Chapter 13

Unpacking Neighborhood Influences on Education Outcomes: Setting the Stage for Future Research

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The link between rising income inequality and rising residential segregation by income suggests that, if neighborhood environments affect educational outcomes, diverging educational outcomes may be due in part to the increasing numbers of youths growing up in neighborhoods of concentrated poverty or concentrated affluence. Most existing social science research on neighborhoods conceptualizes neighborhood effects at the macro-level, hypothesizing how general neighborhood characteristics such as the poverty rate affect a variety of individual and family outcomes. Contemporary research on this topic has largely failed to recognize the diverse types of families living in poor neighborhoods or the potentially wide variety of ways that they may respond to a given set of neighborhood conditions. Our core argument is that future neighborhood research must seriously consider this diversity both conceptually and methodologically.

Our hypothesis is that there is considerable heterogeneity in the experiences of youths in the same neighborhood that might vary by their personal or family resources, their ability to cope with or navigate neighborhood circumstances, and the decisions that youths and their family make about how, with whom, and where their time is spent. Depending on these and other factors, different youths may get a different “dose” of their neighborhood. This heterogeneity can generate substantial variation in how a given neighborhood characteristic affects any one youth. Youths who live in the same neighborhood may experience it in different ways, leading to effect heterogeneity: neighborhood effects of different direction or magnitude for different youths.

Effect heterogeneity is the core of our argument. We therefore motivate future neighborhood research using a simple model that considers youths’ educational outcomes as a function of neighborhood context, neighborhood exposure, individual vulnerability to neighborhood effects, and non-neighborhood educational inputs. Moving this research agenda forward requires three steps. First, researchers need to shift focus away from broad theories of neighborhood effects and examine the specific mechanisms through which the characteristics of a neighborhood might affect an individual. By “mechanisms,” we mean the social, economic, and cultural processes that create associations between the compositional or demographic characteristics of neighborhoods, such as neighborhood poverty, and individual educational outcomes, such as achievement scores or educational attainment.

Second, neighborhood research desperately needs new and far more nuanced data. In particular, we need data that measure how individuals and families of different types
allocate their time between different places, the extent of exposure to different people and locations, and the consequent influences on individual behavior.

Third, we advocate for research designs that can unpack the causal effects, if any, of specific neighborhood characteristics as they operate through well-specified mechanisms. Much current neighborhood research estimates the reduced form or total neighborhood effect. The bane of this literature has been the problem of selection—whether differences in outcomes are due to the neighborhoods themselves or instead reflect differences in the characteristics of individuals who live in different types of neighborhoods. Rather than trying to assess the overall effect of living in a particular type of neighborhood, researchers should strive to examine discrete mechanisms in ways that account for effect heterogeneity.

In the first section of this chapter we introduce our conceptual framework; in the second section we argue for a shift from general theories to concrete specifications of mechanisms and sources of effect heterogeneity. In the third section we describe the need for new, detailed data on social interactions, both neighborhood- and non-neighborhood-based (including schools) that allow for measurement of responses and exposure to people and places. In the fourth section we discuss the types of research designs that might profitably be employed to estimate the effects of such interactions on educational outcomes. In the fifth section we present a substantive example through which we illustrate one possible research design.

Rising income inequality has led to rising residential segregation by income, especially among blacks. Changes in income inequality have particularly increased the spatial segregation of the affluent from middle-class and lower-income families (Reardon and Bischoff 2011). These trends suggest that diverging educational outcomes by family income (see chapters 3 and 5 in this volume) may be due in part to the increasing numbers of youths growing up in neighborhoods characterized by either concentrated poverty or concentrated affluence. These impacts depend on both the nature and magnitude of the effects of concentrated-poverty neighborhoods and concentrated-affluence neighborhoods on educational outcomes. These impacts may also depend on the timing of exposure to different neighborhood environments by developmental stage and the cumulative effects of sustained exposure over time.

Our goal in this chapter is to set the stage for future research—its opportunities as well as challenges—to better understand the influence of neighborhood social settings on youths’ educational outcomes. For the purposes of this chapter, we intentionally define a neighborhood social setting broadly: a setting that is outside home and school. We differentiate social settings according to the characteristics of the place, the types of people with whom the individual interacts, and how time is spent there. Our definition casts a wide net so that consideration may be given to a range of youths’ experiences, whether time spent in an after-school program, hanging out at a basketball court or the local mall, or staying at home.

Because neighborhood context is the most frequently discussed social setting, we ground our discussion in neighborhood effects on education. Our analysis is driven by a simple yet novel conceptual framework in which a youths’ educational outcome \( Y \) is a multiplicative function of the neighborhood context \( N \), individual exposure to that neighborhood context \( E \), and individual vulnerability to the effects of the neighborhood context \( V \) as well as other variables \( X \). 

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Y = f(N, E, V, X) = (N \times E \times V) + X, 
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where each quantity potentially has multiple dimensions. The prior literature has primarily focused on estimating the effects of compositional measures of \( N \) (such as neighborhood poverty rate) and on the methodological challenges of identifying the effect
of $N$, particularly separating the effects of neighborhood context from the preexisting differences between residents of different neighborhoods ($X$).

