1,583	(22)	(53)
Half-year 2	0.521	0.023**	0.043**	$1,153	$27	$44
	0.478	(0.009)	(0.019)	$1,535	(24)	(49)
Half-year 3	0.465	0.021**	0.047**	$1,048	$29	$74
	0.475	(0.010)	(0.021)	$1,509	(26)	(59)
Half-year 4	0.416	0.020**	0.056**	$887	$54**	$140**
	0.472	(0.010)	(0.023)	$1,419	(26)	(62)
Half-year 5	0.376	0.029***	0.078***	$791	$56**	$134**
	0.465	(0.010)	(0.024)	$1,366	(26)	(64)
Half-year 6	0.348	0.027***	0.072***	$698	$40	$113*
	0.454	(0.010)	(0.025)	$1,285	(25)	(66)
Half-year 7	0.287	0.013	0.047*	$571	$43*	$141**
	0.424	(0.009)	(0.024)	$1,178	(25)	(65)
Total number of quarters received TANF over follow-up period	6.022	0.303***	0.776***			
	5.156	(0.096)	(0.247)	-	-	-
Total, all half-years	-	-	-	$6,473	$289**	$739**
				$8,438	(132)	(339)

Notes:
N = 7,622.
ITT = "Intent-to-Treat". TOT = "Treatment-on-Treated". Standard errors in parentheses.
*** indicates p < .01, ** indicates p < .05, * indicates p < .10
[a] Standard deviations of control group outcomes are beneath control means.

Exhibit 4.17 shows similar impacts on the average quarterly receipt rate of food stamps in each half-year. Over the follow-up period, control group receipt of food stamps declined less steeply than TANF receipt, from an average quarterly rate of 71 percent in the first half-year to 64 percent in the seventh half-year. The impact of the voucher on all treatment group members (the ITT estimate) was a statistically significant higher receipt rate of about 2 to 3 percentage points over the first six half-years. For those treatment group members who leased up, the voucher increased receipt rates by about 7 percentage points in most half-years (the TOT estimate). On average, control group members received food stamps in 9.2 quarters out of the first 14 quarters of the follow-up period. Treatment group members who

leased up received an additional quarter of food stamps, representing an impact of an additional 10 percent more receipt. The impact of the voucher on the receipt of food stamp benefits seemed to persist over the entire follow-up period.

Exhibit 4.17
Impacts on Food Stamp Benefits

	Average Quarterly Receipt Rate All Sites Except Fresno and Los Angeles			Amount Received All Sites Except Fresno and Los Angeles		
	Control Mean[a]	ITT Impact	TOT Impact	Control Mean[a]	ITT Impact	TOT Impact
Half-year 1	0.709 0.423	0.024** (0.009)	0.070** (0.028)	$1,098 $926	$49** (19)	$146** (57)
Half-year 2	0.665 0.440	0.026** (0.011)	0.037 (0.025)	$1,043 $947	$70*** (22)	$128** (50)
Half-year 3	0.655 0.455	0.025** (0.011)	0.068** (0.030)	$1,114 $1,034	$46* (24)	$93 (64)
Half-year 4	0.647 0.453	0.029** (0.011)	0.085*** (0.032)	$1,092 $1,041	$72*** (24)	$250*** (68)
Half-year 5	0.643 0.452	0.027** (0.011)	0.073** (0.033)	$1,096 $1,047	$62** (25)	$144** (73)
Half-year 6	0.628 0.464	0.021* (0.012)	0.077** (0.036)	$1,111 $1,092	$63** (26)	$223*** (79)
Half-year 7	0.642 0.456	0.016 (0.011)	0.069* (0.035)	$1,144 $1,105	$34 (26)	$135* (80)
Total number of quarters received food stamps over follow-up period	9.176 5.191	0.334*** (0.119)	0.959*** (0.334)	-	-	-
Total value, all half-years	-	-	-	$7,698 $6,229	$397*** (133)	$1,119*** (369)

Notes:
N = 5,056.
ITT = "Intent-to-Treat". TOT = "Treatment-on-Treated". Standard errors in parentheses.
*** indicates p < .01, ** indicates p < .05, * indicates p < .10
[a] Standard deviations of control group outcomes are beneath control means.

The modest decline in food stamp receipt for control group members is not reflected in the pattern of mean food stamp benefit values, so we may infer either that those who left food stamps had atypically low benefits or that those who continued to receive food stamps saw benefit amounts increase somewhat. The impact of the voucher on treatment group members who leased up (the TOT impact) was (when significant) in the range of $21-$37 per month. Over the first 3.5 years of follow-up, treatment group members who leased up received an additional $1,119 in food stamp benefits, representing an increase in benefits of about 15 percent.

Exhibit 4.18 shows the impact of the voucher on the sum of TANF and food stamp benefits. It is important to examine this sum, because in cases where sample members receive both types of benefits, the amount of food stamp benefits is dependent, in part, upon the TANF benefit payment. Treatment group members have both higher average quarterly receipt and higher total value of public assistance. Over 14 quarters of follow-up, control group members averaged 9.4 quarters in which they received either TANF or food stamps. The TOT impact shows that treatment group members who leased up received 0.8 more quarters of public assistance, an impact of 9 percent. Over this time period, treatment group members who leased up received an additional $1,918 in public assistance, representing an impact of 17 percent.

The follow-up survey provides information on the receipt of public assistance in the fifth year after random assignment. At the time of the follow-up survey, there was no statistical difference in the current quarterly earnings of treatment and control group members (as seen in Exhibit 4.10). Given the finding of no difference in earnings as of the survey date, we would expect to see no difference in the receipt of TANF or food stamps. Exhibit 4.19 shows that the voucher had no impact on TANF receipt or amount in the fifth year. However, the impact on food stamps receipt tells a different story. Although the impact estimate is statistically insignificant, the magnitude is similar to that seen with the food stamps administrative data. The lack of statistical significance could simply be an artifact of the smaller sample size of the follow-up survey. Treatment group members receive food stamps at a rate about 2 percentage points higher than that of the control group. Taken together, these results are somewhat of a puzzle. One would expect that impacts on TANF receipt and food stamps receipt would be similar. It may be that some sample members had reached time limits for TANF receipt, pushing estimated impacts toward zero.

Interestingly, the voucher's impact on the receipt of Supplemental Security Income (SSI) is the reverse of the overall impact on TANF and food stamps. Treatment group members are 3 percentage points *less likely* to have someone in their household receive SSI. This may be a household size effect—with fewer people in treatment group households, the receipt of SSI by someone in the household is less likely. An alternative explanation is that those with lower receipt of TANF and food stamps may out of necessity exert extra effort in establishing eligibility for SSI.

Finally, we found no effect on the size of the 2003 tax refund for treatment group members. This was consistent with the impacts on earnings for this time period, which were insignificant and close to zero.

Exhibit 4.18
Impacts on TANF Cash Benefits and/or Food Stamp Benefits

	TANF or Food Stamps Average Quarterly Receipt Rate All Sites Except Fresno and Los Angeles			TANF and Food Stamps Amount Received All Sites Except Fresno and Los Angeles		
	Control Mean[a]	ITT Impact	TOT Impact	Control Mean[a]	ITT Impact	TOT Impact
Half-year 1	0.734 0.411	0.017* (0.009)	0.050* (0.027)	$1,814 $1,627	$95*** (35)	$283*** (104)
Half-year 2	0.688 0.433	0.017* (0.010)	0.024 (0.025)	$1,629 $1,574	$109*** (38)	$167* (86)
Half-year 3	0.669 0.450	0.022** (0.011)	0.062** (0.030)	$1,653 $1,625	$82** (40)	$206* (108)
Half-year 4	0.657 0.450	0.028** (0.011)	0.076** (0.032)	$1,528 $1,544	$118*** (38)	$391*** (108)
Half-year 5	0.654 0.450	0.025** (0.011)	0.061* (0.032)	$1,483 $1,525	$115*** (38)	$283** (112)
Half-year 6	0.635 0.462	0.022* (0.012)	0.080** (0.035)	$1,458 $1,514	$99*** (38)	$342*** (117)
Half-year 7	0.647 0.455	0.018 (0.011)	0.065* (0.035)	$1,436 $1,460	$56 (37)	$246** (116)
Total number of quarters received either TANF or food stamps over follow-up period	9.368 5.155	0.299** (0.118)	0.837** (0.330)	-	-	-
Total value of TANF and food stamps, all half-years	-	-	-	$11,002 $9,071	$675*** (205)	$1,918*** (579)

Notes:
N = 5,056.
ITT = "Intent-to-Treat". TOT = "Treatment-on-Treated". Standard errors in parentheses.
*** indicates p < .01, ** indicates p < .05, * indicates p < .10
[a] Standard deviations of control group outcomes are beneath control means.

Exhibit 4.19
Impacts on Public Assistance in Prior Month and on Tax Refund in 2003
(Follow-Up Survey)

	Receipt All Sites Except Los Angeles				Amount Received All Sites Except Los Angeles			
	Sample Size	Control Mean[a]	ITT Impact	TOT Impact	Sample Size	Control Mean[a]	ITT Impact	TOT Impact
TANF Cash Assistance in prior month	2,470	0.258 0.437	-0.008 (0.019)	-0.031 (0.076)	2,457	$104 217	$7 (9)	$29 (37)
Food Stamp Benefits in prior month	2,464	0.653 0.476	0.021 (0.022)	0.083 (0.089)	2,450	$218 207	$6 (8)	$25 (33)
Supplemental Security Income (SSI) in prior month	2,460	0.225 0.418	-0.033** (0.017)	-0.133** (0.068)	2,416	$134 294	-$2 (16)	-$8 (65)
Tax refund, including Earned Income Tax Credit (EITC), for tax year 2003	2,441	0.320 0.467	0.016 (0.022)	0.063 (0.088)	2,358	$794 1,440	$35 (66)	$139 (263)

Notes:
These impacts are estimated for the follow-up survey respondent sample.
ITT = "Intent-to-Treat". TOT = "Treatment-on-Treated". Standard errors in parentheses.
*** indicates p < .01, ** indicates p < .05, * indicates p < .10
[a] Standard deviations of control group outcomes are beneath control means.

Education and Training Impacts

One hypothesized effect of the housing voucher was that the increased income represented by the voucher might be used to pursue educational or training opportunities, in order to improve earnings in the long run. The in-depth interviews with treatment group voucher users provide insight into the education experiences of voucher users. About half of those interviewed said they had pursued some form of education or training since receiving the voucher, but in most cases the amount of education was modest, consisting of short-term certificate courses and job training programs. In some cases, respondents would have liked to pursue longer-term educational programs, such as two- and four-year degree programs, but were unable to do so because of income constraints or the requirements of the TANF or voucher program. TANF and PHA caseworkers often encouraged respondents to take vocational courses for which tuition reimbursement and childcare subsidies were available from the state. Among those interviewed who had completed an educational program since receiving the voucher, about half became employed at some point in the field in which they were trained.

There was a great deal of consistency in how voucher users who were interviewed in depth talked about the effect of the voucher on their education since random assignment. Women

who had undertaken little or no education since receiving the voucher usually said that the voucher had no effect on this choice. Other factors, such as the need to work full-time, the desire to stay home with children, or the cost of tuition, were more important in their decision not to pursue education. By contrast, the interviewees who had pursued a non-trivial amount of education since receiving the voucher reported that the voucher influenced their decision to do so. Most of these respondents said that they would not have been able to pursue their education without the voucher because they would have had to work full-time to afford unsubsidized rent.

In order to assess quantitatively how the voucher affected education decisions, we constructed several outcomes capturing the education and training experiences of sample members over the follow-up period. We then tested whether receiving the housing voucher had an effect on these outcomes.

