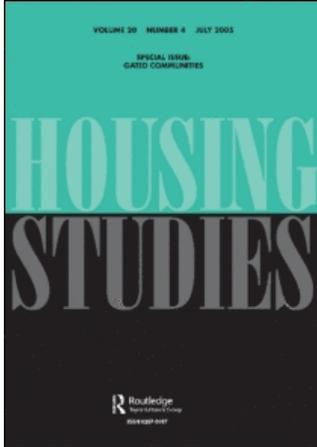


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The Influence of Neighborhood Poverty During Childhood on Fertility, Education, and Earnings Outcomes

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ABSTRACT *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. This 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, instrumental variable estimates are computed for how childhood average values of neighborhood poverty rate relate to fertility, education and labor market outcomes in later life. The paper analyzes data from the US Panel Study of Income Dynamics (PSID) that are matched with Census tract data, thereby permitting documentation of a wide range of family background and contextual characteristics. For children born between 1968 and 1974, data are analyzed on their first 18 years and various outcomes in 1999 when they are between 25 and 31 years of age. The application of instrumental variables substantially attenuates the apparent neighborhood effects. Nevertheless, support is found for the proposition that cumulative neighborhood poverty effects averaged over childhood have an independent, non-trivial causal effect on high school attainment and earnings.*

KEY WORDS: Childhood, neighbourhood effects, socioeconomic success, young adult outcomes, educational attainment, earnings, homeownership, family background, PSID

Introduction and Context

Much recent literature—emanating both from the US 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

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wider range of locations to ascertain the degree to which neighborhood characteristics affect children's developmental context (Galster, 2002, 2005; Galster *et al.* 2003).

The research here 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. This paper focuses 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 the 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 (Earls & Carlson, 2001; Ellen & Turner, 2003; Galster, 2005; Leventhal & Brooks-Gunn, 2000; Robert, 1999; Sampson *et al.*, 2002). It is sufficient to note in summary that the bulk of this literature (e.g. Brooks-Gunn *et al.*, 1997; Furstenberg *et al.*, 1999) examines factors affecting outcomes measured during childhood, ranging from pre-school to adolescence. However important such outcomes are, 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. Gottschalk *et al.*, 1994; Gottschalk, 1996; Moffitt, 1992; Pepper, 2000; Vartanian, 1999), school dropouts (e.g. Clark, 1992; Gleason & Vartanian, 1999; Mayer, 1997; Sawhill & Chadwick, 1999), crime (e.g. Freeman, 1991; Grogger, 1997; Peeples & Loeber, 1994; Sullivan, 1989), teen childbearing (e.g. Barber, 2001; Furstenberg *et al.*, 1990; Haurin, 1992; McLanahan & Bumpass, 1988; Sawhill & Chadwick, 1999), acceptance of deviant behavior (Friedrichs & Blasius, 2003), mental illness (Wheaton & Clarke, 2003), and economic idleness (Haveman & Wolfe, 1994; Mayer, 1997; Payne, 1987; Sawhill & 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 & Wolfe, 1994; Holloway & Mulherin, 2004; Vartanian, 1999) and does not adequately address the methodological challenges with which we are concerned.

As will be explained 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). The study offers what it is hoped will be advances in both areas. First, it treats the aforementioned key explanatory variables above as endogenously determined. Second, to deal both with endogeneity and selection problems, a variant of two-stage least squares is employed to derive an instrumental variable (IV) for childhood values of the neighborhood poverty and it is used to estimate relationships with young adult fertility, educational and earnings outcomes.

Data from the US Panel Study of Income Dynamics (PSID) that are matched with Census tract data are analyzed, thereby permitting documentation of a wide range of family background and neighborhood circumstantial characteristics. For children born between 1968 and 1974, an analysis is made of data on their first 18 years and various

outcomes in 1999 when they are between 25 and 31 years of age. The study finds that, all else equal, the average rate of neighborhood poverty experienced by children during ages 0–18 is strongly related to their fertility, educational attainment and earnings, although only the latter outcomes are robust to IV procedures.

The paper is organized as follows. It first offers a holistic framework for understanding how children's neighborhood environment might influence outcomes when they are young adults. This is then employed as a vehicle for evaluating a range of previous work and establishing a foundation for the modeling efforts. Second, there is a description of the two pre-eminent challenges that must be overcome if accurate measurements are to be gained of the above relationship: selection and endogeneity. Third, there is a description of the dataset and the multi-step IV estimation procedure employed to meet the aforementioned challenges. Fourth, statistical results are presented of the key relationships between a child's neighborhood poverty rate and subsequent fertility, education and earnings outcomes. The final section discusses conclusions, implications and directions for further research.

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

Potential Causal Mechanisms

Neighborhood 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 & Brooks-Gunn, 2000; Sampson *et al.*, 2002). Prior empirical research has thus far been unable to sort out definitively these competing hypotheses (Brooks-Gunn *et al.*, 1997; Dietz *et al.*, 2002; Duncan & Raudenbush, 2001; Ellen & Turner, 1997, 2003; Friedrichs *et al.*, 2003; Leventhal & Brooks-Gunn, 2000; Sampson *et al.*, 2002). It is thought that several not-mutually exclusive possibilities may explain why highly disadvantaged neighborhood conditions experienced during childhood could influence young adult outcomes related to fertility, educations and earnings:

- 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 complete high school and move successfully into either post-secondary education or higher-paying employment.
- Higher levels of exposure to violence, which lead to stresses inhibiting ability to concentrate on their studies or work.
- Social norms that are less supportive of educational attainment and regular employment, and more supportive of teen fertility.
- Seemingly attractive forms of income generation through illegal and quasi-legal activities in the neighborhood 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 of residents in disadvantaged neighborhoods by prospective employers and gatekeepers of post-secondary educational institutions.

Prior Statistical Work and Its Shortcomings

How neighborhood context affects children and adults has been a burgeoning field of empirical enquiry internationally, as evinced by several recent comprehensive and methodologically critical reviews of the literature (Dietz, 2001; Duncan *et al.*, 1997; Duncan & Raudenbush, 1999; Earls & Carlson, 2001; Ellen & Turner, 1997, 2003; Friedrichs *et al.*, 2003; Galster, 2003a, 2005; Gephart, 1997; Leventhal & Brooks-Gunn, 2000; Robert, 1999; Sampson *et al.*, 2002). There will be no attempt to duplicate these reviews here, but the methodological critiques that motivate the current paper will be noted. 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, thereby biasing the apparent impacts of neighborhood. None meet fully the fundamental statistical challenges posed by selection and endogeneity, a topic which is now examined.

