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Deven Carlson
Robert Haveman
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Barbara Wolfe

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The Effect of Housing Assistance on Student Achievement: Evidence from Wisconsin*

Deven Carlson[†], Robert Haveman[‡], Sohyun Kang[§], Hannah Miller[||], Alex Schmidt[¶] and Barbara Wolfe[‡^]

[†] Department of Political Science, University of Oklahoma

[‡] La Follette School of Public Affairs, University of Wisconsin–Madison; Department of Economics, University of Wisconsin–Madison; and Melbourne Institute: Applied Economic and Social Research, The University of Melbourne

[§] Department of Educational Policy Studies, University of Wisconsin–Madison

[||] Abt Associates

[¶] Department of Sociology, University of Wisconsin–Madison

[^] Department of Population Health Sciences, University of Wisconsin–Madison

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Melbourne Institute: Applied Economic & Social Research
The University of Melbourne
Victoria 3010 Australia
Telephone **+61 3 8344 2100**
Fax **+61 3 8344 2111**
Email **melb-inst@unimelb.edu.au**
WWW Address **melbourneinstitute.unimelb.edu.au**

Abstract

Drawing on a unique data set that links information on all Wisconsin households receiving means-tested benefits with the educational performance of all Wisconsin public school students in these households, we estimate the effect of a family's initial receipt of housing assistance on students' subsequent achievement outcomes. We estimate these effects using two different comparison groups. Our first comparison group consists of children living in households that receive housing assistance starting three years after our treatment group—we use observations from students' pre-receipt years as the basis for the comparison. Our second comparison group consists of low-income students whose families never received housing assistance, but did receive other forms of means-tested benefits, such as SNAP, TANF, or Medicaid. The results of our analyses suggest small positive effects of housing benefit receipt on student achievement. We discuss the implications for research and policy.

JEL classification: H53, O18

Keywords: Housing vouchers, public housing, student achievement, administrative data

I. INTRODUCTION

The link between a student's residential location and their assigned public school is tight. In a majority of school districts—particularly those outside of large urban areas—students are assigned to schools on the basis of their residential address. Moreover, the income level of the residential location tends to be related to public school quality. Hence, the main option available to parents seeking higher quality schools for their children is to move to a neighborhood with a higher average income level. Housing assistance, particularly housing vouchers, is a potential vehicle by which families with limited resources can access a better neighborhood and potentially better schools.

In this paper, we estimate the effect of a family's initial receipt of housing assistance on the achievement of children in the recipient families, comparing them to children in similar families who have not received housing assistance or who received housing assistance in later years. The basis of our analysis is a unique dataset containing information on a large sample of low-income families with school-aged children residing in the state of Wisconsin. We use these data to estimate the effects of housing assistance receipt using two different comparison groups. Our first comparison group consists of children living in households that receive housing assistance beginning three years after the treatment group initially received assistance—we use observations from students' pre-receipt years as the basis for the comparison. Our second comparison group consists of low-income students whose families never received housing assistance,[1] but did receive other forms of means-tested benefits, such as Supplemental Nutrition Assistance Program (SNAP) benefits, Temporary Assistance for Needy Families (TANF), or

[1] A very few students in this comparison group did receive assistance at a later date.

Medicaid; these observations have been matched to treatment group observations using observable student and family characteristics, as well as the school students attended in the year prior to subsidy receipt. Results of our analysis provide evidence that family receipt of housing assistance is tied to small increases in student academic achievement. This is important as housing subsidies are one policy tool that might reduce the achievement gap between children in low versus middle and higher incomes.

In the next section, we describe the characteristics of the housing assistance programs whose effects we analyze, followed by a description of the conceptual framework on which we rely. We then review the prior research on the relationship between housing assistance and children's educational outcomes. Next, we describe our unique data set and provide a more extensive discussion of our two comparison groups, followed by a description of the research methods that we employ. We then present the results of our study, and conclude.

II. FORMS OF HOUSING ASSISTANCE

Our analysis is designed to assess the effects of two forms of family-based housing assistance on the educational outcomes of children. The two programs that provide such support are the Housing Choice Voucher Program (often referred to as the Section 8 housing voucher program) and the public housing program.

A. The Housing Choice Voucher Program

The U.S. Department of Housing and Urban Development (HUD) provides housing assistance to low-income households through the Housing Choice Voucher Program. This program, which is operated by HUD in conjunction with over 3,000 local public housing authorities (PHAs), currently serves about 2.0 million families nationally, including more than one million families with minor children, and has a fiscal year budget of approximately $17.7 billion dollars (Center for Budget and Policy Priorities 2009; National Low Income Housing Coalition 2002). The primary objective of the program is to enable "very low-income families, the elderly, and the disabled to afford decent, safe, and sanitary housing in the private market."[2] A secondary objective of the program involves facilitating the relocation of recipients to better neighborhoods.

The process of securing a housing voucher begins with the submission of an application to a Public Housing Authority (PHA) at a time when the waiting list is open to new applicants; upon submission, applicants are assigned a position on the waiting list. When the applicant's name rises to the top of the waiting list, the household meets with housing authority staff who outline the rules and requirements of the Housing Choice Voucher Program and provide recipients with instructions for seeking housing in the private market that meets a minimum standard of health and safety. If a voucher recipient—whose income must, in general, be below 50 percent of the median income of the county or metropolitan area in which they live—is able to locate suitable housing, the recipient household generally contributes 30 percent of its income toward rent. The voucher program then subsidizes the difference between the tenant contribution and actual rent, up to a locally defined "fair market rent" payment standard.[3] Moreover, because a voucher recipient is required to contribute 30 percent of income toward rent and then the voucher subsidizes the difference between the tenant contribution and actual rent, program

[2]http://www.hud.gov/offices/pih/programs/hcv/about/fact_sheet.cfm#10.

[3]This standard is set by HUD at the 40th percentile of the local rental market, as calculated by the monetary value of leases commenced in the previous year.

benefits are effectively income-conditioned; the subsidy value of recipients' voucher falls as their income rises, and rises in the event of a reduction in income including due to, say, becoming unemployed.

B. The Federal Public Housing Program

The federal government public housing program is also administered by HUD through local PHAs. The program is designed to provide "decent and safe rental housing" for eligible low-income families, the elderly, and persons with disabilities. There are a wide variety of forms of public housing, from single-family houses to high-rise apartment buildings. About 1.2 million households live in public housing units, approximately 40 per cent of whom are families with children (Center for Budget and Policy Priorities 2009).

The local PHA determines a family's eligibility for access to a public housing unit. According to HUD, a PHA determines an applicant's eligibility based on: 1) annual gross income (adjusted for family size); 2) whether the applicant qualifies as elderly, a person with a disability, or as a family; and 3) U.S. citizenship or eligible immigration status.[4] If a family is eligible, they will either be offered assistance immediately or placed on a waiting list.
As in the case of housing vouchers, residents contribute 30 percent of their income to rent and face the same work (dis)incentives. And, as with vouchers, families frequently first apply and then are placed on a waiting list before they receive public housing. Once they are offered assistance, the family must sign a lease and may need to put down a small deposit.

