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

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Abstract: Many social scientists presume that the quality of the neighborhood to which children are exposed affects a variety of long-run social outcomes. I examine the effect on the long-run labor market outcomes of adults who were assigned, when young, to substantially different public housing projects in Toronto. Administrative data are matched to public housing addresses to track children from the program for over 15 years. The main finding is that neighborhood quality plays little role in determining a youth's adult earnings, education attainment, or welfare participation, but does affect exposure to crime. While living in contrasting housing projects cannot explain large variances in labor market outcomes, family differences, as measured by sibling outcome correlations, account for up to 30 percent of the total variance in the data. (JEL: I30, J38).

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* I am very grateful to Alan Auerbach, David Card, John Quigley, and Steve Raphael for their guidance and support. I also wish to thank Miles Corak and Sophie Lefebre for assistance with Statistics Canada's administrative data. Frances Beard and Barbara Watson from Metro Toronto Housing Corporation, Brent Donnelly and Ryner Soegtrop from Cityhome, and Monique Volpe from the Ontario Housing Corporation were instrumental in helping me compile a complete list of subsidized housing projects in Toronto. Participants from numerous institutions provided valuable feedback. Funding from the Burch Center for Tax Policy and Public Finance and the Fisher Center for Real Estate and Urban Economics is greatly appreciated. I am solely responsible for the contents of this paper.

I. Introduction

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

A primary advantage of analyzing neighborhood interaction within the context of public housing is that participation in the program limits residential choice. Within public housing, similar households may reside in different locations for reasons beyond their control. Three previous studies use subsidized housing programs to examine neighborhood effects in the United States. The well-known Gatreaux program assisted black households in high-density public housing projects in Chicago to move to less-segregated communities. Rosenbaum et al. [1999], Rosenbaum [1995], and Popkin et al. [1993], who argue that the selection into suburbs or the central city was random, find that outcomes of the parents and children were markedly better for those who moved to the less-segregated suburbs.[1] Early results from the Moving to Opportunity (MTO) program also suggest quality of life improvements from moving to well-off areas [Katz et al., 2001, Ludwig et al., 2001]. Compared to families who remain in high-density housing projects, the randomly selected families who were moved to more affluent neighborhoods enjoy increases in overall resident satisfaction, reductions in exposure to crime, and fewer health problems. When the MTO studies turned to initial economic effects, however, differences across treatment and control groups were much less clear.

Parental welfare participation and employment, for example, do not differ across groups, and child test scores and delinquent behavior vary considerably less than the Gatreaux studies would imply. In another study, Jacob [2000] examines a less extreme experiment in which families living in Chicago housing projects set to close were offered vouchers to relocate. Comparing children from these projects to children from others, he finds no differences in test scores and dropout rates.

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

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

A crucial advantage of this research design is the capacity to match specific housing project addresses to national census and longitudinal administrative data on labor market outcomes. This generates large samples and enables an analysis of both short- and long-run impacts from neighborhood differences, a decade or more after participation in the program. The linked administrative records, in particular, provide a rare opportunity to examine accurate measures of total income, wages, and welfare participation when most youths from public housing are 30 years of age or older.

National data also enable a comparison of the estimated neighborhood effects from a quasi-experimental setting with those estimated from a simple OLS approach for households in the private housing market living in the same neighborhoods as public housing participants. For the private household market sample, I estimate substantial positive effects on youths' labor market outcomes from living in wealthier residential areas, even after controlling for observable family background characteristics. When I estimate the same effects for those children within the housing program, however, the positive effects disappear. This is the main finding of the paper: Despite significant contrast in living conditions across projects, neighborhood quality does not make much difference to chances for labor market success in the long-run. Average education attainment levels, mean earnings, income, and welfare participation rates vary little between adolescents from different public housing types. In fact, estimates of the probability wage and earning distributions for youths from the highest density projects and the lowest projects are virtually identical.

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

I also compare sibling correlations to unrelated neighbor correlations. This approach developed by Solon et al. [2000], accounts for unobserved measures of neighborhood quality and provides an omnibus measure of neighborhood effects relative to family effects. The outcome correlations between youths from the same housing projects are measured around zero. However, family background, as captured through sibling correlation measures, accounts for about 30 percent of the total variance in income and wages.

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

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

Several existing theories attempt to explain why residential location may affect individual behavior.[2] Table I summarizes four main hypotheses. Perhaps the most intuitive explanation by which neighborhoods affect outcomes is through peer group or role model effects. There is rich evidence within the psychology literature on the importance of these effects, both positive and negative [Brown, 1990, Brown et al., 1986]. According to this theory, an individual makes decisions based not just on her own preferences but on whether her decisions would deviate from choices made by others in her reference group [Akerlof and Kranton, 2000]. Second, an individual's social network may be an important resource. Personal contacts can improve an individual's chances of finding a job, receiving advice and psychological support, or getting a temporary loan. Granovetter [1995], for example, concludes that jobs are often found through contacts formed long before seeking employment. Third, resources for local public goods, such as schools, libraries, and law enforcement, are limited by the resources available to community residents. A lack of funding for local schools, for example, exacerbates a poor community's ability to hire exceptional teachers. A final way by which neighborhoods may play a role is through conformism. In contrast to peer group effects, conformism models usually posit that individuals mimic neighbors' behavior because they lack enough information to choose on their own [Bikhchandani et al., 1992, Bernheim, 1994].

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

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

III. Methodology

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

A. Differences in Means

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 as determined by the following equation:

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

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

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

where $\overline{Y}_p$ is the mean of the outcome variable for project p, and η_p is the mean neighborhood effect on individuals from project p. We are interested in the mean outcome difference attributable to variation between project characteristics, $\eta_g - \eta_b$. If assignment is random, $\overline{X}_g = \overline{X}_b$, then the impact from living in project g versus project b can be estimated directly from the mean outcome difference. Without random assignment, this comparison is biased toward a larger effect on the project in which families that tend to have greater positive influence sort into.[3]

B. Sibling and Neighbor Correlations

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

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

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

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 into

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

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

$$(5) \qquad Cov(Y_{sfp}, Y_{s'fp}) = Cov(\gamma X_{sfp}, \gamma X_{s'fp}) + Var(\eta_p) + 2Cov(\gamma X_{fp}, \eta_p).$$

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

$$(6) \qquad Cov(Y_{sfp}, Y_{s'f'p}) = Cov(\gamma X_{fp}, \gamma X_{f'p}) + Var(\eta_p) + 2Cov(\gamma X_{fp}, \eta_p).$$

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

The neighbor covariance in Y_{sfp} provides an upper bound on the possible influence of both observed and unobserved neighborhood characteristics. That bound can be tightened by subtracting measurable parts of the first term that reflect neighbors' similar family backgrounds. Thus, the project covariance in earnings attributable to the observable part of family characteristics in γX_{fp} is subtracted from the overall neighbor covariance in equation (6) to obtain a more precise upper limit on project effects.

