

Why Should Mainstream Social Researchers Be Interested in Action Research?¹

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Abstract: The essay tries to argue why conventional researchers are obliged as researchers to be interested in certain forms of action research. The 60 years of ignorance have been illegitimate. The essay starts by listing two commonly encountered arguments paraphrasing Karl Marx and Francis Bacon via Kurt Lewin. It tries to show why a certain simplified reading of Marx cannot provide the necessary arguments. It then presents different variants of action research in order to single out approaches that according to this author require attention from mainstream social researchers. The action research approach emerging as central, by demonstrating its presence and effectiveness within mainstream research as well, is immanent critique. The method of research methodology is immanent critique. Immanent critique has to be demystified, however. When it is brought down to earth, immanent critique is really the kind of dialogical and experiential learning approach associated with apprenticeship learning and with organisational learning. This conclusion, making self-reflective practitioner-research the “hard-core” of action research, even internal to mainstream research, also requires a revision of the experimentalist-as-interventionist credo of action research.

Keywords: action research, counter-public spheres, immanent critique, method of methodology, practitioner research, research methods

The purpose of this essay is to answer one question: Why should conventional social researchers be interested in action research? My general background for trying to provide an answer is 1) being a philosopher with a special interest in the relations of ancient Greek philosophy of dialogue to modern social research and action research (cf. e. g. Eikeland 2007a, 2007b), and 2) having worked practically as an action researcher in projects in Norwegian work life, mostly municipalities, almost continuously since the mid-1980 s, and with similar projects for several years before that.

There are many reasons why conventional social researchers *should* be interested in action research, but only limited space is available here. Hence, I will try to focus on my arguments searching for reasons, writing as if reasoning actually *could* direct the interests of anyone. Trying to let the arguments speak for themselves, references will be kept to an absolute minimum in what follows². As another consequence of the space limitations, none of the arguments can be elaborated extensively, of course. They will all have to be in outline, only.

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2 Further arguments and references to other authors and research literature indicating what I try to argue here are to be found in e. g. Eikeland (1985, 1995, 1997, 1998, 2001, 2006a, 2006b, 2006c, 2006d, 2007a, 2007b). Interested readers will also find my reasons for thinking that answers to almost all the challenges raised in this text can be developed by taking seriously into account the more than 2000 years old works of Aristotle, among many others, of course.

But there are many different variants of action research as well, requiring some kind of selection. All of them are not equally defensible, neither to the same degree nor in the same ways. Conventional social researchers are hardly obliged to show an equal interest in all forms. But I think they are obliged in relation to some, that is, if they take their own research business seriously. In a way, I will be experimenting my way ahead openly and argumentatively in what follows, having a kind of textual dialogue with myself and with the reader. I will try to select, by way of gradual elimination, what I think are the most promising action research approaches to defend. In real life, many of the approaches are combined, of course. But this makes it even more important to keep them analytically apart when arguing.

On the way through this text, then, many secondary and subsidiary arguments supporting some forms of action research will be examined and passed by, which I am sure will weigh heavily for some. But such arguments are not the *main* reasons why conventional researchers should be interested in action research. I consider them subsidiary arguments because they appeal to researchers *qua* human beings, *qua* socially or politically responsible individuals, *qua* commercially interested actors, etc. but not *qua* researchers. This means, in other words, that subsidiary arguments appeal to all human beings, all socially and politically responsible individuals, etc. but not to researchers in particular. But secondary and subsidiary arguments still carry considerable weight. I will end up with what I think are some quite strong reasons, seen from the perspective of research in general, for some quite promising variants of action research.

Two arguments

The starting point for raising the question to be answered at all is the fact that action research has been carried on by dedicated researcher-practitioners at least since the 1940 s. During the same period of time, and in spite of its origin in “hardcore” social science, action research has been almost completely ignored and neglected by what might be called “mainstream” currents of social research. It still is. But the challenge in this essay is not to explain why 60 years have passed by in this way. It is to provide arguments justifying why this situation should not be allowed to continue. So, why should mainstream social researchers be interested in action research?

Let me start by saying generally and imprecisely that action research is research, somehow concerned with practice and with some kind of social and personal change. Why should mainstream researchers be interested in anything like that? One argument often encountered sounds somewhat like this: (I) Mainstream researchers should be interested because they ought to change the world, instead of just interpreting or explaining it, like Karl Marx pointed out in his famous Feuerbach theses.

