Institute for Research on Poverty
Discussion Paper no. 1363-09

Long-Term Effects of Public Low-Income Housing Vouchers on Labor Market Outcomes

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April 2009

The research presented in this paper was generously supported by a grant from the John D. and Catherine T. MacArthur Foundation. We gratefully acknowledge that support. We would like to thank Larry Buron, Carolyn Heinrich, Jeffrey Kling, John Mullahy, Gary Painter, and Jim Riccio for helpful comments on earlier drafts of this paper. We would also like to thank Dan Ross for his work in securing, cleaning, and organizing the data, and Deborah Johnson for help in editing.

Abstract

The federal Housing Choice Voucher (Section 8) Program provides eligible low-income families with an income-conditioned voucher that pays for a portion of rental costs in privately owned, affordable housing units. This paper extends prior research on the effectiveness of rental support programs in several ways. The analysis employs a unique longitudinal dataset created by combining administrative records maintained by the State of Wisconsin with census block group data. We use a propensity score matching approach coupled with difference-in-differences regression analysis to estimate the effect of housing voucher receipt on the employment and earnings of voucher recipients; we track these effects for five years following voucher receipt. Our results indicate that voucher receipt has a generally positive effect on employment, but a negative impact on earnings. The negative earnings effect is largest in the years following initial receipt of the rental voucher, and dissipates over time. We find that the pattern of recipient labor market responses to voucher receipt differs substantially among demographic subgroups. In addition to our overall results, we present sensitivity results involving alternative estimation methods, as well as distinctions between those who receive transitory voucher support and those who are long-term recipients.

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I. INTRODUCTION

In recent decades, low-income housing policy in the United States has expanded the provision of vouchers that can be used to secure housing in the private market. Research has followed implementation of this policy expansion, and an ever-growing body of studies examines the behavioral responses of individuals to receipt of voucher-based housing assistance (Newman, Holupka, and Harkness 2009; Jacob and Ludwig 2008; Susin 2005). Many of these analyses examine the impact of voucher receipt on economic self-sufficiency and labor market supply, and have provided valuable, if inconclusive, insights into the labor market responses of recipients (see Shroder 2002a for a review essay).

This study examines the effect of voucher-based housing assistance on geographic moves and on recipient labor market behavior, but it extends previous work on the topic in several ways. First, this analysis utilizes a panel dataset that contains earnings and employment information on individuals for up to five years after initial voucher receipt, a longer period of time than most existing studies of the issue. Second, whereas prior research has mostly focused on the effects of voucher receipt in large urban settings, this study analyzes voucher recipients coming from all parts of a medium-sized, diverse state. Third, large sample sizes allow us to analyze a wider range of demographic subgroups than any prior study, and to gauge whether the effect of voucher receipt on labor market outcomes varies by subgroup.

Our results indicate that voucher receipt leads to geographic moves, has little effect on employment, and has a negative impact on earnings. The negative earnings effect is largest in the early years following initial receipt, and dissipates over time. Finally, we find that the pattern of recipient labor market responses to voucher receipt differs substantially across demographic subgroups.

II. THE SECTION 8 VOUCHER PROGRAM

A. Program Background

The U.S. government currently provides housing assistance to low-income renters through three primary programs: Section 8 tenant-based subsidies (since 1999 officially known as the Housing Choice Voucher Program); Section 8 unit-based assistance, under which building owners receive government subsidies to reduce rents; and publicly owned housing units.[1] All three forms of assistance are administered by over 3,000 local public housing authorities (PHAs). These agencies were originally established to build and manage public housing developments, but were also given responsibility for the Section 8 programs in the Housing and Community Development Act of 1974.

Section 8 tenant-based vouchers currently serve about 1.9 million families nationally, including more than 850,000 families with minor children (U.S. Department of Housing and Urban Development 2007). The primary objective of the program is to enable "very low-income families to choose and lease or purchase safe, decent, and affordable privately owned rental housing."[2] Voucher recipients, whose income must be below 50 percent of the median income of the county or metropolitan area in which they live, choose rental housing available in the private market and contribute 30 percent of their incomes toward rent.[3] The Section 8 program then pays the difference between the tenant contribution and actual rent, up to a locally defined "fair market rent" payment standard.[4]

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[1]The "Section 8" designation refers to the program's statutory authorization under Section 8 of the United States Housing Act of 1937, as amended by the Housing and Community Development Act of 1974. Although the official title of Section 8 tenant-based assistance is now called the Housing Choice Voucher Program, most researchers and administrators still refer to it as the "Section 8 voucher" program. We use the "Section 8" designation in this paper.

[2]http://www.hud.gov/offices/pih/programs/hcv/about/index.cfm

[3]A PHA must provide 75 percent of its vouchers to applicants whose incomes do not exceed 30 percent of the area median income.

[4]This standard is set by the Department of Housing and Urban Development (HUD) at the 40th percentile of the local rental market, as calculated by the monetary value of leases commenced in the previous year. The payment standard is typically between 90 percent and 110 percent of area "fair market rent."

A main motivation undergirding the Section 8 program is to "deconcentrate" the poor by making it possible for voucher recipients to leave public housing projects and move to better neighborhoods (U.S. Department of Housing and Urban Development 2000).[5] Empirical research on the effects of low-income housing vouchers is extensive, and some studies show that Section 8 voucher recipients are less likely than public housing residents to live in high-poverty neighborhoods.[6] Whether housing vouchers themselves are responsible for a movement to more prosperous neighborhoods and whether such location change leads to other benefits remain controversial questions.

B. Conceptual Issues

Given both the positive changes in employment opportunities that voucher receipt may offer and the work disincentives that are implicit in the program, standard economic theory is not able to provide unambiguous predictions regarding expected program impacts (Shroder 2002a; Jacob and Ludwig 2008). Voucher recipients could use the opportunity provided by their voucher to find housing closer to areas with available jobs and child care, with better schools, and with lower crime rates. Such moves could increase adult earnings and incomes, reduce reliance on welfare assistance, and offer better outcomes for children.

Alternatively, several factors may lead voucher recipients to reduce work effort and earnings in the short term, the long term, or both. First, the difficulties and disruptions associated with preparation for and execution of a move to a different neighborhood, even one with better job opportunities, may lead a

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[5]As the program has expanded over time, a number of constraints have partially interfered with the goal of geographic mobility for recipients of tenant-based assistance. One constraint has been the limited geographic span of many local PHAs that serve only parts of metropolitan areas, reducing the possibility for recipients to move to neighborhoods with a smaller concentration of poor families. While some PHAs allow recipients to find housing in other jurisdictions, administrative burdens and the need to transfer supporting funds constrains this practice. Such transfers also impose additional costs on recipients in the form of duplicate application, orientation, and program criteria (Katz and Turner 2000).

[6]Newman and Schnare (1997) found that 54 percent of public housing residents lived in neighborhoods in which more than 30 percent of residents were poor, whereas only 15 percent of Section 8 voucher recipients lived in such neighborhoods. Khadduri, Shroder, and Steffan (2003) found that just 9 percent of Section 8 recipients lived in census tracts in which 40 percent of the residents were poor.

new voucher recipient to temporarily work fewer hours in an existing job, or to search for a different job. A move to a new neighborhood may also disrupt natural social and support arrangements, which could lead to lower attainments across a variety of economic and social dimensions (Ross, Reynolds, and Geis 2000; Swartz and Miller 2002). Second, the Section 8 program is designed in such a manner that a voucher recipient's benefit level is partly determined by the recipient's income. Put another way, voucher recipients' Section 8 benefits fall as their income rises. As a result, voucher receipt may discourage recipients from additional earnings in both the short and long run (Van Ryzin, Kaestner, and Main 2003).[7] Finally, voucher recipients may voluntarily choose more nonwork time due to the effective "income" gain associated with housing benefits.

C. Empirical Research on the Labor Market Effects of Section 8 Voucher Receipt

An extensive body of research, both experimental and nonexperimental, examines the labor market effects of Section 8 voucher receipt. A comprehensive review of early work on this topic was performed by Mark Shroder (2002a). In this review, Shroder presents mixed evidence on the effect of voucher receipt, and concludes that housing assistance exhibits no discernable effect on individuals' labor market supply.

In the years since Shroder's review was published, a number of additional studies have analyzed the relationship between receipt of housing assistance and labor market outcomes. These studies are summarized in Tables 1 and 1a.

Overall, neither the experimental nor the nonexperimental research literature seems to reach a definitive conclusion on the effect of housing assistance on labor market outcomes. However, the recent trend in the literature seems to suggest negative work and earnings effects associated with voucher receipt. Among relevant experiments, Moving to Opportunity found lower earnings among voucher

[7]That may be a particular issue for voucher recipients near the income threshold for receipt of benefits; a voucher recipient whose earnings rise too much for voucher eligibility has no assurance that a voucher will be available again following a job loss and a decline in income.

