Spector and Kitsuse's 'Radical' Theory of Social Problems, Forty Years On

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Malcolm Spector and John I. Kitsuse called their constructionist social problems theory "radical" relative to what had been offered before in the sociology of social problems. In this brief essay I argue that the radical ideas of their Constructing Social Problems remain, in many ways and some forty years later, still quite radical next to most of what is written in sociology under the banner "social problems." Reconsidering those radical elements is the aim here, making clear just what that claim summarizes as both the excitement the theory still offers us and, at the same time, the grounds for it being resisted by most professional sociologists.

That I and others, including Malcolm Spector himself, are today still writing and talking with colleagues about the "constructionist approach" to the study of social problems is gratifying if not remarkable. It's not that a more than forty-year sustained interest in an academic theory is unusual, of course, but perhaps it is notable that some of its earliest authors are still alive and part of these discussions. More importantly, the arguments written in the mid-1970s by Malcolm Spector and John Kitsuse (2001) in their *Constructing Social Problems* remain, to borrow one of their concepts, "viable," which is to say alive among professional sociologists. Beyond the biographical relevance of this work to the academic careers of early contributors, this viability is marked by the 2017 session at the annual meetings of The Society for the Study of Social Problems in Montreal, which drew together both long-time and new adherents and interested others (Michael Adorjan, Joel Best, Jim Holstein, Peter Ibarra, Donileen Loseke, Dorothy Pawluch, Malcolm Spector, and myself; with Stephen Pfohl in the audience). There are plans for a special issue of or section of papers in the American Sociological Association's journal, *The American Sociologist* as a result of the Montreal session. And this present collection of papers in *Società Mutamento Politica* gives further evidence of the ongoing interest in Spector and Kitsuse's ideas. Finally, the economic viability of the constructionist approach to social problems is apparent in that their book is

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still in print. John Kitsuse, who, sadly, is no longer part of these exchanges, would, I am sure, smile his usual modest and bemused approval.

I

My aim here is to consider briefly a few so-called "radical" elements of Spector and Kitsuse's argument on social problems theory that I think contribute to its longevity as still provocative scholarship within sociology (see Holstein and Miller 1993; Holstein and Gubrium 2008) and to its ongoing relevance as a critical strategy for claims-making as political action in public life. All of these elements marked the difference of their theory on social problems from those that had come before. Those perspectives provided the points of departure for their argument and critique. In this, there are obvious parallels to elements central to the labeling perspective in the study of deviance that also have made that argument still central to sociological research and thought, more than half a century after Howard Becker (1963) and John Kitsuse (1962) wrote some of its signature texts.[1]

II

Central in the constructionist argument Spector and Kitsuse made is the seemingly simple point that Spector makes again in the interview published in this issue: "Social problems are what people think they are." What made their book and its argument so important is that they insisted on taking that point seriously, in and of itself, as a guide to the development of an academic and scholarly tradition of sociological research and writing. No sociologists writing on social problems had done that before. That remains its guiding and still valuable insight (Schneider 2018).

Spector and Kitsuse of course were not the first to argue that how people define something constitutes—dynamically and in process—what they take it to be and how they might act toward it. Such an insight is at the heart of the symbolic interactionist tradition, from which Becker's, Kitsuse's, and Spector's scholarship and teaching variously emerged. But the genius of their argument

[1] Although these two authors both drew on core elements of the symbolic interactionist tradition, Kitsuse referenced Lemert's (1967) "deviance as societal reaction" argument while Becker reflected the influence of Everett Hughes' (1971) attention to collective processes, subcutures, and "careers" in his writing on occupations.

is to insist on what they themselves then called a "radical" position, elements of which might have meant something somewhat different even between the two authors. I think it is the very "radical" qualities of their argument that contribute to the still-lively scholarly conversation around their work.

