Making Claims, Making Problems, Making Morality:

Spector and Kitsuse's Provocation[*]

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When Malcolm Spector and John Kitsuse (2000, p. 1) wrote, as the first sentence of their 1977 book, *Constructing Social Problems*, "There is no adequate definition of social problems within sociology, and there is not and never has been a sociology of social problems," it was not hyperbolic bravado. Well, at least, surely, not hyperbolic. The great achievement of that book— still today in print—in the four decades since then is that their proposed remedy, "a theoretically defensible, methodologically specifiable, and empirically researchable definition of social problems" and a body of related research have emerged and established the study of social problems as a scholarly respectable subfield (Spector and Kitsuse 2000, p. 27). Conference papers, journal articles, and books have defined this trajectory; parsing, critiquing, and considering their proposal (see Holstein and Miller 1993; Holstein and Gubrium 2008).

Some of these papers in *The American Sociologist* began at a session at the 2017 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 who continue that line (Michael Adorjan, Joel Best, Jim Holstein, Peter Ibarra, Donileen Loseke, Malcolm Spector, and I were on the panel; with Stephen Pfohl and Dorothy Pawluch in the audience). A collection of papers on Spector and Kitsuse's book was published recently in the Italian journal, *Società Mutamento*

Politica. I use a significant portion of the book in the undergraduate social problems class I teach. Its directions on how to think about and study social problems come as revelatory to most of the students. Significant and voluminous academic publishing in sociology organized under the term "social problems" continues (e.g., Trevino 2018). John Kitsuse would be pleased, if not also slightly surprised, could he know this history.

Such success might seem to beg the question of what more needs to be written about the conception of social problems Spector and Kitsuse offered. Beyond the new research being done from their ideas, my aim here is to reiterate some of the most important and still provocative elements of this particular constructionist argument—which, as a genre, perhaps has been all too successful (see, e.g., Latour 2003)—that draw the attention not only of my undergraduates but graduate students, professional sociologists, and other scholars as well. Moreover, beyond that provocation, there is plenty of work for their critique of social problems sociology still to do (Schneider 2018). In what follows I review some of what Spector and Kitsuse themselves called "radical" in their argument and that continues to offer intellectual traction and challenge in the study of social problems sociology. I close by suggesting that their version of social construction, rather modest in scope and less polemical and debunking in aim than much such work, invites a more self-reflexive place for the scholar, gesturing beyond itself and, perhaps, much conventional sociology as a guide for a more confident as well as humble argument and analysis (cf. Latour 2005).

"Social Problems Are What People Think They Are"

Spector comments in an interview for the Italian journal named above that the idea that "social problems are what people think they are" was at the center of his and Kitsuse's early

study that led to their book. In particular, their puzzle was why this simple claim, which they note was not original with them, seemed in past work always to have been compromised and pushed to the side, not only by then-popular functionalist or Marxist accounts, which often take "what people think" as derivative, but also by the so-called value conflict writers, who were mostly sympathetic to the claim. Even given this sympathy, "what people think" was not seen to offer sufficient *sociological* grounds for the study of social problems. "Objective (and undesirable) conditions," the value conflict writers insisted, are also needed. Across the work Spector and Kitsuse review, from the early "social pathologists," to the increasingly more scientific social disorganization and functionalist theorists; through these value conflict writers and even including elements of Howard Becker's (1973) *Outsiders*, "what people think" had not itself been taken as sufficient to establish a sociology of social problems. Kitsuse (1962) earlier had challenged that view, as Spector reminds us, in a paper on deviance, but his was a quite lone voice. Later, they together began to develop what the latter calls a "labeling theory of social problems" that would do just that. No sociologists writing on social problems had done that before. That claim remains the guiding, sociologically provocative, and still valuable insight of their work. But simply because they asserted that claim and elaborated it in a detailed argument doesn't mean it has been fully embraced. It has remained "difficult," even for some who have been drawn to it.