[4] https://portal.hud.gov/hudportal/HUD?src=/topics/rental_assistance/phprog

III. CONCEPTUAL FRAMEWORK AND LITERATURE REVIEW

There are several channels through which family receipt of housing assistance could affect children's outcomes. Some of these channels are expected to positively affect outcomes, while others will likely have a negative effect. Ultimately, the expected effect is ambiguous. First, family receipt of housing assistance—especially vouchers—is likely to improve the quality of neighborhood circumstances and housing conditions for children. Carlson et al. (2012b) find that voucher receipt has little effect on neighborhood quality in the short-term, but positive long-term effects. In particular, they find that voucher recipients in the periods after receipt were living in neighborhoods with lower unemployment rates. Drawing from the findings of others, that paper also notes that opportunities for these improvements have been found to be greater under vouchers than public housing (see Newman and Schnare, 1997), although the nature of the public housing is relevant as well.[5]

This paper also reviewed the limited studies on the relationship of voucher receipt on neighborhood quality, and concluded that the existing literature suggests that Section 8 voucher

[5] This study identifies the effects of housing assistance on children's educational achievement employing the counterfactual of no housing assistance, thus enabling us to record the effect of a treatment that both shifts the budget constraint for housing for the treatment group (including security against an inability to pay rent), and changes the relative prices on the housing services purchased. The Welfare to Work (WtW) experiment (Mills et al., 2006) also adopted this counterfactual. In this study, welfare recipients in five large and midsize cities were randomly assigned to two groups; one group received a housing voucher and the other did not. Other studies have also attempted to estimate the effect of the receipt of housing assistance on children's educational attainments. However, these studies have adopted a counterfactual that differs from our study and those discussed above. These include the Gautreaux experimental study in Chicago (Kaufman & Rosenbaum, 1992; Rubinowitz & Rosenbaum, 2000).

receipt can result in households residing in higher-quality neighborhoods, at least as measured by the poverty rate of the census tract. However, estimates of the extent to which Section 8 recipients reside in higher quality neighborhoods are found to be inconsistent across three experimental studies; estimates of the difference in the average neighborhood poverty rates for the treatment and control groups appear to be larger for the studies using public housing recipients (Jacob 2004, who studied the replacement of demolished public housing by vouchers and the Moving to Opportunity program[6], U.S. Department of Housing and Urban Development 2011)[7] than for those that test the effect of voucher receipt on a more general population of recipients (Mills et al. 2006, who studied the Welfare to Work Program)[8].

[6] Jacob (2004) used the Chicago Housing Authority's scheduled demolition of over 6,400 units of public housing during the 1990s as a source of plausibly exogenous variation in voucher receipt. Residents whose units were demolished were offered a Section 8 voucher that could be used in the metropolitan area. The achievement scores of students in families whose units were demolished—and thus offered vouchers—were not significantly different from the achievement scores of students who remained in their public housing units. Jacob, Kapustin, and Ludwig (2014) used data from a 1997 Chicago randomized housing voucher lottery to examine the long-term impact of family receipt of housing assistance on a wide variety of child outcomes, including schooling, health, and criminal involvement. The families that they study lived in private, unsubsidized housing at the time that the intervention occurred. Using the randomized voucher offer as an instrumental variable (IV) for voucher use, the authors find that family receipt of housing assistance has little effect on the quality of the schools that youth attend, or on education (i.e., achievement test scores and high school graduation), criminal involvement, or health outcomes.

[7] The Moving to Opportunity (MTO) experiment randomly assigned public housing residents in five large cities to (1) a control group that remained in public housing, (2) a Section 8 group that could use their voucher anywhere, or (3) a Section 8 group that could use their voucher only in low-poverty neighborhoods. Numerous outcomes were studied, including the educational experiences and outcomes of recipient children. Results for households that were tracked for 10 to 15 years after random assignment indicated that youth in the two Section 8 groups attended schools with lower proportions of low-income and minority students (Sanbonmatsu et al., 2011; Kling et al. 2007; NBER 2009). Despite these differences in school context, there were no significant differences in the average achievement test scores across the three groups.

[8] In the Welfare to Work (WtW) study (Mills et al 2006), welfare recipients in five large and midsize cities were randomly assigned to two groups; one group received a housing voucher and the other did not. About five years after baseline, the evaluation of the effects of voucher receipt found no statistically significant effects on children's behavior problems, delinquency, or risky behaviors. Voucher receipt was found to reduce the likelihood of not attending school because of health, financial, or disciplinary problems for a few subgroups (e.g., girls and children under age six at baseline). However, voucher receipt appeared to increase the probability of repeating a grade and, for girls, the failure to complete high school. There were no apparent effects of voucher receipt on

Second, receipt of housing assistance, particularly a Section 8 voucher, often leads to a residential move that crosses school district boundaries, and suggests that these moves may reflect increases in the quality of the schools available because of the move. Carlson et al. (2013b) estimated that about 16 percent of families in their low-income sample moved across school districts each year in the years prior to voucher receipt. That number spiked to about 21 percent in the year of voucher receipt, as households are likely attempting to settle on what they hope to be a relatively long-term residence. In subsequent years, the number declines to about 12-15 percent. The authors then estimate the change in the quality of schools—measured by the average standardized reading and math scores—associated with an inter-district move in the year of voucher receipt. In both subjects, voucher receipt results in a significant increase in the quality of the schools in the district in which these households reside; in each subject the point estimate is statistically significant and in excess of one-third of a standard deviation. Ellen, Horn and Schwartz (2016) find evidence that among families granted a housing voucher, those with a child about to start schools are more likely to move to better school districts. See also Schwartz, Stiefel and Cordes (2017), who study the effect of moving schools on educational performance.

Third, receipt of housing assistance has effects similar to an increase in cash income. It enables the family to increase both the amount of housing services it purchases and the purchase of non-housing goods and services. Like an increase in cash income, receipt of a voucher would enable the recipient to "purchase" additional leisure; the income effect of voucher receipt encourages reduced work and earnings. Carlson et al. (2011) estimate that the monetary value of

children being assigned to special education classes or receiving school remedial services, or on the highest grade completed or enrollment in college.

a Section 8 voucher is about $4,300; however, when the value of other associated benefits is included, the full recipient value of a voucher ranges from about $7,000 to $9,000. Newman and Harkness (2002) find that housing assistance results in more stable housing in part because the subsidy makes it easier for the family to pay its rent. They report that in 1995, more than a third of very low-income households spent more than 50 percent of their income on rent (HUD, 1998). Families with housing assistance, on the other hand, spent roughly 30 percent of their income on rent, with government subsidies making up the balance. Duncan and Brooks-Gunn (1997) report that enhanced income is likely to be most effective during early childhood because these are the critical developmental years. To the extent that receipt of a housing subsidy relieves financial pressure on parents, it may reduce stress, depression, and other symptoms of psychological distress, with potentially beneficial effects on their children.[9]

Fourth, receipt of housing assistance is often related to change in residence, which provides the opportunity for parents to change household structure and make other decisions that could benefit children. Carlson et al. (2011) estimate that receipt of a voucher leads to an increased probability of change in household composition in the year of voucher receipt, but greater stability in household composition in subsequent years. Ellen, Horn and Schwartz (2016) find evidence of this pattern.

Fifth, the receipt of assistance is likely to affect housing stability and, hence, schooling experiences. Specifically, children may experience an initial disruption in neighborhood and schooling experiences; Newman and Harkness (2002) suggest that children who move residences

[9] Receipt of housing assistance also influences family receipt of public assistance. See also Mills et al. (2006), Jacob and Ludwig (2012) and Carlson et al. (2012a, 2012b).

are often also likely to change schools more frequently, putting them at greater risk of grade repetition and poor academic performance; see GAO (1994). Astone and McLanahan (1994), Haveman, Wolfe, and Spaulding (1991), and Jordan, Lara, and McPartland (1996) find that the number of residential moves adversely affects the likelihood of a child graduating from high school. However, this initial disruption may be counteracted by greater stability in subsequent years.

An additional consideration includes the incentives for reducing work and earnings that are built into the program. In terms of standard economic theory, voucher receipt increases the marginal tax rate on earnings of all program beneficiaries, hence increasing work disincentives. Response to such incentives could decrease labor supply, earnings and income, hence offsetting some of the gain in real income from voucher receipt and thereby reducing material resources available to the child.[10] Carlson et al. (2012a) study the employment and earnings effect of voucher receipt, and find that voucher receipt has little effect on employment, but a negative effect on earnings. The negative earnings effect is largest in the years immediately following initial receipt, and fades out over time.[11]

IV. DATA

Our estimates are based on a unique data set constructed from administrative records contained in several databases maintained by State of Wisconsin agencies. Central to our construction of this data set is the Multi-Sample Person File (MSPF), which is compiled and

[10] However, a reduction in work could lead to more time spent with children, which is likely to have a positive impact on their achievement.

