Job Market Paper

The Long-run Consequence from Living In a Poor Neighborhood

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Abstract: I examine the effect of neighborhood quality on long-run labor market outcomes among adults who grew up in substantially different public housing projects in Toronto. Subsidized families were assigned to projects throughout the city at the time they applied, with assignment based mainly on the number of bedrooms required. Unlike housing programs used in previous studies, neighborhood quality differences were not attributable to one set of families moving to better neighborhoods and another set remaining at their current residence. I match census data, longitudinal administrative records, and criminal occurrence data to public housing addresses and track participants' outcomes, in some cases, a decade or more after leaving the program. The main finding is that differences in neighborhood quality play little or no role in determining adult earnings, education attainment, or social assistance participation, but do affect residents' exposure to crime. 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. Overall, the results suggest that policies aimed at improving long-run outcomes among children from low-income households are more likely to succeed by addressing family, rather than neighborhood, circumstances. (JEL: I30, J38).

I am very grateful to my advisors, Alan Auerbach, David Card, John Quigley, and Steve Raphael, for their guidance and support. I also wish to thank Miles Corak and Sophie Lefebre for assistance with Statistics Canada's administrative data. Frances Beard and Barbara Watson from Metro Toronto Housing Corporation, Brent Donnelly and Ryner Soegtrop from Cityhome, and Monique Volpe from the Ontario Housing Corporation were instrumental in helping me compile a complete list of subsidized housing projects in Toronto. This research was written while I was employed at the Family and Labor Studies Division of Statistics Canada. Additional funding from the Burch Center for Tax and Public Policy and the Fisher Center for Real Estate and Urban Economics is greatly appreciated. I am solely responsible for the contents of this paper.

I. Introduction

The substantial levels of income segregation that Wilson (1989), Jargowsky (1997) and Myles et al. (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 of many residents would differ if they 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.

A primary advantage of analyzing neighborhood interaction within the context of public housing is that participation in the program limits residential choice. Three previous studies use subsidized housing programs to examine neighborhood effects in the United States. The well-known Gatreaux program assisted black households in high-density public housing projects in Chicago to move to less-segregated communities. Rosenbaum et al. (1999), Rosenbaum (1995), and Popkin et al. (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 et al. (2001), Ludwig et al. (2001)]. Compared to 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. Initial treatment effects on welfare participation and employment are positive, though considerably smaller than those the Gatreaux studies find. In another study, Jacob (2000) examines a less extreme experiment in which families living in Chicago housing projects set to close were offered

[1] Using data from the original paper files of the Gatreaux program, Votruba and Kling (1999) find placement assignments were not entirely random. Pre-program 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.

vouchers to relocate. Comparing children from these projects to children from others, he finds no differences in test scores and dropout rates.

This paper is the first to examine long-run neighborhood effects under the subsidized housing program in Toronto. Studying neighborhood interactions under this program offers unique advantages over United States 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 apply. Assignment is based chiefly on household size and families cannot specify their 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 project also presents a large variety of subsidized housing projects to compare across. Some projects consist only of high-rise apartments; others are only townhouses. Some accommodate more than a thousand low-income families; others provide shelter to less than 50 households. And some projects are located in central downtown, while others are in middle-income areas in the suburbs.

Another unique characteristic of this study is its method of matching specific housing project addresses to census and longitudinal administrative data. This generates large samples and enables me to study both short- <u>and</u> long-run impacts from neighborhood differences, a decade or more after participation in the program. The linked administrative records, in particular, provide an opportunity to examine accurate measures of total income, wages, and social assistance (SA) participation when most youths from public housing are 30 years of age or older.

Despite significant contrast in living conditions across projects, the main finding of the paper is that neighborhood quality does not make much difference to a youth's chances for labor market success. Average education attainment levels, mean earnings, income, and social assistance participation rates vary little between adolescents from different public housing types. In fact, estimates of the probability wage and earning distributions for youths from the best projects and the worst projects are virtually identical.

The only outcome I find related with neighborhood quality is the frequency of crime occurrences on public housing property (on a per household basis). Sexual assaults, assaults causing bodily, drug offenses, and homicides are two to five times more likely to occur at the largest downtown projects than at small projects in middle-income neighborhoods. Families assigned to larger projects are thus more likely to be exposed to crime; a finding consistent with recent MTO studies.

I also compare sibling correlations to unrelated neighbor correlations. This approach, developed by Solon et al. (2000), accounts for unobserved measures of neighborhood quality and provides an omnibus measure of neighborhood effects relative to family effects. 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 discusses theoretical reasons 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 presents the data. Section V describes Toronto's subsidized housing program and the variation in neighborhood quality across projects. The results are displayed in section VI. Section VII gives my conclusions.

II. Why Might Neighborhoods Matter (and Why Not)?

Several social scientists put forward theories as to why residential location may affect individual behavior.[2] Table 1 summarizes four main hypotheses. First, perhaps the most intuitive explanation by which neighborhoods affect outcomes is through peer group or role model effects. There is rich evidence within the psychology literature on the importance of these effects [Brown (1990), Brown, et al. (1986)]. According to this theory, an individual makes decisions based not just on her own preferences but on whether her

[2] 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.

decisions would deviate from choices made by others in her reference group [Akerlof and Kranton (2000)]. 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. 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 et al. (1992), Bernheim (1994)].

Most of us appreciate instinctively that decisions over education attainment, drug use, and careers are often influenced by others, not just family. But social interactions do not take place within isolation of one's neighborhood alone. For them to matter at the neighborhood level, personal contacts must depend on where an individual resides, and neighbor relationships must be important enough to influence individuals' decisions. To clarify these points, consider a simple model of social interaction through role model effects.[3] Suppose there are I young individuals, each with one parent, who must choose whether to pursue higher education or not. The education decision, $D_i \in \{0,1\}$, maximizes the payoff function V_i:

(1) $$V_i = V(D_i, X_i, Z_{-i}, \varepsilon_i)$$

where the payoff function, V_i, is partly determined by the background of the youth's parent characteristics, X_i, and partly by role models -- that is, the characteristics of peers and the families of peers, $Z_{-i} = (X_1, ..., X_{i-1}, X_i, ... X_I)$. The term ε_i represents i's preferences that are independent of others.

[3] A role model theory of social interaction is more helpful than other hypotheses because it facilitates discussion on neighborhood effect estimation. But the role model effect is not the only mechanism by which neighborhoods may influence individuals' choices.

Suppose the education decision also affects another outcome variable Y_i (for example, permanent income) so that $Y_i = f(D_i)$. Now assume Y_i can be expressed in the following reduced form:

$$(2) \qquad Y_i = \gamma \mathrm{X_i} + \sum_{j \neq i} z_{j,i} + \varepsilon_\mathrm{i},$$

where $z_{j,i}$ is a role model fixed effects from individual j's parent on individual i and γ is a vector that captures family effects on Y_i.

The specific neighborhood effect on i, $\sum_{j \neq i, j \in p} z_{j,i}$, is the combined fixed effects of all role models who reside in i's community p. (Note that the effect may differ for youths from the same neighborhood, since role models do not necessarily affect individuals in the community the same way.) The total neighborhood effect, η_p, is defined as:

$$(3) \qquad \eta_\mathrm{p} = \frac{1}{I_p} \sum_{i \in p} \sum_{j \neq i, j \in p} z_{j,i},$$

where I_p is the number of youths in neighborhood p.

Suppose there are two neighborhoods, g and b. We are interested in the expected difference between the effects of the two, $\eta_\mathrm{g} - \eta_\mathrm{b}$. The size of this difference depends on many factors. For $\eta_\mathrm{g} - \eta_\mathrm{b}$ to be large, the $z_{j,i}$'s must be large and vary significantly between both neighborhoods. If a few youths are strongly affected by where they live while the majority are not, then the expected difference may still be small. The definition of neighborhoods is also important. Neighborhood effects at the school-district level may miss the effects of role models formed, say, at weekend hockey practice. Finally, the size of $\eta_\mathrm{g} - \eta_\mathrm{b}$ also depends on how much variation in expected role model characteristics exists between communities. In the context of this paper, variation by neighborhoods comes from youths living in public housing projects of different sizes and from the vicinity's household characteristics.

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.

A. Differences in Means

Let us suppose there are two types of projects, g and b. Like last section's model, let Y_{ip} be an outcome variable -- say permanent earnings -- for individual i in project p as determined by the following equation:

$$(4) \qquad Y_{ip} = \gamma X_{ip} + \eta_{\mathrm{p}} + \varepsilon_{ip},$$

where X_{ip} is a vector of all family characteristics that influence earnings, η_{p} is the neighborhood effect from project p, and ε_{ip} represents unrelated individual factors independent of both family and neighborhood characteristics. The mean outcome difference between project g and project b is

(5) $$\overline{Y}_g - \overline{Y}_b = \alpha(\overline{X}_g - \overline{X}_b) + \eta_g - \eta_b,$$

where $\overline{Y}_p$ is the mean of the outcome variable for project p. We are interested in the mean outcome difference attributable to variation between project characteristics, $\eta_g - \eta_b$. If assignment is random, $\overline{X}_g = \overline{X}_b$, then the impact from living in project g versus project b can be estimated directly from the mean outcome difference. Without random assignment, this comparison is biased toward a larger effect on the project in which families that tend to have greater positive influence sort into.[4]

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 an 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 (4) we get

(6) $$Y_{sfp} = \gamma X_{sfp} + \eta_p + \varepsilon_{sfp}.$$

[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. The reflection problem is avoided here by examining only outcomes of public housing participants whose surrounding neighborhoods consist of both participants and non-participants. I do not examine the impact on non-participants living nearby these projects. See Brock and Durlauf (2000) for a lengthier discussion on addressing the reflection problem.

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

(7) $$Var(Y_{sfp}) = Var(\gamma X_{sfp}) + Var(\eta_p) + 2Cov(\gamma X_{sfp}, \eta_p) + Var(\varepsilon_{sfp}).$$

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

(8) $$Cov(Y_{sfp}, Y_{s'fp}) = Var(\gamma X_{fp}) + Var(\eta_p) + 2Cov(\gamma X_{fp}, \eta_p).$$

where γX_{fp} is a family fixed effect common to both sibling s and s'.

Equation (8) 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. Note that the covariance term between family and project characteristics, $Cov(\gamma X_{fp}, \eta_p)$, is zero if families are randomly assigned.

The covariance between unrelated neighbors from family f and family f' in the same project is

(9) $$Cov(Y_{sfp}, Y_{s'f'p}) = Cov(\gamma X_{fp}, \gamma X_{f'p}) + Var(\eta_p) + 2Cov(\gamma X_{fp}, \eta_p).$$

The third term in right-hand side of the equation (9) is likely 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 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 upper bound on the possible influence of both observed and unobserved neighborhood characteristics. That bound can be tightened by subtracting measurable parts of the first term that reflect neighbors' similar family backgrounds. 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 (9) to obtain a more precise upper limit on project effects.

If the terms $Cov(\gamma X_{fp}, \gamma X_{f'p})$ and $2Cov(\gamma X_{fp}, \eta_p)$ are close to zero, we can estimate the proportion of the sibling covariance due to neighborhood characteristics by dividing equation (9) by equation (8). This measure indicates the relative importance neighborhoods play compared to family factors. The procedure for estimating the sibling and neighbor correlations and calculating the bootstrapped standard errors is straightforward and discussed in Appendix A.

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

A. Background

Public housing buildings vary a great deal throughout Toronto in terms of size, location, and neighborhood surroundings. 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 may actually have 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.[5] Other projects

[5] According to Metro Toronto Housing Security, about one-third of all homicides in Toronto occurred on public housing property, most in the largest and oldest projects (http://51cplc.atuc.net/Membership/mthcs.htm).

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). MTHC maintains 113 family projects.[6] In total, they accommodate 29,173 households (one in twenty family households in metropolitan Toronto). Every MTHC household pays rent geared to income. That is, approximately 25 to 30 percent of a household's total income is charged as rent.[7]

Legislation to the National Housing Act changed in 1974, allowing for more development of public housing at the municipal level. The new housing developments were designed to mix more with the surrounding community and to accommodate far fewer households with subsidies than previous developments. The amendments came directly from concerns about the high concentrations of low-income households in some earlier projects.[8] Cityhome, under the municipal government, was responsible for most of the new construction prior to the mid-1980s, and it administers 97 developments containing 8,966 household units.[9] 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.

Non-profit organizations, including cooperatives, also provide subsidized housing to low-income families in Toronto. The vast majority of non-profit projects were built after 1990. And since my main dataset uses a sample of teenagers living in subsidized housing before this time, I excluded these groups from my analysis. Another reason for omitting them is that non-profits use separate waiting lists and often have different acceptance criteria than MTHC and Cityhome. Unobserved sorting into these projects could further bias the empirical results.

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[6] 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 aboriginals or special needs families.

[7] The percentage paid in rent changed from 25 percent to 30 percent in the 1980s. Social assistance recipients pay a fixed amount set annually by the federal government.

[8] Similar reasons underlay the 1980s shift in the United States policy from providing public housing to providing vouchers for mobility programs.

[9] Cityhome also administers about 225 single homes scattered around the city, but the nature of my data makes it difficult to identify these homes. I exclude them from my study.

B. Variation in neighborhood quality

Figure 1 shows the locations for 160 MTHC and Cityhome family projects built before 1986. [10] The map divides Metropolitan Toronto, with a population of 2.4 million in 1996, into census tracts categorized by the percentage of households within a tract with family incomes below Statistics Canada's Low-Income Cut-Off (LICO). The projects cover a large range of neighborhoods downtown and in the suburbs.[11] Most of seven largest downtown developments, which together accommodate about 25 percent of all subsidized families, are within a short walking distance from each other. In addition to these large developments, however, there are also a considerable number of smaller low-rise and townhouse complexes.

