

RELATIVE POVERTY AND CHILD COGNITIVE AND BEHAVIORAL DEVELOPMENT (Prospectus Draft)

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INTRODUCTION

As more child poverty research attempts to consider not only the effects of individual and family characteristics but also the effects of the context in which families with children live, researchers have been driven to consider the effects of relative poverty. The impact of relative poverty on children's outcomes – that is, having fewer resources than those living in the surrounding area – is far from clear. While one could imagine the negative psychological effects resulting from having to live with less than others in the surrounding area, one could also see the advantages of living in a better neighborhood. Furthermore, weighing the costs of having fewer relative resources and the benefits of living in a neighborhood with more resources is complicated by the fact that families can choose where they live. This is problematic because the same family characteristics that influence children's outcomes also affect a family's decision about where to live, making it difficult to separate the effects of a child's family characteristics from a child's residential area.

This study proposes to address this complexity by using sibling data, based on the assumption that a family's decision about where to live does not depend on any particular child's characteristics (see Aaronson 1998). This technique exploits the fact that siblings not close in age have widely varying histories of family income and resources as well as widely varying histories of where they have lived, since US families move rather

frequently. With longitudinal data spanning a child's entire life, it can be shown that, in aggregate, siblings may experience different levels of relative poverty.

Using longitudinal sibling data, this study addresses the impact of relative poverty on children's cognitive and behavioral development while attempting to control for the family characteristics that influence where a family chooses to live. It assesses the costs and benefits of living in a better neighborhood but having fewer resources, contrasting this with the costs and benefits of having more resources but living in a worse neighborhood. If one situation is significantly better for children than the other, this evidence may be particularly informative in policy discussions about how to address child poverty and the neighborhoods in which they live.

LITERATURE REVIEW

Relative Deprivation Models

Stouffer et al. (1949) coined the term relative deprivation in a study about American soldiers that found that the Military Police were more satisfied with their promotion opportunities than the Air Corps, even though the Military Police actually had about half as many promotion opportunities as the Air Corps. The Military Police did not feel as deprived as the Air Corps because they compared themselves with other MP's who were not getting promoted, while the Air Corps members felt more deprived because they were comparing themselves with other Air Corps members who were getting promoted more frequently. Stouffer et al. concluded that satisfaction depends more on subjective than on objective standards. This model of relative deprivation has since been developed further and can be summarized by five conditions that lead to feelings of

relative deprivation. A person must: 1) see that someone else possesses some desired object, 2) want it, 3) feel entitled to it, 4) think it is feasible to obtain it, and 5) lack a sense of personal responsibility for not having it (Olson & Hazlewood 1986). According to the relative deprivation theory, “a person’s sense of contentment depends not on objective conditions, but on the subjective perceptions and comparisons of self to others” (Dion 1986).

Unfortunately, some research claiming to test the theory of relative deprivation is misinformed about the unit of analysis required to correctly test this theory. For example, some studies compare the behavior of individuals who live in disadvantaged neighborhoods to individuals who live in more advantaged neighborhoods. The problem with this type of comparison is twofold: 1) it uses aggregate data to describe the living conditions of individuals and 2) it assumes that people living in one neighborhood are aware of the living conditions of those in other neighborhoods. Because relative deprivation theory hinges on the idea that people compare themselves to others with whom they identify in some manner (a reference group), they need to at least be aware of the differences that lie between them and the comparison group.

A study by Thomas Vartanian (1997), for example, claimed to have tested relative deprivation theory by comparing the likelihood of exiting AFDC for people living in different neighborhoods. He found that people stay on welfare longer when they live in neighborhoods that are economically worse off than other neighborhoods in the sample (using PSID data). This type of comparison is inappropriate for testing relative deprivation theory because this theory is based on a person’s comparison of his own

standing relative to a reference group, not his standing relative to other neighborhoods he may have never even seen before.

A study by Damarys Canache (1996) addressed this concern by reiterating Merton and Rossi's (1968) statement that people compare themselves to those with whom they share some characteristic or with whom they are in association. Canache therefore compared a person's individual status with that of his or her own neighborhood (as opposed to that of other neighborhoods) and found that poor persons living in wealthy neighborhoods (especially homogeneous neighborhoods) stated that they experienced greater frustration and discontent and supported political violence more than poor persons living in poor neighborhoods. This study more adequately addresses the subjective conditions that relative deprivation theory explores.

