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THE LONG-RUN CONSEQUENCES OF LIVING IN A POOR NEIGHBORHOOD*

PHILIP OREOPOULOS

Many social scientists presume that the quality of the neighborhood to which children are exposed affects a variety of long-run social outcomes. I examine the effect on long-run labor market outcomes of adults who were assigned, when young, to substantially different public housing projects in Toronto. Administrative data are matched to public housing addresses to track children from the program to when they are more than 30 years old. The main finding is that, while living conditions and exposure to crime differ substantially across projects, neighborhood quality plays little role in determining a youth's eventual earnings, unemployment likelihood, and welfare participation. Living in contrasting housing projects cannot explain large variances in labor market outcomes but family differences, as measured by sibling outcome correlations, account for up to 30 percent of the total variance in the data.

I. INTRODUCTION

The substantial levels of segregation that Wilson [1987], Jargowsky [1997] and Cutler, Glaeser, and Vigdor [1999] find within cities imply that many youths grow up surrounded by very wealthy households while others grow up in areas where almost all nearby families are poor. Division by income and by race leads many social scientists to wonder whether social and economic outcomes would differ if some residents could live elsewhere. Yet estimating the importance of neighborhoods has proved problematic. Because households in the private market have the option to relocate, researchers find it difficult to control completely for family circumstance and other individual characteristics. They cannot determine, for example, why two families with identical observable backgrounds would live in contrasting neighborhoods—the possibility that some unobservable familial factor explains the residential difference cannot easily be ruled out.

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A primary advantage of analyzing neighborhood interaction within the context of public housing is that participation in the program limits residential choice. Within public housing, similar households may reside in different locations for reasons beyond their control. Four previous studies use subsidized housing programs to examine neighborhood effects. The well-known Gautreaux program assisted black households in high-density public housing projects in Chicago to move to less-segregated communities. Rosenbaum, De Luca, and Miller [1999], Rosenbaum [1995], and Popkin, Rosenbaum, and Meaden [1993], who argue that the selection into suburbs or the central city was random, find that outcomes of the parents and children were markedly better for those who moved to the less-segregated suburbs.[1] Early results from the Moving to Opportunity (MTO) program also suggest quality of life improvements from moving to well-off areas [Katz, Kling, and Liebman 2001; Ludwig, Duncan, and Hirshfield 2001]. Compared with families who remain in high-density housing projects, the randomly selected families who were moved to more affluent neighborhoods enjoy increases in overall resident satisfaction, reductions in exposure to crime, and fewer health problems. When the MTO studies turned to initial economic effects, however, differences across treatment and control groups were much less clear. Parental welfare participation and employment, for example, do not differ across groups, and child test scores and delinquent behavior vary considerably less than the Gautreaux studies would imply. In another study, Jacob [2000] examines a less extreme experiment in which families living in Chicago housing projects set too close were offered vouchers to relocate. Comparing children from these projects with children from others, he finds no significant differences in test scores and dropout rates. Finally, Gibbons [2002] uses variation from contrasting council tenant housing in the United Kingdom to find slightly higher educational attainment for those raised in neighborhoods containing above average educated households. However, he acknowledges that families from this type of social housing may not

1. Using data from the original paper files of the Gautreaux program, Kling and Votruba [2001] find placement assignments were not entirely random. Preprogram differences were found between the racial makeup of the intake neighborhood, car ownership, and family composition. Not conditioning on these background factors might explain why the more controlled experiment from the Moving to Opportunity Program finds weaker results.

locate randomly and provides no evidence whether random assignment occurred.

This paper is the first to examine the effects of the neighborhood on the long-run labor market outcomes of adults who were assigned as children to different residential housing projects in Toronto. Studying neighborhood interactions under this program offers unique advantages over housing programs analyzed in previous studies. Differences in neighborhood quality do not correspond with the treatment group's moving into better neighborhoods. All families in the Toronto program are assigned to various housing projects throughout the city at the time they reach the top of the waiting list. Assignment is based chiefly on household size, and families cannot specify project preference. In the MTO program and in Jacob's study, treatment families generally are required to move, while control families remain in their original residences. This makes the impact from relocation difficult to disentangle from that of a change in neighborhood environment.

The Toronto housing program also permits comparison across a wide variety of subsidized housing projects. Some projects consist only of high-rise apartments; others are only townhouses. Some accommodate more than 10,000 individuals; others provide shelter to less than 100 individuals. And some projects are located in central downtown, while others are in middle-income areas in the suburbs.

Project addresses are matched to a large tax administrative panel of Canadians and their parents born between 1963 and 1970, and tracked until 1999. The matched data set provides a rare opportunity to examine accurate measures of total income, wages, and welfare participation when most youths from public housing are 30 years of age or older. Another administrative data set that includes nonfiling children and their parent's characteristics (but not outcomes) provides a means to verify that the filing requirement of the main data set does not introduce selection bias.

The administrative data set includes Canadian youths living inside and outside public housing. This enables a comparison of the estimated neighborhood effects from a quasi-experimental setting with those estimated from a simple OLS approach for households in the private housing market living in the same neighborhoods as public housing participants. For the private household market sample, I estimate substantial positive effects on youths' labor market outcomes from living in wealthier residential areas, even after controlling for observable family background characteristics. When I estimate the same effects for those children within the housing program, however, the positive effects disappear. This is the main finding of the paper: despite significant contrast in living conditions and exposure to crime across projects, neighborhood quality does not make much difference to chances for labor market success in the long run. Unemployment, mean earnings, income, and welfare participation rates vary little between adolescents from different public housing types.[2] In fact, estimates of the probability income distribution for youths from the highest density projects and the lowest projects are virtually identical.

I also compare sibling correlations to unrelated neighbor correlations. This approach, developed by Solon, Page, and Duncan [2000], accounts for unobserved measures of neighborhood quality and provides a comparison between the explanatory power of neighborhood influence and family influence on long-run labor market outcome. The outcome correlations between youths from the same housing projects are measured around zero. However, family background, as captured through sibling correlation measures, accounts for about 30 percent of the total variance in income and wages.

The next section gives a brief overview of the previous literature discussing how social interactions may influence outcomes and how these theories apply to consequences from living in different neighborhoods. Section III describes the two empirical approaches I used for the study. Section IV describes Toronto's subsidized housing program and the variation in neighborhood quality across projects. Section V presents the data. The results are displayed in Section VI. Section VII gives my conclusions.

II. WHY MIGHT NEIGHBORHOODS MATTER (AND WHY NOT)?

Several existing theories attempt to explain why residential location may affect individual behavior.[3] Perhaps the most intuitive explanation by which neighborhoods affect outcomes is through peer group or role model effects. There is rich evidence

2. In Canada, welfare receipt is termed social assistance. I use the term welfare throughout to avoid confusion.

3. See Jencks and Mayer [1990], Duncan and Raudenbush [2000], Moffitt [2001], and especially Dietz [2001] and Brock and Durlauf [2000] for comprehensive reviews of the literature.

within the psychology literature on the importance of these effects, both positive and negative [Brown 1990; Brown, Clasen, and Eicher 1986]. According to this theory, an individual makes decisions based not just on her own preferences but on whether her decisions would deviate from choices made by others in her reference group [Akerlof 1997; Akerlof and Kranton 2000; Crane 1991; Glaeser and Scheinkman 2001]. Second, an individual's social network may be an important resource. Personal contacts can improve an individual's chances of finding a job, receiving advice and psychological support, or getting a temporary loan. Granovetter [1995], for example, concludes that jobs are often found through contacts formed long before seeking employment. Third, resources for local public goods, such as schools, libraries, and law enforcement, are limited by the resources available to community residents. A lack of funding for local schools, for example, exacerbates a poor community's ability to hire exceptional teachers [Bénabou 1996; Durlauf 1996; Hoxby 2000]. A final way by which neighborhoods may play a role is through conformism. In contrast to peer group effects, conformism models usually posit that individuals mimic neighbors' behavior because they lack enough information to choose on their own [Bikhchandani, Hirshleifer, and Welch 1992; Bernheim 1994; Jones 1984; Sah 1991].

Not surprisingly, there are few theories that deduce neighborhoods do not matter. Most of us appreciate instinctively that decisions over education attainment, drug use, and careers are often influenced by others, not just family, and the thought that peer groups or role models are formed, in part, by one's residential environment seems natural. Little, in fact, is known about how role models or peer groups are formed. If parents influence those with whom their children interact, and these friends influence the children, such influences are family effects in reduced form. Even within a poor neighborhood, there can be many peers to choose among. Not everyone in a deprived neighborhood is a gang member.

Another important consideration when exploring neighborhood effects is that social interactions do not take place in geographical isolation alone. For interactions to matter at the neighborhood level, social contact must depend significantly on where an individual resides, and neighbor relationships must be important enough to influence individuals' decisions. The definition of a neighborhood is therefore important. Neighborhood effects at the school-district level may miss the effects of role models formed, say, at weekend hockey practice. Finally, if a few youths are strongly affected by where they live while the majority are not, then the expected neighborhood effect may still be small, since researchers usually measure average, rather than individual influences from one's residence.

III. Methodology

I employed two strategies for estimating whether neighborhood quality affects outcomes for youths who lived in public housing. First, I divided housing projects by neighborhood quality and compared mean outcomes across these categories. Second, I estimated the correlation between unrelated neighbors who lived in the same project and compared this measure with the correlation between siblings. The neighbor correlation method has the advantage that it does not require explicitly defining neighborhood quality. Neighbor correlations give estimates of the portion of the total outcome variance explained by differences in project quality, while sibling correlations measure the portion due to family differences. I discuss both strategies below.

