

Differential Effects of Homeownership on Children from Higher- and Lower-Income Families

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Abstract

A growing body of evidence indicates that children benefit from parental homeownership. This article examines whether children from lower-income and higher-income families benefit equally from homeownership. If systematic differences exist, previous research that assumed equal benefits across the income spectrum could have produced erroneous estimates.

For children growing up in families with incomes less than 150 percent of the federal poverty line, homeownership is found to raise educational attainment, earnings, and welfare independence in young adulthood. These positive results do not extend to the long-term outcomes of children in families with incomes more than 150 percent of the poverty line, however. Results are robust across models that test four different instrumental variables for homeownership. This increases confidence that homeownership effects are not attributable to unobserved characteristics of homeowners, but rather indicate causal effects.

Keywords: Homeownership; Instrumental variables; Low-income children

Introduction

The past decade has witnessed substantial efforts to increase the rate of homeownership, including homeownership among low-income families. Among the various currents feeding this notion is a growing body of evidence that growing up in a homeownership family confers positive benefits on children (Aaronson 2000; Boehm and Schlottman 1999; Green and White 1997; Haurin, Parcel, and Haurin 2002).¹ None of this research, however, has focused on the effects of homeownership on children in low-income families. Rather, findings were based on samples that included families from across the income spectrum. Whether the benefits of homeownership are the same across income levels, with equally positive effects on children from rich and poor families, has not been examined.

There are reasons to expect the effects of homeownership on children to vary by income level. Homeownership could fail to benefit poor children if home down payments and maintenance absorb resources that otherwise might be invested in children's development. The tax advantages of homeownership also are reaped disproportionately by those who are more affluent,

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¹ On the other hand, some evidence exists that public housing has beneficial effects (Currie and Yelowitz 2000; Newman and Harkness 2002). This type of subsidized rental housing frequently is charged with having damaging effects on children.

which could lead to better outcomes for their children. Finally, because wealthier homeowners tend to cluster in the same neighborhood, their children are likely to benefit from favorable community effects such as better schools. On the other hand, homeownership may add relatively little to the already advantageous circumstances of children from higher-income families. For example, if homeownership confers its benefits primarily by improving the home environment, the degree of improvement in moving from a rented to an owned dwelling may be greater for lower-income families than for high-income families.

Addressing this question is important for policy reasons. If the benefits of homeownership are restricted to children of higher-income families, current policy efforts to increase the homeownership rate among lower-income families may be misplaced. In addition, gaining an understanding of which children benefit most from parental homeownership may shed light on the mechanism through which the effect operates.

The contributions of this article are twofold. Substantively, it tests whether homeownership has the same effects on children from families with different income levels. It also examines a broader set of outcomes than previous studies, which focus primarily on educational attainment (Aaronson 2000; Boehm and Schlottman 1999; Green and White 1997).² We extend the set of outcomes to include teenage unwed births, idleness, earnings, and welfare receipt.

The second major contribution is methodological. A key difficulty in identifying the effect of homeownership is that it may be associated with a range of personal characteristics, such as responsibility or motivation, that are not measured in most surveys and, therefore, cannot be controlled for in statistical models. As a consequence, it is impossible to be sure that the estimated effects of homeownership actually are attributable to homeownership itself instead of to unmeasured family traits associated with homeownership. To overcome this difficulty, previous studies applied instrumental variable techniques (or instruments), which are the most common method of dealing with unmeasured variable bias in nonexperimental data. Although these studies found that the positive effect of homeownership persists when instrumental variable techniques were used, each of these studies used only a single instrument. Yet, identifying a high-quality instrument is extremely challenging both because it is often difficult to find an instrument that is conceptually plausible and because there is no analytical method for testing the quality of an instrument. Results derived from use of a single instrument therefore may be subject to dispute. We address this problem by testing multiple instruments. This approach allows us to examine the robustness of results for different instruments, increasing confidence in the estimated effects.³

The next section of this article discusses data and methodology. A description of sample characteristics and the estimation results follow. The article concludes with a discussion of the findings and some avenues for further investigation.

² Green and White (1997) also examined the effect of homeownership on teenage unwed childbearing in one of three data sets they considered. Boehm and Schlottman (1999) simulated the indirect effect of homeownership on lifetime earnings through its effect on educational attainment. They also tested whether children of homeowners were more likely to become homeowners themselves.

³ In addition, formal statistical “overidentification” tests can be used to determine whether multiple instruments, as a group, meet the key criterion of serving as a good instrument (i.e., being exogenous with respect to outcomes). However, these tests are of less utility than it would seem, as is discussed later.

Data and Methods

Sample

The analysis uses data from the 1968 to 1993 waves of the Panel Study of Income Dynamics (PSID). Begun in 1968, PSID is an ongoing longitudinal survey of U.S. households conducted by the Survey Research Center at the University of Michigan. All original household members have been followed over time. Recent research confirms that despite considerable attrition, PSID remains representative of the population (Fitzgerald, Gottschalk, and Moffitt 1998a, 1998b; Zabel 1998).

The analysis is performed on a sample of individuals who were born between 1957 and 1973 and who had PSID family data available for each year they were between ages 11 and 15. Results are compared for two groups: (1) “low-income” children from families with parental earnings less than 150 percent of the federal poverty threshold for at least three of the five years the children were between ages 11 and 15, and (2) “high-income” children from families with parental earnings more than 150 percent of the federal poverty threshold for at least three of the five years the children were between ages 11 and 15.⁴ The two groups have roughly the same number of observations. Chow tests indicate strong structural differences between model estimates for low- and high-income families and reject pooling. Splitting the sample into these two groups is therefore appropriate.