However, the ultimate issue in question is not just how relative deprivation makes a person feel but rather what observable behavior, if any, it induces. People in relatively worse positions may not fare well because of the cost of having to compete for scarce resources against people in relatively better positions. Researchers have linked relative deprivation to a variety of behaviors ranging from protests and riots to stress and illness (see Crosby 1976). While relative deprivation theory does not exclude the possibility of a positive reaction (such as self-improvement efforts), it generally assumes that feelings of relative deprivation will lead to stress, conflict, and poor work or school-related performance.

But not all theories predict that there are negative consequences to having fewer relative resources. In a review by Jencks and Mayer (1990) of theories describing neighborhood effects on children, they mention three sets of models that explain how

having more advantaged neighbors may have positive effects on children because it means having access to more community resources.

Epidemic, Collective Socialization, and Institutional Models

While epidemic theory focuses on the influence of peers in the neighborhood, collective socialization theory focuses on the influence of adults who live in the neighborhood, and institutional theory focuses on the influence of adults who work in the neighborhood (Jencks and Mayer 1990). In general, these three models predict that living in a neighborhood with better role models has a positive effect on children. Assuming that neighborhoods with more resources have more role models, these theories predict that poor children living in these neighborhoods are better off than similar children living in poorer neighborhoods.

Crane (1991) found that even after controlling for individual characteristics, black and white adolescents experience a sharp increase in their chances of dropping out of school and having a child as a teenager when the percentage of workers with professional or managerial jobs in their neighborhoods decreases. Similarly, Brooks-Gunn et al. (1993) found that the presence of affluent neighbors, neighbors with high occupational status, and neighbors with two-parent families was associated with higher childhood IQ, fewer teenage births, and lower probability of dropping out of school.

But these studies, and others like them, do not adequately address the issue of selection bias. While they recognize that the factors by which families select their neighborhoods also affect the children's outcomes, they do not take the steps needed to separate those factors from the neighborhood effects. The best way to do this is

unfortunately very difficult, expensive, and (some would argue) unethical – that is, through the use of experimental data.

Although housing experiments are rare, several thousand families have participated since 1976 in a study known as the Gautreaux program, which gives rent subsidies to low-income black families living in Chicago's public housing projects (Rosenbaum 1995). While all selected applicants have the opportunity to move to improved housing, they are randomly offered housing in either low-income black urban neighborhoods or middle-income white suburbs. Rosenbaum (1995) found that the adults that moved to the middle-income neighborhoods were more likely to find employment than those who moved to the low-income neighborhoods, and he found that their children were less likely to drop out of school, more likely to go to college, and more likely to be employed and to have better wages if not in college.

While these results are extremely informative and provide strong causal arguments for neighborhood effects, they do not allow us to separate the effects of neighborhood income from racial composition or urban-suburban status because program participants were not assigned, for example, to low-income white urban neighborhoods or middle-income black suburban neighborhoods. Thus, we cannot determine which specific neighborhood attributes, or combinations of attributes, were responsible for the observed outcomes.

No Effect

While most people believe that a neighborhood's composition has some effect on individual behavior, a few researchers have found that neighborhood effects are small

relative to parental influence, especially for young children. Using data from a Philadelphia study, Frank Furstenberg et al. (1999) found no neighborhood-level differences in academic competence, acting out, parents' assessments of their children's adjustment, or the children's self-assessment of their mental health (p. 219). Furstenberg suggests that neighborhood influences may become more important in later adolescence and that the combination of neighborhood influences and parenting practices is more important than either one.

QUESTIONS

Although there is no consensus about the direction of neighborhood effects, previous research clearly indicates that children's outcomes must consider several different levels of context – individual, family, and neighborhood – and that the process by which families select the neighborhoods in which they live must also be taken into account. Furthermore, because it is possible that relative poverty may simultaneously have an advantageous and a disadvantageous effect on children, it is important to identify and measure these two opposing forces that may even cancel each other out and make neighborhoods appear to have no significant effect on children.

Specifically, the following questions need to be addressed:

What is the psychological cost of relative poverty for children? Using self-esteem measures, what is the association between relative poverty and children's perceived abilities? How does this compare with absolute measures of income?