[11] See also Mills et al. (2006) and Jacob and Ludwig (2012).

maintained by the Institute for Research on Poverty (IRP) at the University of Wisconsin–Madison. The MSPF contains an anonymous, individual-level identifier—an IRPID—for every person ever entered into any of seven databases maintained by Wisconsin state agencies.[12] The IRPID enables us to link individual records across each of the seven databases that compose the MSPF. In addition to individual identifiers, the MSPF contains a second set of identifiers that link children to their parents. Thus, the MSPF allows for the construction of a data set that contains detailed longitudinal information on both individual youth and their families. Currently, the MSPF contains anonymous individual-level identifiers for over six million individuals that were entered into at least one of the seven MSPF-related databases between 1988 and 2012. These six million individuals come from approximately two million unique families. It is from this database that we identify youth for inclusion in our data set.

The Client Assistance for Re-Employment and Economic Support (CARES) database is a major contributor to the MSPF, and serves as the basis for constructing our data set. CARES contains a wide variety of detailed information—including household composition, demographics, address history, and public program participation—on all individuals associated with any case that has applied for or received any form of public assistance from the state since the mid-1990s; CARES contains over 500,000 unique records annually. Most importantly for our analysis, CARES contains an annual indicator of whether households receive housing assistance. Specifically, at the time households apply for TANF or SNAP benefits caseworkers inquire whether the household has a housing voucher, lives in public housing, or receives no housing

[12] The seven databases are the Client Assistance for Re-employment and Economic Support (CARES), the Kids Information Data System (KIDS), the Unemployment Insurance (UI) System, the State Automated Child Welfare Information System (SACWIS), the Department of Corrections (DOC) records, the Milwaukee County Jail (MJ) records, and Court Record Data (CRD).

assistance. Continued receipt of TANF or SNAP benefits requires households to attend renewal appointments—typically at six-month intervals—and caseworkers inquire about households' receipt of housing assistance at each of these renewal appointments. The CARES data record the month of each renewal appointment and the status of housing assistance receipt at that visit. Thus, while our data contain indicators of housing assistance at the household-by-year level, we are able to gain insight into the timing of receipt of such assistance in any given year. The indicators of housing subsidy receipt are available beginning in 2000. However, because the educational records required for our analyses are available beginning only in the 2005-2006 school year, we extract annual CARES information on demographic characteristics, household composition, benefit receipt status, and geographic location from 2005 through 2012.

Upon completion of the CARES extraction, we added household earnings records drawn from the Unemployment Insurance (UI) system to our data set. The UI system is maintained by the State of Wisconsin, which operates a large-scale database that contains quarterly wage records for nearly all working individuals in the state, as well as records on UI benefit payments, dating back to calendar year 2000. We annualize the quarterly records from the UI system, aggregate the individual records to the household level, and then merge this annual, household earnings information to the individual-level records extracted from the CARES database using the IRPID that programmers at IRP have added to each database.

As the final step in constructing our data set, we add annual information on each child's educational outcomes and experiences from records maintained by the Wisconsin Department of Public Instruction (DPI), which we matched to IRPIDs. Hence, these matched data combine students' educational records with their records from the other MSPF-related databases—all matches were done on the basis of the IRPID. For this analysis, we appended annual information on students' academic achievement, and school of attendance from the 2005-2006 through the 2011-2012 school years to the records extracted from the CARES and UI databases. Specifically, with respect to achievement, we added annual reading and math achievement scores from the Wisconsin Knowledge and Concepts Examination (WKCE), a standardized exam administered annually to Wisconsin public school students in grades three through eight and grade ten in order to meet federal accountability requirements. We standardized the scale scores by year, subject, and grade using the statewide mean and standard deviation. In addition to this student-level information, we also appended several school-level characteristics, such as demographic composition and average achievement outcomes.

Considered as a whole, the data set contains a wide range of information—demographic characteristics, educational outcomes and experiences, means-tested benefit receipt (particularly housing assistance), and household earnings, among other information, for a large group of low-income children over a multi-year period.

V. SAMPLE: TREATMENT & COMPARISON GROUPS

Within this data set, we identify a group of students residing in households that received housing assistance during a specified time period—the treatment group—as well as two groups of students residing in households that received no housing subsidies during this period; these groups serve as counterfactuals against which the effects of housing assistance can be estimated.

Treatment Group

We identify a student for inclusion in the assistance group if he or she resides in a household that was a new recipient of housing assistance in the late spring to early fall of 2006, 2007, or 2008.[13] Given that our data from CARES and DPI databases begin in 2005, we identify students for inclusion in the assistance group beginning in 2006 to ensure that we have at least one pre-receipt observation for each student. In addition, we limit the assistance group to students whose households receive housing assistance during the late spring to early fall months to best align receipt of assistance with the timing of the school year. This procedure identifies 10,455 students for inclusion in the assistance group. After dropping students missing test score data—only students in grades 3-8 and 10 are tested each year—and other key variables, the analytic sample for the treatment group in the main models includes 4,066 students.

As noted above, we construct two groups against which the educational outcomes of students in the assistance group can be compared, allowing us to estimate the effect of receipt of housing assistance on student achievement and using two separate comparison groups.

The Future Recipient Comparison Group

The first comparison group includes students who resided in a household that received housing assistance in 2009, 2010, or 2011—but had no recorded receipt of housing assistance in prior years—and had available DPI records at any point between the 2005-2006 and 2011-2012 school years; we refer to this group as the "future recipient" group. Because students in this comparison group reside in households that ultimately receive housing assistance—many of the households may have been on waiting lists at the time that members of our assistance group received their subsidies—bias stemming from unobservable factors that drive households to voucher receipt is mitigated. Using the same SNAP and income restrictions described above, this approach resulted in the identification of 12,324 children for inclusion in this "future recipient"

[13] We define "late spring to early fall" as April, May, June, July, August, or September. As detailed above, our housing assistance dataset begins in calendar year 2000; thus, "new recipients" are those who have no record of housing voucher receipt between calendar year 2000 and April 2006.

comparison group. For each of these students we extracted all available observations beginning with the 2005 to 2006 school year and extending through the year before the case received their housing assistance. Of these students, 3,564 were included in the main models after accounting for missing variables, primarily test scores.

The Broader Comparison Group

The criteria for inclusion in the second comparison group are broader than those for inclusion in the "future recipient" comparison group. This comparison group consists of low-income students whose families never received housing assistance, but did receive other forms of means-tested benefits, such as Supplemental Nutrition Assistance Program (SNAP) benefits, Temporary Assistance for Needy Families (TANF), or Medicaid. In particular, eligibility for inclusion in our second comparison group requires only that a student resided in a household that was active in the CARES database in 2006, 2007, or 2008; had a household income below 200 percent of the federal poverty level in 2005, 2006, or 2007 (i.e., the years prior to treatment); could be linked with the DPI records in 2006, 2007, or 2008; and did not receive a housing subsidy in the year for which the case serves as a comparison.[14]

This group allows for a comparison of the outcomes of recipient children to a broader swath of low- to moderate-income, school-aged children in Wisconsin.[15] We extracted all available observations from 2005-2006 through 2011-2012 that meet this criteria, yielding 859,352 students. These observations were then matched using propensity scores to treatment

[14] Additionally, the student had to be associated with a case that had a "primary person" assigned to the case—only a very small number of cases did not have a primary person assigned, so this condition excludes very few students.

[15] One could imagine that families on a waiting list might make different choices (e.g., whether or not to move or to take another job) than those not on a waiting list and that such choices could influence a child's school performance. The broader group avoids this possible cause of bias in comparison of outcomes.

group observations using personal and school characteristics in the year prior to housing assistance receipt (see the methodological appendix for a description of the propensity score matching procedure). After matching, there were 4,416 broader comparison group students in the main models.

