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## Chapter 3

# Ethnographic Evidence, Heterogeneity, and Neighbourhood Effects after Moving To Opportunity

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## Introduction

Research on neighbourhood effects is at a crossroads. Through the 1990s and early 2000s, researchers in sociology, demography, and economics were overwhelmingly concerned with estimation problems, particularly the problems associated with selection into neighbourhoods (Jencks & Mayer, 1990; Goering & Feins, 2003; see Small & Newman, 2001; Sampson et al., 2002). By the late 1990s, the Moving To Opportunity study—a multi-million dollar randomized control trial that tracked the effects of moving to non-poor neighbourhoods—promised to address many of these concerns and give new life to the neighbourhood effects research program (Goering & Feins, 2003). To the surprise of many, the results have been inconsistent, prompting disagreement over the direction the literature should take, and even over whether studying the effects of neighbourhood conditions remains a viable research agenda (see Clampet Lundquist & Massey, 2008; Ludwig et. al., 2008; Sampson, 2008).

In what follows, we take stock of this work and of the implications of the recent experimental studies. We argue that the first wave of neighbourhood effects research suffered from at least two problems: it assumed that the effect of neighbourhood poverty was homogeneous across subpopulations and across treatment settings, and it failed to integrate effectively ethnographic research into the quantitative empirical research program. These are separate points, but together they help make sense of the findings of MTO and pave the way for a more informed and better-targeted research agenda in neighbourhood effects.

We begin by providing a brief and broad overview of the research on neighbourhood effects up to the MTO experiments. Next, we review several recent studies that suggest that researchers should have expected heterogeneity, not homogeneity in the effect of neighbourhood poverty—that is, that whether and how neighbourhoods mattered depended substantially on individual-, neighbourhood-, and city-level conditions. Then, we turn to the role of ethnographic evidence, arguing that the better integration between qualitative and quantitative research on poor neighbourhoods can help scholars generate clearer research expectations and interpret seemingly inconsistent findings. We suggest that an assumption of heterogeneity should permeate all aspects of the work, from how questions are asked, to how hypotheses are formulated, to how ethnographic research is conducted and interpreted. We conclude by discussing the implications of our argument for future quantitative and qualitative research on neighbourhoods.

## The First Twenty Years of Neighbourhood Effects Research

The start of contemporary research on neighbourhood effects can be traced to the publication of Wilson's *The Truly Disadvantaged* (1987), which argued that American cities had experienced an economic restructuring during the 1970s and 1980s that prompted the departure of both manufacturing jobs and middle class people from the central cities, resulting in urban neighbourhoods with a greater concentration of poverty. Concentrated poverty, in turn, undermined the life chances of the poor. In the years following the book's publication, thousands of studies devoted themselves to addressing one or another of its hypotheses, and many studies specifically tested the proposition that neighbourhood poverty independently affected life chances. As Mayer and Jencks (1989:1441) wrote in an important early paper, the core hypothesis was that "poor children living in overwhelmingly poor neighbourhoods find it harder to escape poverty than poor children living in more affluent neighbourhoods." Over the years, researchers investigated the consequences of not merely neighbourhood poverty but also other neighbourhood conditions, such as disadvantage and racial homogeneity.

The ensuing neighbourhood effects literature has been canvassed many times and in great detail, obviating the need for an extensive review (Mayer & Jencks, 1989; Jencks & Mayer, 1990; Brooks-Gunn et al., 1997; Small & Newman, 2001; Sampson et al., 2002; Sampson, 2008). Nevertheless, several concerns in the literature are worth noting that will prove crucial to our discussion. From its beginnings and through its first twenty years, the literature exhibited three notable concerns: a concern with selection bias, a concern with effects on average, and a concern with mechanisms.

### *Selection bias*

From the beginning, sceptics have argued that the effort to determine whether neighbourhood poverty affects life chances is threatened by the problem of selection bias (Jencks & Mayer 1990; see also chapters by Manley & van Ham, 2011; and Bergström & van Ham, 2011 in this volume). Most quantitative tests of neighbourhood effects were (and continue to be) based on observational survey data, often collected at one point in time. These data rarely contain the information required to determine with certainty why different individuals live in different neighbourhoods. For this reason, in statistical regressions, the coefficients for the effect of neighbourhood poverty may be biased due to unobserved conditions (Jencks & Mayer, 1990; Tienda, 1991; Duncan & Brooks-Gunn, 1997; Small & Neman, 2001; Goering & Feins, 2003; Harding, 2003; Ludwig et al., 2008; DeLuca and Dayton 2009). In Clampet-Lundquist and Massey's (2008:109) words, it has been difficult to determine whether "poor places make people poor, or... poor places attract poor people" (Tienda 1991). While, in the early years, researchers differed in the extent to which they worried about this issue, over time the problem became impossible to ignore.

As work in econometrics and causal inference penetrated the field, the selection bias problem came to be understood increasingly within the counterfactual model of causality (e.g. Harding, 2003; see Rubin 1974; Morgan & Winship, 2007; Morgan 2001). In this model, experiencing neighbourhood poverty is conceived as a treatment, and each individual is assumed to have a potential outcome under the treatment state (living in a poor neighbourhood) and under the control state (living in a non-poor neighbourhood). The causal effect of the treatment for a given individual is simply the difference between her outcomes in the two treatment states. Since it is impossible to observe an individual under two different treatment states—a person cannot simultaneously live in a poor and in a non-poor neighbourhood—causal effects are estimated on average for populations (Morgan and Winship, 2007:4-6). To properly estimate an average causal effect using non-experimental data one must be certain that an unobserved process did not systematically assign different kinds of individuals to different treatment states (or that those differences are ignorable). No solution is better than randomly assigning a large

sample of individuals to treatment and control conditions. This approach helped generate scholarly support for randomized control trials, such as Moving To Opportunity, in the context of neighbourhood and housing research. At the same, it convinced many that in the absence of such trials, it was impossible to rule out that all neighbourhood studies that did not employ or approximate random assignment had reported spurious findings (Ludwig et al. 2008).

