

A Method to My Madness: What Counts as Innovation in Social Science?

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Abstract

This article summarizes a presentation made to a workshop on social science methodological innovation sponsored by the ESRC. First, the notion of methodological innovation is explored on the way to an argument that innovation in social sciences occurs at a conceptual level rather than at the level of techniques for data gathering. Four historical examples are briefly described to exemplify the argument, Zadeh's fuzzy set theory, Goffman's presentation of self, Bartlett's schema, and Glaser and Strauss' grounded theory. In response to questions posed by the organizers, it is noted that the four examples were produced by individuals with a considerable time lag before uptake of their concepts in the diffusion curve. The article ends with speculation about what a social science center for conceptual innovation might look like.

Keywords: Social research, conceptual innovation, methodology, ethnography, complex organization

Brief Bio: Michael Agar is an emeritus professor of linguistics and anthropology who works independently as Ethknoworks LLC in New Mexico (www.ethknoworks.com). He is the

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In the summer of 2010, I spent a day with European and American colleagues at a workshop on methodological innovation in the social sciences. We had been invited as part of a “Methods Fair” sponsored by the UK Economic and Social Research Council. Our task was to address this topic:

The history behind methodological innovations and the processes and mechanisms of their development and diffusion.

As it turned out, the topic, once engaged, morphed into the proverbial jello that’s so difficult to nail to a wall. To my horror, the organizers asked me after the event to convert my presentation into an article for this special issue. I agreed, on the condition that I could write as informally as I had talked.

My first problem with our topic was the usual academic pathology: When I lift the lid off core concepts in most any academic statement—or government or corporate report for that matter—I gaze down into the ninth circle of semantic hell. What is a “method” anyway? I know in social sciences the norm is to think of a specific procedure to obtain a specific kind of data. This psychological test, that kind of survey question, those types of experimental manipulations. But those kinds of method are more about control, about standardization, not about innovation.

Next I looked at some digital dictionaries, one American and two British, the Oxford version and the Cambridge version, so as not to play favorites. Maybe a little of the vulgate would help. Here’s what they said “methodology” meant:

1: a body of methods, rules, and postulates employed by a discipline: a particular procedure or set of procedures

2: the analysis of the principles or procedures of inquiry in a particular field
(Merriam Webster online dictionary)

a system of methods used in a particular field.
(Compact Oxford dictionary, web version)

a system of ways of doing, teaching or studying something
(Cambridge online dictionary)

By these definitions—“body,” “system”—methodology isn’t so much about one specific thing a social scientist does. It is about a general way of looking at problems that changes how one sees them in fundamental ways. More like a paradigm than a procedure. Method isn’t about how to gather a particular kind of data; it is about what kinds of data might be relevant at all. Here methodology approaches epistemology. My Apple dictionary tells me—I have no idea what a Windows machine would say and wouldn’t trust it anyway—that epistemology is:

the theory of knowledge, esp. with regard to its methods, validity, and scope.

Epistemology is the investigation of what distinguishes justified belief from opinion.

That’s a larger conceptual space where methods in a more specific sense are included. And “conceptual space” is where I decided to go for the workshop. But then what about the

“innovation” part? To prepare for a different talk a few years ago, I went to the library to see what kinds of books there were on that concept. There were shelves full, one dominant theme being how to create new products for an oversaturated global market. My computer dictionary isn’t very helpful with “innovation.” The concept means “the action or process of innovating,” specified as “a new method, idea, product, etc.”

Whatever else innovation means, it means something “new,” something that some person or community sees as different from what came before. Perceptions of “new” of course do not map onto some universal metric. In fact, a fair amount of the business-oriented innovation literature I looked at for that earlier conference had to do with how to persuade people that something was an innovation, an important move to determine a unique “market niche” that differentiates it from products already available. Terms like “branding,” “marketing,” “design,” and the like were featured.