Exhibit 4.20 contains treatment effect estimates for education- and training-related outcomes. Looking at education alone, we tested whether total years of schooling was affected by the voucher and found no significant effect. We then tested whether receiving a voucher increased the likelihood of obtaining a H.S. diploma or G.E.D. for those who did not possess either at baseline. There was no statistical difference between treatment and control group members.

Looking at both education and training, we tested whether treatment group members were more likely than control group members to pursue academic, vocational, adult basic education, or job search training. The difference between treatment and control group members in each of these categories was statistically insignificant. There was also no significant difference in the proportions of treatment and control group members who received any type of education or training.

Finally, there was no significant difference between the treatment and control groups in total hours of education or training.

The drop in employment for the treatment group during the first year after random assignment allowed for the possibility that treatment group members were pursuing additional education or training during this time. Given the lack of any significant impacts for the above measures, however, the hypothesized effect on investment in human capital was not supported, the suggestive anecdotal evidence from the in-depth interviews notwithstanding.[99]

[99] It is possible that the most important determinant of receiving a non-trivial amount of education or training is individual motivation. About 37% of those who received a non-trivial amount of education/training did not ever use a housing voucher. (This proportion is roughly constant at definitions of "non-trivial" of over 200 hours, over 500 hours, and over 1000 hours of education/training.) About half of the control group members and one-quarter of the treatment group members who received a non-trivial amount of education/training never used a voucher. It may be that while a voucher is indeed helpful to those who are highly motivated to obtain education/training, the absence of a voucher is not enough to deter these individuals.

Exhibit 4.20
Impacts on Education and Training

Outcome	Sample Size	Control Mean[a]	ITT Impact	TOT Impact
		Fifth Year, All Sites except Los Angeles		
Years of schooling	2,434	11.361	0.021	0.082
		2.194	(0.069)	(0.276)
Obtained H.S. diploma or GED (Of those who had no diploma or GED at baseline)	613	0.027	0.000	0.000
		0.164	(0.000)	(0.001)
Type of education or training received :				
Academic	2,471	0.143	-0.004	-0.016
		0.350	(0.013)	(0.050)
Vocational	2,471	0.218	0.021	0.083
		0.413	(0.018)	(0.071)
Adult basic education	2,471	0.039	0.000	-0.001
		0.193	(0.002)	(0.009)
Job search	2,421	0.100	-0.011	-0.042
		0.300	(0.011)	(0.043)
Any education or training	2,471	0.431	0.002	0.008
		0.495	(0.023)	(0.091)
Hours of education or training received (including those who received zero hours)	2,472	277.07	1.03	4.11
		667.51	(28.68)	(114.16)

Notes:
These impacts are estimated for the follow-up survey respondent sample.
ITT = "Intent-to-Treat". TOT = "Treatment-on-Treated". Standard errors in parentheses.
*** indicates p < .01, ** indicates p < .05, * indicates p < .10
[a] Standard deviations of control group outcomes are beneath control means.

4.6 Impacts Estimated by Subgroup

In this section, we present estimated impacts of the housing voucher on subgroups identified by baseline characteristics. In viewing the pattern of statistically significant estimates by subgroup, it is important to note that sample sizes, and therefore the precision of the estimates, varied widely across subgroups. This means that an impact that would be detected as statistically significant for one subgroup may not be significant for another, smaller, subgroup. Thus, differences in statistical significance across subgroups reflect differences in sample sizes, as well as differences in true impact.

Subgroup Impacts on Employment and Earnings

Exhibits E.1 and E.2 contain estimated impacts on employment and earnings for subgroups defined by the following characteristics:

- Age (<25, 25-34, 35-44, 45+)

- Race/ethnicity (white, non-Hispanic; black, non-Hispanic; Hispanic)
- Education (high school diploma; GED only; neither)
- School enrollment at baseline (yes/no)
- Presence of dependent children at baseline (yes/no)
- Age of youngest household member at baseline (<6, 6-17, 18+)
- Employment status at baseline (employed; not employed, by reservation wage level)
- Ever employed at baseline (yes/no)
- Job training status at baseline (enrolled; enrolled but not yet participating; not enrolled)
- Desired to move for employment reasons (yes/no)
- Housing status at baseline (rents or owns; public housing; lives with others or in shelter)
- TANF receipt at baseline (not receiving; receiving, by months until time limit)

Consistent with the finding of no significant impact for the sample as a whole, only a few subgroups had statistically significant impacts on the two outcomes analyzed by subgroup: total number of quarters employed and total earnings over the first seven half-years of follow-up. For each outcome and subgroup set, we ran an F-test to determine whether the impacts for the subgroups within a set differed significantly from one another.[100] For example, examining subgroups defined by housing status at baseline in Exhibit E.1, we observe a significant negative treatment effect on numbers of quarters employed through half-year 6 for those who resided in public or assisted housing. We do not observe a significant effect for those who rented or owned an apartment or house or for those living with friends/relatives or in a shelter. However, the F-test indicates that the subgroup impacts are not significantly different from one another. Therefore, while we can conclude the treatment had a significant effect through 6 half-years on those living with friends/relative or in a shelter at baseline, we *cannot* conclude that the treatment effect was necessarily different for this group than for those who own or rent or those residing in public or assisted housing.

The F-test results show that no set of subgroups had significantly different effects on employment through 6 half-years of follow-up. After 7 half-years of follow-up, there were significantly different impacts on employment in only one set of subgroups, those defined by expiration of the TANF benefit. Of those who were receiving TANF at baseline, those who knew that their benefits expired in less than 18 months may have had positive effects on employment.[101] In contrast, for those who knew that their benefits expired in more than 18 months, the voucher had more negative employment effects than for the other subgroups. The effect on employment for those who were not on TANF at baseline was insignificant, and the point estimate was close to zero.

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[100] It should be noted that the F-tests for differences in subgroup treatment effects were based on different models than the models that produced the results discussed here. The results presented in these chapters are derived from models run separately for each subgroup. The F-test results are derived from models run on the entire sample, where each subgroup is interacted with treatment in a combined model. In addition, the F-tests test for differences between subgroup ITT estimates, rather than for differences between subgroup TOT estimates.

[101] The ITT was only significant for the subgroup reporting their benefits would expire in 6-12 months.

Four individual subgroups had marginally significant (at the 0.10 level) ITT or TOT impacts on the number of quarters employed. Three of these subgroups had negative impacts: those with neither a high school diploma nor a G.E.D. at baseline, those who had enrolled in but not yet started job training at baseline, and those who resided in public or assisted housing at baseline. It may be that those who were in comparatively more difficult circumstances within the sample had a larger negative employment impact. Those who said that their TANF benefits were expiring within 6 to 12 months may have experienced a positive employment impact. Speculation about what factors might drive these results should be tempered with the knowledge that this number of marginally significant impacts is no more than would be expected by chance alone.

Similarly for few subgroups were there significant impacts of the voucher on total earnings. Those with neither a high school diploma nor a G.E.D. at baseline, those who resided in public or assisted housing at baseline, and those who desired to move for employment reasons[102] experienced negative impacts. The F-test results indicate that the effect on earnings is significantly different for those who desired to move for employment reasons compared to those who did not desire to move for employment reasons. Marginally significant impacts on those with a G.E.D. only and those whose youngest household member is 18 or older appear to be driven by a small number of influential observations, given the changes in point estimates when Los Angeles is dropped from the sample.

The large negative impact on the earnings for those who expressed a desire to move for employment-related reasons at baseline is, we think, quite important for understanding the effects of housing vouchers on employment and earnings. This result, a 32 percent reduction in earnings over the follow-up period, may seem counter-intuitive—one might have thought that those families who wanted to move for employment-related reasons would be best able to take advantage of the voucher to improve their employment and earnings. The results demonstrate that this is not the case.

Our interpretation of this result is as follows. We believe that this group's statement that they wanted to move to get a job or to be near their job (as opposed to, say, to be near better schools or to get away from drugs and gangs) identified them as placing a high priority on employment. Indeed, controls in this subgroup showed strong earnings growth in the period immediately following random assignment. As shown in Exhibit 4.21, control earnings increased by more than 50 percent in the first five quarters after random assignment, a substantially larger increase than that achieved by the subgroup who did not wish to move for employment-related reasons. This subgroup, then, was presumably actively engaged in job search at the time they applied for WtW vouchers and believed that the voucher would aid them in that search. These results suggest that not only did the voucher not assist their job search, it actually hindered it—probably by diverting time and energy from job search to a search for new housing and, if successful, to moving and settling into a new dwelling and/or neighborhood.

[102] Baseline respondents were asked if they would like to move to another house or neighborhood and, if so, their main reason and second most important reason for wanting to move. Those who responded that their main or second most important reason for moving was to get a job or to be near their job were coded as wanting to move for employment-related reasons.

Exhibit 4.21
Earnings by Quarter for the Control Subgroups Did and Did Not Desire To Move for Employment Reasons at Baseline – All Sites Except Los Angeles

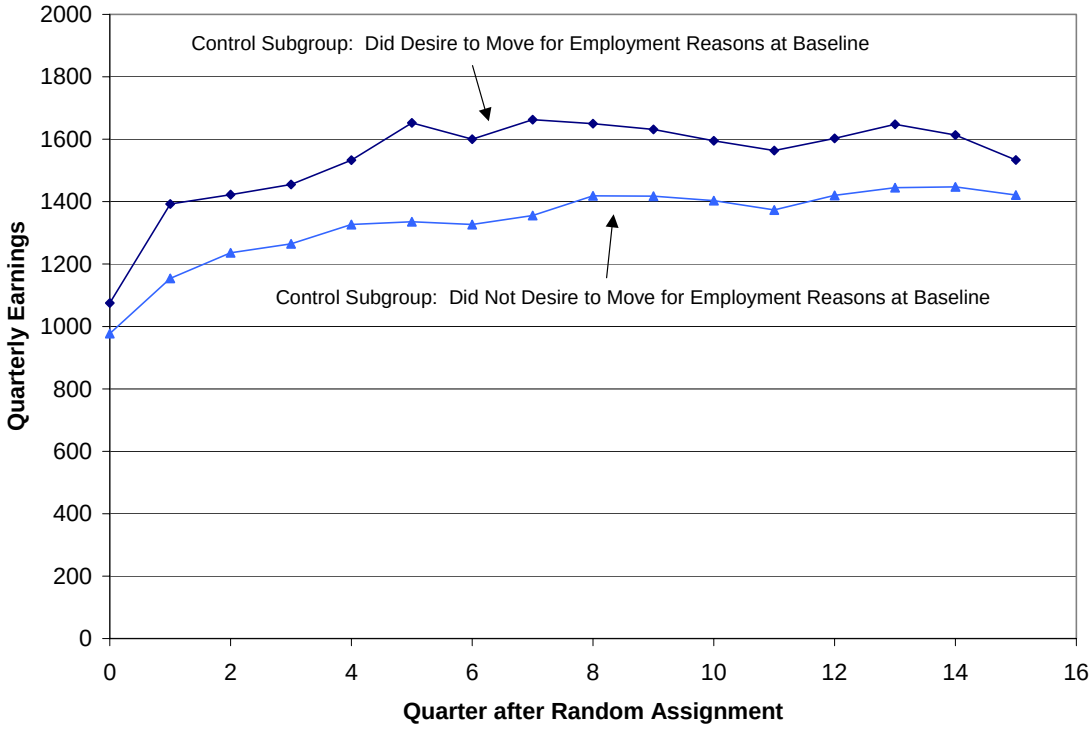

Exhibit 4.22
Earnings by Quarter for the Subgroup Did Desire To Move for Employment Reasons at Baseline – All Sites Except Los Angeles