Taken together, these groups—the voucher group and the two comparison groups—allow for two unique comparisons. First, analysis of the treatment and future recipient groups permits a comparison of the educational outcomes of voucher recipients to the outcomes of future voucher recipients in the years before the latter group receive a voucher. Second, analysis of the voucher group and the broader comparison group allows us to compare the educational outcomes of the voucher group to the outcomes of other low- to moderate-income children across the state who do not receive housing assistance.

Descriptive Statistics for Treatment and Comparison Groups

Table 1 presents the descriptive statistics for school-aged children in the assistance group and in the two comparison groups one year prior to the focal year (i.e., the year when the treatment group first received housing assistance)[16]. The four columns provide the descriptive statistics for the assistance (i.e., treatment) group, the future recipient group, the weighted treatment group (based on the propensity score matching procedure for creating the broader comparison group, as described in the methodological appendix), and the weighted broader comparison group (again, based on the previously propensity score matching procedure). Note that these statistics include all students found in the main achievement models who are not

[16] A focal year is defined as the year members of the treatment group received housing assistance; for future recipients it is three years prior to the year they received a Section 8 voucher or relocated into public housing (i.e., 2006, 2007, or 2008). Hence, for households that received a voucher in 2009, the focal year is 2006; for 2010 recipients, the focal year is 2007; and for households that received a voucher/relocated in 2011, the focal year is 2008. One-third of cases in the broader comparison group have been assigned a focal year of 2006, another third have been assigned a focal year of 2007, and the final third have been assigned a focal year of 2008.

missing data in the year prior to the focal year (see Table 3). Slightly more than 70 percent of the children in both the assistance group and in the "future recipient" comparison group received a housing voucher; just under 30 percent lived in public housing.

In terms of test scores,[17] and a number of other variables, the values for those in the treatment and the future recipient group are similar. This is not surprising as children in both groups lived in families that received a housing subsidy. For some variables, the future recipients group appears somewhat better off than the treatment/assistance group. The future recipients are less likely to have ever been English language learners and more likely to have married parents. They also tend to have higher incomes, lower food stamp receipts, higher TANF and childcare receipts, and higher levels of unemployment benefits. Finally, they tend to have slightly fewer children and slightly more adults in the household. With the exception of number of children, childcare subsidies, unemployment benefits and minor differences in the likelihood of ever receiving free or reduced price lunch, the broader (matched) comparison group is relatively similar to the treatment group.

Table 2 presents the change in the mean math and reading scores for the treatment group and each of the two comparison groups for the two years preceding and up to two years following the focal year. In the table, each cell represents the difference between the mean score in that year (say the year immediately following the focal year) and the mean score in the focal year. Thus, a positive number indicates the average score in that year was higher than the focal year, and a negative number indicates the average score in that year was lower than the focal

[17] The test score patterns across outcome variables, time periods, and sex and race groups are heterogeneous, and can be observed in the table and figures. The scores reported in the table are only for the focal year (see footnote 17). In the actual estimates, scores over all relevant years are included which increases the sample size.

year. For math test scores for the treatment group (unmatched/unweighted), the scores in the years prior to the focal year are negative relative to those of the focal year; the scores in the years after the focal year are positive. It is difficult to discern other patterns in these descriptive data.

VI. EMPIRICAL APPROACH

In this study, we employ the counterfactual of no housing assistance, thus enabling us to estimate the effect of a treatment that both shifts the budget constraint for housing for the treatment group (including security against an inability to pay rent), and changes the relative prices on the housing services purchased. With this counterfactual, the budget constraint of families that receive a housing subsidy differs from those that do not but is the same across types of subsidy. This differs from several prior studies, which compare only the effect of one form of housing assistance (typically vouchers) relative to a second form of housing assistance (usually public housing).[18] As compared to prior studies, which analyzed effects from selected large urban areas, the households in our sample reside across all parts of the State of Wisconsin.[19]

Empirical Model

Using the treatment and two comparison group samples to conduct the analysis described above, we estimate the relationship between housing subsidy receipt and educational

[18] The counterfactual in the Jacob (2004) and MTO (U.S. Department of Housing and Urban Development, 1998) studies is residence in traditional public housing.

[19] According to data on the HUD Web site "A Picture of Subsidized Housing," the demographic profile of Section 8 subsidized households in Wisconsin is similar to that for the United States as a whole with the only major difference being a lower proportion of Hispanics among recipients in Wisconsin (5 percent vs. 17 percent). In terms of other characteristics comparing Wisconsin to the United States: 12 percent vs. 15 percent are 2 adults; 40 percent vs. 36 percent are single adults; 49 percent vs. 48 percent are female heads with dependent children; 20 percent vs. 18 percent are disabled; 59 percent vs. 56 percent are between ages 25 and 50; 18 percent are age 62 plus (both); 38 percent vs. 42 percent are Black; and 1 percent are Native American (both).

achievement. This approach takes advantage of the fact that we have information on students' academic outcomes both pre- and post-receipt.[20]

We estimate the following model:

$$Y_{it} = R_t\beta + (R_{it} \times V_i) + C_{it}\theta + \mu_i + X_{it}\phi + \varepsilon_{it} \quad (1)$$

where Y represents the outcome variable of interest (math or reading achievement) for student i at time relative to focal year t; R is a vector of dummy variables indicating the year relative to the focal year; V is an indicator for being in the voucher group; C is a vector of calendar year indicators; μ_i is a student fixed effect; X is a vector of observed, time-varying student characteristics;[21] and ε_{it} is the error term. The parameter of interest in this model is δ, which represents the association between voucher receipt and educational outcomes and is allowed to vary across years.

We estimate this model twice—the two estimations correspond to the two comparisons described above. We first estimate the model over a sample containing observations from students in the voucher group (the treatment group) and the "future recipient" comparison group. The second estimation is over a sample that contains students in the voucher group (the treatment group) and the second, broader comparison group, weighting each observation by the matching weight (estimated using the propensity score matching method discussed in the methodological

[20] As described earlier, a key concept in our estimation strategy is what we term the "focal year." For students in the voucher group, we define the focal year as the year of subsidy receipt. For students in the future recipient group, we define the focal year as three years prior to the later subsidy receipt. For example, for students in the future recipients group whose household receives a voucher in 2009, the focal year is 2006. For students in the second comparison group (the broader comparison group), the focal year is the year to which we randomly assign their data. Across all three groups, the focal year is either academic year 2006-07, 2007-08, or 2008-09.

[21] These characteristics are students' grade in school, status as an English language learner, and eligibility for free or reduced-price lunch.

appendix). In terms of time span, both sets include observations for the focal year, up to two years prior to that focal year, and up to two years after the focal year.[22]

VII. ESTIMATES OF THE RELATIONSHIP BETWEEN FAMILY RECEIPT OF HOUSING ASSISTANCE & YOUTH EDUCATIONAL ACHIEVEMENT

In this section, we present the results of our empirical models estimating the association between family receipt of housing assistance and achievement test scores of youth from the family.

Overall Estimates

In Table 3, we present estimates of the effects of family receipt of housing assistance—either acceptance of a public housing offer or receipt of a housing voucher—on overall youth math and reading test scores; our estimates are from the model described above. Estimates based on the future recipient comparison group indicate that in the two years after the receipt of assistance, math test scores for the treatment group were significantly higher than those for the youths who received housing assistance in the future, and indicate gains of around 0.10 standard deviations in both years. The estimated effect on reading test scores is also positive in both years using the future recipients group, but significant in only the first year after treatment; in that year a gain of 0.10 standard deviations is indicated. Results based on the second, broader comparison group suggest that the receipt of housing assistance generally has positive, but not statistically significant effects, on math and reading scores in the years following treatment receipt.

[22] We are unable to include additional post-focal years because doing so would overlap with the years in which members of the comparison group received their housing vouchers.