Dependent Variables and Methods

As shown in table 1, the analysis examines the effects of living in an owned home as a child between ages 11 and 15 on seven outcomes: (1) giving birth as an unmarried teenager (women only); (2) idleness (not working, attending school, or caring for children) at age 20; (3) years of education at age 20; (4) high school completion at age 20; (5) acquisition of postsecondary education at age 20; (6) earnings between ages 24 and 28; and (7) receipt of welfare (Aid to Families with Dependent Children, food stamps, or other cash assistance, excluding supplemental security income) between ages 24 and 28.⁵

The focus of this investigation is on the effects of parental homeownership on children between ages 11 and 15 for each of these outcomes. We specify the homeownership variable as the number of years the child lived with a homeownership family during these five years. Because preliminary tests of a categorical specification of homeownership (i.e., zero, one to two, three to four, and five years) demonstrate that effects increase in rough proportion to exposure, the linear specification is appropriate.

⁴ The low-income and high-income designations are shorthand for incomes less than and more than 150 percent of the poverty line, respectively; they are not precise descriptions. Clearly, families with incomes slightly higher than 150 percent of the poverty line generally are not viewed as having high incomes.

⁵ For educational outcomes, about 25 percent of cases are missing data for grades completed at age 20, but data exist for grades completed at some other age. In these cases, we substituted educational attainment in the closest year after age 20, if available, and the closest year before age 20 otherwise. Because educational attainment is affected by age, these models also include a control variable for the age at which educational outcomes are measured.

Table 1. Dependent Variables and Estimation Techniques

Outcome	Age Measured (Years)	Estimation Technique
Gave birth as an unwed teenager (1 = yes)	<20	Probit
Idle (1 = yes)	20	Probit
Years of education	20	Least squares
Completed high school (1 = yes)	20	Probit
Any postsecondary education (1 = yes)	20	Probit
Earnings	24–28	Least squares
Received welfare (1 = yes)	24–28	Probit

The effects of homeownership are estimated both without the application of instrumental variables (uninstrumented) and with instrumental variable techniques (instrumented). For two outcomes, years of education and earnings, ordinary least squares (OLS) is used to estimate the uninstrumented models, and two-stage least squares is used for the instrumented ones.⁶ All of the remaining outcomes are binary (e.g., whether the subjects graduated from high school or not), and probit is used to estimate the uninstrumented models for these outcomes. An Amemiya generalized least squares (AGLS) estimator, as described in Newey (1987), is used for the instrumented probits.⁷ In each case, we estimate the outcome, Y , as a function of years of homeownership, h , plus other exogenous (control) variables X :

$$Y = Y(h, X). \quad (1)$$

In the instrumental variable estimations, years of homeownership is modeled as a function of instruments, Z , plus exogenous variables X :

$$h = h(Z, X). \quad (2)$$

Control Variables

All models control for the following characteristics: (1) race; (2) gender; (3) year born; (4) age of mother when born; (5) educational attainment of household head; (6) number of children in the family; (7) years in a two-parent family; (8) average annual earnings; (9) whether any, and amount of, parental income (not including public assistance) in excess of earnings (average annual); (10) number of years the family relied on Aid to Families with Dependent Children, food stamps, or other cash assistance (excluding supplemental security income); (11) years in a city with a population of more than 500,000; (12) years in a city with a population of 100,000 to 500,000; and (13) primary state of residence. Monetary values are

⁶ For earnings, we also tested tobit as the estimation technique because large proportions of observations in both groups had no earnings. The tobit estimates are somewhat more positive than the OLS estimates, as expected, but they are not qualitatively different. For simplicity, only the OLS estimates are reported.

⁷ Also see Maddala (1983, 247–252) for a description of the AGLS estimator. (This estimator also was used for the instrumented tobits on earnings mentioned above.)

adjusted to 1997 dollars using the consumer price index for all urban consumers. City sizes come from the PSID census geocode.⁸

Each of these variables plausibly is related to one or more outcomes examined here. With the exception of wealth (other than home equity) and city size, these variables have been used extensively in other research on the determinants of children's outcomes. We control for wealth by including a measure of income that is neither earned nor obtained through public assistance.⁹ This approach is based on Conley's (1999, 2001) finding that parental wealth has significant effects on children's outcomes. Asset income also is likely to affect a family's chance of becoming a homeowner. The control for city size is included because of Page and Solon's (1999) research demonstrating "the importance of being urban" on adult earnings, and because homeownership rates are lower in central cities. State dummy variables are included because unmeasured features of states, such as the quality of education or labor market conditions, may affect outcomes (Moffitt 1994).