What is the effect of relative poverty on children's cognitive and behavioral outcomes? Is there a net gain or net loss associated with having fewer family resources than surrounding neighbors?

If there is a cost to relative deprivation, is there a benefit to relative advantage? In other words, is the effect of relative income symmetrical? Does the cost of relative deprivation

increase as its magnitude increases? Does the benefit of relative advantage increase as its magnitude increases?

What is the effect of relative poverty on siblings? Does this attempt to control for selection bias significantly alter the effect of relative poverty?

What are the policy implications of these results regarding child poverty and neighborhood effects?

DATA

The datasets that will be used for this project are shown in Table 1.

TABLE 1. DATASETS

<u>NAME</u>	<u>YEAR(S)</u>	<u>DESCRIPTION</u>
PSID Child Development Supplement	1997	Includes assessments of the cognitive, behavioral, and health status of children up to 12 years old.
PSID Family Level Files	1984-1997	Contain data on the children's family histories, including annual family income since the birth of the oldest child.
PSID Geocode Match Files	1980, 1990	Include the identification codes necessary to link data from the PSID family files to the Census Extract data.
Census Extract Data	1980, 1990	Contain demographic information at several levels of geographic aggregation, including the census tract.

The Panel Study of Income Dynamics Child Development Supplement (PSID-CDS) was released in April 1999. It has data on 3,586 children 12 and under in 2,394 PSID households. Information about the children (up to two per family) was obtained through interviews with the primary caregiver, standardized tests for children three to

twelve years old, questionnaires filled out by parents and second caregivers, a 24-hour diary filled out by the parents, and teacher and child-care provider reports.

The PSID-CDS includes several assessments of cognitive and behavioral development. The Woodcock-Johnson Tests of Achievement provide measures of verbal and math achievement. The verbal tests include “letter-word identification,” which tests symbolic learning, and “passage comprehension,” which tests comprehension and vocabulary skills. The math tests include standard calculation problems and practical or applied problems. In addition, the children’s behavior was measured using a “Behavior Problems Index,” which assesses aggressive, withdrawn, and distressed behavior, and a “Positive Behavior Scale,” which captures positive behavior such as being cheerful, obedient, and self-reliant.

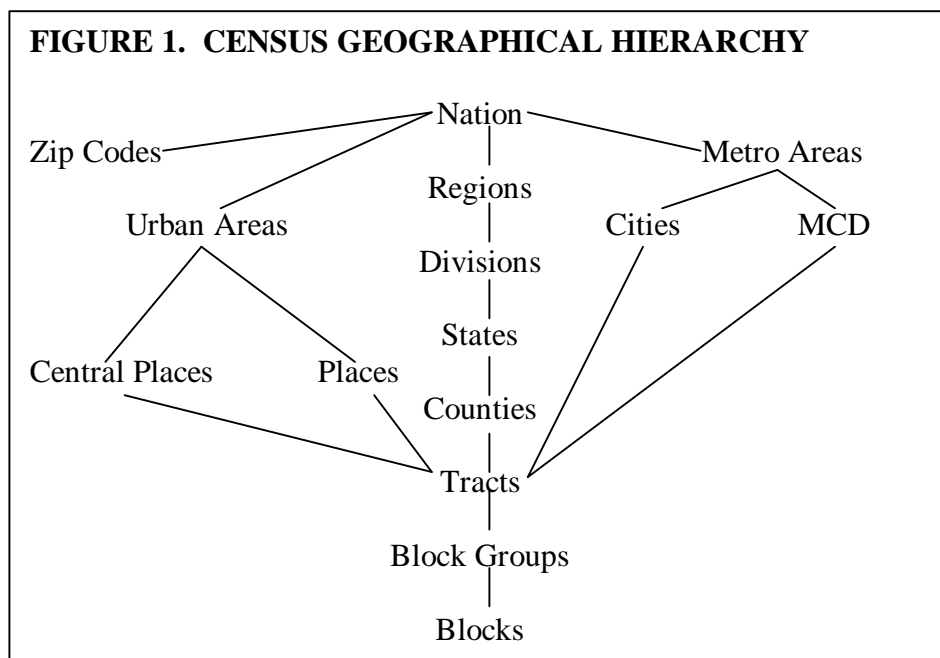
Although the PSID-CDS was designed as a longitudinal study, only one year of data (1997) is currently available. However, longitudinal data about each child’s family is available in the PSID family level files, which date back to 1968. Because the oldest children in the PSID-CDS were twelve years old in 1997, the children’s family data are available since before they were born. Thus, information regarding factors such as family size, family structure, parental education, and family income (labor, asset, and transfer income) can be obtained for the entire duration of the children’s lives.

