Action research

Unfulfilled promises and unmet challenges

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This article examines how and why the academically-based social sciences, both pure and applied, have lost their relevance to practical human affairs (praxis) and links this discussion to the reasons why action research is a marginal activity in the academic and policy worlds. It also contains a harsh critique of action research practice focused on action researchers' combined sense of moral superiority over conventional researchers and general complacency about fundamental issues of theory, method, and validity. The central argument is that "doing good" is not the same as "doing good social research" and that we action researchers need to hold ourselves accountable to higher standards, not only to compete with conventional social research but for the benefit of the non-academic stakeholders in action research projects.

Keywords: Action research, phronesis, positivism, universities

How and why the academically-based social sciences, both pure and applied, have lost their relevance to practical human affairs (*praxis*) and how and why action research, a practice designed to overcome this dilemma is a marginal and largely defensive activity, carried out only in pockets within the academic and policy environment, form the central issues of this essay. Stated differently, I am examining how and why the academic social sciences reconfigured their relationship to other disciplines and their surrounding society. Why did they become primarily internally regulated, university-based, professional activities? Why did action research, despite the last forty years of comprehensive and intellectually successful attacks on positivism that cleared the path, do little more than lament its marginality while adopting a stance of moral superiority? This is an essay about the failures of conventional researchers and the applied researchers that follow them — and of action researchers, too — to live up to

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reasonable intellectual and ethical standards.

Necessarily this analysis begins with my reasons for an apparently unceremonious dismissal of the conventional social sciences. I will not spend much time on this, as I have written a good deal about it already (Greenwood and Levin 1998a, 1998b). I do not know how many times the intellectual incoherence and bankruptcy of conventional social science must be asserted by philosophers and social theorists (Fuller 2000; Rabinow 1984; Bourdieu 1997; Giddens 1973) before those conventional social science practitioners, who claim to be more than fee-for-service researchers in positivist body shops, simply accept their failure and abandon the field.

Anyone who has paid attention to the critiques of positivism over the past forty years knows that the underlying assumptions of positivism are indefensible: objectivity, controls, rational choice, etc. — all of these pillars of positivism have been taken down. Yet everyone also knows that positivist researchers continue to dominate the conventional social sciences at most universities and in the funding priorities of most public agencies and private foundations. While there is some weakening of this hegemonic position in the United States and in parts of Europe, resulting in increasing difficulties in funding for the positivists, these monies are not therefore flowing to alternative forms of social science. They are simply disappearing into the great maw of scientific and technological development or they are being captured by highly priced private consulting companies that promise better social science results than the academy produces.

The persistence of positivist social science is mainly a product of its institutional posture as a self-referential, socially passive activity in universities and its conception of the professional social researcher as an "advisor" to power. To the extent positivist social scientists study anything of interest to anyone other than themselves, they study those issues that power holders are interested in — e.g. does the policy of welfare-to-work function properly, is HIV infection among adolescents decreasing or increasing, at what rate is socioeconomic inequality increasing, etc. Of course there are some exceptions, such as the work of the Frankfurt School in Germany, the industrial democracy movements in Norway and Sweden, the qualitative inquiry movement in the United States, the Bourdieu school in France, and various action research networks around the world. As important as these are, they are certainly neither mainstream nor hegemonic.

To be blunt, it is clear that the conventional social sciences are not driven by the intensity or importance of social problems. They are driven by external funding and oriented around professional communities of social researchers

commenting on each other's work, answering commentaries, and commenting again. This activity occurs in a safely hermetic space that academic social scientists create. They privilege "theory" and "method" over all else, though what theory and method mean in this context is quite out of step with the meanings of these terms in the physical and natural sciences (see Gary Thomas 1997).

Morten Levin and I have written a good deal about this "autopoetic" academic social science (Greenwood and Levin 2000a, 2000b, 2001a, 2001b). Despite this social distance and alienation, the conventional social science disciplines claim to have noble social purposes. For example, the code of ethics of the American Sociological Association includes the following paragraph:

> Principle E: Social Responsibility: Sociologists are aware of their professional and scientific responsibility to the communities and societies in which they live and work. They apply and make public their knowledge in order to contribute to the public good. When undertaking research, they strive to advance the science of sociology and to serve the public good.
> (http://www.asanet.org/members/ecointro.html#preamble)

The American Anthropological Association Code of Ethics states:

> Anthropological researchers, teachers and practitioners are members of many different communities, each with its own moral rules or codes of ethics. Anthropologists have moral obligations as members of other groups, such as the family, religion, and community, as well as the profession. They also have obligations to the scholarly discipline, to the wider society and culture, and to the human species, other species, and the environment. Furthermore, field-workers may develop close relationships with persons or animals with whom they work, generating an additional level of ethical considerations.
> (http://www.aaanet.org/committees/ethics/ethcode.htm)