Their critical review of the earlier sociological writing they called the "value conflict" perspective made it clear that the very insight they wished to foreground—that claims, definitions, and action organized by those claims and used by "claims-makers," in and of themselves, bring "social problems" into existence—should be the guide to inquiry. If social problems are what people think they are, then what people "think," and, more importantly for an empirically-based social science, how such thinking is, arguably, seeable in what they do—in "action"—should be the guide to inquiry. In short, claims-making or "definitional activities" constitute social problems as moral activity, just as, if too simply put, "labeling" brings into view the moral marking that the concept "deviance" announces.[2]

They had to insist on this point, given what they had seen in that prior work, which, ostensibly, also took the idea seriously that social problems are what people think they are. And it of course was not that those writing this prior work tried or intended to write internally contradictory or theoretically muddled arguments. Rather, either through an ill-conceived commitment to a "commonsense" or a "knowledge" shaped by the givens of their own sociological training—and no matter their professed agreement with the definitional tenet—they found it essentially impossible to insist on it as the constitutive feature of "social problems" as a scholarly topic worthy of theory and empirical research in its own right. As Spector here notes, Kitsuse and he could see the difficulty in maintaining this focus in Becker's early writing on deviance (e.g., the misstep of the "secret deviant" [Pollner 1974]). Even the founder of the labeling argument—who did not make it a point to call his view "radical" and who reports that he had no sense of "founding" anything—missed seeing how counter-intuitive his claims about the "secret deviant" were/are; and perhaps especially so for sociologists—and the ease with which a preference for the familiar normative and structural assumptions of the discipline can override the key constructionist insight thus offered. That Spector and Kitsuse themselves would reiterate that misstep even as they set forth their "radical" view (i.e., the marijuana example, pointed out by Woolgar and Pawluch 1985, 216; and see also their comments on tonsils, Spector and Kitsuse 2001, 43-44)

[2] I understand the notion of "related activities," relative to claims and definitions, to be those that arguably and demonstrably are shaped with and from the meanings that the claims and definitions—the words, most simply—used by participants convey.

only serves to underscore both the particularity of their argument and offer evidence of the ontological weight of conventional sociology itself (that is, its proffered view of what the world is, after all, "really like").[3]

What their radical view wanted to hold at bay, of course, was the seemingly obvious reference to "undesirable conditions," "social context," or "social factors" as both the causes and the essence of social problems. The parallel for the labeling argument is Becker's reference to the notion of "rule breaking" and the idea that deviance is, simply, transgression; full stop. But if social problems and deviance are to be defined as "what people 'think' they are," and if one wants to take that claim seriously toward making social problems an academically respectable subfield of sociology, writ inclusively, conditions, social context, and rule breaking have to be treated as resources "people" use to "say" what they "think" is problematic in their worlds and lives. Those terms and ones like them cannot then also be an explanatory resource for professional sociology without calling into question what it offers distinct from the vision and analysis of claims-makers themselves. This insistence on keeping distinct the agendas and resources of claims-makers, on one hand, and the parallel but different resources of those who study them is the ethnomethodological insight Kitsuse no doubt brought from his early work with Aaron Cicourel (Cicourel and Kitsuse 1963). Such analysis gives "conditions," "context," and "rules" to those they called "members"—claims-makers in our authors' terms—and makes the particular versions of these terms used part of the constructionist sociologist's data.

The controversy around what some critics chose to call Spector and Kitsuse's "strict" constructionist argument as distinct from what they themselves named a "contextual" position (e.g., Best 1989, 245-46) blurred this distinction and the key point our authors made about social problems as a sociological topic. The so-called contextual position always struck me as a revised version of the "reasonable compromise" that they had explicitly rejected in earlier social problems writing. I do not think Spector and Kitsuse, in taking this "radical" stance, were offering a thoroughgoing critique of sociology, which seemed to be what some who championed this criticism implied. Rather—and with notably less congeniality than Becker's proposal on deviance[4]--they insisted that if we want to treat social problems seriously as an intellectual

[3] Surely, I attest to this same misstep, as noted by Woolgar and Pawluch, in my own work at the time, so pointing to Becker and Spector and Kitsuse is here is to emphasize the effort necessary to resist the familiar explanatory moves around these topics in the discipline.