Sex-Specific Estimates

Table 4 shows model estimates of the effect of family receipt of housing assistance on youth sex-specific math and reading test scores. Using the future recipient comparison group, our estimates provide evidence of a positive and statistically significant impact of the receipt of housing assistance in the two years after treatment. For both years, the gains in the math test scores for males are 0.16 and 0.21 standard deviations, and are statistically significant at the 95 percent level.[23] The math effects for females in the two years following treatment trend positive. Additional models that interact the treatment-year interaction with gender (not shown here) show moderate to strong evidence of gender differences in these math effects ($p<0.05$ in the focal year, $p<0.05$ in year one, $p<0.01$ in year two). The male reading test score effects in these two years are positive but only marginally statistically significant in the first year after treatment. For females, the future recipient group models suggest a positive impact of the treatment on reading in both of the years after treatment; however, an interacted model finds no evidence of gender differences in the reading effect ($p>0.10$ for all post-treatment years).

Using the broader comparison group, the results indicate a generally positive effect of family receipt of housing assistance on reading in the two years after the treatment; however, none of the estimated post-treatment-year effects is statistically significant. For math, the patterns are mixed, showing decreases followed by gains for males and gains followed by decrease for females. Again, none of these effects are statistically significant.

[23] We also find a positive, significant effect of 0.10 standard deviations in the year of treatment.

Race-Specific Estimates

Table 5 presents our estimates of the effect of receiving housing assistance on race-specific math and reading test scores. Focusing on results using the future recipient comparison group, the math test score gains in the two years after the focal year are both positive and significant (0.19 standard deviations) for Blacks; the effects for Whites and Hispanics in these years are positive overall, but not statistically significant. A model that allows the treatment-year interaction to vary by race (not shown here) finds strong evidence of differences between 1) Black students and White students in the year-one math effects ($p<0.01$ for Whites and $p>0.10$ for Hispanics), and moderate to weak evidence of racial differences in the year-two math effects ($p<0.05$ for Whites and $p<0.10$ for Hispanics). In reading, the estimated effect of housing assistance is both positive and statistically significant for Blacks in the year following housing receipt (0. 11 standard deviations); for Whites and Hispanics, the estimates are positive in direction but are not statistically significant. However, an interacted model finds no evidence of differences in reading effects by race ($p>0.10$ for Whites and Hispanics in all post-treatment years). In the broader comparison group models, the patterns are mixed and we again find no statistically significant effects. Overall, our results strongly suggest that Black students gain more than White students in terms of math achievement test scores when their families receive housing assistance.

Grade-Specific Estimates

Does students' age at the time of housing assistance receipt matter in terms of achievement? In Table 6 we attempt to shed light on this question by analyzing the effect of housing subsidy receipt on student achievement by school grade. For achievement, we distinguish effects for youth who are in the fourth or fifth grade at the time of subsidy receipt (student approximately ages 9 and 10) from those for youth in the sixth through eighth grades at the time of subsidy receipt (student ages 11 through 14).[24]

Examining the magnitudes of the coefficients, housing subsidy receipt appears to have small but likely positive effects on both math and reading test scores regardless of the grade in which housing was received.[25] Again, the statistically significant effects are only found using the future recipients comparison group. For fourth- and fifth-grade recipients, the future recipient model results suggest a 0.14 standard deviation increase in math scores in the focal year, and a 0.11 standard deviation increase in the two years after treatment. The math effects for those who received assistance between sixth and eighth grade are close to zero in the focal year and positive (but slightly smaller and not statistically significant) in the following two years. A model allowing the treatment-year interaction to vary by grade of receipt provides strong evidence of a grade difference in the math effect for the focal year ($p<0.01$), but no evidence of differences for later years ($p>0.10$ for both years after treatment).[26] The reading effect in the year after housing receipt for students who received housing assistance in the sixth through eighth grades is around 0.11 standard deviations. The reading effect for students who received assistance in the fourth or fifth grade is positive, but half as large and statistically insignificant. However, an interacted model finds no evidence of grade differences for the reading effects ($p>0.10$ for all post-treatment years).

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[24] Students in kindergarten through third grade at the time of subsidy receipt are excluded because they have no test scores available prior to receipt, while students in ninth grade or higher are excluded because they have few, if any, test scores available post-receipt. The lack of test scores prior to third grade is also why we chose not to estimate effects three years prior to treatment for students who received housing in grades four or five.

[25] The clear exception to this pattern is the model of math scores for the sixth through eighth grade recipients using the broader comparison group.

[26] The interacted models for grade of receipt do not include observations three years prior to treatment. See footnote 24.

Subsidy Type-Specific Estimates

All our estimates to this point have treated the new receipt of two forms of housing assistance as representing the same treatment. A policy-relevant question is whether the effects of public housing receipt differ from those that accrue in response to receipt of a Section 8 housing voucher. In Table 7, we break out the effects of these two policies on reading and math test scores.

The future recipients models suggest positive effects on math and reading scores for rental subsidy recipients in the two years following receipt. For math, students experienced statistically significant gains of 0.087 and 0.11 standard deviations in math in the two years following receipt, and statistically significant gains of 0.086 standard deviations in reading in the year following receipt. The math and reading effect sizes for public housing recipients in the future recipients models are similar. While the effects for public housing recipients are not statistically significant, interacted models (not shown here) find no evidence of differences in effects by subsidy type ($p>0.10$ for math and reading in all post-treatment years). The results of the broader comparison group models for rental subsidy recipients are consistent in direction with the future recipient models, but the public housing models are not. Again, none of the effects in the broader comparison group models are statistically significant.

VIII. ASSESSING CAUSALTIY

Our models compare the change in outcomes of potentially similar groups (i.e., children in households who receive housing subsidies, children in families who receive housing at a future date, and children in other low-income families) before and after housing assistance, and as such resemble a difference-in-differences design. Under a difference-in-differences framework, estimates support a causal interpretation if the groups exhibit similar (i.e., parallel) trends in the time leading up to treatment. Studies using difference-in-differences frameworks often must rely on the natural similarities between groups to assume equivalence of pre-treatment trends. However, our study allows us to explicitly test the equivalence of achievement trends for (in most cases) three years prior to treatment because of our rich longitudinal data set.[27] Following the logic of difference-in-differences models, if trends appear to be parallel for three years prior to housing receipt, it is reasonable to assume that the groups exhibit similar pre-treatment trends and to consequently treat the model estimates as causal. We assess this assumption using our model results, below.

In Figures 1 and 2, we present the estimates and 95 percent confidence intervals of the time-varying association between housing receipt and math/reading scores (i.e., the Year-Treatment interactions in the models). In order to support a causal interpretation, the coefficient estimates for years negative two and negative three (two and three years prior to treatment) should not be different from zero. These coefficients measure the group differences in time trends between one year prior to treatment and two/three years prior to treatment. This condition is not met for many of the models using the broader comparison group, which can be seen by the numerous confidence intervals that do not cross zero. The condition seems better satisfied by the future recipient group models, of which only two have pre-treatment confidence intervals that do not cross zero (math achievement for males and math achievement for public housing recipients). On the other hand, there is clearly variation in pre-treatment trends above and below zero for the future recipient models, and the confidence intervals prior to treatment tend to be larger. Thus, while the statistical results provide evidence that some models may yield reasonable causal

[27] We can only test for trends two years prior to treatment in the fourth and fifth grade future recipients models because standardized testing does not begin until third grade.

estimates of housing effects on student achievement, the large variation in pre-treatment trends gives reason for skepticism.

IX. CONCLUSION

In this paper, we explore the effect of housing assistance on children's math and reading achievement scores. In particular, we compare the school performance of children who live in households that first received a housing subsidy in a particular period (the focal year) to the achievement of students in two different comparison groups. The first comparison group, which we refer to as the future recipient group, includes children living in households who received housing subsidies several years in the future; the second, broader comparison group, contains children in families that received a means-tested benefit prior to the focal year and whose family income was below 200 percent of the federal poverty line.

Our empirical analysis rests on the uniquely rich data set we have been able to secure and construct. This data set merges data from the MSPF administrative data set constructed at IRP with that from several other data sets, including school-based data from the Wisconsin DPI. Our data set contains a wide range of information including family demographic characteristics, children's educational outcomes and experiences, means-tested benefit receipt, and household income by source for a large group of low-income children over a multi-year period.