Although selection bias remains a central issue worthy of further research, we argue that the literature has too often ignored several other key research problems that are captured in our model. The first is the mechanisms, or social processes, by which neighborhood context ($N$) affects individual outcomes ($Y$). The second is effect heterogeneity, or differences between individuals in the effects of $N$ on $Y$. This heterogeneity is driven by both $E$ and $V$. Different youths living in the same neighborhood will have different histories of exposure ($E$) to the people, places, and activities that drive neighborhood effects. In other words, $E$ can be thought of as the "dose" of different neighborhood characteristics that an individual receives. $E$ may vary by cumulative time exposed to a neighborhood environment or by differential exposure to specific mechanisms. As Patrick Sharkey (2006) argues, individuals to some degree determine their level of exposure to different neighborhood characteristics through the decisions they (and their parents) make about where, how, and with whom to spend their time. Though the neighborhood-effects literature has focused on the selection of individuals and families into neighborhoods, the selection of exposure to different neighborhood characteristics presents a second identification problem, what we might call "within-neighborhood selection bias." The social and economic processes that create differential exposure are worthy of study from both a methodological and a substantive perspective. Effect heterogeneity may also be generated by differences in the vulnerability ($V$) or susceptibility of youths to the effects of the neighborhood ($N$). This variation in vulnerability may be driven by differences in individual and family characteristics that make some youths more or less susceptible to neighborhood-effects mechanisms. One source of difference in vulnerability is age-specific developmental needs, but other factors such as parenting decisions and family resources may also play a role. For example, consider the possible responses to neighborhood violence among parents of male adolescents. Some parents may require their sons to stay inside. For some, this will mean more time studying; for others, more time watching TV. For the first individual, the effect of neighborhood violence will be to increase educational attainment; for the second, the effect will be neutral or to decrease educational attainment. Considerably more theorizing is needed to understand the processes behind effect heterogeneity due to both $E$ and $V$.

Although policy recommendations are not explicitly advanced in this chapter, we think the agenda put forth here is an important step toward building evidence to inform public policy. In conceptualizing neighborhood effects, it is helpful to differentiate between interventions that are designed to affect residential mobility and interventions that are designed to directly change a place or neighborhood. In the former, an individual’s neighborhood environment changes because his or her family moves to a new neighborhood, as in the Gautreaux project and the Moving to Opportunity (MTO) housing mobility experiment. In the latter, the characteristics of the social setting or environment that the neighborhood provides for children is targeted for change (Sampson 2008). As we think about estimating neighborhood effects on education, we will return throughout the chapter to these two types of policy interventions. In the example study design described later in this chapter (see "An Illustrative Example"), we describe an intervention that changes individual exposure to the neighborhood.

CONCEPTUALIZING EFFECTS OF SOCIAL SETTINGS ON EDUCATIONAL OUTCOMES

In this section we motivate our critique of traditional neighborhood research by describing various mechanisms that might affect individual outcomes and, in appropriate cases, why these effects might differ across families or individuals.
Environment and Health  Some of the processes through which neighborhood context may affect educational outcomes occur because of geographic location or physical proximity, rather than through local social interactions. One such mechanism operates through environmental or health effects. For example, a neighborhood adjacent to a major highway may expose children to high levels of particulate-matter pollution that leads to asthma and therefore to more school absences.

Spatial Mismatch  Another mechanism is proximity to jobs, or “spatial mismatch” (Jencks and Mayer 1990; Moww 2000). A neighborhood located near an abundance of job opportunities may affect the way that individual youths think about the rewards of their own education and future opportunities (Anderson 1999).

Violence  Neighborhood violence may affect the amount of time youths spend on homework if going outside means risking an experience with violence or victimization. Witnessing frequent acts of violence may lead to post-traumatic stress disorder or biological responses to stress that can also interfere with learning (Massey 2001, 2004).

Neighborhood Resources  Neighborhoods certainly differ in their institutions and other resources or resource brokers. For example, one neighborhood may have an after-school program that provides homework help, and another may not. One neighborhood may be near a community center that provides a safe, supervised space for teens to hang out, and another may not. We are only beginning to understand which resources are more or less abundant in different neighborhoods (Small 2006, 2009; Small and McDermott 2006; Small and Stark 2005). Mario L. Small and Monica McDermott (2006) find that, on average, poor neighborhoods actually have slightly more commercial establishments such as pharmacies, grocery stores, and child-care centers, but that poor black neighborhoods with declining population density have fewer such establishments. Neighborhood resources can also fluctuate with the residential mobility of middle-class families, who disproportionately sustain community institutions and organizations (Wilson 1987).

Culture  Cultural mechanisms may also be important, as posited by, for example, social isolation theory (Wilson 1987; Massey and Denton 1993). According to this theory, poor, inner-city black neighborhoods are thought to be socially isolated from “mainstream” or middle-class individuals and institutions (particularly the labor market). This leads to cultural isolation and the development of a “ghetto-specific” culture, which orients young people away from schooling by reinforcing norms and values that denigrate the value of education. A similar formulation is Signithia Fordham and John Ogbu’s (1986) oppositional culture theory (see also Ogbu 2004), according to which behaviors that promote academic achievement—such as speaking standard English, doing homework, and engaging in class discussion—in poor black communities become defined as “acting white” in response to discrimination, inferior schools, and blocked labor-market opportunities (see also Massey and Denton 1993). However, subsequent examinations have found no evidence for the core claim that black students are disproportionately sanctioned by their peers for academic effort (Cook and Ludwig 1998; Ainsworth-Darnell and Downey 1998; Harris 2006; Carter 2005). One exception is Roland Fryer (2006), who finds that black students who get good grades are less popular than whites who get good grades, but only in racially mixed public schools. When students’ experiences with oppositional culture are investigated, we begin to see why. Prudence L. Carter (2005) finds that notions of “acting white” among poor black and Latino youths have more to do with musical tastes, fashion, and speech patterns than with academic performance or effort.
An alternative perspective on the cultural context of poor neighborhoods emphasizes the cultural heterogeneity of such neighborhoods. Instead of a distinct subculture, neighborhood cultural context can be conceptualized as culturally heterogeneous, incorporating competing and conflicting cultural models, some of which are shared across society more broadly and some of which are locally developed alternatives (Harding 2007, 2010). For example, Carter (2005) argues that poor youths have a “continuum of cultural attachments” available to them, which different youths embrace to differing degrees. For young people enmeshed in this heterogeneous cultural environment, creating and sustaining strategies for career and school success are major challenges (Harding 2010). When multiple educational and career pathways are locally available and socially supported, some adolescents may jump to alternative pathways when one pathway becomes challenging, whether or not they have full information about the risks and benefits of new strategies or the financial and cultural resources to successfully implement them.

