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Research Paper

Do Neighbourhoods Influence Long-term Labour Market Success? A Comparison of Adults Who Grew Up in Different Public Housing Projects

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Abstract

This paper examines whether long-run labour market outcomes depend on residential environment among adults who grew up in subsidized housing in Toronto. The housing program in Toronto provides a full spectrum of neighbourhood quality types to measure outcome differences, and offers a real-life example of large scale neighbourhood quality reform. A primary advantage with this approach is that, conditional on participation in public housing, residential choice is substantially limited. Families that applied for public housing could not specify which project they wished to be housed in and were constrained to what was offered based on availability at the time they applied and by family size. Unlike previous housing mobility experiments, the availability of administrative tax records are used to measure both short and long run outcomes. The results indicate almost no difference in educational attainment, adult earnings, income, and social assistance participation between children from different public housing types. Average outcomes, estimated wage distributions, and outcome correlations among unrelated project neighbours show no significant neighbourhood impact. In contrast, family differences seem to matter a great deal.

Key Words: neighbourhood, public housing, international mobility

JEL classification: H00, I30, O18


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I. Introduction

The substantial levels of income segregation that Wilson (1987), Jargowsky (1997) and Myles et al. (2000) find within metropolitan cities imply that many youth grow up surrounded by very wealthy households while other youth grow up in areas where almost all surrounding households are poor. Division by income and also by race has led many social scientists to wonder whether residents' social and economic outcomes would differ if they could live elsewhere. Yet estimating the degree that neighbourhoods matter has proved problematic. For households in the private market, the option to relocate makes it difficult to completely control for family background and other individual characteristics. For example, when surveys find a few low-income households living in middle-income neighbourhoods (or vice versa), we might expect that these are atypical low-income households.

A primary advantage with analyzing neighbourhood interaction within the context of public housing is that participation in the program limits residential choice. Three previous studies used subsidized housing programs to examine neighbourhood effects in the United States. The well-known Gatreaux program provided assistance to black households who lived in high density public housing projects in Chicago to move to less segregated communities. Rosenbaum et al. (1999), Rosenbaum (1995), and Popkin, at al. (1993), argue whether households were aided in moving to the suburbs or within the central city was random.[1] They found that outcomes of the parents and children were markedly better for those who moved to the less segregated suburbs. 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 that remained in high-density housing projects, randomly selected families that were moved to affluent neighbourhoods experienced increases in overall resident satisfaction, reductions in exposure to crime, and fewer health problems. Initial treatment effects on welfare participation and employment are positive, but considerably smaller than those found from the Gatreaux studies. In another study, Jacob (2000) examines a less extreme experiment, where families living in Chicago housing projects set to close were offered vouchers to relocate. Jacob finds no differences in test scores and dropout rates, comparing children affected by public housing closures to children from other projects.

This paper is the first to examine long-run neighbourhood effects under the subsidized housing program in Toronto. There are many advantages of studying neighbourhood interactions under this program. First, like the previous public housing studies, neighbourhood choice was restricted. Families applying for public housing in Toronto could not specify which project they wished to be housed in, and were constrained to what was offered based on availability at the time they applied, and by family size. The circumstances of families ending up in the smaller, suburban projects were likely similar to the circumstances of families ending up in larger downtown projects. Second, unlike the previous studies, the variety of housing projects throughout Toronto also enables defining of neighbourhood-quality across many categories: some housing projects consist only of high-rise apartments but others are only townhouses; some accommodate more than a thousand low-income families but others provide shelter to less than 50 households; finally some projects are located in central downtown, while others are in middle-income areas in the suburbs. Differences in neighbourhood quality do not correspond with the treatment group moving into a better neighbourhood. In the MTO program and in Jacob's study, treatment families generally were required to move, while control families remained where they were living before. The

[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 neighbourhood, 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.

impact from the treatment group relocating may be difficult to disentangle from the independent impact from the new neighbourhood. Third, the census and longitudinal administrative data used in this paper provides a rare opportunity to examine short and long run labour market impacts from neighbourhood differences, a decade or more after participation in the program.

Despite significant contrast in neighbourhood quality across public housing projects, the main finding of the paper is that neighbourhood quality does not make much difference to a youth's chances for labour market success. Mean differences in education attainment, earnings, income, and social assistance participation between kids from different public housing types are robustly measured close to zero. Similarly, estimates of earnings and wage distributions for youth from the largest and smallest projects are almost exactly the same. In addition to examining differences in means and wage distributions, sibling correlations are compared to unrelated neighbour correlations. This approach, developed by Solon et al. (2000), accounts for unobserved measures of neighbourhood quality and provides an omnibus measure of neighbourhood effects relative to family effects. The correlation approach also finds a near-zero effect on earnings and income while family background accounts for about 30 percent of the total sample variance. The only outcomes found correlated with neighbourhood quality are per household crime occurrences. Security reports of sexual assault, assault causing bodily, and drug offenses were about 2-5 times more likely to occur in the largest downtown projects than in small projects in middle-income neighbourhoods.

The next section, discusses theoretical reasons how social interactions may influence outcomes and how these theories apply to consequences from living in different neighbourhoods. Section three, describes the two empirical approaches to the paper. The data are presented in section four. Section five describes the subsidized housing program in Toronto and the variation in neighbourhood quality across projects. The results are displayed in section six and conclusions are given in section seven.

II. Why might neighbourhoods matter (and why might they not)?

Several theories have been put forward as to why resident location might matter.[2] Table 1 summarizes four main hypothesis. First, perhaps the most intuitive explanation by which neighbourhoods 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 not just based on her own preferences, but on whether her 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 could improve an individual's chances of finding a job, getting parental advice, or a temporary loan. Granovetter (1995), for example, concluded 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 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 neighbourhoods could play a role is conformism. In contrast to peer group effects, conformism models usually posit that individuals mimic neighbours' behavior because they lack enough information to choose on their own (Bikhchandani et al. 1992, Bernheim 1994).

[2] See Jencks and Mayer (1990), Duncan and Raudenbush (1998), Moffitt (2001), and especially Dietz (2000) and Brock and Durlauf (2000) for comprehensive reviews of the literature.

Most of us appreciate instinctively that decisions over education attainment, drug use, and careers are often influenced by others, not just family. But social interaction does not take place within isolation of one's neighbourhood alone. For social interactions to matter at the neighbourhood level, personal contacts must depend on where an individual resides, and neighbour 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:

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

Suppose the decision of education attainment also affects another outcome variable Y_i (for example, permanent income). The payoff function, V, is partly determined by youth's parent background characteristics, X_i, and partly determined by role models; that is, other youth's parent background characteristics, $Z_{-i} = (X_1,...,X_{i-1},X_i,...X_I)$. The term ε_i represents i's preferences that are independent of others.

For exposition, assume Y_i can be expressed in the following reduced form:

$$(2) \qquad Y_i = \gamma X_i + \sum_{j \neq i} \delta_{j,i} z_j + \varepsilon_i .$$

z_j are role model fixed effects from individual j's parent, γ is a vector that captures family effects on Y_i, and $\delta_{j,i}$ weights the interaction effect of individual j's parent on individual i.

The specific neighbourhood effect on i, $\sum_{j \neq i, j \in n} \delta_{j,i} z_j$, is the combined fixed effect of all role models who reside in i's community n. Note that the effect may be different for youth from the same neighbourhood, since role models do not necessarily affect youth in the community the same way.

Suppose there are two neighbourhoods, g and b. We are interested in the expected difference between the two neighbourhood effects. This difference, call it η, may be expressed as:

$$(3) \qquad \eta = \frac{1}{I_g} \sum_{i \in g} \sum_{j \neq i, j \in g} \delta_{j,i} z_j - \frac{1}{I_b} \sum_{i \in b} \sum_{j \neq i, j \in b} \delta_{j,i} z_j$$

where I_n is the number of youth in community n.

The size of η depends on many factors. For η to be large, the $\delta_{j,i}$'s must be large and vary significantly between both neighbourhoods. If a few youth are strongly affected by where they live while the majority

[3] It is helpful to work with a role model theory of social interaction over other hypotheses because it facilitates discussion on neighbourhood effect estimation. As mentioned above, the role model effect is not the only mechansim that by which neighbourhoods could influence individual's choices.

are not, then the expected difference in equation (3) could still be small. The definition of neighbourhoods is also important. Neighbourhood effects at the school-district level may miss the effects of role models formed, say, at weekend hockey practice. Finally, the size of η also depends on how much variation in expected role model characteristics exists between communities. In the context of this paper, variation by neighbourhoods comes from youth living in different sized public housing projects and by vicinity household characteristics.

III. Methodology

I employ two strategies for estimating whether neighbourhood quality affects outcomes for youth who lived in public housing. First, housing projects are dichotomized into neighbourhood quality groups, then mean outcomes between high and low income groups are compared. A second strategy, that does not involve defining neighbourhood quality, estimates the correlation between unrelated neighbours living in the same project and compares this with the correlation between siblings. These estimates measure the portion of the total outcome variance explained by differences in project quality, relative to the portion explained by family differences. I discuss both strategies below.

A. Differences in Means

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

$$(4) \qquad Y_{ip} = \gamma X_{ip} + \eta z_p + \varepsilon_{ip}$$

where X_{ip} is a vector of all family characteristics that influence earnings, z_p is the fixed effect from project p, and ε_{ip} represents unrelated individual factors orthogonal to both family and neighbourhood characteristics. The mean outcome difference between project g and project p is:

$$(5) \qquad \overline{Y}_g - \overline{Y}_p = \alpha(\overline{X}_g - \overline{X}_p) + \eta(z_g - z_p),$$

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, $\eta(z_g - z_p)$. Under random assignment, $\overline{X}_g = \overline{X}_p$. The impact from living in project g versus project p can be estimated directly from the mean outcome difference. Without random assignment, this comparison is biased towards a larger effect on the project in which families that tend to have greater positive influence sort into. Addressing the possibility of biased estimates due to unobservable family background differences across neighbourhoods is one of the largest obstacles that researchers face when conducting neighbourhood effect research. In the next section I address this issue by showing how assignment of projects to families on the waiting list for

subsidized housing was based mostly on availability and household composition. This procedure makes it much more likely that the circumstances of any given family in any given project are similar.[4]

B. Sibling and Neighbour Correlations

A disadvantage with the methodology above is that neighbourhood quality has to be defined in order to dichotomize and compare mean differences between neighbourhood types. But public housing projects differ across many observable and unobservable dimensions, and condensing these dimensions into two or three categories may miss identifying other significant effects. I follow an approach introduced by Solon, Page, and Duncan (2000) that avoids defining neighbourhood quality and instead compares sibling with neighbour correlations.

Let Y_{sfp} be the outcome variable, now indexed for sibling s in family f in project p. Reindexing equation (4) we get:

$$(6) \qquad Y_{sfp} = \gamma X_{sfp} + \eta z_p + \varepsilon_{sfp} .$$

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 as:

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

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

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

Equation (8) emphasizes the fact that siblings have correlated outcomes because they share both family and project influences. We would like to know 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, looking at the covariance among unrelated project neighbours may shed some light on this question. Note that the covariance term between family and project characteristics, $Cov(\gamma X_{fp}, \eta z_p)$, is zero if families are randomly assigned. On the other hand, if families most in distress are more likely to accept their first offer of housing, the covariance term will be positive.

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

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

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 neighbour covariance could be positive because families with similar

[4] Random assignment does not solve the reflection problem, first mentioned by Manski (1992). 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 neighbourhood quality. Even when neighbourhood effects are zero, the correlation between neighbourhood outcomes and neighbourhood quality will be high. This paper avoids the reflection problem by implicitly assuming, at least for some projects, that other neighbourhood characteristics surrounding the projects matter. Next section describes this variation. See Brock and Durlauf (2000) for a lengthier discussion on addressing the reflection problem.

backgrounds might be assigned similar projects. This would occur, for example, if the same ethnic groups tended to end up in the same projects, or if tenants from downtown tended to be different from tenants from the suburbs.

