

EDITOR'S INTRODUCTION

Officially, this is the initial issue under the aegis of the new editorial team at the University of Iowa. The issue, however, is ours in name only since our editorial predecessors have—mercifully—left us with a large reserve of material to publish. So it is only appropriate to begin by expressing our team's gratitude to Alan Sica and the Penn State group for their good work and their generous assistance in the editorial transition. They have left us a house in good order: the foundation is solid, sensible protocols are in place, and the journal, by all accounts, is a respected and upstanding member of the community of sociological publications.

Given this state of affairs, readers should not expect major changes to the journal. Aside from some fresh paint and fiddling with the furniture, much will stay the same. We will, as have all of our predecessors, continue to squeeze as many reviews into the journal as our page allocation allows, doing our best to cover the field of sociology as well as the most relevant books from other disciplines.

We will also work hard to ensure that the essays and symposia that fill the opening pages of each issue—now a hallmark of *Contemporary Sociology*—remain vital and vibrant. We will continue the excellent series of critical-retrospective essays begun by the previous editorial regime while also experimenting with new forms. In this era of information saturation, *Contemporary Sociology* is positioned to serve as a kind of filter or aggregator for the world of social science books. Recent essays have pointed readers to important work in particular subfields. We will continue to do so, but we will also solicit pieces that introduce notable sociological books from other countries or regions as well as publishing essays on recent books from fields “adjacent to” sociology. It is our hope that these essays will serve as a resource enabling busy readers to keep abreast of what is happening in, say, political science or the sociology of South America; suggestions for future topics are welcome.

It is also our hope that this forum promotes constructive and lively discussions on these topics, encouraging authors to take the essay format seriously by expressing points of view and feeling free to stray a bit from a strict academic voice.

We would also like to take this opportunity to acknowledge the efforts of *Contemporary Sociology's* Editorial Board. Board appointments are designed to reflect as fair a representation of the demographics and interests of our discipline as possible. The work, however, can be painstaking and often goes unrecognized (although, it is worth emphasizing, never by the appreciative editorial offices), so it is not surprising that many people decline the “honor” of racking their brains every two months for the names of suitable reviewers for a long list of new books. We currently have an outstanding board with excellent new members rotating on. The journal could not be what it is without them, so please thank your local board members the next time you see them.

We would like to conclude by appealing to our readers' sense of community. Reviewing books is portrayed by some as a time-consuming and thankless addition to already overcommitted schedules. It is this. But it is important to remember that book reviews are also a valuable public good. At its best, *Contemporary Sociology* can bring together the full spectrum of sociological thinking. From Aging and the Life Course to World Systems, from Social Psychology to Population Studies, sociology of all stripes is represented in these pages. When you consider as well the number and variety of authors and reviewers that are called upon in the process of appraising over 500 books every year, it is not a stretch to argue that *Contemporary Sociology*, more than any other journal, best symbolizes the discipline of sociology as a whole. So when you get that invitation to review a book, try to think of it not just as a way to establish your standing in a particular area of study, or as a way to generate more name recognition, or even as

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a utilitarian exchange between yourself and all the others who have reviewed your books and manuscripts—although it is all these things. Think of it, too, as an opportunity

to ritually reaffirm your membership in the sociological community. How can you say no to that?

REVIEW ESSAYS

Relationalism Emergent

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There has been talk of relationalism in sociology for decades now. These two volumes, *Conceptualizing Relational Sociology* and *Applying Relational Sociology*, make an explicit play to capture the heart and soul of the discipline and send it on a relationalist trajectory. The attempt raises a series of linked questions: how relationalism should be defined, what is a relationalist agenda, and do these volumes advance that agenda?

The term relationalism is itself contested, even by the authors included in these two volumes. I have already taken a stand on this issue, so I should be clear that the way I see it, relationalism is a theoretical perspective based in pragmatism that eschews Cartesian dualism, substantialism, and essentialism while embracing emergence, experience, practice, and creativity. It includes some but not all social network analysts, field theorists, actor-network researchers, economic sociologists, a number of comparative-historical researchers, and of course card-carrying relationalists, such as Mustafa Emirbayer and Margaret Somers (Erikson 2013). These volumes are populated with a slightly different set of researchers: social network analysis and field theory are well represented, but so is critical realism; and there is an explicit attempt to draw in feminist theory (Sarah Redshaw's "Feminist Preludes to Relational Sociology") and Marxism (Kenneth Fish's "Relational Sociology and Historical Materialism: Three Conversation Starters"). John Dewey and Charles Peirce are cited here and there, but the legacy of Norbert Elias dominates the first volume, and Pierre Bourdieu has probably the second strongest presence and appears throughout both.

It is clear that the editors, François Depéteau and Christopher Powell, are interested

Conceptualizing Relational Sociology: Ontological and Theoretical Issues, edited by **François Depéteau** and **Christopher Powell**. Hampshire, UK: Palgrave Macmillan, 2013. 240pp. \$100.00 cloth. ISBN: 9781137379900.

Applying Relational Sociology: Relations, Networks, and Society, edited by **François Depéteau** and **Christopher Powell**. Hampshire, UK: Palgrave Macmillan, 2013. 229pp. \$100.00 cloth. ISBN: 9781137379917.

in drawing in adherents to relationalism. In fact, reading the volume straight through felt at times like being sucked into a vortex: in the beginning you are circling at some distance around the central point, but gradually advance to denser pieces focused more precisely around key issues. Perhaps as part of this strategy, Depéteau and Powell seem hesitant to flesh out too strict a definition of relationalism in the introductions. It is after all a collection of volumes presenting a variety of perspectives on this problem and too strict a definition runs the risk of excluding contributors; however, this leaves us with slightly anemic descriptions of relationalism as based around the importance of relations (*Conceptualizing Relational Sociology*) and as challenging determinism and essentialism within sociological theory and research (*Applying Relational Sociology*). Fair enough. Movements need members, and members are diverse—but there are risks to this strategy also.

One of these risks is incoherence. It is fair to say that much of the usefulness of theory is based in logical consistency. Theory builds

insight, reveals obscure connections, and drives research by generating new hypotheses. It does so largely through the logic stringing the elements of the theory together. Without the logic, theory is a set of unrelated observations about the world that does little to build the intuition necessary to interpret new events. The volumes and the project of relational sociology must necessarily navigate a tight line between embracing a large constituency and risking theoretical pastiche. And there is a little of the latter here, as many authors do not hesitate to draw a concept from Elias here, a concept from Bourdieu there, and a little from Dewey over here. There is a certain richness to this, and these touchstones also serve important legitimizing functions, but do all these bleeding fragments fit together into a coherent whole? Perhaps they do, but the reader deserves a more explicit investigation of this.

Christopher Powell offers the most explicit, comprehensive, and precise vision of relationalism included in the volume in his chapter, "Radical Relationalism: A Proposal." He lays out nine proposals as a foundation for the perspective. In truth, I am something of a relationalist myself, so all of these proposals sound good in and of themselves. However, considering their relation to one another, I become less convinced that they make sense as a whole. For example, the guidelines posit relations as the elementary units of analysis, but then suggest that we "Treat the concepts of 'structure' and 'agency' as opposed but equivalent" and further, we should "Treat macro and micro as relative, not separate" (pp. 197, 201). But what is structure and agency if relations are the elementary unit of analysis? Do relations have agency in that case? Are relations the agents? And how do micro and macro map on to structure and agency? Without making the links between these relatively complex theoretical concepts more explicit, it reads more like a list of (valid) complaints, than a generative basis for future investigation of the social world.

The volume also includes what I believe are potentially serious conflicts and inconsistent conceptual framing. The most important is in regards to the idea of duality. I find the word duality very problematic as it is one of those words, like sanction, that has

evolved over time to mean both one thing and its near opposite. In this case, (1) the complete separation between two distinct parts (i.e., dichotomy), as well as, (2) the inseparability and co-constitution of two related parts. The history of this confusion is long and interesting to think through, but suffice it to say that it haunts this discussion. It is my belief that when network theorists such as Ronald Breiger, Harrison White, and John Mohr use the term duality, they mean the latter: the intertwined, co-constitution, and interaction between two parts (Breiger 1974, Mohr and White 2008). Mohr and White make this more explicit in their co-authored piece on institutions, where they write that structural duality "is a relationship that inheres within and between two classes of social phenomena such that the structural ordering of one is constituted by and through the structural ordering of the other" (2008: 490). This definition is very different from a Cartesian or Kantian mind/body dualism, where the mind is distinct from the world, of a different order and essence, and is definitively not constituted through interacting with the world because it is prior to experience of the world.

This distinction matters for reasons beyond the easily remedied fact that dualism is both summarily dismissed and embraced as a key mechanism by various contributors because they mean different things by the term. Relationalists like to emphasize relations rather than differences between the things that are interacting. This emphasis has produced a little fuzziness about the nature of the things that are interacting. For example, relationalists have claimed in several places that focusing on relations can dissolve the dualism between structure and agency. The question is whether this means showing that structure and agency are the same thing (presumably relations) or whether it means that structure and agency are related and co-constituted, but nevertheless also represent two distinct phenomena. This dilemma currently seems to have the most traction in debates between the critical realist school and the mechanism-based approach of analytical sociology, where critical realism champions emergence, which implies difference, and analytical sociology embraces supervenience, which

implies correspondence. It also has very real implications for what types of research are most likely to be the most promising (for example, the embrace of supervenience leads to a strong emphasis on computational modeling in analytical sociology). Rejecting dualism wholesale can lead to a position that emphasizes the unity of social phenomenon via social relations, where Breiger, White, and Mohr's rehabilitation of dualism gives greater room for the emergence of difference in those phenomenon. Relationalists are going to have to come down on one side or the other, or come up with their own twist.

The way in which dualism is conceptualized suggests two different trajectories for a relationalist research agenda. Many dualisms import a totalizing quality into the perspectives that they inform. For example, everything is either in the material or the ideal realm. What is not of the mind is bodily and what is bodily is not of the mind. And similarly, the entirety of the world can be divided into structures and agents. At the core of this is the perception of the subject as distinct from the world they observe, which again is a duality that encompasses everything in the same way that what is inside this box and outside this box constitutes everything in the universe.