[4] Becker, in presenting his argument, stated that it is one way to think of and see "deviance," but that there are other viable sociological approaches. Spector and Kitsuse refused this compromise position on how to study "social problems."

and academic topic, the position of the sociologist as expert participant in claims-making, could not be taken up. Spector and Kitsuse did not claim that sociologists should give up contextual and structural accounts of, say, poverty, violence, rule breaking, undesirable and of course ostensibly "desirable conditions" of all sorts. This is clear in the first pages of their book, where they distinguish the sociology of the "social problem of organized crime," on the one hand, and the sociological study of "organized crime," per se, on the other (Spector and Kitsuse 2001, 2-5). This insistence is as important today as it was when Spector and Kitsuse first wrote their papers and then their book in an attempt to consider what a "theory" of social problems might be. To compromise here is to erase the essential and still productive contribution that they made.

III

"Putative" is an extraordinary word. Prudence Rains (1975), in an early and important paper, suggested it as a "careful" way to talk about deviance as understood in the societal reaction perspective of Edwin Lemert (1951), later extended by John Kitsuse (1962). Kitsuse had used the notion of "imputation" in a paper on deviance to underscore the centrality of definition to the constitution of deviance. Spector and Kitsuse made "putative" their preferred way of referring to "conditions" typically thought central to social problems. It is, at least in sociology, another radical move. Rains (1975, 3) notes: "Like the term 'alleged,' 'putative' is intentionally, even ostentatiously, careful talk, allowing one to speak of something without commitment to its actuality." For the societal reaction argument, she notes, this word inserted a central ambiguity around the question of what deviance as societal reaction is a reaction to. Our authors push this word to do work for them beyond securing ambiguity. The *OED* tells us that putative is: "That [which] is commonly believed to be such; reputed, supposed; imagined; postulated, hypothetical."[5]

In the use of this word as an adjective to modify "conditions"—still arguably the favored sociological candidate for cause and/or constitution of "social problems" (see, e.g., Treviño 2018)—Spector and Kitsuse announce they will remain agnostic as to the relevance, even the existence, of these alleged phenomena. For some readers, this is not only "going too far," but is a funda-

[5] (http://www.oed.com.cowlesproxy.drake.edu/view/Entry/155203?redirectedFrom=putative#eid). Given this definition, they might initially have pushed this word just a bit farther, saving themselves some later efforts at clarification.

mental refusal of their obligation as sociologists; as social scientists. And given Rains' use of the word "actuality," critics might have misread—willfully and not—a larger denial of the closely-related term, "reality." As Bruno Latour (1999) makes clear from his experience in the science wars, some skeptics critical of and/or misunderstanding the presumptions of "construction," might read "putative" as an affront to all foundations of anything called "science," as well as commonsense, with which sociology always must make some workable connection.

Spector and Kitsuse surely were not denying reality in their use of this powerful term. Indeed, the need for a more solid reality in their search for defensible empirical materials in social problems sociology is one of two bases on which they premise their approach. Rather, their use of putative should be seen as a quite particular ontological indifference with respect not to what exists but to what will be taken, with theoretical warrant, as relevant data. It signals that they will not consider the very element that typical sociological understandings of social problems had given—and still do give—pride of agential and ontological place. This may seem like another version of the "ontological gerrymandering" that Woolgar and Pawluch so clearly and importantly detailed. Is this then merely a "selective realism" instead of the "selective relativism" that they had identified in so much published social constructionist writing; the same sort of error but in the "other direction"?