Columns 1 and 2 of Table 2 present the mean 1996 census tract characteristics for two groups of projects: the largest seven in the central city, and forty-two projects with fewer than 250 units located in census tracts with fewer than 25 percent of households below the LICO. The comparison between the two groups arguably provides the most contrast between residential quality in the program without reducing the sample to an unworkable level. The smaller projects are in middle-income census tracts, where only 15 percent of households fell below the LICO in 1996. In contrast, 49 percent of households around the largest projects are below the LICO. Households around the larger projects were more likely to be female headed, on SA, and less educated than households from the smaller projects. Almost all households around the largest projects were renters, while 41 percent of those around the smaller projects owned their own home.

The variation in neighborhoods within the public housing program was certainly narrower than variation across the entire city. No 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 2 was 15.6 percent. For the city as a whole, the median household lived in a census tract with 12.7 percent of

[10] I only show the 27,931 units in 105 MTHC projects, and 5,232 units in 55 Cityhome projects built before 1986 since my main dataset is for children who entered social housing before this period. The 50 projects built after 1986 were mainly Cityhome projects, and they are included for the portion of my analysis that uses the 1996 Census.

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 into 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 2 lists the mean census tract characteristics among participants of the Moving to Opportunity Program in Boston and Chicago.[12] Column 3 displays mean characteristics for participants from large housing projects in Boston who were not given assistance to move. Column 4 shows mean differences (against column 3) for the census tracts moved into by participants receiving Section 8 vouchers to relocate.[13] Column 5 displays mean differences of tract characteristics for 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, 63.6 percent fewer households in Toronto census tracts around the smaller projects received social assistance than households in tracts around the largest downtown projects. In Boston, SA 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.

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[11] From 1967 through 1997, Metropolitan Toronto comprised Toronto itself plus five boroughs (most of them separate municipalities). In this paper I refer to these boroughs as Toronto's suburbs.

[12] The data for Boston is from Katz and Kling (2000), Table 4. Data for Chicago is from Rosenbaum, et al. (1999), Table 1.

[13] In Katz et al. (2000), mean tract characteristics were computed for participants, whether they moved or not. Given the portion of movers and assuming 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.

In Chicago, SA participation was 46.8 percent less in the section 8 group than in the control group and 82.6 percent less in the experimental group.

Household heads in the largest projects in Boston and Chicago were poorer, less educated, and more likely single mothers than household heads in Toronto's large projects. Perhaps the most striking difference between Toronto and the two U.S. cities was the percentage of blacks in the neighborhood. In Toronto, households in census tracts containing the large downtown projects are 19.3 percent black. In contrast, the portion of blacks within census tracts holding large projects in Boston and Chicago was 44.9 and 99.3 percent respectively.

Overall, Table 2 shows that the neighborhood quality variation within the Toronto housing program was considerable but the characteristics of households in and around the largest projects were not quite as extreme as those for large U.S. cities such as Boston and Chicago.

C. The application process

Until 1995, applicants on the MTHC waiting list were selected on the basis of a points system. Households were given points primarily based on financial need but also on current living conditions, SA 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. Applicants could specify up to seven regions in the city but rarely did so. The fewer the regions a family was willing to live in, the longer it waited for an offer. Applicants who rejected their first two offers were removed from the waiting list.[14] Transfers between projects occurred only for reasons such as change in family size, health, or proximity to work.

Conversations with MTHC administrators revealed that applicants who tended to pass up their first offer were in less urgent need of housing and often did not want to live in larger projects that had negative stigma associated with them. If these more selective

[14] Administrators made occasional exceptions to this rule. The staff members handling an applicant's case could allow a second refusal if, in their discretion, they felt it justified.

parents were also more likely to foster their children's development, estimated differences in mean outcomes between projects with less or more low-income concentration are likely biased upwards. Since only two offers were given, both at random after conditioning for family size, the bias seems likely to be small. Nevertheless, I cannot rule out the possibility that positive differences between neighborhoods are due to non-random sorting. As it turns out, however, in spite of this upward bias, I find no significant impact from residential environment.

Cityhome's waiting list was chronological. The initial applicants to its subsidized units came from MTHC's waiting list. New applicants applied directly, although they were also encouraged to apply to MTHC. As with MTHC, applicants to Cityhome could not specify a project they wished to live in but could request a particular region of the city. After 1995, a central agency was established to process all applications for subsidized housing in Toronto.

Not much is known about the characteristics of tenants leaving public housing in Toronto. Ekos Research Associates Inc (1991), however, conducted a representative provincial survey of families, single households, and seniors who left in the mid-1990s. The annual turnover rate of units for Ontario was about 13.5 percent, a figure similar to the turnover rate in Toronto's private market. Of the sample of leavers, 69.0 percent had lived in public housing for fewer than five years, while only 28.7 of my 1996 census sample of household heads in public housing moved in the last five years. This difference suggests that the hazard rate for leaving public housing falls substantially the longer a family remains in the program.[15]

The main reasons the Ekos respondents gave for leaving public housing were relocation for employment, improved financial situation, and change in marital status. Notably for my study, 29.5 percent of the Ekos sample of leavers said they had reasons for leaving other than those listed. Fewer than half of these respondents actually specified their reasons, but "trouble with neighbors" was among those mentioned most often. Therefore, although the option to move projects because of poor neighborhood environment was restricted, some families may have been willing to leave the program entirely to get away

[15] A caution with interpreting this result is that my census sample included only households with children ages 16 to 25 still living at home, while the Ekos survey used representative of all public housing occupants.

from their neighbors. Given the small number of long-term leavers and the small number of respondents who gave this reason, the slippage from my sample seems likely to be small.

V. Data

I use four datasets. The first is a record of public housing addresses and project characteristics. The second is the 20 percent sample of the 1996 Canadian census. The third is the Intergenerational Income Dataset (IID), a large longitudinal file compiled from income tax records by Statistics Canada. I matched the postal code data with the census and the IID to identify households and individuals in public housing. The fourth dataset is crime occurrence data by project, compiled by MTHC security staff. I discuss each below.

A. Postal Code Addresses

Instead of relying on small survey samples that identify whether a family or household has participated in a public housing program, I took a different approach; matching public housing postal code addresses to micro data.[16] Postal codes in Canada are composed of six alpha-numeric 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 my population sample were located in public housing addresses with unique postal codes. Some smaller public housing dwellings, however, share a code with nearby households not in public housing.

Another difficulty is picking out households in Cityhome projects that received subsidies: some families living in Cityhome projects pay private market rent. Families not participating in subsidized housing programs are more likely to sort across different public housing project neighborhoods, with those unable to relocate to more pleasant environments locating in the worst city neighborhoods and those with (perhaps unobservable) higher

[16] MTHC, Cityhome, and the Ontario Housing Corporation generously provided addresses and other information for each project in Toronto. As mentioned in section II, only MTHC and Cityhome projects, which make up most of Toronto's subsidized family housing stock, are kept for the analysis.

incomes locating in the better neighborhoods. Including children from these families does not invalidate the analysis, but does raise the upper bound of the project effect estimates.

To minimize the number of children selected from families outside public housing, I constructed three samples. Sample 1 included only the population from postal codes unique to MTHC developments. Every household in this sample received rent-geared-to-income. Of the 544 postal code addresses, 317 were uniquely identified so this sample contains most of the family public housing stock. Omitting the other 237 codes removed more dwellings from large than from small projects, but most of the neighborhood quality variance remained. In Sample 2, I included only households with single mothers receiving SA.[17] As described below, more than half of all families in subsidized housing fell into this category. Sample 3 came from estimating a probit model on the probability of living in subsidized housing based on several observable characteristics.[18] I used the sample of households living in census tracts that contained public housing but not living at addresses with unique public housing postal codes, together with the sample of MTHC public housing households uniquely identified in Sample 1. The results are used to estimate the probability of living in public housing among the sample of households with public housing postal codes. Sample 3 includes all households whose estimated probability for living in public housing is above a particular cutoff (see Appendix B).

B. The Cross-Matched Census Data

I matched postal codes to households in the 1996 census. The cross-sectional nature of the census limited the analysis to possible neighborhood interactions on outcome variables for children while still living at home. I therefore restricted the public housing samples to all youths ages 16 to 25 living with at least one parent. Table 3 displays mean

[17] Sample 2 family heads in the administrative data are single mothers who received SA in any year between 1992 and 1998. In the census, Sample 2 family heads include single mothers receiving more than $3,000 in "Other government transfers", which included SA in 1996.

[18] For the administrative data, the independent variables used were household head's age, child's age, family size indicators, marital status when the child was a teenager and when the child was 25, permanent family income, whether receiving SA, and years living in public housing postal code between 1978 and 1990. The probit model with the census data used household head's age, child's age, race indicators, household head's education attainment, total family income, whether on SA, marital status, family size indicators, immigrant status, and whether moved in the last five years. See Appendix B for more details.

characteristics of families and children in these samples in comparison to mean characteristics for the city population. Not surprisingly, the monthly rent reported by households in public housing was much smaller than the average monthly rent among all city renters. Average household income for families in public housing was about 40 percent of that for the city. Fewer than 35 percent of household heads in public housing worked full time, and a high percentage are comprised of single mothers (62 percent in Sample 1). Only seven percent of Toronto's family household population are black, and about 43 percent of families in public housing are black.

C. The Intergenerational Income Database

I also matched postal codes from projects built before 1985 to the Intergenerational Income Database. The IID includes the full sample of Canadian 16- to 19-year-olds who filed tax returns in 1982, 1984, and 1986 while still living at home, a population numbering over 700,000 children.[19] By 1998, these taxfilers were 28 to 35 years old. Mothers and fathers are linked to these youths in the year the child first filed.[20] The IID tracks both parents and children longitudinally from 1978 to 1998. Data exist for each year an individual filed.

Each tax file contains a return address with postal code. The postal code for matching to projects was taken from the child's tax file. If the 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 19. Only children who lived in a project for at least two years were kept in the sample. If neighborhood influences are cumulative, then two years in a project may not be enough to be affected by neighborhood environment, so I also checked whether longer-term stays had a greater influence on outcomes (see section V).

[19] Some children are identified in more than one year. For these cases, only one match was used.
[20] The parents are not always the biological parents and may include step-parents or other caregivers. See Corak and Heisz (1999) for more details.

I averaged each youth's adult earnings and income over a six-year period between 1993 and 1998. SA participation between this period is also recorded. As for information on family background, the IID contains detailed employment and transfer income data, as well as marital status and number of children. However, information about race, ethnic background, and education attainment is not available. 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 over 15 years, between 1978-92, or until the oldest parent reached 65. All dollar amounts were converted to 1992 Canadian dollars using Statistics Canada's Consumer Price Index.

The IID captures young people who file an income tax return while living at home. Therefore, the IID under-represents youths who had no attachment to the labor market during their teenage years, who left home before establishing such an attachment, or who participated in the underground economy without reporting income activity. Unfortunately, all three cases seem quite likely to have arisen in the population of families living in public housing. Hence, if worse outcomes are associated with non-taxfilers and if the likelihood of filing is a function of the public housing project assigned, the analysis may miss important project or neighborhood effects.

One approach to check for this possibility is to examine whether differences exist by neighborhood quality and the average number of years an individual did not file. The relationship between neighborhood quality and the chances of never filing may be similar to the relationship between neighborhood quality and the chances of filing only once or twice. Thus, I used the total number of times not filing for individuals in the sample to examine whether the latter association exists.

Another approach to dealing with youths missing from the IID is to reweigh the sample based on the inverse probability of filing, conditional on observable characteristics. All results from the IID are adjusted for undercoverage along parental income, gender, and regional dimensions (see Appendix B for more details). This approach, however, ameliorates the non-reporting problem only to the extent that outcomes for youths observed in the IID are similar to those who have been excluded along these demographic categories.

The possibility that neighborhood quality affects the chances of filing when young cannot be completely ruled out, and this is one reason I also report my results from the census data. Although restricted to outcomes for youths still living at home, the census is not subject to the same kinds of non-inclusion biases that the IID potentially faces and provides a useful cross-check on whether results from two substantially different datasets lead to similar findings. If the results indicate no neighborhood influences on income for the IID sample, but significant effects on education attainment for the census sample, we cannot exclude the possibility that the missing sample of non-taxfilers prevents us from identifying long-run effects in the IID. If the results indicate no neighborhood influences on outcomes in both the IID and in the census, we can make stronger interpretations from both datasets.

Table 4 covers mean characteristics in the IID samples, which are larger than the census sample because the longitudinal nature of the IID allows identification of youths who once lived in public housing but may have subsequently moved out. The pattern is similar to that produced with census data. More than 60 percent of the household heads in the public housing samples were women, and more than 50 percent of household heads received SA sometime between 1993 and 1998. Only 18 percent of the total city sample was female headed, and 13 percent received SA.

D. Crime Occurrence Data

I was also able to obtain crime occurrence 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. The data were broken up by project. Total occurrences by project were divided by project household size. Importantly, the data included occurrences involving both residents and non-residents on MTHC property.

VI. Results

A. Observable Sorting between Projects

Table 5 compares households from the two neighborhood-quality groups that I argued in Section IV display the largest contrast within the program: those from the seven largest central-city projects and those from projects with fewer than 250 units in census tracts with fewer than 25 percent of households below the LICO.[21] In most cases, I chose characteristics, such as household head's education and race, that are not likely themselves to be affected by current neighborhood background. If sorting between groups is minimal, we can expect to see little difference in means between the two neighborhood-quality types. I subdivided Table 5 into all households (columns 1 and 2) and households with children (columns 3 and 4).