Social Organization

Social organization theory focuses on the capacity of neighborhood residents to regulate behavior that occurs within the neighborhood. It is relevant to another set of neighborhood mechanisms, including school-related behaviors, such as truancy. Much of the current emphasis on the capacity of neighborhoods is on behavior that occurs in public spaces, particularly crime and violence. Collective efficacy, defined as “social cohesion among neighbors combined with their willingness to intervene on behalf of the common good” (Sampson, Raudenbush, and Earls 1997), mediates the relationship between compositional neighborhood characteristics (residential instability, ethnic or racial heterogeneity, and poverty) and crime rates. When parents in a community are connected to one another, what James S. Coleman (1988) calls “intergenerational closure,” they are thought to be better able to present adolescents with a consistent set of cultural ideals regarding education, which leads to more school effort. They should also be better able to monitor and control their children’s education-related behaviors, such as school attendance. However, when the idea of intergenerational closure has been applied to schools, it has not always met with consistent empirical support (see, for example, Morgan and Sorensen 1999), which raises the question of whether, how, or under what circumstances socially connected parents can indeed enforce common educational ideals, and whether only pro-schooling cultural ideals are strengthened by intergenerational closure. It is also unclear what proportion of the parents in a community need to be well connected in order for the community to effectively monitor adolescent behavior. Such tipping points or thresholds remain relatively unexamined.

Local Incentives

Local incentives refers to an economic perspective on social interactions by individuals as agents responding to incentives provided by the local environment and shaped by their time and money resources (Manski 2000). For example, living in a neighborhood in which many students drop out of high school might reduce the stigma of dropping out and thus increase a student’s preference for doing so. Such a preference depends on the actions of other individuals. In another neighborhood, observing neighbors who attend college and obtain high-paying jobs may increase expectations about the benefits of attending college, but one negative consequence of this increase may be a reduced number of spots in advanced-placement courses, reducing the availability of such courses. But in the long run it might also stimulate the development of additional courses to meet the new demand. The difficulty is that such empirical patterns can be generated by many different interaction processes, or even by individuals acting in isolation. In the absence
of measurement of expectations and preferences, researchers are left to infer the presence of interactions from observations of outcomes.

The mechanisms we have discussed often work in tandem. For example, high collective-efficacy neighborhoods may be more effective at securing outside resources, such as police protection or the resources to build and maintain a community center. Because institutions provide social contexts for the creation and maintenance of social ties, the presence of such institutions may affect the nature and extent of neighborhood social interactions. Small’s research on child-care centers in New York City finds, contrary to social isolation and deinstitutionalization theories, that poor neighborhoods have as many child-care centers as wealthier neighborhoods, that centers in poor neighborhoods have more organizational ties to key resources, and that centers are key sites for social interaction, the creation of social ties, and resource brokering in poor neighborhoods (Small, Jacobs, and Massengill 2008; Small 2009).

It should also be noted that mechanisms often operate through parents rather than directly on children. The neighborhood environment may affect parental employment, marital decisions, parenting practices, and psychological or physical health, and any of these can influence their children’s development (for example, see Leventhal and Brooks-Gunn 2000). Parents coping with violent environments may be more likely to use physical forms of discipline because of the heightened dangers that come with misbehavior or because of the stress of living in a violent neighborhood, or parents may take cues from their neighbors when judging the academic achievement of their children. Presumably, most of any neighborhood effect on young children would operate through parenting practices or environmental mechanisms, since children’s direct exposure to neighborhood social interactions are likely to be minimal.

**EFFECT HETEROGENEITY**

These theoretical perspectives provide broad outlines of how neighborhood effects on educational outcomes might work, but none of these frameworks adequately considers how these effects might vary with differences in the daily experiences of youths. These differences, E, in our conceptual model described earlier, are potentially important but largely uninvestigated sources of effect heterogeneity (see also Small 2004).

One possible source of neighborhood-effect heterogeneity is differences among individuals in social networks. Though interaction-based neighborhood-effects theories implicitly assume that neighborhoods play some role in structuring the social networks of their residents, we actually know little about whether—or, more important, for whom—this is the case, particularly among youths. Social networks are one of the key conduits (but by no means the only one) through which information and cultural frames or scripts are transmitted. Social networks of similar-age youths—peer networks—have received considerable attention in the literature (for example, Anderson 1999). Such peer networks may play important roles as cultural conduits, as most theories of peer effects assume, but our theories need to be more specific about who those peers are, which peer attachments are more common among young people in poor neighborhoods, and what is transmitted through peer networks. David Harding (2009b, 2010) argues that older adolescents and young adults on the street in poor, violent neighborhoods have considerable cultural power and play an important role in socializing younger adolescents by exposing them to local cultural frames and scripts regarding schooling and sexual behavior.

A second source of effect heterogeneity is different behavioral adaptations to the challenges of daily life in poor neighborhoods. A focus on behavioral adaptations explicitly considers the individual as an actor who can adapt in different ways to mitigate or overcome challenges faced in different neighborhoods. The distinction developed by Sharkey (2006) between “imposed” envi-
environments (everything present in the neighborhood where an individual lives) and "selected" environments (the people and institutions with whom he or she interacts) highlights the idea that youths living in the same neighborhood may choose very different social environments for themselves. Different choices or adaptations can have different consequences. For example, violent neighborhoods provide particular challenges to adolescents. In order to feel safe, some adolescents may spend as little time as possible in public spaces, thus limiting their exposure to their neighborhood's violence. Others, however, may engage in behaviors such as demonstrating their toughness, forming strong bonds of mutual protection with friends, or relying on older individuals for protection in order to avoid victimization (Anderson 1999; Harding 2009b, 2010). For the latter group, these behaviors may have unforeseen educational consequences because such adolescents can be viewed as resistant or disruptive by teachers (Dance 2002). Another example is provided by Carter (2005), who argues that "cultural authenticity" among ethno-racial minority groups, in the form of speech styles, clothing, music, and other tastes, can have positive payoffs in terms of group membership and solidarity (what Carter calls "nondominant cultural capital") but also can be misinterpreted by white middle-class teachers as oppositional or resistant. Parents may also adapt their parenting practices to the neighborhood environment by, for example, limiting their children's interactions with neighbors (Fürstenberg et al. 1999; Jarrett 1997a, 1997b).

