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Editor's Remarks
MIXING PAST AND FUTURE

The Past

Scholarly reputations that for a time seem unimpeachable now shrink or evaporate so speedily that "intergenerational" perceptions of whom to read will soon be measured in months rather than decades. As some theorists have begun to explain (e.g., Hassan 2012; Rosa 2013), to remain hip in any single intellectual or artistic field becomes ever more *taxing*, more and more breathless, with *returns* that seem weaker and less predictable than one might wish. Where to *invest* one's time and energy for the purpose of edification becomes as tricky and unsettling as trusting hedge fund managers to *grow* one's retirement savings. Who are the *blue chip* writers and thinkers with whom one can confidently *spend* those few hours per day, week, or month that can be wrested from the everyday duties of emailing, meetings, and online surfing? Whose *stock* holds its value?

An expert in marriage and the family recently said to me, "Ernest Burgess wrote an early book about the family, and it's humbling to examine it now, because he was doing all the stuff we continue to do, the correlations and so on, even if his methods were less fancy" (see Burgess and Locke 1945; 4th edition, 1971). So, does one burrow into what's left of the library stacks to find Burgess's 800-page textbook, or even Willard Waller's related monographs from the 1930s, in order to limn the ground floor of family-oriented sociology? Or does one allow oneself to believe that the latest monograph subsumes these antique works, consigning them permanently to the airless forgetfulness of closed stacks? No-one can say with any authority; things are moving too fast to judge.

Today the most visible "sociological imagination" in journalistic form seems to revolve around columnists of *The New York Times*, sometimes Paul Krugman, other times Maureen Dowd or their colleagues, but mostly David Brooks. His right-center cultural conservatism notwithstanding, Brooks regularly invokes social science research in his essays, and because of the internet, his version of what "we" are saying is very widely discussed. Widely, yes; how deeply remains the unanswerable question. Yet long before the ascension of globalized discourse, before the tyrannical computerization of consciousness had reached deep into our minds, prior to the nattering of tiny screens demanding perpetual attention, there was a journalist whose presence was so overwhelming for so long that he became indistinguishable from "what right-thinking people ought to think about important matters." His name, of course, was Walter Lippmann.

In 1943 James Thurber created a cartoon for *The New Yorker* showing a couple reading their morning newspaper, the woman saying to the man "Lippmann scares me this morning" (Steel 1981: 432ff). She was "scared" because Lippmann was famously trusted to tell the unpleasant truth, as a sociologist without portfolio, political scientist, moral philosopher, and presidential advisor. For decades he was the voice of apparent reason in the popular press—his audience numbered ten million of the savviest citizens—and his books were taken as seriously as any scholar's, and far more widely read: "Walter Lippmann had left his fingerprints on, and even helped mold, almost every major issue in American life over six decades" (Steele 1980: xvii, xii). Yet he, too, has now become for most readers merely a somewhat notable name without content or any compulsion to consult. The fact that his books—most notably *Public Opinion* (1922), *The Phantom Public* (1925), *A Preface to Morals* (1929), and *The Good Society* (1937)—were universally read and respected, going through many printings and editions long after their first appearance, no longer seems to carry much weight. Even *The Essential Lippmann* (1963; 1982) is out of print. Were it not for Transaction Publishers reprinting his works "on demand," Lippmann's books would be hard to find except in vanishing used-book stores. At his death in 1974, this situation would have seemed as unlikely as if the names of all *The New York Times'* current columnists were to disappear overnight.

Every U.S. president from Teddy Roosevelt (Lippmann's hero when young) through Nixon knew and listened to the scholar-journalist. In December 1917, at 28, he drafted *The Fourteen Points* which Woodrow Wilson delivered to Congress on January 8, 1918, thereby establishing a new world order. From then on, he was never far from the political center of the United States, and also abroad. Fifty years later, the relationship Lyndon Baines Johnson and him went from being confederates to enemies when Lippmann realized that LBJ had lied to him regarding his intentions in Vietnam. He instantly began to attack LBJ frontally and continuously: "There is a growing feeling that Johnson's America is no longer the historic America . . . it is a bastard empire which relies on superior force to achieve its purposes, and is no longer an example of the wisdom and humanity of a free society. . . . It is a feeling that the American promise has been betrayed and abandoned" (*Newsweek*, October 9, 1967; Steele 1980: 577). LBJ allegedly told his intimates that he knew the war in Vietnam was lost when Walter Cronkite withdrew his support on his television news program; he could as easily have said the same thing about Lippmann, and probably did. In fact, Cronkite was a journalist with a mellifluous voice whereas Lippmann was a thinking person's public intellectual whose opinion counted a great deal more, and had for 40 years.

In 1932 when Lippmann was 42, he was invited to deliver the commencement address at Columbia University, which was soon thereafter printed in *The Atlantic Monthly* with this note from its editor: "This paper is here published because its excellence demands for it a wider audience and the permanence of the printed word" (Lippmann 1932). Lippmann was personally unhappy at that time, only in part because the country had rapidly sunk from the hysterical boom of the late 1920s into the nadir of the Depression in just a few years. But that notwithstanding, he had advice for the Columbia graduates which, when compared with what now passes for commencement addresses (see a sample in *The New York Times*: Pérez-Peña, 2013), remains a classic document of its genre.

Lippmann begins by noting "a special uneasiness which perturbs the scholar. He feels that he ought to be doing something about the world's troubles, or at least to be saying something which will help others to do something about them. The world needs ideas: how can he sit in his study. . . . And yet, at the same time he hears the voice of another conscience, the conscience of the scholar. . . he must preserve a quiet indifference to the immediate and a serene attachment to the processs of inquiry and understanding" (Lippmann 1932: 148). He then quotes from one of Robert Browning's least melodious poems, "A Grammarian's Funeral" (*Men and Women*, 1855). Doing so nowadays would seem not only pretentious, Eurocentric, patriarchal, and condescending, but would also mystify its listeners due to its syntax and vocabulary.

Lippmann reminds his listeners, most of whom had surely read Browning at some point, that "as a man of his time he [the scholar] is impelled against his instincts to enter the arena . . . he is afraid to say with the high assurance of the Grammarian: 'Leave Now for dogs and apes! Man has Forever.' He drops his studies, he entangles himself in affairs, murmuring to himself: 'But time escapes: Live now or never!'" Naturally, there is nothing original here, as Lippmann knew, since Plato's Socrates spent lots of his time talking about the thought/action dichotomy that afflicts all responsible people. Yet given what the world was preparing to do to itself in 1932, and being a man of uncanny political sense, Lippmann felt the need to speak against the grain—Max Weber's speech to a displeased audience in November, 1917, "Science as a Vocation," comes to mind, also disappointing its listeners' expectations—by insisting that scholars should stick to their business and let the politicians fend for themselves. In saying this, he seemed to be contradicting the arc of his entire life to that point.

Lippmann decided to use Browning's imagination to aid him, which was a good choice indeed. Browning's meditation on the burying of an imaginary Renaissance scholar is partly heartfelt, partly mocking. His students must find a suitable burial site:

"Leave we the unlettered plain its herd and crop; / Seek we sepulture / On a tall mountain, citied to the top, / Crowded with culture! / All the peaks soar, but one the rest excels; / Clouds over come it." The poet acknowledges the scholar's superiority to those who people the "unlettered plain" while at the same time noting his anxiety—which Lippmann adopted as his own—regarding his proper role when amidst ordinary humans in action, those outside the walls of his monastery or library. The scholar, "When he had gathered all books had to give!" stood perplexed before life's call for activity: ". . . this in him was the peculiar grace. . . . That before living he'd learn how to live— / No end to learning."

Petrarch might well be the model here, the man who "invented" the Renaissance by sleeping no more than four hours a night and working constantly on ancient texts. He was not unlike Erasmus, Leon Battista Alberti, or many other maniacally dedicated rediscoverers of lost texts whose principal goal in life was the codification of this buried learning. Petrarch himself discovered Cicero's forgotten *Letters to Atticus*, and wrote many letters to "Tully," though his hero had died 1200 years before. The "scholar-hero" seems a much sillier notion now than it did in the mid-nineteenth century (when Mark Twain observed Theodor Mommsen, Roman historian, being treated like royalty by the admiring throng). Yet the problems which confronted scholars then, at least in Browning's imaginings, were not so different from those facing Lippmann's Columbia young audience 80 years ago, nor ours today. As the poet put it: "Others mistrust and say, 'But time escapes: / Live now or never!' / He said, 'What's time? Leave Now for dogs and apes! / Man has Forever.' / Back to his book then: deeper drooped his head."

The pull of *Now* has never been so strong, the Call of the Tweet in combat with the solitary mindset necessary to the *studiolo*—those fortunate enough to inhabit one. "Back to his studies, fresher than at first, / Fierce as a dragon /. . . Oh, if we draw a circle premature, / Heedless of far gain, / Greedy for quick returns of profit, sure / Bad is our bargain!" Does the scholar give an interview to NPR before all the data are analyzed, in eager anticipation of notoriety and esteem, or does s/he wait for a thorough viewing of the data, meanwhile fearful that a competitor will "scoop" the story? Remembering, as we all do, that Darwin waited 20 years before announcing his discoveries in *Origin of Species*, and then only reluctantly, we see the distance we have come since his time and Browning's in terms of the hastening race between speed and cogitation. Darwin's feeble body made a virtue of necessity: "[H]e had neither the strength nor the temperament for an active and public life; he remained secluded at his country house at Down, shunning the furious post-*Origin* controversies and leaving the defense of 'Darwinism' to his more pugnacious friends. But always he worked. Good Victorian that he was, he worked as much every day as his strength permitted, and his industrious life was studded with solid contributions to science in articles, reviews, and books" (Appleman 1970: xiii-xiv). It is noteworthy that Browning's poem and Darwin's book appeared within a few years of each other. "This man decided not to Live but Know" is the poet's verdict on the renaissance scholar's life, and one which much troubled Lippmann. After all, everyone in Mass Society is pushed to want their Fifteen Minutes, and if one can win Fifteen Years, all the better. Where does this leave the undeniable tedium out of which the best scholarship springs?

The Future

The ASA will be selecting several new journal editors in December 2013 and January 2014, including my successor at *CS.* Nominations will be solicited, as always; proposals will be submitted by November 1; the Publications Committee will deliberate in December and make its recommendations to the ASA Council, which will decide in February 2014 who will take over in January 2015. The minimum term of service is three years, or 18 issues of the journal, which can be extended to a maximum of six years at the invitation of the ASA. To help potential candidates for the *CS* editorship consider what is in store for them if they decide to apply, let me quickly run through the operations of the journal, stipulating what is required in order to produce a new issue every two months.

Here are the gross facts: Each year 1300 to 1400 new books arrive in the *CS* mailbox for review consideration, which comes to about 25 per week, or five new books each working day. Of those, perhaps 500 will be reviewed based on the editor's selection criteria and, to a much lesser extent, to suggestions from the Editorial Board and the opinions of the *CS* staff. Each book will be treated either by means of a symposium, a titled review-essay, a regular review, or a "reviewlet" of 250-600 words for the "Briefly Noted" section. Each issue of *CS* generally contains a half-dozen essays of one kind or another, plus 40-60 regular reviews, and 20 reviewlets, in addition to "Publications Received" by category, and an index. This setup varies if longer "critical-retrospective" or other "special essays" are published. Standard reviews are 900-1100 words; review-essays are 1500-3000 words; critical-retrospective essays can run anywhere from 4000 to 7000 words or more. I have also introduced "Editor's Remarks" for each issue in which I write about a topic of interest, the smallest being about 1000 words, the largest going to around 7000. *CS* is allowed 916 total pages of printed material per annum, which comes to about 150 pages per issue.

All this material must be managed correctly or chaos takes over. To this end the ASA hired a programmer to customize Microsoft's Access program, which was done while the journal was at UC/Irvine just prior to my editorship. Not having been updated since, it is slower than is desirable, and despite recent professional tweaking, does not offer the amenities or speed one would expect from modern manuscript control software. Unlike the other ASA journals, we do not use the Sage platform because *CS* is not a conventional journal. Regular journals might publish the work of a half-dozen authors per issue, whereas we must deal with at least 80, sometimes more. No journal is easy to run properly, but *CS* poses special conditions that its software needs to accommodate.

The Editorial Board is made up of about 36-40 scholars, carefully chosen by virtue of their scholarly reputation and understanding of books, their gender, race, geographical location, academic rank, and willingness to work hard. One third of their number rotate off each December and must be replaced, an important process. Every two months the *CS* staff sends to each E.B. member a list of books along with summaries of each taken from publishers' websites, and asks them to nominate as many likely reviewers as possible for each title. After their responses have been received and tabulated, the editor rank-orders the nominated reviewers, sometimes adding names, and the staff begins issuing invitations to potential reviewers. It is unusual to receive a "yes" from the first, second, or third nominee, though we have found during our editorship that scholars have become more willing to review than was earlier the case. We have cases on record of having asked 15 scholars to review a given book, after which we gave up, or finally were able to find someone through our own devices.

Every editor develops their own criteria for deciding which books to review and at what length, culled from all those which are sent for consideration. Whenever an assistant professor publishes a monograph on a sociologically significant topic with a reputable press, their book is reviewed in *CS*, and if a review does not appear, it is because a string of potential reviewers failed to submit a usable analysis. Senior sociologists' books are also routinely reviewed, particularly when a publisher known for their sociology list sends the book. Edited volumes are harder to judge and harder to place with reviewers, but many are published nowadays, especially in the United Kingdom, and each must be evaluated carefully to see how many contributors are sociologists and the extent to which the chapters add to sociological knowledge. When books written or edited by political scientists, anthropologists, urbanists, geographers, cultural studies specialists, historians, or experts on education are sent to the journal's office, they are carefully evaluated along similar criteria. Textbooks and second or third editions of a monograph are not generally reviewed unless they offer material not otherwise available.

I have assembled a file of about 80 publishers' publicity department contact persons (who change with alarming frequency), and I write to them often, asking for books I think we should consider reviewing. Twice a year I go through about 50 publishers' catalogues, printed and online, selecting books for review. (Recently some publishers have stopped printing catalogues and rely on their electronic advertisements to inform journal editors of forthcoming works, but most continue to issue semi-annual printed catalogues, which is highly preferable.) Most publishers are easy to deal with and pleased to work with *CS*. Some require more nudging. We have had less luck securing review copies from Europe.

Every two months the complete manuscript is sent electronically to Sage Publications in California where some fine people begin its production. Typesetters in India then go to work, a proof is generated within a few weeks, the *CS* staff must proofread the issue carefully, corrections are returned to India via Sage, a second proof set is generated and proofread, and after final approval, the journal is printed in North Carolina. The point here is that the staff is constantly working on at least two issues simultaneously, and often more, due to overlapping deadlines.

The *CS* editorial operation requires space (we have two small, adjacent rooms in the sociology building), two computers, two printer/copiers, bookshelves; a large enough graduate program to facilitate the writing of 120 unsigned "reviewlets" each year; nearby faculty who are more or less willing to help out on occasion; two smart, literate, and energetically conscientious senior graduate students who work as "Editorial Associates," and two sociology honors undergraduates who are "Editorial Assistants." A Managing Editor who copyedits everything that goes into the journal is essential, preferably one whose English language ability is top-flight, and who also is familiar with social science jargon. Relying on mechanical, amateur, or geographically distant copyediting invites endless troubles. Finding an affordable artist to provide original works for the cover will benefit the journal's public face. *CS* also requires an editor who is willing to put other scholars' work ahead of his or her own because the journal's bimonthly schedule does not allow for much downtime.

Perhaps needless to say, there are always vexing questions of ethics and aesthetics in deciding which books to review and which reviewers to secure, many of them far more time-consuming to resolve than one would imagine *a priori*. Naturally, great satisfaction lies in publishing well-constructed and entertaining reviews or essays about valuable books. Considerable energy goes into making this happen routinely, and the results are worth every bit of it.

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SPECIAL ESSAY

"What is Critical Realism? And Why Should You Care?"

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A Realist Theory of Science, by **Roy Bhaskar.** London, UK; Verso, 1975. 258pp.

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meta-Reality: The Philosophy of meta-Reality, by **Roy Bhaskar.** Thousand Oaks, CA: Sage Publications, 2002. ISBN: 9780761997153.

Reflections on meta-Reality: Transcendence, Emancipation and Everyday Life, by **Roy Bhaskar.** Thousand Oaks, CA: Sage Publications, 2002. 274pp. ISBN: 0761 996923.

From Science to Emancipation: Alienation and the Actuality of Enlightenment, by **Roy Bhaskar.** New York, NY: Routledge, 2011. 424pp. $135.00 cloth. ISBN: 9780415696593.

The Philosophy of MetaReality: Creativity, Love and Freedom, by **Roy Bhaskar.** New York, NY: Routledge, 2012. 370pp. $45.95 paper. ISBN: 9780415507660.