The definition they offered as theoretically defensible, methodologically specifiable, and empirically researchable is that social problems are "the activities of individuals or groups making assertions of grievances and claims with respect to some putative conditions" (Spector and Kitsuse 2000, 75). A shorter version might be "claims-making and responding activities" or,

as Kitsuse sometimes would say, editing further, "definitional activities." This simplest version strikes me as somewhat resonant with what Michel Foucault (1978, 1979) called discursive practices. That isn't to say Kitsuse and Spector drew on Foucault's work or that their argument needs such a reference to deserve our attention.[1] Rather, I am struck by how both sets of terms draw together language, its use, and related action or behavior in an inescapable and promising analytic combination that grounds "social construction" in language-and-action, similar, I believe, to Bruno Latour's (2005) actor-network theory, which he insists, after all, is not sociology in its most familiar form.

Spector and Kitsuse take the rather homely theme, above, and, preserving the basic insight, add two or three more specific requirements. First, "what people *think*" is of course not the point and cannot provide adequate empirical direction for finding a social problem in the world. Aside from what people may or may not think, it is what they do—and, especially, what they *say* and what they *write*—that sociologists of social problems can observe and describe. Theirs then is a language- and discourse-centered approach. But language use is not its sole focus. In short, claims-making and related activities constitute social problems as moral action, just as "labeling" brings into view the moral marking that the concept deviance announces. These are claims that register complaint, criticism, outrage, demands that "something be done."[2] In this view, the evaluative meanings attributed by "people" or, as the ethnomethodologists put it, "members," are the essence of the matter; constitutive of the phenomena indexed by the concepts social problems and deviance. These phenomena, so named, are "caused" by or exist in the world in and by their very observable collective enaction. That "doing" constitutes the object of study. The theoretical and philosophical grounds for such

thought come in part from the symbolic interactionist traditions these authors shared, with an unmistakable and quite important ethnomethodological flavor, contributed by Kitsuse, perhaps from his earlier work with Aaron Cicourel (Cicourel and Kitsuse 1963). From these joined insights came the non-negotiable claim: no claims-making, no social problems. Full stop. Such definitional activity, they insist, is all we need in order to name the phenomenon a "social problem."[3]

What about the Sociologist? Conditions? Context? Social Factors?

These theoretical claims lead immediately to the question, "What about 'what the *sociologist* thinks' social problems are?" And of course it is not that what the sociologist "thinks" is irrelevant to deciding what will be taken as a social problem. But the requirements in this case are as follows: (1) such "thinking" must be guided by a conceptualization or theory; (2) that theory cannot contain judgments—scientific or popular—about conditions; and (3) the sociologist cannot use their own personal morality or ideology to identify conditions as problems. The first four chapters of Spector and Kitsuse's book detail these requirements in the critique of past work. I refer readers to that discussion, which remains relevant to social problems work published today. Although the details there distinguish the various arguments considered, our authors group virtually all prior social problems work, including that of the functionalists with their more complex set of concepts that examine society as a system with various subsystems, into what they call "normative" theories. These they reject as largely responsible for the muddle that then defined social problems theory. That conclusion is the point of departure for their own argument.

Certainly, however, our authors' argument itself is hardly made without attention to norms. That is apparent in the explicit shoulds and should nots that their theory boldly specifies and that they call radical. Their theory is—as are all theories—a normative phenomenon. They would not dispute that, given their aims. Surely, they could have made that point without undermining those aims; but they did not. I will have more to say about that later. Their use of "normative" to describe past theory reflects a central requirement of their own: what social problems are, for the sociologist, cannot be a matter of them making evaluative judgments about material conditions in society. It is most centrally, then, not a question of "what the sociologist thinks" about such conditions as conditions. Any theory that answers this question by requiring a judgment from the sociologist about conditions or circumstances as undesirable—and, thus, a "problem"—falls into their normative category and is set aside. That is consistent with any argument that insists, as theirs does, that social problems are to be defined as such by "people" rather than by "experts" such as themselves.

