A Critique on Neighbourhood Effects in Canada

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# Table of Contents

Abstract........................................................................................................................................... 1  
Résumé................................................................................................................................................ 3  

1. Introduction.................................................................................................................................. 5  

2. Concentrated poverty in Canada and the United States ......................................................... 6  

3. Social interactions within neighbourhoods and without neighbourhoods......................... 10  

4. What do studies using random or quasi-random experiments find?...................................... 12  
   The Gautreaux Assisted Housing Program ............................................................................. 13  
   Housing vouchers and building demolitions to housing projects in Chicago ................... 14  
   The Moving to Opportunity Program .................................................................................. 14  
   Living in large and small public housing projects in Toronto ............................................ 16  

5. What do studies with non-experimental data find?............................................................... 17  
   Identification problems with non-experimental data ......................................................... 17  
   Studies using regression with non-experimental data ....................................................... 19  
   Some alternative approaches............................................................................................... 22  

6. Related Evidence of Neighbourhood Effects........................................................................ 24  
   Student and roommate effects ............................................................................................ 24  
   Evidence of Spatial Mismatch ............................................................................................. 25  
   Community intervention programs ..................................................................................... 25  
   Neighbourhood Warden Programs ..................................................................................... 26  

7. Conclusions............................................................................................................................. 26  

Notes ............................................................................................................................................. 28  

References..................................................................................................................................... 31  

Appendix: Omitted Variables and Measurement Bias .................................................................. 41
Abstract

Interest in neighbourhoods is growing in Canada
This report provides an overview of the main issues and most notable findings regarding the importance of neighbourhood effects in Canada. Attention to neighbourhoods is growing, as more researchers and policy makers become interested in social networks, and large cities become more ethnically diverse. While most social programs target individuals and households, programs that target particular communities potentially benefit a large number of individuals more effectively than targeting everyone separately. Discouraging a few from committing crime, for example, may make everyone in a neighbourhood feel safer. Supporting a few residents to find work may inspire others to do the same.

Exposure to high-poverty neighbourhoods in Canada is small
In 2000, about 900,000 Canadians lived in 234 high-poverty neighbourhoods, defined as census tracts with at least 40 percent of households below Statistics Canada’s low income cut-off (LICO). Overall lifetime exposure to high-poverty neighbourhoods, however, is small. About 10 percent of low-income households in Canada live in low-income areas. The average length of time these households spend in low-income neighbourhoods is about four years. Much of the research on high-poverty areas originates from the United States, but levels of distress within high-poverty neighbourhoods in Canada is not the same. Canadian low-income neighbourhoods experience much lower levels of crime and visible minority segregation. Many households living in Canadian low-income areas are recent immigrants and move within five years.

Social interactions and neighbourhood effects
Living in a high-poverty neighbourhood may increase the likelihood of negative social interaction, but there is no a priori reason to believe the increase is significant. First, compared to the entire set of people we connect with regularly and in important ways, encounters due to residential location may be small. Second, within any neighbourhood, many opportunities for positive and negative social interaction occur. Adults and children differ enormously in attitudes, aspirations, and ideals within the same community. Third, neighbourhoods may matter in important ways to a few residents, but detecting this importance when most people in the community are not affected is difficult.

Many problems arise when trying to estimate neighbourhood effects
The bulk of the literature using regression methods finds evidence of neighbourhood effects. Regression analysis essentially requires assuming that similar households that live in different neighbourhoods do so for reasons that do not alter subsequent outcomes of interest. Nevertheless, the many location options that face households and the many factors that determine the location decision for which a researcher cannot observe makes this assumption tenuous. As a result, studies that fail to address the pitfalls of regression will produce neighbourhood effect estimates that continue to generate grave scepticism among
many social scientists and statisticians. Recognizing this, most researchers have moved away from using this approach.

**Empirical evidence on neighbourhood effects**

The most persuasive research to date suggests residential environment matters most to an individual's mental health and exposure to crime, but has little influence on self-sufficiency and child development. Studies that exploit random or near-random assignments find almost no difference in subsequent earnings, education attainment, unemployment, and social assistance outcomes. Among low-income households, avoiding crime appears to be the dominant factor influencing a desire to move away from high-poverty neighbourhoods. Poor families that moved away from low-income areas were much more likely to report feeling satisfied with their residential environment, but the move had no effect on other parental socio-economic outcomes and possibly a negative effect on boys' behaviour. Dissatisfaction with one's residence and exposure and fear of crime appear to be the most important factors affected by neighbourhood quality. Small interventions that foster community involvement or prevent antisocial behaviour, such as the Neighbourhood Wardens Program in the United Kingdom, might have a large impact on residential well-being, although costs need to be taken into account.
Résumé

Les quartiers suscitent de plus en plus l’intérêt au Canada

Le rapport présente un aperçu des grandes questions et des principaux constats concernant l’importance des effets des quartiers au Canada. Ce sujet attire de plus en plus l’attention parce que, d’une part, un nombre sans cesse grandissant de chercheurs et de décideurs s’intéressent aux réseaux sociaux et, d’autre part, les grandes villes sont de plus en plus cosmopolites. Alors que la majorité des programmes sociaux s’adressent à la personne ou au ménage, les programmes pourraient être plus efficaces s’ils ciblaient les membres d’une communauté en particulier plutôt que de cibler l’ensemble des citoyens. Par exemple, décourager quelques personnes de commettre un crime peut faire en sorte que les résidents d’un quartier se sentent plus en sécurité ou encore, aider certains citoyens à trouver un emploi peut inciter d’autres personnes à faire de même.

La période de contact avec les quartiers très défavorisés est courte au Canada

En 2000 au Canada, environ 900 000 personnes vivaient dans les 234 quartiers très défavorisés, définis dans les secteurs de recensement comme un quartier comptant au moins 40 % de ménages sous le seuil de faible revenu (SFR) selon Statistique Canada. Toutefois, la période cumulative de contact avec les quartiers très défavorisés au cours d’une vie est relativement courte dans l’ensemble de la population. Environ 10 % des ménages canadiens à faible revenu habitent dans des zones à faible revenu et ce, durant quatre ans en moyenne. La plupart des recherches portant sur les quartiers très défavorisés ont été menées aux États-Unis, mais le niveau de détresse au sein des quartiers très défavorisés canadiens n’est pas comparable. En effet, on constate des taux beaucoup moins élevés de criminalité et de ségrégation des minorités visibles dans les zones à faible revenu d’ici. De plus, de nombreux ménages qui vivent dans ces zones sont des nouveaux immigrants et ils déménagent dans les cinq années suivant leur arrivée.

Interaction sociale et effets des quartiers

Habiter un quartier très défavorisé peut augmenter le risque que l’interaction sociale ait une influence négative, mais, a priori, il n’y a aucune raison de croire que cette augmentation est significative. Premièrement, le nombre de rencontres fondées sur le lieu de résidence est peut-être minime comparé au nombre de personnes avec qui on interagit sur une base régulière de façon significative. Deuxièmement, dans tout quartier, on a la possibilité d’entretenir des relations ayant une influence soit positive, soit négative. Au sein d’une même communauté, il y a une grande différence entre les attitudes, les rêves et les idéaux des adultes et ceux des enfants. Troisièmement, certains résidents peuvent être grandement affectés par leur quartier, mais il est difficile de découvrir dans quelle mesure ils le sont lorsque la majorité des autres résidents ne sont pas affectés.
Beaucoup de difficultés surviennent lorsque l'on tente d'évaluer les effets des quartiers

La majorité des recherches fondées sur les méthodes de régression démontrent l'influence du quartier. Essentiellement, les analyses de régression sont fondées sur le principe que des ménages semblables vivant dans des quartiers différents le font pour des raisons qui n'influencent pas les résultats qui nous intéressent. Néanmoins, cette hypothèse est affaiblie du fait que le chercheur ne peut évaluer les multiples possibilités de lieux où un ménage peut s'installer et la quantité de facteurs qui déterminent le choix du lieu. Par conséquent, les études qui ne tiennent pas compte des difficultés inhérentes aux méthodes de régression donneront des résultats qui ne feront qu'alimenter le scepticisme profond de nombreux spécialistes en sciences sociales et statisticiens. Devant cet état de fait, la plupart des chercheurs ont délaissé cette approche.

Données empiriques sur les effets des quartiers

La recherche la plus convaincante à ce jour suggère que le milieu de vie a une grande influence sur la santé mentale et l'exposition à la criminalité, mais très peu d'influence sur le développement de l'enfant et l'autonomie. Les études ayant recours à l'assignation aléatoire ou quasi aléatoire concluent qu'il n'y a presque pas de différence entre les résultats subséquents relatifs aux revenus, à la scolarité, au chômage et à l'assistance sociale. Parmi les ménages à faible revenu, le désir de fuir la criminalité apparaît comme le facteur déterminant qui pousse les ménages à vouloir s'éloigner des quartiers très pauvres. Les familles pauvres ayant quitté les zones à faible revenu étaient beaucoup plus enclines à se dire satisfaites de leur milieu de vie, mais le déménagement n'avait entraîné aucune répercussion sur les autres résultats socioéconomiques des parents. Par contre, il pourrait avoir eu une influence négative sur le comportement des garçons. L'insatisfaction envers son lieu de résidence et la peur de la criminalité ambiante semblent les deux premiers facteurs sur lesquels la qualité du quartier a une influence. Des initiatives simples visant à encourager l'engagement communautaire et à prévenir les comportements antisociaux, par exemple le Neighbourhood Wardens Program du Royaume-Uni, peuvent avoir une grande influence sur le bien-être des résidents. Évidemment, il faut tout de même tenir compte du coût d'un tel programme.
A Critique on Neighbourhood Effects in Canada

1. Introduction

We interact with people every day. The people we meet influence us in both positive and negative ways by helping to shape goals and attitudes, and by affecting socio-economic well-being. Individuals who are employed may help their unemployed friends find jobs, just as students who smoke may sway their classmates to smoke. The possibility that positive social interactions may be more likely to happen in some neighbourhoods than others intrigues many social scientists and policy makers. While most social programs target individuals and households, programs that target particular communities have potential for benefiting a large number of individuals more effectively than targeting everyone separately. Discouraging a few from committing crime, for example, may make everyone in a neighbourhood feel safer. Supporting a few residents to find work may inspire others to do the same.