If families are assigned into projects at random, $Cov(\gamma X_{fp}, \gamma X_{f'p})$ and $2Cov(\gamma X_{fp}, \eta_{p})$ equal zero, and this approach of estimating relative neighborhood effects can be expressed simpler by correlations. If random assignment into projects, then the sibling outcome correlation,

$$(7) \qquad Cor(Y_{sfp}, Y_{s'fp}) = \frac{Cov(\gamma X_{sf}, \gamma X_{s'fp}) + Var(\eta_{p})}{Var(Y_{sfp})},$$

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

$$(8) \qquad Cor(Y_{sfp}, Y_{s'f'p}) = \frac{Var(\eta_{p})}{Var(Y_{sfp})},$$

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

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

A. Background

Public housing buildings vary a great deal throughout Toronto in terms of size, location, and neighborhood surroundings. Some of the earliest projects were built as part of a large urban renewal effort to provide accommodation to thousands of low-income households living in areas of decay or in overcrowded situations. Many observers, however, argue that these buildings did little to improve the urban environment and may actually have made conditions worse. Property values in neighborhoods surrounding these older projects are among the lowest in the city, and crime rates are among the highest.[4] Other projects built, however, were smaller in scale and located in more suburban communities. From 1949 until the mid-1970s, the construction and administration of subsidized housing was run by the Metro Toronto Housing Corporation (MTHC, formerly known as the Metropolitan Toronto Housing Authority). The federal government provided MTHC with a massive construction budget. The administration used these funds to develop 113 family projects, accommodating 29,173 households (about one in twenty family households in metropolitan Toronto).[5] Every MTHC household pays rent geared to income. That is, approximately 25 to 30 percent of a household's total income is charged as rent.[6]

Legislation to the National Housing Act changed in 1974, allowing for more development of public housing at the municipal level. The new housing developments were designed to mix more with the surrounding community and to accommodate far fewer households with subsidies than previous developments. The amendments came directly from concerns about the high concentrations of low-income households in some earlier projects.[7] Cityhome, under the municipal government, was responsible for most of the new construction prior to the mid-1980s, and it administers 97 developments containing 8,966 household units.[8] Not all households living in Cityhome projects receive subsidies. In an effort to encourage a greater income mix within projects, 25 to 60 percent of Cityhome's units are allocated to private renters -- mostly single, low- to middle-income individuals.[9]

B. Variation in neighborhood quality

Figure I shows the locations for 160 MTHC and Cityhome family projects built before 1986.[10] The map divides Metropolitan Toronto, with a population of 2.4 million in 1996, into census tracts categorized by the percentage of households within a tract with family incomes below Statistics Canada's Low-Income Cut-Off (LICO). Census tracts contain about 1,000 to 3,000 households and are designed to capture geographic and social boundaries to represent common impressions of neighborhoods. The darker the shade in the tract, the smaller the portion of low-income households living there. The projects cover a large range of neighborhoods downtown and in the suburbs.[11] Most of seven largest downtown developments, which together accommodate about 25 percent of all subsidized families, are within a short walking distance from each other. In addition to these large developments, however, there are also a considerable number of smaller low-rise and townhouse complexes in more middle-income and residential areas, constructed over the same period.

Census Tract Characteristics

Columns 1 and 2 of Table II present the mean 1996 census tract characteristics for two groups of projects: the largest seven in the central city, and forty-two projects with fewer than 250 units located in census tracts with fewer than 25 percent of households below the LICO. The comparison between the two groups arguably provides the most contrast between residential quality in the program without reducing the sample to an unworkable level.[12] The low-density projects are in middle-income census tracts, where only 15 percent of households fell below the LICO in 1996. In contrast, 49 percent of households around the high-density projects are below the LICO. Households in the high-density census tracts were more likely to be female headed, on welfare, and less educated than households from the smaller projects. Almost all households around the largest projects were renters, while 46 percent of those around the smaller projects owned their own home. The median income was more than three times greater for the household in the low-density project census tracts than that for the high-density project tract.

The variation in neighborhoods within the public housing program was narrower than variation across the entire city. No housing projects were located in the most affluent areas of the city. The mean percentage of households living below the LICO in census tracts around the set of small projects listed in column 2 of Table II was 15.6 percent. For the city as a whole, the median household lived in a census tract with 12.7 percent of households below the LICO. Thus, the largest contrast in neighborhood quality obtainable within the public housing program is between youths who grew up in the poorest areas in the city and those who grew up in moderately low- to middle-income neighborhoods. (A contrast between the poorest and wealthiest areas is not possible within the program, but this contrast would not be very interesting, since relocation policies are not likely to place low- income families into affluent neighborhoods on a large scale.)

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

The relative neighborhood variation between the two groups of Toronto public housing census tracts was at least as great as the relative variation between households from large projects in Boston and Chicago and households who moved using Section 8 vouchers. The Toronto percentage variation was about the same as that of the Boston households for the experiment versus control group, and somewhat less than that for the Chicago groups. For example, 63.6 percent fewer households in Toronto census tracts around the smaller projects received social assistance than households in tracts around the largest downtown projects. In Boston, welfare participation was 36.2 percent less in the Section 8 census tracts than in the control tracts and 68.7 percent less in tracts for those from the experiment group.

Overall, Table II shows that the neighborhood quality variation within the Toronto housing program was considerable, and similar to variation in the Boston MTO program. The Toronto projects cannot replicate the extreme conditions of poverty prevalent in the surrounding control census tracts in Chicago, where welfare participation was 75.0 percent, and 84.7 percent of households were headed by single females. Another important difference between Toronto and the two U.S. cities was the smaller percentage of blacks in Toronto neighborhoods. Neighborhood quality variation arises mostly from income segregation differences and not racial segregation differences.

Criminal Occurrences

Another approach to describing neighborhood quality variation across projects is by crime occurrences. I was able to obtain occurrence data for 1992 from MTHC's private security service. Beginning that year, MTHC security services collected data on every police or security report that occurred on MTHC property, including those that did not lead to an arrest or conviction. The occurrences were divided by type of crime and by whether the event was minor or serious. All serious events required, at minimum, a written report. The data were broken up by project. Total occurrences were divided by project household size. Importantly, the data included occurrences involving both residents and non-residents on MTHC property.

Table III presents 1992 crime and victimization occurrences, separated by housing project category. The largest projects in downtown had the greatest incidences of arson, bodily and sexual assault, drug offenses, neighbor disputes, and homicides per 1,000 households. 4.18 homicides per thousand household units occurred at the high-density projects in 1992, versus only .41 homicides per thousand household units for the low-density projects. There were no sexual assaults reported in the low-density projects, while 1.45 sexual assaults per thousand households were reported in the high-density ones. The general pattern the table reveals is that criminal activity occurred much more frequently in and around projects with greater concentrations of poverty, though these results do not necessarily imply the conditions of the largest projects led to more crime.[15]

C. The application process and the assignment of families into projects

Until 1995, applicants on the MTHC waiting list were selected on the basis of a points system. Households were given points primarily based on financial need but also on current living conditions, welfare participation, overcrowding, and whether they were living in emergency housing. Those with the most points were housed first, giving preference to families most in distress. High demand for subsidized housing meant only those families who attained the near-maximum number of points were given offers of accommodation, and even then, these families waited an average of one and one-half years. Key for this study, families could not specify which project or what type of project they wished to be housed. They were offered accommodation according to the first available unit with the correct number of bedrooms required while at the top of the waiting list.

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

Families in subsidized housing could request to transfer units if a change in employment location or family size occurred. The option to move projects because of poor neighborhood environment was not permitted. Guaranteed subsidies and tenant security were strong incentives to remain in the program (I find no significant differences in length of occupancy across projects in Section VI). Ekos Research Associates Inc [1991] conducted a representative provincial survey of families, single households, and seniors who left in the mid-1990s. The annual turnover rate of units for Ontario was about 13.5 percent, a figure similar to the turnover rate in Toronto's private market. Of the sample of leavers, 69.0 percent had lived in public housing for fewer than five years, while only 28.7 of my 1996 census sample of household heads in public housing moved in the last five years. This difference suggests that the hazard rate for leaving public housing falls substantially the longer a family remains in the program.[16] The main reasons the Ekos respondents gave for leaving public housing were relocation for employment, improved financial situation, and change in marital status.

An exception to the quasi-random nature of assignment into public housing was that families who expressed great disapproval with an initial offer would normally be given a second offer without being removed from the waiting list. Applicants who rejected their first two offers were removed from the list. Conversations with MTHC administrators revealed that initial rejections were rare because of the immediate desire to begin subsidies. A family could wait more than 6 months before receiving a second offer. Another exception to the assignment process was that applicants could specify up to 6 regional preferences. Regional preference were rarely expressed because the fewer the regions a family was willing to live in, the longer it waited for an offer. In Section VI, adding region fixed effects does not affect the results.