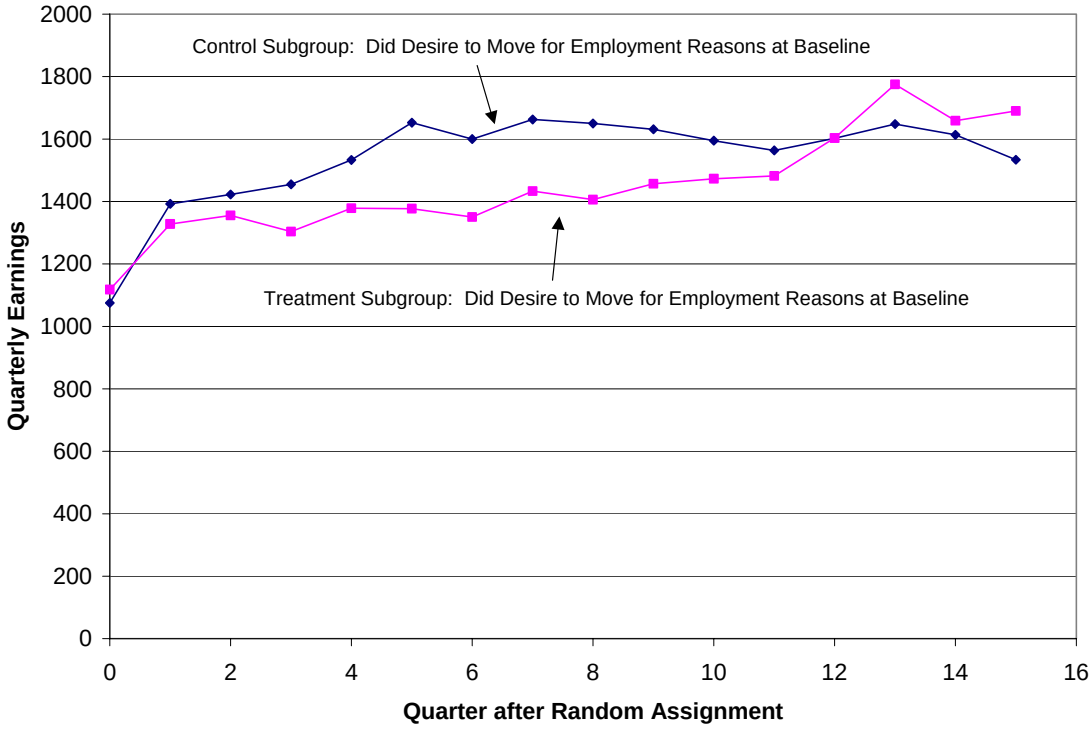

Exhibit 4.22 shows the time paths of quarterly earnings for treatment and control group members in the subgroup who expressed an interest in moving for employment-related reasons. As can be seen, both groups made substantial earnings gains in the first quarter after random assignment. In the next four quarters, control earnings continued to rise while treatment group earnings plateaued, opening a gap of several hundred dollars per quarter between the two groups that persisted through the second year of the follow-up period. Over the third year, the gap gradually diminished until, by the end of that year, it was closed.

Exhibit 4.23 shows the estimated impacts on earnings for the subgroup who desired to move for employment-related reasons and the subgroup who either did not wish to move or wanted to move for other reasons. As can be seen, the impacts on half-yearly earnings for those in the former group who leased up (the TOT estimates) are quite large, rising from 27 percent of the control mean in the first half-year to over 40 percent in the next three half-years, and 36 and 32 percent in half-years 5 and 6, before disappearing in half-year 7. Over the entire 3.5-year follow-up period, this subgroup experienced a 32 percent reduction in earnings because of the vouchers.

Exhibit 4.23
Impacts on Biannual and Total Earnings for Two Subgroups

	Desired to Move For Employment Reasons All Sites Except Los Angeles			Did Not Desire to Move For Employment Reasons All Sites Except Los Angeles		
	Control Mean[a]	ITT Impact	TOT Impact	Control Mean[a]	ITT Impact	TOT Impact
Half-year 1	$2,858	-$350**	-$773**	$2,486	-$127*	-$296*
	$3,467	(175)	(386)	$3,303	(68)	(159)
Half-year 2	$3,191	-$683***	-$1,326***	$2,651	-$63	-$83
	$3,881	(199)	(386)	$3,590	(77)	(158)
Half-year 3	$3,246	-$693***	-$1,334***	$2,684	-$58	-$163
	$3,992	(209)	(428)	$3,726	(83)	(183)
Half-year 4	$3,327	-$584***	-$1,379***	$2,818	$26	$10
	$4,087	(215)	(479)	$3,991	(91)	(220)
Half-year 5	$3,183	-$393*	-$1,161**	$2,822	$49	$37
	$4,253	(227)	(527)	$4,036	(93)	(230)
Half-year 6	$3,223	-$264	-$1,018*	$2,827	$55	$65
	$4,260	(236)	(565)	$4,105	(96)	(246)
Half-year 7	$3,145	$190	-$63	$2,873	$133	$285
	$4,365	(273)	(654)	$4,386	(101)	(265)
Total, all half-years	$22,173	-$2,777**	-$7,054**	$19,159	$16	-$146
	$23,722	(1192)	(2801)	$22,621	(483)	(1219)

Notes:
N = 1,121 for regressions run on ""Desired to Move". N = 6,360 for regressions run on "Did Not Desire to Move".
ITT = "Intent-to-Treat". TOT = "Treatment-on-Treated". Standard errors in parentheses.
*** indicates p < .01, ** indicates p < .05, * indicates p < .10
[a] Standard deviations of control group outcomes are beneath control means.

Several aspects of these estimates are noteworthy. First, although the subgroup who wanted to move for employment-related reasons comprises only 15 percent of the sample,[103] it accounts for virtually the entire impact on earnings for the entire sample. This can be seen from the ITT estimate for the subgroup who did not wish to move for employment-related reasons, which is essentially zero. Second, a large, negative, statistically significant effect on the earnings of the subgroup who wished to move for employment-related reasons persists throughout most of the follow-up period, but becomes small and statistically insignificant in the last half-year. This is consistent with our interpretation that this earnings loss reflects a temporary disruption of this subgroup's search for employment. Finally, it should be noted that after an initial relatively small, marginally significant earnings reduction in the first quarter, impacts on the earnings of the 85 percent of the sample who did not desire to move for employment-related reasons were never large or statistically significant.

Subgroup Impacts on Public Assistance Receipt

Exhibits E.3 through E.5 show subgroup results for TANF receipt, food stamps receipt, and for TANF and food stamps combined. Many of the subgroup impact estimates are statistically significant, which is consistent with the significant impacts for the sample as a whole. In general, we found statistically significant impacts in the largest subgroups; this suggests that sample sizes were an important factor in the determination of which subgroup impacts were statistically significant.

F-test results show that, over 7 half-years of follow-up, impacts on TANF receipt and benefit value differed significantly only within the set of subgroups defined by presence of children at baseline. The voucher significantly increased TANF benefits for those with dependent children at baseline and significantly decreased TANF benefits for those with no dependent children at baseline.

For the whole sample (except Los Angeles), the vouchers increased TANF receipt and benefit value by an estimated 0.78 quarters and $739 for treatment group members who leased up (Exhibit 4.16). Subgroups that experienced significant impacts greater than these average impacts on both TANF receipt and TANF benefit value included Hispanics, those with dependent children at baseline, those whose youngest member of the household was under 6 at baseline, and those who desired to move for employment reasons.

Many of the subgroups in Exhibit E.4 both have significant positive impacts for food stamp receipt and increases in the benefit value that are larger than the average for the four sites shown in Exhibit 4.9.[104] These subgroups include Blacks, Hispanics, those with dependent children at baseline, those whose youngest household member was age 6-17 at baseline, those who were not employed at baseline, those enrolled in job training at baseline, those who desired to move for employment reasons, those living with friends or relatives or in a shelter at baseline, and those who were not receiving TANF at baseline. Those not employed at baseline who had a reservation wage of $3.00 to $5.99 had significant *negative* treatment effects on food stamps

[103] 1,121 sample members out of a total sample of 7,481 in the five sites other than Los Angeles.

[104] Food stamps data were not available for Fresno or Los Angeles.

receipt and value.[105] (However, this subgroup had a small sample size, leaving open the possibility that these results were driven by a few influential observations.)

F-test results show that impacts on food stamp benefits were significantly different within the sets of subgroups defined by presence of children at baseline, employment status and reservation wage at baseline, and whether desire to move was employment-related. Impacts on combined TANF and food stamp assistance, presented in Exhibit E.5, also differed significantly within these three sets of subgroups.

Subgroups that had significant positive treatment effects on combined TANF and food stamp assistance larger in magnitude than the average overall effects shown in Exhibit 4.17 include: those with G.E.D. only at baseline, those whose youngest household member was age 6-17 at baseline, those who were not employed at baseline, those enrolled in job training at baseline, those who had enrolled in but not yet started job training at baseline, those who desired to move for employment reasons, and those who were not receiving TANF at baseline. As with food stamps alone, the small group who were not employed at baseline who had a reservation wage of $3.00 to $5.99 had significant *negative* treatment effects on combined public assistance receipt and value.

Impacts on TANF and food stamps benefits for those who indicated a desire to move for employment-related reasons at baseline were consistent with our earlier estimates of earnings losses for this group. Those in this group who leased up experienced particularly large effects of an additional 1.7 quarters of TANF receipt (a 28 percent impact), an additional $1,890 in TANF benefits (a 33 percent impact), 2.1 additional quarters of food stamps receipt (a 23 percent increase), and an additional $2,483 in food stamps benefits (a 34 percent increase). Use of a voucher increased combined TANF and food stamps benefits within this subgroup by $3,084 (29 percent of the control mean). Moreover, the large, statistically significant impacts for this subgroup persisted throughout the follow-up period; Exhibit 4.24 shows impacts on combined TANF and food stamps benefits for the subgroup who wanted to move for employment-related reasons and the subgroup who did not.

Unlike the earnings analysis, which shows that the subgroup who wanted to move for employment-related reasons accounted for virtually the entire impact, there were also significant positive impacts on public assistance receipt and amount among the remaining 85 percent of the sample who did not express a desire to move for employment-related reasons. The impact on the subgroup who wanted to move for employment-related reasons accounts for only about a quarter of the total impact on public assistance benefits for the entire sample.[106] Since there was no impact on the earnings for the 85 percent of the sample who

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[105] The federal minimum wage over the follow-up period was $5.15 per hour. Therefore, those who are willing to accept a job with benefits that pays $3.00-$5.99 per hour have a low reservation wage.

[106] The proportion of the total impact attributable to this subgroup is calculated as:

(Total impact on benefits to subgroup)/(Total impact on benefits to full sample)

= (701 x $1,083)/(4,953 x $675),

= .227,

where 701 = sample size of subgroup, $1,083 = ITT impact on subgroup, 4,953 = overall sample size, and $675 = ITT impact on overall sample.

did not express a desire to move for employment-related reasons, the increase in public assistance benefits in this subgroup must have occurred for reasons unrelated to the earnings of the household head.

