Homeownership for the Poor in Distressed Neighborhoods: Does This Make Sense?

Joseph Harkness and Sandra J. Newman
Johns Hopkins University

Abstract

Several recent studies have found that homeownership has positive effects on children's development. This article extends these studies by testing whether these effects depend on neighborhood conditions. This extension is important because many low-income families that become homeowners under current policies promoting homeownership for the poor are likely to purchase homes in troubled or distressed neighborhoods.

Homeownership in almost any neighborhood is found to benefit children, while neighborhood effects are weak. This suggests that the children of most low-income renters would be better served by programs that help their families become homeowners in their current neighborhoods instead of helping them move to better neighborhoods while remaining renters. However, the positive effects of homeownership on children are weakened in distressed neighborhoods, especially those that are residentially unstable and poor. Thus, helping low-income families purchase homes in good neighborhoods is likely to have the best effects on children.

Keywords: Homeownership; Low-income housing; Neighborhood

Introduction

A recent press release from the U.S. Department of Housing and Urban Development (HUD) captures the wide-ranging benefits increasingly being attributed to homeownership:

> Homeowners accumulate wealth as the investment in their homes grows, enjoy better living conditions, are often more involved in their communities, and have children who tend on average to do better in school and are less likely to become involved with crime. Communities benefit from real estate taxes homeowners pay, and from stable neighborhoods homeowners create. (2000, 1)

This credo undergirds the past decade's push to extend homeownership to all Americans, particularly low-income families and racial minorities. Because it is believed to strengthen not only families but communities, homeownership is being promoted as an important strategy for regenerating distressed urban neighborhoods.

Enormous amounts of money, both public and private, are being invested in increasing the homeownership rate. From the $2 trillion "American Dream Commitment" of Fannie Mae to the multimillion dollar homeownership programs of the Enterprise Foundation, Local Initiatives Support Corporation, and Neighborhood Reinvestment Corporation to the millions of dollars of programs and incentives under HUD's control, a consistent view of homeownership as a "silver bullet" has emerged. Incentives for homeownership even appear in the welfare reform plans of a number of states.

Despite this significant investment, remarkably little is known about the real effects of homeownership on homeowners, their children, or their communities. This article focuses on one aspect of homeownership: its potential long-term effects on children. Several recent studies have found that growing up in a homeowning family exerts a positive effect on children's development and outcomes (Aaronson 2000; Boehm and Schlottman 1999; Green and White 1997; Haurin, Parcel, and Haurin 2000). But what accounts for this positive effect and whether other features may either strengthen or weaken it are unclear. One such feature is the neighborhood. Since many families that will become new homeowners under current policies promoting homeownership for the poor will purchase homes in areas traditionally thought of as troubled or distressed, it is important to understand whether neighborhood conditions play a role in the effects of homeownership on children's outcomes.

To our knowledge, only Aaronson (2000) has explored this link. He finds that parental homeownership in low-income census tracts has a more positive effect on high school graduation than it does in high-income census tracts. This intriguing result suggests that homeownership may buffer children against the damaging effects of growing up in distressed neighborhoods. But Aaronson (2000) also finds that neighborhood residential stability enhances the positive effects of homeownership on high school graduation, which suggests that at least some of the positive effects of homeownership found in other studies may be attributed to the greater residential stability of the neighborhoods where homeowners live.

Very different policy recommendations emerge from these two results. According to the first, homeownership should be promoted, even—or especially—in very low income neighborhoods. According to the second, neighborhoods that are residentially stable are preferred, but efforts to stabilize distressed neighborhoods by encouraging low-income families to purchase homes there may carry significant risks for the "pioneers," the first homeowners in a distressed area.

Another critical neighborhood feature may be the homeownership rate, which has been largely ignored in the sizable and growing body of research on the effects of distressed neighborhoods on the life chances of children (see reviews by Ellen and Turner 1997; Gephart 1997; Haveman and Wolfe 1995; Jencks and Mayer 1990; Moffitt 1999).[1] But if the silver bullet view of homeownership's benefiting not only the homeowning family but also the surrounding community is correct, then the positive effects of homeownership on children's outcomes may be attributed to the tendency for homeowning families to live in neighborhoods of homeowners, and not to the family's homeownership status per se.

This scenario also raises important policy concerns. As with neighborhood residential stability, if the homeownership rate in a neighborhood is responsible for the improved outcomes of children who live there, then policies encouraging poor families to purchase homes in areas where there are few homeowners may be good for the neighborhood but bad for the individual family. Since moving a neighborhood from a low to a high rate of homeownership is likely to be a long-term process, the early pioneer homeowners would derive little or no benefit and in fact may bear considerable costs, such as low property values, high crime rates, poor schools, and, perhaps most important, the inability to easily move elsewhere (i.e., selling a home is much more difficult than breaking a lease).

This exploratory article examines whether homeownership has positive effects on low-income children, whether these effects can be attributed to differences in neighborhood conditions of homeowners compared with renters, and whether these effects vary depending on the neighborhood. The next section reviews theories of the ways in which homeownership could benefit children and how these benefits could be modified by neighborhood conditions. We then describe the data and methods used in the analysis and explain the results. This is followed by a discussion of the findings and their policy implications.

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[1] Distressed neighborhoods are typically defined as those with high rates of poverty, unemployment, and dependence on public assistance, though researchers differ in their specific operationalizations. Some analysts use an index of factors (e.g., the Ricketts-Sawhill definition of underclass neighborhoods) or factor analysis scores (e.g., the papers collected in Brooks-Gunn, Duncan, and Aber 1997). Others rely primarily on the poverty rate, though the cutoff point for "distress" varies from 20 percent (used by the U.S. Bureau of the Census to define poverty areas) up to 40 percent. These different definitions are substantively quite similar, because the factors that characterize distressed neighborhoods are highly interrelated. Most researchers rely on census tracts as proxies for neighborhoods.

Background

There are three broad sets of explanations for the effects of homeownership on children's outcomes. According to the first, there is a direct link between family homeownership and children's outcomes. By contrast, the second set posits that differences in neighborhoods, not family homeownership, explain why homeowners' children have better outcomes. The third set speculates that neither homeownership nor neighborhoods, by themselves, are the key explanatory factors, but rather that homeownership is associated with more favorable outcomes *only under certain neighborhood conditions*. We refer to these as direct, indirect, and interactive homeownership effects, respectively.

Direct homeownership effects

The literature suggests four paths through which parental homeownership could affect children's outcomes: (1) parenting practices, (2) the physical environment, (3) residential mobility, and (4) wealth.

Haurin, Parcel, and Haurin (2000) find that homeowning parents provide a more stimulating and emotionally supportive environment for their children, one that significantly improves cognitive ability and reduces behavioral problems. They attribute the improved parenting of homeowners either to their greater investment in their properties or to residential stability, both of which are explored below. Another explanation, supported by some empirical evidence, is that homeownership produces greater life satisfaction or self-esteem for adults, which in turn provides a more positive home environment for children (Balfour and Smith 1996; Rohe and Basolo 1997; Rohe and Stegman 1994; Rossi and Weber 1996). Sherraden (1991) argues that for adults, the psychological benefits of homeownership derive from its function as an asset. Green and White (1997) offer several wide-ranging hypotheses of the potential links between homeownership and children's outcomes, including the possibility that experience with contractors and repair personnel may improve homeowning parents' interpersonal and management skills, which may transfer to their children.

