Institute for Research on Poverty
Discussion Paper no. 1338-08

Long-Term Effects of Public Low-Income Housing Vouchers on Work, Earnings, and Neighborhood Quality

Deven Carlson
La Follette School of Public Affairs
University of Wisconsin–Madison

Robert Haveman
La Follette School of Public Affairs
University of Wisconsin–Madison
E-mail: haveman@lafollette.wisc.edu

Thomas Kaplan
Institute for Research on Poverty
University of Wisconsin–Madison
E-mail: kaplan@ssc.wisc.edu

Barbara Wolfe
La Follette School of Public Affairs
University of Wisconsin–Madison
E-mail: wolfe@lafollette.wisc.edu

July 2008

Abstract

The federal Section 8 housing program provides eligible low-income families with an income-conditioned voucher that can be used to lease privately owned, affordable rental housing units. This paper extends prior research on the effectiveness of housing support programs in several ways. We use a quasi-experimental, propensity score matching research design, and examine the effect of housing voucher receipt on neighborhood quality, earnings, and work effort. Results are presented for a wide variety of demographic groups for up to five years following voucher receipt. The analysis employs a unique longitudinal dataset that was created by combining administrative records maintained by the State of Wisconsin with census block group data. The results of our propensity score matching procedure show voucher receipt to have no effect on neighborhood quality in the short-term, but positive long-term effects. Furthermore, the results indicate that on average voucher receipt causes lower earnings in the initial years following receipt, but that these negative earnings effects dissipate over time. Finally, we find that recipient responses to voucher receipt differ substantially across demographic subgroups.

Keywords: Housing policy, Employment, Propensity score matching

I. INTRODUCTION

Does the U.S. voucher-based housing policy targeted to low-income households lead to improvements in the well-being of those who receive assistance? This is the broad question we address in this paper. More specifically, we examine whether the U.S. Section 8 housing policy leads recipients to work more, earn more, and to live in better neighborhoods than they would have if they had not received such assistance.

Currently, the U.S. government provides housing assistance to low-income renters through three primary programs: Section 8 tenant-based subsidies (officially known as the Housing Choice Voucher Program since 1999); 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 program's primary objective 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

[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

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] 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]

II. RESEARCH ON THE EFFECTS OF SECTION 8 VOUCHER RECEIPT

A. 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). 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.

[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."

[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).

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 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, a voucher recipient's 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).[6] Finally, voucher recipients may voluntarily choose more non-work time due to the effective "income" gain associated with housing benefits.

Even if it were clear that receipt of a Section 8 voucher led to increased economic independence and well-being, identifying the causal mechanisms leading to this association is difficult. Evidence of greater labor market success over time for Section 8 voucher recipients relative to nonrecipients could reflect the fact that families obtaining a subsidy are more skillful in navigating complex bureaucracies, or that they are more persistent, or have other attributes that explain both their securing a Section 8 voucher and their economic success. In short, applying for voucher receipt and obtaining a better

[6] 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.

job likely require many of the same personal characteristics, making any causal effect of vouchers difficult to identify.

B. Empirical Research on the Effects of Section 8 Voucher Receipt

Location Change and Neighborhood Quality

Empirical research on the effects of low-income housing vouchers is extensive. Some studies show that Section 8 voucher recipients are less likely than public housing residents to live in high-poverty neighborhoods. For example, 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, Schroder, and Steffan (2003) found that just 9 percent of Section 8 recipients lived in census tracts in which 40 percent of the residents are poor. 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.

Nonexperimental Research on Labor Market Effects

Nonexperimental empirical studies addressing the labor market effects of housing vouchers have come to contrasting conclusions. Some have shown positive results. A study of families receiving Section 8 vouchers that left welfare in Cuyahoga County, Ohio (which encompasses the Cleveland metropolitan area), found that voucher recipients were 16 percent less likely than other welfare leavers to have returned to welfare the following year (Bania, Coulton, and Leete, 2003). The study used administrative data from several sources, and employed a regression model to control for demographic, welfare and earnings history, neighborhood poverty, and transportation

availability differences between welfare leavers who did and did not have a housing voucher. A study of the California Greater Avenues for Independence Program, conducted in four counties across the state, used data from a 1992 survey of California welfare recipients with information on age, education, presence of an infant, years on welfare in the most recent spell, disability, and county of residence. From their Tobit regression model results, the authors concluded that the receipt of housing assistance was positively associated with the number of hours that welfare recipients worked (Ong, 1998).

More recent nonexperimental studies have shown less positive results. Van Ryzin, Kaestner, and Main (2003) used a survey conducted in late 1995 and early 1996 of a representative sample of people eligible for or participating in welfare in New York City. They found no statistically significant effects of Section 8 receipt on movement from welfare to work, after controlling for a wide variety of observed differences between welfare participants who received and did not receive housing assistance. A study conducted by Olson et al. (2005) used HUD administrative data to identify recipients of housing vouchers and compared the work and earnings of these recipients to a comparison group of nonvoucher recipients generated from the Panel Survey of Income Dynamics. The study used a regression model to control for observed differences between the two groups and limited the comparisons to nonvoucher recipients in particular income strata to try to control for unobserved differences. The study found that the earnings of voucher recipients grew substantially less than did those of nonrecipients over the two-year period after voucher receipt.

A recent review of 11 nonexperimental studies of the economic effects of housing vouchers reported that the studies reached varied conclusions (Harkness and Newman, 2006). The review determined that four of these studies found voucher receipt to have a negative effect on earnings, one found a positive effect, and six studies found statistically insignificant effects. Overall, the nonexperimental literature does not reach a definitive conclusion, but the recent trend in the literature seems to suggest negative work and earnings effects associated with voucher receipt.

C. Experimental Studies of the Effects of Low-Income Housing Subsidies

The Gautreaux Program

The lack of clear conclusions from the early, nonexperimental studies led to an increased emphasis on housing policy experiments. The first of these studies resulted from a U.S. Supreme Court decision that required the Chicago Housing Authority to distribute housing vouchers to some 7,000 families that were either residing in Chicago public housing projects or on waiting lists to live in housing projects. The voucher distributions for what became known as the Gautreaux program occurred between 1976 and 1998, and recent studies have followed voucher recipients under the program for an average of 15 years (Mendenhall, DeLuca, and Duncan 2005). The Gautreaux program was a 'neighborhood effects' experiment, testing the effects of moving to city and suburban locations; hence, it is only tangentially relevant to the present study.[7] Evaluation findings generally point to a positive relationship between moving to a

[7] In theory, the Gautreaux project offers the advantages of a randomized trial of the effect of moving to a suburb compared to staying in an apartment in the city, since the project was designed so that receiving a voucher for a city or suburban apartment was to be a random event. However, many of the movers to suburbs returned to the city, leaving those in the suburbs to be a select group.

suburban location and desirable social and educational outcomes.[8] Critics of the study have questioned its generalizability. Because voucher recipients had to meet prescreening criteria, they may not have been representative of the population of the target group.[9] Moreover, evidence exists that assignment to city and suburban vouchers was not random (Durlauf, 2001; Goering, Feins, and Richardson, 2002).

Concerns about the validity of the Gautreaux findings stimulated additional experiments. Two large experiments, Moving to Opportunity (MTO) and Welfare to Work (WtW), were organized and funded by HUD. A natural experiment occasioned by a 1997 decision of the Chicago Housing Authority has also been analyzed.

The Moving to Opportunity (MTO) Experiment

The MTO program randomly assigned 4,608 families living in public housing projects in five cities (Baltimore, Boston, Chicago, Los Angeles, and New York) to one of three groups: 1.) a voucher group, which received Section 8 vouchers that could only be used to secure housing in a "low-poverty" neighborhood, defined as one in which less than 10 percent of the population had incomes below the poverty line;[10] 2.) a comparison group, which received Section 8 vouchers that could be used in any neighborhood; or, 3.)

[8] One study of the Gautreaux program found that children whose families moved to the suburbs were only one-quarter as likely as children whose families remained in the city to drop out of high school, and that adult children who did not go to college had a much higher rate of full-time employment (75 percent compared to 41 percent) if their families moved to the suburbs than if they remained in the city (Rosenbaum, 1995). Another study found that Gautreaux participants who moved to "high education" census tracts were less likely to have received AFDC in 1989 than were families that had moved to "low education" tracts (Rosenbaum and DeLuca, 2000). Mendenhall, DeLuca, and Duncan (2006) found that moving to more racially integrated neighborhoods was associated with better employment outcomes.

[9] Early decisions to limit the information that was gathered and maintained about the demographic and personal characteristics of Gautreaux families that did not choose to move constrained the ability of other researchers to assess this selection issue (Popkin et al., 2000).

[10] This group also received considerable housing counseling, which should also be considered part of the program treatment; the average cost of counseling for each successful "lease-up" in a low-poverty neighborhood was $3,077 (Shroder, 2002b).

a control group in which participants remained in their current housing project.[11] As such, the study is of neighborhood effects and of the effects of the form of housing assistance (Section 8 vs. public housing); the study does not compare the effects of voucher receipt vs. no voucher receipt, as does the research we report below. To date, a number of results from the experiment have been released (Goering, Feins, and Richardson, 2002; Goering, 2003). Many findings have derived from studies of individual sites conducted by independent teams of researchers,[12] although some analyses of cross-site data as much as four to seven years after random assignment have also been conducted (see, for example Kling, Liebman, and Katz, 2007). The full impact evaluation will be reported in 2008 or later.