Table 1
Summary of Nonexperimental Literature on Relationship between Housing Assistance Receipt and Labor Market Outcomes

Study	Comparison Groups	Location and Timing	Data and Methodology	Effects on Employment or Earnings
Bania, Coulton, and Leete (2003)	Welfare leavers receiving Section 8 vouchers; other welfare leavers	Cleveland/Cuyahoga County; 1996 followed through 1997	Administrative data; Regression model	No significant effect
Susin (2005)	Housing assistance recipients; low-income nonrecipients	Nationally representative sample; 1996 followed through 1999	SIPP combined with administrative data on housing assistance receipt; Propensity score matching	Earnings of housing subsidy recipients reduced by about 15 percent.
Van Ryzin, Kaestner, and Main (2003)	Welfare recipients receiving Section 8 vouchers; welfare recipients receiving project housing assistance or no housing assistance	New York City; 1995–1996.	Local survey data; Logistic regression controlling for a variety of observed differences	No significant effect
Olsen et al. (2005)	Housing assistance recipients; housing project residents and low-income households receiving no housing aid	Nationally representative sample; 1995–2002	PSID combined with HUD data on recipients; Regression model with income ceiling imposed on comparison group	Earnings of housing subsidy recipients decline 30-35 percent.
Harkness and Newman (2006)	Single mothers receiving housing assistance; Single mothers receiving no assistance	National HUD data; 1996 and 2001	HUD data combined with CPS for comparison group; Simple comparison of univariate results with some focus on regression	No significant effect
Newman, Holupka, and Harkness (2009)	Project-based housing assistance recipients; Matched group of non-recipients	Nationally representative; Many different cohorts followed 6 years after receipt	PSID-Assisted Housing Database; Propensity score matching	No significant effect

Table 1a
Summary of Experimental Literature on Relationship between Housing Assistance Receipt and Labor Market Outcomes

Study	Comparison Groups	Location and Timing	Data and Methodology	Effects on Employment or Earnings
Gautreaux[a]	Housing project residents who moved to suburbs; project residents who left the projects but remained in the city	Chicago; 1976–1998	Experiment; possible internal validity concerns	Movers to suburbs had higher earnings
Moving to Opportunity[b]	Housing project residents who moved to low-poverty neighborhoods; residents who moved to any neighborhood; remained in project housing	Housing assistance distributed to 4,600 families in 5 cities (Baltimore, Boston, Chicago, Los Angeles, and New York); 1994–1998	Participants were randomly assigned to areas with poverty rates below 10 percent, any rental unit that accepted a Section 8 voucher, or a control group that remained in housing project	No significant differences; Study is not yet completed
Welfare-to-Work[c]	Welfare recipients or eligible recipients who received a housing voucher; welfare recipients or eligible recipients who did not receive a voucher	8,700 families in 6 cities (Atlanta, Augusta, Fresno, Houston, Los Angeles, and Spokane) assigned to treatment and control groups between spring 2000 and spring 2001	Random assignment to a housing voucher or to a control group that received no housing voucher through the program	Voucher recipients initially worked and earned less, but the differences disappeared after 3.5 years; Study is not yet completed
Chicago Housing Authority Experiment[d]	Members of a housing assistance waiting list who were randomly selected to receive a housing voucher; wait-list members not selected to receive a housing voucher	82,600 households in Chicago in July 1997 were placed on a waiting list for Section 8 vouchers and assigned a position on the waiting list from 1997–2003 by random assignment	Random assignment of households that applied for Section 8 to a housing voucher or to no housing voucher	Voucher recipients worked and earned significantly less than those who did not receive a voucher over the full follow-up period

[a]See Rosenbaum (1995); Popkin et al. (2000); Rosenbaum and DeLuca (2000); and Mendenhall, DeLuca, and Duncan (2006).
[b]See Goering, Feins, and Richardson (2002); Shroder (2002b); Goering (2003); Turney et al. (2006); and Kling, Liebman, and Katz (2007).
[c]See Mills et al. (2006).
[d]See Jacob and Ludwig (2006; 2008).

recipients in the first few years of the experiment, but not by a statistically significant amount. The Welfare to Work study also found lower earnings for voucher recipients immediately post-treatment, but evaluators found that this negative earnings effect dissipated over time. The Chicago natural experiment found lower earnings that persisted for a longer time. Taken together, the literature suggests that, if anything, housing assistance receipt exerts a negative effect on employment and earnings.

III. OUR RESEARCH APPROACH

In this paper, we study the effect of housing voucher receipt on geographic movement and labor market outcomes for a large number of low-income individuals and families in Wisconsin that requested or received food stamps and/or TANF benefits. The sample includes both urban and rural residents and those with and without past experience in public housing projects. It includes cases with a variety of familial compositions, from single individuals to married couples with multiple children. By including urban and rural residents and those of all family types, this research studies a substantially broader sample than has been the case in prior research on housing vouchers.

Our data also allow us to pool multiple years of observations and create very large sample sizes. These benefits of our data enable us to follow families who first received Section 8 vouchers in 2001 through 2003 over multiple years after their entry into the program. Hence, we are able to obtain estimates of the relationship between housing voucher receipt and both short- and longer-term labor market success, as measured by earnings and employment rates, for the families we study. Given the large sample sizes, we are able to conduct several subgroup analyses; we distinguish groups by gender, race/ethnicity, age, education level, family composition, and urbanicity. This subgroup analysis represents a unique and important contribution of this research.

A. Data and Estimation Sample

In our analysis, we use detailed information available in administrative records from the State of Wisconsin, and supplement this information with data from the United States Census Bureau. The

assembly of our dataset was a multi-stage process that drew on a wide variety of resources. The first step in this process consisted of extracting records of rental subsidy receipt, demographic characteristics, address history, and participation in means-tested programs from the Client Assistance for Re-employment and Economic Support (CARES) system, a database maintained by the State of Wisconsin.[8] The rental subsidy receipt data come from questions asked in the administration of the Wisconsin food stamp program. We then added quarterly employment and earnings information to each family record over the years of observation by merging the data obtained from the CARES database with employer-reported data on individuals' quarterly earnings from the Unemployment Insurance (UI) system, another database maintained by Wisconsin state government. By using administrative data, our information on the receipt of housing assistance and on work and earnings is likely to be superior to that obtained from survey information (Wallace and Haveman 2007).[9]

Following these steps, we determined the address history for each case and commissioned the University of Wisconsin–Madison's Applied Population Lab to match each address in each case's history to a census block group and provide us with a variety of characteristics associated with each block

[8]CARES includes demographic data on the family and all individuals living in the household, as well as quarterly information on the receipt of benefits from public support programs, including food stamps and TANF. Hence, the data include the age, race, and disability status of all members of the living unit, as well as the years of education for the casehead. In addition, the family's quarterly history of participation in means-tested programs, the income and address of the family, whether the family lives in government or project-subsidized housing, rental costs, and the level of any housing assistance received by a family not residing in government or project-subsidized housing are all included in the database. In 2003, some 470,000 cases were open at some time in the CARES database.

[9]County income maintenance workers ask new applicants and, at regular recertification sessions held every six months, current participants whether they are receiving a housing subsidy or live in government or project-subsidized housing. Those who respond that they receive a housing subsidy are coded separately from those who indicate that they live in government or project-subsidized housing. Shroder and Martin (1996) present evidence that survey respondents do not accurately answer questions about housing assistance in nationally representative datasets. However, administrative data to operate programs like food stamps are collected differently than are survey data on housing benefits; respondents to administrative data questions know that the accuracy of their answers may be verified, and the questions about housing assistance for food stamp administration are asked in the context of other questions on utility and other expenses that are likely to help respondents recall their exact shelter costs and benefits.

group.[10] We then merged these census block group characteristics with the data extracted from the CARES and UI databases to form our final dataset. By matching each address in a case's history to a census block group, we are able to observe, for each case, neighborhood characteristics prior to rental subsidy receipt and at any point after voucher receipt.

Our sample begins with all cases that applied for or received food stamps between 2001 and 2003, yielding three separate calendar year cohorts. Within each cohort, we formed two unique groups, one composed of families that first received a public rental subsidy in that year, and the other made up of families that did not. For the 2001 through 2003 cohorts, a unit is defined as being in the voucher group if the CARES case file indicates that the unit first received a rental subsidy in this particular calendar year or if the case file indicates that the case received a rental subsidy after a minimum of two consecutive months of nonreceipt.[11] Nonrecipient units are those that received (or were in some stage of applying for) food stamps or TANF, but did not join the voucher group according to the criteria described above. Table 2 summarizes the demographic characteristics of the two groups for the three cohorts used in our analysis.

B. Estimation Strategy

As described earlier, our sample of rental subsidy recipients consists of cases from the much larger population of all cases that applied for or receive food stamp benefits. In order to obtain a balanced comparison group that allows for valid inference regarding the effect of voucher receipt on casehead

[10]The neighborhood information that we attach to each family record for each year uses the dimensions identified in Feins (2003) and includes: percentage of persons in poverty, percentage of households receiving public assistance income, percentage of female-headed families with children, percentage of high school dropouts, unemployment rate, labor force participation rate, percentage of families with no workers, percentage of people with incomes twice the poverty level, percentage of people with education beyond high school, percentage of 16- to 19-year-olds in school, percentage of housing stock that is owner-occupied, median family income, racial composition, median house value, and median gross rent.

[11]We also excluded a few cases that were recorded as earning over $50,000 in a calendar year.