A third potential source of neighborhood-effect heterogeneity is variation in family characteristics and the interaction between family characteristics and the properties of social settings. Here, effect heterogeneity is driven less by differences in social interactions and more by differences between individuals and families in their capacity to access resources and to insulate their children from negative aspects of their neighborhood and, as a result, their susceptibility to neighborhood effects (or Ψ in our conceptual model). (Our online appendix provides a detailed example of family-based effect heterogeneity, available at: http://www.russellsage.org/duncan_murnane_online_appendix.pdf.) Finally, differences in cities' economic, social, and geographic characteristics may also be important sources of heterogeneity in neighborhood effects (Small 2007).

**WHO, WHEN, WHERE, AND WHAT: THE NEED FOR NEW DATA AND METHODS**

In order to incorporate the mechanisms and effect heterogeneity just described into neighborhood-effects research, measures of exposure and vulnerability are required. With the easy availability of census data, early sociological research on neighborhood effects (for example, Brewster 1994a, 1994b; South and Crowder 1999; South and Baumer 2000) often relied on compositional measures of neighborhood characteristics as indicators of emergent properties (for example, the percentage of single-mother families as a measure of cultural norms regarding nonmarital childbearing). Economists drew liberally from these sociological roots, continuing the tradition of using compositional measures. These various compositional measures (for example, poverty rate, unemployment rate, rates of welfare receipt) tend to be fairly strongly correlated with one another.

Using compositional measures of neighborhood characteristics as proxies for emergent cultural characteristics assumes a tight connection between culture and behavior, exposure, networks, and interactions, when (1) this assumption is probably incorrect and (2) the connection is something we should be investigating. For example, if we observed high rates of high school dropout in poor neighborhoods, would we assume that neighborhood norms and culture did not place a high value on education? Research suggests that the poor, particularly African American poor, actually place a very high value on education (Solorzano 1992; Goldenberg et al. 2001; Carter 2005; Young 2004; Newman 1999). More recent research has focused on developing
noncompositional measures using “ecometric” methods (Sampson Raudenbush, and Earls 1997; Raudenbush and Sampson 1999). Ecometric methods are an important advance, allowing us to measure the social and cultural characteristics of neighborhoods.

Measuring Emergent Properties

Because our theories are often about emergent properties of neighborhoods rather than neighborhood composition, we must measure emergent properties. The development of constructs and methods of data collection should be guided by relatively detailed questions such as the following:

What are some of the dimensions that determine social-interaction exposures?

Where is a youth spending his or her time: inside the home, in the neighborhood, at school, or outside the neighborhood? When is the youth spending time in this environment?

How long is the youth there, and where could he or she otherwise spend time? Is the youth studying, watching TV, playing sports, participating in an organized activity, or hanging out with family or friends?

Whom is the youth interacting with or observing: family, friends, or unrelated adults?

What are the characteristics, experiences, attitudes, and behaviors of the people with whom the youth is interacting?

What is the youth’s relationship with or attitude toward these individuals (close or trusted friend, casual acquaintance, authority figure, negatively perceived)?

Qualitative, Time-Use, and Social-Network Methods

Developing methods for measurement is as important as conceptualizing what to measure. Qualitative methods are especially well suited to understanding social processes and day-to-day behaviors, particularly when the key dimensions the analyst might consider are not clear at the outset. By interacting with individuals in their natural social contexts or talking to them at length about their experiences and perceptions of those contexts, the ethnographer or interviewer can understand in detail how neighborhoods structure the who, when, where, and what of daily life and the content of the messages or ideas that youths encounter in these contexts.

We see three roles for qualitative methods in research on contextual effects. First, ethnographic participant observation or in-depth, unstructured interviews can be used in the pilot or exploratory phase of a project to generate hypotheses, inform the development of survey measures, or understand the boundaries of a social context in question. Second, qualitative methods can be embedded in a mixed-method study in order to understand the mechanisms by which quantitatively measured effects are operating and to inform the interpretation of estimates from statistical models. One recent example of the utility of qualitative research is the mixed-methods work in the MTO evaluation. Quantitative data showed that MTO improved the outcomes of female youths, particularly their mental health, but had unfavorable effects on male youths (Kling, Liebman, and Katz 2007). The qualitative research revealed that boys in the experimental group were more socially isolated than girls in their new low-poverty neighborhoods, that boys in the control group experienced greater contact with father figures, and that boys generally experienced more negative peer effects. These mechanisms would have been virtually impossible to uncover with the quantitative data alone, but because qualitative data collection
occurred after the quantitative work, qualitative researchers had the opportunity to explore through open-ended interviews why MTO had disparate effects.²

Third, stand-alone qualitative studies can illuminate social organization and daily life in poor communities and inform theorizing about how contextual effects operate, develop evidence for or against hypotheses on the basis of prior research or theorizing, or complicate previous theoretical accounts. For example, Small (2004) shows that neighborhood poverty does not always lead to social disorganization, and he explores the conditions under which poor communities can develop and deploy social capital. A long history of ethnographic research in urban sociology has developed the “stylized facts” that now inform much of the current neighborhood-effects research (for example, Anderson 1999; Hanmerz 1969; Suttles 1968; Whyte 1943; Young 2004).