A Holistic Framework

In order to provide a framework for both illustrating the limitations of previous studies and guiding the efforts in this study, a model is presented and is portrayed in Figure 1. It is posited that young adult outcomes of interest (shown on the right panel of Figure 1) are

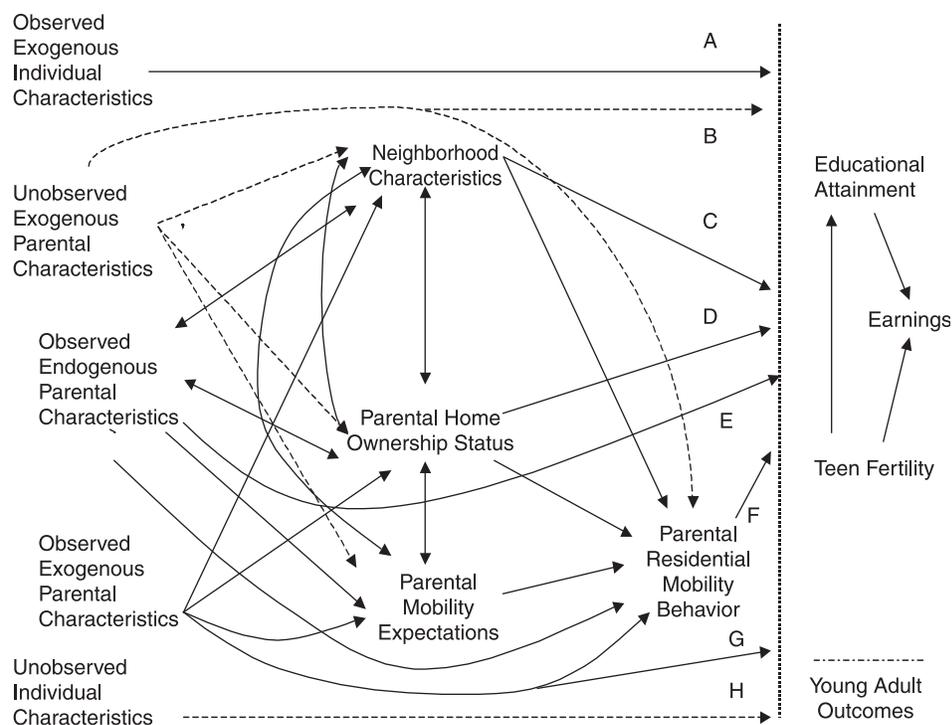


Figure 1. A structural model of young adult outcomes

determined by four sets of exogenous or predetermined variables: observed characteristics of individual children (path A: e.g. gender, race), unobserved characteristics of individual children (path H: e.g. intelligence), observed parental characteristics (path G: e.g. education, age), and unobserved parental characteristics (path B: e.g. ambition, present orientation, concern for their children's future). These unobserved parental factors (shown as dotted lines in Figure 1) are the source of omitted variables bias associated with selection, which will be discussed 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: e.g. parental employment and income history). Finally, it is seen that young adult outcomes are 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).

The key innovation of the model is the specification of the intervening variables neighborhood location / homeownership status / mobility expectations / household socioeconomic status as 'mutually causal phenomena'. It is argued 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 will be returned to below. Brief, heuristic rationales are offered for these bi-directional causal relationships portrayed in Figure 1; supportive evidence is summarized in the aforementioned reviews:

- *Neighborhood and homeownership status*: If economic status, ethnicity or other factors constrain a household to a set of neighborhoods that are afflicted with numerous social problems and concomitant expectations of property value deflation, there will be little motivation to buy a home there; 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.
- *Neighborhood and homeownership status AND mobility expectations* (expected duration of stay): If someone expects to remain long in a dwelling, given their employment and life-cycle stage situation, they may be more willing to bear the high transactions costs of buying *and* will try harder to avoid declining neighborhoods; in turn, if someone is willing and able to purchase a home, and succeeds in doing so in a good neighborhood, they 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 promotes parental efficacy and marital stability, as well as a key financial resource for furthering children's education.
- *Neighborhood and parental characteristics*: Parental income and non-housing wealth will influence which neighborhoods can feasibly be chosen; neighborhood location with respect to potential employment and job information networks, social milieu and environmental features can influence, in turn, parents' health, employment and stigmatization by potential employers and, thereby, their income and wealth subsequently.

The foregoing relationships can be summarized in the following set of equations:

$$HO = f(N, ME, H, [X_1]) \quad (1)$$

$$N = f(HO, ME, H, [X_2]) \quad (2)$$

$$ME = f(HO, N, M, H, [X_3]) \quad (3)$$

$$M = f(N, H, ME, [X_4]) \quad (4)$$

$$H = f(N, HO, [X_5]) \quad (5)$$

where:

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, to be presented in more detail below.

The holistic framework not only underscores the forthcoming econometric specification, but it provides a context for comprehending the difficult challenges faced by investigators of neighborhood effects. The paper now turns to a discussion of these challenges.

Challenges in Measuring Determinants of Young Adult Outcomes

The holistic framework portrayed in Figure 1 suggests 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, homeownership status, mobility and certain parental characteristics. These challenges involve selection and endogeneity.

The Challenge of Selection

Bias in the neighborhood-outcome relationship due to household selection 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 (Dietz, 2001; Duncan *et al.*, 1997; Duncan & Raudenbush, 1999; Manski, 1995, 2000). Ordinary least squares regression provides biased estimates of the effect of neighborhood on outcomes because the neighborhood variable is correlated with the disturbance term in the regression. 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?¹ The implicit omitted variables' relationships in this selection problem are portrayed 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 (Manski, 1995; Tienda, 1991). 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 & White, 1997; Haurin *et al.*, 2002a, 2002b).

Analysis of data on outcomes that can be produced by an experimental design whereby individuals or households are randomly assigned to different neighborhoods has often been seen as the preferred method for avoiding biases from selection. In this regard, the US 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 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 receiving tenant-based rental subsidies (Sampson *et al.*, 2002). 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. However, even the treatment group receiving intensive mobility counseling and assistance, although programmatically constrained to move initially to a neighborhood with less than 10 per cent poverty rates, has the ability nevertheless to choose neighborhoods varying on their school quality, homeownership 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; see Goering *et al.*, 2002). Thus, even studies based on MTO data cannot fully finesse the selection bias issue.

However, the challenge is even deeper. If Figure 1 was adopted as a working premise, the selection process would become much more complicated than merely the parents' 'independent' selection of neighborhood. It is the view of the authors of this study that the holistic challenge embodies the 'interdependent' selections of neighborhood, homeownership status and expected mobility.