Exhibit 4.24
Impacts on TANF Cash Benefits and Food Stamp Benefits for Two Subgroups

	Desired to Move For Employment Reasons TANF and Food Stamps Amount Received All Sites Except Fresno and Los Angeles			Did Not Desire to Move For Employment Reasons TANF and Food Stamps Amount Received All Sites Except Fresno and Los Angeles		
	Control Mean[a]	ITT Impact	TOT Impact	Control Mean[a]	ITT Impact	TOT Impact
Half-year 1	$1,833	-$66	-$189	$1,817	$116***	$339***
	$1,582	(98)	(281)	$1,641	(38)	(112)
Half-year 2	$1,499	$250**	$826***	$1,655	$78*	$41
	$1,467	(104)	(223)	$1,596	(41)	(94)
Half-year 3	$1,546	$265**	$255	$1,673	$49	$183
	$1,611	(111)	(295)	$1,631	(44)	(116)
Half-year 4	$1,496	$203*	$538*	$1,538	$101**	$345***
	$1,504	(106)	(293)	$1,552	(42)	(116)
Half-year 5	$1,522	$226**	$762**	$1,482	$94**	$193
	$1,525	(110)	(316)	$1,528	(41)	(120)
Half-year 6	$1,466	$116	$260	$1,461	$95**	$334***
	$1,501	(104)	(329)	$1,516	(42)	(125)
Half-year 7	$1,460	$87	$632*	$1,437	$53	$187
	$1,452	(104)	(333)	$1,465	(40)	(124)
Total value of cash assistance and food stamps, all half-years	$10,822	$1,083*	$3,084*	$11,063	$585***	$1,621***
	$8,707	(571)	(1623)	$9,139	(223)	(617)

Notes:
N = 701 for regressions run on ""Desired to Move". N = 4,252 for regressions run on "Did Not Desire to Move".
ITT = "Intent-to-Treat". TOT = "Treatment-on-Treated". Standard errors in parentheses.
*** indicates p < .01, ** indicates p < .05, * indicates p < .10
[a] Standard deviations of control group outcomes are beneath control means.

One possible reason for the increase in the food stamps portion of public assistance benefits for those who did not express a desire to move for employment-related reasons is that use of the voucher may have reduced the number of other workers in the household. The point estimate, although not statistically significant, suggests a reduction in the number of other workers in the household of about 25 percent in this subgroup (see Exhibit E.6). Such an impact would be consistent with the statistically significant one-third reduction in the number

of other workers in the household for the sample as a whole and with the more general reduction in household size found in Chapter 3. It appears that the vouchers caused many treatment group families to move out of multigenerational households or other arrangements where they were living with relatives or other adults, some of whom were workers (see Exhibit 3.10). The loss of other earners in the household could be expected to increase food stamp benefits, which are based on household income, but would not affect TANF benefits, which are generally based only on the income of the "assistance unit"—the parent and children.[107] Since the increase in food stamp benefits accounted for more than half of the increase in combined public assistance benefits in this subgroup, this may explain an important part of this impact.[108]

We performed additional subgroup analysis within the large subgroup of those who did not desire to move for employment reasons. The estimated impacts on earnings, TANF benefits, and food stamp benefits are shown in Exhibits E.7 and E.8. We found no obvious pattern of results which would help explain the increase in the TANF portion of public assistance benefits for this large subgroup.

4.7 Interpretation of Impact Estimates

This chapter has examined the effect of Housing Choice Vouchers on employment, earnings, and employment-related outcomes; education and training; and receipt of public assistance, including TANF and food stamp benefits.

We hypothesized that vouchers might affect employment and earnings through a number of different channels, some leading to reductions in labor supply and some leading to increases. Changes in earnings were hypothesized to lead to changes in receipt of public assistance in the opposite direction, since those benefits are conditioned on earnings. The hypothesized channels through which vouchers might affect earnings include:

1. Economic theory predicts a reduction in labor supply because of the increase in unearned income and higher marginal tax rate on earnings associated with the voucher. The effect of the voucher tax rate is probably substantially mitigated by the fact that low income families already face high tax rates in other income-tested programs, such as TANF, food stamps, and the EITC; the voucher may add little to the cumulative tax rate across all programs.
2. Vouchers may increase the stability of the family, decrease stress, and lead to an improved sense of control and ability to plan their lives. This may result in more

[107] Food stamps benefits are also adjusted for household size. It seems likely, however, that the loss of other workers' earnings would more than offset any reduction in benefits because of the decrease in household size.

[108] The proportion of the impact on combined benefits attributable to the impact on food stamps benefits is calculated as:

(TOT impact on food stamps benefits)/(TOT impact on combined public assistance benefits)

= $872/$1621

= 0.538

active job search, greater likelihood of job retention and, therefore, increased employment and earnings.

3. Program participants may use the increased discretionary income freed up by the voucher to pursue educational or training opportunities. This may improve earnings for the family *in the long run*.
4. Residential relocation, or even the search for new housing, may lead to temporary disruptions in earnings and employment for persons who were already working and need to take time off, or change jobs entirely, to engage in housing search and relocation. Voucher recipients who were engaged in job search may suspend their job search in order to search for housing and/or to relocate. Residential relocation may also disrupt pre-existing social support networks that are important sources of informal childcare and labor market information and connections, with resultant negative effects on employment and earning. These disruptions could reduce employment and earnings until new social networks have been established.
5. Residing in an area close to potential sources of employment may reduce job search costs and lead to a broader range of employment opportunities. Also, the expectation of lower commuting costs may reduce the reservation wage during job search. Once the participant is employed, commuting costs may be lower, which may lead to increased hours of work and earnings.
6. Community norms in lower-poverty neighborhoods may be more supportive of work and less supportive of welfare. To the extent that recipients feel increased pressure to work and to leave welfare sooner, this might increase job search, employment, and earnings.
7. Residing in a safer neighborhood may decrease family stress and improve mental and physical health, enabling more active job search, longer job retention, and, therefore, increased employment and earnings.

We found no evidence of increased employment and earnings. For both the sample overall, and for the subgroups analyzed, the only statistically significant impacts observed were reductions in earnings. Thus, hypotheses 2, 5, 6, and 7, all of which predict increased earnings, are not supported by the data, at least during the 3.5-year follow-up period examined here.[109]

Hypothesis 3, that voucher users might use the discretionary income freed up by the voucher to obtain education and training that would increase their earnings, might not be expected to affect earnings within the time frame analyzed here. However, it should affect participation in education and training within that time frame. We found no evidence of such an effect. It should be noted that the impact of vouchers on participation in education and training may be muted in this sample by the fact that TANF recipients in both the treatment and control groups were subject to the same work requirement, which may be satisfied by participating in education and training. Nearly 45 percent of each group participated in some education or training over the course of the follow-up period. This high rate of participation may mask

[109] It is, of course, possible that these mechanisms were operative, but were not sufficiently strong to offset earnings reductions caused by other factors.

any impact that would be observed if the decision to participate in education and training were purely voluntary. At the same time, these work requirements may have prevented voucher recipients from pursuing longer-term educational programs and, therefore, masked a potential impact of the voucher on obtaining additional years of schooling or educational degrees.

The potential explanations for the negative impacts on earnings found here are hypothesis 1, which rests on the work disincentives created by the unearned income and marginal tax rate embodied in the voucher, and hypothesis 4, which points to the disruptions associated with the search for new housing and/or relocation. We would expect these two mechanisms to have different time patterns of effect. The income and tax rate effects of the voucher should be evident as long as the voucher is in use; for most sample members who leased up, that would be the entire follow-up period. Disruption effects should manifest themselves soon after random assignment but then disappear at some point.

The impact estimates tend to support the temporary disruption hypothesis. We observed large earnings reductions in at least some subgroups throughout the first three years after random assignment but no significant effect in the fourth year.[110] While further follow-up would be highly desirable in order to arrive at a definitive conclusion, on the basis of the available evidence we conclude that this was a transitory effect.

Subgroup analysis revealed that the entire impact on earnings for the sample as a whole was attributable to reduced work effort among the 15 percent of the sample who said at baseline that they desired to move for employment-related reasons. This result, a 32 percent reduction in earnings over the 3.5-year follow-up period, may seem counter-intuitive—one might have thought that those families who wanted to move for employment-related reasons would be best able to take advantage of the voucher to improve their employment and earnings. We believe, however, that this group's statement that they wanted to move to get a job or to be near their job (as opposed to, say, to be near better schools or to get away from drugs and gangs) identified them as placing a high priority on employment. Indeed, controls in this subgroup showed strong earnings growth in the period immediately following random assignment. This subgroup, then, was presumably actively engaged in job search at the time they applied for WtW vouchers and believed that the voucher would aid them in that search. These results suggest that not only did the voucher not assist their job search, it actually hindered it—probably by diverting time and energy from job search to a search for new housing and, if successful, to moving and settling into a new dwelling and, in many cases, a new neighborhood.

Given large earnings reductions confined to the 15 percent of the sample who desired to move for employment-related reasons, we would expect positive impacts on public assistance receipt confined to the same subgroup. We did in fact find relatively large positive impacts on the benefit values of public assistance for the sample as a whole (11 percent more in TANF benefits, 15 percent more in food stamp benefits, and 17 percent more in combined benefits). But the impact on the subgroup who wanted to move for employment-related

[110] We have earnings data for the entire sample without Los Angeles only through the first half of the fourth year. For the 93 percent of the five-city sample for whom data for the second half of the fourth year are available, however, there was no effect on earnings during that half-year.

reasons accounted for less than one-quarter of the increase across the entire sample. Since there was no impact on the earnings of the 85 percent of the sample who did not express a desire to move for employment-related reasons, the increase in public assistance benefits in this subgroup must have occurred for reasons unrelated to the earnings of the household head. One possible explanation for this apparent inconsistency is that voucher use may have reduced the number of other earners in the household. A reduction in earnings of household members that are not part of the TANF case would tend to increase food stamps benefits, although it would not affect TANF benefits. But reduction in support—income and informal—from other adult members of the household could have increased the voucher recipient's incentive to use TANF benefits.

These results lead to several conclusions about the costs and benefits of providing housing assistance through vouchers. First, housing vouchers alone should not be seen as a tool for encouraging work. In fact, these results suggest that vouchers actually hinder the transition to work for the subgroup of current and former welfare recipients who are motivated to seek jobs or better jobs. Vouchers may encourage work if combined with other special interventions; we were not able to test that hypothesis in this study, given the way in which Welfare to Work Vouchers were implemented.

Second, we find that the earnings loss associated with use of the voucher is transitory. After three years, voucher users' earnings are about equal to what they would have been in the absence of the voucher.

Third, at least initially, use of housing vouchers has some distributional consequences to taxpayers beyond the cost of the voucher. Taxpayers pay about $550 per year in additional TANF and food stamp benefits to voucher users, for at least the first 3.5 years after the voucher is received. This amount represents a relatively small increase in the cost of the voucher to taxpayers—on the order of 10 percent of the mean value of the voucher.[111]

[111] HUD data from April 2003 indicate the average monthly housing assistance payment was $456. This translates into an average annual payment of $5,472.

Chapter Five
Impacts on Poverty and Material Hardship

In this chapter we turn our attention to the impacts of housing vouchers on the financial and material well-being of families, measured using data from the follow-up survey. We begin with a brief summary of the findings and then discuss the hypotheses and data for our analysis of the vouchers' role in avoiding poverty and reducing material hardship. The chapter's next two sections assess the vouchers' impacts on poverty, while the remaining sections examine impacts on homelessness, independent housing, household crowding, food expenditures, and several other measures of families' material well-being and hardship.

5.1 Summary of Findings

The vouchers significantly reduced poverty at the time of the follow-up survey. This is the finding when near-cash income[112] that households receive from the vouchers and other sources are taken into account along with cash income. The program had this effect even though (1) the effects of the vouchers on earnings and welfare payments were insignificant at the time of the follow-up survey when the poverty outcomes were measured, and (2) the fraction of control group families using housing vouchers had risen nearly to the level for treatment group families.[113] Furthermore, the voucher treatment undoubtedly had an even greater effect on poverty earlier in the evaluation's observation period, when the treatment group's near-cash income from the vouchers far exceeded that of the control group.

The impacts of the vouchers on several measures of material hardship were statistically significant for virtually all types of households in the study:

- A substantial reduction in homelessness;
- An increase in independent housing and a corresponding reduction in doubling up;
- An increase in the average number of rooms for household members and a corresponding reduction in crowding; and
- Increased household expenditures on food, which raised average family consumption but did not significantly reduce food insecurity.