Several studies have shown that residential stability benefits children's outcomes (Astone and McLanahan 1994; Hanushek, Kain, and Rivkin 1999; Haveman, Wolfe, and Spaulding 1991; Jordan, Lara, and McPartland 1996). However, residential mobility is not controlled for here because it is likely to be endogenous. That is, there is strong evidence of a causal connection between homeownership and residential stability (Hanushek and Quigley 1978; Ioannides and Kan 1996; Kan 1999, 2000; Lee, Oropesa, and Kanan 1994; Newman and Duncan 1979; Quigley and Weinberg 1977). In addition, unobserved factors associated with both residential stability and children's outcomes are likely. If the objective is to determine the distinct effects of homeownership and residential stability, it is necessary to include, and instrument for, measures of both in the models. However, this article focuses on the total effect of homeownership, not the pathways through which the effect operates. Therefore, measures of residential stability are excluded from the models.¹⁰ Consequently, the coefficient on homeownership picks up both the direct effect of homeownership on children's long-term outcomes and the indirect effect possibly operating through residential stability. Finally, having *some* nonlabor, nontransfer income appears to be more important than the amount of earnings in determining homeownership. Therefore, we include a dummy variable indicating whether a family had any income of this type, in addition to its level.¹¹

Instruments for Homeownership

A variable must satisfy two criteria if it is to serve as a valid instrument for homeownership by a child's family. First, it must strongly predict whether a family owns its home. Second, net of its effect on homeownership, it should be unrelated to children's outcomes (i.e., exogenous).

⁸ Annual city size values were obtained by logarithmically interpolating between place size values of the two closest decennial census years.

⁹ PSID did not begin collecting detailed data on assets until 1984.

¹⁰ Similar arguments could be made for the exclusion of certain variables included as controls, such as family income, number of children, and marital stability. Each of these plausibly could be affected by homeownership. We know of no research that documents such effects, however.

¹¹ Belsley, Kuh, and Welsch (1980) tests revealed no collinearity problems with the variables used in the models.

Thus, an individual's hair color is a poor instrument for homeownership, because presumably it would fail the first criterion of predicting homeownership, even though it likely would pass the second criterion of being exogenous to outcomes. Family income also is a poor instrument for homeownership, because it fails the second criterion, even though it passes the first.

The first criterion for a good instrument can be tested easily by examining its strength in a regression. The second criterion cannot be evaluated statistically.¹² As a result, it is impossible to be certain that an instrument meets this second criterion. One way to reduce the uncertainty is to test multiple plausible instruments, which provides a way to gauge the robustness of results. In particular, if several different instruments produce results that are roughly comparable, we can be more confident that they are correct. This is the approach taken here. As shown in table 2, four instruments for homeownership are tested:

1. The annual change in the state's per capita highway investment ("highway stock")
2. The ratio of renter to owner costs in the census region in the year of the last residential move or in the first observed year after age five if no residential moves are observed ("cost ratio")
3. The metropolitan area or county ratio of median rent to median property value ("metropolitan area")
4. The state homeownership rate¹³

Each of these instruments is expected to predict greater homeownership. Investments in highway capital spending are expected to facilitate the construction and occupancy of new owner-occupied housing, particularly in suburban areas. Both the cost ratio and metropolitan area instruments should capture the relative cost of owning versus renting. The cost ratio instrument is more attuned to changing conditions over time, and the metropolitan area instrument is more indicative of differences across geographic areas. Because they are meant to capture housing market conditions at the time a family makes its tenure decision, the first three instruments were measured at the time of the most recent residential move after age five or, if no such move occurred, at the first year observed after age five.¹⁴

¹² Overidentification tests can be used to assess the exogeneity of one plausible instrument versus another or one group of instruments versus another (Davidson and MacKinnon 1993; Hausman 1983). However, because instruments can be compared only against each other, not some external standard, these tests are not an infallible way to identify poor instruments. In this research, we found that overidentification tests resulted in the rejection of the null hypothesis that two instruments are exogenous to outcomes only if the two-stage least squares estimates produced using the two instruments were very different. Because instruments that produced outlier estimates invariably were those rejected by overidentification tests, robustness of results across competing instruments provides essentially the same indication of their credibility as overidentification tests, while lending itself to a more straightforward interpretation. The results presented in the text are those produced using individual instruments. The complete overidentification test results are available on request.

¹³ A fifth candidate, the ratio of the annual number of single to multifamily homes constructed in each census region, also was tested, but it performed badly as a predictor of homeownership.

¹⁴ We used age five as a compromise between children for whom we had data from birth and those for whom we had data starting at age 11. (The sample includes children born between 1957 and 1973, and PSID began collecting data in 1968.)

Table 2. Homeownership Instruments

Instrument	Description	Rationale
Highway stock	Annual change in state's per capita highway investment (Bell and McGuire 1997)	An increase in highway stock should lower the cost of commuting, facilitating homeownership.
Cost ratio	Ratio of owner to renter costs in census region (Joint Center for Housing Studies of Harvard University 1989, 1994)	Higher costs of homeowning relative to renting should reduce the incentive to purchase a home.
Metropolitan area	The metropolitan area ratio of median property value to median rent; for nonmetropolitan areas, the ratio for the county is used (U.S. Bureau of the Census) ^a	Higher property values relative to rents should reduce the attractiveness of homeownership.
State homeownership	State homeownership rate (U.S. Bureau of the Census 2002) ^b	Increasing the rate of homeownership in the state should correlate with higher individual probability of homeownership.

Sources: Data from Bell and McGuire (1997); Joint Center for Housing Studies of Harvard University (1989, 1994); and U.S. Bureau of the Census (1976, 1981, 1986, 1991, 1996, 2002).

^a Data for inter-census years obtained by linearly interpolating between the two closest decennial censuses.

^b Data since 1984 obtained from U.S. Bureau of the Census (2002). Data prior to 1984 derived by linearly interpolating between the two closest decennial censuses.

Although there is no causal link between the state homeownership rate and an individual family's probability of homeownership, they likely are correlated. The state homeownership rate may pick up factors promoting homeownership in the state that are not captured in the other instruments. Because this instrument was expected to predict homeownership in a correlational, not causal, fashion, it was measured as the average homeownership level in the child's state of residence between ages 11 and 15. The cost ratio and state homeownership rate also are used in the studies of Green and White (1997) and Aaronson (2000), respectively.