To examine the possibility that some families selected into particular housing projects, we can at least examine observable characteristics of program participants at the time of entry. Table IV compares households from the high-density and low-density projects discussed above. If sorting between groups is minimal, we should see little difference in means between the two neighborhood-quality types. I subdivided Table IV into all households (columns 1 and 2) and households with children (columns 3 and 4).

Single-parent households, immigrants, age of head, and number of children are distributed in similar proportions among large and small projects. Estimated means for household heads receiving welfare at time of entry are exactly the same. Blacks were slightly more likely to reside in the smaller projects, but this difference is not significant. Median income for the household head at entry was about $2,000 more for those in the smaller projects than for the large central-city projects. This was the only characteristic that differed significantly. The point estimates for education and income in Table IV suggest poorer and less-educated family heads were slightly more likely to live in the larger projects, which corroborates the idea that applicants who tended to pass up their first offer were in less urgent need of housing and often did not want to live in larger projects that had negative stigma associated with them. If these more selective parents were also more likely to foster their children's development, estimated differences in mean outcomes between projects with less or more low-income concentration are likely biased upwards. The potential for this bias reinforces my results in Section VI, since I find no significant impact from residential environment.

V. Data

A. Postal Code Addresses

Instead of relying on small survey samples that identify whether a family or household has participated in a public housing program, I took a different approach; matching public housing postal code addresses to micro data.[17] 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, often over only one block or a single apartment building. Approximately three-fourths of the population sample were located in public housing addresses with unique postal codes. Even smaller public housing dwellings often consisted of a row of townhouses with a single corresponding unique code.

Some families living in Cityhome projects pay private market rent. Families not participating in subsidized housing programs are more likely to sort across different public housing project neighborhoods, with those unable to relocate to more pleasant environments locating in the worst city neighborhoods and those with (perhaps unobservable) higher incomes locating in the better neighborhoods. Including children from these families does not invalidate the analysis, but does raise the upper bound of the project effect estimates.

To minimize the number of children selected from families outside public housing, I constructed three samples. Sample 1 included only the population from postal codes unique to MTHC developments. Every household in this sample received rent-geared-to-income. Of the 544 postal code addresses, 317 were uniquely identified so this sample contains most of the family public housing stock. In Sample 2, I included only households with single mothers receiving welfare.[18] As described below, more than half of all families in subsidized housing fell into this category. Sample 3 came from estimating a probit model on the probability of living in subsidized housing based on several observable characteristics.[19] I used the sample of households living in census tracts that contained public housing but not living at addresses with unique public housing postal codes, together with the sample of MTHC public housing households uniquely identified in Sample 1. The results are used to estimate the probability of living in public housing among the sample of households with public housing postal codes. Sample 3 includes all households whose estimated probability for living in public housing is above a particular cutoff (see Appendix B). Oreopoulos (2001) shows similar findings under each sample set. For convenience, all three samples are combined for this paper's analysis, since doing so did not affect the main findings or conclusions.

B. The Intergenerational Income Database

The postal codes from projects built before 1985 are matched to the Intergenerational Income Database (IID). The IID includes the universe of Canadian 16- to 19-year-olds who filed tax returns over a 6 year period beginning in 1982, 1984, and 1986 while still filing from home. By 1998, the cohort was 28 to 35 years old. Mothers and fathers are linked to these youths in the year the child first filed.[20] The IID tracks both parents and children longitudinally from 1978 to 1998. Data exist for each year an individual filed.

Each tax file contains a return address with postal code. The postal code for matching to projects was taken from the child's tax file. When a child did not file, the postal code from the father's tax file was used if both parents reported they were married or if the mother's file was missing that year. Otherwise, the mother's postal code was used. The match was done for all years from 1978 until the child was 17. Only children who lived in a project for at least two years were kept in the sample. If neighborhood influences are cumulative, then two years in a project may not be enough to be affected by neighborhood environment, so I also check in Section VI whether length-of-stay or age-at-entry interact with neighborhood quality.

I averaged each youth's adult earnings and income over a six-year period between 1993 and 1998. Welfare participation within this period is also recorded. As for information on family background, the IID contains detailed employment and transfer income data, as well as marital status and number of children. However, information about race, ethnic background, and education attainment is not available. Parental adjusted income was computed as the mother's and father's total income, divided by family size, with the first parent receiving a weight of 1, the second (if any) a weight of 0.8, and each child receiving a weight of 0.3. Parental income was averaged over 15 years, between 1978-92, or until the oldest parent reached 65. All dollar amounts were converted to 1992 Canadian dollars using Statistics Canada's Consumer Price Index.

The IID under-represents youths who had no attachment to the labor market during their teenage years, who left home before establishing such an attachment, or who participated in the underground economy without reporting income activity. Unfortunately, all three situations are plausibly more likely for children of families living in public housing. Hence, if worse outcomes are associated with non-taxfilers and if the likelihood of filing is a function of the public housing project assigned, the analysis may miss important neighborhood effects.

I examine this possibility with two approaches. One is to check whether differences exist by neighborhood quality and the average number of years an individual did not file (conditional on filing at least once). The relationship between neighborhood quality and the chances of never filing may be similar to the relationship between neighborhood quality and the chances of filing less. Thus, I used the distribution of number of times not filing for individuals by neighborhood quality to examine whether the latter association exists.

The potential that neighborhood quality affects the chances of filing when young cannot be completely ruled out, and this is one reason I also report my results from the 1996 census. Although restricted to outcomes for youths still living at home, the census is not subject to the same kinds of non-inclusion biases that the IID potentially faces and provides a useful cross-check on whether results from two substantially different datasets lead to similar findings. If the results indicate no neighborhood influences on income for the IID sample, but significant effects on education attainment for the census sample, we cannot exclude the possibility that the missing sample of non-taxfilers prevents us from identifying long-run effects in the IID. If the results indicate no neighborhood influences on outcomes in both the IID and in the census, we can make stronger interpretations from both datasets.

C. The Matched 1996 Census Data

I also matched postal codes to households in the 20 percent sample of the 1996 census. The cross-sectional nature of the census limited the analysis to possible neighborhood interactions on outcome variables for children while still living at home. I therefore restricted the public housing samples to all youths ages 16 to 25 living with at least one parent. Table V displays descriptive statistics of families and children in these samples in comparison to mean characteristics for the city population. Not surprisingly, the monthly rent reported by households in public housing was much smaller than the average monthly rent among all city renters. Average household income for families in public housing was about 40 percent of that for the city. Eighty-two percent of households in public housing are below the LICO, and a high percentage of the sample is comprised of single mothers (62 percent). Only seven percent of Toronto's family household population are black, and about 43 percent of families in public housing are black.

VI. Results

A. Differences in Means

A useful starting point is to estimate neighborhood effects for children from families in the private housing market who lived in the same census tracts as children from high or low-density public housing. We can then contrast these results, which make no attempt to account for omitted variable bias, with those that use the quasi-experimental setting of the program. Columns 1 to 3 in the top half of Table VI compare outcome means for youths from the census tracts containing the 7 largest housing projects to those from tracts with the 42 smallest projects (with less than 250 units in size in tracts with fewer than 25 percent of households below the Low-Income-Cutoff). From column 1, mean log income among males from the high-density project census tracts (but not from the projects themselves) is 9.90 compared to 10.14 for those from low-density project census tracts. Boys from the wealthier neighborhoods earned about 23 percent more than boys from the poorest neighborhoods in the city. In column 3, I show the predicted increase in log income from living in the low-density census tracts relative to high-density tracts after controlling for a complete set of age and region indicators, a variable for parental permanent income, parent welfare receipt, and parental marital status. Even when limited family background and regional controls are added, the estimate still implies that children who reside in the smaller project census tracts make, on average, 17 percent more than children from the larger project tracts. For welfare participation between 1993 and 1998, I used a probit model and the coefficient shown in column 3 can be interpreted as the estimated change in probability if an individual with mean background characteristics had lived in a small project tract rather than a large one. The estimated coefficient suggests welfare participation would fall by 27.9 percent if a young adult (male or female) lived in a low-density project census tract rather than a high-density one.

