City & Community

Great American Sociology

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Let me begin with my conclusion: Rob Sampson has written a great book. It is a must-read for almost all sociologists and demographers, and especially important for urbanists. Though I have written numerous book reviews and read countless others, this essay is by far the hardest one I've been asked to do. It is hard for two reasons: first, there is so much excellent material in *Great American City* to write about that it is hard to know where to begin; and second, the book is so good it is difficult to keep from being overly effusive in praising it.

When you first pick up *Great American City*, you expect to begin reading a book about a city. But you very quickly learn that this book is much, much more than that. Early on, Sampson says that his book is " . . . about everything, or at least everything social about the city" (p. 22). And that is indeed the case. It is a review of much of urban sociology, in fact, of sociology in general, while it is also an in-depth analysis of several important and contentious contemporary issues, namely, neighborhood effects and selectivity. Theoretically driven and methodologically sophisticated, *Great American City* challenges the reader in every chapter. Put another way, many will find it impossible to understand everything he's doing in a single reading. In this, it reminds me especially of Stanley Lieberson's classic *A Piece of the Pie*. Different data, different methods, different topics in every single chapter, yet all seamlessly linked by an overarching theory of context developed in the penultimate chapter.

The book has 17 chapters, organized into five sections. Part I sets the stage: topics, location, and thesis. In Part II the reader learns Sampson's analytic approach and gets an overview of the Chicago Project. In the third part community-level processes are explained and investigated, preparing the reader for Part IV, which lays out interlocking structures as they relate to place. The final section is a synthesis and revisits the themes in the early chapters. Throughout, he rejects the specialization (read narrowness) of much social science research and especially rejects the individualism that has dominated so much sociological research in the late 20th century.

It is hard to imagine any reader of this essay who has not already read some of Rob Sampson's work. At the same time, I imagine that many, like myself, will be surprised by the scope of the research reported in this book. Given that academic specialization encourages reading along subdisciplinary lines, it is doubtful anyone would have found all these pieces unless they were reading Sampson's C.V. What Sampson calls "the Chicago Project" is really multiple parts of the Project on Human Development in Chicago Neighborhoods (PHDCN), as well as the Chicago Collective Civic Participation Project

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(CCCP), and the Key Informant Network Study (KeyNet). Equally impressive is the variety of types of data. Systematic social observations (SSO) involved photographing disorder from a van driving through the streets while observers were also coding over 32,000 segments. Then there were surveys of 4,000+ families following them wherever they moved, interviews with elites, coding of items from the *Chicago Tribune* and *Defender,* a CPR study, lost letter mailings—and I have probably missed some things. Though sometimes the data are not as useful as more standard measures (SSO was not as important a predictor of collective efficacy as perceived disorder), it is still an impressive 10 years of data collection about many issues relevant to neighborhoods and segregation. In many sections, the book is an intellectual biography, where Sampson reports key discussions with coauthors that led to a new related project or a new way to deepen a current one. The data are now available at ICPSR, so others can use them. They are a valuable resource for the field. And the fact that similar projects are being undertaken in countries on every continent will extend the influence of this work even further.

The major focus of the book is the neighborhood—defined socially and spatially. Through and through, the book offers neighborhood-centric analysis. Rejecting the idea that globalization and technology have somehow rendered our lives "placeless," Sampson instead argues that neighborhood is even more important now. In a way, he is returning to the emphases of the early Chicago School sociologists, where the individual behaviors that were studied were located in specific contexts that were also studied. The focus was how rates of behavior varied across social spaces, not how individuals behaved. At one point, Sampson even goes back to the Moynihan report and argues that the tangle of pathology was really neighborhood based, not individual. Sampson's concept of neighborhood differs from how many researchers think of it. He goes far beyond the usual measures, beyond census tract versus block group discussions. Rather, he argues that our individual reactions to neighborhood difference become a social mechanism that shapes our other perceptions and behavior both inside and outside the neighborhood, ultimately forming the city's social structure. His emphasis is on the places themselves, and their location in the larger matrices of places. It is not that he ignores individuals, but rather that he takes seriously what sociologists have long theorized, namely, that people are social and it is their relationships that make them who and what they are. Since for him, the neighborhood is the initial location of those relationships, or the place from which people start to find them, it is important to study the neighborhood itself and the location of the neighborhood in the larger context.

Sampson's argument is that the neighborhood is very important to life *as it is lived* in the United States: "I would go so far as to argue that what is truly American is not so much the individual but neighborhood inequality" (p. 356). So the importance of neighborhoods stems in part from the vast inequalities across them, something that is not found to the same extent in other advanced industrial societies. Given this "durable spatial inequality," research that assumes that individuals make decisions regardless of where they live is assuming that neighborhoods have no effect at all, that there is no spatial hierarchy observable in the urban landscape, a point with which few agree.

By taking the neighborhood so seriously, Sampson is able to move us beyond individual-centered questions and beyond surveys which ignore context or consider it as another characteristic of the individual in a multilevel model. What he wants to develop are systematic procedures for measuring neighborhood characteristics and mechanisms. He calls this endeavor "ecometrics" to link it to how psychometricians rigorously evaluate their measures, arguing we need to do this if we are to become really serious about studying neighborhood effects. In addition to census tract (or block group or block or zipcode) definitions of neighborhood, geographers have long been interested in "cognitive maps" of places and planners in "activity spaces." New and ever advancing GIS and computing techniques are going to make measurement of these easier and easier, even in settings where immediate internet access is not available. But Sampson argues that "Without comparable standards to evaluate ecological assessments, the search for individual and ecological explanations is likely to overemphasize the individual component" (p. 155). The development and testing of these standards is enough to fill at least a decade's worth of Ph.D. dissertations.

Another aspect of *Great American City* that struck me as particularly important was the emphasis on the entire distribution of neighborhoods, not just the extremes. Although segregation indices also include the entire range of neighborhoods, *American Apartheid* as well as most journal articles focus on the negative effects of segregation for blacks and Hispanics. The limits of this focus are nicely illustrated in Sampson's discussion of the Moving to Opportunity (MTO) project. While his point by point deconstruction of that project is too detailed to be summarized here (though anyone interested should read Chapter 11), one key point that emerges is that the clustering of participants at their origin made them representative of a very small slice of the population. When he tries to find in his data people who would have been eligible to participate in MTO, he is left with only 139 out of 4,600 families. This point alone should give us pause about interpreting the MTO results as decisive in explaining neighborhood effects. Yes, the random design may make it an improvement over other studies, as Sampson himself says. But by being limited to such a small group of the overall population, it is hard to argue that the results tell us anything about neighborhood effects in general. If living in a good neighborhood is important for 95 percent of the population, but does not have the same benefits for 5 percent, the question becomes why doesn't it work for the latter group. But since MTO only included the latter group, there is no way it can answer the larger question. When Sampson focuses on neighborhood attainment among MTO participants, the results are clearly negative: the treatment group did not wind up in better neighborhoods than the control group, and both moved among a small number of communities with poverty rates higher than most Americans will ever experience.

Sampson also thinks that the literature on "community" has missed some essential points by focusing on individual ties to neighbors and on "urban villages." He shows that in-depth knowledge of one's neighbors is not necessary to a good neighborhood. Nor is it the density of ties that is important in contemporary urban life as, he reminds the reader, gangs often have dense ties. All that it takes for a good neighborhood is for people to share similar expectations about public behavior and trust. Contrary to Putnam, individual memberships in organizations are not as important as the mere existence of the organizations and community events. Using data from the CCCP Sampson shows that collective action events are not correlated with individual behaviors such as voting, but are correlated with the extent of community organizations and services. A surprising finding here is that, given the contemporary concern about religion, if churches are the only institutions in a neighborhood, that place has fewer collective action events than a neighborhood which has other institutions there as well, creating a sort of organizational synergy. The very sociological concept of capacity for collective action matters for neighborhoods. Ties to other individuals may be important for many reasons, but they are not essential to good neighborhoods.