Table 2
Demographic Characteristics for Those Who Receive Rental Subsidies and Those Who Do Not Receive Rental Subsidies: 2001–2003 Cohorts

	2001		2002		2003	
Characteristic	Receive Rent Subsidy	Do Not Receive Rent Subsidy	Receive Rent Subsidy	Do Not Receive Rent Subsidy	Receive Rent Subsidy	Do Not Receive Rent Subsidy
Total Number of Cases	6,159	163,391	6,080	187,276	5,383	216,064
Sex						
Male	15.6	24.8	15.2	26.7	15.3	28.0
Female	84.4	75.2	84.8	73.3	84.7	72.1
Age						
18–30	42.9	37.9	47.0	38.4	48.0	39.1
31–45	29.9	36.0	30.2	36.1	28.3	35.8
46–59	12.6	13.5	12.3	14.2	14.0	14.9
60+	14.2	12.1	10.3	10.8	9.4	9.7
Race						
White	59.3	48.2	58.7	48.9	60.4	50.9
Black	29.0	36.2	28.5	35.6	26.9	34.5
Hispanic	3.3	6.2	3.7	6.3	3.1	6.3
Other	8.5	9.4	9.2	9.2	9.6	8.4
Education Level						
No high school diploma	35.4	39.7	34.1	37.9	33.2	36.3
High school diploma	64.6	60.3	65.9	62.1	66.8	63.7
Marital Status						
Single, never married	50.5	50.2	52.0	51.4	52.0	52.7
Divorced or annulled	20.4	17.9	21.4	17.7	21.7	17.5
Separated	11.3	10.7	11.2	10.4	11.0	9.8
Married	10.7	15.1	10.3	15.3	10.7	15.5
Widowed	7.0	6.0	5.1	5.2	4.6	4.5
County Urbanicity						
Rural	28.0	21.6	26.9	21.6	30.6	22.2
Urban	45.9	31.0	51.8	32.0	51.3	33.7
Milwaukee	26.1	47.4	21.3	46.4	18.1	44.1
Number of Children						
0	39.8	44.9	36.1	45.9	35.9	48.1
1	25.6	21.2	27.6	21.3	27.2	21.1
2	19.1	16.5	19.6	16.3	20.1	15.6
3+	15.5	17.4	16.8	16.4	16.9	15.2

employment and earnings, we employ a propensity score matching procedure.[12] Then, using the balanced sample created through the propensity score matching procedure, we isolate the effect of voucher receipt on labor market outcomes—earnings and employment—using a difference-in-differences regression framework.

Obtaining Voucher and Matched Comparison Groups: A Propensity Score Matching Approach

Propensity score matching procedures have been shown to be effective in achieving balanced samples and eliminating bias when there is both a large group of potential comparison cases, and these cases are demographically similar and geographically proximate. Our sample contains 12,170 voucher cases and 342,373 potential comparison cases to which the voucher cases can be matched. We excluded from our pool of potential comparisons any case living in public housing and any case that received a voucher at any point during our period of evaluation. As a result, the pool of potential matches is composed of cases that have applied for or received food stamps and have not received a housing voucher or lived in public housing at any point between 2001 and 2006. All of these cases reside in Wisconsin, and all have characteristics that enable the family to receive food stamps.

Propensity score matching procedures perform best when baseline measures of outcome variables are included in the matching equation used to estimate the propensity score for each case.[13] Our data contain an extensive set of covariates that are predictive of a case's receipt of a housing voucher,

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[12]The propensity score is the estimated probability that a given case will participate in the program. The primary papers describing propensity score matching approaches include Rosenbaum and Rubin (1983); Heckman, Ichimura, Smith, and Todd (1996, 1998); Heckman, Ichimura, and Todd (1997); Smith and Todd (2005). Applications of the method include Dehejia and Wahba (1999, 2002); Lechner (2002); Hotz, Imbens, and Klerman (2002); and Dyke et al. (2006). A recent study, Wilde and Hollister (2007), compares results on a composite reading and math test score for Tennessee primary students who were randomly assigned (or not) to smaller classes (considered to be the "true" result) with estimates from a propensity score matching analysis. Assuming that the random assignment estimates of smaller class size are 'true', they conclude that the propensity score estimates are deficient when differences exist. However, for several reasons, this conclusion can be questioned (e.g., many of the schools had very few children in the experiment, prior test scores were not used in matching). Other studies, notably Mueser, Troske, and Gorislavsky (2007), have found that analyses employing a propensity score matching design can yield estimates of program impacts that closely align with program impact estimates obtained from experimental designs.

[13]See Pirog et al. 2009 and related citations.

including demographic variables, geographic variables, and baseline measures of the outcomes to be studied. A key decision in specifying the matching procedure involves identifying variables that will be used to estimate the propensity score model. In our model, we use case-specific employment history, prior earnings, gender, race/ethnicity, casehead age, number of children of the casehead or in the family unit, and county of residence.[14] In addition, we include several census block group variables,[15] such as the percentage of persons in poverty, the unemployment rate, and the percentage of households on public assistance.

Among the available matching strategies available in the propensity score matching literature—including "nearest neighbor," "kernel," and "local linear regression" methods—we employ a nearest neighbor matching strategy in this study.[16] This method uses the estimated propensity score for each voucher case and matches it to one or more nonrecipient cases with the closest (or ideally identical) propensity scores.[17]

Diagnostic evaluations indicate that the matching procedure succeeds in balancing the included variables and eliminating pretreatment differences between the voucher and matched comparison groups

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[14]As described above, our extract of information on housing voucher recipients from the CARES and UI databases provides us with quarterly or annual longitudinal information on socioeconomic characteristics of these families, as well as measures of labor market performance (employment and earnings) and welfare participation and indicators of the characteristics of the neighborhoods in which they live (or to which they move). These quarterly or annual measures extend from the year these voucher recipients first receive a voucher to 2006, for an observation period of at least four years. We use information on these variables prior to the year in which they receive housing assistance in securing the propensity score matched families that form our control group.

[15]A complete list of census block group variables included in the model used to estimate the propensity score can be found in footnote 10.

[16]Discussions of various matching metrics and methods can be found in Mueser, Troske, and Gorislavsky (2007) and Smith and Todd (2005).

[17]After specifying the model used to estimate the propensity score, we addressed issues regarding our pool of potential matches. Specifically, we had to decide how to treat public housing recipients and cases that received vouchers in later years. In the end, we decided to exclude any case living in public housing from our pool of potential matches. In addition, we excluded from our pool of potential controls any case that received a voucher at any point during our period of evaluation. As a result, the pool of potential matches is composed of cases that have applied for or received food stamps and have not received a housing voucher or lived in public housing at any point between 2000 and 2006. The condition of food stamp application or receipt insures that the potential matches are knowledgeable about income-conditioned public benefits, and have received or applied for such benefits. The exclusion of cases living in public housing means the comparison is between households receiving a housing voucher and households with no housing assistance.

on every covariate included in the model used to estimate the propensity score. Appendix A provides more detail on our matching strategy and reports tests of its ability to secure a comparison group that provides unbiased and reliable estimates of the impact of Section 8 voucher receipt.

Difference-in-Differences Estimates of the Effect of Voucher Receipt on Casehead Earnings and Employment

Using the balanced sample created through the propensity score matching procedure, we isolate the effect of voucher receipt on labor market outcomes—earnings and employment—using a difference-in-differences regression framework. Specifically, we estimate the following model:

$$Y_{it} = \alpha + \beta_1 V_i + \beta_2 R_{it} + \boldsymbol{\beta}_3 \mathbf{X_i} + \beta_4 A_{it} + \beta_5 (V_i \times A_{it}) + C_i + \varepsilon_{it} \quad (1)$$

In this equation, Y represents the casehead labor market outcome; α represents the intercept; V is a dummy variable indicating voucher receipt in the treatment year; R represents the calendar year; $\mathbf{X}$ is a vector of observed, case-level characteristics; A represents the year of earnings relative to the treatment year; and C represents unobserved, time-invariant, case-level characteristics. Subscripts *i* and *t* index cases and the year of earnings or employment with respect to voucher receipt, respectively.

The model is estimated via generalized least squares (GLS) with random effects controls. For each year, the total estimated effect of voucher receipt on the casehead labor market outcome is equal to the sum of β_1 and β_5.

While this model isolates the effect of voucher receipt on labor market outcomes for all cases that received a voucher in the initial year, we are also interested in determining if the effect of voucher receipt differs by duration of receipt. More precisely, we are interested in knowing whether the effect of voucher receipt differs among cases that are continuing voucher recipients at some point in time after voucher receipt (e.g., three years post-receipt) compared to cases that received a voucher in the initial year but had

subsequently gone off of housing assistance. To investigate this issue, we estimate the following difference-in-differences model:

$$Y_{it} = \alpha + \beta_1 V_i + \beta_2 R_{it} + \boldsymbol{\beta}_3 \mathbf{X_i} + \beta_4 A_{it} + \beta_5 (V_i \times D_{it} \times A_{it}) + C_i + \varepsilon_{it} \quad (2)$$

In this equation, D is a dummy variable indicating that the case was verified as continuing to receive a voucher in the year of earnings. This model allows us to compare cases that remained on housing assistance in a given year to both cases that received a rental subsidy in the initial year, but subsequently relinquished receipt of this subsidy, and to those who never received a housing subsidy in the years of observation. The total effect of voucher receipt for cases that continued to receive a rental subsidy through the year of earnings is equal to the sum of β_1 and β_5.

Taken together, these estimation strategies yield our estimate of the effect of housing voucher receipt on labor market outcomes. By combining propensity score matching with regression analysis we exploit the advantages of each method while mitigating the limitations associated with each procedure. Indeed, prior research has found that combining propensity score matching with regression adjustment is preferable to applying either method by itself (Imbens and Wooldridge 2008).