Our findings provide suggestive evidence that children whose households received housing assistance perform somewhat better academically as captured by scores on standardized math and reading achievement tests. The results are statistically stronger when the comparison is to the more closely matched future recipients group than to the broader comparison group. The relationship between housing receipt and student achievement also varies between important characteristics. We find some evidence that the association is larger for boys and Black students in math.[28] Grade of housing receipt may also be influential, with our models suggesting that initial math gains are greater when housing assistance is received in elementary school. Finally, our findings provide no evidence of differences in the academic effects of rental subsidies versus public housing.

As with all observational studies, causality is an essential consideration for interpreting these results. Above, we discuss the assumption that the treatment and comparison groups follow similar pre-treatment trends required to isolate the causal effect of treatment. We also show that this assumption is statistically satisfied (i.e., cannot be rejected) for our future recipient group models with two exceptions: 1) math models for males and 2) math models for public housing recipients.[29] However, we also make it clear that there are large fluctuations in pre-treatment trends for many of the models. As such, we leave it to the reader to determine how much weight to place on a causal interpretation of our results.

Overall, our core findings provide evidence of a positive effect of receipt of a means-tested housing subsidy on children's performance on standardized tests. These results, whether or not causal, add to a growing literature showing positive effects of housing subsidies that continues to justify support for this policy.[30]

[28] However, as described above, assumptions necessary for causality are not met for the future recipients models of male math scores, so the male math effects should be interpreted carefully. (See the next paragraph.)

[29] Thus, the positive association between housing assistance and math achievement for males should not be treated as causal.

[30]See Carlson et al. (2011, 2012a, 2012b), Kling et al. (2007), and NBER (2009).

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Tables

Table 1: Descriptive statistics.

Dependent Variables	Treatment	Future	Treatment (propensity weights)	Broader (propensity weights)
Math score	-0.68	-0.64	-0.67	-0.68
Reading score	-0.63	-0.6	-0.62	-0.64
Public housing	0.25	0.29**	0.26	
Rental subsidy	0.74	0.71*	0.74	
White	0.43	0.45	0.44	0.44
Black	0.4	0.4	0.4	0.4
Hispanic	0.08	0.08	0.08	0.07
Asian	0.05	0.04**	0.05	0.04
American Indian	0.04	0.04	0.04	0.04
Male	0.48	0.5	0.48	0.48
Female	0.52	0.5	0.52	0.52
Not ever FRL eligible	0.03	0.04	0.03	0.04***
Ever FRL eligible	0.97	0.96	0.97	0.96***
Not ever ELL	0.92	0.94**	0.93	0.94
Ever ELL	0.08	0.06**	0.07	0.06
Primary Person's Marriage-Status				
Married	0.16	0.21***	0.17	0.17
Single, never married	0.43	0.42	0.46	0.44
Divorced, annulled, or separated	0.35	0.33	0.36	0.37
Widowed	0.01	0.02	0.01	0.01
Missing	0.04	0.02***		
Primary Person's Education				
Less than high school diploma	0.24	0.24	0.24	0.25
High school diploma or GED	0.68	0.68	0.72	0.71
Associate degree	0.02	0.03	0.03	0.03
Bachelor's degree or higher	0.01	0.01	0.01	0.01
Missing	0.05	0.04*		
Num. of children in household	3.09	2.96**	3.04	3.11**
Num. of adults in household	1.7	1.88***	1.73	1.74
Num. of children and adults	4.8	4.84	4.77	4.85*
Income, wages & benefits ($)				
Household income	18518.19	23977.03***	18841.19	18991.42
Food stamps	2925.68	2517.81***	2954.42	2976.08
TANF	282.43	401.93**	282.33	302.94
Childcare subsidy	5125.62	7738.75***	5132.25	6948.47***
Wages	14606.99	19651.17***	14913.28	15105.34
Unemployment benefits	1735.74	2243.36***	1740.28	2039.64***
N	1658	1477	1509	4416

Notes: We show rates of public housing and rental subsidy receipt for the "future recipients" comparison group in the year in which they received these benefits, rather than in the focal year. We show students' modal sex and modal race/ethnicity, rather than their sex and race/ethnicity as reported by DPI in the focal year, because a substantial portion of the sample was not observed in the DPI data in the focal year (mostly because they were too young to be in school yet). We show variables for whether students were ever FRL-eligible or ever an English language learner for this reason as well. A household's "primary person" is a designation used in CARES to denote the head of a case or the person who is responsible for receiving benefits. We include wages, TANF, UI benefits, Social Security, Supplemental Security Income, and Social Security Disability Insurance in calculating household income. In 2006, UI benefits data are only available for Oct., Nov., and Dec., which means 3 fewer months of data were available in calculating annual benefits for individuals in focal year 2006. ***p<0.01, **p<0.05, *p<0.10

Table 2: Changes in average math and reading achievement scores.

	Math				Reading			
Years	Treat.	Future	Treat.*	Broad.*	Treat.	Future	Treat.*	Broad.*
3 prior	-0.02	0.078	0.026	0.018	0.052	0.017	0.011	0.001
2 prior	-0.039	-0.028	-0.025	-0.007	0.003	0	-0.008	0.014
1 prior	-0.007	0.028	-0.027	-0.062	0.019	0.006	-0.037	-0.038
Focal year	0	0	0	0	0	0	0	0
1 post	0.03	-0.028	0.062	-0.007	0.035	-0.039	0.043	0.018
2 post	0.037	-0.008	-0.014	-0.011	0.035	-0.008	-0.035	0.005

Notes: Scores are centered on the focal/treatment year. *Scores weighted using the math/reading propensity score weights.

Table 3: Treatment effects on math and reading scores.

	Math		Reading	
Variables	Future	Broader	Future	Broader
Treat.*3 Years Prior	-0.023	-0.069	0.047	-0.028
	(0.053)	(0.050)	(0.053)	(0.048)
Treat.*2 Years Prior	0.057	-0.022	-0.0094	-0.021
	(0.035)	(0.029)	(0.038)	(0.033)
Treat.*Focal Year	0.045*	0.0084	0.0097	0.029
	(0.026)	(0.024)	(0.029)	(0.025)
Treat.*1 Year Post	0.095***	0.027	0.096***	0.021
	(0.028)	(0.026)	(0.030)	(0.027)
Treat.*2 Years Post	0.11***	0.011	0.057*	-0.0040
	(0.030)	(0.030)	(0.032)	(0.030)
N	7619	5925	7576	5925

Notes: *** p<0.01, ** p<0.05, * p<0.1; All models control for year, grade level, FRL status, and ELL status.

Table 4: Treatment effects on math and reading scores by sex.

	Math				Reading			
	Males		Females		Males		Females	
Variables	Future	Broader	Future	Broader	Future	Broader	Future	Broader
Treat.*3 Years Prior	-0.030	-0.049	-0.016	-0.054	-0.0020	-0.035	0.082	-0.13**
	(0.080)	(0.075)	(0.070)	(0.070)	(0.084)	(0.071)	(0.068)	(0.061)
Treat.*2 Years Prior	0.11**	0.026	-0.0023	-0.022	-0.053	-0.0082	0.030	0.015
	(0.054)	(0.045)	(0.045)	(0.037)	(0.061)	(0.053)	(0.046)	(0.038)
Treat.*Focal Year	0.10***	-0.022	-0.013	0.018	0.011	-0.0059	0.0052	0.048
	(0.038)	(0.037)	(0.035)	(0.031)	(0.045)	(0.040)	(0.038)	(0.032)
Treat.*1 Year Post	0.16***	-0.024	0.030	0.050	0.084*	0.060	0.11***	0.058
	(0.042)	(0.038)	(0.038)	(0.036)	(0.045)	(0.041)	(0.041)	(0.036)
Treat.*2 Years Post	0.21***	0.014	0.0040	-0.011	0.052	0.039	0.061	0.0084
	(0.045)	(0.046)	(0.040)	(0.039)	(0.049)	(0.047)	(0.042)	(0.041)
N	3745	2873	3874	3069	3724	2873	3852	3069

Notes: *** p<0.01, ** p<0.05, * p<0.1
All models control for year, grade level, FRL status, and ELL status.