### ***Effects on average***

In their attempts to discern the true effects of neighbourhood poverty, most studies implicitly assumed that the neighbourhood treatment effect was homogeneous across subpopulations and settings. For Wilson (1987) “concentration effects” were a kind of social fact believed to operate in inner cities across the nation, regardless of local political or cultural context. The early influential papers of Jencks and Mayer (1990; Mayer & Jencks, 1989) helped cement this orientation. Reviewing published papers and performing some analyses on their own, the authors tried to ascertain whether the findings collectively suggested that neighbourhoods do, in fact, affect life chances on average. The issue, as the authors framed it in one of their titles, was “Growing up in Poor Neighbourhoods: How Much Does it Matter?” (Mayer & Jencks, 1989). The question presupposed the existence of a single answer for any given outcome, regardless of location, context, or other conditions: either neighbourhoods mattered much or they did not. (There was an important exception: the authors speculated that neighbourhood SES would affect people of different SES differently.)

Following these and other early works, an entire generation of researchers concerned themselves with answering either a yes-or-no question (do neighbourhoods matter?) or a question of degree (how much do they matter?)—rather than a conditional question (under what circumstances do they matter?) (Small 2004). This orientation seemed sensible. From the perspective of the traditional, regression-based statistical models that characterized the early literature, it translated into a primary concern with estimating direct, rather than interaction, effects. Furthermore, few theories in the early literature gave researchers reason to pursue a different strategy: neither the early works of Wilson (1987), Jencks and Mayer (1990), Massey and Denton (1993), Sampson and Groves (1989), or others, nor the early ethnographic studies of Liebow (1967), Anderson (1978, 1990, 1999), Duneier (1992), or others gave researchers reason to be substantially concerned with the possibility that the effects of neighbourhood poverty depended on context, that neighbourhood poverty might substantially affect life chances in some but not other circumstances.

### ***Mechanisms***

A third overarching concern was to specify the mechanisms through which neighbourhoods affect life chances (Tienda 1991; see also chapter Galster, 2011 in this volume). Researchers have introduced scores of models. In an early review, Mayer and Jencks (1989; Jencks & Mayer, 1990) proposed three. First, having disadvantaged neighbours may affect the poor through either contagion or through the weaker ability to maintain social order. Second, having advantaged neighbours may make the poor feel relative deprivation that encourages an oppositional or deviant subculture. Third, living in a disadvantaged neighbourhood may affect the poor by limiting access to strong institutions and resources. Small and Newman (2001: 32) reviewed the literature and argued that researchers have proposed two sets of models for how neighbourhood poverty affects life chances: socialization mechanisms, which describe how neighbourhoods socialize young residents, and instrumental mechanisms, which describe how neighbourhoods limit or otherwise affect people’s ability to exercise their agency. Through socialization mechanisms, neighbourhood poverty is said to help spread negative behaviour through contagion; expose young people to fewer role models; subject them to discouraging treatment by teachers, officers, and other institutional actors; isolate them linguistically from the mainstream; and encourage them to develop an oppositional culture. Through instrumental mechanisms,

neighbourhood poverty is said to limit the number of middleclass people available to meet, the amount of job information available to acquire, and the number of resources available to access. Sampson and colleagues (2002) reviewed over 40 peer-reviewed articles and identified a similar list of mechanisms, in addition to those affecting norms enforcement, collective efficacy, and routine activities.

Other researchers have argued that we should think about mechanisms differently. In a review of MTO findings, Sampson (2008) argued that mechanisms must take into account lifecycle factors—neighbourhoods are likely to matter most among young children, among those who are children and grandchildren of others who lived in poor neighbourhoods, and among those who are exposed for long periods of their life. Galster (2011) argues in his chapter in this volume that neighbourhood effects mechanisms should be thought of in terms of both how they operate and their “dosage.” He groups their operation into four broad categories: social-interactive mechanisms, environmental mechanisms, geographical mechanisms, and institutional mechanisms. He then proposes that a pharmaceutical metaphor of “neighbourhood dosage” can help explain how these mechanisms produce individual responses. For example, if we take the proposed social-interactive mechanism of behavioural contagion, the dosage would refer to how often children are exposed to negative behaviours, how long the exposure occurs, and how intense the behaviours are to which children are exposed.

In spite of all this work, it is unclear that much cumulative progress was made on the question of mechanisms. First, researchers disagreed on what constitutes a mechanism. For example, while many of mechanisms reviewed by Sampson and colleagues (2002) operate at the neighbourhood level (e.g., collective efficacy and informal social control), many of those reviewed by Small and Newman (2001) operate at the individual level (e.g., isolation and oppositional attitudes). Second, these disagreements were largely implicit, as the literature did not debate what constituted a properly specified mechanism or how they should be observed (see Hedstrom and Swedberg, 1998; Hedstrom and Ylikoski, 2010). Third, given the inability of many earlier studies to account for the selection problems, it was unclear which set of proposed mechanisms—and which set of variations on how mechanisms operate—to give greatest attention to or how.

The three concerns we have identified—with selection, with effects on average, and with mechanisms—did not receive equal focus. The first was an initially neglected issue that soon became an obsession; the second, more a running assumption than an intellectual preoccupation; the third, a persistent worry that never arrived at resolution. Nevertheless, the three help understand both how researchers viewed and how we ought to respond to an important study that marks a turning point in the literature, the Moving to Opportunity randomized control trials.

## **A Turning Point: The Moving To Opportunity Studies**

Largely due to the significance of the selection problem, researchers eagerly awaited results of experimental studies, particularly of the Moving to Opportunity randomized control trials.<sup>1</sup> MTO is a voucher-based housing mobility experiment that intervenes at the level of the household. Beginning in 1994, over 4600 families living in public housing projects (in neighbourhoods with a poverty rate of at least 40%) in Baltimore, Boston, Chicago, Los Angeles, and New York City

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<sup>1</sup> In what would come to serve as a prototype for the MTO experiments, the Gatreux program, a court-ordered public housing desegregation in Chicago, provided over 7000 families with housing relocation vouchers (Briggs, Popkin & Goering, 2010; Rubinowitz & Rosenbaum, 2000; Orr et al., 2003). Children of suburban relocaters showed gains in educational outcomes (see DeLuca and Dayton (2009) for a review of child well-being outcomes for Gatreux participants). But Gatreux was not an experiment, and offered no restrictions on movement. Any of the conditions that caused families to move could also have lead to better educational attainment for their children.

were randomly assigned to one of three experimental conditions: (a) the treatment group received vouchers and counselling assistance to move to “opportunity” neighbourhoods — in which the poverty rate was less than 10%; (b) the “Section 8 group” received a voucher but no mobility counselling or restrictions on their movement; (c) and the control group received neither vouchers nor counselling. (For comprehensive reviews of MTO design, history, interim and long-term results, see Goering & Feins, 2003; Orr et al., 2003; Kling, Liebman & Katz, 2007; Briggs, Popkin & Goering, 2010; Sampson, 2008; Ludwig et al., 2008; Clampet-Lundquist & Massey, 2008.) The MTO team collected data on several outcomes: economic self-sufficiency, mental health, physical health, education and risky behaviour (Orr et al., 2003). MTO promised to provide the most compelling test of the effects of neighbourhood poverty.