The American Economics Association is more honest about its professional goals:

Mission Statement

1. The encouragement of economic research, especially the historical and statistical study of the actual conditions of industrial life.
2. The issue of **publications** on economic subjects.
3. The encouragement of perfect freedom of economic discussion, including an **Annual Meeting**. *The Association as such will take no partisan attitude, nor will it commit its members to any position on practical economic questions.* (http://www.vanderbilt.edu/AEA/org.htm) (emphasis added)

No matter what the major professional associations say about their laudable social goals, they exist mainly for the purpose of supporting licensed professionals in these fields and the bulk of their activities have to do with the circulation of professional knowledge within the associations, the defense of their territory, and management of the professional job market. The founders of the social sciences (both liberal and conservative, such as Adam Smith, David Ricardo, Karl Marx) would be stunned to see the narrow and self-serving professionalism of these disciplinary groups and their lack of engagement with the social problems they were founded to ameliorate,

These social scientists claim a critical orientation; they criticize social arrangements on paper or in public presentations, but they either do not know how or do not dare to intervene socially in the situations they describe. While there are the exceptions of public intellectuals like John Dewey, Michel Foucault, Manuel Castels, and Pierre Bourdieu, they are notable precisely because they are so exceptional in their activist focus on problems like inequality, class conflict, war and peace, and racism.

Of course, there are also a large number of applied social scientists whose work does reach the public directly. Probably half the anthropologists in the United States do not work in the academy but in social service agencies, for the World Bank, the International Monetary Fund, the Environmental Protection Agency, and many NGOs. These applied social scientists often do both a great deal of good and face quite complex intellectual and practical challenges. Many applied social scientists make a decent living as freelance consultants also. Oddly, most of this applied work is based both on an unquestioning acceptance of the separation between theory and application and between theory and practice. It also enshrines the "expert" model where the researcher is the expert and does things "to" the stakeholders, not "with" them. To me, the most notable feature of this group is that these applied social scientists are viewed as distinctly inferior to their academic betters in the professional status hierarchy (Hill and Baba, eds. 1999) and many of them accept this worldview, despite its negative impact on their professional prestige and self-image.

Two features of the conventional social sciences, both pure and applied, strike me. First, regardless of the theoretical critiques that have been voiced, conventional social scientists neither acknowledge the contextual nature of their knowledge nor change their intellectual behavior. There is a split discourse between acknowledgement in principle of anti-positivist critiques and their ongoing practices that routinely ignore the core of the critique. They also have achieved a remarkable level of social disengagement while being well paid for

their time. The illusory attempt to develop theory without practice and methods without policy meaning or application is their core practice.

Among other things, this set of circumstances results in a hallucinatory discourse about "pure" or "high" theory versus application. The very fact of application becomes the road to demotion of status, and any misguided young social scientist who wants to apply social science will have to find professional satisfaction in being "useful" rather than in being intellectually "important" or academically successful. So the conventional social sciences that began as social analyses for the purpose of social reform have developed into autopoetic professional associations engaged in intellectual commodity production and career development.

This has partly been a matter of preference of social scientists for a comfortable, campus-bound university life. Social engagement from a campus office or university library study is generally not feasible. And social engagement means having one's time placed at the disposal of extramural stakeholders who are engaged in social processes that do not occur in synchrony with the academic calendar. But this disjuncture too is the long-term outcome of the confrontations between reform-oriented political economists and other early generation social scientists with the raw political and economic power of the ruling class. Critical histories of the social sciences make it abundantly clear that the social sciences have been "domesticated" through purges (Ross 1991; Furner 1973; Lengermann and Niebrugge-Brantley 1997; Fuller 2000). A typical example is Richard T. Ely, who taught political economy at Johns Hopkins in the 1870s. He was a socialist and attempted to organize the American Economics Association to support his goals. He had many supporters but others also felt that showing the public that economists were not all in agreement was bad for future development of the profession. Ely moved to Cornell where he ended up offending one of Cornell's rich benefactors and was forced to leave. Subsequently he had to subject his work to censorship by his colleagues in order to stay out of trouble (Ross 1991: 113–118). There are hundreds of well documented cases, both before and after the development of academic tenure in the United States. Social scientists advocating social reform in directions that offended the rich and powerful were routinely fired from academic jobs.