Single-parent households, immigrants, age of head, and number of children are distributed in similar proportions among large and small projects. Some small differences exist in the percentage of heads who were black or receiving SA. Median incomes in both the census and the IID samples were about $2,000 more for the smaller projects than for the large central-city projects. There were also small differences in mean education attainment. In the large downtown projects, 43 percent of public housing family heads were without a high school degree, compared to 47 percent in the smaller projects. The mean differences for household heads were not particularly large, which makes sense considering the restrictions placed on project choice at the time of application. Poorer and less-educated family heads were slightly more likely to live in the larger projects, which corroborates the idea that more needy families were more likely to accept their first offer for housing.

B. Differences in Means

Table 6 compares outcome means for youths from the largest and smallest projects. The schooling outcomes from the census data show almost no difference between the two groups: 15.5 percent of youths from the largest projects and 15.6 percent of the smallest project sample attained more than a high school degree.

To control for observable family background, I show in column 3 the dummy coefficient for the smaller projects after regressing the outcome variable on a complete set of indicator variables for age and sex, a variable for parental permanent income, parent SA receipt, marital status, race, and immigrant status. For binary outcomes, I used a probit model and the coefficient shown can be interpreted as the estimated change in probability if an individual had lived in another project type.

The difference between projects, after controlling for demographic and family-background factors, remains virtually unchanged. This result is reassuring, since unbiased estimates of the neighborhood effect under random assignment should not change with additional controls. Insignificant differences between large and small projects also result when looking at years of education, less than high school degree, and whether not working and not going to school ("idle").

The second panel of Table 6 focuses on longer-term outcomes based on the IID. The raw mean for whether a youth from a large downtown project received SA for at least one year during the 1993-98 period is 31.9 percent. For the smaller projects, the mean is 29.1 percent. The difference is not significant (p-value>.1). Fewer youths from smaller projects received SA when older, and adding family background controls further reduces this difference to –1.5 percentage points. The small differences between project types for SA participation also translate to small differences in total income. Boys from the smaller projects received, on average, 2.4 percent more in annual income between 1993 and 1998 than did boys from the large downtown projects. Mean annual earnings for men from public housing projects do not differ between project types, whether family-background controls are included or not.

As discussed in section IV, a concern arises that neighborhood conditions may influence the likelihood of not filing a tax form and not being captured in the IID. The next-to-last row from Table 6 displays mean differences in the number of years an individual filed taxes. The mean number of times not filing for adults who filed at least once when age 16 to 19 from the largest projects is 2.3, and the mean number for adults from the largest

[21] To keep the number of observations as large as possible, I combined all three matched samples.

projects is 2.4. These results and the presence of similar findings with census and IID data suggest we should not expect to see conclusions change if we were able to include the missing persons from the administrative data.

Table 7 presents 1992 project crime and victimization occurrences, categorized by the same large and small project types used above. These data are available at the project, rather than the micro, level. The seven largest projects in downtown had the greatest incidences of arson, bodily and sexual assault, drug offenses, neighbor disputes, and homicides per 1,000 households. Assaults were more than twice as likely to occur in the larger downtown projects. Homicides were more than four times as likely. These differences are similar when mean project characteristics, such as the percentage of households receiving SA, are included as controls. The general pattern the table reveals is that criminal activity occurs more frequently in and around projects with greater concentrations of poverty, though these results do not necessarily imply the conditions of the largest projects led to more crime.

Tables 8 and 9 present similar analyses of differences in means using alternative categorizations of neighborhood quality. I redefine project quality by the total size of the project, the percentage of households in the census tract around the project below the LICO, whether the project is administered by MTHC or Cityhome, and whether the project is comprised of all highrises (more than five stories) or all townhouses.

The first part of Table 8 contrasts all large, medium-size, and small projects in the program. From column 1, the mean completed years of schooling for youths ages 16 to 25 in projects with fewer than 150 household units is 12.4. The mean for those in projects with 150 to 700 units is 12.2, and in projects with 700 units or more, 12.4. The null hypothesis that the means are the same cannot be rejected (F-test = 0.42). The other schooling-outcome variables – the percentages of youths with less than and more than a high school degree – also do not differ significantly across small and large projects. The percentages of youth not working and not going to school in small, medium, and large projects is 16.1, 16.0, and 16.1 respectively. When family background controls are added, these outcome differences remain very small (columns 6 through 9).

The next set of rows categorizes public housing projects by whether they are managed by MTHC or Cityhome. MTHC projects are older, usually larger, and have residents who all receive subsidized rent. Cityhome buildings are smaller and mix subsidized tenants with those paying market rents. Even without controlling for observable characteristics, the estimated mean outcomes are not significantly different.[22]

Table 8 also classifies projects by conditions within the surrounding census tract. The mean total years of schooling for youths in public housing located within a census tract with fewer than 15 percent of households below the LICO is 12.1 years. The comparable mean for those within census tracts with more than 40 percent below the LICO is not significantly different:12.3 years.

We might expect differences to arise from whether youths lived in highrises of five or more stories 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 8, however, indicates no substantial differences in schooling and job outcomes between these dwelling types, whether family background controls are included or not.

The same analysis of differences-in-means is applied to IID outcome variables in Table 9. Column 1 shows that 32 percent of youths from both small and large projects received SA for at least two years between 1993 and 1998. Income and earnings differences for men who grew up in different public housing projects are also tiny. The average men's total income is about 1.6 percent more than the amount for those from the largest projects. Average total earnings between these groups are almost identical.

Men in the IID from census tracts with fewer than 15 percent of households with incomes below the LICO earned, on average, about $18,800 between 1993 and 1998; men in census tracts with more than 40 percent of households below the LICO earned about 2 percent less. 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 not statistically significant. Long-term outcomes between individuals in MTHC or Cityhome projects and between highrise and townhouse developments do not appear to differ at all.

[22] It is worth pointing out that all estimates are measured fairly 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.

While labor market outcomes vary little between different categorizations of neighborhood quality, Table 10 shows that crime occurrences per household are not the same across projects. Incidents of sexual assault, physical assault, and homicide per project unit are more frequent particularly in large than in small projects. Criminal occurrences per household are also more prevalent in projects within poorer census tracts. Also notable is the higher occurrences of sexual assault and homicide in highrise than in townhouse projects.

C. Wage and Schooling Distributions for Youth from Different Projects

The large samples from the census and the IID facilitate a more comprehensive analysis of neighborhood and project quality than just comparing means. I estimated the distributions for years of schooling, log income, and log earnings and then graphed them for the two large and small project types discussed above.

Figure 2, panel A, shows the kernel density estimates of total years of schooling for individuals in the census from projects with fewer than 250 units within census tracts that had fewer than 25 percent of households below the LICO. The kernel density estimate for individuals from the seven largest central-city projects is overlaid on top of the density estimate for the smaller projects.

The densities were estimated for Figure 2A by using the residuals from a regression with years of schooling on age dummies and gender. The mean of the residuals, with both samples included, is zero. The two sets of density estimates are remarkably similar. The densities for Figure 2B were estimated after regressing on age, gender, and family background controls. Notice that controlling for observable background variables changes the density estimates very little; the distribution of years of schooling for individuals from the smaller projects becomes slightly more skewed to the left. It appears, after controlling for observable characteristics, that persons from the largest housing projects had slightly more years of education, on average, than those from the smaller projects, a result that corresponds with the results in Table 6.

Figure 3 displays kernel densities of the same project quality types, graphed from the residuals of log total income (for males only). Both distributions spike near the left tail, corresponding to those in the sample with only SA income. There are slightly more individuals near the bottom of the distribution from the largest projects, but differences in the densities fade when family controls are added. The two densities are also similar.

Figure 4 shows the kernel densities for log total earnings. These distributions are skewed to the right because individuals receiving SA earn little additional income. Whether family controls are added or not, the densities between the largest and the smallest projects are almost identical.

D. Differences in means by age at entry and years lived in public housing

The public housing samples presented above include individuals who lived in public housing for at least two years. This subsection examines whether conditioning on age at entry or on years lived in public housing alters the main results. Table 11 presents regressions of log total income (for males only) on age, gender, family background controls, and project quality in columns 1 through 5 and similar regressions for years on SA in columns 6 through 10. To keep the sample large, I dichotomized project quality between projects within census tracts with 35 percent of households below the LICO, and those within census tracts with 35 percent above it. The coefficient on the quality dummy variable in column 1 indicates no effect on log adult income from living in one type of project or another.

Column 2 adds indicator variables for entering public housing at ages 10 to 13 or 14 to 16. (The omitted indicator variable is enterance before age 10 to a project in a census tract with fewer than 35 percent of households below the LICO.) The coefficient for those who entered public housing between ages 10 through 13 is 0.05, indicating slightly better income performance than those who entered earlier. The estimate on log income for those who entered at ages 14 through 16 is 0.01.

Column 3 reflects the interaction of project quality and age of entrance. For children who entered public housing before age 10, the coefficient estimate on the effect from living in a poorer-quality project is -0.01. For those entering after age 13, the coefficient on poorer neighborhood quality is positive but measured imprecisely.

The measure of neighborhood quality interacting with years lived in public housing also appears to make little difference for the subgroup who lived in public housing the longest. As column 5 reports, males who lived in poor-quality public housing for at least 11 years earned an estimated 2 percent less than those who lived in better-quality projects for the same amount of time. Men who lived in poor-quality projects for 5 to10 years earned an estimated 1 percent more than those from better-quality projects. And I find no project effect for men who spent less than five years in the program.

The results when estimating years on SA are similar. The larger sample includes both men and women and improves the precision of the effect estimates. For youths who entered public housing before age 10 and were assigned a project in a more affluent census tract, the predicted effect on the number of years receiving SA is zero. Individuals who lived in public housing for more than ten years are predicted to have spent, on average, 0.01 more years receiving SA (over the six-year period) if they were assigned a poorer-quality project.

E. Sibling and Neighbor Covariances

The results presented above separate project differences specifically into two or three observable categories. Each MTHC and Cityhome project, however, is unique and may have many specific characteristics not adequately captured when the sample is broken down. Recall from section III B that we can also express the importance of neighborhood differences by measuring correlations between unrelated neighbors. If assignment into neighborhoods is random, the neighbor correlation represents the portion of the outcome variance attributable to observable and unobservable neighborhood differences. If some degree of sorting by project occurs, the correlation likely represents an upper bound of this amount. Neighbor correlations are presented below and contrasted with sibling correlations, which approximate the portion of the outcome variance attributable to family factors.

i. Income

Table 12 presents the estimates of adult annual income correlations between brothers and between neighbors. I control for age by calculating the correlations of the residuals after regressing log income on boys' age and age squared in 1998.[23] 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.

Beginning with the full city sample, the "residualized" city variance of log income was 0.335. The corresponding brother covariance was 0.101. Dividing the brother covariance by the city variance gives an estimate for the city-wide income correlation of 0.300.[24] Page and Solon (1999) estimate a similar value, 0.316, for the earnings correlation between brothers in the United States.[25] Interestingly, when I control for observable family characteristics, the brother correlation fell only a little, to 0.241. This means my family-background controls do a poor job at explaining the similarities between brothers' earnings.

When I turn to the earnings correlation estimate for boys from the same enumeration area (EA), the correlation in age-only adjusted income is 0.015. Enumeration areas are much smaller geographically than census tracts. They include about 100 to 400 households, whereas census tracts include about 4,000 households. Despite the expected similarity between neighbors in the same EA, the correlation is many times smaller than the brother correlation. When I used the family-background-adjusted residuals, the correlation fell to 0.005. So it seems much of the EA neighbor income correlation can be explained by a small set of observable family background characteristics, mainly parental income. These results hold when I looked at the subset of families who never moved between 1978 and the time the son was 19 years of age. I measured the age-only adjusted neighbor correlation for non-movers at 0.016 and the family-background-adjusted correlation at 0.002.

[23] For exposition, I sometimes refer to the log income covariance as just the income covariance.
[24] The variance is based on all families with boys in the sample, whereas the brother covariances are based on families with at least two brothers in the sample. Measuring the variance among families with at least two brothers does not change the estimate much. This is true also with the public housing samples.
[25] Caution must be taken with comparing city-wide to nation-wide correlations. Page and Solon (1999) find their brother earnings correlation drops to 0.186 after controlling for urban city and region. No previous studies have estimated sibling earnings correlations in Canada.

In all three public housing samples (Table 12, columns 2 through 4), the income variance is larger than the city-wide variance. The finding seems surprising at first because the boys come from more similar backgrounds than those in the city sample. We might expect low-income outcomes for sons from low-income families, an average outcome reflected in Table 3. Nevertheless, many sons from low-income families escape low income themselves. Corak and Heisz (1999) show the relationship between fathers with low income and their sons' income is weak (at least for the Canadian population), leading to a wider variation in later labor market outcomes. The brother correlation estimates range between 0.261 to 0.287. Family and community background therefore explains a substantial portion of the income variance among sons from public housing households. As with the city sample, much of this correlation remains even when I control for parental income and parents' marital status.

I estimate a small and sometimes negative income covariance between boys from the same public housing projects. For the age-only adjusted neighbor covariance across projects, all three estimates are much smaller than the city covariance estimate for neighbors within EAs. After controlling for observable family background characteristics, I find the measured covariances do not change in comparison to those in the city sample. The level of sorting across projects is much lower than that of sorting across census tracts in the private housing market Therefore, controlling for family background should matter less to the estimates in the public housing samples. The smaller number of observations makes identifying differences between samples more difficult. In general, all three give consistent covariance estimates. All are centered around zero. Using the bootstrapped standard errors, I cannot reject the hypothesis that all estimated neighbor covariances are zero.