However, the results were inconsistent (see Orr et al., 2003; Kling, Liebman & Katz, 2007; Ellen & Turner, 2003; Briggs, Popkin & Goering, 2010). While the number of outcomes is too large and the results are too diverse to summarize here, a few findings are worth noting. The interim studies found robust effects on adult mental health outcomes, but limited effects on physical health. On average, movers made few educational gains, and no gains overall in reading and math test scores. King, Liebman and Katz (2007) reported no gains to economic self-sufficiency, a composite of employment, earnings, and welfare use measures (see also Orr et al., 2003). In addition, while female youth saw gains in education, risky behaviour, and physical health outcomes, male youth experienced worse outcomes in all three measures. However, families who moved with vouchers reported greater levels of satisfaction with their living conditions, including neighbourhood attributes such as litter, graffiti, loitering and abandoned property. Voucher movers (both experimental and section 8) reported living in improved quality housing and feeling safer in their neighbourhoods (Orr et al., 2003).

Responses to the findings have ranged widely. For some, the findings confirmed what many had suspected, that early researchers greatly exaggerated the extent to which neighbourhood conditions independently affect life chances (Ludwig et al., 2008). In their mind, the selection bias problem had been as serious as believed by sceptics. (This position, in turn, obviates the need for research on mechanisms, since there is not much of an effect whose internal processes demand attention.) For others, the study violated the assumption of no interference between units, the idea that a participant’s value depends only on the treatment to which the participant was assigned, not on that assigned to others. Since people were sampled from within housing projects in which people are assumed to interact with others with different treatments, leading to misleading results (Sobel, 2006). For still others, MTO was not especially informative about neighbourhood effects; rather, it provided evidence to assess a policy intervention of voucher-based housing assistance. For example, Sampson (2008) suggested that the MTO results cannot rule out that neighbourhoods matter: only that neighbourhood poverty, for an extremely disadvantaged and limited sub-sample of the population, in a handful of cities, did not affect a particular set of outcomes during the early 2000s. Furthermore, since movers often either relocated to areas that resembled their previous neighbourhoods or failed to stay in lower-poverty areas, the MTO may understate the true treatment effect. Clampet-Lundquist and Massey (2008) argued that the experimental intervention in the MTO may not have been appropriate, as the definition of “opportunity” neighbourhood was simply one in which less than 10% of the residents were poor, and experimental and voucher movers still ended up in predominantly African American neighbourhoods.

We do not believe that the Moving To Opportunity studies provide evidence that neighbourhood conditions are unimportant. Some findings, such as the effects on mental health and feelings of safety, are robust, consistent, and easily interpretable. Other findings, such as limited effects on unemployment, do not seem surprising, given that changing neighbourhoods does not alter an adult’s skill set and the intervention did not require residents to change job markets. Still other findings, such as the improvement in conditions for girls but their worsening for boys simply seem perplexing, particularly because they had not been previously hypothesized.

The principal lesson from the first round of neighbourhood studies through the early findings of MTO seems to be that whether neighbourhood poverty matters *depends*. The second lesson seems to be that the literature requires more information to interpret these results and more refined hypotheses to focus future research. These two lessons inform our argument that future work should be oriented toward expecting and explaining heterogeneity and that it should better integrate ethnographic fieldwork when generating hypotheses and explanations. We begin with the first of these propositions.

### **Moving Forward: From Homogeneous to Heterogeneous Treatment Effects**

We argue that researchers should assume that neighbourhood poverty has different effects not merely (as many have shown) on different outcomes but also, and more importantly, on different kinds of individuals, in different neighbourhoods, and in different cities (Small, 2004:175-89; Harding et al., forthcoming; also, Sobel, 2006). In the traditional regression framework, this argument calls for paying greater attention to the interaction between neighbourhood poverty and individual-, neighbourhood-, and city-level variables. In the counterfactual framework, it calls for assuming that treatment effects are heterogeneous across subpopulations and across settings (see Morgan & Winship, 2007; Hong & Raudenbush, 2006; Morgan 2001; Angrist 2004). Consider several recent studies demonstrating that whether neighbourhoods matter depends on conditions at the individual, neighbourhood, and city levels.

#### ***Individual level***

Several quantitative studies have shown that neighbourhood poverty may affect different individuals differently. For example, using data from the Panel Study of Income Dynamics, Turley (2003) found that higher neighbourhood income was associated with better test scores and behaviour among white children but not among black children. Similarly, as we discussed briefly, the initial evaluation of the MTO found that moving from a poor to non-poor neighbourhood helped females more than males: teen female movers were less likely to get arrested than the control group for violent or property crimes; teen male movers were actually more likely to get arrested for property crimes (Kling, Ludwig & Katz, 2005; Kling, Liebman & Katz 2007).

Ethnographic studies have also found that neighbourhood poverty may affect different residents differently. Small (2004) studied how neighbourhood poverty affected community participation in a predominantly Puerto Rican housing complex in Boston. He found that whether residents became strongly involved or remained uninterested depended substantially on how they perceived their neighbourhood—those who perceived it as a ghetto found little justification to participate. After conducting in-depth qualitative interviews with a subsample of MTO movers, Briggs and colleagues (2010) found that respondents differed in their orientations to family—some were “kin-centered,” others “kin-avoidant” or more oriented to the world of friends and acquaintances. Whether movers were more or less integrated into their new neighbourhoods depended on their familial and social obligations and on the location of those obligations. Along these general lines, Harding and colleagues (forthcoming) proposed that whether neighbourhoods affect life chances depends on the extent to which families have many or few resources. Collectively, these works call for models in which researchers better theorize and then test explicitly the presence of heterogeneity in the effects of neighbourhood conditions across subpopulations.

#### ***Neighbourhood level***

Poor neighbourhoods differ, for systematic and non-systematic reasons. For example, some poor neighbourhoods, because of the outmigration of the middle class, exhibit very low population

densities; others, because of immigration by the poor, exhibit very high density (e.g., Small, 2008). If the treatment is neighbourhood poverty, then the conditions under which the treatment is administered varies substantially across settings—and for reasons related to many outcomes of interest—threatening the validity of statistical inferences.