If the core of relational sociology is to focus on dissolving totalizing dualisms, such as mind/body, structure/agent, then the right thing to do may be to focus efforts on the perceptual boundary between the self and what is exterior to the self. In this case, it seems entirely appropriate to drill deeper down into the experience, constitution, and contents of social relations themselves. There are many reasons to believe that social relations (meaning relations between individuals) are exactly that which traverses this boundary, either because the individual recognizes itself in the exterior world through social relations (Simmel 1971, Levinas 1978) or because the mind is constructed through the experience of relations.

Debbie Kasper suggests this tack in her essay "Advancing Sociology through a Focus on Dynamic Relations" where she asserts fundamental principles of human relations and argues that establishing these premises

will go "a long way toward remedying the seemingly intractable incoherence in sociology" (p. 76). Nick Crossley expands our sense of the ontological character of social relations by comparing them to field positions in his chapter "Interactions, Juxtapositions, and Tastes: Conceptualizing 'Relations' in Relational Sociology." And Jan Fuhse extends work set out in his recent *Sociological Theory* article (2009) in the chapter, "Social Relationships between Communication, Network Structure, and Culture." There he draws from Nicholas Luhmann's communication theory to consider the problems of what "social relationships actually are, how they form and evolve, and how they connect to wider layers of the social" (p. 181).

The dualisms that Mohr and White describe do not split reality into two exhaustive categories. There are multiple dualisms that occur simultaneously within and across different cities, communities, and at all different levels of social, cellular, and physical organization. In the social world alone, there are countless institutions, and these institutions, according to Mohr and White, are all based on different dualisms. The agenda suggested by this approach is not so much "relations" as "mappings"—mappings that occur across many diverse phenomena. This move suggests an alternative agenda for relationalism, which does not focus on dissolving the boundary between the individuals and their environment (social or otherwise), but instead investigates how interactions between distinct systems produce dynamics, change, innovation, and difference.

The chapters in the volumes that exemplify this second trajectory mainly appear in *Applying Relationalism*. They are easy to pick out as the relations of interest in them are between non-human actors. Daniel Monterescu has a fascinating chapter ("Spatial Relationality and the Fallacies of Methodological Nationalism") on Palestinian-Israeli interaction in Jaffa in which the relation of interest is between space and nationalism. In "Survival Units as the Point of Departure for a Relational Sociology," Lars Bo Kaspersen and Norman Gabriel are centrally concerned with shifting relationalism from a focus on social relations to interactions between survival units, which are

autonomous and sovereign political communities. Osmo Kivinen and Tero Piironen are concerned with the relationship between social groups and their environment in their chapter, "Human Transaction Mechanisms in Evolutionary Niches—A Methodological Relationalist Standpoint." John Mohr focuses on the relation between practice and culture in his chapter, "Bourdieu's Relational Method in Theory and in Practice"). And Harrison White, Frédéric Godart, and Matthias Thiemann as well as Jorge Fontdevila and Harrison White are concerned with the relationship between netdoms in their chapters, "Turning Points and the Space of Possibles" and "Relational Power from Switching across Netdoms through Reflexive and Indexical Language." Although Margaret Archer's chapter, "Collective Reflexivity: A Relational Case for It," works through the perceptual boundary of human subjects by thinking through the process of "relational subjects being reflexive about their social relationships" (p. 145), she is not using relations to dissolve the difference between the self and the social, but instead to think through processes through which the social may emerge from a different phenomenon (i.e., the individual); thus putting her in the mapping across diverse, or distinct, phenomena camp.

Various authors acknowledge this tension, if in passing. Margaret Archer notes that her approach may be "unpopular among relationalists who want to keep their ontology flat" (p. 146). And Craig McFarlane, in his chapter, "Relational Sociology, Theoretical Inhumanism, and the Problem of the Non-human," criticizes relationalism generally for a humanist social ontology that ignores the importance of sociality in animals as well as humanity's relations with animals. This criticism might be extended to include materials and environment, and important factors such as the distribution of natural resources or the structure of river networks get short shrift in relationalism despite their significant impact on social organization.

The question of which agenda to pursue hinges upon both the importance of understanding our own subjectivity and perceptual apparatus in order to explain social outcomes, which is related to the extent we think of the social as an emergent

phenomenon, as well as the urgency of the social questions we need to address.

Will these two volumes push relationalism to the center of sociology? First it should be stated that Depélteau and Powell acknowledge the possibility that this goal may not be entirely suitable and that perhaps a Donald Levine-esque vision of multiple theoretical perspectives best suits sociology (Levine 1995). I believe they would still, however, argue that relationalism should be a larger component of the overall discipline. This, however, begs the question of the extent to which relationalism already dominates sociology. The volumes represent relationalism as a minority position, but is that the reality? Students of Harrison White are faculty members at most major universities, and their students fill many other spots (I am both a student of White's and a student of one of White's students). The editor of the most prestigious journal in the field speaks openly against simple linear models (Abbott 1988). The field of social networks is expanding at a rapid pace. Economic sociology, which was founded on a relationalist text by a student of Harrison White (Granovetter 1985), is an extremely healthy section of the discipline. Paul DiMaggio and Walter Powell's 1983 article bringing field theory to organizations has been cited more than 26,000 times. And John Padgett and Walter Powell's new book, *The Emergence of Markets and Organizations* (2012), promises to be extremely influential. There is a strong argument to be made that the new institutionalism, organizational ecology, organizational studies, economic sociology, social networks, and comparative historical sociology are all strongly relationalist. Perhaps an important step for advancing relationalism is going to be working to incorporate much of this already extremely influential work into an explicit and overarching theoretical framework. We might ask of these authors, for example, what is the difference between Elias's figuration (which is mentioned by several as a key relational concept) and an institution? They seem very similar to me. Perhaps we do not need a new word, and relationalists will find that they have conquered vast new territories in one small step.

Either way, these two volumes do valuable work in beginning to chart an explicit

framework for relational sociology. Mustafa Emirbayer's essay, "Relational Sociology as Fighting Words," ends the first volume by calling for relationalists to recall their reactionary roots and a past grounded in the criticism of mainstream sociological thought. But Emirbayer's own relationalist manifesto was solidly grounded in a wonderfully coherent interpretation of pragmatism. He did much more than merely criticize, and indeed I am not sure why we would need an overarching theoretical framework for sociology if the framework lacked consistency and a clearly recognizable logic, that is, was more than mere criticism. That is not necessarily easy to achieve, but it is a project well worth undertaking, and I am grateful to the editors and contributors for making real progress in this task.

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Democratic Ideals and Sobering Realities: The Lifeworlds of Philip Selznick and Amitai Etzioni

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While there is a constant output of books and articles about the founders and classics of the sociological discipline, much less attention is being devoted to the crucial figures of later phases in the history of the discipline. The two books under review here indicate a certain change in this respect. Two of the towering American sociologists of the first decades after the Second World War who later became major public intellectuals of international influence are the subject matter of these two thorough and well-researched monographs: Philip Selznick (1919-2010) and Amitai Etzioni (1929-). There are interesting parallels in the

Philip Selznick: Ideals in the World, by **Martin Krygier**. Stanford, CA: Stanford University Press, 2012. 352pp. \$65.00 cloth. ISBN: 9780804744751.

Etzioni's Critical Functionalism: Communitarian Origins and Principles, by **David Sciulli**. Leiden, NL: Brill, 2011. 504pp. \$140.00 cloth. ISBN: 9789004190436.

biographical developments of these two scholars that could nourish debates about the present state of the discipline. The

methods used in these two books for the reconstruction of the lives and works of these sociologists also merit serious reflection.

The book by Martin Krygier, *Philip Selznick: Ideals in the World*, is a remarkable achievement, both substantively and methodologically. In its first part it is particularly fascinating to read how the major writings of one of the leading sociologists of organization and administration are based on the experiences of leftist circles in New York City in the years before World War II. Selznick belonged to the group of mostly Jewish young intellectuals who later, like Irving Kristol, became the leading voices of American neoconservatism. He shared with them a deep interest in Leon Trotsky's writings and became active in the Trotskyist movement of the time. While conventional Marxism tended to deal with "bureaucracy" as a secondary phenomenon only, Trotsky interpreted the stabilization of Stalin's power as a bureaucratic distortion of authentic socialism. Selznick soon outgrew the remaining Leninist traces of his Trotskyist comrades and discovered two sources for an alternative articulation of his progressivism. One was American pragmatism, particularly John Dewey's philosophy and political theory.

Dewey at the time presided over the Trotsky tribunal in Mexico and demonstrated, with reference to his sophisticated understanding of the interplay between means and ends of action, how naïve many of the self-interpretations of social movements inspired by Marxism had been. Selznick became a life-long proponent of pragmatism. The other source was one of the classical works of early German sociology, Robert Michels' book on the sociology of political parties (originally published in 1911). Michels has become marginalized in the sociological canon because of his trajectory from syndicalism via the sociological claim of an iron law of oligarchical tendencies in mass organizations to his apologetic attitude toward Italian fascism.

Selznick's early work could be characterized as an attempt to synthesize Dewey and Michels, to remain faithful to the project of radical democracy without ignoring the unintended and unanticipated consequences every organization has. Krygier

correctly emphasizes that Michels had concentrated on the most pro-democratic political party in Germany in his time, and that Selznick followed him by choosing the Tennessee Valley Authority for a case study, namely one of the most ambitious American projects of a grassroots approach to governance and administration. Selznick's results (Selznick 1949) are as sobering as those of Michels, but the frequently-cited critique of pessimist "metaphysical pathos" in Selznick's study, as it was most forcefully presented by Alvin Gouldner (1955), is nevertheless misguided. "There is a pattern here that endures throughout Selznick's writings. It is the commitment to moral realism as a means to, rather than an alternative to, the striving for ideals" (p. 28). "Ideals in the World" is, therefore, the appropriate subtitle for this book. Krygier follows Selznick in assuming that Dewey alone would have been a source of pure optimism, and that Selznick needed a dose of Michelsian (and Niebuhrian) realism as an antidote. This is one of the few aspects of this study where the author is a bit superficial: the old stereotype of the naïve optimism of Dewey or of pragmatism in general is today obsolete.