III.A. Differences in Means

Suppose that there are two types of projects, g and b. Let Y_{ip} be an outcome variable—say permanent income—for individual i in project p as determined by the following equation:

$$Y_{ip} = \gamma X_{ip} + \eta_{ip} + \varepsilon_{ip}, \tag{1}$$

where X_{ip} is a vector of all family characteristics that influence earnings (whether the researcher observes them or not), η_{ip} is the individual neighborhood effect from living in project p, and ε_{ip} represents unrelated individual factors independent of both family and neighborhood characteristics. Note that η_{ip} may differ for youths from the same neighborhood. The mean outcome difference between project g and project b is

$$\bar{Y}_g - \bar{Y}_b = \alpha(\bar{X}_g - \bar{X}_b) + \eta_g - \eta_b, \tag{2}$$

where $\bar{Y}_p$ is the mean of the outcome variable for project p, and η_p is the mean neighborhood effect on individuals from project p. We are interested in the mean outcome difference attributable to variation between project characteristics, $\eta_g - \eta_b$. If assignment is random, $\bar{X}_g = \bar{X}_b$, then the impact from living in project type g versus project type b can be estimated directly from the mean outcome difference. Without random assignment, this comparison is biased toward a larger effect on the project type in which families that tend to have greater positive influence on their children sort into.[4] The direction of the bias, $\alpha(\bar{X}_g - \bar{X}_b)$, is ambiguous if not all values of X_{ip} are observed.

III.B. Sibling and Neighbor Correlations

A disadvantage with the difference-in-means methodology described above is that neighborhood quality has to be defined in order to categorize and compare mean differences between neighborhood types. But public housing projects differ across many dimensions, observable and unobservable, and condensing these dimensions into a few discrete categories may miss identifying other significant effects. I followed a second approach introduced by Solon, Page, and Duncan [2000] that avoids defining neighborhood quality and instead compares sibling with neighbor correlations.

Let Y_{sfp} be the outcome variable, now indexed for sibling s in family f in project p. Reindexing equation (1) and assuming that every neighbor is subjected to the same community effect, we get

$$Y_{sfp} = \gamma X_{sfp} + \eta_p + \varepsilon_{sfp}. \tag{3}$$

The expression includes all relevant family and project characteristics, even those that are unobservable to the researcher.

The population variance of Y_{sfp} can be decomposed into

$$\operatorname{var}(Y_{sfp}) = \operatorname{var}(\gamma X_{sfp}) + \operatorname{var}(\eta_p) + 2\operatorname{cov}(\gamma X_{sfp},\eta_p) + \operatorname{var}(\varepsilon_{sfp}). \tag{4}$$

4. Random assignment does not solve the reflection problem, first mentioned by Manski [1993]. The reflection problem arises when the set of individuals whose outcomes are analyzed is the same set of individuals whose background characteristics are used to classify neighborhood quality. Even when neighborhood effects are zero, the correlation between neighborhood outcomes and neighborhood quality will be high. This paper does not isolate "endogenous" effects, wherein an individual's behavior varies with the behavior of the group, from "exogenous" effects, wherein an individual's behavior varies with exogenous characteristics of the group. But it does minimize "correlated" effects, wherein individuals tend to behave similarly because they have similar background characteristics. It does so by examining outcomes of public housing participants whose surrounding neighborhoods consist of both participants and nonparticipants. See Brock and Durlauf [2000] for discussion of the reflection problem.

Similarly, the covariance between sibling s and sibling s' is

$$(5)\quad \operatorname{cov}(Y_{sfp}, Y_{s'fp}) = \operatorname{cov}(\gamma X_{sfp}, \gamma X_{s'fp}) + \operatorname{var}(\eta_p) + 2\operatorname{cov}(\gamma X_{fp}, \eta_p).$$

Equation (5) emphasizes the fact that siblings have correlated outcomes because they share both family and project influences. How much of the covariance in earnings is due to family influences, and how much is due to project influences? We cannot identify these factors separately from the sibling covariance alone. However, observing the covariance among unrelated project neighbors may shed some light on this question. The covariance between unrelated neighbors from family f and family f' in the same project is

$$(6)\quad \operatorname{cov}(Y_{sfp}, Y_{s'f'p}) = \operatorname{cov}(\gamma X_{fp}, \gamma X_{f'p}) + \operatorname{var}(\eta_p) + 2\operatorname{cov}(\gamma X_{fp}, \eta_p).$$

The third term on the right-hand side of equation (6) is likely to be positive if selective sorting occurs by project. Even if no sorting occurs, the neighbor covariance may be positive because families with similar backgrounds may have been assigned to similar projects (for example, if the same ethnic groups tend to end up in the same projects or if tenants from downtown tend to differ from tenants in the suburbs).

The neighbor covariance in Y_{sfp} provides an estimate on the possible influence of both observed and unobserved neighborhood characteristics. Subtracting measurable parts of the first term that reflect neighbors' similar family backgrounds can reduce bias if these observables correlate with project location. Thus, the project covariance in earnings attributable to the observable part of family characteristics in γX_{fp} is subtracted from the overall neighbor covariance in equation (6) to obtain a more precise estimate on project effects. The adjustment does not affect the estimate if families are assigned randomly to projects.

If families are assigned randomly, $\operatorname{cov}(\gamma X_{fp}, \gamma X_{f'p})$ and $2\operatorname{cov}(\gamma X_{fp}, \eta_p)$ equal zero, and this approach of estimating relative neighborhood effects can be expressed more simply by correlations. The sibling outcome correlation with random assignment,

$$(7)\quad \operatorname{cov}(Y_{sfp}, Y_{s'fp}) = \frac{\operatorname{cov}(\gamma X_{sf}, \gamma X_{s'fp}) + \operatorname{var}(\eta_p)}{\operatorname{var}(Y_{sfp})},$$

gives the proportion of variance due to neighborhood effects and to family factors that are common between two siblings. Similarly, the neighbor outcome correlation,

$$\text{(8)} \qquad \frac{\operatorname{cov}(Y_{sfp}, Y_{s'f'p})}{\operatorname{var}(Y_{sfp})} = \frac{\operatorname{var}(\eta_p)}{\operatorname{var}(Y_{sfp})},$$

gives the proportion of variance due to neighborhood effects alone. Using both equations (7) and (8), we can decompose the outcome variance by the portion attributable to neighborhood factors and that attributable to family factors. The procedure for estimating the sibling and neighbor correlations and calculating the bootstrapped standard errors is straightforward and is discussed in the Appendix.

IV. Subsidized Housing in Toronto: Differences across Developments and the Application Process

IV.A. Background

Public housing buildings vary a great deal throughout Toronto in terms of size, location, and neighborhood surroundings.[5] Some of the earliest projects were built as part of a large urban renewal effort to provide accommodation to thousands of low-income households living in areas of decay or in overcrowded situations. Many observers, however, argue that these buildings did little to improve the urban environment and actually made conditions worse. Property values in neighborhoods surrounding these older projects are among the lowest in the city, and crime rates are among the highest.[6] Other projects built, however, were smaller in scale and located in more suburban communities. From 1949 until the mid-1970s, the construction and administration of subsidized housing was run by the Metro Toronto Housing Corporation (MTHC, formerly known as the Metropolitan Toronto Housing Authority). The federal government provided MTHC with a massive construction budget. The administration used these funds to develop 113 family projects, accommodating 29,173 households (about one in twenty family households in metropolitan Toronto).[7] Every MTHC household pays rent geared to income. That is, approximately 25 to 30 percent of a house-

5. For additional discussion about public housing in Canada and Toronto, see Murdie [1994] and Smith [1995].

6. According to Metro Toronto Housing Security, about one-third of all homicides in Toronto occurred on public housing property.

7. Since I am concerned primarily with children who lived in subsidized housing, I omit projects that accommodate only seniors. I also ignore a small number of projects that house exclusively Native Americans or special needs families.

hold's gross total income is charged as rent.[8] All MTHC public housing projects remain in operation, with maintenance, administration, and security supported through federal funding.[9]

MTHC projects were built before 1976. Legislation to the National Housing Act changed that year, allowing for development of public housing at the municipal level. Cityhome, under the municipal government, was responsible for most of the new construction prior to the mid-1980s, and it administers 97 developments containing 8966 household units. Not all households living in Cityhome projects receive subsidies. In an effort to encourage a greater income mix within projects, 25 to 60 percent of Cityhome's units are allocated to private renters—mostly single, low- to middle-income individuals. To ensure that all families identified in public housing faced the same application process and housing constraints, this paper examines only uniquely identified MTHC projects. An earlier study [Oreopoulos 2001] includes the smaller Cityhome projects and finds doing so does not alter the results or conclusions.

IV.B. Variation in Neighborhood Quality

Figure I shows the locations for 106 uniquely identified MTHC family projects. The map divides Metropolitan Toronto, with a population of 2.4 million in 1996 (about 4 million including the entire Metropolitan Census Area), into census tracts categorized by the percentage of households within a tract with family incomes below Statistics Canada's Low-Income Cut-Off (LICO).[10] Census tracts contain about 1000 to 3000 households and are designed to capture geographic and social boundaries to represent common impressions of neighborhoods.[11] The darker the

8. The percentage paid in rent changed from 25 percent to 30 percent in the 1980s. Welfare recipients pay a fixed amount set annually by the federal government.

9. The city of Toronto assumed responsibility for MTHC projects when Cityhome, the Toronto Housing Corporation, and MTHC were combined in 2002 to form the Toronto Community Housing Corporation. This occurred well after most of this paper's sample of youth from MTHC projects left the program.

10. A household falls below the Low Income Cut-Off if they spend more than 20 percentage points above the average comparative household on food, clothing, and shelter. For example, if the average Canadian family spends 35 percent of before-tax income on food, clothing, and shelter, a family that spends more than 55 percent of before-tax income falls below the LICO.

11. A committee of local specialists initially delineates census tracts (CTs) in conjunction with Statistics Canada. The main rules are as follows: 1) CT boundaries must follow permanent and easily recognizable physical features; 2) the population of a CT should range between 2500 and 8000, with a preferred average of 4000; 3) the CT should be as homogeneous as possible in terms of socioeconomic characteristics; and 4) the CT shape should be as compact as possible.