Several recent studies have made clear that neighbourhood-level differences of this kind are important. In the aforementioned study, Turley (2003) found that, for black children, higher neighbourhood incomes sometimes were related to test scores—only when they lived in those neighbourhoods with a high proportion of blacks. That is, the effect of neighbourhood income was conditional on the neighbourhood proportion black. In a study using data from the Zip Business Patterns and the U.S. census Small and McDermott (2006) examined the relationship between neighbourhood poverty and the level of organizational density, the number of banks, credit unions, childcare centres, grocery stores, pharmacies, and other everyday establishments. They found that the negative association between neighbourhood poverty and organizational density depended on the proportion of residents in the neighbourhood who were black—the greater the proportion black, the greater the negative association. In fact, the study found that in the statistically average neighbourhood, there was no relationship between neighbourhood poverty and the presence of most of these establishments.

### *City level*

Finally, several studies suggest that the effect of neighbourhood poverty depends on the city. Burdick-Will and colleagues (forthcoming) reanalyzed student test score data across the five MTO demonstration sites. On average, it appeared that students in the experimental mover conditions fared no better than the control group students on tests of math and reading (evaluated 4-7 years after baseline; see Sanbonmatsu et al., 2006). However, when they disaggregated the educational test results by city, the results differed. The authors compared the treatment-on-treated effect of moving on children's verbal test scores in the full MTO sample, and then separately for Chicago and Baltimore and for New York, Boston, and Los Angeles. While the full sample showed no statistically significant effect, the differences between the first two cities and the latter three were substantial. In fact, in Chicago and Baltimore the effects were strong, with movers performing 0.3 standard deviations better than the control group. While differences in math test scores were not statistically significant between movers and non-movers for either set of cities, the difference between the sets of sites was large, of comparable magnitude, and in the same direction as the results of the verbal scores. That is, neighbourhood effects on children's performance were more likely to be found in Chicago and Baltimore. Burdick-Will and colleagues noted that in Chicago and Baltimore, residents of poor neighbourhoods were more likely to be exposed to extreme levels of violence (see also Sharkey, 2009).

Our own research also demonstrates the extent to which the effects of neighbourhood poverty depend on city-level conditions. For this chapter, we prepared a test of the de-institutionalization hypotheses that produces similar results and makes clear the importance of heterogeneity across treatment settings. Wilson (1987) and others hypothesized that concentrated poverty undermines organizational density—the number of banks, clinics, bowling alleys, churches, recreation centres, and other commercial and non-profit establishments within a neighbourhood. As Wilson has argued, “poverty in ghetto neighbourhoods has sapped the vitality of local business and other institutions, and it has led to fewer... movie theatres, bowling alleys, restaurants, public parks and playgrounds, and other recreational facilities” (1995:9-10; also Wilson, 1987, 1996; Wacquant, 2007; Messner & Rosenfeld, 2001). However, Wilson and others derived their hypothesis based on work primarily in Chicago, without considering whether other cities would exhibit different relationships. In fact, as we discuss later, a long standing tradition has conceived of Chicago as a laboratory where phenomena occurring in the average

city, or the average large city, or the average Rustbelt city, can be observed with clarity. A perspective taking heterogeneity seriously would question that assumption.

We tested Wilson's hypothesis based on data on the presence of a range of establishments for all metropolitan statistical areas of the continental United States.<sup>2</sup> Based on an extensive review of the literature, we selected small establishments (fewer than 20 employees) and large ones (100 or more). Among small establishments, we identified five general types: day-to-day establishments (hardware stores, grocery stores, convenience stores, pharmacies, banks, credit unions, full- and limited-service restaurants, childcare centres, snack centres, laundries, grooming centres); small medical establishments (physicians' clinics, mental health physicians' clinics, dentists' offices, and other small offices of mental health), social service establishments (childcare centres, child and youth services, services for the elderly and disabled, and other individual and family services); recreational establishments (movie theatres, fitness and recreational sports centres, and bowling alleys); and social establishments (religious institutions, full- and limited-service restaurants, cafeterias, book stores, childcare centres, snack centres, and bars).<sup>3</sup> Among large establishments we studied large hardware stores (100 to 999 employees), large grocery stores (100 to 499 employees), and large medical establishments (100 or more employees).

Consider Table 1, which exhibits the number of establishments per 100,000 residents in poor neighbourhoods in Chicago, and in other cities.<sup>4</sup> Poor neighbourhoods are defined as zip codes in which 30% or more of the population is poor.<sup>5</sup> Items in bold are composites based on the sum of the non-bold items immediately below them. The table shows that poor neighbourhoods in Chicago have fewer establishments per 100,000 than poor neighbourhoods in the average city. In fact, poor neighbourhoods in Chicago are consistently below the mean for each of the major establishment types and almost every one of the sub-types. For example, the numbers in bold indicate that the average Chicago poor neighbourhood has 120 day-to-day establishments per 100,000 residents, while the average poor neighbourhood in the average city has 220; with respect to small medical establishments, the figures are 36 for Chicago and 104 for the average city. The pattern holds for social service establishments (29 in Chicago vs. 50 in the average city), small recreational establishments (less than one in Chicago, 4 in the average city), small social establishments (99 vs. 217), large grocery stores (slightly less than 1 vs. slightly more than 1), and hospitals (2.6 vs. 3.5). In short: Chicago poor neighbourhoods are substantially less organizationally dense than the average poor neighbourhood. Perhaps surprisingly, poor

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<sup>2</sup> We employ two datasets. The first is the Zip Business Patterns dataset, compiled by the U.S. Census, which contains data from a range of governmental administrative sources on all businesses and organizations in the United States that have a payroll. Businesses and organizations are identified by over 1,000 North American Industry Classification System (NAICS) codes. Our data consist of the number of establishments of several types (identified below) at the zip code level for the year 2000.) The second dataset is the 2000 U.S. Census, Summary File 3, from which we obtained demographic data at the zip code level and the level of the Metropolitan Statistical Area (MSA) and Primary Metropolitan Statistical Area (PMSA). We matched zip codes to metropolitan areas following a centroid-based approach; we calculated the geographic center of every zip code, and matched the zip code to the metropolitan area that contained that center. Our final two-level dataset contains a tabulation of every establishment with a payroll, by zip code ( $n = 13,776$ ), for every metropolitan area ( $n = 331$ ) in the U.S.