A second argument often found is (II) that researchers should be interested because we simply have to change things in order to understand them, as Francis Bacon argued in promoting experimentalism in natural science 400 years ago, and action researchers often quote Kurt Lewin for paraphrasing. Personally, I think the second argument (II) provides the best starting point for a discussion. It strikes researchers more in particular, while the first one (I) strikes everyone equally.

Don't just interpret the world, change it!

But the first argument (I) still needs to be dismissed properly as part of my justification strategy here. So, let me state the following before leaving it behind: The first argument uses Marx in a somewhat moralistic tapping. It really tells people to drop whatever they're doing and go do something else instead. But why should anyone go do something else, e. g. change things? Of course, with the earth's climate altering dramatically, there are mandatory arguments urging us all to make changes if we want to survive. Global climate changes actually challenge us *qua* biological beings. Also, in large parts of the world, social and economic injustice is so grave and intolerable it seems to demand some change efforts from us *qua* human beings. *Qua* biological beings or *qua* human beings with a moral conscience, researchers should use their expertise in order to contribute to the solution of challenges like these. Indeed, *everyone* should use whatever expertise they dispose of in order to solve challenges like these. But are any dramatic challenges like these facing mainstream researchers *qua* researchers, requiring them to change not only the extrinsic purposes, for which research is done, but the very way they do research?

Basically, the first argument says nothing in particular to researchers e. g. about changing research methods, i. e. about doing what they do in different ways. At best it tells researchers to do what they do but for different purposes; in order to serve other causes. But *qua* researchers, mainstream researchers are normally not into changing things, neither external nor internal to their research activity. Their job *is* to describe, analyse, interpret, explain, and understand. Should researchers simply stop doing that, but still continue to call themselves “researchers”? Hardly! According to conventional wisdom, their ways of doing research do not imply changing things. According to conventional wisdom, the opposite is required in order to eliminate biased and illegitimate researcher influences on their “findings”, i. e. in order to secure general validity. If action research is merely a method for creating change, mainstreamers might ask in return why anyone should call an activity like that “research” at all. The question under discussion is: What arguments will convince mainstream researchers *qua* researchers about the merits of action research? Shaking moralistic fingers at them will probably not do it.

Unqualified, then, the first argument tends to confirm prevalent prejudices among mainstream researchers against action research. Some of the caricatures and prejudices against action research are that it is sloppy research, politically prejudiced research, anti-theoretical or theoretically uninformed research, just short-sighted problem solving and hardly research at all, mere “activism” and “moralism” using insufficient methods, etc. Allegations like these may, of course, be true against some action-researchers. They are undoubtedly pertinent against some other kinds of researchers too. But a serious discussion about action research (and any other kinds) needs to elevate itself above merely discussing poor or good performers singularly. Poor performance by one or another practitioner does not necessarily indicate that poor general standards of measurement are involved just as a novice or amateur performance of a violin concerto does not prove that the concerto itself is no good. Insufficient performance does not necessarily indicate that these poor performers are pursuing the wrong ends.

But the question to be answered is *not*: Why should mainstream researchers be interested in any one (or more) action researcher(s) in particular? The question concerns action research as such, if any “as such” of action research can be distilled. Also, in order to understand and evaluate either good or bad performance in the singular we need to understand what general

standards of excellence or performance and what basic principles are involved and at stake in the singular cases. We need to understand what the performers are trying to achieve. If any one in particular is interesting, it is because s/he is doing something of general interest, i. e. relevant for what others are doing. Hence, the question would be: How is action or changing things relevant not only as a subject of extraneous study, but for the performance of the practice of research itself?

But, furthermore, are there general reasons for suspecting an action research mainly interested in changing things of sloppy research? Unfortunately, I think there are. If the research efforts are actually *subordinated* to the change efforts, the implications are that we should or could only do research to the extent, as exactly as, and as well as necessary for our practical purposes. Working under the exigencies of getting certain things done within certain time limits, we do not need to and hardly ever have the opportunity to do as much research as possible as well as possible in order to delve deeper into the studied subject. For practical purposes, reaching the practical aims set within certain time limits is decisive, and (at best) we take what knowledge we need from wherever it is available to us. For practical purposes, promoting a particular change and acting under time pressure and other exigencies, a part of the truth or some approximate truth often has to suffice. In addition, a lot of different means, apparently having nothing at all to do with truth or knowledge, may be quite expedient for creating all sorts of social changes, of course. Of course, for most practical purposes, the *best* would be to have the necessary knowledge already, i. e. that no extra research was necessary. As perfected professional practitioners, we could then concentrate on performance, applying practically and in the best possible way, the knowledge and competence we already have.

