

Chapter Six

Impacts on Child Well-Being

This chapter examines the effects of housing vouchers on child outcomes, using data from the follow-up survey pertaining to the status and well-being of children who were present in the household at the time of random assignment. The subsidy provided through a housing voucher amounts to a substantial increase in a family's financial resources, often larger than a family's monthly cash assistance benefit.¹²⁶ This increase in resources could be expected to affect many aspects of child well-being and family life. In this chapter we describe the mechanisms through which the receipt of housing assistance might affect child outcomes, the survey data used in this study to explore these effects, and the impact estimates obtained for the full survey sample and for subgroups defined by the child's gender and age.

6.1 Summary of Findings

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 girls and 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. This perhaps contributed to another treatment effect, that girls were less likely to have completed high school. 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. By gender, treatment group boys were significantly less likely—and treatment group girls were significantly more likely—to be in after-school activities on a weekday afternoon. Treatment children under 6 years old at random assignment (under 11 years old at follow-up) were also significantly less likely to be in afternoon school activities. On weekday afternoons, treatment group boys were significantly less likely to be under adult supervision—

¹²⁶ HUD data from April 2003 indicate the average monthly housing assistance payment was \$456 nationally. The assistance is provided as a subsidy to monthly rental payments, and is paid by local public housing agencies to owners of rental properties. The value of the housing assistance is determined locally based on the difference between a payment standard (set within a range of 90 to 110 percent of the HUD-determined fair market rent, which is adjusted for size of the housing unit) and the tenant contribution to the rent, which is limited in most cases to 30 percent of adjusted monthly income. Maximum monthly TANF cash benefits for a parent and two children ranged from \$201 to \$686 in the research sample, depending on state of residence.

and treatment group girls were significantly more likely to be under adult supervision—than their control-group counterparts.

- **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. There were, however, two noteworthy subgroup effects. Treatment boys were significantly less likely to have experienced a problem that led to police-parent contact. In contrast, the voucher was found to have an adverse effect among girls, increasing the likelihood of hanging around with kids who get into trouble.
- **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 boys and 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.

6.2 Hypothesized Effects of Housing Vouchers

In considering the potential effects of vouchers on children, it is useful to think of child development in a resource-based framework.¹²⁷ Following Haveman and Wolfe (1994), we view "resources" as including the purchased resources, time, interpersonal connections, and institutions that parents, schools, and communities may use to promote the development of children. Resources actually spent on promoting child and adolescent development may be considered investments, as they enhance the *future* health, cognitive ability, and productive social behavior of children. The financial and nonfinancial resources from which investments in children are made are derived from multiple contexts, and the intersection of resources from these various levels has implications for child development (Bronfenbrenner 1979).

The Housing Choice Voucher provides financial resources to the family by limiting the household's rent burden (to approximately 30 percent of income). Because the receipt of this subsidy need not require a change in residential location—i.e., a family may use the voucher to "lease in place" if the existing residential unit qualifies and the landlord agrees—the effects of the treatment on child well-being are not specifically tied to mobility. This experiment is thus broader in its focus than the MTO demonstration, the other major randomized study that has looked at the effects of housing assistance on children.¹²⁸

This chapter examines the effects of the increased financial resources to the family—in the form of a housing voucher—on four domains of child wellbeing:

¹²⁷ These opening points draw from Jeffrey R. Kling, et al., "Children and Neighborhoods: A Randomized Study in Mobility," a grant proposal submitted to the National Institute of Child Health and Human Development in conjunction with the Moving to Opportunity Demonstration, June 2000.

¹²⁸ A second key difference between the intervention tested in this demonstration and in MTO was that the MTO control families received housing assistance in the form of public housing, whereas here the nontreated families did not. Thus, the MTO treatment did *not* increase the total resources available to the family.

- School performance and educational progress;
- Behavior and time use;
- Delinquency and risky behavior; and
- Parental involvement and family life.

There are several possible mechanisms through which the housing voucher may affect these outcomes either favorably or unfavorably. Each of these pathways pertains to the expanded economic opportunities provided by the voucher. We focus below on the potential favorable effects on children.

- ***Additional available discretionary income***—Whether (and by how much) the family derives additional discretionary income from use of a voucher will depend on the specifics of the former and current housing and employment arrangements. The simplest case is a family that leases in place, having previously devoted more than 30 percent of its income to rent and where there is no reduction in the labor supply of family members. The added income in the family’s monthly budget may be used for expenditures that directly or indirectly benefit the children (e.g., better child care, health care, or better nutritional intake).
- ***Additional available parental time***—As discussed in Chapter 3, conventional microeconomic theory suggests that the working members of families using a voucher may respond by reducing their labor supply. Both the income effect and the substitution effect of the voucher can be expected to cause working parents to reduce their short-run work hours and thus potentially increase the time devoted to nonwork activities that may benefit the children. This could translate into additional hours of parental supervision in the home, reducing the needs for childcare or limiting the opportunities for delinquent or risky adolescent behavior.
- ***Improvements in quality of the family’s housing unit***—For those who use the voucher to improve their housing arrangements, even if they remain in the same immediate neighborhood, there may be benefits to the children through a more comfortable and less stressful living environment. More space, more privacy, and reduced health risks may lead to improved educational outcomes and improved health outcomes (physical and mental health). More time spent in the home by the children may serve to reduce the unsupervised out-of-home time during which accidents, injuries, or risky behavior may occur.
- ***Improvements in the quality of the family’s neighborhood location***—For those using the voucher to move to a more desirable neighborhood, even if to an otherwise comparable housing unit, there are many hypothesized effects on children. These effects could arise through better schools, better access to child care and health care, more positive peer influences or adult role models, greater community resources (for economic, social, and recreational opportunities), stronger neighborhood “safety nets” (limiting delinquency, gang involvement, or victimization), and greater relative deprivation (and potentially more resentment and deviant behavior).

The latter two mechanisms are related to residential mobility. Possibly offsetting the favorable effects on children of improvements to the family’s housing unit or neighborhood

are the disruptive changes to childcare or schooling arrangements that may occur with a move. Also possibly offsetting these hypothesized effects is the reduced incidence of multigenerational households, as described in Chapter 3. When voucher participants opt to establish independent living arrangements apart from their extended family members, children may be affected adversely through reductions in household income, parental time, or other adult supervision.

All four of these causal linkages to child well-being involve behavioral responses that operate with some time lag. This demonstration allows a rigorous test of whether such effects occur within a follow-up interval of four to five years.

6.3 Data Sources for Impact Estimates

The design of the WtWV follow-up survey included selection of a survey subsample of children for which a series of questions, comprising the Parent-on-Child/Youth Module, were administered to the adult respondent. This respondent was the primary adult member of the household, typically the mother of the children. The data collection was focused on children who were members of the household at baseline and who at that time were younger than 15 years of age. To limit the burden on the adult respondent, the questions in the Parent-on-Child/Youth Module were asked of no more than two age-eligible children.

Considerations in drawing the subsample included the following:

- First, some families did not have dependent children in the relevant age range (0 to 15 years) at baseline. (For example, some families had one dependent child who was 16 to 18 years old at baseline.) Although we included such families in the follow-up survey sample, the Parent-on-Child/Youth module was administered only to families with children in the relevant age range.
- Second, some families had more than two children in the relevant age range in the household at baseline. For these families, random sampling was applied to select the two children for the survey questions.¹²⁹
- Third, some children who were present at baseline were not living in the household at the time of the follow-up survey. To avoid introducing attrition bias into our data collection, we asked a key subset of the child outcome questions about children who had left the household as well as those who were still in the household. We did not replace a child absent from the household at the time of the follow-up survey with a child who was present, even in those families with more than two children.

¹²⁹ The selection of children aged 0 to 15 from among baseline household members was done through simple random sampling. Up to two children were selected from each household in the study, without further stratification on age. In this way, our analyses combined children of different ages without having to weight the data specifically by each group. Where two children were drawn from the same household, appropriate sibling adjustments in the weighting of observations were made for the analysis. In addition, where two children were selected from a household with more than two age-eligible members at baseline, the observations were correspondingly weighted to represent the non-sampled children.

A total of 16,731 children age-eligible were present at baseline in the 5,000 households selected for the follow-up survey. As indicated in the first column of Exhibit 6.1, 40 percent of these children were under age 5 at baseline, 34 percent were between the ages of 5 and 9, 19 percent were 10 to 13 years of age, and the remaining 7 percent were 14 or 15 years old. Two-thirds were attending school, and they were evenly split by gender. In terms of race and ethnicity, more than one-half (51 percent) were non-Hispanic black, 25 percent were Hispanic, and 12 percent were non-Hispanic white.

Ultimately, a sample of 11,925 children was drawn from the eligible child population at baseline. These sampled “focal children” were designated as Child1 or Child2 in administering the follow-up survey. Their characteristics (very similar to those of the children eligible for sampling) are shown in the second column of Exhibit 6.1.

The final analysis sample of focal children contained 4,094 children in 2,481 households.¹³⁰ As shown in the third column of Exhibit G.1, the children for whom an adult completed the follow-up survey questions mirror fairly well the pool from which they were drawn (middle column of the exhibit). There is a slight over-representation of children from Atlanta and Augusta (with lower proportions from the other sites) and a higher proportion of non-Hispanic black children than in the eligible pool.¹³¹ In other respects, the characteristics of children in the follow-up data closely resemble those of the sampled children.

For the 4,094 children in the analysis sample, Exhibit 6.2 compares the characteristics of the children assigned to the treatment group with those assigned to the control group. (Children are given the same assignment as their baseline household.) There are significant differences between treatment and control cases in their distribution by site and by race-ethnicity. As with the impact estimates presented throughout this report, these variables were among the covariates included in all equations that estimated the treatment effects on child outcomes.

In Exhibit 6.2, some child characteristics are shown as of the follow-up period. The children have grown older, with 5- to 9-year-olds the largest group (over 40 percent) and 10- to 13-year olds making up another quarter of the sample. Almost 90 percent of the children are still living at follow-up (as at baseline) with the adult respondent.

¹³⁰ The fact that the number of survey-completed children (4,094 among 2,481 families) is much lower than the number of sampled children (11,925 among 5,000 families) does not indicate a low survey response rate. As described in Appendix A, a subsampling approach was employed to concentrate second-stage survey resources on a random subset of families not interviewed through first-stage efforts. This enabled a high effective response rate (76.7 percent) even though the number of interviewed families (2,481) was less than one-half of the initial survey sample (5,000).

¹³¹ See Appendix C for a detailed analysis of survey nonresponse with respect to the characteristics of the primary adult member and his/her household.

Exhibit 6.1
Baseline Characteristics of Age-Eligible Children

Baseline Characteristic	All Age-Eligible Children ^a (n=16,731)	Sampled Children (n=11,925)	Survey- Completed Children (n=4,094)
Survey site			
Atlanta	13.4%	14.2%	15.2%
Augusta	8.7	9.6	10.7
Fresno	38.8	36.0	33.5
Houston	26.9	26.6	28.9
Spokane	12.2	13.7	11.7
Race/ethnicity			
White, non-Hispanic	11.8%	13.2%	11.7%
Black, non-Hispanic	50.7	52.2	57.1
Hispanic	24.9	24.7	24.0
Other	9.6	6.8	4.2
Missing	3.0	3.1	3.1
Gender			
Male	49.7%	49.5%	50.0%
Female	49.5	49.8	49.3
Missing	0.8	0.7	0.8
Age in years^b			
Under 5	40.3%	43.6%	44.3%
5-9	33.9	32.1	31.7
10-13	18.8	17.4	16.8
14-15	7.0	6.9	7.3
Average age	6.3 years	6.0 years	6.0 years
Attending school?			
Yes	61.6%	58.9%	58.9%
No	34.3	36.9	37.1
Missing	4.1	4.3	4.0

Notes:

^a Children present in the baseline households were eligible for sampling as focal children in the follow-up survey if they had a specified birth date and were ages 0 to 15 at baseline.

^b Age as of the date of random assignment. Cases with missing dates of birth were not eligible for sampling.

Exhibit 6.2
Characteristics of Survey-Completed Children

Characteristic	Treatment Group (n=2,016)	Control Group (n=2,078)	Total (n=4,094)
Site**			
Atlanta	13.7%	16.7	15.2%
Augusta	10.6	10.8	10.7
Fresno	33.9	33.0	33.5
Houston	27.9	29.9	28.9
Spokane	13.8	9.6	11.7
Race/ethnicity**			
White, non-Hispanic	13.3%	10.1%	11.7%
Black, non-Hispanic	54.5	59.6	57.1
Hispanic	24.2	23.7	23.9
Other	4.5	3.9	4.2
Missing	3.5	2.7	3.1
Gender			
Male	50.2%	49.8%	50.0%
Female	49.2	49.4	49.4
Missing	0.7	0.8	0.8
Age in years at follow-up^a			
Under 5	5.0%	5.3%	5.1%
5-9	42.5	43.1	42.8
10-13	26.5	24.5	25.5
14-17	17.7	17.3	17.5
18-20	8.4	9.9	9.2
Average age	10.5 years	10.5 years	10.5 years
Relationship to adult respondent			
Biological, step-, or adopted child	94.5%	93.3%	93.9%
Brother or sister	0.7	1.0	0.8
Grandchild	1.7	2.0	1.9
Other relationship	0.6	0.7	0.6
Missing	2.4	3.1	2.8
Living with adult respondent at follow-up			
Yes	89.1%	89.5%	89.3%
No	10.6	10.2	10.4
Missing	0.3	0.3	0.3

Notes:

^a Age as of December 15, 2004 (approximately at the time of the follow-up survey data collection). Cases with missing dates of birth were not eligible for sampling.

** p < .01 in chi-square test.

6.4 Impact Estimates

The quantitative analysis tests whether the receipt of the housing voucher had impacts in four domains of child well-being:

- School performance and educational progress;
- Behavior and time use;

- Delinquency and risky behavior; and
- Parental involvement and family life.

For each of these domains, we examined a number of different outcome measures to ascertain any patterns of voucher impact. The tests for voucher effects follow the framework and methods used in the previous three chapters. Subgroup effects were estimated for subsamples defined by characteristics of the primary adult member (as in the previous chapters) and also as defined by the specific gender and age characteristics of the child. The child-specific subgroup estimates are shown in Appendix G and are noted in the discussion below.

Child School Performance and Educational Progress

Exhibit 6.3 shows the main effects (across all the children in the follow-up sample) for a series of measures of school performance and educational progress. By and large, there was little evidence of voucher impacts. There were no significant differences between the treatment and control groups on any of the measures related to special classes or other school services for learning, behavioral, or emotional problems.¹³² Nor did being in a voucher household affect the child’s being out of school due to any problem, highest grade completed or whether he/she had finished high school or enrolled in college. Given the age distribution of the child sample, it is not surprising that just 4 percent of the sample members had finished high school (or gotten a GED), and only 2 percent were enrolled in college.

There was one measure that did show a significant sample-wide effect (again, see Exhibit 6.3).

Exhibit 6.3
Impacts on School Performance and Educational Progress
(Survey Data – Parent-on-Child/Youth Module)

	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Child is not in school due to any problem	3,851	0.023 0.151	-0.001 (0.001)	-0.003 (0.002)
Child's highest grade completed	3,754	4.57 3.89	-0.02 (0.04)	-0.09 (0.15)
Child has completed HS or GED	3,929	0.045 0.207	0.000 (0.000)	0.000 (0.000)
Child is enrolled in college	3,856	0.024 0.152	0.000 (0.000)	0.000 (0.000)

¹³² One might regard an increased use of special classes or other school services, if such an effect had been found, as a favorable impact. Some of the parents participating in in-depth interviews indicated that one of their reasons for using their voucher to move was to allow their children to attend schools where such classes and services would be more readily available.

	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Child has ever repeated a grade	3,799	0.179 0.383	0.030** (0.015)	0.116** (0.057)
Child has ever been suspended or expelled	3,796	0.150 0.357	0.008 (0.012)	0.030 (0.047)
Parent was called into school to discuss problems with child in past 2 years	3,700	0.240 0.427	-0.006 (0.016)	-0.023 (0.063)
Child has attended a special gifted class or done advanced work in last 2 years	3,752	0.122 0.327	-0.001 (0.012)	-0.005 (0.046)
Child has gone to a special class or gotten special help due to learning problems in last 2 years	3,771	0.205 0.404	-0.002 (0.016)	-0.009 (0.060)
Child has gone to a special class or gotten special help due to behavioral/emotional problems in last 2 years	3,773	0.112 0.316	0.013 (0.011)	0.052 (0.041)
Child has received special services very frequently for these problems in school in last 2 years	3,959	0.183 0.386	-0.002 (0.015)	-0.006 (0.057)

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.

Specifically, being in a voucher family increased the proportion of sample children who had ever repeated a grade, from 17.9 percent among the control group to 20.9 percent among the treatment group children. This effect may have resulted in part from voucher families tending to move out of their baseline tracts into neighborhoods of better quality, with parents opting (or being required) to have their children repeat a grade. Note that none of the subgroups defined on the basis of children's characteristics showed this effect.

In contrast, favorable effects related to schooling were found for several subgroups on another measure, whether the child was out of school for any problem (including health, financial, and disciplinary problems as well as incarceration). The voucher significantly reduced the out-of-school incidence for girls and for the youngest group of children (those under 6 at baseline), as shown in Appendix G, Exhibit G.1.

Based on the intensive interviews with a sample of treatment group members, school quality was a key consideration among voucher users when making their decisions about whether and where to move. A majority of respondents said that voucher assistance enabled them to move to an area with better schools. The definition of a "good" school or school system varied among respondents, but respondents generally identified structure and discipline in the classroom, young teachers, teachers who encouraged students to go on to college, presence of after school programs to help with homework, support and services for special needs children, and regular

communication with parents as important factors in their satisfaction with children's schools. Finding the right strategies for dealing with children who had problems at school was challenging. Some parents expressed concern because their children had been "held back" while others were disappointed because the school had "passed on" their children whether or not they had met the requirements for the grade level. Despite the fact that many in-depth interview respondents said that school quality figured highly in their decisions about moving, and that moving, when it offered the possibility of attending better schools, was for the most part regarded as advantageous, some respondents noted that moving to new schools could be disruptive and pose disadvantages to their children's school performance.

Some of those interviewed said that voucher assistance improved the educational experience for children in another way: having the voucher allowed them to use discretionary income to purchase school supplies and clothes that their children needed to feel confident and obtain acceptance at school.

Some respondents to the in-depth interviews were able to identify improvements in specific school-related outcomes such as higher grades and tied the improvements to the voucher. Others reported more general positive feelings about the impact of voucher assistance on children's education and about expected future improvement in school performance.

Child Behavior and Time Use

Exhibit 6.4 displays the measures developed from the follow-up survey in the areas of child behavior and time use. The first of these measures is an abbreviated version of the behavior problems index (BPI), that was developed by Zill and Peterson based on the work of Achenbach (see Moore et al. 1999). The index consists of the fraction of these 11 problem behaviors reported by the adult respondent to be "often true" or "sometimes true" of the child:

- Has difficulty concentrating, or cannot pay attention for long;
- Cheats or tells lies;
- Bullies or is cruel or mean to others;
- Is disobedient at home;
- Has trouble getting along with other children;
- Is restless or overly active or cannot sit still;
- Has a very strong temper and loses it easily;
- Is withdrawn and does not get involved with others;
- Hangs around with kids who get into trouble;
- Is disobedient at school; or
- Has trouble getting along with teachers.

Exhibit 6.4
Impacts on Child Behavior and Time Use
(Survey Data – Parent-on-Child/Youth Module)

	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Child Behavior Problems Index [Parental Report]	3,874	0.271 0.264	-0.002 (0.011)	-0.009 (0.042)
Child in childcare at 3:45 PM	3,632	0.050 0.218	-0.001 (0.002)	-0.005 (0.008)
Child in activities at school at 3:45 PM	3,757	0.160 0.366	-0.020 (0.014)	-0.076 (0.054)
Child in other organized activities at 3:45 PM	3,675	0.055 0.228	0.008 (0.007)	0.032 (0.027)
Child could be seen or heard by an adult at 3:45 PM	3,722	0.945 0.228	-0.002 (0.006)	-0.007 (0.022)
Child currently working for pay	3,896	0.039 0.193	0.000 (0.000)	0.000 (0.000)
Child's number of close friends	3,290	3.37 2.52	0.10 (0.11)	0.39 (0.41)
Child involved in sports during current school year	3,800	0.269 0.444	-0.007 (0.019)	-0.029 (0.073)
Child involved in club or extracurricular activity during current school year	3,807	0.268 0.443	0.004 (0.019)	0.016 (0.074)

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 control group mean for the BPI (with a value of 0.27) indicates that, on average, between 2 and 3 of these behaviors were reported to occur often or sometimes for sample children. There was no significant difference between the treatment and control groups on this measure.¹³³

Other measures shown in Exhibit 6.4 concern the child's whereabouts and activities at 3:45 PM on a weekday afternoon, the child's number of close friends (as reported by the adult respondent), whether the child/youth was employed, and other indicators of involvement in organized activities. Although there were no significant voucher effects for the child sample

¹³³ The mean for the control group in the MTO demonstration was very similar: 0.26. In that case, the index was defined only for youth ages 12-19.

as a whole, there were some interesting effects on subgroups (as shown in Exhibit G.2). Treatment group boys were significantly less likely—and treatment group girls were significantly more likely—to be in activities at school on a weekday afternoon; children under 6 at random assignment (thus under 11 years old at follow-up) were also significantly less likely to be in afternoon school activities. A similar pattern appears for whether the child could be seen or heard by an adult at that hour: treatment group boys were significantly less likely to be under adult supervision—and treatment group girls were significantly more likely to be under adult supervision—than their counterparts in the control group. These were the only behavior and time use measures for which there were significant child subgroup effects.

Approximately one-quarter of all respondents in the in-depth interviews reported that the voucher had an effect on their children’s emotional well-being and behavior. Although respondents did not commonly report making housing decisions based on the *physical* health needs of their children, many respondents reported making moves they thought would be better for their children’s *emotional* health. Getting children away from neighborhoods that were violent, had drug activity, or lacked good role models was the primary goal of many moves. After moving to address these concerns, respondents often reported that their children were “more low-key,” had “more freedom” to play outside, and in general had a better life.

Those interviewed in-depth reported further that the voucher’s role in reducing stress for the head of household also benefited children’s emotional well-being. Even those voucher holders who tried to protect children from financial concerns generally acknowledged that their own stress about financial challenges often affected children. For some families the voucher enabled a parent to work less and therefore spend more time with children; for others worrying less about finding and keeping adequate housing enabled parents to focus better on children’s needs.

Delinquency and Risky Behavior

The next group of child outcome measures pertains to delinquency and risky behavior. As shown in Exhibit 6.5, there are four measures for older children (youth ages 12 and over) and two measures across the full child sample. Here, too, there were no significant effects of the household’s voucher offer for the sample as a whole, but there were two interesting subgroup effects (shown in Appendix G, Exhibit G.3). Across the full sample, the proportion of youth about whom the police had contacted parents was 18 percent; for boys, the proportion was 24 percent. But boys in treatment group households were 7 percentage points less likely to be in this situation.

The other outcome in this group for which effects differed by child subgroup was the child’s hanging out with kids who get into trouble. The adult reported this for about 14 percent of all the children in the sample. As shown in Exhibit G.3, the incidence was about 19 percent for boys and 9 percent for girls in the control group. For girls in the treatment group, however, the incidence was higher by 3 percentage points (about 12 percent overall).

Parental Involvement and Family Life

The final group of outcome measures for children in the voucher evaluation concerns family life. Did the offer of a housing voucher affect the family life of the sample children? Did household composition change, or habits of eating meals together, or parental involvement in school-related or outside activities? The measures displayed in Exhibit 6.6 provide some answers to these questions.

Exhibit 6.5
Impacts on Child Delinquency and Risky Behavior
(Survey Data – Parent-on-Child/Youth Module)

	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Child had problems involving police contacting parent since random assignment^b	1,431	0.180 0.385	-0.018 (0.015)	-0.071 (0.057)
Child has been arrested since random assignment^b	1,430	0.113 0.317	-0.004 (0.006)	-0.016 (0.023)
Child's number of arrests since random assignment^b	1,456	0.164 0.521	-0.023 (0.033)	-0.090 (0.128)
Child has been convicted of a crime since random assignment^b	1,328	0.060 0.237	-0.002 (0.002)	-0.007 (0.009)
Child is incarcerated, in detention facility, in boot camp or similar institution	1,623	0.002 0.047	0.000 (0.000)	0.000 (0.000)
Child hangs around with kids who get into trouble	3,841	0.144 0.351	0.005 (0.013)	0.021 (0.049)

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.

^b Outcome based on follow-up survey questions that were asked only about youth ages 12 and over.

There are three distinct groups of measures in the exhibit. The first group includes parental knowledge of the child's friends and teacher, child attendance at religious services, and meals eaten together per week. Some 86 percent of the parental reports indicated the adult knew all or most of the child's friends and 68 percent knew the child's teacher very well or well. Over a third of the children attended religious services at least weekly. The families were reported to eat together an average of 5.6 times per week.¹³⁴ For these measures, no significant differences were found between the treatment and control group children, although there was a subgroup effect—increasing the number of meals eaten together—for children between 6 and 9 at baseline (see Appendix G, Exhibit G.4).

The next outcome measure shown on Exhibit 6.6 relates to the composition of the household in which the child was living at the time of the follow-up survey. About 85 percent of the control-group children were living with the adult survey respondent who was their parent (the remainder had a different relationship to the respondent or were not living in the same household). No significant treatment effect was found on this variable.

¹³⁴ The control group means for the variables in Exhibit 6.6 are all somewhat higher but generally comparable to those found for the same questions asked in conjunction with the interim impacts evaluation for the Moving to Opportunity (MTO) Demonstration. See Orr (2003), Appendix E.

The last group of outcome measures shown on Exhibit 6.6 concerns parental involvement in the child's activities, both in and out of school. Across the child sample, parents reported fairly high levels of involvement: 83 percent had attended a meeting at the child's school in the past year, 69 percent had attended a school event, and 38 percent had volunteered at the school. There were no differences between treatment group and control group members on these measures for the child sample as a whole. In contrast, only 20 percent had worked with a youth group or similar activity outside school during the past year, and on this measure being in the treatment group had a significant impact of reducing the likelihood of such parental involvement, by 3 percentage points. Among subgroups, this effect was significantly negative for boys but not for girls.

There were some additional subgroup effects on parental involvement, but they appeared to be scattered and mixed (see again Exhibit G.4). In some cases, the treatment impact was to reduce parental involvement (attending school meetings or events for the youngest group of children, volunteering with a youth group for boys and for the middle age group in the sample). But in other cases parental involvement increased (attending school meetings for children 6-9 at baseline).

The in-depth interviews suggested that the impact of voucher assistance on parental involvement with children went in both directions. Some parents felt they were able to supervise their children less intensively when they moved to better neighborhoods; while others felt they had more time to become involved after the voucher enabled them to work fewer hours. Respondents in the later category reported that they took time off from work to address their children's health, mental health, or behavioral issues.

6.5 Concluding Assessment

This chapter provides estimates of the effects of vouchers on a wide array of outcomes relating to the well-being of children in low-income families. The impact estimates shown in Exhibits 6.3 through 6.6 encompass fully 36 separate outcome measures. With effects also estimated at the subgroup level for two gender subcategories and three age subcategories, the total number of impact estimates approaches 200.¹³⁵

Given this volume of statistical tests, the number of treatment effects estimated as significantly nonzero (at the 0.10 significance level or better) is arguably no larger than one would expect by chance alone. Furthermore, with the significant estimates divided approximately equally between favorable and unfavorable effects on child well-being, the data offer no evidence to support any particular pattern of effects on child outcomes.

¹³⁵ As noted earlier in the chapter, subgroup effects were also estimated for categories defined according to the baseline characteristics of the adult respondent (the same categories used in the preceding chapters). These estimates showed a similarly disparate pattern and were not included in this report, but can be made available upon request.

Exhibit 6.6
Impacts on Parental Involvement and Family Life
(Survey Data – Parent-on-Child/Youth Module)

	<i>Fifth Year, All Sites except Los Angeles</i>			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Parent knows all or most of child's friends	3,791	0.856 0.351	0.001 (0.016)	0.003 (0.060)
Parent knows child's teacher very well or well	3,649	0.684 0.465	-0.005 (0.023)	-0.018 (0.089)
Child attends religious services	3,846	0.621 0.485	0.009 (0.024)	0.035 (0.095)
Child attends religious services at least weekly	3,821	0.356 0.479	0.007 (0.023)	0.027 (0.091)
Number of days per week that family eats together	3,901	5.62 2.06	0.06 (0.11)	0.25 (0.41)
Child is currently living with adult respondent who is the child's parent^b	3,980	0.851 0.356	0.010 (0.013)	0.039 (0.050)
In past year, parent attended meeting at child's school	3,880	0.825 0.380	-0.017 (0.020)	-0.066 (0.076)
In past year, parent attended event at child's school	3,913	0.689 0.463	-0.011 (0.025)	-0.041 (0.095)
In past year, parent volunteered at child's school	3,915	0.376 0.485	-0.006 (0.026)	-0.024 (0.101)
In past year, parent worked with youth group or other activity outside of school	3,889	0.204 0.403	-0.032 * (0.020)	-0.126 * (0.077)

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.

^b May be a biological, adoptive, foster, or step parent.

Several explanations can be offered for this seeming lack of evidence regarding child effects, relating to the mechanisms (described in Section 6.2) through which these effects would be expected to occur. To recall, these are: additional available discretionary income and parental time, and improvements to the quality of the family's housing unit and neighborhood location.

- Additional available discretionary income would have become available primarily to those voucher users who leased in place.¹³⁶ As noted in Chapter 3, however, among

¹³⁶ Discretionary income would also have become available to some who moved, depending on the market rent of their new unit and their prior rent payment.

treatment group members who leased up in the five survey sites, “movers” outnumbered “stayers” by more than two to one.

- Additional parental time would have become available primarily to the extent that adults reduced their labor supply. As indicated in Chapter 4, however, an initial reduction in labor supply was later found to have diminished to insignificance as one approached the fifth-year follow-up period of the survey.
- Improvements in the quality of the family’s housing unit and neighborhood location were indeed found to have occurred (as described in Chapters 3 and 5). Any such favorable developments for the family, however, would have occurred through residential moves that may have been highly disruptive to the childcare, schooling arrangements, and social networks of children (as might occur, for instance, with the reduction in multigenerational households. Moreover, the beneficial effects for children of improved housing and neighborhood quality may take time to become evident in their school performance and social behavior.

Thus, even if vouchers enhanced the family’s financial situation and living arrangements in ways that could ultimately be expected to influence child outcomes favorably, there was no greater stability in the lives of treatment families than for control families—at least not within the observed five-year follow-up period. The lack of systematic effects among children may reflect the fact that for them, as for their parents, vouchers brought on a series of transitions during key formative periods of their lives.

Chapter Seven

Conclusions

This chapter provides further interpretation of the results presented in Chapters 3 through 6. The primary aim of this chapter is to discuss how the key findings from this study may inform policy debates regarding the role of housing assistance in the lives of low-income families.

How did families use their vouchers in deciding where and with whom to live?

The study finds that Housing Choice Voucher recipients used their vouchers to move out of the Census tracts in which they lived at baseline to neighborhoods of better quality than they would have lived in otherwise. 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.

On the other hand, 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 means that, although treatment-control differences in neighborhood characteristics are statistically significant, they are not large, and that voucher users are able to make only modest improvements in the neighborhoods where they live. 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. Families interviewed in depth for the study reported that, for the most part, they had been able to use the voucher to locate in neighborhoods that they considered safer than where they previously lived. Nevertheless, despite incremental improvements in their neighborhood locations, they still reported concerns and dissatisfaction, especially about the safety of their neighborhoods, and the desire to make future moves to seek out even safer and better quality neighborhoods. Some families lost their voucher assistance during the attempt to make a subsequent move.

These findings suggest that, in order for voucher assistance to markedly increase the quality of the neighborhoods where low-income families live, it would be necessary to build mobility counseling and housing search assistance into the administration of the voucher program, along with measures such as assistance with security deposits that help voucher holders compete for housing in good neighborhoods.

After voucher holders used their housing assistance to move out of the Census tract in which they lived at baseline, they then moved fewer times than they would have without the voucher. This pattern of increased residential stability is reflected in the responses of many of the families interviewed in depth, who said that the voucher had provided stability in their housing and had diminished the disruption and anxiety that typically accompany frequent moves.

Treatment group members also tended to use their vouchers to pursue independent living arrangements for themselves and their children and to establish smaller households than they would have without the voucher. Most prominent was the decreased likelihood of the primary sample member and her children living in a multigenerational household. Vouchers

provide the financial means by which a parent may exit from an overcrowded and stressful living arrangement with extended family members.¹³⁷ At the same time, the absence of any effect on marriage or cohabitation suggests that, while vouchers do not provide a positive economic incentive to find a spouse or partner, neither do they provide a negative incentive.

There has been little prior research on the effects of housing assistance on family composition, and none that was experimental. In their analysis of the determinants of the size of low-income households using the Social Indicators Survey, Ellen and O’Flaherty (2002) found that households receiving housing assistance had fewer adults. Turner (2003) found that unmarried couples who have a baby are less likely to cohabit if the mother lives in government-assisted housing. And Freeman (2005) found that single parents receiving housing vouchers were less likely to have other adults in the household than were unassisted single parents. All of these findings point toward an “independence” effect of voucher receipt, but all are subject to some degree to the risk of selection bias or reverse causality. Describing these studies (including his own), Freeman says:

“These exploratory studies clearly illustrate a relationship between housing assistance and household composition. The next steps should be to determine whether this relationship is causal or not and in what direction(s) this causality may run. This research should be pursued using experimental data or panel data that follow people over time...” (p. 66)

The present study provides such evidence of causality, but only for a reduction in intergenerational households through the use of voucher assistance.

Do vouchers reduce homelessness, poverty, and other measures of material hardship?

The study finds that voucher assistance is an important tool for reducing both homelessness and doubling-up. These voucher effects are large, statistically significant, and consistent across many subgroups. The in-depth interviews with treatment group members indicate that vouchers also lessened respondents’ anxiety about homelessness and having to move in with others. Those interviewed also expressed a strong preference for establishing independent households for themselves and their children away from elder relatives, both as a way of attaining more physical space and for promoting personal independence.

The vouchers also reduced other material hardships. They significantly reduced crowding, increased living space, and—by freeing up money for other family consumption—led to increased expenditures on food. Families who participated in the in-depth interviews also discussed how the increased discretionary income from paying less rent was important for creating a sense of normalcy for children in the families. The additional resources made it possible for the family to purchase necessities like school supplies and clothing, as well as occasional dinners out. Many respondents cited the importance of these discretionary expenditures for helping their children to feel like their peers.

¹³⁷ These parents are not teenagers. The average age at baseline was 31, and less than 1 percent (.3 percent) of the study sample was under age 18 at random assignment. More than 60 percent of the research sample was between the ages of 25 and 44 at baseline.

Vouchers appear to provide 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 barriers – notably unemployment – at the time of random assignment.

How does using a voucher affect children?

This study also estimated the effects of vouchers on a wide array of outcomes relating to the well-being of children in low-income families. Given the large volume of outcomes that were examined across a large number of subgroups, the data offer no evidence to support any systematic pattern of effects on child outcomes.

Several explanations can be offered for this seeming lack of evidence regarding child effects. The mechanisms through which child effects were posited to occur are additional available discretionary income and parental time and improvements to the quality of the family's housing unit and neighborhood location.

Where vouchers enhanced the family's financial situation and living arrangements in ways that could ultimately be expected to influence child outcomes favorably, these enhancements were often small in magnitude. The disruptions associated with the treatment group families' moves to new housing or new neighborhoods may have offset these potential positive effects during the early part of the observed five-year follow-up period. Beyond those initial disruptions, the benefits of the voucher alone, without other sources of increased income and support for parents, may not be great enough to overcome the disadvantages of many of the voucher using families whose financial, health, and other personal situations are often precarious.

What effects do vouchers have on employment rates and earnings amounts, receipt of means-tested public assistance benefits, and education and training?

At the outset of the WtWV program, housing vouchers were hypothesized to affect employment outcomes through a number of different channels. These channels included giving recipients the ability to move closer to job opportunities and to live in safer neighborhoods that are more supportive of work, as well as creating some economic disincentives to work and causing temporary disruptions in recipients' lives and social networks. The findings of this study show that having and using a voucher reduced employment rates and earnings amounts in the first year or two after random assignment. This small negative impact of vouchers disappeared over time, and vouchers had no significant impact on total earnings received during the 3.5 years of follow-up. Thus, despite the fact that treatment group participants came to reside in neighborhoods with slightly lower rates of poverty and other qualities thought conducive to employment, this did not translate into improved employment for treatment group members. The in-depth interviews suggested that employment opportunities are not a high priority consideration when voucher holders consider moving.

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 reductions in employment among voucher users. Although

there was some evidence from in-depth interviews with voucher users that they took advantage of this opportunity to upgrade their skills, the impact analysis shows no significant treatment-control differences 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 significantly the proportion of households consisting of children living with a single parent at the time of the follow-up survey (see Chapter 3). This effect occurred because the voucher made it possible for single parents to live on their own rather than in multigenerational households. While a parent's decision to leave a larger household may not affect her eligibility for a TANF benefit or its amount, the loss of the financial advantages of living in a larger household may have meant parents were more likely to apply for the benefits available to them. For food stamps, establishing an independent household would almost always lead to a larger benefit.

Potential explanations for the short-term negative impacts on earnings found for the first year or two after random assignment but not later are 1) the work disincentives created by the unearned income and marginal tax rate embodied in the voucher, and 2) the disruptions associated with searching for and moving to new housing. We would expect these mechanisms to have different time patterns. The income and tax rate effects of the voucher should be evident as long as the voucher is in use. 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. Although further follow-up would enable a more definitive conclusion,¹³⁸ 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 3½-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, say, to be near better schools or to get away from drugs and gangs) simply 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, presumably was actively engaged in job search at the time they applied for the voucher and believed that the voucher would aid them in that search.

¹³⁸ We have earnings data for the entire sample only through the first half of the fourth year. For the 93 percent of the 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.

However, the voucher, instead of assisting their job search, hindered it in the short term—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.

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. However, the earnings loss associated with use of the voucher appears to be transitory. After about three years, voucher users' earnings are about equal to what they would have been in the absence of the voucher. So while Housing Choice Vouchers do not appear to promote higher near-term employment for welfare families, neither do they have any adverse effect on the self-sufficiency of low-income families.