Table 5: Treatment effects on math and reading scores by race.

	Math						Reading					
	White		Black		Hispanic		White		Black		Hispanic	
Variables	Future	Broader	Future	Broader	Future	Broader	Future	Broader	Future	Broader	Future	Broader
Treat.*3 Years Prior	0.029	0.059	-0.089	-0.17**	0.047	-0.26	0.048	0.056	0.036	-0.15*	0.19	-0.084
	(0.064)	(0.064)	(0.095)	(0.087)	(0.22)	(0.22)	(0.074)	(0.074)	(0.094)	(0.079)	(0.22)	(0.14)
Treat.*2 Years Prior	0.023	0.036	0.10*	-0.0085	0.056	-0.0033	0.029	0.046	-0.047	-0.050	0.014	-0.043
	(0.046)	(0.038)	(0.061)	(0.052)	(0.12)	(0.084)	(0.049)	(0.043)	(0.067)	(0.057)	(0.14)	(0.13)
Treat.*Focal Year	0.0056	-0.015	0.086*	0.044	0.10	-0.012	0.021	-0.023	-0.00012	0.065	-0.0094	-0.038
	(0.036)	(0.033)	(0.045)	(0.041)	(0.088)	(0.082)	(0.041)	(0.036)	(0.051)	(0.045)	(0.100)	(0.074)
Treat.*1 Year Post	-0.0016	0.0091	0.19***	0.063	0.12	-0.071	0.079*	0.0076	0.11**	0.011	0.16	0.11
	(0.038)	(0.035)	(0.051)	(0.047)	(0.096)	(0.085)	(0.041)	(0.037)	(0.053)	(0.048)	(0.10)	(0.091)
Treat.*2 Years Post	0.019	-0.031	0.19***	0.086*	0.0057	0.013	0.077*	-0.0043	0.042	-0.016	0.017	0.048
	(0.042)	(0.041)	(0.052)	(0.052)	(0.099)	(0.092)	(0.043)	(0.043)	(0.056)	(0.052)	(0.11)	(0.097)
N	3187	2636	3026	2319	660	454	3182	2636	3025	2319	637	454

Notes: *** p<0.01, ** p<0.05, * p<0.1
All models control for year, grade level, FRL status, and ELL status.

Table 6: Treatment effects on math and reading scores by grade of receipt.

	Math				Reading			
	4th-5th Grade		6th-8th Grade		4th-5th Grade		6th-8th Grade	
Variables	Future	Broader	Future	Broader	Future	Broader	Future	Broader
Treat.*3 Years Prior			-0.0037	-0.085			0.037	-0.080
			(0.073)	(0.065)			(0.071)	(0.060)
Treat.*2 Years Prior	-0.062	-0.11*	0.040	-0.014	-0.14	-0.046	0.038	0.026
	(0.085)	(0.061)	(0.043)	(0.036)	(0.093)	(0.078)	(0.049)	(0.040)
Treat.*Focal Year	0.14***	0.018	-0.013	-0.035	0.011	0.023	0.024	0.025
	(0.046)	(0.040)	(0.033)	(0.029)	(0.052)	(0.040)	(0.039)	(0.033)
Treat.*1 Year Post	0.11**	0.024	0.079*	-0.0052	0.050	0.051	0.11**	0.035
	(0.049)	(0.045)	(0.041)	(0.036)	(0.054)	(0.044)	(0.047)	(0.040)
Treat.*2 Years Post	0.11**	0.019	0.068	-0.029	0.079	0.062	0.066	-0.0035
	(0.049)	(0.044)	(0.048)	(0.041)	(0.054)	(0.044)	(0.048)	(0.043)
N	1382	1702	2077	2699	1377	1702	2064	2699

Notes: *** p<0.01, ** p<0.05, * p<0.1
All models control for year, grade level, FRL status, and ELL status.

Table 7: Treatment effects on math and reading scores by type of housing assistance.

	Math				Reading			
	Rental Subsidy		Public Housing		Rental Subsidy		Public Housing	
Variables	Future	Broader	Future	Broader	Future	Broader	Future	Broader
Treat.*3 Years Prior	0.040	-0.042	-0.20**	-0.074	0.11*	-0.060	-0.11	-0.068
	(0.063)	(0.063)	(0.098)	(0.077)	(0.063)	(0.055)	(0.10)	(0.092)
Treat.*2 Years Prior	0.058	0.025	0.035	-0.087*	0.010	0.0079	-0.066	-0.049
	(0.040)	(0.035)	(0.067)	(0.052)	(0.044)	(0.038)	(0.076)	(0.065)
Treat.*Focal Year	0.063**	0.054**	-0.011	-0.015	0.018	0.030	-0.017	-0.0032
	(0.031)	(0.027)	(0.050)	(0.047)	(0.034)	(0.028)	(0.060)	(0.056)
Treat.*1 Year Post	0.087***	0.040	0.098*	-0.046	0.086**	0.051*	0.12*	-0.018
	(0.033)	(0.031)	(0.054)	(0.049)	(0.035)	(0.030)	(0.063)	(0.059)
Treat.*2 Years Post	0.11***	0.046	0.083	0.034	0.068*	0.012	0.035	-0.0080
	(0.035)	(0.035)	(0.058)	(0.053)	(0.037)	(0.036)	(0.064)	(0.060)
N	5507	4410	2112	1549	5482	4410	2094	1549

Notes: *** p<0.01, ** p<0.05, * p<0.1
All models control for year, grade level, FRL status, and ELL status.

Figures

Figure 1: Assessing parallel trends for math models.