Great American City also demonstrates that, just as people are embedded in relationships to other people, the neighbors of a neighborhood matter as well. Sampson demonstrates associations across neighborhoods in perceptions of disorder, residential stability, crime, collective efficacy—all things that he has previously analyzed as important to individual neighborhoods. These associations demonstrate how "... neighborhood effects are simultaneously local and interlocking across the city" (p. 248). Reflecting the segregation still present in Chicago, white neighborhoods with low collective efficacy are most often (60 percent of the time) located near neighborhoods with high collective efficacy, but Latino and black neighborhoods with low collective efficacy have high collective efficacy neighborhood neighbors only 25 and 20 percent of the time, respectively. This suggests another way that white neighborhoods are advantaged: even if they themselves are not high in collective efficacy, their neighboring neighborhoods are, so they can still benefit, even though they themselves are not contributing to the overall collective good. They could be thought of as neighborhood level free-riders. Latino and black neighborhoods do not have the high collective efficacy neighbors to lend support.

The discussion about MTO in particular, though also much of the book, demonstrates for me one of the fallacies of much research on the poor, namely, that all too often the research implies that the poor should do far more than we, their wealthier studiers, do in our own communities. Their communities would no doubt be better if their residents spent more time interacting in neighborhood organizations, participating in neighborhood watch groups, getting involved with their children's schools, helping with homework, reading to their children. But what about time? These points ignore that many of us have the luxury of purchasing conveniences, like food deliveries, household help, and home appliances—all of which give us time to do other things. Many of the poor do not have these advantages as they schlep children on buses to daycare, after which they board another bus to get to their own place of work, and afterwards drag laundry to the laundromat, cook dinner, and clean the house. Rising income segregation combined with racial segregation means that the world of the researcher is dramatically different from that of those being researched.

A key, if not the key, element in *Great American City* is how Sampson treats selection bias. The usual, narrow definition is that selection is a statistical problem and researchers should do all they can to control it away. But Sampson takes almost the opposite approach: instead of being a statistical problem, "... selection is not a 'bias' but rather part and parcel of a dynamic social process—another form of neighborhood effect" (p. 29). Since "... selection bias is itself a form of neighborhood effect" (p. 308), Chapter 12 argues that researchers should study the process of selection in its own right. That chapter carefully explores, over three points in time, how the trajectories of movers differ from stayers in terms of neighborhood conditions, how the fact of moving itself affects individuals regardless of the neighborhood they wind up in, and how mobility is conditioned by social ties and networks. The fact that there are no significant pathways to poor neighborhoods outside Chicago or to black and Latino neighborhoods inside the city "... constitutes a hierarchy of racial and economic residential exchange that reproduces neighborhood stratification" (p. 306). During the 7 years between 1995 and 2002, Chicago neighborhoods do change for the better, and they change more for movers than stayers. But the racial hierarchy remains: whites gain most, blacks least, and Latinos are in the middle—with the difference for the movers favoring whites over blacks even more as time goes by. At the same time, moving outside Chicago benefits blacks the most in terms of living with fewer blacks, but very few blacks moved outside Chicago, the same as Massey and Denton reported in 1970–1980. Moving inside Chicago didn't change the racial composition of their neighborhoods much. While moves between neighborhoods construct social ties between neighborhoods, these ties are separated by race. To me, the differences between the maps on page 312 and 313 are astounding, and coincide with residential segregation. And the sparseness of the white moves is very telling—they are both not present and/or are leaving the city. "Moving in separate social worlds" is what Sampson calls it, and the figures graphically show it. These social processes in turn affect children, but in their case, it can hardly be described as a "choice." Seeing how the moves connect the places and reproduce the structure of inequality on the ground is hard to explain with individual models or thinking in terms of free markets and rational choice.

Sampson further explores the implications of the fact that whites and blacks live in different neighborhoods. At one point he says, "Trying to estimate the effect of concentrated disadvantage on whites is thus tantamount to estimating a phantom reality" (p. 101). The knot of inequality is far greater in the black community than the white community, including aspects like incarceration, which only touch certain parts of the city and not others. A stunning graph on page 110 plots black and white incarceration at the community level in 1990–1995 against 2000–2005. Not only is there absolutely no overlap between black and white communities in terms of incarceration, there is a large empty spot in the middle of the graph, between the two sets of communities. Incarceration only affects some communities, separating their social worlds. In another section Sampson looks at where individuals move. Despite the social separation between communities, the pattern of movement by criminals matches that of noncriminals. Their movement is not visible in the overall migration data, leading to the conclusion that "sorting among neighborhoods by individual risk factors is overwhelmed by the more visible and tangible features of neighborhood stratification that defines the American experience" (p. 326).

An annoyance in an otherwise excellent book is the size of many of the maps and figures. Many of them are simply too small to see properly in the detail that the text requires. The shading is not always well defined. Many should have been on a whole page. Though this is not something the author controlled, at this point, it would be advantageous if they were all available on the web where the reader could zoom into particular parts of them.

As I said at the beginning, I think this is a book that everyone should read. At the same time, it is easy to see how some will disagree with the findings. For example, at the end of Chapter 13 about selection effects, Sampson argues, "neighborhoods choose people rather than the common idea that people choose neighborhoods" (p. 327). This is an unfortunate turn of phrase as it implies that neighborhoods are actors, something that American individualism leads many to reject. At the same time, the evidence in the book is very clear that neighborhoods and their characteristics are durable, and what is most durable is the level of inequality across neighborhoods. So even if people are doing the choosing, their choices are limited by the types of neighborhoods out there, as well as by their own socioeconomic characteristics. This factor is often overlooked or underappreciated in studies involving neighborhoods. As an example, think about a very highly segregated city with only two groups, blacks and whites, in which blacks are moving to a new neighborhood. While the forces of discrimination that created that situation may still be operative and influence their move, the fact remains that the high segregation guarantees that there are more all-white neighborhoods in that city, so the chances that the black family moves to an all-white neighborhood may be higher there than in a less segregated city, where the number of all-white neighborhoods available to be moved into—what demographers call the population at risk—would by definition be smaller because segregation is lower.

I must also be honest and say that the "durable spatial logic" that is demonstrated in *Great American City* is sobering. Though Sampson is not pessimistic, it is hard to avoid the feeling that, given the continuous disadvantage Sampson so effectively documents, there is little hope for these neighborhoods. The idea that if we want to test for the important effect of neighborhoods, then we need to randomly assign neighborhoods, not individuals, to interventions is intriguing. But the political will to intervene in these places at the macrolevel, and in multiple domains simultaneously, on a scale large enough to make a difference is lacking. I am cynical. People don't want to believe in the importance of neighborhoods because, when everything is based on individual effort and choice, then society has no responsibility, and we don't have to do anything.

Besides the development of better measures of neighborhood context, several topics emerge from this book as important for future research. One question posed in the book but not answered is whether the migration patterns of Latinos should be considered "flight." Research is also needed on actual, as well as perceptions of social networks across neighborhoods. More could be learned about elites from various institutions than what was presented in Chapter 14. The method of interviewing not those at the very top, but rather those who are actually in the trenches, doing the work, could yield many important insights for other cities. All of these topics could flesh out the processes of how neighborhood inequality is maintained, and show the enduring force of segregation despite declining discrimination and steering by individuals.

One question readers may ask about the book is whether it will change their thinking. Put another way, is this a book that will change the discipline and set a new research agenda? To my mind the answer is a definite yes. It will be very hard to do research on any of the topics covered in *Great American City* without consulting the book to see what Sampson and his colleagues have done. At the same time, it is not going to change the direction of research based on a single idea, as I would argue Wilson's *Truly Disadvantaged* did with concentrated poverty or Massey's and my *American Apartheid* did with segregation. *Great American City* has the potential to change the intellectual terrain in many, many areas: neighborhood effects research, the meaning of selectivity, collective efficacy, inequality, altruism, civic society, social networks, crime, migration, organizations. This breadth will make it harder to identify the "one" single impact of the book, but at the same time, it will enhance its importance. If it lives up to its promise, the field will be immeasurably better.

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Showing that Neighborhoods Matter

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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,

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artifactual, and substantively trivial" (Hauser 1970: 645; see also Blalock 1984; Lieberson 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 inferences[4] 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 Lieberson (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

[1]Disclosure: 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.

[2]The 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.

[3]To 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.

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

[5]Interestingly, 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.