The first three of these impacts demonstrate that the vouchers significantly increased the ***quantity*** of eligible families' housing—for example, whether they had any housing (were not homeless) and how much housing they had (the number of their rooms) at the time of the

[112] As described in Section 5.2 "near-cash income" is a broader definition of income that takes into account the value of non-cash transfer income, net value of federal income taxes, and social security payroll taxes.

[113] The "treatment on treated" (TOT) impact estimates correct for controls' use of vouchers over the entire course of the evaluation's observation period. However, for the reasons discussed later in this chapter, this correction does not fully capture the narrowing of the treatment-control difference in voucher use by the time of the follow-up survey, when the outcomes examined in this chapter are measured.

survey. The vouchers, however, did not significantly improve the ***quality*** of the housing. The treatment group's overall assessment of the physical quality of their housing and the number of specific problems they reported (e.g., faulty plumbing, leaking roof) were not significantly different from those for controls.

The difference in voucher receipt between the treatment and control groups, modest by the time of the survey, had been much greater earlier in the observation period. Some of the substantial effects of the voucher program on material hardship clearly reflect this earlier treatment-control difference.

The impacts of the vouchers were larger for some of the subgroups within the overall sample. One such group includes welfare recipients who said at the time of random assignment that their eligibility for TANF would expire within six months. These recipients, who faced different TANF time-limit rules depending on the state where they live[114], experienced a substantial reduction in poverty when near-cash income was included in the assessment. In addition, impacts on homelessness for this group, which was vulnerable to dislocation because of the impending loss of public assistance, were dramatic: homelessness in the year before the survey was cut in half, and this result was statistically significant. The vouchers significantly reduced other material hardships for this group. Food insecurity was substantially reduced, and instances in which families could not afford needed dental care dropped.

Another important at-risk subgroup consists of households whose heads were unemployed at the time of random assignment. Again, the impacts of the vouchers on poverty (counting near-cash income), homelessness, and independent housing were very large. In addition, the impacts on several other hardship measures – notably the number of rooms in the family residence and the household's food expenditures – were impressive.

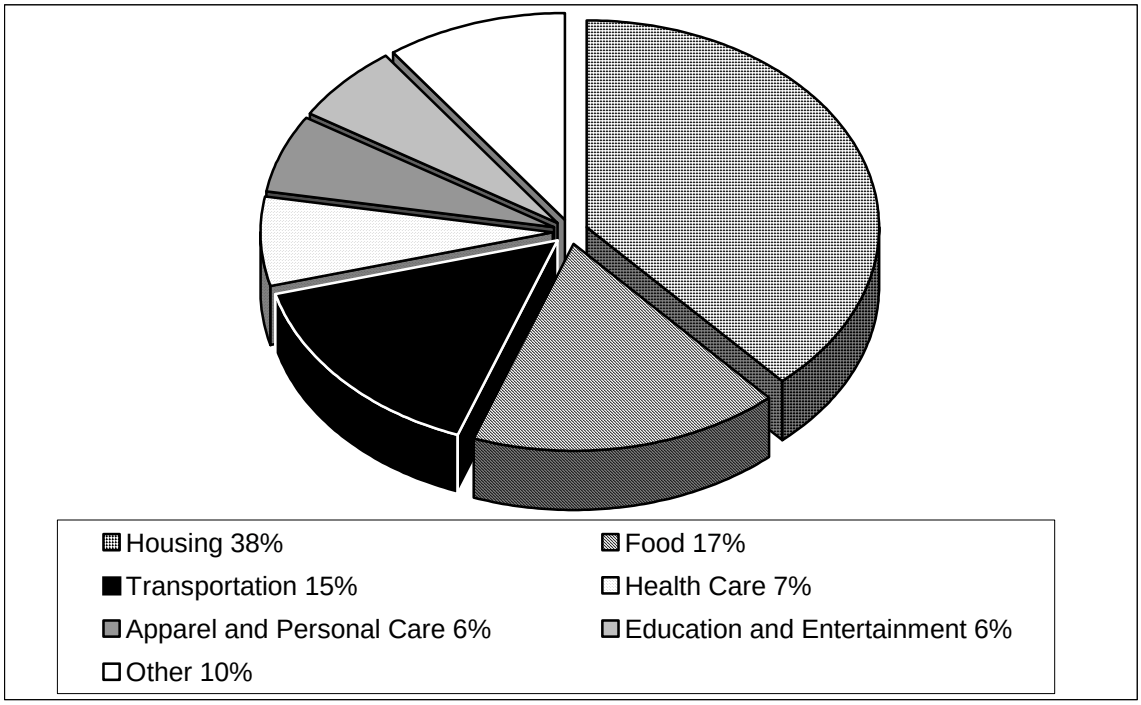
Finally, the vouchers appear to have substantially improved the well-being of families with children, particularly those with children less than six years old. For this group, the vouchers had significant impacts on virtually all outcomes considered in this analysis, including poverty and homelessness. Thus, the reductions in poverty and hardship generated by housing vouchers clearly reached young children.

5.2 Hypothesized Effects of the Vouchers

As explained in the last chapter, the expected effects of the vouchers on earnings were unclear as this analysis began. The vouchers provided stability and a chance to move to areas with more opportunity, both of which could lead to increased earnings. However, the income and substitution effects of the vouchers should tend to reduce earnings. The uncertain net impact on earnings means that the effect of the vouchers on income, and hence on the official federal measure of poverty, was also unclear.

[114] Treatment group members receiving TANF in California (Fresno and Los Angeles sites) and Washington (Spokane site) faced five-year lifetime limits and no intermittent limits on TANF assistance. In California, site-funded assistance continued beyond five years, but only for the children in the household. The lifetime limit was also five years in Texas (Houston site), but shorter limited apllied to specific spells of welfare receipt. In Georgia, (Atlanta and Augusta sites) the lifetime limit was four years, and no intermittent limits were imposed.

Exhibit 5.1
National Spending Patterns of Low-Income Families, 2003



Notes:
Low-income families are those whose incomes were below $10,000 during 2003. Calculations were made by Abt Associates.

Source: U.S. Bureau of Labor Statistics Consumer Expenditure Survey, 2003

In contrast, the boost that housing vouchers provides to families' "near-cash" income is unambiguous. As noted earlier in the report, the dollar value of the vouchers is high for some families and more modest for others. Thus, the vouchers can be expected to lift some families above the federal poverty threshold when such near-cash income is taken into account. This improvement in economic status would then be expected to improve the well-being and reduce the hardships of low-income families.

It is anticipated that housing vouchers will generate impacts on families' housing security by reducing homelessness, the need to double up, and poor housing conditions. Impacts on other measures of material hardship, while less direct, also can be expected. Because the vouchers free up disposable family income for non-housing purchases, we expect that the vouchers will produce impacts on food expenditures and food security. They also might generate effects on spending for other basic needs, notably transportation and medical care. As shown in Exhibit 5.1, low-income families – those with incomes below 200 percent of the poverty level – spend more than $4 out of every $5 they have on housing, food, transportation, and medical care. Housing absorbs 38 percent of all family expenditures, while food accounts for 17 percent of the total.

Housing vouchers can be expected to affect some subgroups of the overall sample more than others. We hypothesized that impacts would be particularly important for the following groups:

- ***TANF recipients whose assistance will expire within six months.*** The behavior of individuals who reported at baseline that they would soon lose their cash assistance is likely to be quite different from that of family heads who are not close to their time limit or do not receive TANF. Families are often in crisis when they begin to receive welfare—because of job loss, separation of parents, and other events—and may be in a better position to pursue financial self-sufficiency later in their TANF spell (that is, closer to their time limit). Also, the incentive to achieve self-sufficiency—and perhaps to use vouchers in reaching this goal—becomes greater as the end of welfare eligibility draws near.
- ***Unemployed.*** Vouchers can potentially make a greater difference for people who are less able to succeed on their own. This could be true for voucher recipients who were unemployed at the time of random assignment. The vouchers could enable them to move to areas with better employment opportunities and/or to stabilize their lives so that obtaining and maintaining employment would be easier.

These two subgroups are also potentially important for policy. If vouchers are found to be more effective for these groups, policymakers may decide to target the groups in future housing assistance programs.

5.3 Data Used in the Analysis

All of the outcome data used in the analysis presented in this chapter come from the follow-up survey.[115] Many of the specific outcome variables were constructed from the responses of sample members to two or more survey questions. Our poverty measures are based on the cash income reported by survey respondents for the month before each interview was completed. Respondents reported their past month's income in detail for the survey. For one of the poverty measures, based on cash income, the last month's income was annualized and then compared to the U.S. Census Bureau's 2004 poverty thresholds for different household sizes. For the second measure, a respondent's annualized income was compared to 75 percent of the poverty threshold.

The other poverty measures use a broader definition of income. To determine the total cash and near-cash income of the households in the survey sample, we calculated the dollar value of pertinent items for respondents in the month prior to the interview. Total monthly cash and near-cash income includes:

- Earnings from all jobs;
- TANF and all other cash transfer income;
- Dollar value of the household's housing assistance;
- Dollar value of food stamps;
- Net value of federal income taxes, taking account of the Earned Income Tax Credit as well as other tax provisions; and
- Social Security payroll taxes.

[115] See Appendix C for analysis of non-response bias for outcomes derived from survey data.

This broader definition of income is consistent with the recommendations of a National Academy of Science committee for the measurement of poverty.[116] Taking account of near-cash income is particularly valuable in this evaluation, because of the dollar value of the housing vouchers, the most important source of near-cash income to the households that receive them. The vouchers substitute for cash in the purchase of housing, freeing up cash for other types of household consumption.

As for the poverty measures, most of the material hardship outcomes addressed in this analysis are binary variables constructed based on responses to one or more survey questions. For example, the homelessness variable is given a value of one if a respondent's interview answers indicate his or her household did not have a place to live at some point in the year before the interview.

We use the in-depth interviews with 141 voucher users to help interpret the estimates of impacts of the vouchers on poverty and material hardship that are based on the follow-up survey of a larger sample of both treatment and control group members.

5.4 Results for the Full Sample

Poverty

Chapter 4 presented our findings for voucher program impacts on earnings and on cash assistance from the TANF program, which together largely determine the cash income of households in the treatment and control groups. Income is the primary measure of the financial well-being for families. Official poverty, which is defined as income below a federally defined threshold, is the most widely accepted measure of financial insecurity. In-kind forms of public assistance, such as subsidized housing and food stamps, do not contribute to the cash income of households and thus do not figure in assessments of poverty using the official definition. However, these forms of assistance clearly affect the financial and material well-being of households.

Cash Income Poverty. The vouchers had no effect on official or "cash income" poverty. Exhibit 5.2 shows how the vouchers affected income poverty—that is, whether or not a family's cash income places it below the federal poverty threshold. The overall intent-to-treat (ITT) impacts on income poverty were small and statistically insignificant. Eighty-five percent of control group members were in poverty at the time of the survey, and the effect of the vouchers on this was not statistically significant. A smaller fraction of controls, 78 percent, had incomes that were less than three-quarters of the poverty threshold. The incomes of essentially the same proportion of the treatment group families fell below this level.

[116] The committee's recommendations also call for consideration of households' work expenses and state and local taxes, but these items could not be estimated with confidence based on our survey data. For discussion of the National Academy committee's recommendations, see C. F. Citro and R. T. Michael, eds., *Measuring Poverty: A New Approach* (Washington, DC: National Academy Press, 1995). It may be noted that unmeasured items, such as state and local taxes, affect cash and near-cash income of households. Some unmeasured items, however, do not qualify as "near cash" or constitute expenses (e.g., child care and other work-related expenses) that affect disposable income available for household consumption.

