Neighborhood Poverty During Childhood and Fertility, Education, and Earnings Outcomes

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ABSTRACT

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Previous studies attempting to estimate the relative importance of family, neighborhood, residential stability, and homeownership status characteristics of childhood environments on young adult outcomes have: (1) treated these variables as though they were independent, and (2) were limited in their ability to control for household selection effects. Our study offers advances in both areas. First, it treats the key explanatory variables above as endogenously determined (sometimes simultaneously so). Second, to deal both with this endogeneity and the selection problem, we compute instrumental variable estimates for how childhood average values of neighborhood poverty rate relate to fertility, education, and labor market outcomes in later life.

We analyze data from the U.S. Panel Study of Income Dynamics (PSID) that are matched with Census tract data, thereby permitting documentation of a wide range of family background and circumstantial characteristics. For children born between 1968 and 1974, we analyze data on their first 18 years and various outcomes in 1999 when they are between 25 and 31 years of age.

We find that, all else equal, children raised in neighborhoods with higher average poverty rates have substantially: (1) greater chances of having a child before age 18; (2) lower chances of completing high school or college; and (3) lower annual earnings, both directly and through intermediate outcomes. Most estimates of neighborhood effects are larger when we employ the instrumental variables approach.

Introduction and Context

Much recent literature—emanating both from the United States and Europe—has focused our attention on the plight of children growing up in neighborhoods of concentrated socioeconomic disadvantage. From a policy perspective, it is critical for the guidance of urban revitalization initiatives and assisted housing programs designed to increase access to a wider range of locations to ascertain the degree to which neighborhood characteristics affect children's developmental context (Galster 2002, 2005; Galster et al. 2003a).

Our research is designed to advance understanding of the extent to which the success of children in young adult life (measured by a variety of indicators) is related to the characteristics of their neighborhoods while controlling for characteristics of their: families (education, income, attitudes, values, family structure), parents' homeownership status, and residential mobility history. In this paper we focus on the relationship between one particular aspect of the child's developmental environment, the cumulative neighborhood poverty rate experienced during the first 18 years of life, and three outcomes: teen fertility, educational attainment, and earnings. This establishes the focus for our literature review, theoretical development, and discussion of findings, with the other background characteristics of young adults essentially being treated here as control variables.

The statistical literature seeking to identify the predictors of various social, economic, behavioral and psychological outcomes for children and adults is voluminous and has been subject to several recent comprehensive reviews (Robert 1999, Leventhal and Brooks-Gunn 2000, Earls and Carlson 2001, and Sampson, Morenoff and Gannon-Rowley 2002; Ellen and Turner, 2003; Galster, 2005). Suffice it to note in summary that the bulk of this literature (e.g., Furstenberg et al., 1999, Brooks-Gunn et al., 1997) examines factors affecting outcomes measured during childhood, ranging from pre-school to adolescence. However important such outcomes are, we believe it is also crucial to examine childhood factors that account for later success as adults. In this regard there is an established literature examining negative adult outcomes, such as welfare usage (e.g., Moffitt, 1992; Gottschalk, McLanahan, and Sandefur, 1994; Gottschalk, 1996; Vartanian, 1999; and Pepper, 2000) school dropouts (e.g., Clark, 1992; Mayer, 1997, Gleason and Vartanian, 1999; and Sawhill and Chadwick, 1999), crime (e.g., Sullivan, 1989; Freeman, 1991; Peeples and Loeber, 1994; Grogger, 1997),

teen childbearing (e.g., Maclanahan and Bumpass, 1988; Furstenberg, Levine and Brooks-Gunn, 1990; Haurin, 1992; Sawhill and Chadwick, 1999; Barber, 2001), acceptance of deviant behavior (Friedrichs and Blasius, 2003), mental illness (Wheaton and Clarke, 2003), and economic idleness (Payne, 1987; Haveman and Wolfe, 1994; Mayer, 1997; Sawhill and Chadwick, 1999). The literature that examines childhood factors that account for economic success as adults is sparse by comparison (but see Corcoran et al. 1992, Haveman and Wolfe, 1994; Vartanian, 1999; Holloway and Mulherin, 2004) and does not address the methodological challenges with which we are concerned.

As we shall amplify below, previous studies attempting to estimate the relative importance of a child's family, neighborhood, residential stability, and homeownership status characteristics on outcomes as an adult must be treated with caution because they have: (1) treated these background variables as though they were independent, and (2) employed inadequate methods to control for household selection effects (Galster, 2003a). Our study offers what we hope will be advances in both areas. First, it treats the key explanatory variables above as endogenously determined (sometimes simultaneously so). Second, to deal both with this endogeneity and the selection problem, we employ a variant of two-stage least squares applied to a structural equation system to instrument mean childhood values of the neighborhood poverty variable and use it to estimate relationships with young adult fertility, educational, and earnings outcomes.

We analyze data from the U.S. Panel Study of Income Dynamics (PSID) that are matched with Census tract data, thereby permitting documentation of a wide range of family background and neighborhood circumstantial characteristics. For children born between 1968 and 1974, we analyze data on their first 18 years and various outcomes in 1999 when they are between 25 and 31 years of age. We find that the average rate of neighborhood poverty experienced by children during ages 0-18 is, all else equal, strongly related to their fertility, educational attainment, and earnings.

Our paper is organized as follows. We first offer a holistic framework for understanding how children's neighborhood environment might influence outcomes when they are young adults, then employ it as a vehicle for evaluating a range of previous work and establishing a foundation for our modeling efforts. Second, we describe the two pre-eminent challenges that must be overcome if one is to gain accurate measurements of the above relationship: selection bias and simultaneity bias. Third, we describe our dataset and the multi-step IV estimation procedure we employ to

meet the aforementioned challenges. Fourth, we present our statistical results of the key relationships between a child's neighborhood poverty rate and subsequent fertility, education, and earnings outcomes. Finally, we discuss conclusions, implications, and directions for further research.

How Might Children's Neighborhoods Influence Their Outcomes as Young Adults?

Prior Work and Its Shortcomings

The study of how neighborhood context affects children and adults has been a burgeoning field of enquiry internationally, as evinced by several recent comprehensive and methodologically critical reviews of the literature (Ellen and Turner 1997; 2003, Duncan, Connell, and Klebanov 1997, Gephart 1997, Robert 1999, Duncan and Raudenbush 1999, Leventhal and Brooks-Gunn 2000, Earls and Carlson 2001, Dietz 2001, Sampson, Morenoff and Gannon-Rowley 2002, Friedrichs, Galster and Musterd, 2003, Galster 2003a; 2005). We will not attempt to duplicate these reviews here, but merely reiterate the consensual methodological critiques that motivate our current paper. Few studies consider young adult outcomes or collect information over the entirety of childhood that may be used to predict such outcomes. Many omit key parental control variables that may bias the apparent impacts of neighborhood. And none meet fully the fundamental statistical challenges posed by an investigation of this sort, a topic to which we now turn.