The results from regressing years of homeownership on each of the instruments (plus control variables) are shown in table 3. All of the instruments had the expected sign and attained at least a modest degree of statistical significance in both groups. In the low-income group, all of the instruments were statistically significant at the 1 percent level, and two instruments—state homeownership and metropolitan area—also achieved this level of statistical significance in the high-income group. The greater weakness of the instruments in predicting homeownership for high-income families probably indicates that their home-buying decisions are less affected by market and other contextual factors. Presumably, this is at least part of the reason that 87 percent of the high-income group achieves homeownership. Nonetheless, two of the instruments are strong predictors of homeownership in the high-income group, and they are highly statistically significant when used jointly ($F = 10.01, p < .0001$).

Table 3. Effectiveness of Instruments in Predicting Homeownership

Instrument	Low Income			High Income		
	Sign	<i>t</i> -Statistic	Significance	Sign	<i>t</i> -Statistic	Significance
Highway stock	+	2.80	0.005	+	1.76	0.078
Cost ratio	+	5.69	0.000	+	1.47	0.141
Metropolitan area	+	4.95	0.000	+	7.17	0.000
State homeownership	+	11.82	0.000	+	6.59	0.000

Sources: Homeownership and family data from Panel Study of Income Dynamics (University of Michigan Institute for Social Research 1995, 1997, 1998, 2000). Instrument data from Bell and McGuire (1997); Joint Center for Housing Studies of Harvard University (1989, 1994); and the U.S. Bureau of the Census (2002).

Note: Each instrument was tested individually in ordinary least squares regression with state fixed effects using the control variables described in the text.

Sample Characteristics

More than 80 percent of children in the high-income group lived in an owned home between ages 11 and 15, compared with roughly half of the children in the low-income group. Table 4 displays the average characteristics of renter and owner children in both groups. Children of homeowners are much more privileged in both groups. Homeowning parents are roughly twice as likely to be college graduates, and they have roughly twice the earnings of renting parents. Owner children also are more likely to live in a two-parent family compared with renter children. The rate of two-parent families is 50 percent higher among owners versus renters in the low-income group, and it is 37 percent higher in the high-income group. Both groups exhibit large racial disparities between homeowners and renters. In the high-income group, for example, 25 percent of renters are black, compared with 8 percent of owners.

In the low-income group, housing tenure is associated with sharp contrasts in children's outcomes. Owner children are 33 percent less likely to give birth as unmarried teenagers, 40 percent less likely to be idle at age 20, and 35 percent less likely to receive welfare. They complete half a year more of school, are 20 percent more likely to graduate from high school, and are 62 percent more likely to acquire postsecondary education. Their early adult earnings are 24 percent higher.

In the high-income group, differences in educational attainment for owner versus renter children are as strong as they are in the low-income group, but the differences in the other outcomes are less striking. There is no association between housing tenure and unwed teenage childbearing or idleness at age 20, for example. Owner children in the high-income group have earnings that are 17 percent higher than renter children, and owner children are 27 percent less likely to receive welfare. Although notable, these latter two differences are somewhat smaller than those for their counterparts in the low-income group.

Table 4. Sample Means

Variable	Low Income		High Income		Total (N = 6,732)
	Renters (N = 1,380)	Owners (N = 1,592)	Renters (N = 639)	Owners (N = 3,121)	
Dependent variables					
Gave birth as unwed teenager (women only) (1 = yes)	0.18	0.12	0.03	0.03	0.05
Idle at age 20 (1 = yes)	0.25	0.15	0.07	0.07	0.11
Years of education at age 20	11.4	11.9	11.8	12.5	12.2
Graduated from high school by age 20 (1 = yes)	0.61	0.72	0.65	0.79	0.75
Attended postsecondary school by age 20 (1 = yes)	0.13	0.21	0.25	0.40	0.33
Average annual earnings ages 24 to 28	\$10,594	\$13,112	\$17,325	\$20,224	\$15,805
Received any welfare ages 24 to 28 (1 = yes)	0.34	0.22	0.11	0.08	0.15
Policy variable					
Years in an owned home as child ages 11 to 15	0.00	4.24	0.00	4.66	3.75
Control variables (measured ages 11 to 15)					
Black	0.44	0.24	0.25	0.08	0.15
Female	0.49	0.50	0.55	0.50	0.50
Year born	1964	1964	1970	1967	1967
Mother's age when born	26	26	25	26	26
Household head is high school graduate	0.34	0.45	0.68	0.79	0.68
Household head is college graduate	0.02	0.05	0.13	0.25	0.18
Mean number of children in family	3.8	3.7	2.4	2.5	2.8
Years in two-parent family	2.3	3.5	3.4	4.6	4.1
Parental earnings (\$000s)	11.00	20.50	36.80	60.60	46.70
Whether income greater than earnings and transfers	0.56	0.78	0.78	0.94	0.86
Amount of family income greater than earnings and transfers (\$000)	2.5	6.7	4.1	6.6	6.00
Number of years receiving food stamps or cash welfare	0.64	0.28	0.18	0.03	0.14
Years in a city with a population of more than 500,000	1.43	0.62	0.83	0.39	0.57
Years in a city with a population of 100,000 to 500,000	1.06	0.82	1.52	1.19	1.14

Source: Data from Panel Study of Income Dynamics (University of Michigan Institute for Social Research 1995, 1997, 1998, 2000).

