

Showing that Neighborhoods Matter

Claude S. Fischer*

University of California, Berkeley

A long-standing contradiction has posed a critical puzzle to urban sociologists: On the one hand, a wealth of ethnographic studies make the point that the kinds of neighborhoods people inhabit profoundly shape their fortunes; on the other hand, statistical analyses of “neighborhood effects” have often failed to confirm that claim. From the earliest studies of the Chicago School, through classic works such as Whyte’s (1943) *Street Corner Society* and Suttles’ (1968) description of defended turfs, up to, for example, Harding’s (2010) recent interviews of young boys in Boston, the message, repeatedly, is: neighborhoods have consequences. Yet, ruling out individual-level explanations for variations among places has proven difficult; econometric studies find that experiential and behavioral differences across neighborhoods largely derive from the different kinds of people who “select in” to them. Some scholars have therefore rejected the idea that neighborhoods matter (e.g., Mayer and Jencks 1989) and even the idea’s defenders struggle to find the evidence (e.g., Sharkey 2012). The question of whether contexts such as neighborhoods matter matters to sociologists far beyond the students of the city.

Rob Sampson, in his magisterial book, *Great American City: Chicago and the Enduring Neighborhood Effect*, is fully aware of this controversy’s broadest implications for sociology (see, e.g., Ch. 15; pp. 435–6) and his book is a major contribution to that debate.¹ I will discuss how after reviewing the state of play in neighborhood effects. (This contribution to the symposium is thus more focused on basic theory and research than are those of my fellow contributors.)

Sociologists assume that contexts—workplaces, schools, organizations, nations, families, and personal networks, as well as neighborhoods—substantially shape individual experience and action. The general public also believes this. Parents, for example, try hard to access the “best” contexts they can find for their children, particularly the “best” neighborhoods, schools, and peer groups. Sociologists have increasingly focused their attention—in part, thanks to Sampson’s Chicago project itself—on whether and how neighborhoods really matter. The number of studies on neighborhood effects grew exponentially since 1990.² But urban sociologists’ and the public’s assumption that neighborhoods matter may be wrong or may be right only in small ways. Variations across places in individuals’ experiences and actions may be satisfactorily explainable by the personal tastes, resources, and habits that the individuals bring with them.

The first direct assault in sociology on the contextual assumption was, I believe, Robert Hauser’s 1970 article on whether the gender composition of schools affects students’ aspirations. He concluded that the “contextual interpretation [in general] is . . . speculative,

*Correspondence should be addressed to Claude S. Fischer, Sociology Department, University of California, Berkeley, 410 Barrows Hall, Berkeley, CA 94720–1980; fischer1@berkeley.edu.

artificial, and substantively trivial” (Hauser 1970: 645; see also Blalock 1984; Lieberman 1985: 137ff). Skepticism grew about school effects, which had already been challenged a few years earlier by the “Coleman Report” (Coleman et al. 1966), which explained away school differences by the traits children brought to class. A growing literature on social networks presumes that friends influence people’s behavior, but many studies suggest that people selecting friends based on behavior better explains why friends are similar to one another (e.g., Haynie and Osgood 2005; Lizardo 2006; versus Kreager and Haynie 2011).³

Mayer and Jencks published their review of contextual studies in 1989, addressing the question, “Growing Up in Poor Neighborhoods: How Much Does it Matter?” by answering, in effect: it’s hard to tell and probably not much (Mayer and Jencks 1989). Yet, the explosion of studies on neighborhood effects followed almost immediately. Many tried to measure effects on children’s behavior and fortunes, with largely unimpressive findings once selection effects were seriously accounted for (e.g., Duncan et al. 2001; also Chen and Brooks-Gunn 2012—esp. p. 352, even though they are sympathetic toward finding effects). Other studies range over a variety of outcome effects, including health and political behavior. Two economists’ recent review of the largely correlational studies concludes that “taken on face value, the empirical evidence . . . is not decisive” (Durlauf and Ioannides 2010; also Durlauf 2004).

Researchers have tried various techniques to rule out selection explanations for neighborhood effects without great success. Most notable are mobility experiments, particularly the Moving to Opportunity (MTO) study, focused on here by Goering. My impression is that the results have provided only modest support: MTO may have increased a sense of well-being among some movers, but not general economic or educational improvement (Kling et al. 2007; Ludwig et al. 2012). And these are the results after what seems like much cherry-picking through the dependent variables. (Denton, in this symposium, raises further concerns.) Researchers have also used techniques such as case-matching algorithms to simulate true experiments, instrumental variables, selection models, longitudinal data, or indirect inferences⁴ to establish contextual effects. Sharkey (2012), for example, recently suggested a creative approach that examines what happens when neighborhoods change around people, but his results are more suggestive than conclusive.