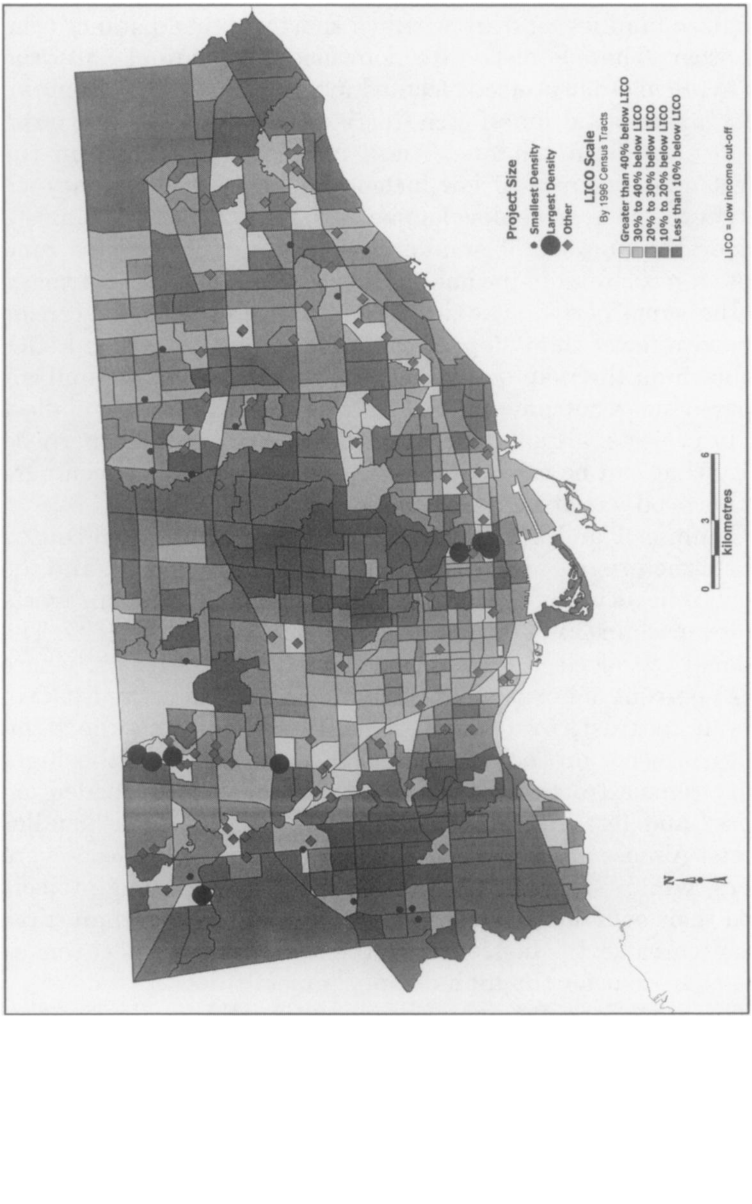

FIGURE I
MTHC Public Housing Projects in Metropolitan Toronto

shade in the tract, the smaller the portion of low-income households living there. The projects cover a large range of neighborhoods downtown and in the suburbs. The four largest downtown housing projects, represented by four large black circles in the center of the map, together accommodate about 30 percent of all subsidized families, and are within a short walking distance from each other. These projects are notorious for criminal and drug activity. Five other projects, located in northwest suburbs known as Rexdale and the Jane-Finch Corridor, also contain large numbers of subsidized tenants. These nine projects make up the highest density areas of low-income households in the city. In addition to these large developments, however, there are also a considerable number of smaller low-rise and townhouse complexes in more middle-income and residential areas, constructed over the same period. The smallest of these projects, in census tracts with fewer than 30 percent of households below the LICO, are shown on the map as small black circles. The main analysis compares mean outcomes of youths from the largest and smallest density projects, arguably the greatest contrast in neighborhood quality that can be created within the program. I also compare neighborhood variation from all 106 projects.

Columns 1 and 2 of Table I present the mean 1996 census tract characteristics for the nine largest density projects, and the sixteen projects with fewer than 250 units located in census tracts with fewer than 30 percent of households below the LICO. The low-density projects are in middle-income census tracts, where only 25 percent of households, on average, fell below the LICO in 1996. In contrast, 61 percent of households around the high-density projects are below the LICO. Households in the high-density census tracts were more likely to be female-headed, on welfare, and less educated than households from the smaller projects. Almost all households around the largest projects were renters, while 53 percent of those around the smaller projects owned their own home. The median income was more than three times greater for the household in the low-density project census tracts than that for the high-density project tract.[12]

The variation in neighborhoods within the public housing program was narrower than variation across the entire city. No

12. I use the 1996 census at Statistics Canada because the in-house version includes postal codes that allow a match to public housing addresses. Neighborhood variation by socioeconomic characteristics by census tract and enumeration area changes very little across the 1981, 1986, 1991, and 1996 censuses.

TABLE I
SELECTED CENSUS TRACT CHARACTERISTICS FOR LARGEST AND SMALLEST TORONTO HOUSING PROJECTS COMPARED WITH REPORTED CENSUS TRACT CHARACTERISTICS FROM BOSTON AND CHICAGO MTO PROGRAMS

	Toronto (1996)		Boston (1990)			Chicago (1990)		
Tract characteristic	Largest projects mean	Diff. in means smallest-largest	Control mean	Diff. in means Sec. 8-control	Diff. in means exp-control	Control mean	Diff. in means Sec. 8-control	Diff. in means exp-control
Female household head	0.482	−0.18	.531	−0.15	−0.28	.847	−0.19	−0.48
Black	0.249	−0.13	0.45	−0.11	−0.20	.993	−0.09	−0.42
Below LICO (Canada) or poverty line (U. S.)	0.611	−0.36	.359	−0.16	−0.25	.750	−0.38	−0.64
Receiving welfare	0.338	−0.17	.294	−0.11	−0.20	.586	−0.27	−0.48
Owner-occupied household	0.081	0.45	NA	NA	NA	.0282	0.23	0.63
Adult population with education of less than high school	0.425	−0.17	NA	NA	NA	NA	NA	NA
Adult population with education of more than high school	0.410	0.20	0.29	0.11	0.13	NA	NA	NA
Adult population with education of college degree	0.103	0.10	NA	NA	NA	.081	0.07	0.15
Median household income (1996 $Cdn)	13,693	28,301	NA	NA	NA	9,007	15,702	39,881
Sample size	2687	1489	176	113	236	118	53	67

"LICO" is Statistics Canada's Low-Income-Cut-Off. "Diff. in means" is the mean difference between census tract characteristics among households in "smallest" public housing projects and households living in the nine "largest" housing projects described in the test. The "smallest" projects are defined as projects with fewer than 250 units, within census tracts with fewer than 30 percent of households living below the LICO. Data for Boston are from Katz, Kling, and Liebman [2001], Table IV. Data for Chicago are from Rosenbaum, Harris, and Denton [1999], Table 1.

housing projects were located in the most affluent areas of the city. The mean percentage of households living below the LICO in census tracts around the set of small projects listed in column 2 of Table I was 25.4 percent. In comparison for the city as a whole, the median household lived in a census tract with 21.5 percent of households below the LICO. Thus, the largest contrast in neighborhood quality obtainable within the public housing program is between youths who grew up in the poorest areas in the city and those who grew up in moderately low- to middle-income neighborhoods. (A contrast between the poorest and wealthiest areas is not possible within the program, but this contrast would not be very interesting, since relocation policies are not likely to place low-income families in affluent neighborhoods on a large scale.)

Do families in the largest Toronto public housing projects live in conditions similar to those from the largest housing projects in other large U. S. cities? Table I lists the mean census tract characteristics among participants of the Moving to Opportunity Program in Boston and Chicago.[13] Column 3 displays mean tract characteristics for control participants in Boston, who were not given assistance to move from their housing project. Column 4 shows means and mean differences (against column 3) for characteristics of the census tracts moved into by participants receiving Section 8 vouchers to relocate.[14] Column 5 displays mean differences of tract characteristics for the experimental group of participants who moved to census tracts with fewer than 10 percent of households below the U. S. poverty line (the experiment group). Columns 6 through 8 show similar comparisons for the MTO program in Chicago.

The relative neighborhood variation between the two groups of Toronto public housing census tracts was at least as great as the relative variation between households from large projects in Boston and Chicago and households who moved using Section 8 vouchers. The Toronto percentage variation was about the same as that of the Boston households for the experiment versus control group, and somewhat less than that for the Chicago groups. For example, 50.3 percent fewer households in Toronto census

13. The data for Boston are from Katz, Kling, and Liebman [2001], Table IV. Data for Chicago are from Rosenbaum, Harris, and Denton [1999], Table I.

14. In Katz, Kling, and Liebman [2001] mean tract characteristics were computed for participants, whether they moved or not. Given the portion of movers and assuming that the mean tract characteristics of those who did not move were the same as those for the control group, mean tract characteristics for movers only can be backed out.

tracts around the smaller projects received welfare than households in tracts around the largest density projects. In Boston, welfare participation was 36.2 percent less in the Section 8 census tracts than in the control tracts and 68.7 percent less in tracts for those from the experiment group.

Overall, Table I shows that the neighborhood quality variation within the Toronto housing program was considerable, and similar to variation in the Boston MTO program. The Toronto projects cannot replicate the extreme conditions of poverty prevalent in the surrounding control census tracts in Chicago, where welfare participation was 75.0 percent, and 84.7 percent of households were headed by single females. Another important difference between Toronto and the two U. S. cities was the smaller percentage of blacks in Toronto neighborhoods. Neighborhood quality variation arises mostly from income segregation differences and not racial segregation differences, although neighborhoods by project type do differ by proportion of visible minority. Around the largest density projects, 62 percent of household heads are visible minority compared with 43 percent around the smallest density projects.