Overall, these early MTO findings indicate that low-income housing project residents who move to low-poverty neighborhoods did not improve their labor market outcomes. Up to four to seven years after random assignment, the families lived in safer neighborhoods with lower poverty rates, but did not have higher earnings or lower welfare utilization than did the control group (Kling, Liebman, and Katz, 2007).

The Welfare to Work (WtW) Experiment

As we have noted, the MTO experiment was limited to residents in public housing projects and thus provides no information on how housing subsidies might affect a broader population of low-income participants. In addition, MTO compares the effect of three treatment alternatives—low-poverty voucher, regular voucher, and public

[11] Although families had to volunteer for the experiment, and families deciding to volunteer might have differed in important ways from public housing project residents who did not volunteer, the random assignment was intended to ensure that, at least among volunteers, any difference in measured outcomes was attributable to the treatment to which they were subjected and not to preexisting differences.

[12] Turney et al. (2006), for example, in a study based on interviews with program participants in Baltimore, found that participants who moved to low-poverty neighborhoods had fewer contacts who could help them find jobs in the retail and health care sectors most likely to employ them than was the case for control group members who stayed in high poverty neighborhoods.

housing—but it was not designed in a manner that permits an estimate of the effect of voucher receipt relative to nonreceipt. The WtW experiment was designed to address these limitations. Over a period of 13 months (from spring 2000 through spring 2001), the experiment enrolled some 8,700 families in six cities (Atlanta, Augusta, Fresno, Houston, Los Angeles, and Spokane), and assigned them to either a treatment or control group. The families did not have to live in public housing at the time of enrollment; just 7 percent were public housing residents. The treatment group received a "Welfare to Work" voucher, basically identical to a Section 8 voucher. The control group received no Section 8 benefits through the program, though a small percentage of them obtained regular Section 8 vouchers, by applying for them on their own. All the families were on, or were eligible for, Temporary Assistance for Needy Families (TANF) at the time of random assignment, or had previously received TANF cash benefits.

Abt Associates is responsible for operating the experiment and performing the evaluation. The evaluators have so far found that voucher use reduced employment rates in the first one to two years of voucher receipt, but that voucher recipients then increased their employment and earnings; after about 3.5 years, the receipt of a voucher appears to have had no significant overall impact overall on employment and earnings (but did increase public-assistance participation) (Mills et al., 2006).

The Chicago Housing Authority Experiment

The Chicago Housing Authority natural experiment makes use of a decision by the Authority to assign Section 8 vouchers at random to applicants on a waiting list (Jacob and Ludwig, 2006). The random assignment began in July 1997 and continued through 2003, and the authors have followed those who did and did not receive vouchers

since the random assignment. Over this time, those who received a voucher were found to work 7 percent less and earn 11 percent less than those who did not receive a voucher. Contrary to the WtW study, these negative employment effects did not dissipate over time.

In summary, the experimental literature on the earnings and employment effects of housing vouchers appears to suggest somewhat negative labor market effects associated with voucher receipt. Among relevant experiments, the MTO study found lower earnings among voucher recipients in the first few years of the experiment, but not by a statistically significant amount. The WtW 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. All three experiments were conducted in at least medium-sized cities; the smallest cities in the experiments, Augusta, GA, and Spokane, WA, (both included in the WtW study) have populations of around 200,000.

III. OUR RESEARCH APPROACH

In this paper, we study the effect that receiving a Section 8 voucher has on labor market success and neighborhood characteristics for low-income families in Wisconsin that requested or received food stamps and/or TANF benefits. The sample includes both urban and rural residents in a medium-sized, diverse state, and also includes those both 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 that first received Section 8 vouchers in 2000 through 2003 over multiple years after their entry into the program. Hence, we are able to obtain separate estimates of the relationship between housing vouchers and both short- and longer-term labor market success (as measured by earnings and employment rates) and neighborhood quality, 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

We use detailed information available in administrative records from the State of Wisconsin, and supplement this information with data from the United States Census. 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.[13] The rental subsidy

[13] 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

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. As a result of 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.[14]

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 group.[15] 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,

---

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.

[14]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.

[15] 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.

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 food stamps between 2000 and 2003, yielding four 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 2000 through 2003 cohorts, a unit is defined as being in the voucher group if its CARES case file indicates that the unit first received a rental subsidy in this particular calendar year or if its CARES case file indicates that the case received a rental subsidy after a minimum of two consecutive months of nonreceipt.[16] Nonrecipient units are those that received (or were in some stage of applying for) food stamps or TANF, but did not meet the voucher group criteria outlined above.

Table 1 summarizes the demographic characteristics of the two groups for the four cohorts used in our analysis. In some of the subsequent results presented below, we emphasize estimated effects for the non-2000 cohorts because of the slightly different criteria for inclusion in the voucher group for the 2000 cohort.[17]

[Insert table 1 here]

[16] We also excluded a few cases that reported earning over $50,000 in a calendar year.

[17] For the 2000 cohort, voucher group cases are those that meet one of two criteria. The first criterion is that the case appeared in the CARES database in November or December of 1999 with no rental subsidy and again at any point in 2000 with a rental subsidy. If a case does not appear in November or December of 1999 it could still be assigned to the treatment group if it appeared twice in 2000, with an indicator of rental subsidy receipt present only in the later entry. This slightly different definition of the treatment group for the 2000 cohort is due to the fact that the CARES database does not contain records of rental subsidy receipt prior to November of 1999.

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

In our research approach, we track and analyze the pattern of labor market and neighborhood quality outcomes for both housing voucher recipients and a *matched comparison* group of families that have not received housing assistance. We establish the matched comparison group through a propensity score matching metric, which yields unbiased estimates under a set of conditions that we are confident our sample and data meet.[18] Our objective is to determine if the neighborhood quality and labor market trajectories of those families and individuals receiving housing vouchers (the voucher group) differs significantly from the matched families that have not received such vouchers.[19]

Within the metric of propensity score matching, a number of matching methods can be used. Examples include the "nearest neighbor," "kernel," and "local linear regression" methods.[20] We employ nearest neighbor matching for this project. This

[18] As Smith and Todd (2005) state, "in order for matching estimators to have low bias, it is important that the data include a rich set of variables related to program participation and labor market outcomes, that the nonexperimental comparison group be drawn from the same local labor markets as the participants, and that the dependent variable (typically earnings) be measured in the same way for participants and non-participants" (p. 306).

[19] Propensity score matching techniques have recently been subject to some criticism. For example, Wilde and Hollister (2007) compare 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 the authors' calculations of results from propensity score matching. They find that the propensity score estimates failed to measure the "true" effect of smaller class sizes across the range of schools at which they were tried. However, many of the schools had very few children in the experiment, and the authors did not have, among other background variables, prior test scores to use in their matching. Although it is certainly the case that small sample sizes and a lack of relevant background variables pose trouble for propensity score estimation procedures, the propensity score matching analysis conducted in this paper is not afflicted with either of these limitations. In addition, 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.

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

method uses the estimated propensity score[21] for each voucher case and matches it to one or more nonrecipient cases with the closest or, more ideally, identical propensity scores. Appendix A provides more detail on our matching strategy and reports tests of its ability to secure unbiased and reliable estimates of the impact of Section 8 voucher receipt.

A key decision when performing the matching procedure involves identifying the variables that will be used to estimate the propensity score for each case. Our data yield an extensive set of covariates that are predictive of a case's participation in housing voucher programs. These variables include employment history, prior earnings, gender, race, age, number of children of the casehead or family unit, and urban-rural location. In addition, we include several census block group variables, such as the percentage of persons in poverty, the unemployment rate, and the percentage of households on public assistance in the model used to estimate each case's propensity score.[22] The matching procedure succeeds in balancing the included variables and eliminating pretreatment differences between the voucher and matched comparison groups on every covariate.[23] Complete results of the balance test can be found in Appendix A.

[21] 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).

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

[23] 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.

IV. ESTIMATION RESULTS[24]

A. Moves and Neighborhood Quality

Tables 2 and 3 indicate the effect of housing voucher receipt on the probability of changing residence within one year and within four years after the end of the month in which the case first received the housing voucher. The results presented in these tables were obtained through the propensity score matching procedure that is described briefly in an earlier section of this paper and in more detail in Appendix A. 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, while 69 percent of matched comparison group cases had moved. The receipt of a housing voucher appears to substantially increase the probability of changing residential location.

[Insert table 2 here]

[Insert table 3 here]

While a greater percentage of families receiving a voucher moved within both one and four years of receiving a voucher, relative to the matched comparisons, evidence on moves alone tells us nothing about the qualities of the neighborhoods to which these families relocated. Tables 2 and 3 also present evidence on the effect of voucher receipt

[24] The results we report here are for the 2001-2003 cohorts, which allow us to estimate impacts for five years post-voucher receipt. In results available from the authors, we also estimate impacts for the 2000-2003 cohorts, enabling six years' of post-treatment observation. The overall pattern of these results is similar to those shown here, with the trend in post-treatment gains continuing in the sixth year. In addition, results for each individual calendar-year cohort, which closely mirror the results presented in this paper, are also available from the authors.

with respect to four indicators of neighborhood quality. The top bank of each table indicates the pretreatment level of the relevant neighborhood variables for both groups.[25] The lack of statistically significant pretreatment differences between the voucher and matched comparison groups indicates the success of the matching procedure in eliminating baseline differences in neighborhood characteristics between the voucher group and their matched comparison cases.

