FORUM: QUALITATIVE SOCIAL RESEARCH SOZIALFORSCHUNG

Response to Wolff-Michael Roth, "But Does 'Ethnography By Any Other Name' Really Promote Real Ethnography?"

Michael Agar

Wolff-Michael ROTH pays me the compliment of considering my article in *FQS* worth the time and trouble to write an elaborate comment. He makes many interesting points that allow me to expand a bit on the original and also reply to a point or two that I respectfully disagree with. I appreciate that he and the journal made room for me to respond. [1]

The mention of Carlos CASTAÑEDA'S books brought back memories, since I was an Achtundsechziger, as the Germans say, at Berkeley during those times. I also enjoyed the first book, *The Teachings of Don Juan*. But if memory serves, that book poses no problems for the argument I made. On the contrary, CASTAÑEDA's first book was a powerful example of IRA logic and the process of tacking back and forth between points of view via context and meaning questions. It fits the argument very well indeed. The book did have an odd structural analysis tacked on at the end, which we 1960s grad students always attributed to the UCLA faculty's need for "theory" of one sort or another before they signed it. The subsequent controversy, though, was about whether or not CASTAÑEDA made it all up, *not* about whether it looked like a "real" ethnography. According to the argument in my original article, it did. [2]

In fact, the IRA/POV model fits many other approaches to understanding as well. Long ago I wrote an article about the then new American interest in hermeneutic philosophy (1980). In it I used the novel *Blood Ties* by Mary Lee SETTLE. Her novel was built, in part, on IRA/POV driven scenes embedded in a Turkish milieu. And another example—during my long career in drug research, I often had the strange experience of having my best conversations with people who worked in drug enforcement intelligence. We disagreed on goals, to put it mildly, but we thought and worked in the same way, IRA/POV. This epistemological model kept growing over the years and helped me understand how an ethnography was more like a good mystery than it was like a traditional social scientific study. [3]

ROTH's next section on "boundaries" is more difficult for me to comment on, since I don't agree with some of the ways he positions me in his discussion, nor do I share his uncritical valorization of the "peer review" process, nor do I think he uses the "boundary" concept in a clear and consistent way, nor am I comfortable with the way "culture" suddenly appears, one of the most problematic concepts in our current era. In fact, I wrote about "culture" based on another of the lectures in the series. It has just been published (2006). [4]

These are all issues that deserve more discussion, but for now let me underline ROTH's last paragraph in this section. What I want to foreground is the fascinating political story he tells and how the ethnography "matters" because it is "compelling, interesting and plausible." This is well said and I will return to it at the end. [5]

Moving on to the section on identity and difference: Again I don't always agree with the way I'm being positioned in the argument. ROTH's experience on review panels keeps coming up, for example. I've reviewed grants from several English-speaking countries since 1973, including four years on a National Institutes of Health panel. Such experiences did motivate writings such as my *FQS* article, but not so much as a self-interested power-play as an effort to correct ignorance and confusion and open up previously closed funding spaces to researchers like readers of this journal. In fact, I don't care much for the actual job. It involves reading deathly dull grant language and then explaining and defending qualitative proposals using the same arguments over and over again. The core of those arguments, phrased more abstractly, is in the IRA/POV model. [6]

Once ROTH shifts to catastrophe theory and the chaos theory that followed it I'm delighted. Much of my recent work draws on the "complexity theory," that followed them, associated in the U.S. with the Santa Fe Institute, though now it has diffused widely in terms of national centers and disciplinary participation. [7]

It's beyond the scope of this response to fully describe, but much of that work links ethnographic practice with a complexity epistemology (2004a, 2004b). I'll just say here that the IRA/POV argument links with the general complexity concept that simple agent-based rules can produce a variety of patterns at the system level, depending on local context and processual contingencies. Though complex adaptive systems can produce different results over time, there are limits on the possibilities. The space of possible results within those limits is an attractor. [8]