Census tract characteristics are designed to capture communities with households of similar socioeconomic backgrounds. Basu [2002], who matches census geography boundaries to school districts, suggests that high school district regions are similar to census tracts. For elementary school district boundaries, however, often more than one district is contained within a census tract. If the geographic scope by which neighborhoods affect outcomes is confined to smaller areas, we should examine the extent to which housing projects in Toronto differ across more finite locations. In an effort to show that neighborhood variation occurs across projects with smaller geographic range, Table II displays the same average household characteristics as Table I, but at the project enumeration area (EA) level. Within cities, enumeration areas delineate city blocks or high-rise apartment buildings. The number of dwellings in an EA does not exceed 440. A large apartment building, townhouse community, or collective dwellings usually forms a single EA. Enumeration areas for the largest density projects essentially contain only those in public housing, while those for the smallest housing projects also contain nearby neighbors. Table II shows that the EA variation of mean characteristics between large and small density projects is smaller than that for census tracts, but still notable. The proportion of house-

TABLE II
NEIGHBORHOOD VARIATION BETWEEN LARGEST AND SMALLEST TORONTO HOUSING PROJECTS USING SELECTED 1996 CENSUS TRACT AND CITY BLOCK CHARACTERISTICS

	By census tract		By city block or high-rise	
Neighborhood characteristic	Largest projects mean	Diff. in means smallest-largest	Largest projects mean	Diff. in means smallest-largest
Female household head	0.482	−0.18	0.567	−0.06
Black	0.249	−0.13	0.349	0.01
Below LICO (Canada) or poverty line (U. S.)	0.611	−0.36	0.743	−0.18
Receiving welfare	0.338	−0.17	0.466	−0.09
Owner-occupied household	0.081	0.45	0.009	0.13
Adult population with education of less than high school	0.425	−0.17	0.472	−0.17
Adult population with education of more than high school	0.410	0.20	0.358	0.16
Adult population with education of college degree	0.103	0.10	0.073	0.05
Median household income (1996 $Cdn)	13,693	28,301	14,780	9,776

City blocks (or enumeration areas) and census tracts are described in the text. "LICO" is Statistics Canada's Low-Income-Cut-Off. "Diff. in means" is the mean difference between census tract characteristics among households in "smallest" public housing projects and households living in the nine "largest" housing projects described in the text. The "smallest" projects are defined as projects with fewer than 250 units, within census tracts with fewer than 30 percent of households living below the LICO.

holds below the low-income cut-off is 74 percent for those in large density project EAs, compared with 56 percent of households surrounding smaller project EAs. Variation in education attainment between surrounding small and large project EAs is almost the same as that for census tract variation, and median income is 66 percent higher for EA households around the small density projects relative to the large density ones. Other characteristics, such as the proportion of owner-occupied housing, the proportion female-headed household, and black, do not vary as much.

Perhaps the strongest evidence that neighborhood quality varies across public housing projects in Toronto comes from comparing surrounding criminal activity. I was able to obtain occur-

TABLE III
CRIMINAL OCCURRENCES IN 1992 FOR SMALLEST AND LARGEST PUBLIC HOUSING PROJECTS

Type of occurrence	Largest projects	Smallest projects	Difference
	per 1000 household units		
Assault causing bodily harm	17.53	4.91	−12.62
Sexual assault	1.84	0.00	−1.84
Break and enter and attempted B&E	21.78	17.20	−4.58
Drug offense	11.74	2.46	−9.28
Neighbor dispute	421	307	−119
Arson	0.99	0.00	−0.99

Occurrences are all incidents on MTHC property that required a written report by MTHC Security Services. Column 2 shows the mean difference between crime occurrences among the nine largest public housing projects (described more in the text) and the 16 "smallest" projects. "Smallest" projects are defined as projects with fewer than 250 units, within census tracts with fewer than 30 percent of households living below the LICO.

rence data for 1992 from MTHC's private security service. Beginning that year, MTHC security services collected data on every police or security report that occurred on MTHC property, including those that did not lead to an arrest or conviction. The occurrences were divided by type of crime and by whether the event was minor or serious. All serious events required, at minimum, a written report, and all written reports were documented. The data were broken up by project. Total occurrences were divided by project household size. Importantly, the data included occurrences involving both residents and nonresidents on MTHC property.

Table III presents 1992 crime and victimization occurrences, separated by housing project category. The largest projects in downtown had the greatest incidence of arson, bodily and sexual assault, drug offenses, and neighbor disputes per 1000 households.[15] Per thousand household units 17.5 physical assaults occurred at the high-density projects in 1992, versus only 4.9 per thousand household units for the low-density projects. There were no sexual assaults reported in the low-density projects, while 1.8 sexual assaults per thousand

15. Similar patterns arise when defining neighborhood quality by project size, percent of households in surrounding census tract below the LICO, and whether in a townhouse or high-rise (see Oreopoulos [2001]).

households were reported in the high-density ones. Break and enters and drug offenses also occurred much more in the larger density projects. Since crime occurrences happen on public housing property but necessarily by those living there, these results do not imply that the neighborhood conditions around the largest projects led to more crime.

IV.C. The Application Process and the Assignment of Families into Projects

Until 1995, applicants on the MTHC waiting list were selected on the basis of a point system. Households were given points primarily based on financial need but also on current living conditions, welfare participation, overcrowding, and whether they were living in emergency housing. Those with the most points were housed first, giving preference to families most in distress. High demand for subsidized housing meant only those families who attained the near-maximum number of points were given offers of accommodation, and even then, these families waited an average of one and a half years. Administrators regularly updated the list and removed households no longer interested in accommodation. Only those who showed high need and continued interest for subsidized housing were kept on the list before making an offer.

Key for this study, families could not specify which project or in what type of project they wished to be housed. They were offered accommodation according to the first available unit with the correct number of bedrooms required while at the top of the waiting list. All MTHC applicants faced the same waiting list procedure.

Transfers happened infrequently. Families in subsidized housing could request transfer if a change in employment location or family size occurred. The option to change projects because of poor neighborhood environment was not permitted. For those whose entry year into the program is identified, the project linked to is the one they first enter, regardless of whether they move later.

An exception to the quasi-random nature of assignment into public housing was that families who expressed great disapproval with an initial offer would normally be given a second offer without being removed from the waiting list. Applicants who rejected their first two offers were removed from the list. The option to wait was not outlined on the application. Conversations with MTHC administrators revealed that initial rejections were rare because of the immediate desire to begin subsidies. A family could wait more than six months before receiving a second offer. Another exception to the assignment process was that applicants could specify up to six regional preferences. Regional preferences were rarely expressed because the fewer the regions a family was willing to live in, the longer it waited for an offer. In Section VI, adding region fixed effects does not affect the results.

To examine the possibility that some families selected into particular housing projects, we can at least examine observable characteristics of program participants at the time of entry. Table IV compares households from the high-density and low-density projects discussed above. If sorting between groups is minimal, we should see little difference in means between the two neighborhood-quality types. The actual administrative data used for the table are discussed in more detail in the next section. The table is subdivided between the group of youth, born between 1963 and 1970, who entered public housing before 1979 and after. The entry date for the latter group is known, and unknown for the former. In both samples youths lived in public housing for at least one year between 1978 and by the year they turned sixteen. The sample of public housing residents who entered before 1979 in the first two columns may not be representative of initial assignees to public housing, since the possibility of nonrandom exit cannot be ruled out. Thus, while this group includes children who entered the program at very young ages, the second group, whose entry years are known, provides a better sample to check for initial random assignment across projects.

Table IV shows no significant differences in family composition, parental income, and age of oldest parent at the time of entry among families assigned to the largest and smallest density projects. The average number of years spent in public housing is also similar. Youths who entered the largest housing projects after 1978 spent, on average, 6.3 years in the program, compared with 6.7 years for those assigned to the smallest projects. Parents spent about two years longer in the program than these children, on average. Cross-section characteristics of families identified in public housing in 1978 that entered that year or earlier also show little variation across project type.

TABLE IV
SELECTED FAMILY CHARACTERISTICS OF HOUSEHOLDS WITH CHILDREN BY YEAR OF ENTRY INTO LARGEST AND SMALLEST PUBLIC HOUSING PROJECTS

	Entered in 1979 or before (Year of entry unknown)		Entered after 1979 (Year of entry known)	
	Largest projects mean	Diff. in means smallest-largest	Largest projects mean	Diff. in means smallest-largest
Number of children in family (in 1978 or at year of entry)	2.7	0.01	3.1	−0.22
		(0.21)		(0.28)
Single household head (in 1978 or at year of entry)	0.57	0.01	0.55	0.05
		(0.05)		(0.04)
Average parental earnings when youth aged 9–18	13,276	698	10,816	642
		(644)		(691)
Parental earnings in year before entered PH	NA	NA	12,733	−432
				(884)
Age entered PH (child) (for columns 1–2: age in 1978)	11.52	0.03	13.78	−0.30
		(0.11)		(0.16)
Age entered PH (oldest parent) (for columns 1–2: age in 1978)	37.98	−0.46	39.77	−0.46
		(0.42)		(0.54)
Years in PH (child) (for columns 1–2: years in PH since 1978)	7.9	−0.22	6.3	0.35
		(0.20)		(0.26)
Number of years in PH (oldest parent) (for columns 1–2: years in PH since 1978)	10.0	−0.54	8.5	0.02
		(0.26)		(0.35)
Sample size	1902	1086	876	412

"Diff. in means" is the mean difference between census tract characteristics among households in "smallest" public housing projects and households living in the nine "largest" housing projects described in the text. The "smallest" projects are defined as projects with fewer than 250 units, within census tracts with fewer than 30 percent of households living below the LICO. Standard errors are reported in parentheses, adjusted for project level clustering.

V. DATA[16]

V.A. *The Intergenerational Income Database*

The Intergenerational Income Database (IID) links tax data of children born between 1963 and 1970 and their parents for all years between 1978 and 1999. The Family and Labour Studies division of Statistics Canada constructed the IID using several administrative files. Parents and children were linked using the T1 Family File (T1FF) of the Small Area and Administrative Data Division of Statistics Canada. The T1FF is a data set of individual tax records that has been processed in a way that matches members of each tax filer's family. Couples (including spouses and common law couples) are linked using Social Insurance Numbers and spousal Social Insurance Numbers, as well as name and address information. Children are matched to their parents primarily using name and address.[17]

Canadians file taxes individually. Identification of a parent and child in the IID requires that the child file from the same address as a parent. Younger children are more likely living at home but less likely to file and vice versa. With the purpose of maximizing the number of parent and child matches, the IID takes all 16- to 19-year-olds in 1982, 1984, and 1986 who filed at least once from a parent's address over a five-year period beginning in these years. For example, a 17-year-old in 1984 would be identified in the IID if she filed from home at least once from 1984 to 1988.