The Formation of Critical Realism, by **Roy Bhaskar** and **Mervyn Hartwig.** New York, NY: Routledge, 2010. 237pp. $45.95 paper. ISBN: 9780415455039.

Dictionary of Critical Realism, edited by **Mervyn Hartwig.** New York, NY: Routledge, 2007. 534pp. $195.00 cloth. ISBN: 9780415260992.

Critical realism (CR) is a philosophical system developed by the Indo-British philosopher, Roy Bhaskar, in collaboration with a number of British social theorists, including Margaret Archer, Mervyn Hartwig, Tony Lawson, Alan Norrie, and Andrew Sayer.[1] It has a journal, a book series, an association, an annual meeting and, in short, all the usual trappings of an intellectual movement. The movement is centered in the UK

[1] Bhaskar's is not the only "critical realism." For the genealogy of the term and broader history of CR, see the cognate entry in Hartwig.

but has followers throughout Europe, Asia, the Americas, and the Antipodes.

If CR is less known in the United States, then *Contemporary Sociology* bears at least a little of the blame. While it has reviewed works by other critical realists, it has not reviewed any of Bhaskar's books for over thirty years. Why Bhaskar's writings have received so little attention from this journal, and from American sociologists more generally, is an interesting question in and of itself, though not one that I can pursue at length here. Suffice it to say that style and relevance are not generally at issue. Most of Bhaskar's writings are clear and lucid—far more so than, say, most of Habermas'. Also, like Habermas, and unlike most philosophers of science, Bhaskar takes sociology seriously. In short, Bhaskar's work is both readable and relevant.

At the present juncture, Bhaskar's work also seems enormously prescient. Realism is making a major comeback in philosophy and sociology these days. Everywhere, one hears realist phrases like "causal mechanisms" and "social ontology." Why? The shortcomings of positivism and empiricism are old news by now. Strong forms of interpretivism and constructivism seem equally problematic. Realism seems like the only way forward if one wishes to call off the search for "general laws" without simply abandoning the goal of causal explanation.

The dominant form of sociological realism at the moment is "analytical sociology" (Hedström 2005; Hedström and Bearman 2009). It is best understood as a type of *conventional* realism. "Conventional" in what sense? Analytical sociologists argue that "individual actors" are the elementary particles of social science, the inner workings of the "black box." There is just one problem: their black box has a false bottom. After all, why should reduction stop with "rational actors"? Why not reduce actors to "brains"? Or brains to "neural pathways"? Reductionism is a never-ending regress.[2] The only possible reason for stopping at the level of the actor—the one that analytical sociologists themselves give—is the (putative) conventions of the social sciences. But this convention has never obtained in sociology and no longer holds in economics either, where psychological and neurological reduction is the new order of the day.

By contrast, CR is realist "all the way down"—and all the way up as well. Instead of a purely conventional distinction between "micro" and "macro" it appeals to the real *ontological* distinctions between the various layers or "strata" in the natural and social worlds. It does not deny that reduction can sometimes be illuminating, but it insists that the social is an emergent reality with its own specific powers and properties (Powell and Padgett 2012; Sawyer 2005). For Bhaskar, then, ontological stratification and emergent properties provide the epistemological warrant and the disciplinary raison d'être, not only for the social sciences, but for the various biological and physical sciences as well.

This is not to say that CR presumes that the structures of reality are somehow self-evident or even directly observable. Critical realism is not naïve or commonsense realism. Even intentionally constructed social structures such as formal organizations or legal codes often have unintended effects that may not be evident to the social actors themselves. Moreover, non-intentional social structures such as fields and networks and culture can usually be observed only indirectly via their causal effects with the help of social scientific instruments (e.g., block modeling and correspondence analysis). Thus, a genuinely scientific realism is necessarily a critical one, which continually reflects on and revises its own categories and instruments. Its ontology is provisional and fallible. As should be clear, CR is a much more internally consistent and philosophically developed framework for those who have decided to follow the "realist turn" away from positivism and constructivism.

In the remainder of this essay, I will provide a brief introduction to CR by way of an omnibus review of Bhaskar's work since 1980. The essay is in three parts. The first situates Bhaskar's approach within the modern philosophy of science and within modern philosophy more generally, with special attention to positivism, interpretivism, and constructivism, the three traditions that

[2] For philosophical critiques of materialist reductions of agency and mind, see Kim 2010 and Nagel 2012.

(still) frame the debate about method. The second part discusses the three main phases of Bhaskar's intellectual development: (1) "basic" or "original critical realism" (BCR, a.k.a, "transcendental realism" or "critical naturalism"); (2) "dialectical critical realism" (DCR); and (3) "transcendental dialectical critical realism" (TDCR or "metaReality") with particular attention to BCR and DCR. The conclusion notes several areas where Bhaskar's system requires further development and highlights several contrasts between CR and the conventional wisdom in contemporary sociology.

Why Should You Care about the Philosophy of Science?

It is important to stress from the outset that CR is not itself a theory of society. It is a philosophy of science, a theory of what (good) science is and does. So, why should an empirically-oriented social researcher even care about it? Does the philosophy of science really have any impact on our research practice? Like Bhaskar, I believe that it does, or at least that it has, and that the impact has so far been mostly negative.

Let me open my case by means of an analogy. In my view, the relationship between the objects of science, the philosophy of science, and the practice of science is a bit like the relationship between the properties of water, theories of hydrodynamics, and the practice of swimming. Good swimming necessarily involves good hydrodynamics: water is a fairly resistant medium, and a "good swimmer" has learned to glide through it. However, there is no necessary relationship between being a good swimmer and having a good knowledge of hydrodynamics. One can have good practical knowledge of swimming without having a good theoretical knowledge of hydrodynamics, and vice versa. Still, an accurate knowledge of hydrodynamics might be a useful propaedeutic for the would-be swimmer. More importantly, an *erroneous* theory of hydrodynamics could make it harder to learn swimming. Imagine if the most influential school of swimming instructors was continually advising their charges to "get as big as possible" in the water. Imagine that a rival school argued that water is actually a two-dimensional medium which creates no resistance, while a third school contended that water resistance is a mere projection of the human mind. A few naturally talented people might still become good swimmers by dint of good instincts and much practice. They might even pass this practical knowledge on to younger swimmers, bypassing the official instructors. But they would do so in spite of the instructors, not because of them. On a practical level, they would have a lot to unlearn.

On Bhaskar's account, the relationship between the properties of social reality, the dominant philosophy of science, its two major rivals, and the practice of social research is analogously (dis)ordered at present. The dominant philosophy is positivism. Its oldest rival is interpretivism. The young upstart is social constructivism.[3] The three approaches are premised on very different social ontologies (i.e., theories of social reality). Positivists draw no ontological distinction between natural and social entities; both are just "phenomena" or "objects of experience" (Hempel 1965; Popper 1959). Interpretivists draw a sharp line between the two domains; they argue that social reality is linguistically constructed (Geertz 1973; Winch 1958). The constructivists go further still. They see the natural sciences as linguistically constituted as well (Feyerabend 1975; Rorty 1979). For them, the natural sciences are just another realm of social life.

The ongoing quarrel between positivists, interpretivists, and constructivists did not prevent social scientists from producing valuable research. Our knowledge of social life is broader and deeper than it used to be. More places, periods, peoples, and cultures have been studied. New forms and levels of social structure have been discovered. We have learned to glide through social reality with some skill. But this progress has

[3] This is not to say that these are the only schools of thought today; nor is it to claim that all or even most social scientists would affirm the central tenets of positivism and interpretivism. Rather, it is to say that positivism is the dominant form of orthodoxy and interpretivism the dominant form of heterodoxy, and that most social scientists position themselves methodologically in relationship to them, even if only tacitly.

occurred partly because social researchers have unlearned things they were taught in graduate school and passed this tacit knowledge of good research practice on to others.

Still, these teachings are hardly irrelevant. First, they make it that much harder for young sociologists to learn how to do good work. Second, they make it that much harder to understand what makes the good work good in the first place. Finally, they create unnecessary animosities and misunderstandings between different methodologies and subfields. And when push comes to shove—when editorial boards and tenure committees weigh in, and final decisions are made about publications and positions—it is to the positivist standards that we must all still appeal.

To see why this should worry us, let us now drill down a little bit deeper into underlying assumptions of positivism and interpretivism to see just how mistaken they are. Positivism presupposes and indeed *requires* that scientific knowledge take the form of ''general'' or ''covering laws''—universal and exception-less statements that enable us to predict and control events. If and only if there are such laws will a ''falsificationist'' method apply (Gorski 2004). Otherwise, a single counter-instance will not be logically sufficient to refute a theory.

Strong versions of interpretivism have (wrongly) accepted the positivistic account of natural science but (rightly) insisted that it does not obtain for the social sciences. Natural life may be governed by laws, they counter, but social life is governed by meanings. Thus, they conclude, the aims and methods of the social sciences are radically different from those of the natural sciences. The social sciences pursue idiographic knowledge by hermeneutic means. They do not attempt to explain what happens in the social world, only to render it comprehensible by reconstructing meaning and intention.

Strong versions of constructivism pushed this argument even further; they agree that social life is linguistically constituted. But they believe that natural science is just another part of social life—that it, too, is governed by (impersonal) ''discourses'' and ''powers.'' On this view, there is no real difference between, say, sociological and literary theory, or in some extreme formulations, between quantum physics and Azande magic. In other words, social constructionists embrace a very strong form of epistemic relativism. They say we are all so enmeshed in our own particular set of ''stories'' and ''language games,'' that there is just no ''real'' or ''neutral'' basis for adjudication between them. The most we can ever hope for is to disentangle ourselves from the web of textuality and weave one of our own in solidarity with our friends. Thus the goal of ''theory'' (social, literary, etc.) is simply to ''destabilize'' or ''subvert'' ''discourse'' and ''power'' and so to create space for individual ''autonomy'' and collective ''performance.''

The problem with all three of these positions is that few social researchers would be seriously willing to defend any of them nowadays.[4] Consider the positivist ideal of covering laws. How many sociologists still think this is an attainable goal? Not many quantitative researchers do, at least not in their actual research practice. A near-perfect correlation between two variables is not a discovery; it is a mistake. A few holdouts in the quantitative camp once tried to defend a ''probabilistic'' version of the covering law model, based on quantum mechanics, which would ultimately aim at specifying the exact, numerical likelihood of certain events (Lieberson 1991).Will they eventually work out the relationship between fathers' and sons' social status to two decimal places? Don't hold your breath.

Now consider the interpretivist ideal of hermeneutic recovery. Again, how many cultural sociologists or sociological ethnographers honestly think that this exhausts the possibilities of social science—that the

[4] Interestingly, until recently, the loudest defenders of logical positivism were neoclassical economists along with some of their rational choice followers. The more strident among them claimed that human rationality was a sort of law. And perhaps it is. But in what sense? Not the positivist one surely. The ''rationality assumption'' is not a covering law. It does not yield precise predictions of the sort that the law of gravity yields for celestial mechanics. It is really just a stylized description of a behavioral propensity one finds among normal, healthy human beings—and one whose generality has been rightly called into question by behavioral economics.

people down the hall who study stratification and networks and so on are just wasting their time? Indeed, how many of them really believe that it exhaustively describes what they themselves do? Everyone knows that good interpretive work always involves various forms of "contextualization." And rightly so, because the reasons for what people do can be different not just from the reasons they *do* give but even from the reasons they *could* give. We are not fully transparent to ourselves, nor is the social world fully transparent to us. Otherwise, what would be the point of sociology?

To their credit, social constructionists have recognized these complex intertwinings of power, language, and reality. They know that researchers do not just give up on their theories because of a single anomaly. They know that our observations of the world are linguistically mediated. They know that social structures partake of linguistic ones. Alas, they often turn these complex intertwinings into a simple chain of causality, such that reality is (solely) constituted by language and language is (merely) a medium of (an impersonal) power. Taken to its logical consequence, a strong version of social constructivism ultimately leads to conclusions like the following: (1) human agents are ventriloquist's dummies for discursive powers; (2) social and natural reality are mere epiphenomena of human language; (3) human language is governed by an ethereal, omnipresent and impersonal medium of "discourse" or "power."

But these positions generate grave "performative contradictions": the very fact that people can propose them at all immediately undermines them. Because if they were true, then it is not clear how anyone could freely choose to become a social scientist, how social science could generate knowledge, or to what use this knowledge could ever be put. And that is perhaps why strong versions of social constructivism have really only achieved a marginal presence within the social sciences (e.g., in some corners of cultural anthropology and cultural sociology) and have mainly taken root in interdisciplinary programs and humanities disciplines (e.g., cultural studies and science studies, and in language and literature departments).

As different as the positivist and interpretivist visions of social science may be, Bhaskar notes, both actually share a common understanding of natural science. Unfortunately, Bhaskar adds, this understanding is quite mistaken. In reality, even the physical sciences do not actually generate "covering laws" of the positivist sort (Cartwright 1983). Nor is scientific knowledge based on a passive observation of empirical events. What the natural sciences mostly do is isolate causal mechanisms by means of active interventions into the world (a.k.a. "experiments") that produce indirect observations of the world (via "instruments") (Hacking 1983). Note that the one major exception, astronomy, merely proves the rule: it seems to involve a closed system, namely, the universe.

One reason why social scientists have been unable to discover any "covering laws" is that they cannot achieve experimental closure. There are others as well. Because social structures are dependent upon human activity and culture, they vary over space and time to a far greater degree than physical structures. Human nature might seem an exception, whence the perennial appeal of methodological individualism in the social sciences. But evolutionary biology teaches us that a high degree of behavioral plasticity is a distinguishing characteristic of the human species. Furthermore, social structures are more than a simple aggregation of individual persons, whence the perennial failures of methodological individualism in the social sciences. Social structures also have inter-subjective (e.g., cultural) and material (e.g., artifactual) components. Further, they generally have emergent properties not possessed by individual actors. The genesis of the social sciences hinged on the discovery of emergence, and major advances in them have typically involved the discovery of emergent properties (e.g., of economic markets, social classes, collective conscience, value spheres, social fields, and so on). A fourth and final reason why human societies defy efforts at experimental closure is that human beings are themselves open systems capable of communication and creativity and resistance. This may be one reason why even the most ruthless efforts to control behavior by creating closed systems (e.g., totalitarian regimes and concentration camps) have ultimately foundered. And this is why experimentation will (hopefully) never play the sort of role in social science that it does in physical science.

At present, there is a yawning gap between the philosophy of social science and the practice of social science. The ghost of logical positivism still haunts contemporary discussions of methodology. The search for a body of ''covering laws'' has been abandoned but the spirit of ''falsificationism'' lives on in ''logic of inquiry'' seminars and undergraduate methodology texts. Interpretivists and constructionists have tried to exorcise it. In the process, however, they have pulled the rug out from under themselves, by denying the very raison d'être of the social sciences, namely, the possibility of causal explanations via social structures. Amidst all this confusion and tumult in the haunted house of philosophy, workaday researchers carry on calmly with their routines. Models are run, ethnographies are written, interviews are conducted, and archives are scanned. Some of the work is very good. Knowledge seems to grow. But no one really knows how or why. Except perhaps Roy Bhaskar.

Who is Roy Bhaskar and What is Critical Realism?

Roy Bhaskar was born in London on May 15, 1944. His father was a medical doctor of Indian background, his mother an Englishwoman and a nurse. Both were practicing Theosophists. In private, they lived an Indian lifestyle. For all these reasons, Bhaskar was something of an outsider in post-war England and was often persecuted at school, perhaps because he was also something of a prodigy. He attended St. Paul's public school, then Balliol College at Oxford, where he obtained first class honors in "PPE" (Politics, Philosophy and Economics") in 1966. Immediately thereafter, he began studying for a PhD in economics, with a focus on "Third World development." He became increasingly disillusioned with orthodox economic theory and gradually turned his attention to the philosophy of science, working closely with the neo-realist philosopher, Rom Harré.

In late 1971, Bhaskar submitted a dissertation entitled ''Some Problems about Explanation in the Social Sciences.'' His examiners rejected it. The official reason was excessive length. The real reason may have been excessive heterodoxy. Bhaskar's realist views were very much outside the analytic mainstream at the time. (They still are.) Undeterred, he continued working on the dissertation project for several more years. It was eventually accepted in 1974, despite the fact that it had grown to some six volumes by this time. Those six volumes were subsequently reworked into his first three books: *A Realist Theory of Science* (1975), *The Possibility of Naturalism* (1979), and *Scientific Realism and Human Emancipation* (1986). Together with *Reclaiming Reality* (1989), a collection of early essays, and *Philosophy and the Idea of Freedom* (1991), a book length critique of Richard Rorty's work, they comprise the first phase of Bhaskar's development, the ''basic'' or ''original critical realism'' which established his reputation. The publication of *Dialectic: The Pulse of Freedom* (1993), certainly Bhaskar's most difficult work, inaugurates a second and shorter phase in his intellectual development, known as ''dialectical critical realism.'' Most of the central ideas of DCR are presented in a shorter and more readable form in *Plato etc.* (1994), a collection of essays from the second period. The difficulty of *Dialectic* put off some readers. But DCR provides important tools for describing structural change, tools that are lacking in BCR and in other contemporary realisms as well.