A Holistic Framework

In order to provide a framework for both illustrating the limitations of previous studies and guiding our own efforts, we present a structural model portrayed in Figure 1. We posit that young adult outcomes of interest (shown on the right panel of Figure 1) are determined by four sets of exogenous or predetermined variables: observed characteristics of individual children (path A: gender, race, e.g.), unobserved characteristics of individual children (path H: intelligence, e.g.), observed parental characteristics (path G: education, age, e.g.), and unobserved parental characteristics (path B: ambition, present orientation, e.g.). These unobserved parental factors (shown as dotted lines in Figure 1) are the source of omitted variables bias associated with selection, which we shall discuss below. Young adult outcomes are also influenced by a

set of parental characteristics that may more properly be modeled as endogenous to the childhood residential context (path E: parental employment and income history, e.g.). Finally, we see young adult outcomes as influenced by a set of intervening endogenous variables: neighborhood characteristics (path C), parental homeownership status (path D), and parental mobility expectations mediated by actual mobility behavior (path F).

[Figure 1 about here]

The key innovation of our model is the specification of neighborhood location / homeownership status / mobility expectations / household socioeconomic status as *mutually causal phenomena*. Put differently, we argue that accurately measuring the relationship of *any one of these phenomena* with young adult outcomes requires that its relationship *with all the others* be taken into account, a key point to which we shall return below. We offer brief, heuristic rationales for these bi-directional causal relationships portrayed in Figure 1; supportive evidence is summarized in the aforementioned reviews:

Homeownership status and neighborhood: if economic status (low income and wealth) constrains a household to a set of "affordable" neighborhoods, which may often be afflicted with numerous social problems and concomitant expectations of property value deflation, there will be little motivation to buy a home; on the other hand, if a household would like to buy, certain neighborhoods may not be selected if they hold the prospect for little property appreciation

Homeownership status and neighborhood AND *mobility expectations* (expected duration of stay): if one expects to remain long in a dwelling, given one's employment and life-cycle stage situation, one may be more likely to bear the high transactions costs of buying *and* will try harder to avoid declining neighborhoods; in turn, if one can purchase a home, and succeed in doing so in a good neighborhood, one will probably expect to move less in the future

Homeownership status and parental characteristics: income, stability of employment, and non-housing wealth will influence the ability to purchase a home; homeownership, in turn, may provide a sense of security and control over environment that promote parental efficacy and marital stability, as well as a key financial resource for furthering children's' education.

Neighborhood and parental characteristics: parental income, non-housing wealth, education, age and race will influence the choice of neighborhood; neighborhood location with respect to potential employment and job information networks, social

milieu, and environmental features can influence, in turn, parents' health and access to employment and thereby, their income subsequently

In this paper we focus on the interrelationships among neighborhood choice, tenure choice, and mobility expectations.[1] The model we operationalize can be summarized more formally in the following set of structural equations that delineate two presumed simultaneous and three lagged (recursive) causal relationships measured for a given year (which is not subscripted for simplicity):

[1] *HO* = f(*N*, ME, H-1, [X_1])

[2] *N* = f(*HO*, ME, H-1, [X_2])

[3] ME = f(HO-1, N-1, M-1, H-1, [X_3])

[4] M = f(N-1, H-1, ME-1, [X_4])

[5] H = f(N-1, HO-1, [X_5])

where italicized acronyms indicate a simultaneous relationship as per above discussion and:
HO = homeownership status (own or rent)
N = neighborhood poverty rate
ME = expectations regarding potential move during next year
M = actual mobility observed during the year
H = endogenous household economic characteristic (poverty status)
[X_i] = vector of exogenous or predetermined predictors appropriate to equation i
-1 = one year lagged value of variable

Challenges in Measuring Determinants of Young Adult Outcomes

The holistic framework portrayed in Figure 1 makes it clear that there are two pre-eminent challenges in obtaining accurate measurements of the relationship between young adult outcomes and key childhood predictors of interest, such as neighborhood,

1 In prototypes we experimented with modeling parental mobility expectations, marital status, and income as simultaneous with neighborhood and tenure choice, and developing instrumental variables for same. These proved challenging to identify instruments distinct from those used for tenure and neighborhood, hence these experiments are not reported here.

homeownership status, mobility, and certain parental characteristics. These challenges are: selection bias and simultaneity bias.

Selection Bias

Selection bias in the neighborhood-outcome relationship is now a well-known challenge. The basic issue is that some parents who have certain (unmeasured) motivations and skills related to their children's upbringing would move to select neighborhoods. Any observed relationship between neighborhood conditions and child or young adult outcomes may therefore be biased because of this systematic spatial selection process, *even if all the observable characteristics of parents are controlled* (Manski 1995, 2000; Duncan et al. 1997; Duncan and Raudenbush 1999, Dietz 2001). The problem can be formulated as omitted variables bias. Is the observed statistical relationship between outcomes and neighborhood indicative of neighborhood's independent effect, or merely unmeasured characteristics of parents that truly affected child outcomes but also led to neighborhood choices as well?[2] We portray the implicit omitted variables' relationships in this selection problem as dashed lines in Figure 1.

When analyzing a sample of households who have chosen their neighborhoods through the private market process, this selection bias is likely severe indeed (Tienda, 1991; Manski, 1995). A variety of econometric techniques, including sibling studies and instrumental variables, have been employed in an attempt to overcome this neighborhood selection bias, but with incomplete success and/or limited general applicability thus far (see review in Galster, 2003a; 2005). In addition, a few studies have attempted to model explicitly the selection process into owner and rental tenures (Green and White, 1997; Haurin, Parcel and Haurin, 2002a,b).