The prediction that neighborhood quality substantially affects future labor market outcomes disappears when examining outcomes of children from within the public housing program. Columns 4 to 6 in the top half of Table VI show the same set of results, but for youths from public housing, living in either low-density or high-density projects. Mean log income for males from the large central-city projects is near-identical to the mean for males from the small projects (9.95 and 9.97 respectively). The null hypothesis that neighborhood quality does not affect income cannot be rejected whether controlling for family background or not. This is reassuring, since unbiased neighborhood effect estimates under random assignment should not change with additional controls.

The fraction of youth from a large downtown project receiving welfare for at least one year during the 1993-98 period is 31.9 percent. For the smaller projects, the mean is 29.1 percent. The difference is not significant (p-value>.1). Fewer youths from smaller projects received welfare when older, and adding family background controls further reduces this difference to –1.5 percentage points. The small differences between project types for welfare participation also translate to small differences in total income. Boys from the larger projects earned, on average, 9.84 in log earnings averaged between 1993 and 1998, almost exactly the same average as those boys from the smaller, low-density projects.

The same pattern arises when comparing outcomes from the 1996 Census. The lower part of Table VI compares education attainment across high-density and low-density project neighborhoods for children living in public housing and those not. For 16 to 25 year-olds not living in public housing, the mean completed years of schooling for youths is 12.4 for those in the high-density census tracts, and 13.1 for those in the low-density tracts. The predicted effect from growing up in one of the wealthier tracts is a 0.34 year increase in education attainment, after controlling for the same set of variables used in Table VI above, plus race and ethnicity. Among public housing participants, however, education attainment is slightly less for those in smaller projects (although the estimates are more imprecise due to small samples). Another outcome variable to examine from the census is idleness – not working and not going to school. The idle rate is lower for non public housing participants in the low-density project census tracts. The idle rate among youths in public housing is considerably higher, but does not differ significantly across project types.

As discussed in section IV, neighborhood conditions may possibly influence the likelihood of not filing a tax form. The fourth row from Table VI displays mean differences in the number of years an individual did not file taxes since age 16. On average, adults who filed at least once from the largest projects did not file 2.27 times, and the mean number for adults from the largest projects is 2.33. A Komogorov-Smirnov test that the two empirical distributions for number of times not filing are the same gives a p-value of .771. These results and the presence of similar findings with census and IID data suggest we should not expect to see conclusions change if we were able to include the missing persons from the administrative data.

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

The first part of Table VII contrasts all large, medium-size, and small projects in the program. Column 3 shows that 32 percent of youths from both small and large projects received welfare at least once between 1993 and 1998. Income and earnings differences for men who grew up in different public housing projects are also tiny. The average men's total income is about 1.6 percent more than the amount for those from the largest projects. Mean log earnings for youth from small, medium, and large projects is 9.80, 9.83, and 9.80 respectively. Family background controls did not alter outcome differences very much. .

The next set of rows categorizes public housing projects by whether they are managed by MTHC or Cityhome. MTHC projects are older, usually larger, and have residents who all receive subsidized rent. Cityhome buildings are smaller and mix subsidized tenants with those paying market rents. Even without controlling for observable characteristics, the estimated mean outcomes are not significantly different.[21]

Table VII also classifies projects by conditions within the surrounding census tract. Those in the IID from census tracts with fewer than 15 percent of households with incomes below the LICO earned, on average, about $18,800 between 1993 and 1998; men in census tracts with more than 40 percent of households below the LICO earned about 2 percent less. The direction of the earnings and income differences are usually what would be expected if neighborhood influences matter. But the differences are mostly between 0 and 2 percent and not statistically significant.

We might expect differences to arise from whether youths lived in highrises of five or more stories or in townhouse complexes. Townhouses offer more space between neighbors and front doors that lead directly outside, rather than to corridors and elevators. Families are more likely to avoid contact with other tenants if they live in a townhouse. Table VII, however, indicates no substantial differences in earnings, welfare participation or education attainment between these dwelling types, whether family background controls are included or not.

B. Wage and Schooling Distributions for Youth from Different Projects

The large samples facilitate a comparison of the entire distributions of long run outcomes between youths from the high and low-density projects. Figure II, panel A, shows the kernel density estimates of total income for youths from projects with fewer than 250 units within census tracts that had fewer than 25 percent of households below the LICO. The kernel density estimate for youths from the seven largest central-city projects is overlaid on top of the density estimate for the smaller projects. Background controls are added in panel B by estimating the densities using residuals from the regression with log total income on age and region dummies, parental income, household head's marital and welfare participation status. The mean of the residuals, with both samples included, is zero.

Although every youth from the sample has a low-income family background and lived in public housing, the variance in Figure II is substantial. Participants in the right tail of the distribution fare quite well. The 85th percentile male from the high-density projects makes \$46,134, while the 85th percentile man from the low-density projects makes \$46,174. A high portion of youths also receives welfare regularly, as indicated by the spike in the left tail of the distribution in Figure II. The two sets of density estimates are remarkably similar, whether observable family and region controls are added or not. The Komogorov-Smirnov test cannot reject the hypothesis that the two empirical distributions are the same (p-value=0.53).

Figure III shows the kernel densities for log total earnings. These distributions are skewed to the right because individuals receiving welfare earn very little. Whether family controls are added or not, the densities between those from the largest and the smallest projects are almost identical. The Komogorov-Smirnov test that the two distributions are the same gives a p-value of 0.69.

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

The results presented above are based on individuals who lived in public housing for at least two years. This subsection examines whether conditioning on age at entry or on years lived in public housing alters the findings. Table VIII presents regressions of log total income (for males only) on age, gender, family background controls, and project quality. To keep the sample large, I dichotomized project quality between projects within census tracts with 35 percent of households below the LICO, and those within census tracts with 35 percent above it.

Column 1 includes indicator variables for entering public housing at ages 10 to 13 or 14 to 16. (The omitted indicator variable is entrance before age 10 to a project in a census tract with fewer than 35 percent of households below the LICO.) The coefficient for those who entered public housing between ages 10 through 13 is 0.07, indicating slightly better income performance than those who entered earlier. The estimate on log income for those who entered at ages 14 through 16 is 0.03.

Column 2 reflects the interaction of project quality and age of entry. For children who entered public housing before age 10, the coefficient estimate on the effect from living in a poorer-quality project is -0.01. For those entering after age 13, the coefficient on poorer neighborhood quality is positive but measured imprecisely.

The measure of neighborhood quality interacting with years lived in public housing also appears to make little difference for the subgroup who lived in public housing the longest. As column 3 reports, males who lived in poor-quality public housing for at least 11 years earned an estimated 2 percent less than those who lived in better-quality projects for the same amount of time. Men who lived in poor-quality projects for 5 to10 years earned an estimated 1 percent more than those from better-quality projects. And I find no project effect for men who spent less than five years in the program.[22]

D. Sibling and Neighbor Covariances

The analysis so far separates 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 in broad categories. Recall from section III B that we can also express the importance of neighborhood differences by measuring correlations between unrelated neighbors. If assignment into neighborhoods is random, the neighbor correlation represents the portion of the outcome variance attributable to observable and unobservable neighborhood differences. If some degree of sorting by project occurs, the correlation represents an upper bound of this amount. Neighbor correlations are presented below and contrasted with sibling correlations, which approximate the portion of the outcome variance attributable to family factors.

The first two columns of Table IX present the estimates of adult annual income correlations between brothers and between neighbors. I control for age by calculating the correlations of the residuals after regressing log income on boys' age and age squared in 1998.[23] I also control for other observable characteristics by computing the correlations of residuals generated from regressing log income on age, age squared, and my additional family background controls.

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

The income variance for the sample of men from public housing is larger than the city-wide variance. The finding seems surprising at first because subsidized housing participants come from more similar backgrounds than those in the city sample. We might expect mostly low-income outcomes for sons from low-income families. Nevertheless, many sons from low-income families escape low income themselves. Corak and Heisz [1999] show the relationship between fathers with low income and their sons' income is weak (at least for the Canadian population), leading to a wider variation in later labor market outcomes. The brother correlation estimate for the public housing sample is 0.287. In other words, knowing a brother's income helps predict about 30 percent of another brother's income.