Except for gross, health-threatening inadequacies, little is known about how children are affected by housing conditions.[2] But it is plausible

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[2] See Sandel et al. (1998) for a discussion of health-threatening conditions in substandard housing. We are aware of only one study that investigates the effects of milder forms of physical deprivation on children's development. Using the National Longitudinal Survey of Youth (NLSY)—Child dataset, Mayer (1997) constructed a "housing

that the physical features of owned versus rental housing may also affect children's development. More than four-fifths of owned homes are single-family, detached structures, compared with less than one-fourth of rental properties.[3] These environments may be better for children because, for example, they are likely to be more spacious and private. Owned homes are also likely to be in better physical condition because owner-occupants are more likely to invest in the quality of their dwellings (Galster 1987; Mayer 1981; Spivack 1991).

Several studies demonstrate that moving can harm children's educational outcomes (Astone and McLanahan 1994; Hanushek, Kain, and Rivkin 1999; Haveman, Wolfe, and Spaulding 1991; Jordan, Lara, and McPartland 1996), and there is substantial evidence that homeowners move far less often than renters do (Hanushek and Quigley 1978; Lee, Oropesa, and Kanan 1994; Newman and Duncan 1979; Quigley and Weinberg 1977). Included here are recent studies that detect a causal, not merely correlational, effect of homeownership on the reduced likelihood of moving (Ioannides and Kan 1996; Kan 2000). Aaronson (2000) investigates this issue and finds that much of the positive effect of homeownership on childhood outcomes can be attributed to its impact on residential stability.

Home equity is the most significant asset held by most U.S. families, and for many, it is their only asset. One function of assets is that they can be leveraged during times of need, which could benefit children. For example, homeowning parents can borrow money against the equity in their home to finance a child's college education. In addition, inheritable wealth constitutes a child's claim on the future, enabling long-term planning and higher expectations (Conley 1999). Empirical evidence suggests a link between home value or equity and favorable youth outcomes, such as the likelihood of acquiring a college education (Aaronson 2000; Boehm and Schlottman 1999; Conley 1999). However, these estimates could be biased upward because they are likely to be picking up at least some of the impact of neighborhood characteristics, which are not controlled for in these studies.

environment" index, based on whether the interviewer observed the respondent's home to be "dark and perceptually monotonous," "minimally cluttered," and "reasonably clean." She found that this index had almost no effect on young children's cognitive test scores or behavioral problems.

[3] Data tabulated from the 1999 American Housing Survey (U.S. Bureau of the Census 1999).

Indirect homeownership effects

A second perspective is that the findings of previous studies on the benefits of homeownership are spurious because it is the better neighborhoods and schools experienced by the children of homeowners, not growing up in an owned home, that account for their better outcomes.[4] Because homeowners generally live in communities characterized by higher incomes, higher rates of homeownership, and greater residential stability, their children will benefit from these positive neighborhood externalities.

Homeownership may generate positive neighborhood externalities through its effect on either physical or social capital. As noted, owner-occupied houses appear to be better maintained than rental properties (Galster 1987; Mayer 1981; Spivack 1991), providing one form of neighborhood amenity that may benefit children. But theory also suggests that because homeowners' financial stake in their properties is illiquid and not easily extracted, homeowners will be more active in maintaining or improving the quality of their neighborhoods, not just their own houses.

A substantial body of research suggests that homeowners are more attached to their communities and more active in community affairs than renters are (Austin and Baba 1990; Blum and Kingston 1984; DiPasquale and Glaeser 1999; Rossi and Weber 1996). Greater community involvement could plausibly lead to greater community social capital. Sampson, Raudenbusch, and Earls (1997) provide strong evidence to support this link by showing that homeownership, in conjunction with residential stability, generates social capital in the form of "collective efficacy," which may produce better outcomes for children.

However, residential stability has also been shown to be an important determinant of community involvement (Kasarda and Janowitz 1974; Sampson 1988). A question raised by this body of evidence is whether homeownership itself—or the residential stability it is correlated with—is more responsible for the positive effects of homeownership on community participation. DiPasquale and Glaeser (1999) explore this issue and find that length of residence is more important across several key measures of community involvement than homeownership. Because residentially stable neighborhoods of renters may be as beneficial to children as neighborhoods of homeowners, it is critical to distinguish analytically between a neighborhood's homeownership rate and its residential stability.

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[4] The better socioeconomic features of homeowning families may be another factor explaining the improved outcomes of homeowners' children, but all previous studies control for income and other family features.

Interactive homeownership effects

Finally, a third view is that the effects of homeownership on children's outcomes vary depending on the type of neighborhood. Homeownership could buffer the effects of a distressed neighborhood if, for example, homeowning parents more aggressively monitor their children's activities, have higher expectations for their children, or have more social capital to draw on. But the child-rearing practices of homeowners living in more prosperous neighborhoods may differ little from those of neighboring renters. This buffering hypothesis is consistent with Aaronson's (2000) finding that growing up in a homeowning family in a low-income neighborhood has a stronger positive effect on the probability of graduating from high school than homeownership in a high-income neighborhood.

Alternatively, homeowners' children might be more, not less, affected by neighborhhood conditions than renters' children because of homeowners' relatively greater residential stability. Greater residential stability reduces or eliminates the need to change schools and increases the opportunity to develop closer ties to neighbors. As a result, neighborhood characteristics—both good and bad—could exert a particularly strong influence.[5] Aaronson's (2000) finding that homeownership has more positive effects on high school graduation in residentially stable neighborhoods is consistent with this speculation.

Data and methods

This study extends and refines previous work on the effects of homeownership on children's outcomes in several ways. The main focus of earlier investigations has been the effects on educational attainment (Aaronson 2000; Boehm and Schlottman 1999; Green and White 1997).[6] We extend the set of outcomes to include teen unwed births, idleness, wage rates, and welfare receipt. Examining multiple outcomes is important because the effects of homeownership on educational outcomes might be different from its effects on other adult outcomes. Children of homeowners may attend higher-quality schools than

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[5] This speculation follows from the collective socialization and epidemic models of neighborhood effects (Jencks and Mayer 1990).

[6] Green and White (1997) also examine the effect of homeownership on teen unwed childbearing in one of the three data sets they consider. Boehm and Schlottman (1999) simulate the indirect effect of homeownership on lifetime earnings via its impact on educational attainment, and they also test whether the children of homeowners are more likely to become homeowners themselves.

children of renters, for example, so that identical educational attainment by the two groups may not translate into identical earnings or welfare receipt.

Second, the analysis focuses on low-income families, defined as having parental earnings below 150 percent of the federal poverty line. Although all previous studies on the effects of homeownership have controlled for income, none has singled out low-income families for separate analysis, nor has any study tested for the interactive effects of homeownership and income. Restricting the analysis to low-income families is critical for two reasons. First, low-income families are the main focus of interest in homeownership promotion policies. Second, higher-income families could differ from lower-income families in a variety of ways that are not adequately controlled for in statistical models, potentially resulting in faulty estimates for the lower-income group.[7]

Third, we examine the effects of neighborhood conditions, both as independent factors and as factors that may change the way homeownership influences outcomes. Since homeowners and renters may live in very different kinds of neighborhoods, and children's outcomes may be affected by them, the failure to control for them could produce estimates that mistakenly attribute neighborhood effects to homeownership.[8]

We test for the simultaneous effects of three measures of neighborhood conditions: the poverty rate, the homeownership rate, and residential stability. We include the poverty rate because we are interested in the effects of homeownership in distressed neighborhoods on children's outcomes, and the poverty rate is a widely used indicator of neighborhood distress. The neighborhood poverty rate is also almost perfectly (negatively) correlated with neighborhood median income, which ensures comparability with Aaronson's (2000) results. We include the

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[7] We tested whether the income restriction was necessary to derive accurate estimates on the low-income population by performing Chow tests with preliminary models on the unrestricted sample. These tests indicated strong structural differences between model estimates for high- and low-income families and rejected pooling. In another paper (Harkness and Newman 2001), we provide a thorough exploration of differences in the effects of homeownership on outcomes of children from high- and low-income families.