Time diaries and social-network analysis methods are additional tools for gathering data that can detail how, where, and with whom time is spent. These methods hold considerable potential for measuring an individual’s involvement in or exposure to neighborhood social processes (see, for example, Fu 2005, 2007, on network data). Traditional time-use measurement studies (Juster and Stafford 1985; Robinson 1977; Csikszentmihalyi and Larson 1987; Larson 1989; American Time Use Survey [Bureau of Labor Statistics 2009]) ask individuals to report on what they are doing either retrospectively, using a time diary, or at the moment, using experience sampling methods. Often data are also gathered on secondary activities (for example, monitoring children, watching television), others present, and where the individual is (home, school, work, store, park, and so on), yet when locations are requested, they are of generic form (for example, grocery store, friend’s house) and do not include geographic information that would allow researchers to measure distances or locations. Time-use and social-network data collection can be expensive and therefore must be weighed against other data-collection needs in any particular study. However, measures of social interaction in and outside of the neighborhood are critical to measuring neighborhood exposure, and data-collection efforts can be efficiently tailored to these purposes.

Recent work in criminology offers a promising example of how time-diary methods can measure where, how, and with whom adolescents spend their time. Motivated by both a concern with neighborhoods and crime and the routine-activity theory of crime, which posits that much crime is based on spur-of-the-moment calculations related to opportunity, potential victims or targets, and likelihood of apprehension (Cohen and Felson 1979), the criminologist Per-Olof Wikström and David A. Butterworth (2006) collected time-use data on a subsample of adolescents in the Peterborough Youth Study (United Kingdom) using “space-time budgets.” They asked respondents to report their primary activity for each hour during the previous seven days. For each activity, respondents also reported where they were (type of location and geographic location), who else was present (number of friends, other peers, family, non-family adults, teachers, and so on), whether they had consumed alcohol or drugs, whether they were engaging in crime, whether they were carrying a weapon, and whether the situation involved elements that might increase the risk of offending or victimization, such as threats, arguments, or harassment (Wikström and Butterworth 2006; Wikström et al. 2010). Even this relatively simple form of data collection led to new descriptive information on adolescent criminal involvement and its relation to neighborhood context. For example, even the most frequent offenders spend very little time during the week offending; offenses most often occurred with peers and in risky situations, and youths in disadvantaged neighborhoods were exposed to more risky situations. This example suggests that neighborhood-effects researchers might profitably adopt and extend this form of data collection in order to examine time-use and social interactions. For instance, by gathering data on the characteristics of others with whom a youth spends time, researchers can gauge exposure to local socialization.
Measuring Institutional Resources

Place can influence whom individuals are interacting with and how much time they spend there (that is, how desirable or appealing it is to engage in interactions), and it can serve as a setting for the transfer of information and resources. Traditional methods to measure availability of institutions rely on geographic mapping to measure distance to supermarkets, or more intensive neighborhood-observation checklists that systematically ask observers to rate neighborhoods on items such as presence of public playgrounds and the condition of such playgrounds (graffiti, trash, barbed wire, and so on). Such methods have grown in sophistication as technologies such as Google Earth have vastly lowered the cost of assembling this type of information. As a result, researchers have used these methods to answer a variety of public-health questions about the role of social and physical environments in health outcomes among individuals living in disadvantaged communities (for example, see Ponce et al. 2005; Zenk et al. 2005).

Although mapping or collecting rater observations (also called “Systematic Social Observation”) is a good strategy for documenting the number and proximity of these types of neighborhood institutions, these methods, as Sampson, Jeffrey D. Morenoff, and Thomas Gannon-Rowley (2002) argue, do not capture the quality and diversity of available institutions—such as gathering places, trusted pharmacies, or safe parks—or the ubiquity and acceptance of their use by residents. For example, recently those working in public health who have examined supermarket availability in impoverished neighborhoods have concluded that travel time may be a better indicator of accessibility than physical distance (Zenk et al. 2005). A fuller understanding of institutional resources can complement how well researchers understand where and how time is spent and how to characterize place. Retrieving this type of data may require a hybrid model that combines low-cost methods (mapping and counting) with more resource-intensive but tailored methods, such as neighborhood-observation checklists along with individual qualitative assessments of institutions (via a subsample of survey respondents or respondents to more in-depth open-ended interviews).

ESTIMATION STRATEGIES: EFFECTS OF CAUSES, EFFECT HETEROGENEITY, AND MECHANISMS

Selection-bias problems present themselves when families and individuals have some control over where they live, with whom they interact, and where they spend their time. Individuals make decisions about the social settings they occupy on the basis of a variety of factors, from preferences to personal resources and other constraints; but because researchers cannot always observe or measure these factors, selection bias may result. This means that individual or family characteristics may confound the estimates of social setting on youth outcomes because the associations we observe between a social setting such as neighborhood context and educational outcomes may be due to unobserved differences in individual or family characteristics in different neighborhoods and not to the effects of residing in different neighborhoods. The magnitude of the bias will depend on two quantities: the association between the confounder and the social setting, and the association between the confounder and the outcome.

A useful way of conceptualizing this identification problem is to consider the sources of variation in social settings experienced by different youths. Take, for example, neighborhood context. One type of variation, endogenous variation, refers to the confounders that produce the selection bias just discussed. The second type of variation is exogenous variation in neighborhood context, variation that is produced by economic or social processes that do not directly affect individual outcomes. For example, a change in public policy may move some families out
of public housing into neighborhoods with lower poverty rates without directly affecting outcomes of interest.

Solving the selection-bias problem requires finding and measuring those exogenous sources of variation. In searching for such exogenous variation in neighborhood contexts, researchers have focused on two types of processes that lead to variation in neighborhood characteristics experienced by different individuals: (1) residential mobility, the movement of families from one neighborhood to another, and (2) changes in neighborhood conditions over time—place-based change. Since both sources of variation can be endogenous or exogenous, both present identification challenges. Regarding moving, we need to understand why some families move to or stay in disadvantaged neighborhoods while others do not. We must also consider the potential negative effects of residential mobility itself, which could dilute the positive effects of an improved neighborhood context (Sampson 2008). Regarding changes in neighborhood conditions, we need to understand why some neighborhoods change and some do not, and we may also worry about why some families move in response to changes in their neighborhoods and others do not. If these reasons are also causally related to the outcomes, selection bias is introduced. Finally, note that these two types of interventions typically will not estimate the same quantity. Residential mobility manipulates an entire set of linked neighborhood characteristics, while a place-based intervention typically manipulates a single key feature (or small number of key features) of the neighborhood context. A place-based intervention will therefore more often provide a narrow test of a specific neighborhood-effects mechanism. For this reason, and because we have already learned much from residential mobility studies such as MTO and Gautreaux, we support the development of place-based interventions for studying neighborhood effects. Key challenges in developing such interventions include designing interventions that are strong enough to produce detectable effects and specific enough to reveal the importance of a single mechanism.