Some of the smaller projects are clustered near the larger projects. It may be more appropriate to treat households in projects near each other as neighbors. The next-to-last section of Table 12 defines neighbors as living in the same census tracts, instead of the same project. The point estimates for the neighbor covariances, after controlling for observable family background, are approximately zero for all three samples.

We can approximate roughly the influence from a one standard deviation increase in the latent variable that includes all observable and unobservable relevant characteristics. Suppose the neighborhood quality covariance accounts for about 0.1 percent of the total log income variance (the average estimate from the neighbor income correlations in Sample 1). Then a one-standard-deviation increase in neighborhood quality may be expected to increase the standard deviation in log income by $\sqrt{0.001} \cong 3.2$ percent. The standard deviation in log income for this sample is about 0.613, so a one-standard-deviation increase in neighborhood quality would raise log income by about 1.9 percent. This result is sensitive to both the outcome variance and neighbor covariance estimate, so some caution is necessary. For example, a neighbor covariance of -0.1 percent, instead of 0.1 percent, easily within the standard error bounds, would have led to a –1.9 percent estimated effect.

ii. Earnings

The point estimates for the neighbor earnings covariance estimates are also close to zero (Table 13). For example, the age-only-adjusted earnings covariance estimates between boys from same projects are 0.002, 0.017, and -0.002 for Samples 1 through 3 respectively.

iii. Years on Social Assistance

Many siblings in my public housing samples receive SA when they are older. Table 14 shows the estimated covariances between brothers and boys in same projects between 1992 and 1998.[26] I used residuals from regressing on age and age squared to measure the covariance. The city variance estimate is 1.51 years. The corresponding brother covariance is 0.30. Family and community factors, therefore, explain about 20 percent of the total variance in years on SA.

The brother correlations in years on SA among the public housing samples are similar; 0.228 for Sample 1, 0.179 for Sample 2, and 0.227 for sample 3. Most of the point estimates for the correlation in years on SA between project and census tract neighbors, however, are about zero, or insignificantly negative.

iv. Years of Education

I calculated sibling covariances of years of schooling only between pairs of young siblings both of whom lived at home and neither of whom had necessarily finished his education (Table 15). The sibling years of schooling correlation for the city is estimated at 0.380. For the public housing samples, the correlation ranges between 0.167 and 0.198.

The schooling correlation between children in the same EA is measured at 0.048, and .033 once observable family background controls are included. The adjusted neighbor correlation is less than one-tenth the sibling correlation. Within public housing, the neighbor correlation estimates are smaller and negative in several cases.

VII. Discussion

In this paper, I use variation in characteristics across public housing projects in Toronto to examine the relative importance of neighborhoods in influencing labor market outcomes among adults from low-income family backgrounds. The advantage of using a sample of public housing participants is that the nature of the application process prevents much selection across neighborhood types. Consequently, upper-bound 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's contribution over previous subsidized housing experiments is that it looks at impacts on long-run labor market outcomes a decade or more after program participation. The study also explores variation between several definitions of neighborhood quality without relying on moves by a treatment group.

The key finding from the analysis is that average education attainment, annual earnings, income, and SA participation among youth from low-income families do not differ by the degree of low-income concentration in the neighborhood that the youth grew up in. I find youths in low-income families gain no advantages from living in middle-income

[26] The covariance framework does not work well with binary outcome variables, such as an indicator for SA participation. Future work is needed to adapt this approach to handle these variables.

neighborhoods in the suburbs and no disadvantages from living in the poorest neighborhoods in downtown Toronto. These results hold whether contrasting housing projects by low-income neighborhood concentrations, whether in townhouses or high-rise apartments, or by length of residency or age of entry.

A second finding is that family differences seem to 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.

I view these results as largely 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 et al. (2001)]. We will have to wait many years before we can compare long-run effects from the MTO experiment with the results in my study. In the meantime, the 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. These non-market variables may be very important to an individual's overall welfare.

As a final caveat, the neighborhood-quality variation in the Toronto public housing sample may not be great enough to permit detection of significant neighborhood effects. The better housing projects in Toronto are in areas where the majority of households are middle-income. The worst housing projects are in areas with the highest concentrations of poor in the city. Even these projects, however, do not exhibit the level of decay and segregation prevalent in some of the larger projects in U.S. cities. Families living in public housing in Toronto come from much more ethnically diverse backgrounds than families in projects in the United States.

Appendix A: Estimating Sibling and Neighbor Correlations[27]

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 dummies. 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:

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

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 et al. (2000), I measure the brother covariance, $\hat{\sigma}^2_{y,y'}$, by the following:

$$\text{(A2)} \quad \hat{\sigma}^2_{y,y'} = \sum_{p=1}^{P} W_p \left\{ \sum_{f=1}^{F_p} W_{fp} \left\{ \sum_{i \neq i'} y_{ifp} y_{i'fp} / [I_{fp}(I_{fp}-1)/2] \right\} \Big/ \sum_{f=1}^{F_p} W_{fp} \right\} \Big/ \sum_{p=1}^{P} W_p$$

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

I estimate the neighbor covariance by:

$$\text{(A3)} \quad \hat{\eta}^2 = \sum_{p=1}^{P} W_p \left\{ \sum_{f \neq f'} W_{ff'p} \left\{ \sum_{i=1}^{I_{fp}} \sum_{i'=1}^{I_{f'p}} y_{ifp} y_{i'f'p} / (I_{fp} I_{f'p}) \right\} \Big/ \sum_{f \neq f'} W_{ff'p} \right\} \Big/ \sum_{p=1}^{P} W_p \, ,$$

[27] See Solon et al. (2000) for additional exposition about estimating neighbor covariances.

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 et al. (2000) give more weight is given 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.[28] 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 half-samples at the project level. For any parameter μ, if $\hat{\mu}$ represents the estimate from the full sample and $\hat{\mu}_k$ the estimate from the kth half-sample, the variance of $\hat{\mu}_k$ is estimated as:

(A4) $$Var(\hat{\mu}) = \sum_{k=1}^{100} (\hat{\mu}_k - \hat{\mu})^2 / 100 .$$

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

Appendix B: Data Specifics

This appendix covers the details of the Intergenerational Income Database (IID), and the samples used for computing the results in Section VI.

A. Intergenerational Income Database

Corak and Heisz (1999) and Corak (2001) discuss how the IID was created with administrative income tax records from Statistics Canada. The dataset contains information on all individuals ages 16 through 19 in 1982, 1984, and 1986 who filed an income tax return in Canada while living at home. Mothers and fathers are linked to these individuals from the T1 Family File (T1FF) in the year the child filed. The T1FF matches members of each taxfiler's family using social insurance numbers, names, and address information. The parents in the file are not necessarily biological parents; rather, they are male and female household heads at the time of the link. The IID contains some family siblings if they fall within the same cohort of taxfilers over the six-year period. Matching each child's family identification number (FIN) identifies siblings. Harris and Lucaciu (1994) describe how the FIN was constructed using the T1FF.

B. Truncation Rules for Variables

In averaging income over a number of years, I used only years where total income was greater than $1,000. Missing values not having a tax record for a particular year were excluded from the calculation. When I counted missing years as zero values for parental income, the coefficient from the parent's log income on the child's log earnings fell from 0.21 to 0.15 for the combined samples. The sibling and neighbor correlations remained about the same. When missing years were counted as zero values for the child's adult income, the sibling correlation fell from 0.26 to 0.17 for the combined samples; the neighbor correlation remained about zero.

C. Weighting the IID

The full weighting methodology is discussed in Cook and Demnati (2000). Since the IID does not include individuals who did not file an income tax return in their teenage years while still living at home, each of the three cohorts fails to capture the entire Canadian population. Compared to the population estimate from the 1986 census, the IID under represents the cohort population by 28 percent. For children in families with lower parental incomes, the coverage rate is lower. Compared to a full sample of Canadian taxfilers in 1998, the IID misses 56.2 percent of children in families with parental income less than \$10,000 and 39.8 percent of children in families with parental income of \$10,000 to \$19,000. As parental income rises, IID's coverage rate goes up. The coverage rate for children with parental incomes greater than \$40,000 is greater than 75.0 percent. Coverage varies across gender and geography dimensions, although these differences are not as pronounced.

The weights are computed in two stages. In the first, the basic weights are constructed for 11 parental income groups and 12 geographic groups. For each category, the basic weight is the number from the IID cohort in the sample of all taxfilers in 1998 divided by the number of people actually matched from this dataset to the IID. In the second stage, the basic weight is multiplied with a gender weight computed from the 1986 census.

D. Sample 3 creation

I created my third public housing sample by estimating a probability model for children in the IID whose parent or parents lived in public housing postal codes. A probit model was estimated for the probability of living in public housing between households in MTHC projects with unique postal codes and households in census tracts that contain public housing that does not have a unique postal code. The control variables were age of household head, average parental income, marital status of household head when the child was 16 and 25, a SA participation indicator, and family size indicators. The proportion of non public housing residents falls sharply for observations with predicted probabilities greater than 0.2. Sample 3 includes all households that lived in public housing postal codes with predicted probabilities for receiving subsidies greater than 0.25.

The control variables for the census sample were age of household head, family total income, marital status of household head, race indicators, an immigrant indicator, a social assistance participation indicator, family size indicators, household head's education attainment, and whether the household moved in the last five years. I restricted Sample 3 with census data to households with public housing postal codes and predicted probabilities greater than 0.15.[29]

[29] The coefficient results and kernel density estimates from the probit models are available from the author upon request.

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Table 1
Theories of Social Interaction

Theory	Main Concept	Literature Examples
1. Peer group influences and role model effects	Individual decisions are influenced by characteristics or behavior of community members.	Akerlof (1997), Akerlof and Kranton (2000) Banjeree (1992), Brown et al. (1986), Brown (1990), Crane (1991)
2. Benefits from social networks	Network of friends, relatives, or neighbors assist in finding jobs, providing loans, or giving psychological support.	Borjas (1995), Bertrand et al. (2000) Coleman (1988), Granovetter (1995) Montgomery (1991)
3. Limited local resources	Quality and efficiency of local institutions are limited by community resources.	Beabou (1996), Durlauf (1996), Hoxby (2000)
4. Conformism	Without full information, individuals emulate observed choices of others.	Bernheim (1994), Bikhchandani et al. (1992) Jones (1984), Sah (1991)

Table 2
Selected Census Tract Characteristics for Largest and Smallest Toronto Housing Projects
Compared to Reported Census Tract Characteristics from Boston and Chicago MTO Programs

	Toronto (1996)		Boston (1990)			Chicago (1990)		
Tract Characteristic	Downtown-Central Largest Projects	Diff. In Means Smallest-Largest	Control Means	Diff. In Means Sec. 8 - Control	Diff. In Means Exp - Control	Control Means	Diff. In Means Sec. 8 - Control	Diff. In Means Exp - Control
Female household head	0.585	-0.18 (0.03)	.531	-0.15	-0.283	.847	-0.192	-0.477
Black	0.193	-0.08 (0.00)	0.45	-0.11	-0.198	.993	-0.093	-0.421
Below LICO (Canada) or poverty line (U.S.)	0.494	-0.34 (0.00)	.359	-0.16	-0.254	.750	-0.384	-0.644
Receiving social assistance	0.343	-0.22 0.01	.294	-0.11	-0.202	.586	-0.274	-0.484
Owner-occupied household	0.035	0.42 (0.01)	NA	NA	NA	.0282	0.234	0.634
Adult population with education of less than high school	0.336	-0.09 (0.01)	NA	NA	NA	NA	NA	NA
Adult population with education of more than high school	0.499	0.14 (0.01)	0.29	0.40	0.133	NA	NA	NA
Adult population with education of college degree	0.157	0.07 (0.01)	NA	NA	NA	.081	0.073	0.149
Median household income (1996 $Cdn)	13,538	27,225	NA	NA	NA	9,007	15,702	39,881
Sample Size	923	770	176	113	236	118	53	67

Notes: "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 seven "largest" downtown housing projects. "Smallest" projects are defined as projects with fewer than 250 units, within census tracts with fewer than 25 percent of households living below the LICO. Standard errors are reported in parentheses, adjusted for household level clustering; Data for Boston is from Katz and Kling (2000), Table 4. Data for Chicago is from Rosenbaum, Harris, and Denton (1999), Table 1.