The second bank of each table presents the level of the neighborhood quality variables one year after voucher receipt (Table 2) and four years after voucher receipt (Table 3). One year after voucher cases first received a housing subsidy, the matched neighborhood quality variables are largely similar for the voucher and matched comparison groups; only the unemployment rate variable indicates a statistically significant gain for voucher recipients. One year after voucher receipt, the mean unemployment rate of the census block groups in which voucher cases reside was 4.78 percent, while the same statistic for the matched comparison group is 4.97 percent. This pattern is consistent with the conjecture that recipients use their voucher primarily to move to a location with better employment opportunities.

The neighborhood quality results measured four years after voucher receipt tell a stronger story. Table 3 indicates that four years after receipt, voucher recipients lived in census block groups with a significantly greater percentage of 16- to 19-year-olds in

[25] Readers may note that the pretreatment means for the treatment and control groups in Table 2 differ slightly from those in Table 3. This difference is due to the fact that there were a small number of cases in the pooled data that were not able to be tracked a full four years post-treatment. Specifically, all address data are current as of July 2007, so any cases entering the voucher or matched comparison groups after July of 2003 are not able to be tracked a full four years post-voucher receipt.

school;[26] a lower poverty rate; and a lower unemployment rate; relative to the matched comparison cases. In addition, the median gross rent of the homes in the neighborhood is also higher for the voucher group, but the t-statistic of 1.5 is not quite statistically significant at conventional levels. In sum, over time, those families receiving a Section 8 voucher experienced statistically significant gains in neighborhood quality, relative to matched comparison cases.

B. Earnings and Employment

Table 4 summarizes our findings regarding the effect of receiving a Section 8 housing voucher on earnings and employment. The top bank of the table presents summary results on earned income from the initial year of housing voucher receipt to five years after receipt. For the full sample, receipt of a housing voucher was estimated to result, on average, in an $858 decline in annual earnings in the initial year of voucher receipt, or 12 percent of the average earnings for the matched comparison cases.[27] Apparently, either the dislocation accompanying the move or a negative work response to either the income or benefit-reduction-rate incentives associated with the voucher led to a reduction in earnings in the initial year of observation. However, by five years after voucher receipt, the negative earnings effect had fallen to $277, or to about 3 percent of the average earnings of the matched comparison cases; this difference is not statistically significant. The average change over five years between the voucher and matched comparison groups is about $580, in favor of the group receiving a voucher. In the years

[26] When the criterion for observing a move is one year (rather than four years), the difference in the schooling variable between the voucher and matched comparison groups actually increases slightly over the four post-receipt years.

[27] 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.

following receipt of a housing voucher, earnings of the voucher group increased by an average of nearly 5 percent per year, compared to an average annual increase of about 3.2 percent for the matched comparisons.

In addition to examining the effect of voucher receipt on earnings for the whole sample, we also analyze the results by selected demographic subgroups. Subgroups selected for analysis include gender, race/ethnicity, age, education level, family composition, and urbanicity. In the case of gender, the results reveal that the earnings difference between the voucher and matched comparison groups in the year of voucher receipt, measured as a percentage of the mean value of the matched comparison group, is far larger for men (20.2 percent) than for women (11.2 percent). In addition, by the end of the observation period, women in the voucher group had narrowed the difference to 3.3 percent of mean earnings of the matched comparison group. Men in the voucher group, on the other hand, still exhibited a difference of 7.9 percent five years after voucher receipt. The reduction in women's earnings differences between the voucher and matched comparison groups appears to be due to a substantial difference in the rate of earnings growth between the two groups (5.1 percent for the voucher group vs. 3.6 percent for the matched comparison cases). Among men, both the voucher and matched comparison groups showed little positive change in earnings over the five years.

In terms of racial differences, large negative differences in the initial year of observation turned into sizable positive, but statistically insignificant, differences after five years for both Blacks and Hispanics. This is reflected in the high annual rates of earnings growth for voucher-receiving members of these racial groups—6.2 percent for Hispanics and 5.2 percent for Blacks. When breaking results down by family

composition, both single- and two-parent families with children experienced substantial initial drops in earnings, but these negative earnings effects largely disappeared by the fifth year. For the cases without children, the negative effects persisted through the observation period. Over time, then, families with children exhibited larger positive earnings increases than did family units without children. This is consistent with the annual earnings growth rates of 5.5 percent for voucher cases with children, compared to decreased earnings over time for cases without children.

Interesting results also emerge when the sample is deconstructed and analyzed by the age of the casehead. For instance, voucher group cases in all three age subgroups exhibit a large earnings decline, relative to their matched comparisons, in the initial year of voucher receipt. The 18- to 30-year-old group exhibits the largest absolute decline, while the +50 group exhibits the largest relative decline. For both of these groups, however, the negative effects of voucher receipt on earnings dissipated over time, and were imperceptible after five years. This trend is at odds with the trend exhibited by the 31- to 49-year-old group, which continued to exhibit a substantial negative earnings effect five years after voucher receipt.[28]

The effect of housing voucher receipt on earnings also differs substantially by county urbanicity.[29] In rural areas, the mean earnings difference between members of the voucher group and their matched comparisons exceeded $1,000 in the initial year of voucher receipt. This difference was nearly 15 percent of the mean earnings for the

[28] The underlying causes of these differential patterns are unknown. Perhaps cases with income 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.

[29] 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.

matched comparison group. In contrast, the mean earnings difference between members of the voucher group residing in Milwaukee County and their matched comparisons was only $533 in the initial year of receipt, which corresponds to just 7.1 percent of the mean earnings for the matched comparison group. These findings indicate that the initial negative earnings response for voucher recipients in rural areas is about twice as large, both absolutely and relatively, as the initial negative earnings response of voucher recipients in Milwaukee County. Furthermore, voucher recipients in rural areas continue to exhibit a substantial difference in earnings, relative to their matched controls, five years after the voucher was first received. For cases in Milwaukee County, the initial negative earnings effect attributable to voucher receipt disappears over time, and eventually becomes positive, but statistically insignificant, after five years.

In terms of education level there is little systematic difference in the earnings response to a housing voucher. Both high school graduates and nongraduates had significantly lower earnings following the initial receipt of a housing voucher than did comparable individuals who did not receive a voucher. Five years after initial voucher receipt, the earnings difference between the voucher and matched comparison groups is not statistically significant for either group.

The pattern for employment (quarters worked per year) is shown in the bottom bank of Table 4. From the year of voucher receipt to five years post-receipt, the average quarters worked difference goes from a significant -.04 to +.05 ($t = 1.62$). This implies that by five years after voucher receipt, recipients work, on average, 4.5 days more per year than their matched comparison group cases (assuming 90 days per quarter). The

voucher group annual growth rate of quarters worked is -1.4 percent compared to a greater negative rate of -2.1 percent for the matched comparison group.

Across the subgroups, the quarters worked pattern is somewhat different than the earnings pattern. The treatment year matched difference is negative and significant only among Whites, cases with no children, and those over age 50; for all of the other subgroups, the first year difference is insignificant, with Blacks and Hispanics indicating a positive quarters worked effect. 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, and those aged over 50 years; the positive difference is statistically significant for Blacks and marginally significant for Hispanics. For nearly all of the groups, the mean level of quarters worked decreased over the five years of observation, for both matched comparison and voucher group families. However, for all of the subgroups, the rate of change in quarters worked indicates a greater probability of working for those receiving a housing voucher.

When we disaggregate the pooled cohorts, we see the same pattern of results for both earnings and quarters worked. The shrinking negative difference in earnings over time is clearly seen for each of the three cohorts in Figure 1, which shows the mean matched difference as well as the 90 percent confidence interval around the estimated difference. The figure also shows the small difference in quarters worked for both cohorts; for each cohort, the negative matched difference in the year of voucher receipt becomes positive by the final year of observation.

[Insert table 4 here]

C. Additional Comparisons

The results presented above summarize the primary neighborhood and labor market effects of the Section 8 program on the sample of observations that we analyze. In addition, we also explored a number of other patterns that may shed light on the overall effects of the program.

Neighborhood Quality Patterns for Those Who Moved

We performed several additional estimations that examine only those voucher recipients who moved within a certain time period after receiving a housing voucher. The first set of estimations analyzed voucher recipients who had moved at least once within four years of receiving the voucher. In this set of estimations, we performed two distinct matching procedures. In the first procedure, we matched each voucher recipient to five nonrecipients, regardless of whether the nonrecipients had moved in the past four years. In the second matching procedure, we restricted the pool of potential matches to nonrecipients who had moved within the past four years. Given the effect that voucher receipt has on the probability of moving, neither of these groups is ideal for comparison purposes, but by using both groups for comparison, we attempt to present a comprehensive picture. The second set of estimations is similar to the first, but analyzes only those voucher recipients that had moved within one year of receiving a voucher. The neighborhood improvements for the voucher group, using both the 365-day and 4-year criteria for observing a move, are similar to those shown in Tables 2 and 3. Table 5 summarizes the effect of voucher receipt on neighborhood quality indicators for voucher recipients who moved at least once within four years of receiving a voucher.[30]

[30] The matched comparison group in these results is composed of nonrecipient cases that had moved at least once in the past four years. Results in which voucher recipients are matched to nonrecipients, irrespective

[Insert table 5 here]

The results provide further evidence that those in the Section 8 program did indeed move to better neighborhoods than their matched comparisons. This appears to be the case for all four indicators: older adolescents attending school, those in poverty, and for the proportion of those in the labor force who are unemployed. Perhaps not surprisingly, the rent paid by voucher recipients is somewhat higher relative to their matched comparisons.