Selznick's way led on to a sociological study of parties and to a synthesis of his insights into the logic of organizations, presented in his book *Leadership in Administration*. (1957). Here and in his following efforts to develop a sociology of law that sees the law in terms of human action and its creativity, Selznick goes beyond Max Weber in important respects. Krygier constantly and fruitfully compares Selznick's empirically-grounded theoretical work with Weber. It is worth mentioning that Selznick's work, despite its pragmatist inspiration, is often ignored both by neo-pragmatist philosophers and by that school of sociologists which claims to be a direct continuation of pragmatism, namely symbolic interactionism. A certain weakness of the pragmatist tradition, namely a lack of interest in the historical and sociological understanding of the state, is continued even in Selznick's work.

In the last twenty years of his life, Selznick's "themes became larger and his ambitions more 'visionary' than is common in the academy" (p. 272). His major works from that phase, particularly *The Moral*

Commonwealth (1992), are attempts to offer a theoretical grounding to the communitarian movement and to sketch the outlines of a contemporary "humanism"—a scholarly approach that "is on the side of particular empathic understanding over the search for general laws, historicism over universalism, concreteness before abstraction, "thick description" over the development of abstract laws, the *Geisteswissenschaften* over the *Naturwissenschaften*" (223). Whoever is looking for an alternative to a Foucauldian or Luhmannian anti-humanism will find rich inspiration in Selznick's lifework admirably presented in Krygier's book.

To the general public, Amitai Etzioni's name is much more familiar than Selznick's, but in the academy his reputation is much more controversial. Nobody can deny the extraordinary range of Etzioni's scholarship and publications, "from organizations and professions to international relations and space exploration, and from genetics and drug abuse to economic behavior and political corruption" (Sciulli: vii). Some social theorists (like the present reviewer) consider *The Active Society* one of the most impressive theoretical achievements in American post-war sociology, while others, when they look back on post-Parsonsian theoretical developments, completely ignore this work or belittle Etzioni's "entire body of work as largely policy analyses and advocacy or, worst of all, popularizations of received or basic sociological ideas" (viii).

Fortunately Etzioni has also found a deeply interested and fair-minded interpreter in David Sciulli. But Sciulli understood his task in a way that is very different from Krygier's approach. A comprehensive biographical study was less urgent in this case because Etzioni has himself published a deeply moving autobiography (Etzioni 2003). Sciulli, apart from brief passages, restricts himself to a theoretical study. This enables him to identify a core question of great relevance that he presents as the guiding thread through Etzioni's work. It is the question of social order, not in the highly abstract sense of classical (normative) functionalism, but as "the problem of democratic social order in a global economy" (p. 22)—a problem he paraphrases in the following words: "Is it possible to institutionalize

democratic norms and practices more fully, from governmental agencies to organizations and associations in civil society, while simultaneously competing more effectively in a global economy?" (p. vii).

In *Etzioni's Critical Functionalism*, a long (and somewhat repetitive) work, Etzioni's basic motives are traced back to his youth. Born as Werner Falk in Cologne (Germany), Etzioni emigrated to Palestine in the 1930s. Life in a kibbutz and active duty as a commando soldier when the state of Israel was founded led to a life-long interest in the virtues of community on the one hand and the dangers of violent conflict-resolution on the other. His most important early intellectual influence was Martin Buber's dialogical philosophy. When Etzioni came to Berkeley to pursue an academic career in sociology, Selznick was one of his teachers. Like him, he first concentrated on the sociology of organizations: some of his articles in this area remain citation classics. Sciulli skillfully demonstrates how Etzioni develops his macrosociology out of the insights gained in the study of organization, combining them with results from peace research and international relations studies as well as with a Buberian anthropology of human needs. Etzioni is presented here as going beyond Weber with regard to the specific problems of democracy—and beyond the symbolic interactionists because of their neglect of the state and of international relations. Sciulli interprets Etzioni's work as built "on the strengths of American pragmatism while compensating for its greatest weakness" (p. 32), namely an alleged exclusive focus on problems at hand. For Sciulli, Etzioni offers a synthesis of pragmatism with Parsons.

It is a fact that in terms of reception, Etzioni's *The Active Society* was a spectacular failure. Why? It is certainly true that the book was published at a time when the rejection of grand theory became hegemonic in American sociology. It is also true that this complex book was easily reduced to a mere expression of an optimistic view of increased state-organized planning, while its title (not "The Active State," but "The Active Society") should already have prevented this misunderstanding. But later, when a "new theoretical movement" (J. Alexander) set in, no belated discovery of Etzioni's achievements

did. As with the case of Parsons, there is more interest in this book outside of the United States, for example in Germany and Scandinavia.

Several interpretations of this phenomenon are possible. Some people will simply say that the book itself was a failure and that it did not deserve greater interest. In an important retrospective article, a prominent German social scientist, Fritz Scharpf (2011), makes the interesting point that Etzioni, assuming at the time that the era of *laissez-faire* ideology was over, neglected the role of the interaction and aggregation of individual actors and proved to be completely unable to predict what really happened in the Reagan-Thatcher period. Sciulli has a third interpretation. For him, Etzioni had relied on shared substantive-normative beliefs in American society which proved to be much weaker than expected. Etzioni's later turn to "communitarianism" is in this view (p. 129, n. 13) almost an act of resignation. If the active society does not emerge, due to a lack of strong basic norms and values, something has to be done to shore them up. Other more radical changes, like the pushing back of what Etzioni calls "capital corruption" in the American political system, will not happen without such a preceding moral reform.

Sciulli derives from Etzioni's disappointment in the reception of his most ambitious theoretical work the focus on more policy-oriented topics and on a critique of the micro-economics paradigm in Etzioni's later work, as well as his turn to organizational activism (i. e., the founding of the Society for the Advancement of Socio-Economics [SASE] and of the Communitarian Network). But Sciulli also burdens his generally convincing interpretations with constant presentations of an alternative approach, namely his own. The main point of the approach that the (late) author had called the "theory of societal constitutionalism" (Sciulli 1992) and that he had applied mostly in the sociology of professions and of law is a Habermasian emphasis on procedures—as distinct from Etzioni's reliance on substantive norms and values. While it is interesting to reevaluate Etzioni in the light of such an alternative, Sciulli goes very far in this direction. He admits self-ironically that a possible

subtitle for his book would have been "How Etzioni's social theory differs from what Sciulli calls societal constitutionalism" (xxv). I think that he should have chosen a different strategy.

Both Krygier and Sciulli are so exclusively interested in the scholars they are dealing with that they make no attempt to compare them with one another—despite the long biographical connection between Selznick and Etzioni and despite obvious commonalities. Both Selznick and Etzioni do not take their driving motives from the condition of an academic career. They are sociologists who do sociology for reasons that precede their careers and they never forget the wider societal picture. Both could be seen as pragmatists, but in the discourse about neopragmatism their writings remained totally marginal. For an adequate view of the history of American social theory in the last decades, both should be taken much more seriously.

Both books are rewarding in this sense. Authors of such books have to find a balance between reconstruction, critique and explanation. Sciulli should perhaps have been a little bit more guarded in presenting his own theory in the framework of such a monograph. Krygier, without being uncritical, chose a more modest approach. He calls his way of reading "holistic," and his book is neither a biography nor a sociological explanation, but a "portrait." Some readers will find this old-fashioned or even methodologically not sophisticated enough. It is certainly true that there has been enormous progress in the sociology of ideas in general and of the social sciences in particular in the writings of, for example, Charles Camic, Randall Collins, and Neil Gross (Camic et al. 2011, Collins 1998, Gross 2008).

They have overcome the speculative quasi-explanation of the earlier sociology of knowledge by focusing on the micro-conditions for creative processes. But in their works, the balance between a theoretically oriented creative appropriation of an earlier thinker's work and such sophisticated sociological explanation also has to be established again and again. For some theoretical purposes, the old-fashioned "holistic" reading at work in the books under review—and in some other writings of the leading

“sociologists of ideas” as well—might remain a good way to proceed.

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Centering the Edge in the Shift from Inequality to Expulsion

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“What are the spaces of the expelled” (p. 222). Most of you would be prepared to answer this question, but I doubt that any of you, without having read Saskia Sassen’s book, would be prepared to answer it with the same breadth, theoretical ambition, and dystopic challenge as she mobilizes.

The economic sociologist would likely point to the number of people outside the labor force. The environmental sociologist may respond with accounts of toxic dumps. The political sociologist might talk about people without rights, whether because they are displaced or incarcerated. And that is Sassen’s point. We have the tools to analyze each of these sectors, but we lack the conceptual arsenal, or perhaps even the sociological imagination, to conceive of their connection. Sassen is helping us close this gap.

Her concept becomes clear when paired with its predecessor. Most sociologists would argue that inequality is the key concept and problem of our discipline. That is because, Sassen argues, inequality’s centrality is associated with a century and system

Expulsions: Brutality and Complexity in the Global Economy, by **Saskia Sassen**. Cambridge, MA: Harvard University Press, 2014. 298pp. \$29.95 cloth. ISBN: 9780674599222.

we are leaving. We could debate measurements, causes, trajectories, and consequences of inequality because we have assumed the systems in which we thought we lived, more or less demarcated by sovereignties and motored by logics of incorporation whether through colonialism, commodification, or democratization. The edges of those processes might have involved genocide, enslavement, and impoverishment, but they also promised the acquisition of human and non-human assets. Expulsion is different, and is the key logic of the system destroying the world as we know it and defining the life, and death, we approach.