An intensive engagement with the South Asian tradition of non-dualist metaphysics then led to the third and final phase of Bhaskar's work, ''transcendental'' DCR. Bhaskar has published four books on TDCR so far: *From East to West* (2000), *The Philosophy of Meta-Reality* (2012), *Reflections on Meta-Reality* (2002), and *From Science to Emancipation* (2011). The third-period works will not be of great interest to most social scientists, at least not qua social scientists, though they do provide some context for thinking about contemporary debates about religion and science, and spirituality and environmentalism. Bhaskar recounts his personal and intellectual development in *The Formation of Critical Realism* (2010), a series of interviews with Mervyn Hartwig. Hartwig has also edited the *Dictionary of Critical Realism*

(2007). While *FCR* provides an entrée for general reader, the *Dictionary* is meant for those with a strong knowledge Bhaskar's system.

The best introduction to BCR is Andrew Collier's *Critical Realism* (1994), while Alan Norrie's *Dialectic and Difference* (2010) provides a helpful introduction to DCR. It should be noted that Bhaskar's writings have an iterative character. Accordingly, the clearest and most compact discussions of BCR are found in the prefatory discussions introducing DCR, and those of DCR in TDCR. Thus, those interested in BCR will want to read some parts of *Plato etc.*, while someone interested in DCR will want to look at the early chapters of *From Science to Emancipation.*

Bhaskar's work is still best read in sequence. His first book, *A Realist Theory of Science*, mostly concerning natural science, is where he describes his approach as "transcendental realism." He means "transcendental" in the loosely Kantian sense that asks: "What would the natural world have to be like for natural science to be the way that it is?" It is "realist" in the generic sense that it takes a "mind-independent" nature as a fundamental "condition of possibility" for natural science.[5] But it is also realist in the "critical" sense that it sees science as a human activity that is inevitably mediated (if not determined) by human language and social power.

Now, as Weber noted a century ago, one of the basic characteristics of modern science is specialization and ever-increasing specialization. Why is this? No doubt, rival epistemologies and jurisdictional battles have played a role in the historical formation of the various disciplines. But is this the whole story, or even the main story? Bhaskar thinks not. If there is a hierarchy of disciplines, each charged with a certain scale or level of reality, then this partly reflects the real structure of the world itself. In other words, if reality can be successfully studied at a variety of different spatio-temporal scales, and if the physical and biological sciences are relatively autonomous from one another, then this is at least partly because nature is actually organized that way, into different strata and domains.

Another persistent feature of the history of science is campaigns of reduction. These campaigns can and do bring epistemic gains, especially when they discover new substrata. But they invariably fall short of their epistemic goals: to explain one strata of reality in terms of a lower-order one. Why? Because of "emergence." The combination and interaction of entities and properties at one level of reality generates "emergent" entities and properties at others (Miller and Page 2007). The textbook example is water. It has causal powers (e.g., to extinguish fires) that are quite different from those of its constituent parts (i.e., hydrogen and oxygen). Water tends to extinguish fires. Hydrogen and oxygen tend to accelerate them. The social world is rife with emergent structures of this sort. One task of the social sciences—their principal task really—is to describe their workings.

A third important feature of modern science—so obvious we might overlook it—is the growth of knowledge over time. How does this occur? Through the simple accumulation of facts? The relentless falsification of theories? Transcendental realism suggests different metrics. If nature is stratified and strata are emergent, then scientific knowledge will grow via the discovery of previously unknown strata (e.g., the quantum level), entities (the Higgs-Boson), and interactions between them (e.g., molecular genetics). But this cannot be the whole story. If it were, says Bhaskar, then the growth of knowledge would be "monistic" in form, that is, purely cumulative and continuous. Instead, as we now know, the history of science is full of leaps and breaks from Ptolemy to Copernicus, Newton to Einstein, Linnaeus to Darwin and so on—"paradigm shifts" in Thomas Kuhn's well-worn phrase. Bhaskar therefore draws a distinction between the "intransitive" and "transitive" dimensions of science, between a natural world as it really is and our changing concepts of it. This is

5 As such, it is opposed to: 1) skeptical methods involving radical doubt about external reality (e.g., Descartes' *Meditations*); 2) empiricist epistemologies which see scientific knowledge as built up out of sense impressions either via association (Locke) or induction (Hume); 3) transcendental idealisms which make causality into a feature of human understanding rather than of the world itself (i.e., Kant).

the difference between naïve and critical realism. CR understands that ontologies are fallible.

A fourth feature of modern science is that the causal laws it discovers do not seem to obtain in the sensible world as we experience it, even if we mostly experience this world as relatively orderly and intelligible. ''Reflect for a moment on the world as we know it,'' Bhaskar urges: ''It seems to be a world in which all manner of things happen and are done, which we are capable of explaining in various ways, and yet for which a deductively-justified prediction is seldom, if ever, possible. It seems, on the face of it at least, to be an incompletely described world of agents. A world of winds and seas, in which ink bottles get knocked over and doors pushed open, in which dogs bark and children play; a criss-cross world of zebras and zebra-crossings, cricket matches and games of chess, meteorites and logic classes, assembly lines and deep sea turtles, soil erosion and river banks bursting. Now none of this is described by any laws of nature. More shockingly perhaps none of it seems even governed by them. It is true that the path of my pen does not violate any laws of physics. But it is not determined by any either'' (95).

Should we therefore give up on the notion of causal laws altogether? Certainly, we should give up on the *positivist definition* of causal laws qua ''constant conjunctions'' between ''observable events.'' Bhaskar proposes that we define causal laws as ''normic statements'' concerning the ''powers'' and ''tendencies'' of particular ''agents'' or ''entities.'' It is these agents and powers—and not logical ''propositions'' about them—which are the principal objects of science (Groff 2008). Because there are multiple layers of agents and powers, moreover, observable events will have a ''laminated'' character; they are simultaneously governed by normic laws at various levels. This has important consequences for causal inference. The mere fact that a particular action does not violate a particular law does not mean that it is fully determined by it either. For example, the movement of my fingers across this keyboard does not violate any laws of physics or neurochemistry or English grammar or academic life. Rather, it is simultaneously and jointly determined by all of them. It is a ''laminated'' process. Good causal inferences will therefore depend less on the rules of logic than on our knowledge of structure.

In his second book, *The Possibility of Naturalism* (1979), Bhaskar introduces a helpful distinction between three ''ontological domains'': the ''real,'' the ''actual,'' and the ''empirical.'' He further clarifies the relationship between causal laws and observable events. The domain of the real consists of all the ''mechanisms'' that exist in the world, which is to say, of all the various levels and types of entities with their various powers and tendencies. The domain of the actual consists of all mechanisms that have been activated, even if they have not been observed. The domain of the empirical, finally, consists of all mechanisms that have been activated *and* observed. Note that the three domains are generally ''out of phase'' in everyday experience. The real purpose of a scientific experiment is to bring them ''into phase,'' and so to activate, isolate, and observe the powers and tendencies of a particular entity or strata. Thus, experiments really do reveal laws. But these laws govern entities, not events, they describe tendencies, not regularities.

The main focus of *The Possibility of Naturalism*, however, is the social sciences and their relationship to the natural sciences. Bhaskar characterizes his own view as ''critical naturalism.'' It is ''naturalistic'' insofar as it rejects any sharp divide between the natural and social sciences. It is ''critical'' insofar as it rejects any reduction of the social to the natural. As such, it avoids the pitfalls of interpretivism and constructivism. What pitfalls? Bhaskar argues that interpretivism and constructivism either explicitly presuppose or tacitly secrete strong forms of dualism.

In the case of interpretivism, this dualism is typically epistemological. Interpretivism presupposes that we can know things about persons that we cannot know about non-persons. For example, we can know a person's thoughts or intentions, but not a quark's. Bhaskar contends that this is really just a category mistake. The difference between quarks and persons is ontological, not epistemological. It concerns the specific properties and powers of quarks and persons, not the sort of knowledge we can have about them. Quarks may not have thoughts and intentions; but neither do persons have "spin" or "flavor." Is it easier to discern a person's thoughts than a quark's spin? Not necessarily!

What about constructivism? Typically, it secretes a dualistic view of human persons. On the one hand, it treats human persons as physical objects, sometimes tacitly, via metaphors of "writing," "inscribing," and "constructing" the body, sometimes explicitly as in the "eliminative materialism" of the early Richard Rorty (Rorty 1970). On the other hand, it imagines them as disembodied powers capable of "self-fashioning," "autonomy," "performance," and so on. How can these two views be squared with one another? In truth, they cannot, and have not been, since Kant. Critical naturalism does not generate this sort of antinomy. That quarks and persons should have quite distinctive properties and powers is no surprise for CR; they occupy altogether different strata of reality. Nor does the relationship between body and self or brain and mind create any fundamental or intractable problem for CR; one simply emerges out of the other.

The ontological difference between nature and society leads to an epistemological difference between natural and social science: we can only ever observe social structures via the activities and concepts of human beings or the material traces and artifacts they generate. (About this much, the interpretivists are right: the dream of social physics is an idle one.) Unlike natural reality, social reality is not independent of human minds. Note that the phrasing is precise. Social reality is independent of any *particular* human mind. The social map in an individual's mind will be partial at best and often mistaken, sometimes systematically so, because some social structures must be misunderstood to be reproduced. The accurate conceptualization of a social structure must be pieced together from the multiple perspectives out of which it emerges in the first place. Further, "independent of" does not imply "exhausted by." The elements of social structures also include material entities and artifacts, such as "arable land" and "administrative buildings"; they are not comprised solely of human persons. Conversely, and paradoxically, they may also include dead persons, whose agency and intentions "live on" in social structures through their mental and physical labor and creations. To say that social structure is not independent of human minds and activities is not to say "this mind" or "that activity" "right here" or "right now" (Archer 1995).

Critical naturalism also has important implications for the relationship between science and ethics (Gorski 2013). Since Weber, it has been customary to insist that "facts" and "values" can and must be kept apart. But Weber's claim that they must be kept apart derives from a highly relativistic and subjectivistic understanding of ethics. Values, he implies, derive solely from the choices of individuals and the contingencies of history. There is no other basis for them. Social scientists can therefore describe the values that people hold and explain how they are conditioned by history. And as citizens, they have a right to defend their own values, even an obligation to do so. But science qua science can say nothing about how we should live.

Was Weber right? Bhaskar does not think so. If we conceive of human beings as an evolved species with certain inbuilt needs and capacities, argues Bhaskar, then the biological and social sciences will indeed have something to say about how we should live, perhaps not in the imperative sense of a table of laws filled with "thou shalt nots," but certainly in the hortatory or prudential sense of "it would be wise to." While social science cannot tell us how to resolve the sorts of highly improbable moral dilemmas that academic ethicists like to fret about (e.g., the infamous "trolley car problem"), surely they can tell us something about what individual and social well-being look like, and how we might improve them. Specifically, they can contribute to our understanding of the social determinants of well-being or flourishing. CR thus leads to a weak form of moral realism. Let us call it "ethical naturalism."

But how can social science cross over the is/ought divide? One possible bridge, says Bhaskar, is "explanatory critique." As noted earlier, the reproduction of certain kinds of social structures may depend upon the production of distorted or inaccurate social beliefs. If one can demonstrate a systematic connection between inaccurate beliefs and oppressive social structures, then one has not only explained the beliefs but also supplied a motivation for changing the structures. One has made the leap from facts to values. Just what sorts of value judgments are warranted by social research is the subject of Bhaskar's third book, *Scientific Realism and Human Emancipation* (1986). Bhaskar does not pretend that social science alone "can determine or uniquely ground values" or that explanatory critique is sufficient in itself to motivate action, since "this is always a matter of will, desire, sentiment, capacities, facilities, and opportunities as well as beliefs." He is not claiming that the realms of fact and value are coterminous; merely that they overlap and interact.

We now come to the second phase of Bhaskar's system. Bhaskar's *Dialectic* is not exactly light reading; the prose is dense, the neologisms abundant, the diagrams perplexing. Interested readers may find it easier to begin with the essays in *Plato etc.* or with Alan Norrie's book. Either way, a good knowledge of basic CR is a definite prerequisite. But why the shift to dialectics? Like Hegel, Bhaskar believes that the movement of our thoughts should follow the movements of reality itself. He contrasts his dialectic to Hegel's. Hegel had three "moments": "identity," "negation," and "sublation" or "synthesis" (*Aufhebung*). Bhaskar has four dialectics: "non-identity," "negation," "totality," and "praxis." In the first moment, we grasp the distinctness of structures qua individuals, suspended in time. In the second moment, we grasp them as agents in interaction across time. In the third, we comprehend the relations within and between these agents in systemic terms. In the fourth and final moment, we reflect on what we have learned and decide how to act on it.

Bhaskar's dialectic supplies a general schema for thinking about change in the world. We begin by analyzing the world into discrete structures, such as "human persons" or "social networks." We proceed by thinking through how interactions between these structures lead to changes in their properties or relationships or even to the emergence of new structures. We then reflect on the temporal and spatial and cultural scope of these interactions as part of a system. Finally, we grasp our own thoughts and actions as part of that system and we see ourselves as agents who have some power to change the system. In Bhaskar's view, all learning has this basic form.

Perhaps this all seems like common sense. But it is not how social scientists are taught to conceive of change. The standard imagery is an empiricist one of two "observations" taken at two successive moments in time. Change between "T1" and "T2" is then conceptualized as "variation" along a "dimension" that is "abstracted" from the observations. This way of thinking about change is highly inadequate for two reasons. First, because it focuses on the empirical level, it obscures structural change and emergence at the level of the real, and conflates causality with generality. Second, by emphasizing the operation of "abstraction," it fails to specify its own context, namely of a particular system with internal relations and spatio-temporal boundaries.[6]

This is one reason why increasing numbers of social scientists now argue that explanations should invoke "causal mechanisms." As we have seen, Bhaskar was already making this case by the early 1970s, well ahead of Boudon, Elster, or Hedström. Since then, however, he has come to see the mechanisms concept as somewhat misleading and inadequate. It is potentially misleading, insofar as it suggests that the macro is driven by the micro, just as a clock's hands are moved by its mechanism (Gorski 2007). The reverse is also possible: a higher order mechanism can also drive a lower order one (as when a clock's hands are adjusted by a human hand). And it is

[6] The interpretivist and constructionist alternatives are no better. Because they tend to reduce social structure to individual interactions, interpretivists often reduce structural change to cultural change. Further, because they disavow explanation in terms of causality, interpretivists can only account for change in terms of intentionality (which they wrongly regard as distinct from causality). For its part, constructivism treats structural change as cultural "rupture." And it accounts for such ruptures either in terms of an impersonal "power" and/or a mysterious "contingency."

certainly inadequate, insofar as it implies structural stasis and repetitive motion.

Thus, dialectical CR is not just a better ''heuristic'' for thinking about change. It is also a more adequate *ontology* of change, a better account of the real forms and processes of change, and one that is more adequate to the radical implications of *emergence* than basic CR was. Basic CR has considerable difficulty moving beyond the first moment of change: the non-identity of extant structures. In part, this is because its theoretical imaginary is still too mired in Newtonian physics, with its causal imagery of small, hard, and indestructible spheres smashing into one another. Now, change to the properties of a structure (e.g., the motion of a particle) is certainly one form of change, but not the only one. Interactions can also lead to the dissolution of a structure (e.g., in a particle accelerator) or to the emergence of a new structure (e.g., in a chemical reaction). And this is just as true in the social world as in the natural world. This is the moment of ''negation'' in Bhaskar's dialectic. What is more, the emergence of new structures reminds us that structures exist in systems (e.g., a material substance or a cultural practice); conversely, the dissolution of old structures reminds us that structures are themselves systems (e.g., an atom is a system of elementary particles, just as a formal organization is a system of agents, rules, and artifacts). This is the moment of ''totality'' in Bhaskar's dialectic. Now, while these three moments may occur independently of any external observation, at least where natural processes are concerned, they can be internalized in the minds of human agents where they generate reflection and action—indeed, they must be so internalized (e.g., in scripts or habits) where social processes are involved. This is the fourth moment of the dialectic: ''praxis.'' In this way, observation is itself incorporated into the dialectic of change.