Table 3
Descriptive Statistics of Metropolitan Toronto and Public Housing Samples, 1996 Census Data

Household Heads, Ages 16-55 in 1996

	(1)	(2)	(3)	(4)	(5)
		Non PH Residents	Public Housing Residents		
	All Toronto	Living in PH Census Tracts	Sample 1	Sample 2	Sample 3
	(sample averages and standard errors)				
Household total income	53,108	20,832	13,707	16,377	7,099
	(68,365)	(27,587)	(1,642)	(10,949)	(7,971)
Household total wages	41,266	13,907	7,471	3,321	2,020
	(58,167)	(24,771)	(14,507)	(9,749)	(4,849)
Monthly rent	749	442	381	377	284
	(331)	(335)	(326)	(322)	(129)
Under LICO	0.22	0.64	0.78	0.91	0.87
	(0.41)	(0.48)	(0.41)	(0.28)	(0.33)
Moved in last five years	0.30	0.29	0.29	0.28	0.31
	(0.70)	(0.71)	(0.70)	(0.69)	(0.70)
Age of household head	44.57	42.29	42.01	37.89	42.15
	(12.50)	(12.23)	(12.46)	(10.12)	(12.84)
No high school diploma	0.23	0.34	0.37	0.37	0.37
	(0.42)	(0.47)	(0.48)	(0.48)	(0.48)
BA or greater	0.24	0.12	0.07	0.03	0.08
	(0.43)	(0.32)	(0.26)	(0.17)	(0.27)
female	0.33	0.56	0.62	1.00	0.63
	(0.47)	(0.50)	(0.49)		(0.48)
Black	0.07	0.32	0.42	0.47	0.37
	(0.25)	(0.47)	(0.49)	(0.50)	(0.48)
Immigrant	0.50	0.63	0.63	0.69	0.63
	(0.50)	(0.48)	(0.46)	(0.46)	(0.48)
Working	0.73	0.46	0.35	0.17	0.30
	0.44	0.50	0.48	0.37	0.46
Total income	37,877	18,103	13,218	13,698	12,015
	(51,854)	(17,918)	(11,389)	(6,935)	(12,830)
Total wages	28,758	11,313	6,493	1,605	5,051
	(45,646)	(18,048)	(11,689)	(5,642)	(12,542)
Dependent Children in Census, ages 16-25					
Age	20.53	20.11	19.97	19.65	19.80
	(3.22)	(3.15)	(3.12)	(3.13)	(3.10)
Black	0.07	0.32	0.40	0.41	0.39
	(0.26)	(0.47)	(0.49)	(0.48)	(0.49)
No high school diploma	0.09	0.14	0.14	0.15	0.14
	(0.28)	(0.34)	(0.35)	(0.36)	(0.35)
BA or greater	0.11	0.04	0.03	0.02	0.03
	(0.32)	(0.19)	(0.17)	(0.17)	(0.16)
Idle	0.06	0.13	0.15	0.13	0.18
	(0.23)	(0.34)	(0.35)	(0.36)	(0.38)
Employed	0.54	0.33	0.27	0.23	0.21
	(0.50)	(0.47)	(0.44)	(0.42)	(0.40)
Immigrant	0.27	0.41	0.43	0.45	0.41
	0.44	0.47	0.50	0.50	0.49
Children sample size	258,201	8,606	5,180	1,382	5,141

Notes: Sample 1 includes all households living in unique MTHC and Cityhome postal codes. Sample 2 includes all single-mother household heads receiving social assistence and living in postal codes containing public housing projects. Sample 3 includes households predicted to live in public housing from using a probit model (discussed in appendix B). The sample of dependent children includes both males and females, but only those living at home.

Table 4
Descriptive Statistics of Toronto and Public Housing Samples, IID Data

	(1)	(2)	(3)	(4)	(5)
		Non PH Residents	Public Housing Residents		
	All Toronto	Living in PH Census Tracts	Sample 1	Sample 2	Sample 3
	(sample averages and standard errors)				
Household Heads with Children Ages 26 to 33 in 1996					
Age	60.19	58.92	58.65	56.61	55.83
	(7.24)	(7.89)	(8.04)	(7.00)	(8.11)
Number of children	2.44	2.70	2.63	2.63	3.22
	(1.38)	(1.41)	(1.41)	(1.36)	(1.56)
Female	0.18	0.48	0.61	1.00	0.65
	(0.38)	(0.50)	(0.49)		(0.48)
Parent income, 15-year average	53,436	25,190	20,122	13,242	17,361
	(53,919)	(18,029)	(13,906)	(6,015)	(9,097)
Recieved social assistance, 1992-98	0.13	0.43	0.52	1.00	0.68
	(0.34)	(0.50)	(0.50)		(0.47)
Children, Ages 26-33 in 1996					
Age	31.70	31.68	31.73	31.60	30.47
	(2.25)	(2.22)	(2.21)	(2.23)	(1.95)
Average income, 1993-98	31,548	24,360	23,064	21,413	21,711
	(33,253)	(20,108)	(17,620)	(15,629)	(14,898)
Average earnings, 1993-98	28,257	21,360	19,978	17,911	18,520
	(33,476)	(21,064)	(18,955)	(17,139)	(16,335)
Received social assistance, 1992-98	0.15	0.35	0.41	0.48	0.42
	(0.36)	(0.48)	(0.49)	(0.50)	(0.49)
Working	0.88	0.82	0.80	0.77	0.80
	(0.33)	(0.39)	(0.40)	(0.42)	(0.40)
Sample size	297,588	12,577	6,559	2,674	1,046

Notes: Sample 1 includes all households living in unique MTHC and Cityhome postal codes. Sample 2 includes all single mother household heads receiving social assistence and living in postal codes containing public housing projects. Sample 3 includes households predicted to live in public housing from using a probit model (discussed in Appendix B). The samples include both male and female children.

Table 5
Selected Mean Characteristics of Household Heads from
Largest Downtown Central Public Housing Projects and Smallest Projects

	All Households		Households with Children	
	Downtown-Central Largest Projects	Diff. In Means Smallest - Largest	Downtown-Central Largest Projects	Diff. In Means Smallest - Largest
Census Data				
Single	0.340	0.025	0.594	-0.003
		(0.02)		(0.02)
Immigrant	0.689	-0.041	0.769	-0.020
		(0.02)		(0.02)
No high school diploma	0.472	-0.032	0.471	-0.032
		(0.02)		(0.02)
Black	0.324	0.029	0.293	0.029
		(0.02)		(0.02)
BA or greater	0.065	0.005	0.062	0.005
		(0.02)		(0.01)
Moved in last five years	0.532	0.004	0.522	0.004
		(0.01)		(0.02)
Age of household head	42.79	-1.62	40.41	-1.62
		(0.65)		(0.59)
Number of children	1.319	0.118	2.301	0.118
		(0.07)		(0.08)
Median income	10,583	2,689	12,589	2,689
Percentage on social assistance	0.538	-0.085	0.579	-0.085
		(0.02)		(0.02)
Sample size	923	770	529	479
IID Data				
Single	NA	NA	0.564	0.007
				(0.02)
Age of household head	NA	NA	61.93	-2.40
				(0.68)
Percentage on social assistance	NA	NA	0.613	0.000
				(0.03)
Median income	NA	NA	15,284	1,888
Sample size	NA	NA	1757	1054

Notes: "Diff. In Means" is the mean difference between census tract characteristics among households in "smallest" public housing projects and households living in the seven "largest" downtown housing projects. "Smallest" projects are defined as projects with fewer than 250 units, within census tracts with fewer than 25 percent of households living below the LICO. Standard errors are reported in parentheses, adjusted for household level clustering;

Table 6
Mean Outcomes and Mean Differences between Youth From Largest and Smallest Public Housing Projects

	(1) Mean Largest Projects	(2) Mean Difference Smallest-Largest, No Controls	(3) Dummy Variable Coeff. for Smallest Projects, with Background Controls
Census Data (Youths ages 16 to 25 living at home)			
Total years of schooling	12.34	-0.076	0.177
		(0.179)	(0.171)
Less than high school	0.15	0.005	0.011
		(0.025)	(0.027)
More than high school	0.16	0.007	0.008
		(0.025)	(0.026)
Idle	0.32	0.046	-0.017
		(0.032)	(0.030)
Sample Size	226	390	390
IID Data (Adults ages 28 to 35 in 1998 who lived in public housing when teens)			
Receiving SA	0.32	-0.028	-0.015
		(0.018)	(0.018)
Log income (males)	9.95	0.024	0.016
		(0.024)	(0.024)
Log earnings (males)	9.84	-0.004	0.011
		(0.033)	(0.033)
Number of times not a tax filer	2.27	0.060	0.119
		(0.114)	(0.112)
N (males)	719	1154	1154

Notes: Column (2) shows the mean difference between outcomes among youths from the "smallest" public housing projects and youths from the seven largest downtown housing projects. "Smallest" projects are defined as projects with fewer than 250 units within census tracts with fewer than 25 percent of households living below the LICO. Standard errors are reported in parentheses, adjusted for household level clustering. None of the differences is significant from zero (p-value < 0.10). Column (3) shows dummy coefficient estimates from regressing the outcome variable on age dummies, gender, log parental income, parental marital status, whether parent received social assistence, family size, and dummy variables for the indicated measure of neighborhood quality. The estimates based on census data also include indicators for blacks and recent immigrants. For binary outcome variables, a probit model was used, and the coefficient estimates shown are the estimated change in probability from a discrete change in the indicated dummy variable.

Table 7
Criminal Occurences in 1992 for Smallest and Largest Public Housing Projects

Type of Occurance	Downtown-Central Largest Projects	Mean Difference Smallest Projects	for Smallest Project with Mean Family Char. Controls
	per 1000 household units		
Arson	1.12	-1.12	-1.22
		(0.54)	(0.83)
Assault causing bodily harm	17.02	-12.69	-11.47
		(2.39)	(3.64)
Sexual assault	1.45	-1.45	-1.40
		(0.28)	(0.44)
Break and enter and attempted B&E	22.00	-3.10	-10.93
		(5.01)	(6.01)
Drug offense	14.61	-7.53	-12.90
		(7.81)	(11.59)
Neighbor dispute	436	-129	-119
		(94.97)	(141.83)
Homicide	4.18	-3.78	-3.43
		(1.24)	(1.70)
Project Sample Size	7	35	

Notes: Occurences are all incidents on MTHC property that required a written report by MTHC Security Services . Column (2) shows the mean difference between crime occurances among the seven largest downtown public housing projects and the 35 "smallest" projects. "Smallest" projects are defined as projects with fewer than 250 units, within census tracts with fewer than 25 percent of households living below the LICO. Standard errors are reported in parentheses. Column (3) shows dummy coefficient estimates from regressing the outcome variable on mean household project characteristics (same as those shown in Table 1) and dummy variables for the indicated measure of crime incidence.

Table 8

Means and Difference from Means for Various Public Housing Neighborhood Quality Measures, 1996 Census Outcome Variables

Youth From Public Housing Projects	Without Controls (1) Total years of schooling	(2) Less than High School	(3) More than High School	(4) Idle	With Individual Background Controls (5) Total years of schooling	(6) Less than High School	(7) More than High School	(8) Idle	(9) Sample Size
By Number of Household Units									
<= 150 Units (mean)	12.36	0.14	0.30	0.16					688
> 150, <= 700 Units (diff)	-0.135	0.004	-0.011	-0.013	-0.064	0.001	-0.003	-0.015	1004
	(0.106)	(0.017)	(0.023)	(0.018)	(0.093)	(0.017)	(0.022)	(0.017)	
> 700 Units (diff)	0.036	0.001	0.012	-0.001	-0.092	0.008	0.013	-0.018	422
	(0.133)	(0.022)	(0.027)	(0.022)	(0.116)	(0.022)	(0.028)	(0.020)	
By MTHC or Cityhome Development									
MTHC (mean)	12.28	0.14	0.30	0.16					1728
Cityhome (diff.)	0.137	-0.004	0.015	-0.008	0.077	0.008	-0.001	0.008	462
	(0.114)	(0.018)	(0.024)	(0.019)	(0.098)	(0.019)	(0.024)	(0.019)	
By Percent in Census Tract Below LICO									
<=0.15 (mean)	12.11	0.15	0.29	0.17					149
>0.15, <=0.40 (diff.)	0.158	-0.010	0.015	-0.013	0.126	-0.012	0.015	-0.004	965
	(0.190)	(0.030)	(0.041)	(0.031)	(0.164)	(0.028)	(0.032)	(0.039)	
>=0.40 (diff.)	0.155	-0.008	0.017	-0.008	0.151	-0.012	0.005	0.008	564
	(0.199)	(0.032)	(0.042)	(0.032)	(0.174)	(0.028)	(0.041)	(0.046)	
By High Rise or Townhouse									
High Rise (mean)	12.34	0.14	0.31	0.14					827
Townhouse (diff.)	-0.013	0.001	-0.019	0.004	-0.072	0.008	-0.015	0.012	1068
	(0.099)	(0.016)	(0.021)	(0.016)	(0.090)	(0.016)	(0.022)	(0.016)	

Notes: Columns 1-4 show raw means for particular neighborhood quality categories, and average deviations from these means for the other cateogories. Columns 5-8 show dummy coefficient estimates from regressing the outcome variable on age, gender, log parental income, parental marital status, whether parent receives social assistence, family size, black, recent immigrant and dummy variables for the indicated measure of neighborhood quality. For binary outcome variables, a probit model was used, and the coefficient estimates shown are the estimated change in probability from a discrete change in the indicated dummy variable. Standard errors are in parentheses, adjusted for clustering by project. The sample includes children ages 16 to 25 still living in public housing with their parents.