Later, in the United States, Senator Joseph McCarthy attacked the social sciences head on and drove many reformers out of the academy, even causing some to leave the country. One of the celebrated cases was that of the historian and anthropologist M. I. Finley who was accused by McCarthy of organizing a Communist study group while a graduate student. While on the faculty at

Rutgers University, Finley was attacked by McCarthy and the McCarran Committee that McCarthy masterminded. Rutgers summarily fired him by arguing that they were dependent on public funding and could not afford to offend what they called "public opinion" (Chomsky *et al.* 1996, 41). The Vietnam era saw a series of purges regarding social science work in Chile and in Southeast Asia. The purges this time were able to count on the direct complicity of the professional social science associations. These actions imperiled or ruined the careers of a number of social scientists engaged in reform work. Of course, as one would expect, some of the cases showed that, indeed, a number of people hiding under the mantle of the social sciences were engaged in counter-insurgency and espionage work, though this work for anti-democratic causes was not criticized by the government.

It is fair to say that, by the late 1950s, the balance in most of the social sciences had been tipped to favor social scientists who used numbers to sanitize the social situations they studied, whose theories were not applied (or applicable), whose elegant theoretical models were written in a language that none but the initiated could understand, and who, most of all, minded their own business. Social engagement came to be seen as atheoretical, methodologically suspect, and professionally dangerous. To cover their retreat, the conventional social scientists made a desperate and losing bet: they attempted to create a "social science" that could be practiced theoretically without application except to contained and professionally-agreed upon data sets.

From almost any point of view this was a hopeless strategy for a research activity that aimed to discover how any part of society works. It is also a parody of the scientific method's requirement for a repeated dialogical relationship between theoretical formulation, methodological operationalization, practice, and reformulation (as can be seen in experiments and other direct engagements with the phenomena in question). Social theory without action does not exist; what is called "theory" in that context is mere speculation. The conventional social sciences have become socially-disconnected, speculative endeavors, with a putative knowledge that does not meet any serious test of application. Yet these same autopoetic professions continue to justify their existence in terms of the value of their knowledge for society at large.

That this system has come into existence and come to dominate the academy should tell us something quite important about the complicity of universities with the designs of the powerful and about the ongoing contribution universities are making to neo-liberal and globalization agendas. Faced with such criticisms, the conventional social scientists generally retort that to

engage in application and in reform would compromise their objectivity, their theoretical controls, and thereby contaminate their results. The only concession they would be willing to make is that, as one colleague of mine recently said, if there is money for people to take these theories and methods out into the world and apply them, then it is acceptable to take that money, so long as one realizes that this an inferior and uninteresting form of activity (personal communication).

This is the main division of labor that the conventional social sciences have now created. There are the "pure" social scientists who build theories, run them against data sets, and publish for and about each other and there are the "applied" social scientists who take these theories and methods out in the public arena and apply them. There is no thought given to the mutually-necessary dialogue between theory and application or between theoretical development and practice capable of disconfirming or altering theorization.

Of course, almost anyone who thinks about it realizes that the very data sets the conventional social researchers use are elaborate social constructions that are socially situated, include and exclude certain types of data, and portray the world in very particular and structured ways (Porter 1995; Bowker and Star 2000). The notion that handling such data sets is the highroad to objectivity is a patent absurdity.

Dialogue between theory and practice

To put the argument more starkly, the division of labor that separates theory from practice and claims that doing so, against all logic, is essential to being scientific — which makes no epistemological or methodological sense — is better understood as the application of "scientific management" *à la* Frederick Winslow Taylor to social research. It is based on a Tayloristic view of inquiry that relies on professional autonomy, the hierarchical separation of tasks, and the integration of the tasks into a whole by the manager/engineer who knows best and who is the only one smart enough and well-trained enough to know what is to be done. Like Taylorism, it is also an elitist social view because it discards the notion that ordinary people in the course of their daily lives could have any role in theory or even knowledge development. They simply have to wait until the professional social scientists bestow their wisdom on them or on the designers of social programs (*e.g.* trickle-down development, structural adjustment, social capital) deploy the theories for the good of the "people".

The applied researchers, too, often depressed by the low prestige of their activities, attempt to maintain their self-respect by claiming to do "good works" even while acknowledging that they are theoretically trivial. This position is complicit with the claims of the "pure" researchers to the professional high ground. Many applied researchers accept the radical distinction between "pure" and "applied" research and thus accept the possibility of separating theory and action, theory and reflection in the research process. In this way, applied and pure researchers have worked out a division of labor that permits both sides to do as they please while each also feels superior to the other, one because they don't apply theory and the other because they do.