Labor Market Effects for Casehead Only

The results shown in Table 4 reflect earnings and quarters worked for all individuals who were listed in the CARES database as members of the voucher and matched comparison cases. We also estimated these patterns for the casehead alone. The casehead-only patterns are very similar to those for the entire case, 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.

Estimated Effects, Using Only Observations with Available Durations

The estimates that we have reported *pool* all of our observations in order to secure results for the entire period from 2001-2006. We also estimated separate models for cases with 3, 4, and 5 years' of post-voucher receipt observations. The following table briefly summarizes these models.[31]

[Insert table 6 here]

---

of whether the nonrecipients had moved, are available from the authors. Results analyzing the effect of voucher receipt for those cases that moved within one year of receiving a voucher are also available from the authors.

[31] The end-year difference for the 5-year sample is statistically insignificant.

Distinguishing Time, Duration, and Cohort Effects

Given that the years over which we observe these families span an employment slowdown and a recovery, is it possible that our estimated effects are reflecting macroeconomic conditions? In an effort to distinguish such possible effects, we estimated several additional models, including the estimation of 3-year effects for all three cohorts, regressing earnings on both cohort and year for the pooled observations, and regressing earnings on duration, year, and cohort, controlling for the effect of the voucher.

Overall, the robustness of our results suggests that the labor markets for our low-earnings cases may have been weaker in 2003 and possibly 2004 than in the other years. Hence, the results for the cohort first receiving a voucher in 2003 may be different than those for the other cohorts. However, in terms of the earnings differential, the results for the 2003 cohort exhibit the same general pattern as the results for the 2001 and 2002 cohorts—the 2003 earnings difference (between the treated and the matched comparison group cases) is $815, which is between the $768/$840 difference for the 2001/2002 cohorts (detailed results are available from the authors, upon request).[32]

We conclude that while 2003 may have been a bad year for potential low-wage workers in our sample, it was equally bad for workers in both the voucher groups and matched comparison groups. Hence, the effects of poor labor market conditions will

[32]Evidence that the year 2003 differs from the remaining observation years is seen in the mean earnings patterns for both the treatment and control groups. Treatment year mean earnings for voucher recipients were $6,602 and $6,474 in 2001 and 2002, respectively. In 2003, mean treatment year earnings for this group fell to $5,935. Mean control group earnings for these three years are $7,371, $7,315, and $6,750, which follow the same pattern.

depress outcomes equally for both groups, leaving our estimate of the effect of voucher receipt unaffected.

V. CONCLUSION

In this analysis, we have studied the causal effect of receipt of a housing voucher on a variety of neighborhood quality and labor market outcomes. 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 program stimulates geographic mobility. Although the initial post-treatment effects on the quality of the neighborhoods in which voucher recipients live suggests little improvement, observations made four years later indicate statistically significant gains in neighborhood quality for voucher recipients. Whereas the initial post-treatment impact of the program on recipient earnings appears to be negative, over the subsequent years voucher recipients increased their earnings at a substantially more rapid rate than did members of the matched comparison group.[33] There is some evidence of an initial negative post-treatment effect of voucher receipt on work effort (quarters worked per year), but by five years after treatment, voucher recipients recorded gains in quarters worked per year relative to the matched comparison group.

Our matched propensity score results reveal interesting patterns by subgroups. In general, we found that any negative earnings and employment effect of voucher participation was insignificant (or in a few cases even positive) for women, Blacks and Hispanics, families with children, and the oldest and youngest recipients (18-30 and 50+ years). The negative effects were larger and statistically significant for Whites, singles or

[33] This pattern is consistent with the results of the Welfare to Work (WtW) experiment, though our period of observation is somewhat longer.

couples without children, and those in the 30- to 50-year-old age groups. We also found that recipients in rural areas (but not urban areas) had significantly lower post-receipt earnings compared to matched comparisons both in the initial year of voucher receipt and four years later.

Our study of a diverse and large group of low-income families, rather than only those observed to have lived in public housing or medium to large urban areas, suggest interesting and substantially different responses to voucher receipt by individual and locational characteristics. Because of these differences among potential target groups, these findings suggest that future research should be expanded to encompass rural and urban groups, a wide spectrum of age groups, multiple racial groups, and those living in a variety of family compositions.

References

Bania, Neil, Claudia Coulton, and Laura Leete. 2003. "Public Housing Assistance, Public Transportation, and the Welfare-to-Work Transition." *Cityscape: A Journal of Policy Development and Research* 6:7-44.

Dehejia, R., and S. Wahba. 1999. "Causal Effects in Nonexperimental Studies: Reevaluating the Evaluation of Training Programs." *Journal of the American Statistical Association* 94(448): 1053–1062.

Dehejia, R., and S. Wahba. 2002. "Propensity Score Matching Methods for Nonexperimental Causal Studies." *Review of Economics and Statistics* 84(1): 151–161.

Durlauf, Steven N. 2001. "The Memberships Theory of Poverty: The Role of Group Affiliations in Determining Socioeconomic Outcomes." In *Understanding Poverty*, eds. Sheldon H. Danziger and Robert H. Haveman. New York: Russell Sage Foundation.

Dyke, Andrew, Carolyn J. Heinrich, Peter R. Mueser, Kenneth R. Troske, and Kyung-Seong Jeon. 2006. "The Effects of Welfare-to-Work Program Activities on Labor Market Outcomes." *Journal of Labor Economics* 24(3): 567-607.

Feins, Judith D. 2003. "A Cross-Site Analysis of MTO's Locational Impacts." In *Choosing a Better Life? Evaluating the Moving to Opportunity Social Experiment*, eds. John Goering and J. D. Feins. Washington, D.C.: Urban Institute Press.

Goering, John. 2003. "Comments on Future Research and Housing Policy." In *Choosing a Better Life? Evaluating the Moving to Opportunity Social Experiment*, eds. J. Goering and Judith D. Feins. Washington, D.C.: Urban Institute Press.

Goering, John, Judith D. Feins, and Todd M. Richardson. 2002. "A Cross-Site Analysis of Initial Moving to Opportunity Demonstration Results." *Journal of Housing Research* 13: 1–30.

Goering, John, Judith D. Feins, and Todd M. Richardson. 2003. "What Have We Learned about Housing Mobility and Poverty Deconcentration?" In *Choosing a Better Life? Evaluating the Moving to Opportunity Social Experiment*, eds. J. Goering and J. D. Feins. Washington, D.C.: Urban Institute Press.

Harkness, Joseph M., and Sandra J. Newman. 2006. "Recipients of Housing Assistance under Welfare Reform: Trends in Employment and Welfare Participation." *Housing Policy Debate* 17 (1): 81-108.

Heckman, James, H. Ichimura, and P. Todd. 1997. “Matching as an Econometric Evaluation Estimator: Evidence from Evaluating a Job Training Programme.” *Review of Economic Studies* 64(4): 605–654.

Heckman, James, H. Ichimura, Jeffrey Smith, and P. Todd. 1996. “Sources of Selection Bias in Evaluating Social Programs: An Interpretation of Conventional Measures and Evidence on the Effectiveness of Matching as a Program Evaluation Method.” *Proceedings of the National Academy of Sciences* 93(23): 13416–13420.

Heckman, James, H. Ichimura, Jeffrey Smith, and P. Todd. 1998. “Characterizing Selection Bias Using Experimental Data.” *Econometrica* 66(5): 1017–1098.

Hotz, V. Joseph, Guido W. Imbens, and Jacob A. Klerman. 2002. “The Long-Term Gains from GAIN: A Re-Analysis of the Impact of the California GAIN Program.” Working paper, Department of Economics, UCLA.

Jacob, Brian A., and Jens Ludwig. 2006. “The Effects of Means-Tested Housing Assistance on Labor Supply: New Evidence from a Housing Voucher Lottery. Presentation prepared for a Joint Center for Housing Studies symposium, “Revising Rental Housing: A National Policy Summit,” Harvard University, November.

Katz, Bruce, and Margery Austin Turner. 2000. “Who Should Run the Housing Voucher Program? A Reform Proposal.” Washington, D.C.: Brookings Institution, Center on Urban and Metropolitan Policy.

Khadduri, J., M. Shroder, and B. Steffen. (2003). “Can Housing Assistance Support Welfare Reform?” In *A Place to Live, a Means to Work: How Housing Assistance Can Strengthen Welfare Policy*, eds. Barbara Sard and Amy S. Bogdon. Washington D.C.: Fannie Mae Foundation.

Kling, Jeffrey R., Jeffrey B. Liebman, and Lawrence F. Katz. 2007. “Experimental Analysis of Neighborhood Effects.” *Econometrica* 75(1): 83-119.

Lechner, M. 2002. “Some Practical Issues in the Evaluation of Heterogeneous Labour Market Programmes by Matching Methods.” *Journal of the Royal Statistical Society*, Series A 165(Part 1): 59–82.