Concluding thoughts on the value of this demonstration for policy development

The value of this research as a basis for policy decision-making lies in several key aspects of its design. Most important was the use of random assignment for constructing comparable treatment and control groups. Coupled with this was the fact that control group members received no housing assistance (other than crossovers, as addressed in the TOT adjustment). This, in conjunction with the large sample size enabling analysis of subgroup effects, has provided a very strong foundation for estimating the effects of vouchers.

Another fundamental aspect of the demonstration allows this research to provide valuable insights on the decision-making of low-income households: the housing choices available to voucher users were to a very high degree unconstrained. 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 restricted by the characteristics of the new neighborhood. Granted, the program was constrained in its deadlines for voucher lease-up, but the program otherwise generally allowed the participants to freely exercise housing choice.

Moreover, the mobility counseling and employment services provided to voucher users were minimal. As described in Chapter One, this was not intended, as WtW sites were to have coupled the voucher with employment- and housing- related services. The services actually provided were typically no more than those offered under the regular Housing Choice Voucher program. This was a fortuitous circumstance, as the findings have become generalizable to the regular HCV program.

The importance of these issues to the interpretation findings 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.

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Appendix A

Data Sources and Dataset Construction

The Effects of Housing Choice Voucher Program on Welfare Families evaluation used many sources of data to measure the impacts of receiving a voucher on the lives of low-income families. The study relied on data collected directly from study participants in:

- A self-administered baseline survey completed at the time of random assignment;
- A follow-up interview completed by phone or in person 4 to 5 years after random assignment; and
- In-depth interviews with selected treatment group members 5 years after random assignment.

Data about sample members were also collected from the following administrative sources:

- Quarterly Unemployment Insurance (UI) wage records collected from state agencies;
- Monthly Temporary Assistance for Needy Families (TANF) and Food Stamps benefits records collected from state and local agencies;
- HUD administrative data on receipt of housing assistance; and
- Tract-level 2000 Census data on neighborhood characteristics merged to geocoded address histories for sample members that were compiled from participant tracking efforts.

This appendix presents a summary of the data sources and steps taken to construct analytic datasets. It is organized as follows: Section A1 discusses the alignment of random assignment date to the follow up period intervals. Section A2 discusses the collection and processing of administrative data sources used in the study and Section A3 describes the follow-up survey data. The construction of address histories and use of Census data to measure neighborhood characteristics is discussed in Section A4 and the in-depth qualitative interviews are described in Section A5.

A.1 Alignment of Random Assignment Date and Follow Up Intervals

As discussed in Chapter 1, the random assignment period varied across the study sites. The earliest random assignment took place in Fresno and Houston (April 2000) and the last random assignment was completed in Los Angeles in May 2001. Exhibit A.1 shows for each site the calendar interval over which random assignment was implemented and the sample size.

Exhibit A.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 length of the follow-up interval over which impacts could be estimated for the available sample in each site was determined by the end-month of the site’s random assignment period and the end-month of the collected outcome data.

For each case, the follow-up period for quarterly outcomes was considered to begin with the first complete calendar quarter following the date of random assignment. For the last-enrolled cases in Fresno and Houston, randomly assigned in June 2000, follow-up quarter 1 was thus July-September 2000. For Los Angeles, whose last case was randomly assigned in May 2001, follow-up quarter 1 was July-September 2001.

As shown in Exhibit A.2, the outcome data collected for this evaluation spanned different time periods depending on the data source (as further described in Section A.2.) The available quarterly follow-up intervals were as follows:

- December 2004 was the end-month for the unemployment insurance (UI) wage data, TANF and food stamp data, and PIC data. For quarterly outcomes measured through these sources, impact analysis could be conducted over 14 quarters for all sites and over 16 quarters for all sites excluding Los Angeles.
- Address history data were collected through June 2005. Impact analysis for address-based locational outcomes could be conducted over 16 quarters for all sites and over 18 quarters for all sites excluding Los Angeles.
- The follow-up survey was conducted during October 2004-May 2005, in all sites excluding Los Angeles. For the analysis of survey outcomes, the follow-up interval ranged from 16 to 20 quarters, depending on respondent’s dates of random assignment and follow-up interview.

Exhibit A.2
Follow-up Intervals for Quarterly Outcomes and Survey Outcomes, by Site

	Site					
	Atlanta	Augusta	Fresno	Houston	Los Angeles	Spokane
	Random assignment period					
Start month	Jun 2000	Jun 2000	Apr 2000	Apr 2000	Apr 2001	May 2000
End month	Sep 2000	Nov 2000	Jun 2000	Jun 2000	May 2001	Dec 2000
	Follow-up interval for quarterly outcomes^a					
Jul-Sep 2000	--	--	1	1	--	--
Oct-Dec 2000	1	--	2	2	--	--
Jan-Mar 2001	2	1	3	3	--	1
Apr-Jun 2001	3	2	4	4	--	2
Jul-Sep 2001	4	3	5	5	1	3
Oct-Dec 2001	5	4	6	6	2	4
Jan-Mar 2002	6	5	7	7	3	5
Apr-Jun 2002	7	6	8	8	4	6
Jul-Sep 2002	8	7	9	9	5	7
Oct-Dec 2002	9	8	10	10	6	8
Jan-Mar 2003	10	9	11	11	7	9
Apr-Jun 2003	11	10	12	12	8	10
Jul-Sep 2003	12	11	13	13	9	11
Oct-Dec 2003	13	12	14	14	10	12
Jan-Mar 2004	14	13	15	15	11	13
Apr-Jun 2004	15	14	16	16	12	14
Jul-Sep 2004	16	15	17	17	13	15
Oct-Dec 2004 ^c	17	16	18	18	14	16
Jan-Mar 2005	18	17	19	19	15	17
Apr-Jun 2005 ^d	19	18	20	20	16	18
	Follow-up interval for survey outcomes^b					
Range (quarters)	17-20	16-20	18-20	18-20	---	16-20

Notes:

^a Based on the last-enrolled case at each site, thus indicating the minimum length of the observed follow-up interval in quarters.

^b The survey follow-up interval differs for each case depending on when the case was enrolled within the random assignment period and when the case was interviewed within the survey period (October 2004-May 2005). The survey was not conducted at the Los Angeles site.

^c December 2004 was the end-month for UI wage files, TANF and food stamp data, and PIC data.

^d June 2005 was the end-month for address history data.

In Chapter 4 of this report, much of the impact analysis using administrative data (UI, TANF, and food stamp data) and survey data (employment history data) was conducted using half-year follow-up intervals. The outcome data were aligned in half-year intervals beginning with October 2000-March 2001 and ending with April-September 2004. The first complete half-year period following the date of random assignment was designated as half-year 1. As

shown in the exhibit, such analyses extended through half-year 6 for all sites and through half-year 7 for all sites excluding Los Angeles.

A.2 Administrative Data

The Effects of Housing Choice Vouchers on Welfare Families drew upon several administrative databases to measure outcomes. Exhibit A.3 summarizes the full set of administrative data sources and their contribution to the evaluation. For each of these administrative sources, data were obtained after first negotiating a data-sharing agreement with the State or local agency, and then matching the individual identifiers (Social Security Numbers) for the evaluation sample to the data systems. Each source of administrative data is described below, after a discussion of verification of individual identifiers.

**Exhibit A.3
Role of Administrative Data**

Research Domain	Outcomes/Purpose	Data Sources
Adult employment and earnings	Quarterly employment and earnings	State Unemployment Insurance (UI) Wage Records
Public assistance	Monthly cash assistance, food stamp benefits	State or local Temporary Assistance for Needy Families (TANF) files, food stamp files
Housing assistance and services	Receipt of tenant-based and project-based housing assistance	PIH Information Center (PIC), Tenant Rental Certification System (TRACS)
Housing mobility and neighborhood environment	Locational tracking and Characteristics of neighborhoods	PIC, TANF, Census

Social Security Number verification

Social Security Numbers (SSNs) have been collected from research sample members since the time they were enrolled in the study. From time to time since random assignment, in conjunction with passive tracking of sample location, new numbers and other new identifiers have been gathered from various sources. As a result, some of the study sample members have multiple SSNs, shared or reversed SSNs, and/or alias (alternative) names in the evaluation data system.

Such situations can make determining matches to administrative data more difficult. Both to facilitate the matching process and ensure the reliability of the matches, Abt Associates and HUD used an agreement with the Social Security Administration that was established under the Interim Evaluation of the Moving to Opportunity Demonstration to verify social security numbers of Voucher study sample members through SSA’s Employment Verification Service (EVS). The EVS results and information in the evaluation data files were used together to select the best SSN for each sample member from among those collected prior to random assignment. These verified identifiers were then used to determine correct administrative data matches.

Unemployment Insurance (UI) Data Files

State Unemployment Insurance (UI) wage and benefit records are a low-cost and accurate source of follow-up data on participant earnings, a key outcome measured in this evaluation.

In the four states participating in this evaluation, the State UI wage records are maintained by the following agencies: California State Employment Development Department, Georgia Department of Labor, Texas Workforce Commission, and Washington State Employment Security Department. In 2001 and 2002, under Task Orders 1 and 2 of this study, we negotiated with these agencies to establish data-sharing agreements through which UI wage records would be provided to us for the research sample. In 2002 we obtained UI wage records covering the time period January 1999 through September 2002. For Los Angeles this represented a time period more than two years prior to random assignment and for all other sites a period of one year prior to random assignment. In 2004 we renegotiated and updated the data sharing agreements and in 2005 collected a final data extract to extend the data coverage period through December 2004.

For this report, the administrative data on earnings were used to construct employment and earnings histories for members of the research sample (both treatment and controls). The raw files received from the states typically included several records per individual matched to the state system in each quarter, reflecting multiple employers per person. The study team reviewed and cleaned these disaggregated files and then aggregated all spells of employment and earnings per quarter to create one record per person per half-year with the sum of all earnings in each quarter. The person-level analysis files for each study site were then merged to create a half-yearly employment and earnings analysis file that was used to estimate impacts. Some problems were encountered with the UI wage files, particularly in cases where the final data extract contained time periods that overlapped with extracts received previously in 2002. Extensive review of the data and discussions with state database staff were required to resolve these inconsistencies and to prepare the final analysis file.

TANF and Food Stamp Data Files

To measure the effects of the HCV program on welfare participation, we collected information on the receipt of TANF and food stamp benefits from state or local welfare agencies. In each of the sites except Fresno and Los Angeles, data requests were made to state agencies: to the Georgia Department of Human Resources (Atlanta and Augusta), the Texas Department of Human Services (Houston), and the Washington State Department of Social and Health Services (Spokane). In California, where TANF is administered by county agencies, the data for Fresno were requested from the Fresno County Human Services Agency and the data for Los Angeles were requested from the Los Angeles County Department of Public and Social Services.

As with the UI wage records, we attempted to collect these data for a time period beginning 12 months prior to random assignment and extending through December 2004 and were able to do so in all cases except Los Angeles (TANF and food stamps) and Fresno (food stamps). The Los Angeles County Department of Public and Social Services was not able to extend the original data sharing agreement to allow for a final round of data collection. As a result, TANF and food stamps data are available for Los Angeles only through September 2002. Food stamps data for the Fresno site were only available for a part of 2004, and were thus not used in the analysis. In all other sites, these data were obtained through December 2004.

Monthly benefit data were used to construct a number of outcome measures for each of these two programs. They were also used to identify families who were TANF or food stamp recipients prior to random assignment. This pre-random assignment information is useful as a control variable in the analyses of impacts on TANF and food stamp receipt (see Chapter 4).

PIC Data Files

The evaluation has also used data from HUD's PIH Information Center (PIC) to monitor the status of sample members in both the treatment and control groups as to their receipt of housing assistance through the Housing Choice Voucher program or public housing, lease-up, and location of residence. This information is recorded by HA staff on HUD forms 50058 and is then transmitted periodically to HUD.

PIC data were used to:

- Profile the entire sample regarding treatment-control differences in voucher participation during the follow-up period;
- Determine which control group members received a Housing Choice voucher and when they received a voucher (or moved into public housing) after random assignment;
- Support locational tracking of the sample by providing address information for sample members

Five extracts from PIC were collected, corresponding to the following times: May 2001; December 2001; September 2002; March 2004; and December 2004. Each extract contained data for the 18 months preceding the extract date. These data provided a record of lease-up with a voucher for all treatment and control group members throughout the follow up period as well as an indicator of receipt of public housing. Information on voucher lease up was needed to measure treatment group take-up rates and control group crossover rates. In addition, the PIC data provided updated address data that were used to construct address histories for the research sample. Adjustment for control group crossovers and treatment group no shows was done on the basis of receipt of voucher assistance not on the basis of receipt of assistance through public housing.

In addition to HCVs and public housing, HUD provides housing assistance to low-income families through contracts with property owners under the project-based Section 8 program. Information regarding households living in Section 8 projects is reported to HUD by property owners on HUD form 50059 and is maintained by HUD in the ***Tenant Rental Assistance Certification System (TRACS)***. Because some members of the WtWV research sample may have received assistance through this program during the follow-up period, we collected a September 2002 extract of TRACS data from HUD and used it to identify sample members who had received project-based assistance. These data were not used in the crossover adjustment but simply to document the extent to which sample members had received project based assistance following random assignment. This was reported in the 2003 Report to Congress.

Census Data

The analysis of former and current neighborhood quality for treatment and control group members used 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 since the evaluation began in early 2000 and random assignment was completed in May 2001. Data from Summary Files 1 and 2 became available in the summer of 2002, and data from Summary File 3 were released in September 2002.

We geocoded addresses collected at the time of random assignment as well as updated addresses gathered from PIC, TRACS, and TANF data files and from the sample tracking efforts. Tract-level data were collected for each geocoded address to assess the changes in neighborhood characteristics experienced by sample members over the follow up period. For the analysis of neighborhood quality, an assumption will be made that, for any given Census tract, the measured indicators for the year 2000 were not subject to significant change during 2001-2004.

A.3 Follow Up Survey Data

To measure key program outcomes not available through administrative data sources and to facilitate analysis of the effects of housing vouchers on such outcomes, a follow-up survey of research sample members was conducted. The survey sample included 5,000 members of the full research sample of 8,731. The survey sample was interviewed 4 to 5 years after random assignment. Interviewing attempts were made first by telephone, with subsequent in-field follow-up. Interviewing commenced in October 2004 and was completed in May 2005. Key details about the follow-up survey are shown in Exhibit A.4.

**Exhibit A.4
Participant Data Collection Features**

	Household Head
Sample Size:	One adult person per family enrolled in WtWV program between April and December 2000 in all sites except Los Angeles. N=5,000 adults
Weighted Response Rate:^a	N=2,481
Field Period:	October 2004 – May 2005
Mode of Data Collection:	Telephone with In-person follow up

Notes:

^a To increase the response from hard-to-find cases and reduce nonresponse bias, data collection continued on a subsample of cases. These cases were then weighted when calculating the final response rates.

The survey instrument consisted of the Core Module and the 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. The Core Module, addressed issues of adult and household well-being, and the Parent-on-Child/Youth Module, which addressed issues related to child well-being. The sampling procedures for both modules are described below.

In July 2004, during the period of OMB review, we conducted a pretest of the questionnaire with seven respondents. Pretest respondents were selected from members of the research sample not selected to participate in the follow-on survey. The pretest allowed us to test the appropriateness of language level and word usage in the questionnaire. The pretest also allowed us to confirm the estimates of interview length. Experienced telephone interviewers conducted the pretest, which was observed by senior survey staff.

Sites included in the Follow-up Survey

The power of the impact analysis is importantly influenced by the treatment-control differential in lease-up rates. Through the sample design, it is possible to improve the power of the analysis by selecting a survey sample with a higher treatment-control lease-up differential. Our initial multivariate analysis of lease-up probabilities (at follow-up month 15 for all six sites) revealed that the single case characteristic most strongly correlated with lease-up was site location itself. In particular, cases from Los Angeles were far less likely to lease-up with a voucher. In consultation with HUD staff, it was agreed that the survey sample should exclude Los Angeles, to increase the statistical power of the survey-based impact estimates. (Impact estimates based on administrative data and Census data included all six sites.) The exclusion of Los Angeles cases from the survey sample also achieved the advantage of greater uniformity in the follow-up period among respondents; interviews occurred approximately 4 to 5 years after random assignment in the included five sites. (For Los Angeles, the follow-up interval would have been less than 4 years.)

Approach to Selecting the Survey Sample for the Core Module

Families were selected for the survey sample of 5,000 from among those members of the full research sample (of 8,731) that met the following criteria: the family was enrolled in one of the five non-Los Angeles sites; a baseline survey had been obtained from the family; the family reported the presence of a dependent child at baseline, and the household roster indicated the presence of a person in the household who was less than 18 years old at baseline. With these exclusions, the survey-eligible sample consisted of 7,258 cases.

The survey sample of 5,000 was selected randomly from the 7,258 survey-eligible cases. The survey-eligible list was randomly ordered, separately for the treatment and control groups. The first-listed 2,500 cases in each group then entered the survey sample.¹³⁷ Exhibit A.5 shows the distribution of the survey sample by site and treatment-control status. The difference among sites in the balance of treatment-control cases reflects primarily the differing treatment-control ratios used in random assignment (ranging between 3:2 and 1:1).

¹³⁷ We considered, but ultimately rejected, several options under which cases would enter the survey sample on the basis of their predicted probability of lease-up, in seeking to maximize the number of treatment-group voucher users in the survey sample. Applying such methodologies, however, tended to also increase the number of control-group voucher users (“crossover” cases) in the survey sample. The resulting treatment-control lease-up differential was thus only marginally larger under these options than under the random selection method that was adopted.

Exhibit A.5
Allocation of the Survey Sample

Site	Treatment	Control	Total
Number of cases:			
Atlanta	337	383	720
Augusta	257	239	496
Fresno	878	849	1,727
Houston	625	705	1,330
Spokane	403	324	727
Total	2,500	2,500	5,000
Distribution of cases:			
Atlanta	13.5%	15.3%	14.4%
Augusta	10.3%	9.6%	9.9%
Fresno	35.1%	34.0%	34.5%
Houston	25.0%	28.2%	26.6%
Spokane	16.1%	13.0%	14.5%
Total	100.0%	100.0%	100.0%

Approach for Selecting the Survey Subsample for the Parent-on-Child/Youth Module

A second aspect of the sampling plan, the selection of the subsample for which the Parent-on-Child/Youth Module will be administered, raised three issues:

- First, some families will not have dependent children in the relevant age range (4 to 19 years) at the time of the follow-on survey. (For example, some families had one dependent child who was 16 to 18 years old at baseline.) We included such families in the follow-on survey sample, because we did not want to restrict our overall analysis to families that had younger children at baseline. However, the Parent on Child/Youth module was to be administered only to families with children in the relevant age range (4 to 19 years).
- Second, some families had more than two children in the relevant age range within the household at baseline. For these families, random sampling was applied to select the two children for the survey questions.¹³⁸
- Third, some children who were present at baseline were not living in the household at the time of the follow-on survey. To avoid introducing attrition bias into our data collection, we attempted to ask a key subset of the child outcome questions about children who had

¹³⁸ The selection of children aged 0 to 15 from among baseline household members was done through simple random sampling. Up to two children were selected from each household in the study, without further stratification on age. In this way, analyses can be conducted combining children of different ages without having to weight the data specifically by each group. Where two children were drawn from the same household, appropriate sibling adjustments will be made during the analysis. In addition, where two children were selected from a household with more than two age-eligible members at baseline, the observations will be correspondingly weighted to represent the non-sampled children.

left the household as well as those who were still in the household. We did not replace a child absent from the household at the time of the follow-on survey with a child who was present, even in those families with more than two children.

Preliminary analysis of the research sample indicated that 97 percent of the baseline households with dependent children under 18 at baseline had children in the appropriate age range for the parent-on-child module of the follow-on survey. As shown in the lower panel of Exhibit A.6, two-thirds of the households in the study with children in the relevant age range had only one or two children in that age range; for these households, there was no need to sample children. The sampling only affected sample households drawn from the remaining 36 percent of the sample.

**Exhibit A.6
Child Population for Follow-on Survey**

	Percent
Age of youngest child at baseline	
Ages 0 – 15	82%
Ages 16 – 17	3%
No household members under 18 according to roster	4%
Respondent reports no dependent children	9%
No survey data	2%
TOTAL	100%
Number of children 15 and younger in baseline household (among the households with at least one dependent child under age 18)	
None	3%
One	30%
Two	31%
Three or more	36%

This section describes the procedures used in conducting the follow-on survey to achieve a high response rate and ensure data quality.

Preliminary Tracking and Locating

A critical aspect of any experimental evaluation is sample retention, requiring up-to-date locating information for the treatment and control group families. It was expected that some portion of the research sample would move each year, whether simply in keeping with the mobility patterns of low-income populations or additionally (as might be hypothesized for the treatment group) as a consequence of having received a voucher and having exercised greater housing choice. Evidence from the first two years of the evaluation indicated that, by the 7th quarter after random assignment, approximately one-half of the total sample (55 percent of the treatment group and 48 percent of the control group) had moved from their

initial Census tract.¹³⁹ Careful participant tracking was thus important to ensure that cases selected for the survey sample could be located for their interview.

For this survey, participant tracking efforts involved two types of tracking: the collection of address, telephone numbers, and social security numbers from *passive tracking* data sources (both administrative and commercial databases), and *active tracking* of participants through a mailout to sample members.¹⁴⁰ The locational information obtained through these efforts was merged into a tracking database. The updated locational information was used as the basis for contacting those sample members who were to participate in the follow-on survey. The address histories constructed for all members of the research sample will also be used to link tract-level Census data on indicators of neighborhood quality for the impact analysis.

Passive Tracking. Passive tracking of the full research sample was conducted at two intervals after the 2003 Report to Congress, in March 2004 and in November 2004. At each interval, the evaluation contractor (Abt Associates) used a commercial vendor (Experian) to perform an automated search for updated locating information, using a series of data services that compile information from such sources as credit applications, employment applications, directory assistance, reverse directories, retailer address lists, and change-of-address registers.

The first round of tracking, in March 2004, provided locational information on sample members (and the contact persons they identified at baseline) in advance of the pre-survey active tracking in April-May 2004, which is described below.

The second round of tracking, in November 2004, then updated the locational information so that the impact analysis can reflect the most current available addresses of sample members. The analysis of administratively measured, address-related outcomes will thus be approximately coincident in calendar time with the survey-measured outcomes, which will reflect the circumstances of respondents during the October 2004-May 2005 interviewing period. Any change-of-address information obtained through the November 2004 round of tracking will also facilitate efforts to locate and interview those survey sample members who, by that time, have not yet been located and interviewed.

Active Tracking. A pre-survey active tracking effort occurred in April-May 2004. In April, all members of the research sample were sent a personally addressed tracking letter, reminding them of their involvement in the WtWV study and requesting an update of their address, telephone number, and the corresponding information for their identified contact persons. The sample member was asked to provide such updated information using either a self-addressed stamped envelope or a toll-free telephone number. Those responding to the active tracking letter received an incentive payment of \$5. (Prior research has indicated that such incentive payments can significantly reduce the need for expensive field locating.¹⁴¹) The April 2004 mailout was timed to occur in the month after a passive tracking was

¹³⁹ Patterson, et al. (2003).

¹⁴⁰ *Passive tracking activities* are defined as those using methods that do not require direct contact with the sample members; instead, external data sources that may contain information on the movement of sample members are consulted. *Active tracking activities* involve direct contact with the household or sample member, by mail, by telephone, or in person.

¹⁴¹ See Duffer et al., *ibid.*

completed, to enable use of the most updated address information. A final active tracking mailout was conducted in July 2005, after completion of the follow-up survey.

Interviewing Procedures

Approximately one week prior to the actual start of interviewing, survey staff mailed a personalized advance letter to each household sample member. This letter was different from the locating letter described above. The advance letter served to:

- Alert them to the upcoming interview;
- Assure them that their responses will remain confidential;
- Inform them about the incentive payment;
- Provide them with a toll-free number they can call to request a special interview time or to ask questions about the study

The telephone interviewing efforts on the full survey sample were conducted first by M. Davis and Company, a Philadelphia-based small business subcontractor to Abt Associates, and then by the Abt Associates Telephone Center located in Amherst, Massachusetts. At both M. Davis and Abt Associates, the telephone interviews were completed using Computer Assisted Telephone Interviewing (CATI) techniques. This served to minimize missing or inconsistent item response and to reduce the time needed to provide clean data for analysis. Telephone interviewers were provided with the contact information collected on the baseline survey conducted at the time of random assignment to the WtW voucher program, as well as other locating information obtained through active and passive tracking efforts and administrative data sources (including the address data contained in TANF and PIC files). Upon being reached by a telephone interviewer, the respondent was allowed to re-schedule his/her interview for a more convenient time.

Abt field interviewers conducted in-person interviews with individuals who could not be located and interviewed by telephone. Field interviewers were provided with laptop computers that contained the same survey instrument used by the telephone interviewers. (Our CATI system was portable to laptops, ensuring that the identical questionnaire was used for both modes.) When pending cases were transferred to the field interviewers, the baseline contact information and the record of calls made by the telephone interviewers was included so that field interviewers would not duplicate earlier locating efforts.

The use of computerized telephone and in-person interviewing served to eliminate two common sources of poor quality data—missing data and inconsistent data. Thorough interviewer training and high levels of supervision assured that interviewers followed the study protocol. In the telephone interviewing, a supervisor was assigned to every shift to provide advice and support as well as monitor interviewer performance. Similarly, Field Managers were always available to field interviewers. Field Managers recontacted at least ten percent of all respondents and administered a brief “validation questionnaire” to assure that the interview was actually done with the correct respondent. Little editing was needed for computerized questionnaires, but open-ended questions required post-coding of the verbatim responses entered by interviewers into the CATI/CAPI system. The open-end coding was conducted by trained coders and coding supervisors.

Analysis staff tracked the survey response rates on a weekly basis, to be able to detect problems early and devise appropriate strategies. The strategy adopted in the field to locate and interview difficult cases depended on the resources available at the particular site and the characteristics of the particular case, but included actions such as assigning the case to an interviewer of a different gender or ethnicity, calling all persons in the area with the same last name, and checking with local utility companies. The Abt survey director and field supervisors managed the sample and the allocation of interviewing effort to avoid substantial differences in the response rates between sites and between the treatment and control groups at each site.

Incentive Payments

The survey design called for an incentive payment of \$25 to each survey respondent. (As noted above, sample members also received a \$5 incentive payment for responding to a final tracking letter, requesting updated locating information in advance of the survey.) Given the observed mobility of the research sample in the first two years after random assignment and the presumption that further mobility would occur in the third and fourth years after random assignment, it was extremely important to complete interviews with virtually all sample members that were successfully contacted. Previous research has shown that sample members with low incomes and/or low educational attainment are responsive to incentives, as are minority group members. These characteristics were heavily represented in this research sample.¹⁴²

In April 2005, as the interviewing period neared its end, the incentive payment was raised from \$25 to \$40 as a means of achieving as high a response rate as possible.

Use of Subsampling to Minimize Nonresponse Bias

It was critical that the follow-up survey achieve the highest possible response rate. In undertaking the survey, the aim was to achieve a response rate of 78 percent. Given uncertainties about the difficulty of locating the research sample, we had allowed for the possibility of a subsampling approach to achieve this target.

In consultation with HUD staff, we adopted a strategy for allocating survey resources to minimize the risk of bias associated with survey nonresponse. This strategy involved concentrating latter-stage survey efforts on a subsample of those not successfully interviewed during the early-stage survey efforts.

The subsampling strategy was implemented as follows:

- First-stage interviewing efforts on the full survey sample of 5,000 – efforts undertaken by telephone, initially by M. Davis interviewers and then by Abt Associates interviewers – yielded 1,123 completed interviews, for an initial response rate (R) of 22.5 percent.

¹⁴² See among the sources documenting this recommendation: Duffer et al. (1994), passim; Educational Testing Service (1991), pp. 2-3.

- Among the 3,877 remaining cases not interviewed in the first-stage efforts, a 50.1 percent subsample of 1,942 cases was randomly selected for the second-stage interviewing.
- Second-stage interviewing efforts on the subsample of 1,942 cases – efforts undertaken initially by telephone and then in the field, by Abt Associates interviewers – yielded 1,358 completed interviews, for a subsample response rate (r) of 69.9 percent.
- The effective survey response rate (R^*) is computed as $R + r(1 - R)$, or $0.225 + 0.699(1 - 0.225)$. This calculation yields an effective (or “weighted”) response rate of 76.7 percent.

The advantage of this approach is that the subsample response rate of 69.9 percent is a much higher proportion than would have been possible if the available survey resources had been spread over all cases that were pending after the first-stage efforts. Because the subsample was drawn randomly from the outstanding cases, the subsample cases – when weighted to reflect their sampling probability – can be used to represent the entire set of cases from which they were drawn. Thus, the 69.9 percent of the subsample that was successfully interviewed can be used to represent 69.9 percent of the cases that had not yet been interviewed at the time the subsample was drawn. From the standpoint of nonresponse bias, we achieved an effective response rate of 76.7 percent, even though only 49.6 percent of the sample – i.e., $(1,123+1,358)/5,000$ – was actually interviewed.

This substantial improvement in the representativeness of the survey respondent population comes with some loss in statistical precision for two reasons. First, subsampling reduces somewhat the number of completed interviews. Second, estimates based on a weighted sample have larger standard errors than estimates based on a self-weighting sample of the same size. The standard errors of the weighted estimates were about 10-20 percent larger than they would have been without subsampling. We believe that this is an acceptable price to pay (statistically) for the reduced risk of nonresponse bias under the subsampling approach. More details about survey nonresponse are provided in Appendix C.

A.4 Address Histories and Census Data

Using Address Data to Construct Address Histories

Throughout the evaluation we collected participant address information from a variety of sources. The quality of this address information depended on the source of the information and how recently it was collected. Address information based on direct contact with respondents was considered highly reliable for the point in time at which it was collected. During the evaluation we had up to four direct contacts with the families—at baseline, during active tracking in 2002, 2004, and 2005, and at the follow-up survey. These data could therefore miss locations where families resided between contacts. Moreover, even the collected address data did not indicate the date on which the family moved to that address. To supplement the addresses gathered from direct contact with sample members, we also collected information more continuously through the passive tracking process, from National Change of Address (NCOA) forms, credit bureaus, the Public Housing Information Center (PIC) system, and TANF data extracts. The addresses provided from these indirect sources

are typically less reliable than those collected through direct contact with the families, but can be useful in filling gaps in an address history and in assigning dates to a families' moves.

In constructing an address history for each sample member, we used all available addresses from both direct contacts and indirect sources, except where the address could not be geocoded to the Census tract level using standard commercial software for standardizing, parsing¹⁴³, and geocoding addresses.

The address history for each case was constructed as a succession of address spells, each at a distinct residential location (i.e., a specific street address) with a specified start date and end date. Rules were necessary to resolve two types of ambiguity in the available "data points" of address information. First, the data points sometimes provided conflicting information, necessitating judgments as to which sources were of greater reliability than others.

Second, the data points typically indicated only that the individual resided (or may have resided) at a specific address as of a particular calendar date, without any indication of the start date or end date.

As to the reliability of address data, any "direct" data points (from the baseline survey, active tracking, or follow-up survey) were regarded as accurate. Among the "indirect" data sources, the following order was established (from greater to lesser reliability):

- Passive tracking data obtained from Insight (a commercial data vendor)
- TANF data extracts
- Food Stamp data extracts
- Passive tracking data obtained from Anchor Data (a commercial data vendor)
- Federal (HUD) program data on voucher users
- Local (housing authority) data on voucher users
- Passive tracking data obtained from Experian (a credit bureau and commercial data vendor)

During preceding phases of this evaluation, the address history for each case had been constructed through March 2003. Each history was then extended through August 2005, using the following steps:

- Step 1: Begin the April 2003-August 2005 address history by ordering chronologically the *direct* data points obtained during this period, accepting as valid any repeat address spells (i.e., any spells that imply a move-back to a previously indicated address).
- Step 2: Add to the address history all *indirect* data points associated with addresses identified in direct data, thus extending the observed length of each identified spell forward and/or backward in time.

¹⁴³ Parsing refers to breaking the address information into component parts (street number, street name, apartment number, city, state, and zip code).

- Step 3: Add to the address history all indirect data points associated with addresses **not** identified in direct data **except** (a) where an address is indicated by only one indirect data point or (b) where indirect data imply a repeat spell. (Thus, a new spell is not introduced if it is supported by a lone indirect data point or if it implies a move-back.)
- Step 4 (eliminate the “overlaps” in adjacent spells): Where spell overlaps are present, resolve these conflicts by dismissing the “lesser” data point (i.e., dismiss a direct data point over any indirect data point, and resolve conflicts in indirect data points using the above-indicated order of priority).
- Step 5 (close the “gaps” between adjacent spells): Where spell gaps are present, assume that the spell break occurs at the midpoint of the gap.

An additional complication arose in cases with a missing baseline address. In such instances, we imputed the baseline address using the next observed data point in the previously constructed address history through March 2003. (When the next indicated address was followed by another within three months, the address with the higher-priority source was used for the imputation.) Of the 8,732 cases in the sample, 20 were deleted for lack of a baseline address and any other address history information, baseline addresses were imputed for 874 cases; and the remaining 7,843 cases required no imputation.

Linking of Census Data to Address History Data

For purposes of conducting quarterly impact analyses on tract-level neighborhood indicators, tract-level Census data were linked to each sample member on a quarterly basis, using their geocoded address history. Where the address history contained multiple addresses within a single calendar quarter, the assigned quarterly Census value was computed as a weighted average of the tract-level indicators corresponding to the addresses within the quarter.

For multi-address quarters, the weights were constructed as follows. Each quarter was regarded as consisting of 13 calendar weeks. For each address occurring within the quarter, the number of weeks in residence at that address during the quarter was determined (from the start- and end-dates of the address spell and the calendar quarter). This number of weeks divided by 13 was the address-specific weight for the quarter in question. (For single-address quarters, the weight was simply 1.)

A.5 Qualitative Participant Data Collection

The intensive interviews were designed to help us understand the kinds of housing choices families who received a voucher made, their experiences in finding and keeping jobs, and the role of housing assistance in their decisions. Importantly, these interviews let us hear about these choices and challenges first-hand, in the individuals’ own words. These interviews have also provided a rare and important opportunity to speak with recipients of housing assistance approximately five years after receiving the voucher, thus allowing them to reflect on the role the assistance has played in the family’s life over a relatively long period of time.

Overview of the Qualitative Interviews

In July, August, and September 2005, a team of six site visitors completed interviews with 141 program participants – members of the evaluation’s treatment group that received a voucher in 2000 – in the five research sites where the follow-up survey took place. Most of the interviews were conducted in individuals’ homes.¹⁴⁴ A lead interviewer conducted each interview, with a second staff person present to assist with logistics and timing.

The in-depth discussions ranged in length from 45 minutes to two hours, with most interviews lasting between 1 and 1 ½ hours. The discussions were conducted with the head of household identified in the research sample. In most cases only the identified individual participated in the interview, but in some cases a spouse or other individual was present (although in these cases the questions were directed to the identified respondent).

The participants received a \$50 incentive payment for completing the interview. Respondents received the incentive payment at the conclusion of the interview. All interviews were audio taped and then transcribed by professional transcribers.¹⁴⁵ Prior to the interview, each respondent signed a consent form acknowledging her/his agreement to participate in the interview and the understanding that his/her identity and responses would remain confidential.

The site visitors used a topic guide to focus the in-depth discussions. The interviews themselves were conducted as semi-structured discussions that covered each of the topics in the guide but not necessarily in that order. The interviewers posed open-ended questions and then probed for additional information on issues raised by the respondents to gather as detailed information as possible about each respondent’s experiences and opinions. The interviews covered the following topics: housing location and moves; neighborhood and housing preferences; employment experiences; household income sources; health; participation in education and training; the family’s use of resources; status of finances; family and child well being; support systems; and effects of the voucher on each of these issues.

The specific content and flow of each interview depended on the ways questions were answered, the issues that were important to the respondent, and the types of experiences she had had since applying for the WtWV program. The interview protocol was used as a guide that provided general topics for the interview. Interviewers also completed a post-interview summary form to summarize key issues raised in the interview and to record information about the respondent’s demeanor and responsiveness.

Recruiting In-Depth Interview Respondents

Altogether, 2,481 members of the research sample completed follow-up interviews. Of those 1,231 were treatment group members. We recruited respondents for the in-depth interviews from among treatment group members who completed a follow-up interview because we

¹⁴⁴ A small number of interviews were conducted in locations other than the respondent’s home, such as a local library or restaurant.

¹⁴⁵ One interview was conducted in Spanish and was not transcribed. However, this interview was summarized in English and is included in the findings presented here.

wanted to gather information about how they used the voucher and the role of housing assistance in decision-making. We did not interview controls because the purpose of these interviews was to gather detailed information on how the voucher had been used in making decisions about housing, employment, education, etc. rather than to explore differences between the experiences of treatment and control group member, an assessment best done through the impact analysis. The follow-up survey was administered between October 2004 and May 2005. As a result, we began recruitment for the in-depth interviews in June 2005. To be eligible for the in-depth interviews, a sample member had to be:

- A treatment-group member (i.e. assigned to the group that received a voucher);
- A respondent to the follow-on survey in either English or Spanish (i.e., without the need for other language translation); and
- A voucher user during some portion of the demonstration period (i.e., a voucher recipient who was successful in using the voucher to rent a housing unit).

We used the follow-up survey data to identify individuals who met these criteria, and more than 700 individuals satisfied the selection criteria. Within this eligible group, three types of cases received higher priority in the recruitment:

- Those who resided at the same address at the time of their follow-on interview as at the time of random assignment (referred to as “stayers”). Although a minority of the sample, such cases are of particular interest as they may provide some insight into the effect of vouchers through freeing up household discretionary income that would otherwise be required for housing. Such individuals also have avoided the disruption of moving to a new location and thus have not experienced the associated interruption in schools, childcare arrangements, and social networks. Through interviews with some number of these sample members, we hoped to better understand the influence of residential stability on the lives of the adult and child members of the household, in contrast to the experiences of “movers.” Altogether, we interviewed 9 stayers in the study.
- Individuals who had reported leaving the voucher program at some point during the follow-up period (questions were asked about this on the larger follow-up survey). This group was given priority in recruitment so that we could explore reasons for losing voucher assistance and implications for the families when housing assistance ends. Altogether, 32 individuals we interviewed had left the voucher program at some point.
- Those who were respondents in the first-round qualitative interviewing conducted in 2002. Altogether, we interviewed 3 individuals during this round of data collection who had also participated in the 2002 interviews.