Concentrated poverty and crime increased dramatically in the United States in the 1980s. Interest in neighbourhoods grew largely out of concerns for residents living in such places. Wilson (1987) was the first to suggest that a vicious cycle of crime, unemployment, and poverty perpetuates the quality of life for low-income households living in low-income areas. Wilson argued that adults in such neighbourhoods experience a social isolation that excludes them from the job network system permeating other neighbourhoods, and that children in these communities mostly interact on a sustained basis with people who are in families with parents unemployed, on social assistance, and with discouraged teachers. Redevelopment, relocation, or subsidizing job and business creation within these communities might help reverse these trends.

Canadian cities also experience substantial levels of neighbourhood segregation by income and ethnicity. A growing number of researchers question whether Canada should also spend more time thinking about the importance of local communities. As in the United States, no consensus exists over the merits of community-targeted policies. This paper hopes to highlight the current literature on neighbourhood effects. It provides an overview of the issues and the most notable findings. While several recent and good review articles on this topic already exist,1 this review differs in its objective to provide a primer on neighbourhood effects from a Canadian policy perspective.

I define neighbourhood effects as social interactions that occur close to an individual's residence and affect social and economic well-being. Neighbourhoods differ across many categories, but I focus on differences by affluence and poverty. The goal is to consider potential policies for improving outcomes for residents in these neighbourhoods, including relocation policies that aid low-income households to move to less segregated areas, redevelopment policies that improve the conditions of existing neighbourhoods, and community intervention programs that provide resources at the local, rather than the family or the individual level.
Much of the discussion carries over to defining neighbourhoods at the school district area, to the extent that segregation by student characteristics also occurs.

Section 2 explores the state of concentrated poverty among neighbourhoods in Canada, and compares this to the United States. The comparison is useful for discerning the extent of distressed areas in Canadian cities. The poorest neighbourhoods in Canada differ in at least two major respects from those in the United States: low-income segregation by race or visible minority status and violent crime occur much less frequently in Canada. These differences should be kept in mind when trying to extrapolate US conclusions to Canadian contexts.

Theories by which neighbourhood effects matter are discussed in Section 3. It is also important to consider why neighbourhood conditions may not matter, or may not matter enough for policies directed at communities to have a large effect. I describe how social interactions might strongly influence career and school success, but do not necessarily translate to important overall effects at the local community level.

Sections 4 and 5 describe Canadian and American empirical evidence for neighbourhood effects. Section 4 presents results from studies using random or quasi-random experiments; Section 5 discusses other evidence from non-experimental data. One main goal is to convey how difficult it is to determine whether neighbourhoods matter at all. This research still struggles in identifying whether links between neighbourhood conditions and outcomes are actually due to social interactions among neighbours or to underlying family circumstance that brought families to these neighbourhoods in the first place. I argue against regression methods that use observational data without carefully and directly addressing the many selection and measurement error problems that plague this approach. Research that explores situations where social contact occurs without individual consent delivers much more convincing results.

Section 6 concludes with a policy-oriented discussion and suggestions for further research.

2. Concentrated poverty in Canada and the United States

Before its recent demolition, Chicago’s Robert Taylor Homes public-housing development represented one of the United States’ poorest neighbourhoods. The development included 28 16-story high-rise apartments, which formed a kind of concrete curtain for traffic passing by on a nearby expressway. In 1999, 95 percent of the housing development’s 20,000 residents were without work and 75 percent of households were single parent. All residents were Black, and 82 percent of households were classified as being below the poverty line. The project experienced an escalation in crime in the 1980s, and several major street gangs began to occupy the property. Tenants described the gang and drug problem as one of total disruption to everyday life. Beatings, shootings, and other violent
crimes happened regularly and were often witnessed (and experienced) first hand.

In comparison, Toronto’s Regent Park represents one of Canada’s poorest neighbourhoods. Regent Park was built around the same time as the Robert Taylor Homes (in the late 1950s) and adopted a similar architectural style as a self-contained downtown community with no through traffic and little open space. The project currently houses about 7,500 residents in 2,087 high-rise and low-rise apartment units. In 2001, 67 percent of Regent Park households fell below Statistics Canada’s low income cut-off (LICO), 56 percent were single parents, and 59 percent of residents had no earnings. In contrast to the total racial segregation in the Robert Taylor Homes, only 16.5 percent of Regent Park’s population in 1996 were Black. The project now consists mostly of immigrants (69 percent), many of whom are recent immigrants from a wide range of countries including Somalia, Bangladesh, the Congo, Vietnam, China, and Latin America. It is difficult to obtain data to determine the extent of criminal activity by neighbourhood in Canada; however, what little there is suggests Regent Park experiences significant crime and drug activity, but at levels that appear much lower than those for Robert Taylor Homes. In 1992, there were 55 reported assaults causing bodily harm on Regent Park property, a rate of about 15 per 1,000 residents (26 per 1,000 households). That number is much higher than the 1995 rate of 1.7 assaults per 1,000 residents in the Toronto Census Metropolitan Area, but about the same rate for the entire population of Chicago (14.3 aggravated assaults in 1995).

To describe the state of concentrated poverty nationwide, we need a metric that applies consistently to thousands of local communities. Since we wish to explore potential effects of social interactions around residential neighbourhoods, it makes sense to choose areas over which we might expect important interactions to occur. Census tracts, containing between 2,500 and 8,000 residents, are used most frequently, in part because census tracts are designed to capture geographic and social boundaries to represent common impressions of neighbourhoods, but also because they are usually the smallest areas for which descriptive data are available. To a preschooler, the potentially influential neighbourhood may be no larger than the block around her house; to the teenager, the relevant area of interest may be the school district, which does not necessarily correspond to the census tract. The smaller the space chosen for analysis, the more likely residents within that space interact with each other, but the more likely it is that the analysis will miss important characteristics of other individuals in the surrounding space.

The common practice in the United States is to classify high-poverty neighbourhoods as census tracts when more than 40 percent of households are below the poverty line. The distribution of the poverty rate by census tract area is not bimodal, with one group of “good” neighbourhoods below this threshold and a separate group of “bad” neighbourhoods above it. But Jargowsky (1997: 11) argued, from site visits and local citizens’ subjective opinion, that using a 40 percent poverty rate cut-off comes close to matching areas that are
“predominantly minority [and] have a threatening appearance, marked by dilapidated housing, vacant units with broken or boarded-up windows, abandoned and burned-out cars, and men ‘hanging out’ on street corners.” Large public housing projects, like the Robert Taylor Homes development tend to dominate the few census tracts with poverty rates above 60 percent.

The United States experienced a notable decline in the number of high-poverty city census tracts, falling 27 percent from 3,414 to 2,510. The decline marked a stark reversal in the trend since 1970 of a rapidly rising neighbourhood poverty concentration. Many cities experienced a decline in the fraction living in high-poverty areas by more than half. Also striking, the proportion of households below the poverty line and living in high-poverty areas fell more sharply, especially for Blacks, even while the overall poverty rate declined marginally, from 13.1 percent to 12.4 percent. This implies a substantial change in US concentration levels of poverty during the 1990s, and suggests the strong economic growth over this period may have helped reduce city poverty and poverty concentration. Jargowsky pointed out, however, that these gains may have eroded due to the subsequent downturn since the 2000 Census, and that there was some evidence of increasing high-poverty areas for inner-ring suburbs in major metropolitan areas.

A number of researchers produce similar results for Canada, and define high-poverty neighbourhoods as census tracts with more than 40 percent of households below the LICO (e.g., Hajnal, 1995, MacDonnell et al., 2004) Heisz and McLeod (2004). In 2000, about 900,000 Canadians lived in 234 high-poverty neighbourhoods, with a disproportionately large number of these neighbourhoods in Montréal. Unlike the United States, the frequency of high-poverty neighbourhoods increased slightly over the decade. For all 27 census metropolitan areas (CMAs), the fraction living in these areas increased from 4.0 to 4.6 percent, and the fraction in these cities living below the LICO and in a high-poverty census track increased from 10.6 to 11.9 percent. From the four censuses over the 1980 to 2000 period, Heisz and McLeod found that both the portion living in high-poverty areas and the portion of low-income households living in high-poverty areas fluctuated in a see-saw pattern, but increased only slightly overall during these 20 years.8 Toronto on its own, however, experienced a steady increase in these rates over the same period (see also MacDonnell et al., 2004).

Unfortunately, the LICO and the United States poverty line are not directly comparable. It should not be assumed that Canadian census tracts that contain more than 40 percent of households below the LICO exhibit similar levels of distress as census tracts in the United States with more than 40 percent of households below the poverty line. I am not aware of any Canadian ethnographic analysis, such as American ones by Anderson (1991), Kirschenman and Neckerman (1991), and Jargowsky and Bane (1991), that assess by case examples the quality of life for residents in these areas. A comparative ethnographic analysis would be extremely helpful in determining just how unfavourable some
areas are in Canada, relative to the United States, and could provide a better understanding of the quality of life for residents living there.\(^9\)

At least one key difference exists between very low-income neighbourhoods in Canada and the United States. Black and Hispanic segregation is crucially intertwined with income segregation in the United States, whereas for Canada, it is not.\(^10\) Labour market outcomes and education attainment vary significantly between poor and non-poor neighbourhoods in similar ways for both countries, but not ethnic composition. Half of the 1990 population in US high-poverty neighbourhoods was Black, and another 28 percent was other visible minorities (by far mostly Hispanic). In other words, about three quarters of the population living in high-poverty areas in the United States are either Black or Hispanic, while only 18 percent of those living in other city neighbourhoods are made up of non-White residents. In Canada, only six percent of the 1996 population in Canadian high-poverty neighbourhoods were Black, and less than 30 percent of all visible minorities (mainly immigrants from many different origins) resided in these areas, compared to 15 percent of all visible minorities in other neighbourhoods. The relationship between ethnic segregation and income segregation is much less in Canada, in part because many recent immigrants start out in poor immigrant enclaves, but move on to more affluent neighbourhoods where their population share scarcely differs from that of the city as a whole. In addition, many Canadian immigrants living in poor ethnic enclaves share their community not only with their own minority group, but also with low-income migrants from many other visible minorities.

