

Participatory Action Research Should Social Inquiry Be Conducted Democratically?

LEONARD KRIMERMAN
University of Connecticut

Participatory Action Research (PAR) insists upon the importance of democratizing social inquiry by actively engaging the subject in the design and conduct of research. Drawing on four examples of PAR-based social science and a democratic reconstruction of “epistemic privilege,” this article argues that philosophers need to take seriously PAR’s notion that democratic norms should guide social inquiry. But it does not advocate replacing mainstream or expert-directed social science by PAR. Instead, it maintains that it is both possible and sensible for PAR practitioners to collaborate with conventional research. Indeed, certain forms of nonparticipatory social science seem indispensable for any extensive application of the PAR framework. The article concludes by drawing out its (controversial) implications for two central issues in the philosophy of social science: first, that the methods of social inquiry are distinct from those in the natural sciences and, second, that there is a sense in which social research can and should be “value neutral.”

If you want to understand what a science is, you should look in the first instance not at its theories or findings . . . you should look at what the practitioners of it do.

—Clifford Geertz (1973)

Clearly, the relations between the expansion of knowledge and the expansion of democratic social relations . . . need to be explored in greater detail.

—Sandra Harding (1998)

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1.

In the final chapter of his remarkable *Philosophy of Social Science: The Methods, Ideals, and Politics of Social Inquiry*, Michael Root (1993) contrasts two notions of social science, which he refers to as “liberal” and “perfectionist.” The first pursues

the liberal ideal of value neutrality [seeking] a form of social research . . . that abstracts from the values people attach to objects and events [and which is] . . . acceptable to constituents with different and competing commitments. (P. 229)

The latter (perfectionist) rejects value neutrality and any attempt to separate facts from values (at least within the social world); instead, it

seeks after the facts in ways that advance some set of substantive values or one conception of human perfection over another. On this alternative view, the social scientist is overtly partisan and openly chooses her methods or validates her findings on the basis of some moral or political commitment. (P. 229)

Root (1993) proceeds to distinguish between various sorts of perfectionist social science and then to provide arguments in favor of one of these, which he labels at some points “communitarian” and at others “participatory.” In this model, research is designed to advance “the way of life or the shared ends of a particular community.” Moreover, this community itself “has a voice or hand in how the research . . . is conducted.” Communitarian or participatory research (also known as “participatory action research” or PAR), then,

includes devices for increasing (rather than limiting) the active presence of the subjects in the conduct of the research and aims more at helping the subjects to learn about themselves than at finding theories or models by which others can explain or predict their behavior. (P. 230)

1.1

For Root (1993), the line between liberal and participatory social research seems to be a sharp and unforgiving one. The former, as just noted, “pretends [on his view, deceitfully or, at least, unsuccessfully] to be nonpartisan,” while the latter is “open and deliberate in [its] partisanship and ties to practice.” Beyond this contrast, however, he sees

the two approaches as conflicting along such key dimensions as the principal goal of the research, who learns from it in the first instance, and the relations between researcher and subject. Summing up their incompatibility, Root contends,

Liberal research is research by disinterested experts. The subjects cannot be given a say in the research because they are not experts and not disinterested. . . . As a philosophy of science, liberalism supports the rule of experts . . . and a limit on popular authority. . . . The democratic ideals of participatory research, by contrast, support no limits on popular authority and encourage citizens to participate in areas of public life previously reserved for experts. (P. 249)

Root (1993) concludes *Philosophy of Social Science* by expressing the hope that his discussion of “communitarian” alternatives to mainstream, liberal social science “has been interesting and attractive enough to make them a central topic of future work in the philosophy of social science” (p. 250).

2.

This hope, as far as can be told from a quick scrutiny of several (English-language) journals dedicated at least in part to the philosophy of social science as well as of the recent Martin and McIntyre (1994) anthology, has yet (after some 7 years) to be realized. This is a shame, since participatory action research is not just an idea in some philosopher’s imagination but a very vital movement within most if not all social sciences, with associations and clearinghouses and Web pages devoted to it arising from all regions of the real and virtual world. (One Internet clearinghouse, www.par.net, maintained by Cornell University researchers, boasts a largely downloadable bibliography of close to 700 items.) Several established journals in diverse social science fields have in the past 10 years devoted one or more entire issues to its proponents and practitioners and, to a lesser extent, its critics. To neglect PAR, it seems to me, is to run widely afoul of Geertz’s (1973) admonition.

2.1

How to explain this philosophical neglect of PAR is not my primary focus here, but some of it may arise from two readily apparent

and apparently weighty objections to this approach. These objections have often surfaced in social science discussions among PAR practitioners and those sympathetic to but critical of PAR. (For a good example, see Petras and Porpora 1993.)