Mendenhall, Ruby, Stefanie DeLuca, and Greg Duncan. 2006. “Neighborhood Resources, Racial Segregation, and Economic Mobility: Results from the Gautreaux Program.” *Social Science Research* 35(4): 892-923.

Mueser, Peter R., Troske, Kenneth R., and Gorislavsky, Alexey. 2007. "Using State Administrative Data to Measure Program Performance." *Review of Economics and Statistics* 89(4): 761-783.

Mills, Gregory, Daniel Gubits, Larry Orr, David Long, Judie Feins, Balbul Kaul, and Michelle Wood. 2006. "Effects of Housing Vouchers on Welfare Families," at http://www.huduser.org/publications/commdevl/hsgvouchers.html.

Newman, Sandra J., and Ann B. Schnare. 1997. "'...And a Suitable Living Environment': The Failure of Housing Programs to Deliver on Neighborhood Quality." *Housing Policy Debate* 8: 703–741.

Olson, Edward O., Catherine A. Tyler, Jonathan W. King, and Paul E. Carrillo. 2005. "The Effects of Different Types of Housing Assistance on Earnings and Employment." *Cityscape: A Journal of Policy Development and Research* 8(2): 163-187.

Ong, Paul. 1998. "Subsidized Housing and Work among Welfare Recipients." *Housing Policy Debate* 9(4): 775–794.

Popkin, Susan, Larry Buron, Diane Levy, and Mary Cunningham. 2000. "The Gautreaux Legacy: What Might Mixed-Income and Dispersal Strategies Mean for the Poorest Public Housing Tenants?" *Housing Policy Debate* 11: 911–942.

Rosenbaum, James E. 1995. "Housing Mobility Strategies for Changing the Geography of Opportunity." *Housing Policy Debate* 6: 231–270.

Rosenbaum, James E., and Stefanie DeLuca. 2000. "Is Housing Mobility the Key to Welfare Reform? Lessons from Chicago's Gautreaux Project." Washington, D.C.: Brookings Institution Center on Urban and Metropolitan Policy.

Rosenbaum, P., and D. Rubin. 1983. "The Central Role of the Propensity Score in Observational Studies for Causal Effects." *Biometrika* 70: 41–55.

Ross, Catherine, John Reynolds, and Karlyn Geiss. 2000. "The Contingent Meaning of Neighborhood Stability for Residents' Psychological Well-Being." *American Sociological Review* 65(4): 581-597.

Shroder, Mark. 2002a. "Does Housing Assistance Perversely Affect Self-Sufficiency? A Review Essay." *Journal of Housing Economics* 11(4): 381-417.

Shroder, Mark. 2002b. "Locational Constraint, Housing Counseling, and Successful Lease-Up in a Randomized Housing Voucher Experiment." *Journal of Urban Economics* 51: 315-338

Shroder, Mark, and Marge Martin. 1996. *New Results from Administrative Data: Housing the Poor, or What They Don't Know Might Hurt Somebody*. Washington, D.C.: U.S. Department of Housing and Urban Development, Office of Policy Development and Research.

Smith, Jeffrey, and Petra Todd. 2005. "Does Matching Overcome Lalonde's Critique of Nonexperimental Estimators?" *Journal of Econometrics* 125: 305-353.

Swartz, Rebecca, and Brian Miller, 2002. "Welfare Reform and Housing." Welfare Reform and Beyond Policy Brief No. 16. Washington, D.C.: Brookings Institution.

Turney, Kristin, Susan Clampet-Lundquist, Kathryn Edin, Jeffrey R. Kling, and Greg J. Duncan. 2006. *Brookings-Wharton Papers on Urban Affairs 2006*, eds. Gary Burtless and Janet Rothenburg Pack. Washington, D.C.: Brookings Institution.

U.S. Department of Housing and Urban Development. 2000. "Section 8 Tenant-Based Housing Assistance: A Look Back after 30 Years." Washington, D.C.: U.S. Department of Housing and Urban Development.

U. S. Department of Housing and Urban Development. 2007. *Public Information Center: Resident Characteristics Report*. Retrieved October 13, 2007 from https://pic.hud.gov/pic/RCRPublic/rcrmain.asp

Van Ryzin, Gregg G., Robert Kaestner, and Thomas J. Main. 2003. "The Effects of Fedral and Local Housing Programs on the Transition from Welfare to Work: Evidence from New York City." *Cityscape: A Journal of Policy Development and Research* 6:45-72.

Wilde, Elizabeth T., and Robinson Hollister. 2007. "How Close Is Close Enough? Evaluating Propensity Score Matching Using Data from a Class Size Reduction Experiment." *Journal of Policy Analysis and Management* 26(3): 455-477.

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.[34] Table A1 presents the results of the logit model used to estimate the propensity scores for the pooled 2001 through 2003 cohorts.[35] [36]

[Insert table A1 here]

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

[34] 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.

[35] Propensity score estimation and matching was performed using Stata's "psmatch2" procedure.

[36] Matching results that include the 2000 cohort are available from the authors.

comparison cases with the most similar, often identical, propensity scores.[37] We then compared the voucher cases with their matched comparison group cases on two labor market outcomes, earnings and employment, and four census block group variables used to indicate neighborhood quality: percentage of people in poverty, percentage of 16- to 19-year-olds in school, median gross rent, and the unemployment rate. Comprehensive results for the neighborhood quality indicators were presented in the body of this paper. Comparable results for the labor market variables can be found in Table A2.

Because we pooled three cohorts to construct our dataset, we ran multiple propensity score estimation and matching procedures to obtain the estimates presented in Table A2. These multiple estimations result from the fact that we are able to track each cohort for varying lengths of time post-treatment. All three cohorts can be used to estimate the effect of receiving a housing voucher for the year of voucher receipt, one year post receipt, two years post receipt, and three years post receipt. Two cohorts, the 2001 and 2002 groups, can be used if the observation period is extended to four years after the voucher was first received, and only the 2001 cohort can yield estimates of the effect of a housing voucher five years post-treatment. The results presented in Table A2 are based on these multiple estimations. Because these estimates are based on multiple estimations, there is no tabulation of cases on common support. That said, for all three estimations used to create these results, every observation, whether treated or untreated, is on support.[38]

[37] 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.

[38] For the three years post-treatment calculation, the number of treated observations is 12,266, and the number of untreated is 383,433. Numbers of treated and untreated observations for the four and five years post-treatment calculations are 8,245 and 235,187, respectively (four years post-treatment), and 3,992 and 106,199, respectively (five years post-treatment).

We performed multiple diagnostic tests to ensure that the matching procedure provided us with valid results. First, we examined the pretreatment values of our six 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. Tables 2 and 3 in the body of this paper present the results of the diagnostic test for the four neighborhood quality outcome variables. 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.[39] 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.

[Insert table A2 here]

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.[40] The results illustrate that the matching procedure was highly successful in balancing the voucher and matched comparison groups on all observed

[39] The pretreatment outcomes were estimated using the 2001-2003 cohorts, the same sample used to estimate the effects for the treatment year through three years post-treatment. Substantively similar test results are achieved if the pretreatment outcomes are estimated for the 2001 and 2002 cohorts (used if the observation period is extended four years post-treatment) or only the 2001 cohort (used if the observation period is extended five years post-treatment). These results are available from the authors.

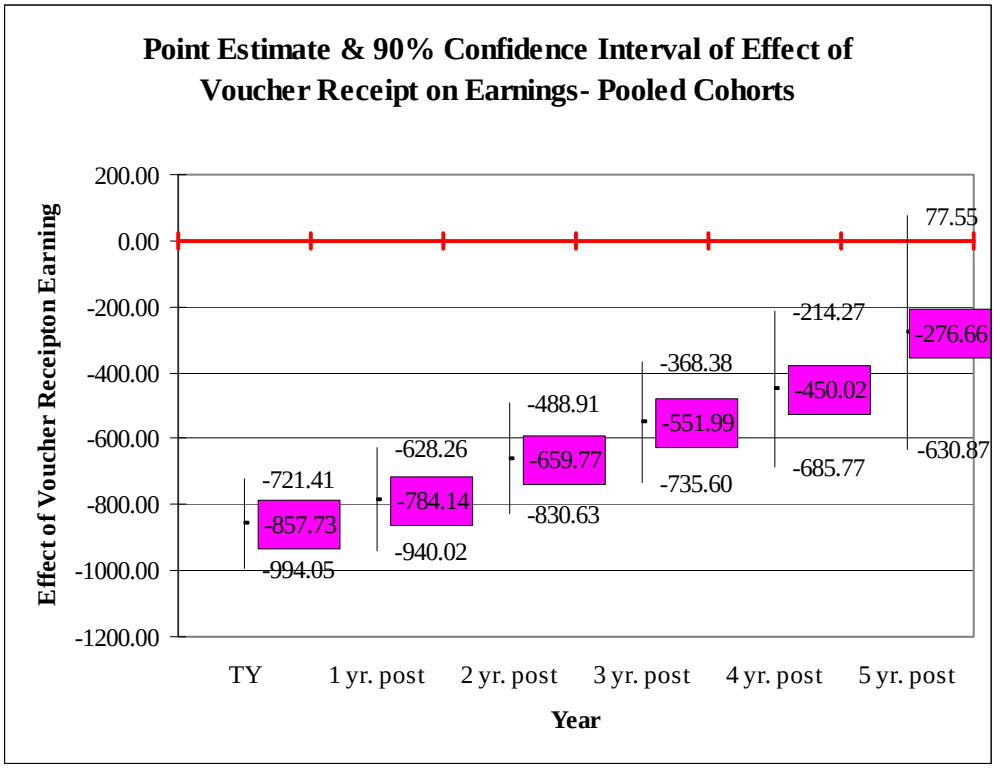
[40] The balance test results are based on the estimation using the 2001-2003 cohorts, the sample used to estimate the effects for the treatment year through three years post-treatment. Substantively similar test results are achieved if the pretreatment outcomes are estimated for the 2001 and 2002 cohorts (used if the observation period is extended four years post-treatment) or only the 2001 cohort (used if the observation period is extended five years post-treatment). These results are available from the authors.

covariates. Indeed, no statistically significant differences exist between the groups for any of the variables used in the propensity score estimation.