**Identifying the Effects of Specific Mechanisms**

Focusing on mechanisms introduces an additional set of complications to research designs that rely on harnessing exogenous variation in social settings. Though it is not cast precisely in terms of mechanisms, it is helpful to start with Charles F. Manski’s (1993, 1995) discussion of identification problems in neighborhood-effects research, for his framework is well known and the fundamental problems of interpreting associations between contextual characteristics and individual outcomes are especially relevant in identifying mechanisms.

Manski describes three sources of association between neighborhood characteristics and individual outcomes. First, “endogenous effects” are the effects of group-level values on individual values on the same variable such as time spent studying. Contagion or peer-effects theories propose that individuals are more likely to do what others around them are doing. A child will spend more time studying when he sees his peers spending more time studying. Endogenous effects generate a social multiplier because they amplify any direct effect of an intervention. These types of effects are particularly challenging to identify because of direction of causality, or what Manski calls the “reflection problem.” Is directionality from the group to the child or vice versa?

Second, in “contextual effects,” individual behavior varies with other characteristics of the group, such as when achievement varies with neighborhood socioeconomic composition. A child may spend more time studying when he observes adults in the neighborhood who have benefited from high levels of education. Third are “correlated effects,” which is simply another name for selection bias.

If we want to identify the total effect of exposure to one neighborhood rather than another, it is not necessary to distinguish between endogenous and contextual effects with respect to the
mechanisms that they specify. The main concern is selection bias at the neighborhood level, as previously discussed. (Note, however, that even if we were able to deal with the selection-bias problem and obtain a causal estimate of the effect of, say, neighborhood poverty on an educational outcome, this would not distinguish between the effects of neighborhood poverty and other neighborhood characteristics correlated with it.)

The reflection problem emerges whenever we are interested in endogenous effects of social settings, but it is not the same identification problem as the selection-bias problem that researchers face when trying to estimate total effects. The reflection problem can be understood as a failure to specify, measure, and manipulate (or find an instrument for) one particular mechanism, the average value of the outcome among a group with whom one interacts. The only other option is to draw on theory to invoke strong identifying assumptions that specify the direction of causality. For instance, perhaps older friends' actions affect younger friends' actions, but not the reverse. The key point is that the reflection problem is not an inherent intractable problem in the estimation of neighborhood effects but rather results from failure to conceptualize mechanisms and develop strategies for identifying their effects.

We now return to the selection-bias problem but consider it in the context of identifying the role of mechanisms. Even if there is a source of random variation in neighborhood context, the selection-bias problem reemerges when mechanism variables are considered, as self-selection into the mechanisms may no longer be random with respect to the outcomes. Identifying the effects of mechanisms on an outcome will require multiple sources of exogenous variation. Consider figure 13.1, which diagrams a simplified research design in which there are three hypothesized mechanisms (M1, M2, M3) for the effect of N (a neighborhood characteristic) on Y (the outcome). For example, N might be the amount of violence in the neighborhood, and Y might be educational achievement. The three mechanisms might be (M1) leveling of educational expectations through a focus on safety, (M2) exposure to violence affecting cognitive development through post-traumatic stress disorder, and (M3) joining a gang for protection, which leads to less time for studying. U represents a set of unobserved X variables that are uncorrelated with Z (it was randomly assigned) but are correlated with neighborhood violence (N), the outcome (Y), and the mechanisms (M's).

If we are interested in the total effect of neighborhood violence (N) on educational achievement (Y), we can use instrumental variables to estimate the effect. This approach involves find-

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**FIGURE 13.1 Identifying the Effects of Multiple Mechanisms**

![Diagram showing the effects of multiple mechanisms on an outcome](image)

*Source: Authors' figure.*
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...ing another variable (Z) that is correlated with neighborhood violence (N) and uncorrelated with the unobserved variables (U). Z is the source of exogenous variation in our neighborhood characteristic (N). For example, the city might randomly assign an antiviolence program to some neighborhoods and not others. Referred to as an instrument, Z is used to purge N of the portion of its variation that is correlated with the unobservables. The exogenous portion of N's variation—that is, the uncorrelated portion that remains—is then used to estimate the effect of N on Y. Intuitively, this means that our analysis is using only the variation in neighborhood violence created by the antiviolence program to identify the effects of violence. Because the antiviolence program is randomly assigned, the communities that did not get the antiviolence program are similar in observed and unobserved ways to the communities that did. We can use a similar procedure to identify the effect of the neighborhood characteristic (N) on each of the mechanisms. For example, b1 can be estimated as the association between the antiviolence program (Z) and educational expectations (M1) divided by the effect of the program on neighborhood violence (a).

The difficulty is that as long as each of the mechanisms (M's) is associated with the outcome Y through the unobserved variables (U), we have no way to identify the effects of these mechanisms on educational achievement without more instruments, that is, without sources of exogenous variation in the mechanisms. Moreover, if the mechanisms are causally related to one another, then even if we could measure all the U's, we would also have to assume that we have measured all the mechanisms as well, lest our estimate of the effect of any one mechanism on Y be biased by its association with an unobserved mechanism. If we thought that violence also directly reduced school attendance and that educational expectations are correlated with school attendance, but we did not observe school attendance, we might attribute some of the effects that operate solely through school attendance to educational expectations. One way to create the additional exogenous variation necessary to identify the effects of specific mechanisms (c's) is to design an experiment with multiple treatment arms that produce exogenous random variation in both N and the M's across the treatment arms.