Knowing a past neighbor's income, however, predicts nothing about another neighbor's income. I estimate a small and sometimes negative income covariance between unrelated boys from the same public housing projects. The estimate for the age-only adjusted neighbor covariance across projects is 0.008, compared with 0.045 for the city sample. Controlling for observable family background characteristics does not change the neighbor covariance estimates, all centered around zero.

Many siblings in my public housing samples receive welfare when they are older. Table IX shows the estimated sibling and neighbor covariances for the number of times on welfare between 1993 and 1998.[26] I used residuals from regressing on age and age squared to measure the covariance. The city variance estimate is 1.51 years. The corresponding brother covariance is 0.30. Family and community factors, therefore, explain about 20 percent of the total variance in years on welfare participation. The brother correlations in years on welfare among the public housing samples are similar. The point estimates for the correlation in years on welfare between project neighbors, however, is .008 and insignificant from zero.

I used the 1996 Census to calculate sibling correlations of years of schooling only between pairs of young siblings both of whom lived at home and neither of whom had necessarily finished their education. The sibling years of schooling correlation for the city is estimated at 0.380. For the public housing samples, the correlation ranges between 0.167 and 0.198. The schooling correlation between children in the same EA is measured at 0.048, and .033 once observable family background controls are included. The adjusted neighbor correlation is less than one-tenth the sibling correlation. Within public housing, the neighbor correlation estimate is small and negative.

VII. Discussion

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

The key finding from the analysis is that average education attainment, annual earnings, income, and welfare participation among youth from low-income families do not differ by the degree of low-income concentration in the neighborhood that the youth grew up in. I find youths in low-income families gain no advantages from living in middle-income neighborhoods in the suburbs and no disadvantages from living in the poorest neighborhoods in downtown Toronto. These results hold whether contrasting housing projects by low-income neighborhood concentrations, whether in townhouses or high-rise apartments, or by length of residency or age of entry.

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

These results are consistent with recent studies from the Moving to Opportunity experiment in the United States. Studies from the MTO program generally find small increases in employment participation and earnings among parents from housing projects who were assisted to move into much more affluent neighborhoods. Parents and children experienced large improvements in measures of well-being, such as overall resident satisfaction, crime incidence, and health. But in terms of standardized test results and school performance, researchers find few effects for the children who move to better neighborhoods. Indeed, one study reports that suspensions and disciplinary action were <u>more</u> likely for children who moved into better communities [Ludwig et al. (2001)]. We will have to wait many years before we can compare long-run effects from the MTO experiment with the results in my study. In the meantime, the findings from the Toronto public housing program suggest that any short-term benefit to parents or children from moving into a more aesthetic living arrangement does not translate into higher earnings or other labor market outcomes later on.

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

Appendix A: Estimating Sibling and Neighbor Correlations[27]

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

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

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

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

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

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

The variable $W_{fp} = \sqrt{[I_{fp}(I_{fp}-1)/2]}$ is the square root of the number of distinct brother pairs in family f and $W_p = \sum_{f=1}^{F_p} W_{fp}$ is the number of distinct pairs within project p.

I estimate the neighbor covariance by:

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

where $W_{ff'p} = \sqrt{I_{fp} I_{f'p}}$. In words, within each project I derive the average covariance between each unrelated neighbor pair. Each project covariance (against the sample population mean) is averaged over projects. Solon et al. [2000] give more weight is given to neighborhoods where there are more neighbor observations. For public housing samples, smaller projects will have fewer observations to work from. To avoid assigning greater weight to projects with larger samples, I allocate equal weight to all projects by setting W_p =1.[28] Another alternative is to group projects in the same census tract; doing so increases the sample to calculate the neighbor covariance.

Standard errors are estimated by bootstrapping with a succession of 1000 randomly chosen samples with replacement.

Appendix B: Data Specifics

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

A. Intergenerational Income Database

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

B. Truncation Rules for Variables

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

C. Weighting the IID

The full weighting methodology is discussed in Cook and Demnati [2000]. Since the IID does not include individuals who did not file an income tax return in their teenage years while still living at home, each of the three cohorts fails to capture the entire Canadian population. Compared to the population estimate from the 1986 census, the IID under represents the cohort population by 28 percent. For children in families with lower parental incomes, the coverage rate is lower. Compared to a full sample of Canadian taxfilers in 1998, the IID misses 36.2 percent of children in families with parental income less than $10,000 and 29.8 percent of children in families with parental income of $10,000 to $19,000. As parental income rises, IID's coverage rate goes up. The coverage rate for children with parental incomes greater than $40,000 is greater than 75.0 percent. Coverage varies across gender and geography dimensions, although these differences are not as pronounced.

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

D. Sample 3 creation

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

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

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Notes

1. Using data from the original paper files of the Gatreaux program, Kling and Votruba [1999] find placement assignments were not entirely random. Pre-program differences were found between the racial makeup of the intake neighborhood, car ownership, and family composition. Not conditioning on these background factors might explain why the more controlled experiment from the Moving to Opportunity Program finds weaker results.

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

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

4. 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.

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

6. 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.

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

8. 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.

9. Non-profit organizations, including cooperatives, also provide subsidized housing to low-income families in Toronto. The vast majority of non-profit projects were built after

1990. And since my main dataset uses a sample of teenagers living in subsidized housing before this time, I excluded these groups from my analysis.

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

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

12. Alternative definitions of neighborhood quality will also be used in section VI.

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

14. In Katz et al. [2000], mean tract characteristics were computed for participants, whether they moved or not. Given the portion of movers and assuming the mean tract characteristics of those who did not move were the same as those for the control group, mean tract characteristics for movers only can be backed out.

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

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

17. 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.

18. Sample 2 family heads in the administrative data are single mothers who received welfare in any year between 1993 and 1998. In the census, Sample 2 family heads include single mothers receiving more than $3,000 in "Other government transfers", which included welfare in 1996.

19. For the administrative data, the independent variables used were household head's age, child's age, family size indicators, marital status when the child was a teenager and when the child was 25, permanent family income, whether receiving SA, and years living in public housing postal code between 1978 and 1990. The probit model with the census data used household head's age, child's age, race indicators, household head's education attainment, total family income, whether on SA, marital status, family size

indicators, immigrant status, and whether moved in the last five years. See Appendix B for more details.

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

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

22. Oreopoulos [2001] shows similar results when interacting age of entry and years in public housing with neighborhood quality in estimating effects on welfare participation for males and female combined.

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

24. The variance is based on all families with boys in the sample, whereas the brother covariances are based on families with at least two brothers in the sample. Measuring the variance among families with at least two brothers does not change the estimate much. This is true also with the public housing samples.

25. Caution must be taken with comparing city-wide to nation-wide correlations. Page and Solon [1999] find their brother earnings correlation drops to 0.186 after controlling for urban city and region. No previous studies have estimated sibling earnings correlations in Canada.

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

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

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

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

Table I
Theories of Social Interaction

Theory	Main Concept	Literature Examples
1. Peer group influences and role model effects	Individual decisions are influenced by characteristics or behavior of community members.	Akerlof (1997), Akerlof and Kranton (2000) Banjeree (1992), Brown et al. (1986), Brown (1990), Crane (1991) Glaeser and Scheinkman (2001)
2. Benefits from social networks	Network of friends, relatives, or neighbors assist in finding jobs, providing loans, or giving psychological support.	Borjas (1995), Bertrand et al. (2000) Coleman (1988), Granovetter (1995) Montgomery (1991)
3. Limited local resources	Quality and efficiency of local institutions are limited by community resources.	Beabou (1996), Durlauf (1996), Hoxby (2000)
4. Conformism	Without full information, individuals emulate observed choices of others.	Bernheim (1994), Bikhchandani et al. (1992) Jones (1984), Sah (1991)

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

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

Notes: "LICO" is Statistics Canada's Low-Income-Cut-Off. "Diff. In Means" is the mean difference between census tract characteristics among households in "smallest" public housing projects and households living in the seven "largest" downtown housing projects. "Smallest" projects are defined as projects with fewer than 250 units, within census tracts with fewer than 25 percent of households living below the LICO. Standard errors are reported in parentheses, adjusted for household level clustering; Data for Boston is from Katz, Kling, and Liebman (2001), Table 4. Data for Chicago is from Rosenbaum, Harris, and Denton (1999), Table 1.