Conclusion

Bhaskar's system currently provides the best available starting point for anyone interested in a post-positivist and post-poststructuralist vision of social science. I say starting point, but not stopping point, because certain aspects of CR are not adequately elaborated in Bhaskar's own writings. Some of these issues have been taken up by other critical realists. For example, key theoretical questions concerning agency, structure, culture, and reflexivity are dealt with at great length in Margaret Archer's work (Archer 1988; Archer 2000; Archer 2003; Archer 2010). Key methodological questions concerning concept building, causal inference, research design, data gathering, and statistical modeling have been addressed at both introductory (Danermark 2002) and advanced levels (Olsen 2010), though much work remains to be done on this front. The relationship between the normative and explanatory facets of social science has also been addressed more extensively in several recent works (Ellis 2012; Sayer 2011). More philosophically oriented readers will find careful discussions of emergence, causality, laws, and powers in recent work by the ''new essentialist'' school of analytical metaphysicians (Groff 2008; Molnar and Mumford 2003; Mumford and Anjum 2011). Those curious about what an ethical naturalist approach to social and economic policy might look like will find some initial answers in the ''capacities approach'' advocated by Martha Nussbaum (Nussbaum 2006) and Amartya Sen (Sen 1999). Finally, questions about social ontology have also been addressed in several important works by social scientists and philosophers (Lawson, Latsis, and Martins 2007; Searle 1995; Searle 2010).

Taken together, these writings provide the philosophical warrant for a new self-understanding for the social sciences. In closing, let me highlight several areas where the critical realist vision transcends and clarifies the received wisdom. (1) *Causality*: In open systems, causality never manifests itself as a ''constant conjunction'' between events. Nor is causality in the social world exhausted by the intentions of social actors. Rather, causation derives from the powers of structures, whether natural or social. (2) *Agency*: There is no ''structure/agency problem.'' Human agents are bio-psycho-social structures with emergent powers of intentionality. Conversely, social structures have agency, an agency that transcends and influences the intentions of the individual agents that co-constitute them. The important problems are "structure/structure" or "agent/agent" ones. (3) *Explanation*: To explain something is to identify the structures and powers that produced it. "Laws" are statements about powers, not events. The fact that human persons have powers of rationality and intentionality does not logically "entail" much, if anything, all by itself. Explanations need not be general in form or ambition. (4) *Knowledge*: Scientific knowledge does not consist of "propositions" or "statements" about events or phenomena; rather, it is comprised of (provisional and fallible) descriptions of structures and powers. Ontology and taxonomy are more central to scientific progress than epistemology or generality. (5) *Values*: The social sciences are not "value-neutral." They presuppose an axiological commitment to human well-being. Social science cannot generate specific directives about how we should order our lives or societies. But it does produce prudential principles.

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SYMPOSIA

Commentary

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Theft of a Nation: Wall Street Looting and Federal Regulatory Colluding, by **Gregg Barak.** Lanham, MD: Rowman and Littlefield Publishers, 2012. 211pp. $29.95 paper. ISBN: 9781442207790.

The global meltdown of 2008 was influenced by flawed financial policies, law-breaking, greed, irresponsibility, and not an inconsiderable amount of concerted ignorance and outright stupidity. To date, the greatest attention regarding that criminality has focused on the $65 billion Ponzi scheme operated by Bernard Madoff, a scam that resembled the tactics of con men, not big-time corporate financiers (Sander 2009; Strober and Strober 2009). Prototypical corporate frauds such as those perpetrated by Wall Street behemoths American International Group (AIG), Countrywide, Lehman Brothers, and Bear Sterns have received much less attention (Bamber and Spencer 2009; Kelly 2009; McDonald and Robinson 2009; Michaelson 2009). These companies, whose balance sheets were saturated with securities containing subprime mortgages, collapsed, were bought by competitors, or were bailed out by the federal government with huge infusions of taxpayer money. For most onlookers, including criminologists and the public in general, the actions of the offending companies represented intricate and arcane business practices that were difficult to fully understand and to portray in sound bytes—and therefore they tended to become trivialized in regard to their criminal components.

The Great Economic Meltdown follows in the wake of the 1980s savings and loan crisis, and the corporate and accounting scandals in the early 2000s, each of which were the biggest financial crises in history at the time. One important question related to these debacles is whether or not fraud played a significant role in creating them. The question is important—not simply to lay blame or to punish wrongdoing, but to provide theoretical models and social policies that will prevent effectively future debacles. If fraud is a central component, then regulations and enforcement should directly take this into account. Not doing so would provide a virtual blueprint for future financial crises.

Economists, who have a much greater role in policy formation than other social scientists, minimize the issue of fraud entirely, and assume that it is of little or no consequence in financial markets. Neo-classical economic theory has dominated American policies for the last 30 years, a period that has seen three major financial crises. This perspective has resulted in an international intellectual stream that has increasingly led to the trivialization of elite white-collar crime, since one of the United States' most prolific exports is neo-classical economics. ''Law and economics'' corporate law scholars assert that ''a rule against fraud is not an essential or even necessarily an important ingredient of securities markets'' (Easterbrook and Fischel 1991:283).

Beginning with studies of the savings and loan crisis, criminologists identified a form of fraud that challenges such conventional understandings, and which was found to

CS would like to thank David O. Friedrichs for his help with the symposia on the two books, by Gregg Barak and by John Hagan.

be a significant factor in the largest financial institution failures. Best understood as crime *by* the organization *against* the organization itself, Calavita and Pontell (1990) labeled this looting of assets by controlling insiders as "collective embezzlement." Later, the term "control fraud" (Black 2005) was introduced to denote fraudulent acts by top executives who used the organizations they led for personal gain. Control fraud has played an integral part in recurring, widespread, and increasingly costly financial debacles. It results from errant policies that give rise to what have been termed "criminogenic" or "crime-facilitative" environments (Needleman and Needleman 1979). Endemic waves of control fraud act to hyper-inflate financial bubbles that inevitably result in major financial crashes (Pontell 2005).

In *Theft of a Nation*, Gregg Barak effectively argues that fraud was central to the current meltdown and carefully documents the phenomena described above. The book takes on the "fraud minimalists" and presents a formidable challenge to those economists and legislators who continue to believe that markets are self-regulating and that fraud plays no significant role in financial debacles. Using detailed historical analysis and major case studies he exposes how and why the American public was victimized by deregulatory policies designed to benefit large private firms and which simultaneously created a criminogenic environment in the housing and financial industries. Barak develops theoretical models of both organizational and institutional control fraud that integrate notions of system capacity and corruption. The institutional model includes "crimes of capitalist control" which are "beyond incrimination." This is an important contribution to the literature as it not only shows how major white-collar crimes are relegated to "non-issue" status (Goetz 1997), but also shows how the state selectively adopts the tactic of "damage control" over "crime control" in response to massive lawbreaking by elites.

The book, although plodding at times given the amount of data, moves easily from theoretical explanations to corresponding policy implications and necessary financial regulatory reforms. Barak also includes a separate chapter with a critical analysis of the 2010 Dodd-Frank Act, the most important and comprehensive legislation resulting from the crisis. He convincingly argues that while the law provides for many worthwhile reforms, it does not go far enough in terms of fundamental structural changes, leaving many loopholes for business, and shallow and toothless provisions regarding executive compensation and corporate governance.

Using Bernard Madoff's crime—the largest Ponzi scheme in history—as a highly effective contrast to what the banksters did on a much larger scale, Barak clearly demonstrates how Madoff's scam pales by comparison. While he was harshly (and justly) punished, there has not been a single major prosecution resulting from the most recent financial meltdown as of this writing. Barak argues that this result is "neither by accident nor conspiracy but mostly by consensus or collusion" (p. 96). Throughout various points in the book Barak makes references to "unenlightened self-interest" and "unfettered victimization" which accurately note characteristics of offenders and their activities. These observations may be part of a broader cultural phenomenon that Rosoff (2007:517), in examining the earlier Enron meltdown, labeled "psychopathic wealth." Unlike the corporate America of the past which was characterized more by a "patient wealth," the new corporate culture aspires to a different wealth. Borrowing a term from the psychiatric lexicon that is used to describe persons intensely selfish, conspicuously lacking in human empathy, and dispositionally unable to delay gratification, "We entered an 'Age of psychopathic wealth' – and the press hardly seemed to notice." As others have also noted, criminologists remain rather indifferent as well (Shichor, Pontell and Geis 2010).

The book also spends much time discussing the lobbying efforts of the financial industry, their connections with government, and how this has confounded attempts to apply strict regulation. While Barak's discussion is on the mark, a minor criticism might be that he fails to capture the true ferocity of their efforts, the largest assault upon Congress by any lobbying body in history.

Overall, the most important contribution of this book is that it provides a significant blow against the "trivialization of fraud" in both academic and policy circles. Mainstream criminology's penchant for statistical studies virtually guarantees that major corporate fraud, which rarely finds its way into government databases (and when it does, is underrepresented in terms of its scale and cost) will remain understudied, underemphasized, or completely ignored in policy discussions and recommendations. As it is currently impossible to conduct studies of such phenomena that would satisfy the scientific criteria of The Campbell Collaboration, the importance of qualitative and historical analyses such as Barak's cannot be overstated. It provides a detailed and thorough (scientific?) assessment of a phenomenon that would otherwise remain completely unstudied "scientifically." In the present case at least, a "scientific study of fraud" that *by design* ignores the highest levels of lawbreaking would be "unscientific." And a more robust interdisciplinary approach in future research on the subject will allow for the greatest potential impact on policy. *Theft of a Nation* provides an excellent foundation.

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Commentary

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Theft of a Nation: Wall Street Looting and Federal Regulatory Colluding, by **Gregg Barak.** Lanham, MD: Rowman and Littlefield Publishing, 2012. 211pp. $29.95 paper. ISBN: 9781442207790.

The hallmark of excellence in a book is that it raises questions as much as it answers them. By this measure, this is an excellent book. In *Theft of a Nation*, Gregg Barak sets his sights on finance capital and institutionalized crime, seeking to shed light on the financial meltdown in the United States and abroad. As a realist criminologist, he adopts an empirically-based and theoretically-driven explication of how Wall Street (the dominant economic player in finance capitalism) used its wealth, privilege, and access to government to make, break, and undermine regulatory law. As Congress "enabled the financial environment that would become one massive system of abuse and fraud" (p. 10), and the Walls came tumbling down (forgive the metaphor), the banks were deemed too big to fail. Consequently, many of the worst offenders were bailed out by taxpayers. In the worst recession since the Great Depression, the government response to the frauds continues to be anemic and ineffective.

Theft of a Nation is an impressive achievement. Barak pulls together extensive documentation for his arguments and offers a multi-level, multi-disciplinary theoretical framework to tie it all together—an ambitious task. For white-collar crime scholars, the relationship between fraud and financial crises is somewhat akin to the classic observation about pornography. We know it when we see it. But showing it is a much more elusive task. He has taken an integrated approach and given extensive attention to detail.

As a white-collar crime scholar who takes a different theoretical and empirical approach to the problem of financial fraud, one of the things that I appreciate and value about critical criminology is its laser-like focus on institutional sources of power and privilege tied to the accumulation of capital. Unlike mainstream criminology that is overwhelmingly preoccupied with street crime, critical criminology has had a longstanding interest in the problem of white-collar crime. Although critical criminology encompasses many diverse approaches, Barak's realist approach is consistent with the state/corporate crime perspective which considers how social harm is produced through the intersection of private capital and governmental interests (Kramer and Michalowski 1990; Friedrichs 1998). Harms can be created through initiation or facilitation. The former is where corporations employed by the government engage in deviance with the implicit approval of the state. The subprime mortgage meltdown, with its complex and often illicit relationships among government-sponsored enterprises and mortgage lenders and sellers is an example of initiation. Facilitation of social harms occurs when the state fails to regulate corporate activity, such as the recent Bernard Madoff case where the Securities and Exchange Commission's failure to monitor or investigate suspicious activity by Madoff's investment company resulted in billions of lost dollars for investors, in spite of the likelihood that assorted banks and hedge funds "had to know" about the illegal activity (Henriques 2011). Consistent with this approach, Barak's analysis of the financial crisis emphasizes the symbiosis between the state and private capital. ". . . [T]he concern here is not those fraudulent businesses within an industry masquerading as legitimate, but rather with an entire financial industry transforming its fraudulent activities into non-criminal and legitimate business practices as usual" (p. 78). Thus, in modern societies, Prechel and Morris (2010) remind us that "state policy gives form to corporate structures and creates opportunities for managers to engage in financial malfeasance'' (p. 332).

The arguments developed in *Theft of a Nation* are compelling, producing important ideas and challenges for mainstream criminology. On the other hand, they lack systematic evidence to test hypotheses and refute alternative arguments. Granted, critical perspectives often reject such ''positivistic'' epistemologies. But, a challenge for the field more generally rests with a paucity of useful data along with methodological weaknesses associated with extant research (Simpson, forthcoming 2013). The salience of the problem is particularly well put by Palmer and Maher (2010) who assert that the mortgage meltdown *likely emerged* as a consequence of the tightly coupled and complex financial system. They also admit that their ''normal accident'' methodology lacks scientific rigor (e.g., a controlled pre-test-posttest design) and that they are unable to ''determine whether the financial system was sufficiently complex and tightly coupled so as to produce an analysis . . . because it is extremely difficult to measure complexity and coupling in a way that allows comparisons of the levels of complexity and coupling within systems over time and between systems at the same time (p. 234).'' These deficiencies affect theory development as well as concrete knowledge about the incidence of fraud, its prevalence and patterns over time. While Barak approaches the problem in a creative fashion, integrating qualitative/quantitative evidence from various sources to draw logical arguments about the structural and institutional sources of fraud, unfortunately the lack of reliable systematic statistics on white-collar crime makes it difficult to assess carefully the veracity or relative strength of his arguments. This criticism is not directed at the work *per se* but is more an observation on the state of white-collar crime research 70 years after Edwin Sutherland called attention to it.

The book is a dense read. And sometimes the logic is elliptical. If the gold standard of theory is parsimony, this book is the opposite. For instance, Barak's theory ''assimilates the rich theoretical tradition of white-collar crime . . . . extends Wang's linear model of securities fraud by bringing together explanations of control frauds, of market corruption, and of system capacity in an interactive model of organizational control fraud'' (p. 71). The integrated approach is impressive, but exceptionally difficult to pull off. Typically, one level is under-theorized relative to the others and this book is no exception because the micro/individual level is given substantially less consideration than the other two levels. This deficiency is symptomatic of critical approaches more generally, but detrimental to a comprehensive picture. As Palmer and Maher remind us, individual agency affects how systems operate, their complexity and coupling, and how slowly or quickly the system reacts to meltdowns or debacles.

A main theme of the book documents how Wall Street has managed to avoid any significant punishment for their misdeeds. Although some evidence of civil and regulatory actions against offenders is cited, the emphasis is on criminal justice sanctioning. Ergo, the fact that the banks and other financial firms avoided criminal sanctions implies that the state response was paltry and inadequate. Absent criminal justice prosecution, penal sanctions will have ''little if any deterrent or enforcement value'' (p. 156). Yet, if we investigate what we know about deterrence generally and corporate deterrence more specifically, we need to be careful about worshiping at the altar of criminal penalties. Some research challenges the subjective utility model, referring to it as a ''stark failure'' in the case of corporate crime (Braithwaite and Makkai 1991). Other studies are more favorable—but the evidence is far from conclusive. Further, it is unclear whether criminal sanctions are more costly (and therefore a better deterrent) than civil sanctions—which can carry punitive damages, result in higher fines, are easier for the state to successfully pursue, and therefore brought more often. White-collar crime scholars presume that punishment works, that criminal punishment works best, and that corporate criminals are particularly sensitive to criminal legal sanctions. This is an empirical question that has not been adequately answered.

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Response

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Back in February 2010 at the annual meetings of the Academy of Criminal Justice Sciences in San Diego I presented the paper, ''Victimization and the Biggest Ponzi Scam in U.S. History: A Preliminary Discussion.'' That paper sparked a comparative discussion of and subsequent analysis of Madoff and Wall Street, which eventually became Chapter Two in *Theft of a Nation*, entitled ''Bernie Madoff's Ponzi Scheme and Wall Street's Financial Meltdown: A Primer on Investment Fraud and Victimization.''

Before the meetings in San Diego, I had no intention of writing a book on Wall Street looting and federal regulatory colluding. After all, while I have written on many crime and justice subjects over the past forty years, I had never written anything original on white-collar or corporate crime, let alone on financial securities fraud. Besides, what did I really know about regulation, deregulation, and re-regulation? So when *Theft of a Nation* received the 2012 Outstanding Publication Award given by the White Collar Crime Research Consortium of the National White Collar Crime Center at the annual meetings of the American Society of Criminology in Chicago on November 16th, 2012, I was very pleased, to say the least.