Another general approach in an experimental setting is to examine patterns of effects on mediating mechanisms by subgroups. This can be useful when the experiment is not designed with multiple treatment arms for the various mechanisms and can be done by dividing the sample into subgroups for which theory and previous evidence suggest that treatment effects of N on Y may differ. A review of the overall pattern of results can provide some evidence to support a theoretical framework by testing, in part, the hypothesized relationships between mechanisms and outcomes across treatment categories and subgroups. The advantage of this type of experimental approach is that the source of variation in the social setting (for example, from something like randomly assigned rent subsidies that affect residential location) is clear, so we can be confident that changes in the mechanisms are causally related to residential location. However, without an exogenous source of variation in the mechanisms, the mechanisms may be spuriously correlated with the outcomes through unobservables.

Focusing on mechanisms can also strengthen our confidence in causal claims. Consider again figure 13.1 in which there are three mechanisms (M's) and some set of unobserved confounders (U). If any one of the observed mechanisms is uncorrelated with U, then we can identify the portion of the effect of the treatment (N) on the outcome (Y) that operates through that mechanism. For example, if we can assume that the "joining gangs" mechanism (M3) is not affected by unobservables (U), then the arrow between U and M3 disappears from the diagram. When M3 is not affected by U and when we can observe the other mechanisms, both the effect (b3) of the treatment on joining gangs and the effect (c3) of joining gangs on achievement can now be identified even if there is no instrument. The effect of the neighborhood violence (N) on achievement (Y) that operates through joining gangs (M3) can be calculated as the product of b3 and c3. Although this does not provide us with an unbiased estimate of the total effect of N on Y, if b3 x c3 is nonzero, it does provide evidence that there is some nonzero effect of N on Y.6
Complications Introduced by Effect Heterogeneity

In the previous example, the subgroups are observed and effect heterogeneity is harnessed to provide further information. The possibility of heterogeneity in effects also introduces complications, particularly when the subgroups cannot be identified in the data. The first complication has already been discussed in the methodological literature on instrumental-variables estimation—that is, unless the treatment effect can be assumed to be the same for all units of analysis, an instrument identifies the local average treatment effect (LATE) (Angrist and Krueger 2001; Angrist, Imbens, and Rubin 1996; and see Gennetian et al. 2005, for a less technical exposition). It answers the question: What is the average effect of the treatment among those who were actually induced to take the treatment by the instrument? The conventional solution to the LATE problem is to use multiple instruments (see applications by Gennetian, Magnuson, and Morris 2008; Kling, Liebman, and Katz 2007). If different LATE estimates from different instruments operate in the same direction and are of similar magnitude, then we can be more confident that the effect of the treatment is not specific to particular subgroups.

A second complication arises because estimating the role of a particular mechanism in the effect of a neighborhood characteristic \(N\) on an educational outcome \(Y\) requires identifying both the effect of \(N\) on the mechanism and the effect of the mechanism on \(Y\). When there is treatment-effect heterogeneity, it is possible to produce an estimate of the role of the mechanism that applies to none of the cases if some cases experience effects of \(N\) on the mechanism and other cases experience an effect of the mechanism on \(Y\) (see also Bullock and Ha 2010). In technical terms, this means that the LATE for the effect of \(N\) has a different group of compliers than the LATE for the effect of \(M\).

This problem highlights the importance of specifying and measuring sources of effect heterogeneity. It is particularly important to choose exogenous family or child characteristics, lest subgroup membership be a product of the treatment. Ideally, one might also purposely stratify one's sample to ensure a sufficient sample size in each subgroup. Our discussion thus far suggests a number of other family or child characteristics on which one might focus. Our emphasis on family capacity or family resources suggests that family income, family structure, number of children, or access to transportation may determine the strength of a neighborhood effect. Our emphasis on social ties and interactions in the neighborhood suggests that proximity to extended kin, parents' social ties to the neighborhood, or parental employment outside the neighborhood may determine the strength of the neighborhood effect. Many of these characteristics are both determinants and effects of the treatment. Further ethnographic work may be required to better understand these and other sources of effect heterogeneity. We emphasize as well that an exhaustive search for effect heterogeneity may lead to multiple testing problems through the mining of sample data for statistical significance. Sources of effect heterogeneity should be closely informed by theory and prior research, and any analyses should be preceded by a clear set of hypotheses limiting the number of subgroups to be examined.

AN ILLUSTRATIVE EXAMPLE

Because violence is spatially concentrated in poor neighborhoods, it may be an important mechanism linking neighborhood poverty to school outcomes. In this section we sketch an example study that examines whether the observed correlation between neighborhood violence and unfavorable schooling outcomes (for example, Harding 2009a) is causal by manipulating exposure to neighborhood violence through youths' time use. Suppose we observe that youths living in more violent neighborhoods have lower school attendance and graduation rates, high rates of suspensions, and poorer math achievement. One can try to test the neighborhood-violence hypothesis by esti-
mating the relationship between an individual’s time spent on the streets in a violent neighborhood and educational achievement using a regression model, but this estimate will be susceptible to within-neighborhood selection bias (different youths choosing to spend their time in different places, in different ways, and with different people). An alternative is to identify this effect by generating exogenous variation in youths’ time use through random assignment.

We can manipulate the amount of time that youths spend in areas of higher neighborhood violence under the hypothesis that such time use affects exposure to violence. Randomization ensures that individuals assigned to different locations are the same in both observed and unobserved characteristics, except for chance variation, so any differences in outcomes across groups can be attributed to the planned variation in exposure to violence.

Details of this example study can be found in the online appendix. Briefly, the design is the following: In the first stage, we select a target set of neighborhoods with high neighborhood violence and match each of them to neighborhoods that are comparable in terms of poverty, race, and educational levels but have lower neighborhood violence (see Seith et al. 2003 on the feasibility of matched-neighborhood designs). An intervention would be designed that is nonacademic but involves engagement with the neighborhood, such as working on a local clothing drive. For the study we would recruit individuals from the neighborhoods with higher levels of neighborhood violence, so that the neighborhoods where subjects are placed through the intervention are all less risky than their own neighborhoods. We would randomly assign these youths to teams in different locations outside of their own neighborhoods, some of which would have lower neighborhood violence than others.