Another key difference is crime. Gannon (2001) documented much higher rates of violent crime in 2000 for US cities than for Canadian ones. Per capita homicides and aggravated assaults, for example were 3.1 and 2.6 times higher, respectively, in US cities. In the three largest cities of both countries, homicides in the United States exceeded those in Canada by a factor of 4.5. No study compares concentrated neighbourhood crime, but the well-documented finding that most violent crime remains heavily concentrated in low-income neighbourhoods is at least suggestive that high-poverty areas in the United States and Canada also differ significantly by crime intensity.\(^11\)

Concern about high-poverty neighbourhoods also depends on how long residents who move to these places stay there. Most new entrants into high-poverty neighbourhoods leave within five years. Frenette et al. (2004) found that households that move into high-poverty neighbourhoods in Montréal, Toronto, and Vancouver stay on average for 3.8 years before moving to less concentrated areas. However, the longer households live in high-poverty neighbourhoods, the less likely they are to leave. About one third of households moving into these places stay past six years. Quillian (2003) obtained similar results for the United States. Among low-income Blacks, he estimated more than half of those entering high-poverty neighbourhoods left within three years. Accounting for re-entry, he estimated the mean length of stay for entrants into high-poverty neighbourhoods at 5.4 years over a 10-year period.
So, to set the scene before examining evidence of neighbourhood effects, we should keep in mind that many neighbourhoods in Canadian cities are poor but do not exhibit the same degree of crime and racial segregation that occurs in high-poverty neighbourhoods in the United States. High-poverty neighbourhoods in Canada contain more immigrants, especially recent immigrants, and most move into and out of these neighbourhoods within five years.

3. Social interactions within neighbourhoods and without neighbourhoods

Proximity to work, parks, waterfront, and stores all affect household quality of living. Neighbourhood effects usually do not refer to these types of community amenities that benefit residents directly; rather, they refer to situations when contact with neighbours influences socio-economic well-being. Perhaps the most intuitive explanation by which neighbour interactions 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 personal preferences but on whether decisions would deviate from choices made by others in that individual’s reference group (Akerlof, 1997; Akerlof and Kranton, 2000; Glaeser and Scheinkman, 2001). Second, an individual’s social network may be an important resource. Personal contacts can improve an individual’s chances of finding a job, receiving advice and psychological support, or getting a temporary loan. Granovetter (1995), for example, concluded that jobs are often found through contacts formed long before seeking employment. Another way by which neighbourhoods may play a role is through conformism. In contrast to peer group effects, conformism models usually posit that individuals mimic neighbours’ behaviour, because they lack enough information to choose on their own (Bikhchandani et al., 1992; Bernheim, 1994; Jones, 1984; Sah, 1991).

In some ways, a preference to live near or far from particular people is itself a neighbourhood effect that impacts mental health and well-being. People may simply dislike living in neighbourhoods with a concentrated number of poor people, or they may prefer living close to family, friends, or individuals with a similar ethnic background. It is obviously hard to measure these impacts, but a growing body of research documents substantial preferences for living close to individuals with the same ethnic background (e.g., Bayer et al., 2002). Believing that neighbourhood effects exist might also be self-fulfilling. For example, parents that feel their children benefit by attending schools where other children perform well value moving close to these schools, whether or not such benefits actually occur. Interestingly, Rothstein (2003) found housing prices sharply rise in school districts where students perform well, while no such premium occurs for houses around schools that show evidence of improving student performance. In other
words, parents place more emphasis on the quality of students than the quality of schools when deciding where to live.

Of particular interest in regards to high-poverty neighbourhoods is the epidemic theory (Schelling, 1978; Wilson and Kelling, 1982). In epidemic models, residential environment matters little to social economic outcomes except after conditions deteriorate past a particular threshold, or “tipping point.” The rate of deterioration escalates suddenly after neighbourhoods at the tipping point experience a small rise in crime, vacancy, or unemployment. Neighbourhoods past this point experience a multiplier effect of increased crime, disregard for community upkeep, and flight by higher income residents. The existence of epidemics has important implications for policy. If tipping points exist, than addressing social problems in communities at this breakpoint can go a long way in preventing a multiplier effect of worsening outcomes. Wilson and Kelling (1982) suggested that basic street maintenance and broken window repair may help prevent an area from rapid decay and escalating crime.

One related community influence is how proximity of available jobs affects employment. The Spatial Mismatch Hypothesis arises when fewer jobs per worker in high-poverty areas make finding work more difficult. Research in this area tends to focus on inner-city minorities in the United States and how access to transportation and migration of jobs from the city to the suburbs affects employment prospects. The mechanisms by which employment is realized is independent of social interactions and so are not considered here as neighbourhood effects. I do mention briefly some empirical results from this literature in Section 5. Ihlanfeldt and Sjoquist (1998 a, b) review this literature. An important distinction needs to be made between social interactions and neighbourhood effects. Social interactions do not take place in geographic isolation; neighbourhood effects do. For interactions to matter at the neighbourhood level, social contact must depend significantly on where an individual resides, and neighbourhood relationships must be important enough to influence individual behaviour or opinion.

Interactions with other people can have positive and negative impacts on people’s well-being. Living in a high-poverty neighbourhood may increase the likelihood of negative social interaction, but there is no a priori reason to believe the increase is significant. First, compared to the entire set of people we connect with regularly and in important ways, encounters due to residential location may be trivial. Consider the census tract, for example. There are about 4,000 people in my census tract; (even more if you include people who work there). I know perhaps about 100 of them by face, and 15 by name. If I choose to limit my exposure to these people, it is relatively easy to do so. Contact with others outside my community – at running practice, work, parties – may be more important. Second, neighbourhood effects may be small because, within any neighbourhood, many opportunities for positive and negative social interaction occur. Adults and children differ enormously in attitudes, aspirations, and ideals within the same community. Venkatesh (2000) and Weyman (1994) documented heroic effort.
among mothers from the poorest city neighbourhoods like the Robert Taylor Homes and Regent Park in watching out for each other, forming action committees, and ensuring safe spaces for their children to play. Neighbourhoods that alter the probability of encountering positive or negative social interactions may make the difference in finding work or dropping out, but this difference may matter only to a few.

4. What do studies using random or quasi-random experiments find?

To verify theoretical models of neighbourhood influence, we turn to data. Many challenges face researchers who try to test empirically for neighbourhood effects. The main objective is to predict what would happen to particular outcomes of interest if we altered individuals’ environments: Susan’s smoking habit if fewer in her school smoked; Jamal’s self-sufficiency if assisted to move to places where more worked; Kate’s education attainment if allowed to attend a school with fewer dropouts. Unfortunately, we never get to observe the same individual under both circumstances.

Social scientists use statistics to infer counterfactual outcomes, estimating expected neighbourhood influences by comparing two (or more) individuals living in two (or more) neighbourhoods. Finding similar individuals to compare across neighbourhoods, however, is extraordinarily difficult and often prevents researchers from drawing strong conclusions. More affluent families can afford more expensive areas and even families with same incomes have a great deal of choice over where to live. The underlying characteristics that determine how and why families move to particular areas may themselves determine the outcomes that interest us. Untangling these background characteristic effects from potential neighbourhood effects is the central challenge that faces anyone trying to test for these forces empirically.

Random assignment helps substantially. By leaving residential location up to chance, we can remove any systematic relationship between family background and location. Individuals randomly selected to live in “good” environments and others selected to live in “bad” ones are initially, on average, the same. Subsequent differences in outcomes between these two groups can be credibly interpreted as caused by the original difference in location assignment. Opportunities to carry out such evaluations are rare, but do happen. Sometimes, organizations sponsor social experiments that randomly select families or communities to participate in programs that could potentially generate benefits for them, and researchers monitor outcomes of both those picked and those not. Natural experiments occasionally happen when a particular event occurs or when a program is set up in a way that generates changes to residential environments as if those changes happened by chance.

Random assignment eliminates biased neighbourhood effect estimates that arise from choice in the housing market, but drawbacks exist to this approach. The
results from an experiment, or natural experiment, apply only to the group affected by the program and may not be extrapolated to a more general population. The experiment also does not provide information on the overall impact if it were implemented on a larger scale. And it is impossible to determine what aspects of the different neighbourhoods led to improved outcomes. Meyer (1995) critiqued the benefits and pitfalls of experimental studies in more detail. Several natural and true experiments that look for evidence of neighbourhood influence are discussed below.

**The Gautreaux Assisted Housing Program**

A United States Supreme Court ruling in 1976 created a unique opportunity to examine neighbourhood effects. Dorothy Gautreaux led a group of plaintiffs to sue the Chicago Housing Authority, claiming that placement of poor families in public housing in poor neighbourhoods constituted a form of discrimination. The court imposed desegregating Black families from inner-city projects. Eligible families who were part of the Gautreaux plaintiff class and who requested to participate in this program received special housing vouchers to pay for private rental apartments in neighbourhoods in which no more than 30 percent of the residents were Black. Participants received assistance in finding housing in racially diverse city and suburban neighbourhoods. Between 1976 and 1998, the Gautreaux Assisted Housing Program moved 7,100 families to more than 100 communities throughout the Chicago metropolitan area: roughly half to integrated suburbs and half to integrated neighbourhoods in the city. Nearly all Gautreaux families placed in suburban areas were placed in tracts where less than 30 percent were Black and in areas with highly educated neighbours. Because the program also allowed moves into revitalizing urban areas, many city placements ended up in segregated areas with more than 30 percent in the census tract Black (in fact, 39 percent of central city moves were to tracts with more than 85 percent Black).

Rosenbaum (1991) argued that participants had little say over the location of the offers. Counsellors offered participants units as openings became available, according to the participant’s position on the wait list and regardless of preference for location. Although clients could refuse an offer, few did so, since they were unlikely to get another. As a result, participants’ preferences for city or suburbs had no effect on their placement location, and analyses indicated that the two groups were nearly identical.