Given the history of social problems sociology and the disciplinary politics of sociologists—mostly liberal, left, progressive, radical—this position can discourage interest in and use of Spector and Kitsuse's argument. That isn't to say their argument is "conservative" politically, but it does insist on the priority of a *disciplinary* politics of science in the study of moral phenomena, which, arguably, is the larger or more inclusive conceptual object of interest. Whether we name it "objectivity," "distance," "neutrality," or even-handedness, fairness, accuracy, and precision in terms of theoretical language and the creation and manipulation of evidence or data (e.g., Becker 1967), this too is normative, but in a sense different from that used by our authors. Clearly, to avoid confusion we need to ask, "normative

with regard to what, and in what and whose terms?" Our authors use these disciplinary norms to distinguish their sociological stance from the stance or position of members or participants.[4] The use of normative language to define undesirable conditions in terms of values such as equality, social justice, fairness, human rights, and the long list of other terms that characterize a liberal-left politics, signifies a *member's* location, not a sociologist's. Such a definitional practice, while laudatory in my view, when taken up *as the sociological basis* for defining social problems is, in that move, indistinguishable from other members' claims. Sociologists who make this move then become themselves participants or claims-makers to be studied.

The insistence that claims are sufficient to provide the subject matter for a theory of social problems led some colleagues otherwise sympathetic to the theory to reject what they called Spector and Kitsuse's (too) "strict" constructionist view. Surely, they argued, social context cannot be ignored. They offered an alternative, "contextual" constructionism in a move to save the contribution (e.g., Best 1989, pp. 245-46). But what the radical view rejected is precisely such a notion as "context," along with "objective conditions," and any other version of "social factors," as explanation or essence of social problems (cf. Latour 2005). The parallel threat for the labeling argument was the notion of "rule breaking" and the idea that deviance is, simply, after all, transgression caused by these and other social and psychological "factors." But in our authors' argument, such conventional explanatory resources can no longer be at the sociologist's disposal. Instead, reflecting the ethnomethodological move, these and similar terms become members' or people's resources and, thus, the sociologist's topic for study.

Spector and Kitsuse do not claim that sociologists should give up contextual and structural accounts of, say, poverty, violence, rule breaking, undesirable and of course desirable

conditions of any sort. And they do not suggest that sociologists as experts on a wide variety of social and cultural phenomena should avoid creating and sharing their knowledge and research results in society. This is what I mean by calling their proposal "modest" in its scope, even if radical in its content. But, compared to Becker's proposal on deviance—which he offered as a supplement to existing work—our authors' opening sentence, quoted here at the outset, can seem like the proverbial "line in the sand" for the study of social problems in sociology, which I think they intended. They illustrate this focus in the first pages of their book, distinguishing a sociological study of organized crime, on one hand, and a study of organized crime *as a social problem*, on the other (Spector and Kitsuse 2000, pp. 2-5). Their object of study is not organized crime or crime; or poverty; or sexual assault; or environmental pollution, and so on. They write instead a theory of a particular moral phenomenon—claims-making activity—and how to study its emergence and movement.

While colleagues whose work focuses on the origins and maintenance of these and other familiar sociological phenomena might find such a constructionist study insightful, the account and the data from those projects would be different, along with the aims of the research, from what Spector and Kitsuse propose. But if you are a "sociologist of social problems," they are, quite pointedly, addressing you. Those who would want to add explanatory "context" to claims-making always have seemed to me to offer an updated version of the "reasonable compromise" position taken by the value conflict forebears; to wit, the insistence that social problems have an "objective," i.e., condition-based, and "subjective" or meaning-based, component. If past is prologue, such a shift likely would take us back to where Spector and Kitsuse began, with claims and definitions—"what people think"—sacrificed to a

professional ideology not all that different from those they dismantled. To compromise here is to erase the essential and still productive contribution that they made.