The Census Extract Files provide data describing the children’s neighborhoods, approximated by census tracts. It is important to note that census tract boundaries are not arbitrary. They are defined locally, in a systematic attempt to capture socioeconomic homogeneity. The Census Bureau (1990) states that census tracts “are designed to be homogeneous with respect to population characteristics, economic status, and living

conditions.” Streets, railroad tracks, or waterways usually serve as boundaries, and each tract usually contains between 2,500 and 8,000 people. For these reasons, census tracts are generally viewed as appropriate proxies for neighborhoods.

The Census Extract Files include variables describing the age, gender, and racial/ethnic make-up of each neighborhood, as well as a variety of income, education, and family structure estimates. Neighborhood income variables include mean family income, mean household wage and salary income, mean household public assistance income, and the percentage of people living in poor households. Neighborhood family structure, education, occupation, and employment/unemployment information is also available.

Neighborhood data from the Census Extract can be matched to the PSID through the PSID Geocode Match Files, which are 1970, 1980, and 1990 census identifiers for the respondents’ addresses. The PSID Geocode Match Files use census tracts to approximate neighborhoods; Block Numbering Areas (BNA’s) in blocked but untraced areas;



Enumeration Districts (ED's) in areas with neither tracts nor blocks; and Minor Civil Division/Census County Division (MCD/CCD), Place, or ZIP Codes where a tract, BNA, or ED cannot be found.

Because the list of residential addresses is used for mailings to respondents (i.e. annual reports and payments for completed interviews), there are some problems with using this list. First, the street address cannot be determined when respondents use PO Boxes, Rural Route numbers, General Delivery, or a relative's address. These respondents therefore cannot be matched with the appropriate census data. Second, the process of geocoding addresses to the 1970 and 1980 census identifiers was very different from that used for geocoding addresses to the 1990 census identifiers since the Census Bureau made different types of electronic files available for assessing geographic boundaries (GBF-DIME files for 1970 and 1980 tracts, and TIGER/Line files for 1990 tracts). Third, tracts may change boundaries over time, making longitudinal analyses of specific neighborhoods problematic. While longitudinal assessments of a given family (including the different neighborhoods it has lived in) are appropriate, longitudinal assessments of a given neighborhood would be inappropriate because the boundaries are not the same over time.

It should also be noted that there is a discrepancy in the timing of data collection because the census data (neighborhood variables) are collected decennially while the PSID data (family variables) are collected annually. Because it is impossible to know the best way to approach this issue without having seen the data, I propose to compare the results of two methods. The first method involves matching the Census data with the nearest 10 years of PSID data. Since none of the children in the PSID were born prior to

1984, this means that 1984 PSID data will be matched to 1980 Census data, and the remaining PSID data (1985-1997) will be matched to 1990 Census data. The second method uses the inter-decennial Census estimates, which provide annual estimates of Census data resulting from interpolation and projection (for post 1990 estimates) techniques.

Table 2 shows how many addresses can be matched with a census tract or tract equivalent per year.

TABLE 2. PSID ADDRESSES MATCHED TO CENSUS DATA

YEAR	PSID FAMILIES	ADDRESSES MATCHED
1984	6,918	6,917
1985	7,032	7,031
1986	7,018	7,017
1987	7,061	7,060
1988	7,114	7,113
1989	7,114	7,113
1990	9,371	9,371
1991	9,363	9,363
1992	9,829	9,829

* 1993-1997 is Early Release Data

METHODS

The models that have been used to investigate neighborhood effects are usually in the form of $y = x_{if} + x_f + n_f$, where some outcome y is a function of x variables for the i th individual of family f , x family-specific variables, and n neighborhood-level variables. These models attempt to capture the effect of living in a specific type of neighborhood while controlling for individual and family-specific factors that may also influence the outcome of interest. For testing relative deprivation theory, I propose to use a

reformulation of these models in order to capture the effect of a family's income *relative* to its neighborhood income. In the new model, relative income is defined as

$$(1) \quad r_{ft} = f_{ft} - n_{ft}$$

where r relative income of family f at time t is equal to the family's income minus the neighborhood's mean (or median) income at time t . Note that r_{ft} is the same for all individuals within the same family. Over time, however, r_{ft} varies and causes individuals of different ages within the same family to have a different average relative income. A person's average relative income is defined as

$$(2) \quad R_{if} = S_t r_{ift} / T_{if}$$

where the average relative income R of the i th individual of family f is equal to the sum of that person's annual relative income (r_{ift}), divided by T_{if} , the total number of years for which data are available.