[6]That 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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City & Community
AMERICAN SOCIOLOGICAL ASSOCIATION

Neighborhood Effects and Public Policy

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Robert Sampson has written an important book, among the best in urban sociology in decades, because it adds importantly to our ability to think more structurally about cities and neighborhoods (Massey 2012). It also allows us to think more creatively about the ways that cities cohere and re-assemble as community. At the core of its contribution is its powerful, thoughtful contextual analysis of crucial urban causal questions set within a framework of the social reproduction of income and racial inequality. I can only praise his effort to create a "holistic picture of how the contemporary city is linked" (p. 238).

He has done this with a degree of methodological innovativeness and rigor that few other cities have ever been subjected to. He has done this with multiple methods and multiple year examinations inside the core of this large American city. He has virtually single handedly created new forms of validation with what he has termed "ecometrics" (p. 385). He had hoped to be able to compare Chicago with other places but resources limited him to a single case study of America's quintessential haven for urban sociology (p. 75). Here is a core strength as well as a potential source of one of the book's inescapable limits.

As solicited, my comments here are confined to policy-related issues. It is not of course necessary for urban analysts to address what are labeled "policy matters" since policy makers often select their own research protocols, contractors, and issues for detailed answers to questions, but my focus is on what urban sociology and public policy might take away from Sampson's work.

The first issue I address, necessarily, is how generalizable are his results? Are its findings unduly restricted to the life and times of Chicago? The next policy matter is the book's comments on a Federal housing experiment, Moving to Opportunity (MTO). My last comments focus on the chapter which has the most explicit statements regarding policy, chapter 17.

GENERALIZABILITY?

The first issue I address is what we may make of the generalizability of Sampson's findings to other American cities. Are there potentially strategic differences among cities that can affect what urban sociology should take from this book and how policy makers should respond to the suggestions emanating from his in-depth, brilliant look into the city of Chicago?

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Are all of the patterns and trends he finds for Chicago fully applicable to other cities, other regions, possibly even to other Rust Belt cities? Does being the quintessential sociological city necessarily make it the archetypical American city? My sense from different types of research evidence is that there are some limits to the sociological generalizability of his work. Thus, some of what Sampson discovers for and in Chicago will need examination in other types of cities (Stone 1993). Research suggests potentially different continuities and structuring. There is evidence that changes he identifies are not ubiquitous.

One example is how racially diverse or integrated American neighborhoods have become recently and whether public policy had anything to do with those changes. We know from research on a number of metropolitan areas, for example, that the rate of stable racial mixing has increased a bit over the past decades, even though we know next to nothing robustly about why this is so. That is, in addition to the modest declines in the levels of racial segregation, there have been modest increases in stable racial mixing. Are such recent analyses of racial mixing relevant to the story of how cities are changing in the current decade, in ways more balanced and tolerant than in the past or in Chicago? (Ellen 2007; Turner and Fenderson 2006; Turner and Rawlings 2009; Snidal 2012: 36; Quillian 2012; Ellen, Horn, and O'Regan 2012). Have any of the programs aimed at managing integration (Nyden, Maly, and Lukehart 1997) been relevant here or might have declines in overt, tested forms of racial discrimination played a part? We cannot tell from *Great American City*, but perhaps in other places the forces that impel such a hard regime of racial segregation and bias as appears in Chicago may not be as powerful.

As another example, Sampson finds that "racial tipping" (p. 119) has been at work in Chicago. He argues that "...once a neighborhood is beyond a certain threshold or 'tipping point' of either percent black or percent poor ... further change is in the direction of greater racial homogeneity or more poverty." The reference to a seemingly inescapable, necessary demographic trend has of course been much debated over the last decades (Ottensmann 1995; Quercia and Galster 2000; Card, Mas, and Rothstein 2008). We know from social scientific studies on nonlinear neighborhood changes that there is nothing necessarily predetermined or inevitable about these shifts; the tipping point appears to vary by urban area. If it is variable and can be managed—even if only marginally—the options for longer term stable neighborhood diversity might well alter some of the dynamics associated with Chicago's fairly rigid racial rules. The fact that the metaphor is regularly misused by those who shelter their biases within the fold and cover of such deterministic predictions (Goering 1978) only adds to my concern that such a label not become heedlessly embedded into future urban sociological pathways. More simply put, the evidence suggests that other cities may differ in the composition and dynamics of racial neighborhood mixing and change, and these differences appear worthy of somewhat greater study.

Another example related to the generalizability of Chicago's experience comes from research that shows that many cities, rather than having relatively stable or persistent patterns of neighborhood income inequality, have in fact recently lost both middle income residents and entire neighborhoods. The loss or thinning out of both middle-income residents and neighborhoods from many large cities suggests caution in describing persistent distributions (Galster, Custinger, and Booza 2006). The loss of middle income people and places from American cities could affect the probable success of some urban policies. It should also affect how we think of the collective uses and needs of cities. This recent discontinuity appears relevant to both urban sociology's and policy makers' tasks.

Allied to this is evidence that the lowest income urban residents face severe shortages of housing units at rents that they can afford (Harvard Joint Center 2011). This dwindling of affordable housing stock severely limits the mobility choices of poor households in Chicago and elsewhere who might seek better housing outside core containment zones. Their choices are eroding, helping to explain why Sampson and others, including MTO, have found such constrained housing searches. This is not a characteristic of the residents of Chicago, but rather is due to the failure of housing markets and urban policies to provide for an adequate affordable supply (Schwartz and McClure 2012).

Thus, there is some potential for ecological holes or discontinuities where affordable housing units and middle income households have disappeared or declined. This suggests the need for additional research on the spatial implications of rising income inequality. If this gap has differentially reduced the residential mobility options for residents, especially with low incomes, within other cities, it is an issue worthy of additional probing before concluding that all cities are alike or before deciding what policies might be most germane (Ehrenhalt 2012; Portney 2013).

A final example regarding whether Chicago is the model American city refers to its tortured history of public housing segregation and the nearly five decades long struggle to desegregate it. I begin farther back in time than Sampson, with the Chicago race riots of 1919, as seen first by Carl Sandburg (1919) and later by historians (Tuttle 1970, 1996). The career of Richard J. Daley, later to become Mayor of Chicago in the 1960s, began as a member and officer in a neighborhood association, the Hamburg Athletic Club, which saw its role as keeping blacks before and after the race riots confined in high density parts of south Chicago. It was into this same area that later Mayor Daley and the Chicago Housing Authority (CHA) stuffed lots of projects aimed at housing only blacks. The ecological boundaries that the Hamburg Club supported informally if violently became, loosely, the foundation for siting decisions made through and with large scale institutions. The U.S. Department of Housing and Urban Development (HUD) and the CHA were found guilty of violating African-American civil rights. Over the succeeding 45 years, efforts have been underway to try to reduce the segregative impacts of federally funded public projects (Hirsch 1983). The relatively recent demolition of most of the very same family housing projects appears as part of Sampson's story of ecological transformation, but not of its historical roots. This historical glance appears to shed light on the roots of the unique racial obduracy of the city's spatial patterning.

Not every city created such large "second ghettos" nor struggled and resisted immediate change for so long, even after civil rights laws were enacted in 1964 and 1968. I do not know how many other cities are different from Chicago, but suspect that the historical depth of the racism, the politicized policy process, and the now slowly evolving continuing impacts on families' lives are not fully replicated in other places with lower densities of public housing, fewer lower income families, or less rigid rules of ecological separation. This comparison appears worthy of additional research if only to learn the limits of public sector supported racism.

MOVING TO OPPORTUNITY AND THE VALUE OF SOCIAL EXPERIMENTS?

My second set of questions relates to Sampson's concerns with social experiments and in particular his reactions to the MTO experiment.[1] Most of these concerns are contained in chapter 11 which focuses on MTO, but they appear elsewhere in the book as well.

His campaign for a new approach to studying cities and neighborhood effects begins by arguing that methodological rigor is not necessarily the sole criterion for evaluating evidence. He states: "Credible knowledge cannot simply be assumed based on the method of origin" (p. 280). Indeed, sociologists have made a point for a long time that getting a true and full picture of causal dynamics and the meaning of contextual change often requires the use of mixed methods, quantitative with qualitative. But it is also necessary to stress that for certain audiences, statistical and quantitative "proof" is an essential ingredient in making decisions about whether policy-caused changes have had a meaningful, cost-effective impact. Social scientists have for decades accepted the fact that demonstrating empirical, statistically important differences is necessary to offer confident explanations of social change. Social experiments are in turn recognized as one of the important tools which help make causal interpretations clear and simple. They are nearly perfect for Congressional audiences (Burtless 1995; Crane 1998). Yes, the experimental or treatment group did change their behavior or no, they did not.