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

Type of Occurance	Downtown-Central Largest Projects	Smallest Projects	Difference
	per 1000 household units		
Arson	1.12	0.00	-1.12
Assault causing bodily harm	17.02	4.33	-12.69
Sexual assault	1.45	0.00	-1.45
Break and enter and attempted B&E	22.00	18.90	-3.10
Drug offense	14.61	7.09	-7.53
Neighbor dispute	436	307	-129
Homicide	4.18	0.39	-3.78
Project Sample Size	7	35	

Notes: Occurences are all incidents on MTHC property that required a written report by MTHC Security Services Column (2) shows the mean difference between crime occurances among the seven largest downtown public housing projects and the 35 "smallest" projects. "Smallest" projects are defined as projects with fewer than 25(units, within census tracts with fewer than 25 percent of households living below the LICO. Standard errors are reported in parentheses.

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

	All Households		Households with Children	
	Downtown-Central Largest Projects	Diff. In Means Smallest - Largest	Downtown-Central Largest Projects	Diff. In Means Smallest - Largest
Census Data				
Single	0.340	0.025	0.594	-0.003
		(0.02)		(0.02)
Immigrant	0.689	-0.041	0.669	-0.020
		(0.02)		(0.02)
No high school diploma	0.472	-0.032	0.471	-0.032
		(0.02)		(0.02)
Black	0.324	0.029	0.293	0.029
		(0.02)		(0.02)
BA or greater	0.065	0.005	0.062	0.005
		(0.02)		(0.01)
Moved in last five years	0.532	0.004	0.522	0.004
		(0.01)		(0.02)
Age of household head	42.79	-1.62	40.41	-1.62
		(0.65)		(0.59)
Number of children	1.319	0.118	2.301	0.118
		(0.07)		(0.08)
Median income	10,583	2,689	12,589	2,354
Percentage on social assistance	0.538	-0.085	0.613	0.000
		(0.02)		(0.03)
Sample size	923	770	529	479

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

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

Household Heads, Ages 16-55 in 1996

	(1) All Toronto	(2) Non PH Residents Living in PH Census Tracts	(3) Public Housing Residents
	(sample averages and standard errors)		
Household total income	53,108	20,832	13,707
	(68,365)	(27,587)	(1,642)
Household total wages	41,266	13,907	7,471
	(58,167)	(24,771)	(14,507)
Monthly rent	749	442	381
	(331)	(335)	(326)
Under LICO	0.22	0.64	0.82
	(0.41)	(0.48)	(0.41)
Moved in last five years	0.30	0.29	0.29
	(0.70)	(0.71)	(0.70)
Age of household head	44.57	42.29	42.01
	(12.50)	(12.23)	(12.46)
BA or greater	0.24	0.12	0.06
	(0.43)	(0.32)	(0.26)
female	0.33	0.56	0.65
	(0.47)	(0.50)	(0.49)
Black	0.07	0.32	0.42
	(0.25)	(0.47)	(0.49)
Immigrant	0.50	0.63	0.63
	(0.50)	(0.48)	(0.46)
Dependent Children in Census, ages 16-25			
Age	20.53	20.11	19.97
	(3.22)	(3.15)	(3.12)
Black	0.07	0.32	0.40
	(0.26)	(0.47)	(0.49)
Idle	0.06	0.13	0.15
	(0.23)	(0.34)	(0.35)
Immigrant	0.27	0.41	0.43
	0.44	0.47	0.50
Children sample size	258,201	8,606	5,180

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

Table VI
Mean Outcomes and Mean Differences between Youth From Largest and Smallest Public Housing Projects and Between Youths from these Census Tracts but Not From Public Housing

	(1) Mean High Density Census Tracts	(2) Mean Difference High-Low Density Tracts, No Controls	(3) Dummy Coeff. For Low Density Tracts with Controls	(4) Mean High Density Projects	(5) Mean Difference High-Low Density Proj., No Controls	(6) Dummy Coeff. For Low Density Proj. with Controls
IID Data (Adults ages 28 to 35 in 1998)						
	Youths Not From Public Housing, but in PH Census Tracts			Youths From Public Housing		
Log income (males)	9.90	0.236	0.178	9.95	0.024	0.016
		(0.033)	(0.032)		(0.024)	(0.024)
Log earnings (males)	9.89	0.201	0.154	9.84	-0.004	0.011
		(0.034)	(0.033)		(0.033)	(0.033)
Receiving SA	0.24	-0.096	-0.067	0.32	-0.028	-0.015
		(0.010)	(0.010)		(0.018)	(0.018)
Number of times not a tax filer				2.27	0.060	0.119
					(0.114)	(0.112)
N (males)	14922	4439		719	1154	
Census Data (Youths ages 16 to 25 living at home)						
	Youths Not From Public Housing, but in PH Census Tracts			Youths From Public Housing		
Total years of schooling	12.41	0.680	0.344	12.34	-0.076	0.177
		(0.065)	(0.073)		(0.179)	(0.171)
Less than high school	0.13	-0.039	-0.037	0.15	0.005	0.011
		(0.003)	(0.003)		(0.025)	(0.027)
Idle	0.06	-0.019	-0.016	0.18	-0.021	-0.017
		(0.002)	(0.002)		(0.031)	(0.030)
N	10839	1072		226	390	390

Notes: The first three columns include the sample of youths from census tracts containing either the "smallest" or "largest" public housing projects, but who are not from public housing themselves. The "Smallest" projects are defined as projects with fewer than 250 units within census tracts with fewer than 25 percent of households living below the LICO. The "Largest" projects are the seven largest downtown housing projects. Columns (2) and (5) show the mean difference between outcomes among youths from the "smallest" project census tracts and projects from the largest housing project tracts or projects. None of the differences in columns (5) or (6) are significant from zero (p-value < 0.10). Columns (3) and (6) show dummy coefficient estimates from regressing the outcome variable on age dummies, gender, log parental income, parental marital status, whether parent received social assistence, family size, and dummy variables for the indicated measure of neighborhood quality. The estimates based on census data also include indicators for blacks and recent immigrants. For binary outcome variables, a probit model was used, and the coefficient estimates shown are the estimated change in probability from a discrete change in the indicated dummy variable, evalualted at the mean values for the independent variables. Standard errors are reported in parentheses, adjusted for household level clustering.

