Great American City: Chicago and the Enduring Neighborhood Effect by Robert J. Sampson
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lic spaces in the earlier chapters and commenting on conversations with 
other black professionals (and to some extent expressing his personal under-
standing of racial and class identity) in the later chapters. There is a very long 
tradition of scholarship on the black middle class, at least as early as W. E. B. 
Du Bois, through the Chicago studies of St. Clair Drake and Horace R. Cayton 
and E. Franklin Frazier, to more recent work by Joe Feagin, Mary Patillo, 
Karyn R. Lacy, and others. Anderson’s new book is not so much an extension 
of this scholarship as an introduction to it. As an introduction it might be more 
helpful if, in addition to offering brief citations of these studies, Anderson 
had commented more expansively on what they had found and how his own 
fieldwork could inform their conclusions. But this book is clearly not in-
tended, either in the discussion of public spaces or in the commentary on the 
black middle class, to develop theory or to challenge or extend scholarship 
on race. The Cosmopolitan Canopy is rather an ethnography carried out in a 
highly personal style, not quite a diary or a memoir, but very much an in-
roduction to urban life and to being black and successful as the author feels 
and experiences it. This style gives the work great force. Reading, you know 
you’ve heard it before but now you’re hearing it in the words of someone who 
fully understands the aspirations and conflicts of a black person on the way 
up, the half acceptance at work, the doubts about self-presentation, pride in 
success, connections with the black inner city but also distance from it, and 
ultimately the nigger moment of racial injury. This is powerful writing, ac-
cessible to a very wide audience, and undoubtedly useful for students and 
others who seek to understand what race and diversity mean in America.

Great American City: Chicago and the Enduring Neighborhood Effect. By 
$27.50.

Jens Ludwig
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It is an honor to be asked to review Robert Sampson’s new book. First a 
disclaimer: I spent 2010–11 visiting the Russell Sage Foundation in the 
office next to Rob’s, who would always share his stash of roasted almonds 
with my then-seven-year-old daughter whenever she visited. This could 
technically be viewed as a bribe, but only by those who believe in the eco-

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distance less important, by those who lament the decline of community, and by evidence from randomized experiments like Moving to Opportunity (MTO), which I helped study, that has been interpreted by some as suggesting that neighborhoods don’t matter. But at some level the book’s war has already been won. Even within economics, home of the rational actor, there has been a turn during the past few decades toward the study of how social context influences individual behavior and aggregate outcomes like economic growth.

One major contribution of Sampson’s book and PHDCN generally is the attempt to answer the why question of neighborhood effects. One underappreciated point has been Sampson’s distinction between “situational” effects, or the role played by the neighborhood context in which a decision or behavior may take place (which might not be where someone lives), versus “developmental” effects from the accumulated history of where someone grew up. More attention to this distinction might change the interpretation and design of much research.

Sampson and his colleagues have played a major role in the “process turn” of the literature. Rather than just look at neighborhood social composition, they asked: What exactly happens inside (and across) neighborhoods that might matter for people’s lives? One answer is collective efficacy—shared expectations about the willingness of local residents to support shared social norms. Even neighborhoods with similar social compositions can vary greatly with respect to collective efficacy, density of social ties, civic activities, perceptions of disorder, altruism, crime, legal or moral cynicism, and presence of community organizations.

Why is the focus on process important? Sampson shows that neighborhood disadvantage is quite persistent over time. While neighborhood social processes also tend to be fairly persistent, in principle social processes might be easier to modify than social composition would be. These are all important lessons that come out of the PHDCN.

Over 20 years ago Christopher Jencks and Susan Mayer asked, if people have at least some say about where they live, how much of the variation across neighborhoods in people’s outcomes is due to causal neighborhood effects versus the sorting of different types of people into different neighborhoods? (See “Growing Up in a Poor Neighborhood: How Much Does It Matter?” Science [243] 1989: pp. 1441–45.) Documenting the clustering of outcomes, regardless of its cause, is valuable for a range of purposes. But isolating causal neighborhood effects is still necessary for testing many theories and guiding policy design.

Sampson argues “selection is not a ‘bias’” but rather “another form of neighborhood effect” (p. 29). He shows that attributes of origin and destination neighborhoods are more predictive of mobility patterns than are individual attributes. But individual heterogeneity must still matter, because within any neighborhood, only some people choose to move out (or in). Data from MTO show that nonexperimental estimates of neighborhood effects on outcomes can sometimes get the sign, not just the magnitude, wrong.
While I would amend Sampson’s point to read “selection is not just a ‘bias,’” the observational and experimental literatures do seem to agree about the importance of neighborhoods for crime and health in particular.

Sampson says he hopes this work “will inspire a new generation of research on neighborhoods, social life, and the city” (p. 359). I would be surprised if his ambitious book did not succeed in this regard. I believe the next wave of advances will come from exploiting changes in policies or other social factors that create exogenous variation in neighborhood contexts (“natural experiments”) and additional randomized experiments.

Many of the limits of experiments hold for observational research also; they are just less obvious. For example, one concern with experiments is that different people may respond differently to changes in neighborhood contexts, and that the response might depend on the underlying reason that people experience a change in neighborhood context. This makes people nervous about the generalizability of experimental findings. But the variation in neighborhood contexts captured by observational data sets comes from a hard-to-measure mix of causes and so captures a mix of different cause-of-move-specific effects. If we are interested in the effects of changing neighborhoods due to a given cause, an experimental test of moves due to that specific cause could turn out to be more “externally valid” than observational studies.

Sampson is right that the possibility of spillovers means there are important limits to studies of individuals. He calls for more neighborhood-level experiments, but these can be expensive and complicated. An alternative would be to make greater use of what Jeffrey Kling, Sendhil Mullainathan, and I call “mechanism experiments” that can provide a more feasible way to learn about spillovers and other issues (“Mechanism Experiments and Policy Evaluations,” *Journal of Economic Perspectives* 25 [2011]: 17–38).

The book also raises important questions about how to help disadvantaged populations. Sampson endorses intensive community-level interventions like Choice Neighborhoods and Promise Neighborhoods, although these are expensive and so may never be done at scale. Moreover, if these programs work they make neighborhoods more desirable, which means many of the program benefits go to less-poor households that can afford higher rents and move in.

An alternative, perhaps more scalable, approach is to stimulate the capacity of communities for collective efficacy. Learning more about how to do this would be very valuable. I could also hardly agree with Sampson more that social policy has not paid nearly enough attention to how crime harms the poorest Americans. Like William Julius Wilson’s *Truly Disadvantaged* (University of Chicago Press, 1987), it is possible that future empirical research will not support every single hypothesis raised in Sampson’s important new book, but it is also hard to imagine that anyone who works on neighborhood effects could (or should) not read this.