Using a small sample of Gautreux participants that moved to both city and suburb apartments, Rosenbaum et al. (1999b), Rosenbaum (1995), and Popkin et al. (1993) concluded that parents’ employment outcomes and children’s education attainment were markedly better for those who moved to the less-segregated suburbs. Indeed, children moving to the suburbs were four times less likely to drop out of high school. One concern arises from the way the Gautreaux survey was taken. Only families still living in their original placement apartment in 1988 were located, so any program participant that did not move or moved since placement was excluded from the study. This limitation might lead to serious pre-move characteristic differences between city and suburb movers and invalidate
the quasi-random nature of the survey. Rosenbaum and DeLuca (2000) avoided this problem by tracking Gautreaux participants with government administrative data, whether or not they move, to look at effects from the program on welfare receipt 15 years later. Although they found no significant overall differences between participants who moved to suburban and city communities, they did find lower welfare rates among adults placed in census tracts with disproportionately more educated neighbours. DeLuca and Rosenbaum (2003) also examined whether Black families tended to move back to more segregated neighbourhoods after being placed into more integrated areas. Even though 84 percent of the families moved again over the 14-year period since placement, the segregation levels and city/suburban locations of the most recent neighbourhoods were strongly related to the characteristics of initial placement neighbourhoods.

**Housing vouchers and building demolitions to housing projects in Chicago**

On their own, Black families given housing vouchers to leave highly segregated public housing projects do not end up in substantially different neighbourhoods. Jacob (2003) examined families offered housing vouchers to move from buildings in Chicago housing projects set for demolition (including ones from Robert Taylor Homes discussed in Section 2). Many families chose to transfer to other public housing units. Families that did take up the voucher relocated close to their original residence, and very few students changed schools. Still, average census tract poverty rates for families given the vouchers did fall significantly. Jacob compared education attainment outcomes for children that moved because of building closures. After five years, he found building closures had no impact on children’s math test scores, attendance, retention, or drop-out rates.

**The Moving to Opportunity Program**

The US Department of Housing and Urban Development created the Moving to Opportunity (MTO) Program to address shortcomings of the Gautreaux research. Volunteers (mostly Black and Hispanic single mothers) from some of the largest public housing projects in five US cities (Boston, New York, Baltimore, Chicago, and Los Angeles) were randomly assigned into three groups. The Section 8 group was offered vouchers to help subsidize renting apartments in the private household market. The experimental group was given vouchers only for apartments in census tracts where fewer than 10 percent of households were below the poverty line. Most families initially resided in census tracts where more than 50 percent poor. The control group was not given any voucher, and had to move on their own if they wanted to leave.

There are two very important characteristics of the MTO Program. First, MTO is a true experiment: families offered vouchers and assistance to move from their current public housing project residence were selected randomly from a set of volunteers who wanted to participate in the Program. Second, the Program targeted the most disadvantaged families living in some of the most disadvantaged places. The literature on neighbourhood effects stems from concern over people living in extremely poor and distressed neighbourhoods, and the MTO Program targeted exactly these people. If neighbourhood differences matter at all,
neighbourhood effects should show up in the MTO experiment. The Program was set up to generate a contrast where one would expect to see an effect.

By far, the single most important reason for wanting to participate in the MTO study was to move away from crime. Perhaps not surprising then, the most dramatic effects of the experiment were on parents’ responses to neighbourhood satisfaction, feelings of safety, and mental health. Comparing parents in the control group to parents selected into the experimental group and moving under the program, the share reporting feeling safe at night climbed from 55 percent to 86 percent, five years after the move. The number of parents that witnessed drugs in the last 30 days fell from 45 percent to 20 percent. The share reporting being satisfied or very satisfied with the current neighbourhood increased from 48 percent to 77 percent. Parents were also 5 to 10 percent more likely to convey feeling calm, not worried, and not depressed. However, aside from these gains, the MTO experiment found virtually no other positive effects from moving to a low poverty neighbourhood after four to seven years. The offer of a housing voucher had no effect on adult earnings, employment, or receipt of public assistance. Children in the treatment group also showed no improvement on a wide range of school performance measures that included achievement scores, high school drop-outs, and post-secondary enrolment. There were, however, some important gender differences in the effects on a variety of behavioural and health outcomes. Some girls in the treatment group experienced moderate reductions in stress and depression, as well as a decrease in arrests for violent crime, while boys experienced an increase in behaviour problems, drug use, along with a rise in arrests for property crimes.17

Clearly, at least some families benefited from the relocation. But these gains need to be weighed against the financial costs of the program, and the potential negative effects on residents of the census tracts into which the MTO families moved. While MTO families in the treatment group felt safer and generally pleased with their neighbourhood, this probably directly contrasts with how neighbours would feel if the MTO Program was implemented on a larger scale.

While the MTO Program offers some of the most definitive evidence on whether neighbourhood influences exist, there are important limitations.

- First, the MTO Program estimated treatment effects off of low-income adults and children that had already been living in highly segregated areas for potentially quite some time; it is possible that the impact from previously living in very distressed housing projects dwarfs subsequent impacts from moving.18
- Second, the experiment involved moving to better neighbourhoods for the Section 8 and experimental groups, while the control group did not have to move. There may be additional effects from having to relocate that cannot be disentangled from the independent effects of the change in neighbourhood environment.
Third, not every family randomly assigned to receive a voucher was able to find a unit to which they wanted to move that met the Section 8 housing quality standards with a landlord who would accept the voucher.\textsuperscript{19} About 47 percent of the families assigned to the experimental group moved under the program, while 62 percent of the families assigned to the Section 8 group participated. The families that moved may have been exceptionally motivated and the effects on them from moving may not reflect the effects for the entire sample.

Fourth, participants in the Section 8 and experimental groups moved to substantially less poor neighbourhoods, but not ones that were substantially less segregated by race. Thus, the experiment cannot explore the effects on mostly Black families from moving to substantially less racially segregated areas.

Fifth, the average difference in neighbourhood conditions between those offered the voucher and those not offered the voucher narrowed somewhat, after subsequent moves by both groups. However, the conditions still remained significant. The average neighbourhood poverty rate after four to seven years for the control group was 39 percent, while the average for the experimental group that moved initially was 20 percent.

Sixth, the MTO studies do not evaluate the potential negative impact on the households in neighbourhoods that MTO participants move into.

**Living in large and small public housing projects in Toronto**

The varied types of public housing projects in Toronto provide another way of examining neighbourhood effects in a quasi-experimental setting. Before the early 1980s, Toronto public housing applicants were assigned points, based on housing need and financial distress. Those deemed most in need of subsidized housing (those with the most points) were offered units as they became available. Applicants had virtually no say over which project was offered to them. Some ended up in a few very large projects accommodating several thousand people, like Regent Park, while others ended up in projects near more residential and middle-income areas, in townhouses lodging far fewer numbers. City-block and census tract characteristics varied considerably across the projects. The share of households below the LICO in census tracts surrounding the eight largest projects was more than double that for the smallest projects (61 percent versus 25 percent respectively).

In a previous paper (Oreopoulos, 2003), I used administrative data to track children who grew up in these projects to more than 30 years of age. The data and quasi-experimental nature of the application process provided a unique opportunity to compare long-term measures of total income, wages, and public assistance among children that grew up in substantially different housing projects. The analysis found no difference in these outcomes across projects. While living conditions and exposure to crime varied substantially, no differences in eventual...
earnings, unemployment likelihood, and public assistance were found, even among youths that lived in projects more than five years.

Not everyone in the sample, however, ended up poor. Some youths from public housing went on to do quite well in the labour market, with wages well above the city average. When one brother from public housing grows up to escape poverty, chances are good that the other brother does the same. The correlation between brothers’ earnings is 0.26, which indicates that about 26 percent of the total variance in earnings in the sample of children from public housing can be accounted for by characteristics common among siblings – characteristics we may or may not be able to observe. This number is similar to the correlation in brothers’ earnings over the entire city. Earnings correlations among children from the same project, however, are zero. Therefore, while the fact that some children end up with high wages and some with low wages can largely be attributed to family differences, none of this variation can be accounted for by project differences.

5. What do studies with non-experimental data find?

Identification problems with non-experimental data

In the private housing market, households choose where to live. Multivariate regression is one approach to estimate neighbourhood effects with non-experimental data by accounting for initial household differences across neighbourhoods, such as wealth, and attributing remaining outcome differences to neighbourhood quality. The approach is akin to matching individuals with similar observable characteristics living in different neighbourhoods, and then attributing average differences between matched individuals to differences in neighbourhood quality.20 If neighbourhoods do matter, we would expect outcomes between individuals with comparable background to differ.

Unfortunately, no matter how many background controls we account for, we can never rule out the possibility that some other unobserved factor explains the regression results. In the private housing market, we expect characteristics to differ across neighbourhoods, whether or not we observe them. If we cannot explain exactly how observationally equivalent households end up in contrasting neighbourhoods, we cannot exclude the possibility that unobservable reasons that could account for residential differences also explain the outcome differences. Any unaccounted household characteristic that is systematically related to both location and outcome biases our neighbourhood effect estimate.

Omitting important background variables likely biases neighbourhood effect estimates toward concluding that they exist. Parents who are ill equipped to handle distressed neighbourhoods are most likely to live in them, because these parents lack the (potentially unmeasured) wherewithal to move to better ones. In this case, the coincidence of a poor neighbourhood or school and poor developmental outcomes of their children results from their inability to avoid
either, thus leading to an overestimation of neighbourhood effects. Parents who are effective in promoting the developmental success of their children may find their neighbourhood choices dominated by considerations of developmental consequences. Parents who are more concerned with what school their child attends are also more likely to make sacrifices to afford a better neighbourhood. The omitted variables bias could go the other direction. Suppose parents choose between holding two jobs in order to live in a better neighbourhood, and a single job with one parent at home while living in a poorer neighbourhood. Suppose further that those who live in a poorer neighbourhood or send their children to worse schools, or both, make up for the deficiencies of the neighbourhood or school through the additional time that parents spend with their children. Neighbourhood or school conditions matter in this scenario, but an empirical analysis will show this to be the case only if it adjusts for differences in parental time use. Failure to adjust for parental employment will cause conventional regression-based approaches to understate neighbourhood or school effects.