Not everyone is convinced by this evidence, but national and even local policy makers typically are. That is, experiments have political value even though some of the MTO researchers may have allowed the intellectual hubris of the method to outpace their understanding of causality.

In this regard, selectivity is an important neighborhood process, as Sampson very wisely points out, but it is *also* a major statistical constraint in measuring program impacts on people. If you cannot tell policy experts whether an outcome has resulted from the enthusiasm of program volunteers or from the program itself, you have wasted your and their time. Using the same terminology for both sets of issues—sociological and statistical—is surely confusing.

Next, permit me to clarify why the Federal government would spend millions of public dollars and take years to learn about whether residential mobility out of deeply poor areas helps families. It was not, as Sampson suspects (p. 268), because of William Julius Wilson's work, but rather because for several years they understood that something needed to be done to unravel the harmful, even tragic effects of segregating public housing families in some of the worst neighborhoods in the country. The multi-decade battering of lawsuits against HUD, such as the Gautreaux and Yonkers cases, made it clear to decision makers that something new had to be done. Both HOPE VI and MTO were created in 1993 to do so. There was a recognized need for an unimpeachable effort to help end public segregation.

MTO began as a search for statistically faultless proof that living in a better off community can positively affect a poor family's life chances. Gautreaux mobility research apparently had shown in the early 1990s that positive effects could emerge from the option to allow families to move out (Rubinowitz and Rosenbaum 2000). This finding was the lightning bolt that jolted the policy process. But the selectivity bias issues associated with this research also impelled the search for a clearer answer to the question of whether encouraging moves out of public housing in deeply poor areas could save lives and help people.

Random assignment was the methodological instrument, however crude and limiting, that offered the best hope of convincing policy makers and funders that this was an acceptable and indeed essential tool. For decades HUD had used social experimentation as the method to unravel how to proceed. Before MTO, in the 1970s, the Experimental Housing Allowance Program had allowed Congress and HUD to fashion a new method of allocating housing assistance. That programmatic predicate was the other foundation for MTO (Shroder 2000).

What both Sampson's research and MTO found is that for all too many, the experience and fear of crime in severely distressed public housing was crippling. For MTO it was the driving motive for families' decisions to move and later, the reason they were pleased to have left the projects. Mobility led to better physical and mental health including reducing obesity and diabetes, but had none of the effects promised by Gautreaux researchers: no improvements in educational outcomes and no better employment prospects. As some of us have said, MTO was a big idea weakly implemented (Briggs, Popkin, and Goering 2010). So while Sampson finds that collective efficacy helps reduce crime and illness (a place based result), for MTO families, the solution was to move away (a person-based solution). I use the parenthetical terms as they figure importantly in the next section in which I address their policy relationship.

POLICY ISSUES

The third question I have is about how to best approach the question of "policy relevance" and Sampson's recommendations. While Sampson's book offers few policy specifics other than in criminal justice matters, it does touch briefly on some core policy disagreements that appear relevant for future analysts. I will mention them because they recur quite frequently in discussions about how one might best or better intervene into how cities change and rebuild.

Sampson argues (p. 420) that "Rather than simply move people out of targeted communities, the idea is to renew what is already there while simultaneously investing in communities on the edge of critical need . . . " This is typical of many similar arguments in a long stylized debate over "people versus places." How much funding and emphasis should be placed on rebuilding poorer parts of inner cities—the places where poverty has its greatest hold and impact—compared to policies that assist individual poor households? More recent policy regimes have accepted the inevitable that both are essential, given how few resources federal and local governments have to do either fully and well.

Full and comprehensive revitalization of all city neighborhoods could only occur with a massive Marshall Plan-like intervention which for most seasoned policy experts is simply not politically or fiscally foreseeable (Haberman 2012). Once you have to intervene selectively, the issue of allocating limited resources within cities with powerfully organized and efficacious neighborhoods would almost inevitably risk the triaging of rebuilding (Stone 2005). That is, given the divisive power of institutionally ingrained income and racial inequities in most if not all cities, won't the pernicious beggaring of one's neighbors necessarily occur (despite Sampson's speculative chapter 9)? Conversely, who will support the city and region's common good when stressed neighborhoods become balkanized through "cohesive" barriers and defensive organizations?

The policy flip side of these risks is to ask what it would take sociologically, fiscally, and politically to support high degrees of collective efficacy for *every* neighborhood and on a *sustained* basis. That is, is it even conceivable to create programs for building collective efficaciousness? Once built, do we know reliably what neighborhoods would do with their collective might? Would it be for narrow parochial and defensive purposes, especially if budget cuts slice deeper into their social fabric? And what would become of at-risk families forced to stay while a new generation of Robert Moseses—magically found and empowered—rebuild the inner cities?

Most boringly for social scientists but most salient for policy makers, what might it cost to rebuild these fragile, wounded, inner city places—if we even knew how to begin and continue? The costs of place-based initiatives can only be hinted at in these short comments. In Cleveland alone, for example, roughly $300 million was spent on fixing or demolishing a few thousand foreclosed homes in the last few years (Bull 2012). In Chicago, $400 million has been allocated through the community development block grant program for only 9,500 units of affordable housing. Thus nearly a billion dollars has recently been committed by just two cities for a tiny fraction of their urban community needs. The 2013 budget for the entire community development block grant program (the Federal government's main tool to rebuild cities) is estimated to be less than $3 billon. What would the budget have to be in order to rebuild the neighborhoods Sampson feels need help? One suspects trillions are needed on a national level over time. This at a time when the Federal government is retracting its fiscal and programmatic reach and where every agency is aware of the potent, looming impact of budget reductions and sequestration (Cooper 2012; Grant 2012; Donovan, Duncan, & Sibelius 2012; Renn 2012).

THE DEFENSE OF THE COMMON GOOD AND SOCIAL ALTRUISM

Sampson's book raises a core issue for sociologists and policy makers concerning the production and maintenance of the "common good" when confronted with balkanized ecology and public choices. Chapter 9 addresses this question analytically and empirically. The empirical data come from a lost letter experiment and from a 1980s study of giving CPR to a stranger simulating a heart attack. In this limited space, I will simply argue that these mini studies in Chicago tell us little about the willingness of Chicago's residents to support complex, controversial, and costly programs aimed at addressing issues of structural inequity or the needs of both their localities and the commonweal. The data do little to help us appreciate either how standard NIMBY-like issues might be addressed or how neighborhoods would engage with projects that will cost them something substantial and real. Sampson's assertions are speculative. They begin to shed some light on the complex issue of what city dwellers will cough up for their own local good, that of their near and far neighbors, and for matters that appear to benefit no one and everyone, like air pollution controls, health education, land use zoning choices, or affordable housing.

For poorer communities, my final worries are what might be the balance point between their core, basic needs for shelter and safety and social and geospatial mobility? Might gangs have differentially more influence on the formation of neighborhood cohesion and risk in some cities than in others? And might George Galster (2012) be correct that suburb versus city dynamics are salient in places like Detroit? If so, for which other cities and metropolitan regions might the strength of metropolitan constraints outweigh narrower city and inner city neighborhood issues? Lastly, the policy programmer in me wonders how causality works in this model of the urban world with and without policy intervention.

Acknowledgments

I wish we had Sampson's book before we started MTO, but I believe some part of the energy and focus of the book appears driven by his challenging MTO pitch-men. Also, it would not have changed the imperative for an agency to try to expunge its legacy of harmfully segregating public housing residents for decades. Thanks to the author for this fine, enabling book.

Note

[1]A disclaimer here is necessary, as I helped design, manage, and conduct research on this experiment from its inception in 1989 to 2009 (Goering and Feins 2003).

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How Sampson's *Great American City* Challenges Age of Reagan Criminology and Where a Critical Urban Sociology of Crime Might Lead

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Robert Sampson's urban sociology of crime is all about the role of social context in understanding human behavior. It therefore makes sense to begin by placing his work in the context of American criminology. After reviewing the background of Sampson's scholarship, and the place of *Great American City* in it, we turn to where his urban sociology of crime can lead: to topics as far afield as genocide in Africa, and as close at hand as the financial crimes of the foreclosure crisis in Chicago.