[8] Green and White (1997) and Haurin, Parcel, and Haurin (2000) include some rough proxies for neighborhood conditions in their models but acknowledge weaknesses in these proxies. Aaronson (2000) examines the interactive effects of homeownership by retesting models on samples split by residence in high- versus low-income neighborhoods and in high- versus low-stability neighborhoods. But this technique could produce misleading results if the difference in neighborhood conditions experienced by homeowners and renters were unequally distributed in the split samples.

homeownership rate to distinguish between the effects of homeownership by a child's parents and the homeownership level of the neighborhood. Finally, we control for neighborhood residential stability because a neighborhood's homeownership rate is plausibly linked to residential stability (Rohe and Stewart 1996), and we want to determine whether it is homeownership or stability that is responsible for neighborhood effects on children's outcomes.

Sample

The analysis uses data from the 1968–93 waves of the geocoded Panel Study of Income Dynamics (PSID; University of Michigan Survey Research Center 1968–93). Begun in 1968, the 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, the PSID remains representative of the population (Fitzgerald, Gottschalk, and Moffitt 1998; Zabel 1998).

The analysis is performed on a sample of children born between 1957 and 1973, with PSID family data available for each year between the ages of 11 and 15, and from low-income families, defined as having parental earnings below 150 percent of the federal poverty threshold for at least three of the five years between 11 and 15.[9]

The sample is also restricted to children whose parents were either always homeowners or always renters when the child was between 11 and 15, which eliminates about 20 percent of the cases. This restriction enables us to derive meaningful coefficients on the effects of homeownership while testing interactions between tenure status and neighborhood conditions (see appendix A for a further discussion of the methodology).

Approach

We examine the effects of living in an owned home as a child on seven outcomes: (1) giving birth as a unmarried teenager (women only); (2) idleness (not working, attending school, or caring for children) at

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[9] We also experimented with defining low income as having parental earnings below the regional median for at least two-thirds of observed years, using the four census-defined regions. This definition has the advantage of providing a more geographically balanced sample, but it does not adjust for family size as the poverty formula does. In any case, the two definitions produce almost identical results.

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) average hourly wage rates between ages 24 and 28; and (7) receipt of welfare (Aid to Families with Dependent Children [AFDC], food stamps, or other cash assistance) between ages 24 and 28.[10]

We estimate three sets of models, corresponding to the three broad conceptualizations of homeownership effects outlined earlier. The first set of models tests for the direct effects of homeownership on children's outcomes without controls for neighborhood features. Next, we test for indirect effects by adding controls for average neighborhood conditions experienced between ages 11 and 15. If neighborhood differences between homeowners and renters account for a substantial portion of the beneficial effects of homeownership, the homeownership effect estimates produced by these models should be much smaller than those produced by the direct effect models. The third set of models tests for the interaction between tenure status and each of the three neighborhood conditions (stability, homeownership rate, and poverty rate).

The analysis uses ordinary least squares (OLS) to estimate the effect of homeownership on years of education and wage rates. The models for the effects of homeownership on high school completion, acquisition of postsecondary education, idleness, and welfare receipt, which are binary (that is, whether completed high school or not), use probit.[11]

A major difficulty in identifying the effects of homeownership and neighborhoods on children is that they may be associated with parental characteristics that are not measured in the data and that therefore cannot be controlled for in statistical models. For example, parents who strongly value a high-quality education for their children may be more diligent about saving for a home in a higher-priced neighborhood with a good school. Such education-oriented parents could also be more likely to ensure that their children succeed in school by engaging in other activities, such as helping them with homework, providing them with instructive books and games, or taking them on educational outings. Without data on these parental characteristics, it is impossible to analytically distinguish neighborhood effects from parental effects.

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[10] Two other outcomes: whether any, and number of, hours employed between ages 24 and 28, were also tested and found to be unaffected by parental homeownership. Results on these two outcomes are not reported below. Hourly wage rates were constructed by dividing total earnings by work hours. Six outliers with calculated wage rates of more than $40 an hour and less than 300 average annual hours of work were excluded from the wage rate model.

[11] Huber-White standard errors are used because the data include siblings, which may not be independent.

The standard technique for dealing with such unmeasured variable problems is to use "instruments," variables that are correlated with key analytical variables (here, homeownership and neighborhood conditions), but are independent of the unmeasured characteristics. However, while finding plausible instruments for homeownership is possible and has been done in other studies (Aaronson 2000; Green and White 1997; Harkness and Newman 2001; Haurin, Parcel, and Haurin 2000), it is difficult to identify credible instruments for the three neighborhood indicators tested here (Duncan, Connell, and Klebanov 1997; Duncan and Raudenbusch 1998; Moffitt 1999). Because this article focuses on homeownership *and* neighborhoods, results based on instrumenting for homeownership alone would not be interpretable.

Results produced using uninstrumented models may be acceptable in the present analysis, however, because the estimates for the effects of homeownership and neighborhoods are both likely to have roughly the same upward bias; that is, families with unobserved features that foster better outcomes for their children are likely to self-select into both homeownership and better neighborhoods. This expectation is confirmed in instrumental variable estimates for the effects of homeownership (Aaronson 2000; Green and White 1997; Haurin, Parcel, and Haurin 2000) and in sibling difference analyses and other attempts to gauge the extent of bias associated with estimates of neighborhood effects (Aaronson 1997; Duncan, Connell, and Klebanov 1997). If, as seems plausible, the key policy variables have roughly the same bias, the conclusions drawn from the uninstrumented results will be qualitatively correct, although the point estimates may be overstated. For example, as long as biases are similar, if the direct homeownership effect estimates are significantly diminished when neighborhood controls are added to the indirect effect models, it is safe to conclude that much of the homeownership effect can be attributed to neighborhood conditions. Likewise, interaction model results indicating that homeownership has stronger beneficial effects in lower-income neighborhoods would support the buffering hypothesis, although the size of the estimating buffering effect could be too large.

A similar argument applies if the estimates for homeownership and neighborhoods were both biased downward. Here, the uninstrumented models would produce lower-bound estimates, but again, the conclusions would be qualitatively correct. The only problematic case would occur if homeownership and neighborhood estimates were biased in opposite directions, but it is difficult to conceive of an unmeasured family characteristic with a strong impact on children's outcomes that could result in a greater tendency to self-select into both homeownership and worse, not better, neighborhoods.

Policy variables

The measure of homeownership is whether a child always lived in an owned home between 11 and 15. Three neighborhood features are included: the poverty rate, the percentage of families owning their home, and residential stability, the last being measured as the percentage of families living in the same housing unit for five or more years.[12] Interactive effects between housing tenure and neighborhood are obtained by multiplying the homeownership variable with each of the neighborhood variables. In the interaction model, the neighborhood variables are specified in mean-deviation form.[13] This implies that the coefficient on homeownership in these models can be readily interpreted as the effect of homeownership in the average sample neighborhood.