Applied researchers are often well financed in what is a highly entrepreneurial field. They invest a good deal of time reviewing the latest "requests for proposals" from the World Bank, USAID, Department of Health and Human Services, United States Department of Agriculture, the European Union, major foundations, etc., trying to find ways to match their skills to the applied research money to be had for the "proposing". They are rarely in a position to set the agenda for the work and so they must learn to be highly responsive. So long as they don't mind having low intellectual status in the eyes of academics (and they often don't mind), they can do quite well. A review of any of the applied journals such as *Human Organization* or *Practicing Anthropology* shows the wide variety of employment opportunities and career paths in the applied field, just in anthropology. Of course, this is also true in sociology, economics, psychology, and, though to a lesser extent, in political science.

The persistence of a radical distinction between theory construction and application is a phenomenon that cries out for explanation, given its epistemological incoherence, its social disfunctionality, academic institutional consequences, and social impact. But one looks in vain for ambitious examinations of these issues.

Action research

Given this situation, which appears to be beneficial to social researchers but not to the public, it is not surprising that alternative approaches to conducting social inquiry continue to exist, albeit modestly, despite the weight of institutional coercion that is exerted against them. Action research is the most general rubric one might use for these approaches. After briefly outlining my views on what action research is and why I believe it has great untapped potential, I go

on to explain how and why action research has been complicit with conventional social research by generally failing to live up to action research's own avowed expectations. I also outline briefly what I think we might do about this. The alternative to well-practiced action research certainly is not positivistic business as usual.

In a few places (Norway and Sweden, and in small pockets in England, the Netherlands, Germany, Canada, the United States, Australia, in many areas of Latin America, and in a few locations in Africa), there are loosely-linked groups of people who practice what they variously call action research, participatory action research, participatory research, collaborative inquiry, human inquiry, action science, adult education, and a few other names. They continue to work to articulate an alternative approach to research in which the spurious dichotomy between theory and practice is mediated, in which multi-disciplinary and multi-stakeholder teams are central, and in which objectivity is replaced by a public commitment to achieving liberating, sustainable, and democratizing outcomes.

Every group of practitioners writes a somewhat different history of these activities. Some emphasize Kurt Lewin and social psychology in the 1930s and 1940s as the starting point. Others point to the "human relations" movement in labor studies, while others point to the U. S. civil rights movement and peaceful mobilization. The role of Latin American intellectuals and reformers like Paolo Freire and Orlando Fals Borda and charismatic reformers like Gandhi looms large in the minds of practitioners of Southern participatory action research. For others, the role of the Tavistock Institute and the therapeutic turn in social change work is central. In Scandinavia, Tavistock and the Scandinavian industrial democracy movement played a collaborative role. In Australia, primary and secondary education was the initial focus of effort, and so on. The upshot is that there is not one story; there are a variety of partially convergent stories (that also reveal important differences in approach and reform commitments). Both the convergences and differences are important but in such a brief essay as this I can do no more than allude to their existence.

Reflection/action

Among the convergences are the repudiation of the theory/practice split. No one in action research accepts this division. Everyone believes that the only meaningful way to theorize is through successive cycles of combined reflection and action, the action feeding back to revise the reflection in ongoing cycles.

This means that action research depends on a major revision in the epistemological and methodological standards that the social sciences use. Returning to Aristotle through the good offices of Stephen Toulmin and Björn Gustavsen (1996) and Bent Flyvbjerg (2001), action research is based on revitalizing the Aristotelian distinctions between *theoria*, *techne*, and *phronesis* as forms of knowledge. Aristotelian *phronesis* strikes action researchers as the pre-eminent form of knowledge that the social sciences must employ, because of their social mission and their complex inter-subjective data sources. This is knowledge forged in endless cycles of reflection and action, the most proximate model being the kind of clinical knowledge excellent health care practitioners develop as they show the ability to interact with patients, to link these to formal knowledge systems, and to contextualize in the specific case within their more general experience base, all while attempting to bring about a positive outcome for specific individual patients. Precisely because social science knowledge is not knowledge of objects, but of co-subjects, the mutual and reciprocal reflections generated in human action must necessarily lie at the center of social science, positivist fallacies and objectivist yearnings notwithstanding.