There are 3,465,000 youth recorded in the IID. Comparing the IID sample population in 1986 with the corresponding population from the 1986 census, the coverage rate is 72 percent [Cook and Demnati 2000]. The high coverage rate is not surprising considering that most children still live at home, even by age twenty. Using the 1996 census, 96 percent of 16-year-olds in Toronto live with a parent, but only 21 percent receive nontransfer income. For 20-year-olds, 81 percent live with a parent, and 73

16. Statistics Canada protects the confidentiality of all data sets used in this paper. The Intergenerational Income Database and the Longitudinal Administrative Database (discussed below) reside within Statistics Canada in Ottawa, and all retrievals are done on site. Only a small staff within Statistics Canada can access the data directly, and only aggregated information that conforms to the Statistics Act is released.

17. See also Corak and Heisz [1999] and Corak [2001], who use the IID for research on the intergenerational mobility in Canada. Harris and Lucaciu [1995] describe in more detail how families were linked using the T1FF.

percent receive nontransfer income. If a child files only once over this period, the IID will still record her. A child may also live away from home (for example, at college), but still file from a parent's address and end up in this data set.

All Canadians must file an annual return if they pay income tax that year. Without income, individuals may still file to claim a nonrefundable tax credit for tuition and a monthly deduction for full-time education enrollment. They may also file to claim a substantial general sales tax rebate, although the rebate did not occur until 1989.

The T1FF imputes youths who did not file from the Federal Family Allowance program. Parents are required to file a return and state the age of each dependent child younger than age nineteen in order to claim a monthly deductible. The identification of nonfilers from the program provides an elegant opportunity to examine whether youths who do not file (and so no outcome variables for them are available) are more likely to come from high-density housing projects than small ones, and whether observable parental characteristics of children who do not file are substantially different from those who do. The IID does not contain these imputed youths, but another data set, the Longitudinal Administrative Database, does. In subsection V.C, below, I discuss this data set and investigate the potential for selection bias from nonreporting children in the IID.

The IID links tax returns from 1978 to 1999 for every child and parent recorded from the T1 Family File. For each year an individual filed, detailed administrative information exists for nongovernment income including earnings and self-employment income, transfer income including unemployment insurance benefits and welfare receipt (after 1990), age, gender, marital status, family composition, and resident address. Notable variables not recorded include education attainment, ethnic composition, and race.

The main outcome variables examined are market earnings, total income, unemployment participation, and welfare receipt. Market earnings are computed as total wages, salaries, and commissions, plus self-employed income and other employment income that includes gratuities and tips. Total income includes market earnings plus pension plan and disability benefits, unemployment insurance benefits, general sales tax credits, and any federal supplement and welfare payment since 1992. Earnings and income are averaged between 1997 and 1999, when youths from the IID are 27 to 36 years old.

Parental background, including number of children, age, and marital status are also noted from the IID, as are the number of years receiving unemployment insurance benefits and welfare payments. Parental adjusted income was computed as the mother's and father's total income, divided by family size, with the first parent receiving a weight of 1, the second (if any) a weight of 0.8, and each child receiving a weight of 0.3. Parental income was averaged while the child was aged 9 to 18. All dollar amounts were converted to 1992 Canadian dollars using Statistics Canada's Consumer Price Index.

V.B. Linking Youths from the IID to Public Housing Projects

Instead of relying on small survey samples that identify whether a family or household has participated in a public housing program, I match public housing postal code addresses to the Intergenerational Income Database. Postal codes in Canada are comprised of six alphanumeric digits and identify very specific geographic locations. Each code generally refers to one side of a city street, often over only one block or a single apartment building. Approximately three-fourths of the population sample were located in public housing addresses with unique postal codes. Even small public housing dwellings often consisted of a row of townhouses with a single corresponding unique code. To ensure that everyone in my sample resides in MTHC public housing, I only use postal codes that uniquely match to these projects.

The postal code for matching to projects was taken from the child's tax file. When a child did not file, the postal code from the father's tax file was used if both parents reported they were married or if the mother's file was missing that year. Otherwise, the mother's postal code was used. The match was done for all years from 1978 until the child was 16. For those who entered public housing after 1978, the total number of years in the projects and prior conditions before entry are known. A majority of youths from the IID entered public housing before 1978, before ages 8 to 15. An advantage of using this sample is that individuals spent many years, and at early ages, exposed to particular neighborhoods. However, youths who entered the program and left before 1978 are not picked up. The smaller sample that entered public housing after 1978 does not face this selection concern, but the length of exposure to a project neighborhood is less. I examine results with both groups.

V.C. Addressing Attrition

The Intergenerational Income Database in the IID does not face the same attrition concerns as other micro panel surveys. As long as individuals file within Canada, the IID links annual information on movers and nonmovers with tax returns. Not filing for one year does not affect linking to information filed the next year. However, the data set does not cover the entire population of 16- to 19-year-olds in 1982, 1984, and 1986 because not everyone filed a return from a parent's address. The IID underrepresents youths who had no attachment to the labor market before they left home, or who participated in the underground economy without reporting income activity. Both situations are plausibly more likely for children of families living in public housing. If worse outcomes are associated with nontax filers and if the likelihood of filing is a function of the public housing project assigned, the analysis may miss important neighborhood effects.

We can explore how many are missing from the IID, their parents' characteristics, and where they are missing from using the Longitudinal Administrative Database (LAD). The LAD was constructed in a similar way to the IID, but instead focuses on the entire population of Canadian tax filers rather than a particular cohort sample. The LAD begins with a 20 percent sample of the T1 Family File in 1982 and links these individuals to their subsequent T1 tax returns. The LAD is augmented up each year with new tax filers so that it consists of approximately 20 percent of tax filers for every year. The crucial difference between the IID and the LAD, for the purpose of this study, is that the LAD includes imputed youths identified by information filed by parents claiming Family Allowance Benefits. As discussed above, parents can claim a deductible for every dependent younger than age nineteen and must state each dependent's age on the return. This allows us to pick out the same cohort of public housing tenants from the IID, through the address of a parent, and compare youth who file and youth who do not before leaving home.

Public housing addresses are matched only to parents' filing addresses for this sample. I use the same cohort of youths matched to parents in public housing as the IID: those 16 to 19 in 1982, 1984, and 1986 living in public housing before age 17 (and remove duplicate matches). Any youth who files at least once over a five-year period beginning in these years when a parent files will be in the IID. To keep the sample as large as possible, I do not distinguish between youths who entered in 1982 or before (the first year of the LAD panel) and those who entered after.

Table V shows the sample from public housing predicted missing and nonmissing in the IID. From the first column, 710 youths aged 16 to 19 in 1982, 1984, and 1986 in the LAD lived in the small density public housing projects before age 17. The IID identifies 470 of them, for a coverage rate of 66.2 percent. The fraction is lower than the national average coverage rate of 72 percent, which is not surprising. Cook and Demnati [2000] for example, find the IID misses 36.2 percent of children in families with parental income less than $10,000, compared with the full sample of tax filers in 1998. More important, the coverage rate for youths from the large density housing projects is 66.5 percent, almost exactly the same as that for the small density projects. I also do not find significant differences in parental characteristics between the missing from small and large density projects. The fraction of one-parent families, the average number of children, and the mean parental income are about the same.

Whether a youth ends up in the IID does not appear determined by the project type assigned. If true, and if everyone in the IID assigned to one type of project would still have filed at another project (but possibly with a different outcome), then comparison of outcomes among large and small density housing projects from the IID sample produces valid and unbiased estimates of an average treatment effect for the observed sample [Lee 2002]. If this monotonicity condition does not hold, we would expect differences in the parental characteristics between small and large density projects. However, empirically this does not seem to be the case.

I also treat "not filing" as an outcome variable (for example, as an indicator for no labor-market participation or leaving home early). In general, missing youths from the IID never worked during their teens and early twenties before they left their parents' home permanently. Possible reasons for not filing include running away, underground employment activity, and going to college without working during summers. The ability to claim a deduction for tuition or full-time employment reduces the likelihood that individuals do not file for "favorable" reasons. Table V indicates that missing youth from the IID are associated with slightly poorer parental background characteristics. The fraction

TABLE V
COUNTS OF MISSING YOUTH IN IID USING PARENTAL RECORDS OF FAMILY TAX ALLOWANCES BY HOUSING PROJECT QUALITY

				Missing youth-parent characteristics			Nonmissing youth-parent characteristics		
	(1) Total sample size	(2) IID sample size	(3) IID coverage rate	(4) Single in 82 or entry year	(5) Number of children	(6) Parental income	(7) Single in 82 or entry year	(8) Number of children	(9) Parental income
By small- or large-density project									
Small-density projects (mean)	710	470	0.662	0.63	2.84	11400	0.60	2.68	13600
Large-density projects (diff)	1180	785	0.665	−0.043	−0.004	−1096	−0.033	0.058	−406
				(0.040)	(0.131)	(951)	(0.040)	(0.093)	(957)
By number of household units in project									
<=250 units (mean)	1180	790	0.669	0.59	2.70	11100	0.57	2.47	14600
>250, <=700 units (diff)	1715	1130	0.659	0.026	0.103	985	0.046	0.010	−1311
				(0.032)	(0.102)	(764)	(0.023)	(0.073)	(765)
>700 units (diff)	1060	680	0.642	−0.057	0.155	491	0.006	0.243	−1061
				(0.035)	(0.112)	(836)	(0.028)	(0.081)	(854)
By percent in Census Tract below LICO									
<=0.25 (mean)	340	220	0.647	0.53	0.28	12200	0.53	2.56	14200
>0.25, <=0.50 (diff.)	1680	1130	0.673	0.07	−0.20	547	0.087	−0.164	−1528
				(0.050)	(0.159)	(963)	(0.036)	(0.114)	(917)
>=0.50 (diff.)	1940	1250	0.644	0.05	0.00	−844	0.005	0.101	−1103
				(0.049)	(0.156)	(892)	(0.032)	(0.114)	(858)

Columns (1) to (3) calculate the coverage rate of youths 16 to 19 in 1982, 1984, and 1986 who lived in public housing in the Intergenerational Income Database (IID). The counts are based on imputed youth from parental family allowance deduction claims in the Longitudinal Administrative Database. Columns (4) to (6) show means and difference in means of parental characteristics between large- and small-density housing projects for the missing sample from the IID. Columns (7) to (9) show the same but for the sample not missing from the IID. See text for details.

of one-parent families from the missing sample is about 61 percent compared with about 58 percent for the nonmissing sample. Family size is higher, and parental income lower. Labor-market outcomes for these children are likely worse, on average, than for the observed sample.