Note: All monetary values are expressed in 1997 dollars. Values are weighted using age 15 Panel Study of Income Dynamics individual weights.

Regression Results

The preceding statistics indicate that the outcomes of homeowner children are more favorable than those of renter children. The objective of the multivariate analyses is to determine whether these differences in outcomes are due to homeownership itself or to the more advantageous family background of homeowners.

Tables 5 and 6 display the estimates for the effects of homeownership from both the uninstrumented and instrumented models using the low-income and high-income groups, respectively.¹⁵ The first model in each table regresses outcomes on years of homeownership alone and furnishes a baseline for comparison with the other results. The second model adds control variables, and the third through sixth models use each of the instruments for homeownership, in turn.

Baseline Results

In the baseline models, years of homeownership has a statistically significant and favorable association with all outcomes except unwed teenage childbearing. In the lower-income group, a year of homeownership is associated with a 2.4 percentage point decrease in idleness, somewhat more than a tenth of a year increase in years of education, a 3 percentage point increase in the probability of high school graduation, a 2 percentage point increase in the likelihood of acquiring postsecondary education, \$626 more in earnings annually between ages 24 and 28, and a 2.4 percentage point reduction in the likelihood of receiving welfare. With the high-income group, the homeownership coefficients tend to be somewhat, but not dramatically, smaller.

Uninstrumented Results

When measured, background characteristics are statistically controlled for in the lower-income group; effects of homeownership on most outcomes are smaller than in the baseline results, shrinking between a fifth (for welfare receipt) and two-thirds (for earnings). Excluding unwed teenage births, which had no significant association with homeownership in the baseline results, on average 41 percent of the homeownership effect is absorbed by control variables. Yet, homeownership remains statistically significant at the conventional 5 percent level for all outcomes except for earnings.

In contrast, in the high-income group, an average of 70 percent of the homeownership effect on outcomes is absorbed by adding control variables. Only the effects on years of education and the acquisition of postsecondary education remain statistically significant. Thus, in the lower-income group, homeownership appears to exert an effect on outcomes above and

¹⁵ Results for all variables from one of the instrumental variable models are presented in the appendix in tables A.1 and A.2. The estimates for the control variables (i.e., other than homeownership) did not vary materially between models.

Table 5. Estimated Effects of a Year of Living in Owned Housing between Ages 11 and 15 on Later Outcomes, Low-Income Families

Model	Outcome						
	Teenage Unwed Birth	Idle	Years of Education	High School Graduate	Postsecondary Education	Average Earnings	Received Welfare
1. Years of homeownership only	-0.007 (0.147)	-0.024 (0.000)	0.112 (0.000)	0.032 (0.000)	0.020 (0.000)	\$626 (0.000)	-0.024 (0.000)
2. Years of homeownership and controls	-0.007 (0.292)	-0.013 (0.024)	0.073 (0.000)	0.023 (0.000)	0.010 (0.003)	\$200 (0.123)	-0.019 (0.001)
3. Highway stock instrument	-0.099 (0.465)	-0.095 (0.220)	1.052 (0.030)	0.430 (0.040)	0.198 (0.037)	\$2,546 (0.018)	-0.187 (0.001)
4. State homeownership instrument	-0.263 (0.200)	-0.112 (0.406)	0.152 (0.697)	0.083 (0.505)	0.034 (0.629)	\$6,812 (0.420)	-0.113 (0.245)
5. Cost ratio instrument	-0.024 (0.619)	-0.015 (0.654)	0.507 (0.002)	0.127 (0.015)	0.094 (0.003)	\$1,754 (0.001)	-0.062 (0.008)
6. Metropolitan area instrument	-0.039 (0.783)	0.110 (0.229)	0.622 (0.025)	0.215 (0.017)	0.025 (0.593)	\$4,310 (0.021)	-0.006 (0.903)

Sources: Homeownership and family data from Panel Study of Income Dynamics (University of Michigan Institute for Social Research 1995, 1997, 1998, 2000). Instrument data from Bell and McGuire (1997); Joint Center for Housing Studies of Harvard University (1989, 1994); and U.S. Bureau of the Census (2002).

Note: *p* values are in parentheses. All monetary values are expressed in 1997 dollars. Least squares estimates are shown for years of education and earnings, and probit estimates are shown for the other outcomes. Probit estimates have been transformed to show the effect of a year of homeownership on change in probability of outcome with all control variables set to their means (i.e., marginal effects).

Table 6. Estimated Effects of a Year of Living in Owned Housing between Ages 11 and 15 on Later Outcomes, High-Income Families

Model	Outcome						
	Teenage Unwed Birth	Idle	Years of Education	High School Graduate	Postsecondary Education	Average Earnings	Received Welfare
1. Years of homeownership only	-0.001 (0.611)	-0.009 (0.015)	0.119 (0.000)	0.018 (0.000)	0.029 (0.000)	\$855 (0.000)	-0.018 (0.000)
2. Years of homeownership and controls	0.001 (0.801)	-0.002 (0.656)	0.043 (0.009)	0.004 (0.405)	0.014 (0.026)	\$148 (0.518)	-0.006 (0.121)
3. Highway stock instrument	0.002 (0.967)	0.016 (0.789)	1.268 (0.513)	0.140 (0.746)	0.573 (0.515)	\$1,696 (0.300)	0.029 (0.437)
4. State homeownership instrument	0.092 (0.477)	0.106 (0.558)	0.030 (0.960)	0.003 (0.989)	-0.236 (0.408)	\$27,286 (0.763)	1.138 (0.689)
5. Cost ratio instrument	-0.010 (0.750)	0.037 (0.184)	1.165 (0.624)	-0.144 (0.780)	-0.073 (0.894)	\$1,301 (0.183)	-0.001 (0.947)
6. Metropolitan area instrument	-0.061 (0.074)	0.036 (0.418)	0.231 (0.171)	0.034 (0.524)	0.002 (0.975)	-\$796 (0.655)	0.044 (0.133)

Sources: Homeownership and family data from Panel Study of Income Dynamics (University of Michigan Institute for Social Research 1995, 1997, 1998, 2000). Instrument data from Bell and McGuire (1997); Joint Center for Housing Studies of Harvard University (1989, 1994); and U.S. Bureau of the Census (2002).