Putative Conditions, Viable Claims, and Values as Discourse

Having seen what happens to social problems theory when one is "reasonable" about the place of social conditions and context as either causal or constitutive, Kitsuse drew on his own past arguments with Edwin Lemert's (1951) societal reaction theory of deviance to add the word "putative" to their definition of social problems. In puzzling over what such "reactions," societal or not, might be reactions to in an argument that seemed also to say deviance is a matter of attribution, Kitsuse (1962) wrote of "imputations" of deviance to clarify the point. Perhaps anticipating the obfuscating link between conditions and definitions that he and Spector would critique, as well as attempting to inoculate himself against the sort of misstep Becker (1973, p. 21) would make around the "secret deviant," Kitsuse refused to "be reasonable" and so affirmed his conviction that deviance is a matter of imputation (regarding Becker, see Pollner 1974). Arguably, it was a stroke of genius as well as an affront (perhaps calculated) to conventional sociology (as was ethnomethodology; as is the late Latour [e.g., 2005]).

The *Oxford English Dictionary* gives us "putative" as "That [which] is commonly believed to be such; reputed, supposed; imagined; postulated, hypothetical."[5] Prudence Rains (1975, 3), commenting on Kitsuse's use of imputation, calls it, and by extension, putative, a "careful" way to talk about something the existence of which one is not quite prepared to affirm or, as she put it, "without commitment to its actuality." It is a destabilizing word to put into the mouths/hands of one who calls themselves a scientist, even a "scientist lite." Yet, they could

not have found a more fitting term with which to build their new and radical definition: Claims-making and responding activities *with regard to some putative conditions* (Spector and Kitsuse 2000, 75). To use putative to modify "conditions"—the favored sociological candidate to secure this prized object, social problems—our authors announce that they will remain agnostic not only as to the relevance of these phenomena to social problems, but to their very existence as well (Spector and Kitsuse 2000, p. 76).

For some colleagues, this was a step too far; read perhaps as a principled refusal of the responsibilities of their profession to describe critically and know deeply the details of the material world their discipline takes as its object of study. Given both the *OED's* use of the word "imagined" and Rains' use of "actuality," it may have appeared that Spector and Kitsuse were here questioning the very existence of reality. And, from Latour's (1999) experience in the science wars, some skeptics critical of and/or (perhaps intentionally) misunderstanding the proffered (and mobile) meaning of "construction," could read "putative" here as a denial of the very foundations of science, knowledge, as well as commonsense, with which sociology always must make some workable connection.

But such a reading would be difficult to sustain, given the authors' reiterated and clear statement of their project's aims. From the beginning, Spector and Kitsuse stand on commitments to conceptual clarity, methodological consistency, and empirical specificity in the study of social problems; hardly a denial of reality. Rather, the genius of putative in their definition is that it allows them a very particular ontological indifference with respect not to what exists but rather to what their theory warrants as relevant data; which is to say, definitional activities. As Peter Ibarra and Kitsuse (1993) point out in a late paper, reiterating

the source text, those conditions can appear in their analysis only as elements of members'

claims. Moreover, as Rains notes, with putative they underline this point ostentatiously, to

ensure that it cannot be missed.

Consistent with their commitment to members' claims, Spector and Kitsuse's theory

also directs the constructionist to assess not the validity of those claims but rather their

"viability," that is, their vitality, their liveliness; the extent to which they are sustained,

repeated, carried across the spacetime under study, in and by the claims-making activities of

participants. As they ask about claims-makers, to illustrate, "can they 'get away with it'?" Can

the claims be "sustained politically" (Spector and Kitsuse 2000, p. 71), or, even

ethnomethodologically? This is important because it is hardly typical of social constructionist

argument. As Latour (1999, 2003, 2005) has repeatedly noted, the unfortunately more common

move has been the ironizing and debunking that ends up saying, paradoxically, "this is (merely)

constructed; *that* is real!" These politics typically seek to critique unjust socialcultural practices

as "constructed," by dominance and power, juxtaposed to a halcyon reality defined by equality

and freedom.