The Challenge of Endogeneity²

Previous statistical studies have taken only a partial view of the causal patterns embodied in Figure 1 and Equations (1)–(5); virtually all have omitted one or more of the intervening variables. To the extent that these variables are mutually causal they will be correlated with the neighborhood variable. Under these circumstances, the coefficient will be a biased estimate of the effect of neighborhood on outcomes because the neighborhood variable is correlated with the disturbance term in the regression. As in the case of selection there is an omitted variables bias problem, but here it is due to the neighborhood variable's causal relationships with other, uncontrolled variables that affect outcomes as well.

However, the solution to this challenge may not be as straightforward as including all intervening variables in the outcome equation. If the causal relationships are as strong

as has been posited above, these intervening variables may be so highly correlated that multicollinearity may arise as a new econometric challenge.

Meeting the Challenges through an Instrumental Variables Approach

It is thought that a promising strategy in response to *both* selection and endogeneity challenges in an analysis of households sampled from non-experimental circumstances is the application of instrumental variable techniques (IV).³ The current study employs a variant of the well-known two-stage least squares technique for producing IVs (Murray, 2006). In the first stage of this technique, the endogenous variable in question (e.g. a neighborhood characteristic) is regressed on one or more other exogenous variables that, hopefully, are highly correlated with the endogenous variable but uncorrelated with the disturbance term. In a model such as (1)–(5), the explanatory variables in this first-stage regression include all exogenous or predetermined variables that appear on the right-hand sides of any of these Equations ([X]). The predicted values for the endogenous variable yielded by this first-stage regression are substituted (in this case, after further manipulation explained below) for the actual endogenous variable's values in a second-stage regression explaining outcomes. It is believed that this newly constructed IV will not be correlated with the disturbance term in the outcome regression (thereby avoiding omitted variable bias) or with other intervening endogenous variables (thereby avoiding multicollinearity).

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 *et al.* (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 & McLanahan (1996) used citywide labor market conditions as identifying variables predicting neighborhood high school dropout rates. It is thought that this strategy for instrumenting not only neighborhood-level but individual-level variables with corresponding variables measured at larger geographic scales is fruitful, and it is used in the present study together 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 analyzed is a prerequisite for understanding the particular instrumental variables approach used here. Beginning in 1967, the PSID began interviewing 5000 American families. In every year through 1996 and every other 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 10000 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, the present analysis is limited to a sample designed to be nationally representative of the US population in 1967. Differential

attrition over the course of the panel is accounted for 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. A PSID geo-matched file is employed, which appends the child's census tract identifier to each observation. Values of census tract variables are interpolated for observations between census years. Therefore it is possible to observe annually the household and (approximate) neighborhood environments in which the sample individuals spend their childhood.

The analysis focuses on the PSID cohort of children born during the period 1968–74 because it provides 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 probably had completed their education and had ample opportunity to enter the labor force.⁴ Here, as throughout, statistics weighted by PSID sampling weights are presented, 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 are included as controls in the model (Ginther *et al.*, 2000). The authors believe that the work here has met this condition in a way superior to prior work. The study not only controls for a wide range of objective characteristics of the household but, unlike prior work, also controls for several attitudinal and behavioral characteristics of the head. Descriptive statistics for these numerous aspects of the sample of children that were analyzed—their themselves, their households, the heads of their households, and their neighborhoods as they were growing up—are provided in Table 1.

Measures of Key Explanatory Variables and Outcomes

This paper considers a commonly used measure of a disadvantaged neighborhood environment: percentage of population with household incomes below the US federal poverty standard. In each case information from the census tract is used, an area of approximately 4000 inhabitants, tabulated in the decennial *Census of Population and Housing*, with values interpolated for inter-census years.⁵ On average during their childhood, children in the sample experienced a census tract having a 10.5 per cent poverty rate, slightly below the national average during this era.

Several studies suggest that census tract data on socio-economic 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: a multi-dimensional index of social processes (Cook *et al.*, 1997); unsupervised peer groups and organizational participation (Sampson & Groves, 1989); informal social control (Elliott *et al.*, 1996); collective efficacy (Sampson *et al.*, 1997); multiple dimensions of social capital (Sampson *et al.*, 1999); and perceived disorder (Coulton *et al.*, 1999; Kohen *et al.*, 2002).⁶ However, it is recognized that neighborhood poverty is not a proximate measure of the underlying processes that may be responsible for neighborhood effects, and thus interpretation of regression results remains somewhat ambiguous, a topic which will be returned to in closing. Several potential causal mechanisms may underpin the correlations between neighborhood poverty during childhood and young adult fertility, education and earnings. These include: low-quality local schools and institutions; greater exposure

Table 1. Characteristics of sample individuals and their mean circumstances during ages 0–18

	Mean
<i>Characteristics of individuals in 1999</i>	
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
<i>Characteristics of their households (calculated over ages 0–18)</i>	
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
<i>Characteristics of their household heads (* = average during ages 0–18)</i>	
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
<i>Characteristics of their neighborhood (calculated over ages 0–18)</i>	
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 per cent population below poverty in census tract [ave_perc_inc_pov]	10.25

Note: Variable acronym used in Appendix 2 shown in square brackets.

Source: Authors' analysis of PSID data for select sample (see text); $n = 755$ (weighted)

to violence, subcultural social norms, lack of collective social controls, resource-poor interpersonal networks and spatial stigmatization.

The 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 three key outcomes: fertility prior to age 18, school attainment and earnings as of 1999. A total of 94 per cent of the sample children born between 1968–74 had not had a child prior to age 18. By 1999, 90 per cent of this cohort had graduated from high school or obtained a Graduate Equivalent Degree, and 20 per cent had graduated from a four-year college or university. The PSID only collects income information from respondents who have formed their own household and worked at some time during the previous year, so income statistics and regression results reported refer only to this subset of the cohort. However,

81 per cent of the cohort had formed a household and were employed by the time of the 1999 survey. On average in 1998, this cohort of household heads who were employed part- or full-time individually earned \$17 348. Note that the analysis of earnings thus excludes all full-time students who were not employed during 1998.