We sent a letter to all 700 individuals who met the eligibility requirements we established for the in-depth interviews, inviting them to participate. The letter displayed a toll-free number that interested individuals could call to schedule an interview. When potential respondents called, members of the study team explained the purpose of the study and the interviews, answered questions from the potential respondents and, for those interested, scheduled a date and time for the interview. We scheduled up to 3 interviews per day, and to complete the desired number of interviews, we conducted 2, 5-day visits to each site. We sent a

confirmation postcard approximately one week prior to the scheduled time as a reminder, and the interviewers called each respondent 1 or 2 days prior to the interview to remind her again of the interview and to make any last minute changes in time or location that might have been necessary.

The response to the initial letters was quite good, and we were able to schedule more than half of the desired interviews from calls to the toll free number from those who were sent letters. However, we had to make additional outreach calls in order to fill the desired number of interview slots and to ensure that we scheduled interviews with respondents in the three groups of special interest (stayers, voucher program leavers, and first round in-depth interview respondents). To do this, the study team placed calls to individuals who had been sent a letter but who did not call the toll free number. The recruitment began in June 2005 and was completed in August prior to the final site visit in September.

A total of 141 interviews were completed across the 5 study sites, beginning July 14 and ending September 16, 2005. The number of interviews by site is:

Atlanta	25
Augusta	28
Fresno	30
Houston	30
Spokane	28

Analysis of In-depth Interviews

The interviews were tape recorded and then transcribed by professional transcribers. The written transcripts were then coded using a detailed coding scheme that allowed for analyzing a detailed set of research topics across the interview respondents. We used a software system often used for qualitative research called QSR NVivo for this purpose. This software and interview coding process was recommended to us in a design meeting with technical experts held in December 2004. During that meeting, qualitative researchers who are conducting similar interviews for another HUD study, the Moving to Opportunity Demonstration, described how they code completed interview transcripts so as to facilitate summarizing information across interviews by research topics. We also met with these researchers to review their coding scheme and processes prior to implementing the data collection for this study and to learn more about how this coding and analysis can be done efficiently.

The transcript coding involved reading each interview transcript and highlighting and marking text to indicate which topics were addressed in each part of the interview. By using a consistent and detailed coding scheme across all interviews, we have been able to analyze key topics, such as the housing search process, by downloading from the NVivo database all discussions of housing search. Having coded each transcript and entered it into the NVivo database thus facilitates the review and synthesis of analysis topics across interviews. Analysts then divided up the key research topics and summarized the responses and patterns across respondents. In analyzing each of the topics, the analysts also assembled specific examples from respondents and quotations. All of the names of respondents have been changed, and consistent names are used throughout the report. We focused the analysis on the following research topics:

1. Housing and Neighborhood
 - Housing search procedures
 - Housing unit and neighborhood preferences
 - Relations with landlords
 - Effect of housing and neighborhood choices on social support systems
2. Employment
3. Adult Education
4. Children’s Education
5. Material Hardship and Household Finances
6. Adult Health and Well Being
7. Children’s Health and Well Being
8. Reasons for Leaving the Voucher Program
9. Homeownership
10. Household Composition and Role of Fathers
11. Use of Support Systems

Appendix B

Samples and Analysis Methods

This appendix describes the basic analysis strategy for the impact analysis of the Effects of Housing Choice Vouchers on Welfare Families evaluation. The appendix includes eight sections:

1. Sample Selection
2. Data Sources
3. Estimation Methods
4. Covariates
5. Constructed Outcome Variables
6. Missing Data and Imputation Rules
7. Weights
8. Reporting of Results

B.1 Sample Selection

Two samples were used for the analyses: an adult sample and a child sample. Both samples draw on members of the household that applied to the WtWV program and were randomly assigned during the enrollment period in 2000 (2001 in Los Angeles). The adult sample consists of the adult head of household from each of the 8,731 households that were randomly assigned after applying to the WtWV program. As discussed in Appendix A, the focal children about whom information was collected on the parent-on-child module of the follow-up interview were children who were present in the household and age 15 or younger at baseline. We included up to two children from each household in the child sample. For households with more than two children in the target age range, we randomly selected two children to be the focus of the parent-on-child survey module. Children remained in the sample even if they no longer lived with the sample adult by the time of the follow-up survey. The final child sample consisted of 4,094 children.

B.2 Data Sources and Analyses

B.2.1 Data Sources

As discussed in Appendix A, our analyses use data from direct survey, proxy report (parent-on-child survey module), and administrative data. The data sources are listed below and in Exhibit B.1 we summarize the data sources for the adult and child samples.

- A) *Direct Survey* – Our direct survey sources include baseline and follow-up interviews with the sample adult.
- B) *Proxy Report* – We asked the sample adult about the education, health, behavior and time use of the sample children using the Parent-on-Child/Youth (POCY) module of the Adult Survey.

C) *Administrative Data* – We collected administrative data from federal, state, and local agencies on an individual’s employment and earnings, housing assistance, AFDC/TANF and food stamps assistance. In addition, we collected information about the characteristics of neighborhoods in which the sample members resided over the follow-up period.

**Exhibit B.1
Samples and Data Sources**

	Adult	Child
Direct Survey	Adult Survey	
Proxy Report		POCY Module of the <i>Adult Survey</i>
Administrative Data on Individuals	Unemployment insurance, AFDC/TANF and food stamps, housing assistance.	
2000 Census Data on Neighborhood Characteristics	Summary File 3 tract-level neighborhood characteristics	

B.2.2 Child-centered and adult-centered analyses

The adult-centered or household-centered analyses use outcomes constructed from the follow-up survey and administrative data. We constructed core household aggregate outcomes using administrative data and constructed current household aggregate outcomes using Section C (Household Composition) of the follow-up survey; these are the only sources of information on all household members.

The child-centered analyses (see Chapter 6) included child outcomes from the Parent on Child/Youth (POCY) module of the adult interview. Because the POCY module was completed as part of the adult interview, if a sample child no longer lived with the sample adult, the POCY information was therefore reported by someone who did not live with the child.

B.3 Estimation Methods

B.3.1 Comparisons

The experimental evaluation of the receipt of voucher assistance was designed to measure the effects of receiving rental housing vouchers on a variety of outcomes associated with family well being. Specifically, the evaluation was designed to estimate the effects of receiving voucher assistance. To address these questions, families were randomly assigned to the treatment group that received a WtW voucher, a control group that did not receive a voucher (but remained on the HA’s waiting list and eligible to receive a voucher through the regular HCV program). To estimate the impacts of receiving voucher assistance, we compare the outcomes of treatment group members to those of the control group (as described below the comparison is based on regression adjustments, not a simple comparison of means).

B.3.2 Estimation of Intent-To-Treat (ITT) effects

In a randomized experiment, the difference in mean outcomes for the treatment and control groups provide an estimate of the impact of being offered the treatment. This estimate

captures the average Intent-to-Treat (ITT) effect across all of the individuals included in the study, regardless of whether or not an individual assigned to the treatment actually complied with the treatment. Using a linear regression model, we can estimate the effect of Intent-to-Treat on outcome (Y) using whether an individual (indexed by i) was randomly assigned to the group offered the treatment ($Z=1$) or to the group not offered the treatment ($Z=0$):

$$(1) \quad Y_i = \alpha + Z_i\pi_{ITT} + \varepsilon_i$$

where Z_i indicates assignment status and π_{ITT} (the coefficient on Z_i) captures the ITT effect. To reduce the residual variation and thereby increase the precision of our estimate, we included in our regression models individual and household characteristics observed prior to random assignment (i.e., baseline characteristics):

$$(2) \quad Y_i = \alpha + Z_i\pi_{ITT} + X_i\beta + \varepsilon_i$$

where X represents a vector of characteristics for each individual (indexed by i), β represents the vector of coefficients for X , and α represents a constant. For all analyses using data pooled across sites, X included fixed-effects or dummy variables for each of the sites (with Fresno serving as the omitted or reference category). We used a linear regression model for continuous outcomes and a probit model for dichotomous outcomes.¹⁴⁶ Robust standard errors were used with both models due to the study's complex sampling design.

We obtained the ITT impacts for dichotomous outcomes using the “dprobit” command of the Stata statistical package. For dichotomous outcomes, the ITT impact is the effect of being in the treatment group versus the control group on the probability of the outcome, and is defined as follows:

$$ITT = \Phi(\Pi + \bar{X}_T\beta) - \Phi(\bar{X}_T\beta)$$

where:

$\Phi(\)$: the normal cumulative density function

Π : the estimated coefficient on the treatment group dummy variable from the probit regression

\bar{X}_T : the vector of covariate mean values within the treatment group

β : the vector of estimated covariate coefficients from the probit regression

Thus, the ITT impact for dichotomous outcomes is a transformation of the probit coefficient on the treatment dummy, rather than the probit coefficient itself. Likewise, the standard errors presented in the report are transformations of the probit robust standard errors. The statistical significance of the treatment effect, however, is tested directly within the probit model, rather than using the transformed probit coefficient (i.e. the ITT impact) and the transformed probit robust standard error.

¹⁴⁶ Impacts on all dichotomous survey outcomes were estimated with a probit model. The single dichotomous administrative outcome, whether a sample member had moved out of his or her baseline Census tract, was fit with a linear probability model to enable the calculation of TOT standard errors in the manner described in Appendix B.3.3. Impacts on this outcome are shown in Exhibit 3.5.

In contrast to the linear probability model, the probit model is unable to estimate coefficients for dichotomous covariates that perfectly predict the outcome. In these cases, the observations that generate perfect prediction must be dropped. This feature of the probit model led to some variation in the number of observations used to estimate the impact on each dichotomous outcome.

B.3.3 Estimation of Treatment-on-Treated (TOT) effects for Census and Administrative Data 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). 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) impact. 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.¹⁴⁷

The standard error of the TOT estimate is just the standard error of the ITT estimate times the same adjustment factor.¹⁴⁸ Since 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.

For follow-up survey outcomes, we use this simple Bloom adjustment factor ($1/(p-c)$) to calculate TOT impacts. For Census and administrative data outcomes, we use a more complex adjustment procedure to calculate TOT impacts and standard errors. This more complex procedure, which is an extension of the Bloom adjustment, is described below.

In this evaluation, the assumption that the treatment has no effect on nonparticipants seems quite reasonable. It seems unlikely that the mere *offer* of a housing voucher will have appreciable effects on the outcomes of interest if the family does not actually use the voucher. We recognize that the process of searching for a unit to lease could have either positive or negative effects – e.g., exposing the family to opportunities or possibilities they might not otherwise have considered or, conversely, discouraging them from further efforts to improve their situation. But it seems likely that, on balance, these effects will be quite small relative to the effects of actually receiving a rent subsidy. Therefore, we are comfortable applying the adjustment for nonparticipation (i.e., failure of treatment group members to lease up), which rests on this assumption.

The second assumption, however, seems less tenable in this study. 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. As noted above, 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 the effects of the voucher on this outcome 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 is less appropriate in this case.

¹⁴⁷ 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.

¹⁴⁸ 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).

Accounting for the Timing of Lease-up in Correcting for Nonparticipants and Crossovers

In this section, we derive a correction for nonparticipants and crossovers that takes into account the difference in timing between lease-ups in the treatment group and lease-ups in the control group.¹⁴⁹

The standard ITT estimate in period k after random assignment (RA) can be expressed as:

$$ITT_k = \bar{Y}_k^T - \bar{Y}_k^C$$

$$1) = (1/n_T) \sum_{i=1}^{n_T} (Y_{ik}^{T_0} + \delta_{ik}^T) - (1/n_C) \sum_{i=1}^{n_C} (Y_{ik}^{C_0} + \delta_{ik}^C)$$

where:

\bar{Y}_k^T = treatment group mean of outcome Y in period k after RA

\bar{Y}_k^C = control group mean of outcome Y in period k after RA

$Y_{ik}^{T_0}$ = outcome of the i th treatment group member in period k in the absence of the intervention

$Y_{ik}^{C_0}$ = outcome of the i th control group member in period k in the absence of the intervention

δ_{ik}^T = impact of the intervention on the i th treatment group member in period k after RA

δ_{ik}^C = impact of the intervention on the i th control group member in period k after RA

n_T = number of treatment group members

n_C = number of control group members

Without loss of generality, we can segment the summations in equation (1) into separate summations for the sets of individuals who have been leased up different lengths of time, so that:

$$ITT_k = (1/n_T) \left[\sum_{i=1}^{P_{k,0}n_T} (Y_{ik}^{T_0} + \delta_{ik}^T) + \sum_{i=P_{k,0}n_T+1}^{P_{k,1}n_T} (Y_{ik}^{T_0} + \delta_{ik}^T) + \dots + \sum_{i=P_{k,k-1}n_T+1}^{n_T} (Y_{ik}^{T_0} + \delta_{ik}^T) \right]$$

$$2) - (1/n_C) \left[\sum_{i=1}^{C_{k,0}n_C} (Y_{ik}^{C_0} + \delta_{ik}^C) + \sum_{i=C_{k,0}n_C+1}^{C_{k,1}n_C} (Y_{ik}^{C_0} + \delta_{ik}^C) + \dots + \sum_{i=C_{k,k-1}n_C+1}^{n_C} (Y_{ik}^{C_0} + \delta_{ik}^C) \right]$$

$$= \bar{Y}_k^{T_0} - \bar{Y}_k^{C_0} + \sum_{j=0}^k p_{k,j} \delta_j - \sum_{j=0}^k c_{k,j} \delta_j = \bar{Y}_k^{T_0} - \bar{Y}_k^{C_0} + \sum_{j=1}^k p_{k,j} \delta_j - \sum_{j=1}^k c_{k,j} \delta_j$$

where:

¹⁴⁹ The estimation procedure presented here is based on a suggestion by Steve Bell.

$p_{k,j}$ = proportion of treatment group in period k who have been leased up for *exactly* j periods

$c_{k,j}$ = proportion of control group in period k who have been leased up for *exactly* j periods

$P_{k,j}$ = proportion of treatment group in period k who have been leased up for j *or fewer* periods

$C_{k,j}$ = proportion of control group in period k who have been leased up for j *or fewer* periods

δ_j = effect of intervention on a sample member who has been leased up for (exactly) j periods

The last equality follows from the assumption of no treatment effect for those leased up for 0 periods.

With random assignment:

$$E(\bar{Y}_k^{T_0} - \bar{Y}_k^{C_0}) = 0$$

so that:

$$3) \quad E(ITT_k) = \sum_{j=1}^k E(p_{k,j} - c_{k,j})\delta_j = \sum_{j=1}^k \Delta p_{k,j} \delta_j$$

where:

$\Delta p_{k,j}$ = expected value of the difference between treatment and control groups in the proportion of individuals in period k who have been leased up for j periods (i.e. who leased up in period k-j)

The ITT impact in a given period, then, is the sum of the TOT impacts on sample subgroups who have been leased up for varying periods of time, weighted by the treatment-control difference in participation rates for each of those durations. To determine the time path of the effect after lease-up, we can solve equation (3) for δ_j , the TOT impact on each of these groups.

Equation (3) can be solved recursively as follows (it is also possible to solve simultaneously for this entire set of impact estimates; see below for details):

$$4) \quad \begin{aligned} \delta_1 &= \frac{ITT_1}{\Delta p_{1,1}} \\ \delta_2 &= \frac{ITT_2 - \Delta p_{2,1} \delta_1}{\Delta p_{2,2}} \\ &\vdots \\ \delta_t &= \frac{ITT_t - \sum_{j=1}^{t-1} \Delta p_{t,j} \delta_j}{\Delta p_{t,t}} \end{aligned}$$

Note that this estimation procedure not only corrects for nonparticipants and crossovers, but also takes account of the lag between random assignment and lease-up.¹⁵⁰ Impacts are dated from the period in which the sample member leased up; impacts in earlier periods are assumed to be zero.

This solution depends critically on the assumption that δ_j is constant across all individuals – i.e., that the effect of the program j periods after lease-up is the same for controls as for treatment group members and the same for sample members who lease up early as for those who lease up late (relative to both random assignment and calendar time).

Simultaneous Solution for Period-Specific Impact Estimates, Adjusted for Nonparticipants and Crossovers

In the text above, we provided a recursive solution for δ_j , the TOT impact of the intervention j periods after lease-up. It is also possible to solve simultaneously for this entire set of impact estimates. In matrix notation, the t -period set of equations corresponding to text equation (3) can be written as:

$$5) \begin{pmatrix} ITT_1 \\ ITT_2 \\ ITT_3 \\ \vdots \\ ITT_t \end{pmatrix} = \begin{pmatrix} \Delta p_{1,1} & 0 & 0 & \cdots & 0 \\ \Delta p_{2,1} & \Delta p_{2,2} & 0 & \cdots & 0 \\ \Delta p_{3,1} & \Delta p_{3,2} & \Delta p_{3,3} & \cdots & 0 \\ \vdots & \vdots & \vdots & \ddots & \vdots \\ \Delta p_{t,1} & \Delta p_{t,2} & \Delta p_{t,3} & \cdots & \Delta p_{t,t} \end{pmatrix} \begin{pmatrix} \delta_1 \\ \delta_2 \\ \delta_3 \\ \vdots \\ \delta_t \end{pmatrix}$$

Solving for δ_j :

$$6) \begin{pmatrix} \delta_1 \\ \delta_2 \\ \delta_3 \\ \vdots \\ \delta_t \end{pmatrix} = \begin{pmatrix} \Delta p_{1,1} & 0 & 0 & \cdots & 0 \\ \Delta p_{2,1} & \Delta p_{2,2} & 0 & \cdots & 0 \\ \Delta p_{3,1} & \Delta p_{3,2} & \Delta p_{3,3} & \cdots & 0 \\ \vdots & \vdots & \vdots & \ddots & \vdots \\ \Delta p_{t,1} & \Delta p_{t,2} & \Delta p_{t,3} & \cdots & \Delta p_{t,t} \end{pmatrix}^{-1} \begin{pmatrix} ITT_1 \\ ITT_2 \\ ITT_3 \\ \vdots \\ ITT_t \end{pmatrix}$$

¹⁵⁰ The simple Bloom adjustment is a special case of this correction, in which all participants and crossovers are exposed to the intervention immediately after random assignment. In that case, $\Delta p = 0$ for all durations except t , and equation (4) reduces to $\delta_t = ITT_t / \Delta p_{t,t}$.

The variance-covariance matrix for δ_j is:

$$7) \text{VarCov} \begin{pmatrix} \delta_1 \\ \delta_2 \\ \delta_3 \\ \vdots \\ \delta_t \end{pmatrix} = (\Delta p)^{-1} \text{VarCov} \begin{pmatrix} ITT_1 \\ ITT_2 \\ ITT_3 \\ \vdots \\ ITT_t \end{pmatrix} (\Delta p')^{-1}$$

where

$$8) (\Delta p) = \begin{pmatrix} \Delta p_{1,1} & 0 & 0 & \cdots & 0 \\ \Delta p_{2,1} & \Delta p_{2,2} & 0 & \cdots & 0 \\ \Delta p_{3,1} & \Delta p_{3,2} & \Delta p_{3,3} & \cdots & 0 \\ \vdots & \vdots & \vdots & \ddots & \vdots \\ \Delta p_{t,1} & \Delta p_{t,2} & \Delta p_{t,3} & \cdots & \Delta p_{t,t} \end{pmatrix}$$

B.3.4 Estimation of Treatment-on-Treated (TOT) effects for Follow-up Survey Outcomes

We do not know the full time-paths of most of the outcomes that were constructed from responses to the follow-up survey. Instead, we only know the values of these outcomes at a single point in time in the fifth year after random assignment. Therefore, we are unable to use the method described in B.3.3 to adjust for the timing of lease-up. As an alternative, we use the simple Bloom adjustment factor:

$$\frac{1}{p-c},$$

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). We use this adjustment factor for impacts on both continuous and dichotomous survey outcomes. As noted in B.3.3, this adjustment factor does not account for the timing of lease-up. It assumes that the effect of the voucher for someone who has recently leased-up is identical to the effect for someone who first leased up years ago. For outcomes where the effect of the voucher is immediate, this assumption is appropriate. For other outcomes where we expect the effect of the voucher to cumulate over time, this assumption will tend to overstate the true TOT impact, since crossovers generally leased up later than treatment group voucher users, and so should be viewed as an approximation. The divergence of the TOT impact calculated by the simple Bloom adjustment from the true TOT impact *increases* as the timing differential between treatment group and control group lease-up *increases*. In the last 12 months of lease-up data for the survey respondent sample, the participation rate increases 0.7 percentage points and the crossover rate increases 2.7 percentage points.

B.3.5 Clustering on Household

In our child analyses we included up to two individuals from each household. As a result, we could not assume that the residuals were independent across all observations. To address this issue, we allowed residuals for observations within the same household to be correlated when estimating standard errors.

B.3.6 F-tests on Subgroup Impacts

For selected outcomes, we estimated impacts by subgroup. Each set of subgroups was defined by a baseline characteristic (such as age, race/ethnicity, or education) and consisted of 2, 3, 4, or 5 separate subgroups. We performed F-tests on regression parameter estimates to determine whether impacts differed by subgroup within a set of subgroups. Pooling all individuals within a set of subgroups (essentially the whole sample, minus any individuals for whom we were missing information on the baseline characteristic that defined the set), we ran the following regression model:

$$Y_i = \alpha + Z_i\pi + \sum_{s=1}^{S-1} \gamma_s I_{si} + \sum_{s=1}^{S-1} \lambda_s (Z \times I_s)_i + X_i\beta + \varepsilon_i$$

where individuals are indexed by i , and

Y : an outcome of interest

Z : dummy variable for membership in the treatment group

I_s : dummy variable for membership in subgroup s

$(Z \times I_s)$: interaction term that equals 1 if member of both treatment group and subgroup s

X : other individual characteristics

S : the number of subgroups in the subgroup set.

Technically, the F-test tests the null hypothesis that all the λ parameters, which are the coefficients on the treatment-subgroup interaction terms, are equal to zero. A rejection of the null hypothesis implies that ITT impacts differ significantly between the subgroups within the subgroup set.

B.4 Covariates

We included covariates in our regression models to improve the precision of our estimates. Since individuals were randomly assigned to control and treatment groups, the addition of these covariates does not affect the expected value of the estimate itself. All covariates had to be characteristics that were known (or determined) prior to randomization. In selecting covariates, we considered (a) the importance of the variable in predicting the outcomes of interest, (b) the extent of variation on the variable for the sample, and (c) the completeness of the data.

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;

- whether the respondent was working at baseline;
- whether the respondent had ever worked;
- the respondent's reservation wage per hour, 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 because 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, 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 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;
- 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.

Also included in the regressions were dummy variables which indicated whether particular baseline covariate values were missing. In instances where a covariate value was missing for an observation, we imputed an artificial value for the covariate and set the appropriate dummy variable equal to one. The following variables had associated dummies to indicate missing values: earnings, working at baseline, had ever worked, educational attainment, in school at baseline, enrolled in job training program, had car that ran, had current driver's license, on TANF at baseline, ever on TANF/AFDC, received food stamps at baseline, received SSI at baseline, received Medicaid at baseline, ever married, had any dependent children, household size at baseline, age, ratio of monthly household rent payment to monthly household income, desired to move for employment reasons, type of housing situation, and was a frequent mover.

For the analyses using the child data, we also included child characteristics from the baseline survey as covariates. These were:

- Age of focal child at baseline
- Gender of focal child

Each of these variables also had an associated dummy variable to indicate missing values. In addition to this set of common covariates, the regressions for which results are presented in Chapters 3, 4, 5, also controlled for baseline values of the outcome variable (when baseline values were known). In Chapter 3, which presents impacts on locational characteristics, regressions estimating impacts on neighborhood satisfaction and number of moves in the follow up period also included baseline values of these variables. In Chapter 4, the following baseline values of outcomes were included in the relevant regressions: years of schooling; working full time at main job; length of current main job in months; hours worked per week; receipt of employer-provided benefits; mode of transportation to work; travel time from home to work; TANF cash assistance amount; food stamp cash assistance amount; and monthly SSI benefit amount. Some of the 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. In Chapter 5, the following baseline values of outcome variables were included in the relevant regressions: housing condition; and major housing problems.

B.5 Constructed Outcome Variables

We constructed two key outcome measures using data from several items on the follow-up survey. For the analysis of children, we constructed a behavior problems index using items from the parent-on-child module. This **behavior problems index** was calculated as the fraction of behavior problems of child 5-19 as reported by adult. This index consists of 11 items: difficulty concentrating; cheating or lying; bullying or being cruel or mean to others; disobedient at home; difficulty getting along with other children; restless or overactive; strong temper; withdrawn; hanging around with kids who get into trouble; disobedient at

school; and trouble getting along with teachers. The value of the numerator equals the total number of behavior problems the adult reports is "Often True" or "Sometimes True" of the child and the denominator is the total number of the items that the adult answered. The variable was coded as zero if the child displayed none of the behavior problems.

In addition we constructed a **composite measure of household income** that takes into effect various sources of household income reported to be received by the household at the time of the follow-up interview. This composite measure of household income is the sum of current monthly earnings, earnings from small jobs, and income from TANF, welfare, food stamps and the housing assistance. Social security taxes and an imputed value for federal income tax is subtracted from this total. The income tax imputation was done as follows: If the respondent reported receiving a refund and gave the amount of the refund, we use this amount. For respondents who do not report a refund, we computed income over the tax threshold and multiply by .1, which is the lowest marginal rate applying to a head of household. The threshold in 2005 is:

$S + (E * H)$, where S is the standard deduction (\$7150 for a head of household), E is the exemption amount (\$3100), and H is the # of exemptions, or the number of household members in the tax filing unit.

For observations where the Current Monthly Income could not be accurately computed due to missing wage information or employment dates, outcome variables involving monthly income were set to missing.

The value of the housing assistance was calculated based on responses to the follow-up interview, as the difference between the total monthly rent for the current housing unit and the amount of rent reported paid by the household for the rent, at the time of the follow-up interview for those who reported current receipt of housing assistance.

Other imputations were used in the following instances when data items were missing:

- SSI: imputed based on site-group mean.
- Tax Refund: imputed based on site-group mean.
- Small job earnings: if small jobs earnings missing, but small jobs hours present, imputed based on minimum wage for site; if small jobs hours also missing, imputed based on site-group mean.
- Food stamps: if respondent reported receiving food stamps, but value missing, imputed food stamp value as 30 percent of earnings from main job and small jobs, and subtracted this from food stamp standard based on household size to impute value; if food stamp value less the \$10, set to \$10 (the minimum).
- TANF: if respondent reported receiving TANF, but value missing, used $((\text{sum of earnings from all jobs}) - 225) \times 0.5$ as the TANF income, took the TANF payment standard by site and household size to impute value.
- Housing assistance: if housing assistance amount was missing or negative, used fair market rent (FMR) by site and number of bedrooms and 30 percent of total income to

impute value; if still missing because of missing response for number of rooms, imputed based on site-group mean.¹⁵¹

B.6 Missing Data

Baseline and follow-up survey data were occasionally missing due to a subject not knowing or refusing to answer a particular question. In most cases, where the outcome itself was missing, the observation had to be excluded from the analysis; individuals with unknown values of the outcome can contribute nothing to the impact estimate. The only exceptions to this rule were cases where data were missing for a component of household income; such components were imputed using the procedure described in Section B5.

Included in impact regressions were dummy variables which indicated whether particular baseline covariate values were missing. In instances where a covariate value was missing for an observation, we imputed an artificial value for the covariate and set the appropriate dummy variable equal to one.

The follow-up survey asked respondents to provide details about all the jobs they had held since random assignment. From these responses, we constructed an earnings history for each respondent. The details for each job in the job history included start and end dates (month and year), earnings per week, and usual weekly hours. Respondents were also asked about their current main job (the job in which they worked the most hours). In 37 cases, earnings amounts from the current main job were deemed to be extreme outliers and they were set to missing. Observations were dropped from the impact regressions presented in Exhibit 4.9 for any of the following reasons: if more than one job in the job history had missing earnings; if there was only one job in the job history and this had missing earnings; if one job had missing earnings and one job had a missing month from start or end dates; or if any of the jobs in the job history had a missing year from start or end date. A total of 212 observations were dropped for these various reasons. Start and end dates were assumed to occur on the 15th of the month. If a month, but not a year, was missing from a start or end date, and no earnings were missing from any job in the history, then July 1 was imputed as the start or end date. If no start or end dates were missing, only one job had missing earnings, and at least one other job had non-missing earnings, then the missing weekly earnings were imputed as the average of weekly earnings of all other jobs in the job history. There were 26 observations that had one job with imputed earnings in the job history.

Missing earnings for the current main job were imputed if no other jobs in the job history had missing earnings, at least one job in the job history had non-missing earnings, and no start or end date was missing the year. The imputed value for the current main job was the average of weekly earnings of all other jobs in the job history. There were 13 observations that had imputed values for earnings for the current main job. Observations were dropped from the current earnings regression presented in Exhibit 4.9 and from the household composite income regressions presented in Chapter 5 if the current main job had missing earnings after

¹⁵¹ The follow-up survey asked for a report of the number of rooms besides bathrooms in the residence. We assumed that the number of rooms was equal to the number of bedrooms plus two (for a kitchen and a living room). Fair market rent data was obtained at the following website: www.huduser.org/datasets/fmr.html.

the imputation process. A total of 24 observations were dropped from the current earnings and the household composite income regressions for this reason.

A small fraction of the lease-up data used to calculate TOT impacts was also imputed. The MTCS data covered a period which ended in December 2004. Some Chapter 3 outcome measures used address history data that extended beyond this period by as much as six months. In order to calculate TOT impacts for these measures, we imputed “ever leased up” status for some sample members at the end of follow-up period by assuming no changes in the imputed months. For 18-quarter impacts, we needed 54 months of “ever leased up” status to calculate TOT impacts. If a sample member only had 53 months of “ever leased up” status (because of the timing of random assignment), then the status for the 54th month was assumed to be the same as the status in the 53rd month. Similarly, if a sample member had 52 months of “ever leased up” status in the MTCS data, we assumed that the status in the 53rd and 54th months were the same as the 52nd month. Since there was little lease-up activity at this point during the follow-up period, we expect that these imputations did not materially affect the TOT estimates. Exhibit B.6.1 details the lease-up data that was imputed for Chapter 3 outcomes.

Exhibit B.6.1
Imputation of “Ever Leased Up” Status

Number of Sample Members	Number of Months with Imputed “Ever Leased Up” Status
5,191	0
337	1
468	2
1219	3
316	4
1198	5
2	6

B.7 Weights

We weighted the Effects of Housing Choice Vouchers on Welfare Families data for all of the analyses. We weighted both the administrative and survey data to adjust for the changes in random assignment ratios during the study. In addition, we weighted the survey data to account for sampling and survey response for the adult respondent. We also sampled up to two children from each family for the parent-on-child module of the follow-up survey. In the sections below, we describe the weights that were needed for each of these adjustments.

B.7.1 Randomization ratio weights

In Atlanta and Houston, sample members were assigned to the treatment group with a probability of 50 percent. In Augusta and Spokane, initially sample members were assigned to the treatment group with a probability of 50 percent. Later in the random assignment period, it became more important to distribute the full allotment of vouchers; so sample members were assigned to the treatment group with a probability of 60 percent. In Los Angeles during the entire random assignment period, sample members were assigned to the treatment group with a

probability of 60 percent. In Fresno, there were three RA periods: initially, assignment was to treatment group with a probability of 50 percent; then a period with ex-post probability of 53.7 percent; in the last period, sample members were assigned to the treatment group with a probability of 60 percent. Randomization ratio weights were constructed such that the weighted treatment group size was equal to the weighted control group size in each site. For each group, site, and ratio period, the weight applied was equal to:

$$(16) \quad w_{jkh} = 0.5 / \text{Pr}(j)_{kh}$$

where:

- j indexes the treatment and control groups,
- k indexes the site,
- h indexes the RA ratio period, and
- $\text{Pr}(j)$ is the probability of being assigned to treatment or control group.

B.7.2 Survey sample selection weights

We had follow-up survey data for two samples: the sample adults and sample children. As a result, two types of survey sample selection weights were needed; one to adjust for the selection of adults into the survey sample of 5,000 and for survey response; and a second for the selection of focal children for the parent-on-child survey module. For the children we needed weights to adjust for the fact that we selected a random sample of children (up to two per family) rather than all eligible children in the target age range (up to age 15 at baseline). As described in Appendix A, the follow-up survey was conducted in two stages. The first stage attempted to interview a sample of 5,000 out of 7,258 survey-eligible cases. The second stage interviewed a subsample of the non-respondents from the first stage. Respondents to the first stage of the survey were assigned a survey weight given by:

$$w_h = \frac{N_h}{n_h}$$

where:

- N_h denotes the size of the survey-eligible sample in stratum h ,
- n_h denotes the size of the survey sample (first stage) in stratum h , and
- stratum h is one of 10 strata, each group (treatment or control) at each of the 5 survey sites.¹⁵²

Respondents to the second stage of the survey were assigned a survey weight given by:

¹⁵² Los Angeles was not included as a survey site.

$$w_h^* = \left(\frac{N_h n_{2h}}{n_h n_{2h}^*} \right) \left(\frac{n_{2j}^*}{n_{2rj}^*} \right)$$

where:

- n_{2h} denotes the number of non-respondents in the first stage of the survey in stratum h ,
- n_{2h}^* denotes the size of the subsample (second stage of the survey) in stratum h ,
- n_{2j}^* denotes the size of the subsample (second stage of the survey) in stratum j ,
- n_{2rj}^* denotes the (actual) number of respondents in the second stage of the survey in stratum j , and
- stratum j is one of 50 strata, defined by group, site, and propensity to respond to the survey.

The fraction $\left(\frac{n_{2j}^*}{n_{2rj}^*} \right)$ is the nonresponse adjustment component of the weight for second stage

survey respondents. Comparisons shown in Appendix C indicate the presence of some degree of survey nonresponse bias. In order to minimize this bias, we calculated the nonresponse adjustment component based on more finely-grained strata than used elsewhere in the weighting method. This allowed the second stage respondents to more closely represent the entire second stage subsample. To create the strata for the nonresponse adjustment component, we first regressed survey response for the second stage subsample on all the baseline covariates using a probit model. We then divided the subsample into quintiles based upon the predicted probability of responding to the survey. Each quintile was then further divided into 10 strata based upon group (treatment or control) and site.

The final weight used in analysis of adult survey data was the product of the randomization weight and the adult survey sample selection weight.

The likelihood of including a child in the sample depended on the number of children ages 0-15 in the household at baseline. Since a maximum of two children were sampled from each family, children in families with more than two children had a lower probability of selection. We weighted each observation by the inverse probability of the child being selected into the sample.

$$\text{Child Survey Sample Weight} = \frac{\# \textit{eligible}}{\# \textit{selected}}$$

where:

$\# \textit{selected}$ = the number of children ages 4-19 in the family who were selected by the parent for the parent-on-child module,

$\# \textit{eligible}$ = the total number of children ages 4-19 in the family who were eligible to be selected for the parent-on-child module.

The final weight used in analysis of child survey data was the product of the randomization weight, the parent’s survey sample selection weight, and the child survey sample weight.

B.8 Interpretation and Reporting of Results

We presented only regression-adjusted impacts. While unadjusted impacts (simple differences between treatment and control group means) have some intuitive appeal as representing what “really” happened, they are clearly less reliable indicators of impact than the adjusted estimates. To the extent that two estimates differ, we wanted the reader to focus on the more reliable estimate. Therefore, we reported only the control mean, its standard deviation, and the impact. These numbers give the reader a sense of what difference the intervention made (the impact estimate) and how large that difference was relative to what would have happened in the absence of the intervention (the control mean). The standard deviation of the control mean allows the reader to compare the impact estimate to naturally occurring variation within the control group, which is another way to gauge the size of the impact.

Appendix C

Tests for Nonresponse Bias

Many of the outcomes analyzed in this report were measured with survey data. As in all surveys, there was some nonresponse in the follow-up survey conducted for this evaluation. In this appendix, we analyze the extent to which the survey nonresponse may have biased the impact estimates for survey-measured outcomes. First, we review the follow-up survey sample design and show that the key comparison in assessing the presence of nonresponse bias is between respondents and nonrespondents in the second phase of the survey. Next, we use sample-wide data for administratively measured outcomes to examine whether the estimates of voucher effects differed between second-phase respondents and nonrespondents. We refer here to “administrative outcomes” as those measured by address history data, and Census data, and unemployment insurance (UI) earnings data. Finally, we present results on the direction and magnitude of bias.

The survey data collection strategy adopted for this evaluation aimed to limit the potential for nonresponse bias. As described in Appendix A, after completion of the first phase of the follow-up survey data collection, we implemented a subsampling procedure to reduce nonresponse bias. Second-stage interviewing efforts were focused on a randomly selected one-half of the cases that had not been completed during the initial interviewing period. By focusing survey resources on a random subsample of cases, we were able to achieve a higher effective response rate (76.7 percent) than if we had used the same resources to continue to work the full survey sample.

Exhibit C.1 depicts the 5,000 individuals randomly selected for the survey out of the 7,258 survey-eligible cases. Note that differences between first-phase respondents and nonrespondents are immaterial because first-phase respondents do not represent any of the 3,877 first-phase nonrespondents. As described in Appendix B.7, the second-phase respondents are weighted to represent the entire second-phase subsample (2R + 2NR), which in turn represent all first-phase nonrespondents (2NIS + 2R + 2NR). Since the second-phase subsample was randomly selected from all 3,877 first-phase nonrespondents, we can assume that it fairly represents this larger group. However, it is not obvious that the second-phase respondents can faithfully represent the entire second-phase subsample.

If nonresponse in the second phase of the survey was not random, but rather was due to differences in observed or unobserved characteristics that also affect outcomes of interest, then survey results will suffer from nonresponse bias.

To examine this possibility, we have used Census and address history data on locational outcomes and administrative data on earnings outcomes, which are available for virtually all sample members, to analyze the extent to which impacts on these outcomes differed between second-phase respondents and nonrespondents. Although this does not provide a definitive test for nonresponse bias on other outcomes, it offers an indication of the potential extent of nonresponse bias in the impact estimates based on survey data.