With this type of random-assignment study design, we can learn about the effect of exposure to neighborhood violence by comparing the educational outcomes of youths in the different intervention groups. Although we do not directly manipulate exposure to violence, we can estimate the intervention’s impact on educational outcomes and the intervention’s impact on exposure to violence. For example, we hypothesize that youths working in less violent neighborhoods would experience more beneficial educational effects than those working in more violent neighborhoods. The benefits of the interventions may vary at the individual level by pre-intervention exposure and vulnerability to neighborhood violence, as determined by family connections to resources outside of the neighborhood, parenting skills, parental motivation and capacity to seek positive alternative environments for children, and the youth’s own social relationships and decisions about how to use his or her time.

Measures of youths’ time use, neighborhood characteristics and exposure to violence, and a range of information about families and their youths can be collected through surveys. With these data we can also check for any other important differences in youth experiences that arise during the study that may be correlated with exposure to violence. Survey data can be complemented by in-depth qualitative interviews with a small subsample of youths in both the experimental and control groups to better understand aspects of youth time use and exposure to violence that are difficult to detect or measure through a survey.

CONCLUSION

Our aim in this chapter has been to set the stage for future research—its opportunities as well as challenges, both conceptually and empirically—whose purpose is to better understand the influence of neighborhood social settings on youth educational outcomes. Conceptually, we argue that the field is ready to move away from estimating the effects of compositional properties of neighborhoods and toward an analysis of specific mechanisms and effect heterogeneity. A new focus on mechanisms, exposure, and vulnerability can only be achieved with more nuanced data. Our rec-
ommendation is to build on the successes of prior efforts and invest in new, mixed-methods data-collection strategies that can measure individual interactions, networks, and time use, and provide reliable assessments of neighborhood resources, cultural contexts, and physical conditions. We see particular promise in adapting time-use and social-network data-collection methods to assess exposure to the social and cultural processes that produce contextual effects. Finally, we point to both across-neighborhood and within-neighborhood selection biases as long-standing challenges in identifying neighborhood effects, and we illustrate some promising approaches for designing studies to identify mechanisms and assess effect heterogeneity. Although we tailor our discussion to social settings outside of the school and home, much of what we discuss might also be applied to school settings or home environments.

Neither neighborhoods nor individual residential decisions are static. Our proposed framework recognizes this to some extent by highlighting the importance of effect heterogeneity. Differences in cumulative exposure to advantaged or disadvantaged neighborhoods as well as differences in exposure depending on children's developmental stage may also be important sources of effect heterogeneity. We have not examined other forms of neighborhood dynamics. Neighborhoods change over time as residents move in and out, and community investment increases or decreases. When families move in response to changes in the neighborhoods around them or in response to the outcomes experienced by their children as a result of neighborhood context, more sophisticated statistical methods, such as marginal structural models or inverse probability of treatment weighting (Robins, Hernan, and Brumback 2000), are required to estimate unbiased causal effects, even when all sources of selection bias are accurately measured (Sampson, Sharkey, and Raudenbush 2008; Sharkey and Sampson 2010; Sharkey and Elwert forthcoming).

Identifying mechanisms and the types and extent of effect heterogeneity might uncover influences that contribute to income inequality as well as disparate educational outcomes. We have discussed identification of mechanisms for neighborhood effects without regard to the absolute magnitude of these effects, to their relative magnitudes compared with the effects of schools or of families, or to their resulting policy significance. Policy trade-offs between housing vouchers that offer the opportunity to move compared with place-based neighborhood investment (such as building a community center or clinic) are best informed by quantifying the size of effects and comparing costs and benefits. Mechanisms by which neighborhoods can have their effects are particularly important in considering these types of policy trade-offs. For example, if the mix of peers in neighborhood-based after-school activities is an important predictor of youths' math achievement, policymakers can evaluate the size of this beneficial peer effect (and any spillover effects) compared with the cost of busing disadvantaged youths to programs that offer this same mix of peers, or the cost of offering an equivalent program in the schools attended by disadvantaged youths.

Heterogeneous treatment effects are equally important in considering policy trade-offs. Policy interventions that primarily benefit only some subgroups may be more or less appealing. For example, an intervention with a large effect on a small group of youths may be more or less appealing than an intervention with a small effect on a very large group of youths.

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NOTES

1. \(Y\) may therefore be a subset of \(X\).

2. In addition to the promise of MTO qualitative research for uncovering potential mechanisms, the MTO follow-up surveys collected data on items that also provide some supporting evidence for particular pathways by which MTO affected outcomes. Although the MTO study was primarily designed to answer questions about the causal effects of housing vouchers and neighborhoods and not to identify specific mechanisms, one method of glean- ing possible mediating mechanisms is to examine the pattern of MTO’s experimental effects on outcomes (such as math achievement) and mediators (such as school discipline) to see if the effects align with hypotheses.

3. Michael Sobel (2006) discusses another identification problem in mobility experiments, interference between units that we do not fully describe here. This is a particular challenge in this context because encouraging and succeeding in changing residential mobility inherently can have possible confounding effects on the families in the receiving neighborhood as well as those in the neighborhood left behind.

4. Note that one might further theorize the existence of mechanisms for these mechanisms. The level of detail in the specification of mechanisms depends on one’s substantive and policy goals. See Stephen L. Morgan and Christopher Winslup (2007, chapter 8) for a discussion of this issue.

5. For simplicity, we omit the observed \(X\)’s from the figure. The discussion in this section will assume conditioning on observed \(X\) variables.

6. This discussion is an example of identification through what Judea Pearl (2000) calls his “front door” criterion. See Winslup and Harding (2008) for an example and further details.

7. One might also consider family type to be a latent class and conduct a latent class analysis.

REFERENCES


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