IV. ESTIMATION RESULTS

A. Geographic Movement

Using our propensity score matching procedure, we first estimate the effect of housing voucher receipt on the probability of the household changing residence within one year and within four years after the end of the month in which the case first received the housing voucher. At both points in time, a higher percentage of those cases that received a voucher changed their residence, relative to members of the matched comparison group. One year after voucher receipt, 58 percent of families with a voucher had moved, compared with 44 percent of matched families that were not receiving a voucher. By four years

after voucher receipt, 77 percent of voucher recipients had moved, whereas 69 percent of matched comparison group cases had moved. As expected, the receipt of a housing voucher appears to substantially increase the probability of changing residential location.

B. The Effect of Voucher Receipt on Casehead Earnings

Tables 2 and 3 summarize our findings concerning the effect of receiving a Section 8 housing voucher on the earnings of the casehead in the recipient household. These estimates are based on equation (1), above, and hence capture the effect of voucher receipt for all cases that received a voucher in the initial year. In Table 2, we report mean effects for both casehead earnings and the percentage of the comparison group's earnings, with the coefficients that are statistically significant at the alpha = .1 level denoted in bold; Table 3 presents the effect of voucher receipt as a percentage of mean control group earnings. The top row of each table presents summary results on earned income from the initial year of housing voucher receipt to five years after receipt.[18]

For the full sample, receipt of a housing voucher was estimated to result, on average, in a $600 decline in casehead annual earnings in the initial year of voucher receipt, or nearly 12 percent of the average earnings for the matched comparison cases.[19]

Apparently, the dislocation accompanying the move, or a negative work response to the additional income, or the benefit reduction rate incentives associated with the voucher led to this negative earnings effect in the initial year of observation. However, by five years after voucher receipt, the

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[18]The years over which we observe these families span an employment slowdown and a recovery. To control for the effect of macroeconomic conditions on our estimated effects, we have controlled for calendar year in our regression analysis. Calendar year cohort, and the amount of time since receipt of the voucher are also included as control variables in the analysis.

[19]Average earnings of the voucher group were not statistically different from the average earnings of the matched comparison group in the year prior to treatment. This suggests that assessing the voucher group's average earnings decline as a percentage of the matched comparison group's average earnings provides a valid measure of the earnings decline associated with a Section 8 voucher.

Table 3
Effect of Voucher Receipt on Earnings, by Demographic Subgroup (Dollars)

Subgroup	Year of Receipt	One Year Post	Two Years Post	Three Years Post	Four Years Post	Five Years Post
Full Sample	**-598.00**	**-558.33**	**-469.96**	**-382.97**	**-229.91**	-47.46
	(70.75)	(70.75)	(70.74)	(70.73)	(78.95)	(100.55)
Sex						
Male	**-706.53**	**-642.02**	**-600.55**	**-319.65**	-144.71	-49.21
	(180.38)	(180.43)	(180.40)	(180.39)	(204.08)	(262.41)
Female	**-569.45**	**-544.78**	**-425.11**	**-341.28**	**-267.33**	-117.22
	(75.57)	(75.56)	(75.55)	(75.55)	(84.26)	(107.42)
Race						
White	**-820.85**	**-752.17**	**-622.11**	**-512.19**	**-422.36**	**-364.33**
	(88.72)	(88.72)	(88.70)	(88.69)	(99.73)	(128.02)
Black	**-374.29**	-186.06	-103.54	41.85	214.45	**464.09**
	(133.10)	(133.11)	(133.13)	(133.13)	(147.42)	(185.13)
Hispanic	-546.14	-272.31	-215.00	-200.92	-156.77	-670.84
	(410.13)	(410.05)	(410.08)	(409.98)	(448.85)	(592.11)
Other race	-315.83	-310.61	-329.11	**-600.99**	-467.99	-168.46
	(270.42)	(270.44)	(270.43)	(270.43)	(304.92)	(400.75)
Age						
Age 18–30	**-625.83**	**-550.65**	**-431.62**	**-401.91**	**-203.27**	65.60
	(107.85)	(107.83)	(107.80)	(107.80)	(121.54)	(157.74)
Age 31–54	**-555.43**	**-521.06**	**-482.93**	**-334.31**	**-351.45**	-230.21
	(110.34)	(110.35)	(110.35)	(110.34)	(122.54)	(155.90)
Age 55+	**-159.57**	**-193.25**	**-156.11**	-42.69	-62.24	**-222.10**
	(66.59)	(66.60)	(66.59)	(66.59)	(73.69)	(89.32)
Urbanicity						
Milwaukee	**-364.18**	-98.34	-161.99	36.70	97.80	160.89
	(151.25)	(151.31)	(151.35)	(151.38)	(165.54)	(203.27)
Other urban	**-656.66**	**-698.62**	**-557.00**	**-456.58**	-167.39	7.99
	(100.09)	(100.09)	(100.07)	(100.06)	(112.28)	(146.64)
Rural	**-552.43**	**-512.66**	**-413.91**	**-367.84**	**-311.46**	-83.59
	(127.06)	(127.06)	(127.07)	(127.05)	(143.36)	(181.70)
Family Composition						
Couple with child	**-758.41**	**-701.92**	-326.48	-306.29	**-658.95**	-285.60
	(226.33)	(226.37)	(226.37)	(226.36)	(258.42)	(336.56)
Single parent	**-798.79**	**-720.73**	**-622.18**	**-470.86**	**-288.26**	-130.15
	(102.68)	(102.66)	(102.65)	(102.64)	(114.42)	(146.52)
No child	**-353.86**	**-379.19**	**-355.04**	**-291.26**	**-235.31**	-171.91
	(83.04)	(83.07)	(83.05)	(83.05)	(92.93)	(116.88)
Education						
HS Diploma	**-684.88**	**-637.64**	**-478.21**	**-384.70**	**-242.76**	-61.49
	(94.35)	(94.35)	(94.33)	(94.32)	(105.82)	(136.01)
No HS Diploma	**-270.31**	-143.61	-89.49	-115.03	-0.46	2.06
	(95.00)	(95.01)	(95.01)	(95.00)	(105.96)	(134.73)

Note: Standard errors in parentheses below point estimates. Point estimates in bold are statistically significant at alpha = .10 level.

negative earnings effect had fallen to less than $50. All of the annual negative differences are statistically significant except that for the fifth year after voucher receipt.

In addition to examining the effect of voucher receipt on casehead earnings for the entire sample, we also analyze the results by gender, race/ethnicity, age, education level, family composition, and urbanicity. In performing this analysis, we hypothesize that traditionally disadvantaged populations—such as minorities or those with limited education—may exhibit positive labor market outcomes as they may be most likely to use a voucher to move to neighborhoods with superior employment or earnings opportunities.

For male caseheads, the negative earnings effect of voucher receipt in the initial year is nearly 25 percent of the mean earnings of caseheads in the control groups, compared to a -10 percent effect for female family heads; both of these estimates are statistically significantly different from zero. By the end of the observation period, both male and female heads of families receiving a voucher had narrowed the negative difference to about 2 percent of mean earnings of the matched comparison group.[20]

For Blacks, a large and statistically significant negative earnings effect in the treatment year (about $375, or -6.4 percent) turned into a sizable and statistically significant earnings difference of over $450 (+7.2 percent) after five years. For all of the other racial groups, large negative effects in the treatment year eroded over time, but remained negative, though statistically insignificant, after five years.[21]

Both single- and two-parent families with children experienced statistically significant initial negative earnings differences of over $750; this effect is in excess of 11 percent of matched comparison group earnings for both groups. For both groups, these negative earnings effects fell substantially, and became statistically insignificant by the fifth year. For the cases without children, the first year negative

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[20]At the five-year mark, neither of these results is statistically significantly different from zero.

[21]The large negative effect recorded for Hispanics in the fifth year after receipt is anomalous, and is inconsistent with the generally falling negative effect; it is based on a small sample size of 121 voucher recipients.

effect was smaller in absolute dollars, and it too eroded to a statistically insignificant effect by the fifth year of the observation period. Over time, families with children moved toward pre-voucher earnings more quickly than did family units without children.

Voucher group caseheads in all three age subgroups exhibit large and statistically significant initial-year negative earnings effects, ranging from nearly 10 percent of mean comparison group earnings for those aged 18 to 30 years to nearly -30 percent for those older than 55 years. The 18- to 30-year-old group exhibits the largest absolute decline (-$625), whereas the +55 group exhibits the largest relative decline. For both of the younger age groups, the negative significant effects of voucher receipt on earnings in the initial years after receipt dissipated over time, and were not statistically significantly different from zero after five years; for the youngest age group the difference became positive, although not statistically significant. This trend is at odds with the trend exhibited by the oldest group, which continued to exhibit a substantial and statistically significant negative earnings effect five years after voucher receipt.[22] In fact, by the fifth year post-receipt, the negative effect of voucher receipt on earnings had reached its maximum, both absolutely and in percentage terms, for the +55 age group.

The effect of housing voucher receipt on earnings also differs substantially by county urbanicity.[23] In rural areas and urban areas other than Milwaukee, the mean earnings difference between voucher group caseheads and their matched comparisons exceeded $550 in the initial year of voucher receipt, about 12 percent of the mean earnings for the matched comparison group. For both groups, the negative earnings effect dissipated over the observation period, and became statistically insignificantly different from zero. In contrast, the mean casehead earnings difference between voucher recipients residing in Milwaukee County and their matched comparisons was only -$364 in the initial year of

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[22]The underlying causes of these differential patterns are unknown. Perhaps cases with income to needs related to children or with more steep normal earnings trajectories tended to increase earnings beyond the Section 8 eligibility level, resulting in the loss of the voucher and the negative work incentives that it imposes. In subsequent research, we will attempt to understand the potential role of voucher loss in explaining these patterns.