Table XII

Means and Difference from Means for Various Public Housing Neighborhood Quality Measures, Without Family Background Controls

	IID Sample				Census Sample				
Youth From Public Housing Projects	(1) Log Income (males)	(2) Log Earnings (males)	(3) Receiving Welfare	(4) Number of Times Did Not File Taxes	(5) Total years of schooling	(6) Less than High School	(7) More than High School	(8) Idle	(9) Samp. Size (for col. 3)
					By Number of Household Units				
<= 150 Units (mean)	10.00	9.80	0.32	2.20	12.36	0.14	0.30	0.16	1065
> 150, <= 700 Units (diff)	-0.003	0.025	-0.021	0.175	-0.135	0.004	-0.011	-0.013	3505
	(0.023)	(0.031)	(0.013)	(0.076)	(0.106)	(0.017)	(0.023)	(0.018)	
> 700 Units (diff)	-0.016	0.002	0.002	0.136	0.036	0.001	0.012	-0.001	1189
	(0.026)	(0.036)	(0.011)	(0.091)	(0.133)	(0.022)	(0.027)	(0.022)	
					By MTHC or Cityhome Development				
MTHC (mean)	9.99	9.81	0.31	2.33	12.28	0.14	0.30	0.16	5432
Cityhome (diff.)	0.03	0.03	-0.025	0.105	0.137	-0.004	0.015	-0.008	324
	(0.036)	(0.049)	(0.020)	(0.119)	(0.114)	(0.018)	(0.024)	(0.019)	
					By Percent in Census Tract Below LICO				
<=0.15 (mean)	10.03	9.84	0.29	2.33	12.11	0.15	0.29	0.17	390
>0.15, <=0.40 (diff.)	-0.02	-0.02	0.013	0.027	0.158	-0.010	0.015	-0.013	3656
	(0.037)	(0.049)	(0.020)	(0.116)	(0.190)	(0.030)	(0.041)	(0.031)	
>=0.40 (diff.)	-0.02	-0.02	0.014	-0.060	0.155	-0.008	0.017	-0.008	1710
	(0.034)	(0.050)	(0.020)	(0.122)	(0.199)	(0.032)	(0.042)	(0.032)	
					By Highrise or Townhouse				
Highrise (mean)	9.99	9.81	0.31	2.39	12.34	0.14	0.31	0.14	1884
Townhouse (diff.)	0.005	0.001	-0.014	-0.140	-0.013	0.001	-0.019	0.004	3537
	(0.018)	(0.025)	(0.011)	(0.064)	(0.099)	(0.016)	(0.021)	(0.016)	

Notes: Columns 1-4 show raw means for particular neighborhood quality categories from the IID sample, and average deviations from these means for the other categories. Columns 5 to 8 show means and mean differences for variables from the 1996 Census sample. Standard errors from regressing the outcome on dummy variables for neighborhood quality are in parentheses, adjusted for clustering by project. 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 columns 4 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. The census sample includes children ages 16 to 25 still living in public housing with their parents.

Table VIII
Log Adult Income Regressed on Background Variables and Project Characteristics, Interacted with Age Entered Public Housing and Years Lived in Program

	Log Adult Income			
	(1)	(2)	(3)	(4)
Age Entered Public Housing				
10-13	0.05	0.07		
	(0.03)	(0.04)		
14-16	0.01	0.03		
	(0.03)	(0.03)		
More than 35 percent in CT below LICO	-0.01	-0.01	-0.02	0.00
	(0.02)	(0.04)	(0.02)	(0.03)
More than 35 percent in CT below LICO		-0.02		
*** Entered age 10-13**		(0.05)		
More than 35 percent in CT below LICO		0.05		
*** Entered age 14-16**		(0.05)		
Years lived Public Housing				
5-10			0.00	0.00
			(0.03)	(0.04)
11+			0.01	0.02
			(0.03)	(0.03)
More than 35 percent in CT below LICO				0.01
*** 5-10 years in Public Housing**				(0.04)
More than 35 percent in CT below LICO				-0.02
*** 11+ years in Public Housing**				(0.04)
N	4530	4530	4530	4530

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. All regressions include additional controls for age, age squared, female (for welfare particpation), log parents' total income, and an indicator for single parent and parent on welfare.

Table IX

Estimated Sibling and Neighbor Covariances

	Total Income (males)		Earnings (males)		Number of Years of Welfare (93-98)		Total Years of Education (16 to 25 year-olds Living in PH)	
	All Toronto	Public Housing	All Toronto	Public Housing	All Toronto	Public Housing	All Toronto	Public Housing
Variance	0.335	0.376	0.477	0.603	1.515	3.655	3.826	3.360
	(0.007)	(0.008)	(0.005)	(0.018)	(0.039)	(0.097)	(0.043)	(0.190)
Siblings								
Sibling covariance	0.101	0.108	0.116	0.102	0.301	0.833	1.455	0.563
	(0.006)	(0.019)	(0.006)	(0.031)	(0.022)	(0.162)	(0.041)	(0.101)
Sibling covariance after controlling for observable family characteristics	0.081	0.098	0.098	0.091	0.253	0.722	1.319	0.490
	(0.004)	(0.018)	(0.005)	(0.028)	(0.020)	(0.150)	(0.039)	(0.177)
Neighbours within EAs (Toronto sample) or projects (Public Housing sample)								
Neighbor covariance	0.016	-0.001	0.016	0.005	0.025	-0.063	0.185	-0.015
	(0.013)	(0.028)	(0.013)	(0.004)	(0.028)	(0.133)	(0.065)	(0.131)
Neighbor covariance after controlling for observable family characteristics	0.002	-0.002	0.002	0.005	0.033	-0.073	0.125	-0.059
	(0.000)	(0.028)	(0.000)	(0.004)	(0.015)	(0.129)	(0.028)	(0.111)
Sample size	132,412	4,192	132,412	4,192	208,514	5,329	91,212	1,341
Number of sibling pairs	16,485	772	16,485	659	38,541	1,042	35,043	542
Number of neighbour pairs	1,025,426	61,468	1,025,426	68,853	2,556,912	98,633	621,201	13,109