Analysis of data on outcomes that can be produced by an experimental design whereby individuals or households are randomly assigned to different neighborhoods has been seen as the preferred method for avoiding biases from selection. In this regard, the Moving To Opportunity (MTO) demonstration has been touted conventionally as *the* study from which to draw conclusions about the magnitude of neighborhood effects. Although the MTO research design indeed randomly assigns those public housing residents who volunteer to one of three experimental groups, it does not fully control the assignment of neighborhood characteristics of the two experimental groups

2 The direction of the bias has been the subject of debate, with Jencks and Mayer (1990) and Tienda (1991) arguing that neighborhood impacts are biased upwards, and Brooks-Gunn, Duncan, and Aber (1997) arguing the opposite.

receiving tenant-based rental subsidies (Sampson, Morenoff and Gannon-Rowley, 2000). Of course, the group that receives only a rental subsidy with no mobility counseling and no geographic restrictions can select from a wide range of neighborhoods. But even the treatment group receiving intensive mobility counseling and assistance, though programmatically constrained to move initially to a neighborhood with less than 10% poverty rates, has the ability nevertheless to choose neighborhoods varying on their school quality, home ownership rates, racial composition, local institutional resources, etc. Moreover, subsequent to their initial, constrained location they are free to move to different, higher-poverty neighborhoods should they choose (as many have; Goering, Feins and Richardson, 2002). Thus, even studies based on MTO data cannot fully finesse the selection bias issue.

However, the challenge is even deeper. Were Figure 1 to be adopted as a working premise, the selection process becomes much more complicated than merely the parents' *independent* selection of neighborhood. In our view, the holistic challenge embodies the *interdependent and simultaneous* selections of neighborhood, homeownership status, and expected mobility.

Simultaneity Bias

Previous statistical studies have taken only a partial view of the causal patterns embodied in Figure 1. Some have omitted one or more of the intervening variables. But of more import here, none have modeled these variables as mutually endogenous.[3] This simultaneity bias provides an additional reason why the accuracy of the relationships they measure between outcomes and key predictors of interest may be called into question.

Meeting the Challenges through an Instrumental Variables Approach

We believe that the most promising strategy to both selection and simultaneity biases in an analysis of households sampled from non-experimental circumstances is the application of instrumental variables (IV).[4] The IV technique (or two-stage least

3 While other studies have discussed the issue of "simultaneity bias," they used the term to refer to the reflection problem (Manski 1995) of people in the neighborhood tautologically causing the aggregate neighborhood characteristics to be what they are as well as the neighborhood causing constituent residents' behaviors (Duncan and Raudenbush 1999).

4 Other recent research has employed natural experiments where the selection bias was minimized through geographic assignment of households through governmental housing program auspices; Edin, Fredricksson, and Aslund (2003); Oreopolis (2003) and Aslund and Fredriksson (2005).

squares (2SLS) is well known as a solution to the simultaneity challenge.. In the case of the selection challenge, instrumental variables have rarely been used, and only in the case of neighborhood selection (Evans, Oates, and Schwab, 1992; Foster and McLanahan, 1996). In the first stage, the dimension of neighborhood in question is regressed on one or more explanatory variables that, hopefully, are highly correlated with the neighborhood characteristic but uncorrelated with unmeasured parental characteristics. The predicted values for the neighborhood characteristic yielded by this first stage regression, which presumably are purged of spurious correlation with unmeasured parental characteristics, are employed in a second-stage regression explaining outcomes.

The challenge of this method, of course, is identifying first-stage variables that reasonably meet the aforementioned correlation criteria. In the seminal example of instrumental variables applied to the neighborhood selection problem, Evans, Oates, and Schwab (1992) used metropolitan-level variables for unemployment rate, median family income, poverty rate, and percentage of adults completing college as identifying variables predicting the "neighborhood variable:" proportion of students in the local school who are economically disadvantaged. Analogously, Foster and McLanahan (1996) used citywide labor market conditions as identifying variables predicting neighborhood high school dropout rates. We believe that this strategy for instrumenting not only neighborhood-level but individual-level variables with corresponding variables measured at larger geographic scales is fruitful, and employ it in our work along with other identifying instruments, as explained below.

Data to be Analyzed and Key Measures

The Panel Study of Income Dynamics

A brief overview of the Panel Study of Income Dynamics (PSID) data we analyze is a prerequisite for understanding our particular instrumental variables approach. Beginning in 1967, the PSID began interviewing 5,000 American families. In each year since then, those families have been interviewed, as have all families subsequently formed by individuals in those families and by future spouses and children of those individuals. So, by 1999, the PSID was following nearly 10,000 families. While the PSID over sampled poor households in order to obtain relatively large sample sizes for such households, the poverty over-sample was subsequently dropped in the 1990s.

Consequently, our analysis is limited to a sample designed to be nationally representative of the U.S. population in 1967. We account for differential attrition over the course of the panel by adjusting individuals' PSID sampling weights by the inverse of the reciprocal of the attrition rate of PSID sample members with the same race, gender and poverty status at birth. We employ a PSID geo-matched file, which appends the child's census tract identifier to each observation. We interpolate values of census tract variables for observations between census years. We are thus able to observe annually the household and (approximate) neighborhood environments in which our sample individuals spend their childhood.

We focus our analysis on the PSID cohort of children born during the period 1968-1974 because it provides us with data on their first 18 years as well as a variety of outcomes measured in 1999 when they were young adults (ages 25-31) who most likely had completed their education and had ample opportunity to enter the labor force.[5] Here, as throughout, we present statistics weighted by PSID sampling weights, adjusted for group-specific attrition.

A necessary condition for the precise measurement of neighborhood effects is that the widest possible array of characteristics of the children and their household while growing up be included as controls in the model (Ginther, Haveman and Wolfe 2000). We believe that our work has met this condition in a way superior to prior work. We not only control for a wide range of objective characteristics of the household but, unlike prior work, also control for several attitudinal and behavioral characteristics of the head. Descriptive statistics for these numerous aspects of the sample of children we analyzed —themselves, their households, the heads of their households, and their neighborhoods as they were growing up—are provided in Table 1.

[Table 1 about here]

Measures of Key Explanatory Variables and Outcomes

In this paper we consider a commonly used measure of a disadvantaged neighborhood environment: percentage of population with household incomes below the U.S. federal poverty standard. In each case we employ information from the census tract, a homogeneous area of roughly 4,000 inhabitants, tabulated in the decennial

5 Such a longitudinal analysis has been strongly recommended as the vehicle for overcoming the reflection problem (Manski 1995, Duncan and Raudenbsuh 1999).

Census of Population and Housing, with values interpolated for inter-census years.[6] On average during their childhood, children in our sample experienced a census tract having a 10.5% poverty rate.