The question above harkens to an epistemological one asked in science studies, namely,

"What practices are used to secure 'certain knowledge'?" The kind of answer sought is not to

make reference to a "method," but, rather, by a close study of the sustained *in situ* material-

semiotic exchanges by those whose claims and related activities keep those definitions alive

(Haraway 1997; cf. Latour 1999, pp. 24-79). While social problems claims-makers could, surely,

draw on the presumptions and resources of scientific argument and data in hopes of greater

viability, Spector and Kitsuse treat that as an empirical matter. Claims framed and "grounded"

in these terms may well be more viable than those not so framed, but that is to be determined

through observation and not assumed by the sociologist in advance. This leaves open the

possibility that complete fabrications, even lies, might be held higher and carried farther than,

to be simple here, "truths." Our authors could not have known how timely that insight would

become in US politics some forty years later.

Moreover, this requirement to focus attention on the viability of claims rather than their

validity reiterates the repositioning of the sociologist/analyst announced by the word putative.

It makes a move typical of sociological expertise—the adjudication of the truth value of claims

made about socialcultrual conditions—beside the point. The agnosticism required of the

sociologist relative to conditions here extends to how they address the core material of their

analysis, the claims actually made by participants in the process being studied—our "what

people 'think.'" The expert analyst no longer addresses a familiar epistemological question that

may have grounded their own prior professional identity—is the claim "true"?—but, rather, is

asked instead to address another that may strike them as less significant: the detailed recording

and description through spacetime of words, phrases, and other symbols given meaning by

participants and the actions taken, arguably, in their name. Surely still empirical and realist, this

assignment asks the sociologist to provide evidence of how, in detail, claims emerge, are

maintained—if in fact they are—change, and, perhaps, disappear, all carried by the claiming

and responding activities of those involved.

Their constructionist argument requires a third shift for the sociologist on another

resource central to much conventional social problems sociology: the use of values to explain

behavior. Here the typical behavior in question is claims-making itself. Spector and Kitsuse

refuse the argument that values cause claims-making. Although social science explanation of behavior by reference to values has a long history, it has been a difficult one to sustain using empirical data so as to avoid a circular argument. That is, it is often said, for example, that people behave as they do because they hold certain values; but then, when we turn to secure how it is that we know that they hold said values, references to their behavior as indicative typically are offered up. As our authors point out, conventional notions such as socialization are used to tell a story of how we learn values that then propel our conduct. The challenge of course is how it is that the sociologist can get at those values on the person's "inside" independent of the behavior that they are said to cause on the "outside."

Given their view of social problems as collective achievements of people acting together in realtime and place, our authors draw on C. Wright Mills' (1940) comments on how sociologists use the notion of motives. Reiterating Mills' critique of sociologists' attribution of motives to the people they study, and using Mills' terms, "vocabularies of motives" and "motive mongering," they ask on what basis similar attributions of values might be made by sociologists of social problems (and beyond) to the people studied. Not surprisingly, they find no empirically adequate answer. Similar to how ethnomethodologists treat the sociological concept of norms or rules, Spector and Kitsuse, like Mills—who certainly was no ethnomethodologist—propose values as a linguistic resource that members specifically and explicitly use in their writing and speaking to characterize themselves, others, situations, and objects.

Rather than continuing to use this term as a problematic technical and explanatory resource, which brings its own problems, Spector and Kitsuse (2000, pp. 91-95) treat "values" as words or phrases used by claims-makers, wittingly and not, to "ground" or defend their

claims, e.g., "Donald Trump should be impeached because he lies!" 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, instead making that language and its use topic for social problems analysis. And of course, there is no more mystery in how values become part of sociological analysis, even as they are no longer used empirically as explanation for what people do.

Sociologist as Claims-Making Member

If the requirements I've noted above for the constructionist sociologist of social problems that Spector and Kitsuse set forth were not provocative enough, their book, in a way unusual for sociology of the time, and, arguably still today, brings the social scientist under analytical scrutiny. While the sociology of science was already an established field, it then came from a mostly functionalist and structural perspective. New work in science studies was beginning to emerge that examined not the social structures, "schools," and reward systems of—or fraud/deviance in—science, which were questions typical of the earlier approach, but, rather, how scientists create, challenge, change, and sustain scientific knowledge in their mundane work (see Lynch 2005). Latour and Woolgar's (1986) pathbreaking ethnographic study of scientists at the bench—*Laboratory Life: The Social Construction of Scientific Facts*—was published in 1979, two years after *Constructing Social Problems*.