ROTH concludes with a section called "Toward a Difference Perspective." After some discussion of various examples, he returns to my article and concludes that readers should make up their own minds. I couldn't agree more. He says there are no panaceas. No panacea was on offer. He is suspicious of anyone who claims to offer a simple model. So am I, though I am also suspicious of complicated models, or any model at all for that matter. But remember the complexity idea that simple models can produce wondrously complicated results. At the end he says he can live with a motto: "We are different because we are singular rather than being singular because different (from the same)." I don't understand what this means, and I don't know what this conclusion tells me to say to someone who asks me to talk about ethnography. The final lines ask to what degree we are the same and what is the degree of overlap between what two ethnographers do. That's the question I was answering. At the end I'm at a loss for what the debate is about. [9]

All of which brings me to the major issue that ROTH's commentary brought up for me. "How do you tell if it's a real ethnography" is, on the face of it, a silly question, the more so in academic environments where the notion that anything might be "real" is diagnosed immediately as false consciousness. With the luxury of hindsight I'd have framed the question differently, though I did like the way the framing evoked a parody of commercial advertising, as in "Coke: It's the real

thing." A variation on the old classic CHOMSKY sentence, meant to show ambiguity in surface structures, comes to mind: "Telling jokes can be dangerous." [10]

But the question, or a better version of it, is critical for my work. Most of it has been tangled up with the non-academic world, partly due to a long history in the U.S. substance abuse field, most recently because I left the university ten years ago to work independently. That world is heavily populated with three kinds of people who—increasingly over the last couple of decades—have become interested in ethnography, or qualitative research more broadly. Those people are: 1) Researchers deeply embedded in the positivist tradition of social research; 2) Practitioners with a history of suspicion of social research as an imposition on their time that produces little that is useful; and, 3) Policy-makers who require credible information that covers a larger population than qualitative research provides to help with a range of choices limited by severe political constraints. It's not an accident that the invitation to lecture came from ethnographers in a graduate school of education. They work with the same kinds of people. [11]

What we—we ethnographers—do is strange from all those points of view, counter-intuitive even. It has surface characteristics that lead to suspicions that it is "just anecdotal," in other words, lacking in credibility. What we do requires training and experience, probably the apprentice model is the best. It is a professional expertise for which there is no instant microwave-like preparation. I think that ROTH would agree. [12]

This kind of work—I'd say in the "real" world but I'm in enough trouble already—raises two issues. The first is, the surface features of our research practice look like ordinary life rather than a chemistry lab, so novices decide it's easy to do, the more so if they are steeped in standard social research training, which to their shock turns out to be an impediment. Bad research is the result. We need a general framework in terms of which to offer a principled critique rather than a display of knee-jerk territoriality. [13]

The second issue is, the three kinds of people described above will have reasonable questions about what we do, before, during and after the work. We need a general way to talk about it so that we can participate in the collective conversation and help shape a multi-perspectival discourse that opens minds and changes actions. I think ROTH would agree with this as well. [14]

The situation just outlined requires us to come up with a general way to talk about ethnography, how it is done and why it is different, and how to tell a good one from a bad one. I'm in the business because of intellectual fascination, but also because of its moral and political value. All three motives move me to seek a language to describe what we do and to train newcomers in its specifics. My proposal for an IRA/POV framework may be the wrong one, though of course I think it's useful. It would be interesting to gather a number of ethnographies that are "compelling, interesting and plausible" to use ROTH'S words and see if an IRA/POV process shines through. I'll bet it would. At any rate I'd be glad to see a better alternative. [15]

If ROTH is arguing that the kind of question I asked shouldn't be asked and answered, I couldn't disagree more. On the other hand, if he is arguing that the question should be answered in a different way, I'm open to the possibility, but he doesn't leave me with a clear alternative. He offers some good issues about our work and the way we do it, but he doesn't answer the question that must be answered if we are to participate in and contribute to the world where research, practice and policy intersect. [16]

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Citation