The rise of American criminology in the Age of Roosevelt, from roughly the 1930s to the 1970s, was dominated by classical sociologists such as Cloward and Ohlin who theorized the interconnected legitimate and illegitimate opportunity structures of crime in U.S. cities of this era. Sampson entered the field of criminology after this, as the field was changing course during the Age of Reagan. The latter era has arguably lasted from the 1970s on. During this period, sociology has remained important to criminology, but it is now rivaled in influence by fields as different from sociology as operations research, developmental psychology, and economics.

What most distinguishes Age of Roosevelt from Age of Reagan criminology is the commitment of the latter to American individualism (Hagan 2012). Robert Sampson and his colleague John Laub (Sampson and Laub 1993; Laub and Sampson 2003) challenged this Age of Reagan criminology in the 1980s and 1990s by reasserting a sociological emphasis on context and by using time and place to leverage this shift. They grounded their challenge in panel data, initially collected by Sheldon and Eleanor Glueck at Harvard, tracing adolescents transitioning to adulthood in the neighborhoods of mid-20th century Boston.

Sampson and Laub rediscovered the Gluecks' data in the basement of the Harvard Law Library and extended the panel through adulthood. Many in the sample were World War II veterans and experienced the rapid expansion of Boston's post-war economy. Sampson and Laub's contribution was to integrate and subordinate the assumptions of individualistic or developmental criminology within a more encompassing urban sociological framework. They seriously engaged the Age of Reagan emphasis on individual stability in developmental criminology, but they also focused on the malleability of individuals across the life course.

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In the 1980s, Age of Reagan criminology was preoccupied with predatory violent criminals. This preoccupation grounded a developmental perspective that emphasized early onset predictors of chronic offending and career criminals. Reagan's Attorney General, Ed Meese, latched on to the finding of Marvin Wolfgang (1972) from mid-century Philadelphia that relatively few offenders accounted for a large proportion of official crime. Following Harvard's James Q. Wilson (1975), Age of Reagan criminologists pursued a well-financed research program using empirically grounded predictive models to identify, deter, and incapacitate would-be career criminals.

However, armed with their Boston life cycle data, Sampson and Laub found little evidence of such predictors. In contrast to Age of Reagan expectations, they observed that many delinquents whose backgrounds seemingly predicted career criminality in fact turned their lives around when they found supportive spouses and/or good jobs. Military service (pre-Vietnam) was a further turning point. They concluded with mordant irony and mock bafflement that "why criminologists continue to search ... for ever-more perfect prediction from the distant past is something we leave for future historians of the field" (Sampson and Laub 2005: 907–8). They interpreted their own findings as evidence for Glen Elder's life course emphasis on "turning points." Their findings contextualized stability and change in criminal behavior in relation to specific events and experiences over the life cycle.

The reanalysis of the Glueck data resulted in prestigious journal publications and two award-winning books. Sampson's early accomplishments positioned him for a significant role in planning a generously funded 1990s study of crime and human development. This became the Project on Human Development in Chicago neighborhoods and the foundation for *Great American City*. The project incorporated developmental premises from a National Academy of Sciences report, *Understanding and Preventing Violence*. However, the report also explicitly recommended a multi-community research program.

The MacArthur Foundation in Chicago provided core funding with the National Institute of Justice, and the multi-community focus evolved into a multi-neighborhood Chicago design. Sampson was now on faculty at the University of Chicago and became the locally powerful figure guiding the project. Rejecting the Age of Reagan preoccupation with predicting chronic career offenders, Sampson redirected the Project focus to contextual effects of Chicago neighborhoods on families and children.

Thus the major theoretical and empirical innovation of Sampson's Chicago Project—his concept of "collective efficacy"—emerged at the community-contextual rather than the individual-developmental level. Sampson built on Bandura's concept of self-efficacy by emphasizing that individuals can organize in neighborhoods that have their own distinctive collective capacities. This was the key micro-macro link in Sampson's theory. Using the new Chicago data, Sampson demonstrated that, net of individual factors and community disadvantages, some neighborhoods—such as Avalon Park and Chatham—exhibited enhanced capacity to prevent crime.

Not only collective efficacy, but also Sampson's research design and methodology dramatically impacted the criminology paradigm. His innovations included sampling neighborhoods with careful attention to class and racial composition, and applying Raudenbush and Bryk's (2002) hierarchical linear models to establish the neighborhood-level collective efficacy effect. The focus on neighborhoods as well as individuals, combined with the new methodology, empowered a new urban sociology of crime that eclipsed criminology's Age of Reagan individualistic bias.

The versatility of the concept of collective efficacy is especially noteworthy for its potential application across a wide range of crimes and contexts. For example, one implication of Sampson's work is that the concept of collective efficacy and the cohesion and trust that it rests upon may help to explain what is sometimes known as the Latino paradox—the tendency for immigrant Latino communities to sustain lower levels of crime in the face of many socio-economic challenges. Sampson makes the provocative point that rising levels of Latino immigration in recent decades may have contributed to declining crime rates and, by implication, that the collective efficacy of Latino communities can help to explain this (see also Hagan and Palloni 1999).

The concept of collective efficacy also recalls earlier Age of Roosevelt sociological thinking about crime and its control. For example, Edwin Sutherland extended his individual level differential association theory of crime to the level of group responses to crime through "differential social organization." Like Sampson, Sutherland explained how forces could be mobilized to counteract crime at the macro as well as micro level.

Drawing on Sutherland and Sampson, Ross Matsueda (2006) has added the concept of social efficacy to refer to the capacity of particular individuals to mobilize others in realizing shared communal goals of the kind Sampson calls collective efficacy. Thus the concept of social efficacy is a linking mechanism that highlights individual agency leading to collectively organized communal action; for example, organizing individuals in a neighborhood to jointly supervise children and collectively maintain public order.

A recent *New York Times* (Carey 2013) article on Sampson's *Great American City* suggested that collective efficacy is perhaps best understood as social cohesion, although trust is also clearly involved. The micro-macro links identified in the theory of collective efficacy are both nuanced and complex. This theory clearly leads away from the individualism of Age of Reagan criminology and toward the more sociological realization—of Sutherland and other criminologists—that similar but opposing processes of social and collective efficacy at the micro or individual and the macro or neighborhood levels play crucial roles in organizing *both criminal behavior and responses to it.*

Collective efficacy is a foundational element of group empowerment. As Sampson typically uses this concept, it refers to an internally driven process, with collective efficacy emerging from inside a community and expressing itself in the preventive collective willingness to intervene and respond to criminal behavior. Collective efficacy is thus empowering for the community (see also Hackler et al. 1974). Community cohesion and trust are key elements of this kind of collective efficacy.

The versatility of this concept encourages broader thought about its genesis and consequences. The willingness to intervene inherent in collective efficacy involves the inclination for communal self-defense. Lines between crime and self-defense can be notoriously elusive. The claim of "pre-emptive" self-defense is only one example. One person's self-defense is another person's unprovoked assault. One nation's pre-emptive war is another nation's invasion. Collective efficacy can mobilize both crime perpetration and prevention (see also Portes 1998), with the distinction a contested reflection of "whose side we are on."

We explain below how top-down political and economic power can create conditions of collective efficacy that permit and persuade groups of individuals to perpetrate crimes. We explain how this happens in contexts as different and consequential as mass atrocity and financial crime.

The challenge for urban sociologists going forward is to understand the power dynamics of collectively organized criminal behavior *and* crime control, for example, as fought over territorial domains such as neighborhoods, towns, and villages. Both perpetrators and potential victims can be mobilized with collective efficacy into groups that empower the pursuit of goals in contested settings. Articulating these power dynamics can take us well beyond the confines of Age of Reagan criminology. We make this point first in the far away African Darfur region of Sudan, and second in the more familiar American context of Chicago.