There was in this interdisciplinary work a distinct flavor of what is summarized by the word "reflexivity," a term central to Harold Garfinkel's (1967) ethnomethodology. What this word invites us to consider also was being addressed in a slightly different way by feminist, postcolonial, and anti-racism critiques of how knowledge not only is always located in

history/culture/society—the familiar sociology of knowledge argument, but insisted on a more particular and fine-grained examination of the "How?" and "Who?"and "For whom?" that are always part of making knowledge (e.g., Clough 1994, 2000). The "post" work in cultural anthropology to which, for instance, George Stocking's (1983) *Observers Observed: Essays on Ethnographic Fieldwork*, James Clifford and George Marcus' (1986) *Writing Culture: The Poetics and Politics of Ethnography*, and Marcus and Michael M.J. Fischer's (1986) *Anthropology as Cultural Critique: An Experimental Moment in the Human Sciences* contributed importantly, examines the place and writing practices of the anthropologist "in the field" and "in the texts" that constitute the published knowledge of cultural anthropology. In this work, the notion of social construction is revised and becomes inclusive and, arguably, closer to Jacques Derrida's (1976) deconstruction in its effect in that we begin to see clearly the import of the latter for what we do as science and, by extension, all knowledge making.

While Woolgar and Dorothy Pawluch's (1985) important and appreciative critique charged Spector and Kitsuse and many of us who wrote from their theory with ontological gerrymandering and a selective relativism, the significance of the full critique they offered goes far beyond that particular charge, which does not apply only to the social constructionist sociology of social problems. But their more proximate criticism, that even as "we" insisted on the constitutive force of claims-making and ignored "objective conditions" and an expert knowledge of the world "as it really is," we nonetheless drew, usually in passing, on the latter to secure the definitional argument we made. This move in Spector and Kitsuse—seeable in their reference to the constancy of marijuana and the condition of children's tonsils to underline the power of definition; and in many similar examples in related work by others,

including Becker—performs something not unlike their own critique of the value conflict

authors, who required both "subjective" and "objective" components of social problems

(Becker 1973, pp. 20-21; Pollner 1974; Spector and Kitsuse 2000, pp. 43-44, p. 128; Woolgar

and Pawluch 1985, pp. 216-217). Woolgar and Pawluch of course detail this recurring

contradiction found across the rather substantial body of work they cite. Although several of us

responded in variations of defense, I suspect we also saw the acuity of their insight (Hazelrigg

1985, Pfohl 1985; Schneider 1985).

And if I hadn't seen it clearly before, their paper made crystal clear what Spector and

Kitsuse themselves had invited, even if they had not taken that step themselves: to turn onto

their own argument and practices their examination of how prior sociology of social problems

argument and practice also were pursued in the interests of viability. Just as Spector and

Kitsuse critically examined how their predecessors defined social problems and developed

theories of the phenomena in question, Woolgar and Pawluch invite us to make that very move

for the social constructionist argument, in social problems and beyond. They call it ontological

gerrymandering, and I accept that critique. But I would prefer seeing it—contradictions

removed—as an ontological and selective agnosticism with regard to the conditions that appear

in participants' claims. It seems to me this kind of agnosticism is altogether familiar in the

history of US sociology and what was and perhaps still is called its "mainstream" preference for

quantitative and structural/aggregate analysis. While surely legitimate and professionally

respectable work, sustained and focused attention to the nature of human experience

expressed through narrative, interview data, and observational record largely have been

deemphasized or ignored as a matter of research design and, if I may put it this way, "taste" in

much sociology. This is to extend Spector and Kitsuse's own reference to a sociology of organized crime distinct from a study of it as a social problem in their terms. That there is then a selectivity in the ontological attention to the world studied by various kinds of sociology strikes me as, to use a familiar term, the norm. The question of an untheorized writing of a contradiction is of a different, lesser order of significance in that it can indeed be remedied.