Model and Estimation Procedure

Model Overview

Expressed symbolically, the model for outcomes of young adults is:

$$\text{FER} = f(N_c, \text{HO}_c, M_c, H_c, [X_6], [X_7], [X_8]) \quad (6)$$

$$\text{HS} = f(N_c, \text{HO}_c, M_c, H_c, [X_6], [X_7], [X_8], \text{FER}) \quad (7)$$

$$\text{COL} = f(N_c, \text{HO}_c, M_c, H_c, [X_6], [X_7], [X_8], \text{FER}) \quad (8)$$

$$\text{INC} = f(N_c, \text{HO}_c, M_c, H_c, [X_6], [X_7], [X_8], \text{FER}, \text{HS}, \text{COL}, \text{HRS}) \quad (9)$$

where:

FER = 1 if reached age 18 without having a child, 0 otherwise

HS = 1 if received a high school diploma or equivalency degree by 1999, 0 otherwise

COL = 1 if received a college bachelor's (4-year) degree by 1999, 0 otherwise

INC = natural logarithm of 1998 income from earnings (only for those who had formed a household and were employed some time during 1998)

N_c = average poverty rate in census tract during ages 0–18

HO_c = proportion of childhood years that household owned the dwelling occupied

M_c = proportion of childhood years that the household moved between dwellings

H_c = proportion of childhood years that the household earned less than poverty income

$[X_{6c}]$ = exogenous characteristics of the individual in 1999; see Table 1 for listing

$[X_{7c}]$ = exogenous characteristics of the household during childhood; see Table 1

$[X_{8c}]$ = exogenous characteristics of the neighborhood during childhood (average number of neighbors the household knew by name and proportion of years when family members were within walking distance); see Table 1

HRS = hours worked during 1998 and c subscripts indicate variables computed for the entire childhood period.

The coefficients of variables in the model above were estimated using ordinary least squares (OLS) when the outcome is continuous (Equation (9)) and logit when the outcome is dichotomous (Equations (6)–(8)). The sample for estimating these coefficients includes all children in the initial 1968–74 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 virtually identical right-hand sides measuring (exogenous or predetermined) characteristics of the individual and the individual's household and (endogenous) aforementioned childhood conditions; descriptive statistics of these last variables are presented in Table 1. For all of these variables in the model, proportional figures calculated over the first 18 years of the child's life (or for however many years data are available) are used.

It should be noted that the set of outcomes are modeled 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.

Instrumentation Procedure

It is suspected that N_c will be correlated with the disturbance terms in Equations (6)–(9) because of aforementioned selection and endogeneity issues. Therefore, the study experiments with instrumental variables. The approach for estimating IVs proceeds in the following three steps.

First, an OLS regression is estimated based on observations of individual child-years. In this regression the left-hand side is the observed value of the census tract poverty rate in a given child's neighborhood in a particular PSID year and the right-hand side contains observed values of every exogenous variable $[X]$ in the system of Equations (1)–(5). These exogenous variables include contemporaneous values of 'countywide' characteristics corresponding to the N_c , HO_c , M_c , and H_c variables and dummy variables for calendar year. The complete listing is shown in Appendix 1. In this first step, the regression is estimated based on all observations from age 1 to 18 of each child in the sample. All observations of children having data for at least 10 years of their childhood were included. What is of prime importance here is how well the first-stage regressions predict the values of N_c , not their estimated parameters in and of themselves, since this will determine the power of the instrument (Murray, 2006). As a result, for this first stage OLS is used, not needlessly complicated panel estimation procedures.

In the second step of the approach, the aforementioned regression is employed to generate predicted values of neighborhood poverty for each of the first 18 years of each child's life, based on values of all exogenous variables appropriate for the given year. There must now be a switch from a child-year unit of observation to a child-childhood average unit of observation, which necessitates a step not normally required in two-stage least squares. In the third step the 'average' of these predicted values is computed over all observed years of childhood. These childhood averages of annual predicted values for each sampled individual become the IV measures for neighborhood poverty experienced during childhood N_c .

Identifying and Evaluating 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 Equation (1)–(5) as there are endogenous variables included in each equation. This condition is met; indeed, the equation system (1)–(5) is over-identified.

Moreover, each equation must have one or more clearly exogenous variables that appear only in the given equation as strong predictors. In the case of childhood neighborhood poverty rate, the corresponding county-level value was employed as the unique identifying instrument.⁷ Indeed, 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 the first-stage regression for neighborhood poverty rate performed moderately well (the R^2 was 0.45). Moreover, because there were 12 500 child-year observations in this first-stage regression and only 31 regressors, confidence is high that the study

substantially reduced the bias associated with using OLS coefficients (Han & Hausmann, 2005) and avoided the problem of weak instruments (Murray, 2006).⁸

Complicating Issues

Five technical issues require further discussion. The first of these is the operational definition of neighborhood. While imperfect, census tracts are employed as the preferred approximation to neighborhood, as is common in US studies. However, until 1990 rural areas were not divided into census tracts. In order to avoid the potential problems of (1) missing data and (2) mixing urban and rural scales of 'neighborhood', the analysis is confined to children who spent at least 12 of their first 18 years in tracted, metropolitan area neighborhoods.

Second, the attitudes and behaviors of the household head that are employed as controls (see Table 1) are not measured annually in the PSID. Indeed, for most variables the questions were asked only during the years 1968–72.⁹ Each attitude and behavior 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 0.17 to 0.40. 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 0.70. Pair-wise correlations between responses to the question 'plan ahead' over the six points at which this question was asked ranged from 0.20 to 0.46; Cronbach's alpha was 0.77. Pair-wise correlations between responses to the question 'trust' over the five points in time at which this question was asked ranged from 0.40 to 0.54; Cronbach's alpha was 0.81.

Third, although the model provides unusually strong controls, the spectre of omitted variables must be considered nevertheless. There is no control in (6)–(9) for neighborhood characteristics or numerous personal characteristics of the young adult in 1999 (such as a variety of experiences, attitudes and behaviors). However, these omitted variables do not confound the basic estimates of childhood neighborhood effects. If these variables prove to be uncorrelated with N_c there will be no bias in its coefficient, even though the overall explanatory power of the outcomes regressions will be reduced. On the other hand, it may be that these omitted variables may be correlated because they are (partially) influenced by the childhood endogenous and exogenous characteristics specified in (6)–(9). In this case reduced-form estimates of childhood neighborhood poverty on young adult outcomes are essentially obtained, some of which may transpire through the omitted (but intervening) variables.

Fourth, given the conceptual model in Figure 1 it would have been desirable to simultaneously instrument for N_c but *all* the endogenous variables in (6)–(9): HO_c , M_c and H_c . This would have permitted the estimation of less-biased parameters for these variables as well. Unfortunately, in the preliminary experiments it proved challenging to identify instruments that uniquely identified all these variables. The result was that the resulting instruments for N_c , HO_c , M_c and H_c proved too inter-correlated to be meaningfully employed in the same regression.

Finally, as noted above, the instrumentation procedure involves estimates for N_c that are 'multi-year averages of predicted values of neighborhood poverty'. Given that the distribution of this new, 'average' instrument is not known, the standard errors yielded by conventional OLS or logit procedures cannot be interpreted in a straightforward fashion. Thus, as is standard practice under these circumstances, 'bootstrapped' parameter values will be reported, as estimated by STATA when examining the IV estimates.