These results should not be surprising, given that this assessment of poverty is based on the income of survey respondents in the month before their interviews. (Appendix Exhibit F.2 compares respondents' reported incomes during all of 2003—the last full calendar year before most of the interviews—to the federal poverty thresholds for the same year.) At the time individuals were interviewed, the earnings and TANF assistance of treatment and control group families were essentially the same. Earlier in the follow-up period the control group's earnings had been somewhat higher than those of the treatment group.

The estimated treatment-on-treated (TOT) impacts on income poverty are also shown in Exhibit 5.2. They offer a sense of the magnitude of the impacts on families that actually received housing vouchers—insight that is less important for income poverty than for other outcomes discussed below.

Total Income Poverty. Exhibit 5.2 also presents the results for poverty assessed in terms of total after-tax income, taking account of in-kind as well as cash income. As indicated earlier, the dollar values of housing assistance, food stamps, and several other items are counted. Using this broader definition of income, the percent of treatment and control group members in poverty drops, as does the fraction below 75 percent of the poverty threshold. Housing vouchers significantly reduce the proportion of households in poverty when both cash and near-cash income are considered. The estimated impact of the voucher on the proportion of households below 75 percent of poverty is larger and is statistically significant at the 0.05 level.

The reduction in poverty, when near-cash income is considered, is smaller at the time of the survey than it was a year or more earlier. Because of the housing vouchers, the near-cash income of treatment group members was much higher than that of controls earlier in the evaluation's observation period, when fewer controls were using vouchers. As noted in Chapter 4, income from earnings and TANF assistance changed over the course of the observation period. These changes are small, however, compared to the changes in near-cash income from housing vouchers occurring during the same time period.

During the first year following random assignment, many treatment group members, but few controls, received substantial near-cash income from housing vouchers. As discussed in Chapter Two (see Exhibit 2.5), more than half of treatment group members had leased-up, and begun to use their vouchers, five months after random assignment. Only a tiny fraction of controls had obtained vouchers by this time. The monthly value of a voucher was variable—ranging from a small amount to more than $1,000—but, on average, far exceeded the greater earnings received by the control group during the early months of the observation period. Thus, there is little question that the vouchers had a positive and probably substantial short-term impact on poverty.

Exhibit 5.2
Impacts on Poverty

Poverty Measure	Impacts on Poverty in Month before Survey Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean[a]	ITT Impact	TOT Impact
Cash income below poverty threshold	2,431	0.846	0.012	0.048
		0.361	(0.014)	(0.056)
Cash income below 75% of poverty threshold	2,451	0.775	-0.006	-0.025
		0.418	(0.018)	(0.073)
Cash and near-cash income below poverty Threshold	2,438	0.647	-0.042 *	-0.165 *
		0.478	(0.022)	(0.088)
Cash and near-cash income below 75% of poverty threshold.	2,451	0.452	-0.045 **	-0.181 **
		0.498	(0.023)	(0.092)

Notes:
ITT = "Intent-to-Treat". TOT = "Treatment-on-Treated". Standard errors in parentheses.
*** indicates p < .01, ** indicates p < .05, * indicates p < .10
[a] Standard deviations of control group outcomes are beneath control means.

This impact faded over time. As shown in Exhibit 2.5, the fraction of the treatment group using vouchers gradually rose to more than 60 percent, while the percentage of controls using vouchers increased at a much more rapid pace. In addition, some of the early voucher users—mostly treatment group members—stopped using their vouchers. Thus, by the time of the survey, 50.5 percent of survey respondents in the treatment group were benefiting from vouchers, compared to 37.2 percent of the control group. The average dollar value of housing vouchers to the full control group sample was actually nearly equal to the average value to the treatment group in the month before the survey.

The other types of cash and near-cash income received by the two groups were generally similar in the month before the survey. The earnings of the treatment group, which had trailed those of controls in most earlier months, were higher in the month prior to the survey: $584 compared to $557 for controls. The average TANF payment was $110 for the treatment group and $107 for the control group. The value of the food stamps received by the treatment group was $222 for treatment and $216 for the control group. All told, the total cash and near-cash income received by the treatment group was slightly higher than that received by the control group.

Material Hardship

Having assessed the effects of the housing vouchers on poverty, we now examine some of the consequences of low income and poverty, namely poor living conditions and families' inability to meet their basic needs. Material needs fall into several categories, and there are varying opinions among researchers about the most appropriate measures to use in assessing

families' well-being or material hardship in any of these areas.[117] We will consider a number of the most widely used measures of material hardship in this analysis, looking first at housing security measures and then at food security, health security, and transportation.

Housing Security. Exhibit 5.3 presents the impacts of the vouchers on several measures of housing security. The first is homelessness, one of the most closely watched of these measures. A quarter of the control group members who answered the pertinent survey questions report that their household did not have a place of his/her own to live at some point in the year before the interview.[118] The effect of the voucher was to reduce this by more than one third, a statistically significant result.

The results for homelessness can be divided into two categories: (1) families living on the streets or in shelters while homeless, and (2) families who moved in with or among friends, relatives, or others.[119] More than a quarter of the control group members who reported being homeless were in the first group, while the remainder was in the second. The vouchers reduced homelessness in the first category from 7 to 5 percent and they cut homelessness in the second category from 18 to 12 percent. Both impacts (ITT impacts) were statistically significant.

The TOT impact estimates, while not definitive, suggest that housing vouchers eliminated much of the homelessness that families would have faced without the vouchers. The estimated impacts on overall homelessness, and on both categories of homelessness, are much larger than the control means for these outcomes.

Homelessness was assessed in terms of the household's experience in the past year. This one-year period, which is routinely used in evaluation studies that address homelessness, includes months in which the housing assistance received by the treatment group was still much greater than for the control group.[120] Given this, it is not surprising that the housing vouchers generated these impacts on homelessness.

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[117] See T. Oullette, N. Burstein, D. A. Long and E. Beecroft, *Measures of Material Hardship* (Cambridge, MA: Abt Associates, 2004).

[118] This was the survey question: "Was there ever a time during the past year (that is, since MONTH/YEAR) when you did not have your own place to stay?" The same question has been used in other studies, including the evaluations of HOPE VI and the Moving to Opportunity (MTO) initiative. In response to the same question, 16 percent of HOPE VI respondents answered "yes." The question was posed to 17 percent of MTO survey respondents, 26 percent of whom answered "yes."

[119] Those responding affirmatively to the question in the previous footnote were then asked a follow-up question. "During the past year, when you did not have our own place to stay, we would like to know about any places you stayed. Did you stay with a relative (yes/no); stay with a friend (yes/no); stay in a shelter (yes/no); stay on the street (yes/no).

[120] One year before the survey, about twice as many treatment group members had received and used housing vouchers as had controls.

Exhibit 5.3
Impacts on Housing Security

Housing Security Measure	Impacts on Housing Security, Fifth Year, All Sites except Los Angeles: Sample Size	*Control* Mean[a]	ITT Impact	TOT Impact
Did not have a place of ones own to stay at some point during the past year or living with others	2,478	0.248	-0.089 ***	-0.355 ***
		0.432	(0.017)	(0.068)
On the streets or living in shelters at some point during past year	2,478	0.068	-0.023 ***	-0.092 ***
		0.251	(0.008)	(0.032)
Living with friends, relatives, or others at some point during past year	2,448	0.175	-0.055 ***	-0.218 ***
		0.380	(0.015)	(0.059)
Independent housing at time of survey	2,479	0.831	0.059 ***	0.234 ***
		0.375	(0.014)	(0.055)
Number of rooms at time of survey	2,455	3.99	0.15 **	0.59 **
		1.38	(0.06)	(0.23)
Crowding at time of survey	2,451	0.389	-0.055 **	-0.219 **
		0.488	(0.023)	(0.090)
Quality of housing at time of survey	2,472	0.616	0.020	0.079
		0.487	(0.022)	(0.088)
Housing problems at time of survey	2,415	0.135	-0.018	-0.073
		0.341	(0.014)	(0.056)
Amount spent in rent, including utilities, in month before survey	2,174	$529	-$53 ***	-$211 ***
		$437	(18)	(72)

Notes:
ITT = "Intent-to-Treat". TOT = "Treatment-on-Treated". Standard errors in parentheses.
*** indicates p < .01, ** indicates p < .05, * indicates p < .10
[a] Standard deviations of control group outcomes are beneath control means.

The in-depth interviews with voucher users suggest that the vouchers reduced a closely related family hardship—namely, the ***fear*** of homelessness. While only one of those interviewed had been homeless since the time of random assignment, the concern about becoming homeless was more widespread. Many respondents reported that the voucher relieved worry about how to pay the rent, and a number said that, without the voucher, their family probably would have been homeless or doubled up with others. A few families did become homeless, at least for a time, after losing the voucher.

The impacts of the vouchers on another fundamental housing security measure, independent housing, are shown in the next row of Exhibit 5.3. While homeless families lack ***any*** place to

live, families without independent housing lack ***their own*** place to live. They usually live with friends, extended family members, or neighbors, and their housing consequently often is unstable. The hardship experienced by families without independent housing – families that must double up with others – translates into crowding, a lack of privacy, and household tension. Both homelessness and the lack of independent housing are caused by a family's inability to afford or locate appropriate housing. Some of the people who cannot afford suitable housing have been evicted from their previous housing, an event that is sometimes treated as a hardship measure.[121]

Survey respondents are considered to have "independent housing" if they rented or owned their own housing at the time of the survey. The overall ITT impact of six percentage points and the TOT impact of 23 points easily pass the statistical significance tests. These impacts, like those for homelessness, are dramatic.

Independent housing was assessed at the time of the survey, not over the one-year period before the survey. However, the *decisions* that produced the housing circumstances reported by survey respondents were made in the months before the survey—often many months before the survey. A decision to rent one's own apartment, instead of doubling up with friends or relatives, is followed by months of searching for an apartment before a move takes place. The treatment group, which received much more housing assistance early in the observation period, had more time than controls to make and implement such decisions.

The intensive interviews with treatment group members provide us a better sense of how this impact on independent housing occurred and what it meant to families. More than a third of the families interviewed were doubled up at the time they received the voucher. This group includes individuals who used the voucher assistance to make an initial break from their families and those who had been on their own but had returned to their family or moved in with friends or other relatives because of financial reverses or personal circumstances. The most common of these circumstances were loss of a job, birth of a new child, health or substance abuse issues, or a family break-up.

For those who were doubled up, the voucher generally provided much-needed relief from overcrowding. However, many in-depth interview respondents in this situation noted that having adequate space was not the primary advantage. These women instead talked about the voucher making it possible for them to become independent for the first time—a rite of passage that several characterized as a time of "growing up" that was long overdue. Others reported that the voucher enabled them to escape from unhealthy interactions with other household members and to better supervise their children without interference from other adults living in the unit. In addition, several interviewees mentioned the importance of having their names on leases. With this official acknowledgment, they felt they had more control over their living environment and were safer from the arbitrary actions of others. Those who were doubled up identified stress reduction as a major impact of the voucher.

[121] Evictions often have other causes. They are a relatively weak measure of hardship when inability to pay rent is controlled for. See S. E. Mayer and C. Jencks, "Poverty and the Distribution of Material Hardship," *Journal of Human Resources,* vol. 24, no. 1 (1989).

Exhibit 5.3 also presents the impacts of the vouchers on two measures of the adequacy of families' living space. The first is the average number of rooms in a family's housing unit. The average control household had just under four rooms at the time of the survey. As shown by the ITT and TOT impact estimates in the exhibit, the housing vouchers significantly increased this number.