Our first objection can be put as a question: Why involve, in the design and practice of scientific research, those who are inexperienced, if not incompetent, and who have a clear and present stake in the results? Would not the quality of research produced by PAR (the reliability of any data it claims to uncover and the credibility of the hypotheses it advances) be undermined by “increasing (rather than limiting) the active presence of the subjects in the conduct of the research” or “by allowing their conception [of perfection or of the good] govern the [scientific] practice”? (Root 1993, 229-30). Let us call this the “Objection from Popular Incompetence and Bias.”

A somewhat less obvious but equally important objection is that there appears to be no way for PAR practitioners to distinguish good scientific research carried out according to their precepts from good community or social change organizing. A statement by Maria Mies (1983), an early advocate and practitioner of PAR, sets the stage for considering this second objection:

Participation in social actions and struggles, and the integration of research into these processes, further implies that the change of the status quo becomes the starting point for a scientific quest. . . . *According to this concept, the “truth” of a theory is not dependent on the application of methodological rules, but on its potential to orient the processes of praxis towards progressive emancipation and humanization* [italics added]. (P. 125)

In short, good PAR research here, and often, seems simply to be research that has an emancipatory or empowering effect. It is research that benefits the excluded, impoverished, marginalized, oppressed, and so forth by, for example, increasing their self-esteem, their participation in institutional decision making, and their access to political influence or economic resources. If so, according to this second objection, PAR might well be praiseworthy and desirable, but it fails to represent a contribution to or an alternative framework within social science. As Petras and Porpora (1993) phrase this objection, if participatory research is to prosper within “the larger discipline of sociology,” it needs “to yield substance, methods, and theory the larger field finds valuable quite apart from the participatory way in which these yields are generated” (p. 122). We can call this the “Objection from Confounding Political Ideals with Scientific Criteria.”

2.2

Despite these two *prima facie* objections and in the face of its neglect by philosophers, I am still drawn to support PAR's notion of social science research. In what follows, I will join Root in his effort to draw more philosophical attention to PAR and will build a case for it—albeit one that is less sweeping, less all or nothing, than his appears to be, and one that directly confronts the two objections I have just sketched. To make this project more concrete, I provide brief summaries of four pieces of research done intentionally from participatory models and then turn to the more general issues raised by them.

3.

These cases are (1) roofless women of Boston, (2) child laborers of Bogota, (3) battered wives of Cologne, and (4) schizophrenic recidivists in New Haven.

3.1. Roofless Women's Action Research Mobilization (R-WARM). Here, six formerly homeless women, with "technical advice" from a "steering committee" comprised of social scientists and service providers as well as homeless women, carried out research into the experience and causes of and remedies for homelessness. They designed a survey instrument and conducted interviews with more than 100 homeless women of many different ages as well as racial and ethnic backgrounds. According to Marie Kennedy (1998), one of the steering committee social scientists, the supportive professionals were committed to "making their expertise and broader perspectives available . . . while making sure that ultimate power for directing the project rested with the formerly homeless women" (p. 7). In addition, dialogue between researchers and researchees was encouraged on the grounds that

when the investigator has experienced the problem being investigated, dialogue is a means of discovering the shared nature of the problem and the common ground for action. Dialogue is the basis of eliciting unusually forthright responses, and more detailed and possibly more truthful answers to interview questions. (P. 7)

Among the many “findings” of this enterprise was that “domestic violence was cited as a cause of homelessness” (p. 8) by greater than half of those surveyed.

3.2. *Child laborers of Bogota.* Some 350 child laborers (younger than 18) worked on this project (1985-1987) with assistance and direction from social workers, social scientists from a nearby university, and officials from the Ministry of Labor. Maria Salazar (1991), one of the social scientists, described the goals of this PAR initiative as twofold: first, to enable the children to see themselves as producers of valid, important knowledge about their own working and living conditions and, second, to assist them in developing the capacity to envision and start creating alternatives to those (wretched, dangerous, exploitive) conditions. To reach these goals, the children, along with the outsider team, were engaged in workshops “stressing creativity, art, painting, drama, puppet shows, and pantomime.” In this way, a “cultural space” was developed for the child laborers “to articulate their own personal history” and “to understand that the expression of their ideas and feelings, such as in the poems they wrote, was an element of the useful knowledge they acquired” (p. 56). After a period of several months, the children’s self-esteem had increased to the point that they were willing to “introduce change into their surroundings and to establish organizations of young laborers as the beginning of a new social movement” (p. 56). For example, job training in such skills as bread making, carpentry, and mechanics was introduced, leading (again, with adult assistance of many sorts) to “cottage industries to be managed by the child laborers themselves.” More than 150 laborers (mainly those older than 14) collaborated in the

implementation of a bakery and carpentry shop. They planned, discussed alternatives, undertook organizational decisions and eventually established four bakeries and carpentry shops under the new rules of self-determination. . . . The shops have been functioning well after two years, generating income for the children and their families. (P. 59)