[Insert table A3 here]

Figures and Tables

Figure 1. Effect of Voucher Receipt on Earnings and Quarters Worked: Pooled Cohorts



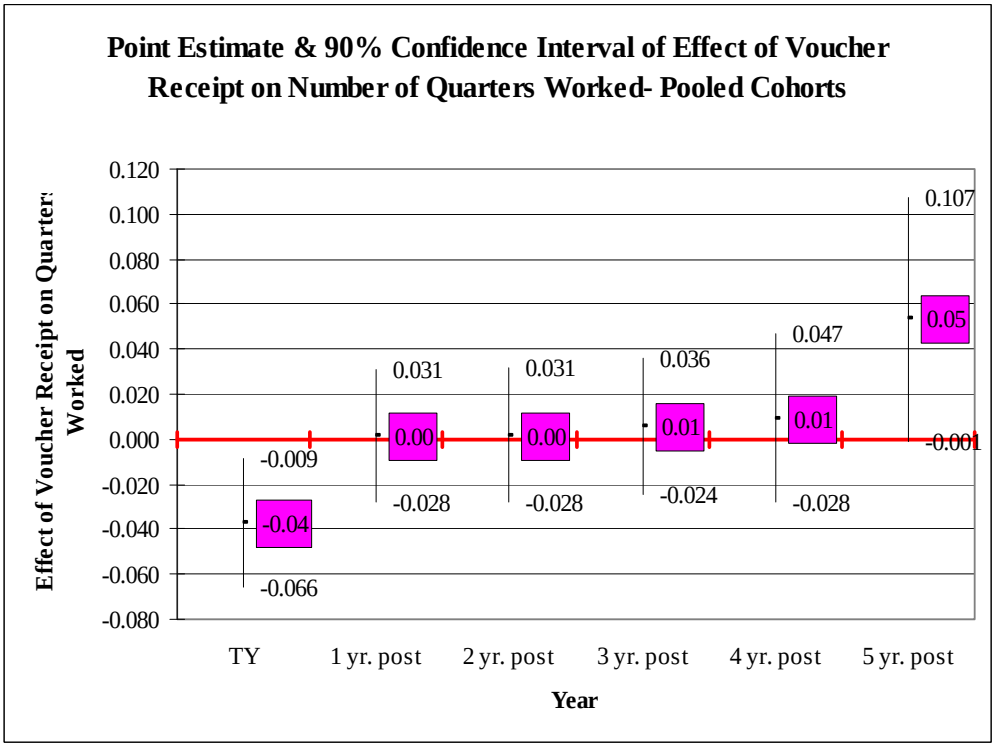


Table 1: Demographic characteristics for those who receive rental subsidies and those who do not receive rental subsidies: 2000-2003 cohorts

	2000		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	Receive Rent Subsidy	Do Not Receive Rent Subsidy
Total Number of Cases	1,903	146,848	6,159	163,391	6,080	187,276	5,383	216,064
Sex								
Male	7.4	22.7	15.6	24.8	15.2	26.7	15.3	28.0
Female	92.6	77.3	84.4	75.2	84.8	73.3	84.7	72.1
Age								
18-30	52.2	38.1	42.9	37.9	47.0	38.4	48.0	39.1
31-45	34.7	35.8	29.9	36.0	30.2	36.1	28.3	35.8
46-59	8.1	13.1	12.6	13.5	12.3	14.2	14.0	14.9
60+	4.9	12.5	14.2	12.1	10.3	10.8	9.4	9.7
Race								
White	48.4	48.2	59.3	48.2	58.7	48.9	60.4	50.9
Black	40.2	36.5	29.0	36.2	28.5	35.6	26.9	34.5
Hispanic	4.1	6.0	3.3	6.2	3.7	6.3	3.1	6.3
Other	7.3	9.4	8.5	9.4	9.2	9.2	9.6	8.4
Education Level								
No high school diploma	37.7	40.9	35.4	39.7	34.1	37.9	33.2	36.3
High school diploma	62.3	59.1	64.6	60.3	65.9	62.1	66.8	63.7
Marital Status								
Single, never married	56.7	49.1	50.5	50.2	52.0	51.4	52.0	52.7
Divorced or annulled	17.1	18.3	20.4	17.9	21.4	17.7	21.7	17.5
Separated	11.3	11.0	11.3	10.7	11.2	10.4	11.0	9.8
Married	12.2	14.6	10.7	15.1	10.3	15.3	10.7	15.5
Widowed	2.7	6.9	7.0	6.0	5.1	5.2	4.6	4.5
County Urbanicity								
Rural	22.4	21.6	28.0	21.6	26.9	21.6	30.6	22.2
Urban	41.6	30.5	45.9	31.0	51.8	32.0	51.3	33.7
Milwaukee	36.0	47.9	26.1	47.4	21.3	46.4	18.1	44.1
Number of Children								
0	16.6	43.9	39.8	44.9	36.1	45.9	35.9	48.1
1	27.3	21.1	25.6	21.2	27.6	21.3	27.2	21.1
2	28.0	16.7	19.1	16.5	19.6	16.3	20.1	15.6
3+	28.1	18.3	15.5	17.4	16.8	16.4	16.9	15.2

Table 2. Effect of housing voucher receipt on the probability of changing residence and on quality of neighborhood of residence: One year after voucher receipt

Outcome variable	Voucher Group	Matched comparisons	Difference	T-stat
Number of Observations	12,840	413,576	NA	NA
Probability of Moving within 365 days of voucher receipt	0.58	0.44	0.14	**27.71**
Median Gross Rent-Pre voucher receipt	504.05	504.86	-0.82	-0.66
Percent of 16-19 year olds in school- Pre voucher receipt	74.53	74.63	-0.10	-0.50
Percent in Poverty- Pre voucher receipt	16.34	16.24	0.10	0.71
Unemployment Rate- Pre voucher receipt	4.95	4.94	0.01	0.15
Median Gross Rent-One year after voucher receipt	507.28	507.72	-0.44	-0.34
Percent of 16-19 year olds in school-One year after voucher receipt	74.48	74.83	-0.34	**-1.72**
Percent in Poverty-One year after voucher receipt	16.13	16.18	-0.05	-0.37
Unemployment Rate-One year after voucher receipt	4.78	4.97	-0.19	**-4.86**

Table 3. Effect of housing voucher receipt on the probability of changing residence and on quality of neighborhood of residence: Four years after voucher receipt

Outcome variable	Voucher Group	Matched comparisons	Difference	T-stat
Number of Observations	10,303	381,998	NA	NA
Probability of Moving within four years of voucher receipt	0.77	0.69	0.08	**16.18**
Median Gross Rent-Pre voucher receipt	505.16	505.02	0.14	0.10
Percent of 16-19 year olds in school- Pre voucher receipt	74.36	74.37	-0.01	-0.03
Percent in Poverty- Pre voucher receipt	16.51	16.49	0.02	0.12
Unemployment Rate- Pre voucher receipt	5.01	5.00	0.01	0.24
Median Gross Rent-Four years after voucher receipt	516.69	514.41	2.28	1.50
Percent of 16-19 year olds in school-Four years after voucher receipt	75.58	75.12	0.47	**2.10**
Percent in Poverty-Four years after voucher receipt	15.50	15.79	-0.30	**-1.90**
Unemployment Rate-Four years after voucher receipt	4.71	4.93	-0.22	**-4.95**

Table 4. Matched differences and average annual growth rates for earnings and quarters worked, by subgroup: Pooled cohorts 2001-2003

	Subgroup									
	Full sample	**Male**	**Female**	**White**	**Black**	**Hispanic**	**Other race**	**Age 18-30**	**Age 31-49**	**Age 50+**
Earnings										
Treatment year matched difference	**-857.73**	**-756.25**	**-852.19**	**-986.73**	**-629.96**	-654.65	**-734.64**	**-936.66**	**-786.95**	**-442.65**
Matched difference as percent of control value	-11.9	-20.2	-11.2	-14.6	-8.3	-7.5	-9.0	-10.8	-10.8	-27.5
Ending year matched difference- 5 yrs. post treatment	-276.66	-264.99	-306.42	**-682.30**	341.57	843.54	-97.94	-84.58	**-671.89**	46.70
Matched difference as percent of control value	-3.2	-7.9	-3.3	-8.3	3.7	7.9	-1.0	-0.8	-7.2	3.3
Average annual growth rate- Treatment	4.9	0.6	5.1	4.6	5.2	6.2	4.7	5.3	5.0	3.8
Average annual growth rate- Control	3.2	-1.8	3.6	3.4	3.1	3.5	3.2	3.4	4.3	-2.2
Quarters worked										
Treatment year matched difference	**-0.04**	-0.08	-0.03	**-0.06**	0.01	0.07	0.04	0.00	-0.02	**-0.08**
Matched difference as percent of control value	-1.6	-5.8	-1.5	-2.8	0.3	2.8	1.7	-0.1	-0.8	-11.8
Ending year matched difference- 5 yrs. post treatment	0.05	0.03	0.05	0.00	**0.11**	0.30	0.18	0.09	0.02	0.00
Matched difference as percent of control value	2.5	3.6	2.3	0.0	5.1	14.2	8.6	3.6	1.0	0.0
Average annual growth rate- Treatment	-1.4	-6.0	-1.2	-2.2	-0.4	-0.2	-1.3	-1.1	-1.0	-5.6
Average annual growth rate- Control	-2.1	-7.4	-1.9	-2.7	-1.1	-1.9	-2.3	-1.7	-1.3	-7.5

NOTE: Estimates that are statistically significant at the 5 percent level are indicated in bold.