Expulsion allows us to view in common a variety of processes typically understood

by specialists within their own knowledge cultures. Among other things, Sassen explores fracking and mountain-top-removals not only for their technological achievements and energy outcomes but for their poisoning of water and damage to the ecosystem. The application of those technologies can lead to the expulsion of the proximate biosphere, and public health, from the system's logic.

She explores foreign land acquisitions driven to provide industrial and food crops as well as to mine rare earth metals and water. Here, Global South and Global North disappear as obvious categories because agents from South Korea, China, the Persian Gulf and others, alongside more familiar colonial powers, grab land and water most obviously in Africa and southeast Asia, but also in Russia and Ukraine. And as they grab, ways of human and non-human life are expelled from the system, with attendant poverty, death, and environmental destruction.

The financialization of the global economy and the securitization of mostly everything does not logically lead to expulsion, for it could generate the capital needed for virtuous projects. But it hasn't. That assemblage of experts making financial instruments led to the expulsion of the middle class from their foreclosed homes, municipalities from credit worthy recognition, and sovereignty from states. Greece enjoys extensive attention here, for its financial rebound within the system depended on expelling people from the system, from its labor force and public goods.

Sassen has many more examples, too. No doubt experts could trip her up on particular elements of her argument, or lament that she does not nuance her accounts with a focus on the various degrees of expulsion depending on different intersections of rules, technologies, and contexts. Those experts would, however, miss her point in such critique for they would overlook her distinctive methodology. Those familiar with her 2006 book are better prepared.

Although that 2006 work had its historical moments, she worked hard to evoke a "sociology of the incipient / yet-illegible" as her problematic. Her conceptual arsenal depended on notions like "capabilities,"

"organizing logics," and "tipping points" to explore how new constellations of territory, authority, and rights would form emergent logics of organization in the midst of the old. Some seven years ago, my former students and I (Kennedy et al. 2007) appreciated in those terms how difficult it was to recognize that emergent. This interim has clearly helped in refining her methodology, for Sassen has now moved ahead. She retains those old concepts but has developed new complementary approaches that allow us to see what she calls the "subterranean."

Sassen approaches various kinds of "facts" without embedding them in familiar knowledge categories. For example, she revels in the juxtaposition of analogous processes in Global North and Global South or in North America and Russia, to show that formal systems bear little impact on the brutal expulsions that exist on the edge of the system we inhabit but cannot see. She directs our attention to extremes so that the immanent logics buried deep in our knowledge cultures might become apparent from beneath the mounds of expertise that hide those harsh and deadly facts emerging as central but now only visible on the edge. She is most adept at rendering financial expertise, and how things like "Dark Holes" created the possibilities for monstrous failures. Sassen is drawn to environmental expulsions, the dead lands and waters that predatory formations enable.

Introduced before in her scholarship, Sassen nonetheless uses this concept of predatory formations to link "elites and systemic capacities with finance a key enabler, that push toward acute concentration" (p. 13) in the elaboration of conditions enabling expulsions. Although she speaks of expulsions of people and biospheres, one might argue that the key expulsion is a sense of responsibility and morality from the global systems that govern us. That's the point of complexity: we can admire the brilliance of lawyers, accountants, and physicists in developing those instruments that securitize, even while the system they make enables the expulsion of moral judgment from that world. Nobody is accountable because everyone acts according to laws designed to enable the superrich and powerful only to become more so at the growing expense of others. Looking for

variations in accountability only distracts us from the global logic that moves a system of the professionally accountable toward global destruction.

Coupled with her earlier work, this may be a paradigm breaking/making work, especially if those who engage it could address some issues circling around its edges. Two stand out for me.

On the comparative and historical side of our discipline, expulsions may shake those accustomed to knowledgeable life at the imperial core, but expropriation has been a longstanding theme of those who have been destroyed by colonialism's spread. Jonathan Kay Kamakawiwo'ole Osorio (2002) writes powerfully about what happened in Hawai'i, but his account is not the exception for the indigenous. His account is different, it seems to me, from Sassen's notion, but the distinction deserves refinement. She finds the system's organizing logic in "the ways some of the most powerful actors in the economy use people, governments, and the world's resources to ensure corporate economic growth with an absolute minimum of global restraints and as few local responsibilities as possible" (p. 220). Does the difference with past expropriations rest in the capacities of those who expel? In their motives or identifications? In the overall system of which they are a part? Or all of the above? That comparative inquiry attending historical context is important on its own, but it is also important for the future.

While I appreciate Sassen's effort to move beyond the limits of context to look for deeper and global systemic tendencies, I cannot escape it because I am as much drawn to questions of resistance and transformation as exploitation and expulsion. Sassen attends magnificently to the oppressive side of the critical theoretical triad with her predatory formations. She invokes the Keynesian model of growth as at least an eutopic norm. But it is harder to see the praxis which her account might inspire.

That might be because, as she reports in her autobiography, she has kept her full time engagements in politics and academics apart (2005:222–23). A decade ago, at least, Sassen would recall that her politics was defined by a struggle "against the abuse of power... more than power per se"

(2005:227). Her special interest in the lawful abuse of power is quite clear in this volume on expulsions, but her struggle against that system in this scholarship appears principally in its naming. There is, however, more there for those who wish to follow its implications for praxis and its scholarship. I think it begins with the different notions of causality implicit in this volume.

One might distinguish those causalities that produce, immediately and ultimately, the expulsions at hand and those which, if made sufficiently explicit, might disrupt their reproduction through changes in policy and mobilization by publics. For that latter causality to become apparent, one could return to her earlier (2005b) emphasis on imbrication. Then, she used it to show the variable effects of digital technology in context. This time, we might consider the variable articulation of struggles against expulsion in context.

For example, Sassen mentions that demineralization is illegal in Brazil, and in 2006 that law enabled residents to stop Nestlé from extracting more water (p. 196). It was too little, too late, but it was more optimistic than her other tales of expulsion. Praxis needs hope. It also needs visions that travel.

Thus, I would like to see a companion volume that could identify those local efforts that derailed expulsions from their most extreme expressions. Sassen herself notes variations in the ways states have responded to fracking—France, South Africa and others have banned it (p. 174), but Poland, Ukraine and others now embrace it as a source of energy security, with the eager support of the U.S. government and energy corporations. The logic of expulsion has powerful motors behind it, but its imbrication is variable. There may be different prior conditions, but those variations also could be shaped by mobilizations that follow.

I would then like to see how those notions of resistance and transformation travel, much like Baiocchi and Ganuza (forthcoming) explain the mutations of participatory budgeting as its practice moves across the world. And by assembling these forms of resistance and transformation in response to the deeper structures of expulsion which Sassen identifies, we might, in the end, have a better chance, if not of emancipation,

then at least at life. We might be especially able to live if we could recognize the logic of the system that emerges, and not only applaud the virtuous resisting the spaces of the expelled.

Sassen focuses on the deep structure generating expulsions across different spheres and places of our world. Her account, like many that get to deep structures, could be powerfully dispiriting, but I do not believe that is a necessary outcome. It is, however, a rallying call to craft a different intellectual and institutional responsibility, one that works across knowledge cultures to find connections in order to develop a praxis dedicated to the extension of survival. And here, we cannot rely on those expelled from the system to save us. Those not yet beyond that system need to find the path back to an incorporation of people and biosphere into our systemic logic. Otherwise the "generalization of extreme conditions" (p. 29) that began on the edge will consume us all.

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Beyond the Nation State and the Comparative Method? Decolonizing the Sociological Imagination

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George Steinmetz' edited volume, *Sociology and Empire: The Imperial Entanglements of a Discipline*, is a massive tome. Six hundred and ten pages with seventeen chapters, it really amounts to three very satisfying books in one, each of which takes on the question of sociology's imperial "entanglements" in a different way. If the book simply consisted of Parts II and III, Current Sociological Theories of Empire and Historical Studies of Colonialism and Empire, it would be fairly uncontroversial: an insightful addition to a growing list of studies being done under the general umbrella of "sociology of globalization" or "transnational and global sociology." The book's contributors aim to go far beyond simply providing additional theoretical accounts of colonial and imperial social formations and processes, however. Their mission is to arrive at something much

Sociology and Empire: The Imperial Entanglements of a Discipline, edited by **George Steinmetz**. Durham, NC: Duke University Press, 2013. 610pp. \$34.95 paper. ISBN: 9780822352792.

more profound and potentially destabilizing for the discipline as a whole. The authors' reflections on the "latent and manifest colonial assumptions and imperial ideologies informing current sociological theory and research" are undertaken with the aim of fundamentally altering sociology's current theoretical, methodological, and conceptual apparatus (p. xi). Therein lies the immense value of this edited collection.

The stage is set for this in Part I: National Sociological Fields and The Study of Empire.

This section, made up of five chapters, examines the historical development of sociology in Russia, the United States, Italy, Germany, and France. These essays question how we think about the relationship between social context (in this case the imperial context) and the content of sociological work. Since at least the 1950s, historians of sociology have been trying to understand the social origins of sociology, its objects of study, and its legitimate research practices. Having never developed a grand, overarching theory of social processes and having an unwieldy and ill-defined object of study ("society"), we sociologists have relied, perhaps more heavily than other disciplines, on constructing a history of who we were and where we have been in order to tell us who we are and where we are going. Attempts at writing (and rewriting) the history of sociology have been motivated by a desire to secure "the discipline's grasp of its own past and trajectory" (Calhoun 2007:x). Practitioners of what Turner and Turner (1990) call the "impossible science" have used this shared view of their history to overcome our lack of a common theoretical and methodological ground, the fuzziness of our boundaries with other disciplines, the questionable relationship of "general sociology" to its subfields, and the status of sociology as the "undefined residual category in the social sciences" (Turner and Turner 1990:23). That shared view of our history, the authors of a number of essays in this volume point out, has until now tended to "elide sociology's imperial orientation" (p. 101). In making clear the "depth to which sociological thought has been shaped by its location in empire," these histories of sociology's development in the various metropolises demonstrate how urgent the need is for us to revisit and question our core beliefs about the abstract universality of our foundational terms and concepts (p. 490).