Several studies suggest that census tract data on socioeconomic disadvantage may serve as reasonable (if admittedly imperfect) proxies for intra-neighborhood social processes through which neighborhood effects reasonably might transpire. Measures similar to neighborhood poverty rate have proven statistically related to: unsupervised peer groups and organizational participation (Sampson and Groves, 1989); informal social control (Elliott et al. 1996); collective efficacy (Sampson, Raudenbush, and Earls 1997); a multi-dimensional index of social processes (Cook, Shagle, and Degirmencioglu 1997); multiple dimensions of social capital (Sampson, Morenoff and Earls 1999); and perceived disorder (Coulton, Korbin, and Su 1999; Kohen, Brooks-Gunn, Leventhal, and Hertzman forthcoming).[7] We recognize, however, that neighborhood poverty is not a proximate measure of the underlying processes that are responsible for any neighborhood effects, and thus interpretation of regression results remains somewhat ambiguous, at topic to which we will return in closing.

Our goal here is to relate a child's neighborhood poverty rate, controlling for all the other characteristics of the child's family and environment listed in Table 1, to fertility prior to age 18, school attainment, and earnings as of 1999. Ninety-four percent of our sample children born 1968-1974 had not had a child prior to age 18. By 1999, 90 percent of this cohort had graduated from high school or obtained a Graduate Equivalent Degree, and 20 percent had graduated from a four-year college or university. The PSID only collects income information from PSID respondents who have formed their own household and worked at some time during the previous year, so income statistics and regression results we report refer only to this subset of our cohort. However, 81 percent of our cohort had formed a household and were employed by the time of our 1999 survey. On average in 1998, this cohort of household heads who were employed part- or full-time individually earned $17,348.

Estimation Procedure

6 We employ a database that adjusts data in 1970, 1980 and 1990 tracts that have changed their boundary definitions over the years to values that would appertain had boundaries remained at their 1990 specifications.

7 For details, see Galster (2003a).

Overview of our Approach

Because our structural equations [1] – [2] involve a simultaneous relationship between childhood neighborhood poverty and parental housing tenure, it is necessary to use an instrumental variables approach to obtain consistent coefficient estimates. Our approach for estimating IVs represents a variant of the two-stage least squares technique, and proceeds in the following three steps.

First, for both childhood neighborhood poverty and tenure variables we estimate an Ordinary Least Squares (OLS) regression in which the left-hand side is the observed value of the endogenous variable in question in a given PSID year and the right-hand side contains observed values of *every exogenous (including predetermined) variable in the system of structural equations [1]-[5].* These exogenous variables are too numerous to list individually, but include: (1) lagged values of certain household characteristics, (2) housing price information for the region, and (3) contemporaneous values of countywide characteristics corresponding to the endogenous variables.[8] Dummy variables for calendar year are also included on the right-hand side of these equations. In this first step, regression equations are estimated based on all observations from age 0 to 18 of each child in our initial sample.[9] Thus, in the absence of missing data, eighteen sets of observations are used for each child in our initial sample.[10]

In the second step of our approach, the aforementioned regressions are employed to generate predicted values of endogenous variables for each of the first 18 years of each child's life, based on values of all exogenous and predetermined variables used in the prior step's regressions. We compute the average of these predicted values over all observed years of childhood. These childhood averages of annual predicted values become our IV measures for neighborhood poverty and tenure during childhood. It is important to note that what is of prime importance here is how well the prior regressions predict the values of the endogenous variables, not their coefficient estimates in and of themselves.

The third step of our procedure consists of estimating the coefficients of exogenous variables and IVs in the fertility, education, and earnings outcome equations, using OLS when the outcome is continuous (earnings) and logit when the outcome is dichotomous. The sample for estimating these coefficients includes all children in our

8 Note that lagged values of neighborhood characteristics are not included in this list of right-hand side variables. This is because neighborhood characteristics are interpolated among different census years and hence would be almost perfectly correlated with the current value of neighborhood characteristics.

9 Because each first-stage equation includes lagged variables, we cannot estimate a first-stage equation for age 0.

10 We included all observations of children having data for at least ten years of their childhood.

initial 1968-1974 PSID cohort who have "survived" in the sample to the point at which the outcome in question is observed: 1999. Equations for fertility, education, and earnings outcomes have many variables on the right-hand sides that are identical. Both sets employ (exogenous or predetermined) characteristics of the individual and the individual's household (including proportions of years during childhood during which the family was below the poverty line and moved residence) and (IV estimates of endogenous) average neighborhood poverty rate during childhood and proportion of years the family resided in a home they owned; descriptive statistics of these variables were presented in Table 1. For all of these variables in our model we use figures calculated over the first 18 years of the child's life (or for however many years we have data).

Furthermore, we model our set of four outcomes as causally interrelated, as shown in the right panel of Figure 1. Educational attainment is a function of fertility prior to age 18. Earnings are a function of fertility and education.[11]

Identifying Instruments for Childhood Neighborhood Poverty Rate

In order to satisfy the rank condition in performing two-stage least squares, there must be at least as many exogenous variables excluded from each structural equation as there are endogenous variables included in each equation. We meet this condition; indeed our equation system is over-identified.

Moreover, each structural equation must have one or more clearly exogenous variables that appear only in the given equation as strong predictors. In the case of our measure of childhood neighborhood—poverty rate—we employed as the identifying instrument the corresponding county-level value.[12] This proved highly predictive of the tract-level values (t statistic of 34), and typically was minimally correlated with other endogenous variables in the models.

Overall our first-stage regression for neighborhood poverty rate performed only modestly well (the R^2 was .45), however, so we cannot be confident that we have avoided the problem of weak instruments.[13]

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11 In preliminary models we specified neighborhood as potentially affecting employment status and hours worked, which in turn affected earnings. There were no statistically significant relationships observed, so these intermediate outcomes are omitted from discussion here.

12 There are two exceptions to this. First, for those years in which the family lived in a rural area, we use the observed value of county characteristics. Second, for child age zero we use the observed value since we cannot estimate a first-stage equation for year zero (due to unavailability of lagged variables).

13 Details of the first-stage regressions are available upon request.

Complicating Issues

Four issues require further discussion. The first of these is the operational definition of neighborhood. While imperfect, we employ census tracts as our preferred approximation to neighborhood, as is common in U.S. studies. However, until 1990 rural areas were not divided into census tracts. We avoid the potential problems of (1) missing data and (2) mixing urban and rural scales of "neighborhood" by confining our analysis to children who spent at least 12 of their first 18 years in tracted, metropolitan area neighborhoods.

Secondly, the attitudes and behaviors of the household head that we employ as controls (see Table 1) are not measured annually in the PSID. Indeed, for most variables the questions were asked only during the years 1968-1972.[14] Each attitude and behavior we employed as a control proved stable over time. Pair-wise correlations between responses to the question "carry out plans" over the six points in time at which this question was asked ranged from .17 to .40. Cronbach's alpha, a measure of internal consistency, for a scale consisting of the sum of the responses to this question over the six years, was .70. Pair-wise correlations between responses to the question "plan ahead" over the six points at which this question was asked ranged from .20 to .46; Cronbach's alpha was .77. Pair-wise correlations between responses to the question "trust" over the five points in time at which this question was asked ranged from .40 to .54; Cronbach's alpha was .81.