So let me underscore from the beginning why I thought it was important for the study of white collar crime that I apply my particular ''bag of integrated criminological tools'' to an examination of the world of big time financial fraud and state-regulatory collusion. First, when it comes to investigations of crime and justice by criminologists and others, there is a gargantuan dearth of research and scholarship, scientific or otherwise, on white-collar, corporate, and financial crime. This is especially the case when it comes to those crimes that are committed by the most powerful financial violators and their organizational networks as well as to the state and federal apparatuses of regulatory control, both at home and abroad. Furthermore, the omissions or the lack of institutional studies by criminologists or other social scientists that cut across the financial services industry and the political, economic, and social spheres of both individual and collective interaction, are even more glaring.

Second, what the public at large and the academic community in particular have been subjected to, and thereby constricted by, are non-criminological interpretations or analyses of the Wall Street meltdown and its victimization of the American people. Written exclusively by investigative journalists, economists, Wall Street traders, consumer advocates, and most recently, by former high-profile government regulators, these traditional accounts needed and still need to be augmented by criminologically-informed accounts. Accordingly, I believed that the time was past due for criminologists to step up to the fraudulent plates of Wall Street, to fill the voids of criminological neglect and ignorance, and to explain just how and why the vast majority of the members of the U.S. Congress, the White House, the Federal Reserve, and the Department of Justice had all agreed to treat the biggest financial crime in U.S. history as a non-crime.

Let me now turn to the two commentaries on *Theft of a Nation* included in this symposium, written by Henry Pontell and Sally Simpson. In terms of "full disclosure," permit me to share with the readers that both Pontell and Simpson have their blurbs endorsing this book on its back cover. Not only that, Simpson was also a member of the selection committee for the WCCRC outstanding publication award. In light of their respective remarks, I will respond briefly here to Pontell and mostly to Simpson.

As Pontell really has only one minor substantive criticism of *Theft of a Nation*, which I have no argument or disagreement with, my remarks are fairly limited. First, I think Pontell has contextualized quite nicely both the recent history of financial crises and crime in the United States as well as the state of criminological knowledge about these types of organizational behavior. Second, Pontell provides a nice overview of my work, especially capturing the general argument that securities fraud on Wall Street drove the financial meltdown and that the collusion from all the necessary power players in Washington, DC facilitated the denial or dismissal of the criminal realities. Third, Pontell gets it correct when he writes that my discussion on financial lobbying was on the mark, but less developed than it could have been.

Originally, I was going to devote more attention to the lobbying efforts by Wall Street, but in terms of issues having to do with book length, I had to choose between a full chapter on Dodd-Frank versus a full chapter on lobbying. At the end of the day, I figured that most folks were/are well aware of the growing number of lobbyists on K Street and the growing number of dollars spent by lobbyists, Superpacks, and individual donors to influence, if not, purchase or defeat specific bills of legislation, and so on. In contrast, learning about the strengths and weaknesses of Dodd-Frank seemed much more useful to or valuable for the average policy wonk, college student, or movement activist.

Compared to Pontell's overview, Simpson's favorable one, at the same time, also raises some issues that have much more to do with the field of white collar crime study in general than they do with *Theft of a Nation*. More critically, Simpson's review actually misrepresents some of the key arguments of the book. Accordingly, in the rest of this essay I focus primarily on her critique. But suffice it to say in the affirmative that Simpson gets it correct when she writes: "Barak pulls together extensive documentation for his arguments and offers a multi-level, multi-disciplinary theoretical framework to tie it all together . . . . I applaud the integrated approach taken and I applaud the extensive attention to detail."

Turning to Simpson's criticisms, the primary issue raised has to do with her claim that my arguments lack systemic evidence to test hypotheses and to refute alternative arguments. I would like to know to what competing arguments is Simpson referring? I am unaware of other systemic evidence in the field of white collar crime studies or of any alternative arguments that could be tested to explain Wall Street looting and federal regulatory colluding. However, I do agree with Simpson (and I am sure that Pontell does as well) when she quotes from Palmer and Maher's critique of positivistic methodologies and their lack of scientific vigor or their inability to capture the complexities of systems or institutions and the connections between these and the crime they produce, especially over time.

I would further add that "the lack of reliable systematic statistics on white-collar crimes" in general does not necessarily impede assessing "the veracity or relative strength of [my] arguments" in particular.

Not only do I provide all kinds of integrated evidence—qualitative, quantitative, systematic, and otherwise—that confirms my general arguments, but new evidence supporting my arguments appears daily in the financial news. What is more, without rejecting positivistic epistemologies, allow me to paraphrase Karl Popper who argued even though something is not falsifiable, and therefore not scientific, that does not do anything to negate its truth, soundness, or validity.

Simpson is correct that my approach to Wall Street securities fraud is consistent with the perspectives of state-corporate or corporate-state crimes. However, this does not capture the core of my approach to the ''crimes of capitalist control.'' In a few words, Simpson does not do justice to either my reciprocal model or to my analysis of high-stakes financial fraud, whose arguments are ultimately held together by both an integration and a proof of both William Chambliss' *structural contradictions theory of crime* and Donald Black's *theory of law*.

Therein lies the ''decriminalization'' of what otherwise was an epidemic of institutionalized securities fraud across the financial services industry as a whole, which was driven first and foremost by the demands and corresponding incentives of Wall Street.

In terms of clarity of what my book does and does not do, allow me to set the theoretical record straight. Nowhere throughout the text did I propose, for example, something like a ''reciprocal theory'' of high-stakes financial crime, equipped with formal propositions and/or hypotheses that were ready to be tested by the data or to predict other outbreaks of Wall Street securities fraud. I would argue that to do so would have been premature and subject to some of the criticisms leveled by both Pontell and Simpson at the underdeveloped study of white-collar crime.

What I have provided is a reciprocal model/approach/analysis of the political economy of Wall Street looting and federal regulatory colluding. In doing so, I build upon existing integrated theories of white-collar criminality and introduce new interactive models of ''organizational control fraud'' and of ''institutional financial control fraud.'' Hopefully, these models will generate further research on corporate-state crime and the collection of the type of data that is capable of establishing the scientific criteria called for by The Campbell Collaboration. I would further hope that these new models will be used by white collar criminologists not only to operationalize the complex behavioral interactions of securities frauds and their regulation, but also to provide the types of systematic data that Simpson envisions.

On the issue of whether or not the ''gold standard of theory is parsimony'' as Simpson suggests, since I proffered no formal theory of securities fraud, such criteria whether appropriate or not, should not be used as a measure of my book. Without digressing too far, let me stress that I believe that most of the best criminological theories of organizational crime do not lend themselves to parsimony nor to pre- and post-experimental testing. This is especially true of those comprehensive theories that attempt to integrate micro, meso, and macro levels of analysis, such as a multi-level integrated theory of violations of international criminal law developed by Christopher Mullins and Dawn Rothe in *Blood, Power, and Bedlam: Violations of International Criminal Law in Post-Colonial Africa* (2008).

As to Simpson's closing thoughts on the punishment of white-collar crime and my position on the same in *Theft of a Nation*, there is surprisingly some confusion here. Yes, throughout most of the book I highlight the absence or omission of any criminal sanctions for the Wall Street securities fraud that caused the financial meltdown. And yes, I do understand and I do try as best as I can through the running multiple narratives to explain why the ''lack of criminality'' exemplifies the contradictions of bourgeois legality. However, such evidence should not be misconstrued or conflated with my believing or arguing that if there were successful criminal prosecutions or penal sanctions, that that would make any real criminal difference in the scheme of high-stakes financial transactions.

In addition, I also emphasize that when Wall Street did, in fact, experience punitive action, on the civil and regulatory fronts, that these too were not significant fines (penalties). Comparatively speaking, these regulatory fines were so small that they did not amount to even ''slaps on the wrist.'' Notably, of the three possible sanction types, the civil fines—the only ones subject to little or no governmental control—were the most significant in punitive dollars.

Most importantly, as the last two chapters and postscript underscore, I am not calling for criminal sanctions as some kind of panacea for I do not think that these would have much, if any, deterrent effect on those future securities frauds capable of bringing about the next financial bubble or meltdown on Wall Street.

For example, in the very last section of the conclusion entitled, ''A Fantasy Bailout for the American People,'' it is noted that none of the fraudsters are being imprisoned for their crimes that caused the financial collapse nor are any of their corporate charters to expire. Moreover, as the imaginary bailouts unfold during the ''National Tribunal for Reparations for the Crimes of Securities Fraud Committed against the American People,'' those types of criminal punishments are not even on the negotiating table.

Lastly, in the postscript, I emphasize as I did in the body of the text that reasonable alternatives to preventing future financial collapses call for structural changes of the securities markets, including the breakup of the too-big-to-fail institutions and/or the public ownership of these same institutions.

Bottom line: so long as the book ratio of non-criminological to criminological treatments of Wall Street illegalities and federal regulatory inefficacies remains, on the order of something like two hundred to one, then fraud minimalism with its limited understanding of criminalization and the administration of legal justice will continue as the governing ideological means used to deny both domestically and internationally the elite-dominated financial-state crimes of a global political economy.

Commentary

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Who Are the Criminals?: The Politics of Crime Policy from the Age of Roosevelt to the Age of Reagan, by **John Hagan.** Princeton, NJ: Princeton University Press, 2010. 301pp. $45.00 cloth. ISBN: 9780691148380.

In *Who are the Criminals?* John Hagan paints an historically detailed, incisive, and remarkably personal retrospective account of the development and framing of crime policy. The connections made between crime policy and the field of criminology are simply fascinating. It is no small feat to blend a rich historical account of progressive and conservative politics with criminological theory in the broader context of a novel framing perspective. It is also nothing short of inspiring to think about the prospects of rebalancing the way in which street and suite crime are prioritized. Thus, I will limit my critical comments below to what is missing from a narrow part of Hagan's account, that of *corporate* crime policy. This book stands very strong even with my concerns over its completeness.

If there was a clearly bounded subspecialty of ''corporate criminology,'' connecting the politics of crime policy to ''crime in the suites,'' it would likely prompt some obvious if not trite questions about how our framing of ''who are the criminals?'' affects the allocation of criminal justice resources. I pose some of these questions below, and wonder in a brief conclusion as to how Hagan's work might have accommodated the same. Asking these questions may be valuable if we are to accept the proposed interdependence between street and suite crime policy.

First, do rates of white collar and corporate crime vary with the amount or alignment of state regulation? Hagan's ''critical

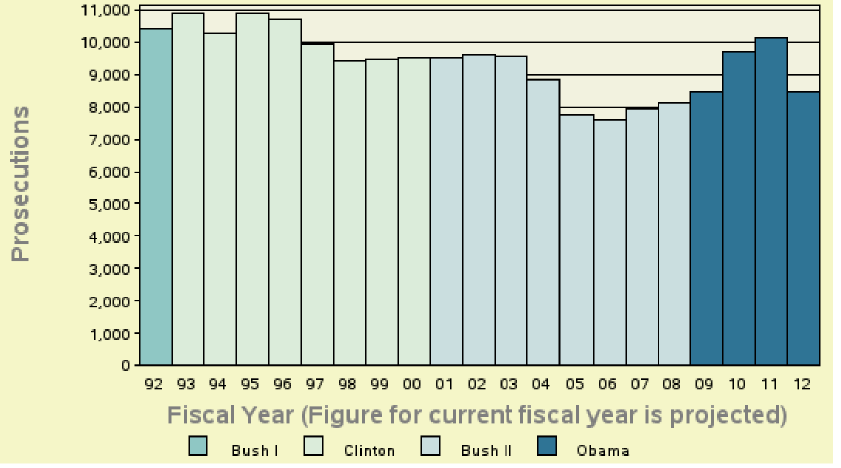

Figure One[1]

collective framing perspective'' supports conventional intuitions about such a regulatory correspondence, but it is not so clear that this is the case. Data on white collar prosecution are not dispositive (see Figure One), and federal sentencing data on organizations offer few discernible trends. In the absence of good data on corporate crime commission, we are left to speculate. Elsewhere I have argued that every generation discovers corporate and white collar crime, with some naïve surprise (Laufer, 2008). The scandals of one generation may look different from the next—from corrupt payments to foreign government officials and massive procurement fraud to cleverly packaged derivatives and notorious mortgage fraud. All along, though, white collar and corporate deviance seems to persist and, quite unremarkably, cases are brought against both individuals and firms through all of the eras' lax and heightened regulation discussed in *Who Are the Criminals?* In short, there appears to be a relatively robust base rate for this deviance that must be considered when highlighting only the most notable cases.

What can be said with some confidence is that the history of white collar crime prosecution and corporate criminal liability reflect a perennial tension between the regulatory power of government and corporate power that may be far more nuanced than Hagan's framing account. It is captured, in large part, by the need for the formal social control of business while at the same time deferring to the business community and our markets. It is all about balancing the power to regulate corporations and the specter of regulatory overreaching. And, finally, it is all about controlling business deviance through a combination of regulation and resort to the most formal of social controls, the criminal law. Throughout our history, an argument may be made that maintaining the appearance of legitimacy for the government's response to corporate deviance has mediated most if not all of these tensions.

Maintaining this appearance requires a carefully choreographed regulatory approach where, at times, the limited use of state power through symbolic prosecutions will suffice. These prosecutions may make a media frenzy, but are often far from representative of a regulatory trend or underlying rates of deviance. In *Who Are the Criminals?* the twenty or so corporations mentioned are iconic, but perhaps nothing more. Survey research on employee deviance reveals relatively stable base rates of white collar deviance over time, rates suggesting that our media-driven appreciation of corporate deviance may be just that, media driven. Survey data also highlight

[1] Twenty year Report on White Collar Prosecutions, TRAC (2013), available at: http://trac.syr.edu/tracreports/.

just how little white collar and corporate deviance is formally processed. Of course, we do not have to reach back to Clinard and Yeager (1980) to mourn all of the undistributed justice coming from the way in which we fail to prioritize the investigation and prosecution of this kind of offending. But, at the same time, it may be imprudent to think of scandals, crises, and notable cases of corporate wrongdoing as representing discernible and meaningful periods of regulatory scrutiny or ''eras'' of crime.

There are deeper meanings to shifts and trends in the regulation of Wall Street or the ''suites of America.'' Perhaps most notable is our remarkable ambivalence with the idea of a *criminal* corporation, found in the apparent illogic of attributing criminal wrongdoing to the engines of our economic growth. This ambivalence, reflected in the tilted allocation of criminal justice expenditures, is captured by the fact that: (1) corporations are aggregates of innocent stakeholders who unfairly suffer from a criminal investigation, indictment, and conviction, but serious consequences must result from corporate deviance; (2) markets encourage corporate risk taking and innovation, but corporations particularly in certain sectors and industries require vigilant regulation and faithful compliance; (3) civil and administrative law remedies for organizational deviance already exact a huge toll on corporations, but few doubt the unique role of the criminal law to encourage law abidance or voluntary disclosure of wrongdoing; and (4) the government must support and maintain close ties to the business community, but such ties may inhibit regulation or make resort to the criminal law problematic.

A second obvious question follows: If there is such significant ambivalence, why is there so much corporate crime legislation in certain historically notable periods? Khanna (1994, 2004) provides a range of possible explanations from public choice to institutional politics. These and other explanations require sensitivity to the subtle interactions between regulators and the regulated. Focusing somewhat narrowly on the apparent effects of regulation and deregulation, as Hagan does, might make one less likely to ask: How do private sector actors powerfully influence both regulation and the attribution of criminal responsibility? Our history is rife with examples of these influences, from the evisceration of legislation designed to reform corporate misbehavior (e.g., Sarbanes Oxley, and Dodd Frank), and the undermining of efforts to limit self-regulation and self-governance by firms, to the shifting of risks for criminal liability away from the corporate form. Corporate crime policy, it is fair to say, has as much to do with the sustained and instrumental use of corporate power as it has with the ebbs and flows of corporate regulation or deregulation.

Third, how is regulatory power exercised, more generally, in relation to the application of the corporate criminal law? The history of corporate crime regulation is also far more complex than a simple function of broad political phases or eras, no matter how defining political leadership might be over the regulation of the suites. A broad range of regulatory milestones adapting to a stagnant body of substantive law cannot be disregarded. This includes government issued guidelines, memoranda, and principles—from the United States Sentencing Guidelines for Organizations to a series of iterative memoranda from the Department of Justice that provides guidance on how prosecutors should proceed in bringing cases against corporations (Laufer, 1999a).

Tired and muddled conceptions of corporate criminal liability and corporate blame must make regulation quite difficult if not truly frustrating. At times, existing liability rules, over a century old, are disregarded by prosecutors and regulators. They are replaced by a brand of cooperative regulation that allows firms of means and scale to opt out of the criminal justice system through elaborate plea agreements, strategically recast blame, and cleverly manage an otherwise pristine reputation. Other times, liability rules are employed by large corporate fraud task forces to make a regulatory point. The causalities are supposed to reassure us that markets are safe and fair. Making sense of all of this at the level of broad regulatory trends or eras, however, may be difficult if not impossible. Trying to make sense of it, though, may be necessary for a framing account to fairly reflect the correspondence of crime policy.