Even in the best of circumstances, the strongest challenge made by the individual reductionist—that some unobserved trait accounts for both an individual’s place and fate—is hard to rebut totally. So long as subjects can choose to move, cooperate, or drop out, even field experiments cannot confidently eliminate selection bias.

Despite these disappointments and difficulties, many scholars more expert than I conclude that estimates of neighborhood effects, even in observational data, are robust enough to draw some important lessons: for example, that less poverty or more affluence in a neighborhood tends to improve school performance (e.g., Turley 2003; Harding 2003; Sastry and Pebley 2010).

Even if neighborhood effects are granted to exist, a theoretical challenge arises: What *about* the context matters and *how* does it work? Several scholars, including Sampson (Sampson et al. 2002), have pointed out this concern. Here are four general kinds of processes: (1) Something about the setting exterior to the residents—for example, a neighborhood’s location, lead contamination, or external reputation—affects individual

outcomes. (2) The *aggregation* of those individual traits that determine individual outcomes itself affects the individuals. For example, the neighborhood's *average* income, *percentage* single-parent households, or *median* age matters above and beyond the effects of each person's income, household status, or age. (3) The aggregation of individual *outcomes* affects the chances that any one individual has that outcome. For example, local rates of school drop-out, criminal offending, or poverty affect a resident's chances of being a drop-out, committing a crime, or being poor above and beyond the resident's personal background. These last two aggregation processes need to be fleshed out with a mechanism. Does, for instance, neighborhood-level poverty reduce individual-level school success by creating high rates of classroom turnover? Do high rates of delinquency encourage individuals to be delinquent by imitation? (4) Then, there are group dynamics, what economists have called social interactions (Durlauf and Ioannides 2010) and what Sampson calls emergent properties: "social-interactional and institutional processes that involve the collective aspects of community" (p. 47). He focuses, in particular, on local norms, local organizations, and shared expectations—especially "collective efficacy," the *shared* feeling that neighbors trust one another and would act to protect the neighborhood (see 152ff). These emergent dynamics could account for *why* neighborhoods matter when they matter. Brief reflection makes clear that this fourfold categorization of how contextual effects could occur is incomplete and underspecified.

Into this thicket of theories, studies, and methodological quandaries comes Sampson's *Great American City*—actually, comes a decade of publications for which the book is a capstone and compendium. Sampson makes two critical interventions in the debate. One is to present a great volume of evidence, piling up finding upon finding, that lend *plausibility*, albeit not closure, to the claim that contexts matter. The other is to press us to rethink the very counterposing of contextual and selection effects.

As to the findings: Sampson reports the results of a stunningly comprehensive and creative empirical project—the Project on Human Development in Chicago Neighborhoods (PHDCN). From repeated interviews of residents and of community leaders to video recordings of street life, cataloguing of collective action events, analyses of organizations, and dropping of "lost" letters, Sampson and his colleagues have generated a treasury of data about Chicago's neighborhoods and their residents.

These data lend strong plausibility to the claim that neighborhoods *qua* neighborhoods matter. First, Sampson shows how the social features of neighborhoods persist over decades, despite the turnover of residents. Strikingly, how black or poor or relatively crime-ridden a neighborhood was at one time is pretty much how that neighborhood is years later. More striking, the institutional character of neighborhoods, such as the presence of local organizations, lasts over time. Most striking, the cultural character of neighborhoods, such as their reputations and the degree of collective efficacy residents express, persists over the years. Second, Sampson can show, through rigorous statistical analyses, the plausibility of a causal sequence that begins with (a) contextual structural circumstances, notably but not only concentrated "structural disadvantage," which (b) affects contextual cultural conditions, notably but not only collective efficacy, which then (c) affects individual responses, such as people's experiences with and perceptions of crime and disorder. In other reports from the PHDCN mentioned only briefly in this book, Sampson and other scholars have found significant effects of neighborhood poverty and of collective efficacy on other individual outcomes, such as adolescent sexual behavior

(Browning et al. 2004, 2008) and female violent offending (Zimmerman and Messner 2010).⁵

While I am persuaded, I am not sure the strong advocates of selection explanations would concede that neighborhood effects have been demonstrated. A couple of issues arise. One concerns generalizing from Chicago, which is a distinctively segregated city (a point also raised here by Goering). The connection between concentrated poverty and the percentage of residents who are black is so tight that “trying to estimate the effect of concentrated disadvantage on whites is tantamount to estimating a phantom reality” (101). The stability over time in neighborhoods’ poverty is true for predominantly black but *not* for predominantly white neighborhoods; for whites, neighborhoods may not be such lasting contexts (113). Are neighborhood effects just black neighborhood effects? Perhaps, the difficulty blacks have in choosing neighborhoods explains the power of neighborhood over selection effects in the Chicago data—although, to Sampson’s credit, he points to studies in other cities and countries (e.g., 166–168) that reinforce his central claims.

