The Resident Researcher in Social Ethical Perspective

Richard Walsh-Bowers

Wilfrid Laurier University

Wicker and Sommer (1993) propose an interesting alternative to the conventional career path of academic social scientists. In my commentary on their proposal I reflect on what I understand to be the goal of their model in terms of the core values of community psychology. While emphasizing community-based development, I concentrate on the social responsibility of researchers in relationships with other citizens by drawing from overlapping historical, epistemological, and ethical critiques of psychologists' scientific foundations and their social roles as scientists (Danziger, 1990; Prilleltensky & Walsh-Bowers, 1993; Sampson, 1991; Sullivan, 1984; Walsh, 1987; Walsh-Bowers & Danziger, 1991; Wine, 1989). My premises are that, like all scientists, psychologists socially construct their research methods from historical antecedents, and that the conduct of human psychological research is an inherently relational activity.

My initial reaction to Wicker and Sommer's article was, haven't community psychologists, like Jim Kelly and Don Klein, already been practicing "resident research"? For another example, I am aware of my colleague, Ed Bennett's (1992) work on community-based economic development in his home region of rural southwestern Ontario. When I recognized that Wicker and Sommer were speaking with an environmental psychology voice, their argument for "applying scientific scrutiny to familiar people and settings on a long-term basis" became clearer. In their presentation the authors note that training in and the practice of social research emanate from the traditional framework of generating universal laws of human behavior according to the hypothetico-deductive, statistical model of making science. But in other nations qualitative and action-research approaches based on a different paradigm have been taught and practiced by community-minded psychologists for several decades (e.g., Serrano-Garcia, 1990;

Walsh, 1988a; Zuniga, 1975), and in U.S. community psychology there is some evidence that these approaches are gaining credibility (Tolan, Keys, Chertok, & Jason, 1990). Given that some researchers have been dancing to a different beat for some time, Wicker and Sommer's point deserves historical and cultural boundaries.

Similar contextualizing is required for the authors' recommendation that researchers should study the settings, contexts, and solutions associated with their topics. In fact, the historical record shows that mainstream psychologists have obstructed others from engaging in these practices in several ways. First, they virtually banished from their canon the Lewinian precedents for this type of research and they marginalized contemporary sequels, such as the work of Argyris (Walsh-Bowers & Danziger, 1991). Well before he emigrated to the United States, Lewin and his colleagues concluded that the proper object of research was not the isolated individual but the person-in-a-situation and that the relationship between "experimenters and subjects" is an essential aspect of any investigation. It was on this basis, then, that Lewin later developed the concept and practice of action research. However, this development was thwarted by extremely powerful disciplinary norms that insisted on strict adherence to a natural science model, not a Lewinian *social* science model, for research. So pervasive was this influence that the founding "fathers" of U.S. community psychology pledged allegiance to their natural science heritage at the 1965 Swampscott conference (Walsh, 1987). Second, mainstream psychologists inhibited early U.S. community psychologists from fully expressing their incipient research values of collaboration with citizens. Eminent community researchers disclosed in extensive interviews with me that they adhered to positivist research norms, at first to establish and then to maintain their credibility in the eyes of more powerful academic psychologists. Although our orientation is to practice field research, it consumes valuable time, and, as these informants attested, in a workaday world of pressure to publish according to natural sciences criteria we have been rewarded for producing "hard" data and punished for cultivating "soft" data. Third, up to recently at least, mainstream psychologists effectively contained the impulses for paradigmatic change abundant in feminist methodology. Despite feminist psychologists' espoused values of inherently relational research methods, oriented to social action, the first decade of research articles published in two prominent feminist psychology journals did not reflect these values; rather, most articles exemplified the mainstream mode of impersonal, decontextualized lab-like investigations (Walsh, 1989).

Social historians of U.S. psychology have observed that adherents of the predominant positivist vision, with their focus on the abstracted, universalized "subject," have not perceived settings, contexts, and solutions as relevant to their science (Danziger, 1990; Sarason, 1981). Consequently, there is an internal contradiction in Wicker and Sommer's argument between their notion of what constitutes science, which is derived from the natural sciences model, and their proposed resident researcher model which, thanks to Louise Shedd Barker and Roger Barker, represents the Lewinian model. However, the epistemological tenets of naturalistic inquiry (Lincoln & Guba, 1985), participatory community research (Serrano-Garcia, 1990), and many feminist methods (Reinharz, 1992) *are* compatible with Wicker and Sommer's proposal, because, in part, they emanate from a democratized research relationship (Walsh-Bowers, 1992). Perhaps the contradiction surfaces as a consequence of traditional appeals to the natural sciences for legitimacy, yet the biology analogy is just as problematic as the physics analogy was for laboratory psychologists. For instance, both epistemological analogies assume separation between knower and known as if researchers in human environments have as little effect on their settings as field biologists allegedly do behind duck-blinds. But in the revised philosophy of science all scientific inquiry is an inherently transactional process between knower and known (Manicas & Secord, 1983); that is, observer and observed are engaged in a system of mutual influence (Oppenheimer, 1956). Furthermore, Altman (1987) pointed out, in this journal, that in transactional methodology:

> Events may be construed in different ways by different participants and observers, no one interpretation is "correct," and the investigator ideally should triangulate on an event from the perspectives of participants and observers. (pp. 623-624)

Curiously, Wicker (1990) alluded to the transactional epistemology in his contribution to the Tolan et al. (1990) volume, but a comparably explicit relational consciousness does not appear in the present article.