Not only does the neighbour covariance in Y_{sfp} provide an upper bound on the possible influence of both observed and unobserved neighbourhood characteristics; that bound can be tightened by subtracting off measurable parts of the first term reflecting neighbours' similar family backgrounds. The project covariance in earnings attributable to the observable part of family characteristics in γX_{fp} can be subtracted from the overall neighbour 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 $2Cov(\gamma X_{fp,}, \eta z_p)$ were close to zero, we could estimate the proportion of the sibling covariance due to neighbourhood characteristics by dividing equation (9) by equation (8). This measure indicates the relative importance neighbourhoods play compared to family factors. From a policy perspective, we might be more concerned with the size of the project effect. This can be estimated by examining the change in earnings from a one standard deviation shift in overall neighbourhood, or project, quality. Suppose the neighbour earnings covariance, $Var(\eta z_p)$, is estimated to be q percent of the size of the total earnings covariance, $Var(Y_{sfp})$. Then a one standard deviation increase in the latent variable for overall project quality, Z_p, should be expected to increase the standard deviation in earnings by $\sqrt{q}$ percent. Multiplying $\sqrt{q}$ by the earnings standard error gives an estimate of the effect from a one standard deviation increase in project quality on the outcome variable.

Estimation of sibling and neighbour correlations 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 neighbourhood 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, have argued that these buildings did little to improve the urban environment, and may actually have made conditions worse. Property values in neighbourhoods surrounding these older projects are among the lowest in the city, and crime rates are among the highest.[5] Other projects built, however, were smaller in scale and located in more suburban communities. Beginning in 1949 and until the mid 1970s, administration and new construction 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, there are 29,173 households within these projects (1 in 20 family households in metropolitan Toronto). Every

[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).

[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 Native or special needs families.

MTHC household pays rent geared to income. That is, approximately 25-30 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 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. Cityhome 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, 25-60 percent of Cityhome's units are allocated to private renters, mostly low to middle income single 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 exclude these groups from my analysis. There is another reason to exclude them. Since non-profits used separate waiting lists, and often had different acceptance criteria than did MTHC and Cityhome, selective unobserved sorting into these projects could further bias the empirical results.

B. Variation in neighbourhood quality within the program

Figure 1 shows the locations for 160 MTHC and Cityhome family projects built prior to 1986.[10] The map divides the city of Metropolitan Toronto, with a population of 2.4 million in 1996, into census tracts categorized by the percent of households within a tract with family incomes below Statistics Canada's Low-Income Cut-Off (LICO). The projects cover a large range of neighbourhoods downtown, and in the suburbs. The seven of the largest downtown developments together accommodate about 25 percent of all subsidized families, and most 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. In total, about 20 percent of MTHC and Cityhome family households live in 72 projects with less than 250 units.

Columns 1 and 2 of Table 2 present mean census tract characteristics for these two groups of projects: the largest seven in the central city, and 42 projects with less than 250 units and in census tracts with less than 25 percent of households below the low-income-cut-off. These two groups will be examined later when comparing outcomes of youth from different projects. The comparison between the two groups arguably provides the most contrast between residential quality within the program, without reducing the sample to an unworkable level. The smaller projects are located in middle-income census-tracts, mostly in the suburbs. Median household income in census tracts around the smaller projects is more than twice as

[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 underlie the shift in the United States policy in the 1980s from providing public housing to providing vouchers for mobility programs. The basic premise of theories of neighbourhood interaction is that, if children's reference groups or role models differ systematically by neighbourhood, then career choice, school attainment, and other critical decisions may also differ, depending on the neighbourhood lived (Duncan and Raudenbush 1998).

[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.

[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 looks at children who entered social housing before this period. The other 50 projects built after were mainly Cityhome projects. These projects are included for my analysis with the 1996 Census.

large as that for the largest city-core projects. Forty-nine percent of households around the largest projects are below the LICO, while only fifteen percent are below the LICO in the census tracts around the smaller projects. Households around the larger projects are more likely to be black, female headed, on social assistance, and less educated than households from the smaller projects. Almost all households around the largest projects rent their home, while 41 percent of households around the smaller projects own their own home.

The variation in neighbourhoods within the public housing program is certainly narrower than variation across the entire city. There are no housing projects in the most affluent areas in the city. The mean percentage of households living below the LICO in census tracts around the set of small projects listed in column 2 is 15.6 percent. For the city as a whole, the median household lives in a census tract with 12.7 percent of households below the LICO. Thus, the largest contrast in neighbourhood quality obtainable within the public housing program is between youth who grew up in the poorest areas in the city to youth who grew up in moderately low to middle-income neighbourhoods. A contrast between the poorest and wealthiest areas is not possible within the program, but nor is this contrast very interesting, since relocation policies are not likely to place low income families into affluent neighbourhoods on a large scale.

Do families who live in the largest Toronto public housing projects live in similar conditions to families from the largest housing projects in other large U.S. cities? Table 2 examines mean census tract characteristics among participants of the Moving to Opportunity Program in Boston and Chicago. 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 census tracts moved into by participants receiving Section 8 vouchers to relocate.[11] Column 5 displays mean differences of tract characteristics for participants who moved to census tracts with less than 10 percent below the U.S. poverty line (the experiment group). Columns 6-8 show similar comparisons for the MTO program in Chicago. The data for Boston is from Katz, Kling, and Liebman (2001), Table 4. Data for Chicago is from Rosenbaum et al. (1999), Table 1.

In terms of the percentage change in neighbourhood characteristics, the neighbourhood variation between the Toronto public housing census tracts is at least as great as the variation between households from large projects in Boston and Chicago and households who moved using Section 8 vouchers. The percentage variation is about the same compared to Boston households in the experiment group and in the control group. The variation between experiment and control groups in Chicago is somewhat larger. For example, 68.5 percent fewer households in Toronto census tracts around the smaller projects receive social assistance than households from tracts around the largest downtown projects. In Boston, social assistance participation was 44.9 percent less in the Section 8 census tracts than in the control tracts, and 70.8 percent less in tracts for those from the experiment group. The percentage difference in households on social assistance for Chicago, between the control and experimental group is 82.6 percent.

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

[11] In Katz et al. (2001), 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.

99.3 percent respectively. The overall picture painted from this table is that the neighbourhood quality variation within the Toronto housing program is considerably large, however, the characteristics of households in and around the largest projects are not as bad as those for large U.S. cities like 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, social assistance participation, overcrowding, and whether living in emergency housing. Those with the most points were housed first, giving preference to families most in distress. Applicants could specify up to 7 regions in the city where they wished to be located. The fewer regions willing to live in, the longer a family waited for an offer. Applicants who rejected their first two offers were removed from the waiting list.[12] 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 for housing, and often did not want to live in larger projects that had negative stigma associated with them. If these more selective 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 will likely be 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 neighbourhoods 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. Initial applicants to Cityhome's subsidized units came from MTHC's waiting list. New applicants applied to Cityhome directly, although they were also encouraged to apply to MTHC. As with MTHC, applicants could not specify a project they wished to live in, but could request a particular region in 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 public housing households in the mid-1990s. The annual turnover rate of units for Ontario was about 13.5 percent, similar to the turnover rate in Toronto's private market. Of the sample of leavers, 69.0 percent had lived in public housing for less than 5 years, while only 28.7 of my 1996 Census sample of household heads in public housing moved in the last 5 years. This suggests the hazard rate for leaving public housing falls substantially the longer a family remains in the program.[13] The main reasons given for leaving public housing were relocation for employment, improved financial situation, and change in marital status. Notable for this study, 29.5 percent of the sample of leavers said there were other reasons for leaving than those listed. Fewer than half of these respondents actually specified their reasons, but 'trouble with neighbours' was among those mentioned most often. Therefore, while the option to move projects because of poor neighbourhood environment was restricted, some families may have been willing to leave the program entirely to get away from their neighbours. Given the

[12] There were occasional exceptions to this rule. The staff member handling an applicant's case could allow a second refusal if, in their discretion, they felt it justified.

[13] A caution with interpreting this result is that my Census sample includes only households with children aged 16-25 still living at home, while the Ekos survey is representative of all public housing occupants.

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 three datasets. The first is a record of public housing addresses and project characteristics that are matched with the other two datasets to determine which households receive rent subsidies. 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.

Instead of relying on small survey samples that identify whether a family or household participates in a public housing program, I take a different approach of matching public housing postal code addresses to micro data. 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. Postal codes in Canada are comprised of six alpha-numeric digits, and identify very specific geographic locations. Each code generally refers to one side of a city street, or an apartment building with more than four stories. Approximately three-fourths of my population sample are located in public housing addresses with unique postal codes. Some smaller public housing dwellings, however, may have near-by households not in public housing but with the same postal code. Another difficulty is picking out those in Cityhome projects receiving subsidies: some families living in Cityhome projects pay private market rent. Families not participating in subsidized housing programs are more likely sorting across different public housing project neighbourhoods, with those unable to relocate to more pleasant environments locating in the worst city neighbourhoods, and those with (perhaps unobservable) higher incomes locating in the better neighbourhoods. 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 construct three samples. Sample 1 includes only the population from postal codes that match uniquely to MTHC developments. Every household in this sample receives rent-geared-to-income. Of the 544 postal code addresses, 317 are uniquely identified. Most of the family public housing stock is contained within this sample. Omitting the other 237 codes removes more dwellings from large than small projects, but most of the neighbourhood quality variance remains. In Sample 2, I include only households with single mothers receiving social assistance.[14] As described below, more than half of all families in subsidized housing fall into this category. Sample 3 comes from estimating a probit model on the probability of living in subsidized housing based on several observable characteristics.[15] I use the sample of households living in census tracts with public housing, but not living in addresses with 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

[14] Sample 2 family heads in the administrative data are single mothers who received social assistance 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 includes social assistance in 1996.

[15] For the administrative data, the independent variables used are 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 social assistance, and years living in public housing postal code between 1978 and 1990. The probit model with the census used household head's age, child's age, race indicators, household head's education attainment, total family income, whether on social assistance, marital status, family size indicators, immigrant status, and whether moved in the last 5 years. See the appendix for more details.

housing postal codes. As discussed in the appendix, Sample 3 includes all households whose estimated probability for living in public housing is greater than 0.3 in the administrative dataset, and 0.15 in the census. These cut-offs generated samples with high relative portions of actual public housing families in them, without reducing the total size to an unmanageable level.

The postal codes are matched to households in the 1996 census. The cross-sectional nature of the census limits the analysis to possible neighbourhood interactions on outcome variables for children while still living at home. I therefore restrict the public housing samples to all youth aged 16-25 living with at least one parent. Table 3 displays mean characteristics of families and children in these samples, compared to mean characteristics for the city population. Not surprisingly, monthly rent reported by households in public housing is much smaller than the average monthly rent among all city renters. Average household income for families in public housing is about 40 percent the average household income for the city. Less than 35 percent of household heads in public housing work full time, and a high percentage are comprised of single mothers (62 percent in Sample 1). A large notable difference between Toronto and other U.S. cities like Chicago and New York is the much smaller percentage of blacks. Only seven percent of Toronto's family household population are black, and about 43 percent in public housing are black. This contrasts to the U.S. neighbourhood studies using public housing, where about 95 percent of individuals in the sample were black.

The postal codes from projects built before 1985 are also matched to families in the Intergenerational Income Database. The IID includes the full sample of Canadian 16-19 year olds who filed taxes in 1982, 1984, and 1986 while still living at home, a population size over 700,000 children.[16] By 1998, these tax filers were between 28-35 years old. Mothers and fathers are linked to these youth in the year the child first filed.[17] 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 is taken from the child's tax file. If the child did not file, the postal code from the father's tax file is used if both parents report they are married, or if the mother's file is missing that year. Otherwise, the mother's postal code is 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 are kept. If neighbourhood influences are cumulative, then two years in a project may not be enough to be affected by neighbourhood environment, so I also I check whether longer-term stays have a greater influence on outcomes in section V.

The youth's adult earnings and income are averaged over a 6-year period between 1993 and 1998. Social Assistance 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 is computed as mother 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 .8, and each child receiving a weight of .3. Following the approach of Solon (1992) and Mazumder (2000), family background variables are averaged over time to reduce measurement error. Parental income is averaged over fifteen years, between 1978-92. All dollar amounts are converted to 1992 Canadian dollars using Statistics Canada's Consumer Price Index.

[16] Some children are identified in more than one year. For these cases, only one match was used.