We have recently used Sampson's concept of collective efficacy to analyze the ways—at the village level in Darfur—the Sudanese state mobilized and empowered local militia—referred to as Janjaweed—to perpetrate genocide against local Black African tribal groups (Hagan and Rymond-Richmond 2009). *We suggest that genocide is probably impossible to perpetrate without a mobilization of social and collective efficacy.* This kind of broadly based, macro-level effectiveness is an essential feature of massive state-led organized atrocities.

Black African rebel groups challenged the Sudanese government's refusal to provide services or invest resources in Darfur and they succeeded in mounting token attacks on government forces. Sudan insisted its disproportionate and overwhelming response—including the onset of mass atrocities—was its sovereign right to self-defense against a domestic insurgency. Sudan's militarized security state domestically and internationally rationalized the rebel attacks as an incipient revolutionary threat that demanded and justified a collective counter-insurgent willingness to intervene and overwhelmingly respond—to the point of mass atrocities.

The social and collective efficacy with which genocide was accomplished in Darfur involved a process of defining the perpetrator and victim groups in an "us" versus "them" justificatory framework. In genocidal contexts, perpetrators use "us" versus "them" framings to foster identities that valorize and stigmatize, devaluing the "other" as enemy. We use a critical collective framing perspective to explain how this kind of stigmatization process was used to achieve collective efficacy in instigating and organizing large-scale collective violence in Darfur.

State-supported agents provided the "social efficacy" that Matsueda describes as leading from individual initiative to ideologically framed collective action—in this case, collective violence organized by the Sudanese "us" against the Darfuri "them." Classical criminologists such as Cloward and Ohlin explained how criminal collective action is often organized through intertwined legitimate (e.g., national and local government) and illegitimate (e.g., local gangs or militias) opportunity structures. Analogous processes apply in Darfur. This kind of linkage requires collective efficacy.

In Darfur, the Sudanese government and its agents exercised its state based collective efficacy by training and equipping Arab nomadic (Janjaweed) herding groups to attack and take over the pastoral lands of the indigenous Black African farming groups. The power of the Sudanese state was essential in permitting the genocidal violence. A deputy minister was sent to Darfur for a four-month period during which he told local groups that he held and could grant to others the power "to kill or forgive whoever." He singled out the Black African Fur tribal group in encouraging taking from "all the Fur what they had," which he characterized as "booty."

The key to unleashing mass atrocities involved training camps where individual expressions of racial hatred were converted into group mobilizing collective expressions of racial intent to kill, rape, and plunder. The key mediating mechanism involved collectively chanting and shouting racial epithets to mobilize the attacks. This kind of racially targeted and viciously violent collective behavior is expressed in a furious and frenzied form of vengeful rage. Government and militia leaders used their social skills and efficacy to ignite this fury with racial epithets, providing the racial sparks that were required to incite ordinary militia members to commit mass racial atrocities.

Like Sampson, we used hierarchical linear modeling to demonstrate the collective efficacy with which stigmatizing racial epithets were invoked. Thus we used HLM models to analyze how the attacks by individuals on Black African villages were organized through racial epithets within groups. The Sudanese-backed Janjaweed militia killed more than three hundred thousand and displaced more than three million Darfuries (Hagan and Palloni 2006). The latter survivors remain today, nearly a decade later, in massive displacement camps inside Sudan and across the border in Chad.

There is little opportunity for a collectively efficacious defense by the Darfuri survivors, although some Black African rebel groups have sometimes succeeded in mounting counterattacks. True, these groups demonstrate a remarkable social cohesion based on centuries of cultural tradition necessitated by the environmental challenges of survival in sub-Saharan Africa. However, the Black African Darfuri groups lack access to the lethal weaponry and empowering resources of the Sudanese state, which the latter exploits through trade relationships with countries like China. The Sudanese state further blocks reliable provision of even humanitarian supplies and support required in the camps to meet the basic life-sustaining needs of the Black African groups. Collective efficacy works to the decided advantage of the Sudanese state in Darfur. This is collective efficacy with a state-led vengeance so economical that it has been called "counter-insurgency on the cheap" (de Waal 2004).

Closer at hand, Sampson concludes *Great American City* by considering the home foreclosures that followed the 2008 financial crisis in Chicago. His analysis confirms that the foreclosures were heavily concentrated in African American neighborhoods also burdened by concentrated poverty and violent crime. The banks rationalized using subprime loans as the means to extend the American Dream of home ownership. When borrowers could no longer make payments on these loans, the banks rationalized that they needed to repossess the homeowners' properties. Thus, the banks rationalized their foreclosures as their willingness (and responsibility to stockholders) to intervene and respond. They claimed they acted in collective self-defense against losses from defaulting borrowers, further neutralizing these often "robotically authorized" foreclosures as necessary to mitigate these losses.

Yet there is optimism in Sampson's finding that the organizational richness of some challenged Chicago neighborhoods acts as a buffer (see also the work of Pattillo 1999, 2007) that preventively protects these threatened communities from the negative effects of potential foreclosures. The *New York Times* concludes that Chatham is "holding its own." In italics, Sampson concludes that "*Community-based prevention is apparently not just for crime.*"

Sampson is right to close with the home foreclosure problem, as these foreclosures constitute one of the most massive financial crime waves in Chicago and U.S. history, challenging the misplaced emphasis on street crime in the Age of Reagan. Legal scholars and prosecutors can quibble about whether several million of the home foreclosures connected to the American financial crisis were products of civil or criminal fraud, but the reality is that financial fraud is at their root, and Edwin Sutherland would remind us that such frauds are best sociologically understood as white collar crimes, regardless of what the legal system calls them.

It is heartening that urban collective efficacy in communities like Avalon Park and Chatham can mitigate the consequences of financial fraud resulting in home foreclosures. This finding is important because it leads to further questions about how and why it is that these communities find themselves needing to summon *their own resources* to defend themselves against problems imposed by powerful financial institutions often behaving criminally. (Note: relatively little of the financial industry bailout funding has been used to remediate fraudulent mortgages and foreclosures.) A critical urban sociology of crime can use the concept of collective efficacy and the empowerment associated with it to unravel how the massive white collar fraud leading to the foreclosures was perpetrated.

The perpetration of this fraud was collective efficacy on social and political steroids. We have argued elsewhere that the financial industry in America harnessed the empowering collective efficacy of state initiated financial deregulation, the hierarchical integration of the home mortgage sector, and the rationalization and neutralization of its fraudulent activities—and that it did so to target minority disadvantaged communities for financial fraud (Hagan 2012: Chapter 6). It is the last part of this story that is ultimately most disturbing.

The NAACP led a class action suit that charged more than a dozen banks with racially discriminatory lending practices. Decades earlier, in the 1960s, banks "red lined" African-American neighborhoods where they refused to make home loans. Realizing that previously red-lined borrowers could be enticed with newly available and attractively packaged loans, banks and their subsidiaries reversed course in the 1990s and 2000s to make sub-prime loans to homeowners who could not sustain the payments with deceptive incentives. As is now widely known, these sub-prime loans were repackaged for rapid resale to investors as securitized mortgages and derivative products. As these loans unraveled, they led to high rates of foreclosure, especially on the south and west sides of Chicago.

Five leading banks in Chicago were the most persistent foreclosers: the Bank of New York, Chase Bank, Deutsche Bank, US Bank, and Wells Fargo. In civil law settlements, these banks have been allowed to pay fines for their fraudulent practices without acknowledging guilt. It is estimated that over 50 percent of home loans held by African-American and Latino borrowers were subprime loans with elevated interest rates, and that these minority borrowers were three times more likely than whites to hold such loans. In the years before the 2008 crash, middle-income African-Americans were nearly five times more likely than whites to hold subprime mortgages (Rivera et al. 2008).

The *New York Times'* Michael Powell (e.g., 2009) has exposed how Wells Fargo Bank, the nation's largest home mortgage lender, advanced its sub-prime lending program. Powell found that the Bank had a "ghetto loans" program and an "emerging-markets unit" that specifically targeted Black churches. The former euphemisms were collectively efficacious as rationalizations used in mobilizing an aggressive sales force that willingly inserted itself into minority communities to market unsustainable visions of home ownership as the American Dream.

A key strategy was to exploit the Black Churches as a historically powerful source of neighborhood legitimacy and collective efficacy: "it figured church leaders had a lot of influence and could convince congregants to take out sub-prime loans." In July 2012, Wells Fargo agreed to pay more than $175 million to settle charges that independent brokers, who were vertically integrated into its lending operations, discriminated against Black and Hispanic borrowers during the housing boom.

