

COMMENTARY

The paper by Joseph Harkness and Sandra Newman combines two important issues in community development research: the questions of “neighborhood effects” and of “homeowner effects.” It is interesting that researchers widely accept the notion that neighborhood characteristics influence the outcomes of children and adolescents, although the empirical evidence of such effects remains spotty (Ginther, Haveman, and Wolfe 2000; Evans, Oates, and Schwab 1992). However, there is widespread skepticism that homeownership effects are real, although the statistical evidence for them is fairly robust. The Harkness and Newman paper is one of the first to explore those hypothesized influences in combination, an avenue of inquiry that can potentially have significant policy implications.

1. TRADE-OFFS IN RESEARCH DESIGN

A number of researchers have found that parental homeownership is associated with substantially improved outcomes for children (Green and White 1997; Aaronson 2000). Of course, it is natural and prudent for researchers to question whether the improved outcomes are due to homeownership per se, or to unobservable characteristics of the parents that cause them to both self-select into homeownership and to rear

more successful kids. The standard techniques for dealing with the problem are to seek a more complete set of parental control variables, or to instrument for homeownership. As might be expected, when this is done, the simple estimated effects of homeownership tend to diminish somewhat, but heretofore have remained stubbornly positive and significant. Harkness and Newman choose to deal with this issue in a related paper, presenting a thorough analysis of how instrumentation changes, or does not change, their basic conclusions. In the present paper, they assume that the interaction of homeownership effects and neighborhood effects should be relatively unbiased in single-equation probit models. That appears to be a sensible approach, given that the alternatives would be methodologically complex and might risk obscuring the policy implications.

Of course, all statistical studies also involve database choice and sample selection trade-offs, and it would be useful here to note some of those implicit in the Harkness and Newman paper. The data are drawn from the Panel Study of Income Dynamics (PSID), which provides unparalleled information on family structure and living arrangements throughout the individual’s childhood and adolescence. That provides an excellent set of parental and, with the PSID geocoding, neighborhood control variables. Unfortunately, one trade-off that is inevitable is sample size. Apparently because of sample size considerations, Harkness and Newman have combined

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males and females in most of their basic regressions, with a dummy variable to indicate gender. However, I generally prefer the segregation of the sexes, at least in statistical samples. My own research indicates that homeownership, as well as other characteristics of a family's housing and neighborhood situation, have differential effects on young men and women. This is intuitively plausible, insofar as the socialization and expectations of adolescent males and females are so different. A thorough understanding of neighborhood and homeownership effects, I am convinced, will require separate investigations of their effects on boys' and girls' development.

Harkness and Newman also make an effort to analyze homeownership and neighborhood effects on a variety of child outcomes. Much of the research so far, including my own, has focused more narrowly on the effects on high-school graduation or on teenage or out-of-wedlock births. Expanding the inquiry to include a number of other outcome variables is a useful step at this stage in the game. This broadening of the agenda also involves trade-offs, however. In particular, different characteristics of neighborhoods may influence child development in different ways, requiring a proliferation of neighborhood control variables that may be highly correlated with one another. One of the pioneering papers in the field (Case and Katz 1991) suggests that families and neighborhoods influence youths along like dimensions; for instance, a youth's likelihood of completing high school will be most directly influenced by family and peer propensities toward school completion. The neighborhood variables Harkness and Newman test may not be specific enough to capture all of the particular neighborhood effects on each of their outcome variables, and hence omitted variable bias may be present. It would be interesting to know what other neighborhood variables the authors tested.

2. IMPLICATIONS FOR OWNERSHIP PROGRAMS

With the methodological caveats duly considered, the research of Harkness and Newman addresses pressing questions in community development policy. Promotion of homeownership opportunities has been a favorite policy prescription of government officials, private financial leaders, and policy analysts for a number of years, especially since the large-scale rental production programs, typified by public housing, fell into disfavor. By facilitating, even encouraging, low-income families' purchases of homes in distressed areas, are we doing them, and their children, harm? Those families

are probably among the more capable and motivated, and left to their own devices, might well migrate toward more stable communities. Do we do them a disservice by anchoring them to troubled neighborhoods with homeowner incentives?