Reading Woolgar and Pawluch Again

At the end of their 1985 paper, Woolgar and Pawluch offer three possible readings of their critique. One is that, in effect, if you want to write a constructionist argument of social problems as it had been done, which is to say flawed and contradictory, then we have shown you how to do it by showing you how Spector and Kitsuse and a number of other authors have engaged in a selective relativism. Of course, they would not have expected that reading to be taken seriously. Second, they tell us that while they have pointed out these contradictions and inconsistencies, "more caution [can] be exercised in attempting empirical studies in the definitional perspective" (Woolgar and Pawluch 1985, p. 224). That reading is what I have been urging here as both possible and worthwhile, quite sure that it will satisfy a smaller segment of the sociological community today than the initial and contradictory formulations. That may be due to the boundary it insists on drawing between the sociologist's personal political commitments and their professional identity as sociologists. As noted above, this boundary prohibits factual claims about the so-called "conditions" long considered central to the cause and constitution of social problems, both by the sociologist-as-member-of-society-with-their-own-politics or by the warrants demanded by the theory itself. I would guess that a large segment of professional sociologists writing about social problems would not be happy with the

requirements of these disciplinary politics (see Schneider 2018). Beyond that objection, some

may feel that the insistence to focus only on claims-making and responding activities, as

detailed above, and a fine-grained empirical description of the definitional processes, with a

narrative about these processes, offers not enough "payoff," intellectually, to warrant the work

required. Both of these latter objections are hardly unusual in the worlds of professional

scholarship. There are many choices available, and made, as to what one may ignore and take

up in one's work. This kind of "gerrymandering" is rarely mentioned yet everywhere to be seen.

Woolgar and Pawluch offer a third reading that I think has not been taken up or taken

seriously by those of us who have written in this tradition of social problems theory. It reflects

more explicitly Woolgar's affection—at least at the time—for a radically relativist argument he

was making in the science studies work that he co-authored with Latour in *Laboratory Life* but

then more clearly so in his sole-authored work (see, e.g., Woolgar 1988). Unlike his colleague

Latour, Woolgar did not denigrate the deconstructive arguments from Derrida's writing in

literary studies, which arguably resonate provocatively with his own ethnomethodological

insights and interests. In both traditions, put too simply, there is a commitment to the notion

that the whole of the socialcultural world is put together, maintained, and changed *in situ* and

in "work" done by, to resurrect one of Becker's (1986) homely but insightful phrases, people

"doing things together." Clearly, that notion has a constructionist flavor. Most of the social

problems theory referenced here and in Woolgar and Pawluch's critique reflects versions of

symbolic interactionist sociology, in which an assumption of shared meaning extant in language

use provides a terrain on which sociological arguments are made about how reality is

constructed. Clearly, such a claim has a long history in US sociology.

Ethnomethodology, at least in its Garfinkel-influenced version, opens attention to how the "done together" in Becker's phrase is approximated or made "good enough" to enable a "going on" in the interpersonal exchanges under review, which always are subject, moment by moment to break or "breach." The politics of deconstruction aim to uncover how this always fragile order of any text—thought inclusively—is produced by deflecting attention from its inevitable supporting and "othered" background, which is more or less skillfully kept in the shadows. *Apropos* Spector and Kitsuse's argument, Woolgar and Pawluch, in effect, turn on the "backstage lights" to show us the references to conditions that they say secure the selective social constructionist claims in this work.