Given all these ambiguities, I came to the conference confused about what it was that the conference was really about. Perhaps if I’d had more experience with the “innovation” concept I’d have known better how to approach it. So I decided on this strategy. I would look at a few examples. A slide at the beginning of the presentation linked to a YouTube video of Rahsaan Roland Kirk playing and singing “Bright Moments.” I used it to announce that I was going to look at a few “bright moments” in social science. But then I also took some advice from the famous Hegel quote, that the owl of Minerva only spreads its wings at dusk. Enduring bright moments only become apparent with time. In fact, as the examples will show in a moment, the initial announcement of what would later become a bright moment can be met by hostility, rejection, or a response that it is no innovation at all.

In the end, the best I could do was say that “methodological innovation” means a recognizably new conceptual scheme taken up and put to use by one or more communities of practice—in this case social science, but probably in other fields and among a general audience as well—that changes how a phenomenon is viewed, approached, researched, interpreted and acted upon. The definition is still jello to be nailed to the wall, but in a slightly more manageable quantity. At least so it felt as I boarded the trans-Atlantic flight to the conference.

Only later, as I wrote this summary of the presentation, did I realize that in some ways the conclusion reinvented a more modest version of Kuhn (1962). The re-invention is the simple argument that methodological innovation in social science is *conceptual* innovation. It is much more modest than Kuhn in that there is no claim to a “paradigm change” in that grand historical revolutionary sense that he describes. On the other hand, the four examples I picked might just qualify. They were major “bright moments,” with legs.

The Four Examples

The first example I thought of, probably because I was there, was poor old Lofti Zadeh in Berkeley back in the 1960s. It is the least well-know example in social science compared to the other three to come, but publications and web pages show it to be in the ascendancy, and it is clearly a conceptual innovation.

Zadeh was an engineering professor who invented something called “fuzzy set theory” (1965). This wasn’t just a reworking of probability, as some critics claimed. It was a way of formalizing how a particular item was “more or less” of a member of a set. It undermined some of the fundamental laws of logic. No longer could you say something either was or wasn’t a

member of a category, the famous “law of the excluded middle,” P or not P. Now you could say that something was “sort of a P” or “a little P but not much” or “close to a perfect P but no cigar.” It was *both P and* not P. It changed the rules and accommodated and formalized the vagueness and ambiguity with which we humans describe our world.

I wrote “poor old” above because his colleagues thought Zadeh was nuts. He was so desperate that he wandered the campus and even tried talking to anthropologists of a formalist persuasion, who of course loved the concept, including this unwashed graduate student. A Berkeley psychologist, Eleanor Rosch took up the fuzzy concept, melded it with Wittgenstein’s notion of “family resemblance,” and came up with pioneering work in “fuzzy cognition” (Rosch and Mervis, 1975). But, by and large, Zadeh’s fuzzy sets didn’t take off.

Then Japanese business started making money with the idea. Consider the fuzzy washing machine. I don’t know how it really works, but something along the lines of sensors that tell the machine “more or less dirty water,” “more or less weight,” “more or less whites,” things like that. Then the fuzzy set controller figures “more or less soap,” “more or less hot,” and so on. Fuzzy washers, and other fuzzy home appliances, did take off in the Japanese marketplace. Then, finally, the Americans noticed and got interested.

Now there are popular and professional books on fuzzy sets (Kosko, 1993; Ragin, 2008) and the concept generates long lists of results in internet searches. I think Zadeh’s work clearly counts as a “methodological innovation,” in that it is a new way to see numerous problems and approach them differently in human social research. It has consequences for problem definition and action. One thinks and does things differently compared to how one thought and acted before.

I hope Zadeh is comfortable and happy and rich now, because he was very kind to anthropology graduate students in the 1960s. But then in those days he was a pretty lonely guy.

The second example, Erve Goffman, you wouldn't exactly have called "kind." In 1959 he published a book called *The Presentation of Self in Everyday Life* (1959). It was the sociological version of Shakespeare:

All the world's a stage,

And all the men and women merely players:

They have their exits and their entrances;

And one man in his time plays many parts...

...and so on. He started out in a sociological tradition built on the philosophy of George Herbert Mead that made us think of those most individual of attributes—mind and self—as products of our desires and strategies to fit in with the social world.