[23]Our county urbanicity measure contains three categories: rural counties, Milwaukee County, and other urban counties. We use the county classifications assigned by the State of Wisconsin.

receipt, which corresponds to 6.5 percent of the mean earnings for the matched comparison group. This initial negative earnings effect attributable to voucher receipt for Milwaukee County caseheads disappears over time, and eventually becomes positive (but not statistically significantly different from zero) after three years.

Educational subgroups exhibit little systematic difference in the earnings effect of receiving a housing voucher. For caseheads who both did and did not graduate from high school, the earnings effect is negative and statistically significant immediately following the receipt of a housing voucher — -$685 for those with a high school degree and -$270 for those without a degree. Five years after initial voucher receipt, the earnings difference between the voucher and matched comparison groups is not statistically significant for either group.

In sum, we conclude that the effect of voucher receipt on casehead earnings is negative and statistically significant in the year of voucher receipt. This negative effect is nearly $600 per year on average, and across the subgroups ranges from over -$820 to -$160; relative to the average earnings of caseheads in the control group, these treatment year effects range from nearly -25 percent to -6 percent. Over the six-year observation period, the negative effect decreased substantially overall, and five years after receipt of the subsidy, the overall negative effect is not statistically different from zero. Virtually all of the subgroups also experienced reductions in the negative earnings effect with the passage of time since voucher receipt. Five years after voucher receipt, the point estimate of the earnings effect is positive for Blacks, young workers, urban residents, and those with less than a high school degree; in only the case of Blacks is the positive, long-term earnings effect statistically significant.

C. The Effect of Voucher Receipt on Casehead Employment

The effects of voucher receipt on casehead employment (quarters worked per year) are shown in Tables 4 and 5. From the year of voucher receipt to five years post receipt, the average-quarters-worked difference for the entire sample of caseheads goes from a statistically insignificant -.012 in the year of voucher receipt to a marginally significant ($z = 1.84$) +.040 five years after voucher receipt. Relative to

Table 4
Effect of Voucher as a Percentage of Mean Control Group Casehead Earnings, by Demographic Subgroup

Subgroup	Year of Receipt	One Year Post	Two Years Post	Three Years Post	Four Years Post	Five Years Post
Full Sample	**-11.5**	**-10.0**	**-8.1**	**-6.4**	**-3.9**	-0.8
Sex						
Male	**-24.5**	**-20.7**	**-19.5**	**-10.7**	-5.0	-2.0
Female	**-10.3**	**-9.3**	**-7.0**	**-5.4**	**-4.2**	-1.8
Race						
White	**-16.8**	**-14.4**	**-11.4**	**-9.1**	**-7.6**	**-6.7**
Black	**-6.4**	-3.1	-1.7	0.7	3.3	**7.2**
Hispanic	-9.6	-4.5	-3.5	-3.3	-2.4	-10.0
Other race	-5.9	-5.5	-5.5	**-9.7**	-7.6	-2.8
Age						
Age 18–30	**-9.8**	**-7.9**	**-5.9**	**-5.2**	**-2.6**	0.8
Age 31–54	**-11.2**	**-10.1**	**-9.3**	**-6.4**	**-6.7**	-4.5
Age 55+	**-29.1**	**-40.1**	**-37.2**	-11.8	-21.7	**-81.7**
Urbanicity						
Milwaukee	**-6.5**	-1.7	-2.7	0.6	1.6	2.5
Other urban	**-12.5**	**-12.5**	**-9.6**	**-7.6**	-2.9	0.1
Rural	**-11.9**	**-10.1**	**-7.7**	**-6.7**	**-5.7**	-1.6
Family Composition						
Couple with child	**-12.9**	**-11.1**	-4.9	-4.4	**-9.1**	-4.1
Single parent	**-11.3**	**-9.7**	**-8.1**	**-6.0**	**-3.6**	-1.6
No child	**-21.6**	**-21.6**	**-19.9**	**-16.0**	**-14.7**	-12.9
Education						
HS Diploma	**-11.5**	**-10.0**	**-7.3**	**-5.6**	**-3.5**	-0.9
No HS Diploma	**-7.3**	-3.7	-2.3	-2.8	0.0	0.0

Note: Estimates in bold are statistically significant at alpha = .10 level.

Table 5
Effect of Voucher Receipt on Quarters Worked, by Demographic Subgroup

Subgroup	Year of Receipt	One Year Post	Two Years Post	Three Years Post	Four Years Post	Five Years Post
Full Sample	-0.01	0.01	0.00	0.00	0.01	**0.04**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Sex						
Male	**-0.09**	-0.05	-0.05	-0.02	0.04	0.02
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.06)
Female	0.01	0.03	0.01	0.02	0.01	0.03
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Race						
White	**-0.08**	**-0.06**	**-0.05**	**-0.03**	-0.03	-0.02
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
Black	**0.05**	**0.13**	**0.10**	**0.10**	**0.09**	**0.12**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
Hispanic	0.12	0.09	0.08	0.04	0.09	0.00
	(0.08)	(0.08)	(0.08)	(0.08)	(0.09)	(0.12)
Other race	0.08	**0.13**	**0.10**	0.04	0.03	0.06
	(0.05)	(0.05)	(0.05)	(0.05)	(0.06)	(0.09)
Age						
Age 18–30	0.02	**0.06**	**0.05**	**0.05**	**0.05**	**0.09**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
Age 31–54	-0.02	-0.03	-0.03	-0.02	-0.01	0.01
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)
Age 55+	**-0.05**	**-0.07**	**-0.06**	**-0.04**	**-0.04**	**-0.06**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
Urbanicity						
Milwaukee	-0.01	**0.07**	**0.06**	**0.07**	0.05	0.04
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
Other urban	0.00	-0.01	-0.01	0.00	0.01	0.03
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
Rural	-0.03	-0.01	0.00	-0.01	0.00	0.06
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
Family Composition						
Couple with child	-0.07	-0.04	-0.02	-0.04	-0.02	-0.04
	(0.05)	(0.05)	(0.05)	(0.05)	(0.06)	(0.07)
Single parent	0.00	0.03	0.01	0.03	0.03	**0.06**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
No child	**-0.06**	**-0.07**	**-0.06**	**-0.05**	-0.04	-0.05
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)
Education						
HS Diploma	-0.03	-0.01	0.00	0.01	0.02	0.04
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
No HS Diploma	0.04	**0.07**	**0.06**	0.04	0.04	0.06
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)

Note: Standard errors in parentheses below point estimates. Point estimates in bold are statistically significant at alpha = .10 level.

matched comparison group casehead, as shown in Table 6, the average casehead receiving a Section 8 voucher had 2.4 percent more quarters worked per year by five years after voucher receipt.

Across the subgroups, the quarters-worked pattern is somewhat different than the earnings pattern. For only the Black subgroup was the effect of voucher receipt positive and significant in the treatment year. The treatment year matched difference is negative and significant for males, Whites, cases with no children, and those over age 55; for the other subgroups the first year difference is insignificant. By the fifth year after voucher receipt, all of the matched differences (the full sample and all subgroups) are positive with the exception of Whites, cases without children, couples with children, and those aged over 55 years.[24]

The patterns are notable for a few of the subgroups. The effect of voucher receipt significantly increases the average work effort of Black caseheads in every year after voucher receipt. By the fifth year after receipt, Black caseheads had nearly 7 percent more quarters worked on average than did their matched comparison group. A positive and significant program effect is also found for young caseheads and single parents, and those without a high school degree. A four percent increase in average quarters worked is estimated for the fifth year after voucher receipt for young workers, and an increase of nearly 3 percent for single parents, and nearly 4 percent for those without a high school degree.[25] These subgroups for which voucher receipt had a positive and significant five-year effect—Blacks, young caseheads, single parents, and those with the least schooling—are disadvantaged groups often thought to respond negatively, if at all, to the work disincentives in income-conditioned public programs. However, in the case of housing vouchers, the better employment opportunities tied to the ability to relocate to a better neighborhood may overwhelm the work disincentives for these traditionally disadvantaged groups.

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[24]Although not shown, for nearly all the groups, the mean level of quarters worked decreased over the five years of observation, for both matched comparison and voucher group families.

[25]The effect for those without a high school degree is marginally significant.

Table 6
Effect of Voucher as a Percentage of Mean Control Group Casehead Quarters Worked, by Demographic Subgroup

Subgroup	Year of Receipt	One Year Post	Two Years Post	Three Years Post	Four Years Post	Five Years Post
Full Sample	-0.6	0.4	0.1	0.2	0.5	**2.4**
Sex						
Male	**-8.4**	-5.0	-5.5	-2.4	5.6	3.4
Female	0.4	1.3	0.7	1.1	0.7	1.9
Race						
White	**-4.2**	**-3.5**	**-2.7**	**-2.0**	-1.9	-1.3
Black	**2.7**	**6.8**	**5.3**	**5.2**	**4.8**	**6.7**
Hispanic	6.2	4.7	4.7	2.2	5.0	0.3
Other race	4.1	**6.8**	**5.3**	2.3	1.9	3.5
Age						
Age 18–30	0.8	**2.7**	**2.3**	**2.0**	**2.1**	**4.0**
Age 31–54	-0.9	-1.7	-1.6	-1.1	-0.8	1.0
Age 55+	**-16.7**	**-31.5**	**-33.5**	**-24.8**	**-32.9**	**-51.6**
Urbanicity						
Milwaukee	-0.3	**4.0**	**3.5**	**4.1**	3.2	2.5
Other urban	-0.2	-0.6	-0.8	-0.1	0.8	1.9
Rural	-1.8	-0.7	0.2	-0.6	0.3	3.6
Family Composition						
Couple with child	-3.2	-1.7	-1.0	-2.1	-0.9	-1.8
Single parent	-0.1	1.1	0.6	1.3	1.4	**2.9**
No child	**-7.1**	**-9.3**	**-8.9**	**-6.8**	-6.6	-9.4
Education						
HS Diploma	-1.4	-0.6	0.0	0.7	0.9	2.1
No HS Diploma	2.3	**4.4**	**4.0**	2.5	3.0	3.9

Note: Estimates in bold are statistically significant at alpha = .10 level.