3.3. *Battered wives of Cologne.* This research project, conducted in 1976-1977 and discussed by Root (1993), grew out of the “need to document the seriousness and extent of the problem” and was conducted by “women activists” engaged in a campaign to create a publicly financed and accessible shelter for those suffering from this form of

violent abuse. These activists organized street actions and conducted interviews on the street, attracting more and more women to their cause, some of whom they sheltered in their own homes, thus generating lots of media attention. Eventually, city authorities themselves began to document wife battering, finding that many women indeed needed protection from it and, in time, granting a subsidy for a public shelter. According to Root, who draws on an article by Mies, the Cologne campaign “did not generate data in the usual sense of evidence that independent reviewers could use to validate claims of wife battering and domestic abuse” (p. 242). This is “because wife battering was not yet a privately or publicly recognized kind of conduct in the community.” The campaign itself, he claims, changed the status of violence against women by

establishing a practice within which the husbands’ behavior would be seen as abusive and criminal rather than as a show of temper, a fit of pique, a lovers’ quarrel, a domestic squabble, or a matter of family discipline. The campaign was needed to invent the kind . . . only through participatory research . . . could the facts of wife battering be discovered, because only through such shared effort could the kind have been invented. (P. 242)

3.4. *New Haven schizophrenics*. Psychiatrist Larry Davidson and colleagues (1997) summarize their PAR-based inquiry into “the problem of recurrent inpatient admissions for individuals diagnosed with serious mental illness” in the following way:

Conventional approaches . . . have neglected to invite the perspective or input of the person with the disorder, further exacerbating the passive and helpless role of mental patient into which these individuals have become socialized by virtue of prevailing patterns of cultural stigma and clinical practice. . . . This paper describes the failure of one [conventional] attempt to institute such an approach to addressing the problem of recidivism, and the use of phenomenological and participatory research methods to involve patients themselves in exploring the reasons for this failure and to suggest alternative approaches. These methods involved patients describing their experiences of hospitalization, discharge, and readmission, identifying the precipitants and reasons for their readmissions, and participating in the design of a new intervention that has proven more successful in assisting them in the application of phenomenological and participatory research methods in establishing more satisfying lives for themselves in the community as an alternative to returning to the hospital. The authors suggest that such participatory methods provide an antidote to the passive and

helpless role of mental patient often encouraged by conventional modes of clinical research and practice allowing for the recovery of an active role for the person with the disorder both in the theory and practice of clinical psychology. (P. 767)

4.

Given this modest grounding in actual PAR-animated social science, let me raise two philosophical questions:

- First, what kind of case and how strong a case can be made for PAR-based social inquiry? More specifically, how might the force of those two fairly obvious objections stated earlier be offset?
- Second, what relationship obtains between PAR and more conventional, or “liberal,” forms of social science? Must social researchers, as some PAR supporters and practitioners appear to allege, choose between them, a commitment to one approach requiring the rejection of the other? Or can these frameworks, at least under some conditions, complement and strengthen one another?

4.1

In addressing these questions, PAR advocates have mainly relied on two sorts of arguments. The first of these is an appeal to PAR’s connection to or strengthening of participatory democratic ideals; consider, for example, this statement by M. A. Rahman (1991), who has conducted participatory research in Asia and Africa:

PAR is a philosophy and style of work to promote people’s empowerment. [This includes] the formation of new people’s organizations if none suitable exist or the strengthening of existing popular organizations and promotion of a self-reliant, assertive culture within them. (P. 16)

The second strand of argument rests on the assumption that those with whom PAR works and identifies have a special or privileged access to certain sorts of social facts. One explicit formulation of this idea is advanced by Petras and Porpora (1993), who defend the “epistemic privilege of the poor”:

It is their [the poor’s] experience of reality that is privileged, that must be listened to, that must be taken seriously. If the overall project is liber-

ation, then it is to the experience of oppression that we must attend to first. We need to vividly understand not only how the poor see and feel their oppression but the solutions they envision. The solutions after all must come from them. (P. 117)

4.2

Now, for some time, I (rather unconsciously) treated these two strands of argument in isolation and as entirely independent. As such, it seemed easy enough both (1) to find counterexamples to their general premises and (2) to question their cogency as responses to my two objections to PAR.

4.2a

For example, to focus first on PAR's supposed connection to democratic ideals, if this mode of research "promotes or advances" the way of life of specific real-life communities or "oppressed groups," what of those groups or communities—they are far from uncommon—that are themselves ethnocentric, homophobic, racist, sexist, fascistic, and so forth? In such cases, PAR would ally itself with and serve extremely undemocratic arrangements. Moreover, on its face at least, promoting (in Rahman's [1991] words) people's autonomous or self-reliant communities seems unrelated to the scientific tasks of generating, testing, and confirming hypotheses or those of producing reliable and replicable data.