<sup>3</sup> We separated medical establishments from other service establishments due to their importance; we excluded schools due to the complicated dynamics underlying the spatial distribution of schools, as well as their ubiquity. Note that the categories are not mutually exclusive, and establishments playing more than one role are included in more than one category.

<sup>4</sup> Seventy zip codes with fewer than 100 residents were dropped from the full dataset. One hundred ninety-two zip codes with more than 1,000 establishments per 100,000 residents were dropped from the full dataset. These were unlikely to be residential. Finally, we dropped 1,384 zip codes that were larger than 100 square miles. Including them does not change the basic picture, but artificially inflates the numbers for the average city.

<sup>5</sup> Since zip codes are on average larger than tracts, we use a 30% cutoff, rather than the common 40% cutoff for very high poverty neighbourhoods. Less than .02% of zip codes in our dataset are 40% poor or greater.



neighbourhoods in Chicago also differ from those in Rustbelt cities, the subcategory of cities that many scholars suggest Chicago represents and resembles most.<sup>6</sup> The Chicago figures most resemble the ten largest cities, but its poor neighbourhoods are still less dense on average.

[INSERT TABLE 1 HERE]

Table 2 compares organizational density between poor and non-poor neighbourhoods. It displays, in summary form, the number of establishments per 100,000 in poor zip codes minus the number in non-poor zip codes for Chicago, the country, Rustbelt cities and the ten largest cities. As shown in the table, small and large establishments differ. With respect to small establishments, poor neighbourhoods in Chicago exhibit lower organizational density than non-poor neighbourhoods. In the average U.S. city, however, the opposite is true, as is the case in Rustbelt cities. That is to say, Wilson and others accurately observed conditions in Chicago, but these observations were improperly thought to be representative of the average U.S. city and Rustbelt cities. Researchers should have hypothesized that the effect of neighbourhood poverty on organizational density depends on the city.

[INSERT TABLE 2 HERE]

In sum, there is abundant evidence to suggest that heterogeneity across subpopulations and treatment settings should be a starting assumption, rather than an afterthought, in neighbourhood effects research. The MTO results can be read as confirming this fundamental thesis. Do neighbourhoods matter? The only sensible answer at this juncture is that it depends. The next phase of the literature should be devoted to understanding under what conditions neighbourhood poverty matters.

## **Moving Forward: Better Integrating Ethnographic Research**

In addition to encouraging a concern for heterogeneity, the new phase of neighbourhood effects research calls for integrating ethnographic research more tightly into the research agenda. By “ethnographic research” we refer to both open-ended interviews and participant observation conducted by researchers interacting one-on-one with people, organizations, and public places in urban neighbourhoods. In the neighbourhood effects field, ethnographers often cite demographers or economists and vice versa. Nevertheless, we argue that building the cumulative knowledge that overcomes past limitations in neighbourhood effects research requires developing a more symbiotic relationship between quantitative and qualitative research, a relationship that, over the long run, should resemble a mixed method, rather than merely multi-method, enterprise (Tashakkori & Teddlie, 2003).

### ***Two roles for ethnographic research***

Ethnographic work should be integrated into the research agenda in at least two general forms: to help explain the results of prior to studies and to help generate hypotheses for future ones.<sup>7</sup> We discuss each in turn.

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<sup>6</sup> Because we could not find a definitive description of what constitutes the “Rustbelt” we drew from three sources: a) Jargowsky (1997), in which Indiana, Illinois, Wisconsin, Ohio and Michigan comprise the Rustbelt; b) Teaford’s (1993) history of Midwest industrialization, which encompasses Jargowsky’s definition and adds Missouri; and c) a standard Census Bureau (1998) measure, which adds Minnesota, Iowa, Kentucky and West Virginia to Teaford’s list. We count as “Rustbelt cities” all MSA/PMSAs within the abovementioned states. Regardless of the Rustbelt definition we employed, the results for Tables 1 and 2 remain substantively unchanged (available upon request).

First, ethnographic studies should be deployed to help explain the results of prior quantitative work, since only such studies provide direct access to the conditions of poor neighbourhoods and their residents' interpretations of these conditions. To the extent that researchers seek to identify the mechanisms that produced an observed effect or account for the absence of an expected relationship, they require access to how residents of poor neighbourhoods make decisions about their circumstances, a process impossible to capture fully without fieldwork. The first phase of neighbourhood effects research, culminating in the MTO studies, has produced a plethora of findings, many of them inconsistent and even contradictory, in need of explanation. Why did girls fare better than boys? Why was organizational density lower in black poor neighbourhoods but not in other poor neighbourhoods? Why did movers improve test scores in Baltimore and Chicago but not in New York, Boston, and Los Angeles? Why was neighbourhood income positively associated with white but not black children's test scores? Hundreds of findings call for explanation.

Ethnographic studies have played a rather limited role in that undertaking. Many quantitative researchers have relied less on ethnographies than on existing theory to explain their results, and many qualitative researchers have neglected to propose explanations for the results of quantitative studies. In fact, most of the ethnographic studies produced in the 1990s and early 2000s did not propose interpretations of the collective, accumulating findings of the neighbourhood effects literature (but see Patillo, 1999; Small, 2004; Harding 2010). And with the notable exception of Briggs and colleagues (2010; see also DeLuca et al., 2011 in this volume), most ethnographers did not design their research projects to help explain the particular results of previous large-scale studies, in spite of the contradictions in the quantitative findings that were arising early on. Fully making sense of the current slew of quantitative neighbourhood effects findings will prove difficult without deeply observing and interviewing people in their neighbourhoods and cities.

Second, ethnographic studies should play a central role in the process of developing hypotheses, since hypotheses based strictly on theoretical reflection, rather than at least some empirical engagement, face the risk that anthropologists have long attributed to arm-chair theories: they generate expectations that, after the fact, appear to be obviously misguided. For example, the fact that the MTO studies found no effects on unemployment seems unsurprising in retrospect, since participants were not required to change job markets. At the same time, however, no ethnographic studies had suggested that low-income job seekers would find the job hunt easier if they moved to neighbourhoods that had lower poverty rates but not necessarily more jobs. Prior fieldwork has proven repeatedly to help discipline the theorist's imagination.