Another problem with regression analysis is that background controls may be measured with error. This may lead us to under- or over-predict systematically an individual’s performance relative to the neighbourhood in which he or she resides. The potential for measurement error bias often goes unrecognized, but is just as serious as omitted variables bias. Consider the example of using parental income as a control variable for children’s education attainment while estimating the effect from living in a wealthy neighbourhood. Suppose that there really is no neighbourhood effect, but that children from wealthy families tend to attain more school than those from less wealthy families. We should control for parental wealth, not income, since wealth better captures financial status of the parents, and also better predicts neighbourhood location. Annual income serves as a proxy for wealth, but is measured with error; some years annual income is above normal, some years below it. The regression analysis controlling for income treats a wealthy family in a wealthy neighbourhood, but with temporarily low income, as less well-off living with more well-off neighbours. Based on income alone, the regression application predicts that a child from this family attains fewer years of school than other children in the same neighbourhood. If we controlled correctly for wealth, it predicts the same number of years. Instead, regression analysis attributes the child’s better-than-predicted performance to living in a wealthy neighbourhood. In general, when control variables that help to predict both neighbourhood sorting and the outcomes of interest are measured with error, we end up with biased neighbourhood effect estimates.21

The first bar shows the average difference in adult earnings among children that lived in low- and middle-income census tracts in Toronto. Those from the low-income neighbourhoods lived close to the seven largest housing projects in the city. Sixty-one percent of households in these tracts were below the LICO, whereas only 25 percent were below the LICO in the middle-income tracts. The first bar shows earnings for adults from the high-poverty neighbourhoods were 19 percent lower than wage earners of adults from the middle-income neighbourhoods. In the second bar, I regress earnings on which neighbourhood a
child grew up in, plus family background controls for parental income, parental marital status, years any parent was on social assistance, and family size at the time these sampled individuals were teenagers. The result suggests growing up in low-income neighbourhoods in Toronto lowers adult annual earnings by 12.8 percent, on average. Taken literally, if we move a child from a poor to more middle-income neighbourhood, that child has a much more likely chance of earning substantially more later as an adult. In contrast to the previous findings, children from the large and small housing projects earned, on average, the same, whether accounting for family background or not. The main difference between the two samples is that we know little about the circumstances by which children in the first sample ended up living in these contrasting neighbourhoods, whereas children in the second sample were likely assigned to the different neighbourhoods, since families applying for public housing had no say over which apartment was offered. The quasi-experimental nature of the public housing sample leads to fewer biases from unobservable neighbourhood selection and, in this case, leads to dramatically different conclusions on the size of the neighbourhood influence on earnings.

Studies using regression with non-experimental data
A large number of studies, across many disciplines, use multivariate regression to estimate the existence and importance of neighbourhood effects. These studies vary broadly by how they define neighbourhood quality and size and the outcome measures examined. Below, I survey a few of the more widely cited US papers, along with the Canadian evidence.22

Datcher (1982) was one of the first to test empirically for neighbourhood effects. She linked people's school attainment and earnings for a nationally representative sample of US men aged 23 to 32 to the average income level in their neighbourhood when aged 13 to 22. The dataset did not allow her to define neighbourhood regions smaller than the five-digit code level, which meant looking at areas that often included more than 25,000 residents. Despite this broad definition, Datcher found a strong effect from regressing these outcomes on neighbourhood quality, even after including linear controls for age, gender, parental education, family size, parental income, and more abstract background factors, such as parental education aspirations for children, whether parents like challenges, and carry out plans. She concluded that more than one quarter of schooling and earning differences can be attributed to code neighbourhood influences, and more than 40 percent of the racial differences in these outcomes arise from poorer neighbourhoods of origin for Blacks.

Corcoran et al. (1992) updated Datcher’s (1982) study using a larger sample and more detailed background controls over more than one point in time. In contrast to Datcher’s results, they found mostly negligible neighbourhood effects, one exception being that growing up in a neighbourhood with high welfare rates matters to male earnings. The authors suggested their definition of neighbourhood that includes average characteristics of households in a five-digit code area may be too broadly defined. But they also drew away from making strong conclusions,
noting “unavoidable problems of measurement error and omitted variables pose a formidable obstacle to identification of underlying causal processes” (Corcoran et al., 1992: 595).

Crane (1991) examined whether adolescent risky behaviour increases exponentially among those in neighbourhoods beyond some critical point of distress. Using data linked to US census tract conditions, Crane showed that the likelihood of teenage pregnancy and dropping out jumps dramatically for those living in neighbourhoods where fewer than five percent of workers hold professional or managerial jobs. Apart from neighbourhoods in this extreme category, however, he found little evidence that neighbourhood characteristics mattered. This non-linear neighbourhood effect holds whether including few or many family background controls, and Crane argued that this is evidence that the inclusion of additional family characteristics would not alter the results. There is no reason to reject this assertion, nor can it be confirmed. The circumstances that lead families to the most distressed neighbourhoods may also relate non-linearly by neighbourhood quality.

Brooks-Gunn et al. (1993) found opposite results to Crane’s. They showed that children growing up in more affluent neighbourhoods (defined at the census tract level) perform better on aptitude tests and drop out less than children in low-income neighbourhoods, even after adding basic family controls like income, marital status, and race. They also found that a decrease in the number of affluent neighbours has a positive influence on child outcomes, but there is no corresponding negative impact from living close to poorer neighbours.

The pathways through which average neighbourhood characteristics affect outcomes in this literature follow largely through a black box. Few papers link specific theories on how residential environment influences behaviour and outcomes. Recent research tries to look at the mechanisms more closely, but it still faces basic identification hurdles in dealing with selection and measurement error. Sampson et al. (1997) for example, used a new dataset from the Community Survey of the Project on Human Development in Chicago Neighbourhoods (PHDCN), to measure collective efficacy, which they defined as a combination of social trust, common values, and social interaction within neighbourhoods. The PHDCN collected neighbourhood quality data by videotaping street and housing conditions as a van drove by, and researchers later coded the videotapes, noting many specific levels of distress and social activity. The project also interviewed almost 9,000 respondents from all neighbourhoods, asking them questions specific for research on residential impact. Sampson et al. (1997) found that collective efficacy relates strongly to neighbourhood levels of violence, personal victimization, and homicide in Chicago, after controlling for social composition. They used a random effects approach, which estimated how much of the difference in violent behaviour across the entire sample was due to overall differences in neighbourhood location. The random effects model is an alternative approach to regression that models neighbourhood quality as a latent variable and captures observed and unobserved effects. For causal inference, this approach
assumes that the random error associated with each cross-section unit is uncorrelated with the other regressors, something that is not likely to be the case. The bias will be similar to the omitted variables bias from regression, and Sampson et al. acknowledged this potential concern.

Many Canadian studies follow Datcher’s (1982) original approach and regress measures of child development on neighbourhood quality, controlling for observable family background factors that relate to both. Hertzman et al. (1999) used Statistics Canada’s National Longitudinal Survey of Children and Youth (NLSCY) and regressed various neighbourhood characteristics at the city-block level on school readiness evaluations for children aged 2 to 5. They found a significant negative association between living in a neighbourhood with a larger portion of households with incomes less than $20,000 and the index score of a child’s social and cognitive development. A similar paper by Kohen et al. (2002) found a strong association between neighbourhood unemployment rates and preschoolers’ social and emotional outcomes. Using several controls of family background, including maternal emotional distress, perceived social support, and self-reported ratings of poor health, the authors concluded that any remaining unobserved factors are unlikely to bias the results. Curtis et al. (2003) also used the NLSCY to estimate a strong positive relationship between parents’ perception of neighbourhood safety and preschool behaviour problems. Boyle and Lipman (1998) estimated a random effects model to conclude that seven percent of the variation in child problem behaviour reported in the NLSCY is associated with neighbourhood quality differences at the city-block level, compared to 37 percent of the variation due to differences in circumstances across families. Tremblay et al. (2001) carried out a similar analysis, but defined neighbourhoods at the census tract level. They estimated that about four percent of the variance in childhood physical aggression and hyperactivity in the NLSCY is captured through census tract differences, compared to about 30 percent due to family differences. The random effects model assumes that the random error associated with each neighbourhood cluster is uncorrelated with the other random effects and controls, something that is not likely the case. Small samples within neighbourhood clusters, present in the Boyle and Lipman study, also exacerbate measurement error biases discussed in the previous section.

Canadian research on neighbourhood effects also tends to focus on physical and mental health outcomes, rather than on school attainment, self-sufficiency, and labour market outcomes, which US studies often consider. Hou and Myles (2004) regressed self-reported individual health on neighbourhood income levels, controlling for a variety of observable characteristics using Statistics Canada’s National Population Health Survey. They found a non-linear relationship between neighbourhood income level and individual health; the health benefits of neighbourhood income are greater at higher than at lower neighbourhood levels. Wilson et al. (2004) tested for the effects of perceived neighbourhood quality on self-reported health and emotional distress in Hamilton Ontario, using a phone survey of 748 people and a response rate of 49.8 percent. They concluded that people who report they like physical characteristics of their neighbourhood are
less likely to rate their health as fair/poor, as compared with those who report they like nothing about their neighbourhood. Dunn and Hayes (2000) compared health outcomes between two neighbourhoods in Vancouver. Sunset was relatively more wealthy and composed of more homes and Chinese immigrants than Mount Pleasant. The response rate to solicited mail-in surveys was only nine percent, and 30 of the 559 returns omitted filling out most of the questions. Dunn and Hayes used logistic regression to analyze the simultaneous influence of the various explanatory variables upon the health outcomes. They found neighbourhood satisfaction and living in Sunset to be more likely associated with self-reported health and low-levels of stress. Wheaton and Clarke (2003) used a random effects model to estimate effects of neighbourhood quality on adolescent mental health, controlling for reported parental socio-economic status, parental mental health, age and gender, family structure and, sometimes, earlier child mental health.