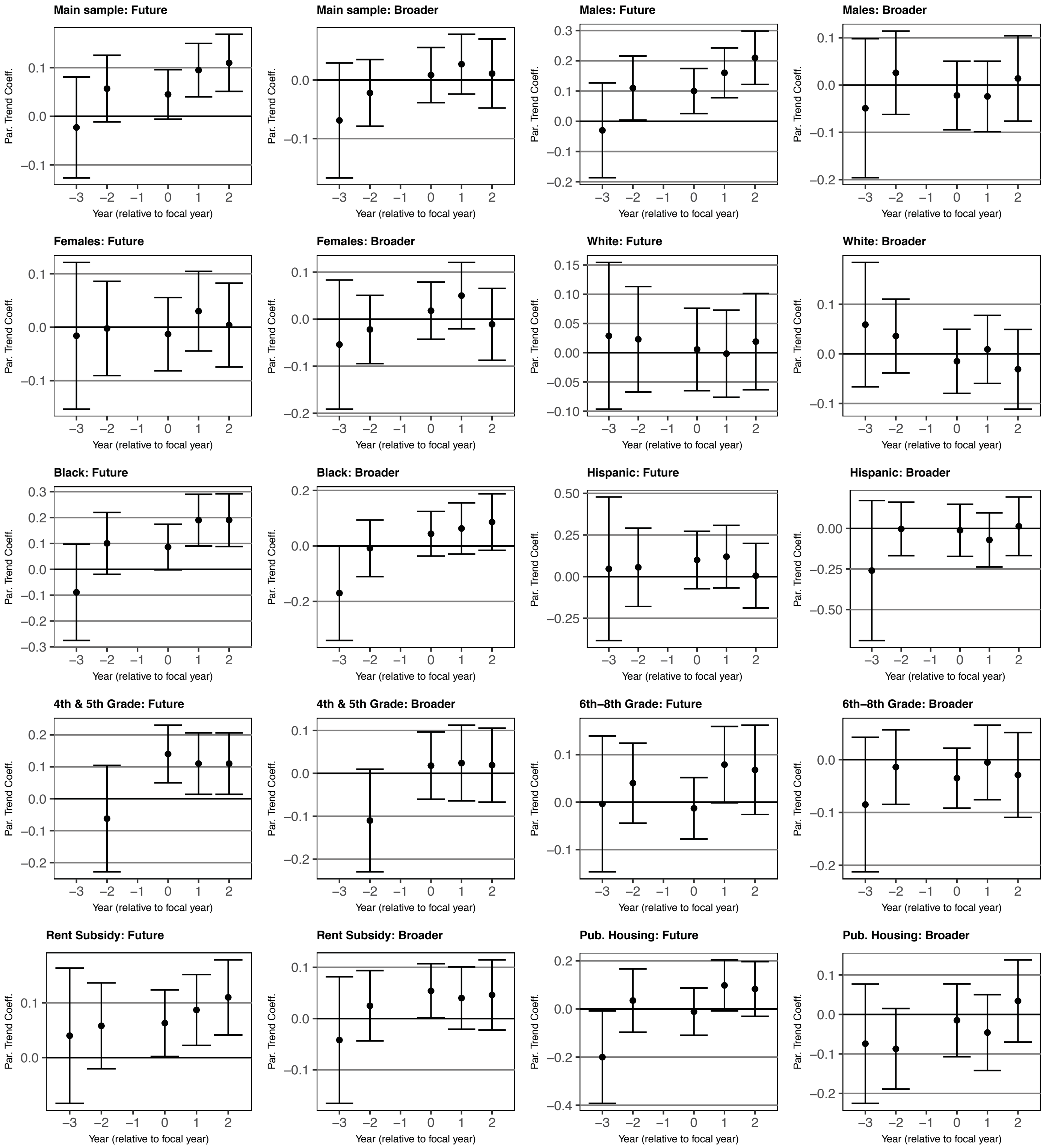
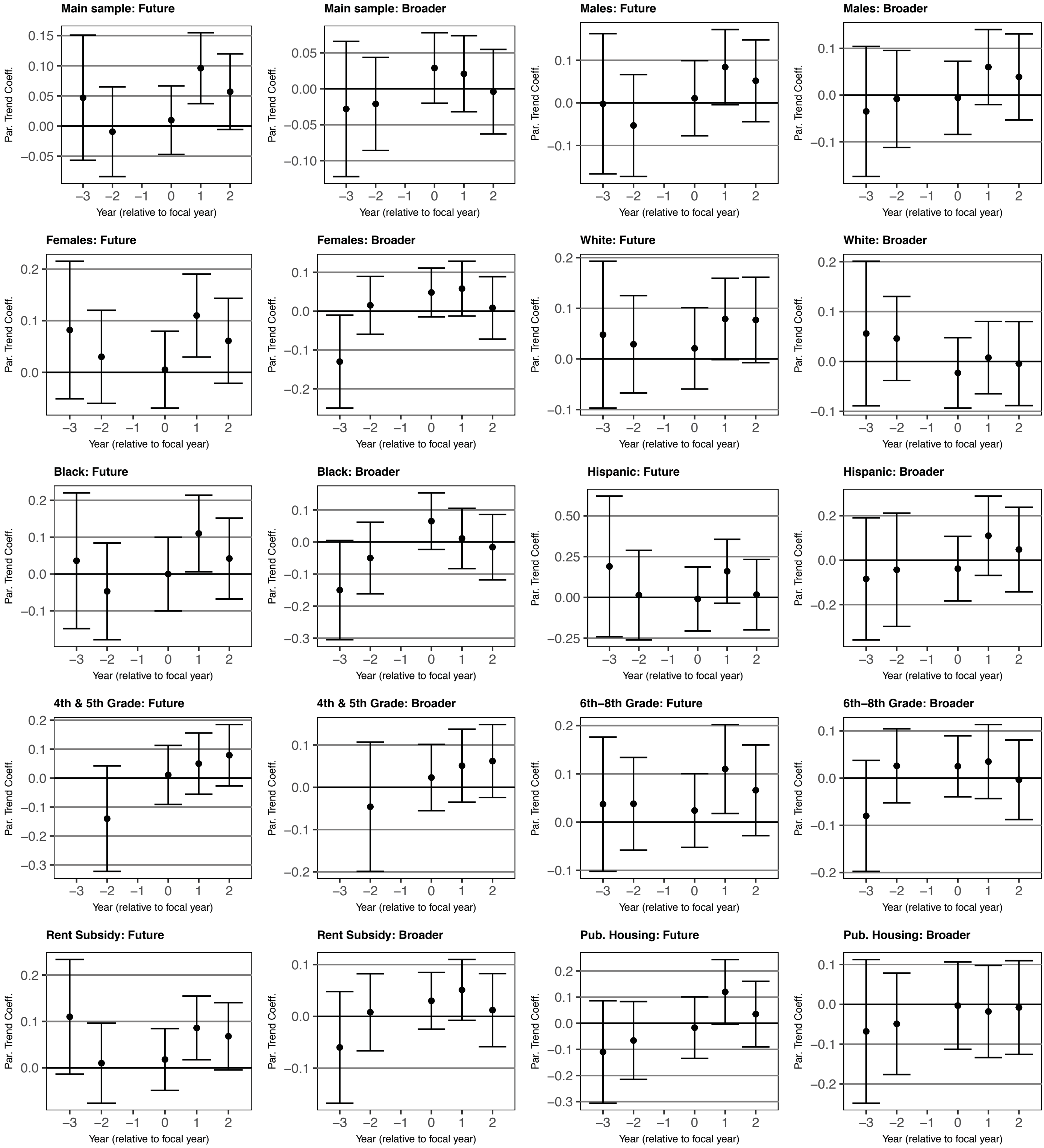


Figure 2: Assessing parallel trends for reading models.



Methodological appendix: Propensity score matching

To match treatment students to the broader comparison group students in our analyses, we used psmatch2, a propensity score matching command in Stata. We ran the match in StataSE 14. For our main treatment models, we predicted treatment using a combination of student, family, and school characteristics in the year prior to treatment. Our estimation model was as follows.

$Y = \beta_0 + \beta X$

Where Y represents the logit of treatment, and X represents a vector of predictors of treatment:

- Standardized reading test scores
- Standardized math test scores
- Race: White, Black, Hispanic, Asian or Pacific Islander, American Indian or Alaskan Native
- Gender: male, female
- Grade-level
- Whether student is economically disadvantaged (FRL)
- Whether student has a disability
- Whether student is an English Language Learner (ELL)
- Primary Person's (PP[1]) highest level of education: no high school diploma, high school diploma or GED, associate degree, bachelor's degree or higher
- Primary Person's marital status: married; single-never married; annulled, divorced, legally separated, or separated; widowed
- Total amount of W2, wages, UI benefit, and Social Security income
- Total household wages in dollars (from UI wage records)
- Any wage income (binary)
- Total dollar amount of food stamps
- Ever received food stamps
- Total dollar amount of W2 cash benefit
- Number of months where at least one family member was covered by medical assistance
- Number of children in each household
- Number of adults in each household
- Cohort (Year of housing subsidy receipt for the treatment group. For the comparison group, it represents the year for which they serve as the comparison.)
- Percentage of students who are Black in school
- Percentage of students who are Hispanic in school
- Percentage of students who receive Free or Reduced Price Lunch in school
- Student-teacher ratio in school
- Weighted math and reading scores in school
- Number of students in school

We used the following options with psmatch2: logit (uses logistic regression instead of probit), common (requires common support between treatment and control group), and neighbor(3) (matches to nearest three neighbors). We then saved the weights, multiplied them by 3 (to make them integers), and applied them to each observation of a given student.

For the subgroup analyses (by gender, race, grade, and housing assistance type), we repeated this process by restricting the match to the subgroup of interest. For example, for male-only models, females were removed from the data prior to matching. We applied the resulting weights to the regression models predicting treatment effects using the frequency weight option (*fw=weight*).

[1] We sought to identify each household's "Primary Person (PP)" in order to include information about the racial/ethnic background, educational attainment, and marital status of an adult in the house. Since there were often multiple adults associated with the household, identifying the characteristics of the household's "head" or "primary person" enabled us to provide consistent background information about the household in which the student was living.