The urban sociology of this kind of fraudulent behavior leading up to the financial crisis and its impact on minority communities is only beginning to develop (e.g., Rugh and Massey 2010). Part of the difficulty of telling this story is the empowering collective efficacy of the financial industry, and especially the "too big to fail" banks and their CEOs, in eluding criminal prosecution for their instigation of predatory lending. Too little attention is yet given to documenting and explaining how collective efficacy can empower this kind of crime downwards through the social structure. Robert Sampson's *Great American City* is a large step toward a bigger project of creating a more comprehensive post-Age of Reagan urban sociology of crime that broadens our attention beyond street crimes—to suite crimes, to war crimes—that is, upward, downward, and outward across the global horizon.

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City & Community

Thinking About Context

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I am deeply grateful to the four reviewers in this *City & Community* symposium for taking *Great American City* so seriously and sharing their thinking. An author cannot ask for more, and from scholarly heavyweights at that. Better yet, they raise excellent questions, making me think anew and I hope provoking readers of the journal. In this limited space to respond to four reviews, I am not able to delve into all the issues I might otherwise. I thus briefly address each review in the alphabetical order in which it appears, at the same time highlighting cross-cutting themes and implications for future research.

Nancy Denton's authority as an urban scholar and her seminal work on segregation prepare the reader for her incisive comments. I want to highlight several analytic points she makes, starting with the fundamental importance of studying individual behaviors *but as located in contexts that are independently studied.* The social sciences mostly moved away from this stance in the latter half of the 20th century, adopting a kind of "methodological individualism" that, in my view, impoverished the study of context. This privileging of the individual was a key motivation for my work with Stephen Raudenbush in developing the idea of ecometrics. Not only is the independent study of context important, but also relations among higher-order contexts (e.g., spatial interdependence among neighborhoods; elite network or residential mobility ties that concatenate across the metropolis). I would argue that ecometrics is a flexible and scalable paradigm that can be adapted to these broad goals.

Denton makes another interesting point about researchers who imply that the poor should do more in their own communities than the wealthy do in theirs. I agree but would go further. The places that academic researchers typically live in inure them to the daily struggles and fears of those living in distressed neighborhoods, especially regarding personal violence. A broader concern is that our theories of neighborhood selection typically ignore what residents tell us they care about and how they make decisions. The "folk theories" of residents, if you will, are neglected or deemed irrational. One of the reasons crime plays such a prominent role in the book is because that is what many residents, key leaders, and those in power react to. That much of the urban disorder problem is socially constructed—a major theme of the book—does not undermine the central importance of perceptions in shaping the urban environment. Quite the opposite—at the most general level, collective perceptions and shared understandings have been undertheorized in urban sociology, usually subordinated to "structure." The way forward, I would argue, is to study how cultural and structural forces are intertwined causally over time. "Cognition and context," then, is a major agenda for the future.

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A major thrust of Denton's analysis of *GAC* centers on my treatment of "selection bias." Although largely in agreement, she wonders about the phrase that "neighborhoods choose people rather than the common idea that people choose neighborhoods." Others have questioned this phrasing as well (Venkatesh 2012), so let me briefly explain. Although it is true that in a literal sense neighborhoods do not act, I think it is theoretically plausible to posit supra-individual action just as we do with organizations and the state (cf. Tilly 1973).

There are at least two ways neighborhoods "act" despite actions of the individual resident. One is structural and formally constraining, such as zoning restrictions (e.g., lot sizes that require McMansions), racial covenants (in an earlier era), or age restrictions. If an under-55-year-old with kids wants to move to many "retirement" neighborhoods in the Phoenix area, for example, he or she simply cannot. The second is informal. Suppose a black Chicagoan selects to move to a largely white neighborhood because of its lower violence and better resourced schools, and others do likewise. But suppose soon thereafter that white neighbors quietly move out, leading soon to a (re)segregated neighborhood. This unfortunately too-common scenario means that the selected environment changes around the in-mover, undermining both a pure selection argument and, more fundamentally, motivating the idea that the context itself (albeit through an aggregation of decisions) is selecting. In these examples, what the selector "selects" is either (a) formally rejected at the outset, or (b) itself changes and is therefore not ultimately what was chosen. Because of these and other mechanisms, I argue in the book that "the strong version" (p. 327) of my findings implies that neighborhoods "choose people." Sometimes the strong version of a theory can aid in making analytic distinctions.

Denton also points to other areas of needed research, such as Latino migration, the study of elites, real estate steering (another kind of selection), and housing policy as reflected in the Moving to Opportunity (MTO) project. I take up the latter in my discussion of Goering's policy response, but agree with these directions. I would only add that information technology may be creating a new kind of seemingly impersonal neighborhood steering that does not require bigoted real estate agents, reproducing segregationist outcomes.

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I turn now to the essay by Claude Fischer, a long-time leader of urban sociology. Here again the author does not disappoint. If anything Fischer is too modest. *To Dwell Among Friends* is a modern classic and its meticulous design inspired many elements of the Chicago Project. Some of the creative aspects of the latter may therefore be traced to northern California (and that is hard for a Chicago School scholar to say!). I would also point to his influential article on the subcultural theory of urbanism (Fischer 1975), which was *about places, not people* (see his 1995 assessment). Many social scientists seem not to fathom why it is of theoretical interest to study ecological-level relationships in their own right, much less contextual effects on individuals. The intellectual history of the inevitable critique of contextual effects, which Fischer traces to Hauser, is nicely laid out.

What I find problematic in the field is precisely what Fischer states is the prevailing or starting ethos—the demand "to rule out selection explanations for neighborhood effects." Readers of *Great American City (GAC)* will not find a researcher who ignored this demand—like many others, I tried multiple strategies and if closure is expected, perhaps without great success. But why this framing? I can imagine a different world where the demand is reversed, where researchers are put on notice to "rule out contextual explanations for individual effects." (How often have you read that in a journal review?) The power of individualistic theory (ideology?) in the social sciences has been to create a hierarchy of explanatory standards and preferred accounts. A result is that the field has been dominated by a litany of statistical techniques all in search of ruling out context ("instrumental variables" and propensity scoring being the latest). Fischer astutely dissects the limitations of many of these techniques, such as controlling mediating pathways, or the fact that individuals may be selecting on the context itself. And randomized experiments have their own problems, as the MTO debate has revealed.

I would, therefore, be the first to agree with Fischer that there has been no "closure" in the attempt to demonstrate neighborhood effects net of the individual, as traditionally defined. But my larger argument was to reframe what is of most interest to explain. Here I would return to Denton's query about neighborhoods choosing, calling on Fischer's interpretation of my thesis: "Neighborhoods attract, repel, and indeed *select* the people who would live there" (emphasis in original). In addition to the structural dimensions of neighborhood stratification and spatial hierarchy, he adds cultural sorting, which is not well understood and calls out for future inquiry. I would further suggest that there has been no closure in empirical attempts to prove that "individual effects" trump context, especially when we take a developmental and intergenerational view of the life course (Sharkey 2008). Recent analyses showing early childhood effects of neighborhood poverty are extraordinarily important, I think. To me the data imply that we need to consider "contextual bias" as an equal rival to "selection bias."

Overall, *GAC* can be fairly read as rejecting a strict methodological individualism and for claiming what I defined as a "family of neighborhood effects." Included in this family are Fischer's (1975) ecological-level relationships and higher-order structures that link contexts. There is also no theoretical reason to limit the effects of neighborhoods to their residents only (many residential neighborhoods attract visitors or workers, and many nonresidential areas attract crime) or to individual outcomes. Where a library is sited or how the police patrol a neighborhood has no individual analog, but can constitute a neighborhood effect. *GAC* thus attempts to elevate the rigorous study of a family of institutional, interactional, normative, and structural forces, including those extending beyond the border of any particular neighborhood.