The power of the dialogical relationship between theory and action is one of the most exciting experiences that action researchers have. Rather than stating this as a principle, I will give an example. In my first major action research project in the labor-managed industrial cooperatives of Mondragón (Greenwood, González Santos, *et al.* 1992), I was searching for materials in the literature on industrial organizations that would help the collaborative group conceptualize their problems more clearly. In the process, we all found Chris Argyris and Donald Schön's concepts of organizational learning very useful (Argyris and Schön 1996, although we were using the earlier edition of that work at the time: Argyris and Schön 1978) and I also found myself trying to find ways to bring Donald Schön's notions about "reflective practice" to bear usefully on our deliberations. However, I noticed in both cases that our experiences in the cooperatives, though illuminated by these formulations, also showed these approaches to be lacking in certain ways. The collaborative character of the research process and the management of the cooperatives revealed an apolitical and dyadic bias in these theorizations that needed correction. In the case of the Argyris and Schön book, I was able to articulate this point in a review of the work that suggested the need for an even more "social" and "political" conceptualization of organizations (Greenwood 1997). In the case of reflective practice, Donald Schön invited me to contribute an essay to his volume *The Reflective Turn* (Schön ed., 1991) and there I found

myself struggling against the homogenizing bias of most of the then popular writing on organizational culture. Using his notion of reflective practice but pointing out that, in the cooperatives and, by extension, in all organizations, reflective practice is largely a collective, noisy, and conflict-filled process, particularly if it is about issues that matter to the participants (Greenwood 1991). Thus, the process of using their frameworks both helped us in the action research process and led back to theoretical critiques and revisions of these very important frameworks.

Local knowledge

Action researchers do not believe that even the best trained social scientist can aspire to knowing the lives and conditions of social stakeholders as well as they know their own situations. Thus action research is not about imposing expert knowledge on stakeholders but about creating collaborative environments where research experts and local stakeholders can share their very different kinds of knowledge in the process of analyzing their problems, studying them, and collaboratively designing actions that can ameliorate the problems.

Multi-disciplinary "messes"

Borrowing the term "messes" from Russell Ackoff (Ackoff 1999), one of the key features of action research is that we all study extremely complex, dynamic, and difficult problems. The world does not deliver social problems in neat disciplinary packages, despite the pathetic insistence of most academic social scientists in defending their academic turfs against all other forms of knowledge. Thus action research is not only insider/outsider, multi-party work; it also requires the mobilization of research expertise from any and all academic and research locations relevant to the problem at hand — engineering, basic science, ethics, education, etc.

Quantitative/qualitative methods

Nor does action research respect methodological territories. Any social science research methods can be relevant to an action research project, insofar as they have something specific to contribute. Thus a micro-economic approach, a cost-benefit analysis, a hydrological survey, a questionnaire, a focus group, or a search conference are all equally suitable methods, once the collaborating

parties have settled on a problem to address. The notion that action research must be qualitative is an absurd result of the hegemonic claim of the positivists to quantitative methods. In their totemic logic, since action researchers are not positivists, we therefore must believe in soft, fuzzy, and thus qualitative data. Never mind that a Norwegian action research project on safety procedures on an oil platform is guided by the tight tolerances that life in the North Sea within a few meters of a devastating gas explosion or oil fire have a higher stake in "getting it right" than any armchair social scientist ever could.

Intellectual property

In action research, the intellectual property created is a collaborative product and thus belongs to all the participants. Action researchers oppose the extractive cartel behavior of the academic social sciences because we find this extractive approach to other human beings a form of class-based exploitation and a false representation of the various sources of social knowledge. This is not to say that action researchers do not write for each other occasionally (*e.g.* Peter Reason and Hilary Bradbury, eds., 2001) but that this writing for the action research community is only one of the kinds of writing we do. We also write both for and with our collaborators (depending on the situation, available time, levels of literacy, etc. see Greenwood, González, *et al.* 1992; Grudens-Schuck 2000, as examples).

Liberating intent

Action research exists to promote liberating social change. While different strands of action research differ greatly in what they understand as liberating, the value commitment is an essential part of this work. This is not produced by the ethics code or a professional association but by the very nature of the collaborative work we engage in. This is truly a "social" science because it begins and ends in socially meaningful, reciprocal relationships.

Why has action research not taken over the social sciences?

From what I have written, it seems that action research should dominate the social sciences. It has methods that are far more "scientific" in the sense of knowledge tested and refined in action. It mobilizes relevant knowledge from

people in a position to know their conditions far better than conventional research can with its extractive approach. It puts more hands and minds to the research job. And it is driven by strongly-held democratic values. It would seem that it should simply overwhelm the other forms of social research in the "free market" of ideas and methods. But, of course, it does not. Indeed, most people get a lengthy education in the social sciences without ever having heard of action research. Most policy makers find that action research, despite the recently greater diffusion of the term, sounds oddly "mushy" and even risky.