The thing about Goffman—he was around when I was a visiting assistant prof at Berkeley in the early 1970s—is that he didn't do surveys and statistics like most sociologists. He wrote well and told stories and built concepts on top of them. I wish I had a nickel for every time I heard a traditional social science type make the snide remark that he did "science fiction." And he was abrasive, but he appreciated the same in others. I remember walking into a packed room where he was about to give a talk, going up to the carousel on the slide projector, this being a long time ago, and starting to re-arrange the slides. He yelled at me in four letter words but laughed a lot while he was yelling. I liked the guy and felt honored that he appeared to think I was good for an exchange of wisecracks.

At any rate, it was only later in life that his work was acknowledged for the "methodological innovation" it was. He came out of a tradition at the University of Chicago and

shared with many of his colleagues, like Howard Becker, an ethnographic orientation. But, like Zadeh, the innovation was in the concepts, the title of that first book, “presentation of self in everyday life,” being an example. That early book continued with concepts like “backstage” and “frontstage” behavior. He created concepts for the rest of his life, typically not with any specific research focus, but rather based on scattered observations from life and literature.

In a perfect Goffman finish, he wrote his acceptance speech on his ascendancy to the presidency of the American Sociological Association knowing he was going to be dead before it was delivered (1983). It amazes me even today how often I see his work re-discovered in domains that deal with humans. I’m betting that it’s mostly because of the concepts.

Here’s a third example: Some of Goffman’s later work, and some of the early uses of Zadeh’s fuzzy sets, drew on an even older idea that had laid dormant for years. In the early 1930s Sir Frederic Bartlett published a book called *Remembering* (1932). He based his work on stories that he would tell and then ask subjects to remember. Stories connected persons, objects, actions and states in a set of relationships that developed over time. He named the structures that organized these configurations “schemas,” or “schemata” as he wrote back then, using the Greek plural. In rough summary, he found out that when a story he told fit schemas the subject already had, he or she remembered it better.

“Schema” emerged from its long slumber around the 1960s, as behaviorism was drying up and wilting on the vine. Psychologists started thinking seriously about how cognition was built out of chunks of knowledge rather than individual fragments that had to be tacked together into stimulus/response chains. Ulric Neisser wrote a book laying out this new schema-based version of psychology (1967) and resurrected Bartlett’s work. It was funny back then, you’d read a cite to Bartlett in 1932 and then nothing for 35 years. Out of schema came many other notions,

including Goffman's later concept of "framing." Nowadays the concept of "schema" is ubiquitous in numerous different fields, not to mention in ordinary conversations.

The concept is everywhere. "Schema," after its long absence, turned out to be a conceptual dog that could hunt.

One final example, the fourth one, another from the 1960s. I know, younger readers are muttering, "not another 60s example." There is a method to my nostalgia, though, so that I can make a point about time lag in a moment.

American social research has a long history of positivism, based on the "social physics" concept of August Comte. In the U.S. part of the story, ethnography in anthropology and a marginal wing of sociology, that wing including Goffman, provided the main alternative. We took pride at working in a different way, starting wide-open and looking for structure only later, marking up transcripts and notes with colored pens in search of concepts that the words and actions of our subjects were trying to teach us.

Out of this marginal research world grew a book, called *The Discovery of Grounded Theory*, by two ethnographic sociologists, Barney Glaser and Anselm Strauss (1967). We anthropology students at the time, at least those of us who were methodology freaks, were delighted. The book did two things for us. First, it made a part of what we did more explicit, and therefore more systematic and easier to explain. Second, the concepts in the book co-opted the concepts of the positivists. We did grounded "theory," theoretical "sampling," and the like.

Their book was year one of what would become the qualitative research boom today. I lived through it, and it's far from over. In fact, I made part of my living from it even as it grew. Nothing much happened at first, except I noticed that even the positivists on NIH review panels started saying "grounded theory" now and again, not always knowing what it meant. But along

about the 1980s, and then almost exponentially in the 1990s, “qualitative research” exploded onto the social research scene in virtually every field that dealt with humans. A book exhibit at a conference that might have had only had a few volumes in the 1970s morphed into a room full of tables with stacks of books and journals from several publishers.