Table 9

Means and Difference from Means for Various Public Housing Neighborhood Quality Measures, IID Outcome Variables

Youth From Public Housing Projects	without controls (1) Receiving SA	(2) Log Income (males)	(3) Log Earnings (males)	(4) Number of Times Did Not File Taxes	with individual background controls (5) Receiving SA	(6) Log Income (males)	(7) Log Earnings (males)	(8) Number of Times Did Not File Taxes	(9) Samp. Size (for col. 7)
By Number of Household Units									
<= 150 Units (mean)	0.32	10.00	9.80	2.20					1065
> 150, <= 700 Units (diff)	-0.021	-0.003	0.025	0.175	-0.019	0.001	0.008	0.015	3505
	(0.013)	(0.023)	(0.031)	(0.076)	(0.012)	(0.023)	(0.008)	(0.012)	
> 700 Units (diff)	0.002	-0.016	0.002	0.136	0.004	-0.014	0.013	0.014	1189
	(0.011)	(0.026)	(0.036)	(0.091)	(0.015)	(0.026)	(0.089)	(0.014)	
By MTHC or Cityhome Development									
MTHC (mean)	0.31	9.99	9.81	2.33					5432
Cityhome (diff.)	-0.025	0.03	0.03	0.105	-0.017	0.03	0.102	-0.02	324
	(0.020)	(0.036)	(0.049)	(0.119)	(0.020)	(0.036)	(0.120)	(0.019)	
By Percent in Census Tract Below LICO									
<=0.15 (mean)	0.29	10.03	9.84	2.33					390
>0.15, <=0.40 (diff.)	0.013	-0.02	-0.02	0.027	0.014	-0.03	-0.014	-0.03	3656
	(0.020)	(0.037)	(0.049)	(0.116)	(0.020)	(0.035)	(0.120)	(0.019)	
>=0.40 (diff.)	0.014	-0.02	-0.02	-0.060	0.013	-0.02	-0.016	0.00	1710
	(0.020)	(0.034)	(0.050)	(0.122)	(0.020)	(0.036)	(0.124)	(0.020)	
By Highrise or Townhouse									
Highrise (mean)	0.31	9.99	9.81	2.39					1884
Townhouse (diff.)	-0.014	0.005	0.001	-0.140	0.002	-0.009	-0.060	-0.006	3537
	(0.011)	(0.018)	(0.025)	(0.064)	(0.011)	(0.018)	(0.063)	(0.010)	

Notes: Columns 1-4 show raw means for particular neighborhood quality categories, and average deviations from these means for the other cateogories. Columns 5 to 8 show dummy coefficient estimates from regressing the outcome variable on age, gender, log parental income, parental marital status, whether parent receives social assistence, family size, and dummy variables for the indicated measure of neighborhood quality. For binary outcome variables, a probit model was used, and the coefficient estimates shown are the estimated change in probability from a discrete change in the indicated dummy variable. Standard errors are in parentheses, adjusted for clustering by project. The sample includes children who entered public housing before age 17, and follows them after they leave. Income and earnings are averaged between 1993 and 1998. Receiving SA equals one if an individual received welfare income for at least two years between 1993 and 1998. The variable in columns 4 and 8 is the total number of missing annual tax files since an individual started filing. The results with earnings and income as outcome variables are estimated for males only.

Table 10

Means and Difference from Means for Various Public Housing Neighborhood Quality Measures, 1992 Crime Occurances

Youth From Public Housing Projects	(1) **Arson**	(2) **Assault causing bodily harm**	(3) **Sexual Assault**	(4) **Break and Enter or attempted B&E**	(5) **Drug Offense**	(6) **Neighbor Dispute**	(7) **Homicide**
				per 1000 household units			
				By Number of Household Units			
<= 150 Units (mean)	0.00	4.92	0.62	18.14	6.15	366	0.31
> 150, <= 700 Units (diff)	0.42	7.45	0.61	2.69	7.75	156	1.72
	(0.9)	(3.6)	(0.5)	(6.9)	(13.0)	(152.1)	(1.7)
> 700 Units (diff)	1.50	16.90	1.76	5.00	10.14	267	5.46
	(1.0)	(4.0)	(0.5)	(7.7)	(14.6)	(170.3)	(1.9)
				By Percent in Census Tract Below LICO			
<=.15 (man)	0.00	3.65	0.00	10.34	0.61	259	1.82
>.15, <=.40 (diff)	0.30	3.17	1.32	4.26	7.06	203	-0.30
	(1.3)	(5.8)	(0.6)	(3.3)	(5.5)	(154.0)	(1.6)
>.40 (diff)	1.26	12.18	1.44	9.81	14.67	216	1.68
	(1.3)	(6.1)	(0.7)	(4.5)	(6.6)	(164.0)	(1.1)
				by Highrise or Townhouse			
Highrise (mean)	0.45	14.74	2.45	14.48	7.79	450	2.77
Townhouse (diff)	0.25	-1.58	-1.89	4.54	4.35	-26	-1.78
	(0.3)	(5.5)	(0.7)	(3.8)	(3.8)	(158.0)	(0.8)

Notes: Occurrence data is from MTHC Security Services (for MTHC projects only). Highrises are defined as buildings with at least five stories. Rows with (diff) indicate dummy coefficient of neighborhood quality measure, after controlling for mean project characteristics (same as those used in Table 1).

Table 11

Income and Social Assistence Regressed on Background Variables and Project Characteristics, Interacted with Age Entered Public Housing and Years Lived in Program

	Dependent Variable									
	Log Adult Income (regression coefficients)					Years on Social Assistance (derivatives of change in probability from probit estimates)				
Age	0.30	0.22	0.22	0.35	0.35	-0.01	-0.02	-0.02	0.02	0.02
	(0.12)	(0.13)	(0.13)	(0.13)	(0.13)	(0.07)	(0.08)	(0.08)	(0.07)	(0.07)
Age squared	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Female						0.02	0.02	0.02	0.02	0.02
						(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Single Parent	-0.07	-0.07	-0.07	-0.08	-0.08	0.06	0.06	0.06	0.06	0.06
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
log parent's total income	0.05	0.05	0.05	0.06	0.06	-0.06	-0.06	-0.06	-0.06	-0.06
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Parent on Social Assistance	-0.12	-0.12	-0.11	-0.13	-0.12	0.09	0.09	0.09	0.09	0.09
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Age Entered Public Housing										
10-13		0.05	0.07				0.00	0.00		
		(0.03)	(0.04)				(0.02)	(0.02)		
14-16		0.01	0.03				0.00	0.00		
		(0.03)	(0.03)				(0.02)	(0.02)		
More than 35 percent in CT below LICO	0.00	-0.01	-0.01	-0.02	0.00	0.01	0.01	0.00	0.00	-0.02
	(0.02)	(0.02)	(0.04)	(0.02)	(0.03)	(0.01)	(0.01)	(0.03)	(0.02)	(0.02)
More than 35 percent in CT below LICO			-0.02					0.01		
*** Entered age 10-13**			(0.05)					(0.03)		
More than 35 percent in CT below LICO			0.05					0.01		
*** Entered age 14-16**			(0.05)					(0.03)		
Years lived Public Housing										
5-10				0.00	0.00				0.00	-0.01
				(0.03)	(0.04)				(0.01)	(0.03)
11+				0.01	0.02				-0.01	-0.01
				(0.03)	(0.03)				(0.02)	(0.02)
More than 35 percent in CT below LICO					0.01					0.01
*** 5-10 years in Public Housing**					(0.04)					(0.03)
More than 35 percent in CT below LICO					-0.02					0.01
*** 11+ years in Public Housing**					(0.04)					(0.03)
Constant	4.09	5.38	5.41	3.31	3.25					
	(1.94)	(2.10)	(2.10)	(2.02)	(2.02)					
N	4530	4530	4530	4530	4530	9477	9477	9477	9477	9477

Notes: Omitted variables are fewer than 35 percent in census tract below low-income cut-off, and entered public housing before age 10 or spent fewer than five years in public housing. Regressions with Income as the dependent variable use only males from the sample. For the binary dependent variable, the probablity of receiving social assistance for at least two years between 1993 and 1998, the coefficient results from estimating a probit model are presented as estimated derivatives from a change in one of the independent variables.

Table 12

Brother and Neighbor Covariances of Adult Log Income

		Public Housing Samples		
	All Toronto	**Sample 1**	**Sample 2**	**Sample 3**
Variance	0.335 (0.007)	0.376 (0.008)	0.364 (0.012)	0.369 (0.007)
		Siblings		
Brother covariance	0.101 (0.006)	0.108 (0.019)	0.096 (0.031)	0.096 (0.018)
Brother covariance after controlling for observable family characteristics	0.081 (0.004)	0.098 (0.018)	0.086 (0.031)	0.087 (0.017)
	Neighbours within EAs (Toronto sample) or projects (PH samples)			
Neighbor covariance	0.015 (0.011)	0.003 (0.014)	0.011 (0.016)	-0.005 (0.015)
Neighbor covariance after controlling for observable family characteristics	0.005 (0.002)	0.004 (0.013)	0.011 (0.015)	-0.004 (0.016)
Neighbor covariance tenure >= 5 years	0.016 (0.013)	-0.001 (0.028)	-0.002 (0.003)	-0.009 (0.026)
Neighbor covariance adjusted tenure >= 5 years	0.002 (0.000)	-0.002 (0.028)	-0.002 (0.003)	-0.008 (0.026)
		Neighbours within census tracts		
Neighbor covariance	0.013 (0.008)	0.005 (0.014)	-0.007 (0.017)	0.005 (0.014)
Neighbor covariance after controlling for observable family characteristics	0.005 (0.003)	0.000 (0.014)	-0.006 (0.017)	0.000 (0.014)
Neighbor covariance tenure >= 5 years	0.017 (0.013)	-0.001 (0.005)	0.000 (0.009)	-0.001 (0.012)
Neighbor covariance adjusted tenure >= 5 years	0.004 (0.001)	-0.001 (0.002)	0.000 (0.003)	-0.001 (0.013)
Sample size	132,412	4,192	1,118	4,884
Number of sibling pairs	16,485	772	156	889
Number of neighbour pairs	1,025,426	61,468	10,125	88,620

Notes: Adult men's incomes are averaged over 6 years for children in the IID from 1993-98. Sample 1 includes all households living in uniquely matched MTHC and Cityhome postal codes. Sample 2 includes all single mother household heads living in postal codes containing public housing projects. Sample 3 includes households predicted to live in public housing from using a probit model (discussed in appendix B). The estimated "effect" is the squared covariance for neighbors in a census tract with tenure>=5years multiplied by the squared sample variance. See text for details.

Table 13

Brother and Neighbor Covariances of Adult Log Earnings

		Public Housing Samples		
	All Toronto	**Sample 1**	**Sample 2**	**Sample 3**
Variance	0.477	0.603	0.602	0.604
	(0.005)	(0.018)	(0.023)	(0.020)
Siblings				
Brother Covariance	0.116	0.102	0.042	0.153
	(0.006)	(0.031)	(0.032)	(0.032)
Brother Covariance after controlling for observable family characteristics	0.098	0.091	0.048	0.150
	(0.005)	(0.028)	(0.034)	(0.032)
Neighbours within EAs (Toronto sample) or projects (PH samples)				
Neighbor covariance	0.009	0.002	0.017	-0.002
	(0.008)	(0.016)	(0.042)	(0.014)
Neighbor covariance after controlling for observable family characteristics	0.001	0.002	0.018	0.001
	(0.002)	(0.015)	(0.042)	(0.015)
Neighbor covariance tenure >= 5 years	0.016	0.005	0.009	0.006
	(0.013)	(0.004)	(0.036)	(0.018)
Neighbor covariance adjusted tenure >= 5 years	0.002	0.005	0.012	0.005
	(0.000)	(0.004)	(0.036)	(0.018)
Neighbours within census tracts				
Neighbor covariance	0.010	0.002	0.001	0.004
	(0.008)	(0.016)	(0.032)	(0.011)
Neighbor covariance after controlling for observable family characteristics	0.003	0.002	0.002	0.005
	(0.003)	(0.015)	(0.003)	(0.011)
Neighbor covariance tenure >= 5 years	0.017	0.005	-0.005	0.006
	(0.013)	(0.004)	(0.026)	(0.016)
Neighbor covariance adjusted tenure >= 5 years	0.004	0.005	-0.004	0.005
	(0.001)	(0.004)	(0.026)	(0.016)
Sample Size	132,412	4,192	2,140	3,337
Number of Sibling Pairs	16,485	659	353	518
Number of neighbour Pairs	1,025,426	68,853	10,125	55,959

Notes: Adult men'searnings are averaged over 6 years for children in the IID from 1993-98. Sample 1 includes all households living in uniquely matched MTHC and Cityhome postal codes. Sample 2 includes all single mother household heads living in postal codes containing public housing projects. Sample 3 includes households predicted to live in public housing from using a probit model (discussed in appendix B). The estimated "effect" is the squared covariance for neighbors in a census tract with tenure>=5years multiplied by the squared sample variance. See text for details.

Table 14

Sibling and Neighbor Covariances of Number of Years Receiving Social Assistance between 1992-1998

		Public Housing Samples		
	Toronto	**Sample 1**	**Sample 2**	**Sample 3**
Variance	1.515 (0.039)	3.655 (0.097)	4.221 (0.138)	3.980 (0.132)
Siblings				
Sibling Covariance	0.301 (0.022)	0.833 (0.162)	0.757 (0.234)	0.905 (0.189)
Sibling Covariance after controlling for observable family characteristics	0.253 (0.020)	0.722 (0.150)	0.685 (0.235)	0.853 (0.191)
Neighbours within EAs (Toronto sample) or projects (PH samples)				
Neighbor covariance	0.039 (0.025)	0.030 (0.075)	-0.035 (0.149)	0.073 (0.117)
Neighbor covariance after controlling for observable family characteristics	0.016 (0.011)	0.028 (0.075)	-0.040 (0.149)	0.048 (0.115)
Neighbor covariance tenure >= 5 years	0.025 (0.028)	-0.063 (0.133)	-0.111 (0.188)	-0.004 (0.147)
Neighbor covariance adjusted tenure >= 5 years	0.033 (0.015)	-0.073 (0.129)	-0.112 (0.036)	-0.028 (0.140)
Neighbours within census tracts				
Neighbor covariance	0.033 (0.024)	0.009 (0.086)	-0.013 (0.131)	0.005 (0.112)
Neighbor covariance after controlling for observable family characteristics	0.012 (0.011)	0.018 (0.088)	-0.024 (0.130)	-0.055 (0.095)
Neighbor covariance tenure >= 5 years	0.031 (0.019)	-0.049 (0.104)	-0.108 (0.136)	-0.069 (0.125)
Neighbor covariance adjusted tenure >= 5 years	0.034 (0.018)	-0.083 (0.098)	-0.127 (0.135)	-0.087 (0.116)
Sample Size	208,514	5,329	2,619	3,993
Number of Sibling Pairs	38,541	1,042	502	718
Number of neighbour Pairs	2,556,912	98,633	10,125	55,959

Notes: Sample 1 includes all households living in uniquely matched MTHC and Cityhome postal codes. Sample 2 includes all single mother household heads living in postal codes containing public housing projects. Sample 3 includes households predicted to live in public housing from using a probit model (discussed in the appendix). The estimated "effect" is the squared covariance for neighbors in a census tract with tenure>=5years multiplied by the squared sample variance. See text for details.