VI. Results

VI.A. *Differences in Means*

A useful starting point is to estimate neighborhood effects for children from families in the private housing market who lived in the same census tracts as children from high- or low-density public housing. We can then contrast these results, which make no attempt to account for omitted variable bias, with those that use the quasi-experimental setting of the program. Columns (1) to (3) in Table VI compare outcome means for youths from the census tracts containing the 9 largest density housing projects (discussed above) to those from tracts with the 16 smallest density projects with less than 250 units in size in tracts with fewer than 30 percent of households below the Low-Income-Cut-Off. From column (1) mean log income among males aged 27 to 36 between 1997 and 1999 from the high-density project census tracts (but not from the projects themselves) is 10.05 compared with 10.29 for those from low-density project census tracts. Boys from the wealthier neighborhoods earned about 24 percent more than boys from the poorest neighborhoods in the city. In column (3) I show the predicted increase in log income from living in the low-density census tracts relative to high-density tracts after controlling for a complete set of age indicators, a variable for parental permanent income, parent welfare receipt, family size, and parental marital status. Even when limited family background controls are added, the estimate still implies that men from the smaller project census tracts make, on average, 17 percent more than men from the larger project tracts. Similar results hold when looking at males and females combined. For welfare participation between 1992 and 1999, I used a probit model, and the coefficient shown in column (3) can be interpreted as the estimated change in probability if an individual with mean background characteristics had lived in a small project tract rather than a large one. The estimated coefficient suggests welfare participation would fall by 30.4 percent if a young adult

TABLE VI

MEAN OUTCOMES AND MEAN DIFFERENCES BETWEEN YOUTH FROM LARGEST AND SMALLEST PUBLIC HOUSING PROJECTS AND BETWEEN YOUTHS FROM THESE CENSUS TRACTS BUT NOT FROM PUBLIC HOUSING

	(1) Mean high-density census tracts	(2) Mean difference low-high density tracts, no controls	(3) Dummy coeff. for low-density tracts with controls
IID data (adults aged 29 to 36 in 1999)			
	Youths not from public housing, but in PH Census tracts		
Log earnings, 1997–1999	9.89	0.189	0.128
		(0.022)	(0.023)
Log earnings, 1997–1999 (males)	10.04	0.240	0.163
		(0.028)	(0.030)
Receiving welfare, 1992–1999	0.23	−0.121	−0.070
		(0.007)	(0.007)
Receiving unemployment insurance, 1992–1999 (males)	0.44	−0.089	−0.055
		(0.013)	(0.014)
Log income, 1997–1999	9.95	0.180	0.124
		(0.020)	(0.021)
Log income, 1997–1999 (males)	10.05	0.244	0.169
		(0.026)	(0.027)
Missing from IID			
IID male and female sample size (by large and small projects)	3334	9432	

(male or female) lived in a low-density project census tract rather than a high-density one.

The prediction that neighborhood quality substantially affects future labor-market outcomes disappears when examining outcomes of children from within the public housing program. Columns (4) to (6) in Table VI show the same set of results, but for youths from public housing who entered after 1978 in either low-density or high-density projects. This particular sample al-

TABLE VI
(CONTINUED)

(4) Mean high-density projects	(5) Mean difference low-high density proj., no controls	(6) Dummy coeff. for low-density proj. with controls	(7) Mean high-density projects	(8) Mean difference low-high density proj., no controls	(9) Dummy coeff. for low-density proj. with controls
IID data (adults aged 29 to 36 in 1999)					
Youths who entered public housing after 1978			Youths from public housing		
9.72	0.008	0.042	9.70	0.012	0.018
	(0.052)	(0.057)		(0.029)	(0.026)
9.86	0.056	0.048	9.89	0.006	−0.001
	(0.048)	(0.056)		(0.054)	(0.054)
0.37	−0.018	−0.003	0.39	0.003	0.007
	(0.032)	(0.033)		(0.015)	(0.015)
0.42	0.024	0.023	0.45	0.018	0.013
	(0.039)	(0.040)		(0.023)	(0.024)
9.77	0.018	0.045	9.79	0.003	−0.004
	(0.034)	(0.039)		(0.029)	(0.026)
9.92	0.041	0.050	9.92	−0.018	−0.029
	(0.054)	(0.060)		(0.045)	(0.045)
			0.34	−0.003	0.002
				(0.022)	(0.023)
940	412		3012	1498	

The first three columns include the sample of youths from census tracts containing either the smallest or largest public housing projects, but who are not from public housing themselves. The smallest projects are defined as projects with fewer than 250 units within census tracts with fewer than 30 percent of households living below the LICO. The nine largest projects are mostly located in central-downtown Toronto. Columns (2), (5), and (8) show the mean difference between outcomes among youths from the smallest project census tracts and projects from the largest housing project tracts or projects. None of the differences in columns (5) and (8) are significant from zero (p-value < 0.10). Columns (3), (6), and (9) show dummy coefficient estimates from regressing the outcome variable on age dummies, gender, log parental income, family composition, years any parent on social assistance, family size, entry year indicators (for project sample) and a dummy variable for the indicated measure of neighborhood quality. Standard errors are reported in parentheses, adjusted for project level clustering.

lows us to track program participants regardless of when they moved. Average log earnings for those from the large density projects is nearly identical to the average for those from the small projects (9.72 and 9.73, respectively). The null hypothesis that neighborhood quality does not affect income cannot be rejected whether controlling for family background or not, although the standard errors indicate somewhat imprecise estimates.

The fraction of youth from the large density projects receiving welfare for at least one year from 1992 to 1999 is 37.2 percent. For the smaller projects the fraction is 35.4 percent. The difference is not significant (p-value $> .1$). Adding family background controls further reduces this difference to 0.3 percentage points. The small differences between project types for welfare participation also translate to small differences in total income. Those from the larger projects received, on average, 9.77 in log earnings averaged between 1997 and 1999, almost the same average as those from the smaller, low-density projects.

The confidence interval for the effect of living in a small density project on earnings is considerably smaller using the full sample of youths from public housing that includes those who entered before 1979. We do not know who entered the program and left before this year. The full sample therefore introduces a selection bias if the number of youths who leave the program before 1979 is dependent on project type. The previous results from Table IV indicate little evidence of this. The average number of years in public housing since 1979 is virtually the same between those from the small and large density projects, as is the age of parent and age of child. These comparisons do not rule out the possibility of selected attrition (conditional on age and remaining length in public housing), but they do suggest the full sample results that include a large number of early-entries are worthwhile examining.

Columns (7) to (9) in Table VI show the estimated effects from growing up in a small versus large density housing project for the full sample of public housing residents in the IID. Average labor-market outcomes are about the same whether from a large or small density project. Mean log earnings, for example, is 9.70 for the group from the largest density projects, compared with 9.71 for the group from the smallest. Adult welfare participation rates are almost identical. Including family controls in column (9) does not alter the prediction that neighborhood quality does not affect long-run labor-market outcomes. This is reassuring, since unbiased neighborhood effect estimates under random assignment should not change with additional controls.[18]

Using the Longitudinal Administrative Database to impute missing youths from the IID because they did not file a tax return

18. It is worth pointing out that all estimates with the full sample are measured precisely. Not rejecting that mean outcomes between alternative project types are equal arises because of similar estimates for the means and not because of high standard errors.

before leaving home, we find no differences in the likelihood of missing whether from the small density projects or large ones. The LAD estimates 34 percent of the IID cohort are missing, from both groups. Adding family background controls does not change this result.[19]

Table VII presents a similar analysis of differences in means using alternative categorizations of neighborhood quality. I redefine neighborhood quality by the total size of the project, the percentage of households in the census tract around the project below the LICO, and whether the project is comprised of all high-rises (more than five stories) or all townhouses. The first three columns show results for the sample of males and females who entered public housing after 1979. Columns (4) to (7) show the same results but for the full sample that includes those who entered before that year.

The first part of the table contrasts outcomes for youths from all large, medium-size, and small-sized projects. Column (1) shows that adult earnings for those from projects larger than 700 units are about 2.6 percent more, on average, than earnings among those from projects with less than 250 units. The difference estimates for the sample that includes children from projects before 1978 are closer to zero, and precisely estimated. For example, the fraction of youths receiving welfare between 1992 and 1999 remains constant across project-size type. Mean log earnings for youth from small, medium, and large projects is 9.72, 9.73, and 9.71, respectively. Family background controls did not alter these outcome differences significantly. The fraction missing from the IID is not significantly different across these tract categories.

The next set of rows categorizes public housing projects by conditions within the surrounding census tract. Those in the IID from census tracts with fewer than 25 percent of households with incomes below the Low-Income-Cut-Off earned, on average, about \$16,800 between 1997 and 1999; those in census tracts with more than 50 percent of households below the LICO earned about \$16,400. The full sample shows negligible differences in earnings,

19. In Oreopoulos [2001] I also show that the number of times filing a tax return, conditional on being in the IID, does not depend on neighborhood quality. I also report outcomes of children living in different housing projects from the 1996 Census. Although restricted to outcomes for youths still living at home, the census is not subject to the same kinds of noninclusion biases that the IID potentially faces. I find no differences in education attainment and idleness among 16- to 25-year-olds living at home.