Finally, the effect of voucher receipt on the work effort of older workers is negative and significant in each year, and increases persistently; by five years after voucher receipt, caseheads aged more than 55 years had a 50 percent reduction in average quarters worked as a result of voucher receipt relative to their matched comparison group.

D. Duration-Specific Labor Market Effects of Voucher Receipt

Our estimates in sections B and C describe the effect of voucher receipt on labor market outcomes for all cases that received a voucher in the initial year. An additional important question concerns the differential effect of voucher receipt by the length of time that families retain their subsidy. As a specific example, we are interested in knowing whether the labor market effect of voucher receipt three years after initial receipt is different for a family that retained its subsidy throughout this time relative to a family that has relinquished its voucher at some point during this period.

We estimated these duration-specific impacts using equation (2), above. Table 7 summarizes the pattern of earnings effects for all cases, continuous voucher recipients (stayers), and initial recipients who could not be verified as remaining on housing assistance three/five years post-receipt (leavers); stayer and leaver results for selected subgroups are also shown.[26] The initial (treatment year) negative voucher effect on earnings is similar for all three groups. However, by three years after the initial receipt of the voucher, cases that had retained their voucher for that period exhibited virtually no change in this negative earnings effect. By contrast, cases that had relinquished their voucher within three years (leavers) experienced a much smaller negative voucher effect than in the treatment year. By five years post-receipt, those who had relinquished their voucher by that time had virtually no negative earnings effect, while the stayers—the continuous voucher recipients—earned $280 (z = 1.53) less than their matched comparison group.

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[26]"Stayers" are cases that received a voucher in the treatment year and were verified as continuing to receive a voucher in the year of earnings being analyzed. "Leavers" are cases that received a voucher in the treatment year, but could not be verified to still be receiving a voucher in the year of earnings being analyzed. Full subgroup results for stayers and leavers are available from the authors.

Table 7
Estimated Effects of Voucher Receipt on Earnings for Full Sample and Selected Subgroups: All, Stayers, and Leavers

Subgroup	All Cases	Cases Continuously Receiving Voucher	Case Not Verified as Receiving Voucher at Three/Five Years Post Receipt
Full Sample			
Difference in year of voucher receipt	**-598.00**	**-598.22**	**-598.19**
Difference three years post receipt	**-382.97**	**-560.09**	**-256.29**
Difference five years post receipt	-47.46	-280.85	-21.30
White			
Difference in year of voucher receipt	**-820.85**	**-821.50**	**-821.52**
Difference three years post receipt	**-512.19**	**-834.73**	**-326.67**
Difference five years post receipt	**-364.33**	**-1,033.64**	**-244.55**
Black			
Difference in year of voucher receipt	**-374.29**	**-374.28**	**-374.18**
Difference three years post receipt	41.85	96.91	-3.08
Difference five years post receipt	464.09	**636.33**	**357.36**
Age 18–30			
Difference in year of voucher receipt	**-625.83**	**-626.02**	**-625.97**
Difference three years post receipt	**-401.91**	**-564.13**	**-272.14**
Difference five years post receipt	65.60	-169.74	88.24
Age 31–54			
Difference in year of voucher receipt	**-553.43**	**-555.49**	**-555.51**
Difference three years post receipt	**-334.31**	**-604.10**	-151.32
Difference five years post receipt	-230.21	**-646.24**	-139.37
Milwaukee			
Difference in year of voucher receipt	**-364.18**	**-364.17**	**-364.21**
Difference three years post receipt	36.70	46.06	21.21
Difference five years post receipt	160.89	268.49	100.16
Rural			
Difference in year of voucher receipt	**-552.43**	**-553.02**	**-553.01**
Difference three years post receipt	**-367.84**	**-900.97**	-96.59
Difference five years post receipt	-83.59	**-756.62**	5.57
HS Diploma			
Difference in year of voucher receipt	**-684.88**	**-685.38**	**-685.37**
Difference three years post receipt	**-384.70**	**-712.69**	-166.74
Difference five years post receipt	-61.49	**-609.47**	39.03
No HS Diploma			
Difference in year of voucher receipt	**-270.31**	**-270.16**	**-270.15**
Difference three years post receipt	-115.03	-11.32	-173.11
Difference five years post receipt	2.06	364.89	-106.36

Note: Estimates in bold are statistically significant at alpha = .10 level.

After five years, the earnings effect of voucher receipt for the leavers closely resembles that for the entire sample of voucher recipients. This general pattern persists across the subgroups that we study.

Table 8 reports duration-specific quarters worked results for the full sample as well as selected subgroups. For caseheads who remained voucher recipients for three and five years, the employment effects of the program are positive, significant, and quantitatively large. By the end of the observation period, the program led to an increase of over 9 percent in average quarters worked for those who remained voucher recipients for that duration. Conversely, for those who had relinquished their voucher, the effect of the program on quarters worked is, if anything, negative; this apparent effect may be associated with the dislocation accompanying the increased mobility.

The overall pattern of the duration-specific results is quite interesting. For the full sample, and most subgroups, caseheads that remained on voucher assistance exhibited a more negative earnings response compared to caseheads that received a voucher in the initial year, but subsequently went off assistance. At the same time, however, cases that remain on assistance are found to work a greater number of quarters than cases that left assistance. This suggests that voucher recipients may use the voucher to relocate to areas with a greater number of, and perhaps more desirable, employment opportunities, but choose to work fewer hours in their primary job, or perhaps stop working a second job. We plan to further explore this proposed explanation in future research.

E. Additional Sensitivity Analyses

In addition to the results presented above, we also estimated a variety of additional models to test the robustness of our conclusions.

Casehead Labor Market Effects Using OLS Estimation

The results we have presented are from a generalized least squares difference-in-differences estimation with random effects controls. The model was also estimated using ordinary least squares with

Table 8
Estimated Effects of Voucher Receipt on Quarters Worked for Full Sample and Selected Subgroups: All, Stayers, and Leavers

Subgroup	All Cases	Cases Continuously Receiving Voucher	Case Not Verified as Receiving Voucher at Three/Five Years Post Receipt
Full Sample			
Difference in year of voucher receipt	-0.01	-0.01	-0.01
Difference three years post receipt	0.00	**0.06**	**-0.03**
Difference five years post receipt	**0.04**	**0.16**	0.01
White			
Difference in year of voucher receipt	**-0.08**	**-0.08**	**-0.08**
Difference three years post receipt	**-0.03**	-0.03	**-0.04**
Difference five years post receipt	-0.02	0.02	-0.03
Black			
Difference in year of voucher receipt	**0.05**	**0.05**	**0.05**
Difference three years post receipt	**0.10**	**0.20**	-0.00
Difference five years post receipt	**0.12**	**0.35**	0.04
Age 18–30			
Difference in year of voucher receipt	0.02	0.02	0.02
Difference three years post receipt	**0.05**	**0.12**	-0.00
Difference five years post receipt	**0.09**	**0.24**	0.05
Age 31–54			
Difference in year of voucher receipt	-0.02	-0.02	-0.02
Difference three years post receipt	-0.02	0.02	-0.04
Difference five years post receipt	0.01	**0.11**	-0.02
Milwaukee			
Difference in year of voucher receipt	-0.01	-0.01	-0.01
Difference three years post receipt	**0.07**	**0.12**	0.02
Difference five years post receipt	0.04	**0.15**	0.00
Rural			
Difference in year of voucher receipt	-0.03	-0.03	-0.03
Difference three years post receipt	-0.01	0.01	-0.02
Difference five years post receipt	0.06	0.12	0.05
HS Diploma			
Difference in year of voucher receipt	-0.03	-0.03	-0.03
Difference three years post receipt	0.01	**0.05**	-0.01
Difference five years post receipt	0.04	**0.15**	0.01
No HS Diploma			
Difference in year of voucher receipt	0.04	0.04	0.04
Difference three years post receipt	0.04	**0.12**	-0.02
Difference five years post receipt	0.06	**0.19**	0.02

Note: Estimates in bold are statistically significant at alpha = .10 level.

standard errors clustered at the case level. The results of the two models were substantively similar and are available from the authors.

Labor Market Effects for the Entire Case

The results shown above reflect earnings and employment for the caseheads in families included in our data. We also estimated these labor market effects for all individuals who were listed in the CARES database as members of the voucher and matched comparison cases. The earnings and work patterns for the whole case are very similar to the casehead-only results, and are available from the authors. For both the voucher and matched comparison groups, casehead earnings accounted for, on average, approximately 60 percent of the earnings of all case members in the initial year of voucher receipt.