There is I think a simpler and more familiar way to look at it. First, such a move is, in effect, precisely what much quantitative and structural sociology does in its turn away from the processes of meaning construction typical of narrative and discursive analysis or what is sometimes called "interpretive" or "qualitative" sociology. This turn, as it is in Spector and Kitsuse's work, is also typically warranted by the theories and methods those sociologies use. A critique of inconsistent and contradictory analysis is one thing—to be taken seriously, of course—but a theoretically-warranted turn toward what our authors call claims-making or definitional activities is hardly in itself to be rejected as illegitimate.

Second, I direct us back to Woolgar and Pawluch's (1985, 224) productive essay. They offer three possible readings of the charge that their paper's title names: as (1) a "handbook" for how to "do" social problems analysis of this sort; (2) a critique of the inconsistencies found in the social problems analyses that call themselves "social constructionist" and a request for more care in how these studies are done; and (3) an illustration of the inevitable and essential dilemmas that all such explanatory argument of this scientific sort faces. They here refer to a dilemma that Woolgar (1988, 30-39) later detailed in his writing on science studies as the "methodological horrors." There he argues, as he

does with Pawluch, that the ontological gerrymandering under examination is a dilemma inherent to the sort of explanation common to science and thus to the work in question; that it is a dilemma that cannot be solved, but rather presents such inquiry with something to be "managed" rather than denied or not seen (which was the case, it seems to me, in the social constructionist writing that the critics considered in their paper). They write: "the inconsistencies we identify…are not mere technical difficulties in social problems arguments, but pervasive features of all attempts to explain social phenomena" (Woolgar and Pawluch 1985, 224). This, arguably, is what Spector and Kitsuse were offering in their constructionist theory: a strategy for managing this dilemma is their move to agnosticism on "conditions" except as found in the claims of members' constitutive work in making social problems; that is, except as members' claims, as Ibarra and Kitsuse (1993, 30) point out. That more than a few authors using Spector and Kitsuse's argument, myself included, made errors in following that theoretical direction (reading 2, above) is not an indictment of the argument itself.

In marking this refusal to address conditions independent of their appearance in members' claims, they make another bold move. It foregrounds what has come to be called the "positionality" of the analyst. In stepping back from the uncritical use of the term "condition," they – to borrow a word from Rains – "ostentatiously" distinguish their own stance on the question of what will be taken seriously as a resource in their analysis. Some could see this as an unseemly level of self-reference or regard, especially for scientists; as an inappropriate insertion of or attention to their own action or presence into what they are studying. I prefer to see it as a provocative and productive token of reflexivity in their analysis, saying, in effect, "we don't do it that way because, as noted, 'social problems are what people think they are.' QED!"

Closely linked to "putative conditions," our authors offer another provocation: that it is the "viability" of claims made rather than their validity that deserves our attention in the study of social problems (and, indeed, well beyond). This insists, yet again, that we take the words, meanings, characterizations, "quasi-theories" (Hewitt and Hall 1973), and what Pollner (1987) called "mundane reason" and related actions by claims-makers seriously as empirical instances of "what people 'think'" – about what social problems are. Like "putative," viability directs sociological analysis to search not for whether the claim is "true" or accurate – hoping thus to secure for the sociologist of structure and context a "safe" place to stand as expert – but rather whether and how it is sustained across time/space and various claimants. Again, our authors step away not from a realist argument but rather from one that focuses on observational matters that their theory writes as irrelevant to their very

question: what are social problems in sociology and how might they be studied in a more productive and scholarly way?

Both in the refusal to regard conditions as other than definitions – that is, as relevant to their analysis only as found in the claims by participants – and then keeping the bright light of analysis on those definitions and how they are reiterated, challenged, changed, dropped and linked by members to related actions rather than evaluating them as true or false, our authors pushed sociologists to study empirically something central to "morality-in-the-making" (cfr. Haraway 1997). They thus draw our attention to what conventional concepts of structure and aggregate as explanations of social problems either background or leave out of the analysis altogether.