The bulk of the literature using regression methods finds evidence of neighbourhood effects. A policy maker with a strong prior belief that neighbourhood effects matter would not have these beliefs seriously challenged by this body of work. However, many of these studies do not make explicit the assumptions required to draw causal inferences or address head on the potential for biased estimates. Ginther et al. (2000) showed that the magnitude and statistical significance of neighbourhood effects are often not robust to different choices of neighbourhood quality and family background controls. Without plausible explanations how similar households sort into neighbourhoods for reasons unrelated to the outcomes that interest us, regression analysis acts on faith (assumption) that omitted variables bias and measurement error bias are negligible. Studies that fail to address the pitfalls of regression will produce neighbourhood effect estimates that continue to generate grave scepticism among many social scientists and statisticians.  

**Some Alternative Approaches**

**Sibling Correlations**

Some researchers estimate the correlation between siblings’ earnings to describe how important overall family factors are at explaining nation or citywide earnings differences. Solon et al. (2000) applied this methodology to neighbours to examine how correlated earnings are between children who grew up close. They used data for a nationally representative sample of the United States and found after controlling for some basic background characteristics, that the residual correlation in neighbouring children’s earnings was around 0.05. This compares with the substantial sibling correlation of 0.40, which suggests that observed and unobserved family factors play a much larger role in accounting for earnings differences across the population than neighbourhood factors when young. Page and Solon (2001) studied adult incomes and found that the correlation between neighbouring girls is only one third that of sisters. They further found that much of the correlation between neighbours is driven by income differentials between urban and non-urban areas. Duncan et al. (2001) studied correlations between
siblings, friends, schoolmates (members of a common grade at a school), and
neighbours for a measure of high school achievement and delinquency. They
found that sibling correlations are much higher than the others, and that
friendship correlations are much larger than neighbourhood and schoolmate
effects. I found similarly small neighbour correlations in earnings among youths
that grew up in Toronto (Oreopoulos, 2003). While these findings suggest a larger
role for family background than neighbourhood characteristics as determinants of
behaviour, they do not directly address the economic significance of
neighbourhood effects, nor do they provide much insight into what factors
underlie the family and possible neighbour relationships.

**Structural Models**

Structural approaches use models of neighbourhood effects to generate
hypothesized data outcomes that would not otherwise exist if neighbourhood
effects did not exist. Structural models use theory to derive empirical implications
of neighbourhood effects and then to test those implications. Glaeser et al. (1996),
for example, found that crime rates across cities and precincts are far more
variable that would be predicted if individual decisions were independent. Topa
(2001) studied interactions across physically contiguous neighbourhoods in
Chicago. He estimated a non-linear regression model in which the unemployment
rate in one neighbourhood is allowed to depend on the unemployment rates of
adjacent communities; the non-linear relationship is structural in the sense that it
is derived from a stochastic process designed to model information transmission
across neighbourhoods. He found spatial interdependencies that are quantitatively
important.

**Ethnographic Studies**

Ethnographic studies explore neighbourhood effects directly through case study
observations and interviews. Researchers ask residents of high-poverty
neighbourhoods how their living arrangements affect them, or document changes
in behaviour as responses to neighbourhood exposure. Ethnographic studies may
reveal important insights into the potential mechanisms by which neighbourhoods
matter, in ways other approaches cannot. Lewis (1966) pioneered work describing
explicitly how a culture of poverty prevailed in poor Puerto Rican communities
and ascribed a number of social ills to the norms that exist in those places.
Wilson’s (1987) original thesis documented the exodus of middle-income Blacks
from inner cities in the 1980s, as neighbourhood conditions deteriorated. Another,
more recent, study is by Klinenberg (2003), who documented how social isolation
worsened the impact of a 1995 heat wave in Chicago. Vulnerability among seniors
differed systematically by social contact during the heat wave. Death rates in
predominantly Black neighbourhoods were substantially higher than in similar
and nearby Hispanic neighbourhoods. Klinenberg interviewed residents about
their experiences to attribute this difference in death rates to a more supportive
and active environment in the Hispanic community.

Ethnographic studies are often regarded as suspect, because of their particularity
and because of fears that the observer's prejudices determine the findings. But
studies, such as the ones by Klinenberg (2003) and Venkatesh (2000) offer significant promise for providing a clearer picture by which residential environment plays an important role on socio-economic well-being. I am not aware of any ethnographic studies on neighbourhood effects in Canada.

6. Related Evidence of Neighbourhood Effects

**Student and roommate effects**

A number of studies look at peer group influences at school. Hoxby (2000) used idiosyncratic fluctuations in racial and gender classroom composition from year to year to examine test score outcomes in Texas. Grade 3 boys and girls tended to perform better when a class contained a higher portion of girls (who tended to outperform boys, on average, at this grade). Boozer and Cacciola (2001) studied school peer effects using data from Project Star, a state of Tennessee program originally designed to assess the effects of smaller class sizes on performance. From Kindergarten to Grade 3, students in 79 schools were randomly selected each year into different class sizes. New entrants to these schools were also randomized into different classes. Since students were “re-shuffled” into small and large classes each year, along with new entrants, an individual’s exposure to better performing students from previously small classes varied randomly. Boozer and Cacciola (2001) used this variation to separate the independent class size effect on test performance from the spillover effect of interacting with better performing peers. They concluded that especially by Grade 3, the impact of small class assignment is almost entirely due to the feedback effect of the enhanced peer qualities due to the treatments in the earlier grades. Angrist and Lang (2002) studied the effects of the Metco program in Boston, a desegregation program that sends (primarily) African American students to suburban schools. Focusing on Brookline Massachusetts, they found little evidence of any adverse peer effects induced by the transfer of lower achieving inner-city students into the Brookline schools.

Some universities randomly assign incoming resident first year students to roommates. Sacerdote (2001) examined the effects of freshmen room-mate assignments at Dartmouth College and found small but significant effects of room-mates’ grade point averages (GPAs). A one-standard-deviation increase in a room-mate GPA is associated with a 0.5 increase in the other room-mate’s GPA (a grading scale out of 4). By senior year, no such relationship between grade performances existed. If one room-mate joined a fraternity/sorority, the other room-mate was 8 percent more likely to join. Kremer and Levy (2003) also examined room-mate effects for a large US public university. On average, males assigned to room-mates who reported drinking in the year prior to entering college had one quarter-point lower GPA than those assigned to non-drinking room-mates. Kremer and Levy found no similar effect for females, and no relationship between GPA and first-year room-mates’ high school grades, admission test scores, or family background.
Evidence of Spatial Mismatch

Spatial mismatch presents a different kind of neighbourhood effect. It arises for residents with limited access to transportation living in neighbourhoods with few local job openings. The theory suggests areas in which the poor and unemployed often reside are also areas in which it is most difficult to find work. Residents in these neighbourhoods may respond by accepting longer commutes. When controlling for mode of transportation, findings are generally mixed. McLafferty and Preston (1996), for example, found significantly longer commute times for Blacks living in inner cities, but no differences for those in the suburbs.

Another empirical approach examines how car ownership affects employment and earnings. Holzer et al. (1994) and Ong (1996) demonstrated that individuals who own cars are more likely to be employed and, conditional on being employed, earn more than individuals without cars. While these findings are consistent with the causal effects of car ownership, there are also several alternative explanations. For example, causation may run in the opposite direction; namely, those with jobs can afford cars. Alternatively, car ownership may be determined in part by unobserved factors that also affect employability. Raphael and Rice (2002) used an ingenious way to predict car ownership by looking at differences in average car insurance premiums and gas taxes across states. These measures are unlikely to be correlated with unobserved skills or motivation to find work, yet are strongly correlated with car-ownership rates. Their estimates implied that for those who don’t own a car, because of high gas taxes or insurance premiums, the likelihood of employment falls by 15 percentage points. The authors found negligible and sometimes negative effects of car ownership on wages.

Holzer et al. (2003) examined the effects of extending a commuter rail system on accessibility to jobs. In 1997, The Bay Area Rapid Transit (BART) system extended services to a rapidly expanding region in a suburb of Oakland, providing a new direct public-transit link between the city’s largely minority, high unemployment urban core and this predominantly White, high growth and low unemployment suburban community. The authors surveyed hires by firms in areas surrounding the stations along the new BART line immediately before service began and about one year later. They compared the average change in the propensity to hire minority workers at firms near the new stations with the average change for firms farther away, and they found a sizable relative increase in the tendency to hire Hispanic workers near the station, but little evidence of a relative effect on hiring Blacks.

Community Intervention programs

Community intervention programs make available resources to communities for encouraging neighbourhood interaction and child development. Beauvais and Jenson (2003) documented several of these programs in Canada. Such programs sponsor neighbourhoods to foster more positive social interaction and typically focus on improving child development and mental health. The Better Beginnings, Better Futures Project is one example started by the Government of Ontario in 1990. Three primarily disadvantaged sites were chosen: one focuses on four
primary schools in Cornwall, the second on a large junior school in Etobicoke; the third on two neighbourhoods in Sudbury. Each selected community was funded to develop a local prevention project designed to promote child development and encourage community participation in carrying out these programs. The Cornwall and Etobicoke sites integrated classroom enrichment programs that included homework help, summer tutoring, a breakfast program, and toy library. Sudbury’s site developed after-school and holiday programs that included games, craft activities, and promoted community activities. Peters et al. (2003) evaluated the effectiveness of the project by comparing reported behavioural outcomes of children from the project sites to children from comparable control sites. They estimated significant reductions in parent anxiety and depression, and increases in neighbourhood satisfaction in the Etobicoke site, but generally no differences in the other sites. Average per child costs ranged from $1,100 to $1,900.