Results

Overview and Discussion of Control Variables

Before turning to results for the neighborhood poverty variable of primary interest, the study briefly highlights some of the more interesting relationships involving other variables; details are presented in Appendix 2. As an overarching assessment, all four outcome equations evinced decent explanatory power according to the criteria appropriate for logit and OLS estimations (see Appendix 2). Moreover, there is strong support for the specification of recursive relationships between teen fertility, educational attainments and subsequent earnings. Not surprisingly, having a child before 18 clearly appears to reduce the chances of graduating from high school. Educational attainments, especially a college degree, are strongly related to earnings, in turn. Finally, there is strong support for the claim here that the intervening variables (that it is argued are mutually causal with neighborhood) are important predictors of young adult outcomes. Each of these intervening childhood conditions—time spent in a poor household, a two-parent household, an owner-occupant household, and a residentially stable household—proved predictive of one or more subsequent outcomes. This reinforces the contention here that models of neighborhood effects that do not control for these contexts probably suffer from severe omitted variables bias.

As for predictors of not having a child before age 18, growing up in a family that did not move often and whose head aspired to ‘planning ahead’ proved efficacious. Perhaps it is the case that residential stability both reduces the need of adolescents to make friends by engaging in risky behaviors and increases the likelihood that neighbors will provide both normative and supervisory sanctions against such behaviors. Future-oriented parents may be more prone to instill these attitudes in their children, thereby encouraging them to avoid future prospect-stunting actions such as teen childrearing. Older cohorts in the sample, those born in the late 1960s, were more likely to have a child as a teen. This might be attributable to radically shifting sexual mores during that period, although this is only speculation. It is also noted that women (both black and white roughly equally) are substantially less likely to reach age 18 without having a child, although part of this result is probably due to gendered response bias.

Consider next the educational attainment equations. Not surprisingly, having well-educated parent(s) was strongly correlated with greater chances of later graduating from high school and college. More surprising, the same pattern held for children raised by parents who knew more of the neighbors by name. There is uncertainty about why this greater degree of parental neighborhood social integration (controlling for mobility) seemingly translates into greater educational achievements, although it may be due to the implied intensification of neighbors’ monitoring of children’s behaviors, both pro- and anti-educational. This explanation would also be consistent for the finding that children of parents who never belonged to social organizations were less likely to graduate from high school. Children raised in homes owned by their parents had higher probabilities of completing high school and (especially) college. There are a number of developmental and behavioral reasons why this may be so, which are explored in depth in another paper (Galster *et al.*, forthcoming). Older members of the cohorts evinced higher achievements, probably because they had more time to obtain graduate equivalency exams and complete college coursework. Interestingly, blacks were more likely to graduate from high school than whites in the sample (black makes statistically significant more so), once the battery of parental and contextual characteristics were controlled. Other statistically significant

relationships do not have obvious explanations. Children from homes where the head was more trusting of other people, a veteran or not a union member were less likely to graduate from high school. Children who were raised in a large city (instead of a suburb), and came from homes where both parents were not present were more likely to get a college degree.

Finally, in the wage earnings equation it was observed that children raised by a head who was more future-oriented earned more, all else equal. This suggests that these children have learned a set of attitudes and behaviors related to delayed gratification and longer-term strategizing that has substantial labor market payoffs. Children from households experiencing longer spells of poverty and/or single parenting earn less, consistent with the hypothesis that material and psychological deprivation associated with these circumstances creates developmental disadvantages with lasting earnings impacts. Older cohorts earn more, as is predicted from their typically longer tenure in the workforce. Employees earn more who are better-educated, white males and work more weeks annually, as expected. It is less clear why children raised in dual-parent homes with mothers who worked more earned more as young adults, though role modeling may be at work.

Of course, the results of primary interest relate to neighborhood poverty during childhood; these are reported in the columns denoted 'no-IV' in Table 2. As an overview, it is found that experiencing more neighborhood poverty on average as a child is associated in a statistically significant way (both directly and indirectly) with: (1) greater chances of having a child before age 18; (2) lesser chances of graduating from high school; and (3) earning lower wages as a young adult. Experiencing more neighborhood poverty as a child is also associated with a lower rate of college graduation, although the coefficient is only slightly larger than its standard error. The non-trivial magnitudes of these associations can be assessed as follows. For each one percentage-point higher average childhood poverty rate, the probability of the individual having a child before age 18 increases by 0.005, the probability of the individual graduating from high school decreases by 0.006 (both calculations conducted at the respective outcome means), and the individual's earnings decrease by 2.1 per cent.

Several features about these relationships warrant emphasis, which suggest that they are a lower-bound estimate. First, the earnings relationship is only estimated for those who have formed households and were employed in 1998, and thus does not count any potential impacts of childhood neighborhood poverty on likelihoods of forming households or being employed. Second, the earnings relationship does not consider any potential impacts of childhood neighborhood poverty on number of hours worked if employed. (However, in preliminary experiments it was not possible to identify any strong relationship between neighborhood poverty and either employment or hours worked.) Third, the relationships for education and earnings are only the direct effects, and do not consider the indirect paths from teen fertility to educational attainments to earnings.

Assessing Magnitude of Implied Impacts of Childhood Neighborhood Poverty

To explore this last aspect above further, simulations were conducted that utilized the entire recursive structure of the outcomes. Counterfactual changes were imposed 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. Predicted changes in these fertility probabilities were then added as input into the models explaining educational attainments, along with initial changes in childhood neighborhood

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 poverty rate	-0.104 [0.035]***	0.04 [0.074]	-0.059 [0.029]**	-0.04 [0.048]	-0.038 [0.031]	-0.015 [0.039]	-0.021 [0.012]*	-0.019 [0.014]
No child pre-age 18	N/A	N/A	1.208 [0.458]***	1.44 [0.368]**	-0.081 [0.559]	0.481 [0.610]	0.032 [0.214]	0.101 [0.140]
High school graduate +	N/A	N/A	N/A	N/A	N/A	N/A	0.111 [0.135]	0.234 [0.117]
College graduate	N/A	N/A	N/A	N/A	N/A	N/A	0.361 [0.107]***	0.261 [0.113]***
<i>n</i>	755	680	755	680	755	680	541	486

Note: Robust standard errors in brackets; estimated by bootstrap technique in case of IVs.

Parameters estimated by logit, except OLS for earnings.