TABLE VII

MEANS AND DIFFERENCE FROM MEANS FOR VARIOUS PUBLIC HOUSING NEIGHBORHOOD QUALITY MEASURES, WITHOUT FAMILY BACKGROUND CONTROLS

	Youths who entered after 1978			Youths from public housing				
	(1) Log earnings	(2) On welfare (1992–1999)	(3) Log income	(4) Log earnings	(5) On welfare (1992–1999)	(6) Log income	(7) Missing from IID	(8) Sample size (for column (6))
	By number of household units in project							
<=250 units (mean)	9.71	0.41	9.79	9.72	0.40	9.79	0.33	2588
>250, <=700 units (diff)	−0.042	0.000	−0.012	−0.005	−0.001	0.016	0.011	4007
	(0.054)	(0.024)	(0.045)	(0.033)	(0.016)	(0.024)	(0.018)	
>700 units (diff)	0.026	−0.036	−0.017	−0.015	0.005	−0.029	0.031	2379
	(0.059)	(0.026)	(0.049)	(0.031)	(0.026)	(0.027)	(0.020)	
	By percent in census tract below LICO							
<=0.25 (mean)	9.73	0.38	9.81	9.72	0.38	9.79	0.35	655
>0.25, <=0.50 (diff.)	−0.030	0.032	−0.026	0.00	0.023	0.01	−0.03	3866
	(0.055)	(0.027)	(0.046)	(0.030)	(0.023)	(0.027)	(0.028)	
>=0.50 (diff.)	−0.028	0.001	−0.052	−0.02	0.012	−0.01	0.00	4453
	(0.053)	(0.024)	(0.044)	(0.029)	(0.026)	(0.028)	(0.028)	
	By townhouse or high-rise apartment							
Townhouse (mean)	9.71	0.40	9.80	9.72	0.38	9.84	0.34	1889
High-rise (diff.)	0.067	−0.026	−0.014	0.002	0.005	−0.003	0.004	1549
	(0.061)	(0.034)	(0.048)	(0.034)	(0.017)	(0.030)	(0.025)	

Columns (1)–(3) show raw means for particular neighborhood quality categories from the sample of male and female youth who entered public housing after 1978, when entry date can be identified. Average deviations from these means are for the other categories. Columns (4) to (7) show results for the full sample of past public housing tenants. Standard errors from regressing the outcome on dummy variables for neighborhood quality are in parentheses, adjusted for clustering by project. Income and earnings outcomes are averaged over 1997–1999 for the sample who were born between 1963 and 1970.

income, welfare participation, and likelihood missing. The direction of the earnings and income differences are usually what would be expected if neighborhood influences matter. But the differences are mostly between 0 and 2 percent and are not statistically significant.

We might expect labor-market outcomes to vary depending on whether youths lived in high-rise apartments or in townhouse complexes. Townhouses offer more space between neighbors and front doors that lead directly outside, rather than to corridors and elevators. Families are more likely to avoid contact with other tenants if they live in a townhouse. Table VI, however, indicates no substantial differences in income welfare participation between these dwelling types, especially when using the full sample.

VI.B. Wage and Schooling Distributions for Youth from Different Projects

The large sample of public housing participants facilitates a comparison of the entire distributions of long-run outcomes between youths from the high- and low-density projects. Figure II, Panel A, shows the kernel density estimates of total income for the full sample of 29 to 36 year olds in 1999 from the smallest density projects with fewer than 250 units within census tracts that had fewer than 30 percent of households below the LICO. The kernel density estimate for youths from the largest density projects is overlaid on top of the density estimate for the smaller projects. Background controls are added in Figure III, Panel B, by estimating the densities using residuals from the regression with log total income on age indicators, gender, parental income, parental welfare participation, family composition, and family size. The mean of the residuals, with both groups included, is zero.

Although every youth from the sample has a low-income family background and lived in public housing, the variance in Figure II is substantial. Participants in the right tail of the distribution fare quite well. The eighty-fifth percentile from the high-density projects receives $43,503, while the eighty-fifth percentile from the low-density projects receives $43,802. These amounts are close to the seventy-first percentile for the entire city population. Persons in the lower end of the distribution receive much less. Many are on welfare, as indicated by the disproportional left-end tail. The fifteenth percentile from the large density projects receives $10,133 compared with $10,099 for the fifteenth percentile from the small projects. Overall, the two sets of density

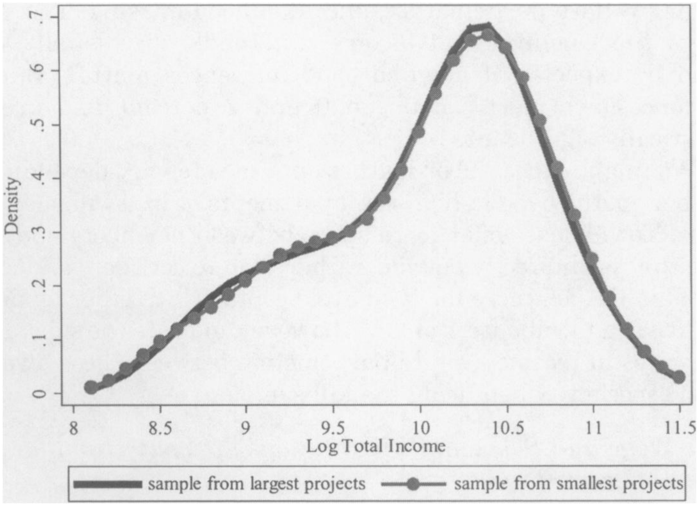

FIGURE II

Kernel Densities for Log Total Income for 29 to 36 Year-Olds in 1999 from High- and Low-Density Public Housing Projects

A: No Controls: Bandwidth = 0.20

The two kernel densities overlaid in Panel A and B are for the sample from the nine projects with the highest density of low-income households in the surrounding neighborhood and the sample from lowest density projects with 250 units or fewer, and in census tracts with fewer than 30 percent below the LICO. Residuals in Panel B are generated from regressing log total income on a full set of age and region dummies, period of entry dummies, plus family background controls. See text for further details.

estimates are remarkably similar, whether observable family and region controls are added or not. The Komogorov-Smirnov test cannot reject the hypothesis that the two empirical distributions are the same (p-value = 0.98). None of the densities of other labor-market outcome variables, for males, females, or both, and for those who entered public housing before or after 1978 are significantly different between project type.

VI.C. Sibling and Neighbor Correlations

The analysis so far separates project differences specifically into two or three observable categories. Each MTHC project, however, is unique and may have many specific characteristics not adequately captured in broad categories. Recall from subsection III.B that we can also express the importance of neighborhood differences by measuring correlations between unrelated

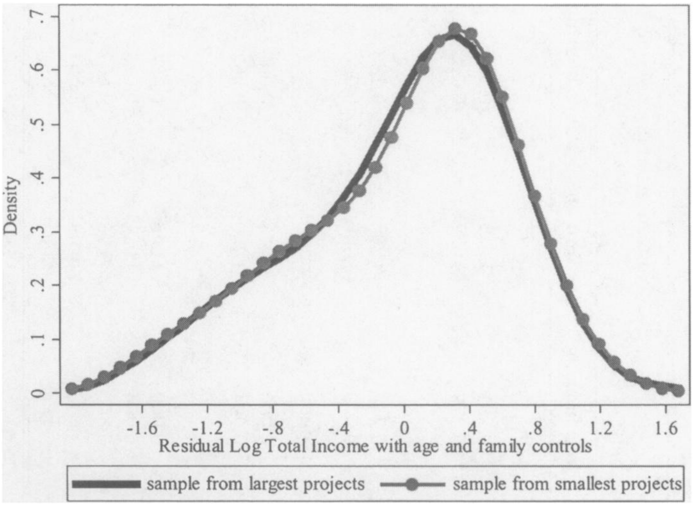

FIGURE III

Kernel Densities for Log Total Income for 29 to 36 Year-Olds in 1999 from High- and Low-Density Public Housing Projects

B: Age and Family Background Controls: Bandwidth = 0.20

The two kernel densities overlaid in Panels A and B are for the sample from the nine projects with the highest density of low-income households in the surrounding neighborhood and the sample from the lowest density projects with 250 units or fewer, and in census tracts with fewer than 30 percent below the LICO. Residuals in Panel B are generated from regressing log total income on a full set of age and region dummies, period of entry dummies, plus family background controls. See text for further details.

neighbors. The neighbor correlation estimates the portion of the outcome variance attributable to observable and unobservable neighborhood differences. Perhaps the greatest usefulness of this approach is the ability to contrast neighbor correlations with sibling correlations (which approximate the portion of the outcome variance attributable to characteristics common between siblings). The comparison provides perspective of the relative importance of family versus neighborhood factors in explaining labor market outcome differences.

The first two columns of Table VIII present the estimates of adult annual income correlations across brothers and across neighbors.[20] I control for age by calculating the correlations of the residuals after regressing log income on age and age squared in

20. Including both brothers and sisters produces smaller sibling correlation estimates.

TABLE VIII
ESTIMATED SIBLING AND NEIGHBOR CORRELATIONS

	Total income (males)		Earnings (males)		Number of years of welfare (1992–1999)	
	All Toronto	Public housing	All Toronto	Public housing	All Toronto	Public housing
Siblings						
Sibling correlation	0.284	0.312	0.280	0.261	0.241	0.217
	(0.006)	(0.048)	(0.006)	(0.079)	(0.022)	(0.022)
Sibling correlation after controlling for observable family characteristics	0.268	0.296	0.265	0.244	0.205	0.185
	(0.004)	(0.043)	(0.005)	(0.079)	(0.020)	(0.023)
Neighbors within enumeration areas (Toronto sample) and projects (public housing sample)						
Neighbor correlation	0.043	0.004	0.054	0.000	0.071	0.005
	(0.013)	(0.004)	(0.023)	(0.004)	(0.030)	(0.003)
Neighbor covariance after controlling for observable family characteristics	0.028	0.005	0.041	0.000	0.046	0.004
	(0.019)	(0.004)	(0.018)	(0.004)	(0.021)	(0.003)
Sample size	184,600	4,060	150,617	3,855	369,200	6,601
Number of sibling pairs	20,082	684	21,421	622	25,450	1,851
Number of neighborhoods	3,391	81	3,391	81	3,391	81

Adult men's incomes are averaged over six years for children in the IID from 1992–1999. The public housing sample combines all households living in uniquely matched MTHC postal codes. See text for details.