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John Goering was asked to focus on the policy aspects of the book and he does so with clarity. As one of the major researchers closely involved with the MTO project he is well positioned to weigh in on my critique of the experimental paradigm and the broader issue of housing policy. He makes a surprising point early on that I fully endorse but which many in the policy world reject: it is not necessary for urban analysts to address what are labeled "policy matters." This acknowledgement is perhaps more important than Goering realizes. I have noted the hegemony of selection bias, but here it is the potential hegemony of policy bias. I am all for policy relevance, but to dictate a priori that research must address policy questions poses a considerable intellectual risk that scholars must carefully evaluate—above all, research should be driven by the theoretical problem at hand.

Goering goes on to focus on two issues—generalizability and the MTO. I grant that Chicago is not exactly like every other city, but as I noted in the book, I believe there are a number of lessons and mechanisms that have legs, and I think the data bear this out as results come in from cities around the world. Even where there are manifest differences, there can be similarity in underlying mechanisms. I also agree with Goering about racial "tipping points"—I explicitly state that the processes I observed are "not inevitable or natural" (p. 99). And whereas it is true that Chicago is famously segregated (see also Fischer's comments), neighborhood effects are not defined by race. Multi-dimensional inequality by place is the more general interest of the book and for which I posit broad relevance. In particular, Goering notes modest declines in racial segregation, but one could as easily point to the *increases* in recent years of income segregation (Fry and Taylor 2012). I think we are likely to see further status, class, and resource sorting by place and thus, neighborhood effects that go well beyond race, although like Denton I remain somewhat pessimistic on prospects for substantial racial integration.

Goering is also right to point to the tradeoffs in drilling deep to study mechanisms in a single place versus national and purportedly more "generalizable" results. I have faced the "it's only Chicago" charge for years and am fine with it—understanding our nation's third largest city is no small task. But I confess to feeling like an ethnographer must feel in making the case for case studies. There are wonderful national-level studies that do many things better, but they often end up thin on explanatory mechanisms (see also Fischer on the "why" question), culture, history, and most of what makes a place a place (Sugrue 2012). This of course reflects an old debate in the social sciences that has pitted anthropologists and historians against sociologists, who in turn are pitted against economists, and so on. I leave it for the readers of *GAC* to judge how well I have balanced competing concerns and elided pernicious distinctions. Going forward, however, I agree that comparative studies of neighborhood effects by city will be essential.

Goering then turns to the MTO and makes an important point about the centrality of housing policy to urban sociological studies. The good news is that a new generation of research seems to be taking hold that focuses on housing mechanisms to better understand neighborhood effects. Unfortunately, as seems largely agreed upon, using housing vouchers to undo concentrated poverty is a tall order, an issue I take up in depth. I still maintain that the MTO treatment did not change movers' environments in kind, rather than in degree. For the latest in MTO results and what I view as the power of durable neighborhood inequality to shape individual-level policy interventions, see Ludwig et al. (2012) and Sampson (2012).

The final issue Goering discusses is place-based versus person-based interventions. He raises good questions and points to the enormous expense of "Marshall Plan-like" interventions, which I agree are not feasible in the current political era. But a rethinking of place-based interventions may not necessarily require huge sums of money or massive rebuilding. In particular, I am cautiously optimistic about mixed-income housing policies and especially violence prevention at the place-based level. Many cities have shown that crime can be reduced without large expense (e.g., through "hot spot" policing), with benefits that redound disproportionally to low-income, minority neighborhoods. Another point is that policy tends to focus on the very "worst" neighborhoods rather than thinking more systemically about the distribution of all neighborhoods (see Denton)—spillover effects can be positive.

Policy approaches can also leverage insights from neighborhood-effects research to craft what I would call a *place-based logic*. Although we can't flood the country with Harlem Children's Zone-like interventions, future efforts might well look to policies that apply to all neighborhoods but which impact the places most in need. Some examples might include redistributive policies on property taxes that currently pit neighborhood against neighborhood, cutting the linkage of school boundaries to neighborhood, tax incentives that support reverse migration while maintaining housing supply for the poor, and reduction of mortgage deductions. Macrolevel policies combined with localized interventions are thus conceivable without wholesale urban redesign à la Marshall. This is important because many poor residents do not want to leave their neighborhoods, even if elite policy makers deem them not worth saving (Gans 1962). What most everyone wants is a safe and affordable environment with decent resources. Individual choice policies are not the enemy in this goal, but neither should they automatically be the policy of first resort.

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John Hagan is a leader in the fields of criminology and law and society, and has for decades stimulated us with thought provoking ideas. Most recently, Hagan and Wenona Rymond-Richmond (HR) have taken on the study of genocide and white collar crime. HR's essay focuses first on the challenge of my emphasis on context to the dominant "individualist" paradigm in criminology. They then highlight the concept of collective efficacy and how it is a linking mechanism that bridges the micro and macro levels, but which is fundamentally social in character. They also discuss the consequences of micro–macro models for methodology, in the form of ecometrics and the importance of taking social units of analysis (whether neighborhoods or societies) seriously in their own right (see also Fischer and my discussion earlier).

What is likely to be provocative for readers is the terrain on which HR see implications of *GAC*. Perhaps most innovatively, they apply collective efficacy theory to understand actions by the state to create social divisions within communities, using the example of Darfur. They argue that the state can mobilize both crime perpetration and prevention, and that "top down" political and economic power influence conditions of collective efficacy on the ground. HR's take away is that urban sociologists need to understand political power dynamics better, invoking their important research on Darfur to show the state was implicated in defining who was the insider or outsider, thus influencing who became a victim of genocide. The state had the raw power and supplied the weapons of destruction—village-level collective efficacy may have tamped down the ultimate tally but it was far from enough to prevent atrocities.

I also quite agree that social networks and collective action can be appropriated for good or ill (p. 151). Urban scholars have long been concerned about the potential dangers of the "defended neighborhood" (Suttles 1972). That is why it is important to study the goals to which ties are directed and why I address social exclusion and public goods, bringing in Rawls (1971) and theories of social justice as a way to think through the sorts of dilemmas that HR contemplate (pp. 212–215). Although perhaps "speculative" (cf. Goering), I develop and assess a community-level conception of other-regarding public behavior, with implications for individual rights (which potentially could be applied to

HR's interest in human rights). The alarming example of Darfur shows how political forces can create distinctions that undermine the collective good for horrific ends.

HR go on to analyze power dynamics at the level of financial malfeasance, tracing how the fraudulent actions of banks ultimately led to the torrent of foreclosures disproportionately visited upon the disadvantaged. Analyzing the effects of the Great Recession in the form of foreclosures, I show in the book that at the community level, the poor got poorer and the rich got richer (e.g., figure 16.4, p. 405). I thus agree that a critical urban sociology of crime needs to incorporate larger economic forces, although I would not define the acts of Wall St. *as* collective efficacy. It is important here to emphasize that a major ingredient of the theory is *social control*. As I argue in the book (p. 482): "the theoretical ideas of collective efficacy presented here could even be applied to financial firms and the recent economic crisis, which I would argue stems from a failure of control (social regulation) combined with a weakening of shared expectations about moral behavior (e.g., a cynical maximizing of short-term profit for personal gain at the expense of the long-term wellbeing of a company)." In other words, although power was indeed implicated in Wall St.'s pillaging, such conduct was allowed because of state capitulation to market fundamentalism and the idea that individual "greed is good." We do not have the data, but I would not predict much trust among Tom Wolfe's "masters of the universe," nor much concern at all for the common good. Many elites simply looked the other way, the opposite of the intervening at the heart of collective efficacy theory. Bringing in cultural norms and deterrence processes might go a long way toward explaining Wall Street actions (p. 482).

The larger point that HR make is nonetheless important, and one I tackle in Parts IV and V—one cannot understand neighborhood-level dynamics only by reference to internal processes. Higher-order structures and the political role of the state are important, which we saw quite dramatically in the case of foreclosures. The trick is to not overstate the unified power of the state or denude individuals of agency or the power of collective action to fight against structural injustices. A critical urban sociology for the future needs to be theoretically nimble up and down the explanatory spectrum. It also needs to conduct more rigorous empirical inquiry. More needs to be done to study political elites, for example, a point made by HR but Denton as well. *Great American City* may fall short in this respect, but the scaffolding for further advance in the area of inter-organizational and leadership ties is, I would submit, present (see chapters 12–15).

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There is much more that might be said, but I leave it for readers to reflect and take the next steps. A large research agenda looms, and the reviewers have collectively posed important questions and provided insights about how to proceed.

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