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$ (two-tailed tests).

poverty, yielding new estimates for these intermediate outcomes. Finally, 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). The conclusion is that realistic variations in average neighborhood poverty rates experienced by the 1968–74 cohort from ages 0 to 18 are associated with a substantial variation in their outcomes in 1999, all else equal. Take an admittedly extreme, although certainly plausible, difference in neighborhood environments. Compared with otherwise identical children raised by otherwise identical parents in a neighborhood with a low average poverty rate of 5 per cent (approximately half the sample mean), children experiencing an average 40 per cent rate (a conventional US benchmark for ‘concentrated poverty’ neighborhoods; Jargowsky, 1997) are predicted by the simulation to have a:

- 24 percentage-point (24 per cent of the mean) greater chance of having a child before age 18;
- 14 percentage-point (15 per cent of the mean) lower probability of graduating from high school;
- 10 percentage-point (70 per cent of the mean) lower probability of graduating from college; and
- \$13 334 (54 per cent of the mean) lower annual earnings.

It is believed that these simulated values represent socio-economically significant differences. This evidence is supportive of the position that poverty neighborhoods in America create important limitations on the life chances of children who are raised there.

The Importance of Neighborhood Poverty Relative to Other Predictors

The prior simulation results beg the question of how important is neighborhood poverty compared to other characteristics of the child’s household or residential environment. The answers are explored by employing a simulation process analogous to the one above, except that it is applied to selected variables besides neighborhood poverty that proved predictive in the models. To ease comparisons across multiple characteristics, the 10th, 25th, 50th, 75th and 90th percentile values for each are identified and the associated outcome value is computed when all other predictors are held at their sample means. In each case the 10th percentile represents the least desirable situations. The results are presented in Table 3. As a convenient summary means of comparing strength of relationships, the differences in the given outcome associated with changing the given predictor from the 10th to the 25th percentile and from the 25th percentile to the mean values are computed, as shown in the last two rows of each section of Table 3.

Consider initially the strength of childhood neighborhood poverty’s relationships with young adult outcomes relative to the other aspects of context that are viewed here as endogenous. Neighborhood poverty proves stronger than: (1) residential stability for all outcomes; (2) family poverty for all outcomes except earnings; (3) family homeownership for all outcomes except college degree. Consider next the strength of childhood neighborhood poverty’s relationships with young adult outcomes relative to two exogenous characteristics of parents that often proved predictive. Neighborhood poverty proves stronger than: (1) living with both parents for all outcomes except earnings; and (2) parental education for all outcomes except educational attainment. These results suggest

Table 3. Comparative effects of childhood neighborhood poverty and other variables

Non-IV results from Table 3 re-organized 1-07						
	<i>Probability of having no child by Age 18</i>					
	Neighborhood poverty	Family poverty*	Family owns home*	Residential stability	Lived with 2 Parental*	
10th percentile	0.880	0.948	0.985	0.933	0.955	
25th percentile	0.945	0.967	0.978	0.957	0.964	
Mean	0.968	0.968	0.968	0.968	0.968	
75th percentile	0.984	0.973	0.952	0.979	0.971	
90th percentile	0.988	0.973	0.949	0.982	0.971	
10th to 25th percentile	0.065	0.019	-0.007	0.024	0.009	
25th percentile to mean	0.023	0.001	-0.010	0.011	0.004	
	<i>Probability of High School Diploma by 1999</i>					
	Neighborhood poverty	Family poverty*	Family owns home*	Residential stability	Lived with 2 Parental*	
10th percentile	0.891	0.905	0.925	0.958	0.946	
25th percentile	0.935	0.951	0.941	0.955	0.951	
Mean	0.953	0.953	0.953	0.953	0.953	
75th percentile	0.969	0.964	0.963	0.949	0.955	
90th percentile	0.973	0.964	0.964	0.947	0.955	
10th to 25th percentile	0.044	0.046	0.016	-0.003	0.005	
25th percentile to mean	0.018	0.002	0.012	-0.002	0.002	
	<i>Probability of having College Degree by 1999</i>					
	Neighborhood poverty	Family poverty*	Family owns home*	Residential stability	Lived with 2 Parental*	
10th percentile	0.041	0.101	0.032	0.063	0.178	
25th percentile	0.055	0.068	0.047	0.065	0.093	
Mean	0.067	0.067	0.067	0.067	0.067	
75th percentile	0.085	0.057	0.097	0.069	0.045	
90th percentile	0.093	0.057	0.103	0.070	0.045	
10th to 25th percentile	0.014	-0.032	0.015	0.002	-0.085	
25th percentile to mean	0.011	-0.002	0.020	0.001	-0.026	

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Table 3. Continued

Non-IV results from Table 3 re-organized 1-07

	<i>Earnings of householders employed during 1998</i>				
	Neighborhood poverty	Family poverty*	Family owns home*	Residential stability	Lived with 2 Parental*
10th percentile	12953	10494	19627	16254	9962
25th percentile	15468	16882	18416	16904	14467
Mean	17348	17348	17348	17348	17348
75th percentile	20139	20801	16321	18005	21495
90th percentile	21248	20801	16166	18286	21495
10th to 25th percentile	2516	6388	- 1211	651	4505
25th percentile to Mean	1880	466	- 1067	444	2881

Note: 10th percentile = highest neighborhood and family poverty; lowest homeownership and stability; least time spent living with 2 parents; lowest parental education.

that the cumulative importance of childhood neighborhood poverty may be at least as great as many other family and contextual characteristics that have often been central to the child development discussion (e.g. Haveman & Wolfe, 1994).

Non-linear Effects of Neighborhood Poverty

There is considerable theoretical basis for arguing that the impact of neighborhood poverty in shaping the developmental context for children will be non-linear, and the US empirical evidence consistently supports this position (see reviews in Galster, 2002, 2003b). The current results add still more support. Although it is recognized that the logit and semi-logarithmic models necessarily involve some non-linearities, it is thought that the extent of such evinced in the estimated parameters is noteworthy. Table 3 provides the clearest presentation of this. The difference in the probability of having no child by age 18 between growing up in a neighborhood with the mean poverty rate (10 per cent) compared to the 25th percentile poverty rate (19 per cent) is estimated as 0.023; the comparable difference between neighborhoods with the 25th percentile poverty rate and 10th percentile poverty rate (28 per cent) is 0.065, almost three times as great. Similarly, the difference in the probability of graduating from high school between growing up in a neighborhood with the mean poverty rate compared to the 25th percentile poverty rate is estimated as 0.018; the comparable difference between neighborhoods with the 25th percentile poverty rate and 10th percentile poverty rate is 0.044, over twice as great. Qualitatively similar non-linearities are evinced in the case of earnings as well. The consequences of neighborhood poverty in deleteriously distorting the developmental environment for children thus appear especially pernicious when it passes roughly 20 per cent.