To date, ethnographies have not been integral to the specific hypotheses tested in neighbourhood effects research. To be sure, several ethnographic studies have played some role. For example, Wilson (1987) explained that many of his ideas about the effects of concentrated poverty on organizational capacity derived from his personal observations of the South Side of Chicago (also Wacquant & Wilson, 1989). In addition, the more systematically ethnographic studies of Edin and Lein (1997), Duneier (1992), Klinenberg (2002), Newman (1999), Pattillo (1999), and Venkatesh (2000), have also been cited in much of the work on neighbourhood effects (see Newman & Massengill, 2006; Sampson et al., 2002). Nevertheless, many of the hypotheses tested in the first wave of neighbourhood effects research, including those tested by MTO, did not seem to have much grounding in ethnographic fieldwork. For example, when proposing the mechanisms through which neighbourhoods should affect life chances, Jencks and Mayer (1990) referred to few ethnographies (in part because these lacked the comparative assessments being reviewed in their study). Similarly, in their paper generating hypotheses for why neighbourhoods might matter, Ellen and Turner (2003) proposed mechanisms such as

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<sup>7</sup> We recognize that this categorization does not do justice to the many ways that different research traditions can be integrated from a mixed methods perspective. For reviews of some alternatives, see (Tashakkori & Teddlie, 2003).

lower quality of local institutions, weaker norms and collective efficacy, and ineffective social networks, but cited virtually no ethnographies to inform these conjectures. Along these lines, when hypothesizing the possible pathways through which neighbourhood poverty may affect youth outcomes in their New York City MTO evaluation, Leventhal and Brooks-Gunn (2003) relied on well-established theoretical models but not on ethnographic field research. And in their studies of the effects of neighbourhood disadvantage on collective efficacy and other outcomes, Sampson and colleagues derive their hypotheses less from current ethnographic studies than from prior theories and large-*n* studies of the neighbourhood-level precursors of crime (Shaw & McKay, 1942; see Sampson, Raudenbush & Earls, 1997).

We must also note, however, that ethnographic studies had only rarely produced concrete hypotheses for quantitative studies to evaluate. On the contrary, while many ethnographers dating back to the 1960s and 1970s painted vivid pictures of conditions in poor urban neighbourhoods (Liebow, 1967; Hannerz, 1969; Stack, 1974; Anderson, 1978), few of them structured their work with the express purpose of generating the explicit, testable hypotheses that guide large-*n* research. At this juncture, the field beckons for such hypotheses from fieldworkers.<sup>8</sup>

We see little hope of progress in the neighbourhood effects literature unless these two approaches to qualitative work—accounting for the results of prior quantitative research and generating hypotheses for future quantitative studies—become more central than they have been to the larger enterprise. The contradictory findings of MTO and prior studies call for a coherent interpretation, or set of interpretations, which require a strong grounding in carefully targeted ethnographic research. Furthermore, the cacophony of ideas about how neighbourhood matters—including a large and ever-growing set of notions about how to conceive of the underlying mechanisms of neighbourhood effects—has greater chances of arriving at some harmony if it is disciplined by a strong engagement with the field. Of the (now) scores of extant hypotheses about how neighbourhoods matter, those supported by targeted fieldwork should be prioritized strongly by researchers.<sup>9</sup>

### ***Ethnographic data and heterogeneity***

Nevertheless, while the findings of the first major wave of neighbourhood effects research call for integrating ethnography more effectively, this integration is unlikely to prove useful unless researchers also change their practical and theoretical assumptions toward a model in which, until proven otherwise, treatment effects are assumed to be heterogeneous and relationships are assumed to be conditional. This reorientation affects what questions are asked, what answers are expected, how cities and neighbourhoods and individuals are selected for study, how hypotheses

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<sup>8</sup> We are presuming that hypotheses derived from data are superior to those derived from intuition or prior beliefs. This need not be the case: Poorly conceived or non-testable hypotheses can derive from any source. Nonetheless, we do not believe that our presumption is unwarranted. At a minimum, a hypothesis derived from actual cases will necessarily be supported at least some of the time; *ceteris paribus*, it is more likely to be successful.

<sup>9</sup> We do not propose that qualitative work in urban neighbourhoods should devote itself exclusively to these two rather narrowly defined tasks. In fact, several ethnographic traditions would probably scoff at the idea of narrowly focusing ethnographic work in this manner, in part because while sharpening the nature of at least some contributions it could easily hamper creativity of other kinds. Many important discoveries evolve serendipitously. For example, several ethnographies have demonstrated that conditions *other* than neighbourhood poverty or disadvantage matter as much as neighbourhoods, such as family, schools, and, particularly in recent years, local organizations (McRoberts, 2003; Marwell, 2004, 2007; Small, 2009b; more on this below). Furthermore, few ethnographers could motivate investing in large-scale, multi-year ethnographic studies that promise to generate multiple ideas only to narrowly target those ideas, at the end of the effort, toward an existing quantitative literature. Nevertheless, it seems clear that without at least some researchers dedicating some of their work to these tasks, the neighbourhood effects research has little hope of cumulative progress.

are produced, what kinds of hypotheses are generated, and, in general, how neighbourhoods are expected to affect behaviour.

Consider Table 3. The left column represents the orientation toward neighbourhood effects that, from both quantitative and qualitative perspectives, has characterized most of the literature until recently. Under that model, if ethnographies were more tightly integrated to quantitative research, they would take the following form. When developing explanations for observed neighbourhood effects, ethnographers would begin with the discovery of direct effects (such as greater fear of safety), find an assumed representative city (such as Chicago), select an assumed representative poor neighbourhood (such as Woodlawn on the South Side) or population (such as black poor mothers), and try to find the mechanisms linking neighbourhood poverty to the outcome (the reasons those who feel unsafe seem to do so). From the perspective on the left column, only this procedure would ensure an explanation likely to be applicable regardless of context. When generating hypotheses for future quantitative studies, researchers would also find an assumed representative city, select an assumed representative poor neighbourhood, housing project, or population; and generate hypotheses about how neighbourhood poverty affects a given outcome—for example, neighbourhood poverty would be hypothesized to increase social isolation (outcome) by increasing distrust (mechanism). From the perspective on the left column, this procedure would help ensure the generalizability of the proposition.