An increase in rooms does not indicate, by itself, a reduction in hardship (families might have adequate space without an increase in rooms). However, the second measure, crowding, takes account of both rooms and the number of people who live in them. Specifically, crowding is defined as housing that has less than one room per person. Thirty-nine percent of the control group met this standard for being crowded. The vouchers significantly reduced this fraction.

The need for more space, particularly for more bedrooms, was cited frequently as a reason to move during the open-ended interviews with treatment group families. The voucher enabled many to move to larger quarters, and this was especially important to those with teenagers and children of opposite genders. The desire for more space also included closet space and common areas (living room, dining room, kitchen, and yards). Many respondents reported that for safety reasons "they never let their children go outside." For some families this concern continued even after making moves with voucher assistance. This may be one reason why having enough internal space was so important.

Even with the voucher, having more space presented challenges. A single-family unit was often described as the ultimate in space, privacy, and security, but concerns about high utilities caused some families to forgo this option. Others who had opted for single-family units or larger apartments had or were planning to step back to smaller quarters to reduce utility costs.

The next two rows in Exhibit 5.3 provide our impact findings for two measures of the physical quality of families' housing as reported by the respondent[122]. The first is whether the overall condition of the survey respondent's housing was reported to be "good" or "excellent." The second outcome is the presence of major housing problems, including rats or mice, broken windows, plumbing that does not work, cockroaches, a heating system that does not work, broken locks or no locks, and broken plaster or peeling paint. In this case, we measured whether the respondent to the follow-up survey reported two or more of these problems.

In contrast to their impacts on family living space, the housing vouchers did not have significant effects on housing quality. The ITT and TOT impacts on the self-reported overall physical quality of the housing were positive, but not significant. Similarly, a smaller fraction of the treatment group reported at least two major problems with their housing, but this fraction was not significantly lower than that for the control group. It is noteworthy that most control group members reported they had good-quality housing and only one in eight controls report major housing problems. As a result, the room for improvement in these

[122] Note that the measures of housing quality discussed here are from self-reported assessments of unit characteristics. They should not be interpreted to represent housing quality as determined by outside inspectors as part of HUD's Housing Quality Standards process.

areas was limited.[123] In addition, the in-depth interviews indicate that, for many respondents, housing quality was a lower priority than neighborhood characteristics, particularly crime and safety, school quality, and proximity to work and family.

As indicated in the last row of Exhibit 5.3, the vouchers substantially reduced the amount spent on rent and utilities. The reduction was statistically significant during the time just before the survey, despite the fact that the difference in the use of between treatment and control group members was only eight percentage points at the time of the survey. Families who had received a voucher at the time of random assignment were spending $53 less per month than those who had not (the ITT estimate). Families using vouchers were spending $211 per month less on housing than those not using vouchers (the TOT estimate). This effect, in turn, could reduce material hardships other than housing, which are discussed below.

Food Security. Exhibit 5.4 shows the effects of the housing vouchers on several measures of food security, beginning with the dollar amount of household food expenditures in the month before the survey. Treatment group members, having to spend less on rent because of the vouchers, were able to spend more on food for their families. The impacts on food spending are large and statistically significant. The estimated overall ITT impact is $10 in the month prior to the interview, nearly a ten percent increase compared to the monthly expenditure by the control group. The estimated TOT impact is almost $40.

The substantial impact on food expenditures clearly demonstrates how the vouchers improved families' financial situations, allowing them to spend more on other basic needs. Our intensive interviews with voucher users confirmed that the near-cash subsidy provided by the voucher had dramatic effects on family finances, but the extent of the improvement depended both on a family's circumstances at the time of voucher issuance and the housing choices they subsequently made. For families who were paying market rent at voucher issuance, and especially for those who leased-in-place, the financial impact was typically immediate and positive. Rent reductions in the hundreds of dollars were not uncommon.

However, families that were already receiving housing subsidies through another program such as public housing saw little change, and families that were doubled up when they received a voucher frequently experienced increased costs when they moved to their own units. In addition to changes in monthly housing costs, some of the doubled-up families reported additional costs related to furnishing their new homes. Beds and mattresses were mentioned most often.

Exhibit 5.4 also presents the impacts of the housing vouchers on four measures of food security. The first is a food security status binary variable that equals one if a household was "food insecure" according to criteria used by the U.S. Department of Agriculture (USDA).[124] Survey respondents were asked a series of questions used by USDA, as well

[123] Grigsby and Bourassa argue that the nation's housing stock has improved to the point that very few housing units are grossly inadequate, reducing the policy value of Section 8 vouchers. See "Section 8: The Time for Fundamental Program Change?", *Housing Policy Debate*, vol. 15, no. 4 (2004). The findings of this evaluation indicate that vouchers do not significantly improve housing *quality*, but do significantly increase the quantity of housing (both whether a family has any housing and the number of rooms it has).

[124] See M. Nord, M. Andrews, S. Carlsen, *Household Food Security in the United States, 2004*, ERS Food Assistance and Nutrition Report No. ERR11 (Washington, DC: U.S. Department of Agriculture, 2005). Our assessment of

as by the U.S. Census Bureau, to measure food security. More than two affirmative answers to these questions mean that a household is considered "insecure." For the second measure, survey answers are translated into a food security scale, and households with high scores on this scale are determined to be insecure. The other two measures in Exhibit 5.5 are average security status score and the proportion of respondents indicating that household members had gone without food for at least one day in the last month. None of the overall impacts on these measures is statistically significant.

The questions pertaining to food security were as follows. Respondents were asked to indicate whether each of the following statements were often, sometimes or never true in the 30 days prior to the interview.

- The food that (I/we) bought just didn't last, and (I/we) didn't have money to get more.
- (I/we) couldn't afford to eat balanced meals.

Respondents were also asked whether in the past 30 days, they or other adults in the household had ever cut the size of meals or skipped meals because there wasn't enough money for food. For those responding that any of these conditions had occurred, they were asked how many times each had happened in the 30 days prior to the interview. Finally, the respondents were also asked whether in the 30 days prior to the interview they had ever eaten less than they felt they should because there wasn't enough money to buy food, or if they had ever been hungry but didn't eat because they couldn't afford enough food.

We found that 42 percent of control group members met the criteria for food insecurity at the time of the follow-up survey, based on their responses to the USDA questions (see Exhibit 5.4). Not surprisingly, given the characteristics of the study sample, this is substantially higher than the national prevalence of food insecurity (11.9 percent of households in 2004) that USDA reported in their 2005 report on food security in the U.S. The national estimate of food insecurity for low-income households (annual income below 130 percent of poverty line) is more comparable to the estimate of food insecurity for control group members in the study group. According to the USDA's 2005 report, 34 percent of low-income households were food insecure.

Other Hardship Measures. Exhibit 5.5 presents the impacts of the vouchers on three measures of health security, the first of which is whether the member of the follow-up survey sample had any form of government-provided or private insurance. Not surprisingly, the ITT and TOT impacts were statistically insignificant. The impact of the vouchers on employment at the time of the survey, and thus on employer-provided insurance, was insignificant. Similarly, the impact on TANF eligibility, which conveys categorical Medicaid coverage, was insignificant by the time the interviewing was done.

food insecurity is based on two Department of Agriculture's "short form" metrics, which are scores assigned to household based on answers to six survey questions.

Exhibit 5.4
Impacts on Food Security

Poverty Measure	Impacts on Food Security, Fifth Year, All Sites except Los Angeles: Sample Size	Control Mean[a]	ITT Impact	TOT Impact
Food expenditures in the month before the survey	2,324	$106	$10 ***	$39 ***
		$71	(4)	(14)
Food expenditures per person in the month before the survey	2,320	$28	$3 ***	$13 ***
		$21	(1)	(5)
Household food security scale score	2,478	3.126	-0.057	-0.226
		3.054	(0.132)	(0.526)
Number of food related hardships in the past 30 days	2,478	2.233	-0.041	-0.162
		2.182	(0.094)	(0.376)
Household was food insecure during the past 30 days	2,477	0.424	-0.001	-0.004
		0.494	(0.023)	(0.091)
Household member(s) went without meals at least one day in last month	2,442	0.096	0.008	0.030
		0.295	(0.011)	(0.045)

Notes:
ITT = "Intent-to-Treat". TOT = "Treatment-on-Treated". Standard errors in parentheses.
*** indicates p < .01, ** indicates p < .05, * indicates p < .10
[a] Standard deviations of control group outcomes are beneath control means.

Exhibit 5.5 provides the impact results for two other health security outcomes—a family not having enough money to pay for medical or dental care. The binary dependent variable is set at one if family members were unable to get needed medical or dental care during the year before the survey because the family did not have enough money. The overall ITT and TOT impacts are not statistically significant. Given that health insurance coverage was not significantly changed by the vouchers, it is not surprising that the impacts on unaffordable care did not reach the level of statistical significance.

Exhibit 5.5 also provides impact estimates for several self-reported health outcomes collected on the follow-up survey. These are the respondent's assessment of: general health status; whether they were worried, tense, or anxious for at least one month during the past 12 months; whether they were worried, tense, or anxious more than most people would be in the same situation; typical number of hours of sleep each night; and current smoking status. We found no significant impacts of the vouchers on any of these measures of health status.

Exhibit 5.5
Impacts on Other Material Hardship Measures

Measures of Well-Being	Impacts on Well-Being Fifth Year, All Sites except Los Angeles Sample Size	Control Mean[a]	ITT Impact	TOT Impact
Had health insurance coverage at the time of the Survey	2,471	0.611 0.488	0.007 (0.023)	0.028 (0.091)
Could not afford needed medical care during the year before the survey	2,477	0.175 0.380	-0.008 (0.015)	-0.030 (0.060)
Could not afford needed dental care during the year before the survey	2,479	0.285 0.452	-0.011 (0.020)	-0.043 (0.078)
Had working motor vehicle at time of survey	1,178	0.765 0.424	-0.004 (0.030)	-0.018 (0.121)
General health is excellent, very good, or good	2,463	0.670 0.471	0.023 (0.021)	0.090 (0.084)
Worried, tense, or anxious for at least one month during the past 12 months	2,468	0.577 0.494	-0.016 (0.023)	-0.065 (0.091)
Worried, tense, or anxious more than most people would be in the same situation.	2,447	0.493 0.500	0.010 (0.023)	0.041 (0.092)
Time spent sleeping each night	2,429	6.38 1.76	0.03 (0.08)	0.10 (0.30)
Currently smoke cigarettes	2,473	0.290 0.454	0.021 (0.021)	0.085 (0.084)

Notes:
ITT = "Intent-to-Treat". TOT = "Treatment-on-Treated". Standard errors in parentheses.
*** indicates p < .01, ** indicates p < .05, * indicates p < .10
[a] Standard deviations of control group outcomes are beneath control means.

The in-depth interviews with treatment group members offer some additional insights into the use of freed-up resources by families who received housing vouchers. Providing for the needs of their children was the number one priority for using freed-up funds. Necessities such as shoes, other clothing and school supplies topped the list. Respondents also spoke of buying certain "luxuries" for their children, such as clothes that were not strictly needed, toys, games, sports activities, or outings to restaurants, movies and theme parks. For these parents, expenditures for their children had both a practical and an emotional component. Parents often felt strongly about sheltering their children from the stigma of being poor and wanted to give them a sense of feeling "normal." Expenditures that might otherwise have been considered luxuries sometimes seemed essential for this reason. The ability to purchase gifts for birthdays and Christmas was also mentioned as an important benefit of having more discretionary income.

A second major use of discretionary funds was for household expenses, an area in which families could and did scrimp when finances were tight. Supplies like dish soap, and personal hygiene supplies such as toilet paper, toothpaste, feminine hygiene products, and diapers, frequently were mentioned. In addition, purchasing appliances, mattresses and other furniture was a high priority for those who did not have them.