The third issue requiring some discussion is the handling of endogenous variables that are dichotomous (such a whether the family fell below the poverty line in a given year). As noted in Wooldridge (2002: 478), it is not appropriate to apply probit or logit models to such variables and then use the predicted probability on the right-hand side in second-stage estimation. Hence, in our first stage we applied linear probability models to each dichotomous endogenous variable and used the predicted value obtained from that estimation in our second stage.

Finally, as noted above, our estimation procedure involves the creation of IV estimates for endogenous variables in the outcome equations that consist of *multi-year averages (or proportions) of predicted values* of these variables. Given that the distribution of these new "average" instruments is not known, the standard errors yielded by OLS or logit cannot be interpreted in a straightforward fashion. Thus, although we

14 However, some were asked again in 1975 and a question about union membership was collected from 1968 through 1981.

will report conventional tests for statistical significance below, they must be interpreted cautiously when examining our IV estimates.

Results

Overview of Selected Control Variables' Results

Before turning to results for the variable of primary interest, we briefly note some of the more interesting relationships involving control variables; details are presented in the Appendix. As for predictors of not having a child before age 18, growing up in a family that did not move often and whose head ascribed to "planning ahead" proved efficacious. Having well-educated parent(s) who knew more of the neighbors by name was correlated with greater chances of later graduating from high school and college. Being raised in a household that never fell into poverty, always had two parents present, and a head who planned ahead, was associated with higher earnings as a young adult, after controlling for educational attainment, fertility, and hours worked.

Comparing Results for Neighborhood Poverty With and Without IVs

We next present coefficients of childhood neighborhood poverty rate estimated using observed values, not IV values generated as per the above procedure. These are shown in the left-hand member of each pair of columns of Table 2.[15] We find consistent evidence supporting the hypothesis of an independent, nontrivial impact of childhood neighborhood poverty on subsequent fertility, educational attainment, and earnings, both directly and indirectly, controlling for a range of parental and other background characteristics.

[table 2 about here]

Average neighborhood poverty rate experienced during childhood proved strongly negatively associated with the probability that children would reach age 18 before producing a child. This result is especially noteworthy because of the interrelationships we observed between teen childbearing and subsequent opportunities for high school attainment and, ultimately, labor market success, as shown in Table 2. Higher childhood neighborhood poverty rates were also associated with lower chances of graduating from high school but, having done so, were not associated with a statistically significantly lower chance of completing a four-year college degree. Moreover, even while controlling for fertility and educational attainment, average

15 In all outcome equations the models as a whole were statistically significant; details available upon request.

childhood neighborhood poverty was strongly negatively associated with annual earnings of those employed during 1998.[16]

For comparative purposes we present estimates for coefficients of our childhood average of neighborhood poverty variable as produced by our IV estimation procedure described above; see the right-hand member of each pair of columns of Table 2. Our IV estimates of childhood neighborhood poverty effects are generally *larger* in magnitude than those produced without instruments. In the cases of high school graduation or more, college graduation, and earnings outcomes, the IV estimates are 10, 63 and 57 percent higher, respectively, than the comparable estimate produced without instrumenting. The only exception is in the case of no child before age 18, where the IV estimate is 31 percent smaller. These remarkable findings must be interpreted in light of the unusually large number of parental attitudinal and behavioral variables that we employ as controls but which most studies omit. They suggest that, when such parental characteristics are controlled, the measured childhood neighborhood effect is biased downward in the case of education and earnings outcomes. We discuss possible explanations for this below.

Unfortunately, the IV estimates are generally less precise than the estimates not employing instruments. This is to be expected since IV estimates are inherently less efficient than OLS estimates, and here the problem is likely exacerbated by the problem of relatively weak instruments discussed above. We also remind the reader, however, of the aforementioned point that conventional tests of statistical significance must be interpreted cautiously here.

Assessing Magnitude of Implied Impacts

In order to gain a clearer sense of the magnitude of estimated coefficients of the childhood neighborhood poverty variable in the context of our recursive model of outcomes (right-hand side of Figure 1) we conducted a series of simulations. To do so, we imposed counterfactual changes to the value of average childhood neighborhood poverty rate to generate corresponding predicted values for the probability of reaching age 18 before having any children; results are shown in Figure 2. Predicted changes in these fertility probabilities were then added as input into the models explaining educational attainments, along with corresponding changes in childhood neighborhood

16 We also experimented with participating in the labor force, being employed, and hours of work as outcomes, but the results did not suggest further influences of neighborhood poverty, either directly or indirectly through fertility or education.

poverty, yielding estimates shown in Figures 3 and 4. Predicted changes in fertility and educational attainments were then added as input into the model explaining earnings (as well as the direct effect of altered childhood neighborhood poverty), yielding estimates shown in Figure 5. For all figures we present simulation results for the models that do not use the instrumental variables and those which do; results are quite similar in either case.

[Figures 2-5 about here]

The main conclusion to be drawn from Figures 2-5 is that average neighborhood poverty rates experienced by the 1968-1974 cohort from ages 0 to 18 are associated with a substantial variation in their outcomes in 1999. For example, compared with otherwise identical children raised by otherwise identical parents in a neighborhood with a low poverty rate of five (5) percent (approximately half the sample mean), children experiencing a 40 percent rate (a conventional benchmark for "concentrated poverty" neighborhoods; Jargowsky, 1997) is predicted to have a:

7-24 percentage-point (7-24 percent) greater chance of having a child before age 18;
11-14 percentage-point (11-14 percent) lower probability of graduating from high school;
10-15 percentage-point (70-87 percent) lower probability of graduating from college; and
$13,334-$19,855 (54-71 percent) lower annual earnings

We believe that, regardless of which set of model estimates one employs, these simulated values represent socio-economically significant differences. This suggests to us that poverty neighborhoods in America create important limitations on the life chances who are raised there.