One thing sociologists have been able to agree upon is that our intellectual task has been to "establish parameters for defining modernity both spatially and temporally" (Bhambra 2007:3). The foundational myth that has been our foundation story's conjoined twin is the myth of the "European miracle" or the idea that "the economic and social modernization of Europe is

fundamentally a result of Europe's *internal* qualities, not of interaction with the societies of Africa, Asia, and America after 1492" (Blaut 1993:3). The idea that modernity developed endogenously in Europe is a critical, if rarely acknowledged, part of sociology's conceptual architecture. The comparative method requires and assumes the analytical bifurcation of distinct domains—the "inside" and "outside" of nations; the "nation-state" and "empire"; the "domestic" and the "foreign" (Go 2013). The opening chapters constitute a profound challenge to received sociological wisdom because they show that "European/American modernity" was, in many ways, a product of "events, processes, and structures in the peripheries" (p. 2). Andrew Zimmerman's essay on Max Weber, for example, demonstrates how Weber's comparative studies of the economic ethics of Protestantism, Confucianism, Hinduism, and Islam, which attributed the failure of most regions of the world to achieve the rational, capitalist, economies of Protestant civilizations to a "cultural explanation of economic behavior," elided Germany's imperial entanglements (p. 181). Weber's argument that the "Protestant Ethic" was the motive force behind the development of rational capitalism in the West can only cohere if the existence of Germany's overseas empire in tropical Africa and lead role played by Germany in promoting the "internal colonization" of Prussia are ignored. Only through the analytical bifurcation of "East" and "West" can we compare the development of "the East" with "the West" (and find "the East" lacking) without acknowledging the dependence of "the West" on "the East" for its own development.

Saskia Sassen has made the point that sociology needs a "new conceptual architecture" that will help scholars to understand "the structurations of the global inside the national" (pp. 3–4). A key assumption in the social sciences has been that there is a correspondence between national territory, national institutions and the national, "i.e., if a process or condition is located in a national institution or in a national territory it must be national" (Sassen 2010:4). The seven essays that make up the volume's penultimate section, *Historical Studies of Colonialism and Empire*, make it clear that

sociologists' conceptual reliance on the nation state as a "naturalized unit of analysis, understood as a container of social processes" has never been particularly tenable (p. x). These empirical studies seek alternatives to the "analytic closure" that the abstraction "the nation" once provided (Sassen 2010:3). Besnik Pula's essay, "Urban Planning in the Colonial Cities of Italy's Fascist Empire" is one example of how to challenge "nation centered" ways of viewing the world. Pula demonstrates that urban planners and architects in Italy "saw in colonial empire the opportunity to demonstrate the social utility and transformative capabilities of their trade" (p. 390). In other words, the development of "national" traditions in architecture and urban planning could only be fully realized in the context of transnational engagements.

The chapters that make up Part III show the extent to which "Western" modernity is a product of what Bhambra (2013:296) calls "the colonial global." Essays on topics like "Nation and Empire in the French Context" and "State Formation in British Malaya and the American Philippines" not only explode the myth of Europe and America's endogenous development, they also throw into question the continued efficacy of theoretical and methodological apparatuses that have, until quite recently, un-problematically accepted this notion. Daniel Goh aptly sums up exactly what is at stake for all sociologists, not just sociologists of empire, when he explains why neo-Weberians have "carefully avoided the question of colonial state formation." The comparison of empires, he explains, "poses theoretical problems, since they involved a complex jostling of states, institutions, and social groups that defies the state-society framework. The comparison of colonial states also poses methodological issues, since these involved the extension of metropolitan sovereignty into colonial territories through subsidiary state [sic], thereby complicating the independence of the neo-Weberian unit of analysis" (p. 465).

Once sociology's "imperial entanglements" have been un-masked and the "Pandora's box" of new analytical possibilities has been opened, the question of what this means for the discipline as a whole still

remains. Do we finally abandon our quest for "grand theory"? Should we do away with the comparative method given its theoretical and methodological utility in displacing "imperial power over the colonized into an abstract space of difference" (Connell 1997:1530)? The four chapters that make up Part II, Current Sociological Theories of Empire, engage specific empirical questions such as the rise of a new security empire post-9/11 (Kim Scheppele) or China's imperial thrust into Africa (Albert Bergensen) in ways that seek to avoid the theoretical and methodological traps of previous studies. The purpose of the chapters is not to construct a singular, overarching "grand theory" of empire, however. The essays are more in keeping with Merton's idea of "middle range" theories, which recognize that any attempt to generate sociological generalizations must account for the fact that all complex systems are also historically variable systems. Michael Mann's essay, which asks whether the "recent intensification of American economic and military imperialism" are connected or not, concludes that "societies are not systems and states are not cohesive. In fact, they are both a bit of a mess, full of contradictions, muddles, mistakes." This is not to deny that societies have enduring and powerful social structures. Rather, it emphasizes the fact that "these are plural, with logics that are distinct" (p. 243). Bergensen speculates that we "may be entering a new twenty-first-century phase of neocolonial relations that are qualitatively different from what the world system has seen before" (p. 300). Although he cannot say for certain whether the evidence points to definitive proof of a new economic model or a "momentary instance" he nevertheless maintains that China's pursuit of economic dominance in Africa via state-owned enterprises may mean that "historical development has outrun the scope of social theory" (p. 311). Scheppele's essay on the "Terrorism and the New Security Empire After 9/11" likewise suggests that "empires are not what they used to be" (p. 244). Since empires are no longer "predictably universal" theories of empire must also seek to synthesize different schools of thought. "As we rethink this new security empire, we need to recall not just the

analyses of economic globalization but the lessons learned from postcolonial studies" (p. 252).

Sociology and Empire should be seen as an important intervention in a longstanding trend whereby sociologists have charted a new course for the future by rewriting the history of the past. As Gurminder Bhambra pointed out in her reflection on "The Possibilities of, and for, Global Sociology," our only hope of being able to "understand and address the necessarily postcolonial (and decolonial) present of 'global sociology'" lies in "reconstruct[ing] backwards" our "historical understandings of modernity and the emergence of sociology" (Bhambra 2013:296–297). For at least two decades sociologists have worried about their declining prestige in the academic marketplace. The questions that sociologists have been pondering relating to their "value" in the marketplace have now hit the social sciences and the humanities as a whole. At the same time, the world has grown ever more complex and interdependent and politicians and lay people alike ponder everything from terrorism to "post-racialism." A century ago sociology emerged in the context of the "political difficulties" and contradictions created by the co-existence of imperialism and liberalism (Connell 1997:1530). Sociology's theories and models of the world "offered a resolution" (ibid.). Today we find ourselves in a similar position. Sociology stands poised, once again, to provide a level of analysis desperately needed by policymakers and the educated public. As

Steinmetz points out in his preface, "today we are confronting two crises that are often experienced as separate but are actually interwoven: 'the crisis of the universities' and the 'crisis of empire'" (p. xiii). With the benefit of hindsight, such as provided by Parts I and III of *Sociology and Empire*, we can avoid making the mistakes we did in the past. The questions, problems, and theories for understanding the world today, such as provided in Part II, open up the exciting possibility that we can chart a different course for the future.

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The Re-Appearance of Race and Ethnicity

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This book provides a compelling and comprehensive account of the boundary making approach as it relates to race and ethnicity. It asks and answers key sociological questions: Under what conditions do ethnic groups and communities emerge? When will individuals strongly identify with ethnic or racial categories? How and why are racial

Ethnic Boundary Making: Institutions, Power, Networks, by **Andreas Wimmer**. New York, NY: Oxford University Press, 2013. 293pp. \$24.95 paper. ISBN: 9780199927395.

and ethnic boundaries sharper in certain contexts, associated with higher degrees of social closure and inequality, than in others? In doing so, Andreas Wimmer takes us on a tour de force with examples from around the world, and establishes a typology of ethnic boundary making. It elaborates upon how individual and collective actors relate to and enforce existing group boundaries by enacting strategies to shift boundaries or reinterpret their meanings, and employing practices such as discrimination, coercion, or political mobilization. The book also articulates a theory of ethnic boundary making, identifying the factors and mechanisms underlying the stability, salience, and reach of group boundaries, and tests key hypotheses with the innovative use of interview, network, and survey data.

The theoretical approach is a major achievement. Put simply, the ethnic boundary making approach asserts that ethnic groups do not simply appear as natural entities, each associated with unique cultural values and tight-knit communities bound by shared identity. Ethnic groups are made, and cultural differences and salient identities develop, under certain social conditions such as exclusion. At the same time, this does not mean that ethnic boundaries are fluid and in constant motion. Some group boundaries are relatively stable, and not all individuals can shift ethnic boundaries due to their positions in the larger hierarchy and access to resources.

The theory itself claims that institutions, resources, and political networks shape boundary strategies. Briefly, actors can respond to institutional incentives and choose to emphasize ethnic rather than other types of social divisions. The distribution of political, economic, and symbolic resources also play a role in this process, as they shape which boundary-making strategy will be pursued (i.e., boundary shifting, normative inversion), and the extent to which said strategy will be accepted by or consequential for others. In addition, established political networks determine the location of group boundaries—where group boundaries will be drawn. Importantly, the theory accounts for consensus and change, and also addresses how macro-level structures influence micro interactions, and how these

interactions feedback into the larger structure, which helps us to understand how and why group boundaries form. Ultimately, the ethnic boundary making approach claims that ethnic group formation is a process of social closure, where individual and collective actors work to increase their economic opportunities and political power, and institutionalize inequality (see Chapter Seven for a useful analysis that demonstrates how cultural differences emerge from a process of social closure rather than ethnicity).