Fourth, it is worth asking: How is our sense of the exercise of state power over corporations affected by the images that we construct of corporations? There are many different kinds of corporations, many different kinds of corporate cultures, some that are self-regulating with principle, others that cannot and never will. We are at some risk when in an effort to present an account of corporate crime policy, we craft archetypal images of crime in the suites as a function of large, diversified financial institutions who, without reflection, seek to exploit lax regulation and enforcement. There is no evidence that corporate and white collar crimes are so limited in distribution by firm size, industry, or market. Deviance considered ''white collar'' or ''corporate'' is no longer thought of as necessarily ''elite'' and, not surprisingly, shares many characteristics with street offenses (Weisburd et. al., 1994).

At the same time, it is also fair to say that there is an uncomfortable naiveté on the part of all stakeholders that accept, without suspicion, the glossy moral images of corporations. Corporations are deft in convincing all that their commitment to accountability and responsibility is strong and in place. The apparent inauthenticity of some corporate communications, combined with the very closed nature of the corporate form, frustrate any good assessment of white collar and corporate deviance from the outside. In short, criminologists still know relatively little about white collar and corporate offending—relative, that is, to street crime.

Fifth, when we think of who are the criminals in the suites, must we also consider if firms act criminally? What we mean when we say that firms versus agents act, reflects broad normative values that powerfully influence corporate crime policy. When the Department of Justice, for example, creates a Corporate Fraud Task Force, does it matter that indictments are brought only against individual offenders and not firms? If articulated crime policy, found in prosecutorial or sentencing guidelines, prompt indictments of agents rather than principals, or principals rather than agents, does that matter? These questions, rarely posed, are more than wasted metaphysical musings. It is difficult, if not impossible, to ask *who are the criminals* without a good sense as to who are, in fact, the criminals. As we move into a new phase of thinking about corporate personhood, post-*Citizens United*, corporate criminologists might start asking these questions.

Hagan covers much ground in *Who Are the Criminals?* He makes a series of connections that are creative and quite novel, with a conclusion that calls for a long overdue reconsideration of our priorities in policing, prosecuting, and punishing. In this brief review, I raise some simple-minded questions and concerns narrowly concerned with corporate crime policy. I conclude where I began, by bemoaning the fact that there is no bounded subspecialty of ''corporate criminology'' to do more than frame good, if not trite questions.

It is far from clear that the descriptive and analytic work necessary to speculate broadly about reapportioning risks and rights is done—at least with respect to corporate crime policy. The foundational work necessary to support Hagan's thesis must reflect a good mix of normative and empirical inquiry. This includes addressing the perennial ambivalence over attributing criminal wrongdoing to corporations, attending to the possibility that the base rate of this kind of deviance may be stable over time, resisting the temptation to think of cases highlighted by the media or picked by prosecutors for symbolic enforcement as representative or otherwise important, wrestling with how corporations successfully undermine legislative and regulatory reforms, appreciating the general failure of corporate criminal liability as a regulatory device, considering the complexity of the corporate form and how strategic moral imagery frustrate and limit attributions of liability, and finally, asking who are the criminals when agents act on behalf of principals in complex organizations.

As I finished Hagan's book I could not help but wonder whether a comparable or analogous framing challenge follows the discipline of criminology in ways that reflect the broader framing question posed by *Who Are the Criminals?* Hagan does a wonderful job connecting the history of crime policy with corresponding developments in criminological theory. But what about the bounded subject matter of criminology?

Some years ago I argued that the field of criminology was in denial for its failure to wrestle with the crime of genocide (Laufer, 1999b). Hagan, author of the most definitive work on genocide in criminology, literally reframed the field with his work on Darfur and war crimes. That criminologists focus so exclusively on street crime and explanations of the same, to the near exclusion of crimes of the state or the suite, must also say something important.

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Commentary

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Who Are the Criminals?: The Politics of Crime Policy from the Age of Roosevelt to the Age of Reagan, by **John Hagan.** Princeton, NJ: Princeton University Press, 2012. 301pp. $22.95 paper. ISBN: 9780691156156.

The *leitmotif* of E.M. Forster's novel *Howard's End* is "Only connect," which is also the central theme of this absorbing book, one of the rare books—a combination of textbook and research monograph—to seek to juxtapose and connect crimes in the suites with crimes in the streets. The penultimate chapter is an absorbing deconstruction of state crime and crimes against humanity, an area he has made his own. He concludes with a thoughtful epilogue on what we might now call Obama 1.

One commentator (Kramer 2011) asserted that "Hagan's three key arguments are (1) conceptual frameworks regarding crime shifted considerably from the Roosevelt and Reagan eras and changed crime policies, (2) race and social class permeate crime policies, and (3) U.S. prison populations have grown, as a result, to unsustainable levels." Through the lens of a white-collar and organized crime researcher, I see it somewhat differently. Hagan notes the importance to Reagan crime policies not just of sloganistic thinking about outgroups but also of "(1) data revealing that the majority of crimes were committed by a small minority of highly active offenders, and (2) studies challenging the value of indeterminate sentences in reducing criminal behavior" (p. 13).

The book is broader than Jonathon Simon's *Governing Through Crime* in another important way. Hagan says of the Reagan shift: "The result was a major redistribution of risk and regulation in American life. American minorities and the poor lost in several ways: they were prosecuted and incarcerated for street crimes at massively increased rates, and they were victimized by evolving forms of financial manipulation, including subprime mortgage lending and similar kinds of lending arrangements for credit cards, cars, and the like. The unsustainable subprime mortgage lending and resulting defaults and foreclosures disproportionately affected minority neighborhood homeowners and counteracted efforts to reduce street crime by stabilizing minority neighborhoods'' (p. 31).

Hagan is right to note the importance of chronic offenders as a term, but it is not clear what he is alleging about the relationship between Reagan's politics and the involvement of Blumstein et al. in this research. (It is interesting to speculate what would have happened to the United Kingdom's involvement in this criminal careers research if Donald West, a sophisticated critic of repressive policies on sexuality, and who started the Cambridge study of delinquent development with David Farrington as his research assistant, had not retired. Farrington took over the study from him and joined the Blumstein set, becoming the only Brit to ascend to the Presidency of the American Society of Criminology.) Hagan expresses the transformation very acutely: ''the policy-driven concerns about chronic offending shifted the emphasis in criminology from the causes of someone ever committing a crime—a prominent concern in the age of Roosevelt—to the causes of someone chronically continuing to commit crimes, which became *the* prominent concern in the age of Reagan'' (p. 121). So while black street criminals went to jails like lambdas to the slaughter, the career criminality of executives was totally neglected except by some senior figures at Yale such as Albert Reiss, Stan Wheeler, and their set of academic collaborators, including David Weisburd—and at Wisconsin by Marshall Clinard and his collaborators.

He concludes the chapter on the age of Reagan by noting: ''while power politics during the age of Reagan enabled the crisis framing and the ensuing fear of street crime that led to mass imprisonment, a corresponding normalized framing encouraging a misguided absence of fear resulted in a retreat from the regulation of suite crimes and thereby contributed to the economic collapse'' (p. 149). He envisages a ''critical collective framing theory,'' applying Goffmanesque frame analysis to street crime control and suite crime non-control: there is some particularly acute discussion of the shaping of racialized street crime and drugs sanctioning. That he does not *wholly* explain why *and how* these framings occurred is just a way of saying I am looking forward to the next volume!

Hagan discusses the Milken/Boesky cases of the 1980s and notes that ''Despite the publicity surrounding these crimes, this meant that the framing of the actual insider trading practices as criminal remained vague and obscure .... Thus, no adversarial frame could effectively challenge and contain the deregulation frame of the age of Reagan. Yet there is historical precedent for such a challenge in the now nearly forgotten hearings of the Senate Banking and Currency Committee, held during the waning months of the Hoover presidency and the early days of Roosevelt's New Deal. These hearings are instructive in suggesting how an adversarial framing can occur and even succeed in a time of crisis.'' Later he adds: ''The financial sector grew from about 16 percent of U.S. domestic corporate profits between 1973 and 1985 to double this figure in the 1990s, peaking at more than 40 percent of these profits in the current decade'' (pp. 175–182).

As Hagan puts it, after discussing Akerlof and Romer's corporate looting thesis: ''The innocence-of-optimism framing may be a latter-day version of Sutherland and Cressey's explanation of white-collar crime, which dates to the age of Roosevelt and their differential association theory, discussed in chapter 3. Cressey's more specific version of this theory was that embezzlers take other people's money only after they first rationalize their guilt. The institutional belief in spreading risk through securitizing mortgages may have been a similar kind of framing that could neutralize guilt and in this way excuse the claim of fraudulence against the bank managers. The ''too big to fail'' rationalization for the bailouts may be a similar kind of rationalization from the government's side. The question is whether these are causes or justifications of the practices involved. Part of the answer involves the subprime mortgages that were securitized'' (p. 209).

Importantly, he connects the sub-prime issues to both crime reduction (homeowners are less criminal, at least when able to prepay loans), and to white-collar (and, understressed in the book, lower level white/blue collar) victimization of poor and blacks:

"Mortgage lenders recognized that blacks who were historically redlined from receiving home loans might now be susceptible, ready-made customers for the new manipulative mortgages" (p. 195).

Hagan sidesteps the familiar white-collar crime definitional problem by noting that "as Sutherland emphasized, social science is based on behavioral probabilities and does not demand legal certainty. Sutherland helped us understand that criminology is concerned with establishing and explaining systematic patterns of criminal behavior rather than with convicting individuals of crimes" (p. 199). But where should we draw the line? Discussing Angelo Mozilo, CEO of Countrywide Financial, (if only he had been a *Mafioso* with a name like that!), Hagan notes "Although the concept of criminal fraud is uncertain in definition and proof, *Black's Law Dictionary* places its emphasis on gaining advantage by false suggestions or suppression of the truth. Mozilo's e-mails offer unusually explicit and compelling evidence that he knowingly manipulated the truth in just this way. Yet Mozilo's public image was framed quite differently by himself and others" (p. 201). Indeed, and subsequent to this book, the Department of Justice decided there was insufficient evidence to prosecute him. Admitting no wrongdoing, Mozilo paid $22.5 million of the SEC settlement himself (showing how much money he had made, saved, and could spare) with corporate insurance policies covering most of the balance of $67.5 million: the largest fine ever imposed by SEC (http://www.sec.gov/news/press/2010/2010-197.htm). He was also permanently barred from serving as an officer or director of a public company.[1] Mozilo gets a much kinder ride from McLean and Nocera's *All the Devils are Here* (2010) than he does from Hagan. In my own view, as in the Irish property boom catastrophe, recklessness by some lenders can enable them to corner so much of the market that if others do not join in, they will disappear. But there is room for multiple interpretations here!

Hagan summarizes the theme beautifully as follows: "The age of Reagan featured a frame realignment process that simultaneously advocated more severe punishment of U.S. street crimes and the deregulation of American financial practices. The result was an institutional redistribution of risk and regulation. American minorities and the poor lost out in two ways: they were prosecuted and incarcerated for street crimes at massively increased rates, and they were victimized by evolving forms of financial manipulation, including subprime mortgages and similar kinds of lending arrangements for credit cards, cars, and related loans .... Now as then, it is possible to reframe our understanding of the feared and the fearless, and I have argued that a key step in doing so is to emphasize the link between the two. A new cycle of reform can rebalance the ledgers of the twenty-first century by reconsidering our conceptions of the feared and the fearless. A critical collective framing perspective is an explanatory pathway toward this goal" (p. 232).

This is a fine book, like his previous work. Here are a few small reservations. Kramer comments that Hagan's analysis would have been strengthened by including the institutionalization of vested interests in mass incarceration, and by consideration of how the national politics affected local criminal justice systems. This is true. Woodiwiss (2011) notes that he might usefully have included the Reagan *Commission on Organized Crime.* This held televised hearings to dramatize its findings, and successfully reframed American and even international thinking on the structure and threat of organized crime. One might argue further that though it would have detracted from the clarity and neatness of his juxtaposition, the middle classes (unmentioned here, but repeatedly stressed by Democrats and Republicans)—whose boundaries are really a floating signifier, in Levi-Strauss' terms—suffer also from the tax costs of bailouts and tax offsets by corporations, though not to the extent of impoverishment. This is not

[1] We might wonder what happens to those fines: they certainly do not go into white-collar crime research, or Hagan might find that career criminality researchers in North America and the United Kingdom would take a greater interest in this area. The National White-Collar Crime Research Consortium could use the money!

just victimization of the poor, making the lack of elite prosecutions compared with the S&L crisis of the late 1980s even more intriguing. (For some early illumination, see Gretchenson and Story 2011; Levi 2009).

The chapters that reflect on criminological theories in the age of Roosevelt and the age of Reagan are well accomplished, but it is not obvious that they account for anything to do with the politics of that time, or if they do, that this is integrated with the policy histories and analyses. The causal direction (from policies to theories or vice versa) is unclear, though there is a sense in which theories get picked up if they match the *zeitgeist* and/or bureaucratic interests (like Cressey's work on organized crime and the FBI). However as a sociology of ideas and criminological culture, it is intriguing and unlike any existing texts of which I am aware. (David Garland's *The Culture of Control* does a more satisfying job, but his is not an American history in the way that this is.)

Moreover, if the book is clear enough about what went wrong and about the politics of deregulation, it is a little hazy about what we should do *reactively* to crimes in the suites. The Deferred Prosecution Agreements (not mentioned in the book) might be termed ''Too Big To Be Prosecuted,'' and though politics (and campaign funding) enters into what is done and not done about cases that some might describe as unprosecuted white-collar crime, the objective risks of collateral damage are substantive enough to generate genuine anxiety from the Lords of Misrule, as well as from the rest of us who might have to transfer our banking to somewhere more ethical—and on what data might we judge that? Likewise the book offers little about what should be done about crimes on the streets. It may be that to have tried to deal with those themes would have detracted from the analytical force of this already substantial book.

One of the fascinating things about *Who are the Criminals?* is its frame. In one sense, it is an erudite and many-layered companion volume to Reiman and Leighton's radical populist *The Rich Get Richer and the Poor Get Prison* (2013). But whereas the latter is primarily a juxtaposition and contrast, *Who are the Criminals?* is both a juxtaposition and a connector. How many of the thousands of students whizzing through our criminal justice degrees will get to read and reflect upon it? Not enough, I fear. But they should.

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Response

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What does it mean that criminologists overwhelmingly ignore the salient criminological questions of our time? Why is there so little attention given to the financial frauds that led to massive overbuilding of prisons and houses in America? Why is there no investigation of executive branch authorizations of secret renditions, brutal torture, and extrajudicial killings? Why is there minimal explanation of the American government's failure to assist in arresting and placing on trial the President of Sudan for charges of genocide in Darfur? Why do criminologists largely overlook these pressing questions and topics?

Michael Levi and William Laufer recognize that the central concern of my book is to explain how and why the United States as a nation over-controls street crime and under-controls suite and state crimes. Beyond this, Laufer wants to know what it means that the field of criminology itself largely ignores state and suite crimes: "That criminologists focus so exclusively on street crime and explanations of the same, to the near exclusion of crimes of the state or the suite, must also say something important." Levi raises a similar issue with regard to street crime, saying that "Hagan is right to note the importance of chronic offenders as a term, but it is not clear what he is alleging about the relationship between Reagan's politics and the involvement of Blumstein et al in this research." Levi and Laufer want to know exactly what I am saying about the *field of criminology* as well as national crime policy.

My thesis is that the political winds of American public opinion sway the field of criminology as well as criminologists. My book explicitly draws on Bourdieu's theory of academic fields and his thesis that scholars compete, using concepts and logics, to leverage their status positions and paradigmatic dominance within their disciplines. I argue that theory and policy shifts in the field of criminology have occurred in response to the political preferences of the age of Roosevelt (from roughly the 1930s through the 1970s) and the Age of Reagan (circa the 1980s to the present; I argue the jury is still out about the possibility that there will be a new Age of Obama.)

To be more explicit, I argue that an Age of Reagan framing of voters' "fears of the street" had much to do with building the criminological audience for James Q. Wilson's resurrection of the deterrence doctrine, Alfred Blumstein's promotion of the concept of "Lambda" to study chronic offenders and career criminals, and Travis Hirschi and Michael Gottfredson's social-psychological arguments for a self-control theory of the causes of criminal behavior. It is important to recognize that these scholars competed strenuously—sometimes in collaboration with one another, and sometimes in opposition to one another—for research funding, publication outlets, and leadership positions in the field. They did so while nonetheless joining in dismissing the rehabilitative ideals of Age of Roosevelt criminologists and that era's discovery of white-collar crime. Their conceptualizations of individualistic rational actor frameworks fit well with Age of Reagan concerns about the calculated predations of recidivist offenders. It was important to their success that they not stray too far from the middle majority of public opinion that apprehensively embraced fearful framings of street crime, especially involving young minority drug crime activists in America.