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

Figure 1 MTHC and Cityhome Public Housing Projects in Metropolitan Toronto

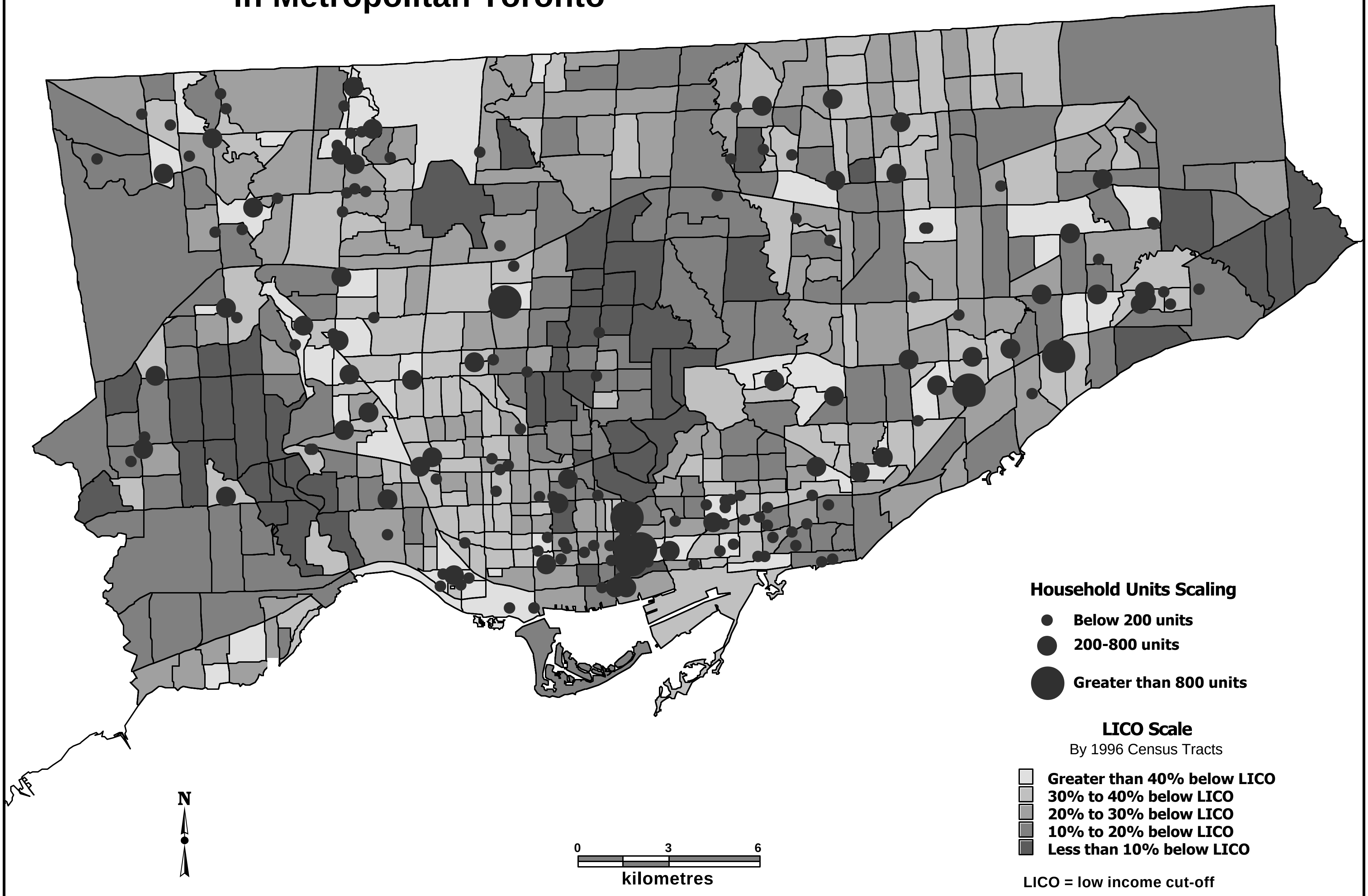
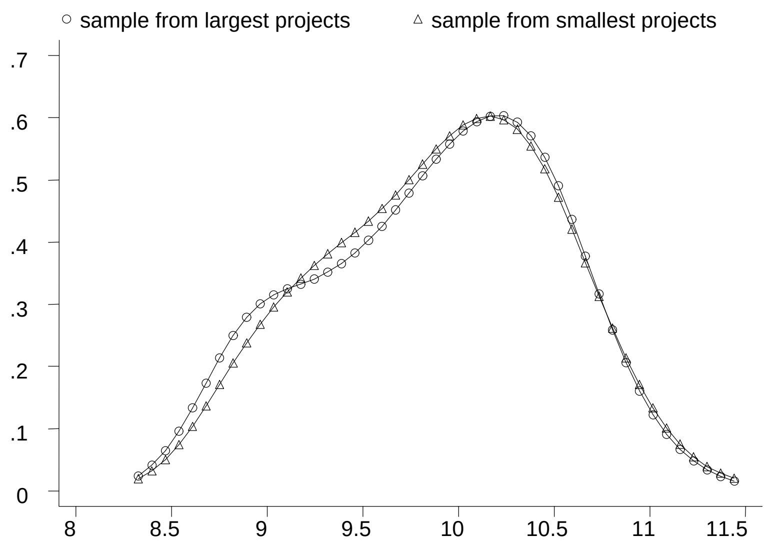
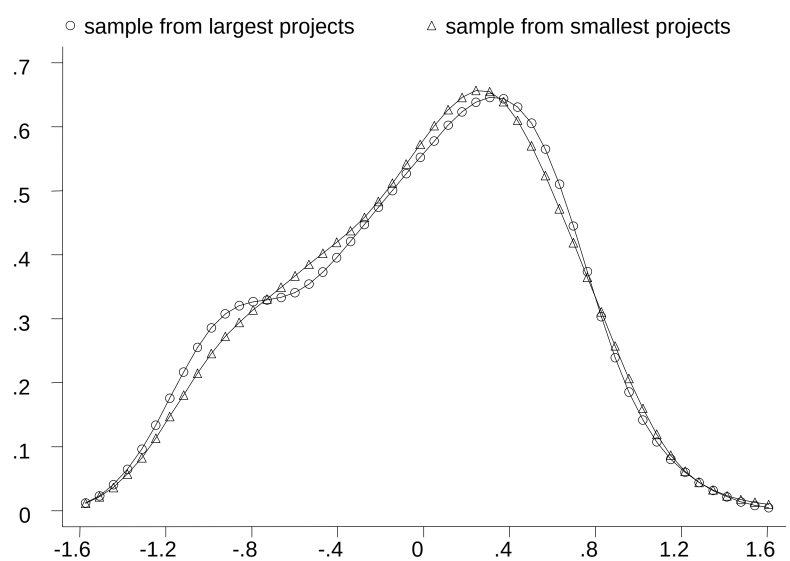


Figure 2
Kernel Densities for Log Total Income
For Men from High and Low Density Public Housing Projects

A: No Controls: Bandwidth = 0.165



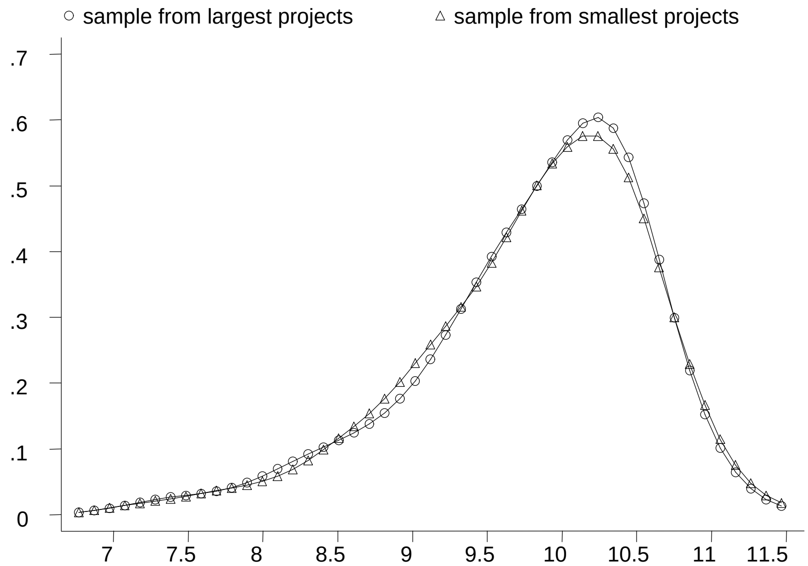
B: Age and Family Background Controls: Bandwidth = 0.165



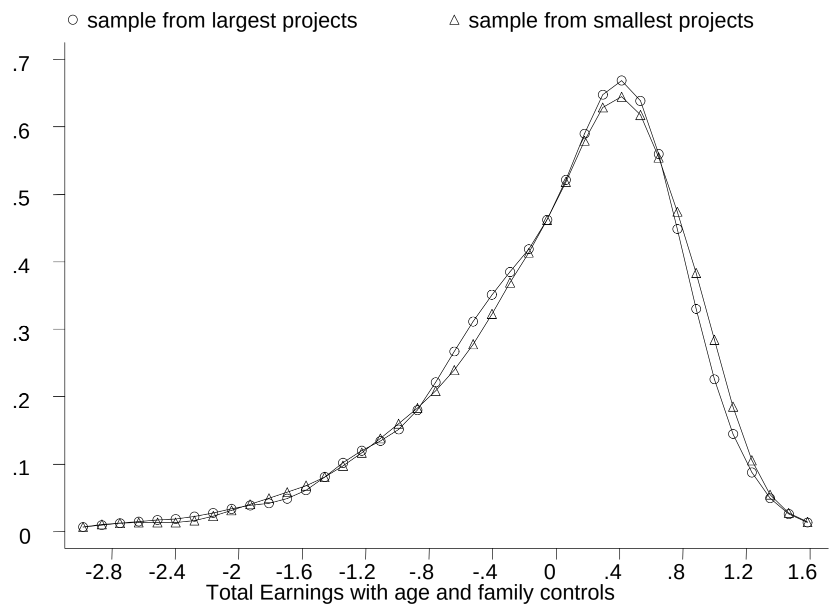
Notes: The two kernel densities overlaid in Panel A and B are for the sample from the 7 largest downtown projects and the sample from projects with 250 units or fewer, and in census tracts with fewer than 25 percent below the LICO. Residuals in Panel B are generated from regressing log total income on a full set of age and region dummies, plus family background controls. See text for further details.

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

A: No Controls: Bandwidth = 0.165



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



Notes: The two kernel densities overlaid in Panel A and B are for the sample from the 7 largest downtown projects and the sample from projects with 250 units or fewer, and in census tracts with fewer than 25 percent below the LICO. Residuals in Panel B are generated from regressing log earnings on a full set of age and region dummies, plus family background controls. See text for further details.