More strongly, a critic could argue that all the efforts to “hold constant” individual traits so as to identify neighborhood effects is not—and cannot be—complete. The youth who does better in a high collective-efficacy neighborhood may do so, in the end, because he or she has parents with an unmeasured trait, say, self-sacrifice or “pluck,” that both shapes the child and leads the parents to choose that neighborhood. Even findings that neighborhood effects emerge after a period of exposure may really be the consequence of developmental features of the youth, not of the place. Although the neighborhood effects literature, especially recent, statistically sophisticated analyses of *cumulative* neighborhood influences (e.g., Sampson et al. 2008; Sharkey and Elwert 2011), is increasingly persuasive, there is no limit to the imaginative possibilities for a self-selection argument.

Thus, another critical contribution Sampson makes is pressing us to rethink the terms of the neighborhood effects debate. Three points emerge here. One, discussion has too often taken for granted that the questions of interest are or ought to be reduced to the level of individuals. But sociologists are often interested in social units in their own right—say, in how high crime rates impair the reputation of neighborhoods so as to discourage economic investment and that abandonment reinforces the high crime rate. For such an investigation, do we really care whether we explain much of the variation in, say, individual attitudes about the neighborhood?⁶ Here the “ecological fallacy” is actually the ecological reality (see also Fischer 1995). Many of Sampson’s findings are appropriately about communities as the units of analysis, not just as settings for individuals.

Two, Sampson points out, along lines suggested by Lieberman (1985), that too often researchers reflexively assume that everything other than neighborhood-level attributes must be “held constant” in order to confirm a contextual effect. What must be done, instead, is to have a clear model. Often, so-called “control variables” are actually themselves the product of a contextual phenomenon. If, for example, a mother’s job prospects are constrained by her anxiety that neighborhood dangers require her to be home when her children come home from school, controlling for her occupational status as if it were a selection factor, when it is in part a consequence of the neighborhood, would be an error. Many contextual effects may be underestimated because all individual attributes are swept into the control variable list.

Three, Sampson argues and demonstrates empirically that often self-selection itself is not an alternative to a neighborhood effect, but “is itself a form of neighborhood effect”

(308 and 375; see also Fischer 1982: 256–7; Fischer 1995: 552–3). Neighborhoods attract, repel, and indeed *select*, metaphorically speaking, the people who would live there. A simple example is that community zoning for large plots and big homes screens out all but the wealthiest. Similarly, a neighborhood reputed to have a high crime rate will repel many kinds of would-be residents, but young, single men not so much. Neighborhoods deemed children-friendly will attract families and reinforce that culture. Would we say, to take a colorful example, that the strolling about of nearly-nude (sometimes fully nude), buffed-up men on San Francisco's Castro Street is fully explained by the self-selection of gay men to live in that neighborhood? Should we not appreciate that their choices to come to the Castro testify to something real about that neighborhood's culture?

Sampson's *Great American City* is a landmark work in urban sociology in part because it contributes so richly in different ways to our understanding of how neighborhoods operate in Chicago. But it is also a landmark work because it so richly in so many different ways strongly asserts that the community in community studies matters.

Notes

¹Disclosure: I am thanked in the preface for having provided some advice to Sampson that I forgot having provided him, but I am certain that it made a gnat's weight impression on the book.

²The JSTOR database contains 40 articles mentioning "neighborhood effect(s)" in the first 90 years of the twentieth century (almost all after 1970), about 100 articles in the 1990s, and over 300 articles in the 2000s.

³To be sure, peer effects can be demonstrated experimentally, as in the classic conformity studies and otherwise (e.g., Sacerdote 2001), but in natural situations they seem dwarfed by selection.

⁴I refer here to variance-based approaches discussed by Durlauf and Ioannides (2010: 459ff).

⁵Interestingly, a favorite topic in much of the earlier neighborhood effects literature, student achievement, hardly appears in the PHDCN papers. The terms school and student are not even in Sampson's book's index, although he has published other work that addresses cognitive skills.

⁶That banal observation that aggregate-to-aggregate effects pass through individual action (the Coleman "boat" discussed by Sampson, p. 63) does not undercut the point, since including variation in individual action is often of minor importance to the analysis. For example, we can just assume for most purposes that home-seekers try to avoid crime.

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