Note: *p* values are in parentheses. All monetary values are expressed in 1997 dollars. Least squares estimates are shown for years of education and earnings, and probit estimates are shown for the other outcomes. Probit estimates have been transformed to show the effect of a year of homeownership on change in probability of outcome with all control variables set to their means (i.e., marginal effects).

beyond measured family characteristics, but in the high-income group, almost all of the homeownership effect stems from other measured differences between homeownership and renting families. The one exception is the acquisition of postsecondary education, for which the uninstrumented results are about the same in both groups. It may be that home equity is an important source of wealth to fund children's postsecondary education for families in both groups.

Instrumented Results

None of the coefficients in the instrumented models estimated for the high-income group attain the conventional 5 percent level of statistical significance, and they often have conflicting signs. In particular, effects of homeownership on postsecondary education and years of education, which remain statistically significant when controls are added to the baseline model, are not statistically significant in the instrumented results and often have negative signs. These results indicate an absence of causal effects of homeownership on outcomes for the high-income group. The remainder of this section focuses on the low-income group only.

No instrument produces statistically significant estimates for the effect of homeownership on either the likelihood of giving birth as an unwed teenager or the likelihood of being idle at age 20. For all the remaining outcomes, at least two of the four instruments produce statistically significant results, and all the instruments produce estimates of the expected (beneficial effect) sign. Both the highway stock and the cost ratio instruments produce statistically significant estimates for all outcomes (excluding unwed teenage births and idleness). The metropolitan area instrument produces statistically significant estimates for years of education, high school graduation, and earnings, but not for the acquisition of postsecondary education and welfare receipt. The state homeownership rate instrument does not produce statistically significant estimates of homeownership effects for any outcomes.

It is possible that one, or even two, of the instruments that produced statistically significant results for homeownership could be related to outcomes and, hence, fail to satisfy a key condition for serving as an adequate instrument. It is not apparent, however, why all three should fail to satisfy this condition and, moreover, why they should fail only in the lower-income group. Thus, the fact that three of the four instruments tested produce statistically significant results for favorable long-term effects of homeownership on the outcomes of low-income children serves as strong evidence of causal, not merely correlational, effects.

Discussion

The results support the hypotheses that for children from low-income families, homeownership increases educational attainment, raises earnings, and reduces welfare use. Whether it has similarly strong effects among children from high-income families appears doubtful.¹⁶

¹⁶ Definitively ruling out this possibility requires statistically testing whether coefficients from the instrumented models are different between the two groups. However, the standard *F*-tests for distinguishing between coefficients cannot be used across two different groups, and the groups cannot be combined because the Chow test rejects

The apparent beneficial effects of homeownership for the high-income group largely are attributable to observable individual and family characteristics such as income and parental education. For high-income children, homeownership seems to provide only marginal, if any, additional benefits above and beyond the benefits already enjoyed by children in more affluent families.¹⁷

For the lower-income group, the statistically significant coefficients from the instrumental variable models are much larger than their uninstrumented counterparts. This suggests that, contrary to expectations, unobserved characteristics associated with an increased likelihood of homeownership lead to worse, not better, long-term outcomes for children.

This possibility of adverse selection is not as counterintuitive as it may appear at first glance. Homeownership represents, in part, a household resource allocation decision. As Aaronson (2000) suggests, among resource-constrained families, investments of time and money in a home may crowd out other investments that could benefit children more directly. For example, parents who, all else equal, elect not to own may spend more time with their children, choose to rent in areas with better schools, send their children to private schools, or provide a richer extracurricular environment for the children.

On the other hand, the suggestion of adverse selection (with respect to children's long-term outcomes) into homeownership does conflict with other studies, which found the opposite—that effect estimates shrink when instrumental variables are used, indicating positive selection into homeownership (Aaronson 2000; Green and White 1997; Haurin, Parcel, and Haurin 2002). The discrepancy may occur because of differences in model specification, derivation of instruments, and estimation techniques, but it is difficult to pin down the precise reason. It does not stem from splitting the sample into high- and low-income groups, because the statistically significant results from the instrumental variable models are larger than their uninstrumented counterparts even when the low- and high-income groups are pooled.

Beyond offering robust evidence that homeownership has causal beneficial effects on the long-term outcomes of children from low-income families, this article is the first to test whether these beneficial effects are sustained into early adulthood. In contrast with the more typical scenario of social interventions eroding over time, these results suggest that homeownership has staying power. The size and durability of the effects, and their robustness over different

pooling. One way to test for differences in coefficients is to test whether the estimated effects of homeownership in the high-income group are less than certain threshold values. However, aside from the issue of deciding what these values should be, the instrumental variable estimates obtained using the high-income group are so imprecise that such tests would not be conclusive. Nonetheless, although coefficients cannot be shown to be statistically different, it remains true that strong effects of homeownership are found in the lower-income group but not in the high-income group.