Making the viability of members' claims the focus of attention avoids the familiar expert move to give epistemological priority to the sociologist's scientific and ideological claims over against those of the people studied as they mark moral aspects of their own lives. Spector and Kitsuse criticize the functionalist theory of social problems foregrounded in an influential textbook by Robert Merton and Robert Nesbit (1971) on this matter in their sociological claims that presumed to "correct" people about their own moral and ethical judgments. While allowing that through certain technical training a sociologist might claim more accuracy in claims than social problems participants, Spector and Kitsuse (2001, 36-37) challenge what they call a "professional ideology" apparent in Merton and Nesbit's functionalist position that imagines "the sociologist as the conscience and protector of society." By focusing on the viability rather than the truth or accuracy of member claims, Spector and Kitsuse refuse the position of moral experts who claim that they "know better" than the lay or non-expert citizen about their own moral and ethical judgments. Again, this is not to suggest that our authors eschew a realist ontology and familiar epistemology. It does, however, represent a refusal to adopt such an evaluative view on the very question of moral judgment itself. If "social problems are what people think they are," just as, in another homely but instructive phrase, "deviance is in the eye of the beholder," the sociological apparatus used to examine these phenomena in the world should not be in the business of refusing or denying precisely what "people" think and see.

IV

And while perhaps I cannot today call an insight dating to the 1940s still "radical," one more piece of Spector and Kitsuse's argument that I'm quite sure still "feels" radical to many readers is their use of C. Wright Mills' (1940)

take on values and motives. Long a challenge for sociological analysis, if not for all social science, our authors use these familiar terms in a way that reiterates their turn to "what people 'think.'" That is, elaborating Mills' insight evident in his notion of "motive mongering," values in social problems inquiry are marked as linguistic resources that members specifically and explicitly deploy in their writing and speaking to characterize themselves, others, situations, and objects. Rather than technical and explanatory resources, which bring their own problems, Spector and Kitsuse (2001, 91-95) treat "values" as words or phrases typically used by participants in definitional activity to "ground" or ostensibly defend their claims made or to be made, wittingly and strategically—or not. The consistency here is apparent both in terms of staying focused on what those studied say and do as the "data" for analysis and also in a refusal to comment evaluatively on the value language members use in their claims. It is both the theoretical consistency that such a move enables—we remain within the definitional space of members—and the turn away from the vague, circular attempt to portray values as causing behavior, that appeals.

V

These various refusals of the conventional position of the social problems sociologist as scientific expert on "undesirable conditions" as well as the arguably proto-reflexive stance toward their own work, are at the heart of our authors' contributions to social problems theory. I think this is invited—even if implicitly—in the way they critically examine prior theory and research. If I hadn't seen it clearly before Woolgar and Pawluch's critique, it became crystal clear after: that Spector and Kitsuse's critical examination of how sociologists of social problems before them made their arguments might be turned deconstructively toward their own theory; if not by them, then by others. As Woolgar and Pawluch suggest, and drawing on insights from science studies (e.g., Kuhn 2012; Latour and Woolgar 1986; Latour 1999; Haraway 1997), it is apparent that our authors are, from ethnomethodology, "members" too, a point they explicitly do not deny, even if they do not pursue that question in their argument (see Schneider 1993). They invite this in raising the question of the location of the sociologist relative to whom and what are studied, a fundamentally reflexive move. What I have detailed above is surely part of such an examination. It easily goes further to consider our authors' own professional claims-making on behalf of their argument, their strategies for managing the unsolvable tensions inherent in scientific argument itself, as Woolgar

and Pawluch productively pointed out (and what scientist does not face these, wittingly or not?); and a close and critical reading of texts that draw on their theory but are less than careful in its use. *Pacé* Latour, deconstruction and reflexivity need not be destructive, but at the very least, such choices invite a strong dose of humility for the experts themselves and offer no guarantees on how one's work will be received, which of course is precisely what all claims-makers face, witting, professional, and not.

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