The Age of Reagan framing of chronic offenders and career criminals spawned a developmental criminology that reigned supreme in criminology into the 1990s, and remains prominent to this day. This school of criminology is still strongly represented in the American Society of Criminology, and indeed advocates of this perspective recently created a large new section within this professional organization. Liberalizing elaborations of past premises of developmental criminology include the contextualized community-level focus of Robert Sampson's theory of collective efficacy and the punishment-based emphasis of Bruce Western's theory about the life-course effects on individuals of mass incarceration policies. The policy implications of these perspectives on the effects of concentrated disadvantage and disproportionate punishment challenge Age of Reagan criminology's signature policy ideas and may represent the best prospects for moving the field of criminology and the country from a center right to a center left policy framework during a new Age of Obama. It has not happened yet, but the possibility remains.

Meanwhile, the 1990s and 2000s also brought new academic competitors into the field. Figures like David Garland, Loïc Wacquant, Jonathan Simon, and Katherine Beckett gained new and important footholds, in significant part by highlighting the contrasts between the Age of Reagan preoccupation with chronic offending and more classical Age of Roosevelt concerns addressed by earlier criminologists like William Chambliss, Stuart Scheingold, and Austin Turk. These concerns focus on the extent to which highly politicized state and federal laws have entrenched punishment regimes and now block mass incarceration reforms and remedies for hardened police practices and pipelines that send youth from locked-down public schools and communities to prison warehouses across America. These scholars often place at the center of their work interwoven comparative and historical accounts of the role of the politicized state in criminal law-making and enforcement.

There is irony in the emphasis of Age of Reagan criminology on careers in crime. A Bourdieuian perspective notes that alternative career trajectories are also apparent in the divergent professional paths of rising criminologists. The intellectual careers of criminologists are increasingly polarized along political fault lines that divide American voters as well. Thus a growing and more progressive collection of criminologists are traveling an alternative path than that characterizing Age of Reagan criminology. Many have found a newly congenial home in the Law and Society Association, an organization which traces its roots to the legal realist movement, the Society for the Study of Social Problems, and Age of Roosevelt scholars.

It is also noteworthy that the academic competition among crime scholars has created spheres of influence that parallel the evolving political pathways that divide print media and cable television networks. This professional divergence adds a welcome clarity and engaging edginess that can make the field of criminology more exciting. But there are costs as well.

The polarization of the field is leading criminologists—I'll call them American Society criminologists, and Law and Society criminologists—to talk past one another. Imagine a "conversation" between *The New York Times'* David Brooks and *The Nation's* Katrina vanden Heuvel, talking over and around one another. Similarly, the flagship journal of the American Society criminologists, *Criminology*, stresses the role of quantitative falsification, while the flagship journal of Law and Society criminologists, *Law and Society Review*, highlights the legally grounded and hyper-politicized role of the state. It is tempting for new cohorts of criminologists entering the field to pursue one or the other of these diverging ways of studying crime without really engaging the other. Criminological thinking is narrowed rather than broadened in this way.

Laufer especially laments criminology's overwhelming neglect of corporate crime. He is surely correct that the absence of attention given to corporate and white collar crime has limited our understanding of how the under-control of this behavior relates to its frequency, prevalence, and persistence. Yet incomplete knowledge is not the same as inaccurate knowledge, and we surely now know that something different in scale and intensity has unfolded in the Age of Reagan world of financial criminality. Nothing like the leveraged level of fraud involved in the 2008 financial crisis occurred previously in America. The concepts of "too big to fail" and therefore "too big for trial" have no comparable precedents in the annals of upper-world crime.

I have suggested a critical collective framing perspective to organize thinking about the criminality involved in the financial crisis. This perspective highlights an Age of Reagan framing of the ''freeing of the suites'' as a source of this crisis. The most obvious current example of this still ongoing Age of Reagan regulatory weakness and its relevance to fears of the streets post-dates the publication of my book. It involves the decision of the U.S. Department of Justice to bring no criminal charges against some the world's largest banks, including the British based HSB and its American subsidiaries, for massive and persistent money laundering for the Mexican drug cartels—the same cartels that illegally export drugs into our street economy. State and federal authorities decided against criminal indictments of HSB for money laundering because of concerns that criminal charges could jeopardize one of the world's largest banks and ultimately destabilize the global financial system.

Instead of criminal charges against HSB executives, the U.S. Department of Justice agreed to a nearly two billion dollar settlement. *Authorities allege HSB transferred as much as seven billion dollars for Mexican drug cartels, taking advantage of the lax monitoring of its American subsidiaries.* HSB is similarly alleged to have laundered money for Iran. The scale of these transactions makes the amounts of money and drugs at issue in the Reagan Administration's Iran-Contra scandal seem quaint. Many believe that Ronald Reagan would have been impeached and removed from office if the Iran-Contra scandal had not occurred so soon after Nixon's Watergate and during Reagan's lame duck second term.

The HSB case, only four years after the financial crisis and its bank bailouts, answers the question of whether the still large and extensively interconnected banks remain too big to fail as a result of being placed on trial. The Justice Department and HSB instead chose the path of settlement, which set the precedent for pursuing in parallel ways other large banks engaged in similar practices. As noted, the concern of the Department of Justice was that the successful prosecution of a money-laundering indictment would cause the failure of HSB and again destabilize the global financial system.

Laufer is right that we have much to learn about the relationship between financial crimes and their deterrence through criminal law enforcement. I argue in *Who Are the Criminals?* that it is nonetheless even more important that we acknowledge and begin to study how suite and street crimes are interconnected. Drug cartels cannot prosper without state protection from legal prosecution. Criminologists like Jerome Skolnick in *Justice Without Trial* long ago revealed and explained how this protection occurs on the streets of our largest cities. The HSB case dramatically illustrates the volume and heights to which these relationships reach across the national boundaries of our globalized economy, and how powerless we may currently be to intervene through criminal law enforcement. In *Who Are the Criminals?* I argue that a field of criminology that ignores and thereby fails to study these growing interconnections of street and suite crime is naïve in the sociological extreme. Mike Levi's dark humor about criminologists ''leading Lambdas to slaughter'' is broadly and deeply ironic.

Inheritance of Poverty or Inheritance of Place? The Emerging Consensus on Neighborhoods and Stratification

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Great American City: Chicago and the Enduring Neighborhood Effect, by **Robert J. Sampson.** Chicago, IL: University of Chicago Press, 2012. 534pp. $27.50 cloth. ISBN: 9780226734569.

Stuck in Place: Urban Neighborhoods and the End of Progress Toward Racial Equality, by **Patrick Sharkey.** Chicago, IL: University of Chicago Press, 2013. 304pp. $30.00 paper. ISBN: 9780226924250.

Sociologists have long sought to understand the mechanisms by which socioeconomic disadvantage persists over time and across generations. They have paid particular attention to understanding why poverty appears to be so deep, obdurate, and lasting for certain social groups, such as African Americans. Whereas economists theorize human welfare as the end product of rational choices made by individuals and households within free markets subject to resource and informational constraints, sociologists emphasize the stratifying effects of social structures that embed institutionalized practices of exclusion and exploitation within and outside of markets.

The early theorists of the Chicago School grounded their structural analysis of social stratification firmly in space, emphasizing how people and households are embedded within neighborhoods which are themselves embedded within cities and metropolitan areas. Space largely disappeared from structural-functionalist accounts of inequality during the 1950s and 1960s, however. Although functionalist theories did embed individuals and families within larger social institutions, they did not situate them in space. Likewise, in the status attainment model that dominated the 1960s and 1970s, family background was explicitly recognized to shape individual social mobility whereas little or no attention was paid to the spatial context within which mobility occurred.

The neglect of space in the sociological study of stratification came to an end in the 1980s. Nancy Denton and I, for example, incorporated spatial assimilation into the status attainment model explicitly to capture the stratifying potential of neighborhoods with respect to social mobility (Massey and Denton 1985). Nonetheless, it was not our article but the publication of William Julius Wilson's book *The Truly Disadvantaged* (1987), that galvanized the field. He was the first to note that black poverty was becoming more spatially concentrated, and went on to hypothesize that rising rates of poverty within black neighborhoods undermined the welfare of African Americans in new ways, deepening their social, economic, and cultural isolation from U.S. society.

Wilson saw economic isolation as resulting from the structural transformation of the urban economy, which eliminated steady, high-paying jobs in manufacturing and replaced them with a two-tiered service economy that contained stable, high paying jobs for well-educated workers but poorly-paid, unstable jobs for those lacking education. The resulting rise of joblessness and the loss of earnings among black males, he argued, undermined black family stability, weakened connections to the labor market, rendered productive role models scarce, and ultimately changed cultural practices and normative structures in ways that fueled a rising cycle of disorder and deprivation.

Massey and Denton (1993) built on Wilson's theory by arguing that black segregation was not a natural part of the urban environment, but a discriminatory configuration that was deliberately created by whites in order to isolate African Americans socially and spatially. They saw racial segregation not as a neutral social fact, but a powerful contributor to the concentration of black poverty observed by Wilson, pointing out that poverty inevitably became more concentrated whenever poverty rates increased for a segregated group. Lincoln Quillian (2012) later broadened this insight by showing that concentrated poverty actually resulted from the interplay of three kinds of segregation: racial segregation, poverty-status segregation within race, and segregation between blacks and high- and middle-income members of other racial groups. Nonetheless the fact remains that persisting segregation interacts with rising income inequality to produce spatially concentrated poverty.

Publication of *The Truly Disadvantaged* set off a wide-ranging search across the social sciences for evidence of ''neighborhood effects.'' Researchers sought data to assess the degree to which living in a disadvantaged neighborhood contributed to the perpetuation of poverty above and beyond individual and family characteristics. Unfortunately, in the late 1980s, multi-level data sets linking individuals and families to neighborhood data were few and far between, and statistical methods for multilevel analysis had not yet been developed (Jencks and Mayer 1990). Rising interest in neighborhood poverty coincided, however, with the implementation of the Gautreaux Assisted Housing Program in Chicago, a coincidence that seemed to offer good evidence in support of Wilson's hypotheses.

The Gautreaux Program allocated housing vouchers to residents of Chicago public housing as part of a court-ordered remedy for past racial discrimination by the Chicago Housing Authority. Viewing residential segregation as a metropolitan-wide problem achieved by racial exclusion in suburbs as well as the city, the court required half the voucher recipients to move to white suburban neighborhoods whereas the other half were free to use their vouchers to move to neighborhoods within the city. In a series of studies based on the Gatreaux Project, James Rosenbaum and colleagues compared the socioeconomic status of city versus suburban movers and found significant improvements in the lives of those who had relocated to white suburbs (Rubinowitz and Rosenbaum 2000).

In general, he found that suburban residents displayed higher rates of employment, earnings, school completion, and lower rates of welfare dependency compared to those who remained in the city. These findings were hailed as evidence for the existence of neighborhood effects and seen by some as a blueprint for promoting the desegregation and socioeconomic advancement of poor minority families (Polikoff 2006). Critics, however, quickly pointed out that Gautreaux program participants had not been randomly allocated to city and suburban locations, subjecting Rosenbaum's findings to the charge of selection bias. In response, a team of economists with support from the U.S. Department of Urban Development designed and implemented the Moving to Opportunity Demonstration Project (Briggs, Popkin, and Goering 2010).

Unlike Gautreaux, MTO randomly assigned residents of public housing projects in five metropolitan areas to one of three treatment groups. One group was offered a voucher to use in moving to a low-poverty neighborhood and received counseling to help them do so; another group was offered a voucher that could be used anywhere but got no counseling; and a third group received no voucher or counseling at all. When the interim evaluation appeared in 2003, however, the findings were less impressive than those emanating from the Gautreaux Program. Although families who moved to low-poverty neighborhoods did experience lower crime rates, improved housing, and better mental health, there were no significant differences between treatment groups with respect to employment, earnings, or educational achievement, leading some observers to conclude that earlier estimates from the Gautreaux Program were indeed biased by selectivity and that ''neighborhoods don't really matter.''

A turning point in the debate on neighborhood effects came in 2008, when the *American Journal of Sociology* sponsored a symposium on MTO in which Susan Clampet-Lundquist and I (2008) documented features of MTO's design and implementation that mitigated against finding strong neighborhood effects. Families in the experimental group, for example, were only required to move into low-poverty neighborhoods, not low-poverty *white* neighborhoods as in the Gautreaux Program. Whereas the Gautreaux settlement was explicitly about race, in MTO race was pushed aside in favor of class. As a result, most voucher recipients simply moved within the confines of the black ghetto, usually relocating to segregated black neighborhoods adjacent to or near high poverty areas, often with the same school catchment area. In his contribution to the *AJS* symposium, Robert Sampson (2008) indeed showed that experimental and control families in Chicago moved to the same disadvantaged minority neighborhoods.

Other problems in MTO stemmed from the inevitable gap between the project's design and its implementation in practice. Only around half of those offered mobility vouchers accepted them and moved into a low-poverty neighborhood, and the process of voucher uptake was itself highly selective (Clampet-Lundquist and Massey 2008). Moreover, those who accepted the vouchers and moved exhibited a high and again selective propensity to return migration to high-poverty ghetto neighborhoods after the first year. Sampson (2008) convincingly argued that the geographic mobility of voucher recipients, as among Chicagoans generally, was highly structured along the lines of race and class and that instead of moving to opportunity, most program participants ended up "moving to inequality."

In the United States, this kind of segmented mobility produces distributions of neighborhood disadvantage for white and black households that barely overlap. White families, even very poor white families, rarely experience concentrations of poverty that are routinely experienced by poor and even middle-class black families. Given this structural reality, rather than seeking to randomize selective geographic mobility away as an experimental nuisance, Sampson (2008) and others have argued that a better approach would be to recognize selective mobility into high- and low-poverty neighborhoods as a fundamental component of the stratification process itself, and to model it theoretically and measure it empirically in order to reveal how selective mobility generates the divergent social worlds inhabited by black and white Americans.

Since 2008 a consensus seems to have emerged among social scientists that neighborhoods do indeed matter in determining human welfare across a variety of salient dimensions. In 2010, Ruth Peterson and Lauren Krivo published *Divergent Social Worlds*, which painstakingly documented the vast gap in neighborhood quality experienced by white families, on the one hand, and black and Latino families, on the other. The racial differential in neighborhood circumstances was particularly stark in terms of exposure to crime, disorder, and violence. At the same time, a growing number of studies have used a range of methodologies to demonstrate the negative effects of concentrated neighborhood disadvantage on human well-being, especially over the long term (Wodtke, Harding, and Elwert 2011).

Reviewing the evidence from MTO in the 10 to 15 years after the study's initiation, Jens Ludwig, one of the project's lead investigators, noted that "what is particularly remarkable about the MTO health impacts is how massive they are" (2012:18) and that "data about neighborhood safety from MTO participants show similarly large effects" (2012:14). In the end he rejected only the extreme hypothesis that "neighborhoods always matter" and in the article even discussed the possibility that MTO may have offered a "weak treatment" to detect neighborhood effects on outcomes such as employment and education. A recent quasi-experimental study I directed, however, offered a "strong treatment," contrasting residents of high poverty minority neighborhoods with a matched sample of people who moved into an affordable housing complex built within an affluent white suburb. The study replicated the MTO finding of a significant connection between neighborhood disadvantage and mental health (Casciano and Massey 2012a), but also found a strong causal effect of neighborhood disadvantage on rates of employment, earnings, and household income (Casciano and Massey 2012b), not to mention education (Casciano and Massey 2012c).

In the end, I conclude that MTO reveals both the power of neighborhoods to influence key human outcomes (such as health) but also the limitations of what can be accomplished using voucher programs to send poor minorities families into a social system and urban landscape that is highly segmented on the basis of race and class, especially if the vouchers offer only modest subsidies (DeLuca, Garboden, and Rosenblatt 2012) and landlords are not required to accept them (Edin, DeLuca, and Owens 2012). The social structure of urban America is such that absent a forceful intervention, powerful, institutionalized, socially-embedded processes will operate to replicate the existing ecological landscape, despite the noble intentions of voucher program designers.

This urban reality is magnificently exemplified by Robert Sampson in his outstanding book *Great American City: Chicago and the Enduring Neighborhood Effect*, which represents the capstone publication of his justly celebrated Project on Human Development in Chicago Neighborhoods. In the tradition of the Chicago School, Sampson seeks explicitly to analyze the connections between social and residential mobility and to study it as a central feature of the stratification system. He does so by conceptualizing and measuring relevant social structures and processes operating at the micro-, meso-, and macro-levels, grounding them firmly in space, and then showing how they operate selectively to channel people and resources to different positions in the geospatial order, and in so doing to replicate and reinforce existing structures of social and spatial inequality.