V. CONCLUSION

Our results suggest that voucher receipt leads to a significantly higher initial and long-term probability of changing residence, relative to a matched comparison group. The initial post-treatment impact of the program on recipient earnings is negative (about 12 percent of mean casehead earnings in the comparison group), but over the subsequent years this negative earnings effect tended to decrease. By five years after initial voucher receipt, recipients continued to have lower earnings than those of members of the matched comparison group, although these effects were not statistically significant. There is a negative effect of voucher receipt on work effort (quarters worked per year) in the years immediately after voucher receipt, but by five years after treatment, voucher recipients recorded a statistically significant gain in quarters worked per year relative to the matched comparison group. These results are consistent with recent experimental work on the effects of Section 8 voucher receipt. The Welfare to Work evaluation found voucher receipt to cause earnings to drop by 11 percent in the treatment year, while work by Jacobs and Ludwig (2008) that takes advantage of a natural experiment in Chicago concludes that voucher receipt caused a drop in earnings of approximately 10 percent.

Our study of a diverse and large group of low-income families, rather than only those observed to have lived in public housing or in medium to large urban areas, suggests interesting and substantially different responses to voucher receipt by subgroup. With the exception of caseheads aged 55 or more, the negative effect of the program on earnings for all demographic subgroups decreases over time. By the end of the period of observation, the point estimate of the earnings effect is positive for Blacks, young workers, urban residents, and those with less than a high school degree, and for Blacks the positive five-year effect is statistically significant. In general, we found that by five years after voucher receipt, any early negative employment effect of voucher participation had decreased, and for some of the most disadvantaged groups, positive significant effects of the program on average quarters worked are observed. These findings suggest that traditionally disadvantaged populations—such as racial minorities and poorly educated individuals—respond to housing voucher receipt with behavior that is most consistent with that originally envisioned by policy designers. In the long term, there is some evidence that these populations may use this rental assistance to improve their economic self-sufficiency and labor market success.

Appendix A

In this appendix, we provide information on our propensity score matching analysis. As described in the body of the paper, we use a nearest neighbor matching method to identify matched comparison cases for members of our voucher group. The first step in the analysis was to specify the model used to estimate the propensity of each case to receive the treatment, in our case a housing voucher. These scores were estimated using a logit model that contained a rich set of variables thought to be predictive of a case's likelihood of receiving a housing voucher. These variables include employment history; prior earnings; and sociodemographic variables on the family or individual unit, such as gender, race, education, marital status, age, number of children; and dummy variables indicating the county of residence for each case. In addition, several census block group variables, such as percentage of people in poverty, the unemployment rate, and the percentage of households on public assistance, are included in the propensity score estimation model.[27] Table A1 presents the results of the logit model used to estimate the propensity scores for the pooled 2001 through 2003 cohorts.[28]

After the model used to estimate each case's propensity score was specified, attention next turned to using those scores to generate a matched comparison group. We use nearest neighbor matching to match each treatment case to the five matched comparison cases with the most similar, often identical, propensity scores.[29]

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[27]The full list of census block group variables is as follows: percentage of persons in poverty, percentage of households receiving public assistance income, percentage of female-headed families with children, percentage of high school dropouts, unemployment rate, labor force participation rate, percentage of families with no workers, percentage of people with incomes twice the poverty level, percentage of people with education beyond high school, percentage of 16- to 19-year-olds in school, percentage of housing stock that is owner-occupied, median family income, racial composition, median house value, and median gross rent.

[28]Propensity score estimation and matching was performed using Stata's "psmatch2" procedure. Matching results that include the 2000 cohort are available from the authors.

[29]Nearest neighbor matches were also performed with each treatment case matched to the 1, 3, and 10 nearest neighbors. The results, which are available from the authors, did not differ substantively.

Table A1
Coefficients, Standard Errors and P-Values from Logistic Regression Used to Estimate Propensity Score for Receiving a Rental Subsidy: Pooled 2001–2003 Cohorts

Independent Variable	Coefficient	Std. Error	P-Value
Male	-0.64	0.033	0.000
Black	0.34	0.039	0.000
Hispanic	-0.30	0.075	0.000
Other race	-0.13	0.052	0.011
Years of education	0.01	0.006	0.128
Annulled	1.11	0.472	0.019
Divorced	0.42	0.039	0.000
Single	0.40	0.036	0.000
Separated	0.43	0.042	0.000
Widowed	0.17	0.069	0.014
Unknown marital status	1.62	0.613	0.008
Adjusted wages two years prior	0.00	0.000	0.001
Adjusted wages one year prior	0.00	0.000	0.000
One quarter worked one year prior	0.09	0.042	0.036
Two quarters worked one year prior	0.14	0.040	0.000
Three quarters worked one year prior	0.24	0.040	0.000
Four quarters worked one year prior	0.36	0.042	0.000
One quarter worked two years prior	0.13	0.041	0.002
Two quarters worked two years prior	0.05	0.041	0.217
Three quarters worked two years prior	0.15	0.039	0.000
Four quarters worked two years prior	0.18	0.041	0.000
Age of casehead	-0.04	0.003	0.000
Age of casehead squared	0.00	0.000	0.000
Number of eligible children	0.13	0.025	0.000
Number of eligible members	-0.08	0.023	0.001
Other race x Adjusted wages two years prior	0.00	0.000	0.726
Black x Adjusted wages two years prior	0.00	0.000	0.219
Hispanic x Adjusted wages two years prior	0.00	0.000	0.423
Other race x Adjusted wages one year prior	0.00	0.000	0.003
Black x Adjusted wages one year prior	0.00	0.000	0.000
Hispanic x Adjusted wages one year prior	0.00	0.000	0.066
Percent of people in poverty	0.00	0.002	0.009
Percent of households on public assistance	-0.01	0.003	0.000
Percent of female-headed families with child	0.01	0.001	0.000
Unemployment rate	-0.01	0.004	0.000
Percent of males in the labor force	0.00	0.001	0.630
Percent of females in the labor force	0.00	0.002	0.037
Percent of families with no workers	0.00	0.001	0.482
Percent of families with incomes less than two times the poverty line	0.01	0.002	0.000
Percent of families with wage income	-0.01	0.002	0.000
Percent of individuals with some college	0.00	0.002	0.413
Percent of individuals with a college degree	0.01	0.002	0.000
Percent of 16- to 19-year-olds enrolled in school	0.00	0.001	0.339
Percent of households that are owner occupied	-0.01	0.001	0.000
Percent of individuals who dropped out of high school	0.00	0.002	0.749

(table continues)

Table A1, continued

Independent Variable	Coefficient	Std. Error	P-Value
Median income	0.00	0.000	0.699
Median gross rent	0.00	0.000	0.000
Median value of owner occupied households	0.00	0.000	0.762
Percent of individuals who speak a language other than English	0.00	0.002	0.206
Percent of Whites	0.00	0.002	0.209
Percent of Blacks	0.00	0.002	0.383
Percent of Hispanics	0.00	0.002	0.928
Percent of households with two or more Non-Hispanics	0.07	0.012	0.000
Regression Statistics			
N		354,543	
Pseudo R-squared		0.1003	
Log likelihood		-47,680.20	

Note: Dummy variables for county of residence and cohort were included in the estimation but are not shown.

We performed multiple diagnostic tests to ensure that the matching procedure was successful in eliminating bias and balancing the voucher and matched comparison groups on all observed covariates. First, we examined the pretreatment values of the earnings and employment outcome variables for the voucher group and their matched comparison group cases to ensure that the matching procedure succeeded in eliminating pretreatment differences between the voucher and matched comparison groups. The results illustrate that the matching procedure was successful in matching voucher cases to matched comparison cases that were not statistically different in the pretreatment values of the outcome variables. Results of this diagnostic test for the labor market outcome variables can be found in Table A2. These results indicate that the matching procedure was successful in eliminating pretreatment differences between the treatment group and their matched comparison group cases on these outcome measures.

In addition to the diagnostic test described above, a balance test was performed to assess the success of the matching procedure in eliminating bias between the voucher and matched comparison groups on all observed covariates used to estimate the propensity scores. The results of this balance test for the pooled 2001 through 2003 cohorts are presented in Table A3. The results illustrate that the matching procedure was highly successful in balancing the voucher and matched comparison groups on all observed covariates. Indeed, no statistically significant differences exist between the groups for any of the variables used in the propensity score estimation.