The ethnic boundary making approach also turns our attention toward a deeper understanding of the processes, structures, and outcomes that we assume to be racial or ethnic in nature. For example, Wimmer draws upon interview and network data of residents from three neighborhoods in Switzerland to understand how and why social and symbolic boundaries develop (Chapter Five). Interestingly, the logic of boundary making that emerges is not based on ethnicity, race, or citizenship—as we might have presumed—but on an established-newcomer divide. Swiss, Italian, and Turkish residents view new immigrants as not fitting within established notions of order and decency, and therefore consider them to be distinct. Wimmer provides additional evidence to challenge our assumptions about the nature of race and its determination of group boundaries in his analysis of the networks of Facebook picture friends of American college students (Chapter Six). He demonstrates how the racial homogeneity of network ties is due to ethnicity, physical proximity, and a host of other tie-formation mechanisms, in addition to race. Such an analysis reminds us that there may be other factors underlying what we presume to be a racial process.

In general, the ethnic boundary making approach questions the essential nature of race and ethnicity, and emphasizes the importance of understanding these concepts in terms of social and symbolic boundaries, and struggles over categorization and classification within the context of power hierarchies. Yet, within the subfield of race and ethnicity, there has been a healthy resistance to such a framework. Perhaps it is because, as race and ethnicity scholars, such an

approach has the potential to undermine what we do: to study ethnic and racial groups as such. Traditional research in the area of race and ethnicity takes these categories for granted as embedded within the structure of the larger society and everyday life, focusing on the experiences and consequences of race. Instead of undermining this work, it may simply be that some race and ethnicity scholars are asking different questions and examining different parts of the racial and ethnic process than those who are interested in ethnic boundary making (i.e., the emergence of ethnic groups and cultural differences).

For some scholars, embracing an approach that focuses on boundaries rather than race per se shifts the focus away from racial inequality, oppression, and domination. Adopting an ethnic boundary making framework distracts us from understanding race as a fundamental structure in society and its consequences for life chances, minimizing the social facts of race and racism in American society. Because the study of race in the United States is often equated with the experiences of African Americans and their devastating history of subordination and oppression, to say that the social and symbolic boundaries surrounding their experiences may be due to processes other than racism or discrimination is viewed as problematic. Additionally, one may ask what we gain from the ethnic boundary making approach if individuals experience the world as "ethnic" or "racial." It could be argued that it does not matter which mechanisms or factors shape racial disparities because inequality is still a social fact and racial discrimination and closure are still operating. Does it matter that established groups in Switzerland are drawing boundaries based on perceived distance from the norms of order if material differences, and negative sentiments and threats, are directed at non-citizens? Does it matter which mechanisms influence network composition of college students' ties when blacks in the United States still experience racial discrimination, and for them, racial closure is a key factor in producing friendship ties? This is an issue that scholars will continue to debate (see Bobo 2004).

While the former view is not without its merits, I would argue that the project of

ethnic boundary making may be much closer to Michael Omi and Howard Winant's racial formation project than we might realize. In their seminal book, *Racial Formation in the United States*, Omi and Winant (1994:48) assert that race should not be reduced to ethnicity, social class, or another social indicator, but understood as operating as its "own autonomous field of social conflict, political organization and cultural/ideological meaning." On the other hand, they describe the pitfalls of thinking about race "as an essence" and aptly demonstrate how race is historically and politically constructed. They emphasize the importance of unpacking "racial projects" and understanding the ways in which race is enacted, produced, and institutionalized. More recently, Omi and Winant (2009:123) warn that recent theorizing about structural racism can be a slippery slope because it can lead to essentializing racial difference. To combat such a turn, they urge scholars to distinguish between race—struggles over the meanings of race and racial formation—and racism in their work. More recent studies of the social construction of race also take on a similar project by challenging claims about the essential nature of race and the notion of an individual's race as given (Morning 2009, Saperstein and Penner 2012, Saperstein et al. 2013).

If we are serious about de-essentializing race and racial difference and inequality, we need to be willing to examine how ethnic and racial projects may be driven by non-ethnic and non-racial forces, and how culture develops under certain conditions. In the process, we may find further evidence for the ways in which certain bodies, behaviors, and ideologies are racialized. This is precisely what key research in race and ethnicity has done. For example, in their study of achievement in North Carolina public schools, Karolyn Tyson and her colleagues (2005) found that in contrast to past research, the majority of black students were achievement oriented and the burden of "acting white" was not a key explanatory factor for their lower achievement levels. In fact, high-achieving black and white students were stigmatized in similar ways as "nerds" and "geeks." Most importantly, Tyson and her colleagues discovered that school

structures (schools with large black/white gaps in income and placement in advanced courses) rather than culture helped to explain when this stigma became racialized (also see Ainsworth-Darnell and Downey 1999, Carter 2005, Lewis 2003, Warikoo and Carter 2009). So if we understand, for example, whether and to what extent racial disparities in education, wealth, and access to housing originate from racial discrimination as well as other processes and factors (some of which may be impacted by race or ethnicity), we could have a deeper understanding of the social phenomena at hand and develop better policy solutions.

The call to interrogate race and ethnicity more systematically can also benefit our theorizing, which shapes our research. The ethnic boundary making project is one that advocates seeing how, when, and where race and ethnicity operate to produce inequalities. By not doing so, scholars may fall into the trap of making assumptions about how race and ethnicity work and subsequently assert faulty arguments and take on empirical projects with flawed designs. In a similar vein, Moon-Kie Jung (2011:389) argues that scholars working in the assimilation tradition have “built-in assumptions” about race, which often result in the omission or misspecification of the role of race in shaping the assimilation process. Part of this “racial unconscious” is reflected, for example, in uncritically using cultural values to explain the upward mobility of immigrant groups, or “oppositional culture” to understand the stalled progress of racial minorities. As Jung argues, studying the politics of national belonging rather than assimilation would lead us to examine unequal relations of power and struggles over resources rather than groups and their cultural differences, which is consistent with an ethnic boundary making approach.

What are the implications of using an ethnic boundary making framework for current research? Would the study of race and ethnicity disappear? This is the concern—what we now understand as racialized, and what individuals and groups may experience as racialized, will be simply understood as an aggregation of other types of processes at work, taking away from the power of race to shape individuals’ lives, their choices, as

well as their life chances. It is clear that race and racism exist around the world, but this social fact needs to be studied, as it still begs the question of how race became entrenched, what social conditions and mechanisms led to its emergence and reproduction, and why it has more devastating effects in certain contexts and time periods than others. We need to take the time to unpack race and ethnicity if we want a better understanding of the processes and outcomes related to immigrant incorporation, national belonging, intergroup relations, and inequality.

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An Old Idea Whose Time Has Come Again?

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"We hold these truths to be self-evident, that all men are created equal, that they are endowed by their Creator with certain unalienable rights, that among these are Life, Liberty, and Property." That is what the Declaration of Independence would have said, if Thomas Jefferson had gotten his way. It was Jefferson's colleagues on the committee drafting the Declaration who made him replace his proposed right to property with the more vague "pursuit of happiness." Jefferson's fellow Virginian James Madison shared his view that ownership of property should be acknowledged as a right, and wanted this right to be guaranteed by the Constitution.

The Citizen's Share: Putting Ownership Back into Democracy, by Joseph R. Blasi, Richard B. Freeman, and Douglas L. Kruse, begins with the writings of the Founders, and uses them to make the case that the distribution of property ownership should be returned to the central place it once occupied in American national policy. The Founders viewed the concentration of property ownership as a threat to the stability of democracy, and shared with Jefferson the view that "legislators cannot invent too many devices for subdividing property" (p. 16). During the first century of this nation's existence, this general aim of broadening the distribution of property was embodied in numerous pieces of legislation, from grants of land to Revolutionary War veterans as a reward for their service, through the Homestead Act of 1862. In contemporary economies, in which most people work for companies with many employees, the most practical way to broaden the ownership of property is no longer with individual homesteads, but is

The Citizen's Share: Putting Ownership Back into Democracy, by **Joseph R. Blasi, Richard B. Freeman, and Douglas L. Kruse**. New Haven, CT: Yale University Press, 2013. 293pp. \$38.00 cloth. ISBN: 9780300192254.

instead in the sharing of ownership and/or profits with a firm's employees.

This book makes the case for wider use of employee ownership and profit sharing, both as a goal of national policy, and as ways to motivate employees of individual firms. It would be difficult to find three authors who are better qualified for this task. For more than three decades, sociologist Joseph Blasi has been the nation's leading authority on employee stock ownership. For most of this time, the economist Douglas Kruse has been Blasi's colleague and co-author in a series of increasingly larger studies of employee shareholding in the United States. In 2012, Kruse was appointed to serve as a member of the President's Council of Economic Advisors. Economist Richard Freeman is a well-known authority on labor unions and compensation.

In the introduction and Chapter One, the authors cover the period from the Revolutionary War to the Homestead Act. The authors found more quotable lines from the Founders than they could integrate into the essay, so Chapter One ends with an appendix containing ten pages of additional quotes from John Adams, Alexander Hamilton, Thomas Jefferson, and James Madison. This is more than any reader can digest at one sitting, but I share the authors'

fascination with the quotes themselves. All of these Founders wrote with eloquence and passion on a subject that the nation has now virtually forgotten.

The authors do not cover developments that occurred in the late nineteenth and twentieth centuries until Chapter Four, but because that chapter provides a useful historical perspective on the contemporary developments that are described in Chapter Two and Chapter Three, I will review its contents first. In the nineteenth century, relatively few companies issued shares of stock that were publicly traded, but most were profit-seeking businesses. In this period, therefore, profit sharing was the most important way in which employees could share in the benefits of owning a business.

In the twentieth century, more businesses were incorporated, and shareholding by employees became an increasingly important alternative or supplement to profit sharing. Proctor and Gamble, a pioneer of shared capitalism, had both, with profit-sharing bonuses creating balances that employees can use to purchase shares of stock in the company. In the 1920s, employee stock purchase plans became increasingly popular, but in the 1930s, both forms of shared capitalism declined in the wake of shrinking share prices and nonexistent profits.