[17] The parents are not always the biological parents, may include step parents or other caregivers. See Corak and Heisz (1999) for more details.

The IID requires that the dependent child file an income tax return while living at home. Therefore, the IID under-represents youth who had no attachment to the labour market during their teenage years, who left home before establishing an attachment, or who participated in the underground economy without reporting income activity. Unfortunately, all three cases seem quite likely to arise for the population of families living in public housing. Hence, there may be important project or neighbourhood effects that are missed from the analysis if worse outcomes are associated with non-tax filers, and the likelihood of filing is a function of the public housing project assigned. One approach to check for this possibility is to examine whether differences exist by neighbourhood quality and the average number of years an individual did not file. The relationship between neighbourhood quality and never filing when young may be similar to the relationship between neighbourhood quality and filing only once or twice when young. I use the total number of times not filing for individuals in the sample to examine whether such an association exists. Another approach to dealing with missing youth in the IID is to reweight 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 for more details). This, however, will only ameliorate the non-reporting problem to the extent that outcomes for youth observed in the IID are similar to those who are excluded, along these demographic categories.

The possibility that neighbourhood quality affects the chances of filing when young cannot be completely ruled out. This is one reason results from the census are reported. While restricted to outcomes for youth 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 whether results from two substantially different datasets lead to similar findings. If the results indicate no neighbourhood 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-tax filers prevents us from identifying long-run effects in the IID. If the results indicate no neighbourhood influences on outcomes both in the IID and in the census, stronger interpretations from both datasets can be made.

Table 4 covers mean characteristics in the IID samples. A similar pattern holds compared with the census. More than 60 percent of the public housing family samples are comprised of female heads. More than 50 percent of household heads in the sample received social assistance sometime between 1993 and 1998. Only 18 percent of the total city sample is female headed, and 13 percent receive social assistance. The IID sample is larger than the census sample because the longitudinal nature of the IID allows identification of youth who once lived in public housing, but may have subsequently moved out.

VI. Results

A. Observable Sorting between projects

I begin by comparing households from two neighbourhood quality groups: those from the seven largest central-city projects, and those from projects with less than 250 units in size and in census tracts with less than 25 percent of households below the LICO. To keep the number of observations as large as possible, all three matched samples are combined for this subsection. Table 2 described the large variation in neighbourhood quality between these two groups. Table 5 presents mean characteristics of households living in these projects. In most cases, I chose characteristics, such as household head's education and race, which are not likely themselves to be affected by current neighbourhood background. If sorting between groups is minimal, we should expect to see little difference in means between the two

neighbourhood quality types. Columns 1 and 2 show mean characteristics for all households, while columns 3 and 4 show means for households with children.

The mean household head characteristics between the largest and smallest projects are similar, but not all the same. Single parent households, immigrants, age of head, and number of kids are distributed in similar proportions among large and small projects. Some small differences exist in the percent of heads that are black or receiving social assistance. Median incomes in both the census and the IID samples are about $2,000 more for the smaller projects than for the large central-city projects. There are also differences in mean education attainment. Forty-three percent of public housing family heads in the large downtown projects are without a high school degree, compared to forty-seven percent of heads in the smaller projects. Thus, families more in need, on average, seem to end up in the larger projects. This finding corresponds with the application process. The mean differences for household heads are not particularly large, which makes sense considering the restrictions placed on project choice at the time of application. Poorer and less educated family heads are more likely to live in the larger projects, which collaborates the story that more needy families were more likely to accept their first offer for housing. With randomization across projects not assured, significant differences in outcomes across projects may not entirely be due to neighbourhood effects. The bias seems likely to be small and upward.

B. Differences in Means

Table 6 compares outcome means for youth from the larger central city projects with outcome means for youth from the smaller projects with fewer than 250 units in census tracts less than 25 percent of households below the LICO. The schooling outcomes from the census data are no better for the small density projects than for the larger projects: 15.5 percent of youth from the largest projects attained more than a high school degree, while 15.6 percent of the small townhouse sample had more than a high school degree. To control for observable differences by project, column 3 displays the dummy coefficient for the smaller project after regressing the outcome variable on a complete set of indicator variables for age and sex, a variable for parental permanent income, social assistance receipt, marital status, race, and immigrant status. The difference between projects, after controlling for demographic and family background factors, remains virtually unchanged. This is reassuring, since unbiased estimates of the neighbourhood effect under random assignment should not change with additional controls. Insignificant differences between large and small projects also results when looking at years of education, less than high school degree, and whether not working and not going to school. All estimates are measured precisely.

The bottom of Table 6 focuses on longer-term outcomes from the IID. The raw mean for whether a youth from a large downtown project received social assistance for at least one year during the 1993-98 period is 31.9 percent. For the smaller projects, the mean was 29.1 percent [the difference is not significant (p-value>0.1)]. While fewer youth receive SA when older coming from smaller projects, adding family background controls further reduces the estimated effect from the neighbourhood quality difference to –1.5 percent. The small differences between project types for social assistance 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 1993-98 than did boys from the large downtown projects. Mean annual earnings for men from public housing projects does not differ between project types, whether family background controls are included or not. This suggests the small differences in total income are entirely attributable to differences in social assistance participation between the project types. It seems likely that the estimated effect size could be reduced further with more controls available, such as parents' social assistance dependency, part-time or full-time status, occupation, and parenting skills. Even without including these additional controls, the estimated effect on education attainment and labour market outcomes from living in a different project type is quite small.

As discussed in section IV, a concern arises from the IID that the likelihood of not filing a tax form, and therefore not being captured in the IID, may be influenced by neighbourhood conditions. The last row from Table 6 displays mean differences in the number of years an individual filed taxes. For those who filed at least once while aged 16 to 19, there does not seem to be a large difference between chances of filing and having lived in a small or large project. The mean number of times not filing for adults from the largest projects is 2.3, and the mean number for adults from the largest projects is 2.4. These results, and similar findings between the census and the IID 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 at the project rather than micro level. As such, I cannot determine the characteristics of the victim or the person who committed the crime. The seven largest projects in downtown have the greatest incidences of arson, bodily and sexual assault, drug offenses, neighbour disputes, and sudden deaths per 1,000 households. Assaults are more than twice as likely to occur in the larger downtown projects. Sudden deaths are more than four times as likely. These differences are similar when mean project characteristics, such as percentage of households receiving social assistance, are included as controls.

The general pattern revealed from the table is higher crime occurrences in projects with greater concentrations of poverty. We cannot interpret these results to mean poor neighbourhoods led to higher crime occurrences, but nor can we rule this possibility out. Since households were not entirely assigned at random to projects, I refrain from implying causation. The results are highly suggestive, however, and I shall return to them in my discussion in section VI.

Tables 8 and 9 present a similar analysis of differences in means between alternative project quality types. I contrast 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 high-rises (more than 5 stories) or all townhouses. The first part of the Table 8 dichotomizes between large, medium-sized and small projects. From column 1, the mean completed years of schooling for youth aged 16-25 in projects with less than 150 household units is 12.4. The mean for youth living in projects with 150-700 units is 12.2, and the mean for youth living in projects with 700 units or more is 12.4. The differences are very small, and the null hypothesis that the means are the same cannot be rejected (F-test = 0.42). The other schooling outcome variables – percentage of youth with less than a high school degree and percentage of youth with more than a high school degree – also are not significantly different across small and large projects. Turning to column 4, the percentage of youth not working and not going to school in small, medium, and large projects is 16.1, 16.0, and 16.1 respectively. Table 9 offers longer term outcomes of individuals from the Intergenerational Income Database. Column 1 shows that 32 percent of youth from both small and large projects received social assistance 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. Compared to the smaller projects, average men's total income is about 1.6 percent more than those from the largest projects. Average total earnings between these groups are almost identical.

The next set of rows in tables 8 and 9 categorize public housing projects by whether they are MTHC or Cityhome managed. MTHC projects are older, usually larger, and all residents within receive subsidized rent. Cityhome buildings are smaller, and mix subsidized tenants with tenants paying market rent. Even without controlling for observable characteristics, the estimated mean outcomes are not significantly different. It is worth pointing out that all estimates are precisely measured. 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.

Table 8 also classifies projects by conditions within the surrounding census tract. The mean total years of schooling for youth in public housing and living in a census tract with less than 15 percent of households below the Low Income Cut-off, is 12.1 years. Comparatively, the mean years of schooling for those living in census tracts with more than 40 percent above the LICO is 12.3 years (not significantly different from 12.1). The information from the Intergenerational Income Database in table 9—average total earnings, total income, and social assistance participation—are very similar over these categories. For example, men in the IID from census tracts with less than 15 percent of households with incomes below the LICO earned, on average, about $18,800 between 1993 and 1998, while men in census tracts with more than 40 percent households above the LICO earned about 2 percent less. The direction of the earnings and income differences are usually what would be expected if neighbourhood influences mattered. However, the differences are mostly between 0 and 2 percent, and this could be explained by not having complete random assignment in the data.

We might expect differences to arise from whether youth lived in 5 or more story high-rises or townhouse complexes. Townhouses allow for more space between neighbours, and a front door that leads directly outside, rather than to a hallway corridor and a building elevator. Families are more likely to avoid contact with other tenants if they lived in a townhouse. Table 7, however, indicates no substantial differences in schooling and job outcomes between these dwelling types. I consistently find no indication of significant differences for the outcome variables examined.

The information in the second panels of Table 8 and 9 check whether the results change by controlling for observable individual characteristics. Outcome variables are regressed on neighbourhood quality dummies, and other background controls. For the IID, control variables include age, gender, average parental income, parental marital status, whether parents receive social assistance, and family size. The census results include these controls, plus indicators for whether black or recent immigrant. The estimated raw means for the base (control) projects are presented again for reference. The estimated difference between these means and the mean outcomes for the alternative project types, holding constant the other variables, are the coefficients on the alternative project dummy variables. For a binary outcome, a probit model is used, and the coefficient shown can be interpreted as the estimated change in probability if an individual lived in another project type.

The figures in column 6 of table 8 show differences in total years of schooling by project type. After adjusting for age, gender, and observable family background, youth aged 16-25 living in projects with 700 units or more are estimated to have 0.1 less years of education than those living in projects with less than 150 units. For all the outcome variables in the census and the IID, I find no significant difference between these project types. All point estimates for the differences in means are small and close to zero. Without going through the entire table, the results are robust in showing no discernable effect on the schooling and labour market outcome variables from differences in neighbourhood quality.

While labour market outcomes vary little between different categorizations of neighbourhood 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 neighbourhood

and project quality than just comparing means. I estimate the distributions for years of schooling, log income, and log earnings and graph them for the two large and small project types discussed above. Figure 2 shows the kernel density estimate of total years of schooling for individuals in the census from projects with less than 250 units and in census tracts with less 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.

In the top half of Figure 2, the densities were estimated 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 density estimates are remarkably similar. In the bottom half of Figure 2, the densities were estimated after regressing on age, gender, and family background controls. As shown, 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 observables, that persons from the largest housing projects have slightly more years of education, on average, than those from the smaller projects. This corresponds with the results in Table 6 and Table 7. Neither of these differences is large enough to constitute any significant effect.

In Figure 3, kernel densities between the same project quality types are graphed from residuals of log total income, for males only. Both distributions spike near the left tail, corresponding to those in the sample with only social assistance income. There are slightly more persons 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 those with social assistance earn little additional income. Whether family controls are added or not, the densities between those from the largest projects and those from the smallest 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 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 1-5, and similar regressions for years on social assistance in columns 6-10. To keep the sample large, project quality is dichotomized between those projects in census tracts with 35 percent of households below the LICO, and those in census tracts with 35 percent above. 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 two adds indicator variables for whether entered public housing at age 10-13, or 14-16. The omitted indicator variable is entered before age 10 in a project with less than 35 percent in the census tract below the LICO. The coefficient for those who entered public housing between ages 10-13 is 0.05, indicating slightly better income performance than those who entered earlier. The estimate on log income for those who entered in ages 14-16 is 0.01. In column 3, project quality and age entered are interacted. For children who entered public housing before age 10, the coefficient estimate on the effect from living in a lower quality project is -.01. For those entering after age 13, the coefficient on lower neighbourhood quality is positive, but measured imprecisely.