4.2b

Similarly, the postulate of "epistemic privilege," taken by itself, is (in these same two ways) doubly problematic. One can certainly doubt its soundness: why, for example, should homeless women be credited with more insight into their impoverishment than anyone else? People are often self-deceived about the factors responsible for their own situations, especially if those situations are the object of scorn or contempt. Or consider the battered women of Cologne, to whom Root (1993) assigned a kind of epistemic privilege—arising from their alleged invention, through collective practice, of a new social type or concept. Looking back at the Mies (1983) article where this case is described in detail, however, things became a bit blurry. Here is why:

Prior to the initiative in Cologne, according to Mies (1983), "there had been reports in the press about increasing wife-beating in German families and houses for battered women in England and Holland" (p. 129). Moreover, her article strongly implies that even the social welfare authorities in that German city knew quite well what wife beating/battering was: their response to the women's initiative was not to reject the idea of wife battering as absurd or unintelligible but (duplicitously) to claim that such women already had a place to which they could turn, that is, homes for "destitute women." In fact, in that city at that time (as those authorities well knew), battered women could not (safely) turn to such places, since most were not legally destitute, and all found that the lengthy red-tape process for gaining admission exposed them to high risk of retaliation from their abusive partners. Finally, the authorities, Mies tells us, wanted "exact figures about the extent of wife-beating" before they would create a special sort of refuge for victims of it; had there been no concept of wife beating, this sort of response (which presupposes an understanding of wife battering) would have made no sense.

In brief, the category in question here appears to have predated the initiative: wife battering was not an invention of the women activist researchers but had already taken shape in the wider society. Perhaps other activists played a role in the shifting norms and practices that enabled wife battering to emerge from its conceptual closet. Perhaps. But perhaps, like chattel slavery, the concept (and its heinousness) were there all along; what was lacking were enough courageous people willing to rise up against the abuse and demand protection for its victims as well as its immediate cessation.

Finally, it seems clear to me that social scientists themselves can, do, and must play a major role in the invention and extension of key concepts; think of Freud's discovery of unconscious intentions. The crusade to treat or release mental patients (to view lockup without treatment as abuse), to acknowledge certain forms of mental illness as excusing factors that allay or diminish responsibility, to view certain sorts of industrial production as unacceptably exploitative and alienating, and no doubt many others were all, at least largely, products of nonparticipatory, researcher-controlled inquiry.

To take one example in more detail: consider the "action research" on so-called severely retarded persons conducted by Renee Fuller (1977), a clinical psychologist, herself labeled "reading-disabled" and "dyslexic" as a child. Using a novel (visual and storytelling) method

for teaching reading, she claims to have enabled hundreds of persons with IQs lower than 50 to read at modest levels and even to write autobiographical accounts of their experience of institutionalization and of their awakening from it. (Her claims have been confirmed by outside observers; see, e.g., Conner 1989.) To wait for these terribly demeaned and damaged folks to construct a research design to better understand their own (mis)classification and institutionalized (mis)-treatment or to mount a campaign against it seems to me absurd, if not complicit. Social scientists can, do, and indeed must play at least an initiating if not a central role in identifying and responding to oppressive conditions on behalf of those oppressed by them.

4.2c

Beyond these questions as to its soundness, however, the postulate of epistemic privilege runs into logical difficulties as well. If we grant such privilege to the impoverished or to homeless or battered women, why not as well to those with wealth, those who are well housed, or those who do the battering? Presumably, if one such group knows its own story or special situation best or most fully, then so do the others. What makes epistemic sense for the gander must do likewise for the goose. If so, this may submerge us in a relativistic mud hole of indefinitely many "valid" or "privileged" perspectives between which there is no hope of communication, much less adjudication.

Furthermore, what exactly does epistemic privilege come to? Often, it seems to reduce to a couple of very prosaic or commonsense ideas:

1. insider advantage: most social situations—families, neighborhoods, organizations, and so forth—are quite complex, and those who inhabit them for long periods of time usually know more about them than those fresh to them (see the Bogota case above); and
2. respect-generated advantage: people will reveal themselves, or intimate facts about themselves, only or more readily to those they trust and who treat them with respect, consideration, reciprocity, and so forth (see Marie Kennedy's account of R-WARM).

But the truth of the above points hardly favors PAR to the exclusion of other types of social research. As in the case of participant-observer research, outsiders can become insiders after awhile without actively advocating on behalf of those they are studying. And academics who retain control over an inquiry can certainly show respect for the time, the opinions, and so forth of those they interview, observe, or other-

wise research. So even if Points 1 and 2 above are granted, this falls short of providing PAR, or the groups it champions, with any sort of exclusive epistemic privilege.