Now, in 2011, “grounded theory” is only one of many flavors in the qualitative cafeteria. The theory in particular, and qualitative research in general, started splitting into numerous subfields like an amoeba on steroids, and it continues to do so. It has produced good research, but also some terrible work, and sometimes it’s nothing more than positivism with propositions. For several years I’ve been arguing that all “qualitative” means now for certain is propositional rather than numeric data. Much, perhaps most of it seems to me to have lost the link to the 19th century founders of social research who argued that social science was different because it had to include subject intentionality and lived experience in order to describe or explain the phenomenon of interest, namely, us. There’s neither time nor space to fully develop the argument here, but I’ve gotten to the point where I’m no longer so sure it was a good thing, this conceptual innovation.

At the time the Glaser and Strauss book appeared, though, it was a conceptual innovation that gave a vocabulary to a part of what ethnographers did--had always done in one way or another--in their research.

Creation and Diffusion

So there you have four examples of methodological innovation, new ways of seeing a problem with consequences for action, be it research or practice. Other people would no doubt

have different lists of candidate examples. Mine are obviously selected out of a biography oriented to ethnographic research. But I'm pretty sure that most social scientists—and others—would agree that whatever a general model of “methodological innovation” should look like, it would have to explain these four cases.

Which brings me to the second part of the topic that the innovation workshop asked us to address, namely, how did the innovations “develop” and “diffuse?” Do the four examples have anything to suggest about a candidate answer to that question?

These four cases—and I make no claim for their representativeness—hark back to the romantic image of the individual innovator rather than to an organizational or team model. We can assume that all the innovators were well-paid tenured academics, though I'm not sure about Bartlett. So the romantic image doesn't extend to going hungry and working by candlelight in the chilly air of winter because the utilities couldn't be paid. And of course the four innovators had colleagues and presented papers and the like, though in the case of Zadeh and Goffman, at least, I know for a fact that in the early days of their innovations colleagues scorned more than praised them. Again, I don't know how it went with Bartlett, or Glaser and Strauss, when they first proposed their innovations, though I did hear many wisecracks about “grounded theory” when it first appeared, as in “be sure and run a wire from your theory to a water pipe.” And of course the two of them unloaded their book into a world, at least then in the U.S., where the norm was to say of anything that didn't offer a quantitative test of a hypothesis that it was “mere journalism.”

But it does look like these four examples were products of individual genius rather than of teams working in well-supported organizational settings. Of course back in the day a “social science innovation institute” on a par with, say, Bell Labs, didn't exist, as far as I know, with the exception of a few places that offered short term visits, like the Center for Advanced Study in the

Behavioral Sciences at Stanford. And later in life, for example, Goffman must have benefited from participation in the interdisciplinary Center for Urban Ethnography at the University of Pennsylvania. The relation between powerful conceptual innovations of the sort described here and the world of a person to whom the innovation is attributed calls for much more work. Material is available in fields like the recent field of Science and Technology Studies and in research like the recent book by Hage and Meeus (2006), among other places.

Which leads into the final question that the workshop organizers posed. And how did the four innovations diffuse? They didn't, certainly not right away, not for a long time in Bartlett's case. Work on the diffusion of innovation becomes relevant here, another field that is beyond the scope of this brief essay (Rogers, 1995). The only clear theme across the four cases I used here is this: They were all outside their disciplinary paradigms of the moment, "paradigm" in the traditional Kuhnian sense of a socially ratified framework among a group of scientists enforced with the mechanisms of disciplinary power. Diffusion of a concept outside of, or, worse, in opposition to such Kuhnian paradigms requires a substantial number of disciplinary heretics willing to experiment, and the experiment can cost the "early adopters" all the different kinds of capital that Bourdieu described.