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 the 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 AFDC, food stamps, or other cash assistance (excluding supplemental security income); (11) years in a city of 500,000 or more; (12) years in a city of 100,000 to 500,000; and (13) the child's primary state of residence.[14]

For educational outcomes, about 25 percent of the cases are missing data on grade completed at age 20, but have data on grade completed at some other age. In these cases, we substituted educational attainment in

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[12] Each of these measures was extracted from the PSID census geocode and averaged over observed years. Census tract–level measures were available for roughly 70 percent of the cases and ZIP code areas were available for the remainder. Direct census measures were obtained only for decennial census years. For intercensal years, we linearly interpolated between the two closest decennial censuses. For example, for 1975, we interpolated between the 1970 and 1980 census values for the tract (or ZIP code area). (Appendix B provides more detail on the construction of neighborhood measures.)

[13] That is, each neighborhood variable is transformed by subtracting off its sample mean.

[14] A variety of nonlinear specifications for several of these variables (e.g. parental earnings, maternal age when born) were tested and found to have no impact on the key results, and diagnostics for colinearity problems with these variables using the techniques of Belsley, Kuh, and Welsch (1980) revealed no such evidence.

the closest year after age 20, if available, and in the closest year before age 20 otherwise. Because educational attainment is affected by age, the models also include a control variable for the age to which the educational attainment measure applies. Monetary values are expressed in 1997 dollars using the CPI-U, the consumer price index for all urban consumers. City sizes come from the PSID census geocode.[15]

Each of these variables is plausibly related to one or more outcomes examined here, and most have been used extensively in other research on determinants of children's outcomes. The exceptions are controls for (1) wealth other than home equity and (2) city size. Based on Conley's (1999) finding that parental wealth has significant effects on children's outcomes, we control for wealth by including a measure of income that is neither earned nor obtained through public assistance.[16] We control for city size because Page and Solon (1999) demonstrated "the importance of being urban" on adult earnings. State dummy variables are included to account for the fact that unmeasured features of states, such as quality of education or labor market conditions, may affect outcomes (Moffitt 1994).

Although children's outcomes may be affected by a family's home equity and residential mobility, as described earlier, we did not include controls for these factors in the initial models because both are also likely to be affected by whether a family owns its home, as well as by neighborhood conditions. Consequently, the estimates for the effects of homeownership and neighborhoods will include the effects that operate through home equity and residential moves, and they should be interpreted accordingly. After reviewing the main results, a supplementary analysis using these excluded variables is conducted.

Sample characteristics

Table 1 shows the mean differences in outcomes, neighborhood conditions, and family background characteristics between children of homeowners and renters. The differences are stark. Relative to homeowner children, renter children are 40 percent more likely to give birth as an unmarried teenager, and they are nearly twice as likely to be idle at age 20 and to rely on welfare as an adult. Their high school graduation rate

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[15] Annual city size values were obtained by logarithmically interpolating between place size values in the two closest decennial census years.

[16] The PSID did not begin collecting detailed data on assets until 1984.

Table 1. **Sample Means for Renters and Homeowners**

	Renters (N=1,495)	Homeowners (N=1,081)	
Outcomes			
Gave birth as an unwed teen (women only)	0.14	0.10	*
Idle at age 20	0.25	0.14	***
Years of education at age 20	11.3	12.0	***
Graduated from high school by age 20	0.57	0.70	***
Obtained some postsecondary education by age 20	0.12	0.23	***
Average hourly wage at ages 24 to 28	$9.16	$10.35	
Received any welfare at ages 24 to 28	0.34	0.18	***
Neighborhood conditions			
Mean neighborhood poverty rate	23.9	17.9	***
Mean neighborhood homeownership rate	56.0	72.2	***
Mean neighborhood percentage that had not moved in 5+ years	56.7	58.0	***
Individual and family background features			
Female	0.52	0.52	
Black	0.44	0.21	***
Year born	1966	1966	*
Mother's age when born	25.2	26.8	***
Whether income > earnings + transfers	0.55	0.81	***
Parental earnings	$11,080	$20,920	***
Mean amount family income > earnings + transfers	$2,380	$8,070	***
Years in a two-parent family	2.25	3.65	***
Mean number of children in the family	3.64	3.45	
Years receiving AFDC, food stamps, or "other" cash welfare	0.62	0.22	***
Household head graduated from high school	0.36	0.49	
Household head had some postsecondary education	0.18	0.30	**
Fraction of years in a city of between 100,000 and 500,000	1.12	0.73	***
Years in a city > 500,000	1.31	0.53	***

Source: Panel Study of Income Dynamics 1968–93.
Notes: Monetary figures are expressed in 1997 dollars. Statistical significance indicators refer to one-tailed *t*-test results for differences in means; unequal variances are assumed. Values are weighted using age 15 PSID individual weights.
* $p < 0.05$. ** $p < 0.01$. *** $p < 0.0001$.

is 19 percent lower than that of homeowner children, they are only half as likely to acquire some postsecondary education, and their average hourly wage lags behind homeowners by more than a dollar. These differences are all statistically significant.

Differences in the family backgrounds of renters' and homeowners' children are also dramatic. The parental income of renters' children is half that of homeowners' children, and the former are twice as likely to grow up in a single-parent household or be on welfare. These children experience an average neighborhood poverty rate of 24 percent, compared with 18 percent for homeowners' children, and a substantially lower neighborhood homeownership rate (56 percent versus 72 percent, respectively). Surprisingly, there is little difference in the residential stability. In renter neighborhoods, 57 percent of families had lived in the same residence for five years or more, compared with 58 percent in homeowner neighborhoods. The neighborhood poverty and homeownership rates experienced by the sample children are somewhat correlated ($r = -0.45$), but the correlation between neighborhood residential stability and homeownership rates is surprisingly weak ($r = 0.25$), as is the correlation between residential stability and poverty rates ($r = 0.11$).[17]

Regression results

Models without neighborhood/tenure interactions

Table 2 presents estimates for the policy variables obtained from direct and indirect effects models (i.e., without and with controls for neighborhood features).[18] In the direct effects models, homeownership is estimated to have statistically significant benefits for all outcomes except for teen unwed childbearing, where homeownership has a favorable but not a significant effect. The inclusion of neighborhood controls has modest impacts on some estimates, but overall, there is little effect. Even with neighborhood controls, homeownership has strong, favorable effects on most outcomes. Thus, the beneficial effects of homeownership on children's long-term outcomes appear to be only marginally attributable, if at all, to the better neighborhood conditions experienced by the children of homeowners. The estimates for educational outcomes and

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[17] Diagnostics revealed no colinearity problems with these neighborhood variables and the other control variables.

[18] Table A.1 presents estimates for the control variables from the indirect effects model. Control variable estimates from other models are similar (they are available from the authors). To aid interpretation, coefficients from the probit estimates in all tables have been transformed to indicate marginal effects with all independent variables set to their means. Essentially, they can be interpreted in a way similar to OLS estimates, as the average percentage point effect of a variable on outcomes. Table A.2, which presents transformed probit estimates from the indirect effects model with control variables set to values other than their means, demonstrates that effects shown in table 2 are largely unaffected by the alternatives. (Untransformed probit coefficients are also available from the authors.)