**Exhibit C.1
Follow-up Survey Design**

Not in second phase subsample (2NIS) n=1,935	Second Phase Non- Respondents (2NR) n=584
	Second Phase Respondents (2R) n=1,358
First Phase Respondents (1R) n=1,123	

We first tested to see whether there were differential response rates between treatment and control groups. Using a probit model, we regressed survey response within the second-phase subsample on assignment group as well as all other covariates included in the study’s impact estimation. Assignment to the treatment group was not significantly related to survey response.¹⁵³

Next, we tested for differences between second-phase respondents and nonrespondents by estimating impacts on nine outcomes for which we had data for the entire subsample. If nonresponse in the second-phase was random, then estimated impacts on these outcomes should have been insignificantly different between the two groups. A separate Bloom adjustment factor was calculated for each group based on the lease-up rates for the two groups. These lease-up rates are shown in the first two columns of Exhibit C.2.

¹⁵³ The probit coefficient on the treatment dummy variable was -0.015 , the robust standard error for this coefficient was 0.063 , and the p-value was 0.81 .

Exhibit C.2
Bloom Adjustment Factors for Second-phase Respondents And Non-Respondents

	Treatment group participation rate p	Control group crossover rate c	Participation differential p-c	Bloom adjustment factor 1/(p-c)
Second-phase Respondents (2R)	71.3%	47.8%	23.5%	4.247
Second-phase Nonrespondents (2NR)	65.1%	38.3%	26.8%	3.732

Our approach to testing for nonresponse bias in selected administrative outcomes is to treat second-phase respondents and nonrespondents as two mutually exclusive subgroups of the second-phase subsample of 1,942. We then apply the same methodology as used elsewhere in this evaluation for testing for differences in impacts within a subgroup set. Specifically, for each outcome to be tested, a single estimating equation is run, with the following specification:

$$\text{Eq. C.1} \quad O_i = \alpha_0 + \sum \alpha_k X_{ki} + \beta T_i + \gamma N_i + \delta N_i T_i + \varepsilon_i$$

where O_i is the outcome at follow-up for survey sample member i ; X_{ki} is a set of k baseline characteristics of sample member i ; T_i is a dichotomous variable equal to 1 if sample member i belongs to the treatment group and zero if s/he is a control; N_i is a dichotomous variable equal to 1 if the sample member i is a survey nonrespondent and zero if s/he is a respondent; ε_i is a random error term; and α_0 , α_k , β , γ , and δ are coefficients to be estimated. Under this specification, β is the ITT treatment effect for subsample respondents, and $\beta + \delta$ is the ITT treatment effect for subsample nonrespondents. The statistical significance of the coefficient δ indicates whether impacts are significantly different between respondents and nonrespondents.

For each of five locational outcomes and four earnings outcomes tested for nonresponse bias, Exhibit C.3 shows the control mean of the outcome variable, the point estimate (and standard error) of the ITT treatment effect for respondents and nonrespondents, and the point estimate (and standard error) of the difference in effects.¹⁵⁴

The respondents and nonrespondents have significant differences in ITT impacts on 2 out of the 9 outcomes and significant differences in TOT impacts on 3 out of 9 outcomes. This number of differences is more than would be expected by chance alone and indicates that some nonresponse bias may be present.

¹⁵⁴ The standard error of the ITT impact for nonrespondents was obtained by estimating a separate equation, computationally equivalent to Eq. C.1, that included a dummy variable and treatment interaction term for respondents (R_i and $R_i T_i$) rather than for nonrespondents.

Exhibit C.3
Differences between Second-Phase Respondents and Non-Respondents

Outcome	Full Sub-sample (n=1942)	Second-Phase Respondents (n=1358)		Second-Phase Non-Respondents (n=584)		Second-Phase Respondents – Non-Respondents	
	Control Mean	ITT Impact	TOT Impact	ITT Impact	TOT Impact	ITT Impact	TOT Impact
Locational outcomes (address history and Census data)^a							
Poverty rate	27.62	-1.920 *** (0.704)	-8.152 *** (2.988)	-1.144 (1.073)	-4.268 (4.003)	-0.776 (1.278)	-3.884 (4.995)
Employment rate	87.27	0.573 * (0.337)	2.435 * (1.429)	0.309 (0.513)	1.152 (1.914)	0.265 (0.611)	1.283 (2.389)
Educational attainment	83.97	0.779 (0.551)	3.308 (2.341)	1.320 (0.840)	4.927 (3.136)	-0.541 (1.001)	-1.619 (3.913)
Youth idleness	9.43	-0.145 (0.363)	-0.617 (1.541)	-0.564 (0.553)	-2.103 (2.064)	0.418 (0.659)	1.486 (2.576)
Welfare concentration	10.10	-0.816 ** (0.345)	-3.463 ** (1.466)	-0.173 (0.526)	-0.647 (1.963)	-0.642 (0.627)	-2.816 (2.450)
Employment and earnings outcomes (UI data)^b							
Number of quarters employed in half-year 7	0.962	-0.123 ** (0.048)	-0.522 ** (0.205)	0.074 (0.073)	0.275 (0.273)	-0.197 ** (0.087)	-0.797 ** (0.341)
Number of quarters employed during half-years 1 through 7	6.988	-0.548 ** (0.233)	-2.329 ** (0.991)	0.002 (0.354)	0.007 (1.321)	-0.550 (0.422)	-2.336 (1.651)
Amount of earnings in half-year 7	\$3,123	-411 * (230)	-1,747 * (978)	380 (349)	1,418 (1304)	-791 * (417)	-3,165 * (1630)
Total amount of earnings during half-years 1 through 7	\$20,166	-2,745 ** (1121)	-11,658 ** (4759)	399 (1700)	1,490 (6343)	-3,144 (2027)	-13,148 * (7930)

Notes:

ITT = "Intent-to-Treat". Standard errors in parentheses.

*** indicates $p < .01$, ** indicates $p < .05$, * indicates $p < .10$

^a Tract-level outcomes associated with the address at the 18th follow-up quarter. See Chapter 3 text for definitions.

^b See Chapter 4 text for definitions.

The standard error for the ITT difference is the standard error for the coefficient on the treatment-nonresponse interaction term.

The standard error for the TOT difference is equal to the square root of the sum of the respondent TOT SE squared and the nonrespondent TOT SE squared.

The results shown in Exhibit C.3 encouraged us to carefully design the weights through which the second-phase respondents represent the entire second-phase subsample (2R +2NR). One of the dimensions upon which the weighting strata were defined was propensity to respond to the survey. The weighting method is described in Appendix B.7

Exhibit C.4 compares ITT impacts estimated on the 9 administrative outcomes for the actual survey respondents (1R + 2R) with ITT impacts estimated on a sample that includes first-phase respondents and all individuals in the second-phase subsample (1R + 2R + 2NR). The latter sample assumes a 100 percent response rate in the second-phase of the survey. The difference in impacts estimated on these two samples represents the bias that results from nonresponse in the second-phase of the survey.

**Exhibit C.4
Estimated Non-Response Bias**

Outcome	Actual Response in Second-Phase Subsample				100% Response Rate in Second-Phase Subsample			Bias
	Sample Size	Control Mean ^a	ITT Impact		Sample Size	Control Mean ^a	ITT Impact	
Locational outcomes (address history and Census data)^b								
Poverty rate	2,472	27.81 14.61	-1.965 *** (0.516)		3,049	27.66 14.71	-1.899 *** (0.466)	-0.066
Employment rate	2,472	87.09 6.95	0.708 *** (0.249)		3,049	87.25 6.92	0.658 *** (0.224)	0.050
Educational attainment	2,472	83.83 13.36	0.938 ** (0.401)		3,049	84.09 13.20	1.075 *** (0.360)	-0.137
Youth idleness	2,472	9.39 6.81	-0.167 (0.260)		3,049	9.44 7.01	-0.287 (0.241)	0.120
Welfare concentration	2,472	10.13 7.87	-0.871 *** (0.253)		3,049	10.02 7.83	-0.791 *** (0.227)	-0.081
Employment and earnings outcomes (UI data)^c								
Number of quarters employed in half-year 7	2,481	1.01 0.95	-0.076 ** (0.035)		3,065	0.96 0.94	-0.037 (0.032)	-0.039
Number of quarters employed during half-years 1 through 7	2,481	7.17 5.05	-0.359 ** (0.168)		3,065	7.05 5.07	-0.257 * (0.154)	-0.102
Amount of earnings in half-year 7	2,481	\$3,235 4,306	-179 (162)		3,065	\$3,089 4,311	-53 (150)	-125
Total amount of earnings during half-years 1 through 7	2,481	\$20,665 22,970	-1,654 ** (784)		3,065	\$20,415 23,199	-1,233 * (735)	-421

Notes:

ITT = "Intent-to-Treat". 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 Tract-level outcomes associated with the address at the 18th follow-up quarter. See Chapter 3 text for definitions.

^c See Chapter 4 text for definitions.

When compared with the magnitude of the ITT impacts, the bias appears relatively small for 5 out of 9 outcomes. In the remaining four outcomes, the bias is relatively close in size to or larger in size than the ITT impacts.

One way to think about the direction of the bias is that in 7 of the 9 outcomes, ITT impacts are biased *away* from zero because of nonresponse. In all 3 of the outcomes where significant differences were found between second-phase respondents and nonrespondents, ITT impacts are biased away from zero. Interpreting the bias in this way suggests that actual impact magnitudes may be smaller than the survey results presented in this study.

Another way to think about the direction of bias is that in 6 of 9 outcomes, ITT impacts are less favorable to the treatment group than they would have been in the absence of nonresponse bias. Interpreting the bias in this second way suggests that actual impacts of the voucher may be somewhat more favorable to the treatment group than the results presented in this study.

Only ITT impacts are shown in Exhibit C.4. Theoretically it was possible that nonresponse could have biased TOT impacts if lease-up patterns for nonrespondents differed greatly from those of respondents. Exhibit C.5 shows the Bloom adjustment factors for the two samples examined in Exhibit C.4. There is almost no bias from survey nonresponse in the Bloom adjustment factor. Therefore, to the extent that survey nonresponse bias exists in TOT impacts on survey outcomes, it is produced by nonresponse bias in the ITT impacts, rather than by nonresponse bias in the adjustment factor.

Exhibit C.5
Bloom adjustment factors for actual respondents and sample with 100 percent response rate in second-phase

	Treatment group participation rate p	Control group crossover rate c	Participation differential p-c	Bloom adjustment factor 1/(p-c)
Actual Survey Respondents (1R+2R)	71.9%	46.7%	25.1%	3.980
Sample with 100% Response Rate in Second-Phase Subsample (1R+2R+2NR)	70.6%	45.2%	25.4%	3.938

Exhibit D.1: Impacts by Subgroup on "Moved Out of Baseline Census Tract" (Address History Data)

Subgroup	Quarter 16, All Sites				Quarter 18, All Sites Except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Age at baseline								
Less than 24	2,588	0.592 0.492	0.032 * (0.019)	0.118 ** (0.051)	2,505	0.636 0.481	0.041 ** (0.019)	0.134 *** (0.052)
25-34	3,258	0.546 0.498	0.035 ** (0.017)	0.118 ** (0.051)	2,946	0.599 0.490	0.025 (0.018)	0.117 ** (0.053)
35-44	2,015	0.485 0.500	0.010 (0.022)	0.063 (0.074)	1,554	0.562 0.496	-0.006 (0.026)	0.012 (0.078)
45 or older	687	0.421 0.495	0.038 (0.039)	0.143 (0.134)	513	0.474 0.500	0.061 (0.048)	0.239 (0.161)
Race/Ethnicity								
White, Non-Hispanic	1,660	0.422 0.494	0.057 ** (0.024)	0.200 *** (0.070)	1,322	0.495 0.500	0.051 * (0.028)	0.178 ** (0.075)
Black, Non-Hispanic	4,241	0.599 0.490	0.009 (0.015)	0.067 (0.047)	3,883	0.657 0.475	0.009 (0.015)	0.080 * (0.048)
Hispanic	1,815	0.518 0.500	0.066 *** (0.024)	0.183 *** (0.066)	1,630	0.570 0.495	0.045 * (0.025)	0.130 * (0.067)
Education at baseline								
High school diploma	3,372	0.518 0.500	0.011 (0.017)	0.055 (0.054)	2,817	0.583 0.493	0.021 (0.018)	0.080 (0.056)
GED only	1,460	0.560 0.497	0.049 * (0.026)	0.182 ** (0.071)	1,388	0.634 0.482	0.020 (0.026)	0.128 * (0.072)
Neither high school diploma nor GED	3,016	0.562 0.496	0.034 * (0.018)	0.114 ** (0.050)	2,711	0.604 0.489	0.035 * (0.018)	0.124 ** (0.052)
School Enrollment at baseline								
Enrolled in school	1,397	0.542 0.499	0.036 (0.027)	0.106 (0.082)	1,219	0.589 0.492	0.061 ** (0.028)	0.178 ** (0.085)
Not enrolled in school	6,716	0.541 0.498	0.027 ** (0.012)	0.106 *** (0.035)	5,920	0.601 0.490	0.022 * (0.012)	0.101 *** (0.036)
Presence of children at baseline								
Any dependent children ^{b,c}	7,661	0.545 0.498	0.022 ** (0.011)	0.088 *** (0.033)	6,691	0.605 0.489	0.019 * (0.012)	0.090 *** (0.034)
No dependent children ^{b,c}	804	0.458 0.499	0.066 * (0.037)	0.207 * (0.114)	743	0.513 0.501	0.068 * (0.038)	0.216 * (0.119)
Youngest Household Member at baseline								
Youngest household member less than 6	5,532	0.563 0.496	0.032 ** (0.013)	0.119 *** (0.039)	5,107	0.617 0.486	0.030 ** (0.013)	0.123 *** (0.039)
Youngest household member 6-17	2,717	0.492 0.500	0.020 (0.019)	0.089 (0.062)	2,146	0.564 0.496	0.013 (0.022)	0.074 (0.067)
Youngest household member 18 or more	408	0.421 0.495	0.022 (0.055)	0.058 (0.147)	361	0.454 0.499	0.032 (0.061)	0.103 (0.162)
Employment Status at baseline								
Employed	3,777	0.524 0.500	0.011 (0.016)	0.047 (0.048)	3,049	0.590 0.492	0.008 (0.018)	0.050 (0.051)
Not employed, with reservation wage of:								
\$3.00 to \$5.99	254	0.627 0.486	-0.066 (0.070)	-0.147 (0.173)	243	0.653 0.478	-0.054 (0.071)	-0.098 (0.176)
\$6.00 to \$8.99	2,258	0.573 0.495	0.033 * (0.020)	0.121 ** (0.058)	2,131	0.630 0.483	0.018 (0.021)	0.102 * (0.060)
\$9.00 to \$12.99	1,070	0.521 0.500	0.076 ** (0.030)	0.258 *** (0.090)	914	0.581 0.494	0.101 *** (0.032)	0.315 *** (0.092)
\$13.00 to \$15.99	301	0.554 0.499	-0.025 (0.065)	-0.223 (0.295)	230	0.629 0.486	-0.041 (0.076)	-0.298 (0.282)

Exhibit D.1: Impacts by Subgroup on "Moved Out of Baseline Census Tract" (Address History Data)

Subgroup	Quarter 16, All Sites				Quarter 18, All Sites Except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Total not employed	4,413	0.552 0.497	0.038 *** (0.014)	0.144 *** (0.043)	4,117	0.605 0.489	0.035 ** (0.015)	0.139 *** (0.043)
Employment Background at baseline								
Ever employed	7,225	0.533 0.499	0.022 * (0.011)	0.092 *** (0.034)	6,311	0.592 0.492	0.021 * (0.012)	0.096 *** (0.036)
Never employed	1,220	0.560 0.497	0.066 ** (0.029)	0.198 ** (0.083)	1,105	0.624 0.485	0.051 * (0.029)	0.169 ** (0.085)
Job Training Status at baseline								
Enrolled in job training	1,076	0.568 0.496	0.022 (0.030)	0.131 (0.095)	982	0.628 0.484	0.018 (0.031)	0.140 (0.097)
Enrolled in (but yet to start) job training	591	0.556 0.498	0.078 * (0.042)	0.228 * (0.118)	562	0.604 0.490	0.116 *** (0.042)	0.302 ** (0.123)
Not enrolled in job training	6,764	0.532 0.499	0.023 * (0.012)	0.089 ** (0.035)	5,870	0.592 0.492	0.017 (0.013)	0.081 ** (0.036)
Moving for Employment Reasons at baseline								
Desired to move for employment reasons	1,234	0.548 0.498	0.018 (0.028)	0.057 (0.078)	1,117	0.611 0.488	0.015 (0.029)	0.080 (0.082)
Did not desire to move for employment reasons	7,246	0.535 0.499	0.030 *** (0.011)	0.118 *** (0.035)	6,333	0.595 0.491	0.029 ** (0.012)	0.116 *** (0.036)
Housing Status at baseline								
Rents or owns apartment or house^b	4,925	0.521 0.500	0.007 (0.014)	0.032 (0.042)	4,059	0.588 0.492	0.005 (0.015)	0.040 (0.043)
Resides in public or assisted housing^b	1,086	0.608 0.489	0.078 *** (0.029)	0.332 *** (0.094)	1,041	0.667 0.472	0.046 (0.029)	0.230 ** (0.097)
Lives with friends/relatives or in shelter^b	2,394	0.536 0.499	0.047 ** (0.020)	0.166 *** (0.058)	2,279	0.579 0.494	0.049 ** (0.021)	0.173 *** (0.059)
TANF Receipt at baseline								
Not receiving TANF	1,598	0.508 0.500	0.037 (0.025)	0.179 ** (0.087)	1,528	0.570 0.495	0.021 (0.025)	0.106 (0.090)
Receiving TANF, expiring in:								
Less than 6 months	1,012	0.571 0.495	0.029 (0.031)	0.115 (0.098)	960	0.634 0.482	0.028 (0.031)	0.127 (0.100)
6 to 12 months	542	0.618 0.487	0.025 (0.043)	0.144 (0.133)	492	0.668 0.472	-0.013 (0.043)	0.084 (0.131)
12 to 18 months	377	0.637 0.482	-0.041 (0.052)	-0.090 (0.129)	352	0.681 0.468	-0.027 (0.054)	-0.060 (0.132)
More than 18 months	702	0.524 0.500	0.064 * (0.038)	0.297 ** (0.118)	654	0.585 0.493	0.070 * (0.039)	0.311 *** (0.115)
Total receiving TANF	6,564	0.547 0.498	0.024 ** (0.012)	0.091 *** (0.034)	5,608	0.606 0.489	0.027 ** (0.013)	0.106 *** (0.035)

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.

^b An F test on the equality of treatment effects between subgroups through 16 quarters indicates that ITT impacts differ significantly between subgroups at p<.10

^c An F test on the equality of treatment effects between subgroups through 18 quarters indicates that ITT impacts differ significantly between subgroups at p<.10

Exhibit D.2: Impacts by Subgroup on "Number of Moves During Follow-up Period"
(Survey Data)

Subgroup	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Age at baseline				
Less than 24	833	2.47 2.17	-0.32 * (0.17)	-1.32 * (0.68)
25-34	977	1.94 1.77	-0.08 (0.11)	-0.30 (0.41)
35-44	499	1.54 1.68	-0.24 (0.15)	-0.83 (0.52)
45 or older	142	0.98 0.98	0.00 (0.34)	0.03 (3.19)
Race/Ethnicity				
White, Non-Hispanic ^b	384	2.47 2.40	-0.34 (0.23)	-1.07 (0.73)
Black, Non-Hispanic ^b	1,403	1.90 1.63	-0.07 (0.09)	-0.33 (0.42)
Hispanic ^b	522	1.98 2.17	-0.57 *** (0.18)	-2.06 *** (0.65)
Education at baseline				
High school diploma	959	1.91 1.70	-0.15 (0.11)	-0.57 (0.42)
GED only	482	2.36 2.10	-0.33 ** (0.17)	-1.20 ** (0.60)
Neither high school diploma nor GED	844	1.87 2.00	-0.17 (0.13)	-0.73 (0.56)
School Enrollment at baseline				
Enrolled in school	399	2.16 2.02	-0.53 *** (0.20)	-2.03 *** (0.76)
Not enrolled in school	1,956	1.95 1.88	-0.18 ** (0.08)	-0.72 ** (0.33)
Presence of children at baseline				
Any dependent children	2,258	1.98 1.89	-0.21 *** (0.08)	-0.85 *** (0.31)
No dependent children	179	1.97 2.10	-0.13 (0.31)	-0.51 (1.21)
Youngest Household Member at baseline				
Youngest household member less than 6	1,706	2.13 1.94	-0.23 ** (0.09)	-0.89 ** (0.36)
Youngest household member 6-17	746	1.64 1.80	-0.18 (0.12)	-0.75 (0.50)
Youngest household member 18 or more	-	-	-	-
Employment Status at baseline				
Employed ^b	1,020	1.83 1.55	-0.08 (0.11)	-0.33 (0.47)
Not employed, with reservation wage of:				
\$3.00 to \$5.99	73	1.87 1.65	0.02 (0.46)	0.04 (0.97)
\$6.00 to \$8.99	698	2.04 1.81	-0.23 * (0.14)	-1.20 * (0.72)
\$9.00 to \$12.99	314	2.04 1.69	-0.30 * (0.18)	-1.59 * (0.96)
\$13.00 to \$15.99	80	2.39 2.41	0.08 (0.71)	0.66 (5.94)

Exhibit D.2: Impacts by Subgroup on "Number of Moves During Follow-up Period"
(Survey Data)

Subgroup	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Total not employed^b	1,333	2.12 2.13	-0.35 *** (0.11)	-1.28 *** (0.39)
Employment Background at baseline				
Ever employed	2,122	1.97 1.89	-0.19 ** (0.08)	-0.75 ** (0.32)
Never employed	305	1.98 2.01	-0.43 * (0.26)	-1.74 * (1.05)
Job Training Status at baseline				
Enrolled in job training ^b	339	2.27 2.12	-0.67 *** (0.23)	-2.55 *** (0.88)
Enrolled in (but yet to start) job training ^b	186	1.94 1.87	0.05 (0.30)	0.16 (1.02)
Not enrolled in job training ^b	1,905	1.94 1.87	-0.18 ** (0.08)	-0.75 ** (0.35)
Moving for Employment Reasons at baseline				
Desired to move for employment reasons ^b	358	2.27 2.00	-0.65 *** (0.21)	-3.76 *** (1.23)
Did not desire to move for employment reasons ^b	2,085	1.91 1.83	-0.13 (0.08)	-0.49 (0.30)
Housing Status at baseline				
Rents or owns apartment or house ^b	1,303	1.99 1.88	-0.34 *** (0.09)	-1.28 *** (0.36)
Resides in public or assisted housing ^b	373	1.74 1.44	0.04 (0.16)	0.17 (0.70)
Lives with friends/relatives or in shelter ^b	748	2.05 2.14	-0.13 (0.16)	-0.53 (0.69)
TANF Receipt at baseline				
Not receiving TANF	500	2.01 1.67	-0.09 (0.16)	-0.55 (0.98)
Receiving TANF, expiring in:				
Less than 6 months	327	1.94 1.68	-0.28 (0.19)	-1.35 (0.93)
6 to 12 months	181	2.04 1.89	-0.15 (0.30)	-0.59 (1.22)
12 to 18 months	142	2.09 2.34	-0.42 (0.42)	-1.30 (1.29)
More than 18 months	231	2.20 1.96	-0.40 (0.26)	-1.74 (1.15)
Total receiving TANF	1,838	2.00 1.98	-0.28 *** (0.09)	-1.00 *** (0.32)

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.

^b An F test on the equality of treatment effects between subgroups indicates that ITT impacts differ significantly between subgroups at p < .10

Exhibit D.3: Impacts by Subgroup on Poverty Rate in End-Period Census Tract (Address History and Census Data)

Subgroup	Quarter 16, All Sites				Quarter 18, All Sites Except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Age at baseline								
Less than 24 ^{b,c}	2,588	27.68 14.62	-1.87 *** (0.46)	-5.09 *** (1.21)	2,505	27.48 14.86	-1.95 *** (0.48)	-5.22 *** (1.27)
25-34 ^{b,c}	3,258	26.70 14.41	-0.23 (0.39)	-0.74 (1.16)	2,946	26.72 14.65	-0.47 (0.43)	-1.37 (1.25)
35-44 ^{b,c}	2,015	27.60 13.93	-0.24 (0.45)	-0.69 (1.48)	1,554	28.31 14.66	-0.20 (0.57)	-1.17 (1.69)
45 or older ^{b,c}	687	26.02 12.62	0.24 (0.76)	0.63 (2.57)	513	26.91 12.96	0.20 (0.99)	0.25 (3.31)
Race/Ethnicity								
White, Non-Hispanic ^{b,c}	1,660	20.63 11.06	0.06 (0.41)	0.29 (1.18)	1,322	20.74 11.49	0.14 (0.50)	0.25 (1.34)
Black, Non-Hispanic ^{b,c}	4,241	28.02 14.69	-1.11 *** (0.36)	-3.65 *** (1.11)	3,883	27.76 15.07	-1.62 *** (0.39)	-4.86 *** (1.20)
Hispanic ^{b,c}	1,815	30.64 13.90	-0.78 (0.52)	-2.31 (1.44)	1,630	30.82 14.33	-0.58 (0.57)	-2.01 (1.52)
Education at baseline								
High school diploma	3,372	24.94 13.59	-0.51 (0.36)	-1.54 (1.10)	2,817	25.17 14.12	-0.74 * (0.41)	-1.98 (1.21)
GED only	1,460	25.33 14.39	-0.32 (0.61)	-0.98 (1.66)	1,388	24.79 14.19	-0.16 (0.64)	-0.87 (1.73)
Neither high school diploma nor GED	3,016	30.26 14.36	-0.85 ** (0.41)	-2.53 ** (1.17)	2,711	30.55 14.80	-1.08 ** (0.46)	-3.20 ** (1.27)
School Enrollment at baseline								
Enrolled in school ^{b,c}	1,397	25.57 14.36	0.70 (0.61)	2.36 (1.84)	1,219	25.41 14.53	0.81 (0.67)	2.54 (2.00)
Not enrolled in school ^{b,c}	6,716	27.42 14.21	-0.90 *** (0.26)	-2.70 *** (0.77)	5,920	27.63 14.60	-1.14 *** (0.30)	-3.37 *** (0.83)
Presence of children at baseline								
Any dependent children	7,661	26.96 14.23	-0.55 ** (0.25)	-1.59 ** (0.73)	6,691	27.14 14.67	-0.81 *** (0.28)	-2.29 *** (0.80)
No dependent children	804	29.05 14.06	-1.28 (0.78)	-4.81 ** (2.39)	743	28.95 14.15	-0.79 (0.85)	-3.53 (2.56)
Youngest Household Member at baseline								
Youngest household member less than 6	5,532	27.41 14.46	-0.90 *** (0.30)	-2.68 *** (0.88)	5,107	27.39 14.70	-1.11 *** (0.32)	-3.19 *** (0.93)
Youngest household member 6-17	2,717	26.83 13.86	-0.02 (0.39)	-0.03 (1.27)	2,146	27.36 14.55	-0.08 (0.49)	-0.64 (1.49)
Youngest household member 18 or more	408	26.64 14.00	0.28 (1.22)	0.43 (3.18)	361	26.89 14.46	-0.60 (1.42)	-2.03 (3.80)
Employment Status at baseline								
Employed ^c	3,777	26.39 14.08	-0.47 (0.35)	-1.62 (1.02)	3,049	26.66 14.97	-1.02 ** (0.41)	-2.89 ** (1.16)
Not employed, with reservation wage of: \$3.00 to \$5.99	254	30.61 15.56	-0.50 (1.67)	-2.08 (4.13)	243	30.71 15.39	-2.17 (1.71)	-5.93 (4.16)
\$6.00 to \$8.99	2,258	28.57 14.28	-0.97 ** (0.48)	-2.95 ** (1.35)	2,131	28.40 14.43	-0.92 * (0.51)	-3.08 ** (1.43)

Exhibit D.3: Impacts by Subgroup on Poverty Rate in End-Period Census Tract (Address History and Census Data)

Subgroup	Quarter 16, All Sites				Quarter 18, All Sites Except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
\$9.00 to \$12.99	1,070	25.20 13.81	-0.99 (0.68)	-2.42 (1.97)	914	25.30 14.56	-0.94 (0.80)	-2.60 (2.23)
\$13.00 to \$15.99	301	26.34 14.18	-1.32 (1.31)	-5.80 (5.83)	230	27.01 14.95	-1.20 (1.87)	-4.99 (6.41)
Total not employed^c	4,413	27.68 14.24	-0.63 * (0.33)	-1.71 * (0.97)	4,117	27.70 14.32	-0.57 (0.36)	-1.80 * (1.02)
Employment Background at baseline								
Ever employed	7,225	26.83 14.16	-0.71 *** (0.25)	-2.10 *** (0.75)	6,311	26.89 14.56	-0.91 *** (0.29)	-2.67 *** (0.82)
Never employed	1,220	29.04 14.43	-0.10 (0.67)	-0.80 (1.91)	1,105	29.67 14.76	-0.01 (0.74)	-0.57 (2.05)
Job Training Status at baseline								
Enrolled in job training	1,076	27.08 14.62	-0.03 (0.71)	-0.58 (2.16)	982	26.76 15.19	0.10 (0.78)	-0.50 (2.36)
Enrolled in (but yet to start) job training	591	29.46 15.12	-1.31 (1.06)	-4.42 (2.95)	562	29.45 15.29	-1.46 (1.16)	-5.26 (3.20)
Not enrolled in job training	6,764	26.93 14.08	-0.67 ** (0.26)	-1.97 ** (0.77)	5,870	27.17 14.45	-0.90 *** (0.29)	-2.52 *** (0.83)
Moving for Employment Reasons at baseline								
Desired to move for employment reasons	1,234	26.69 14.00	-1.23 ** (0.61)	-3.63 ** (1.64)	1,117	26.38 14.30	-1.00 (0.67)	-3.11 * (1.77)
Did not desire to move for employment reasons	7,246	27.23 14.25	-0.50 * (0.26)	-1.50 * (0.77)	6,333	27.48 14.66	-0.77 *** (0.29)	-2.24 *** (0.84)
Housing Status at baseline								
Rents or owns apartment or house^{b,c}	4,925	26.99 13.78	-0.09 (0.29)	-0.31 (0.87)	4,059	27.51 14.27	-0.28 (0.35)	-0.77 (0.97)
Resides in public or assisted housing^{b,c}	1,086	32.63 16.34	-3.20 *** (0.88)	-11.02 *** (2.78)	1,041	31.99 16.32	-3.07 *** (0.91)	-11.59 *** (2.96)
Lives with friends/relatives or in shelter^{b,c}	2,394	25.03 13.41	-0.45 (0.43)	-1.20 (1.19)	2,279	24.82 13.76	-0.50 (0.45)	-1.36 (1.25)
TANF Receipt at baseline								
Not receiving TANF	1,598	25.11 14.64	-1.54 *** (0.56)	-5.22 *** (1.90)	1,528	24.50 14.69	-1.59 *** (0.59)	-5.06 ** (2.00)
Receiving TANF, expiring in:								
Less than 6 months	1,012	27.30 14.39	-1.04 (0.74)	-2.97 (2.25)	960	27.08 14.44	-1.29 * (0.78)	-3.59 (2.39)
6 to 12 months	542	28.61 14.40	-2.01 ** (1.01)	-6.40 ** (3.19)	492	28.38 14.94	-1.26 (1.14)	-5.06 (3.33)
12 to 18 months	377	28.18 13.97	0.22 (1.26)	0.46 (3.09)	352	28.40 13.98	0.09 (1.34)	-0.31 (3.31)
More than 18 months	702	27.36 13.50	-1.28 (0.87)	-4.60 * (2.64)	654	27.13 13.60	-1.37 (0.97)	-4.53 * (2.75)
Total receiving TANF	6,564	27.73 14.09	-0.36 (0.27)	-1.17 (0.76)	5,608	28.16 14.55	-0.57 * (0.31)	-1.79 ** (0.83)

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.

^b An F test on the equality of treatment effects between subgroups through 16 quarters indicates that ITT impacts differ significantly between subgroups at p<.10

^c An F test on the equality of treatment effects between subgroups through 18 quarters indicates that ITT impacts differ significantly between subgroups at p<.10

Exhibit D.4: Impacts on Total Household Size, by Subgroup

Subgroup	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Age at baseline				
Less than 24	841	4.32 1.52	-0.12 (0.10)	-0.50 (0.42)
25-34	988	4.46 1.73	-0.17 * (0.09)	-0.65 * (0.34)
35-44	508	4.32 2.15	-0.14 (0.14)	-0.47 (0.47)
45 or older	143	3.78 1.96	-0.46 (0.35)	-4.30 (3.25)
Race/Ethnicity				
White, Non-Hispanic	387	4.11 1.69	-0.14 (0.14)	-0.46 (0.44)
Black, Non-Hispanic	1,418	4.13 1.61	-0.14 * (0.07)	-0.68 * (0.35)
Hispanic	531	4.58 1.63	-0.25 * (0.13)	-0.88 * (0.48)
Education at baseline				
High school diploma	971	4.06 1.60	-0.24 *** (0.09)	-0.89 *** (0.33)
GED only	482	4.06 1.55	0.04 (0.13)	0.13 (0.46)
Neither high school diploma nor GED	855	4.80 1.97	-0.26 ** (0.11)	-1.12 ** (0.48)
School Enrollment at baseline				
Enrolled in school	404	4.40 1.71	-0.26 (0.16)	-0.98 (0.60)
Not enrolled in school	1,977	4.31 1.78	-0.20 *** (0.07)	-0.79 *** (0.27)
Presence of children at baseline				
Any dependent children	2,282	4.34 1.75	-0.18 *** (0.06)	-0.71 *** (0.25)
No dependent children	184	4.35 2.06	-0.31 (0.30)	-1.19 (1.18)
Youngest Household Member at baseline				
Youngest household member less than 6	1,722	4.52 1.75	-0.15 ** (0.07)	-0.60 ** (0.29)
Youngest household member 6-17	759	3.94 1.77	-0.24 ** (0.11)	-0.99 ** (0.45)
Youngest household member 18 or more	-	-	-	-

Exhibit D.4: Impacts on Total Household Size, by Subgroup

Subgroup	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Employment Status at baseline				
Employed	1,030	4.12 1.68	-0.20 ** (0.10)	-0.84 ** (0.40)
Not employed, with reservation wage of:				
\$3.00 to \$5.99	75	4.54 2.45	0.21 (0.64)	0.43 (1.35)
\$6.00 to \$8.99	705	4.40 1.75	-0.07 (0.12)	-0.39 (0.62)
\$9.00 to \$12.99	318	4.30 1.73	-0.01 (0.17)	-0.06 (0.88)
\$13.00 to \$15.99	82	4.25 1.51	-0.28 (0.46)	-2.33 (3.89)
Total not employed	1,351	4.49 1.84	-0.14 * (0.08)	-0.50 * (0.30)
Employment Background at baseline				
Ever employed	2,145	4.26 1.73	-0.17 *** (0.06)	-0.66 *** (0.25)
Never employed	311	4.82 2.00	-0.07 (0.20)	-0.29 (0.82)
Job Training Status at baseline				
Enrolled in job training	344	4.19 1.57	-0.03 (0.18)	-0.10 (0.69)
Enrolled in (but yet to start) job training	187	4.55 1.54	-0.42 * (0.23)	-1.43 * (0.77)
Not enrolled in job training	1,928	4.34 1.83	-0.21 *** (0.07)	-0.86 *** (0.28)
Moving for Employment Reasons at baseline				
Desired to move for employment reasons	363	4.06 1.63	-0.18 (0.17)	-1.02 (0.97)
Did not desire to move for employment reasons	2,108	4.39 1.79	-0.20 *** (0.07)	-0.76 *** (0.25)
Housing Status at baseline				
Rents or owns apartment or house	1,321	4.41 1.89	-0.15 * (0.08)	-0.57 * (0.31)
Resides in public or assisted housing	376	4.18 1.47	-0.02 (0.12)	-0.07 (0.54)
Lives with friends/relatives or in shelter	755	4.26	-0.26 **	-1.06 **

Exhibit D.4: Impacts on Total Household Size, by Subgroup

Subgroup	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
		1.68	(0.12)	(0.50)
TANF Receipt at baseline				
Not receiving TANF	507	3.95	-0.15	-0.95
		1.45	(0.11)	(0.71)
Receiving TANF, expiring in:				
Less than 6 months	330	4.22	-0.25	-1.20
		1.69	(0.16)	(0.76)
6 to 12 months	183	4.56	-0.01	-0.06
		2.04	(0.30)	(1.21)
12 to 18 months	142	4.14	0.01	0.03
		1.62	(0.28)	(0.86)
More than 18 months	233	4.46	-0.46 **	-1.99 **
		1.52	(0.19)	(0.82)
Total receiving TANF	1,858	4.46	-0.19 ***	-0.70 ***
		1.84	(0.07)	(0.26)

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.