What accounts for this situation? I think there are two answers: suppression by the social sciences and political elites and the sloppiness and negligence of action researchers themselves. Both business leaders and administrators of public funds who allocate research monies and contracts are often fearful of action research. They have had a couple of generations to get used to the positivist researchers who have learned not to make trouble and the paid consultants who rarely challenge power structures in organizations because they would be likely to lose their consulting contracts. To these leaders and funders, action research, by reaching deep into the organization and community to bring together stakeholders with diverse and even divergent interests and by promising liberating social change, unsettles local routines and structures while not providing the typical-sounding research outputs that are delivered solely into the hands of those in charge. The vast inequalities in most communities and organizations, the increasing levels of inequality and tension in most parts of the world, and the failure to connect to the vast stores of knowledge and experience that are broadly diffused across communities are brought to light in action research projects. Action research makes visible the waste of human capacities and organizational potential in conventional social science.

Suppression

By and large, action researchers feel oppressed in the academy and in public sector agencies where they are dominated by conventional social researchers who have captured the academic turf and by conservative policy makers who dislike the focus on democratic social change and the validation of knowledge claims coming from all levels of organizations and communities. This puts action researchers into a defensive posture and often results in a complaining, even whining discourse that convinces no one of the value of their approaches. The hegemony of positivist social science and the opposition to fundamental change by those in power forces action researchers constantly to respond to

hostile questions — "Prove how your work is better!" "Show us how it is better!" We have all experienced this situation, despite the indefensibility of most of the theories and methods that dominate positivism.

In response, action researchers often walk away, feeling superior to the positivists but without having answered the perfectly legitimate epistemological and methodological challenges they have issued. By doing this, many action researchers adopt precisely the same passive posture that the applied researchers do in the face of their purely academic "betters". The difference is that the action researchers claim to reject the positivist framework and do not have the luxury of being silent in the face of such positivist critiques.

Negligence

What has received less attention is the self-indulgent or even negligent behavior of many action researchers, behavior that contributes directly to their being ignored. Part of the reason action research has not gained a hearing is because it often has been sloppy, passive, inadequately reflective, and occasionally, flatly incompetent.

This is not a happy conclusion to reach and I do not articulate this critique lightly. I know I can be accused of providing arguments to the conventional social researchers and their funders that are not in the interest of action research. But it seems to me that we cannot ever hope to gain control of the agenda of the social sciences unless we begin holding ourselves accountable to meaningful standards of theorization, methodological sophistication, and clarity of writing.

When is action research really "research"?

One of the most unsettling features of much of the action research literature is the use of the term action research without attaching any serious meaning to the concept of research. I recognize that it is also the case that the term research is often used sloppily as a synonym for data gathering in the conventional social sciences. But in action research, the term "research" is part of the name of the activity and we cannot afford to use the term as part of an empty rhetorical gesture.

Going out to collaborate with a group of people in solving an important problem is not by itself tantamount to doing research. Collecting and analyzing data by itself is not research; it is just collecting and analyzing data. To my

mind, conducting research means developing habits of counterintuitive thinking, questioning definitions and premises, linking findings and process analyses to other cases, and attempting to subject favorite interpretations to harsh collaborative critiques. Throughout these processes the collaborative process of reflection is the guiding thread that integrates the work.

The vast bulk of conventional social science and applied social science fails to meet this standard. Much of it is not even Kuhn's "normal science"; it is just data collection and logical game playing, all conventionally called "research" and theory construction. There are two problems with this. First, the conventional social sciences currently occupy the high ground. If we want to occupy that ground, then we have to hold ourselves and them accountable to higher standards. Second, as I have already argued, action research, unlike conventional research, is so constructed that it has real potential to be research in a strong sense of that term. Through the collaborative process of question formulation, data collection, data analysis, and testing in action, action research is the social science approach that is most likely to produce something that could legitimately be called rigorous research. However, it cannot do so unless action researchers are competent in research methods and discourse and serious about increasing the research values and methods associated with their work. Data collection, program design, and doing good are not tantamount to doing research.

What evidence is there that action researchers are good at research? There is some, as in the massive intervention programs designed by Björn Gustavsen and his colleagues, particularly in the overall design of those programs (Gustavsen *et al.* 1996). But even there, underneath the larger structure there are many local projects that are little informed by a research mindset. Some of the Tavistock's early work clearly qualifies (Trist and Bamforth 1951), as do a number of the early ship-manning and oil platform projects in Norway (Emery and Thorsrud 1976). Morten Levin's essay on 'Trailing Research' (Levin, Finne, and Nilssen 1995) certainly qualifies as research, as do a number of the projects of Peter Reason and his colleagues at the University of Bath have developed in the study of the British social services system (Reason, ed. 2001).

Despite these good examples, when I read the bulk of the material from around the world that calls itself one kind of action research or another, I often bridle at the use of the term research to cover what are essentially applied programs, organizing efforts, or mere consulting. If we action researchers really want to gain the general academic and political respect we feel we deserve, we have to be better at research than our conventional colleagues. We have to hold

ourselves accountable to the highest standard we can manage and stop taking so much comfort in being on the side of the angels. We appear to demand reflective practice as a core element in action research but we do not appear to like reflecting very much on the weaknesses in our own practices.