But it would be rather naïve to think that having research as such – i. e. for the sake of theory, understanding the studied subject increasingly better – as an independent and superior aim, could not easily come into conflict with practical aims like this. In a way, it is elementary research ethics that it could and easily will. There are common precautions circulating to prevent it. I would be quite surprised if many action researchers have not experienced situations like I have, where they would have liked to pursue better knowledge and understanding but were prevented because of the “practical requirements” of the project(s) they’re in.

Unfortunately for the first argument (I), I think history also has shown many times how settings like these result in half-hearted research, and a halfhearted commitment to truth and knowledge in order to promote (or restrain) social or personal change sufficiently wholeheartedly. I continue to presume that the primary interest of researchers is to do research wholeheartedly even if and when it might be critical of certain practical change (or restraint) efforts. So, once more, if what we primarily want to do is to promote (or restrain) changes in the world in directions pre-specified by ideology, theory, tradition, or whatever, why not drop the somewhat pretentious research bit and concentrate on action? In most countries, there are still many legitimate ways of doing this openly and politically, democratically (or privately, for that matter). Where there aren’t, the arguments take us outside what can be pursued in this essay.

Of course, the real insufficiency of the first argument (I) is that the challenge hardly allows an either (action) – or (research) solution. The meaning, even of Marx, is hardly: Change the world (in whatever way) *instead of* interpreting and explaining it! Neither could it be simply: *Stop* interpreting and explaining the world! Not only researchers but all of us inevitably interpret and explain the world somehow. Hence, the questions to everyone have to

be: *How* do we interpret the world, *how* do we know our interpretation or explanation is any good, and what has changing the world got to do with that? In deed, if our understanding isn't any good, how could we possibly act appropriately in order to achieve the wanted changes? With an erroneous or insufficient interpretation and explanation we will probably end up with results quite different from (and actually subversive of) those we started out with wanting (and with many casualties on the way). I believe the experiences from a number of socio-political revolutions, small and big, bear witness to this. Of course, this also shows why good practitioners need adequate knowledge. The question is, then: how can they get that?

But then again, maybe action research should be construed as the *application* of existing research results – theories and data – in order to promote e.g. development, democracy, and social justice? Maybe so, provided that extant knowledge is sufficient and adequate, but will this convince mainstream researchers? They're into *doing* research, not into applying its results. Is the mere application of theories and research-results the same as doing research? Hardly! Making action research into the mere application of existing knowledge may easily just confirm the existing division of labour between knowledge producers and knowledge appliers. In a way, this answer begs the whole question since it raises new questions about how the applied knowledge is produced, tested, and validated. As I return to below, the division of labour between knowledge producers and appliers is part of the problem but hardly of the solution.

But assuming, for the argument's sake, that existing knowledge (theories and data), or knowledge produced by conventional research methods, is all we need. We have the knowledge. All we need is to apply it. One of the challenges is that the knowledge is produced and provided by people different from those supposed to use or apply it. This produces transaction costs of interpretation and misunderstandings at every joint. Currently, attempts at making research knowledge relevant as “actionable” or “workable” are high on the agenda, but quite often without asking questions about what *kind* of knowledge is attempted conveyed, i.e. without asking any questions about *how*, by *whom* and for *what* the knowledge is *generated* in the first place; about the relationship between application, mediation, learning, research, and knowledge generation in general. But this is decisive. All kinds of existing knowledge generated in any ways whatsoever, are neither available, nor applicable, nor actionable in the same way or to the same degree by just anyone.

But the quality of the application-answer also depends, of course, on *how* existing knowledge is applied. The application could be done 1) *technically*, treating people as objects to be manipulated. It could be done 2) *didactically*, treating people as “containers” of verbalised knowledge or as listening students supposed to apply their thus acquired learning for themselves. Or, it could be done 3) *deliberatively*, through some form of conversation or dialogue, trying to convey and adjust insights from conventional research, philosophy, or whatever, to practitioners and to practical problem solution, searching together deliberatively for the best action alternatives. It could also be done 4) *manipulatively* of course, persuading or seducing people into “buying” solutions offered by research. To the extent that application is part of their business at all, most action researchers would undoubtedly subscribe explicitly to the third way no matter what their tacit practices were. It adjusts better to where practitioners are, taking that as its starting point.