¹⁷ The one exception may be the effect of homeownership on the acquisition of postsecondary education, which remains statistically significant when controls are added to the baseline model. This effect is plausible if homeownership is a more effective vehicle for financing college expenses than other investments. However, it is not statistically significant in the instrumental variable results using any combination of instruments, casting doubt on this speculation.

studies, approaches, and data sets, warrant serious consideration of homeownership promotion as a social policy tool. However, because homeownership may not be a suitable option for all low-income families under any policy scenario, additional rigorous research to identify what about homeownership contributes to better outcomes may help children of renters reap some of these benefits.

Two especially fruitful avenues may be examining whether the positive effects of homeownership can be traced to its function as an asset or to its reduction of residential mobility. Conley (1999, 2001) showed that children of wealthier parents have better outcomes. Because the value of a home constitutes a large part of most families' total wealth, it is plausible that much of the wealth effect on children's outcomes stems from homeownership. It is unknown, however, whether other forms of wealth, such as savings accounts, have effects similar to those of homeownership.

Another possibility is that the primary mechanism through which homeownership improves children's outcomes is by increasing residential stability. As mentioned earlier, several studies have found that moving frequently harms children's educational outcomes, especially low-income children, and homeownership has been found to reduce residential moves. It is possible that other forms of housing, such as assisted housing, also may improve residential stability, which may explain why public housing appears to have beneficial effects on children (Currie and Yelowitz 2000; Newman and Harkness 2002). We are examining this possibility in our current work.

Appendix

Results from the Cost Ratio Instrument Model

Table A.1. Complete Results Obtained Using Cost Ratio Instrument, Low-Income Families

Variable	Outcome						
	Teenage Unwed Birth	Idle	Years of Education	High School Graduate	Postsecondary Education	Average Earnings	Received Welfare
Individual characteristics							
Female		-0.08 (0.00)	0.31 (0.00)	0.10 (0.00)	0.06 (0.00)	-4,804 (0.00)	0.11 (0.00)
Black	0.06 (0.08)	0.11 (0.00)	0.48 (0.00)	0.18 (0.00)	0.09 (0.00)	-2,476 (0.00)	0.11 (0.00)
Mother's age when born	0.00 (0.42)	0.00 (0.20)	-0.02 (0.04)	0.00 (0.33)	0.00 (0.19)	-34 (0.45)	0.01 (0.01)
Family socioeconomic characteristics between ages 11 and 15							
Years in an owned home	-0.02 (0.62)	-0.02 (0.65)	0.51 (0.00)	0.13 (0.01)	0.09 (0.00)	1,754 (0.00)	-0.06 (0.01)
Average parental earnings (\$000s)	0.01 (0.54)	-0.02 (0.24)	-0.02 (0.75)	0.03 (0.13)	0.00 (0.80)	1,157 (0.00)	-0.03 (0.08)
Years family received food stamps or welfare	0.01 (0.64)	0.02 (0.11)	0.04 (0.35)	0.02 (0.19)	0.01 (0.29)	7 (0.98)	0.00 (0.69)

Table A.1. Complete Results Obtained Using Cost Ratio Instrument, Low-Income Families (continued)

Variable	Outcome						
	Teenage Unwed Birth	Idle	Years of Education	High School Graduate	Postsecondary Education	Average Earnings	Received Welfare
Whether average income greater than earnings and transfers	-0.01 (0.67)	0.03 (0.37)	-0.04 (0.62)	-0.02 (0.42)	-0.01 (0.64)	-683 (0.31)	0.04 (0.19)
Amount of average income greater than earnings and transfers (\$000s)	0.04 (0.26)	0.00 (0.88)	-0.17 (0.12)	-0.01 (0.81)	-0.02 (0.28)	-502 (0.37)	-0.01 (0.63)
Years in two-parent family	-0.02 (0.04)	0.00 (0.64)	-0.07 (0.00)	-0.01 (0.04)	-0.01 (0.00)	-487 (0.00)	0.02 (0.01)
Mean number of children in family	0.01 (0.17)	0.01 (0.08)	-0.13 (0.00)	-0.05 (0.00)	-0.03 (0.00)	-583 (0.00)	0.01 (0.03)
Years in a city with a population of 100,000 to 500,000	0.00 (0.63)	0.00 (0.99)	0.06 (0.08)	0.01 (0.24)	0.01 (0.05)	78 (0.66)	-0.02 (0.03)
Years in a city with a population of more than 500,000	-0.01 (0.21)	0.00 (0.68)	0.02 (0.57)	0.00 (0.73)	0.01 (0.02)	-151 (0.42)	-0.01 (0.17)
Educational attainment of household head							
No high school vs. some high school	0.03 (0.33)	0.05 (0.09)	-0.37 (0.00)	-0.13 (0.00)	-0.03 (0.08)	-1,282 (0.05)	0.05 (0.06)
High school graduate vs. some high school	-0.08 (0.02)	-0.06 (0.08)	0.35 (0.00)	0.09 (0.01)	0.04 (0.07)	2,773 (0.00)	-0.03 (0.37)
Some college vs. some high school	-0.01 (0.77)	-0.06 (0.08)	0.53 (0.00)	0.11 (0.00)	0.14 (0.00)	777 (0.38)	-0.07 (0.05)
College graduate vs. some high school	-0.09 (0.27)	-0.12 (0.26)	0.80 (0.00)	0.21 (0.01)	0.23 (0.00)	219 (0.91)	-0.07 (0.51)
Age educational attainment measured			0.30 (0.00)	0.08 (0.00)	0.04 (0.00)		
Number of observations used in estimation	939	1,726	3,015	2,924	2,843	1,795	2,393

Sources: Homeownership and family data from Panel Study of Income Dynamics (University of Michigan Institute for Social Research 1995, 1997, 1998, 2000). Instrument data from Bell and McGuire (1997); Joint Center for Housing Studies of Harvard University (1989, 1994); and U.S. Bureau of the Census (2002).