5.

But what—following Harding’s (1998) proposal quoted at the outset—if we were to join together these two lines of argument, that from strengthening democracy and that from epistemic privilege? The resulting whole might be greater than the sum of its parts in isolation. Let me explain by sketching two separate ways of reconstructing epistemic privilege so as to meet democratic criteria.

5.1

On the first reconstruction, the poor, the disenfranchised, the marginalized are privileged in this sense: those who have been silenced, whose voices have yet to be heard or lack influence or reach, both need and merit more than equal consideration; concretely, they merit access to opportunities and resources, now denied them, that will enable their voices to be raised and to be counted. Unless and until this is done, any piece of social research (at the least, any which concerns such individuals) will be flawed or incomplete. As a corollary, those in power, whose voices already carry weight and have widespread access and influence—those already heard from—do not merit this sort of special consideration.

In short, on this first reconstructed account of epistemic privilege, social research should be governed by a kind of Rawlsian difference principle or, better, a principle of “democratic inclusion”: good or sound social research affords those at the bottom, the marginalized, the least heard from or considered, those opportunities and resources that will enable their voices to be raised and counted. (The insightful research conducted by Mary Belenky and her colleagues frequently exemplifies this principle; see Belenky, Bond, and Weinstock 1997; and Belenky et al. 1997.)

This principle of cognitive justice or democratic inclusion is a modest one; that is, it does not warrant ascribing any sort of exclusive or unlimited epistemic access to the marginalized or silenced among us. Nor does it view their judgments or reports as incorrigible—as exempt from challenge by findings derived from professional controlled or

non-PAR research. In the useful phrase of Harding (1998), it affords epistemic advantage; see here also Alison Wylie (2000) on situation-based advantage.

5.2

My second construction of epistemic privilege has its source not in the exclusions or inequities imposed on folks but on their ability to get beyond such obstacles. More precisely, one main goal of PAR, as Mies (1983), Rahman (1991), and others insist, is that of “emancipation”—in the Freirean idiom, “conscientization” (Freire 1970). This goal involves people, first of all, becoming conscious of their prior socialization, of the beliefs, attitudes, aspirations, and ideologies that they have unconsciously “internalized” or been indoctrinated into; and, second, reflecting on these received norms, as sociologist Peter Park (1993) puts it, “in the light of what they wish to achieve as self-reliant and self-determining social beings.” As a result, Park continues, those engaged in PAR come to see that neither their attitudes and behaviors nor the macro-level sources of them “have to remain the way that they are and that they can engage in actions to transform those realities.”

Viewed in these terms, the process of PAR tends to engender or enhance conscientization, thus affording those involved in it with a definite sort of epistemic advantage, at least in comparison with their prior nonemancipated selves and to others still trapped, unknowingly, within prevailing habits and dogmas. For through PAR, people become more capable of recognizing, questioning, and of going beyond epistemic obstacles imposed by widespread norms or prevailing beliefs, of discounting many of these as self-serving propaganda, and of initiating and assessing alternatives to them. PAR’s intent and frequent effect is, in the apt phrase of Naomi Scheman, to augment “epistemic agency.”¹

This second “democratized” reconstruction of epistemic privilege is modest as well. Like the first, it bestows neither incorrigibility nor exclusive or unlimited access upon PAR-based research or those with whom it allies. Its principle of democratic or emancipatory advantage avows only that, other factors remaining equal, those captive to external socialization are less credible sources or producers of knowledge than those capable of independent thought and initiative. In other words, according to this principle, good or sound social research credits the reports and inquiries of the emancipated more highly than

those of the unemancipated. But even if one accepts this second democratic norm of social inquiry, research that fulfills it, although less hampered by one sort of obstacle to credibility, may still fall short of the truth or be outweighed by research conducted in accord with non-PAR approaches.

5.3

My proposal, then, is that by combining the appeal to democratic participation with that to epistemic privilege, we can arrive, by two different paths, at a more modest and a more tenable sense of the latter. These reconstructions of epistemic privilege, appealing as they do to principles of cognitive fairness/democratic inclusion and democratic or emancipatory advantage, both place epistemic restraints on social research that are designed to increase its credibility. The first insists that such research has a special responsibility to ensure that it hears from and takes full account of those silenced or at the margins. The latter requires that researchers not conflate reports from the nonemancipated with reports from persons capable of independent inquiry and that they treat the latter as more credible than the former. Both principles, despite their contrasts, are alike in providing responses to the two criticisms of PAR raised previously. The first maintains that good scientific practice, being subject to a principle of fairness or democratic inclusion, must be connected to good politics. And the second alleges that the practical (or social change) initiatives of PAR are important for the cultivation of social research competency in that they foster the recognition, questioning, and overcoming of widespread sources of unconscious bias.