Table 2. **Effects of Homeownership on Early Adult Outcomes (*p* Values in Parentheses)**

	Age 20 Outcomes					Age 24 to 28 Outcomes	
	Teen Unwed Birth (Probit)	Idle (Probit)	Years of Schooling (OLS)	High School Graduate (Probit)	Any Postsecondary (Probit)	Wage Rate (OLS)	Received Welfare (Probit)
Direct effects: No controls for neighborhood							
Homeowner family, ages 11 to 15	−0.030 (0.285)	−0.066 (0.038)	0.417 (0.000)	0.131 (0.000)	0.058 (0.002)	0.698 (0.018)	−0.091 (0.009)
Indirect effects: With controls for neighborhood features							
Homeowner family, ages 11 to 15	−0.037 (0.198)	−0.045 (0.153)	0.039 (0.000)	0.124 (0.000)	0.052 (0.006)	0.514 (0.090)	−0.095 (0.008)
Neighborhood poverty rate	−0.002 (0.878)	0.005 (0.715)	−0.048 (0.164)	−0.016 (0.176)	−0.007 (0.365)	−0.172 (0.133)	0.023 (0.072)
Neighborhood homeownership rate	0.010 (0.324)	−0.017 (0.145)	−0.003 (0.913)	−0.005 (0.672)	0.000 (0.960)	0.072 (0.525)	0.016 (0.191)
Neighborhood percentage staying 5+ years	−0.025 (0.035)	−0.005 (0.737)	0.040 (0.254)	0.020 (0.122)	0.012 (0.115)	0.227 (0.098)	−0.006 (0.664)
Joint significance of neighborhood features (*p* value of *F* test)	0.151	0.238	0.327	0.298	0.300	0.106	0.295
N	844	1,364	2,404	2,397	2,391	1,240	1,902

welfare receipt are particularly strong. In the direct effects models, children of homeowners are estimated to complete almost half a year more of education and to have a 13 percentage point higher high school graduation rate, a 6 percentage point greater likelihood of acquiring postsecondary education, and a 9 percentage point lower chance of receiving welfare between ages 24 and 28. All of these estimates are highly statistically significant ($p \leq 0.01$), and they decline only slightly, if at all, when controls for neighborhood features are added.

The estimated effects of homeownership on children's subsequent idleness and wage rates are also favorable, but somewhat less impressive. In the direct effects models, idleness at age 20 among the children of homeowners is reduced by 7 percentage points, and their average wage rates between ages 24 and 28 increases by $0.70 relative to the children of renters. Both of these estimates are statistically significant ($p < 0.05$), but when controls for neighborhood features are added, they decline by about 30 percent and are of only moderate statistical significance ($p = 0.15$ for idleness and $p = 0.09$ for wage rates). The estimate for the effect of homeownership on teen out-of-wedlock childbearing is also favorable but weak in the direct effect estimate ($p = 0.29$).

The smaller samples used to estimate homeownership effects on idleness, wage rates, and teen unwed childbearing partially explain the weaker results for these outcomes.[19] There may also be greater measurement error for these outcomes, which could produce a downward bias, compared with education or welfare receipt.[20] Thus, it would be hazardous to conclude that the effects of homeownership on education and welfare receipt are, in reality, stronger than for the other outcomes examined. Instead, homeownership appears to be associated with positive effects across-the-board, although these effects are statistically

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[19] The smaller sample for teen unwed births is attributable to missing data and the restriction of the sample to women. A substantial portion of the data needed to construct the idleness measure is also missing. The sample used for the wage rate model is smaller because there are fewer cohorts with data for ages 24 to 28, when wage rates were measured, and also because it is restricted to cases with nonzero work hours. (Six cases with less than 300 annual average work hours and wage rates exceeding $40 per hour were also excluded from the wage rate sample.)

[20] An individual's average wage rate between ages 24 and 28 is likely to be difficult to measure accurately because earnings and work hours (from which we constructed the wage rate variable) can be quite volatile from month to month (Duncan 1988), and it may be difficult for individuals to accurately recall their wage rates when surveyed annually (as in the PSID). The variables for teen unwed childbearing and idleness were also constructed from other, more basic variables in the PSID, which could introduce measurement error as well.

significant at conventional levels only for outcomes that are precisely measured and tested using the largest samples.

The estimated effects of neighborhood conditions are weak.[21] Only in the model for wage rates do they jointly attain a moderate level of statistical significance ($p = 0.11$). The estimated effects of neighborhood residential stability and poverty, but not the homeownership rate, have the expected sign for virtually all outcomes. Neighborhood residential stability exhibits the strongest effects, with a statistically significant impact on reduced teen out-of-wedlock childbearing ($p < 0.05$) and modestly significant positive effects on high school graduation, acquisition of postsecondary education, and wage rates ($p < 0.13$). Neighborhood poverty is a weaker determinant of long-term outcomes, with a moderate effect on increased probability of welfare receipt ($p < 0.10$) and some weak deleterious effects on other outcomes. Estimates for the effects of neighborhood homeownership are inconsistent and weak. For four of the seven outcomes, it has an unexpected sign suggesting deleterious effects, and it is not statistically significant for any outcome. Contrary to expectations, these results indicate that there are no spillover benefits of homeownership to the neighborhood beyond the immediate homeowning family. Instead, they suggest that residential stability may foster a neighborhood's social capital, with beneficial effects on children.[22]

The finding that the beneficial effects of homeownership cannot be attributed to the better neighborhood conditions of homeowners may be surprising. It arises because residential stability, the neighborhood characteristic that matters most for children's outcomes, is nearly identical for homeowners and renters in this sample, as shown in table 1. Differences in the neighborhood poverty rate, which also appears to affect outcomes, are fairly modest as well, at 6 percentage points on average. Only the neighborhood homeownership rate differs substantially between owner and renter families, but this feature has virtually no effect on childrens' outcomes. Thus, on the dimensions that matter most for this analysis, the neighborhood conditions of owner and renter families are very similar, and they differ substantially only on the dimension that matters least, at least in this sample.

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[21] For expository purposes, the coefficients on the neighborhood variables are scaled to represent the effect of a 10 percentage point change.

[22] It may be that by fostering greater residential stability, homeownership *could* play an indirect role in creating neighborhood conditions beneficial to children's development. This role appears to be weak, however. In supplementary models that exclude neighborhood residential stability, the estimated effects of neighborhood homeownership are only slightly more favorable than those shown in table 2.

Models with tenure/neighborhood interactions

Table 3 shows the results for models testing the interaction of tenure status and neighborhoods.[23] The indirect effects models imposed the assumption that neighborhood conditions have identical effects on children of homeowners and renters. In the present results, this assumption is relaxed; that is, in the interaction models, the effects of homeownership are allowed to depend on neighborhood characteristics.

The key result of these models is that homeownership does not buffer children against the deleterious effects of bad neighborhoods. If anything, the pattern of results points in the opposite direction—toward an amplification effect. Homeowners' children appear to be more adversely affected by neighborhood poverty than renters' children and to benefit more from neighborhood homeownership and residential stability. Effects of neighborhood residential stability, in particular, appear to be better for children of homeowners than for children of renters.

The first row of coefficients in table 3 shows that in a neighborhood with average sample characteristics (27 percent poverty, 59 percent homeownership, and 57 percent residential stability), the estimated effects of homeownership are nearly the same as in the direct and indirect effects models. Subsequent rows in the table show how these average effects are modified by neighborhood conditions. For example, the coefficient on homeownership (first row) in the wage rate model is $0.397. A 10 percentage point increase in the poverty rate of the neighborhood where the child lived between ages 11 and 15 is estimated to reduce the early adult wage rate of homeowner children by $0.322 and of renter children by $0.102, with a net difference of $0.22. Thus, homeownership in a neighborhood with a 37 percent poverty rate, rather than the sample mean of 27 percent, would raise a child's early adult wage rate by $0.177 ($0.397 minus $0.22) rather than $0.397.

Comparing coefficients in this way indicates that neighborhood poverty generally has worse effects on the outcomes of homeowners' children than on renters' children, and neighborhood homeownership and residential stability generally have better effects. But none of the differences between the estimated effects of neighborhoods on children of homeowners and renters are highly statistically significant. In the strongest case, a 10 percentage point increase in neighborhood residential stability is associated with a statistically significant $0.43 increase in the wage rate of homeowners' children ($p < 0.05$), but it has no effect

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[23] In these results, all interactions were tested simultaneously, not in separate models or entered in the same model sequentially.