[INSERT TABLE 3 HERE]

The core assumptions behind that model, particularly the idea that researchers should begin by identifying a representative city, are deeply entrenched in the literature. First, that model is a manifestation of one of the tenets of the Chicago School of Sociology: cities take form as a result of inherent ecological processes, and the city of Chicago is an ideal “laboratory” for observing them. Because of this assumption, and the long-standing tradition of urban research at the University of Chicago, an overwhelming number of studies in the neighbourhood effects literature are based in the city of Chicago. Most of the ethnographic research cited in the neighbourhood effects literature was conducted, like Wilson’s work, in Chicago (Wacquant & Wilson, 1989; Duneier, 1992; Pattillo, 1999; Venkatesh, 2000; Klinenberg, 2002). In fact, some of the most highly used datasets to examine neighbourhood effects are sited in Chicago. The important Project on Human Development in Chicago Neighbourhoods (PHDCN) has been used to support many of the propositions that neighbourhoods matter (Sampson, Raudenbush & Earls, 1997). The earlier Urban Poverty and Family Life Survey, which was the basis of Wilson (1996) was also fielded in Chicago. In fact, many of the multi-city studies, such as the Multi-City Study of Urban Inequality, MTO, and the Three-City Study, included Chicago in their samples. No American city has been studied more carefully by urban social scientists, in part because of the idea that it exhibits universal phenomena. Second, the idea that researchers should identify a representative neighbourhood is a natural extension of the logic of demographic quantitative research—select a sample that is representative—applied to ethnography (e.g. King, Keohane & Verba, 1994; for a critique, see Small, 2009). In the ethnographic tradition, researchers have rarely selected neighbourhoods presumed to be representative in a statistical sense (but see McDermott, 2006); most commonly, they have studied neighbourhoods that seemed to typify concentrated poverty, such as large public housing projects or predominantly black poor neighbourhoods (e.g., Venkatesh, 2000; Wacquant, 2007; see Small, 2004, 2007, 2008).<sup>10</sup> Either way, the model has been to look for cases that appear to be typical poor neighbourhoods.

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<sup>10</sup> There is a larger methodological debate regarding the proper approach to conduct field research, conceive of case studies, and address the issue of generalizability. For discussions, see Small (2009) and prior work by Mitchell (1983), Ragin (1989), Ragin and Becker (1992), Small (2004, 2008), Yin (2002), and Znaniencki (1934).

Despite its popularity, continuing to operate under that model for producing explanations and hypotheses will help neither the integration of ethnographies nor the progress of literature, since it remains mired in the assumptions of homogeneous treatment effects, unconditional relationships, and inherent neighbourhood poverty forces that led researchers to overestimate what the MTO would show and has left them at a loss to explain a slew of disparate findings.

By contrast, consider the right column of Table 3, which illustrates the heterogeneity assumption that, we argue, is called for by the most recent wave of research on neighbourhood effects and should inform ethnographic study. Rather than centre solely or even primarily on direct effects, researchers would probe the extent to which the effects of neighbourhood poverty depend on city-, neighbourhood-, or individual-level conditions. From the traditional regression perspective, it may seem curious to pursue what appear to be interaction effects in favour of direct effects, but the first twenty years of work clearly suggest that whether neighbourhood poverty matters depends on the circumstances. From the counterfactual perspective, our proposition probably appears more straightforward; it calls for assuming heterogeneity across subpopulations and across treatment settings unless proven otherwise. This heterogeneity—e.g., that poor neighbourhoods affect boys and girls differently, that some neighbourhoods are depopulated while others overcrowded, that those in New York differ from those in Chicago—which once appeared secondary, can no longer be considered unimportant; it should form the foundation of ethnographers' efforts to provide explanations and identify the mechanisms underlying observed associations. Ethnographers should begin, for example, with the fact that the MTO treatment group improved verbal scores in Baltimore and Chicago but not in New York, Boston, or Los Angeles (Burdick-Will et al., forthcoming). As Small (2004: 176) argued based on research in a Puerto Rican housing complex in Boston, researchers should “use heterogeneity in responses to neighbourhood poverty as the starting point rather than [something] to ignore...” Observed differences between populations, neighbourhoods, and cities should constitute the point of departure for those developing explanations.

Furthermore, as we indicate in the bottom right panel of Table 3, ethnographers generating new hypotheses should abandon altogether the effort to produce hypotheses about how neighbourhoods (universally) affect life chances, and instead hypothesize for which kinds of individuals and in which kinds of neighbourhoods or cities neighbourhood poverty should matter (Small, 2004). The question of how to generate hypotheses based on fieldwork gains poignancy when selecting sites for analysis. An ethnographer operating under the old model would seek neighbourhoods and cities that appeared representative. The fact that ethnographies, by necessity, are usually limited in scope to one or two neighbourhoods in one or two cities resulted in the persistent worry that the cases might not be representative or typical.<sup>11</sup>

An ethnographer operating under the heterogeneity model differs in several respects. First, the ethnographer who assumes that effects, to greater or lesser extent, depend on conditions at the city, neighbourhood, and individual levels must seek comparisons at one or more of those levels. Comparative designs become favoured. Second, since no city, neighbourhood, or individual is assumed to capture the essence of neighbourhood effects, the ethnographer would abandon the notion that any site or kind of actor is representative. Rather than studying Chicago on the assumption that it bears evidence to universal phenomena, the researcher would study under-explored cities such as Memphis, Pittsburgh, Phoenix, Eugene, New Haven, or Denver, under the assumption that conditions may differ substantially from oft-repeated stories and that these differences may condition the effects of neighbourhood poverty. Rather than studying, or only studying, predominantly black housing projects in areas losing low-skilled manufacturing jobs, the researcher would study, or also study, comparatively under-

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<sup>11</sup> In fact, many would question the logic of pursuing single-case studies by searching for average cases; see Small (2004, 2007, 2009).

explored sites such as Chinese-American neighbourhoods with high proportions of poor immigrants, predominantly white poor neighbourhoods with high levels of drug abuse, or aging multi-ethnic neighbourhoods with high proportions of residents on fixed retirement incomes. These differences form the bases for new hypotheses about how neighbourhoods matter. Third, the ethnographer adopting this model would generate a different kind of hypothesis. Since the objective is to hypothesize the causes behind differences, the hypotheses would necessarily focus on the mechanisms through which either different kinds of individuals respond to the stimulus of neighbourhood poverty (in the case of heterogeneity across subpopulations) or different kinds of neighbourhood or city contexts alter the stimulus (in the case of heterogeneity across settings). Rather than providing expectations about inherent descriptive traits, they would provide expectations about the mechanisms generating differences.