For neighbourhood quality interacted with years lived in public housing, there also appears to be little difference for the subgroup who lived in public housing the longest. In column 5, males who lived in lower quality public housing for at least 11 years are estimated to earn 2 percent less than those who lived in higher quality projects for at least 11 years. Men who lived in low quality projects for 5-10 years are estimated to earn 1 percent more than those from better quality projects. And no project effect was found for men who spent less than 5 years in the program. The effect estimates for youth who entered earlier or spent more time in the projects are not very large and unobservable differences between long term residents in low quality projects and residents in better projects seem more likely than for the entire sample.

The results when estimating years on social assistance are similar. The larger sample includes both men and women and improves the precision of the effect estimates. The predicted effect on the number of years receiving social assistance for youth who entered public housing before age 10 and are assigned a project in a more affluent census tract is zero. Children who lived in public housing for more than 10 years are predicted to spend, on average, 0.01 more years receiving social assistance (over the 6 year period) if they were assigned a lower quality project.

E. Sibling and Neighbour Covariances

These results 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 dichotomizing the sample. This subsection presents correlations between unrelated neighbours, that identify both observable and unobservable neighbourhood characteristics by project.

i. Income

Table 12 presents the estimates of adult annual income covariances between brothers and neighbouring boys. I use the residuals from regressing log income on boy's age and age squared in 1998 to estimate variances and covariances for my city and public housing samples.[18] In the noted cases when I also control for family characteristics, I generate residuals from regressing log income on age, age squared, average parental income, marital status of household head, and whether a parent received social assistance between 1992-1998.

I begin with the full city sample, which provides a useful comparison to the public housing samples. I measure the 'residualized' city variance of log income at 0.335. The corresponding brother covariance is 0.101. Dividing the brother covariance by the city variance gives an estimate for the city-wide income correlation of 0.300.[19] The brother correlation provides an omnibus measure of the proportion of income variation attributable to all family and community background characteristics shared by brothers. Page and Solon (1999) estimate a similar value, 0.316, for the earnings correlation between brothers in the U.S.[20] Interestingly, when I control for observable family characteristics the brother correlation falls only

[18] For exposition, I sometimes refer to the log income covariance as just the income covariance.

[19] The variance is based on all families with boys in the sample, whereas the brother covariances is based on families that contribute at least two brothers to the sample. Measuring the variance among families with at least two brothers does not change the estimate very much. This is true also with the public housing samples.

[20] Caution must be taken with comparing city-wide to nation-wide correlations. Page and Solon find their brother earnings correlation drops to .186 after controlling for urban city and region. There have been no previous studies estimating sibling earnings correlations in Canada.

a little, to 0.241. This means my family background controls do a very poor job at explaining the similarities between brothers' earnings.

Turning to the earnings correlation estimate for boys from the same enumeration area, the correlation in age-only adjusted income is 0.046. Despite the expected similarity between neighbours in the same EA, the correlation is more than 6 times smaller than the brother correlation. When I use the family-background-adjusted residuals, the correlation falls to .013. So it seems much of the EA neighbour income correlation can be explained by a small set of family background observable characteristics, mainly parental income. These results hold when looking at the subset of families who never moved between 1978 and when the son was 19 years of age. I measure the age-only adjusted neighbour correlation for non-movers at 0.047 and the family-background-adjusted correlation at 0.007.

Income variation across small neighbourhoods explains very little of the total city variation and the large correlation between brothers. But what about the actual size effect from living in neighbourhoods? The neighbourhood characteristics that might matter in influencing child behavior are unknown with this framework. Yet we can approximate the influence from a one standard deviation increase in the latent variable that includes all observable and unobservable relevant characteristics. If the neighbour covariance, after adjusting for observable family background characteristics, is 0.004 (the estimate with census tract non-movers), then a one standard deviation increase in neighbourhood quality increases the standard deviation in income by $\sqrt{0.004} = 6.3$ percent. The standard deviation in income for this sample is $\sqrt{0.335} = 0.579$, so the estimated effect from a one standard deviation increase in neighbourhood quality is a 3.5 percent increase in (log) annual income. This estimate would likely be lower if we could factor in all additional family background influences that are not already accounted for by my controls. The result that neighbourhoods seem to play little role at the city level on adult incomes is precisely that found by Page and Solon (1999) for the U.S.

I now move to the public housing samples. The income variance is larger for all three samples compared to the city variance. The finding seems surprising, at first, because the boys come from more similar backgrounds than the city sample. We might expect low-income outcomes for sons from low-income families. While Table 3 shows this is true, on average, 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). The weak association leads to a wider variation in later labour 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 after controlling for parental income and parent's marital status.

I estimate a small, and sometimes negative income covariance between boys from same public housing projects. For the age-only adjusted neighbour covariance across projects, all three estimates are much smaller than the city covariance estimate for neighbours within EAs. After controlling for family background observable characteristics, the measured covariances don't change much compared to the city sample. We should expect this from looking at Table 4. The level of sorting across projects is much lower than 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 centered around zero. Using the bootstrapped standard errors, I cannot reject the hypothesis that all estimated neighbour 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 neighbours. The bottom section of Table 12 defines neighbours as living in the same census tracts, instead of the same project. The point estimates for the neighbour covariances, after controlling for family background observable, are approximately zero for all three samples.

ii. Earnings

For the earnings covariance estimates, presented in Table 13, the results are similar. The measured earnings variances are considerably higher than the income variances from the previous table. The exclusion of social assistance income from earnings may explain the higher estimates. About 40 percent of sons from the public housing samples received social assistance at least once between 1992 and 1996 (compared to 15 percent for the entire city sample). The earnings variance between SA participants and non-participants is higher than the earnings variance for non-participants alone. The earnings covariance estimates for brothers in public housing are much lower than the income covariance estimates. For Sample 2 containing brothers with single mother on SA, the earnings correlation is 0.070. This compares to .244 for the city sample. The smaller measures might again be due to the exclusion of SA income.

The point estimates for the neighbour covariances are close to zero. 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. The small covariances do not rule out potentially significant effects. With the higher earnings variance, a standard deviation change from a one standard deviation increase in neighbourhood quality will also be higher. For example, the neighbour covariance for public housing boys from the same census tract, living there for at least 5 years, is only 0.005. Yet this implies a 7.1 percentage increase to the 0.77 standard deviation in earnings, or a 5.5 percent increase in overall earnings. A policy to improve public housing quality by one standard deviation is entirely feasible. Indeed, a shift from the higher density projects to smaller ones could constitute more that a standard deviation change. On the other hand, none of the public housing neighbour covariance estimates are significantly different from zero. The back-of-the-envelope approach for estimating effect sizes using neighbourhood covariances does an imprecise job when the sample variance is high. To glean more on whether public housing effects on earnings, I look at mean earnings by project below.

iii. Years on Social Assistance

Many sons in my public housing samples receive social assistance when they are older. This subsection looks at whether SA participation varies by project. Table 14 shows the estimated covariances between brothers and boys in same projects for number of years receiving SA between 1992 and 1998.[21] The residuals from regressing on age and age squared are used 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 an estimated 0.199 of the total variance in years on SA.

Despite the higher variance among the public housing samples, the brother correlations in years on SA are quite similar. The brother correlation is 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 neighbours, however, are negative. None of these estimates are significantly different from zero, suggesting

[21] 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.

neighbourhood variation across public housing projects plays little role in determining SA participation or number of years on SA.

iv. Years of Education

A problem with the IID data, from which the previous covariances were estimated, is the omission of children who had no attachment to the labour force when they were teenagers, or who left home before beginning to file taxes. The IID may miss important differences by project for children who never filed while living at home. As a partial check, I estimate brother and neighbour correlations with an entirely different dataset, the 1996 Census. The Census is not subject to the same types of selection bias that the IID has, and offers more family background variables to control for. However, I can only look at children who were living at home with their parent(s) in 1996. I restrict my analysis to boys and girls between the ages of 16 and 25 (both genders are included to increase the sample size). The outcome variable I look at from the Census is total years of education, which includes the number of elementary and secondary years of education, and the number of years of community college, or university. Of course, many of the children in this sample have not completed their schooling. Only differences in education within young age groups are captured. I estimate sibling and neighbour correlations after regressing years of schooling on a full set of age and gender dummies.[22] For family controls, I use total income, family size, race dummies, whether born outside the country, and whether family is below the low-income cut-off. Table 15 displays the results.

I include the estimated sibling covariances only to compare with the neighbour covariances. If the sibling covariances were small, we would be less confident in interpreting the neighbour covariances estimated with the Census. The sibling covariances are calculated only between young sibling pairs both living at home, who have not necessarily finished their schooling. The sibling education correlation for the city is estimated at 0.380, surprisingly high given the caveats above. 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 0.033 once observable family background controls are included. The adjusted neighbour correlation is less than one-tenth the sibling correlation. Using this estimate, a standard deviation increase in neighbourhood quality would increase education by 0.69 years. Solon et al. similarly estimate a 0.70 increase in years education from a standard deviation increase in U.S. neighbourhood quality. When we look at the public housing estimates, the neighbour correlation estimates are smaller, even negative in some cases. To get an idea of the effect size from a change in neighbourhood quality among children in public housing, take, for example, the 0.012 covariance estimate for children in Sample 2 with tenure more than 5 years (one of the higher estimates). The implied effect from a standard deviation increase in project quality is an increase of 0.176 years of education.

In general the pattern that emerges from the Census estimates is the same from the IID. First, the variation in outcomes attributable to differences across public housing projects explains virtually none of the total variation, while differences across families explain between 20 and 30 percent of the sample variance. These findings hold whether defining neighbours within projects, or grouping projects within census tracts. They also hold when restricting the sample to individuals who lived in a project during all their teenage years. Second, rough imputations for the effect on outcomes from a standard deviation rise in

[22] The results did not change when I controlled for age with a linear and quadratic term, instead of controlling by single age category.

neighbourhood quality suggest little role that neighbourhoods have in influencing long run economic success.

VII. Discussion

In this paper, I use variation in characteristics across public housing projects in Toronto to examine the relative importance neighbourhoods play in influencing labour market outcomes among adults from low-income family backgrounds. The advantage with using a sample of public housing participants is that the nature of the application process prevents much selection across neighbourhood types. Consequently, upper bound estimates for neighbourhood effects within public housing are likely closer to true estimates 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 labour market outcomes, a decade or more after program participation. The study also explores variation between several definitions of neighbourhood quality, without relying on a treatment group to move.

The key finding from the analysis is that average education attainment, annual earnings, income, and social assistance participation among youth from low-income families do not differ by the degree of low-income concentration in the neighbourhood that the youth grew up in. I find no advantages to low-income family kids living in middle-income neighbourhoods in the suburbs, and no disadvantages to low-income kids living in the lowest-income neighbourhoods in downtown Toronto. These results hold whether contrasting housing projects by low-income neighbourhood concentration, whether in townhouses or high-rise apartments, and by length of residency or age of entry.

A second finding is family differences seem to matter a great deal. While living in alternative housing projects cannot explain large variances in labour 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 have different 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 fair worse. Taken overall, the results strongly 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 to be largely consistent with recent studies from the Moving to Opportunity experiment in the U.S. Studies from MTO generally find small increases in employment participation and earnings among parents from housing projects that were assisted to move into much more affluent neighbourhoods. Parents and children experienced large improvements in measures of well-being, such as overall resident satisfaction, crime incidence, and health. But few effects have been found for the children who moved to better neighbourhoods, in terms of standardized test results and school performance. In one study, suspensions and disciplinary action were more likely for children that moved into better communities. We will have to wait many years before we can compare long-run effects from the MTO experiment with the results in this study. In the meantime, the findings from the Toronto public housing program suggest any short-term benefit to parents or children from moving into a more aesthetic living arrangement does not translate into higher earnings or other labour market outcome 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 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 neighbourhood quality variation in the Toronto public housing sample may not be great enough to detect neighbourhood effects. The better housing projects in Toronto are in areas where a majority of middle-income households reside. The worst housing projects are in areas with the highest concentrations of low-income households 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 U.S.