Table 3. **Results of Housing Tenure/Neighborhood Interaction Models (*p* Values in Parentheses)**

	Age 20 Outcomes					Age 24 to 28 Outcomes	
	Teen Unwed Birth (Probit)	Idle (Probit)	Years of Schooling (OLS)	High School Graduate (Probit)	Any Postsecondary (Probit)	Wage Rate (OLS)	Received Welfare (Probit)
Homeowner family, ages 11 to 15	−0.041 (0.190)	−0.041 (0.210)	0.369 (0.000)	0.11 (0.001)	0.045 (0.023)	0.397 (0.209)	−0.086 (0.022)
Neighborhood poverty rate							
Homeowners	−0.002 (0.926)	−0.003 (0.895)	−0.092 (0.111)	−0.034 (0.072)	−0.015 (0.192)	−0.322 (0.051)	0.019 (0.352)
Renters	−0.004 (0.761)	0.010 (0.492)	−0.026 (0.502)	−0.010 (0.466)	−0.003 (0.734)	−0.102 (0.478)	0.027 (0.068)
Neighborhood homeownership rate							
Homeowners	0.014 (0.345)	−0.024 (0.190)	−0.006 (0.893)	0.001 (0.942)	0.003 (0.787)	0.094 (0.564)	0.002 (0.929)
Renters	0.008 (0.517)	−0.011 (0.365)	−0.001 (0.973)	−0.008 (0.548)	−0.003 (0.761)	0.038 (0.770)	0.023 (−0.085)
Neighborhood stability							
Homeowners	−0.041 (0.011)	−0.013 (0.574)	0.065 (0.239)	0.037 (0.052)	0.019 (0.052)	0.431 (0.012)	−0.001 (0.952)
Renters	−0.010 (0.545)	0.001 (0.971)	0.023 (0.594)	0.009 (0.596)	0.005 (0.652)	0.039 (0.835)	−0.007 (0.678)
Tests for equality of neighborhood coefficients							
Poverty rate	0.886	0.605	0.303	0.274	0.355	0.277	0.729
Homeownership rate	0.742	0.521	0.925	0.626	0.647	0.759	0.303
Stability rate	0.155	0.619	0.517	0.227	0.274	0.097	0.816

Source: PSID 1968–93.
Notes: In all probit estimates, the coefficient is transformed to indicate marginal effects with all independent variables set to their means. Monetary figures are expressed in 1997 dollars. Huber-White standard errors are used to account for the nonindependence of sibling observations. Neighborhood coefficients show the effects of a 10 percentage point change in neighborhood conditions.

on the wage rates of renters' children. However, the difference between these two estimates is statistically significant at only a moderate level ($p = 0.10$). In another case, the difference between homeowners' and renters' children in the impact of neighborhood residential stability on teen out-of-wedlock childbearing is modest ($p = 0.16$). None of the other differences is statistically distinguishable at even this weak level.

Despite this lack of statistical significance, however, the pattern of homeowners' children being more adversely affected by neighborhood poverty and more favorably affected by neighborhood stability and homeownership is consistent. Although the statistical evidence to support the neighborhood amplification effect of homeownership is modest, the underlying theory (that is, that homeowners' children may develop closer ties with other community members and therefore be more affected by them) is consistent with the data used here, where renters' children experienced 40 percent greater variability in neighborhood condition than homeowners' children did. If there were truly no difference in the impact of neighborhoods on children, we would expect a more random pattern of results. In addition, tests of an additive (admittedly crude) neighborhood quality index[24] reveal that on three of the seven outcomes (high school graduation, acquisition of postsecondary education, and wage rates), the difference between the estimated effects on homeowners' and renters' children is moderately significant ($p < 0.10$). On balance, these results suggest that neighborhood conditions may have different effects on children, but that these differences are weak and require further exploration.

Discussion

Policy implications

One possible implication of this analysis is that under certain adverse neighborhood conditions, homeownership could result in worse, not better, outcomes for children than renting. To gain a sense of what these conditions might be, we used the coefficients from the interaction model results to calculate the effects of homeownership if the three neighborhood conditions considered here were worsened by one standard deviation from their means, both individually and simultaneously; results are shown in table 4.[25] With one exception (the effect of reduced

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[24] This index was formed by adding together the homeownership and residential stability rates and subtracting the poverty rate.

[25] These standard deviations are 14, 20, and 11 percentage points for the poverty rate, homeownership rate, and residential stability rate, respectively.

Table 4. **Effects of Homeownership on Early Adult Outcomes under Different Neighborhood Conditions (*p* Values in Parentheses)**

	Age 20 Outcomes					Age 24 to 28 Outcomes	
	Teen Unwed Birth (Probit)	Idle (Probit)	Years of Schooling (OLS)	High School Graduate (Probit)	Any Postsecondary (Probit)	Wage Rate (OLS)	Received Welfare (Probit)
Poverty rate, increase 1 SD	-0.037 (0.374)	-0.059 (0.248)	0.273 (0.054)	0.076 (0.103)	0.027 (0.351)	0.079 (0.850)	-0.097 (0.062)
Homeownership rate, decrease 1 SD	-0.052 (0.335)	-0.015 (0.779)	0.379 (0.014)	0.092 (0.092)	0.035 (0.270)	0.284 (0.610)	-0.043 (0.485)
Residential stability, decrease 1 SD	-0.005 (0.876)	-0.026 (0.562)	0.320 (0.006)	0.078 (0.066)	0.028 (0.270)	-0.049 (0.903)	-0.092 (0.040)
Worsen all neighborhood features by 1 SD	-0.013 (0.800)	-0.018 (0.774)	0.235 (0.170)	0.026 (0.660)	0.001 (0.998)	-0.480 (0.412)	-0.061 (0.357)

Source: PSID 1968–93.
Notes: This table uses the coefficients from the interaction models (table 3) to show the estimated effects of homeownership when neighborhood measures are worsened by 1 standard deviation (SD) from their mean values, both individually and simultaneously. In all probit estimates, the coefficient is transformed to indicate marginal effects with all independent variables set to their means. Monetary figures are expressed in 1997 dollars. Huber-White standard errors are used to account for the nonindependence of sibling observations.

neighborhood residential stability on earnings), all of the estimated effects of homeownership remain favorable. For educational outcomes and welfare receipt, many of these effects remain statistically significant near conventional levels when individual neighborhood features are worsened. None remain significant when all neighborhood features are simultaneously worsened by one standard deviation, but these sorts of neighborhood conditions—a poverty rate of 42 percent, a homeownership rate of 39 percent, and only 46 percent of residents remaining in their dwellings for five years or more—roughly characterize the worst quintile of neighborhoods in the sample. It is noteworthy that even under these extremely poor neighborhood conditions, and under the assumption that homeowners' children are, in fact, more adversely affected by these conditions than renters' children, the effects of homeownership on children's outcomes tend to be positive.