In addition to freeing funds for basic needs, some treatment group members said voucher receipt had enabled them to save, reduce debts, or clean up credit problems. However, even with voucher assistance, many interview respondents continued to face substantial barriers to financial stability. Unstable employment income was the primary barrier, but other unstable sources of income, such as child support, also contributed to financial instability. Beyond inadequate and unstable income, respondents seemed to face an endless series of unpredictable expenses. As respondents struggled to get their families into financial equilibrium, every unexpected financial burden threw barriers in the way. Respondents noted that unexpected utility bills, health care costs, and car maintenance and repairs were frequent problems. Many unexpected expenses resulted in short-term and long-term debt. Medical expenses in particular were a common reason for longstanding debt.

5.5 Results for Subgroups

The general pattern of results for the full survey sample is repeated in the results for individual subgroups. However, some of the impacts are more pronounced for selected groups, notably welfare recipients nearing the TANF time limit, family heads who were unemployed at the time of random assignment, and families with children under the age of six.

TANF Recipients within Six Months of Time Limit

The impacts of housing vouchers on TANF recipients who were aware that their assistance would expire within six months of the baseline interview are noteworthy. The impact of the voucher on proportion of these households below the federal poverty threshold (when considering both cash and near-cash income) is large and significant at the 0.10 level for this subgroup. A smaller fraction of treatment group families were below 75 percent of the federal poverty threshold when near-cash income was included in the assessment (41 percent, compared to 49 percent of control families).

As shown in Exhibit 5.6, the treatment-control differences in two key housing security measures are very large and highly significant for this subgroup. For TANF recipients whose assistance would soon expire—and thus were especially vulnerable to material hardships such as homelessness—the vouchers were clearly very important. In the year before the interviews, fewer than 14 percent of treatment group families experienced homelessness. In sharp contrast, 28 percent of the control group reported being homeless, and nearly a third of this homelessness involved time on the streets or in shelters. In addition, more than 85 percent of the treatment group families in this subgroup had independent housing at the time of the survey, 8 percentage points higher than for the control group. The magnitude of the estimated TOT impacts on these outcomes suggests that the vouchers were extremely effective in minimizing the problems of homelessness and doubling-up for the families who were close to their TANF time limit at the time they received the voucher.

Exhibit 5.6
Impacts for Families within Six Months of TANF Time Limit

Subgroup	Impacts on Poverty and Material Hardship Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean[a]	ITT Impact	TOT Impact
Poverty				
Cash income below poverty threshold	203	0.838	0.016	0.078
		0.369	(0.093)	(0.447)
Cash income below 75% of poverty threshold	282	0.771	-0.051	-0.243
		0.422	(0.048)	(0.232)
Cash and near-cash income below poverty threshold.	314	0.690	-0.131 **	-0.630 **
		0.464	(0.058)	(0.276)
Cash and near-cash income below 75% of poverty threshold.	321	0.491	-0.090	-0.432
		0.501	(0.070)	(0.336)
Housing Security				
Homelessness during year prior to survey	320	0.280	-0.142 ***	-0.682 ***
		0.450	(0.039)	(0.186)
Independent housing at time of survey	313	0.777	0.078 ***	0.374 ***
		0.418	(0.031)	(0.147)
Number of rooms at time of survey	328	4.11	-0.08	-0.36
		1.41	(0.17)	(0.80)
Crowding at time of survey	323	0.338	0.040	0.193
		0.474	(0.055)	(0.266)
Quality of housing at time of survey	322	0.685	-0.122 *	-0.586 *
		0.466	(0.066)	(0.315)
Housing problems at time of survey	303	0.110	0.001	0.003
		0.313	(0.021)	(0.102)
Amount spent in rent, including utilities, in month before survey	284	$514	-$47	-$226
		$351	(42)	(201)
Other Measures				
Household Food expenditures in the month before the Survey	310	$113	-$1	-$5
		$73	(11)	(51)
Household Food expenditures per person in the month before the Survey	310	$30	$2	$8
		$23	(3)	(16)
Household food security scale score	330	3.949	-0.958 **	-4.601 **
		3.174	(0.374)	(1.798)
Number of food related harships in the past 30 days	330	2.821	-0.685 **	-3.289 **
		2.268	(0.268)	(1.285)

Subgroup	Impacts on Poverty and Material Hardship Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean[a]	ITT Impact	TOT Impact
Household was food insecure in the past 30 days	325	0.548	-0.213***	-1.023***
		0.499	(0.067)	(0.321)
Household member(s) went without meals at lease one day in last month	300	0.124	0.009	0.043
		0.331	(0.035)	(0.167)
Had health insurance coverage at the time of the survey	324	0.573	-0.099	-0.477
		0.496	(0.065)	(0.314)
Could not afford needed medical care during the year before the survey	307	0.219	-0.029	-0.141
		0.415	(0.046)	(0.220)
Could not afford needed dental care during the year before the survey	327	0.407	-0.119**	-0.573**
		0.493	(0.060)	(0.289)
Had working motor vehicle at time of survey	118	0.818	3.8E-10***	1.8E-09***
		0.389	(6.8E-10)	(3.3E-09)

Notes:
ITT = "Intent-to-Treat". TOT = "Treatment-on-Treated". Standard errors in parentheses.
*** indicates p < .01, ** indicates p < .05, * indicates p < .10
[a] Standard deviations of control group outcomes are beneath control means.
Statistical significance is derived from underlying probit coefficient and robust standard error.
See Appendix B.3 for more details about impact estimation with the probit model.

The impact of the vouchers on food expenditures for this subgroup was not statistically significant. However, the effect on food security was large and significant; the vouchers cut the fraction of families classified as "insecure" from 55 percent to 33 percent. The vouchers also reduced the fraction of families reporting that they could not afford needed dental care.

Families with Unemployed Heads

The impacts of housing vouchers on unemployed family heads are also noteworthy. Consistent with the results for the overall sample, the impacts on cash poverty measures were not significant. However, many of the impacts on housing security and other measures did reach a level of statistical significance. As shown in Exhibit 5.7, the impacts on both categories of homelessness were both large and significant. As for welfare families close to reaching the TANF time limit, families with unemployed heads were at high risk of homelessness without housing assistance. The results show that the vouchers successfully addressed this danger.

In addition, we found for this subgroup very large and statistically significant impacts on independent housing, the number of rooms, and crowding, which are all important housing security measures. The impact on monthly food expenditures was also substantial and statistically significant.

Exhibit 5.7
Impacts for Families with Unemployed Heads

Subgroup	Sample Size	Control Mean[a]	ITT Impact	TOT Impact
	Impacts on Poverty and Material Hardship Fifth Year, All Sites except Los Angeles			
Poverty				
Cash income below poverty threshold	1,305	0.889	0.016	0.057
		0.315	(0.013)	(0.047)
Cash income below 75% of poverty threshold	1,311	0.835	-0.003	-0.010
		0.372	(0.019)	(0.069)
Cash and near-cash income below poverty threshold.	1,324	0.693	-0.048 *	-0.176 *
		0.462	(0.029)	(0.106)
Cash and near-cash income below 75% of poverty threshold.	1,326	0.459	-0.043	-0.154
		0.499	(0.031)	(0.114)
Housing Security				
Homelessness during year prior to survey	1,329	0.270	-0.121 ***	-0.440 ***
		0.444	(0.025)	(0.089)
Independent housing at time of survey	1,312	0.824	0.083 ***	0.302 ***
		0.381	(0.022)	(0.078)
Number of rooms at time of survey	1,336	3.93	0.15 *	0.55 *
		1.41	(0.08)	(0.28)
Crowding at time of survey	1,334	0.425	-0.055 *	-0.198 *
		0.495	(0.031)	(0.114)
Quality of housing at time of survey	1,336	0.596	0.011	0.039
		0.491	(0.031)	(0.113)
Housing problems at time of survey	1,304	0.151	-0.028	-0.103
		0.359	(0.020)	(0.074)
Amount spent in rent, including utilities, in month before survey	1,168	$508	-$64 ***	-$232 ***
		$325	(19)	(70)
Other Measures				
Household Food expenditures in the month before the survey	1,258	$108	$11 **	$40 **
		$71	(5)	(18)
Household food expenditures per person in the month before the survey	1,255	$28	$3 *	$11 *
		$22	(2)	(6)
Household food security scale score	1,349	3.286	-0.183	-0.665
		3.155	(0.184)	(0.668)
Number of food related hardships in the past 30 days	1,349	2.347	-0.131	-0.477
		2.254	(0.131)	(0.477)

Subgroup	Sample Size	Control Mean[a]	ITT Impact	TOT Impact
		Impacts on Poverty and Material Hardship Fifth Year, All Sites except Los Angeles		
Household was food insecure in the past 30 days	1,338	0.435 0.496	-0.010 (0.032)	-0.037 (0.116)
Household member(s) went without meals at lease one day in last month	1,323	0.106 0.309	-0.007 (0.018)	-0.024 (0.065)
Had health insurance coverage at the time of the survey	1,342	0.603 0.490	0.023 (0.032)	0.085 (0.115)
Could not afford needed medical care during the year before the survey	1,330	0.184 0.388	-0.020 (0.021)	-0.072 (0.078)
Could not afford needed dental care during the year before the survey	1,343	0.295 0.456	-0.021 (0.027)	-0.076 (0.100)
Had working motor vehicle at time of survey	495	0.737 0.441	0.015 (0.052)	0.054 (0.189)

Notes:
ITT = "Intent-to-Treat". TOT = "Treatment-on-Treated". Standard errors in parentheses.
*** indicates p < .01, ** indicates p < .05, * indicates p < .10
[a] Standard deviations of control group outcomes are beneath control means.

Families with Young Children

The results for families with children,[125] particularly children under the age of six, are consistently impressive. The vouchers significantly reduced homelessness, increased independent housing, increased the average number of rooms, and boosted spending on food and other items. These program effects, which are shown in Appendix Exhibit F, undoubtedly benefited the children in these households.

Other Subgroups

The impacts of housing vouchers on other subgroups also are presented in Appendix Exhibit F. It is noteworthy that the findings for black and Hispanic families are very encouraging for most outcomes.

5.6 Conclusions

We draw four conclusions from the results presented in this chapter. First, housing vouchers reduced, but did not eliminate poverty. Vouchers had no discernable effect on income poverty, but did reduce poverty when near-cash income, including the value of the housing assistance itself, was taken into account. In this study, the impact on poverty faded over time

[125] Some of the households that received vouchers are former TANF recipients and had no dependent children at the time of random assignment.

as the control group became eligible for vouchers, reducing the net treatment effect. By the time of the survey, however, the impact of the voucher on the proportion of households below 75 percent of poverty was statistically significant.

Second, the vouchers minimized both homelessness and the need to double-up for the families that used them. The impacts of the vouchers for these outcomes are large, statistically significant, and consistent across many subgroups. The findings of the in-depth interviews with treatment group members indicate that vouchers also lessened respondents' anxiety about homelessness and having to move in with others.

Third, the vouchers reduced other material hardships. They significantly increased living space, reduced overcrowding, and, by freeing up money for other family consumption, increased household food expenditures. The overall impacts on other hardships were not significant, but the effects on selected subgroups were significant.

Fourth, the impacts of vouchers appeared to be generally greater for the most disadvantaged segments of the eligible population—those without a job, education and/or work experience, and still relying on welfare to support their families. The impacts are especially noteworthy for some specific subgroups. In particular, the vouchers provided financial protection to families nearing the end of their eligibility for TANF cash assistance. The impacts of the vouchers on this group's homelessness and independent housing were especially dramatic. The vouchers also appear to make a greater difference for young families and families facing particular barriers – notably unemployment – at the time of random assignment.