Table 1
Theories of Social Interaction

Theory	Main Concept	Literature Examples
1. Peer Group, or 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. Social Network Externalities	Network of friends, relatives, or neighbours assist in finding jobs, providing, loans, or psychological support	Borjas (1995), Bertrand et al. (2000) Coleman (1988), Granovetter (1995) Montgomery (1991)
3. Limited Local Resources	Quality and efficiency of local institutions limited by community resources	Beabou (1996), Durlauf (1996), Hoxby (2000)
4. Conformism	Without full information, emulate observed choices of others	Bernheim (1994), Bikhchandani et al. (1992) Jones (1984), Sah (1991)

Table 2
Selected Census Tract Characteristics (1996) for Largest and Smallest Toronto Housing Projects Compared to Reported Census Tract Characteristics from Boston and Chicago Moving to Opportunity Programs

Tract characteristic	Toronto (1996)		Boston (1990)			Chicago (1990)		
	Downtown-Central Largest Projects	Diff. In Means Smallest-Largest	Control Means	Diff. In Means Sec. 8 - Control	Diff. In Means Exp - Control	Control Means	Sec. 8 - Control mean	Exp - Control mean
Black	0.193	-0.08**	0.45	-0.11	-0.198	0.993	0.900	0.572
		0.00				(0.01)	(0.21)	(0.38)
Below Low-Income-Cut-Off (Canada) or proverty line (U.S.)	0.494	-0.34**	.359	-0.16	-0.254	0.750	0.366	0.106
		0.00				(0.14)	(0.16)	(0.09)
Receiving Social Assistance	0.343	-0.22**	.294	-0.11	-0.202	0.586	0.312	0.102
		0.01				(0.16)	(0.13)	(0.09)
Female household head	0.176	-0.05**	.531	-0.15	-0.283	0.847	0.655	0.370
		0.01				(0.06)	(0.17)	(0.17)
Owner occupied households	0.035	0.42**	NA	NA	NA	0.0282	0.262	0.662
		0.01				0.048	0.216	0.199
Adult population with education less than high school	0.336	-0.09**	NA	NA	NA	NA	NA	NA
		0.01						
Adult population with education more than high school	0.499	0.14**	0.29	0.40	0.133	NA	NA	NA
		0.01						
Adult population with education college degree	0.157	0.07**	NA	NA	NA	0.081	0.154	0.230
		0.01				0.034	0.104	0.131
median household income (1996 $Cdn)	13500	27,200	NA	NA	NA	9007	24710	48888
N	923	770	176	113	236	118	53	67

Notes: Diff. In Means is the mean difference between census tract characteristics among households in "Smallest" public housing projects and households living in the 7 largest downtown housing projects. "Smallest" projects are defined as projects with less than 250 units, and in census tracts with less than 25 percent of households living below the Low-Income-Cut-Off. Standard errors are reported in parentheses, adjusted for household level clustering; * = p-value<.1;**=p-value<.05;***. Data for Boston is from Katz and Kling (2000), Table 4. Data for Chicago is from Rosenbaum, and Denton (2000), 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) Public Housing Residents	(5)
	All Toronto	Non Public Housing Residents Living in Public Housing Census Tracts	Sample 1	Sample 2	Sample 3
	(sample averages and standard errors)				
Household Total Income	53108	20832	13707	16377	7099
	(68365)	(27587)	(1642)	(10949)	(7971)
Household Total Wages	41266	13907	7471	3321	2020
	(58167)	(24771)	(14507)	(9749)	(4849)
Monthly Rent	749	442	381	377	284
	(331)	(335)	(326)	(322)	(129)
Under Low Income Cut-off	0.22	0.64	0.78	0.91	0.87
	(0.41)	(0.48)	(0.41)	(0.28)	(0.33)
Moved in Last 5 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	37877	18103	13218	13698	12015
	(51854)	(17918)	(11389)	(6935)	(12830)
Total Wages	28758	11313	6493	1605	5051
	(45646)	(18048)	(11689)	(5642)	(12542)

Table 3 - continued
Descriptive Statistics of Metropolitan Toronto and Public Housing Samples

	1996 Census Data				
Dependent Children in Census, ages 16-25	(1)	(2)	(3)	(4) Public Housings Residents	(5)
	All Toronto	Non Public Housing Residents Living in Public Housing Census Tracts	Sample 1	Sample 2	Sample 3
Age	20.53 (3.22)	20.11 (3.15)	19.97 (3.12)	19.65 (3.13)	19.80 (3.10)
Black	0.07 (0.26)	0.32 (0.47)	0.40 (0.49)	0.41 (0.48)	0.39 (0.49)
No High School Diploma	0.09 (0.28)	0.14 (0.34)	0.14 (0.35)	0.15 (0.36)	0.14 (0.35)
BA or Greater	0.11 (0.32)	0.04 (0.19)	0.03 (0.17)	0.25 (0.17)	0.03 (0.16)
Idle	0.06 (0.23)	0.13 (0.34)	0.15 (0.35)	0.13 (0.36)	0.18 (0.38)
Employed	0.54 (0.50)	0.33 (0.47)	0.27 (0.44)	0.23 (0.42)	0.21 (0.40)
Immigrant	0.27 0.44	0.41 0.47	0.43 0.50	0.45 0.50	0.41 0.49
Children Sample Size	258201	8606	5180	1382	5141

Notes: Sample 1 includes all households living in uniquely matched MTHC and Cityhome postal codes. Sample 2 includes all single mother household heads receiving social assistance 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 sample of dependent children includes only those living at home. The samples include both male and female children.

Table 4
Descriptive Statistics of Toronto and Public Housing Samples

	Intergenerational Income Data				
Household Heads of Children Aged 26-33 in 1996					
	(1)	(2)	(3)	(4) Public Housing Residents	(5)
	All Toronto	Non Public Housing Residents Living in Public Housing Census Tracts	Sample 1	Sample 2	Sample 3
	(sample averages and standard errors)				
Age	60.19 (7.24)	58.92 (7.89)	58.65 (8.04)	56.61 (7.00)	55.83 (8.11)
Number of Children	2.44 (1.38)	2.70 (1.41)	2.63 (1.41)	2.63 (1.36)	3.22 (1.56)
Female	0.18 (0.38)	0.48 (0.50)	0.61 (0.49)	1.00	0.65 (0.48)
Parent Income, 15 year average	53436 (53919)	25190 (18029)	20122 (13906)	13242 (6015)	17361 (9097)
Recieved Social Assistance, 1992-98	0.13 (0.34)	0.43 (0.50)	0.52 (0.50)	1.00	0.68 (0.47)
Children, aged 26-33 in 1996					
Age	31.70 (2.25)	31.68 (2.22)	31.73 (2.21)	31.60 (2.23)	30.47 (1.95)
Average Income, 1993-98	31548 (33253)	24360 (20108)	23064 (17620)	21413 (15629)	21711 (14898)
Average Earnings, 1993-98	28257 (33476)	21360 (21064)	19978 (18955)	17911 (17139)	18520 (16335)
Received Social Assistance, 1992-98	0.15 (0.36)	0.35 (0.48)	0.41 (0.49)	0.48 (0.50)	0.42 (0.49)
Working	0.88 (0.33)	0.82 (0.39)	0.80 (0.40)	0.77 (0.42)	0.80 (0.40)
Sample Size	297588	12577	6559	2674	1046

Notes: Sample 1 includes all households living in uniquely matched MTHC and Cityhome postal codes. Sample 2 includes all single mother household heads receiving social assistence 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 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	
Census Data	Downtown-Central Largest Projects	Smallest Projects	Downtown-Central Largest Projects	Smallest Projects
Black	0.324	0.354	0.293	0.361
	(0.47)	(0.48)	(0.46)	(0.48)
Single	0.340	0.365	0.594	0.587
	(0.47)	(0.48)	(0.49)	(0.49)
Immigrant	0.689	0.648	0.769	0.749
	(0.46)	(0.48)	(0.42)	(0.43)
No High School Diploma	0.472	0.440	0.471	0.443
	(0.50)	(47.00)	(0.50)	(0.47)
BA or Greater	0.065	0.070	0.062	0.061
	(0.25)	(0.29)	(0.24)	(0.24)
Moved in Last 5 years	0.532	0.536	0.522	0.501
	(0.50)	(0.50)	(0.50)	(0.50)
Age of Household Head	42.787	41.170	40.409	40.461
	(12.63)	(11.64)	(10.66)	(9.92)
Number of Kids	1.319	1.436	2.301	2.309
	(1.48)	(1.44)	(1.25)	(0.49)
Median Income	10583	13272	12589	14160
Percent on Public Assistance	0.538	0.452	0.579	0.541
	(0.50)	(0.48)	(0.49)	(0.49)
N	923	770	529	479
IID Data				
Single	NA	NA	0.564	0.571
			(0.50)	(0.49)
Age of Household Head	NA	NA	61.930	59.530
			(8.41)	(7.83)
Percent on Public Assistance	NA	NA	0.613	0.613
			(0.49)	(0.49)
Median Income	NA	NA	15284	17172
N	NA	NA	1757	1054

Notes: "Smallest" public housing projects and households living in the 7 largest downtown housing projects. "Smallest" projects are defined as projects with less than 250 units, and in census tracts with less than 25 percent of households living below the Low-Income-Cut-Off. 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

Census Data **(Youth 16-30 living at home)**	(1) Mean largest projects	(2) Mean Difference Smallest-Largest no controls	(3) Dummy Variable Coeff. for Smallest Project with background controls
Total years of schooling	12.34	-0.076 (0.179)	0.177 (0.171)
Less than high school	0.15	0.005 (0.025)	0.011 (0.027)
More than high school	0.16	0.007 (0.025)	0.008 (0.026)
Not working, Not at school	0.32	0.046 (0.032)	-0.017 (0.030)
N	226	390	390
IID Data **(Adults aged 28-35 in 1998** **who lived in public housing when teen)**			
Receiving SA	0.32	-0.028 (0.018)	-0.015 (0.018)
Log income (males)	9.95	0.024 (0.024)	0.016 (0.024)
Log earnings (males)	9.84	-0.004 (0.033)	0.011 (0.033)
Number of times did not file	2.27	0.060 (0.114)	0.119 (0.112)
N (males)	719	1154	1154

Notes: Column (2) shows the mean difference between outcomes among youth from "Smallest" public housing projects and youth from the 7 largest downtown housing projects. "Smallest" projects are defined as projects with less than 250 units, and in census tracts with less than 25 percent of households living below the Low-Income-Cut-Off. Standard errors are reported in parentheses, adjusted for household level clustering. None of the differences are significant from zero (p-value<.1). Column (3) shows dummy coefficient estimates from regressing the outcome variable on age dummies, gender, log parental income, parental marital status, whether parent receives social assistance, family size, and dummy variables for the indicated measure of neighbourhood quality. Estimates with census estimates also include indicators for whether black or recent immigrant. 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 Occurances in 1992 for Smallest and Largest Public Housing Projects

Type of Occurance	Downtown-Central Largest Projects	per 1000 household units Mean Difference Smallest Projects		Dummy Variable Coeff. for Smallest Project with mean family char. Controls	
Arson	1.12	-1.12 (0.54)	*	-1.22 (0.83)	
Assault causing bodily harm	17.02	-12.69 (2.39)	***	-11.47 (3.64)	***
Sexual Assault	1.45	-1.45 (0.28)	***	-1.40 (0.44)	***
Break and Enter and attempted B&E	22.00	-3.10 (5.01)		-10.93 (6.01)	*
Drug offense	14.61	-7.53 (7.81)		-12.90 (11.59)	
Neighbor Dispute	436	-129 (94.97)		-119 (141.83)	
Sudden death	4.18	-3.78 (1.24)	***	-3.43 (1.70)	***
N	7	35			

Notes: Occurances are all incidents that required a written report by MTHC Security Services (for MTHC projects only). Column (2) shows the mean difference between crime occurances among the 7 "Smallest" public housing projects and the 35 largest downtown housing projects. "Smallest" projects are defined as projects with less than 250 units, and in census tracts with less than 25 percent of households living below the Low-Income-Cut-Off. 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 Neighbourhood Quality Measures for 1996 Census Outcome Variables

	without controls					with individual background controls			
Youth From Public Housing Projects: **by number of household units**	(1) Total years of schooling	(2) Less than High School	(3) More than High School	(4) Not working, Not at school	(5) N	(6) Total years of schooling	(7) More than High School	(8) More than High School	(9) Not working, Not at school
Less than 150 Units (mean)	12.4	0.14	0.30	0.16	688				
Between 150 and 700 Units (diff.)	-0.135	0.004	-0.011	-0.013	1004	-0.064	0.001	-0.003	-0.015
	(0.106)	(0.017)	(0.023)	(0.018)		(0.093)	(0.017)	(0.022)	(0.017)
Greater than 700 Units (diff.)	0.036	0.001	0.012	-0.001	422	-0.092	0.008	0.013	-0.018
	(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	462	0.077	0.008	-0.001	0.008
	(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)	12.1	0.15	0.29	0.17	149				
>0.15, <= 0.40 (diff.)	0.158	-0.010	0.015	-0.013	965	0.126	-0.012	0.015	-0.004
	(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	564	0.151	-0.012	0.005	0.008
	(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	1068	-0.072	0.008	-0.015	0.012
	(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 neighbourhood quality categories, and average deviations from these means for the other cateogories. Columns 6-9 show dummy coefficient estimates from regressing the outcome variable on age, gender, log parental income, parental marital status, whether parent receives social assistance, family size, black, recent immigrant and dummy variables for the indicated measure of neighbourhood 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 aged 16-25 still living in public housing with their parents.