Comparing Alternative Estimates of Neighborhood Effects

It has been previously observed that there is little consensus in the literature on the magnitude of neighborhood effects (Robert 1999, Leventhal and Brooks-Gunn 2000, Ginther, Haveman and Wolfe 2000, Earls and Carlson 2001, and Sampson, Morenoff and Gannon-Rowley 2002) and our study adds yet more variance. Indeed, the implied magnitude of childhood neighborhood poverty impacts presented in Figures 2-5 is greater than that measured by earlier studies using comparable neighborhood measures and outcomes: teen fertility (Hogan and Kitagawa 1985, Brewster, Billy and Grady 1993, Brewster 1994a, b), educational attainment (Datcher 1982, Garner and Raudenbush 1991, Clark 1992, Duncan 1994, Aaronson 1998), employment (Datcher 1982, Vartanian 1999) and earnings (Page and Solon 1999).[17] There are at least three reasons for this. First, we measure neighborhood poverty averaged over childhood, not just for a shorter span, as most other studies have. We thus are measuring the cumulative impact of this neighborhood condition.[18] Second, we model a recursive relationship among various outcomes. This reveals several paths by which the neighborhood context may affect education and labor market outcomes both directly and, to a non-trivial degree, indirectly mediated by fertility. Third, we not only instrument for neighborhood poverty but do so in the context of a simultaneous equation model of tenure choice. A comparison of our IV estimates and non-IV estimates for the childhood neighborhood poverty variable reveal that the IV estimates are often considerably larger in magnitude; see Table 2. This suggests that the failure to generate IVs by using all information contained in a structural model like equations [1]-[5] underestimates neighborhood impacts.

Potential reasons why a failure to adequately instrument may bias downward the measured neighborhood effect have been provided by Duncan and Raudenbush (1999). First, suppose that: (1) parents can choose to work more or less (by varying hours of work or taking on different numbers of jobs), and (2) if they work more they will move into a better neighborhood but they will have less time available to nurture their children (and vice versa). Lower-poverty neighborhoods may independently yield better outcomes, but the associated harmful effects of less parental time with children will blunt this measurement if (as is typical) parental time allocation cannot be controlled in the regression. Second, suppose that parents who are most capable of resisting and

17 Our estimates also differ substantially from those finding no statistically significant impacts from neighborhood poverty and associated measures of disadvantage; e.g., see Corcoran et al. (1992), Ensminger et al. (1996), Plotnick and Hoffman (1999),

18 Wheaton and Clarke (2003) find cumulative neighborhood conditions much more powerful in explaining various child developmental outcomes than contemporaneous conditions.

compensating for the negative impacts of high-poverty neighborhoods more often choose to live in them to take advantage of lower housing costs, shorter commuting times, a more bohemian social milieu, etc. In this case the failure to control for parenting competence will lead to an understatement of the deleterious effects of poor neighborhoods. We cannot know the degree to which these or other scenarios are borne out, of course, because the key parental characteristics in question are unobserved.

Discussion: Tentative Conclusions, Caveats and Next Steps

This paper represents the first attempt to estimate the cumulative effect of neighborhood poverty on children's outcomes in later life in the context of a holistic model involving the simultaneous parental choice of neighborhood and homeownership status. We have argued that an IV approach based on such a model is necessary for obtaining estimates of neighborhood effects that are purged from the twin confounding influences of selection and simultaneity biases. Our prototype, two-stage least squares estimates using a cohort of children born 1968-1974 and interviewed through the PSID in 1999 provided provocative indications that these cumulative neighborhood poverty effects averaged over childhood are likely greater than has previously been measured in the realms of teen fertility, educational attainment, and earnings.

As befits a prototype, or modeling experiments suggested that our approach can only reach its full potential if superior instruments for childhood neighborhood poverty rate can be identified. Our reliance upon coincident county-level data as identifying instruments for census tract poverty rates proved only partially successful, even when combined with exogenous predictors found elsewhere in our structural model. An intensified future search for *uniquely* identifying instruments would also permit researchers to more fully operationalize hypothesized endogenous relationships between neighborhood choice, tenure choice, mobility, and household head characteristics, as portrayed in Figure 1. Our efforts fell short in this regard, yielding instruments for many endogenous variables that were too collinear to be employed in modeling.

We also cannot be sure, of course, what causal mechanisms may be at work behind these statistical associations. These mechanisms are thought to operate through various individual-, family-, school-, peer-, and community-level processes. Scholars have proposed various theoretical models, typically highlighting different underlying

processes, to explain potential pathways of neighborhood influences (Jencks & Mayer, 1990; Leventhal and Brooks-Gunn, 2000; . Prior empirical research has thus far been unable sort out these competing hypotheses (Ellen and Turner, 1997, 2003; Brooks-Gunn, Duncan, and Aber, 1997; Leventhal and Brooks-Gunn, 2000; Duncan and Raudenbsuh, 2001; Sampson, Morenoff and Rowley, 2002; Dietz, 2002; Friedrichs, Galster and Musterd, 2003). Though our study similarly is not definitive as to neighborhood effect mechanisms, several not-mutually exclusive possibilities seem reasonable given our findings. U.S. higher-poverty neighborhoods may be associated with:

Lower-quality public schools and other institutional infrastructure (health clinics, recreational areas, family support services, etc.) that offer less skill-building resources for their students to move successfully into either post-secondary education or employment

Higher levels of exposure to violence

Social norms that are less supportive of educational attainment and more supportive of teen fertility

Seemingly attractive forms of income generation through illegal and quasi-legal activities that require little educational credentialing or participation in the mainstream labor force

Less information about and geographic access to places of higher-quality, post-secondary education and higher-wage employment

Spatial stigmatization by prospective employers

Of course, our study has identified statistical associations, not proven causal links. However, we have been careful to purge the measured association of the common confounding elements in a fashion we believe offers an important advance. Moreover, we have noted above several, not mutually exclusive hypotheses that offer plausible causal mechanisms about how neighborhood poverty rates might provide an independent contribution to the environment in which children are raised.

More work clearly is needed at drilling below readily available census data to better uncover the underlying neighborhood processes at work here (Gephart, 1997; Friedrichs, 1998; Raudenbush and Sampson, 1999; Sampson, Morenoff and Gannon-Rowley, 2002). Measures for institutional infrastructure, organizational participation, collective supervision of youth, clarity and consensus regarding group norms, intra- and

extra-neighborhood social networks for adults and children, are especially salient. In addition, much more needs to be done to measure perceptions held by external actors that may affect opportunities of neighborhood residents and, thereby, their behaviors.