Sampson begins by showing that despite extensive residential mobility over time and constantly churning neighborhood population, the socioeconomic and racial-ethnic composition of Chicago's neighborhoods is extremely stable over time and that across neighborhoods "things go together" in very consistent and highly predictable ways. The same neighborhoods that were disadvantaged in 2000 were disadvantaged in 1990, not to mention 1980, 1970, and 1960. In addition, irrespective of year, neighborhoods that were disadvantaged with respect to socioeconomic status were also disadvantaged with respect to health, crime, collective efficacy, civic organization, altruism, and other factors relevant to human welfare.

Thus, attempting to disentangle whether low socioeconomic status causes a lack of collective efficacy or vice versa is beside the point. Owing to Chicago's highly stable configuration of interlocking social and spatial structures, these two conditions almost always go together. Along with poor health, high neighborhood disadvantage simultaneously predicts high crime, weak civic organization, isolated social networks, and cynical social attitudes, exposing residents to the combined influence of these maladies so that their independent effects cannot really be disentangled theoretically or empirically. According to Sampson, the interlocking of social and spatial structures begins in human social cognition. He shows that the perceived level of crime and disorder within neighborhoods increases systematically as the black percentage and poverty rate rise, irrespective of actual rates of crime and delinquency. Although observed disorder may predict perceived disorder, racial and economic composition matter far more in determining the perceived safety and desirability of neighborhoods and strongly shape residential decisions.

Not only do race and class segment human social cognition, they also shape the structure of interpersonal networks and social organizations, which in turn map onto ecological structure and operate in interlocking ways to constrain choices and constrict the range of possible outcomes for individuals depending on race, ethnicity, and socioeconomic status. As a result, the vast majority of residential moves within urban systems such as Chicago, however frequent they may be, tend to produce marginal changes in the social world experienced by movers. Absent some kind of intervention, poor black families who move simply go from one poor black neighborhood to another, whether or not they have a voucher. It is for this reason that MTO in Chicago and other cities failed to produce significant movement outside the ghetto and had disappointing effects on employment, earnings, and education.

The consequences of such structural selection are amply detailed in Patrick Sharkey's excellent and provocative new book *Stuck in Place*. Not only are African Americans far more likely than whites to experience concentrated poverty at any point in time, but exposure to its pernicious effects has actually increased over time, despite the passage of landmark civil rights legislation. Among African Americans born prior to the end of the civil rights era (1955–1970), for example, 62 percent grew up in neighborhoods that were more than 20 percent poor whereas among those born afterward (1985–2000) the figure had risen to 66 percent. In contrast, the respective figures for whites in the same birth cohorts were just 4 percent and 5 percent.

As a result, for African Americans in the post-civil rights era exposure to high levels of neighborhood disadvantage are more than just common; it is persistent and multigenerational. According to Sharkey, roughly half of the African Americans he studied had lived in the poorest quarter of urban neighborhoods for at least two consecutive generations, compared with just 7 percent of whites; and this inability to escape ghetto poverty cannot be attributed to individual or family characteristics. In Sharkey's words, ''the reason children end up in neighborhood environments similar to those of their parents is not that their parents have passed on a set of skills, resources, or abilities to their children . . . . Instead, parents pass on the place itself to their children'' (p. 21).

African Americans' unique multigenerational exposure to concentrated poverty goes a long way toward explaining the persistence of black/white gaps in socioeconomic status. Racial gaps in variables such as education and occupational status are determined by a combination of disadvantaged family background and exposure to concentrated neighborhood poverty. However, racial gaps in income and wealth are determined far more by neighborhood conditions than family background, ''For these outcomes, aspects of the *family* environment play little role in explaining black/white gaps, while *neighborhood* conditions explain a substantial portion of the racial gap in each outcome'' (p. 114, emphasis in original).

Although differential exposure to neighborhood poverty is important in explaining racial gaps in attainment, when it comes to patterns of inter-generational mobility the effect of poverty concentration depends on whether one considers upward or downward mobility. Although the likelihood of upward mobility is not strongly affected by neighborhood circumstances, the prospects for downward mobility are much greater for blacks than whites. Whereas almost half of all black children with middle-class parents fall into the bottom of the income distribution as adults, only 16 percent of white children do so. Put succinctly, ''the social environments surrounding African Americans . . . make it difficult for families to preserve their advantaged position in the income distribution and to transmit these advantages to their children'' (p. 115).

The pernicious effects of multigenerational exposure to concentrated poverty are particularly evident with respect to the inculcation of cognitive skills. To demonstrate this effect, Sharkey divided African American children into four groups: one in which neither parent nor child ever lived in a poor neighborhood; one in which the parent but not the child grew up in a poor neighborhood; one in which the child but not the parent grew up in a poor neighborhood, and one in which both parent and child grew up in a poor neighborhood. For those that did not experience high neighborhood poverty in either generation the average score on a standardized test of reading skills was 110, whereas the score was 94 for those who experienced high neighborhood poverty in both generations—a shift of more than one standard deviation. Those in the two middle groups experienced a score of around 102. Controlling for individual and family characteristics slightly diminished but did not eliminate the differential.

In my view, *Great American City* and *Stuck in Place* are critical to understanding the persistence of poverty and deprivation among African Americans today and are also fundamental to explaining the relative lack of progress in closing salient racial gaps in achievement. Sampson in his book expertly describes the social and spatial structure by which segregation and concentrated poverty are generated and reproduced. Sharkey ably documents the multigenerational exposure of African Americans to concentrated poverty that inevitably follows from these structural conditions and how it systematically undercuts black prospects for education, employment, occupational status, earnings, and wealth while simultaneously making it difficult for affluent African American parents to pass on class advantages to their children. Taken together, these two books convincingly demonstrate that neighborhood effects are very real indeed, and that selection into advantaged and disadvantaged segments of the urban landscape is not a confounding nuisance to be eliminated through randomization in a field experiment but is a core mechanism of stratification to be modeled and understood in and of itself.

In 1968 Otis Dudley Duncan, writing from the perspective of the newly developed status attainment model, argued that ''if we could eliminate the inheritance of race, in the sense of the exposure to the discrimination experienced by Negroes, the inheritance of poverty in this group would take care of itself'' (1968:103). His article, written in the same year the Fair Housing Act passed Congress, was entitled ''Inheritance of Poverty or Inheritance of Race?'' If he were writing today, a more appropriate title would be ''Inheritance of Poverty or Inheritance of Place?'' The more things change, the more they stay the same.

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Chicago as Seen by the Chicago School's Greatest Practitioner

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Great American City: Chicago and the Enduring Neighborhood Effect, by **Robert J. Sampson.** Chicago, IL: University of Chicago Press, 2012. 534pp. $27.50 cloth. ISBN: 9780226734569.

Great American City: Chicago and the Enduring Neighborhood Effect is the rare example of a highly anticipated book by a leading scholar that exceeds expectations. Combining Robert Sampson's observations with major data collection on neighborhoods in Chicago and original analytic methods, the result is a compelling argument for the importance of place. Neither a purely theoretical nor empirical study, *Great American City* is an impressive model of how theory, data collection, and analytic methods can be combined to the benefit of all three endeavors.

The main agenda of *Great American City* is to demonstrate the importance of place of residence on individual and collective life in light of intellectual trends that have called the importance of place into question. ''Place'' in *Great American City* is location of residence within Chicago, meaning the neighborhood where an individual lives, but also the supra-neighborhood context of location within the city's larger geography. On the one hand, the role of place has been questioned by social scientists who take individual characteristics as key determinants of lives and social position. On the other hand, the role of place has increasingly been questioned by theorists who posit that new technologies of communication and travel are making place increasingly irrelevant to daily life. In response to these views, the book analyzes Chicago neighborhoods as a system of persistently unequal social contexts with powerful effects above and beyond individual characteristics. Sampson argues that this system of unequal neighborhood contexts has been very stable over recent decades and strongly influences individual lives.

What makes *Great American City* so notable is not its claim that places matter—many books and articles, of course, say as much. Instead, Sampson's innovative theories about why places matter are the book's notable contributions, combined with massive, well-designed data collection and state-of-the-art analyses that allow him to test and refine these ideas. The amount of data collected by Sampson's research teams is astonishing. The original data include a longitudinal survey study that followed 3,800 families for seven years, a separate survey of more than 8,000 adults, a survey of 2,800 community leaders, data coded from street videotaping of thousands of blocks, an analysis of 30 years of newspaper accounts of collective action events, and results from a field experiment that dropped more than 3,000 ''lost'' letters across neighborhoods. Sampson also makes heavy use of data from the U.S. Censuses and from governmental sources. It seems likely this is the most comprehensive set of data ever collected about a city over a brief span of years.

One of the book's most distinctive proposals is that intersubjectively shared perceptions of neighborhoods—perceptions shared by residents about their own neighborhood—are important social facts that strongly influence neighborhood life. Three of these shared perceptions are developed in some detail: perceived neighborhood disorder, collective efficacy, and moral and legal cynicism. These perceptions are subjective, but by combining survey responses from neighborhood residents, Sampson shows them to be as measurable as basic demographics like the neighborhood poverty rate or age composition. Further, these collectively shaped perceptions are enacted in neighborhood life, and Sampson argues that they form ''cultural structures'' with a permanence and stability that influences neighborhood character in the long term.

After an overview of the theoretical framework and data, the book develops a series of chapters that mix theoretical ideas and empirical analysis. Concluding chapters synthesize and discuss the results.

The analytical chapters begin with an historical analysis of neighborhood inequality in Chicago from 1960 to the present. Sampson shows there is remarkable stability in the income levels of Chicago neighborhoods across this span of time: poor areas in 1960 are poor now, and affluent areas in 1960 are affluent now. Racial composition shows more change, but only in one direction: some neighborhoods became blacker, but not a single neighborhood transitions from black to white. Majority black Chicago neighborhoods are much poorer on average than majority white neighborhoods. Economic position in the neighborhood economic hierarchy can be said to be nearly inherited and is deeply racialized.

The book then turns to an analysis of neighborhood influences on perceptions of disorder and crime. On perceived disorder, which is the perception of physical decay and social rowdiness, Sampson shows that more disorder is ''seen'' in more heavily black and Latino neighborhoods, even when actual measures of visible disorder are controlled. On crime, Sampson continues his argument from past articles on the importance of *collective efficacy*: the combination of social cohesion among residents and shared expectations that residents will take action in the face of threats to neighborhood order. Sampson reports that collective efficacy is a key characteristic explaining variations in neighborhood crime. Neighborhood collective efficacy explains both why poorer neighborhoods have more crime and accounts for variability in neighborhood crime independent of neighborhood poverty.

Two less-studied outcomes the book considers are neighborhood rates of collective action and social altruism. Collective civic action events are much less strongly linked to neighborhood race or poverty than the other outcomes examined in the book. The chapter on altruism relies on novel data: the rate of return of ''lost letters'' that Sampson's research team dropped on the ground in several Chicago neighborhoods, and data on the rate of citizen CPR performed by neighborhood residents. Altruism by these measures is lower in more disadvantaged communities. Finally, Sampson shows that altruism is inversely related to the rate of homicides and the teen birth rate, and positively related to moral and legal cynicism among residents.

A later chapter extends place effects beyond the immediate neighborhood, showing that characteristics of surrounding neighborhoods often predict outcomes in the focal neighborhood (controlling also for other focal neighborhood characteristics). Sampson then addresses the results of the well-known Moving to Opportunity (MTO) experiment, arguing that the fact that many experimental group families stayed in or near poor zones of the city accounts for null effects of relocation on some outcomes, and this tendency itself reflects neighborhood effects on mobility.

The final analytic chapters examine neighborhood migration and connections among neighborhood elites. Most interesting from these is a migration analysis employing data on migration flows between all pairs of neighborhoods, which allows an analysis of how similarities between neighborhoods are related to the density of migration. The most important characteristic guiding migration is distance, but Sampson also finds distinct clusters of movement between contiguous groups of neighborhoods defined by levels of disorder.

If this sounds like a lot to pack into a single manuscript, it is. The reader may need a scorecard to keep track of all the empirical results. Yet the book also does more: it provides a history of the data-collection project that forms the basis of much of the book's empirical analyses, the Project in Human Development in Chicago Neighborhoods (PHDCN); it provides a methodological treatise on the measurement of collective phenomenon and their irreducibility to individual phenomenon; and it is punctuated by Sampson's personal observations of Chicago. Remarkably, Sampson mostly succeeds at making these many analyses and discussions cohere into a single narrative, always connecting findings and theories to key themes and arguments.

What emerges from these analyses is the idea that Chicago neighborhoods can be assessed along several key dimensions of neighborhood social space that can explain their variations in levels of crime, perceived disorder, altruism, and several other outcomes. These major dimensions include the ''usual suspects'' of neighborhood socioeconomic status (measured by income level or, inversely, by the poverty rate) and racial composition. These two traditional dimensions emerge as the most important overall. But he shows that several other dimensions are also very important, including the perceived level of disorder, collective efficacy, moral/legal cynicism, and position in the supra-neighborhood geography of Chicago.

Readers who are familiar with Sampson's articles published over the last 15 years will recognize many of the analyses and arguments in *Great American City*. Many of the chapters repeat these arguments, but with significant modifications. Put together, the empirical chapters cumulatively build a persuasive case for the importance of place and for the importance of the key dimensions of place which Sampson develops. One of the book's most important themes, that the temporal stability of neighborhood socioeconomic status is accounted for by the stability of cultural structures and the geography of supra-neighborhood spillover effects, is an argument only really developed in the book.

Sampson's empirical analyses are careful and rigorous. Yet as well-done as they are, they will likely not fully satisfy the high standards for causal proof held by our most careful methodologists. Often it is difficult to be sure that the intersubjective perceptions viewed as causes might not partly be the result of the outcomes a few years prior. For instance, it seems plausible that violent events in a neighborhood might make respondents answer some collective efficacy survey questions in ways that indicate reduced collective efficacy. Stability in neighborhood violence levels over time, combined with this effect on the survey responses, might account for some of the association Sampson finds. To his credit, he acknowledges these sorts of limitations. But it remains possible to argue with the causal story he presents.

Strangely, in a work of such great scope, the most notable limitations of *Great American City* are the elements of urban life that receive little mention. The book has a strong focus on informal neighborhood life and mechanisms of neighborhood social control, consistent with approaches of the ''Chicago School.'' With the exception of neighborhood organizations, formal institutions are generally left out: there is little discussion of neighborhood businesses or metropolitan economic conditions, little discussion of Chicago's troubled public schools or their private counterparts, and little discussion of city government or politics.

In fairness, no study can cover everything, and it would not have been feasible to add detailed consideration of these topics in a volume that already covers a tremendous amount. Surely, Sampson would agree these are important topics; instead, practical limits lead to their omission or de-emphasis. Yet when we think about the major neighborhood outcomes which the book considers, such as crime and migration, it is worth remembering that businesses, schools, and government are also important for these neighborhood outcomes, although their role is not explored in *Great American City*.

Likewise, the suburbs of Chicago receive only a few brief mentions. Since the 1960s, Chicago's suburbs have expanded dramatically, and two-thirds of the metropolitan population now lives in suburbs, many in far outlying areas more than an hour's drive from the central city. Suburban expansion is one of the most notable changes in the greater Chicago landscape since 1960. The book rightly emphasizes the highly interdependent nature of urban life across distinct neighborhoods, but the interdependence of the city and its suburbs, and the way in which these two parts form a system, each with a distinct role, is not considered. It seems likely that the heavily central-city residence of the PHDCN sample in part dictated the exclusive focus on the city proper.

The lack of discussion of businesses, schools, government, and suburbs does not undercut the key processes and arguments of the book, in my opinion, but they do illustrate that the explanations of the book are not fully complete. It is a testament to the extent to which *Great American City* adopts a modernized Chicago School of Sociology framework that one can reasonably apply some of the longstanding criticisms of the Chicago School—in particular lack of attention to business interests and political power—to the volume's analysis, even if this omission is driven more by practical than principled concerns.

All future studies, however, will have the considerable benefit of *Great American City*'s findings and example. The book will encourage future authors to take seriously the collective mental life of neighborhoods, showing that these can be measured rather than merely be the subject of speculative theorizing. The book argues persuasively for intersubjective perceptions as important dimensions of neighborhood context that are distinct from the beliefs of individuals, and that these perceptions and reputations have become stable cultural structures that contribute to the stability of the neighborhood hierarchy over time. Methodologically, the book also demonstrates the considerable benefits of combining theorizing with detailed empirical evidence and treating phenomena like the altruistic character at the neighborhood level.

Great American City has an analytical depth and importance that place it among the most important works of sociology in recent decades. It deserves a place alongside two great modern classics in urban sociology: Massey and Denton's *American Apartheid* and W.J. Wilson's *The Truly Disadvantaged*. Fittingly in a study of Chicago, the book consistently employs a modernized Chicago School of Sociology framework. Through its publication, Sampson establishes himself as our modern Robert Park, and *Great American City* can be justly regarded as the most sophisticated Chicago School study ever produced.