Table 9
Means and Difference from Means for Various Public Housing Neighbourhood Quality Measures for IID Outcome Variables

Youth From Public Housing Projects: **by number of household units**	without controls					with individual background controls			
	(1) Receiving SA	(2) Log Income (males)	(3) Log Earnings (males)	(4) Number of Times Did Not File Taxes	(5) N (for col. 8)	(6) Receiving SA	(7) Log Income (males)	(8) Log Earnings (males)	(9) Number of Times Did Not File Taxes
Less than 150 Units (mean)	0.32	10.00	9.80	2.20	1065				
Between 150 and 700 Units (diff.)	-0.021	-0.003	0.025	0.175	3505	-0.019	0.001	0.008	0.015
	(0.013)	(0.023)	(0.031)	(0.076)		(0.012)	(0.023)	(0.008)	(0.012)
Greater or equal than 700 Units (diff.)	0.002	-0.016	0.002	0.136	1189	0.004	-0.014	0.013	0.014
	(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	324	-0.017	0.03	0.102	-0.02
	(0.020)	(0.036)	(0.049)	(0.119)		(0.020)	(0.036)	(0.120)	(0.019)
by Percent in Census Tract Below LICO									
<=.15 (mean)	0.29	10.03	9.84	2.33	390				
>.15, <=.40 (diff.)	0.013	-0.02	-0.02	0.027	3656	0.014	-0.03	-0.014	-0.03
	(0.020)	(0.037)	(0.049)	(0.116)		(0.020)	(0.035)	(0.120)	(0.019)
>=.40 (diff.)	0.014	-0.02	-0.02	-0.060	1710	0.013	-0.02	-0.157	0.00
	(0.020)	(0.034)	(0.050)	(0.122)		(0.020)	(0.036)	(0.124)	(0.020)
Youth From Public Housing Projects: **by number of household units**									
High Rise (mean)	0.31	9.99	9.81	2.39	1884				
Townhouse (diff.)	-0.014	-0.005	0.001	0.140	3537	0.002	-0.009	-0.060	-0.006
	(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 neighbourhood quality categories, and average deviations from these means for the other cateogories. Columns 6-9 show dummy coefficient estimates from regressing the outcome variable on age, gender, log parental income, parental marital status, whether parent receives social assistance, family size, and dummy variables for the indicated measure of neighbourhood 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 9 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 Neighbourhood Quality Measures for 1992 Crime Occurances

	(3) Arson	(4) Assault causing bodily harm	(5) Sexual Assault	(6) Break and Enter or attempted B&E	(7) Drug Offense	(8) Neighbour Dispute	(9) Sudden Death
by number of household units				per 1000 units			
Less than 150 Units (mean)	0.00	4.92	0.62	18.14	6.15	366	0.31
Between 150 and 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)
Greater or equal than 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 High Rise or Townhouse							
High Rise (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. LICO stands for Low Income Cut-Off. High-rises are defined as buildings with at least 5 stories. Rows with (diff) indicate dummy coefficient of neighbourhood quality measure, after controlling for mean project characteristics (same as those used in Table 1).

Table 11

Income and Social Assitance 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 *Entered age 10-13			-0.02					0.01		
			(0.05)					(0.03)		
More than 35 percent in CT below LICO *Entered age 14-16			0.05					0.01		
			(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 *5-10 years in Public Housing					0.01					0.01
					(0.04)					(0.03)
More than 35 percent in CT below LICO *11+ years in Public Housing					-0.02					0.01
					(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 probability 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
Covariances between Brothers and Neighbouring Boys in Adult Log Income

	Public Housing Samples			
	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)				
neighbour Covariance	0.015 (0.011)	0.003 (0.014)	0.011 (0.016)	-0.005 (0.015)
neighbour Covariance after controlling for observable family characteristics	0.005 (0.002)	0.004 (0.013)	0.011 (0.015)	-0.004 (0.016)
neighbour Covariance tenure >= 5 years	0.016 (0.013)	-0.001 (0.028)	-0.002 (0.003)	-0.009 (0.026)
neighbour 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				
neighbour Covariance	0.013 (0.008)	0.005 (0.014)	-0.007 (0.017)	0.005 (0.014)
neighbour Covariance after controlling for observable family characteristics	0.005 (0.003)	0.000 (0.014)	-0.006 (0.017)	0.000 (0.014)
neighbour Covariance tenure >= 5 years	0.017 (0.013)	-0.001 (0.005)	0.000 (0.009)	-0.001 (0.012)
neighbour Covariance Adjusted tenure >= 5 years	0.004 (0.001)	-0.001 (0.002)	0.000 (0.003)	-0.001 (0.013)
Sample Size	132412	4192	1118	4884
Number of Sibling Pairs	16485	772	156	889
Number of neighbour Pairs		61468	10125	88620

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 the appendix). The estimated "effect" is the squared covariance for neighbours in a census tract with tenure>=5years multiplied by the squared sample variance. See text for details.

Table 13
Sibling and Neighbour Covariances of Adult Men's Log Earnings

		Public Housing Samples		
	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	0.098	0.091	0.048	0.150
after controlling for observable	(0.005)	(0.028)	(0.034)	(0.032)
family characteristics				
Neighbours within EAs (Toronto sample) or projects (PH samples)				
neighbour Covariance	0.009	0.002	0.017	-0.002
	(0.008)	(0.016)	(0.042)	(0.014)
neighbour Covariance	0.001	0.002	0.018	0.001
after controlling for observable	(0.002)	(0.015)	(0.042)	(0.015)
family characteristics				
neighbour Covariance	0.016	0.005	0.009	0.006
tenure >= 5 years	(0.013)	(0.004)	(0.036)	(0.018)
neighbour Covariance Adjusted	0.002	0.005	0.012	0.005
tenure >= 5 years	(0.000)	(0.004)	(0.036)	(0.018)
Neighbours within census tracts				
neighbour Covariance	0.010	0.002	0.001	0.004
	(0.008)	(0.016)	(0.032)	(0.011)
neighbour Covariance	0.003	0.002	0.002	0.005
after controlling for observable	(0.003)	(0.015)	(0.003)	(0.011)
family characteristics				
neighbour Covariance	0.017	0.005	-0.005	0.006
tenure >= 5 years	(0.013)	(0.004)	(0.026)	(0.016)
neighbour Covariance Adjusted	0.004	0.005	-0.004	0.005
tenure >= 5 years	(0.001)	(0.004)	(0.026)	(0.016)
Sample Size	132412	4192	2140	3337
Number of Sibling Pairs	16485	659	353	518
Number of neighbour Pairs		68853	10125	55959

Notes: Adult men's earnings 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 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 14
Sibling and Neighbour 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				
Brother Covariance	0.301 (0.022)	0.833 (0.162)	0.757 (0.234)	0.905 (0.189)
Brother 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)				
neighbour Covariance	0.039 (0.025)	0.030 (0.075)	-0.035 (0.149)	0.073 (0.117)
neighbour Covariance after controlling for observable family characteristics	0.016 (0.011)	0.028 (0.075)	-0.040 (0.149)	0.048 (0.115)
neighbour Covariance tenure >= 5 years	0.025 (0.028)	-0.063 (0.133)	-0.111 (0.188)	-0.004 (0.147)
neighbour 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				
Neighbour Covariance	0.033 (0.024)	0.009 (0.086)	-0.013 (0.131)	0.005 (0.112)
Neighbour Covariance after controlling for observable family characteristics	0.012 (0.011)	0.018 (0.088)	-0.024 (0.130)	-0.055 (0.095)
Neighbour Covariance tenure >= 5 years	0.031 (0.019)	-0.049 (0.104)	-0.108 (0.136)	-0.069 (0.125)
Neighbour Covariance Adjusted tenure >= 5 years	0.034 (0.018)	-0.083 (0.098)	-0.127 (0.135)	-0.087 (0.116)
Sample Size	132412	5329	2619	3993
Number of Sibling Pairs	16485	1042	502	718
Number of neighbour Pairs		98633	10125	55959

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 Neighbour Covariances of Total Years of Schooling

		Public Housing Samples		
	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	1.319	0.490	0.501	0.555
after controlling for observable	(0.039)	(0.177)	(0.104)	(0.087)
family characteristics				
Neighbours within EAs (Toronto sample) or projects (PH samples)				
neighbour Covariance	0.185	-0.015	-0.028	-0.165
	(0.065)	(0.131)	(0.156)	(0.229)
neighbour Covariance	0.125	-0.059	-0.025	-0.197
after controlling for observable	(0.028)	(0.111)	(0.156)	(0.211)
family characteristics				
neighbour Covariance	0.211	-0.124	0.009	0.006
tenure >= 5 years	(0.053)	(0.066)	(0.036)	(0.018)
neighbour Covariance Adjusted	0.142	-0.115	0.012	0.005
tenure >= 5 years	(0.074)	(0.066)	(0.036)	(0.018)
Neighbours within census tracts				
neighbour Covariance	0.204	-0.032	-0.012	-0.049
	(0.081)	(0.089)	(0.169)	(0.104)
neighbour Covariance	0.126	-0.063	0.010	-0.085
after controlling for observable	(0.020)	(0.078)	(0.168)	(0.082)
family characteristics				
neighbour Covariance	0.231	-0.133	0.003	-0.134
tenure >= 5 years	(0.088)	(0.075)	(0.204)	(0.092)
neighbour Covariance Adjusted	0.152	-0.097	0.121	-0.072
tenure >= 5 years	(0.077)	(0.064)	(0.156)	(0.069)
Sample Size	91212	1341	607	1819
Number of Sibling Pairs	35043	542	440	1522
Number of neighbour Pairs		13109	2800	16289

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 the appendix). The estimated "effect" is the squared covariance for neighbours 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 Neighbourhood 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) N	(6) Receiving SA	(7) Log Income (males)	(8) Log Earnings (males)	(10) N (for col. 8)
by number of household units									
by number of household units	12.26	0.14	0.16	0.29	353.00	0.31	10.01	9.81	598.00
Between 150 and 700 Units (diff.)	-0.078 (0.136)	-0.009 (0.022)	-0.010 (0.023)	0.012 (0.030)	753.00	-0.016 (0.015)	0.001 (0.027)	0.007 (0.037)	2357.00
Greater or equal than 700 Units (diff.)	0.138 (0.152)	-0.004 (0.025)	-0.011 (0.026)	0.021 (0.033)	421.00	0.007 (0.016)	-0.016 (0.030)	-0.018 (0.041)	906.00
by MTHC or Cityhome Development									
MTHC (mean)	12.04	14.90	0.17	0.24	571.00	0.31	9.99	9.81	2028.00
Cityhome (diff.)	0.023 (0.186)	0.013 (0.038)	-0.002 (0.039)	-0.017 (0.035)	104.000	-0.019 (0.023)	0.02 0.05	-0.01 0.06	112.00
by Percent in Census Tract Below LICO									
<=.15 (mean)	12.17	0.16	0.17	0.28	87.00	0.29	10.03	9.84	259.00
>.15, <=.40 (diff.)	0.076 (0.246)	-0.014 (0.039)	-0.019 (0.040)	0.013 (0.053)	551.00	0.012 (0.022)	0.00 0.04	-0.02 0.05	2393.00
>=.40 (diff.)	0.187 (0.248)	-0.026 (0.039)	-0.023 (0.040)	0.021 (0.053)	487.00	0.029 (0.022)	-0.01 0.04	-0.02 0.05	1209.00