^b An F test on the equality of treatment effects between subgroups indicates that ITT impacts differ significantly between subgroups at $p < .10$

Exhibit D.5: Impacts on Whether Household Type Is "single parent with children only", by Subgroup

		Fifth Year, All Sites except Los Angeles			
		Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Subgroup					
Age at baseline					
	Less than 24	841	0.663 0.473	0.074 ** (0.037)	0.306 ** (0.152)
	25-34	970	0.672 0.470	0.032 (0.034)	0.122 (0.128)
	35-44	508	0.597 0.492	0.021 (0.054)	0.072 (0.184)
	45 or older	135	0.329 0.473	0.554 *** (1.093)	5.215 *** (10.279)
Race/Ethnicity					
	White, Non-Hispanic	380	0.535 0.500	-0.031 (0.064)	-0.097 (0.204)
	Black, Non-Hispanic	1,400	0.748 0.434	0.072 *** (0.024)	0.339 *** (0.115)
	Hispanic	519	0.525 0.500	0.048 (0.051)	0.173 (0.184)
Education at baseline					
	High school diploma	961	0.645 0.479	0.073 ** (0.034)	0.276 ** (0.129)
	GED only	477	0.673 0.470	0.044 (0.050)	0.158 (0.181)
	Neither high school diploma nor GED	844	0.601 0.490	0.039 (0.040)	0.166 (0.171)
School Enrollment at baseline					
	Enrolled in school	397	0.610 0.489	0.076 (0.058)	0.289 (0.220)
	Not enrolled in school	1,976	0.644 0.479	0.057 ** (0.025)	0.225 ** (0.099)
Presence of children at baseline					
	Any dependent children	2,281	0.636 0.481	0.064 *** (0.023)	0.257 *** (0.094)
	No dependent children	171	0.618 0.489	0.029 (0.130)	0.114 (0.507)

Exhibit D.5: Impacts on Whether Household Type Is "single parent with children only", by Subgroup

Subgroup	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
<u>Youngest Household Member at baseline</u>				
Youngest household member less than 6	1,721	0.649 0.477	0.070 *** (0.027)	0.277 *** (0.106)
Youngest household member 6-17	754	0.600 0.490	0.035 (0.042)	0.143 (0.171)
Youngest household member 18 or more	-	-	-	-
<u>Employment Status at baseline</u>				
Employed	1,028	0.654 0.476	0.051 (0.035)	0.213 (0.148)
Not employed, with reservation wage of:				
\$3.00 to \$5.99	52	0.636 0.488	0.000 *** (0.000)	0.000 *** (0.000)
\$6.00 to \$8.99	702	0.664 0.473	0.036 (0.044)	0.191 (0.232)
\$9.00 to \$12.99	308	0.664 0.474	0.079 (0.062)	0.419 (0.329)
\$13.00 to \$15.99	62	0.605 0.496	0.823 *** (0.148)	6.897 *** (1.238)
Total not employed	1,349	0.621 0.485	0.051 * (0.030)	0.184 * (0.110)
<u>Employment Background at baseline</u>				
Ever employed	2,140	0.636 0.481	0.055 ** (0.024)	0.220 ** (0.095)
Never employed	300	0.623 0.486	0.086 (0.075)	0.354 (0.309)
<u>Job Training Status at baseline</u>				
Enrolled in job training	334	0.680 0.468	-0.003 (0.064)	-0.011 (0.243)
Enrolled in (but yet to start) job training	173	0.619 0.488	-0.034 (0.110)	-0.114 (0.373)
Not enrolled in job training	1,920	0.628 0.484	0.067 *** (0.025)	0.275 *** (0.104)

Exhibit D.5: Impacts on Whether Household Type Is "single parent with children only", by Subgroup

Subgroup	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Moving for Employment Reasons at baseline				
Desired to move for employment reasons	353	0.627 0.485	0.092 (0.067)	0.531 (0.389)
Did not desire to move for employment reasons	2,107	0.635 0.482	0.062 *** (0.024)	0.236 *** (0.091)
Housing Status at baseline				
Rents or owns apartment or house	1,308	0.598 0.491	0.038 (0.032)	0.142 (0.121)
Resides in public or assisted housing	367	0.803 0.399	0.047 (0.066)	0.201 ** (0.284)
Lives with friends/relatives or in shelter	750	0.626 0.485	0.093 ** (0.041)	0.387 ** (0.171)
TANF Receipt at baseline				
Not receiving TANF	499	0.691 0.463	0.114 ** (0.045)	0.713 ** (0.285)
Receiving TANF, expiring in:				
Less than 6 months	325	0.643 0.481	0.079 (0.055)	0.379 (0.263)
6 to 12 months	168	0.646 0.481	-0.076 (0.614)	-0.309 (2.494)
12 to 18 months	138	0.663 0.476	0.358 ^c	1.103 ^c
More than 18 months	229	0.634 0.484	0.121 (0.186)	0.525 (0.805)
Total receiving TANF	1,851	0.617 0.486	0.052 ** (0.026)	0.189 ** (0.095)

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.

^b An F test on the equality of treatment effects between subgroups indicates that ITT impacts differ significantly between subgroups at p<.10

^c Standard error for this impact cannot be reliably estimated.

Exhibit E.1: Impacts on Number of Quarters Employed, by Subgroup

Subgroup	Number of quarters employed Through Half-year 6, All Sites				Number of quarters employed Through Half-year 7, All Sites Except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Age at baseline								
Less than 24	2,605	6.44 4.13	-0.04 (0.14)	-0.15 (0.33)	2,522	7.40 4.69	-0.02 (0.16)	-0.14 (0.39)
25-34	3,270	6.34 4.45	-0.16 (0.13)	-0.43 (0.33)	2,958	7.21 5.08	-0.19 (0.16)	-0.51 (0.41)
35-44	2,015	5.91 4.82	-0.05 (0.18)	-0.18 (0.49)	1,555	6.33 5.41	-0.03 (0.24)	-0.15 (0.62)
45 or older	683	4.05 4.74	0.37 (0.29)	0.97 (0.80)	510	3.73 5.08	0.33 (0.38)	0.85 (1.06)
Race/Ethnicity								
White, Non-Hispanic	1,681	6.16 4.60	-0.10 (0.19)	-0.31 (0.50)	1,342	6.83 5.08	-0.03 (0.25)	-0.18 (0.61)
Black, Non-Hispanic	4,267	6.30 4.36	0.00 (0.11)	-0.07 (0.29)	3,909	7.06 4.99	-0.02 (0.13)	-0.18 (0.35)
Hispanic	1,792	6.03 4.53	-0.06 (0.18)	-0.10 (0.44)	1,610	6.88 5.13	-0.03 (0.22)	-0.04 (0.52)
Education at baseline								
High school diploma	3,382	7.11 4.43	0.04 (0.13)	0.07 (0.34)	2,828	8.06 5.05	0.05 (0.16)	0.09 (0.42)
GED only	1,475	6.20 4.41	0.02 (0.20)	-0.03 (0.48)	1,404	7.09 5.01	-0.03 (0.24)	-0.19 (0.57)
Neither high school diploma nor GED	3,017	5.12 4.39	-0.21 (0.13)	-0.55 * (0.33)	2,713	5.71 4.94	-0.21 (0.16)	-0.56 (0.40)
School Enrollment at baseline								
Enrolled in school	1,406	6.60 4.27	-0.02 (0.21)	-0.08 (0.55)	1,228	7.60 4.80	-0.11 (0.25)	-0.30 (0.67)
Not enrolled in school	6,733	6.06 4.55	-0.08 (0.09)	-0.24 (0.22)	5,939	6.80 5.15	-0.08 (0.11)	-0.25 (0.27)
Presence of children at baseline								
Any dependent children	7,682	6.20 4.51	-0.06 (0.08)	-0.19 (0.21)	6,713	7.00 5.11	-0.08 (0.10)	-0.26 (0.26)
No dependent children	811	5.21 4.40	-0.12 (0.26)	-0.32 (0.72)	751	5.82 4.95	-0.06 (0.31)	-0.17 (0.86)
Youngest Household Member at baseline								
Youngest household member less than 6	5,529	6.18 4.38	-0.10 (0.10)	-0.29 (0.25)	5,104	7.10 4.97	-0.13 (0.12)	-0.37 (0.30)
Youngest household member 6-17	2,725	5.96 4.78	0.09 (0.15)	0.22 (0.40)	2,155	6.42 5.40	0.06 (0.19)	0.11 (0.50)
Youngest household member 18 or more	410	5.36 4.53	-0.28 (0.43)	-0.77 (1.05)	363	5.70 5.04	0.16 (0.53)	0.30 (1.30)
Employment Status at baseline								
Employed	3,794	8.18 4.07	-0.04 (0.12)	-0.10 (0.31)	3,068	9.30 4.64	-0.13 (0.15)	-0.35 (0.39)
Not employed, with reservation wage of:								
\$3.00 to \$5.99	253	4.84 4.04	0.61 (0.55)	1.54 (1.19)	242	5.77 4.66	0.57 (0.67)	1.44 (1.48)
\$6.00 to \$8.99	2,265	5.51 4.17	-0.04 (0.16)	-0.20 (0.38)	2,138	6.39 4.75	-0.06 (0.19)	-0.28 (0.46)
\$9.00 to \$12.99	1,078	6.16 4.38	-0.12 (0.24)	-0.31 (0.61)	921	6.99 4.95	-0.03 (0.30)	-0.18 (0.74)
\$13.00 to \$15.99	301	5.91	0.26	0.89	231	6.64	-0.38	-1.22

Exhibit E.1: Impacts on Number of Quarters Employed, by Subgroup

Subgroup	Number of quarters employed Through Half-year 6, All Sites				Number of quarters employed Through Half-year 7, All Sites Except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
		4.45	(0.52)	(1.83)		5.00	(0.68)	(2.10)
Total not employed	4,420	4.46	-0.10	-0.31	4,124	5.23	-0.07	-0.27
		4.16	(0.11)	(0.27)		4.74	(0.13)	(0.33)
Employment Background at baseline								
Ever employed	7,253	6.53	-0.07	-0.21	6,341	7.33	-0.09	-0.29
		4.46	(0.09)	(0.23)		5.06	(0.11)	(0.27)
Never employed	1,220	3.53	-0.15	-0.38	1,105	4.27	-0.13	-0.34
		3.97	(0.21)	(0.49)		4.54	(0.25)	(0.60)
Job Training Status at baseline								
Enrolled in job training	1,089	6.32	-0.04	-0.19	995	7.03	0.08	0.08
		4.30	(0.25)	(0.65)		4.88	(0.29)	(0.78)
Enrolled in (but yet to start) job training	593	5.35	-0.58 *	-1.47 **	564	6.13	-0.53	-1.47
		4.25	(0.31)	(0.74)		4.79	(0.37)	(0.90)
Not enrolled in job training	6,777	6.14	-0.05	-0.15	5,885	6.93	-0.09	-0.26
		4.56	(0.09)	(0.23)		5.17	(0.11)	(0.28)
Moving for Employment Reasons at baseline								
Desired to move for employment reasons	1,237	6.58	-0.26	-0.66	1,121	7.51	-0.34	-0.94
		4.37	(0.22)	(0.51)		4.96	(0.26)	(0.62)
Did not desire to move for employment reasons	7,272	6.01	-0.03	-0.12	6,360	6.76	-0.03	-0.13
		4.53	(0.09)	(0.22)		5.13	(0.11)	(0.27)
Housing Status at baseline								
Rents or owns apartment or house	4,932	6.13	0.05	0.11	4,068	6.92	0.00	-0.04
		4.64	(0.11)	(0.27)		5.24	(0.14)	(0.34)
Resides in public or assisted housing	1,091	6.58	-0.38 *	-1.02 *	1,046	7.39	-0.27	-0.91
		4.32	(0.22)	(0.59)		4.93	(0.26)	(0.72)
Lives with friends/relatives or in shelter	2,410	5.83	-0.20	-0.57	2,295	6.60	-0.17	-0.48
		4.29	(0.15)	(0.37)		4.91	(0.18)	(0.44)
TANF Receipt at baseline								
Not receiving TANF^c	1,612	7.64	-0.18	-0.49	1,541	8.64	-0.09	-0.41
		4.18	(0.18)	(0.53)		4.84	(0.21)	(0.65)
Receiving TANF, expiring in:								
Less than 6 months^c	1,016	5.58	0.12	0.18	965	6.29	0.24	0.49
		4.48	(0.24)	(0.61)		5.12	(0.28)	(0.74)
6 to 12 months^c	542	5.58	0.41	0.96	491	6.28	0.71 *	1.57
		4.27	(0.34)	(0.88)		4.75	(0.41)	(1.03)
12 to 18 months^c	378	5.37	0.05	0.04	353	6.24	0.21	0.25
		4.34	(0.43)	(0.91)		5.01	(0.52)	(1.12)
More than 18 months^c	705	5.55	-0.35	-1.06	658	6.44	-0.50	-1.46
		4.34	(0.30)	(0.78)		4.96	(0.36)	(0.93)
Total receiving TANF	6,574	5.67	-0.01	-0.07	5,621	6.32	-0.04	-0.16
		4.51	(0.09)	(0.23)		5.06	(0.11)	(0.27)

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.

^b An F test on the equality of treatment effects between subgroups through 6 half-years indicates that ITT impacts differ significantly between subgroups at p < .10

^c An F test on the equality of treatment effects between subgroups through 7 half-years indicates that ITT impacts differ significantly between subgroups at p < .10

Exhibit E.2: Impacts on Total Earnings, by Subgroup

Subgroup	Total Earnings Through Half-year 6, All Sites				Total Earnings Through Half-year 7, All Sites Except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Age at baseline								
Less than 24	2,605	\$16,479 \$19,353	-\$430 (618)	-\$1,139 (1433)	2,522	\$19,044 \$21,694	-\$379 (725)	-\$1,208 (1728)
25-34	3,270	\$19,192 \$20,925	-\$729 (616)	-\$1,923 (1562)	2,958	\$21,886 \$23,900	-\$964 (749)	-\$2,597 (1909)
35-44	2,015	\$17,940 \$20,495	\$272 (828)	\$521 (2264)	1,555	\$18,864 \$22,616	\$45 (1034)	-\$196 (2672)
45 or older	683	\$12,360 \$19,558	\$1,117 (1181)	\$2,660 (3271)	510	\$11,121 \$19,779	\$1,150 (1550)	\$3,085 (4311)
Race/Ethnicity								
White, Non-Hispanic	1,681	\$18,418 \$20,567	-\$164 (931)	-\$625 (2360)	1,342	\$20,008 \$22,373	\$57 (1125)	-\$108 (2732)
Black, Non-Hispanic	4,267	\$17,779 \$20,739	-\$98 (524)	-\$489 (1361)	3,909	\$19,719 \$23,194	-\$413 (622)	-\$1,450 (1649)
Hispanic	1,792	\$17,571 \$19,698	-\$446 (749)	-\$1,070 (1781)	1,610	\$19,811 \$22,047	-\$69 (934)	-\$292 (2165)
Education at baseline								
High school diploma	3,382	\$22,549 \$22,735	\$396 (676)	\$777 (1767)	2,828	\$25,566 \$25,647	\$228 (849)	\$165 (2173)
GED only	1,475	\$17,703 \$19,698	-\$1,177 (926)	-\$2,942 (2179)	1,404	\$20,358 \$22,507	-\$1,928 * (1059)	-\$4,948 * (2538)
Neither high school diploma nor GED	3,017	\$12,896 \$16,722	-\$827 * (501)	-\$2,033 * (1222)	2,713	\$14,103 \$18,580	-\$815 (609)	-\$2,012 (1488)
School Enrollment at baseline								
Enrolled in school	1,406	\$18,629 \$19,413	\$687 (993)	\$1,780 (2568)	1,228	\$21,531 \$21,995	\$434 (1169)	\$1,013 (3059)
Not enrolled in school	6,733	\$17,609 \$20,672	-\$477 (410)	-\$1,339 (1016)	5,939	\$19,501 \$23,099	-\$633 (500)	-\$1,781 (1227)
Presence of children at baseline								
Any dependent children	7,682	\$18,134 \$20,643	-\$277 (396)	-\$886 (988)	6,713	\$20,313 \$23,188	-\$474 (481)	-\$1,479 (1191)
No dependent children	811	\$12,473 \$16,668	\$269 (1012)	\$743 (2749)	751	\$13,915 \$18,414	\$247 (1181)	\$751 (3235)
Youngest Household Member at baseline								
Youngest household member less than 6	5,529	\$17,125 \$19,689	\$6 (464)	-\$128 (1159)	5,104	\$19,873 \$22,673	-\$358 (551)	-\$1,146 (1388)
Youngest household member 6-17	2,725	\$18,665 \$21,845	-\$325 (655)	-\$1,060 (1754)	2,155	\$19,736 \$23,833	-\$581 (826)	-\$1,728 (2150)
Youngest household member 18 or more	410	\$14,106 \$18,470	-\$2,705 * (1630)	-\$6,898 * (3990)	363	\$14,071 \$19,127	\$250 (1941)	\$209 (4753)
Employment Status at baseline								
Employed	3,794	\$26,270 \$21,999	-\$301 (628)	-\$916 (1611)	3,068	\$29,674 \$24,904	-\$587 (816)	-\$1,689 (2041)
Not employed, with reservation wage of: \$3.00 to \$5.99	253	\$11,000 \$13,269	\$2,325 (1825)	\$5,102 (3901)	242	\$13,627 \$15,980	\$1,538 (2292)	\$3,520 (4934)
\$6.00 to \$8.99	2,265	\$13,301 \$17,375	-\$532 (656)	-\$1,516 (1554)	2,138	\$15,656 \$19,772	-\$596 (757)	-\$1,756 (1841)

Exhibit E.2: Impacts on Total Earnings, by Subgroup

Subgroup	Total Earnings Through Half-year 6, All Sites				Total Earnings Through Half-year 7, All Sites Except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
\$9.00 to \$12.99	1,078	\$17,201	-\$425	-\$1,064	921	\$19,491	\$16	-\$146
		\$18,964	(1073)	(2694)		\$21,520	(1349)	(3338)
\$13.00 to \$15.99	301	\$17,213	\$408	\$640	231	\$18,287	-\$2,211	-\$7,809
		\$22,653	(2818)	(9673)		\$21,779	(2936)	(9107)
Total not employed	4,420	\$10,728	-\$79	-\$391	4,124	\$12,858	-\$245	-\$882
		\$15,932	(449)	(1092)		\$18,453	(520)	(1280)
Employment Background at baseline								
Ever employed	7,253	\$19,260	-\$338	-\$1,018	6,341	\$21,445	-\$501	-\$1,505
		\$20,918	(414)	(1054)		\$23,502	(506)	(1279)
Never employed	1,220	\$7,402	\$240	\$434	1,105	\$9,203	\$30	-\$121
		\$12,178	(781)	(1802)		\$14,343	(852)	(1987)
Job Training Status at baseline								
Enrolled in job training	1,089	\$17,404	\$265	\$484	995	\$19,504	\$162	-\$62
		\$18,814	(1170)	(3000)		\$21,292	(1329)	(3436)
Enrolled in (but yet to start) job training	593	\$12,632	-\$1,727	-\$4,398	564	\$14,972	-\$1,849	-\$4,989
		\$15,839	(1200)	(2773)		\$18,523	(1449)	(3470)
Not enrolled in job training	6,777	\$18,072	-\$312	-\$904	5,885	\$20,185	-\$486	-\$1,375
		\$20,866	(411)	(1036)		\$23,437	(506)	(1260)
Moving for Employment Reasons at baseline								
Desired to move for employment reasons ^{b,c}	1,237	\$19,615	-\$2,439 **	-\$5,852 **	1,121	\$22,173	-\$2,777 **	-\$7,054 **
		\$21,038	(978)	(2280)		\$23,722	(1192)	(2801)
Did not desire to move for employment reasons ^{b,c}	7,272	\$17,192	\$133	\$171	6,360	\$19,159	\$16	-\$146
		\$20,206	(400)	(1018)		\$22,621	(483)	(1219)
Housing Status at baseline								
Rents or owns apartment or house	4,932	\$18,505	\$218	\$338	4,068	\$20,512	\$115	-\$11
		\$21,158	(507)	(1281)		\$23,475	(631)	(1547)
Resides in public or assisted housing	1,091	\$18,014	-\$1,836 *	-\$4,944 **	1,046	\$20,489	-\$1,707	-\$5,281 *
		\$19,093	(960)	(2500)		\$22,175	(1146)	(3093)
Lives with friends/relatives or in shelter	2,410	\$15,576	-\$850	-\$2,137	2,295	\$17,854	-\$1,041	-\$2,656
		\$19,105	(663)	(1625)		\$21,833	(779)	(1946)
TANF Receipt at baseline								
Not receiving TANF	1,612	\$24,992	-\$248	-\$688	1,541	\$27,842	\$303	-\$212
		\$24,545	(1063)	(3129)		\$27,271	(1253)	(3805)
Receiving TANF, expiring in:								
Less than 6 months	1,016	\$15,435	-\$223	-\$1,047	965	\$17,337	-\$332	-\$1,535
		\$19,381	(1058)	(2635)		\$22,067	(1171)	(3023)
6 to 12 months	542	\$15,020	\$573	\$1,086	491	\$17,044	\$1,911	\$4,213
		\$18,376	(1476)	(3725)		\$21,061	(1803)	(4467)
12 to 18 months	378	\$14,278	-\$1,791	-\$4,146	353	\$17,050	-\$1,176	-\$3,232
		\$17,658	(1688)	(3533)		\$20,591	(2077)	(4387)
More than 18 months	705	\$13,931	-\$1,646	-\$4,193	658	\$16,670	-\$2,235	-\$5,748
		\$15,714	(1138)	(2920)		\$18,740	(1393)	(3542)
Total receiving TANF	6,574	\$15,712	-\$229	-\$676	5,621	\$17,297	-\$583	-\$1,550
		\$18,897	(394)	(946)		\$21,018	(475)	(1119)

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.

^b An F test on the equality of treatment effects between subgroups through 6 half-years indicates that ITT impacts differ significantly between subgroups at p<.10

^c An F test on the equality of treatment effects between subgroups through 7 half-years indicates that ITT impacts differ significantly between subgroups at p<.10

Exhibit E.3: Impacts on TANF Cash Benefits, by Subgroup

Subgroup	Sample Size	Number of Quarters Received All Sites Except Los Angeles			Amount Received All Sites Except Los Angeles		
		Control	ITT	TOT	Control	ITT	TOT
		Mean ^a	Impact	Impact	Mean ^a	Impact	Impact
Age at baseline							
Less than 24	2,522	6.26 5.01	0.19 (0.16)	0.45 (0.40)	\$5,878 \$6,769	\$153 (200)	\$317 (484)
25-34	2,958	5.72 5.10	0.30 * (0.15)	0.75 * (0.40)	\$6,287 \$8,290	\$332 (217)	\$873 (565)
35-44	1,555	6.13 5.32	0.43 * (0.22)	1.14 ** (0.58)	\$7,627 \$10,061	\$439 (330)	\$1,148 (865)
45 or older	510	6.15 5.54	0.30 (0.41)	0.95 (1.14)	\$7,196 \$10,815	-\$175 (628)	-\$404 (1720)
Race/Ethnicity							
White, Non-Hispanic	1,342	5.67 4.71	0.05 (0.23)	0.13 (0.57)	\$6,994 \$7,597	-\$34 (346)	-\$78 (862)
Black, Non-Hispanic	3,909	5.36 5.12	0.25 * (0.13)	0.63 * (0.35)	\$3,893 \$5,827	\$135 (123)	\$330 (329)
Hispanic	1,610	7.54 5.05	0.53 ** (0.22)	1.24 ** (0.52)	\$10,201 \$9,330	\$678 * (373)	\$1,548 * (881)
Education at baseline							
High school diploma	2,828	4.97 4.94	0.07 (0.16)	0.17 (0.41)	\$5,066 \$7,184	-\$21 (196)	-\$42 (513)
GED only	1,404	5.64 4.97	0.59 ** (0.23)	1.41 ** (0.57)	\$5,816 \$7,690	\$327 (311)	\$776 (758)
Neither high school diploma nor GED	2,713	7.33 5.15	0.28 * (0.16)	0.67 * (0.40)	\$8,542 \$9,590	\$495 ** (247)	\$1,178 * (611)
School Enrollment at baseline							
Enrolled in school	1,228	6.06 4.85	0.15 (0.24)	0.39 (0.64)	\$6,785 \$7,869	-\$51 (335)	-\$187 (903)
Not enrolled in school	5,939	6.06 5.19	0.27 ** (0.11)	0.66 ** (0.27)	\$6,533 \$8,534	\$342 ** (150)	\$855 ** (375)
Presence of children at baseline							
Any dependent children ^{b,c}	6,713	5.99 5.14	0.36 *** (0.10)	0.89 *** (0.26)	\$6,520 \$8,404	\$383 *** (140)	\$953 *** (354)
No dependent children ^{b,c}	751	6.41 5.18	-0.54 * (0.32)	-1.55 * (0.89)	\$6,309 \$8,524	-\$1,041 ** (430)	-\$2,900 ** (1194)
Youngest Household Member at baseline							
Youngest household member less than 6	5,104	6.16 5.10	0.33 *** (0.12)	0.81 *** (0.30)	\$6,544 \$8,199	\$396 ** (159)	\$983 ** (408)
Youngest household member 6-17	2,155	5.90 5.25	0.25 (0.18)	0.70 (0.49)	\$6,760 \$9,300	-\$56 (265)	-\$121 (698)
Youngest household member 18 or more	363	4.98 5.27	0.27 (0.56)	0.68 (1.38)	\$4,009 \$5,729	\$492 (576)	\$1,244 (1436)
Employment Status at baseline							
Employed	3,068	4.67 4.91	0.23 (0.14)	0.57 (0.37)	\$5,052 \$7,443	\$329 * (198)	\$802 (504)
Not employed, with reservation wage of: \$3.00 to \$5.99	242	8.15 5.24	0.05 (0.62)	0.27 (1.35)	\$10,619 \$9,870	\$239 (1058)	\$649 (2334)
\$6.00 to \$8.99	2,138	6.81 5.06	0.28 (0.19)	0.77 * (0.46)	\$6,970 \$8,090	\$254 (248)	\$645 (611)
\$9.00 to \$12.99	921	5.52 4.84	0.40 (0.27)	0.91 (0.68)	\$5,002 \$7,193	\$110 (329)	\$303 (828)
\$13.00 to \$15.99	231	5.87 5.08	0.34 (0.65)	1.07 (2.01)	\$4,896 \$6,665	\$131 (800)	\$256 (2564)

Exhibit E.3: Impacts on TANF Cash Benefits, by Subgroup

Subgroup	Sample Size	Number of Quarters Received All Sites Except Los Angeles			Amount Received All Sites Except Los Angeles		
		Control	ITT	TOT	Control	ITT	TOT
		Mean ^a	Impact	Impact	Mean ^a	Impact	Impact
Total not employed	4,124	7.03	0.33 **	0.83 **	\$7,729	\$241	\$627
		5.11	(0.13)	(0.33)	\$9,070	(187)	(467)
Employment Background at baseline							
Ever employed	6,341	5.68	0.29 ***	0.72 ***	\$5,951	\$284 **	\$709 *
		5.07	(0.11)	(0.27)	\$7,817	(141)	(363)
Never employed	1,105	8.11	0.10	0.28	\$9,637	\$72	\$217
		5.13	(0.26)	(0.61)	\$10,745	(392)	(930)
Job Training Status at baseline							
Enrolled in job training	995	6.26	0.43	1.11	\$6,754	\$590	\$1,486
		4.81	(0.27)	(0.72)	\$7,862	(379)	(1005)
Enrolled in (but yet to start) job training ^c	564	7.43	0.79 **	1.85 **	\$8,659	\$244	\$444
		5.06	(0.35)	(0.83)	\$9,558	(558)	(1341)
Not enrolled in job training	5,885	5.86	0.25 **	0.64 **	\$6,250	\$267 *	\$691 *
		5.19	(0.11)	(0.28)	\$8,364	(147)	(373)
Moving for Employment Reasons at baseline							
Desired to move for employment reasons	1,121	5.89	0.70 ***	1.65 ***	\$5,682	\$791 **	\$1,890 **
		4.96	(0.26)	(0.61)	\$6,942	(333)	(803)
Did not desire to move for employment reasons	6,360	6.06	0.24 **	0.60 **	\$6,633	\$219	\$546
		5.18	(0.11)	(0.27)	\$8,634	(145)	(373)
Housing Status at baseline							
Rents or owns apartment or house	4,068	6.25	0.28 **	0.66 **	\$7,905	\$425 **	\$1,010 *
		5.16	(0.13)	(0.33)	\$9,579	(209)	(520)
Resides in public or assisted housing	1,046	5.54	0.34	0.84	\$4,033	\$318	\$860
		5.20	(0.26)	(0.72)	\$5,673	(259)	(719)
Lives with friends/relatives or in shelter	2,295	5.87	0.33 *	0.85 *	\$5,111	\$115	\$348
		5.06	(0.18)	(0.45)	\$6,533	(192)	(487)
TANF Receipt at baseline							
Not receiving TANF	1,541	2.68	0.29	0.92	\$1,893	\$92	\$313
		3.84	(0.18)	(0.56)	\$3,677	(156)	(482)
Receiving TANF, expiring in: Less than 6 months	965	5.97	0.51 *	1.51 **	\$4,450	\$419	\$1,266
		5.05	(0.29)	(0.76)	\$6,205	(299)	(788)
6 to 12 months	491	7.02	0.10	0.15	\$7,131	\$147	\$310
		4.96	(0.40)	(1.03)	\$7,405	(515)	(1344)
12 to 18 months	353	7.26	0.04	0.20	\$8,295	-\$11	\$86
		5.03	(0.51)	(1.10)	\$8,521	(778)	(1683)
More than 18 months	658	6.90	0.57	1.48	\$7,360	\$747	\$2,022
		4.76	(0.35)	(0.91)	\$7,788	(499)	(1300)
Total receiving TANF	5,621	7.11	0.32 ***	0.77 ***	\$7,953	\$365 **	\$875 **
		5.03	(0.12)	(0.28)	\$8,946	(169)	(406)

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.

^b An F test on the equality of treatment effects between subgroups indicates that ITT impacts on number of quarters received differ significantly between subgroups at $p < .10$

^c An F test on the equality of treatment effects between subgroups indicates that ITT impacts on amount received differ significantly between subgroups at $p < .10$

Exhibit E.4: Impacts on Food Stamp Benefits, by Subgroup

Subgroup	Sample Size	Number of Quarters Received			Amount Received		
		All Sites Except Fresno and Los Angeles			All Sites Except Fresno and Los Angeles		
		Control Mean ^a	ITT Impact	TOT Impact	Control Mean ^a	ITT Impact	TOT Impact
Age at baseline							
Less than 24	1,811	9.43 5.10	0.32 (0.20)	0.97 * (0.51)	\$7,923 \$5,881	\$325 (212)	\$945 * (544)
25-34	2,023	9.11 5.24	0.36 * (0.19)	0.89 * (0.51)	\$8,205 \$6,681	\$402 * (222)	\$989 * (598)
35-44	861	8.77 5.19	0.35 (0.31)	0.93 (0.97)	\$6,763 \$6,005	\$492 (333)	\$1,532 (1050)
45 or older	312	8.92 5.38	0.25 (0.57)	1.29 (2.03)	\$5,606 \$5,223	-\$75 (537)	-\$19 (1892)
Race/Ethnicity							
White, Non-Hispanic	1,011	9.23 4.65	0.35 (0.29)	1.04 (0.78)	\$6,389 \$4,759	\$162 (264)	\$458 (721)
Black, Non-Hispanic	3,419	9.01 5.40	0.39 *** (0.14)	1.06 *** (0.39)	\$8,048 \$6,607	\$470 *** (164)	\$1,270 *** (441)
Hispanic	312	10.04 4.80	1.06 ** (0.51)	3.28 ** (1.52)	\$8,496 \$6,291	\$439 (656)	\$1,621 (1964)
Education at baseline							
High school diploma	1,935	8.16 5.39	0.37 * (0.21)	1.05 * (0.60)	\$6,230 \$5,575	\$386 * (214)	\$1,078 * (603)
GED only	1,065	9.42 4.90	0.68 *** (0.26)	1.65 ** (0.65)	\$7,623 \$5,744	\$611 ** (285)	\$1,501 ** (710)
Neither high school diploma nor GED	1,504	10.10 4.92	0.31 (0.20)	0.92 * (0.56)	\$9,349 \$6,637	\$208 (253)	\$625 (692)
School Enrollment at baseline							
Enrolled in school	769	9.41 4.70	-0.07 (0.32)	-0.23 (0.84)	\$7,563 \$5,599	\$298 (346)	\$729 (891)
Not enrolled in school	3,906	9.15 5.25	0.42 *** (0.14)	1.18 *** (0.38)	\$7,726 \$6,266	\$374 ** (151)	\$1,036 ** (414)
Presence of children at baseline							
Any dependent children ^{b,c}	4,379	9.10 5.21	0.45 *** (0.13)	1.22 *** (0.35)	\$7,787 \$6,307	\$465 *** (144)	\$1,257 *** (387)
No dependent children ^{b,c}	558	9.63 4.99	-0.40 (0.39)	-1.08 (1.27)	\$7,051 \$5,580	\$12 (390)	\$49 (1252)
Youngest Household Member at baseline							
Youngest household member less than 6	3,437	9.50 5.10	0.24 * (0.14)	0.71 * (0.39)	\$8,527 \$6,442	\$271 (167)	\$791 * (454)
Youngest household member 6-17	1,314	8.36 5.35	0.56 ** (0.25)	1.66 ** (0.74)	\$6,017 \$5,430	\$481 * (249)	\$1,350 * (746)
Youngest household member 18 or more	305	9.14 5.13	0.96 (0.60)	2.60 (1.63)	\$5,860 \$5,183	\$1,176 ** (583)	\$3,106 * (1578)
Employment Status at baseline							
Employed ^{b,c}	1,895	7.81 5.36	0.32 (0.21)	0.93 (0.63)	\$6,102 \$5,586	\$343 * (208)	\$964 (624)
Not employed, with reservation wage of:							
\$3.00 to \$5.99 ^{b,c}	101	10.88 4.39	-3.44 *** (1.20)	-7.90 *** (2.84)	\$8,920 \$6,214	-\$3,282 * (1838)	-\$7,698 * (4427)
\$6.00 to \$8.99 ^{b,c}	1,380	9.88 4.92	0.54 ** (0.22)	1.40 ** (0.56)	\$8,972 \$6,647	\$436 * (256)	\$1,166 * (658)
\$9.00 to \$12.99 ^{b,c}	733	8.79 5.30	1.03 *** (0.32)	2.72 *** (0.85)	\$7,519 \$6,237	\$1,319 *** (376)	\$3,359 *** (974)

Exhibit E.4: Impacts on Food Stamp Benefits, by Subgroup

Subgroup	Sample Size	Number of Quarters Received			Amount Received		
		All Sites Except Fresno and Los Angeles			All Sites Except Fresno and Los Angeles		
		Control Mean ^a	ITT Impact	TOT Impact	Control Mean ^a	ITT Impact	TOT Impact
\$13.00 to \$15.99 ^{b,c}	176	8.90 5.36	1.68 ** (0.78)	5.28 ** (2.41)	\$7,323 \$5,801	\$1,764 * (911)	\$5,527 ** (2788)
Total not employed	2,780	10.00 4.89	0.42 *** (0.16)	1.15 *** (0.40)	\$8,628 \$6,343	\$440 ** (186)	\$1,179 ** (471)
Employment Background at baseline							
Ever employed	4,229	8.92 5.25	0.32 ** (0.13)	0.91 ** (0.37)	\$7,404 \$6,145	\$383 *** (145)	\$1,053 *** (404)
Never employed	690	10.68 4.54	0.30 (0.31)	0.89 (0.75)	\$9,468 \$6,332	\$293 (391)	\$870 (940)
Job Training Status at baseline							
Enrolled in job training	615	9.48 4.90	0.81 ** (0.36)	1.91 ** (0.91)	\$7,887 \$5,844	\$1,048 *** (387)	\$2,474 ** (963)
Enrolled in (but yet to start) job training	325	9.44 5.08	1.20 ** (0.49)	3.49 ** (1.41)	\$8,354 \$6,320	\$838 (547)	\$2,548 (1566)
Not enrolled in job training	3,979	9.08 5.25	0.27 * (0.14)	0.79 ** (0.38)	\$7,620 \$6,273	\$310 ** (151)	\$888 ** (418)
Moving for Employment Reasons at baseline							
Desired to move for employment reasons ^c	701	9.05 5.08	0.74 ** (0.34)	2.08 ** (0.95)	\$7,402 \$5,853	\$864 ** (360)	\$2,483 ** (1000)
Did not desire to move for employment reasons ^c	4,252	9.19 5.21	0.28 ** (0.13)	0.80 ** (0.36)	\$7,766 \$6,299	\$317 ** (146)	\$872 ** (396)
Housing Status at baseline							
Rents or owns apartment or house	2,164	8.99 5.06	0.19 (0.19)	0.49 (0.56)	\$7,126 \$5,874	\$330 (203)	\$858 (600)
Resides in public or assisted housing	851	9.36 5.51	0.57 ** (0.25)	1.62 ** (0.66)	\$9,026 \$7,062	\$282 (334)	\$861 (882)
Lives with friends/relatives or in shelter	1,874	9.33 5.16	0.40 * (0.20)	1.11 ** (0.52)	\$7,793 \$6,115	\$430 ** (219)	\$1,208 ** (563)
TANF Receipt at baseline							
Not receiving TANF	1,477	7.26 5.43	0.69 *** (0.23)	2.17 *** (0.74)	\$6,037 \$5,940	\$556 ** (244)	\$1,687 ** (766)
Receiving TANF, expiring in:							
Less than 6 months	772	10.10 4.97	0.47 (0.30)	1.47 * (0.84)	\$8,976 \$6,643	\$243 (357)	\$863 (996)
6 to 12 months	283	9.81 5.05	0.29 (0.55)	0.70 (1.54)	\$8,917 \$6,428	\$689 (666)	\$1,733 (1856)
12 to 18 months	169	9.92 4.93	1.08 (0.69)	2.18 (1.41)	\$8,511 \$6,469	\$1,757 * (899)	\$3,650 * (1845)
More than 18 months	437	10.19 4.85	0.64 (0.41)	1.85 * (1.10)	\$8,949 \$6,186	\$307 (497)	\$812 (1325)
Total receiving TANF	3,179	10.16 4.78	0.35 ** (0.14)	0.94 ** (0.37)	\$8,720 \$6,249	\$374 ** (171)	\$994 ** (436)

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.