All that said, the more interesting point they make, the third suggested reading of their critique, is a version of Woolgar's fully reflexive critique of how not only scientific argument is made and sustained but how all arguments—including "ethno-theories" or what Melvin Pollner (1987) called "mundane reason"—are always jerry-rigged affairs. He insists that, if critically examined in enough detail, arguments that aim to *explain* something cannot be sustained in their own terms; or that they rely for that sustenance not on some compelling force internal to the arguments themselves (e.g., "method") or "nature" speaking to the scientist in a language only they understand, but rather are due to the mostly unspoken and tacit collective "agreement" of those using and making them to "look the other way" or to suspend disbelief in their impossibility. Arguably, the most interesting thing here is that the matters Woolgar points to in his radical critique are not, as he and Pawluch note, "problems to be solved." They are, he writes, "unavoidable. They are not mere technical difficulties in social problems arguments, but pervasive features of all attempts to *explain* social phenomena" (Woolgar and Pawluch 1985, p.

224, emphasis supplied). Woolgar (1988) elsewhere makes it clear that the lynchpin of this practice of explanation enacts an ideology of representation itself. In science, including sociology, response to this claim often takes the form of a grumbling hesitance, claims that "it doesn't matter"; impatient calls to "get on with the work," and criticisms of "losing the object" and "the point" of the research itself.

While I have sympathy for these expressions and have offered them myself earlier in my work, it seems to me that by now in the discipline, enriched as it has been by relevant insights from other fields—and with (even grudging) respect for the wisdom of various "post" critiques—we should be prepared to think more seriously about this third reading rather than ignoring it. It takes us beyond the moves that title their critique and urges us to give more careful attention not only to the very constitution of the object or topic of our study, but the ways that we make the arguments and explanations that we do; in order to bring to light—or to light to a greater degree—just how we make what we would hope are "compelling" arguments. This reflexive consideration could make those arguments, those claims, more open or porous to examination; to help us notice what we silence and background; and what we foreground as professional claims-makers doing the science that we do (if that is what we call what we do).

This is not to "weaken" science—even as we know that science and truth are now under attack in the United States and elsewhere, and that such questioning might seem to do that. Rather, following Latour's (1999, 2003) familiar claim noted above, "the more constructed, the more real," the point is to acknowledge the put-together and relative nature of all claims such that the very practices of that making are themselves available for scrutiny. All arguments, in other words, are always already variously "weak." To claim otherwise is to engage in a political

fantasy, extreme versions of which are, sadly, all too apparent in today's America around what is called, derisively, "fake" on the question of what are lies presented as truth; and what are not. Indeed, it is being able to appreciate the "not fake" as, nonetheless, always constructed, with the details of that constructing offered for all to see, that offers a ground on which to step in order to move elsewhere. The moment would seem to call not for an embattled, defensive stance in the production of knowledge and treatment of the notion of truth but, rather, one more fiercely open to critique and study of how claims are made and supported by those who make them and by those who challenge. Deconstruction and reflexivity are themselves not inherently destructive, but at the very least, such choices invite a strong dose, paradoxically, of both humility for all claims and claimants, and greater confidence for us sociologists and other scholars in making the claims that we do. Surely, the extraordinary success of science in its long history might be thought sufficient to allow us, with our sisters and brothers in the humanities, to acknowledge that there are the no guarantees on how one's work—one's claims—will be received, in whatever venue at whatever time it is offered, which of course is precisely what all claims-makers face; witting, professional, and not (cf. Haraway 1997, pp. 23-48).

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Endnotes

[1] David Matza (1969, p. 103, p. 116, p. 176) in his *Becoming Deviant*, which shares many

theoretical points with these authors, does cite Foucault in three footnotes.

[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.

[3] While the "and responding activities" is central to their conception of social problems, the

initial claims, which must be responded to and carried, or not, are sufficient to garner the

attention of the researcher using this argument.

[4] Such a position or location in the sociological study of what has been called social problems

seems always to have been a point of contention in US sociology. It is easily seen as too

distance, too cool, too removed, too safe, too professional, and so on; even perhaps "too

conservative." This is still the case.

[5] *Oxford English Dictionary*, online, consulted June 22, 2018 at: oed.com.cowles-

proxy.drake.edu/view/Entry/155203?redirectedFrom=Putative#eid.