Thus, the basic model that will be used in this project is

$$(3) \quad y_{if} = \beta_0 + R_{if} \beta_1 + e_{if}$$

where y_{if} is the outcome for the i th individual of family f , and is a function of that child's average relative income as defined by equation (2). This model assumes that the effect of the child's relative resources is the same over the course of the child's life. This assumption may not present a problem since the children in the PSID Child Development Supplement are all under the age of 13, and previous research suggesting that neighborhood effects vary by age group has only found differences between young children and adolescents (see Brooks-Gunn et al. 1993). If there are different effects among young children, this could be tested by averaging the children's relative resources across shorter time periods. The usefulness of this test, however, depends on the amount

of data available per age group and the variation among the new relative resources averages.

This model may also be problematic since it does not address the selection bias issue mentioned earlier. Unless families are randomly assigned incomes and neighborhoods, selection bias poses a serious threat to the validity of this study. Neighborhood effects must therefore be separated from family effects. One way to do this is to compare children from similar families who have lived in different neighborhoods. Using a sibling model, we can take this one step further and compare children from the *same* family. Although children in the same family generally live in the same household (and therefore in the same neighborhood) at a given time, longitudinal data reveal that because of age differences, siblings spend varying amounts of time in a given neighborhood. Furthermore, siblings in the US generally spend varying amounts of time living in *different* neighborhoods because US families migrate frequently. According to the 1997 Current Population Survey, about 42 million people (or 16% of the US population) moved to a different house between March 1996 and March 1997. Over the course of several years, the chances of a family moving to a different house are much greater. Based on the premise that families frequently move to different neighborhoods and that siblings vary sufficiently in age, the family and neighborhood resources available to children during the course of their lives should vary among siblings (see Aaronson 1998).

Assuming that families do not decide where to live based on any particular child's characteristics, a fixed-effects model can be used to predict children's outcomes while controlling for family characteristics, including unmeasured or unobservable factors that

affect how a family chooses where to live. The fixed-effects model, which is based on the basic model shown in equation (3) is

$$(4) \quad (y_{if} - Y_f) = \beta_0 + (r_{if} - R_f)\beta_1 + (e_{if} - E_f)$$

where

$$(5) \quad Y_f = S_f y_{if} / 2$$

is the average child outcome score for family f (it is divided by two because there are at most two siblings per family in the PSID-CDS),

$$(6) \quad R_f = S_f r_{if} / 2$$

is the average relative resources for family f , and

$$(7) \quad E_f = S_f e_{if} / 2$$

is the family-specific error-term. Thus, the fixed-effects model in equation (4) subtracts each child's family averages from his or her individual-level variables, taking account of family characteristics that may be affecting the outcome of interest.

OBTAINING SENSITIVE DATA

Because the PSID has promised anonymity to its respondents, the PSID Geocode Match Files are only available under a special contractual arrangement with the Institute for Social Research (ISR) at the University of Michigan. The contract for use of sensitive data requires Human Subjects Review clearance, a Data Protection Plan, a \$1,000 security deposit, which will be refunded when it is determined that I am in full compliance with the terms of the contract, and a \$500 non-refundable fee, which covers the cost of creating and shipping the data files and documentation, as well as up to four hours of consultation with the PSID user services staff. The funding will come from the

Harvard University NSF Integrated Graduate Training and Research Grant on Inequality and Social Policy.

TIMELINE

Fall 1999	Complete process of obtaining PSID Geocode Match files Set up all four datasets
Spring 2000	Merge PSID data with Census data Data cleaning Return PSID Geocode Match files to ISR Begin preliminary analyses
Summer 2000	Continue analyses Begin writing dissertation
Fall 2000	Write dissertation
Spring 2001	Write dissertation

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