Table 15

Sibling and Neighbor Covariances of Total Years of Schooling

		Public Housing Samples		
	All Toronto	**Sample 1**	**Sample 2**	**Sample 3**
Variance	3.826	3.360	2.706	3.321
	(0.043)	(0.190)	(0.294)	(0.196)
Siblings				
Brother covariance	1.455	0.563	0.538	0.658
	(0.041)	(0.101)	(0.125)	(0.098)
Brother covariance after controlling for observable family characteristics	1.319	0.490	0.501	0.555
	(0.039)	(0.177)	(0.104)	(0.087)
Neighbours within EAs (Toronto sample) or projects (PH samples)				
Neighbor covariance	0.185	-0.015	-0.028	-0.165
	(0.065)	(0.131)	(0.156)	(0.229)
Neighbor covariance after controlling for observable family characteristics	0.125	-0.059	-0.025	-0.197
	(0.028)	(0.111)	(0.156)	(0.211)
Neighbor covariance tenure >= 5 years	0.211	-0.124	0.009	0.006
	(0.053)	(0.066)	(0.036)	(0.018)
Neighbor covariance adjusted tenure >= 5 years	0.142	-0.115	0.012	0.005
	(0.074)	(0.066)	(0.036)	(0.018)
Neighbours within census tracts				
Neighbor covariance	0.204	-0.032	-0.012	-0.049
	(0.081)	(0.089)	(0.169)	(0.104)
Neighbor covariance after controlling for observable family characteristics	0.126	-0.063	0.010	-0.085
	(0.020)	(0.078)	(0.168)	(0.082)
Neighbor covariance tenure >= 5 years	0.231	-0.133	0.003	-0.134
	(0.088)	(0.075)	(0.204)	(0.092)
Neighbor covariance adjusted tenure >= 5 years	0.152	-0.097	0.121	-0.072
	(0.077)	(0.064)	(0.156)	(0.069)
Sample size	91,212	1,341	607	1,819
Number of sibling pairs	35,043	542	440	1,522
Number of neighbour pairs	621,201	13,109	2,800	16,289

Notes: The 1996 Census sample is for children 16-25 living with their parent or parents. Sample 1 includes all households living in uniquely matched MTHC and Cityhome postal codes. Sample 2 includes all single mother household heads living in postal codes containing public housing projects. Sample 3 includes households predicted to live in public housing from using a probit model (discussed in appendix B). The estimated "effect" is the squared covariance for neighbors in a census tract with tenure>=5years multiplied by the squared sample variance. See text for details.

Table A1

Means and Difference from Means for Various Public Housing Neighborhood Quality Measures with Age, Gender, and Family Background Controls,

Public Housing Sample 1

	Census Variables					IID Variables			
Youth From Public Housing Projects	(1) **Total years of schooling**	(2) **Less than High School**	(3) **Idle**	(4) **More than High School**	(5) **Sample Size**	(6) **Receiving SA**	(7) **Log Income (males)**	(8) **Log Earnings (males)**	(10) **Samp. Size (for col. 8)**
By Number of Household Units									
<= 150 Units (mean)	12.26	0.14	0.16	0.29	353	0.31	10.01	9.81	598
> 150, <= 700 Units (diff)	-0.078	-0.009	-0.010	0.012	753	-0.016	0.001	0.007	2357
	(0.136)	(0.022)	(0.023)	(0.030)		(0.015)	(0.027)	(0.037)	
> 700 Units (diff)	0.138	-0.004	-0.011	0.021	421	0.007	-0.016	-0.018	906
	(0.152)	(0.025)	(0.026)	(0.033)		(0.016)	(0.030)	(0.041)	
By MTHC or Cityhome Development									
MTHC (mean)	12.04	14.90	0.17	0.24	571	0.31	9.99	9.81	2028
Cityhome (diff.)	0.023	0.013	-0.002	-0.017	104	-0.019	0.02	-0.01	112
	(0.186)	(0.038)	(0.039)	(0.035)		(0.023)	(0.052)	(0.061)	
By Percent in Census Tract Below LICO									
<=0.15 (mean)	12.17	0.16	0.17	0.28	87	0.29	10.03	9.84	259
>0.15, <=0.40 (diff.)	0.076	-0.014	-0.019	0.013	551	0.012	0.00	-0.02	2393
	(0.246)	(0.039)	(0.040)	(0.053)		(0.022)	(0.038)	(0.053)	
>=0.40 (diff.)	0.187	-0.026	-0.023	0.021	487	0.029	-0.01	-0.02	1209
	(0.248)	(0.039)	(0.040)	(0.053)		(0.022)	(0.040)	(0.054)	
By High Rise or Townhouse									
High Rise (mean)	12.29	0.14	0.14	0.32	737	0.32	9.99	9.81	1402
Townhouse (diff.)	-0.041	-0.006	0.008	-0.018	604	0.00	-0.02	-0.02	2250
	(0.115)	(0.019)	(0.019)	(0.026)		(0.021)	(0.019)	(0.026)	
By Largest Projects or Smallest Projects									
less than 250 units, no highrises in CT with less than 25% below LICO	12.02	0.15	0.15	0.29	112	0.28	10.04	9.83	425
Seven Largest Projects (difference)	0.218	-0.005	-0.002	0.03	374	-0.01	0.00	0.00	871
	(0.258)	(0.038)	(0.037)	(0.050)		(0.020)	(0.036)	(0.049)	

Notes: The tables shows raw means for particular neighborhood quality categories, and average deviations from these means for the other cateogories for all youth in uniquely identified public housing projects.. Standard errors are in parentheses. Except for columns 7and 8, the samples include both men and women. The sample sizes given in column 10 are for the sample of men used in column 8. The census sample includes children aged 16-25 still living in public housing with their parents. The IID sample includes children who entered public housing before age 17, and follows them after they leave. Income and earnings are averaged between 1993 and 1998. Receiving SA equals one if an individual received welfare income for at least two years between 1993 and 1998. The variable in column 9 is the total number of missing annual tax files since an individual started filing. See text for further details.

Table A2

Means and Difference from Means for Various Public Housing Neighborhood Quality Measures with Age, Gender, and Family Background Controls,

Public Housing Sample 2

Youth From Public Housing Projects	Census Variables (1) Total years of schooling	(2) Less than High School	(3) Idle	(4) More than High School	(5) Sample Size	IID Variables (6) Receiving SA	(7) Log Income (males)	(8) Log Earnings (males)	(10) Samp. Size (for col. 8)
By Number of Household Units									
<= 150 Units (mean)	12.19	0.15	0.17	0.25	169	0.38	9.95	9.71	437
> 150, <= 700 Units (diff)	-0.106	0.020	-0.020	-0.026	358	-0.012	0.005	0.058	1306
	(0.162)	(0.033)	(0.034)	(0.033)		(0.026)	(0.031)	(0.044)	
> 700 Units (diff)	-0.164	0.013	0.013	-0.030	128	-0.021	0.033	-0.029	397
	(0.204)	(0.042)	(0.045)	(0.036)		(0.033)	(0.037)	(0.051)	
By MTHC or Cityhome Development									
MTHC (mean)	12.04	14.90	0.17	0.24	571	0.38	9.94	9.71	2028
Cityhome (diff.)	0.023	0.013	-0.002	-0.017	104	0.050	0.02	-0.01	112
	(0.186)	(0.038)	(0.039)	(0.035)		(0.048)	(0.052)	(0.061)	
By Percent in Census Tract Below LICO									
<=0.15 (mean)	12.19	0.15	0.18	0.02	27	0.37	10.02	9.74	116
>0.15, <=0.40 (diff.)	0.214	-0.014	-0.016	0.010	297	0.017	-0.01	-0.01	1372
	(0.353)	(0.067)	(0.052)	(0.043)		(0.043)	(0.055)	(0.047)	
>=0.40 (diff.)	0.101	-0.018	-0.005	0.009	194	0.017	-0.02	-0.01	652
	(0.364)	(0.055)	(0.047)	(0.056)		(0.044)	(0.056)	(0.044)	
By High Rise or Townhouse									
High Rise (mean)	11.98	0.15	0.17	0.25	272	0.37	9.92	9.72	629
Townhouse (diff.)	-0.016	-0.020	-0.006	-0.014	313	0.01	0.01	0.01	1375
	(0.160)	(0.029)	(0.030)	(0.034)		(0.021)	(0.026)	(0.037)	
By Largest Projects or Smallest Projects									
less than 250 units, no highrises in CT with less than 25% below LICO	12.16	0.14	0.15	0.25	51	0.38	9.94	9.71	285
Seven Largest Projects (difference)	-0.094	0.016	0.047	-0.01	127	0.02	0.00	0.02	430
	(0.342)	(0.061)	(0.065)	(0.062)		(0.040)	(0.046)	(0.064)	

Notes: The tables shows raw means for particular neighborhood quality categories, and average deviations from these means for the other cateogories for all youth with single female parents on social assistance. Standard errors are in parentheses. Except for columns 7and 8, the samples include both men and women. The sample sizes given in column 10 are for the sample of men used in column 8. The census sample includes children aged 16-25 still living in public housing with their parents. The IID sample includes children who entered public housing before age 17, and follows them after they leave. Income and earnings are averaged between 1993 and 1998. Receiving SA equals one if an individual received welfare income for at least two years between 1993 and 1998. The variable in column 9 is the total number of missing annual tax files since an individual started filing. See text for further details.

Table A3

Means and Difference from Means for Various Public Housing Neighborhood Quality Measures with Age, Gender, and Family Background Controls,

Public Housing Sample 3

	Census Variables					IID Variables			
Youth From Public Housing Projects	(1) **Total years of schooling**	(2) **Less than High School**	(3) **Idle**	(4) **More than High School**	(5) **Sample Size**	(6) **Receiving SA**	(7) **Log Income (males)**	(8) **Log Earnings (males)**	(10) **Samp. Size (for col. 8)**
						By Number of Household Units			
<= 150 Units (mean)	12.33	0.14	0.16	0.29	647	0.29	9.99	9.78	149
> 150, <= 700 Units (diff)	-0.105	0.004	-0.013	-0.004	976	-0.010	0.043	0.009	402
	(0.108)	(0.018)	(0.018)	(0.023)		(0.036)	(0.096)	(0.078)	
> 700 Units (diff)	0.013	0.007	0.008	0.016	361	-0.007	-0.041	-0.022	155
	(0.139)	(0.023)	(0.024)	(0.030)		(0.042)	(0.076)	(0.067)	
						By MTHC or Cityhome Development			
MTHC (mean)	12.26	0.15	0.16	0.29	1627	0.27	10.03	9.80	690
Cityhome (diff.)	0.155	-0.009	-0.010	0.012	433	0.032	-0.04	-0.02	16
	(0.115)	(0.019)	(0.019)	(0.025)		(0.051)	(0.059)	(0.073)	
By Percent in Census Tract Below LICO						**By Percent in Census Tract Below LICO**			
<=0.15 (mean)	11.93	0.16	0.19	0.28	147	0.30	10.03	9.81	33
>0.15, <=0.40 (diff.)	0.281	-0.013	-0.032	0.042	915	-0.007	-0.03	-0.03	476
	(0.190)	(0.031)	(0.032)	(0.041)		(0.043)	(0.065)	(0.049)	
>=0.40 (diff.)	0.266	-0.021	-0.014	0.041	496	-0.021	-0.03	-0.03	197
	(0.200)	(0.033)	(0.034)	(0.043)		(0.046)	(0.060)	(0.078)	
						By High Rise or Townhouse			
High Rise (mean)	12.28	0.14	0.15	0.31	751	0.29	10.04	9.82	74
Townhouse (diff.)	0.040	0.001	-0.004	-0.018	1020	0.01	-0.06	-0.06	571
	(0.101)	(0.017)	(0.017)	(0.022)		(0.045)	(0.072)	(0.081)	
						By Largest Projects or Smallest Projects			
less than 250 units, no highrises in CT with less than 25% below LICO	12.01	0.17	0.19	0.28	209	0.30	10.01	9.82	122
Seven Largest Projects	0.359	-0.005	-0.003	0.01	327	-0.02	-0.03	-0.03	134
	(0.205)	(0.033)	(0.034)	(0.041)		(0.049)	(0.073)	(0.078)	

Notes: The tables shows raw means for particular neighborhood quality categories, and average deviations from these means for the other cateogories for all youth in public housing sample 3. Standard errors are in parentheses. Except for columns 7and 8, the samples include both men and women. The sample sizes given in column 10 are for the sample of men used in column 8. The census sample includes children aged 16-25 still living in public housing with their parents. The IID sample includes children who entered public housing before age 17, and follows them after they leave. Income and earnings are averaged between 1993 and 1998. Receiving SA equals one if an individual received welfare income for at least two years between 1993 and 1998. The variable in column 9 is the total number of missing annual tax files since an individual started filing. See text for further details.