The intriguing result obtained by Harkness and Newman is that neighborhoods do appear to affect the children of homeowners and renters differently. They find that children of homeowners appear to be more adversely affected by neighborhood poverty and more favorably affected by neighborhood stability and homeownership rates. While the authors' estimates indicate that, even in distressed neighborhoods, the net effect of homeownership on children is positive, their findings should not be taken too casually. Many of the New York neighborhoods in which homeownership projects have been completed are actually much worse than the worst case estimated by Harkness and Newman. For example, in some of the Bronx neighborhoods in which affordable homes were built in the early 1990s, the poverty rates exceeded 50 percent and the homeownership rate was less than 5 percent. Moreover, the more telling comparison might not be with renter children in the same neighborhood, but with renter children in the neighborhood the family lived in prior to becoming homeowners, or in the neighborhood they might have moved to if left to their own devices.

These concerns should be mitigated, to an extent, if homeownership projects are undertaken on a large scale. In the South Bronx between 1988 and 1997, more than 3,200 units in one- and two-family homes were built, often in large clusters, through the New York City Partnership and Nehemiah programs. Inner-city homeownership development on that scale can change the character of the neighborhoods themselves, possibly diluting the effects of bad neighborhoods on the children of the homeowners. A more disturbing policy conclusion could be drawn, however, if the homeowner effect turned out to be illusory. If the measured gains to children's outcomes are actually due to unobservable characteristics of homeowner parents themselves, public policies that facilitate ownership would actually contribute nothing to the children's outcomes, and could harm them if the ownership opportunities are in more adverse neighborhood environments than the families would otherwise choose.

3. BROADER POLICY IMPLICATIONS

Harkness and Newman add to a growing body of literature relating to the most fundamental question in housing policy: Is the issue of affordable housing simply a question of rent

burdens? If so, it may be better addressed through income policies, such as minimum wages or earned income tax credits. Conventional economic models suggest that, if given the equivalent income supplements, most low-income families would not spend as much on housing as housing programs would implicitly have them spend. So, skewing the consumption of the poor toward housing, through affordable housing programs, can only be justified if there are societal benefits that are not apparent to the beneficiaries themselves. If housing conditions affect health, educational attainment, and other important outcomes in subtle and sometimes imperceptible ways, then a justification exists for giving the poor more housing than they would otherwise choose to purchase.

Homeownership and neighborhood effects each can be used to justify government programs that give the poor more housing, or more stable neighborhoods, rather than an equivalent amount of money. But much more research needs to be done to target housing programs effectively. What produces the homeownership effect and what characteristics of neighborhoods promote good outcomes for children? Harkness and Newman further this effort by exploring the interaction between housing tenure and neighborhood context.

The authors note that, as they move from simple to more complete specifications, “the inclusion of neighborhood controls has modest effects on some model estimates, but overall, there is little effect. Even with neighborhood controls, homeownership has strong, favorable effects on most outcomes.” Those results are consistent with my own research findings on housing conditions and high-school completion. In fact, approaching it from the other direction, I first tested a model with only parental and neighborhood controls, then one

that added housing variables such as homeownership, mobility, overcrowding, and maintenance condition. I found that the housing variables actually dominate the neighborhood variables. That finding has led me to wonder if some of the neighborhood effects commonly reported are not, in fact, actually due to missing housing variables for which the neighborhood variables are proxying. At the least, I believe that more research is needed to understand the effects of the physical aspects of the home environment on children, some of which could affect kids’ educational attainment through their health and school attendance.

Researchers have found that residential mobility can adversely affect the educational attainment of children (Haveman, Wolfe, and Spaulding 1991), and that the housing stability that usually accompanies homeownership may account for some, though probably not all, of the positive effect that homeownership seems to have (Aaronson 2000). There are other housing conditions that may plausibly affect children’s outcomes that have received less research attention. Overcrowding was thought by early housing reformers to have adverse effects on children, but there is surprisingly little research into the issue. Poor maintenance conditions, including insufficient heat, inoperable plumbing, or rodent infestation, could also adversely affect the health or study habits of children. The effects of such physical deficiencies on children’s development and behavior need to be investigated more thoroughly. A better understanding of which housing and neighborhood conditions maximize children’s chances for success would assist in formulating public programs that not only improve housing conditions, but contribute to solving other social problems as well.

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