Concretely, the contradiction between conventional science and resident research is played out in Wicker and Sommer's recommendation that resident researchers should identify their values and advocacy stance when presenting findings in formal scientific outlets. I strongly agree that researchers should personalize reporting, but is there such a report-writing tradition among contemporary psychology editors and reviewers? Again, the social history of our discipline's research practice is instructive (Danziger, 1990; Walsh-Bowers, in press; Walsh-Bowers & Danziger, 1991). Although there have been some historical exceptions, indicating sympathetic authors might have had more latitude than they thought, such reportage has been rare. Accordingly, a broad-based systemic transformation

of reporting standards is necessary for this recommendation to bear fruit (Trickett, Kelly, & Vincent, 1985; Walsh, 1987, 1989; Walsh-Bowers, in press). Along these lines, for several years now, the *Journal of Community Psychology* and the *Canadian Journal of Community Mental Health* have published reasonably comprehensive guidelines for contextualized reporting in their instructions to contributors. *The American Journal of Community Psychology* briefly did so, then severely reduced the guidelines to a lone statement about using the term "participants." The new editorial guidelines, as well as the revised instructions to contributors, now explicitly prescribe descriptive attention to the research relationship (Trickett, 1993). Overall, the historical pattern in AJCP suggests that even among community psychologists there has been some resistance to changing socially constructed scientific mores.

From a social ethical perspective, Wicker and Sommer promote a cost–benefit, utilitarian approach to researcher–citizen relations, which incorporates "selling" the local public on the benefits of research. Besides the fact that other psychologists have proposed somewhat similar approaches (e.g., Chavis, Stucky, & Wandersman, 1983), Wicker and Sommer do not address the self-serving nature of what is perhaps an ethically dubious enterprise. How is community-based development aided and abetted by social scientists' selling their expert-created products? The fundamental error of "giving psychology away" — or selling it — is that the process strengthens psychologists' role as expert and citizens' role as passive recipients of the formers' expertise while enfeebling citizens' own capacity for creating knowledge and solutions in their communities. In a *democratic* approach, citizens would contribute as equal partners with researchers in every phase of their joint efforts (Sampson, 1991; Walsh-Bowers, 1992), hence obviating the need for persuading the public of researchers' worth. To be sure, at several points Wicker and Sommer emphasize consulting other local residents about research issues, including the use of citizen assistants and community feedback, but these specific practices do not flow organically from an overall conceptual and value base of collective democracy in research. Rather, in their model the research is seeded and fed by the scientist, who apparently knows more than the residents, instead of by the community. In my opinion, this type of research relationship is not conducive to constituent validity and community-based development, in contrast to a truly collaborative relationship (Walsh, 1987). A democratic alternative would provide an empowering climate nurtured by the social ethical values of self- and community-determination, distributive justice, democratic participation, and relationality expressed in compassionate caring (Prilleltensky & Walsh-Bowers, 1993).

Some specific ethical questions about community research arise in Wicker and Sommer's proposal. The authors assume that, overall, the resident researcher model is benign except for potential problems with confidentiality and researchers' dual roles as investigator and citizen. But what about the special interpersonal difficulties endogenous researchers encounter, as noted in the utilization-focused evaluation research literature (e.g., Patton, 1986)? For example, researcher-evaluators in these situations are assessing quality of life indirectly if not directly, and then, as any investigation is also an intervention (Trickett et al., 1985), their work has implications for change at personal as well as community levels. In other words, their own personal, hidden resistance to change is triggered. And what about the resident researcher's ethical responsibility for anticipating cooptation by the bearers of local political power? Echoing O'Neill (1989), to whom and for what is the resident researcher responsible? These considerations deserve more development than a cost–benefit orientation to ethical matters can provide. Rather, they require a social systemic perspective in which acknowledgment of social power and relational values are primary.

In conclusion, I am moved to ask, to what extent does the resident researcher model actualize community-based development while promoting career development? Given the long, entrenched history of psychologists' adoption of the parentalistic expert role in their relations with the public (Napoli, 1981; Reiff, 1974), my fear is that career development not only will take precedence but the iatrogenic potential for harm to citizens inherent in professional domination will escalate (Rappaport, 1981; Walsh, 1988b). The resident researcher model needs firm grounding in a social ethical framework of democraticized relationships.

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