## Conclusion

We have argued that the conflicting and inconsistent results of the first wave of studies on neighbourhood effects call for placing heterogeneity at the centre of the research agenda and for better mobilizing ethnographic research to explain these results and generate new expectations. We have also argued that better integrating ethnographies requires that this process, too, be structured toward heterogeneity, toward explaining why different kinds of actors respond differently to neighbourhood poverty, why poor neighbourhoods of different kinds exhibit different patterns, and why poor neighbourhoods in different cities vary so substantially and appear to affect individuals differently. In this endeavour, the old notion that particular kinds of neighbourhoods or cities—notably Chicago—represent ideal laboratories to observe universal social processes is increasingly unhelpful.

The early phase of neighbourhood effects research posed a clear set of questions—does neighbourhood poverty affect life chances (net of selection bias), and, if so, how?—that provided a clear target for a diverse set of sociologists, geographers, developmental psychologists, demographers, and economists to pursue. The first of these questions helped generate strong academic support for an ambitious randomized control trial whose results were eagerly awaited. It seems increasingly clear, however, that much of this work would be better served if informed by clearer theories and stronger fieldwork, and that the greater empirical payoffs would have been found in the search for conditional relationships. As researchers pursue new kinds of questions, they would do well to transcend the limitations of the past.

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**Table 1. Mean number of establishments per 100,000 residents, high poverty zip codes, 2000**

	Chicago	All cities	Ten largest cities	Rustbelt cities
<b>Small day-to-day establishments</b>	<b>120.21</b>	<b>219.85</b>	<b>179.67</b>	<b>209.04</b>
Hardware stores	3.55	3.46	3.32	2.57
Grocery stores	32.78	33.11	37.38	35.39
Convenience stores	4.70	14.29	11.82	16.16
Pharmacies	10.04	11.47	11.57	10.91
Banks	0.83	2.97	1.45	3.03
Credit unions	2.27	11.21	4.62	12.71
Childcare centers	16.00	23.07	14.67	21.64
Full-service restaurants	11.82	43.68	32.67	36.70
Limited-service restaurants	23.18	46.07	34.64	41.68
Cafeterias	0.13	2.18	1.40	1.83
Snack and non-alcoholic beverage centers	3.30	9.32	8.22	8.65
Laundries	8.31	4.94	7.08	4.81
Grooming centers	3.31	14.08	10.83	12.96
<b>Small medical establishments</b>	<b>36.00</b>	<b>104.35</b>	<b>79.61</b>	<b>87.44</b>
Offices of physicians (exc. mental health)	25.99	74.69	59.66	62.52
Offices of physicians, mental health	2.27	4.44	2.94	3.70
Offices of dentists	6.87	22.13	16.04	18.31
Offices of other mental health practitioner	0.86	3.09	0.97	2.91
<b>Small social service establishments</b>	<b>29.30</b>	<b>50.29</b>	<b>32.51</b>	<b>55.40</b>
Childcare centers	16.00	23.07	14.67	21.64
Child and youth services	3.97	5.84	4.32	7.88
Services for elderly and disabled persons	2.20	6.28	4.38	7.02
Other individual and family services	7.13	15.10	9.13	18.87
<b>Small recreational establishments</b>	<b>0.39</b>	<b>3.93</b>	<b>2.73</b>	<b>3.72</b>
Movie theaters	0.00	0.53	0.59	0.78
Fitness and recreational sports centers	0.22	3.18	1.92	2.50
Bowling alleys	0.17	0.23	0.22	0.44
<b>Small social establishments</b>	<b>98.96</b>	<b>216.79</b>	<b>145.12</b>	<b>228.88</b>
Religious orgs (e.g. churches, mosques)	34.03	60.16	33.79	77.23
Full-service restaurants	11.82	43.68	32.67	36.70
Limited-service restaurants	23.18	46.07	34.64	41.68
Cafeterias	0.13	2.18	1.40	1.83
Book stores	0.99	4.44	3.11	4.77
Childcare centers	16.00	23.07	14.67	21.64
Snack and non-alcoholic beverage centers	3.30	9.32	8.22	8.65
Alcoholic beverage drinking places	9.52	27.86	16.61	36.39
<b>Large hardware stores</b>	<b>0.00</b>	<b>0.01</b>	<b>0.00</b>	<b>0.00</b>
<b>Large grocery stores</b>	<b>0.74</b>	<b>1.33</b>	<b>0.98</b>	<b>1.03</b>
<b>Large medical establishments</b>	<b>2.57</b>	<b>3.48</b>	<b>2.54</b>	<b>3.48</b>
HMO medical centers	0.26	0.09	0.06	0.17
General medical and surgical hospitals	2.31	2.98	1.85	3.15
Other specialty hospitals	0.00	0.42	0.63	0.16

Source: Zip Business Patterns, 2000. U.S. Census. Zip code-level data GIS-matched to 331 metropolitan areas. Figures limited to zip codes smaller than 100 square miles with more than 100 residents, and no more than 1,000 day-to-day establishments per 100,000 residents, and those in which 30% or more of all persons are poor. There are 10 such zip codes in Chicago; 517 in all cities combined; 128 in the 10 most populous metro areas; and 124 in cities that are located in Rustbelt states (IN, IL, WI, OH, MI, MO, MN, IA, KY, WV). Small establishments have fewer than 20 employees. Large hardware stores have between 100 and 999 employees; large grocery stores have between 100 and 499 employees; large medical establishments have 100 or more employees. Categories listed in bold font are additive indices of the establishments listed beneath the category heading.

**Table 2. Difference in organizational density between poor and non-poor zip codes, 2000**

	Number of establishments per 100,000 persons in poor zip codes minus non-poor zip codes			
	Chicago	All Cities	Ten largest cities	Rustbelt cities
Small day-to-day establishments	-66.22	37.61	-16.94	44.23
Small medical establishments	-78.08	13.23	-43.85	16.72
Small social service establishments	4.60	21.72	3.50	28.94
Small recreational establishments	-7.53	-4.62	-6.20	-4.28
Small social establishments	-76.79	33.44	-26.37	36.47
Large hardware stores	0.00	0.00	-0.01	-0.01
Large grocery stores	-2.56	-1.27	-1.86	-1.17
Large medical establishments	1.72	2.38	1.47	2.47

Source: See Table 1.

**Table 3. Role of ethnographic research under different assumptions about nature of neighbourhood effects**

	<i>Homogeneity assumption</i>	<i>Heterogeneity assumption</i>
<i>Explain existing results (post quant work)</i>	Explain observed effects	Explain why effects observed in some settings but not others, and for some populations but not others
<i>Generate new hypotheses (pre quant work)</i>	Hypothesize how neighbourhood poverty will affect life chances	Hypothesize for which populations and under which settings neighbourhood poverty will affect life chances