Writing

One of the acknowledged banes of action research is the paucity of good action research publications. Indeed, the savage reviews of programs like Norway's famous Work Research Institute have focused on this Achilles' heel. So serious is this problem that some years ago a joint Swedish and Norwegian training program called the Scandinavian Action Research Program (ACRES) was created to deal with it. Some thirty middle-rank and senior action researchers from Norway, Sweden, Finland, Holland, Great Britain, and the United States met for two years at intervals under the supervision of an international staff of mentors (myself, Morten Levin, Hans van Beinum, Claude Faucheaux, René van der Vlist, and Kjell S. Johannessen). Our original idea was to deal with the writing problem by creating a supportive space for effective writing about the participants' action research projects.

This turned out to be a more complex proposition than any of us imagined. To begin with, we discovered that many of the participants needed to polish their research skills and much time was spent on that. However, we also found that very complex problems arose in attempts to write about action research projects. To begin with, action research is conducted in the first person plural ("we"), not in the third person singular ("it") language of positivistic academic research. Yet most of the participants identified the "it" language with the appropriate language of research. As a result, they tried to write about "we" experiences in "it" language and almost always became stymied and felt that the narratives they were producing were unreal and not true to their experiences. Thus we unintentionally discovered that writing about action research, as logic should have told us, is much more than mere writing. It is about constructing a language for reporting on collaborative knowledge creation activities in which the first person voice is primary and in which process is as central to the research story as are results. The positivist rhetoric does not work for action research.

In addition, these accounts include presentations of the complex and often imperfect processes at work in the projects, not merely a triumphal account of the successes. Action researchers, precisely because of the ongoing dialogue

between theorization, action, and re-theorization, cannot separate the research process from the findings in the dichotomous way that conventional researchers do. We thus face very hostile criticism in peer reviewed publications dominated by conventional researchers. We have written up some of our experiences with this process and problem (Greenwood, ed. 1999), but there is much more to be done.

Paradoxically, this very turn to the first person precisely matches the directions that feminist, postmodern, and positional writing have been suggesting as part of their critique of the hegemonic positivist doctrines (*e.g.* Behar 1993; Gupta and Ferguson, eds. 1997). In going in this direction, action researchers, if we have the courage to press on, are actually standing with those in the vanguard of the critique of social science business-as-usual in the academy.

Intellectual property

In addition to the rhetorical problems, there are many others. The issues of intellectual property are quite difficult. Who owns the results of an action research project? A collaborative project belongs to the collaborators. Yet there are many different kinds of products: internal reports, reports to a municipal government, reports to funders, articles, books, etc. Sometimes the professional researcher writes them all; occasionally they are collaboratively written, or at least some of them are. Whose they are becomes an issue and who has the right to edit and to veto elements is not easy to sort out.

This problem is made much worse by the extractive fictions of conventional social science. Using the monopoly model of social research and treating the co-subjects as alienated objects permits the fiction that the data and interpretations belong soleley to the professional social scientists. Further, even when papers are co-authored, our entire conception of authorship is based on a commodity production model of knowledge that treats individuals as solo inventors of the ideas and concepts that they actually have learned in multiple interactions with others and through broad reading. Yet we hand them copyrights and unproblematically assign authorship to them.

This is a crucial point. To change these practices would lead to a fundamental change in academic careers or career paths; the criteria for academic careers would have to be changed and this, in turn, means a fundamental change of the university as an institution. In this regard, action researchers are disappointingly silent. Only a few, like Peter Reason (Reason, ed. 1994) have dared challenge some of the rhetorical conventions of academic social research and have begun

to explore the ways writing must be consistent with the experience and structure of action research processes.

The protection of human subjects

Around the world, but especially in the highly litigious United States, the issue of protecting the "human subjects" of research is a major one for most researchers. All major research universities in the U. S. now have Human Subjects Committees that have to review and certify the sponsored social research projects that are undertaken by the faculty, staff, and students. These reviews include issues of informed consent, lying, physical and psychological abuse, etc.

Action research is the only modality of research that starts with the premise that social research is not a professional right of researchers but a duty and a process of taking on obligations for the welfare of the non-professional collaborators. The entire action research relationship is based on active co-determination and no action can be taken without agreement among the collaborators. It is hard to imagine a research process with greater human subjects protection.