These principles, in short, suggest that a case can be made for PAR and perhaps even that such research, under many conditions, needs to be included for social inquiry to avoid incompleteness or distortion. But they do not warrant ascribing to PAR any sort of most favored status or exclusive access to social knowledge nor do they imply that other social research models can be discounted or invariably carry less weight. In sum, my response to the title question of this article, "Should democratic norms guide social research or inquiry?" is twofold:

- If by "democratic norms," one refers to the two modest democratic reconstructions of epistemic privilege advanced above, the answer I believe is yes.

- If by “democratic norms,” one means the stronger (or immodest) claim that PAR alone fulfills those two principles or is otherwise possessed of exclusive epistemic privilege or most favored status, my answer is no.

6.

The rationale for these responses will become clearer by considering the second philosophic question concerning PAR: the relationship between it and liberal or nonparticipatory social science. Are these exclusive and clashing alternatives, informed by incompatible norms between which researchers are forced to choose? At certain points in his final chapter, this seems to be Root’s (1993) position; for example, he argues that liberal social science supports the rule of experts, placing limits on popular authority, thereby directly conflicting with the “democratic ideals” of participatory research. On the other hand, my moderated or tempered account of PAR’s claims to epistemic privilege allows, and may even require, it to collaborate with other, more mainstream and nonparticipatory social research. Here I agree with the conclusion drawn by Petras and Porpora (1993) from their instructive analysis of different forms of participatory research:

The point is not to abandon nonparticipatory or less participatory kinds of research, but to begin doing more research that is more participatory. . . . *Even nonparticipatory research, whether it contributes methods, theory or substance, can inform research that is highly participatory* [italics added]. (P. 124)

To illustrate and support this more collaborative perspective, let me describe some potential social science research—done from minimally (or non-) participatory frameworks—that I would like to see carried out, research that would fill both a practical and theoretical gap while “informing research that is highly participatory.”

At present, considerable research of two different sorts has been accumulated concerning what have been called “working models” of participatory democracy. The first concentrates mainly on large-scale, economy-centered innovations, such as Mondragon or Emilia-Romagna’s “flexible manufacturing network” (FMN) system, that have proven themselves to be productive, competitive, long lasting—with little or no compromise of democratic organization or principles. Among the best of these studies are Whyte and Whyte (1991) on the Mondragon phenomenon, Piore and Sabel (1990) on the northern Ital-

ian FMN economy, and a recent full-length study by Isaac and colleagues (1999) of Kerala Dinesh Beedi, an industrial co-op in southern India with more than 30,000 members.

The second strand appears to have little interest in social entrepreneurship or democratically controlled workplaces. It focuses, following the work of many feminist researchers, on what factors—or, better, what (cultural, educative, etc.) environments—enable those on the margins of society, those with negligible power or diminished status, to “come to voice,” to develop into self-respecting and self-directing community participants. One of the best of these works, Belenky, Bond, and Weinstock’s (1997) *A Tradition that Has No Name*, presents historical and qualitative evidence for the hypothesis that what the authors call “public homespaces”—which provide both inclusive and empowering forms of leadership and a safe refuge from outside and disempowering influences—are an essential ingredient in this process. They cite numerous historians and social researchers who, using different sources of data, arrive at much the same hypothesis.

What in my judgment is now urgently needed (from both a theoretical and a practical point of view) is to connect these two strands of research and of prefigurative democratic activity; the goal would be to find out, for example, whether Mondragon (or Mondragon replications) would become even more successful and more democratic if they incorporated aspects of the “tradition that has no name” and whether the latter would be sustained or enhanced by ideas, innovations, and priorities central to Mondragon or similar initiatives. Now neither Mondragon nor the folks studied by Belenky, Bond, and Weinstock (1997) (largely in the black South) have the time or resources to carry out this sort of bridge-building, cross-border research. It must, therefore, be designed and carried out by academics, of course with the permission and informed consent (and input) of those they are researching for, although not necessarily with. Such research, although done mainly from a conventional, nonparticipatory framework, could nonetheless yield results that could strengthen or refine PAR inquiries while having the potential to contribute mightily to the practical prospects of participatory democracy.

Or consider the homeless, or roofless, women of Boston, to come back to a real-life case. If social researchers share the aim of putting an end to or drastically reducing homelessness, can they rely exclusively on participatory research findings? I do not think so. For one thing, that project did not examine the influence on available and low-cost housing of macro- and meso-level policies concerning “urban devel-

opment," redlining, transportation and highway construction, the availability (or lack of it) of "living wage" jobs and of what is needed to secure them, and so forth. Nor, I think, is PAR the optimum approach for establishing the influence of such factors.