But even without full understanding of the underlying causal processes, the findings here hold powerful implications for policymakers in their efforts to create neighborhoods that provide healthier developmental environments children. Numerous community development efforts are underway aimed at revitalizing distressed core neighborhoods in the U.S., often supported by municipalities and charitable foundations, such as the Annie E. Casey Foundation's Making Connections and the MacArthur Foundation's New Communities Programs. Similarly, several strands of the Department of Housing and Urban Development's assisted housing policy have similar goals for enhancing developmental contexts, such as the Moving To Opportunity (MTO) program involving rental voucher subsidies, public housing desegregation remedies in dozens of locales across the country, and redevelopment of distressed public housing as mixed-income communities through the HOPE VI program (Rubinowitz and Rosenbaum, 2000; Popkin et al., 2003; Galster et al., 2003). Our results imply that all of these initiatives should work to deconcentrate poverty both by creating mixed-income developments in revitalized core neighborhoods and targeting locations for assisted housing developments or rental subsidies in other, low-poverty neighborhoods.

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Figure 1

A Structural Model of Young Adult Outcomes

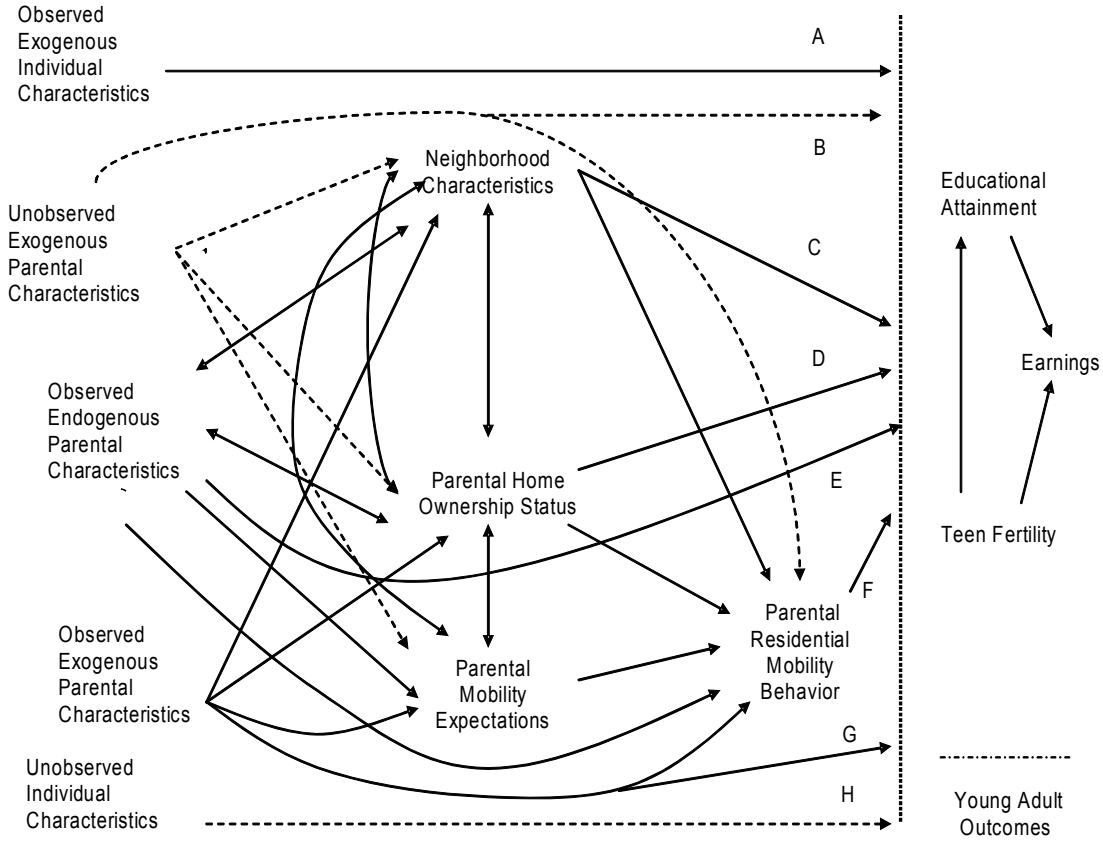

TABLE 1
Characteristics of Sample Individuals & Their Mean Circumstances During Ages 0-18
[variable acronym used in Appendix shown in brackets]

	Mean
Characteristics of Individuals in 1999	
Black Female [blackfem]	0.041
Black Male [blackmale]	0.057
White Female [whitefem]	0.331
Order of birth (1=first, 2=2nd, etc.) [birthorder]	2.233
Age in Years [age99]	28.74
Married [married]	0.481
Characteristics of Their Households (Calculated over Ages 0-18)	
Proportion of years lived in poverty [pro_live_under_poverty0to18]	0.069
Proportion of years when not changed residence [pro_stability_year0to18]	0.809
Proportion of years when head owned the home [pro_own0to18]	0.722
Proportion of years lived with two parents [pro_livew_2_parents0to18]	0.842
Proportion of years lived in metropolitan area [ave_smsa0to18]	0.731
Characteristics of Their Household Heads (=Average During Ages 0-18)*	
Education of Household Head* [ave_education_head0to18]	13.24
Occupational Prestige of Household Head* [ave_hdocc_pre0to18]	43.92
Proportion of Years Head was self employed [ave_self_employed0to18]	0.141
Proportion of Years Wife of Head employed [ave_employed_wife0to18]	0.491
Annual Hours Head Worked* [ave_annu_hrs_wkd0to18]	2123
Head self-identified as Protestant, Catholic, or Jewish [religion]	0.901
Proportion of Years Head Read Newspaper Every Day [ave_readnewspaper]	0.802
Proportion of Years Head Belonged to a Union [ave_union]	0.285
Proportion of Years Head Did Not Attend Religious Service Weekly [ave_nochurch]	0.226
Proportion of Years Head Never Participated in Social Clubs [ave_no_socialclubs]	0.538
Proportion of Years Head "Planned His/Her Life Ahead" [ave_plan_ahead]	0.581
Proportion of Years Head "Trusted Most People" [ave_trust]	0.604
Head is a Veteran [veteran]	0.392
Mother first gave birth as teen [momteen]	0.045
Head raised in large city (not suburb) [largecity]	0.419
Head raised in rural or small town (not suburb) [farm]	0.174
Characteristics of Their Neighborhood (Calculated over Ages 0-18)	
Average Number of Neighbors Head Knew by Name [ave_num_neigh_known]	12.33
Proportion of Years Lived With Family in Walking Distance [ave_relatives]	0.392
Average Percent Population below poverty in census tract [ave_perc_inc_pov]	10.25

Source: authors' analysis of PSID data for select sample (see text); N=755 (weighted)