Table A1 – continued
Means and Difference from Means for Various Public Housing Neighbourhood Quality Measures with age, gender, and family background controls

	Public Housing Sample 1								
	Census Variables					IID Variables			
	(1) Total years of schooling	(2) Less than High School	(3) Idle	(4) More than High School	(5) N	(6) Receiving SA	(7) Log Income (males)	(8) Log Earnings (males)	(10) N (for col. 8)
By High Rise or Townhouse									
High Rise (mean)	12.29	0.14	0.14	0.32	737.00	0.32	9.99	9.81	1402.00
Townhouse (diff.)	-0.041	-0.006	0.008	-0.018	604.00	0.00	-0.02	-0.02	2250.00
	(0.115)	(0.019)	(0.019)	(0.026)		0.02	0.02	0.03	
by Largest Projects or Smallest Townhouse Projects									
less than 250 units, no highrises in CT with less than 25% below LICO	12.02	0.15	0.15	0.29	112.00	0.28	10.04	9.83	425.00
Largest Projects	0.218	-0.005	-0.002	0.03	374.00	-0.01	0.00	0.00	871.00
(difference)	(0.258)	(0.038)	(0.037)	0.050394		0.0203006	0.0356171	0.0488813	

Notes: The tables shows raw means for particular neighbourhood 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 7 and 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 Neighbourhood Quality Measures with age, gender, and family background controls

	Public Housing Sample 2								
	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) N	(6) Receiving SA	(7) Log Income (males)	(8) Log Earnings (males)	(10) N (for col. 8)
by number of household units									
Less than 150 Units (mean)	12.19	0.15	0.17	0.25	169.00	0.38	9.95	9.71	437.00
Between 150 and 700 Units (diff.)	-0.106 (0.162)	0.020 (0.033)	-0.020 (0.034)	-0.026 (0.033)	358.00	-0.012 (0.026)	0.005 (0.031)	0.058 (0.044)	1306.00
Greater or equal than 700 Units (diff.)	-0.164 (0.204)	0.013 (0.042)	0.013 (0.045)	-0.030 (0.036)	128.00	-0.021 (0.033)	0.033 (0.037)	-0.029 (0.051)	397.00
by MTHC or Cityhome Development									
MTHC (mean)	12.04	14.90	0.17	0.24	571.00	0.38	9.94	9.71	2028.00
Cityhome (diff.)	0.023 (0.186)	0.013 (0.038)	-0.002 (0.039)	-0.017 (0.035)	104.000	0.050 (0.048)	0.02 0.05	-0.01 0.06	112.00
by Percent in Census Tract Below LICO									
<=.15 (mean)	12.19	0.15	0.18	0.02	27.00	0.37	10.02	9.74	116.00
>.15, <=.40 (diff.)	0.214 (0.353)	-0.014 (0.067)	-0.016 (0.052)	0.010 (0.043)	297.00	0.017 (0.043)	-0.01 0.05	-0.01 0.05	1372.00
>=.40 (diff.)	0.101 (0.364)	-0.018 (0.055)	-0.005 (0.047)	0.009 (0.056)	194.00	0.017 (0.044)	-0.02 0.06	-0.01 0.04	652.00

Table A2 –continued
Means and Difference from Means for Various Public Housing Neighbourhood Quality Measures with age, gender, and family background controls

	Public Housing Sample 2								
	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) N	(6) Receiving SA	(7) Log Income (males)	(8) Log Earnings (males)	(10) N (for col. 8)
by High Rise or Townhouse									
High Rise (mean)	11.98	0.15	0.17	0.25	272.00	0.37	9.92	9.72	629.00
Townhouse (diff.)	-0.016	-0.0.20	-0.006	-0.014	313.00	0.01	0.01	0.01	1375.00
	(0.160)	(0.029)	(0.030)	(0.034)		0.02	0.03	0.04	
by Largest Projects or Smallest Townhouse Projects									
less than 250 units, no highrises	12.16	0.14	0.15	0.25	51.00	0.38	9.94	9.71	285.00
in CT with less than 25% below LICO	-0.094	0.016	0.047	-0.01	127.00	0.02	0.00	0.02	430.00
Largest Projects (difference)	(0.342)	(0.061)	(0.065)	0.061913		0.0399073	0.0458	0.0638884	

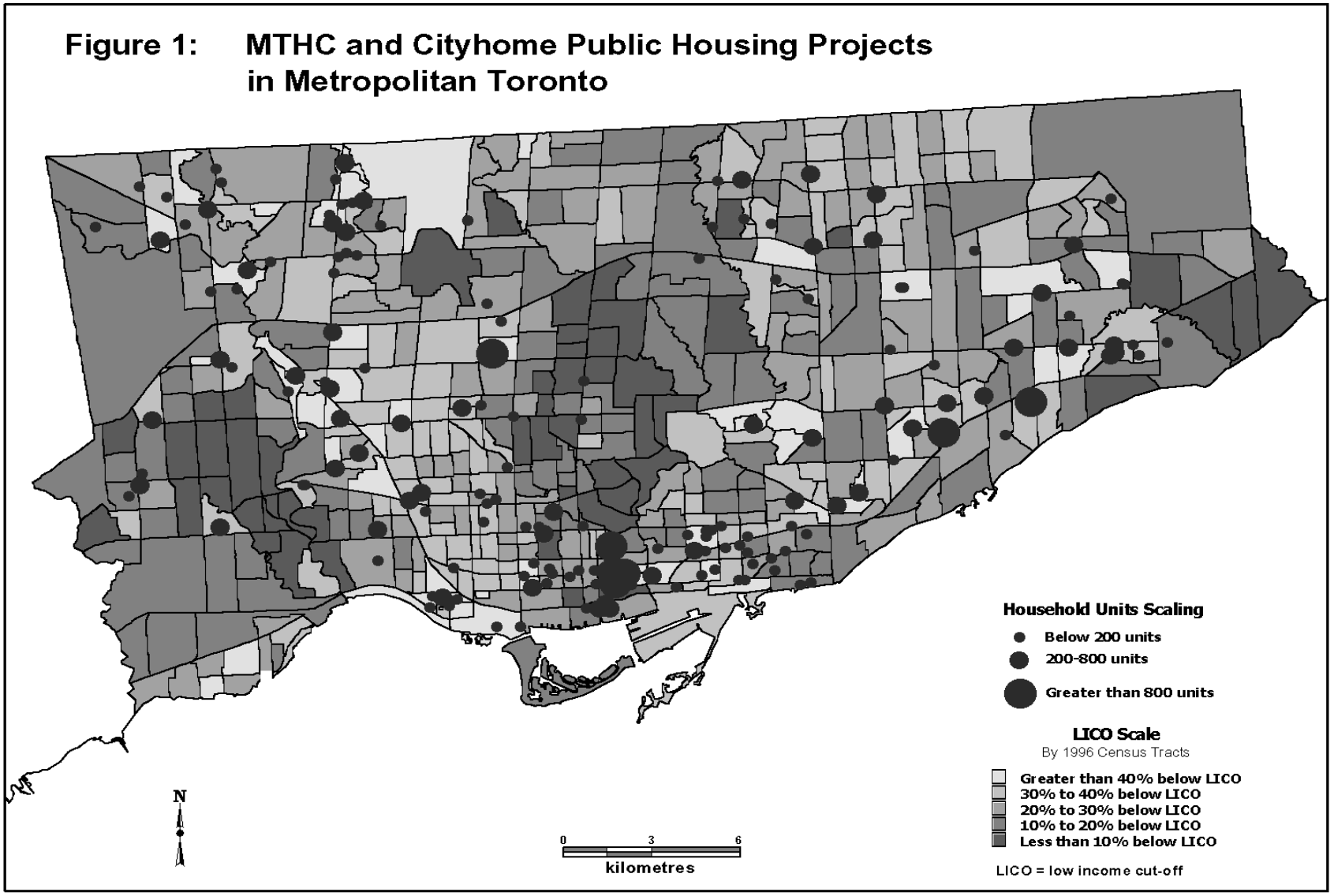
Notes: The tables shows raw means for particular neighbourhood quality categories, and average deviations from these means for the other cateogories for all youth with single female parents on social assistance. Standard errors are in parentheses. Except for columns 7 and 8, the samples include both 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 furthur details.

Table A3
Means and Difference from Means for Various Public Housing Neighbourhood Quality Measures with age, gender, and family background controls

	Public Housing Sample 3								
	Census Variable					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) N	(6) Receiving SA	(7) Log Income (males)	(8) Log Earnings (males)	(10) N (for col. 8)
by number of household units									
Less than 150 Units (mean)	12.33	0.14	0.16	0.29	647.00	0.29	9.99	9.78	149.00
Between 150 and 700 Units (diff.)	-0.105	0.004	-0.013	-0.004	976.00	-0.010	0.043	0.009	402.00
	(0.108)	(0.018)	(0.018)	(0.023)		(0.036)	(0.096)	(0.078)	
Greater or equal than 700 Units (diff.)	0.013	0.007	0.008	0.016	361.00	-0.007	-0.041	-0.022	155.00
	(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	1627.00	0.27	10.03	9.80	690.00
Cityhome (diff.)	0.155	-0.009	-0.010	0.012	433.000	0.032	-0.04	-0.02	16.00
	(0.115)	(0.019)	(0.019)	(0.025)		(0.051)	0.06	0.07	
by Percent in Census Tract Below LICO									
<=.15 (mean)	11.93	0.16	0.19	0.28	147.00	0.30	10.03	9.81	33.00
>.15, <=.40 (diff.)	0.281	-0.013	-0.032	0.042	915.00	-0.007	-0.03	-0.03	476.00
	(0.190)	(0.031)	(0.032)	(0.041)		(0.043)	0.07	0.05	
>=.40 (diff.)	0.266	-0.021	-0.014	0.041	496.00	-0.021	-0.03	-0.03	197.00
	(0.200)	(0.033)	(0.034)	(0.043)		(0.046)	0.06	0.08	
by High Rise or Townhouse									
High Rise (mean)	12.28	0.14	0.15	0.31	751.00	0.29	10.04	9.82	74.00
Townhouse (diff.)	0.040	0.001	-0.004	-0.018	1020.00	0.01	-0.06	-0.06	571.00
	(0.101)	(0.017)	(0.017)	(0.022)		0.05	0.07	0.08	
by Largest Projects or Smallest Townhouse Projects									
less than 250 units, no highrises in CT with less than 25% below LICO	12.01	0.17	0.19	0.28	209.00	0.30	10.01	9.82	122.00
Largest Projects	0.359	-0.005	-0.003	0.01	327.00	-0.02	-0.03	-0.03	134.00
(difference)	(0.205)	(0.033)	(0.034)	0.040693		0.0492818	0.0734923	0.078001	

Notes: The tables shows raw means for particular neighbourhood 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 7 and 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