^b An F test on the equality of treatment effects between subgroups indicates that ITT impacts on number of quarters received differ significantly between subgroups at p<.10

^c An F test on the equality of treatment effects between subgroups indicates that ITT impacts on amount received differ significantly between subgroups at p<.10

Exhibit E.5: Impacts on TANF and/or Food Stamp Benefits, by Subgroup

Subgroup	Sample Size	TANF or Food Stamps Number of Quarters Received All Sites Except Fresno and Los Angeles			TANF and Food Stamps Amount Received All Sites Except Fresno and Los Angeles		
		Control	ITT	TOT	Control	ITT	TOT
		Mean ^a	Impact	Impact	Mean ^a	Impact	Impact
Age at baseline							
Less than 24	1,811	9.64 5.09	0.32 (0.194)	0.94 * (0.502)	\$11,583 \$8,709	\$493 (334)	\$1,380 (865)
25-34	2,023	9.22 5.21	0.29 (0.189)	0.69 (0.511)	\$11,352 \$9,504	\$647 * (337)	\$1,644 * (925)
35-44	861	8.94 5.16	0.32 (0.304)	0.86 (0.957)	\$9,860 \$9,019	\$999 * (524)	\$3,400 ** (1693)
45 or older	312	9.52 5.17	0.11 (0.561)	0.55 (1.995)	\$8,350 \$7,951	-\$179 (813)	-\$709 (2975)
Race/Ethnicity							
White, Non-Hispanic	1,011	9.35 4.65	0.34 (0.284)	1.01 (0.775)	\$11,861 \$9,614	\$300 (536)	\$879 (1497)
Black, Non-Hispanic	3,419	9.21 5.36	0.33 ** (0.140)	0.87 ** (0.381)	\$10,615 \$8,866	\$693 *** (229)	\$1,858 *** (621)
Hispanic	312	10.18 4.79	1.05 ** (0.504)	3.19 ** (1.501)	\$12,194 \$9,432	\$933 (1000)	\$3,382 (3048)
Education at baseline							
High school diploma	1,935	8.36 5.37	0.31 (0.211)	0.88 (0.601)	\$8,928 \$8,331	\$292 (321)	\$771 (922)
GED only	1,065	9.56 4.87	0.59 ** (0.26)	1.42 ** (0.64)	\$11,308 \$8,846	\$1,140 ** (464)	\$2,842 ** (1178)
Neither high school diploma nor GED	1,504	10.29 4.86	0.28 (0.20)	0.81 (0.54)	\$13,199 \$9,415	\$346 (390)	\$1,051 (1079)
School Enrollment at baseline							
Enrolled in school	769	9.61 4.72	0.00 (0.32)	-0.06 (0.83)	\$10,904 \$8,252	\$1,030 * (542)	\$2,601 * (1429)
Not enrolled in school	3,906	9.32 5.20	0.36 *** (0.13)	1.00 *** (0.37)	\$11,057 \$9,136	\$511 ** (234)	\$1,444 ** (649)
Presence of children at baseline							
Any dependent children ^{b,c}	4,379	9.29 5.18	0.40 *** (0.13)	1.07 *** (0.34)	\$11,053 \$9,122	\$786 *** (221)	\$2,143 *** (604)
No dependent children ^{b,c}	558	9.83 4.92	-0.41 (0.38)	-1.12 (1.24)	\$10,758 \$8,732	-\$265 (617)	-\$876 (2028)
Youngest Household Member at baseline							
Youngest household member less than 6	3,437	9.67 5.07	0.21 (0.14)	0.61 (0.39)	\$12,028 \$9,284	\$542 ** (256)	\$1,514 ** (705)
Youngest household member 6-17	1,314	8.58 5.32	0.53 ** (0.25)	1.53 ** (0.75)	\$8,831 \$8,229	\$767 ** (388)	\$2,299 * (1192)
Youngest household member 18 or more	305	9.39 5.08	0.59 (0.61)	1.57 (1.65)	\$9,074 \$8,210	\$783 (975)	\$1,960 (2646)
Employment Status at baseline							
Employed ^{b,c}	1,895	7.99 5.38	0.28 (0.21)	0.79 (0.63)	\$8,167 \$7,738	\$537 * (298)	\$1,583 * (904)
Not employed, with reservation wage of:							
\$3.00 to \$5.99 ^{b,c}	101	11.13 4.30	-2.86 ** (1.17)	-6.34 ** (2.71)	\$12,541 \$9,560	-\$5,019 * (2496)	-\$11,774 * (6216)
\$6.00 to \$8.99 ^{b,c}	1,380	10.05 4.88	0.48 ** (0.21)	1.24 ** (0.55)	\$12,575 \$9,069	\$802 ** (396)	\$2,182 ** (1032)
\$9.00 to \$12.99 ^{b,c}	733	9.01 5.24	1.01 *** (0.32)	2.63 *** (0.83)	\$10,716 \$8,711	\$1,788 *** (554)	\$4,562 *** (1452)

Exhibit E.5: Impacts on TANF and/or Food Stamp Benefits, by Subgroup

Subgroup	Sample Size	TANF or Food Stamps Number of Quarters Received 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
\$13.00 to \$15.99 ^{b,c}	176	9.10	1.56 **	4.85 **	\$10,775	\$1,482	\$4,636
		5.25	(0.77)	(2.36)	\$9,202	(1412)	(4425)
Total not employed	2,780	10.20	0.35 **	0.95 **	\$12,752	\$765 **	\$2,062 ***
		4.82	(0.15)	(0.39)	\$9,409	(298)	(769)
Employment Background at baseline							
Ever employed	4,229	9.12	0.29 **	0.80 **	\$10,555	\$663 ***	\$1,843 ***
		5.22	(0.13)	(0.37)	\$8,899	(220)	(625)
Never employed	690	10.88	0.21	0.66	\$13,762	\$311	\$1,045
		4.47	(0.31)	(0.74)	\$9,593	(637)	(1567)
Job Training Status at baseline							
Enrolled in job training	615	9.67	0.81 **	1.92 **	\$11,300	\$1,757 ***	\$4,135 ***
		4.85	(0.36)	(0.90)	\$8,403	(593)	(1507)
Enrolled in (but yet to start) job training	325	9.61	1.20 **	3.49 **	\$11,889	\$1,673 **	\$5,068 **
		5.09	(0.47)	(1.36)	\$8,967	(839)	(2452)
Not enrolled in job training	3,979	9.27	0.20	0.58	\$10,906	\$477 **	\$1,394 **
		5.21	(0.13)	(0.37)	\$9,185	(233)	(655)
Moving for Employment Reasons at baseline							
Desired to move for employment reasons ^b	701	9.30	0.72 **	2.02 **	\$10,822	\$1,083 *	\$3,084 *
		5.03	(0.34)	(0.94)	\$8,707	(571)	(1623)
Did not desire to move for employment reasons ^b	4,252	9.37	0.24 *	0.66 *	\$11,063	\$585 ***	\$1,621 ***
		5.18	(0.13)	(0.35)	\$9,139	(223)	(617)
Housing Status at baseline							
Rents or owns apartment or house	2,164	9.19	0.12	0.23	\$10,606	\$824 **	\$2,308 **
		5.01	(0.19)	(0.56)	\$9,037	(329)	(991)
Resides in public or assisted housing	851	9.51	0.56 **	1.52 **	\$11,727	\$405	\$1,159
		5.48	(0.24)	(0.63)	\$9,391	(459)	(1226)
Lives with friends/relatives or in shelter	1,874	9.52	0.37 *	1.05 **	\$11,235	\$491	\$1,394
		5.14	(0.20)	(0.52)	\$8,949	(334)	(869)
TANF Receipt at baseline							
Not receiving TANF	1,477	7.35	0.64 ***	2.04 ***	\$7,574	\$743 **	\$2,312 **
		5.45	(0.23)	(0.74)	\$7,631	(338)	(1070)
Receiving TANF, expiring in:							
Less than 6 months	772	10.23	0.45	1.39 *	\$12,092	\$578	\$2,036
		4.92	(0.29)	(0.81)	\$9,056	(528)	(1496)
6 to 12 months	283	10.04	0.20	0.46	\$12,725	\$404	\$808
		4.95	(0.53)	(1.50)	\$8,702	(947)	(2706)
12 to 18 months	169	10.07	1.11	2.26	\$12,534	\$3,214 **	\$6,757 **
		4.87	(0.67)	(1.39)	\$9,303	(1414)	(2936)
More than 18 months	437	10.36	0.63	1.69	\$14,123	\$548	\$1,341
		4.78	(0.40)	(1.07)	\$9,763	(851)	(2308)
Total receiving TANF	3,179	10.39	0.30 **	0.77 **	\$12,985	\$709 ***	\$1,902 ***
		4.69	(0.14)	(0.36)	\$9,261	(274)	(717)

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.

^b An F test on the equality of treatment effects between subgroups indicates that ITT impacts on number of quarters received differ significantly between subgroups at p<.10

^c An F test on the equality of treatment effects between subgroups indicates that ITT impacts on amount received differ significantly between subgroups at p<.10

Exhibit E.6: Impacts on the Number of Workers in the Household, by Subgroup

Subgroup	All Sites Except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Age at baseline				
Less than 24	830	0.765 0.678	-0.030 (0.046)	-0.125 (0.192)
25-34	962	0.715 0.683	-0.079 * (0.045)	-0.302 * (0.174)
35-44	485	0.762 0.902	-0.040 (0.086)	-0.137 (0.296)
45 or older	139	0.598 0.691	-0.082 (0.176)	-0.769 (1.655)
Race/Ethnicity				
White, Non-Hispanic	376	0.962 0.709	-0.112 (0.082)	-0.357 (0.259)
Black, Non-Hispanic	1,381	0.620 0.665	-0.017 (0.036)	-0.082 (0.168)
Hispanic	520	0.812 0.751	-0.073 (0.068)	-0.262 (0.245)
Education at baseline				
High school diploma	944	0.818 0.690	-0.010 (0.048)	-0.038 (0.182)
GED only	472	0.672 0.688	-0.002 (0.067)	-0.006 (0.239)
Neither high school diploma nor GED	832	0.684 0.778	-0.128 ** (0.056)	-0.551 ** (0.239)
School Enrollment at baseline				
Enrolled in school	396	0.776 0.744	-0.018 (0.080)	-0.070 (0.306)
Not enrolled in school	1,923	0.726 0.727	-0.076 ** (0.034)	-0.302 ** (0.135)
Presence of children at baseline				
Any dependent children	2,226	0.759 0.739	-0.076 ** (0.033)	-0.308 ** (0.133)
No dependent children	177	0.477 0.598	0.089 (0.127)	0.344 (0.492)
Youngest Household Member at baseline				
Youngest household member less than 6	1,686	0.735 0.709	-0.046 (0.037)	-0.182 (0.144)
Youngest household member 6-17	731	0.738 0.785	-0.101 * (0.058)	-0.413 * (0.238)

Exhibit E.6: Impacts on the Number of Workers in the Household, by Subgroup

Subgroup	All Sites Except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Youngest household member 18 or more	-	-	-	-
Employment Status at baseline				
Employed	1,004	0.888 0.691	-0.054 (0.050)	-0.228 (0.210)
Not employed, with reservation wage of:				
\$3.00 to \$5.99	73	1.058 1.073	-0.033 (0.556)	-0.069 (1.169)
\$6.00 to \$8.99	685	0.710 0.737	-0.090 (0.061)	-0.479 (0.325)
\$9.00 to \$12.99	313	0.720 0.734	0.061 (0.076)	0.323 (0.402)
\$13.00 to \$15.99	79	0.605 0.599	-0.033 (0.345)	-0.275 (2.889)
Total not employed	1,314	0.645 0.747	-0.056 (0.042)	-0.202 (0.152)
Employment Background at baseline				
Ever employed	2,088	0.773 0.731	-0.072 ** (0.034)	-0.288 ** (0.135)
Never employed	304	0.545 0.718	0.031 (0.090)	0.128 (0.368)
Job Training Status at baseline				
Enrolled in job training	337	0.754 0.701	-0.065 (0.088)	-0.247 (0.335)
Enrolled in (but yet to start) job training	182	0.603 0.721	-0.329 *** (0.118)	-1.113 *** (0.400)
Not enrolled in job training	1,877	0.749 0.740	-0.071 ** (0.036)	-0.290 ** (0.145)
Moving for Employment Reasons at baseline				
Desired to move for employment reasons	349	0.818 0.763	-0.127 (0.079)	-0.734 (0.456)
Did not desire to move for employment reasons	2,058	0.723 0.728	-0.048 (0.033)	-0.184 (0.126)
Housing Status at baseline				
Rents or owns apartment or house	1,291	0.778 0.764	-0.071 (0.043)	-0.267 (0.164)

Exhibit E.6: Impacts on the Number of Workers in the Household, by Subgroup

Subgroup	All Sites Except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Resides in public or assisted housing	371	0.565 0.611	0.006 (0.071)	0.027 (0.304)
Lives with friends/relatives or in shelter	726	0.744 0.714	-0.058 (0.055)	-0.243 (0.227)
TANF Receipt at baseline				
Not receiving TANF	495	0.820 0.676	-0.020 (0.061)	-0.123 (0.384)
Receiving TANF, expiring in:				
Less than 6 months	321	0.694 0.741	0.005 (0.087)	0.023 (0.420)
6 to 12 months	178	0.746 0.774	-0.121 (0.130)	-0.491 (0.530)
12 to 18 months	139	0.621 0.589	0.045 (0.138)	0.138 (0.425)
More than 18 months	230	0.776 0.733	-0.142 (0.100)	-0.617 (0.432)
Total receiving TANF	1,811	0.718 0.744	-0.061 * (0.036)	-0.219 * (0.131)

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 An F test on the equality of treatment effects between subgroups indicates that ITT impacts on number of workers in the household differ significantly between subgroups at $p < .10$

Exhibit E.7: Impacts on Total Earnings, for those who did not desire to move for employment reasons.

Subgroup	All Sites Except Los Angeles				All Sites Except Fresno and Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Age at baseline								
Less than 24	2,035	\$18,839 \$21,515	-\$393 (795)	-\$1,197 (1890)	1,490	\$18,607 \$21,348	-\$138 (923)	-\$802 (2320)
25-34	2,516	\$21,508 \$23,939	-\$584 (813)	-\$1,613 (2101)	1,719	\$21,391 \$23,757	-\$553 (981)	-\$1,698 (2648)
35-44	1,359	\$18,407 \$22,252	\$624 (1112)	\$1,368 (2865)	762	\$17,920 \$22,190	-\$149 (1504)	-\$941 (4615)
45 or older	444	\$10,228 \$18,604	\$1,502 (1621)	\$4,228 (4511)	279	\$8,919 \$17,130	\$3,986 * (2188)	\$14,628 * (7576)
Race/Ethnicity								
White, Non-Hispanic	1,140	\$19,471 \$22,288	\$1,430 (1237)	\$3,192 (3069)	854	\$19,596 \$21,610	\$688 (1376)	\$1,128 (3779)
Black, Non-Hispanic	3,344	\$19,346 \$22,915	-\$154 (669)	-\$655 (1774)	2,941	\$19,402 \$23,031	-\$27 (720)	-\$422 (1945)
Hispanic	1,298	\$19,193 \$21,952	\$277 (1022)	\$512 (2436)	261	\$15,325 \$19,711	-\$296 (2305)	-\$622 (6718)
Education at baseline								
High school diploma	2,369	\$24,894 \$25,628	\$1,199 (934)	\$2,787 (2445)	1,652	\$24,800 \$25,556	\$1,205 (1148)	\$3,087 (3216)
GED only	1,195	\$20,004 \$22,397	-\$1,700 (1137)	-\$4,393 (2710)	905	\$19,736 \$22,257	-\$2,050 (1269)	-\$5,597 * (3186)
Neither high school diploma nor GED	2,318	\$14,014 \$18,538	-\$729 (663)	-\$1,761 (1629)	1,291	\$12,482 \$16,650	-\$525 (838)	-\$1,724 (2271)
School Enrollment at baseline								
Enrolled in school	1,072	\$20,971 \$21,762	\$821 (1254)	\$1,875 (3257)	669	\$20,596 \$21,086	\$520 (1569)	\$1,304 (4031)
Not enrolled in school	5,002	\$19,110 \$22,966	-\$225 (545)	-\$722 (1358)	3,328	\$19,021 \$22,877	-\$3 (675)	-\$411 (1846)
Presence of children at baseline								
Any dependent children	5,718	\$19,818 \$22,990	\$34 (520)	-\$156 (1301)	3,759	\$19,770 \$22,924	-\$110 (646)	-\$658 (1733)
No dependent children	616	\$13,570 \$18,211	-\$121 (1306)	-\$397 (3616)	468	\$12,540 \$16,888	\$456 (1424)	\$818 (4528)
Youngest Household Member at baseline								
Youngest household member less than 6	4,163	\$19,598 \$22,510	-\$168 (613)	-\$581 (1526)	2,825	\$19,532 \$22,323	-\$64 (746)	-\$427 (1969)
Youngest household member 6-17	1,891	\$19,304 \$23,390	\$136 (870)	\$89 (2286)	1,167	\$19,043 \$23,310	-\$770 (1105)	-\$2,923 (3350)
Youngest household member 18 or more	306	\$13,017 \$18,451	\$755 (2183)	\$1,504 (5461)	260	\$12,146 \$18,213	\$1,531 (2456)	\$3,596 (6410)
Employment Status at baseline								
Employed	2,546	\$29,754 \$24,821	-\$123 (909)	-\$377 (2288)	1,600	\$29,328 \$24,804	\$566 (1178)	\$1,182 (3527)
Not employed, with reservation wage of:								
\$3.00 to \$5.99	197	\$14,958 \$16,320	\$1,464 (2720)	\$3,641 (6518)	81	\$16,324 \$17,597	\$5,454 (5901)	\$7,106 (16881)
\$6.00 to \$8.99	1,761	\$15,354 \$19,534	-\$190 (833)	-\$725 (2048)	1,149	\$14,856 \$19,149	-\$595 (1003)	-\$2,025 (2599)
\$9.00 to \$12.99	766	\$19,527 \$21,879	\$1,131 (1527)	\$2,552 (3719)	612	\$19,038 \$21,242	\$283 (1646)	\$621 (4227)
\$13.00 to \$15.99	199	\$17,714 \$20,723	-\$3,137 (3005)	-\$9,103 (8359)	149	\$19,760 \$22,399	-\$6,900 * (3531)	-\$18,671 * (9751)

Exhibit E.7: Impacts on Total Earnings, for those who did not desire to move for employment reasons.

Subgroup	All Sites Except Los Angeles				All Sites Except Fresno and Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Total not employed	3,554	\$12,250 \$17,965	\$154 (554)	\$80 (1384)	2,401	\$12,963 \$18,453	-\$404 (678)	-\$1,436 (1724)
Employment Background at baseline								
Ever employed	5,350	\$20,996 \$23,318	-\$21 (550)	-\$241 (1404)	3,605	\$20,312 \$23,108	\$156 (670)	\$122 (1871)
Never employed	971	\$8,940 \$14,337	\$113 (919)	\$105 (2190)	609	\$10,118 \$14,474	-\$1,363 (1134)	-\$3,758 (2720)
Job Training Status at baseline								
Enrolled in job training	852	\$18,609 \$20,632	\$446 (1413)	\$674 (3689)	532	\$19,103 \$20,904	-\$1,056 (1811)	-\$2,528 (4539)
Enrolled in (but yet to start) job training	484	\$14,445 \$17,130	-\$1,314 (1449)	-\$3,622 (3419)	286	\$16,683 \$17,608	-\$692 (2122)	-\$2,503 (5627)
Not enrolled in job training	4,980	\$19,835 \$23,431	-\$48 (554)	-\$225 (1397)	3,393	\$19,210 \$23,109	\$179 (671)	\$133 (1862)
Moving for Employment Reasons at baseline								
Desired to move for employment reasons	-	-	-	-	-	-	-	-
Did not desire to move for employment reasons	6,360	\$19,159 \$22,621	\$16 (483)	-\$146 (1219)	4,252	\$18,890 \$22,413	-\$28 (590)	-\$405 (1609)
Housing Status at baseline								
Rents or owns apartment or house	3,543	\$20,064 \$23,335	\$626 (677)	\$1,335 (1684)	1,898	\$20,559 \$23,542	\$907 (948)	\$2,082 (2779)
Resides in public or assisted housing	898	\$20,927 \$22,311	-\$2,142 * (1229)	-\$6,231 * (3266)	743	\$19,631 \$21,950	-\$2,069 (1322)	-\$6,097 * (3444)
Lives with friends/relatives or in shelter	1,845	\$16,773 \$21,296	-\$259 (861)	-\$728 (2191)	1,542	\$16,691 \$21,161	-\$141 (957)	-\$357 (2471)
TANF Receipt at baseline								
Not receiving TANF	1,302	\$27,307 \$27,073	\$1,129 (1376)	\$2,225 (4251)	1,259	\$27,461 \$27,098	\$795 (1407)	\$1,032 (4492)
Receiving TANF, expiring in:								
Less than 6 months	792	\$16,851 \$21,700	\$15 (1273)	-\$309 (3365)	641	\$14,353 \$20,117	\$1,181 (1364)	\$2,813 (3819)
6 to 12 months	418	\$17,281 \$21,391	\$2,582 (2049)	\$6,184 (5262)	251	\$16,390 \$22,421	-\$614 (2731)	-\$2,513 (7619)
12 to 18 months	301	\$17,407 \$20,730	-\$883 (2287)	-\$2,945 (5028)	147	\$15,280 \$18,760	-\$2,491 (3354)	-\$5,459 (6616)
More than 18 months	564	\$16,251 \$18,411	-\$3,116 ** (1497)	-\$8,421 ** (3987)	379	\$15,635 \$17,391	-\$3,074 * (1789)	-\$8,707 * (4917)
Total receiving TANF	4,772	\$16,893 \$20,857	-\$377 (512)	-\$1,033 (1217)	2,722	\$14,763 \$18,735	-\$672 (624)	-\$1,930 (1581)

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.

^b An F test on the equality of treatment effects between subgroups indicates that ITT impacts on earnings in 5 site sample differ significantly between subgroups at p<.10

^c An F test on the equality of treatment effects between subgroups indicates that ITT impacts on earnings in 4 site sample differ significantly between subgroups at p<.10

Exhibit E.8: Impacts on TANF and Food Stamp Benefits, for those who did not desire to move for employment reasons.

Subgroup	Sample Size	TANF Amount Received			Food Stamps Amount Received			
		All Sites Except Fresno and Los Angeles			All Sites Except Fresno and Los Angeles			
		Control Mean ^a	ITT Impact	TOT Impact	Control Mean ^a	ITT Impact	TOT Impact	
Age at baseline								
Less than 24	1,490	\$3,506	\$324 *	\$798 *	\$8,004	\$356	\$945	
		\$4,070	(184)	(468)	\$5,956	(234)	(586)	
25-34	1,719	\$3,256	\$114	\$321	\$8,350	\$208	\$483	
		\$4,275	(173)	(488)	\$6,725	(241)	(653)	
35-44	762	\$3,150	\$516 *	\$1,818 *	\$6,816	\$571	\$1,722	
		\$4,704	(293)	(941)	\$6,117	(361)	(1102)	
45 or older	279	\$2,823	-\$312	-\$1,404	\$5,664	-\$199	-\$370	
		\$3,856	(410)	(1520)	\$5,279	(580)	(1979)	
Race/Ethnicity								
White, Non-Hispanic	854	\$5,355	\$178	\$525	\$6,432	\$84	\$215	
		\$5,998	(371)	(1065)	\$4,834	(291)	(804)	
Black, Non-Hispanic	2,941	\$2,543	\$186 *	\$487 *	\$8,087	\$414 **	\$1,119 **	
		\$3,115	(100)	(275)	\$6,669	(177)	(476)	
Hispanic	261	\$3,908	\$486	\$1,712	\$8,710	-\$69	\$79	
		\$4,833	(559)	(1672)	\$6,490	(753)	(2196)	
Education at baseline								
High school diploma ^b	1,652	\$2,660	-\$45	-\$168	\$6,255	\$372	\$1,023	
		\$3,952	(161)	(469)	\$5,609	(232)	(651)	
GED only ^b	905	\$3,723	\$603 **	\$1,538 **	\$7,727	\$540 *	\$1,328 *	
		\$4,797	(276)	(715)	\$5,841	(312)	(783)	
Neither high school diploma nor GED ^b	1,291	\$3,797	\$124	\$382	\$9,404	\$136	\$428	
		\$4,282	(206)	(575)	\$6,754	(274)	(746)	
School Enrollment at baseline								
Enrolled in school ^b	669	\$3,297	\$611 **	\$1,536 **	\$7,495	\$417	\$1,050	
		\$3,933	(290)	(775)	\$5,391	(372)	(953)	
Not enrolled in school ^b	3,328	\$3,305	\$132	\$390	\$7,787	\$230	\$632	
		\$4,319	(124)	(351)	\$6,389	(165)	(450)	
Presence of children at baseline								
Any dependent children ^c	3,759	\$3,273	\$257 **	\$708 **	\$7,844	\$397 **	\$1,064 **	
		\$4,257	(116)	(322)	\$6,369	(156)	(419)	
No dependent children ^c	468	\$3,515	-\$122	-\$394	\$7,114	-\$293	-\$841	
		\$4,321	(364)	(1211)	\$5,680	(415)	(1323)	
Youngest Household Member at baseline								
Youngest household member less than 6	2,825	\$3,500	\$255 *	\$645 *	\$8,697	\$211	\$589	
		\$4,315	(138)	(373)	\$6,546	(185)	(487)	
Youngest household member 6-17	1,167	\$2,793	\$201	\$732	\$6,002	\$363	\$1,037	
		\$3,984	(201)	(647)	\$5,376	(266)	(808)	
Youngest household member 18 or more	260	\$3,397	-\$549	-\$1,476	\$6,029	\$843	\$2,252	
		\$4,576	(590)	(1573)	\$5,443	(680)	(1797)	
Employment Status at baseline								
Employed ^c	1,600	\$1,895	\$245 *	\$785 *	\$6,075	\$180	\$469	
		\$3,000	(144)	(443)	\$5,668	(228)	(681)	
Not employed, with reservation wage of: \$3.00 to \$5.99 ^c	81	\$3,987	-\$1,227	-\$1,856	\$8,361	-\$1,557	-\$4,109	

Exhibit E.8: Impacts on TANF and Food Stamp Benefits, for those who did not desire to move for employment reasons.

Subgroup	Sample Size	TANF Amount Received			Food Stamps Amount Received		
		All Sites Except Fresno and Los Angeles			All Sites Except Fresno and Los Angeles		
		Control Mean ^a	ITT Impact	TOT Impact	Control Mean ^a	ITT Impact	TOT Impact
		\$5,271	(1377)	(4338)	\$6,624	(1998)	(5295)
\$6.00 to \$8.99 ^c	1,149	\$3,617	\$301	\$860	\$9,191	\$399	\$1,073
		\$3,797	(207)	(552)	\$6,745	(283)	(729)
\$9.00 to \$12.99 ^c	612	\$3,145	\$553 **	\$1,416 **	\$7,389	\$1,482 ***	\$3,762 ***
		\$3,611	(273)	(715)	\$6,292	(423)	(1079)
\$13.00 to \$15.99 ^c	149	\$3,646	-\$785	-\$2,186	\$7,488	\$1,211	\$3,280
		\$4,710	(740)	(2070)	\$5,857	(1031)	(2765)
Total not employed	2,401	\$4,168	\$296 *	\$814 *	\$8,709	\$426 **	\$1,145 **
		\$4,779	(163)	(431)	\$6,400	(201)	(510)
Employment Background at baseline							
Ever employed	3,605	\$3,121	\$244 **	\$680 **	\$7,475	\$290 *	\$778 *
		\$4,138	(116)	(335)	\$6,258	(158)	(439)
Never employed	609	\$4,322	\$23	\$171	\$9,541	\$238	\$759
		\$4,812	(334)	(854)	\$6,255	(417)	(1019)
Job Training Status at baseline							
Enrolled in job training	532	\$3,479	\$486	\$1,058	\$7,974	\$1,192 ***	\$2,847 ***
		\$3,950	(320)	(833)	\$5,837	(423)	(1059)
Enrolled in (but yet to start) job training	286	\$3,345	\$933 **	\$2,490 **	\$8,290	\$1,261 **	\$3,510 **
		\$3,897	(447)	(1227)	\$6,194	(567)	(1488)
Not enrolled in job training	3,393	\$3,271	\$171	\$515	\$7,677	\$172	\$492
		\$4,348	(123)	(354)	\$6,369	(165)	(457)
Moving for Employment Reasons at baseline							
Desired to move for employment reasons	-	-	-	-	-	-	-
Did not desire to move for employment reasons	4,252	\$3,297	\$252 **	\$705 **	\$7,766	\$317 **	\$872 **
		\$4,254	(109)	(308)	\$6,299	(146)	(396)
Housing Status at baseline							
Rents or owns apartment or house	1,898	\$3,450	\$465 **	\$1,334 **	\$7,070	\$255	\$614
		\$4,664	(188)	(572)	\$5,908	(218)	(637)
Resides in public or assisted housing	743	\$2,651	\$102	\$229	\$9,060	\$312	\$923
		\$2,996	(196)	(531)	\$7,149	(356)	(931)
Lives with friends/relatives or in shelter	1,542	\$3,458	\$43	\$147	\$7,999	\$287	\$860
		\$4,274	(173)	(455)	\$6,193	(244)	(628)
TANF Receipt at baseline							
Not receiving TANF	1,259	\$1,497	\$254 *	\$795 *	\$6,156	\$293	\$848
		\$2,461	(145)	(478)	\$6,054	(265)	(846)
Receiving TANF, expiring in: Less than 6 months	641	\$3,172	\$215	\$821	\$8,941	\$217	\$820
		\$3,536	(271)	(786)	\$6,749	(398)	(1111)
6 to 12 months	251	\$3,783	\$147	\$228	\$8,972	\$698	\$1,833
		\$4,153	(441)	(1315)	\$6,466	(736)	(2054)
12 to 18 months	147	\$4,020	\$1,367 *	\$2,789 *	\$8,049	\$2,222 **	\$4,478 **
		\$4,239	(724)	(1480)	\$6,246	(951)	(1890)

Exhibit E.8: Impacts on TANF and Food Stamp Benefits, for those who did not desire to move for employment reasons.

Subgroup	Sample Size	TANF Amount Received			Food Stamps Amount Received		
		All Sites Except Fresno and Los Angeles			All Sites Except Fresno and Los Angeles		
		Control Mean ^a	ITT Impact	TOT Impact	Control Mean ^a	ITT Impact	TOT Impact
More than 18 months	379	\$5,170	\$412	\$989	\$9,023	\$621	\$1,602
		\$5,324	(495)	(1439)	\$6,250	(538)	(1518)
Total receiving TANF	2,722	\$4,285	\$266 *	\$738 *	\$8,774	\$374 **	\$986 **
		\$4,658	(152)	(402)	\$6,313	(185)	(470)

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.

^b An F test on the equality of treatment effects between subgroups indicates that ITT impacts on TANF amount received differ significantly between subgroups at $p < .10$

^c An F test on the equality of treatment effects between subgroups indicates that ITT impacts on food stamp amount received differ significantly between subgroups at $p < .10$

Exhibit F.1: Impacts on cash income below poverty threshold, by Subgroup

Subgroup	Cash income below poverty threshold				Cash income below 75% of poverty threshold			
	Fifth Year, All Sites except Los Angeles				Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Total not employed	1,305	0.889 0.315	0.016 (0.013)	0.057 (0.047)	1,311	0.835 0.372	-0.003 (0.019)	-0.010 (0.069)
Employment Background at baseline								
Ever employed	2,111	0.827 0.378	0.015 (0.016)	0.060 (0.065)	2,113	0.750 0.433	-0.005 (0.021)	-0.021 (0.084)
Never employed	155	0.951 0.217	0.000 (0.000)	0.000 (0.000)	173	0.910 0.287	0.021 (0.018)	0.085 (0.075)
Job Training Status at baseline								
Enrolled in job training ^c	311	0.797 0.404	0.040 (0.040)	0.153 (0.152)	311	0.704 0.458	0.048 (0.060)	0.182 (0.228)
Enrolled in (but yet to start) job training ^c	135	0.905 0.295	0.000 *** (0.000)	0.000 *** (0.000)	151	0.824 0.382	0.000 *** (0.000)	0.000 *** (0.000)
Not enrolled in job training ^c	1,883	0.848 0.359	0.003 (0.015)	0.012 (0.063)	1,888	0.781 0.413	-0.022 (0.021)	-0.088 (0.086)
Moving for Employment Reasons at baseline								
Desired to move for employment reasons	338	0.842 0.366	-0.028 (0.032)	-0.160 (0.187)	344	0.784 0.412	-0.044 (0.043)	-0.252 (0.249)
Did not desire to move for employment reasons	2,068	0.847 0.360	0.013 (0.014)	0.050 (0.055)	2,075	0.774 0.419	-0.007 (0.020)	-0.028 (0.075)
Housing Status at baseline								
Rents or owns apartment or house ^b	1,297	0.850 0.358	-0.015 (0.020)	-0.057 (0.074)	1,297	0.773 0.419	-0.019 (0.026)	-0.070 (0.099)
Resides in public or assisted housing ^b	304	0.865 0.343	0.000 (0.000)	0.000 (0.001)	326	0.786 0.411	0.004 (0.010)	0.018 (0.042)
Lives with friends/relatives or in shelter ^b	727	0.824 0.382	0.033 (0.023)	0.139 (0.094)	727	0.764 0.425	-0.014 (0.028)	-0.059 (0.118)
TANF Receipt at baseline								
Not receiving TANF	476	0.721 0.450	0.043 (0.043)	0.267 (0.270)	478	0.656 0.476	-0.044 (0.051)	-0.275 (0.322)
Receiving TANF, expiring in:								
Less than 6 months	203	0.838 0.369	0.016 (0.093)	0.078 (0.447)	282	0.771 0.422	-0.051 (0.048)	-0.243 (0.232)
6 to 12 months	129	0.827 0.381	0.042 * (0.032)	0.171 * (0.129)	144	0.821 0.385	-0.049 (0.039)	-0.200 (0.157)
12 to 18 months	103	0.812 0.394	0.000 (0.000)	0.000 (0.000)	115	0.737 0.444	0.000 *** (0.000)	0.000 *** (0.000)
More than 18 months	157	0.888 0.317	0.000 (0.000)	0.000 (0.000)	205	0.794 0.406	0.002 (0.050)	0.010 (0.216)
Total receiving TANF	1,787	0.875 0.331	0.008 (0.014)	0.030 (0.051)	1,794	0.805 0.397	0.003 (0.019)	0.012 (0.070)

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.

^b An F test on the equality of treatment effects between subgroups indicates that ITT impacts on cash income below poverty threshold differ significantly between subgroups at p<.10

^c An F test on the equality of treatment effects between subgroups indicates that ITT impacts on cash income below 75% of poverty threshold differ significantly between subgroups at p<.10

Exhibit F.2: Impacts on cash and near-cash income below poverty threshold, by Subgroup

Subgroup	Cash and near-cash income below poverty threshold				Cash and near-cash income below 75% of poverty threshold			
	Fifth Year, All Sites except Los Angeles				Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Age at baseline								
Less than 24	814	0.622 0.485	0.001 (0.040)	0.004 (0.167)	823	0.392 0.489	-0.001 (0.041)	-0.003 (0.169)
25-34	964	0.631 0.483	-0.043 (0.036)	-0.164 (0.137)	967	0.463 0.499	-0.062 (0.038)	-0.236 (0.144)
35-44	487	0.684 0.466	-0.113 ** (0.050)	-0.388 ** (0.173)	494	0.492 0.501	-0.110 ** (0.055)	-0.378 ** (0.190)
45 or older	114	0.770 0.424	0.204 (0.169)	1.922 (1.591)	129	0.599 0.493	-0.278 ** (0.544)	-2.617 ** (5.120)
Race/Ethnicity								
White, Non-Hispanic ^c	367	0.597 0.492	-0.071 (0.061)	-0.225 (0.192)	368	0.485 0.501	-0.099 (0.063)	-0.314 (0.199)
Black, Non-Hispanic ^c	1,399	0.638 0.481	-0.065 ** (0.030)	-0.308 ** (0.141)	1,396	0.439 0.497	-0.064 ** (0.030)	-0.303 ** (0.144)
Hispanic ^c	498	0.671 0.471	0.008 (0.046)	0.029 (0.165)	513	0.447 0.498	0.043 (0.052)	0.153 (0.187)
Education at baseline								
High school diploma	949	0.580 0.494	-0.029 (0.038)	-0.108 (0.141)	942	0.413 0.493	-0.051 (0.038)	-0.190 (0.143)
GED only	464	0.640 0.481	-0.092 * (0.051)	-0.330 * (0.184)	465	0.459 0.499	-0.093 * (0.054)	-0.335 * (0.193)
Neither high school diploma nor GED	831	0.701 0.458	-0.001 (0.036)	-0.006 (0.153)	844	0.473 0.500	-0.006 (0.040)	-0.027 (0.171)
School Enrollment at baseline								
Enrolled in school	386	0.595 0.492	-0.061 (0.059)	-0.231 (0.226)	392	0.370 0.484	-0.058 (0.061)	-0.223 (0.231)
Not enrolled in school	1,944	0.651 0.477	-0.034 (0.024)	-0.136 (0.096)	1,948	0.462 0.499	-0.045 * (0.026)	-0.180 * (0.102)
Presence of children at baseline								
Any dependent children	2,248	0.636 0.481	-0.038 * (0.023)	-0.154 * (0.093)	2,250	0.449 0.498	-0.052 ** (0.024)	-0.208 ** (0.097)
No dependent children	175	0.746 0.438	-0.077 (0.083)	-0.298 (0.323)	173	0.472 0.502	-0.022 (0.113)	-0.085 (0.439)
Youngest Household Member at baseline								
Youngest household member less than 6	1,689	0.657 0.475	-0.054 ** (0.026)	-0.214 ** (0.103)	1,700	0.443 0.497	-0.052 * (0.028)	-0.203 * (0.109)
Youngest household member 6-17	743	0.623 0.485	-0.017 (0.042)	-0.070 (0.171)	743	0.474 0.500	-0.058 (0.044)	-0.237 (0.179)
Youngest household member 18 or more	-	-	-	-	-	-	-	-
Employment Status at baseline								
Employed	1,014	0.564 0.496	-0.006 (0.036)	-0.024 (0.151)	1,014	0.443 0.497	-0.059 (0.036)	-0.248 (0.153)
Not employed, with reservation wage of: \$3.00 to \$5.99	55	0.668 0.478	-1.000 *** (0.000)	-2.103 *** (0.000)	59	0.509 0.507	0.000 (0.000)	0.000 (0.000)
\$6.00 to \$8.99	676	0.663 0.473	-0.019 (0.042)	-0.100 (0.223)	676	0.463 0.499	-0.025 (0.045)	-0.134 (0.238)
\$9.00 to \$12.99	297	0.672 0.471	-0.145 ** (0.068)	-0.768 ** (0.360)	306	0.456 0.500	-0.103 (0.069)	-0.547 (0.365)
\$13.00 to \$15.99	62	0.659 0.480	-0.851 *** (2.365)	-7.136 *** (19.829)	69	0.363 0.487	0.399 (0.387)	3.342 (3.248)

Exhibit F.2: Impacts on cash and near-cash income below poverty threshold, by Subgroup

Subgroup	Cash and near-cash income below poverty threshold				Cash and near-cash income below 75% of poverty threshold			
	Fifth Year, All Sites except Los Angeles				Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Total not employed	1,324	0.693 0.462	-0.048 * (0.029)	-0.176 * (0.106)	1,326	0.459 0.499	-0.043 (0.031)	-0.154 (0.114)
Employment Background at baseline								
Ever employed ^b	2,111	0.617 0.486	-0.015 (0.024)	-0.060 (0.095)	2,113	0.447 0.497	-0.051 ** (0.025)	-0.202 ** (0.098)
Never employed ^b	300	0.810 0.393	-0.117 * (0.062)	-0.480 * (0.255)	304	0.482 0.501	-0.016 (0.068)	-0.066 (0.280)
Job Training Status at baseline								
Enrolled in job training	334	0.584 0.494	0.007 (0.065)	0.028 (0.249)	334	0.335 0.473	0.030 (0.063)	0.116 (0.242)
Enrolled in (but yet to start) job training	164	0.659 0.476	0.109 (0.120)	0.368 (0.405)	175	0.447 0.500	-0.007 (0.104)	-0.023 (0.353)
Not enrolled in job training	1,897	0.654 0.476	-0.052 ** (0.025)	-0.214 ** (0.102)	1,903	0.472 0.499	-0.061 ** (0.026)	-0.250 ** (0.108)
Moving for Employment Reasons at baseline								
Desired to move for employment reasons	347	0.616 0.488	0.007 (0.062)	0.039 (0.360)	350	0.468 0.500	-0.083 (0.067)	-0.479 (0.384)
Did not desire to move for employment reasons	2,073	0.653 0.476	-0.057 ** (0.024)	-0.215 ** (0.090)	2,075	0.451 0.498	-0.051 ** (0.025)	-0.196 ** (0.095)
Housing Status at baseline								
Rents or owns apartment or house	1,296	0.635 0.482	-0.056 * (0.031)	-0.211 * (0.118)	1,294	0.447 0.498	-0.056 * (0.032)	-0.212 * (0.122)
Resides in public or assisted housing	349	0.634 0.483	0.003 (0.008)	0.015 (0.036)	358	0.411 0.493	0.047 (0.068)	0.202 (0.292)
Lives with friends/relatives or in shelter	745	0.670 0.471	-0.036 (0.040)	-0.152 (0.164)	748	0.494 0.501	-0.069 (0.044)	-0.288 (0.183)
TANF Receipt at baseline								
Not receiving TANF	492	0.552 0.498	-0.037 (0.054)	-0.234 (0.335)	492	0.395 0.490	-0.029 (0.052)	-0.179 (0.326)
Receiving TANF, expiring in:								
Less than 6 months	314	0.690 0.464	-0.131 ** (0.058)	-0.630 ** (0.276)	321	0.491 0.501	-0.090 (0.070)	-0.432 (0.336)
6 to 12 months	171	0.637 0.484	0.022 (0.099)	0.091 (0.404)	171	0.503 0.503	-0.029 (0.108)	-0.120 (0.440)
12 to 18 months	130	0.598 0.494	0.046 (0.149)	0.143 (0.458)	126	0.281 0.453	0.173 ** (0.196)	0.533 ** (0.605)
More than 18 months	211	0.649 0.479	-0.101 (0.127)	-0.438 (0.550)	225	0.478 0.502	-0.154 * (0.089)	-0.668 * (0.386)
Total receiving TANF	1,829	0.667 0.471	-0.051 ** (0.025)	-0.186 ** (0.090)	1,829	0.463 0.499	-0.050 * (0.027)	-0.182 * (0.096)

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.