Comparing Results for Neighborhood Poverty With and Without IVs

Given the absolute and relative magnitudes of variation demonstrated by the prior simulations, the study explores the extent to which they probably reflect causal influences of neighborhood poverty instead of biases from selection and simultaneity issues. Therefore, this section presents parameters of the childhood neighborhood poverty rate estimated using IVs generated as per the procedures described above. These are shown in the right-hand member of each pair of columns of Table 2.

The main penalty from employing two-stage least squares estimators as IVs is an increase in the standard errors compared to OLS (Murray, 2006). The effort is doubly impaired by the need to estimate the standard errors via bootstrapping methods. Here these penalties have the effect of rendering all neighborhood poverty IV coefficients statistically insignificant. However, it is inappropriate to interpret this as the coefficients are zero, given the aforementioned difficulties with standard errors. Instead, the focus is on how the point estimates of the coefficients have changed.

In this regard, for all outcomes it can be seen that the magnitude of the childhood neighborhood poverty coefficient falls substantially when IVs are applied. The magnitude of decline varies by outcome: (1) no child before age 18 by 62 per cent; (2) high school graduate by 32 per cent; (3) college graduate by 61 per cent; and (4) earnings by 10 per cent. Moreover, the sign switches in the teen fertility equation, which suggests that there is no reliable evidence of an independent causal effect whereby neighborhood poverty leads to greater teen childbearing rates.

In sum, the IV evidence in Table 2 strongly supports the earlier concerns that neighborhood effect models that fail to confront the empirical challenges of selection and endogeneity will produce biased results. Nevertheless, although it is thought that OLS modeling overstates the causal impact of neighborhood poverty, it should be added that the application of IV techniques did not make the apparent effect of neighborhood poverty disappear. Indeed, it is thought that the totality of evidence supports the hypothesis of an independent, non-trivial impact of childhood neighborhood poverty on high school attainment and earnings, controlling for a wide range of parental and other background characteristics.

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 (Earls & Carlson, 2001; Ginther *et al.*, 2000; Leventhal & Brooks-Gunn, 2000; Robert, 1999; Sampson *et al.*, 2002) and the present study adds yet more variance. Indeed, the implied magnitude of childhood neighborhood poverty impacts presented in Table 3 is greater than that measured by earlier studies using OLS with comparable neighborhood measures and outcomes: teen fertility (Hogan & Kitagawa, 1985; Brewster *et al.*, 1993; Brewster, 1994a, 1994b), educational attainment (Aaronsen, 1998; Clark, 1992; Datcher, 1982; Duncan, 1994; Garner & Raudenbush, 1991), employment (Datcher, 1982; Vartanian, 1999) and earnings (Page & Solon, 1999).¹⁰ One possible reason for this is that neighborhood poverty is measured averaged over childhood, not just for a shorter span, as most other studies have. Thus the study measures the 'cumulative impact' of this neighborhood condition.¹¹ Another reason is that a recursive relationship is modeled among various outcomes, thereby allowing both direct and indirect effects of neighborhood poverty.

Conclusions, Caveats and Next Steps

This paper represents the first attempt to estimate the cumulative effect of neighborhood poverty on several interrelated children's outcomes in later life in the context of a holistic model involving the simultaneous parental choice of neighborhood, mobility, and homeownership status. It has argued that an IV approach based on such a model is helpful for obtaining estimates of neighborhood effects that are purged from the twin confounding influences of selection and endogeneity. Using a cohort of children born 1968–74 and interviewed through the PSID in 1999, the IV estimates provided indications that these cumulative neighborhood poverty effects averaged over childhood have an independent, non-trivial causal effect on high school attainment and earnings. The IV evidence is not compelling with regard to teen fertility or college attainment.

As befits a prototype, the IV modeling experiments suggested that this approach can only reach its full potential if stronger and unique instruments for childhood neighborhood poverty rate can be identified. A 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 the system of equations. 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. The efforts here fell short in this regard, yielding instruments for many endogenous variables that were too collinear to be employed in modeling.

Of course, this study has identified statistical associations, not proven causal links. However, in the IV modeling care was taken to purge the measured association of the common confounding elements in a fashion that it is thought offers an important advance. Moreover, several, not-mutually exclusive hypotheses have been offered above 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 is clearly needed at drilling below readily available census data to better uncover the underlying neighborhood processes at work here (Friedrichs, 1998; Gephart, 1997; Raudenbush & Sampson, 1999; Sampson *et al.*, 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, and exposure to violence are especially salient. In addition, much more needs to be done to measure perceptions and stereotypes held by external actors that may affect opportunities of neighborhood residents and, thereby, their behaviors. Indeed, the mechanisms of how neighborhood effects transpire provide crucial information for guiding prospective policy responses aimed at deconcentrating poverty spatially (Galster, 2005, 2007, forthcoming)

But even without full understanding of the underlying causal processes, the findings here hold powerful implications for policy makers in their efforts to create neighborhoods that provide superior developmental environments for children. Numerous community development efforts are underway aimed at revitalizing distressed core neighborhoods in the US, 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 US Department of Housing and Urban Development's assisted housing policy have similar goals of enhancing developmental contexts by expanding residential options for the poor, 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 (Galster *et al.*, 2003; Popkin *et al.*, 2003; Rubinowitz & Rosenbaum, 2000). The results here imply that all of these initiatives should aim to deconcentrate extreme 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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Notes

- ¹ The direction of the bias has been the subject of debate, with Jencks & Mayer (1990) and Tienda (1991) arguing that neighborhood impacts are biased upwards, and Brooks-Gunn *et al.* (1997) arguing the opposite.
- ² While other studies have discussed this issue, it has been in the context of the reflection problem (Manski, 1995) of people in the neighborhood tautologically cause the aggregate neighborhood characteristics to be what they are as well as the neighborhood causes constituent residents' behaviors (Duncan & Raudenbush, 1999).
- ³ Other recent research has employed natural experiments where the selection bias was minimized through geographic assignment of households through governmental housing program auspices (Aslund & Fredriksson, 2005; Edin *et al.*, 2003; Oreopolis, 2003).
- ⁴ Such a longitudinal analysis has been strongly recommended as the vehicle for overcoming the reflection problem (Duncan & Raudenbush, 1999; Manski, 1995).
- ⁵ A database from Geolytics is used, the 'Neighborhood Change 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.
- ⁶ For details, see Galster (2003a).
- ⁷ There are two exceptions to this. First, for those years in which the family lived in a rural area, the observed value of county characteristics is used. Second, for child age zero the observed value is used since a first-stage equation for year zero (due to unavailability of lagged variables) cannot be estimated.
- ⁸ Details of the first-stage regressions are available upon request.
- ⁹ However, some were asked again in 1975 and a question about union membership was collected from 1968 through to 1981.
- ¹⁰ The estimates here 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 & Hoffman (1999).
- ¹¹ Wheaton & Clarke (2003) find cumulative neighborhood conditions much more powerful in explaining various child developmental outcomes than contemporaneous conditions.