Table 4 (continued). Matched differences and average annual growth rates for earnings and quarters worked, by subgroup: Pooled cohorts 2001-2003

	Subgroup									
	Full sample	**Milwaukee county**	**Other urban county**	**Rural county**	**Any case with children**	**Single parent with children**	**Any case with no children**	**Couple with children**	**No HS diploma**	**HS diploma**
Earnings										
Treatment year matched difference	**-857.73**	**-533.79**	**786.06**	**1025.20**	**1071.98**	**-821.77**	**-433.28**	**1917.09**	**-695.82**	**-980.14**
Matched difference as percent of control value	-11.9	-7.1	-11.0	-14.9	-12.0	-10.1	-19.0	-14.2	-11.9	-12.4
Ending year matched difference- 5 yrs. post treatment	-276.66	297.12	-55.66	-557.78	-68.00	-179.41	-341.26	-721.09	-40.19	-297.47
Matched difference as percent of control value	-3.2	3.3	-0.6	-6.6	-0.6	-1.8	-16.4	-4.2	-0.6	-3.1
Average annual growth rate- Treatment	4.9	4.7	4.7	5.2	5.5	5.4	-1.4	5.9	4.4	5.2
Average annual growth rate- Control	3.2	2.8	2.8	3.5	3.4	3.9	-2.4	4.0	2.4	3.4
Quarters worked										
Treatment year matched difference	**-0.04**	-0.02	-0.02	-0.06	-0.03	-0.02	**-0.06**	-0.01	-0.01	-0.04
Matched difference as percent of control value	-1.6	-0.7	-0.7	-2.7	-1.3	-0.7	-6.0	-0.3	-0.6	-1.7
Ending year matched difference- 5 yrs. post treatment	0.05	0.09	0.07	0.02	0.07	0.06	-0.04	0.08	0.06	0.04
Matched difference as percent of control value	2.5	4.6	3.5	1.0	2.8	2.4	-5.9	2.7	3.3	1.9
Average annual growth rate- Treatment	-1.4	-0.5	-1.7	-1.6	-0.7	-0.7	-6.9	-0.9	-1.3	-1.5
Average annual growth rate- Control	-2.1	-1.4	-2.4	-2.2	-1.4	-1.2	-7.0	-1.3	-1.9	-2.1

NOTE: Estimates that are statistically significant at the 5 percent level are indicated in bold.

Table 5. Results of propensity score matching for selected outcome variables: Matched comparisons include only those that moved

Outcome variable	Voucher Group	Matched Comparisons	Difference	T-stat
Median Gross Rent-4 years after voucher receipt	522.60	520.50	2.10	1.19
Percent of 16-19 year olds in school- 4 years after voucher receipt	76.04	75.52	0.51	2.03
Percent in Poverty-4 years after voucher receipt	15.12	15.51	-0.39	-2.19
Unemployment Rate-4 years after voucher receipt	4.67	4.89	-0.22	-4.41

Table 6. Estimated effects of voucher receipt on earnings, by duration of case observation

	Cases with 3 years of post-receipt observation	Cases with 4 years of post-receipt observation	Cases with 5 years of post-receipt observation
Matched difference in initial year of observation	-857.73	-773.30	-768.60
Matched difference in final year of observation	-551.99	-450.02	-276.66
Change in matched difference from initial to final year	305.74	323.28	491.95

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	-0.57	0.032	0.000
Black	0.28	0.038	0.000
Hispanic	-0.25	0.074	0.001
Other race	-0.12	0.052	0.019
Years of education	0.01	0.006	0.089
Annulled	1.05	0.467	0.025
Divorced	0.38	0.039	0.000
Single	0.35	0.036	0.000
Separated	0.39	0.042	0.000
Widowed	0.17	0.068	0.014
Unknown marital status	1.63	0.609	0.007
Adjusted wages two years prior	0.00	0.000	0.026
Adjusted wages one year prior	0.00	0.000	0.000
One quarter worked one year prior	0.09	0.041	0.038
Two quarters worked one year prior	0.15	0.039	0.000
Three quarters worked one year prior	0.23	0.040	0.000
Four quarters worked one year prior	0.34	0.042	0.000
One quarter worked two years prior	0.12	0.041	0.003
Two quarters worked two years prior	0.04	0.040	0.320
Three quarters worked two years prior	0.12	0.039	0.003
Four quarters worked two years prior	0.14	0.040	0.000
Age of casehead	-0.03	0.003	0.000
Age of casehead squared	0.00	0.000	0.000
Number of eligible children	0.12	0.025	0.000
Number of eligible members	-0.07	0.023	0.002
Other race x Adjusted wages two years prior	0.00	0.000	0.703
Black x Adjusted wages two years prior	0.00	0.000	0.121
Hispanic x Adjusted wages two years prior	0.00	0.000	0.291
Other race x Adjusted wages one year prior	0.00	0.000	0.005
Black x Adjusted wages one year prior	0.00	0.000	0.000
Hispanic x Adjusted wages one year prior	0.00	0.000	0.049
Percent of people in poverty	0.00	0.002	0.005
Percent of households on public assistance	-0.01	0.003	0.000
Pct of female headed families with child	0.00	0.001	0.000
Unemployment rate	-0.01	0.004	0.001
Percent of males in the labor force	0.00	0.001	0.622
Percent of females in the labor force	0.00	0.002	0.035
Percent of families with no workers	0.00	0.001	0.603
Percent of families with incomes less than two times the poverty line	0.01	0.002	0.001
Percent of families with wage income	-0.01	0.002	0.000
Percent of individuals with some college	0.00	0.002	0.840
Percent of individuals with a college degree	0.01	0.002	0.000
Percent of 16-19 year olds enrolled in school	0.00	0.001	0.931
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	0.596

Median income	0.00	0.000	0.337
Median gross rent	0.00	0.000	0.025
Median value of owner occupied households	0.00	0.000	0.732
Percent of individuals who speak a language other than English	0.00	0.002	0.341
Percent of Whites	0.00	0.002	0.212
Percent of Blacks	0.00	0.002	0.764
Percent of Hispanics	0.00	0.002	0.995
Percent of households with two or more Non-Hispanics	0.07	0.012	0.000

Regression Statistics	
N	395,699
Pseudo R-squared	0.0840
Log likelihood	-50,092.394

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

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	7154.87	7887.19	-732.32	93.29	-7.85
	Matched	7154.87	7145.35	9.52	86.37	0.11
Adjusted wages- Treatment Year	Unmatched	6338.99	7490.35	-1151.36	90.13	-12.77
	Matched	6338.99	7196.73	-857.73	82.62	-10.38
Adjusted wages- 1 year post	Unmatched	6912.44	7907.42	-994.98	100.74	-9.88
	Matched	6912.44	7696.58	-784.14	94.47	-8.30
Adjusted wages- 2 years post	Unmatched	7399.31	8255.13	-855.82	107.93	-7.93
	Matched	7399.31	8059.08	-659.77	103.55	-6.37
Adjusted wages- 3 years post	Unmatched	7840.93	8589.41	-748.48	113.99	-6.57
	Matched	7840.93	8392.92	-551.99	111.28	-4.96
Adjusted wages- 4 years post	Unmatched	8205.33	8730.68	-525.36	144.17	-3.64
	Matched	8205.33	8655.35	-450.02	142.88	-3.15
Adjusted wages- 5 years post	Unmatched	8441.25	8845.82	-404.57	213.53	-1.89
	Matched	8441.25	8717.91	-276.66	214.67	-1.29
Quarters worked- One Year Prior	Unmatched	2.40	2.19	0.21	0.02	13.46
	Matched	2.40	2.40	0.00	0.02	-0.11
Quarters worked- Treatment Year	Unmatched	2.24	2.07	0.17	0.02	10.42
	Matched	2.24	2.28	-0.04	0.02	-2.15
Quarters worked- 1 year post	Unmatched	2.17	1.98	0.19	0.02	11.37
	Matched	2.17	2.17	0.00	0.02	0.08
Quarters worked- 2 years post	Unmatched	2.12	1.94	0.18	0.02	10.60
	Matched	2.12	2.12	0.00	0.02	0.09
Quarters worked- 3 years post	Unmatched	2.10	1.93	0.17	0.02	10.02
	Matched	2.10	2.09	0.01	0.02	0.33
Quarters worked- 4 years post	Unmatched	2.08	1.90	0.18	0.02	8.73
	Matched	2.08	2.07	0.01	0.02	0.41
Quarters worked- 5 years post	Unmatched	2.06	1.88	0.18	0.03	6.08
	Matched	2.06	2.00	0.05	0.03	1.62