Note: *p* values are shown in parentheses. Results for state and birth year dummies are not shown. All monetary values are expressed in 1997 dollars. Least squares estimates are shown for years of education and earnings, and probit estimates are shown for the other outcomes. Probit estimates have been transformed to show the effect of a year of homeownership on change in probability of outcome with all control variables set to their means (i.e., marginal effects).

Table A.2. Complete Results Obtained Using Cost Ratio Instrument, High-Income Families

Variable	Outcome						
	Teenage Unwed Birth	Idle	Years of Education	High School Graduate	Postsecondary Education	Average Earnings	Received Welfare
Individual characteristics							
Female		-0.02 (0.07)	0.33 (0.16)	0.03 (0.52)	0.07 (0.22)	-7,570 (0.00)	0.04 (0.00)
Black	0.06 (0.01)	0.06 (0.01)	0.04 (0.77)	0.01 (0.60)	0.01 (0.75)	-5,174 (0.00)	0.10 (0.00)
Mother's age when born	0.00 (0.86)	0.00 (0.25)	-0.03 (0.73)	0.01 (0.62)	0.01 (0.76)	19 (0.79)	0.00 (0.37)
Family socioeconomic characteristics between ages 11 to 15							
Years in an owned home	-0.01 (0.75)	0.04 (0.18)	1.16 (0.62)	-0.14 (0.78)	-0.07 (0.89)	1,301 (0.18)	0.00 (0.95)
Average parental earnings (\$000s)	0.00 (0.45)	-0.01 (0.05)	-0.04 (0.84)	0.02 (0.65)	0.03 (0.55)	469 (0.00)	-0.02 (0.00)
Years family received food stamps or welfare	0.00 (0.83)	0.02 (0.08)	0.25 (0.76)	-0.10 (0.56)	-0.07 (0.71)	224 (0.76)	0.01 (0.37)
Whether average income greater than earnings and transfers	-0.01 (0.61)	-0.03 (0.26)	-0.25 (0.71)	0.06 (0.72)	0.06 (0.70)	-671 (0.59)	0.00 (0.83)
Amount of average income greater than earnings and transfers (\$000s)	-0.01 (0.52)	-0.01 (0.36)	0.03 (0.63)	-0.01 (0.64)	0.01 (0.58)	729 (0.03)	-0.01 (0.27)
Years in two-parent family	0.01 (0.56)	-0.01 (0.20)	-0.31 (0.65)	0.05 (0.72)	0.03 (0.87)	-69 (0.86)	0.01 (0.45)
Mean number of children in family	0.01 (0.01)	0.00 (0.63)	-0.10 (0.19)	0.00 (0.93)	-0.02 (0.14)	-107 (0.74)	0.01 (0.09)
Years in a city with a population of 100,000 to 500,000	0.00 (0.77)	0.00 (0.77)	0.04 (0.70)	-0.01 (0.56)	0.00 (0.98)	54 (0.78)	0.01 (0.19)
Years in a city with a population of more than 500,000	-0.01 (0.41)	0.01 (0.14)	0.13 (0.62)	-0.02 (0.68)	0.00 (0.94)	99 (0.73)	0.00 (0.82)
Educational attainment of household head							
No high school vs. some high school	0.04 (0.11)	-0.02 (0.44)	-0.46 (0.22)	-0.05 (0.59)	-0.01 (0.89)	-786 (0.54)	0.03 (0.20)
High school graduate vs. some high school	-0.02 (0.13)	-0.03 (0.07)	0.17 (0.68)	0.08 (0.30)	0.15 (0.12)	1,934 (0.08)	-0.03 (0.07)
Some college vs. some high school	0.01 (0.54)	-0.03 (0.09)	0.15 (0.82)	0.12 (0.37)	0.21 (0.18)	1,465 (0.16)	0.00 (0.94)
College graduate vs. some high school	-0.03 (0.16)	-0.03 (0.15)	0.71 (0.11)	0.13 (0.10)	0.36 (0.00)	2,240 (0.07)	-0.03 (0.19)
Age educational attainment measured			0.50 (0.00)	0.08 (0.00)	0.09 (0.00)		
Number of observations used in estimation	867	1,587	3,116	3,110	3,103	1,699	1,963

Sources: Homeownership and family data from Panel Study of Income Dynamics (University of Michigan Institute for Social Research 1995, 1997, 1998, 2000). Instrument data from Bell and McGuire (1997); Joint Center for Housing Studies of Harvard University (1989, 1994); and U.S. Bureau of the Census (2002).

Note: *p* values are in parentheses. Results for state and birth year dummies are not shown. All monetary values are expressed in 1997 dollars. Least squares estimates are shown for years of education and earnings, and probit estimates are shown for the other outcomes. Probit estimates have been transformed to show the effect of a year of homeownership on change in probability of outcome with all control variables set to their means (i.e., marginal effects).

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