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Appendix 1**Exogenous and Predetermined Variables [X] from Equations (1)–(5) Used in First-Stage of Instrumentation Procedure for Neighborhood Poverty Rate***

1. Index of owner-occupied housing prices in metropolitan area (lag 1 year)
2. Index of owner-occupied housing prices in metropolitan area (lead 1 year)
3. Index of gross rents paid by renter occupants in metropolitan area (lag 1 year)
4. (3.) \times renter status in prior year
5. Home mortgage interest rate for 30-year fixed-rate loan
6. (5.) \times renter status in prior year
7. Ratio of costs of renting to owning in metropolitan area (lag 1 year)
8. (7.) \times renter status in prior year
9. Whether family's oldest child reached age 5 in prior year (1 = yes; 0 = no)
10. Whether family's oldest child reached age 13 in prior year (1 = yes; 0 = no)
11. Whether any other child in family reached age 5 in prior year (1 = yes; 0 = no)
12. Whether any other child in family reached age 13 in prior year (1 = yes; 0 = no)
13. Age of household head
14. Household head received a lump-sum monetary payment since child's birth;
e.g. inheritance (1 = yes; 0 = no)
15. (14.) \times renter status in prior year
16. Difference in household's real income from prior to current year (if GT 0; 0
otherwise)
17. (16.) \times renter status in prior year
18. Poverty rate of county (lag 1 year)
19. Household expects to move next year (lag 1 year)
20. Household owns home occupied (lag 1 year)
21. Logarithm of deflated household income (lag 1 year)
22. Year (denoted by a set of dummy variables, 1968 = excluded year)

*Note: First-stage procedure also uses all exogenous variables noted in Table 1

Appendix 2**Table A1.** Estimated parameters for baseline model of neighborhood effects (no IVs)

	No child pre-18	HS graduate	College grad	ln(earnings)
Blackfem	-1.524 [0.700]**	0.788 [0.644]	-0.145 [0.876]	0.151 [0.317]
blackmale	0.839 [0.908]	1.59 [0.763]**	-0.958 [0.792]	0.404 [0.311]
whitefem	-1.708 [0.528]***	0.183 [0.367]	-0.429 [0.282]	-0.36 [0.087]***
birthorder	-0.03 [0.105]	0.05 [0.102]	0.012 [0.097]	-0.083 [0.040]**
age99	-0.203 [0.099]**	0.157 [0.078]**	0.411 [0.066]***	0.074 [0.023]***
pro_live_under_poverty0to18	-1.162 [1.262]	-1.716 [1.097]	1.051 [1.657]	-1.142 [0.434]***
pro_livew_2_parents0to18	0.538 [0.934]	0.188 [0.834]	-1.705 [0.774]**	0.951 [0.255]***
pro_own0to18	-1.248 [0.931]	0.839 [0.679]	1.251 [0.690]*	-0.232 [0.213]
pro_stability_year0to18	2.956 [1.357]**	-0.636 [1.006]	0.228 [1.219]	0.211 [0.334]
ave_pert_inc_below_pov0to18	-0.104 [0.035]***	-0.059 [0.029]**	-0.038 [0.031]	-0.021 [0.012]*
religion	-0.07 [0.703]	-0.57 [0.506]	-0.549 [0.557]	-0.035 [0.156]
largecity	0.625 [0.459]	0.541 [0.454]	0.97 [0.310]***	0.02 [0.110]
farm	0.309 [0.531]	-0.109 [0.450]	0.419 [0.423]	-0.027 [0.121]
veteran	-0.481 [0.460]	-0.985 [0.451]**	0.211 [0.285]	0.028 [0.104]
momteen	-0.637 [0.631]	0.866 [0.556]	-0.634 [0.869]	-0.489 [0.302]
ave_education_head0to18	0.189 [0.214]	0.66 [0.290]**	0.337 [0.106]***	-0.03 [0.043]
ave_hdoce_pre0to18	0.037 [0.027]	0.028 [0.033]	0.005 [0.017]	-0.008 [0.007]
ave_self_employed0to18	-0.051 [0.973]	-0.247 [1.356]	0.523 [0.555]	0.181 [0.240]
ave_employed_wife0to18	0.499 [0.819]	-0.617 [0.711]	0.662 [0.532]	-0.431 [0.183]**
ave_smsa0to18	-0.389 [0.641]	-0.168 [0.472]	0.966 [0.502]*	-0.04 [0.128]
ave_annu_hrs_wkd0to18	0.0003 [0.0006]	0.00001 [0.0005]	-0.0001 [0.001]	0.0001 [0.0002]
ave_readnewspaper	-0.62 [0.615]	0.9 [0.572]	0.087 [0.549]	0.193 [0.172]
ave_union	-0.875 [0.624]	0.942 [0.518]*	-0.005 [0.460]	0.086 [0.135]
ave_nochurch	0.357 [0.690]	0.517 [0.636]	-0.675 [0.588]	0.084 [0.179]

Table A1. *Continued*

	No child pre-18	HS graduate	College grad	ln(earnings)
ave_no_socialclubs	0.226 [0.667]	-1.172 [0.160]*	0.879 [0.405]**	0.014 [0.119]
ave_relatives	-0.223 [0.559]	0.065 [0.547]	0.086 [0.374]	-0.074 [0.124]
ave_num_neigh_known	0.02 [0.032]	0.055 [0.033]*	0.044 [0.019]**	0.01 [0.006]
ave_plan_ahead	1.186 [0.585]**	-0.096 [0.494]	0.599 [0.458]	0.268 [0.140]*
ave_trust	-0.738 [0.726]	-1.081 [0.529]**	0.0002 [0.438]	-0.13 [0.109]
no_child_before_18	NA	1.208 [0.458]***	-0.081 [0.559]	0.032 [0.214]
at_least_hs	NA	NA	NA	0.111 [0.135]
collgrad	NA	NA	NA	0.36 [0.107]***
married	NA	NA	NA	0.098 [0.082]
annu_hrs_wkd99	NA	NA	NA	0.0001 [0.00004]**
constant	5.194 [4.181]	-11.126 [4.075]***	-19.622 [2.731]***	8.183 [1.053]***
N of Observations	755	755	755	541
Pseudo R-squared (R-Squared)	0.31	0.26	0.19	(0.31)
Wald Chi-squared	97***	26***	49***	NA

Note: Robust standard errors in brackets NA = Not Applicable;

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