Table A3. Balance test results for pooled 2001-2003 cohorts

Variable	Sample	Mean		Percent Bias	Percent Reduction Bias	T-test	
		Treated	Matched comparison			T-stat	p-value
Male	Unmatched	0.11	0.21	-29.8		-28.93	0.000
	Matched	0.11	0.11	-0.4	98.6	-0.37	0.709
Black	Unmatched	0.31	0.38	-15.2		-16.18	0.000
	Matched	0.31	0.30	0.9	93.9	0.75	0.453
Hispanic	Unmatched	0.03	0.06	-11.0		-10.83	0.000
	Matched	0.03	0.03	0.4	96.7	0.33	0.743
Other race	Unmatched	0.08	0.07	0.4		0.44	0.658
	Matched	0.08	0.08	-0.6	-39.0	-0.44	0.662
Years of education	Unmatched	11.60	11.44	8.4		8.67	0.000
	Matched	11.60	11.60	-0.4	95.6	-0.31	0.758
Annulled	Unmatched	0.00	0.00	0.9		1.10	0.273
	Matched	0.00	0.00	0.0	100.0	0.00	1.000
Divorced	Unmatched	0.20	0.18	5.1		5.61	0.000
	Matched	0.20	0.20	0.0	99.3	-0.03	0.980
Single	Unmatched	0.53	0.51	3.7		3.99	0.000
	Matched	0.53	0.53	0.2	94.4	0.16	0.872
Separated	Unmatched	0.12	0.10	4.2		4.73	0.000
	Matched	0.12	0.12	-0.8	81.8	-0.59	0.558
Widowed	Unmatched	0.04	0.06	-6.8		-6.97	0.000
	Matched	0.04	0.04	0.2	96.5	0.20	0.840

Unknown marital status	Unmatched	0.00	0.00	1.0		1.42	0.155
	Matched	0.00	0.00	-0.9	17.1	-0.51	0.607
Earnings two years prior	Unmatched	7604.20	8392.90	-7.8		-7.90	0.000
	Matched	7604.20	7612.60	-0.1	98.9	-0.07	0.943
Earnings one year prior	Unmatched	7154.90	7887.20	-7.8		-7.85	0.000
	Matched	7154.90	7145.40	0.1	98.7	0.09	0.930
One quarter worked one year prior	Unmatched	0.08	0.08	0.1		0.11	0.911
	Matched	0.08	0.08	0.0	52.7	0.04	0.970
Two quarters worked one year prior	Unmatched	0.10	0.09	2.9		3.23	0.001
	Matched	0.10	0.10	0.4	86.6	0.30	0.764
Three quarters worked one year prior	Unmatched	0.13	0.11	5.7		6.39	0.000
	Matched	0.13	0.13	-0.2	96.9	-0.13	0.895
Four quarters worked one year prior	Unmatched	0.43	0.40	7.1		7.83	0.000
	Matched	0.43	0.43	-0.1	98.1	-0.11	0.916
One quarter worked two years prior	Unmatched	0.08	0.07	2.2		2.39	0.017
	Matched	0.08	0.08	-0.9	57.7	-0.70	0.485
Two quarters worked two years prior	Unmatched	0.09	0.09	1.2		1.32	0.187
	Matched	0.09	0.09	0.5	57.3	0.40	0.689
Three quarters worked two years prior	Unmatched	0.13	0.11	5.3		5.96	0.000
	Matched	0.13	0.13	0.1	97.6	0.10	0.921
Four quarters worked two years prior	Unmatched	0.44	0.42	5.4		5.87	0.000
	Matched	0.44	0.44	-0.3	93.8	-0.26	0.795

Age of casehead	Unmatched	34.41	37.76	-22.4		-23.71	0.000
	Matched	34.41	34.40	0.1	99.5	0.09	0.929
Age of casehead squared	Unmatched	1393.60	1662.90	-19.4		-20.07	0.000
	Matched	1393.60	1390.10	0.3	98.7	0.21	0.834
Number of eligible children	Unmatched	1.50	1.33	12.6		13.16	0.000
	Matched	1.50	1.50	-0.1	99.6	-0.04	0.967
Number of eligible members	Unmatched	2.62	2.47	9.4		9.75	0.000
	Matched	2.62	2.62	-0.2	97.7	-0.18	0.860
Other race x Adjusted wages two years prior	Unmatched	667.43	707.41	-1.0		-1.07	0.285
	Matched	667.43	687.46	-0.5	49.9	-0.42	0.674
Black x Adjusted wages two years prior	Unmatched	2261.10	2925.70	-10.1		-10.19	0.000
	Matched	2261.10	2218.00	0.7	93.5	0.57	0.569
Hispanic x Adjusted wages two years prior	Unmatched	259.23	500.16	-8.2		-7.68	0.000
	Matched	259.23	245.38	0.5	94.3	0.48	0.628
Other race x Adjusted wages one year prior	Unmatched	642.21	674.12	-0.9		-0.90	0.367
	Matched	642.21	658.13	-0.4	50.1	-0.36	0.719
Black x Adjusted wages one year prior	Unmatched	2319.00	2824.10	-7.8		-7.99	0.000
	Matched	2319.00	2257.60	1.0	87.8	0.81	0.418
Hispanic x Adjusted wages one year prior	Unmatched	276.70	513.85	-8.2		-7.64	0.000
	Matched	276.70	259.12	0.6	92.6	0.61	0.539
Percent of people in poverty	Unmatched	16.34	21.05	-30.8		-30.52	0.000
	Matched	16.34	16.24	0.6	97.9	0.57	0.569

Percent of households on public assistance	Unmatched	3.73	5.30	-30.6		-29.45	0.000
	Matched	3.73	3.70	0.6	98.0	0.58	0.562
Pct of female headed families with child	Unmatched	20.75	25.51	-26.3		-26.40	0.000
	Matched	20.75	20.66	0.5	98.2	0.42	0.678
Unemployment rate	Unmatched	4.95	6.22	-29.6		-29.60	0.000
	Matched	4.95	4.94	0.1	99.5	0.12	0.903
Percent of males in the labor force	Unmatched	71.14	68.22	23.6		25.14	0.000
	Matched	71.14	71.21	-0.5	97.9	-0.40	0.688
Percent of females in the labor force	Unmatched	62.02	60.03	18.4		19.91	0.000
	Matched	62.02	62.08	-0.5	97.0	-0.43	0.668
Percent of families with no workers	Unmatched	25.37	29.50	-30.1		-30.76	0.000
	Matched	25.37	25.27	0.7	97.5	0.62	0.534
Percent of families with incomes	Unmatched	62.54	56.76	27.5		28.23	0.000
less than two times the poverty line	Matched	62.54	62.75	-1.0	96.4	-0.82	0.412
Percent of families with wage income	Unmatched	77.88	77.35	5.0		5.55	0.000
	Matched	77.88	77.88	0.0	99.0	-0.04	0.970
Percent of individuals with some college	Unmatched	26.96	26.17	11.3		11.86	0.000
	Matched	26.96	26.98	-0.2	97.8	-0.20	0.844
Percent of individuals with a college degree	Unmatched	16.36	14.18	19.1		20.88	0.000
	Matched	16.36	16.44	-0.7	96.3	-0.55	0.584
Percent of 16-19 year olds	Unmatched	74.53	74.34	1.0		1.11	0.269
enrolled in school	Matched	74.53	74.63	-0.5	49.9	-0.39	0.693

Percent of owner occupied households	Unmatched	51.09	49.58	6.2		6.74	0.000
	Matched	51.09	51.23	-0.6	90.5	-0.47	0.640
Percent of individuals who dropped out of high school	Unmatched	21.99	25.93	-29.2		-29.73	0.000
	Matched	21.99	21.89	0.7	97.6	0.60	0.546
Median income	Unmatched	41390.00	37952.00	23.8		25.11	0.000
	Matched	41390.00	41550.00	-1.1	95.4	-0.89	0.375
Median gross rent	Unmatched	504.05	496.47	6.3		6.89	0.000
	Matched	504.05	504.86	-0.7	89.2	-0.52	0.601
Median value of owner occupied households	Unmatched	89480.00	79202.00	20.3		21.98	0.000
	Matched	89480.00	89859.00	-0.7	96.3	-0.57	0.567
Percent of individuals who speak a language other than English	Unmatched	10.34	11.82	-12.4		-12.10	0.000
	Matched	10.34	10.33	0.1	99.5	0.06	0.953
Percent of Whites	Unmatched	69.81	57.41	35.3		35.71	0.000
	Matched	69.81	70.08	-0.7	97.9	-0.64	0.524
Percent of Blacks	Unmatched	17.81	29.02	-33.9		-33.74	0.000
	Matched	17.81	17.62	0.6	98.2	0.52	0.601
Percent of Hispanics	Unmatched	6.70	8.06	-10.9		-10.39	0.000
	Matched	6.70	6.68	0.2	98.4	0.17	0.864
Percent of households with two or more Non-Hispanics	Unmatched	1.51	1.50	0.8		0.89	0.376
	Matched	1.51	1.50	0.4	48.2	0.31	0.755