Neighbourhood Warden Programs
In 2001, the United Kingdom instigated the National Strategy for Neighbourhood Renewal to combat neighbourhood poverty and deprivation, with goals of reducing unemployment, crime, and improving health and the neighbourhood environment in the country’s poorest communities. The Neighbourhood Renewal Unit is responsible for administering about $6 billion of funding for these objectives, and targets many local community initiatives. One major initiative is the Neighbourhood Warden Program. Neighbourhood wardens are uniformed community officers who patrol a local community, predominantly deprived urban areas, to reduce criminal activity, graffiti, fear of crime, and other anti-social behaviour. A report by the Office of the Deputy Prime Minister (UK, 2004) evaluated the effectiveness of the Neighbourhood Warden Program. About 2,000 residents were surveyed in 15 locations before and after the Warden Program was implemented, and in eight comparable urban areas where no program was implemented. Fear of crime and actual crime fell with the implementation of the Warden Program, relative to the comparable areas; and residents with the Program were significantly more satisfied with their neighbourhood conditions. The Program cost about $60 million over a two and a half year period in 85 communities. The report, however, concluded that even if no more than 10 percent of the crime reduction found in these areas were attributable to the Program itself, the benefits would exceed these costs.  

7. Conclusions
Interest in neighbourhoods is growing in Canada, in line with a growing focus on the importance of social networks in promoting well-being and growing ethnic diversity within large cities. Policies directed at reinforcing positive social interactions within communities may offer, in some cases, more potential for improving well-being than spending the same resources directly on individuals. Redevelopment or relocation, for example, could help prevent a neighbourhood becoming infested by crime and decay or help strengthen positive community
relations. This paper argues, however, that there are a number of important issues to consider before implementing such community-based policies.

First, household exposure to high-poverty neighbourhoods in Canada is small. About 10 percent of low-income households live in low-income areas where more than 40 percent fall below the LICO. The average length of time spent in these neighbourhoods is about four years. About one third of households living in low-income neighbourhoods leave within two years, but another third remain after six years. Much of the research on high-poverty areas originates from the United States, but the level of distress within high-poverty neighbourhoods in Canada is not the same. Canadian low-income neighbourhoods experience much lower levels of crime and visible-minority segregation than US low-income neighbourhoods experience. Many households living in Canadian low-income areas are recent immigrants and move within five years.

Second, much of the existing Canadian research on neighbourhood effects relies on regression analysis, which is prone to bias and misinterpretation. Regression analysis essentially requires assuming that similar households that live in different neighbourhoods do so for reasons that do not alter subsequent outcomes of interest. The many location options that face households and the many factors that determine the location decision for which a researcher cannot observe makes this assumption tenuous. Most researchers have moved away from using this approach to test for the significance of neighbourhood effects. The potential for measurement error and omitted variables bias are difficult to overcome. Alternative approaches, such as using natural experiments, instrumental variables, and detailed ethnographic studies offer more convincing results.

Third, the most persuasive research to date suggests residential environment matters most to an individual’s mental health and exposure to crime, but has little influence on self-sufficiency and child development. Studies that exploit random, or near-random, assignment into different public housing environments find almost no difference in subsequent earnings, education attainment, unemployment, and social assistance outcomes. Among low-income households, avoiding crime appears to be the dominant factor influencing a desire to move away from high-poverty neighbourhoods. Poor families enabled to move away from low-income areas were much more likely to report feeling safe, calm, and more satisfied with the residential environment, but the move had no impact on other parental socio-economic outcomes and possibly a negative impact on boys’ behaviour. Overall, family differences seem to be much more important determinants to career success and job security.

Missing from much of this analysis are the potential costs involved from relocation or redevelopment policies. Costs arise not only from financing community-related projects, but also from externalities on neighbours that arise from implementing such policies. Subsidizing low-income families to live in better suburban communities obviously would impact families currently living there. Redevelopment might alter housing prices, making it more difficult for low-
income families originally targeted to live in the area. Removing or retrofitting public housing projects might also lead to fewer affordable housing places available.

The current research on neighbourhood effects in Canada offers no clear suggestions for improving socio-economic well-being through community-based policies. New data and new experimental designs would help greatly to improve our knowledge on this issue. Relying exclusively on US studies is not recommended, since neighbourhood segregation in the two countries differs substantially. We should continue to investigate the importance of reinforcing positive and negative social environments. Recent evidence, however, suggests these forces may matter less than previously supposed.

Notes


2 It is worth mentioning that the initial plan to demolish Robert Taylor Homes did not arise from public pressure to desegregate the project. Several heating pipes burst in the winter of 1999, causing the Chicago Housing Authority to evacuate families in four high rises and later permanently relocating them through transfer or offering them housing vouchers (Jacob, 2003).


4 A proposed revitalization plan for Regent Park would reintroduce through traffic.

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

6 The housing project data come from previous Metro Toronto Housing Corporation security tabulations. Toronto and Chicago assault tabulations are from Statistics Canada (1995) and the Federal Bureau of Investigation (1995) respectively.

7 The official US poverty line was established in 1963 to reflect a rough threshold defining poverty, based largely on the ability to afford food and housing and adjusted by such factors as family size, state, family composition, and number of children. The Census Bureau updates the poverty line each year, based on changes in the Consumer Price Index.

8 I thank the authors for generously giving permission to use their results.

9 Myles et al. (2000) proposed documenting neighbourhood inequality by the distribution of mean family-adjusted income across census tracts within cities. Such an approach could be used to isolate distressed areas. Interestingly, they found the main source of neighbourhood income inequality occurred not from a substantial fall in mean income for the poorest neighbourhoods, but from a sharp spike in mean income for the richest.


11 For spatial analysis of city crime, see Anselin et al. (2000), Pierce et al. (1988), Sherman (1989), and Weisburd and Green (1994). Sherman et al. (1989), for example, found 3.3 percent of street addresses and
intersections in Minneapolis generated 50.4 percent of all dispatched police calls for service. Perception of crime as a problem also varies by low-income and high-income neighbourhoods (DeFrances and Smith, 1998).

12 A related community influence is how proximity of available jobs affects employment. The Spatial Mismatch Hypothesis, first suggested by Kain (1968), arises when fewer jobs per worker in high-poverty areas make finding work more difficult. The hypothesis focuses on inner-city minorities in the United States and how access to transportation and migration of jobs from the city to the suburbs affects job prospects. The mechanisms by which employment takes place happen independently of social interactions and so are not considered here as neighbourhood effects. I do mention briefly some empirical results from this literature in Section 5. See also Ihlanfeldt and Sjoquist (1998) for a review. I am not aware of any study that looks at the Spatial Mismatch Hypothesis in Canada.

13 Interestingly, Rothstein (2003) finds large housing premiums to live close to neighbourhoods with high performing students, but these premiums do not correlate with actual school productivity.

14 The appendix provides a more technical overview of the measurement difficulties of neighbourhood effects.


16 Interim evaluations of MTO include Kling and Liebman (2004), Kling, Ludwig, and Katz (2004 a, b) Also see <http://www.mtoresearch.org> for all recent and past MTO studies.

17 Earlier evaluations of MTO revealed small, but more positive effects on child outcomes. See everyone.

18 It will be interesting to look at the final report which will look 10 years out, which will also allow the authors to compare children who were 5 or so when the program started and where the children had much more exposure to the better environment after. I wonder whether the authors find any differences conditioning on length of time in the original public housing project at the time the study began.

19 Through the US federal government, a Section 8 voucher provides rent subsidies to eligible low-income families and individuals. Rather than being provided with a specific unit at a subsidized housing site, Section 8 participant-tenants are free to use their voucher to locate and contract for housing from a network of participating landlords throughout a particular region.

20 The matching analogy applies only when a considerable overlap of individuals with similar characteristics but in different neighbourhoods exist. If individuals living in different neighbourhoods are not observationally similar, then regression analysis relies heavily on the assumption that the individual effects on outcomes are linear. The linear estimated relationship between the outcome and individual characteristics for individuals within the same neighbourhood is used to control for outcomes across neighbourhoods. But any non-linear relationship violates this model and biases the neighbourhood effect estimate. The fewer the number of similar individuals across neighbourhoods, the less credible regression analysis becomes.

21 Another problem with regression analysis is what Manski (1993) referred to as the reflection problem. Manski noted that when we specify the regression model using linear neighbourhood effects and linear family background effects (as done in classical ordinary least squares regression), it might not be possible to identify the two separately. For example, suppose only two individuals lived in each neighbourhood, and we wish to examine whether the behaviour of one neighbour affects the behaviour of the other. Factors that affect one neighbour and influence the other go both ways. Manski provided the technical details of how this social multiplier complicates the identification of the average neighbourhood effect, even without the presence of omitted variables bias. A priori knowledge that households sort into neighbourhoods in ways linearly independent of individual background characteristics helps avoid the reflection problem (Brock and Durlauf 2001). Random assignment often avoids the reflection problem by focusing on only those individuals selected into the experiment or natural experiment, without observing effects for those outside the experiment.

22 See Durlauf (2003) and Dietz (2001) for more comprehensive reviews of the American literature.

23 Freedman (1991) provides a general criticism with using regression in the social sciences.
The overall rate of “offences” experienced by residents of the Neighbourhood Wardens program fell by 27.6 percent, compared with an increase of 4.7 percent in the comparison area over the same period. The report is not clear on what is meant by an offence, but assumes that the benefit of preventing an average offence is around $4,000. The report also notes that a number of other initiatives in the Wardens Program areas were going on at the same time, and also designed to improve the local environment.
References


Appendix: Omitted Variables and Measurement Bias

This appendix provides a more technical overview of estimation biases that arise when testing for neighbourhood effects using regression. Durlauf (2003) and Battistin and Chesher (2004) provided a more in-depth discussion.

Consider the case where we wish to estimate the consequences of living in a “good” or “bad” neighbourhood. Let $d_i = 1$ if an individual $i$ lives in a good neighbourhood and $d_i = 0$ if not. A typical linear model of neighbourhood effects takes the following form:

$$ y_i = \alpha + \beta d_i + \delta x_i + \varepsilon_i $$

where $y_i$ is the outcome of interest for individual $i$, such as education attainment or employment. In this model, three variables may affect $y_i$. First, is the neighbourhood. Notice I allow the neighbourhood effect, $\beta$, to vary by individual. The neighbourhood does not influence every individual’s outcome the same way. The second variable affecting $y_i$ is some observable background control, $x_i$. This variable could represent parental income or education attainment, or an overall index that captures all observable family controls. The last term, $\varepsilon_i$, is a catch-all for unobservable variables or random factors that account for the remaining discrepancy for explaining differences in $y_i$ across the population.