Figure 1 MTHC and Cityhome Public Housing Projects in Metropolitan Toronto

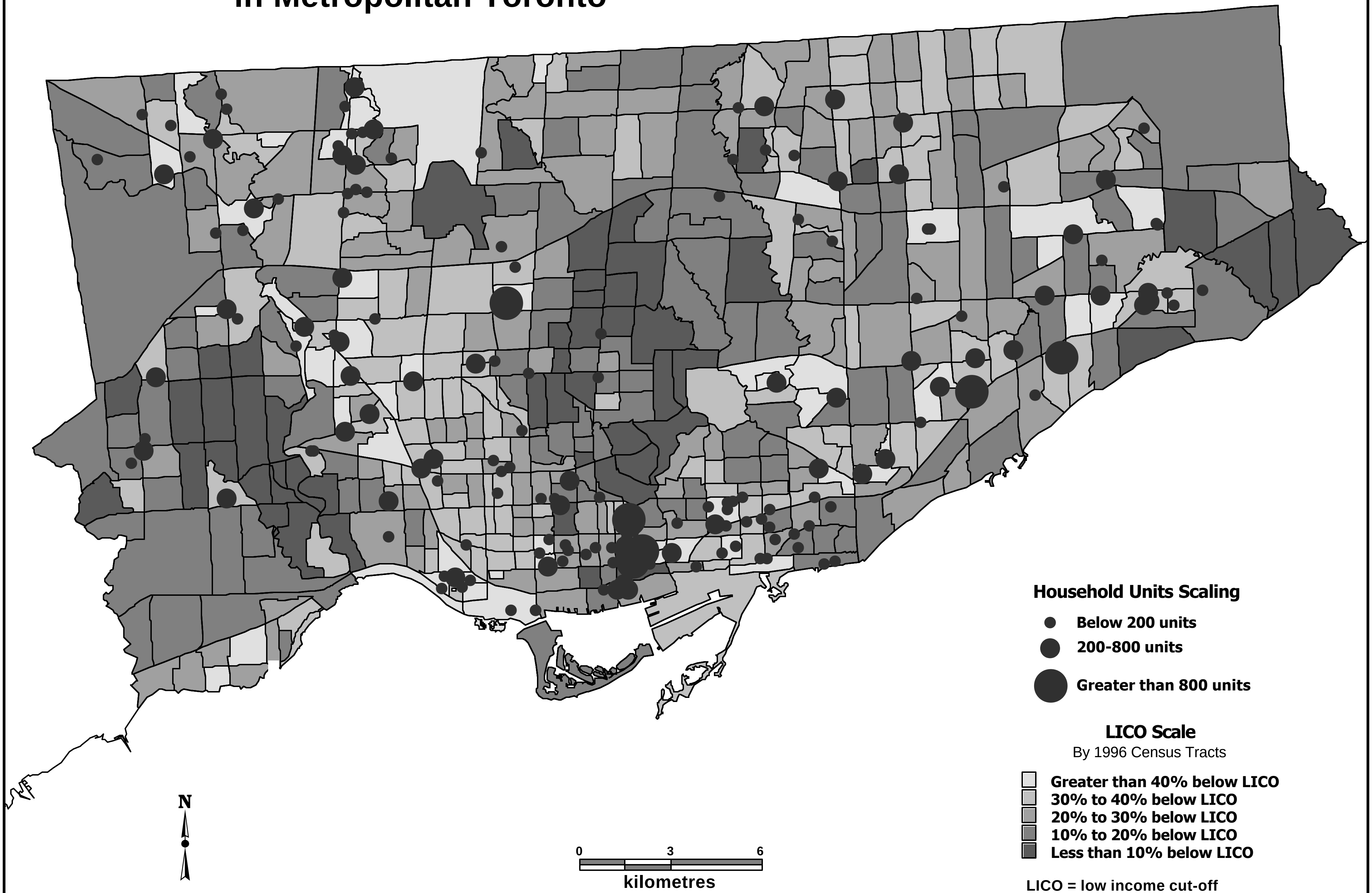
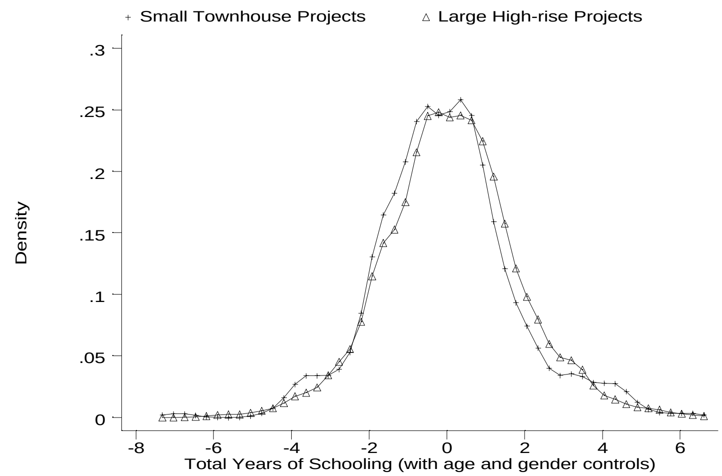
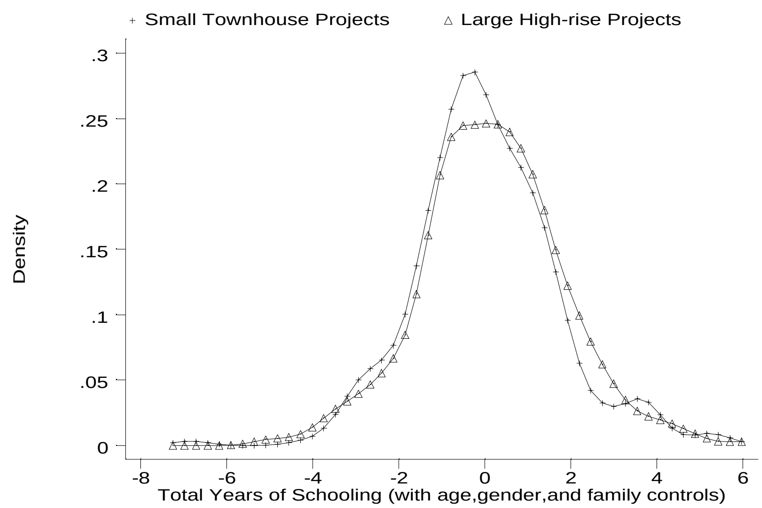


Figure 2
Kernel Densities for Total Years of Schooling
For Smallest and Largest Public Housing Projects

A: Age and Gender Controls: Bandwidth = 0.45



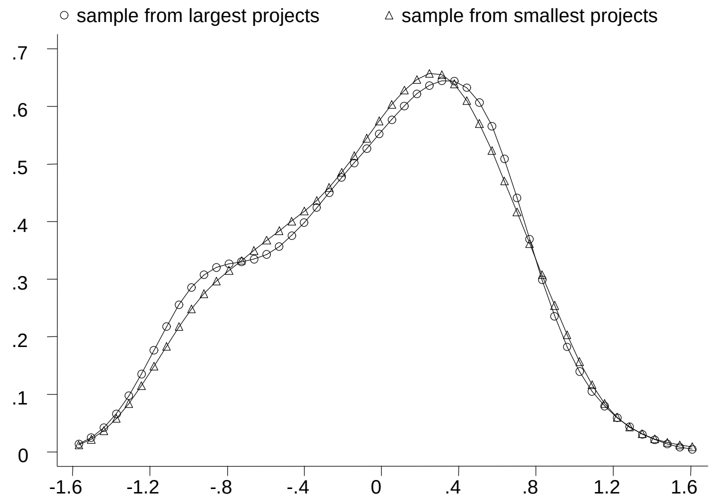
B: Age, Gender, and Family Background Controls: Bandwidth = 0.45



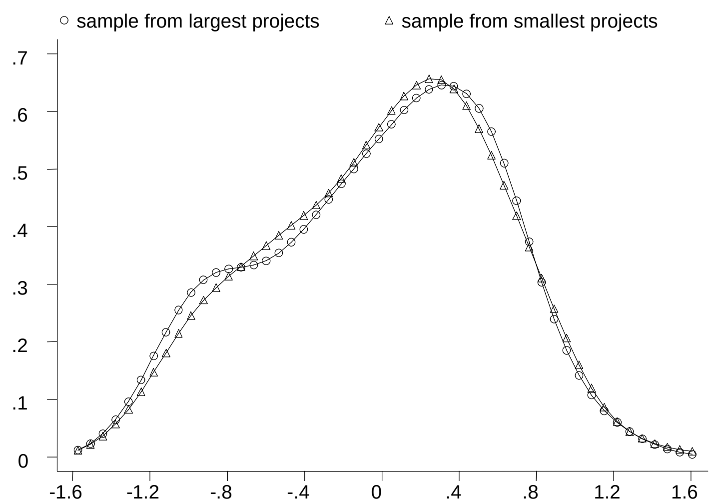
Notes: Residuals generated from regressing total years of education on a full set of age and gender dummies for the sample of youth in the 1996 census living in public housing are used to estimate the two kernel densities overlaid in panel A. The first is for the sample living in the six largest housing projects. The second is for the sample living in small projects, with 250 townhouse units or fewer, and in census tracts with fewer than 25 percent below the LICO. The second panel estimates the residual densities from regressing total years of schooling on age, gender, and a set of family background controls. See text for further details.

Figure 3
Kernel Densities for Log Total Income
For Men from Smallest and Largest Public Housing Projects

A: Age and Gender Controls: Bandwidth = 0.165



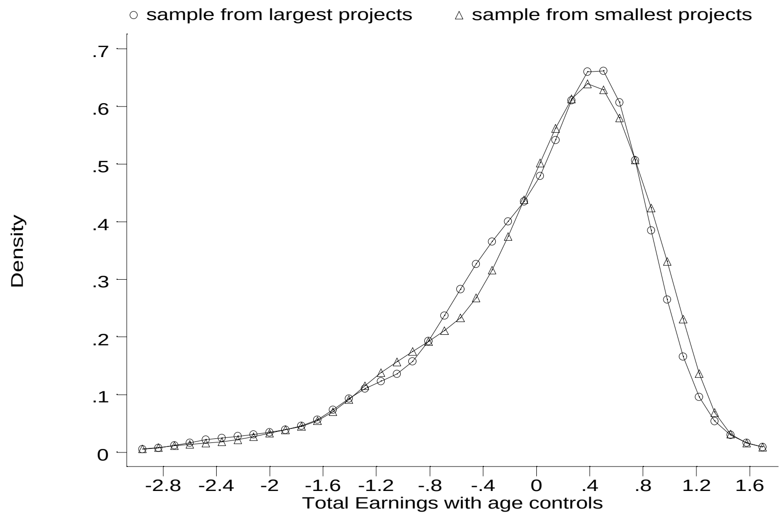
B: Age and Family Background Controls: Bandwidth = 0.165



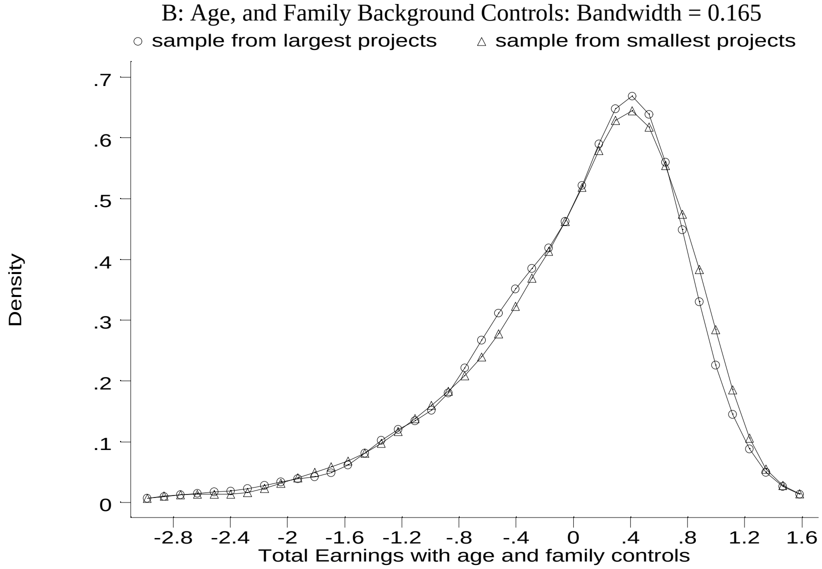
Notes: Residuals generated from regressing average log total income on a full set of age and gender dummies in the IID are used to estimate the two kernel densities overlaid in panel A. The first is for the sample that lived in the six largest housing projects. The second is for the sample that lived in small projects, with 250 townhouse units or fewer, and in census tracts with fewer than 25 percent below the LICO. The second panel estimates the residual densities from regressing log income on age, gender, and a set of family background controls. See text for further details.

Figure 4
Kernel Densities for Log Total Earnings
For Men from Smallest and Largest Public Housing Projects

A: Age and Gender Controls: Bandwidth = 0.165



B: Age, and Family Background Controls: Bandwidth = 0.165



Notes: Residuals generated from regressing average log total earnings on a full set of age and gender dummies in the IID are used to estimate the two kernel densities overlaid in the first panel. The first is for the sample that lived in the six largest housing projects. The second is for the sample that lived in small projects, with 250 townhouse units or less, and in census tracts with less than 25 percent below the LICO. The second panel estimates the residual densities from regressing log earnings on age, gender, and a set of family background controls. See text for further details.