Comparison with Aaronson's (2000) results

Because this article uses a different approach than Aaronson (2000) does to examine the role of neighborhood in homeownership effects, it is important to compare results. Although both analyses find that neighborhood residential stability enhances the positive effect of homeownership on children's outcomes, the findings on the effect of neighborhood poverty disagree. Aaronson (2000) finds that homeownership has a more positive effect on high school graduation in low-income neighborhoods; we find that neighborhood poverty reduces the positive effect of homeownership on high school graduation and other outcomes.[26]

When we attempt to replicate Aaronson's (2000) results using a sample unrestricted by income, our results are consistent with his: homeownership in a high-poverty neighborhood has a significantly more positive effect on high school graduation than homeownership in a low-poverty neighborhood. His result therefore appears to be attributable to the inclusion of higher-income families in his sample. In our results using the low-income sample, homeownership is estimated to increase the probability of high school graduation by about 10 percentage points, roughly equal in magnitude to the effect Aaronson (2000) finds in low-income neighborhoods. Because the families living in low-income neighborhoods in his sample probably have low incomes themselves and therefore roughly match the sample we use, our results are consistent with his. Excluded from our sample are the wealthier families that live

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[26] These findings can be compared because neighborhood poverty and income are almost perfectly negatively correlated.

in the most affluent neighborhoods, and for whom homeownership has no effect on children's high school graduation, according to his results. Thus, the difference Aaronson (2000) finds in high- versus low-income neighborhoods may, in fact, be attributable to differences in the type of families that live in such neighborhoods, not the neighborhoods themselves.

Supplementary models

Measures of home equity and the family's history of residential mobility were not included in the foregoing models because they could be affected by homeownership or neighborhood conditions, as discussed earlier. However, when supplementary models including these measures were tested, the effects of home equity were not statistically significant for any outcome except wage rates. A history of frequent residential moves was associated with the most adverse effects for outcomes, and these effects were statistically significant for all educational outcomes and for wage rates. Like Aaronson (2000), we find the positive effects of homeownership to be weaker when residential moves are added to the model, which suggests that these effects can be partially attributed to the reduced residential mobility of homeowners. But even after controlling for residential moves, homeownership continued to exhibit statistically significant favorable effects on all three educational outcomes and on reduced welfare usage ($p < 0.05$). It thus appears that the impact of homeownership on other features, not simply residential stability, needs to be examined to explain the beneficial effects of homeownership on children.

Conclusion

The key finding of this article is that homeownership improves children's outcomes in almost any neighborhood. However, because better neighborhoods are associated with better outcomes for homeowners' children, homeownership in better neighborhoods is an even stronger combination. Residentially stable neighborhoods are particularly beneficial for these children, and low neighborhood poverty also increases homeownership benefits. It is interesting to note, however, that the neighborhood homeownership rate has no effect.

Are better neighborhoods also better for renters' children? The answer appears to be no. One possible explanation is that because renter families move more often, their children do not develop close ties with others in their community and are consequently less influenced by them.

The one compensation is that distressed neighborhoods may also be less deleterious for renters' children, since they appear to be less influenced by their neighborhoods—good or bad.

These provocative findings imply that the children of most low-income renters would be better served by programs that help their families become homeowners in their current neighborhoods instead of helping them move to better neighborhoods but remain renters. The best evidence to date on the effects of neighborhoods on renters' children comes from the Moving to Opportunity (MTO) demonstration program, in which one group of families living in public housing in highly distressed neighborhoods was offered a Section 8 certificate, counseling, and assistance to help them move out of public housing and into rental housing in very low poverty neighborhoods. Another group of families was offered a Section 8 certificate, but no additional assistance, and allowed to move as they chose. This latter group generally moved to somewhat better neighborhoods than they had occupied before, but much worse than the experimental group that received assistance in moving to very low poverty neighborhoods.

The early MTO results demonstrate a variety of benefits for both groups of families moving out of public housing. But it is not yet evident whether the children whose families moved to low-poverty neighborhoods are faring much better than those whose families generally remained in fairly distressed neighborhoods. For example, Ludwig, Duncan, and Ladd (2001) report significant gains in reading scores for both Section 8 mover groups, whether they moved to a low-poverty neighborhood or not. Thus, while it seems clear that helping families move out of public housing in highly distressed neighborhoods is beneficial, the MTO research has not yet demonstrated that neighborhoods matter significantly for children of renters.[27]

The research reported here is only an initial step toward understanding the role of neighborhood conditions in the effects of homeownership on children. But the research is limited by its small sample size, and methodological issues, including the likelihood of upwardly biased estimates because of failure to control for important family characteristics, render the results extremely tenuous. Further research, preferably using an experimental design, is therefore necessary to solidly measure the relative benefits of homeowning and renting for children under a variety of neighborhood conditions.

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[27] Complete documentation of the MTO research to date can be found on the Internet at <http://www.mtoresearch.org>.

Finally, homeownership may generate broader social benefits beyond its favorable effects on children, such as a more active and informed citizenry (DiPasquale and Glaeser 1999) and more residentially stable neighborhoods. The case for greater investment in homeownership must take the full range of these potential benefits into account.

Appendix A

Discussion of sample restrictions and implications

Suppose we want to estimate how the neighborhood poverty rate differentially affects children of homeowners and renters. Some children are always homeowners between ages 11 and 15, some are always renters, and some experienced both forms of tenure. One solution might be to specify homeownership as years in a homeowning family and multiplicatively interact this variable with the average neighborhood poverty rate experienced over the period. But for those with mixed tenure, the average neighborhood poverty rate comprises the rate while renting and while owning—two quantities whose effects we want to estimate separately.

Another solution might be to separately specify the average neighborhood poverty rate experienced while owning versus while renting. The problem here is that average neighborhood poverty rate while owning (renting) is undefined for renters (owners). To correct for this problem, we can set the average neighborhood poverty rate while owning (renting) to zero for renters (owners) and introduce a dummy variable to control for the fact that this substitution has been made. But the dummy variables thus introduced also act as indicators of zero and five years of homeownership between ages 11 and 15, which means that the model estimates for the effects of homeownership rely solely on the relatively few cases with mixed tenure status over the period.

The most likely effect of eliminating children who had mixed tenure status between ages 11 and 15 from the sample would be to overestimate the favorable effects of homeownership on children's outcomes because it generally indicates better household conditions, and families that did not become homeowners until their children were 11 or older are more likely to have been worse off financially and in other ways than families that became homeowners earlier. Likewise, families that were already homeowners and became renters after their children were 11 or older are likely to be undergoing serious difficulties, such as job loss or divorce. (The question of whether homeownership is good for children in families undergoing serious stress is an important one, but

Table A.1. **Estimates for Control Variables from Models with Uninteracted Neighborhood Features ("Indirect Effects" in table 2) (*p* Values in Parentheses)**