^b An F test on the equality of treatment effects between subgroups indicates that ITT impacts on cash and near-cash income below poverty threshold differ significantly between subgroups at p<.10

^c An F test on the equality of treatment effects between subgroups indicates that ITT impacts on cash and near-cash income below 75% of poverty threshold differ significantly between subgroups at p<.10

Exhibit F.3: Impacts on homelessness and independent housing, by Subgroup

Subgroup	Homelessness during year prior to survey				Independent Housing at time of survey			
	Sample Size	Fifth Year, All Sites except Los Angeles			Sample Size	Fifth Year, All Sites except Los Angeles		
		Control Mean ^a	ITT Impact	TOT Impact		Control Mean ^a	ITT Impact	TOT Impact
Age at baseline								
Less than 24	840	0.268 0.443	-0.070 *** (0.032)	-0.290 *** (0.133)	841	0.843 0.365	0.028 * (0.020)	0.115 * (0.081)
25-34	969	0.253 0.435	-0.086 *** (0.039)	-0.328 *** (0.151)	964	0.842 0.365	0.055 *** (0.036)	0.211 *** (0.138)
35-44	496	0.233 0.424	-0.080 ** (0.034)	-0.274 ** (0.117)	483	0.793 0.406	0.113 *** (0.026)	0.389 *** (0.088)
45 or older	106	0.144 0.353	0.000 *** (0.000)	0.000 *** (0.000)	86	0.826 0.382	0.000 (0.000)	0.000 (0.000)
Race/Ethnicity								
White, Non-Hispanic	375	0.196 0.398	-0.019 (0.032)	-0.061 (0.102)	347	0.890 0.314	0.005 (0.012)	0.017 (0.037)
Black, Non-Hispanic	1,411	0.273 0.446	-0.101 *** (0.024)	-0.478 *** (0.113)	1,396	0.828 0.378	0.060 *** (0.019)	0.284 *** (0.090)
Hispanic	505	0.240 0.428	-0.128 *** (0.043)	-0.459 *** (0.156)	499	0.819 0.386	0.074 * (0.072)	0.268 * (0.257)
Education at baseline								
High school diploma	959	0.216 0.412	-0.042 (0.026)	-0.160 (0.098)	949	0.852 0.356	0.013 (0.020)	0.050 (0.074)
GED only	461	0.279 0.449	-0.120 *** (0.050)	-0.429 *** (0.180)	450	0.824 0.382	0.073 ** (0.037)	0.263 ** (0.133)
Neither high school diploma nor GED	840	0.256 0.437	-0.099 *** (0.026)	-0.427 *** (0.112)	830	0.841 0.366	0.033 * (0.019)	0.143 * (0.083)
School Enrollment at baseline								
Enrolled in school	383	0.216 0.413	-0.031 (0.075)	-0.119 (0.285)	362	0.876 0.330	0.025 (0.021)	0.095 (0.079)
Not enrolled in school	1,967	0.256 0.436	-0.098 *** (0.019)	-0.389 *** (0.076)	1,950	0.825 0.380	0.065 *** (0.017)	0.257 *** (0.067)
Presence of children at baseline								
Any dependent children	2,273	0.242 0.428	-0.078 *** (0.018)	-0.316 *** (0.073)	2,254	0.836 0.370	0.061 *** (0.015)	0.246 *** (0.062)
No dependent children	174	0.309 0.465	0.000 *** (0.000)	0.000 *** (0.000)	144	0.795 0.406	0.000 ** (0.001)	0.002 ** (0.005)
Youngest Household Member at baseline								
Youngest household member less than 6	1,719	0.269 0.444	-0.089 *** (0.021)	-0.352 *** (0.083)	1,721	0.827 0.378	0.052 *** (0.015)	0.205 *** (0.060)
Youngest household member 6-17	751	0.200 0.401	-0.045 * (0.027)	-0.182 * (0.112)	739	0.839 0.368	0.041 (0.026)	0.166 (0.105)
Youngest household member 18 or more	-	-	-	-	-	-	-	-
Employment Status at baseline								
Employed ^b	1,019	0.212 0.409	-0.054 ** (0.026)	-0.226 ** (0.111)	1,022	0.851 0.357	0.029 (0.019)	0.120 (0.081)
Not employed, with reservation wage of:								
\$3.00 to \$5.99	55	0.409 0.499	0.000 (0.000)	0.000 (0.000)	-	-	-	-
\$6.00 to \$8.99	691	0.259 0.438	-0.093 *** (0.033)	-0.494 *** (0.178)	676	0.827 0.379	0.068 ** (0.027)	0.363 ** (0.144)
\$9.00 to \$12.99	303	0.252 0.435	-0.003 *** (0.019)	-0.014 *** (0.100)	298	0.815 0.389	0.025 ** (0.013)	0.131 ** (0.070)
\$13.00 to \$15.99	40	0.221 0.421	0.000 (0.000)	-0.002 (0.000)	-	-	-	-
Total not employed ^b	1,329	0.270 0.444	-0.121 *** (0.025)	-0.440 *** (0.089)	1,312	0.824 0.381	0.083 *** (0.022)	0.302 *** (0.078)

Exhibit F.3: Impacts on homelessness and independent housing, by Subgroup

Subgroup	Homelessness during year prior to survey				Independent Housing at time of survey			
	Fifth Year, All Sites except Los Angeles				Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Employment Background at baseline								
Ever employed	2,136	0.249 0.432	-0.081 *** (0.019)	-0.320 *** (0.075)	2,119	0.835 0.372	0.061 *** (0.016)	0.242 *** (0.063)
Never employed	292	0.227 0.420	-0.102 *** (0.042)	-0.416 *** (0.170)	272	0.830 0.377	0.106 ** (0.047)	0.433 ** (0.194)
Job Training Status at baseline								
Enrolled in job training	321	0.246 0.432	-0.064 (0.045)	-0.244 (0.170)	297	0.832 0.375	0.028 (0.031)	0.107 (0.119)
Enrolled in (but yet to start) job training	162	0.263 0.443	-0.007 (0.131)	-0.022 (0.444)	115	0.839 0.369	0.000 *** (0.000)	0.000 *** (0.000)
Not enrolled in job training	1,915	0.247 0.432	-0.095 *** (0.020)	-0.387 *** (0.083)	1,898	0.831 0.375	0.061 *** (0.017)	0.247 *** (0.070)
Moving for Employment Reasons at baseline								
Desired to move for employment reasons	353	0.269 0.445	-0.153 *** (0.067)	-0.885 *** (0.384)	352	0.813 0.391	-0.003 (0.032)	-0.019 (0.182)
Did not desire to move for employment reasons	2,100	0.244 0.430	-0.087 *** (0.018)	-0.331 *** (0.069)	2,070	0.835 0.371	0.072 *** (0.016)	0.272 *** (0.060)
Housing Status at baseline								
Rents or owns apartment or house	1,307	0.223 0.417	-0.068 *** (0.031)	-0.258 *** (0.116)	1,304	0.857 0.350	0.053 *** (0.028)	0.201 *** (0.107)
Resides in public or assisted housing	364	0.202 0.402	-0.104 ** (0.091)	-0.447 ** (0.394)	357	0.897 0.305	0.000 (0.000)	0.000 (0.000)
Lives with friends/relatives or in shelter	747	0.312 0.464	-0.146 *** (0.038)	-0.608 *** (0.159)	743	0.747 0.435	0.082 *** (0.036)	0.343 *** (0.149)
TANF Receipt at baseline								
Not receiving TANF	497	0.227 0.420	-0.065 * (0.035)	-0.409 * (0.219)	471	0.849 0.359	0.019 (0.049)	0.121 (0.304)
Receiving TANF, expiring in:								
Less than 6 months	320	0.280 0.450	-0.142 *** (0.039)	-0.682 *** (0.186)	313	0.777 0.418	0.078 *** (0.031)	0.374 *** (0.147)
6 to 12 months	161	0.301 0.461	-0.007 (0.069)	-0.029 (0.279)	155	0.793 0.408	-0.005 (0.074)	-0.021 (0.300)
12 to 18 months	138	0.243 0.432	-0.134 * (0.130)	-0.413 * (0.400)	121	0.828 0.380	0.017 (0.043)	0.053 (0.132)
More than 18 months	193	0.202 0.403	0.013 (0.060)	0.054 (0.258)	155	0.904 0.296	0.000 (0.000)	0.000 (0.000)
Total receiving TANF	1,849	0.247 0.431	-0.096 *** (0.020)	-0.347 *** (0.071)	1,816	0.832 0.374	0.074 *** (0.018)	0.266 *** (0.064)

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.

^b An F test on the equality of treatment effects between subgroups indicates that ITT impacts on homelessness during year prior to survey differ significantly between subgroups at p<.10

^c An F test on the equality of treatment effects between subgroups indicates that ITT impacts on independent Housing at time of survey differ significantly between subgroups at p<.10

Exhibit F.4: Impacts on rooms and crowding, by Subgroup

Subgroup	Number of rooms at time of survey Fifth Year, All Sites except Los Angeles				Crowding at time of survey Fifth Year, All Sites except Los Angeles			
	Sample	Control	ITT	TOT	Sample	Control	ITT	TOT
	Size	Mean ^a	Impact	Impact	Size	Mean ^a	Impact	Impact
Age at baseline								
Less than 24	836	3.89 1.37	0.200 * (0.106)	0.827 * (0.437)	827	0.447 0.498	-0.064 (0.040)	-0.264 (0.167)
25-34	974	4.08 1.36	0.109 (0.091)	0.416 (0.347)	964	0.394 0.489	-0.044 (0.036)	-0.167 (0.140)
35-44	503	3.98 1.41	0.205 (0.136)	0.707 (0.468)	503	0.315 0.465	0.008 (0.050)	0.028 (0.172)
45 or older	141	4.06 1.44	-0.140 (0.312)	-1.316 (2.932)	122	0.285 0.454	0.000 *** (0.000)	0.000 *** (0.000)
Race/Ethnicity								
White, Non-Hispanic	381	4.44 1.30	0.215 (0.153)	0.682 (0.486)	370	0.227 0.420	-0.034 (0.062)	-0.109 (0.196)
Black, Non-Hispanic	1,403	4.01 1.35	0.118 (0.074)	0.559 (0.348)	1,398	0.355 0.479	-0.044 (0.028)	-0.209 (0.134)
Hispanic	528	3.59 1.36	0.275 ** (0.131)	0.988 ** (0.473)	521	0.533 0.500	-0.123 ** (0.052)	-0.443 ** (0.189)
Education at baseline								
High school diploma	958	4.05 1.38	0.282 *** (0.094)	1.059 *** (0.353)	946	0.326 0.469	-0.085 ** (0.034)	-0.320 ** (0.128)
GED only	477	4.13 1.31	0.187 (0.141)	0.670 (0.505)	464	0.317 0.466	-0.029 (0.050)	-0.103 (0.178)
Neither high school diploma nor GED	847	3.86 1.41	0.021 (0.099)	0.088 (0.427)	846	0.494 0.501	-0.052 (0.041)	-0.224 (0.178)
School Enrollment at baseline								
Enrolled in school	403	4.10 1.35	0.106 (0.150)	0.404 (0.574)	395	0.360 0.481	0.018 (0.053)	0.070 (0.204)
Not enrolled in school	1,952	3.97 1.39	0.134 ** (0.065)	0.532 ** (0.259)	1,949	0.390 0.488	-0.058 ** (0.026)	-0.232 ** (0.101)
Presence of children at baseline								
Any dependent children ^b	2,257	4.04 1.38	0.117 * (0.061)	0.473 * (0.246)	2,249	0.380 0.486	-0.049 ** (0.024)	-0.197 ** (0.096)
No dependent children ^b	183	3.41 1.28	0.361 (0.253)	1.403 (0.982)	179	0.481 0.502	-0.302 *** (0.106)	-1.174 *** (0.411)
Youngest Household Member at baseline								
Youngest household member less than 6	1,703	3.97 1.35	0.195 *** (0.070)	0.768 *** (0.275)	1,700	0.437 0.496	-0.069 ** (0.028)	-0.274 ** (0.109)
Youngest household member 6-17	752	4.04 1.46	0.057 (0.112)	0.230 (0.455)	734	0.281 0.450	-0.029 (0.038)	-0.116 (0.153)
Youngest household member 18 or more	-	-	-	-	-	-	-	-
Employment Status at baseline								
Employed	1,021	4.05 1.34	0.164 * (0.092)	0.688 * (0.387)	1,006	0.337 0.473	-0.032 (0.036)	-0.134 (0.153)
Not employed, with reservation wage of: \$3.00 to \$5.99	74	4.04 1.43	-0.833 (0.827)	-1.751 (1.739)	48	0.414 0.500	0.000 (0.000)	0.000 (0.000)
\$6.00 to \$8.99	697	3.89 1.39	0.195 * (0.112)	1.038 * (0.598)	694	0.433 0.496	-0.089 ** (0.045)	-0.473 ** (0.240)
\$9.00 to \$12.99	318	3.92 1.42	0.424 ** (0.170)	2.246 ** (0.897)	309	0.379 0.487	-0.037 (0.062)	-0.198 (0.331)
\$13.00 to \$15.99	81	3.78 1.47	-0.074 (0.499)	-0.623 (4.184)	56	0.472 0.506	-0.493 (0.253)	-4.132 (2.118)

Exhibit F.4: Impacts on rooms and crowding, by Subgroup

Subgroup	Number of rooms at time of survey Fifth Year, All Sites except Los Angeles				Crowding at time of survey Fifth Year, All Sites except Los Angeles			
	Sample	Control	ITT	TOT	Sample	Control	ITT	TOT
	Size	Mean ^a	Impact	Impact	Size	Mean ^a	Impact	Impact
Total not employed	1,336	3.93 1.41	0.152 * (0.078)	0.553 * (0.285)	1,334	0.425 0.495	-0.055 * (0.031)	-0.198 * (0.114)
Employment Background at baseline								
Ever employed	2,123	4.00 1.38	0.148 ** (0.063)	0.589 ** (0.250)	2,114	0.372 0.483	-0.044 * (0.024)	-0.174 * (0.097)
Never employed	308	3.90 1.42	0.274 (0.179)	1.121 (0.732)	303	0.487 0.501	-0.040 (0.071)	-0.163 (0.290)
Job Training Status at baseline								
Enrolled in job training^b	341	3.94 1.35	0.222 (0.162)	0.847 (0.617)	335	0.388 0.489	0.005 (0.064)	0.019 (0.246)
Enrolled in (but yet to start) job training^b	184	4.32 1.35	-0.600 *** (0.213)	-2.031 *** (0.721)	169	0.329 0.472	0.096 (0.084)	0.325 (0.284)
Not enrolled in job training^b	1,908	3.97 1.39	0.155 ** (0.067)	0.633 ** (0.272)	1,902	0.393 0.489	-0.073 *** (0.026)	-0.298 *** (0.107)
Moving for Employment Reasons at baseline								
Desired to move for employment reasons	360	3.82 1.33	0.193 (0.175)	1.114 (1.010)	356	0.431 0.496	-0.182 *** (0.069)	-1.048 *** (0.399)
Did not desire to move for employment reasons	2,085	4.02 1.39	0.141 ** (0.063)	0.538 ** (0.239)	2,081	0.381 0.486	-0.046 * (0.025)	-0.173 * (0.094)
Housing Status at baseline								
Rents or owns apartment or house	1,305	3.95 1.40	0.176 ** (0.083)	0.666 ** (0.312)	1,295	0.396 0.489	-0.048 (0.032)	-0.181 (0.121)
Resides in public or assisted housing	372	3.99 1.39	0.097 (0.154)	0.419 (0.665)	358	0.406 0.492	-0.058 (0.058)	-0.248 (0.251)
Lives with friends/relatives or in shelter	749	4.05 1.36	0.117 (0.107)	0.486 (0.446)	745	0.363 0.481	-0.044 (0.042)	-0.183 (0.174)
TANF Receipt at baseline								
Not receiving TANF	500	4.22 1.36	0.097 (0.132)	0.605 (0.825)	485	0.275 0.448	-0.019 (0.046)	-0.118 (0.289)
Receiving TANF, expiring in:								
Less than 6 months	328	4.11 1.41	-0.075 (0.167)	-0.361 (0.802)	323	0.338 0.474	0.040 (0.055)	0.193 (0.266)
6 to 12 months	182	3.74 1.34	0.548 ** (0.210)	2.227 ** (0.854)	167	0.482 0.502	-0.140 * (0.575)	-0.571 * (2.337)
12 to 18 months	138	3.99 1.45	-0.219 (0.311)	-0.676 (0.960)	125	0.327 0.473	0.000 * (0.000)	0.000 * (0.000)
More than 18 months	231	4.14 1.42	0.182 (0.193)	0.790 (0.835)	222	0.353 0.480	-0.020 (0.078)	-0.086 (0.338)
Total receiving TANF	1,840	3.93 1.39	0.172 ** (0.067)	0.621 ** (0.242)	1,834	0.420 0.494	-0.070 *** (0.027)	-0.252 *** (0.097)

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.

^b An F test on the equality of treatment effects between subgroups indicates that ITT impacts on number of rooms at time of survey differ significantly between subgroups at p<.10

^c An F test on the equality of treatment effects between subgroups indicates that ITT impacts on crowding at time of survey differ significantly between subgroups at p<.10

Exhibit F.5: Impacts on quality of housing and housing problems, by Subgroup

Subgroup	Quality of housing at time of survey				Housing problems at time of survey			
	Fifth Year, All Sites except Los Angeles				Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Age at baseline								
Less than 24	840	0.642 0.480	-0.018 (0.039)	-0.075 (0.162)	813	0.126 0.333	0.000 (0.018)	-0.002 (0.074)
25-34	966	0.618 0.486	0.053 (0.035)	0.202 (0.134)	952	0.120 0.326	-0.009 (0.012)	-0.033 (0.047)
35-44	507	0.571 0.496	0.029 (0.056)	0.099 (0.193)	473	0.163 0.370	-0.009 (0.038)	-0.029 (0.130)
45 or older	122	0.604 0.492	-1.000 *** (0.000)	-9.407 *** (0.000)	61	0.182 0.389	0.000 (0.000)	0.000 (0.000)
Race/Ethnicity								
White, Non-Hispanic ^b	373	0.549 0.499	0.188 *** (0.065)	0.596 *** (0.207)	325	0.102 0.304	0.000 (0.000)	0.000 (0.000)
Black, Non-Hispanic ^b	1,409	0.616 0.487	0.030 (0.029)	0.142 (0.138)	1,385	0.130 0.337	-0.012 (0.018)	-0.057 (0.084)
Hispanic ^b	516	0.648 0.479	-0.093 * (0.050)	-0.334 * (0.181)	502	0.179 0.384	-0.038 (0.036)	-0.138 (0.131)
Education at baseline								
High school diploma	960	0.656 0.476	0.029 (0.035)	0.109 (0.132)	903	0.090 0.287	-0.009 (0.013)	-0.034 (0.049)
GED only	473	0.628 0.484	-0.001 (0.053)	-0.004 (0.191)	432	0.134 0.341	-0.023 (0.030)	-0.084 (0.107)
Neither high school diploma nor GED	840	0.591 0.492	0.021 (0.039)	0.090 (0.167)	814	0.178 0.383	-0.020 (0.022)	-0.088 (0.096)
School Enrollment at baseline								
Enrolled in school	388	0.628 0.485	0.027 (0.055)	0.102 (0.210)	376	0.138 0.346	-0.014 (0.016)	-0.052 (0.060)
Not enrolled in school	1,964	0.619 0.486	0.012 (0.025)	0.049 (0.099)	1,933	0.133 0.339	-0.013 (0.015)	-0.053 (0.060)
Presence of children at baseline								
Any dependent children	2,274	0.614 0.487	0.028 (0.023)	0.114 (0.093)	2,231	0.137 0.344	-0.020 (0.014)	-0.081 (0.058)
No dependent children	173	0.658 0.477	-0.135 (0.107)	-0.526 (0.416)	127	0.119 0.326	0.000 *** (0.000)	0.000 *** (0.000)
Youngest Household Member at baseline								
Youngest household member less than 6	1,715	0.625 0.484	0.003 (0.026)	0.010 (0.104)	1,662	0.132 0.338	-0.014 (0.014)	-0.053 (0.057)
Youngest household member 6-17	749	0.595 0.492	0.086 ** (0.043)	0.350 ** (0.176)	697	0.141 0.349	-0.034 (0.026)	-0.137 (0.108)
Youngest household member 18 or more	-	-	-	-	-	-	-	-
Employment Status at baseline								
Employed	1,016	0.660 0.474	0.016 (0.035)	0.069 (0.147)	998	0.112 0.315	-0.025 (0.018)	-0.107 (0.076)
Not employed, with reservation wage of:								
\$3.00 to \$5.99	54	0.595 0.498	0.006 (0.014)	0.013 (0.029)	-	-	-	-
\$6.00 to \$8.99	689	0.601 0.490	-0.014 (0.043)	-0.075 (0.228)	661	0.155 0.362	-0.033 (0.026)	-0.177 (0.139)
\$9.00 to \$12.99	310	0.617 0.488	0.011 (0.065)	0.058 (0.342)	292	0.117 0.322	0.005	0.028
\$13.00 to \$15.99	56	0.502 0.507	0.000 *** (0.000)	0.000 *** (0.000)	29	0.094 0.295	-1.000 (0.000)	-8.384 (0.000)

Exhibit F.5: Impacts on quality of housing and housing problems, by Subgroup

Subgroup	Quality of housing at time of survey				Housing problems at time of survey			
	Fifth Year, All Sites except Los Angeles				Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Total not employed	1,336	0.596 0.491	0.011 (0.031)	0.039 (0.113)	1,304	0.151 0.359	-0.028 (0.020)	-0.103 (0.074)
Employment Background at baseline								
Ever employed	2,137	0.625 0.484	0.008 (0.024)	0.032 (0.095)	2,117	0.129 0.336	-0.018 (0.014)	-0.070 (0.056)
Never employed	297	0.582 0.495	0.061 (0.074)	0.248 (0.303)	276	0.177 0.383	-0.004 (0.045)	-0.014 (0.183)
Job Training Status at baseline								
Enrolled in job training	334	0.647 0.479	-0.009 (0.063)	-0.035 (0.241)	313	0.137 0.345	-0.029 (0.019)	-0.110 (0.073)
Enrolled in (but yet to start) job training	166	0.527 0.502	0.331 ** (0.133)	1.119 ** (0.451)	104	0.120 0.327	0.000 *** (0.000)	0.000 *** (0.000)
Not enrolled in job training	1,920	0.619 0.486	0.017 (0.025)	0.071 (0.104)	1,874	0.137 0.344	-0.020 (0.016)	-0.082 (0.064)
Moving for Employment Reasons at baseline								
Desired to move for employment reasons	345	0.644 0.480	0.005 (0.061)	0.031 (0.352)	329	0.107 0.310	-0.014 (0.019)	-0.080 (0.108)
Did not desire to move for employment reasons	2,099	0.612 0.488	0.022 (0.024)	0.082 (0.092)	2,045	0.140 0.347	-0.021 (0.015)	-0.079 (0.058)
Housing Status at baseline								
Rents or owns apartment or house	1,309	0.641 0.480	-0.009 (0.031)	-0.036 (0.115)	1,279	0.131 0.337	-0.015 (0.018)	-0.058 (0.067)
Resides in public or assisted housing	356	0.606 0.490	0.027 (0.065)	0.118 (0.279)	325	0.157 0.364	0.006 (0.075)	0.027 (0.325)
Lives with friends/relatives or in shelter	746	0.583 0.494	0.050 (0.043)	0.206 (0.178)	730	0.130 0.337	-0.023 (0.021)	-0.094 (0.089)
TANF Receipt at baseline								
Not receiving TANF ^c	502	0.642 0.480	0.036 (0.049)	0.225 (0.308)	475	0.086 0.282	0.032 * (0.019)	0.200 * (0.118)
Receiving TANF, expiring in:								
Less than 6 months	322	0.685 0.466	-0.122 * (0.066)	-0.586 * (0.315)	303	0.110 0.313	0.001 (0.021)	0.003 (0.102)
6 to 12 months	171	0.627 0.486	0.015 (0.088)	0.060 (0.357)	111	0.155 0.364	0.000 *** (0.000)	0.000 *** (0.000)
12 to 18 months	129	0.456 0.502	0.091 (0.240)	0.279 (0.740)	109	0.184 0.390	0.000 *** (0.000)	0.000 *** (0.000)
More than 18 months	223	0.626 0.486	0.113 (0.084)	0.489 (0.366)	189	0.122 0.328	0.200 (0.525)	0.869 (2.275)
Total receiving TANF^c	1,847	0.612 0.487	0.013 (0.026)	0.048 (0.093)	1,796	0.148 0.356	-0.038 ** (0.017)	-0.138 ** (0.061)

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.

^b An F test on the equality of treatment effects between subgroups indicates that ITT impacts on quality of housing at time of survey differ significantly between subgroups at p<.10

^c An F test on the equality of treatment effects between subgroups indicates that ITT impacts on housing problems at time of survey differ significantly between subgroups at p<.10

Exhibit F.6: Impacts food expenditures in the month before the survey, by Subgroup

Subgroup	Household Food expenditures in the month before the survey				Food expenditures per person in the month before the survey			
	Fifth Year, All Sites except Los Angeles				Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Age at baseline								
Less than 24	804	\$105 \$69	\$14 * (8)	\$59 * (31)	803	\$27 \$23	\$5 * (3)	\$19 * (10)
25-34	922	\$111 \$76	\$9 (6)	\$36 (25)	920	\$28 \$18	\$3 (2)	\$11 (7)
35-44	471	\$100 \$62	\$3 (7)	\$10 (23)	471	\$30 \$22	\$2 (3)	\$6 (9)
45 or older	126	\$97 \$68	-\$5 (18)	-\$43 (172)	125	\$33 \$21	-\$1 (7)	-\$10 (69)
Race/Ethnicity								
White, Non-Hispanic ^b	369	\$99 \$51	\$14 ** (6)	\$44 ** (20)	368	\$28 \$17	\$3 * (2)	\$10 * (6)
Black, Non-Hispanic ^b	1,308	\$108 \$81	\$14 ** (6)	\$68 ** (28)	1,306	\$29 \$22	\$5 *** (2)	\$26 *** (9)
Hispanic ^b	518	\$105 \$60	-\$5 (6)	-\$17 (22)	517	\$26 \$17	\$1 (2)	\$3 (7)
Education at baseline								
High school diploma	902	\$100 \$62	\$5 (5)	\$19 (19)	900	\$28 \$17	\$3 * (2)	\$10 * (6)
GED only	464	\$105 \$69	\$19 * (10)	\$69 * (37)	463	\$29 \$21	\$4 (3)	\$16 (12)
Neither high school diploma nor GED	803	\$109 \$68	\$8 (6)	\$35 (26)	802	\$28 \$25	\$3 (2)	\$14 (9)
School Enrollment at baseline								
Enrolled in school	393	\$111 \$62	\$14 (9)	\$53 (36)	392	\$29 \$17	\$6 ** (3)	\$23 ** (12)
Not enrolled in school	1,841	\$104 \$67	\$9 ** (4)	\$36 ** (16)	1,838	\$28 \$22	\$3 ** (1)	\$11 ** (5)
Presence of children at baseline								
Any dependent children	2,143	\$105 \$68	\$11 *** (4)	\$43 *** (15)	2,139	\$28 \$19	\$3 *** (1)	\$14 *** (5)
No dependent children	166	\$107 \$92	\$8 (18)	\$31 (70)	166	\$31 \$38	\$6 (7)	\$24 (28)
Youngest Household Member at baseline								
Youngest household member less than 6 ^{b,c}	1,623	\$107 \$72	\$18 *** (5)	\$70 *** (19)	1,620	\$27 \$20	\$5 *** (1)	\$21 *** (5)
Youngest household member 6-17 ^{b,c}	701	\$103 \$68	-\$2 (6)	-\$8 (23)	700	\$31 \$23	\$0 (2)	\$1 (8)
Youngest household member 18 or more ^{b,c}								
Employment Status at baseline								
Employed	971	\$100 \$59	\$9 * (5)	\$38 * (21)	970	\$28 \$18	\$3 ** (1)	\$13 ** (6)
Not employed, with reservation wage of: \$3.00 to \$5.99	71	\$99 \$81	\$2 (36)	\$4 (75)	70	\$28 \$23	\$4 (12)	\$8 (24)
\$6.00 to \$8.99	662	\$112 \$82	\$6 (8)	\$33 (45)	661	\$28 \$18	\$3 (3)	\$16 (14)
\$9.00 to \$12.99	305	\$104 \$63	\$19 * (10)	\$101 * (55)	305	\$27 \$16	\$3 (2)	\$17 (13)

Exhibit F.6: Impacts food expenditures in the month before the survey, by Subgroup

Subgroup	Household Food expenditures in the month before the survey				Food expenditures per person in the month before the survey			
	Fifth Year, All Sites except Los Angeles				Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
\$13.00 to \$15.99	76	\$101 \$57	\$23 (49)	\$189 (411)	76	\$26 \$13	\$11 (13)	\$89 (110)
Total not employed	1,258	\$108 \$71	\$11 ** (5)	\$40 ** (18)	1,255	\$28 \$22	\$3 * (2)	\$11 * (6)
Employment Background at baseline								
Ever employed	2,013	\$105 \$72	\$10 *** (4)	\$41 *** (16)	2,010	\$28 \$20	\$4 *** (1)	\$14 *** (5)
Never employed	286	\$109 \$65	\$10 (11)	\$42 (44)	285	\$28 \$26	\$0 (4)	-\$1 (16)
Job Training Status at baseline								
Enrolled in job training	330	\$107 \$57	\$11 (9)	\$44 (33)	329	\$29 \$15	\$4 (3)	\$15 (11)
Enrolled in (but yet to start) job training	179	\$113 \$100	-\$2 (12)	-\$8 (41)	178	\$26 \$17	\$4 (3)	\$13 (10)
Not enrolled in job training	1,794	\$105 \$69	\$10 ** (4)	\$40 ** (17)	1,792	\$28 \$22	\$3 ** (1)	\$12 ** (6)
Moving for Employment Reasons at baseline								
Desired to move for employment reasons	339	\$100 \$70	\$3 (7)	\$18 (40)	338	\$29 \$29	\$0 (3)	\$1 (14)
Did not desire to move for employment reasons	1,975	\$107 \$71	\$11 *** (4)	\$41 *** (15)	1,972	\$28 \$19	\$4 *** (1)	\$14 *** (5)
Housing Status at baseline								
Rents or owns apartment or house	1,244	\$104 \$67	\$7 (5)	\$27 (18)	1,243	\$28 \$23	\$1 (2)	\$5 (6)
Resides in public or assisted housing	343	\$115 \$95	\$20 (12)	\$85 (54)	343	\$29 \$20	\$6 * (4)	\$28 * (15)
Lives with friends/relatives or in shelter	711	\$104 \$64	\$13 * (7)	\$52 * (28)	708	\$28 \$18	\$5 ** (2)	\$21 ** (9)
TANF Receipt at baseline								
Not receiving TANF	473	\$108 \$81	\$10 (10)	\$62 (62)	472	\$31 \$28	\$3 (3)	\$16 (20)
Receiving TANF, expiring in:								
Less than 6 months	310	\$113 \$73	-\$1 (11)	-\$5 (51)	310	\$30 \$23	\$2 (3)	\$8 (16)
6 to 12 months	176	\$113 \$69	\$15 (17)	\$60 (71)	176	\$28 \$13	\$1 (3)	\$5 (14)
12 to 18 months	131	\$86 \$51	\$36 ** (15)	\$110 ** (47)	130	\$25 \$15	\$8 (6)	\$26 (18)
More than 18 months	227	\$114 \$99	-\$7 (14)	-\$30 (59)	227	\$28 \$19	\$1 (3)	\$4 (14)
Total receiving TANF	1,743	\$106 \$68	\$10 ** (4)	\$36 ** (14)	1,740	\$28 \$19	\$3 ** (1)	\$11 ** (4)

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.