In the 1950s and 1960s, both profit sharing and employee shareholding made modest recoveries, often as part of "welfare capitalist" human resource strategies in nonunion firms. Their use would have been even further limited, if the Employee Retirement Income Security Act (ERISA) of 1974 had passed in the form in which it was initially drafted. To protect employees from the risk of losing their jobs and their savings at the same time, this law would have barred pension plans from investing more than a small portion of their assets in securities issued by the employer. Before this bill became law, however, Senate Finance Committee Chair Russell Long met employee shareholding advocate Louis Kelso. Kelso persuaded Long to include authorization in ERISA of a new form of pension plan that would be known as an Employee Stock Ownership Plan (ESOP) and that would have the explicit purpose of investing its assets in stock issued by the employer.

ERISA and subsequent legislation not only permitted the formation of ESOPs, but also offered employers an ever-shifting mix of incentives for doing so. One enduring advantage of ESOPs is that they alone among pension plans can use their assets as collateral for loans. For a company, this means that if a loan is routed through an ESOP, the principle as well as the interest becomes tax deductible as the loan is repaid. For an ESOP, it means that an ESOP that owes 20 percent or 30 percent of the stock in a company can take out a loan to buy all of the remaining shares. This in turn makes an ESOP an attractive way for a retiring owner to divest a firm by selling it to its own employees.

In Chapter Two and Chapter Three, the authors paint a vivid picture of the economy that this combination of history and legislation has created. Chapter Two is the most stimulating, because it uses portraits of one well-known and successful company after another, from high-tech leaders like Google and Apple to Southwest Airlines, to make the point that shared capitalism is already a much more widespread and accepted part of doing business in America than any other source has previously recognized. The example of Southwest Airlines is particularly attractive because there, employee shareholding is part of a diverse mix of employee benefits that includes profit sharing and a retirement plan whose assets must be invested outside of the company.

Later in Chapter Two, the authors describe such "failures" as Enron, Worldcom, and Lehman Brothers, in which employee shareholding led employees to lose both their jobs and their savings at the same time. When United Airlines declared bankruptcy in 2002, the corporation survived, but employee shareholders lost their investments. The authors' treatment of these cases is unsatisfying, because it attributes these failures to untrustworthy managers or faulty business judgment, rather than treating them as risks from which employees need to be protected.

Chapter Three supplements these portraits of individual companies with statistics on the prevalence of various forms of shared capitalism in the contemporary United States. Two virtues make this section stand out. First, with the help of unique questions

included in the General Social Surveys of adult Americans conducted by the National Opinion Research Center (NORC) in 2002, 2006, and 2010, this chapter provides the best estimates available on the prevalence of various forms of shared capitalism in the contemporary United States. Second, the questions used for this purpose differentiate among four distinct forms of compensation that are more commonly lumped together: profit sharing, gain sharing, stock options, and employee shareholding. The authors find that profit sharing or gain sharing together cover “about 40 percent” of American workers, and add that “This is nearly twice the 21 percent who have employee ownership in the company for which they work and four times the 10 percent who report having stock options in their employer” (p. 113).

Many of the survey results reported in *The Citizen's Share* originally appeared in a more rigorous previous work by these same authors (Kruse, Freeman, and Blasi 2010). Because this later work aims at a wider audience, it is less informative than the earlier work about details of the survey, such as the wording of survey questions. In *The Citizen's Share*, for example, the authors report that “Employee stock ownership refers to situations in which a worker owns a share of stock outright” (p. 111). Taken literally, this definition appears to exclude ESOPs, because in an ESOP individual employees own shares not directly but through the ESOP trust. The actual wording of the survey question used to measure employee stock ownership appears only in the earlier book. That work makes clear that employees were asked, “Do you own any shares of stock in the company where you now work, either directly or through some type of retirement or stock plan?” (2010:389).

In Chapter Five, the authors review evidence on the effects on employees of various forms of shared capitalism. Surprisingly, given the authors’ avowed preference for employee shareholding, profit sharing and gain sharing appear to be more effective in affecting the attitudes and behavior of employees. For example, in the NORC’s General Social Survey, “Workers with profit or gain sharing expressed the highest loyalty, while those with employee stock

ownership and stock options had somewhat more modest increases in loyalty that nevertheless still exceeded that of workers without these forms of shares” (p. 178). In response to a question about the extent to which a worker is “willing to work harder than I have to in order to help the company I work for succeed,” the authors report that “Workers with profit sharing and gain sharing were at the top of the willingness-to-work-hard ladder, whereas those who had only broad-based stock ownership and stock options did not differ from other workers” (p. 179).

Chapter Six is devoted to policy recommendations that the authors see as emerging from this research. Some are innocuous, such as disseminating information and measuring the incidence of shared capitalism. Some, like progressive capital gains taxation, the Founders would have loved. The most imaginative proposal would grant “baby bonds” to all newborn citizens to assure them of a capital stake at age twenty-one. The two that had me most worried would make shared capitalism a prerequisite to participate in government procurement and tax abatement programs or to receive “tax expenditures for business.” I would not expect much from forms of shared capitalism that are introduced solely to meet such a governmental requirement.

Overall, the book is a fascinating tour, but when I got to the end, I felt that the parts did not hang together well. The introduction and Chapter One offer stirring evocations of the words of the Founding Fathers. In Chapter Two, we learn that many well-known American companies make extensive use of both employee shareholding and profit sharing. In the middle of the book, we learn that employee shareholding exposes workers to unusual risks, and that profit sharing and gain sharing are both more widespread than employee shareholding, and more effective in motivating employees. The policy recommendations at the end of the book have the same broad legislative sweep as the introduction and Chapter One, but bear little relation to the intervening chapters.

As I watched profit sharing and gain sharing acquire increasing prominence in Chapter Three and Chapter Five, my first reaction was to feel that I had been a victim of bait-

and-switch. Chapter One and Chapter Two seem to promise a book about employee shareholding, whereas Chapter Three and Chapter Five sell the reader primarily on the importance of profit sharing and gain sharing. While this result comes as a surprise, credit is due to the authors for being good enough scientists to report these results clearly even though they are not what the authors were hoping for.

It is profit sharing rather than employee shareholding that emerges as the big winner and big surprise of this book. Profit sharing was on the minds of the Founders, when Congress stipulated in 1792 that to qualify for tariff reduction allowances being allocated to the cod fishing industry, ship owners would have to enter into written profit-sharing contracts with their seamen. Profit sharing also figures prominently in the profiles in Chapter Two, where we learn that Proctor and Gamble has been practicing profit sharing for more than a hundred years. Finally, profit sharing emerges as the statistical winner in the later chapters, whether one judges by prevalence (Chapter Three) or effectiveness (Chapter Five).

In light of the statistical superiority that the authors demonstrate for profit sharing and gain sharing, I find portions of this work overly enthusiastic about employee shareholding, and under-appreciative of the risks associated with this form of shared capitalism. For example, they suggest several times that the risks of employee shareholding need concern us only to the extent that the shares were paid for with cash that came out of the workers' pockets. Personally, I am just as worried when I hear that an employer is investing employees' retirement

assets in an undiversified pension plan, as I am when I hear that employees' spare cash is being invested this way.

While the authors and I might argue about which form of shared capitalism we like the most, this work should give all forms of shared capitalism a big boost. Both profit sharing and employee shareholding have rich histories in the United States, and play important roles in the American economy today. This work shines a bright light on both of them, and deserves a broad audience for doing so.

Sociologists who study work and organizations have a tendency to produce a large number of publications about a small number of concepts. At various times, concepts like "bureaucracy," "de-skilling," and "emotional labor" have mobilized whole generations of scholars. Blasi, Freeman, and Kruse are calling our attention to an issue that is potentially just as broad in its reach, but has until now been relatively neglected by sociologists. This past term I used this work as a textbook in an upper-division sociology class on "Alternatives to Bureaucratic Organization." Students appeared to find the content stimulating. I would encourage colleagues teaching similar courses to give it a try. The book is now available in paperback as well as hardcover.

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Disciplines, Area Studies, and Publics: Rethinking Sociology in a Global World

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Why did *Contemporary Sociology*, an official journal of the American Sociological Association, ask me to review these two books on Japan edited and written by anthropologists? This question sounds trivial and even irrelevant at first. However, when the question's three overlapping registers—why Japan, why anthropology, and why me (a Japanese sociologist trained in the United States)—are recognized, they should prompt readers of *Contemporary Sociology* to reexamine the relationship between disciplines and area studies, on the one hand, and the relationship between sociologists and publics, on the other. In fact, I suggest that this reexamination be an urgent task in an increasingly global world, where linguistic and institutional barriers that safely separated the observing-self from the observed-other are breaking down, as many anthropologists have already pointed out.

The first register ("Why Japan?") appears to be relatively straightforward. Both books present Japan still reeling from the triple disaster of March 2011—the earthquake, tsunami, and the nuclear accident—whose consequences reverberated far beyond the island country. Contemporary Japan therefore offers fertile ground for examining how people, organizations, and institutions cope with profound structural disruptions and ruptures caused by large-scale disasters. The nuclear accident, in particular, raised critical questions, both empirically and normatively, about the roles of science and technology in society and the relationship between experts and citizens in policymaking. Put another way, the case studies of contemporary Japan have the potential to help sociologists advance a theory of structural transformations, disaster research, and the sociology of science and technology, among others. In this sense, these two books on contemporary Japan should deserve attention even from those who do not study the country.

Japan Copes with Calamity: Ethnographies of the Earthquake, Tsunami and Nuclear Disasters of March 2011, edited by **Tom Gill, Brigitte Steger, and David H. Slater**. New York, NY: Peter Lang, 2013. 316pp. \$64.95 paper. ISBN: 9783034309226.

Precarious Japan, by **Anne Allison**. Durham, NC: Duke University Press, 2013. 246pp. \$23.95 paper. ISBN: 9780822355625.