	Teen Unwed Birth (Probit)	Idle (Probit)	Years of Schooling (OLS)	High School Graduate (Probit)	Any Postsecondary (Probit)	Wage Rate (OLS)	Received Welfare (Probit)
Female		−0.079 (0.003)	0.318 (0.000)	0.102 (0.000)	0.060 (0.000)	−1.465 (0.000)	0.111 (0.000)
Black	0.059 (0.066)	0.108 (0.001)	0.379 (0.000)	0.149 (0.000)	0.071 (0.001)	−0.504 (0.152)	0.162 (0.000)
Year born	−0.015 (0.000)	0.005 (0.158)	−0.016 (0.075)	−0.008 (0.008)	−0.004 (0.016)	−0.158 (0.000)	−0.002 (0.547)
Mother's age when born	−0.003 (0.081)	−0.002 (0.386)	−0.006 (0.338)	0.001 (0.656)	−0.001 (0.648)	0.013 (0.499)	0.005 (0.025)
Parental earnings (in $10,000)	0.000 (0.994)	−0.027 (0.114)	0.177 (0.000)	0.080 (0.000)	0.029 (0.004)	0.606 (0.000)	−0.044 (0.014)
Whether income > earnings + transfers	−0.024 (0.408)	0.030 (0.353)	−0.017 (0.836)	0.031 (0.291)	−0.001 (0.947)	−0.448 (0.155)	0.021 (0.516)
Amount income > earnings + transfers (in $10,000)	0.018 (0.388)	−0.023 (0.385)	0.076 (0.224)	0.057 (0.014)	0.026 (0.034)	0.166 (0.496)	−0.054 (0.042)
Years in a two-parent family	−0.011 (0.140)	0.006 (0.395)	−0.076 (0.001)	−0.013 (0.086)	−0.009 (0.038)	−0.298 (0.000)	0.014 (0.080)
Number of children in the family	0.009 (0.218)	0.015 (0.059)	−0.123 (0.000)	−0.050 (0.000)	−0.023 (0.000)	−0.186 (0.017)	0.010 (0.226)
Years of welfare receipt	0.039 (0.378)	0.074 (0.134)	−0.145 (0.268)	0.024 (0.588)	−0.005 (0.857)	−0.451 (0.327)	−0.006 (0.904)
Years in a city of 100,000 to 500,000	0.005 (0.481)	−0.005 (0.525)	−0.004 (0.855)	0.002 (0.844)	0.000 (0.997)	0.103 (0.221)	−0.007 (0.447)
Years in a city > 500,000	−0.002 (0.843)	−0.016 (0.086)	−0.031 (0.274)	−0.011 (0.283)	0.004 (0.539)	0.064 (0.538)	−0.010 (0.345)

Table A.1. **Estimates for Control Variables from Models with Uninteracted Neighborhood Features** *(continued)* **("Indirect Effects" in table 2) (*p* Values in Parentheses)**

	Teen Unwed Birth (Probit)	Idle (Probit)	Years of Schooling (OLS)	High School Graduate (Probit)	Any Postsecondary (Probit)	Wage Rate (OLS)	Received Welfare (Probit)
Parent has no high school education	0.017	0.021	-0.305	-0.121	-0.026	-0.720	0.058
	(0.561)	(0.532)	(0.002)	(0.001)	(0.201)	(0.030)	(0.101)
Parent is a high school graduate, no college	-0.070	-0.068	0.323	0.070	0.041	0.441	-0.039
	(0.033)	(0.075)	(0.001)	(0.067)	(0.081)	(0.312)	(0.352)
Parent has some college education	0.010	-0.067	0.359	0.071	0.098	0.189	-0.06
	(0.784)	(0.050)	(0.000)	(0.064)	(0.000)	(0.692)	(0.146)
Parent is a college graduate	-0.070	-0.163	0.427	0.104	0.163	1.061	-0.056
	(0.379)	(0.163)	(0.076)	(0.304)	(0.008)	(0.164)	(0.585)
Age when the education outcome was measured			0.342	0.122	0.045		
			(0.000)	(0.000)	(0.000)		
R^2	0.154	0.089	0.227	0.184	0.179	0.192	0.099

Source: PSID 1968–93.
Notes: In all probit estimates, the coefficient is transformed to indicate marginal effects with all independent variables set to their means. Monetary figures are expressed in 1997 dollars. Huber-White standard errors are used to account for the nonindependence of sibling observations. The excluded category for parental education is "some high school." The excluded category for city size is < 100,000. R^2 for OLS estimates is adjusted r^2; for probit estimates, it is pseudo r^2. Estimates for state dummy variables are not shown.

it is not examined here.) Thus, the estimates obtained by eliminating families of mixed tenure status should produce the most favorable picture of homeownership effects on children's outcomes. Tests of basic models (i.e., those without tenure/neighborhood interactions) using the full low-income sample support this expectation.

Table A.2. **Alternative Probit Estimates for Indirect Effects Model (*p* Values in Parentheses)**

	Teen Unwed Birth (Probit)	Idle (Probit)	High School Graduate (Probit)	Any Post-secondary (Probit)	Received Welfare (Probit)
Unfavorable family background					
Homeowner family, ages	−0.057	−0.056	0.128	0.029	−0.101
11 to 15	(0.176)	(0.145)	(0.000)	(0.029)	(0.007)
Neighborhood poverty rate	−0.003	0.006	−0.017	−0.004	0.024
	(0.879)	(0.715)	(0.178)	(0.378)	(0.070
Neighborhood home-	0.016	−0.020	−0.005	0.000	0.017
ownership rate	(0.324)	(0.142)	(0.672)	(0.960)	(0.192)
Neighborhood percentage	−0.038	−0.006	0.020	0.007	−0.006
staying 5+ years	(0.041)	(0.737)	(0.122)	(0.132)	(0.665)
Favorable family background					
Homeowner family, ages	−0.016	−0.014	0.076	0.096	−0.072
11 to 15	(0.403)	(0.373)	(0.010)	(0.006)	(0.038)
Neighborhood poverty rate	−0.001	0.001	−0.010	−0.013	0.017
	(0.878)	(0.727)	(0.216)	(0.363)	(0.103)
Neighborhood home-	0.004	−0.005	−0.003	−0.001	0.012
ownership rate	(0.443)	(0.362)	(0.672)	(0.960)	(0.218)
Neighborhood percentage	−0.011	−0.001	0.012	0.023	−0.004
staying 5+ years	(0.340)	(0.739)	(0.151)	(0.122)	(0.666)

Source: PSID 1968–93.
Notes: In all probit estimates in this article, coefficients were transformed to indicate marginal effects with all independent variables set to their means. This table shows how these estimates remain stable with different choices for the values of the independent variables. For the "unfavorable family background" estimates, maternal age at birth was set to 15, parental earnings to zero, parental education to no high school, years of childhood welfare usage to 100 percent, and asset income to zero. For the "favorable family background" estimates, maternal age at birth was set to 30, parental earnings to $30,000, parental education to college, years of childhood welfare usage to zero, and asset income to $1,000 annually. Variables other than those mentioned were set to their means. Monetary figures are expressed in 1997 dollars. Huber-White standard errors are used to account for the nonindependence of sibling observations.

Appendix B

Data

For intercensus years, we interpolated using the values from the two bracketing decennial censuses; for census years and for cases where the data from only one of the bracketing censuses were available, we used values from a single census. (From 1986 onward, the PSID geocode match provided data from the 1990 census only.) Data from two censuses were used in 79 percent of the cases, and one census was used for the rest. Approximately 68 percent of the two-census interpolations were obtained from tract data alone, 10 percent used ZIP code data alone, and 4 percent used a combination of tract and ZIP code measures. In the remaining 18 percent of the two-census cases, data at the tract or ZIP code level were available for only one of the bracketing censuses. For these, we used the tract or ZIP code value that was available relative to the Minor Civil Division (MCD) value for that census to impute a tract or ZIP code value for the missing census based on its MCD value. That is, we imputed $z_1 = Z_1 * z_2 / Z_2$, where z_1 is the missing ZIP code or tract datum from census year 1, z_2 is the available ZIP code or tract datum from census year 2, and Z_1 and Z_2 are the MCD level values. (The MCD corresponds roughly to a township or a quarter of a county. Values for the MCD, or something conceptually similar to it, were available for all years.) About 0.4 percent of two-census interpolations used MCD values for both bracketing census years. Of the single-census cases, 73 percent used tract-level data, 21 percent used ZIP code data, and 6 percent used MCD values.

Authors

Joseph Harkness is a Research Statistician at the Institute for Policy Studies at Johns Hopkins University. Sandra J. Newman is Professor of Policy Studies and director of the Institute for Policy Studies at Johns Hopkins University.

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