So where is the action research writing on this subject? To my knowledge, there is none. Why? I speculate that a significant portion of the action research community has not given this weighty issue a second thought. Few see how pressing the arguments about human subjects could dramatically enhance the role of action research in most organizations. Thus, here again, I see few action researchers who are willing to enter into arguments at the centers of power where the rules are made, even when they have the better arguments at their disposal. I see this as self-willed marginality. If we do not change our behavior, we surely deserve this marginality.

Academic politics

One of the most distressing things I notice in many interactions with action researchers is their tendency to call conventional researchers and academics by the term "them" and to define themselves in opposition to "them". Many action researchers refuse to set foot in universities, in governmental or international agencies, and generally adopt the "moral high ground" on the grounds that "purity" demands staying away. This is a both a failed and foolish political strategy. Universities, governmental and international agencies have accumulated huge masses of resources, mainly at public expense. Most action researchers believe that these resources are largely either wasted or misapplied. Yet few are

willing stand within those institutions and fight for their place. There are exceptions like Morten Levin, Yvonna Lincoln, Peter Reason and others, but the general trend is toward a kind of acceptance of marginality and a corresponding lack of institutional ambition, activism, and standing. It is as if being marginal were a virtue, some kind of guarantee of moral superiority rather than what it is: an act of complicity with power. Unless action researchers challenge universities and public agencies to reallocate their resources in ways that serve "the people" who underwrite them, then we are as much to blame for the current situation as our supposed adversaries.

Universities

In a paper that is already long enough, a proper discussion of the topic of universities and how they could and should support action research would take us too far afield. Recognizing the centrality of universities as institutions for the development of action research, Morten Levin and I have published a number of papers on the co-optation of the university social sciences and on the reconstruction of universities by making action research central to their functioning (Greenwood and Levin 1998a, 2000a, 2000b, 2001a, 2001b). Now we are launching a larger-scale international and comparative project on the fate of the social sciences in universities worldwide, aimed at understanding what inhibits the changes.

For the purposes of this essay, I will just say that the team-based, multidisciplinary, insider-outsider character of action research makes it amenable to dealing with extremely complex and dynamic problems that have technical, historical, social, cultural, and political components. We also think that having research agendas set by the relevant stakeholders outside the university promises better relationships with the constituencies that support universities with public monies and with governmental and private funders. Thus we think that making action research central to the practice of social research at universities could do much to revitalize what are becoming increasingly corporate and sclerotic institutions.

Of course, for this to happen would require a willingness to struggle. We have to stop referring to universities and conventional social researchers as "them" and we must fight against our marginalization within universities, a marginalization that leaves the massive resources of one of society's most powerful institutions entirely in the hands of people whom we claim not to respect. We have to show that our research can deliver better outcomes than

theirs and that the impact, in terms of public support and institutional reputation, can be very beneficial to all concerned.

Macro-structural issues

With the exception again of some of the programs designed by Björn Gustavsen and his collaborators (Gustavsen, Hofmaier, Ekman Phillips, and Wikman 1996), a number of multi-stakeholder land use interventions (Buck *et al.*, eds. 2001), Paolo Freire during his term as a state minister of education in Brazil, and some of the aims of individuals like Dan Bar-On (Bar-On 1989), very few action research projects really focus on more than highly localized problems. Interventions in individual plants, organizations, communities are very common, but action research approaches to larger macro-structural problems are relatively rare. Action research on the Middle East conflict, on globalization, sweatshop exploitation of the working classes in poor countries, terrorism, and other such large-scale issues is rare.

This makes it appear to most onlookers that action research is limited mainly to local interventions. If this is not the case, under what circumstances can it have broader effects? What kinds of methods are appropriate to broader interventions? How do the processes differ in large multi-sector projects? With the small number of exceptions noted above, one looks in vain for meaningful writing about these issues. Here again, it seems to me that action *researchers* show a startling lack of ambition and too great a comfort level with business as usual.

This is a hopeless situation. If action research is mainly limited to local intervention while the ever richer beneficiaries of structural adjustment, the arms race, globalization, global warming, etc. move ahead under their positivist cover, then action research is already doomed to making small differences in local situations, a meaningful pass time but a travesty in view of the liberating potential of these approaches.

Conclusions

It is time for us as action researchers to take responsibility for ourselves and our own agenda by worsening the already evident crisis in the conventional social sciences and in higher education systems worldwide. I suggest worsening the crisis by taking the approach of action research to the centers of power and to the public and exacerbating their dissatisfaction with conventional social

science by demonstrating the strong research values of action research and by piloting the multi-party and multi-disciplinary teamwork to deal with some of the most complex and intractable issues our societies face. But to do so will require a kind of discipline and courage that seems to be scarce. I would like to be wrong.

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