TABLE 2

Estimated Parameters for Neighborhood Poverty and Intermediate Outcome Variables

Variable	No Child Pre-18		High School Graduate +		College Graduate		ln (Earnings)	
	no IV	IV	no IV	IV	no IV	IV	no IV	IV
Neighborhood	-0.104	-0.072	-0.059	-0.065	-0.038	-0.062	-0.021	-0.033
Poverty Rate	[0.035]***	[0.081]	[0.029]**	[0.049]	[0.031]	[0.049]	[0.012]*	[0.019]*
No Child	N/A	N/A	1.208	1.319	-0.081	0.337	0.032	0.186
Pre- Age 18			[0.458]***	[0.541]**	[0.559]	[0.639]	[0.214]	[0.225]
High School	N/A	N/A	N/A	N/A	N/A	N/A	0.111	0.037
Graduate +							[0.135]	[0.122]
College	N/A	N/A	N/A	N/A	N/A	N/A	0.361	0.325
Graduate							[0.107]***	[0.108]***
N	755	680	755	680	755	680	541	486

(robust standard errors bracketed; parameters estimated by Logit, except OLS for earnings)

*** p<.01; ** p<.05; * p<.10 (two-tailed tests)

FIGURE 2
ESTIMATED RELATIONSHIP BETWEEN CHILDHOOD NEIGHBORHOOD POVERTY AND PROBABILITY OF HAVING A CHILD BEFORE AGE 18

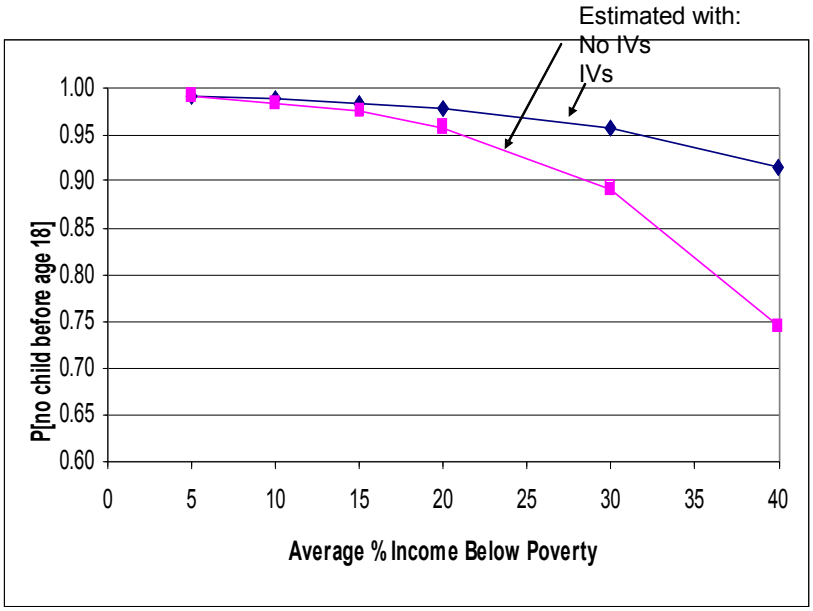

FIGURE 3
ESTIMATED RELATIONSHIP BETWEEN CHILDHOOD NEIGHBORHOOD POVERTY AND PROBABILITY OF GRADUATING FROM HIGH SCHOOL

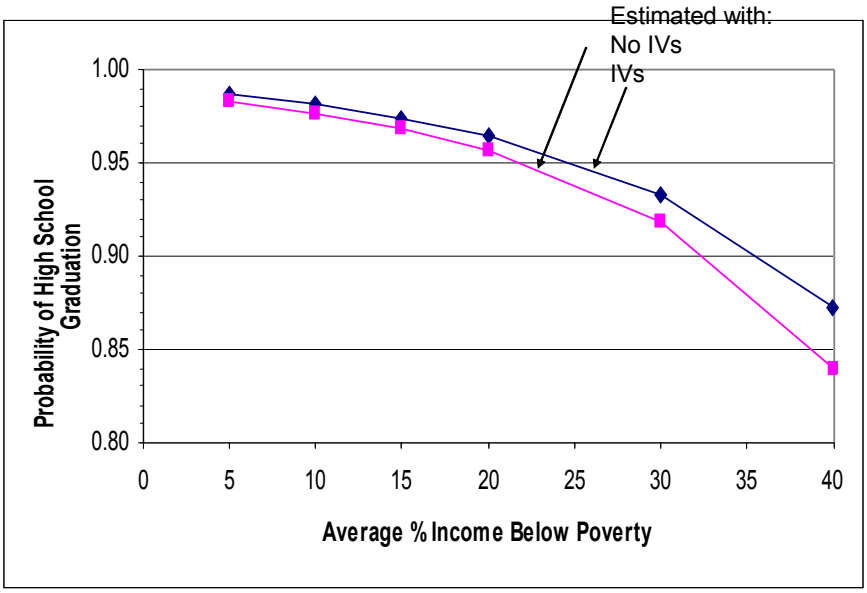

FIGURE 4
ESTIMATED RELATIONSHIP BETWEEN CHILDHOOD NEIGHBORHOOD POVERTY AND PROBABILITY OF GRADUATING FROM COLLEGE

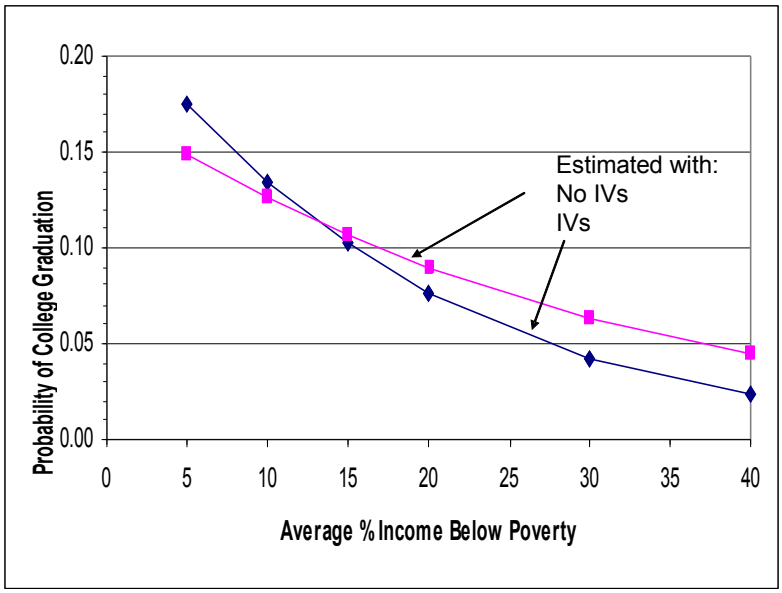

FIGURE 5
ESTIMATED RELATIONSHIP BETWEEN CHILDHOOD NEIGHBORHOOD POVERTY AND ANNUAL EARNINGS