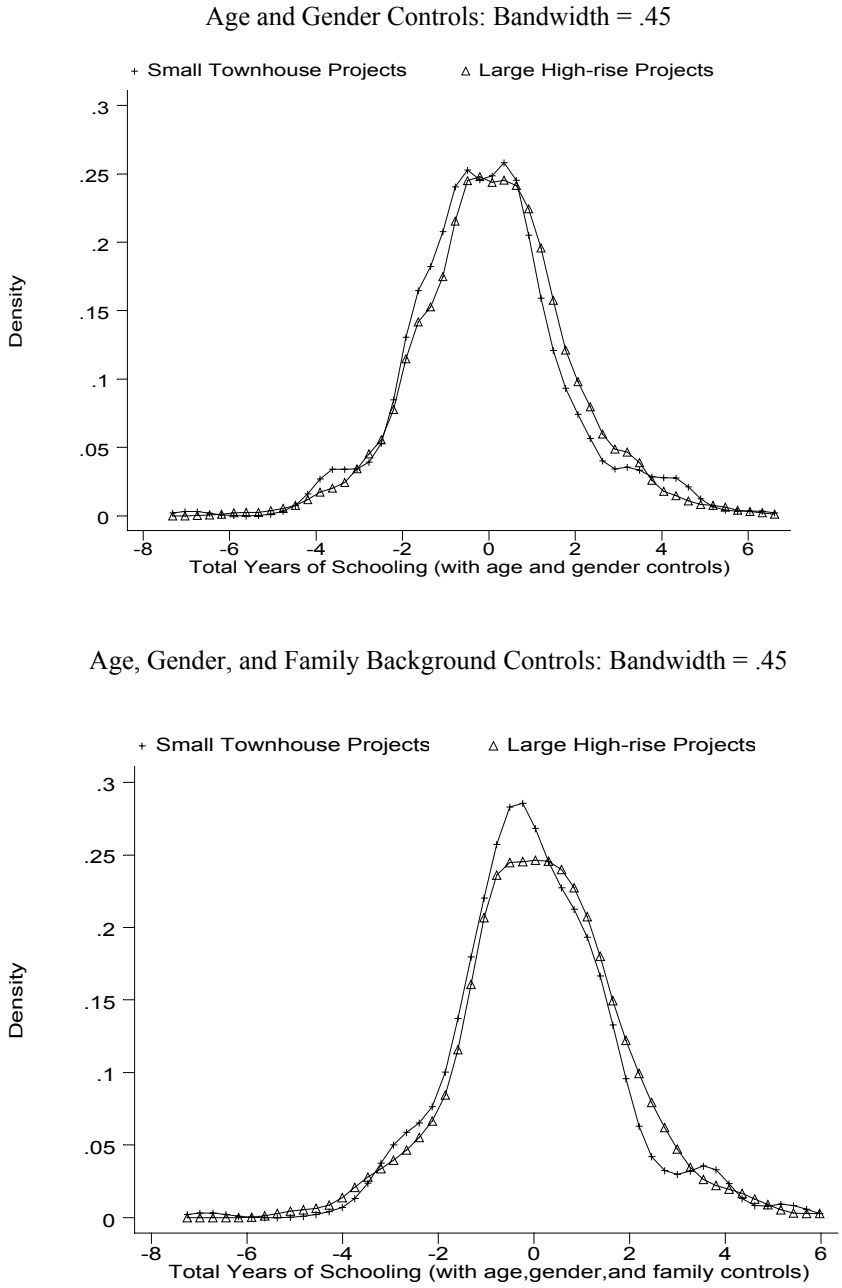


Statistics Canada / Statistique Canada

Source: 1996 Census of Canada. Produced by the Geography Division, Statistics Canada, 2001
Recensement du Canada, 1996. Préparé par la division de la géographie, Statistique Canada, 2001

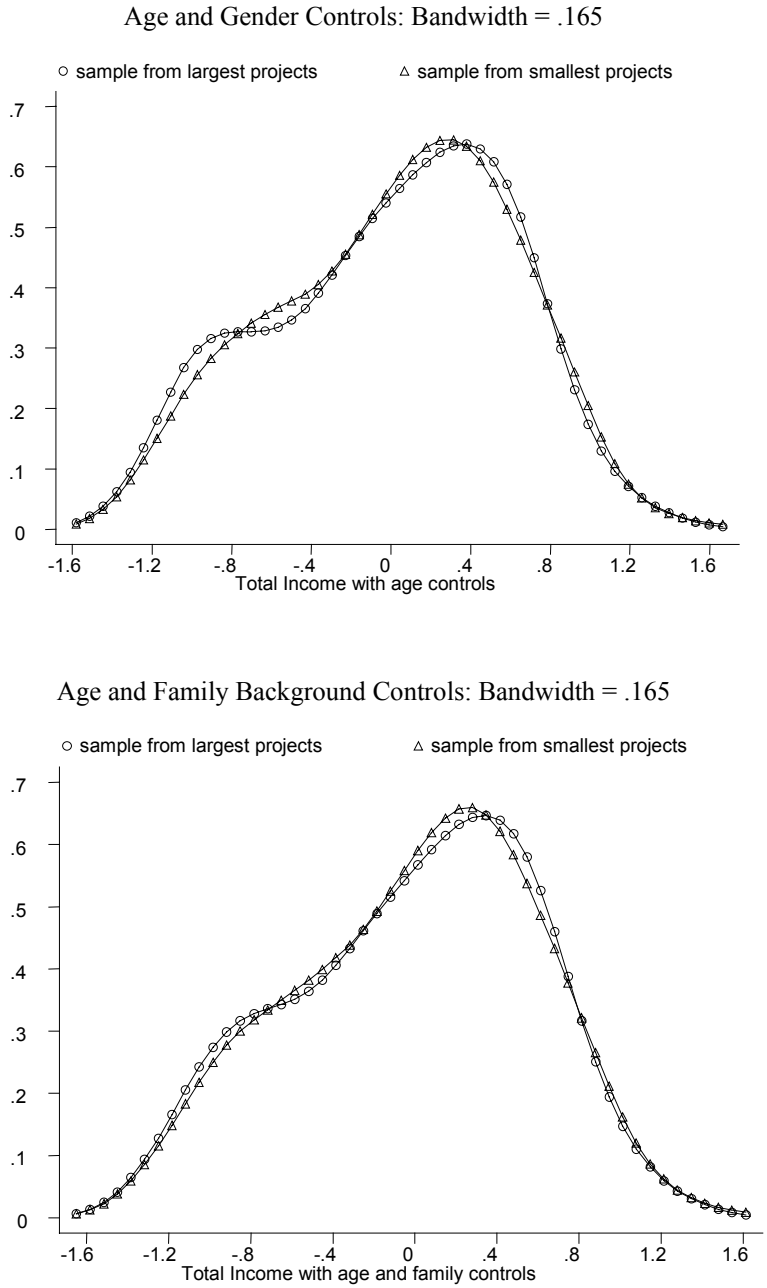
Canada

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



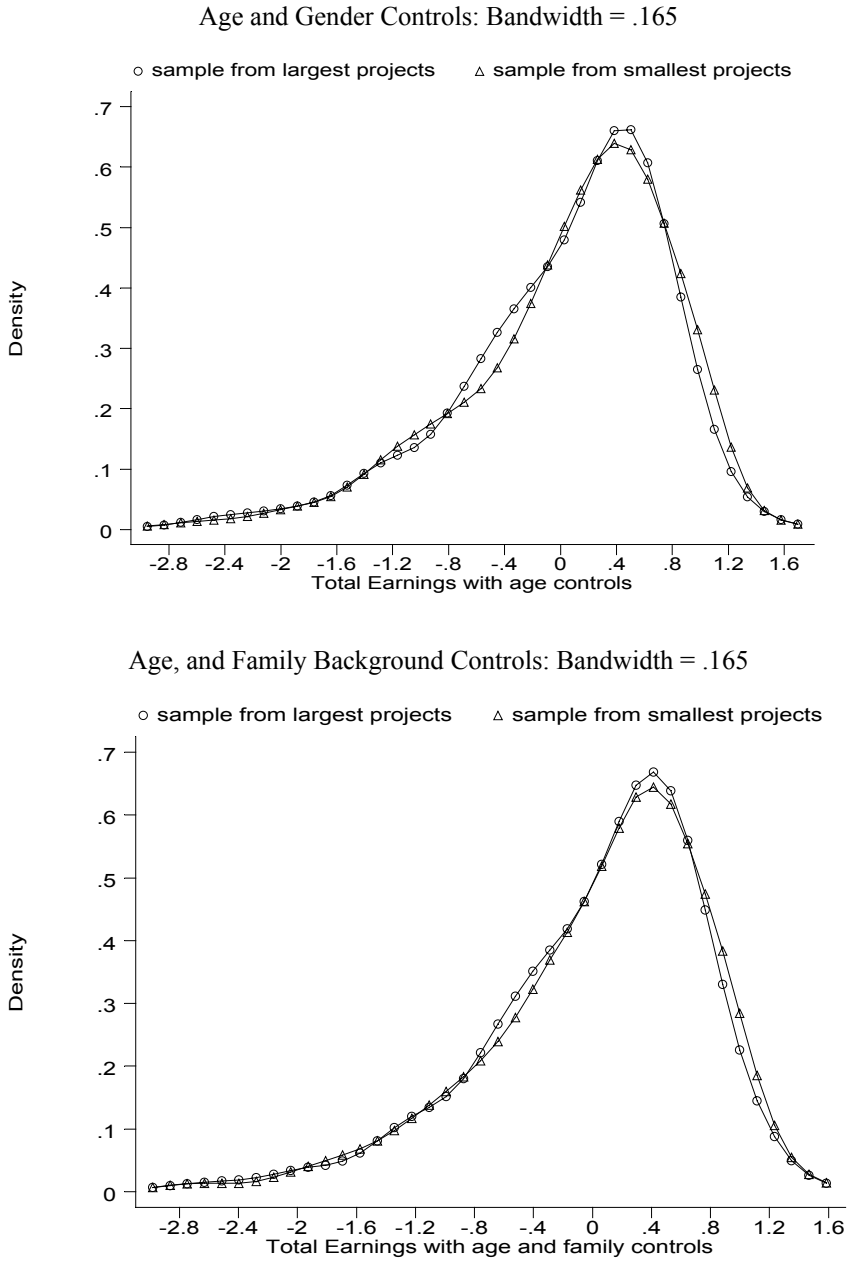
Notes: Residuals generated from regressing total years of education on a full set of age and gender dummies for the sample of youth in the 1996 census living in public housing are used to estimate the two kernel densities overlaid in the first panel. The first is for the sample living in the six largest housing projects. The second is for the sample living in small projects, with 250 townhouse units or less, and in census tracts with less 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



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 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 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



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.

Appendix A: Estimating Sibling and Neighbour Correlations[23]

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, $E(Y_{ifp})$, as a weighted average of the sample mean square of y_{ifp}:

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

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 that the number of brothers per family and the number of families per project do vary. Weighting families with more brothers and projects with more families gives more information. Following Solon et al. (2000), I measure the brother covariance by the following:

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

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 neighbour covariance by:

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

where $W_{ff'p} = \sqrt{I_{fp} I_{f'p}}$. In words, within each project I derive the average covariance between each unrelated neighbour pair. Each project covariance (against the sample population mean) is averaged over projects. In Solon et al., more weight is given to neighbourhoods where there are more neighbour observations. For the 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 $W_p = 1$.[24] Another alternative is to group projects in the same census tract. Doing so increases the sample to calculate the neighbour 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:

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

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

[24] 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 aged 16-19 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 tax filer's family using Social Insurance Numbers, names, and address information. The parents in the file are not necessarily biological parents, rather, the male and female household head at the time of the link. The IID contains some family siblings if they fall within the same cohort of tax filers 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, only years where total income was greater than $1,000 were used. Missing values from no tax record in a particular year were excluded from the calculation. When I counted missing years as zero values for parental income, the coefficient from log parental income on child's log earnings fell from 0.21 to 0.15 for the combined samples. The sibling and neighbour 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 neighbour 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 those who did not file an income tax return in their teenage years while still living at home, each of the three cohorts under-represent the Canadian population. Compared to the estimate of population 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 tax filers in 1998, the IID misses 56.2 percent of children in families with parental income less than $10,000, and misses 39.8 percent of children in families with parental income between $10,000 and $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 geography groups. For each category, the basic weight is the number from the IID cohort in the sample of all tax filers 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

My third public housing sample was created 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 uniquely identified to live in MTHC projects and households living in census tracts with public housing, but not in a public housing 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 social assistance participation indicator, and family size indicators. The kernel densities from the estimated probabilities are plotted in Figure A1, for the sample in public housing, and the sample not. 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.

Figure A2 shows the kernel densities from the probit estimate between public housing and non-public housing samples for the 1996 Census. The control variables 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 5 years. I restrict Sample 3 with Census data to households with public housing postal codes and predicted probabilities greater than .15.[25]

[25] The coefficient results from the probit models are available from the author upon request.

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