Table A2
Results of Propensity Score Matching for Selected Outcome Variables

Outcome Variable	Sample	Treated	Controls	Difference	Std. Error	T-stat
Adjusted wages- One Year Prior	Unmatched	7150.79	8086.26	-935.47	95.31	-9.81
	Matched	7150.79	7157.54	-6.75	87.36	-0.08
Quarters worked- One Year Prior	Unmatched	2.40	2.20	0.20	0.02	12.43
	Matched	2.40	2.40	0.00	0.02	0.21

Table A3
Balance Test Results for Pooled 2001–2003 Cohorts

		Mean			Percent	T-Test	
Variable	Sample	Treated	Matched Comparison	Percent Bias	Reduction Bias	T-Stat	P-Value
Male	Unmatched	0.11	0.22	-32.1		-30.84	0.000
	Matched	0.11	0.11	-0.5	98.5	-0.44	0.657
Black	Unmatched	0.30	0.38	-16.4		-17.39	0.000
	Matched	0.30	0.30	0.5	97.0	0.40	0.688
Hispanic	Unmatched	0.03	0.06	-12.5		-12.07	0.000
	Matched	0.03	0.03	0.0	99.7	0.04	0.972
Other race	Unmatched	0.08	0.07	0.9		1.00	0.315
	Matched	0.08	0.08	-0.6	32.0	-0.48	0.630
Years of education	Unmatched	11.59	11.44	8.7		8.82	0.000
	Matched	11.59	11.59	0.4	94.9	0.36	0.718
Annulled	Unmatched	0.00	0.00	1.0		1.27	0.205
	Matched	0.00	0.00	0.5	54.4	0.33	0.739
Divorced	Unmatched	0.21	0.18	5.8		6.45	0.000
	Matched	0.21	0.20	0.1	98.6	0.06	0.952
Single	Unmatched	0.53	0.51	3.7		4.05	0.000
	Matched	0.53	0.53	0.0	99.5	-0.02	0.988
Separated	Unmatched	0.12	0.10	4.1		4.61	0.000
	Matched	0.12	0.12	0.1	96.6	0.11	0.914
Widowed	Unmatched	0.04	0.05	-5.9		-6.05	0.000
	Matched	0.04	0.04	0.2	95.9	0.20	0.840

(table continues)

Table A3, continued

Variable	Sample	Mean		Percent Bias	Percent Reduction Bias	T-Test	
		Treated	Matched Comparison			T-Stat	P-Value
Unknown marital status	Unmatched	0.00	0.00	1.0		1.37	0.172
	Matched	0.00	0.00	0.1	87.9	0.08	0.934
Earnings two years prior	Unmatched	7603.50	8604.10	-9.8		-9.81	0.000
	Matched	7603.50	7588.40	0.1	98.5	0.13	0.898
Earnings one year prior	Unmatched	7150.80	8086.30	-9.9		-9.81	0.000
	Matched	7150.80	7157.50	-0.1	99.3	-0.06	0.950
One quarter worked one year prior	Unmatched	0.08	0.08	-0.1		-0.09	0.925
	Matched	0.08	0.08	-0.6	-564.8	-0.45	0.655
Two quarters worked one year prior	Unmatched	0.10	0.09	2.6		2.89	0.004
	Matched	0.10	0.10	0.2	94.1	0.12	0.906
Three quarters worked one year prior	Unmatched	0.13	0.11	5.6		6.24	0.000
	Matched	0.13	0.13	0.0	99.9	0.00	0.997
Four quarters worked one year prior	Unmatched	0.43	0.40	6.5		7.09	0.000
	Matched	0.43	0.43	0.2	96.7	0.17	0.866
One quarter worked two years prior	Unmatched	0.08	0.07	1.7		1.92	0.055
	Matched	0.08	0.08	0.6	64.6	0.48	0.632
Two quarters worked two years prior	Unmatched	0.09	0.09	1.0		1.09	0.274
	Matched	0.09	0.09	0.1	89.7	0.08	0.936
Three quarters worked two years prior	Unmatched	0.13	0.11	5.4		6.09	0.000
	Matched	0.13	0.13	-0.2	96.1	-0.16	0.873

(table continues)

Table A3, continued

Variable	Sample	Mean		Percent Bias	Percent Reduction Bias	T-Test	
		Treated	Matched Comparison			T-Stat	P-Value
Four quarters worked two years prior	Unmatched	0.44	0.42	4.8		5.20	0.000
	Matched	0.44	0.44	-0.1	97.4	-0.10	0.922
Age of casehead	Unmatched	34.43	37.68	-21.9		-23.28	0.000
	Matched	34.43	34.46	-0.2	99.1	-0.16	0.874
Age of casehead squared	Unmatched	1394.50	1648.80	-18.5		-19.23	0.000
	Matched	1394.50	1397.80	-0.2	98.7	-0.20	0.845
Number of eligible children	Unmatched	1.50	1.33	12.6		13.11	0.000
	Matched	1.50	1.50	0.0	99.9	-0.01	0.995
Number of eligible members	Unmatched	2.62	2.48	9.2		9.45	0.000
	Matched	2.62	2.62	-0.1	98.7	-0.10	0.922
Other race x Adjusted wages two years prior	Unmatched	663.52	711.03	-1.2		-1.25	0.212
	Matched	663.52	688.39	-0.6	47.7	-0.53	0.599
Black x Adjusted wages two years prior	Unmatched	2256.70	2988.50	-11.0		-10.99	0.000
	Matched	2256.70	2243.10	0.2	98.1	0.18	0.858
Hispanic x Adjusted wages two years prior	Unmatched	254.40	534.25	-9.4		-8.57	0.000
	Matched	254.40	249.88	0.2	98.4	0.16	0.874
Other race x Adjusted wages one year prior	Unmatched	641.69	679.28	-1.0		-1.04	0.297
	Matched	641.69	672.98	-0.9	16.7	-0.70	0.482
Black x Adjusted wages one year prior	Unmatched	2312.50	2878.50	-8.7		-8.78	0.000
	Matched	2312.50	2284.70	0.4	95.1	0.37	0.715

(table continues)

Table A3, continued

Variable	Sample	Mean		Percent Bias	Percent Reduction Bias	T-Test	
		Treated	Matched Comparison			T-Stat	P-Value
Hispanic x Adjusted wages one year prior	Unmatched	269.55	549.44	-9.5		-8.65	0.000
	Matched	269.55	266.45	0.1	98.9	0.11	0.914
Percent of people in poverty	Unmatched	16.31	21.20	-31.9		-31.43	0.000
	Matched	16.31	16.24	0.4	98.7	0.38	0.707
Percent of households on public assistance	Unmatched	3.72	5.40	-32.3		-30.82	0.000
	Matched	3.72	3.70	0.5	98.6	0.43	0.671
Percent of female-headed families with child	Unmatched	20.69	25.71	-27.7		-27.67	0.000
	Matched	20.69	20.61	0.5	98.4	0.39	0.693
Unemployment rate	Unmatched	4.94	6.30	-31.6		-31.33	0.000
	Matched	4.94	4.92	0.4	98.9	0.31	0.758
Percent of males in the labor force	Unmatched	71.17	68.11	24.9		26.37	0.000
	Matched	71.17	71.23	-0.4	98.2	-0.35	0.727
Percent of females in the labor force	Unmatched	62.04	59.95	19.3		20.73	0.000
	Matched	62.04	62.09	-0.5	97.3	-0.40	0.690
Percent of families with no workers	Unmatched	25.34	29.72	-32.0		-32.43	0.000
	Matched	25.34	25.29	0.3	98.9	0.28	0.777
Percent of families with incomes less than two times the poverty line	Unmatched	62.58	56.60	28.3		28.89	0.000
	Matched	62.58	62.64	-0.3	99.0	-0.24	0.812
Percent of families with wage income	Unmatched	77.90	77.54	3.4		3.79	0.000
	Matched	77.90	77.96	-0.6	83.9	-0.42	0.672

(table continues)

Table A3, continued

Variable	Sample	Mean		Percent Bias	Percent Reduction Bias	T-Test	
		Treated	Matched Comparison			T-Stat	P-Value
Percent of individuals with some college	Unmatched	26.96	26.10	12.3		12.79	0.000
	Matched	26.96	26.92	0.6	95.2	0.48	0.633
Percent of individuals with a college degree	Unmatched	16.37	13.96	21.2		23.03	0.000
	Matched	16.37	16.36	0.0	99.8	0.03	0.975
Percent of 16- to 19-year-olds enrolled in school	Unmatched	74.54	74.29	1.3		1.41	0.160
	Matched	74.54	74.50	0.2	86.3	0.14	0.891
Percent of owner occupied households	Unmatched	51.14	49.94	5.0		5.34	0.000
	Matched	51.14	51.12	0.1	98.6	0.05	0.957
Percent of individuals who dropped out of high school	Unmatched	21.97	26.23	-31.4		-31.66	0.000
	Matched	21.97	21.99	-0.1	99.7	-0.09	0.932
Median income	Unmatched	41417.00	37805.00	24.9		26.14	0.000
	Matched	41417.00	41448.00	-0.2	99.1	-0.17	0.862
Median gross rent	Unmatched	504.38	499.76	3.9		4.28	0.000
	Matched	504.38	504.62	-0.2	94.8	-0.15	0.878
Median value of owner occupied households	Unmatched	89536.00	77908.00	23.6		26.06	0.000
	Matched	89536.00	89363.00	0.4	98.5	0.26	0.797
Percent of individuals who speak a language other than English	Unmatched	10.34	12.03	-13.8		-13.31	0.000
	Matched	10.34	10.44	-0.8	94.5	-0.69	0.488
Percent of Whites	Unmatched	69.89	56.42	38.1		38.31	0.000
	Matched	69.89	70.04	-0.4	98.9	-0.37	0.713

(table continues)

Table A3, continued

Variable	Sample	Mean		Percent Bias	Percent Reduction Bias	T-Test	
		Treated	Matched Comparison			T-Stat	P-Value
Percent of Blacks	Unmatched	17.74	29.93	-36.7		-36.21	0.000
	Matched	17.74	17.47	0.8	97.8	0.70	0.483
Percent of Hispanics	Unmatched	6.71	8.30	-12.5		-11.79	0.000
	Matched	6.71	6.84	-1.0	91.7	-1.00	0.319
Percent of households with two or more Non-Hispanics	Unmatched	1.51	1.50	0.8		0.86	0.392
	Matched	1.51	1.51	-0.5	35.0	-0.38	0.704

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