1999.[21] I also control for other observable characteristics by computing the correlations of residuals generated from regressing log income on age, age squared, and my additional family background controls.

The "residualized" log income correlation among brothers for the city of Toronto is 0.284. Page and Solon [1999] estimate a similar value, 0.316, for the earnings correlation between brothers in the United States.[22] Interestingly, when I control for observable family characteristics, the brother correlation falls only a little, to 0.268. This means my family-background controls do a poor job at explaining the similarities across brothers' income.

The income variance for the full sample of men from public housing is larger than the citywide variance. The finding may seem surprising because subsidized housing participants come from more similar backgrounds than those in the city sample. We might expect mostly low-income outcomes for sons from low-income families. Nevertheless, many sons from low-income families escape low income themselves. The brother correlation estimate for the public housing sample is 0.312. Despite the greater level of homogeneity in family circumstance across public housing participants, much of the fact that some end up with very high incomes and some very low can be attributed to characteristics common among brothers.

Knowing a past neighbor's income, however, predicts virtually nothing about another neighbor's income. I estimate a negligible income correlation across unrelated neighbors from the same public housing projects. The estimate, adjusted for age, is 0.004, compared with 0.043 for the city sample of neighbors from the same enumeration area.[23] Controlling for observable family

21. For exposition, I sometimes refer to the log income covariance as just the income covariance.

22. Caution must be taken with comparing citywide with nationwide correlations. Page and Solon [1999] find their brother earnings correlation drops to 0.186 after controlling for urban city and region. See Corak and Oreopoulos [2003] for a presentation of sibling correlations for Canada and comparison of these results with other countries.

23. As Duncan and Raudenbush [2001] note, small correlations may still imply significant neighborhood influence. For example, with citywide neighbor variation explaining an estimated 5.4 percent of the total log earnings variance, a one-standard-deviation increase in the latent variable for citywide neighborhood quality should increase earnings by approximately $\sqrt{0.054}$ times the standard deviation in log earnings (0.8), or 18.6 percent. The link between neighbor correlations and effect sizes is more direct by comparing only two neighborhoods. Consider the regression equation $Y_{in} = \eta T_{in} + \varepsilon_{in}$, where Y_{in} is an outcome variable for individual i in neighborhood n, T_{in} is a neighborhood indicator variable, and ε_{in} is a residual term uncorrelated with T_{in}. The neighbor covari-

background characteristics does not change the project correlation estimates, all centered at zero. Similar results hold when using only the sample that entered public housing after 1978.

Many siblings in my public housing samples receive welfare when they are older. Table VIII also shows the estimated sibling and neighbor correlation for the number of times on welfare between 1992 and 1999.[24] I used residuals from regressing on age and age squared to measure the correlation. The city variance estimate is 1.51 years. The corresponding brother covariance is 0.36. Family and community factors, therefore, explain about 24 percent of the total variance in years on welfare participation. The brother correlations in years on welfare among the public housing samples are similar. The point estimate for the correlation in years on welfare between project neighbors, however, is .005 and insignificantly different from zero.

VII. Discussion

Natural variation in the characteristics across public housing projects in Toronto is used to examine the relative importance of neighborhoods in influencing the long-run labor-market outcomes among adults from low-income family backgrounds. The advantage of using a sample of public housing participants in Toronto is that the nature of the application process prevents much selection across neighborhood types. Consequently, estimates for neighborhood effects within public housing are likely closer to reality than estimates that use a sample of households in the private housing market. The study also explores variation between several definitions of neighborhood quality without relying on moves by a treatment group, and is able to contrast its findings with previous approaches that estimate neighborhood effects in the private household market while attempting to control for family background with observable characteristics.

City blocks and census tracts surrounding the Toronto housing projects differ substantially in terms of average household income,

ance is $\eta^2 p(1-p)$, where p is the fraction of the sample from the first neighborhood. Caution must be taken when comparing sibling and neighbor correlations within cities and nationwide. Across country sibling correlations fall significantly after introducing controls for urban residence and region [Page and Solon 1999]. For more discussion on sibling earnings correlations in Canada, see Corak and Oreopoulos [2003].

24. The correlation framework does not work well with binary outcome variables, such as an indicator for welfare participation. Future work is needed to adapt this approach to handle these variables.

parental education attainment, family composition, and parental welfare participation. Exposure to crime also varies markedly by project. But none of these neighborhood quality differences correlate with a young resident's chances for long-run labor-market success. This is the key finding of the paper. The distributions of annual earnings, income, and years on welfare for youth from public housing remain markedly similar across project.

A second finding is that family differences, within this relatively homogeneous sample of low-income family background and public housing residence, matter a great deal. Although living in alternative housing projects cannot explain large variances in labor-market outcomes, family differences, as measured by sibling outcome correlations, account for up to 30 percent of the total variance in the data. The results arise in part because families in the sample differ in their dependence on housing subsidies, and some leave the program earlier than others. The large sibling correlations, however, do not change very much when basic parental income and marital status controls are added. Further research should be undertaken to understand why some siblings end up with relatively high annual earnings, while other siblings, with parents in similar low-income situations fare worse. Taken overall, the results suggest that policies aimed at improving outcomes among children from low-income backgrounds are more likely to benefit by addressing cases of household distress and family circumstance than by improving residential environment conditions.

These results are consistent with recent studies from the Moving to Opportunity experiment in the United States. Studies from the MTO program generally find small increases in employment participation and earnings among parents from housing projects who were assisted to move into much more affluent neighborhoods. Parents and children experienced large improvements in measures of well-being, such as overall resident satisfaction, crime incidence, and health. But in terms of standardized test results and school performance, researchers find few effects for the children who move to better neighborhoods. Indeed, one study reports that suspensions and disciplinary action were *more* likely for children who moved into better communities [Ludwig, Duncan, and Hirshfield 2001]. Findings from the Toronto public housing program suggest that any short-term benefit to parents or children from moving into a more aesthetic living arrangement does not translate into higher earnings or other labor-market outcomes later on.

I do not look at other, less tangible outcomes, such as overall satisfaction in life, drug use, and health status. Crime occurrences per household vary substantially between projects. The possibility that individuals assigned to larger housing projects are more likely to be exposed to serious crimes or to commit them cannot be ruled out. At the very least, families assigned to high-crime projects live in less safe conditions than other families in the program. Nonmarket variables may be very important to an individual's overall well-being and should be considered when evaluating desegregation or redevelopment policy options.

Appendix: Estimating Sibling and Neighbor Correlations[25]

The sample of public housing residents varies by age. To adjust for differences in outcomes due to differences in life-cycle, I regress all outcome variables on age and age squared. Let y_{ifp} denote this "residualized" outcome measure for individual i from family f in project p. Therefore, y_{ifp} is measured in deviation-from-mean form. I estimate the variance, $\hat{\sigma}_y^2$, as

$$\text{(A1)} \qquad \hat{\sigma}_y^2 = \sum_{p=1}^{P} \sum_{f=1}^{F_p} \sum_{i=1}^{I_{fp}} y_{ifp}^2 \Bigg/ \sum_{p=1}^{P} \sum_{f=1}^{F_p} I_{cf},$$

where I_{fp} is the number of individuals from family f in project p, F_p is the number of families in project p, and P is the total number of projects in the sample.

We can estimate the sibling covariance more efficiently by taking advantage of the fact that the number of brothers per family and the number of families per project vary. Weighting families with more brothers and projects with more families gives more information. Following Solon, Page and Duncan [2000], I measure the brother covariance, $\hat{\sigma}_{y,y'}^2$, by the following:

(A2)

$$\hat{\sigma}_{y,y'}^2 = \sum_{p=1}^{P} W_p \left\{ \sum_{f=1}^{F_p} W_{fp} \left\{ \sum_{i \neq i'} y_{ifp} y_{i'fp} \Big/ [I_{fp}(I_{fp} - 1)/2] \right\} \Bigg/ \sum_{f=1}^{F_p} W_{fp} \right\} \Bigg/ \sum_{p=1}^{P} W_p,$$

where W_{fp} is the weight assigned to family f in project p, and W_p is the weight assigned to project p.

The variable $W_{fp} = \sqrt{[I_{fp}(I_{fp} - 1)/2]}$ is the square root of

25. See Solon, Page, and Duncan [2000] for additional exposition about estimating neighbor correlations.

the number of distinct brother pairs in family f and $W_p = \Sigma_{f=1}^{F_p} W_{fp}$ is the number of distinct pairs within project p.

I estimate the neighbor covariance by

(A3)

$$\hat{\eta}^2 = \sum_{p=1}^{P} W_p \left\{ \sum_{f \neq f'} W_{ff'p} \left\{ \sum_{i=1}^{I_{fp}} \sum_{i'=1}^{I_{f'p}} y_{ifp} y_{i'f'p} \middle/ (I_{fp} I_{f'p}) \right\} \middle/ \sum_{f \neq f'} W_{ff'p} \right\} \middle/ \sum_{p=1}^{P} W_p,$$

where $W_{ff'p} = \sqrt{I_{fp} I_{f'p}}$. In words, within each project I derive the average covariance between each unrelated neighbor pair. Each project covariance (against the sample population mean) is averaged over projects. Solon, Page, and Duncan [2000] give more weight to neighborhoods where there are more neighbor observations. For public housing samples, smaller projects will have fewer observations to work from. To avoid assigning greater weight to projects with larger samples, I allocate equal weight to all projects by setting $W_p = 1$.[26] Another alternative is to group projects in the same census tract; doing so increases the sample to calculate the neighbor covariance.

Standard errors are estimated by bootstrapping with a succession of 100 randomly chosen samples with replacement.

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26. Assigning larger weight to the projects with larger sample observations reduces the standard errors and strengthens the results and conclusions.

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