Second, do not we need to inquire about what other communities in similar situations have done that in fact reduced or prevented homelessness? What actually works? Not far (less than 5 miles) from the main site of this instance of participatory research is Boston's Dudley Street Neighborhood Initiative, which, through innovative community organizing and land ownership strategies, has over the past two decades virtually eliminated local homelessness (see here Medoff and Sklar 1994). But this (highly participatory) initiative is never mentioned in the R-WARM study, nor are any other locally designed and community-controlled approaches to land and home ownership (e.g., community land trusts). By remaining pure and participatory, this instance of PAR seems to have closed its collective mind to what may well be important pieces of the full story about homelessness and how to reduce it. A collaborative approach using what can be learned from both PAR and non-PAR research about other communities faced with similar problems would seem to make much more sense. Moreover, such an approach has been clearly articulated and used by Elden and Levin (1991).

6.1

Not only is it possible and sensible for PAR to collaborate with or draw upon nonparticipatory social research, but without the latter, the prospects for PAR might well remain quite meager. Certain sorts of nonparticipatory research seem essential for—and not just compatible with—PAR.

Let me return to that key tenet of the PAR community, which Root (1993) states explicitly in this way: "The democratic ideals of PAR . . . encourage citizens to participate in areas of public life previously reserved for experts" (p. 249). But what if (most) citizens are unwilling or unable to engage in such participation? This is the deeply troubling message of those controversial Milgram (1974) experiments, which apparently uncovered a surprisingly widespread acceptance of expert authority—to the point that most subjects obeyed orders from a white-coated scientist even at what they believed to be the cost of a human life and of their own moral convictions.

If this message is credible, exclusive PAR advocates seem faced with a paradox. If Milgram's (1974) research is sound, most ordinary people, at this point in time, would not want to engage in PAR or be capable of doing so. They would rather, and happily, turn over public life and social inquiry to academic professionals or "experts." The way out of this paradox, if there is one, can come (it seems to me) only from "liberal" social science—designed and conducted by professional researchers—into the factors and environments (educational, economic, cultural, familial, etc.) that would enable people to break free of their (currently slavish) dependence on expert authority, to reverse the damage done to their capacity to be critical of and stand up against those who misuse that authority. Fortunately, such research does exist. To take just one example, it is precisely what animates what child and social psychologist Urie Brofenbrenner (1972) called his "strategy" of "transformative experiments": these are (usually nonparticipative) research projects "that call into question or actively alter practices or beliefs that are part of the prevailing macrosystem in which the research subjects live." Their aim, again according to Brofenbrenner, is twofold: to understand the nature, strengths, and weaknesses of existing structures of socialization and (far more important, he says) "to modify these practices in ways that will enhance developmental processes" (p. 211). (There is a substantial history of such nonparticipatory but participation-enhancing "action research" in the social sciences, some of it arising from studies, like those I have cited above, of working models of participatory democracy. For a useful account of certain parts of this intellectual history, see Greenwood and Levin 1998.)

In short, then, given something like the Milgram (1974) results (or, if you like, given "the prevailing macrosystem of [obedience-to-authority] socialization") as a starting point to be overcome, not only are certain forms of nonparticipatory social science compatible with the PAR framework, but they are indispensable for its application. For only through inquiries based on and applications of those potentially liberatory forms of expert-driven social science can we discover how to promote participatory research and its democratic ideals.

7.

Let me conclude by noting, as the opening quote from Harding (1998) is designed to indicate, that the idea of linking democratic

norms with scientific inquiry is hardly my own invention. In recommending my two democratic norms, I have been treading on ground that has been mapped and cultivated with great care and ingenuity over the past two decades by numerous “post-positivist” feminist and standpoint theorists. (Wylie 2000 provides a lucid account of this research and its varieties.) Helen Longino (1990), for example, argues that a theory

which is the product of the most inclusive scientific community is better, other things being equal, than that which is the product of the most exclusive. It is better not as measured against some independently accessible reality but *better as measured against the cognitive needs of a genuinely democratic community* [italics added]. (P. 214)

And according to Harding (1996), “one strain of feminist epistemology” contends that

socially advantaged people and their institutions are, in fact, *epistemically disadvantaged*, and that *social disadvantage* creates a certain kind of limited but important *epistemic advantage*. (P. 146)

My own contribution to these lively and productive discussions, I believe, is twofold. First, my democratic norms are intentionally specific to the human or social sciences; that is, they are designed to help distinguish this sort of inquiry from what goes on within physics, chemistry, geology, and so forth. So far as I can tell, most feminist and standpoint perspectives appear to view notions such as objectivity, value neutrality, epistemic privilege, evidence, and the like as having much the same meaning throughout all fields of inquiry. Thus, for example, what Longino (1990) proposes as a democratic standard for science seems to be (roughly) “the widest inclusion of divergent views, methods, etc.” But this is clearly a standard that applies as well to experimental physics or molecular biology as it does to narrative-based history or participatory action anthropology. And while I am certainly sympathetic to democratic regulative principles of this sort, they will not by themselves throw much light on objectivity, evidence, and so forth within the human sciences. My two democratic norms represent an initial effort to close this gap.