So then why did the work of the four innovators described here take off eventually, because they all did? This, too, is a question beyond what a workshop presentation can handle. If I were going to launch a project to answer the question, I'd look to broader histories and how they changed the larger context such that a marginal innovation suddenly becomes a central one. Many things changed in the U.S. in the 1960s, things that now, for better or for worse, are normal parts of the social and intellectual fabric. My personal hypothesis would be that two hundred years of effort to make social science look like a laboratory science based on Mill's

experimental methods met an emergent norm to question authority, all at a time when social science failed to predict, describe and explain events during a period of dramatic change. I'm writing a book called *Un-Science: The Arrested Development of Human Social Research* with Left Coast Press based on that premise right now, so I might as well plug it.

The four conceptual innovations used here—this is my bias as a member of the generational cohort that adopted them—answered the call to produce more accurate models of what William James called “blooming buzzing confusion.” He was describing an infant’s perception of the world, though the phrase could describe how a lot of us felt as the 50s cascaded into the 60s. I liked Margaret Mead’s book at the time where she argued that we were all immigrants to a new world where there weren’t adults with experience of it to offer any guidelines (1970).

The conclusion—or rather continuing question—is this: The takeoff of social science innovation, in the conceptual sense used here, is as much, if not more, about larger contexts of history as it is about perceptions of a proposed innovation within a social science research community. When “Question Authority” becomes a bumper sticker, innovation rates will rise. And of course it wasn’t just social science. Think tectonic plates and DNA and complexity theory just to get started.

A Conceptual Think Tank?

The four cases described in this article were lone rangers. What if there had been an organization to support them collectively? What if there were social science think tanks? Would

innovation and diffusion increase, with new Zadehs and Goffmans and Bartletts and Glaser and Strausses finding an institutional home and synergizing away every day?

In an interview with Jean Piaget, the source of which I can no longer remember, the interviewer asked him if there was any question that American audiences were particularly fond of asking. Yes, he said. After he had presented his stages of development, a hand would go up. Piaget would nod at the enthusiastic audience member. He or she would ask, “Is there any way that we could speed that up?” He called it “the American question” and you can discuss it with an internet search.

So, is there any way that we could speed up conceptual innovation?

I don’t know. I do know that conversations during and after the workshop produced collective ideas that reminded me of an organization type I know about because I used to work with social service programs out of a complexity model of organizational development (2010). Here’s what I thought I heard.

First of all, such an organization needs continual support. It can’t be a one-off conference. It needs to be a place where people stay, and for a long time. The purpose is to support and encourage conceptual evolution in the social sciences. Evolution has a hard time if there is a major extinction every week.

Second, the organization must be built on what the complexity types call “generative relationships” (Lane and Maxfield, 1996) or “complex adaptive responses” (Fonseca, 2002). Another way of thinking of it would be in terms of Vygotsky’s “zone of proximal development.” The point is to gather people together who are similar enough to communicate but different enough to disagree continuously in interesting ways.

A third necessary feature is a few specific shared problems to work on, problems that everyone agrees are worth thinking about that have defied previous attempts to solve them. Without a shared problem to anchor the arguments and against which to evaluate the innovations, the discussion will float off—or explode into—conflicting personal and disciplinary agenda, in other words, regress to the mean.

The fourth feature is personal security and trust. Threatened and paranoid participants will be so obsessed with playing defense that there'll never be a good game. (The workshop was held during the World Cup semi-finals).

And finally, aligned interests will ensure diffusion. The innovating group itself will of course have aligned interests, otherwise they wouldn't be participating. But diffusion means the innovation will spill out of the innovating group into social networks of all kinds. Ideally the aligned interests won't just link up academics with other academics. It will link up all kinds of people with academic, organizational, and political/economic interests in the problem as well. By aligning a variety of interests, social science also earns an automatic answer to the question of publics who fund its activities—“why in the hell are we *paying* for this?”

All four of the examples used here eventually aligned with interests outside their academic discipline of origin. All of them diffused into practices outside the university as well.

Just for fun. I googled “social science institute” and then clicked on the yahoo directory that came up. The first entry was the ESRC, the sponsor of the “Methods Fair” and the workshop I'd been invited to. The second entry was the Institute of Social Research at the University of Michigan, where I'd taught for two summers and then stopped because what I was teaching didn't fit the survey research paradigm. It was an amicable and mutually desired separation, but

the description of the reason for the separation is correct. The irony of the workshop was the great conversations I had with faculty from that Institute who were also workshop participants.