This straightforward explanation, however, is incompatible with the second register ("Why anthropology?") because neither of the two books advances theory. Chapters in *Japan Copes with Calamity*, for example, collectively offer what one of its editors David Slater calls an "urgent ethnography" by painstakingly documenting everyday lives of disaster victims and empathically conveying the urgency of their struggles. As a result, engagement with existing theories in the social sciences, which is not "urgent" for people in Japan, falls outside the scope of the book. In contrast, *Precarious Japan* by Anne Allison does engage with theories of neoliberal globalization, flexible citizenship, precarity, and so on. However, her book merely uses contemporary Japan as a case to illustrate how these theories work and therefore falls short of pushing the frontiers of theoretical thinking. Thus, the two books, edited and written by anthropologists, do not meet the expectations that sociologists typically hold for case studies, as reflected in the aforesaid explanation of the first register: case studies are understood as most productive when they use rich empirical details to propose a new theory.

Then, why did *Contemporary Sociology* choose to review the two anthropological books that make no theoretical contribution? This is indeed puzzling because the majority

of readers are U.S.-based sociologists who are unlikely to care about non-American cases unless they advance theory (or methodology). A possible answer for this ironic question is troubling: the journal had to pick the two books because no sociological work was available in English on this important topic of how people in Japan coped with the triple disaster. To be sure, there are some English-language journal articles on the topic published by Japanese sociologists, but I do not think that the preference for journal articles among sociologists alone can explain the non-existence of sociological books worthy of being reviewed in *Contemporary Sociology*. In fact, this troubling phenomenon seems to be anchored in specific institutional configurations of disciplines and area studies.

To begin with, since disciplines are organized around sets of particular theoretical (or methodological) problems, as Andrew Abbott, Charles Ragin, and other methodologists point out, they normally focus on cases that are relevant to disciplinary debates. Thus, when sociologists submit papers on non-American cases to U.S.-based sociology journals, they often receive reviews demanding better justifications of their case selections in terms of existing sociological literature, even though papers on American cases are typically not subjected to the same degree of scrutiny. The asymmetry is understandable because readers of U.S.-based journals are mostly Americans interested in American issues and, even in the so-called age of globalization, professional activities of sociologists (and other social scientists) revolve around national associations. This asymmetry, however, seems to create intellectually detrimental consequences.

First of all, the disciplinary focus tends to turn studies of non-American cases into mere instruments for advancing theory. When I review papers and applications for U.S.-based journals and fellowships, I regularly see authors and applicants offering rigorous justifications for studying non-American cases. From the disciplinary perspective, their case selections are skillfully justified. And yet, their justifications are so disciplinary that I cannot but wonder, "Why do they have to go all the way to

non-American countries (e.g., Malawi) to answer these disciplinary questions?" because they could easily examine the same questions by collecting data inside the United States. Thus, the aforesaid asymmetry can be reversed and rephrased as follows: what is the point for U.S.-based sociologists to study non-American cases if their primary purpose were merely to contribute to disciplinary debates?

Here, area studies might help sociologists better appreciate the significance of research on non-American cases in their own light by looking beyond disciplinary constraints. Both *Japan Copes with Calamity* and *Precarious Japan* are essentially area-studies books, albeit to different degrees, and they describe everyday practices in the economy, families, neighborhoods, and other arenas of contemporary Japan. While the former provides detailed descriptions of the immediate aftermath of the triple disaster, the latter helps place these descriptions within the wider structural transformations of Japanese society since the 1990s. In contrast with typical sociological work dissecting non-American cases with analytical categories, area-studies books like these are often able to construct empirically rich narratives that vividly convey the atmosphere and texture of social life. Thus, the asymmetry between disciplines and area studies cuts both ways: disciplines are good at advancing theoretical debates, whereas area studies are good at documenting empirical complexities of cases at hand.

In principle, the respective strengths of disciplines and area studies do not have to be mutually exclusive. However, in practice, they often are, as exemplified by the ways in which the two books try to deal with anthropological theories and ethnographic descriptions. On the one hand, *Japan Copes with Calamity*, in a way, represents a deliberate refusal to engage in disciplinary debates. Even though chapters in the edited volume do refer to theoretical debates in anthropology, they give priority to "thick descriptions." This seems to have something to do with biographies of the contributors: some of them are Japanese citizens, others are foreigners who have lived or worked in Japan for many years, and all of them have strong ties with people in Japan, socially and

professionally. Take, for example, two of the editors, Tom Gill and David Slater. Both of them are college professors in Tokyo. They lived, and continue to live, the "calamity" that hit Japan. The majority of their colleagues and audiences are also Japanese citizens, as evinced by the fact that the Japanese-language edition of the book was published earlier than the English-language edition. When anthropologists live among "natives," they are likely to hesitate to impose theoretical interpretations on the latter's own words, concerns, and actions. This is partly because such imposition violates the sense of solidarity that the anthropologists feel toward natives and partly because natives can easily talk back to them and question empirical accuracy of jargon-laden explanations.

On the other hand, Allison's *Precarious Japan* can be said to be a failed attempt to combine the strengths of disciplines and area studies. From the very beginning, the book does not hesitate to impose theoretical interpretations on everyday lives in contemporary Japan by making extensive references to Judith Butler, Gaston Bachelard, and other social and cultural theorists. But, as mentioned above, the book fails to advance the frontiers of theoretical thinking not only because it merely invokes theories instead of systematically applying them to the Japanese case, but also because it does not deeply engage with empirical realities of contemporary Japan. Overall, the book offers very thin descriptions of Japanese people's lives, for Allison collected data mostly from her "hit-and-run" interactions and interviews in Japan, and from parts of Japanese-language books, newspapers, and movies that were translated into English. In fact, throughout the book, ordinary Japanese citizens remain nameless (e.g., "a young woman in her twenties told me," "I asked several Japanese people"), and only prominent Japanese intellectuals and activists, as well as Allison's educated Japanese friends, appear with names and are allowed to express their views in detail. This is an unfortunate example of what an anthropologist ends up producing when she can always fly back to the United States and write about Japan while keeping a safe distance.

Why are "natives" not talking back to Allison? One obvious reason is a language barrier. The majority of Japanese professors received their graduate training in Japan, and they teach and write in Japanese. As a result, even if they did manage to read *Precarious Japan*, they are unlikely to write a book review in English. Another reason is an institutional barrier. Since Allison is based in the United States, opinions among scholars in Japan do not affect her reputation among her U.S.-based colleagues. This institutional separation between American and Japanese academic communities creates incentives for U.S.-based anthropologists not to cultivate intellectually meaningful ties with Japanese scholars. In turn, Japanese scholars can remain indifferent to Allison's work because it has little impact on intellectual debates taking place inside Japan. (Ordinary Japanese citizens, too, can simply ignore the book because it is irrelevant to their daily lives.) Given the linguistic and institutional barriers discouraging collaboration between foreign and native anthropologists, the possibility of combining the strengths of the discipline and area studies (theoretical rigor and empirical richness) gets lost.

This is where the third register ("Why me?") comes in. Although *Contemporary Sociology* happened to ask me to review Allison's book, there are other "natives" who were trained in North America, Europe, or Australia and are therefore capable of talking back. As a growing number of students in Japan as well as in other countries go to the "West" to earn doctoral degrees, the linguistic and institutional barriers that previously provided "Western" scholars with a "luxury" (being able to ignore critical reactions from natives) are beginning to break down. This also presents a new opportunity for foreign and native scholars to engage in dialogue across national borders and collectively produce research that draws on strengths of both disciplines and area studies.

Nonetheless, this breakdown of the barriers seems to be uneven in terms of geographical areas and disciplines. Indian historians and literary critics, for example, have been more effective than their Japanese

counterparts in talking back to “Western” scholars partly because they are far more fluent in English. At the same time, Japanese scholars in the humanities seem to have greater presence in Western academic communities than those in the social sciences because the humanities are generally more open to area studies. In fact, many of the U.S.-based sociologists studying Japan (including those who were born and raised in Japan) lack the ability to combine respective strengths of the discipline and Japanese studies, as well as to engage with both Japan- and U.S.-based sociologists, because their research is often constrained to speak exclusively to disciplinary debates inside the United States. Above all, the majority of Japan sociologists, including myself, lack the ability to speak to Japanese citizens who, I believe, should be one of their most important audiences. In this respect, the critique that I raised against *Precarious Japan* is in effect a self-critique.

Moreover, the contrast between *Japan Copes with Calamity* and *Precarious Japan* can, and should, prompt U.S.-based sociologists to reflect on their own relationships with “publics” that are often missing from the formulation and dissemination of sociological research. U.S.-based sociologists, whether studying non-American or American cases, confront the similar institutional barrier separating them from objects of their research (e.g., American citizens). Americans that U.S.-based sociologists study rarely talk back because much of sociological research, driven by disciplinary debates, has little bearing on their lives. This seeming irrelevance of sociology in public life is one of the motivations behind “public sociology” advocated by Michael Burawoy and endorsed, albeit with modifications and critiques, by Craig Calhoun, Patricia Hill Collins, and many other sociologists.

Collectively, they have raised hard questions: for whom and for what purposes should sociologists write, how can the discipline be transformed to increase its public engagement, and what is the “public,” anyway?

Here, I suggest that a primary task of public sociology, and public social science more generally, is not to offer “truths” to guide publics to formulate efficacious policies, but to provide publics with descriptions of their activities, so that they can become more reflexive in trying to move toward more democratic and effective governance of their collective lives. In this respect, *Japan Copes with Calamity* serves as an example of public social science, notwithstanding its theoretical weakness, because it stays close to urgent concerns among Japanese citizens and illuminates various problems and challenges in responding to the triple disaster. Moreover, *Japan Copes with Calamity*, together with *Precarious Japan*, forcefully illustrate that “publics” are ultimately transnational, encompassing scholars, students, and citizens of multiple nationalities. It is crucial to recognize this transnational nature of publics in the contemporary world, where academic communities are beginning to traverse not only national borders, but also all sorts of problems, ranging from economic inequalities to ecological crises, are increasingly global.

Thus, in the eyes of a Japanese sociologist trained in the United States, the two anthropological books bring together the two related, but often disconnected, debates on disciplines and area studies and on sociologists and publics. To deepen this critical self-reflection in the transnational context, rather than to advance theory in the American, disciplinary context, the two books on contemporary Japan, coping with the triple disaster, deserve to be taken seriously by readers of *Contemporary Sociology*.