Examining outcome differences between neighbourhoods without accounting for any systematic differences simply involves comparing outcome means between good and bad communities:

$$ E(y_i \mid d_i = 1) - E(y_i \mid d_i = 0) = \bar{\beta} + E(\delta x_i + \varepsilon_i \mid d_i = 1) - E(\delta x_i + \varepsilon_i \mid d_i = 0) $$

Since we cannot observe systematic differences in background characteristics by comparing mean outcome differences, we must rely on assuming no such differences to obtain a crude estimate for $\bar{\beta}$, the average neighbourhood effect for all individuals in the sample:

$$ \hat{\beta} = E(y_i \mid d_i = 1) - E(y_i \mid d_i = 0) $$

Our estimate of the neighbourhood effect, $\hat{\beta}$, includes the systematic difference in background characteristics. For example, if $y_i$ is education attainment, the difference in attainment between neighbourhoods includes influences from growing up in different environments, as well as differences in parental income and other circumstances which led parents to move to these places.
Regression analysis tries to account for observable differences in family background to get closer at estimating $\bar{\beta}$. By explicitly including the control variable $x_i$, regression sorts out systematic differences between the neighbourhood effect and the observable control. The regression estimate for $\bar{\beta}$ becomes:

$$\hat{\beta}_{OLS} = \left[ E(y_i \mid d_i = 1) - E(y_i \mid d_i = 0) \right] - \hat{\delta} \left[ E(x_i \mid d_i = 1) - E(x_i \mid d_i = 0) \right]$$

where we are now able to condition out the observable different effects from $x_i$ from a consistent estimate of $\delta$. The estimate of the observable background control effect comes from the auxiliary regression of

$$y_i - E(y_i \mid d_i) = \hat{\delta}[x_i - E(x_i \mid d_i)] + \epsilon_i.$$

Our regression estimate of $\bar{\beta}$ is unbiased only if the remaining unobservable background factors do not systematically differ between neighbourhoods. That is, we avoid omitted variables bias only if $E(\epsilon_i \mid d_i = 1) - E(\epsilon_i \mid d_i = 0) = 0$. Any unaccounted factor that explains part of the neighbourhood sorting across the population and influences the outcome of interest will bias the result up or down, depending on the pattern of this sorting.

Regression analysis also assumes that the model is correctly specified. In this example, the observable family factor influences $y_i$ linearly, and in the same way independent of the neighbourhood in which an individual resides. If the incremental effect of $x_i$ is high for individuals with low levels of $x_j$ while the incremental effect of $x_i$ is low for individuals with high levels of $x_j$, the neighbourhood effect estimate will also be biased when individuals differ systematically between neighbourhoods by this characteristic.

Now consider the case where we have no omitted variables bias, $E(\epsilon_i \mid d_i = 1) - E(\epsilon_i \mid d_i = 0) = 0$, and the model is correctly specified. Even in this case, estimates of the neighbourhood effect may still be biased from measurement error in the observed controls. Suppose that $z_i = x_i + u_i$ is observed instead, and serves as a proxy for $x_i$. The measurement error is not systematically related to living in a particular neighbourhood, the outcome variable, or the unobserved factors. The degree of measurement error is captured by the variance around zero: $Var(u_i) = \sigma_u^2$, $E(u_i) = 0$, $Var(x_i) = \sigma_x^2$. If $\sigma_u^2$ is zero, there is no measurement error, if $\sigma_x^2$ is high, we often over or under predict an individual’s true background measure.

Unfortunately, the confounding effect of $x_i$ is only partially controlled for when measurement error exists. The regression estimate of $\bar{\beta}$ is now:
\[
\hat{\beta}'_{OLS} = \left[ E(y_i \mid d_i = 1) - E(y_i \mid d_i = 0) \right] - \delta \left[ E(x_i \mid d_i = 1) - E(x_i \mid d_i = 0) \right] \frac{\sigma_x^2}{\sigma_x^2 + \sigma^2}
\]

Even without omitted variables bias, measurement error waters down the ability to control for background characteristics. This leads to bias in the neighbourhood effect estimate, in the same direction as that from omitting the observable control entirely.
Table 1
Frequency of High-Poverty Census Tracts in the United States and Canada
Selected Metropolitan Areas for 1990 and 2000

<table>
<thead>
<tr>
<th></th>
<th># poor CTs 1990</th>
<th># poor CTs 2000</th>
<th>% of CMA pop in poor CTs 1990</th>
<th>% of CMA pop in poor CTs 2000</th>
<th>% of poor living in poor CT 1990</th>
<th>% of poor living in poor CT 2000</th>
<th>% of Vis Min in poor CT 1990</th>
<th>% of Vis Min in poor CT 2000</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>United States</strong></td>
<td></td>
<td></td>
<td>(Blacks/Hispanics)</td>
<td>(Blacks/Hispanics)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>New York</td>
<td>279</td>
<td>253</td>
<td>10.2</td>
<td>10.1</td>
<td>31.3</td>
<td>24.9</td>
<td>40.1/40.9</td>
<td>32.5/32.2</td>
</tr>
<tr>
<td>Detroit</td>
<td>150</td>
<td>53</td>
<td>9.9</td>
<td>2.4</td>
<td>36.0</td>
<td>10.4</td>
<td>53.9/36.1</td>
<td>16.4/6.9</td>
</tr>
<tr>
<td>Chicago</td>
<td>187</td>
<td>114</td>
<td>5.5</td>
<td>2.8</td>
<td>26.4</td>
<td>13.7</td>
<td>45.3/12.4</td>
<td>26.4/4.7</td>
</tr>
<tr>
<td>LA</td>
<td>56</td>
<td>137</td>
<td>3.0</td>
<td>5.9</td>
<td>9.0</td>
<td>14.9</td>
<td>17.3/9.1</td>
<td>21.3/16.9</td>
</tr>
<tr>
<td>Seattle</td>
<td>9</td>
<td>4</td>
<td>0.6</td>
<td>5.0</td>
<td>2.4</td>
<td>6.8</td>
<td>3.4/1.3</td>
<td></td>
</tr>
<tr>
<td>All 330 CSMAs</td>
<td>3417</td>
<td>2510</td>
<td>5.2</td>
<td>3.5</td>
<td>15.0</td>
<td>10.0</td>
<td>30.4/21.5</td>
<td>18.6/13.8</td>
</tr>
<tr>
<td><strong>Canada</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Montreal</td>
<td>117</td>
<td>108</td>
<td>9.2</td>
<td>10.9</td>
<td>21.3</td>
<td>24.8</td>
<td>19.3</td>
<td></td>
</tr>
<tr>
<td>Ottawa - Hull</td>
<td>12</td>
<td>10</td>
<td>4.0</td>
<td>4.1</td>
<td>11.0</td>
<td>11.1</td>
<td>20.0</td>
<td></td>
</tr>
<tr>
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<td>23</td>
<td>1.7</td>
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<td>5.3</td>
<td>7.8</td>
<td>38.8</td>
<td></td>
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<tr>
<td>Edmonton</td>
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<td>4</td>
<td>4.6</td>
<td>2.4</td>
<td>10.5</td>
<td>6.2</td>
<td>12.8</td>
<td></td>
</tr>
<tr>
<td>Vancouver</td>
<td>7</td>
<td>9</td>
<td>2.5</td>
<td>2.4</td>
<td>7.7</td>
<td>6.1</td>
<td>22.7</td>
<td></td>
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<tr>
<td>All 27 CMAs</td>
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<td>234</td>
<td>4.0</td>
<td>4.6</td>
<td>10.6</td>
<td>11.9</td>
<td>17.8</td>
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</tr>
</tbody>
</table>

Notes: High-Poverty Neighbourhoods defined as 40 percent of more of households in tract below U.S. poverty line or Canadian LICO

Sources; U.S. figures from Jargowsky (2003), Canadian figures from Heisz and McLeod (2004)
<table>
<thead>
<tr>
<th></th>
<th>percent of working age adults not employed</th>
<th>Unemployment rate</th>
<th>percent of adults without high school education</th>
<th>percent Black</th>
<th>percent visible minority</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>High-Poverty CTs</td>
<td>Other CTs</td>
<td>High-Poverty CTs</td>
<td>Other CTs</td>
<td>High-Poverty CTs</td>
</tr>
<tr>
<td>All U.S. CMSAs</td>
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<td>39.4</td>
<td>10.5</td>
<td>4.3</td>
<td>51.7</td>
</tr>
<tr>
<td>Montreal</td>
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<td>35.4</td>
<td>10.2</td>
<td>5.4</td>
<td>48.3</td>
</tr>
<tr>
<td>Ottawa - Hull</td>
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<td>39.6</td>
<td>9.0</td>
<td>4.8</td>
<td>43.7</td>
</tr>
<tr>
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<td>32.4</td>
<td>9.9</td>
<td>4.9</td>
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</tr>
<tr>
<td>Edmonton</td>
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<td>28.2</td>
<td>7.7</td>
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<tr>
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</tr>
<tr>
<td>All 27 CMAs</td>
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<td>47.5</td>
</tr>
</tbody>
</table>

Notes: High-Poverty Neighbourhoods defined as 40 percent or more of households in tract below U.S. poverty line or Canadian LICO

Sources: U.S. figures from Jargowsky (2003), Canadian figures from Heisz and McLeod (2004)
Figure 1
Estimated Average Annual Earnings, Ages 29 – 35
Adults that Once Lived near and in Toronto Public Housing

Lived near Public Housing

- Lived Near Large Project: 19732
- Lived Near Small Project: 23837
- Lived Near Small Project, Adjusted for Family Background Controls: 22426

Lived in Public Housing

- Lived In Large Project: 16647
- Lived In Small Project: 16780
- Lived In Small Project, Adjusted for Family Background Controls: 17361