I still don't know if an organization would make a difference. Mostly I've always worked alone, first in the academic tradition, then for the last 15 years or so as an independent. There have been moments that, in a modest way, exemplified the concept I'm describing here. They were non-academic, though, social research groups linked with treatment centers for heroin addiction, one in Kentucky in the late 1960s and the other in New York in the early 1970s and the last one in Baltimore in the early 2000s. And one of the reasons I've enjoyed my post-academic life since the mid-1990s is because of working with mixed groups on a shared problem, though sometimes it can all go south. Some projects have the volatility of rock bands.

Right this minute, my favorite project is a group involving academics and a business, a mix of artificial intelligence and anthropology (hold the wisecracks please), around the problem of putting "culture" into language learning software. Academic parts of my life have had their synergistic moments, to be sure, like the first few years of helping build a practitioner-oriented graduate program at the University of Maryland. But, by and large, the several permanent and visiting academic positions I've had weren't innovation-breeding organizations. I'm thinking more and more that innovations produced by academically based social scientists happen in spite of, rather than because of, the university settings. Another major topic that I have to neglect here for reasons of time and space, but I think changes in the university and the academic market over the last few decades have made conceptual innovations in social science more difficult to accomplish in the very place that should generate them.

Before I briefly summarize the presentation, let me mention the one disagreement I had with the preliminary conclusions of the workshop organizers. None of the innovations discussed

in this article were technology-driven. No disrespect intended. Technological innovation in social sciences has been impressive in my lifetime. I can't imagine research without my QDA software, digital recorder, and bibliographic software. But none of them changed the conceptual system I used when I did my research, at least in my case. They made it possible to do it more efficiently, but not differently in any fundamental way.

I may have the problem here that my father described. He was an old-time film professional who started out as a news photographer before World War II. He showed me how in the early days of television dramas, the crew basically filmed the production of a play. The fixed camera simply watched what happened on the stage like a member of the audience would. Gradually the industry learned what they could do with multiple cameras and location work and post-production editing and now computer-generated animation and special effects. My comment on technology might well be like the guys who first used the TV camera. But, at least as of today, I think the important innovations in social science are *conceptual*, a way of seeing the social world, and that the most important innovations that matter will be at that level. Facebook postings are still symbolic data in service of models of intentionality and lived experience.

I wouldn't mind being wrong. It would probably be a lot of fun learning why and give me some good stories to tell at the senior center.

Summing It Up

So there you have most of the themes I squeezed into my workshop presentation. I'm not sure I achieved much. That's not false modesty; it's true modesty. For what it's worth, here's a summary of the main points:

1. "Methodological innovation" in social science is about conceptual innovation, not technology or specific ways of gathering particular kinds of data.
2. The four cases that came to mind that I used in the presentation were examples of this kind of innovation.
3. The four were products of academically based individuals rather than organizations.
4. In most cases it was clear that the innovation was met at first with skepticism if not ridicule.
5. Substantial time passed before "uptake" occurred, in the sense of diffusion of the innovation into several communities of practice, academic and otherwise.
6. It's not clear to me if an organization of one form or another would enable, encourage and accelerate innovation in the social sciences, but such an organization was imagined and described.
7. In the end, a serious look at the question that the workshop organizers posed calls for more case studies, both individual and organizational, and a review of several relevant literatures.

At this point, though, I've kept my promise to the organizers, who are also the editors of this issue, to write up something that resembles the presentation I gave in the ESRC workshop. Preliminary and informal as it might have been, my hope is that it is useful to some up-and-coming social science innovator out there in cyberspace.

My advice to that future innovator would be more or less what a member of the U.S. House of Representatives said in a conversation some years ago, on the island of Cozumel where we were both scuba diving. Half the time, he said, I have to do things to get elected. The other half, I can work on policy initiatives that I care about. I love that second half.

Chasing new ideas half the time isn't such a bad deal. He was a Republican, but he was a good diver.

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