^b An F test on the equality of treatment effects between subgroups indicates that ITT impacts on Household food expenditures in the month before the survey differ significantl

Exhibit F.7: Impacts on computational and standard metric for food security, by subgroup

Subgroup	Household food security scale score				Household food security status score			
	Fifth Year, All Sites except Los Angeles				Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Age at baseline								
Less than 24	841	2.87 3.01	-0.122 (0.227)	-0.505 (0.937)	841	2.05 2.15	-0.087 (0.162)	-0.360 (0.670)
25-34	986	2.98 3.00	0.103 (0.210)	0.394 (0.804)	986	2.13 2.15	0.073 (0.150)	0.281 (0.574)
35-44	507	3.69 3.19	-0.177 (0.320)	-0.608 (1.103)	507	2.63 2.28	-0.126 (0.229)	-0.435 (0.788)
45 or older	143	3.62 2.90	-0.713 (0.808)	-6.711 (7.602)	143	2.59 2.07	-0.510 (0.577)	-4.793 (5.430)
Race/Ethnicity								
White, Non-Hispanic	386	3.40 3.12	-0.414 (0.362)	-1.313 (1.149)	386	2.43 2.23	-0.295 (0.259)	-0.937 (0.821)
Black, Non-Hispanic	1,416	3.12 3.02	-0.070 (0.172)	-0.331 (0.815)	1,416	2.23 2.16	-0.050 (0.123)	-0.237 (0.582)
Hispanic	531	2.97 3.08	-0.137 (0.285)	-0.492 (1.026)	531	2.12 2.20	-0.098 (0.204)	-0.352 (0.733)
Education at baseline								
High school diploma	968	2.90 2.93	-0.187 (0.213)	-0.704 (0.800)	968	2.07 2.09	-0.134 (0.152)	-0.503 (0.571)
GED only	482	3.44 3.35	-0.309 (0.331)	-1.108 (1.188)	482	2.46 2.39	-0.221 (0.237)	-0.793 (0.849)
Neither high school diploma nor GED	855	3.23 3.04	0.226 (0.228)	0.971 (0.977)	855	2.31 2.17	0.161 (0.163)	0.693 (0.698)
School Enrollment at baseline								
Enrolled in school	404	2.91 3.05	0.007 (0.344)	0.025 (1.312)	404	2.08 2.18	0.005 (0.246)	0.018 (0.938)
Not enrolled in school	1,974	3.19 3.07	-0.047 (0.148)	-0.185 (0.586)	1,974	2.28 2.20	-0.034 (0.106)	-0.133 (0.419)
Presence of children at baseline								
Any dependent children	2,280	3.14 3.08	-0.043 (0.138)	-0.175 (0.558)	2,280	2.24 2.20	-0.031 (0.099)	-0.126 (0.399)
No dependent children	183	3.02 2.74	-0.188 (0.564)	-0.732 (2.193)	183	2.16 1.96	-0.134 (0.403)	-0.523 (1.567)
Youngest Household Member at baseline								
Youngest household member less than 6	1,720	2.98 3.03	-0.016 (0.160)	-0.062 (0.630)	1,720	2.13 2.16	-0.011 (0.114)	-0.044 (0.450)
Youngest household member 6-17	758	3.46 3.10	-0.186 (0.245)	-0.759 (1.000)	758	2.47 2.21	-0.134 (0.175)	-0.544 (0.714)
Youngest household member 18 or more	-	-	-	-	-	-	-	-
Employment Status at baseline								
Employed	1,029	2.88 2.92	0.109 (0.200)	0.459 (0.839)	1,029	2.06 2.09	0.078 (0.143)	0.328 (0.599)
Not employed, with reservation wage of:								
\$3.00 to \$5.99	75	2.84 3.17	3.251 ** (1.403)	6.836 ** (2.950)	75	2.03 2.26	2.322 ** (1.002)	4.883 ** (2.107)
\$6.00 to \$8.99	705	3.24 3.12	-0.011 (0.262)	-0.058 (1.393)	705	2.31 2.23	-0.008 (0.187)	-0.043 (0.996)
\$9.00 to \$12.99	318	3.24 3.00	0.206 (0.394)	1.093 (2.085)	318	2.31 2.14	0.147 (0.281)	0.780 (1.489)

Exhibit F.7: Impacts on computational and standard metric for food security, by subgroup

Subgroup	Household food security scale score				Household food security status score			
	Fifth Year, All Sites except Los Angeles				Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
\$13.00 to \$15.99	82	3.21	0.267	2.239	82	2.29	0.190	1.593
		3.33	(1.397)	(11.710)		2.38	(0.998)	(8.371)
Total not employed	1,349	3.29	-0.183	-0.665	1,349	2.35	-0.131	-0.477
		3.15	(0.184)	(0.668)		2.25	(0.131)	(0.477)
Employment Background at baseline								
Ever employed	2,143	3.10	0.003	0.011	2,143	2.22	0.002	0.008
		3.05	(0.142)	(0.565)		2.18	(0.102)	(0.404)
Never employed	310	3.25	-0.284	-1.162	310	2.32	-0.203	-0.830
		3.15	(0.412)	(1.687)		2.25	(0.295)	(1.205)
Job Training Status at baseline								
Enrolled in job training	344	3.04	-0.312	-1.191	344	2.17	-0.223	-0.850
		3.09	(0.390)	(1.490)		2.21	(0.279)	(1.065)
Enrolled in (but yet to start) job training	187	3.60	-0.219	-0.741	187	2.57	-0.157	-0.531
		3.17	(0.586)	(1.984)		2.26	(0.419)	(1.418)
Not enrolled in job training	1,925	3.09	0.004	0.017	1,925	2.20	0.003	0.011
		3.04	(0.149)	(0.610)		2.17	(0.107)	(0.436)
Moving for Employment Reasons at baseline								
Desired to move for employment reasons	363	3.28	-0.601	-3.464	363	2.34	-0.429	-2.475
		3.13	(0.390)	(2.251)		2.24	(0.279)	(1.608)
Did not desire to move for employment reasons	2,105	3.11	0.006	0.023	2,105	2.22	0.004	0.016
		3.05	(0.142)	(0.541)		2.18	(0.102)	(0.386)
Housing Status at baseline								
Rents or owns apartment or house	1,320	3.09	-0.175	-0.662	1,320	2.21	-0.125	-0.473
		3.05	(0.180)	(0.678)		2.18	(0.128)	(0.485)
Resides in public or assisted housing	375	2.94	-0.357	-1.541	375	2.10	-0.255	-1.101
		3.00	(0.346)	(1.493)		2.15	(0.247)	(1.067)
Lives with friends/relatives or in shelter	754	3.29	0.121	0.505	754	2.35	0.086	0.360
		3.11	(0.261)	(1.087)		2.22	(0.187)	(0.777)
TANF Receipt at baseline								
Not receiving TANF	506	2.87	0.177	1.106	506	2.05	0.126	0.789
		2.87	(0.285)	(1.781)		2.05	(0.203)	(1.273)
Receiving TANF, expiring in: Less than 6 months	330	3.95	-0.958 **	-4.601 **	330	2.82	-0.685 **	-3.289 **
		3.17	(0.374)	(1.798)		2.27	(0.268)	(1.285)
6 to 12 months	183	2.71	-0.121	-0.492	183	1.93	-0.086	-0.351
		3.18	(0.522)	(2.120)		2.27	(0.373)	(1.515)
12 to 18 months	142	3.58	0.002	0.007	142	2.56	0.000	0.001
		3.01	(0.613)	(1.888)		2.15	(0.438)	(1.349)
More than 18 months	233	2.63	-0.154	-0.666	233	1.88	-0.110	-0.476
		2.78	(0.499)	(2.163)		1.98	(0.356)	(1.545)
Total receiving TANF	1,856	3.23	-0.104	-0.377	1,856	2.31	-0.075	-0.270
		3.10	(0.154)	(0.558)		2.21	(0.110)	(0.399)

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.

^b An F test on the equality of treatment effects between subgroups indicates that ITT impacts on the food security scale score differ significantly between subgroups at p < .10

^c An F test on the equality of treatment effects between subgroups indicates that ITT impacts on the food security status score differ significantly between subgroups at p < .10

Exhibit F.8: Impacts on food insecurity, by Subgroup

Subgroup	Household food security status				Household member(s) went without meals at least one day in last month			
	Fifth Year, All Sites except Los Angeles				Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Age at baseline								
Less than 24	841	0.375 0.485	0.009 (0.039)	0.036 (0.162)	779	0.098 0.297	-0.007 (0.014)	-0.027 (0.056)
25-34	980	0.416 0.493	-0.008 (0.036)	-0.029 (0.139)	961	0.088 0.284	0.014 (0.015)	0.055 (0.059)
35-44	507	0.511 0.501	-0.046 (0.056)	-0.160 (0.194)	468	0.110 0.313	-0.002 (0.023)	-0.007 (0.079)
45 or older	132	0.452 0.501	-0.154 (0.206)	-1.448 (1.939)	72	0.089 0.287	0.000 *** (0.000)	0.000 *** (0.000)
Race/Ethnicity								
White, Non-Hispanic	379	0.463 0.500	-0.065 (0.062)	-0.205 (0.195)	332	0.099 0.299	-0.003 (0.019)	-0.009 (0.059)
Black, Non-Hispanic	1,410	0.425 0.495	-0.003 (0.031)	-0.016 (0.147)	1,394	0.114 0.318	-0.007 (0.016)	-0.035 (0.075)
Hispanic	524	0.396 0.490	-0.023 (0.051)	-0.081 (0.183)	445	0.074 0.262	0.001 (0.003)	0.002 (0.010)
Education at baseline								
High school diploma	955	0.414 0.493	-0.061 (0.038)	-0.231 (0.141)	949	0.059 0.235	0.008 (0.012)	0.030 (0.045)
GED only	466	0.449 0.498	-0.030 (0.056)	-0.107 (0.201)	439	0.131 0.338	-0.046 (0.035)	-0.166 (0.126)
Neither high school diploma nor GED	845	0.431 0.496	0.062 (0.040)	0.264 (0.173)	823	0.108 0.310	0.012 (0.017)	0.051 (0.074)
School Enrollment at baseline								
Enrolled in school	390	0.396 0.490	-0.005 (0.058)	-0.021 (0.220)	354	0.082 0.275	0.002 (0.003)	0.009 (0.012)
Not enrolled in school	1,962	0.436 0.496	0.002 (0.026)	0.008 (0.103)	1,952	0.098 0.297	0.004 (0.012)	0.018 (0.048)
Presence of children at baseline								
Any dependent children ^c	2,273	0.426 0.495	-0.002 (0.024)	-0.010 (0.097)	2,238	0.099 0.299	0.002 (0.012)	0.006 (0.048)
No dependent children ^c	175	0.404 0.494	-0.016 (0.123)	-0.062 (0.477)	89	0.062 0.242	0.000 *** (0.000)	0.000 *** (0.000)
Youngest Household Member at baseline								
Youngest household member less than 6	1,719	0.397 0.489	0.019 (0.027)	0.074 (0.107)	1,691	0.088 0.283	0.014 (0.012)	0.054 (0.048)
Youngest household member 6-17	743	0.487 0.500	-0.053 (0.044)	-0.215 (0.181)	711	0.115 0.320	-0.002 (0.025)	-0.010 (0.103)
Youngest household member 18 or more	-	-	-	-	-	-	-	-
Employment Status at baseline								
Employed	1,020	0.408 0.492	0.007 (0.037)	0.029 (0.153)	1,001	0.075 0.264	0.018 (0.013)	0.074 (0.054)
Not employed, with reservation wage of: \$3.00 to \$5.99	56	0.318 0.472	0.841 (0.315)	1.768 (0.662)	-	-	-	-
\$6.00 to \$8.99	695	0.439 0.497	-0.017 (0.045)	-0.091 (0.238)	683	0.125 0.331	-0.011 (0.020)	-0.057 (0.106)
\$9.00 to \$12.99	308	0.428 0.496	0.042 (0.070)	0.223 (0.372)	262	0.103 0.305	0.025 (0.094)	0.134 (0.498)
\$13.00 to \$15.99	67	0.430 0.502	0.900 *** (0.048)	7.547 *** (0.399)	-	-	-	-

Exhibit F.8: Impacts on food insecurity, by Subgroup

Subgroup	Household food security status				Household member(s) went without meals at least one day in last month			
	Fifth Year, All Sites except Los Angeles				Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Total not employed	1,338	0.435 0.496	-0.010 (0.032)	-0.037 (0.116)	1,323	0.106 0.309	-0.007 (0.018)	-0.024 (0.065)
Employment Background at baseline								
Ever employed	2,134	0.421 0.494	0.005 (0.025)	0.022 (0.098)	2,110	0.090 0.286	0.015 (0.012)	0.060 (0.047)
Never employed	299	0.438 0.498	-0.041 (0.075)	-0.169 (0.306)	231	0.129 0.336	-0.349 ** (1.703)	-1.427 ** (6.969)
Job Training Status at baseline								
Enrolled in job training	336	0.427 0.496	-0.083 (0.063)	-0.318 (0.239)	275	0.101 0.302	0.000 (0.000)	0.000 (0.000)
Enrolled in (but yet to start) job training	178	0.504 0.502	-0.025 (0.110)	-0.085 (0.373)	103	0.065 0.247	0.000 (0.000)	0.000 (0.000)
Not enrolled in job training	1,915	0.414 0.493	0.013 (0.026)	0.052 (0.107)	1,894	0.098 0.298	0.005 (0.012)	0.022 (0.050)
Moving for Employment Reasons at baseline								
Desired to move for employment reasons	359	0.415 0.494	-0.063 (0.068)	-0.363 (0.390)	331	0.113 0.317	-0.006 (0.009)	-0.036 (0.052)
Did not desire to move for employment reasons	2,099	0.429 0.495	-0.002 (0.025)	-0.006 (0.095)	2,083	0.094 0.292	0.013 (0.012)	0.051 (0.047)
Housing Status at baseline								
Rents or owns apartment or house	1,309	0.414 0.493	-0.003 (0.032)	-0.013 (0.120)	1,286	0.077 0.267	0.009 (0.013)	0.034 (0.047)
Resides in public or assisted housing	366	0.436 0.497	-0.125 ** (0.066)	-0.539 ** (0.284)	338	0.110 0.314	-0.130 (0.104)	-0.559 (0.450)
Lives with friends/relatives or in shelter	742	0.437 0.497	0.034 (0.045)	0.142 (0.185)	725	0.128 0.334	0.014 (0.022)	0.060 (0.091)
TANF Receipt at baseline								
Not receiving TANF	500	0.414 0.494	0.008 (0.054)	0.048 (0.338)	418	0.064 0.245	0.002 (0.009)	0.015 (0.057)
Receiving TANF, expiring in:								
Less than 6 months	325	0.548 0.499	-0.213 *** (0.067)	-1.023 *** (0.321)	300	0.124 0.331	0.009 (0.035)	0.043 (0.167)
6 to 12 months	177	0.391 0.491	0.101 (0.112)	0.412 (0.457)	126	0.089 0.287	0.000 (0.000)	0.000 (0.000)
12 to 18 months	138	0.464 0.502	0.017 (0.110)	0.052 (0.340)	62	0.089 0.287	0.000 (0.000)	0.000 (0.000)
More than 18 months	227	0.325 0.470	-0.016 (0.082)	-0.070 (0.356)	140	0.075 0.265	0.000 *** (0.000)	0.000 *** (0.000)
Total receiving TANF	1,849	0.435 0.496	-0.007 (0.027)	-0.025 (0.096)	1,829	0.103 0.305	0.011 (0.014)	0.038 (0.051)

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.

^b An F test on the equality of treatment effects between subgroups indicates that ITT impacts on household food status differ significantly between subgroups at p<.10

^c An F test on the equality of treatment effects between subgroups indicates that ITT impacts on household members without meals differ significantly between subgroups at p<.10

**Exhibit G.1: Impacts by Subgroup on School Performance and Educational Progress Outcomes
(Survey Data--Parent-on-Child/Youth Module)**

Outcome Measure/ Subgroups	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Child is not in school due to any problem				
Boys	2,011	0.023 0.151	0.000 (0.008)	-0.001 (0.030)
Girls	1,986	0.024 0.153	-0.018 ** (0.008)	-0.079 ** (0.034)
Child under age 6 at baseline ^b	2,064	0.004 0.066	-0.004 ** (0.002)	-0.015 ** (0.007)
Child ages 6 - 9 at baseline ^b	1,002	0.001 0.029	-0.001 (0.001)	-0.003 (0.003)
Child age 10 or over at baseline ^b	954	0.082 0.274	-0.018 (0.020)	-0.071 (0.081)
Child's highest grade completed				
Boys	1,870	4.355 3.849	-0.016 (0.056)	-0.054 (0.197)
Girls	1,858	4.840 3.920	-0.056 (0.051)	-0.247 (0.224)
Child under age 6 at baseline ^b	1,874	1.167 1.276	-0.038 (0.039)	-0.157 (0.160)
Child ages 6 - 9 at baseline ^b	975	5.457 1.463	-0.018 (0.072)	-0.062 (0.247)
Child age 10 or over at baseline ^b	902	9.887 1.621	0.029 (0.099)	0.118 (0.396)
Child has completed HS or GED				
Boys ^b	2,019	0.031 0.174	0.007 (0.010)	0.025 (0.034)
Girls ^b	1,988	0.060 0.237	-0.033 *** (0.011)	-0.146 *** (0.046)
Child under age 6 at baseline ^b	2,066	0.000 0.000	0.000 (0.000)	0.000 (0.000)

**Exhibit G.1: Impacts by Subgroup on School Performance and Educational Progress Outcomes
(Survey Data--Parent-on-Child/Youth Module)**

Outcome Measure/ Subgroups	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Child ages 6 - 9 at baseline ^b	1,002	0.000 0.000	0.000 (0.000)	0.000 (0.000)
Child age 10 or over at baseline ^b	962	0.173 0.378	-0.006 (0.025)	-0.023 (0.100)
Child is enrolled in college				
Boys	2,019	0.016 0.126	0.006 (0.008)	0.022 (0.028)
Girls	1,988	0.032 0.175	-0.014 (0.009)	-0.062 (0.039)
Child under age 6 at baseline ^b	2,066	0.000 0.000	0.000 (0.000)	0.000 (0.000)
Child ages 6 - 9 at baseline ^b	1,002	0.000 0.000	0.000 (0.000)	0.000 (0.000)
Child age 10 or over at baseline ^b	962	0.091 0.288	0.004 (0.021)	0.018 (0.085)
Child has ever repeated a grade				
Boys	1,900	0.200 0.400	0.021 (0.021)	0.072 (0.075)
Girls	1,894	0.156 0.363	0.029 (0.022)	0.127 (0.095)
Child under age 6 at baseline ^b	1,936	0.104 0.305	0.024 (0.019)	0.096 (0.076)
Child ages 6 - 9 at baseline ^b	978	0.288 0.454	0.002 (0.034)	0.007 (0.117)
Child age 10 or over at baseline ^b	902	0.209 0.407	0.032 (0.033)	0.129 (0.130)
Child has ever been suspended or expelled				
Boys	1,897	0.217 0.412	-0.018 (0.022)	-0.064 (0.076)

**Exhibit G.1: Impacts by Subgroup on School Performance and Educational Progress Outcomes
(Survey Data--Parent-on-Child/Youth Module)**

Outcome Measure/ Subgroups	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Girls	1,888	0.084 0.277	0.023 (0.016)	0.100 (0.069)
Child under age 6 at baseline^b	1,935	0.062 0.242	-0.001 (0.012)	-0.003 (0.048)
Child ages 6 - 9 at baseline^b	975	0.233 0.423	-0.016 (0.031)	-0.053 (0.105)
Child age 10 or over at baseline^b	897	0.233 0.423	0.020 (0.034)	0.080 (0.135)
Parent was called into school to discuss problems with child in past 2 years				
Boys	1,848	0.321 0.467	-0.012 (0.024)	-0.042 (0.085)
Girls	1,836	0.157 0.364	0.000 (0.022)	-0.001 (0.097)
Child under age 6 at baseline^b	1,889	0.229 0.420	-0.011 (0.021)	-0.045 (0.087)
Child ages 6 - 9 at baseline^b	959	0.320 0.467	-0.016 (0.036)	-0.055 (0.122)
Child age 10 or over at baseline^b	859	0.179 0.384	-0.008 (0.033)	-0.034 (0.132)
Child has attended a special gifted class or done advanced work in last 2 years				
Boys	1,874	0.108 0.310	-0.006 (0.017)	-0.022 (0.058)
Girls	1,860	0.135 0.342	0.008 (0.019)	0.034 (0.083)
Child under age 6 at baseline	1,914	0.116 0.320	-0.012 (0.016)	-0.048 (0.065)
Child ages 6 - 9 at baseline	972	0.140 0.347	-0.002 (0.027)	-0.006 (0.093)

**Exhibit G.1: Impacts by Subgroup on School Performance and Educational Progress Outcomes
(Survey Data--Parent-on-Child/Youth Module)**

Outcome Measure/ Subgroups	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Child age 10 or over at baseline	870	0.115 0.319	-0.004 (0.028)	-0.016 (0.112)
Child has gone to a special class or gotten special help due to learning problems in last 2 years				
Boys	1,883	0.255 0.436	-0.017 (0.024)	-0.058 (0.084)
Girls	1,877	0.154 0.361	0.005 (0.021)	0.024 (0.093)
Child under age 6 at baseline ^b	1,924	0.187 0.390	0.022 (0.020)	0.088 (0.083)
Child ages 6 - 9 at baseline ^b	975	0.247 0.431	-0.031 (0.035)	-0.105 (0.121)
Child age 10 or over at baseline ^b	883	0.198 0.399	-0.047 (0.030)	-0.188 (0.121)
Child has gone to a special class or gotten special help due to behavioral/emotional problems in last 2 years				
Boys	1,887	0.159 0.366	0.013 (0.019)	0.045 (0.066)
Girls	1,880	0.066 0.248	0.021 (0.015)	0.091 (0.067)
Child under age 6 at baseline	1,926	0.102 0.303	0.010 (0.016)	0.041 (0.066)
Child ages 6 - 9 at baseline	974	0.122 0.327	0.018 (0.026)	0.062 (0.088)
Child age 10 or over at baseline	889	0.122 0.327	-0.002 (0.025)	-0.008 (0.099)
Child has received special services very frequently for these problems in school in last 2 years				

Exhibit G.1: Impacts by Subgroup on School Performance and Educational Progress Outcomes (Survey Data--Parent-on-Child/Youth Module)

Outcome Measure/ Subgroups	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Boys	1,982	0.232 0.422	-0.016 (0.023)	-0.057 (0.080)
Girls	1,970	0.131 0.337	0.008 (0.020)	0.036 (0.087)
Child under age 6 at baseline^b	2,046	0.162 0.369	0.023 (0.019)	0.092 (0.077)
Child ages 6 - 9 at baseline^b	986	0.247 0.432	-0.048 (0.034)	-0.163 (0.116)
Child age 10 or over at baseline^b	943	0.160 0.367	-0.037 (0.027)	-0.147 (0.108)

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.

^b An F test on the equality of treatment effects between subgroups indicates that ITT impacts differ significantly between subgroups at $p < .10$

**Exhibit G.2: Impacts by Subgroup on Child Behavior and Time Use
(Survey Data--Parent-on-Child/Youth Module)**

Outcome Measure/ Subgroups	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Child Behavior Problems Index [Parental Report]				
Boys	1,936	0.318 0.284	-0.011 (0.015)	-0.038 (0.053)
Girls	1,912	0.223 0.233	0.004 (0.014)	0.016 (0.063)
Child under age 6 at baseline ^b	2,013	0.297 0.258	-0.019 (0.014)	-0.077 (0.058)
Child ages 6 - 9 at baseline ^b	975	0.274 0.276	0.018 (0.020)	0.060 (0.070)
Child age 10 or over at baseline ^b	882	0.218 0.256	-0.003 (0.021)	-0.013 (0.086)
Child in childcare at 3:45 PM				
Boys	1,885	0.058 0.234	-0.006 (0.012)	-0.021 (0.040)
Girls	1,854	0.043 0.202	-0.005 (0.010)	-0.021 (0.043)
Child under age 6 at baseline ^b	1,999	0.093 0.290	-0.011 (0.016)	-0.044 (0.064)
Child ages 6 - 9 at baseline ^b	964	0.009 0.096	0.004 (0.007)	0.014 (0.024)
Child age 10 or over at baseline ^b	798	0.000 0.000	0.000 (0.000)	0.000 (0.000)
Child in activities at school at 3:45 PM				
Boys ^b	1,885	0.184 0.388	-0.062 *** (0.020)	-0.217 *** (0.069)
Girls ^b	1,854	0.134 0.341	0.033 * (0.020)	0.144 * (0.087)
Child under age 6 at baseline ^b	1,999	0.132 0.339	-0.044 ** (0.017)	-0.178 ** (0.071)

**Exhibit G.2: Impacts by Subgroup on Child Behavior and Time Use
(Survey Data--Parent-on-Child/Youth Module)**

Outcome Measure/ Subgroups	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Child ages 6 - 9 at baseline ^b	964	0.208 0.406	0.033 (0.030)	0.112 (0.103)
Child age 10 or over at baseline ^b	798	0.166 0.373	-0.023 (0.034)	-0.090 (0.137)
Child in other organized activities at 3:45 PM				
Boys	1,885	0.061 0.239	0.001 (0.013)	0.003 (0.045)
Girls	1,854	0.050 0.217	0.014 (0.014)	0.063 (0.062)
Child under age 6 at baseline	1,999	0.044 0.206	0.008 (0.012)	0.031 (0.047)
Child ages 6 - 9 at baseline	964	0.088 0.283	0.004 (0.021)	0.013 (0.072)
Child age 10 or over at baseline	798	0.042 0.200	0.016 (0.019)	0.063 (0.074)
Child could be seen or heard by an adult at 3:45 PM				
Boys ^b	1,879	0.959 0.199	-0.029 ** (0.014)	-0.101 ** (0.047)
Girls ^b	1,854	0.930 0.254	0.025 * (0.015)	0.108 * (0.065)
Child under age 6 at baseline	2,001	0.986 0.117	0.000 (0.008)	0.001 (0.031)
Child ages 6 - 9 at baseline	959	0.954 0.210	-0.007 (0.019)	-0.025 (0.065)
Child age 10 or over at baseline	795	0.843 0.364	0.006 (0.034)	0.025 (0.135)
Child currently working for pay				
Boys	1,982	0.032 0.176	0.006 (0.010)	0.020 (0.034)
Girls	1,958	0.046 0.210	-0.015 (0.010)	-0.067 (0.046)

**Exhibit G.2: Impacts by Subgroup on Child Behavior and Time Use
(Survey Data--Parent-on-Child/Youth Module)**

Outcome Measure/ Subgroups	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Child under age 6 at baseline ^b	2,061	0.000 0.000	0.000 (0.000)	0.000 (0.000)
Child ages 6 - 9 at baseline ^b	998	0.000 0.000	0.000 (0.000)	0.000 (0.000)
Child age 10 or over at baseline ^b	903	0.156 0.363	0.018 (0.028)	0.074 (0.111)
Child's number of close friends (extreme values reduced)				
Boys	1,634	3.357 2.515	0.238 (0.150)	0.831 (0.524)
Girls	1,635	3.386 2.535	-0.019 (0.142)	-0.083 (0.625)
Child under age 6 at baseline	1,725	3.297 2.348	0.051 (0.131)	0.209 (0.532)
Child ages 6 - 9 at baseline	841	3.398 2.442	-0.034 (0.199)	-0.115 (0.680)
Child age 10 or over at baseline	720	3.467 2.947	0.089 (0.254)	0.357 (1.018)
Child involved in sports during current school year				
Boys	1,912	0.289 0.454	0.007 (0.026)	0.025 (0.090)
Girls	1,873	0.251 0.434	-0.020 (0.026)	-0.089 (0.112)
Child under age 6 at baseline ^b	1,995	0.207 0.405	-0.015 (0.023)	-0.063 (0.092)
Child ages 6 - 9 at baseline ^b	968	0.419 0.494	-0.042 (0.039)	-0.145 (0.134)
Child age 10 or over at baseline ^b	843	0.244 0.430	0.016 (0.038)	0.064 (0.150)

**Exhibit G.2: Impacts by Subgroup on Child Behavior and Time Use
(Survey Data--Parent-on-Child/Youth Module)**

Outcome Measure/ Subgroups	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Child involved in club or extracurricular activity during current school year				
Boys	1,911	0.221 0.415	0.004 (0.024)	0.013 (0.083)
Girls	1,875	0.319 0.466	0.002 (0.028)	0.010 (0.122)
Child under age 6 at baseline^b	1,992	0.223 0.416	-0.024 (0.023)	-0.098 (0.092)
Child ages 6 - 9 at baseline^b	967	0.399 0.490	0.008 (0.037)	0.029 (0.128)
Child age 10 or over at baseline^b	848	0.225 0.418	0.014 (0.036)	0.057 (0.144)

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.

^b An F test on the equality of treatment effects between subgroups indicates that ITT impacts differ significantly between subgroups at $p < .10$

**Exhibit G.3: Impacts by Subgroup on Delinquency and Risky Behavior
(Survey Data--Parent-on-Child/Youth Module)**

Outcome Measure/ Subgroups	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Post random assignment, child had problems involving police contacting parent^c				
Boys	723	0.244 0.430	-0.065 * (0.035)	-0.228 * (0.124)
Girls	729	0.121 0.326	0.018 (0.029)	0.077 (0.127)
Child under age 6 at baseline	-	-	-	-
Child ages 6 - 9 at baseline	564	0.131 0.338	-0.026 (0.032)	-0.091 (0.111)
Child age 10 or over at baseline	895	0.211 0.409	-0.040 (0.032)	-0.159 (0.130)
Post random assignment, child has been arrested^c				
Boys	722	0.175 0.380	-0.027 (0.032)	-0.093 (0.113)
Girls	731	0.055 0.229	0.007 (0.019)	0.030 (0.083)
Child under age 6 at baseline	-	-	-	-
Child ages 6 - 9 at baseline	563	0.050 0.218	-0.016 (0.025)	-0.054 (0.086)
Child age 10 or over at baseline	897	0.153 0.360	-0.011 (0.029)	-0.044 (0.117)
Child's number of arrests since random assignment^c				
Boys	720	0.256 0.638	-0.032 (0.055)	-0.113 (0.192)
Girls	729	0.076 0.356	-0.010 (0.032)	-0.046 (0.138)
Child under age 6 at baseline	-	-	-	-

**Exhibit G.3: Impacts by Subgroup on Delinquency and Risky Behavior
(Survey Data--Parent-on-Child/Youth Module)**

Outcome Measure/ Subgroups	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Child ages 6 - 9 at baseline	563	0.059 0.272	-0.030 (0.036)	-0.103 (0.122)
Child age 10 or over at baseline	893	0.230 0.620	-0.026 (0.050)	-0.104 (0.198)
Post random assignment, child has been convicted of a crime^c				
Boys	719	0.103 0.304	-0.029 (0.026)	-0.100 (0.091)
Girls	732	0.018 0.134	0.003 (0.014)	0.015 (0.060)
Child under age 6 at baseline	-	-	-	-
Child ages 6 - 9 at baseline	563	0.043 0.203	-0.031 (0.021)	-0.105 (0.072)
Child age 10 or over at baseline	895	0.070 0.256	-0.004 (0.021)	-0.016 (0.086)
Child is incarcerated, in detention facility, in boot camp or similar institution				
Boys	2,011	0.004 0.066	0.006 (0.005)	0.022 (0.016)
Girls	1,986	0.000 0.000	0.001 (0.001)	0.005 (0.006)
Child under age 6 at baseline^b	2,064	0.000 0.000	0.000 (0.000)	0.000 (0.000)
Child ages 6 - 9 at baseline^b	1,002	0.000 0.000	0.000 (0.000)	0.000 (0.000)
Child age 10 or over at baseline^b	954	0.009 0.092	0.010 (0.011)	0.040 (0.044)

**Exhibit G.3: Impacts by Subgroup on Delinquency and Risky Behavior
(Survey Data--Parent-on-Child/Youth Module)**

Outcome Measure/ Subgroups	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Child hangs around with kids who get into trouble				
Boys^b	1,925	0.193 0.395	-0.019 (0.020)	-0.067 (0.070)
Girls^b	1,909	0.092 0.290	0.034 ** (0.017)	0.151 ** (0.076)
Child under age 6 at baseline	2,008	0.106 0.308	-0.001 (0.016)	-0.006 (0.067)
Child ages 6 - 9 at baseline	974	0.165 0.372	0.026 (0.029)	0.089 (0.099)
Child age 10 or over at baseline	874	0.198 0.399	-0.019 (0.032)	-0.075 (0.128)

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.

^b An F test on the equality of treatment effects between subgroups indicates that ITT impacts differ significantly between subgroups at $p < .10$

^c Follow-up survey questions were asked only about youth ages 12 and over.

Exhibit G.4: Impacts by Subgroup on Child's Family Life (including Family Composition)
(Survey Data--Parent-on-Child/Youth Module)

Outcome Measure/ Subgroups	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Parent knows all or most of child's friends				
Boys	1,899	0.853 0.354	-0.004 (0.021)	-0.014 (0.074)
Girls	1,891	0.863 0.344	-0.006 (0.021)	-0.026 (0.092)
Child under age 6 at baseline	1,986	0.862 0.345	0.007 (0.021)	0.030 (0.085)
Child ages 6 - 9 at baseline	964	0.870 0.336	0.011 (0.026)	0.038 (0.089)
Child age 10 or over at baseline	860	0.832 0.374	-0.023 (0.036)	-0.091 (0.142)
Parent knows child's teacher very well or well				
Boys	1,827	0.677 0.468	0.007 (0.025)	0.023 (0.089)
Girls	1,816	0.691 0.462	-0.017 (0.027)	-0.075 (0.118)
Child under age 6 at baseline	1,933	0.829 0.377	-0.004 (0.023)	-0.018 (0.095)
Child ages 6 - 9 at baseline	972	0.601 0.490	0.000 (0.037)	-0.001 (0.125)
Child age 10 or over at baseline	761	0.453 0.498	-0.009 (0.045)	-0.038 (0.181)
Child attends religious services				
Boys	1,924	0.612 0.488	0.000 (0.029)	0.001 (0.102)
Girls	1,902	0.633 0.482	0.014 (0.027)	0.060 (0.118)
Child under age 6 at baseline	2,002	0.659 0.474	0.003 (0.028)	0.014 (0.114)

Exhibit G.4: Impacts by Subgroup on Child's Family Life (including Family Composition)
(Survey Data--Parent-on-Child/Youth Module)

Outcome Measure/ Subgroups	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Child ages 6 - 9 at baseline	971	0.623 0.485	0.025 (0.038)	0.086 (0.129)
Child age 10 or over at baseline	875	0.539 0.499	-0.008 (0.042)	-0.032 (0.170)
Child attends religious services at least weekly				
Boys	1,916	0.335 0.472	0.017 (0.028)	0.058 (0.098)
Girls	1,894	0.379 0.485	0.004 (0.029)	0.017 (0.126)
Child under age 6 at baseline	2,000	0.389 0.488	-0.009 (0.030)	-0.038 (0.123)
Child ages 6 - 9 at baseline	968	0.357 0.480	0.040 (0.037)	0.136 (0.126)
Child age 10 or over at baseline	864	0.285 0.452	0.003 (0.040)	0.013 (0.158)
Number of days per week that family eats together				
Boys	1,947	5.546 2.115	0.128 (0.137)	0.446 (0.478)
Girls	1,926	5.677 2.001	0.001 (0.123)	0.006 (0.540)
Child under age 6 at baseline ^b	2,015	5.976 1.777	-0.152 (0.110)	-0.617 (0.448)
Child ages 6 - 9 at baseline ^b	980	5.447 2.112	0.332 ** (0.167)	1.134 ** (0.570)
Child age 10 or over at baseline ^b	901	5.080 2.364	0.087 (0.243)	0.347 (0.973)

Exhibit G.4: Impacts by Subgroup on Child's Family Life (including Family Composition)
(Survey Data--Parent-on-Child/Youth Module)

Outcome Measure/ Subgroups	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Child is currently living with adult respondent who is the child's parent^c				
Boys	2,012	0.846 0.361	0.024 (0.019)	0.085 (0.067)
Girls	1,983	0.855 0.352	0.002 (0.019)	0.010 (0.085)
Child under age 6 at baseline ^b	2,061	0.910 0.287	-0.003 (0.016)	-0.013 (0.064)
Child ages 6 - 9 at baseline ^b	1,000	0.889 0.314	0.055 ** (0.022)	0.189 ** (0.075)
Child age 10 or over at baseline ^b	956	0.708 0.455	-0.027 (0.033)	-0.110 (0.134)
Child is currently living in a multigenerational household				
Boys	2,019	0.124 0.330	-0.041 ** (0.019)	-0.142 ** (0.068)
Girls	1,988	0.160 0.367	-0.078 *** (0.020)	-0.344 *** (0.087)
Child under age 6 at baseline	2,066	0.133 0.340	-0.056 *** (0.019)	-0.230 *** (0.078)
Child ages 6 - 9 at baseline	1,002	0.149 0.357	-0.072 *** (0.025)	-0.247 *** (0.086)
Child age 10 or over at baseline	962	0.144 0.352	-0.032 (0.030)	-0.130 (0.121)
Child is currently living in a nuclear family (two parents plus children only)				
Boys ^b	2,019	0.232 0.422	-0.015 (0.024)	-0.052 (0.085)
Girls ^b	1,988	0.197 0.398	0.037 (0.024)	0.161 (0.105)

Exhibit G.4: Impacts by Subgroup on Child's Family Life (including Family Composition)
(Survey Data--Parent-on-Child/Youth Module)

Outcome Measure/ Subgroups	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Child under age 6 at baseline	2,066	0.207 0.405	0.010 (0.024)	0.043 (0.097)
Child ages 6 - 9 at baseline	1,002	0.173 0.378	0.057 * (0.030)	0.196 * (0.102)
Child age 10 or over at baseline	962	0.267 0.443	-0.031 (0.038)	-0.123 (0.154)
Child is currently living in a single parent family with children only				
Boys	2,019	0.626 0.484	0.065 ** (0.027)	0.228 ** (0.094)
Girls	1,988	0.615 0.487	0.055 ** (0.028)	0.240 ** (0.121)
Child under age 6 at baseline	2,066	0.637 0.481	0.060 ** (0.027)	0.243 ** (0.109)
Child ages 6 - 9 at baseline	1,002	0.658 0.475	0.028 (0.034)	0.097 (0.116)
Child age 10 or over at baseline	962	0.559 0.497	0.074 * (0.042)	0.298 * (0.170)
Child is currently living in a another family configuration				
Boys	2,019	0.018 0.134	-0.010 (0.007)	-0.034 (0.025)
Girls	1,988	0.028 0.166	-0.013 (0.008)	-0.057 (0.036)
Child under age 6 at baseline	2,066	0.023 0.150	-0.014 * (0.007)	-0.056 * (0.029)
Child ages 6 - 9 at baseline	1,002	0.019 0.138	-0.013 (0.009)	-0.046 (0.030)
Child age 10 or over at baseline	962	0.029 0.168	-0.011 (0.012)	-0.045 (0.048)

Exhibit G.4: Impacts by Subgroup on Child's Family Life (including Family Composition)
(Survey Data--Parent-on-Child/Youth Module)

Outcome Measure/ Subgroups	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
In past year, parent attended meeting at child's school				
Boys	1,956	0.831 0.375	-0.027 (0.024)	-0.093 (0.084)
Girls	1,928	0.818 0.386	-0.016 (0.027)	-0.071 (0.118)
Child under age 6 at baseline ^b	2,020	0.871 0.335	-0.066 *** (0.023)	-0.268 *** (0.092)
Child ages 6 - 9 at baseline ^b	986	0.806 0.396	0.064 * (0.033)	0.220 * (0.112)
Child age 10 or over at baseline ^b	901	0.758 0.429	-0.019 (0.043)	-0.078 (0.172)
In past year, parent attended event at child's school				
Boys	1,958	0.690 0.463	-0.019 (0.030)	-0.065 (0.104)
Girls	1,934	0.684 0.465	-0.001 (0.028)	-0.005 (0.124)
Child under age 6 at baseline ^b	2,025	0.728 0.445	-0.048 * (0.028)	-0.197 * (0.113)
Child ages 6 - 9 at baseline ^b	987	0.693 0.462	0.020 (0.037)	0.068 (0.126)
Child age 10 or over at baseline ^b	903	0.608 0.489	0.026 (0.046)	0.104 (0.183)
In past year, parent volunteered at child's school				
Boys	1,955	0.378 0.485	-0.009 (0.030)	-0.031 (0.105)
Girls	1,934	0.374 0.484	-0.008 (0.030)	-0.034 (0.133)

Exhibit G.4: Impacts by Subgroup on Child's Family Life (including Family Composition)
(Survey Data--Parent-on-Child/Youth Module)

Outcome Measure/ Subgroups	Fifth Year, All Sites except Los Angeles			
	Sample Size	Control Mean ^a	ITT Impact	TOT Impact
Child under age 6 at baseline	2,024	0.438 0.496	-0.031 (0.031)	-0.127 (0.125)
Child ages 6 - 9 at baseline	986	0.378 0.486	0.000 (0.040)	-0.001 (0.136)
Child age 10 or over at baseline	902	0.253 0.435	0.065 (0.043)	0.260 (0.173)
In past year, parent worked with youth group or other activity outside of school				
Boys	1,957	0.204 0.403	-0.041 * (0.023)	-0.142 * (0.081)
Girls	1,933	0.204 0.403	-0.017 (0.025)	-0.077 (0.112)
Child under age 6 at baseline	2,024	0.192 0.394	-0.017 (0.023)	-0.069 (0.093)
Child ages 6 - 9 at baseline	987	0.240 0.427	-0.056 * (0.032)	-0.190 * (0.108)
Child age 10 or over at baseline	902	0.190 0.393	-0.031 (0.038)	-0.124 (0.154)

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.

^b An F test on the equality of treatment effects between subgroups indicates that ITT impacts differ significantly between subgroups at $p < .10$

^c May be a biological, adoptive, foster, or step parent.