In other words, I am here arguing, as I did some 30 years ago, for what I then called *methodological separatism*: the view that inquiries into human activities and social relationships require (at least some) different methods or approaches from those that operate within the

natural or physical sciences. The case I built for this sort of separatism is advanced in the introductions to chapters IV through VIII of my anthology, *The Nature and Scope of Social Science* (Krimerman 1969), and in my paper, "Autonomy: A New Paradigm for Social Research" (Krimerman 1972).

The second contribution is likely to be seen as more controversial. It concerns the notion of value neutrality; this is a notion that has been roundly repudiated by the feminist and standpoint communities. There are some excellent reasons for this repudiation, many of which are advanced in illuminating detail in chapters 1 and 4 through 9 of the book by Root (1993) on which this article draws heavily. Nonetheless, I think that value neutrality has been given a bad rap and that it has some redeeming features; in any case, my two norms of social inquiry do not require or imply that it be scrapped altogether. Indeed, the "collaborative position" sketched in Sections 6 and 6.1 can be understood as combining both partisanship (e.g., in PAR) and value neutrality (e.g., in non-PAR research into issues and communities similar to those on which PAR focuses). Yes, this may seem contradictory, but consider the somewhat analogous cases of a dissertation defense, a criminal trial, or most situations involving conflict resolution. Advocacy in these contexts is or can be combined with judgments from parties that (ideally at least) have little or no stake in the outcome. In these contexts—and, I submit, in social research as well—neither partisanship nor impartiality by themselves can be relied on as much as both together. If these analogies hold up, there may still be a place for a reconstructed notion of value neutrality within scientific inquiry.

Moreover, it seems to me that feminist and standpoint theorists could consistently accept this walking-on-two-legs approach, combining partisanship and impartiality, even though in general they have tended to agree with Longino's (1990) rather uncompromisingly one-legged position:

The idea of a value-free science presupposes that the object of inquiry is given in and by nature, whereas the contextual analysis shows that such objects are constituted in part by social needs and interests. . . .
From this [contextual] perspective the idea of a value-free science is not just empty but pernicious [italics added]. (P. 191)

Finally, it might legitimately be asked what dire consequences, if any, flow from the outright rejection of my two democratic norms of social inquiry.² To do so is not obviously self-contradictory, incoher-

ent, or otherwise irrational. There are, after all, many prominent examples of nondemocratic social research; indeed, it is a bit difficult to find examples of social science that comply very fully with these two proposed norms. Have I, then, violated Geertz's (1973) advice?

Well, maybe. But my contentions in this article are not meant primarily as a descriptive account of the whole panorama of social science practice but as a proposal to shift that practice toward more democratic arrangements and relationships. (Recall here Petras and Porpora's 1993 conclusion, quoted in Section 6.) I would support this proposal by claiming that the cost of neglecting or violating my two democratic norms is the construction of a myopic and distorted picture of human action and social phenomena. One can, of course, conduct social research non- or undemocratically, but the price is a very high one: the disappearance of much that is unique and valuable in our subjects of inquiry. If this claim is correct—I have argued for it elsewhere (Krimmerman 1969)—then it is irrational to do social research without observing democratic norms of inquiry.³

NOTES

1. This was a phrase she introduced, in a different but closely related context, into discussion at the Philosophy of Social Science Roundtable Conference in April 2000.

2. A similar question was posed to me by Michael Root, in private correspondence.

3. Additional evidence for this claim can be obtained by examining the gaps left by social research where these norms are not followed. For example, Greenwood and Levin (1998), following other action researchers, question Milgram's (1974) results because he failed to intervene in ways that would challenge his subjects' uncritically subservient responses:

This lost him the possibility of understanding the genesis of the observed behavior . . . *his experiments could not yield knowledge that might help individuals break out of this dilemma* [italics added]. (Greenwood and Levin 1998, 194)

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Leonard Krimerman teaches about anarchism, participatory democracy, and the philosophy of education, as well as the philosophy of social science at the University of Connecticut in Storrs, Connecticut, USA. He edited The Nature and Scope of Social Science (1969), as well as coediting, with Frank Lindenfeld, When Workers Decide (1991) and From the Ground Up (1992), and is a founder and editorial coordinator of the Grass-roots Economic Organizing Newsletter.