Many of the activities called action research are clearly conceived as collaborative efforts where researchers and practitioners cooperate in practical efforts to change and develop organisations or communities, bringing in their different perspectives and background

knowledge to complement each other mutually. The researchers do not necessarily do research within this kind of collaboration, however. Frequently, they work as advisors or consultants based on their mostly theoretical, pre-existing knowledge. Engineers, shop stewards, directors, board members, and researchers could all meet as peer participants in project work contributing their share to the project. But none of these collaborators necessarily perform *as* engineers, *as* shop stewards, *as* directors, *as* researchers, etc. during their project work. Doing this and that kind of specialist work is where each one comes from, and from whence his or her practical advice springs. It is also what s/he returns to after and outside the collaborative project work.

For many working with participatory methods in development and change, this is an end in itself, as a way of broadening democratic practices. But although participation and democracy are important ends, mere participation or democratic procedures cannot guarantee the quality of the knowledge produced, nor can it guarantee the quality of the participation itself. In general, development projects often have practical ends, making knowledge generation and the quality of such generative processes into a secondary and subordinated concern. For conventional researchers who still want to actually do research, not consultancy, teaching, therapy, democracy promotion, or something else, this is hardly convincing.

Various ways of doing action research

But still, many questions remain: How was the applied or conveyed knowledge produced and tested in the first place? What *kind* of knowledge is applied? General rules or statements taken from textbooks? Competence acquired through practical experience? A little of both this and that? How is practical and experiential knowledge and competence developed? Most action researchers would *not* agree that they simply apply theories and data produced by *other kinds* of research. In addition, hardly any action researchers would admit that they are simply applying already *existing* research results. So, if knowledge and competence is applied at all, where does it come from? How has it been validated? If pre-existing knowledge is not simply applied, how, when, and where is necessary and relevant knowledge produced?

What kind of knowledge and competence are we talking about?

Change efforts are at least sometimes open ended or oriented towards vaguely formulated ends or “visions”. They are not always pre-specified, aiming for given ends, simply following predefined precepts and patterns, implementing recipes. In the production of advanced technical solutions, this may be the only way to attain anything, of course. But, in deed, social and personal changes rarely proceed or achieve anything like this, even if they were planned that way. Social and personal change and development is not algorithmic, nor can they rely on unambiguous connections between efficient causes and effects. Far from everything involved in changing social relations or organisations can be fully known in advance. Local conditions and individual starting points vary. They require adjustments. More open-ended change projects also require different approaches from merely promoting ideologically or theoretically defined changes. It means, of course, that new knowledge of some kind must somehow be produced as part of the change effort itself and actually be used formatively to develop the project and its aims as well. Knowledge generation and application would then have to be

included as a running concern within the change effort itself. If local conditions serve as the starting point for improvements and adjustments, as they necessarily must, knowledge generation and change efforts have to be integrated.

Providing the lacking knowledge or supplementing insufficient knowledge could reasonably be called action learning or research. Of course, since the mid-1960 s, with the decline of the first wave of action research from the 1940 s and 1950 s, similar efforts have often been labelled “evaluation research” as well. But introducing such integrated and open-ended *change-cum-research* as iterative and formative cycles of knowledge generation into development projects – as cycles of action research or action learning – is probably what most existing action researchers would agree on in principle as identifying their work. So, *how* do action researchers produce, test, and validate knowledge?

On this question, the really existing action research approaches and researchers differ much. As far as I can see, the divisions in the following table capture the most important differences. For reasons of space and in order to avoid discussing singular approaches, I will not fill in the numbered blank squares.

Table 1

	C. Applying conventional research techniques	D. Radical self-reflection
A. Collaborative action research	1	2
B. Practitioner action research	3	4

Many confessed action researchers in category A define action research as an essentially collaborative effort between a) professional researchers located at specialised academic research and educational institutions and b) practitioners (non-researching or co-researching) in different parts of work life. Others, in category B, think of action research primarily as the practitioners doing the research themselves. Within both of these categories (A and B) there are many (C) who think of action research primarily as the application of conventional research techniques like gathering and analysing the results from i) posing questions in different ways, from ii) observing in different ways, or from iii) experimenting. Others (D) within both categories (A and B) think of action research as basically something different or something more than applying conventional research techniques, i. e. as some form of radical self-reflection.

Collaboration between researchers and practitioners in category A may attract researchers for many reasons, of course, not the least pecuniary. In the current social climate where international competition is apparently forcing politicians everywhere to become increasingly business oriented and overtly utilitarian, more public money is channelled to finance research that seems useful for business innovation and commercialisation, less to free and independent research. But money is very persuasive, independently from personal convictions and from any intrinsic qualities of the money-conveyors. So is power and force, of course. If persuasive arguments like these count, then, this will certainly persuade many conventional researchers into something that could somehow pass as action research. But they are probably persuaded because of the money, the influence, and the personal opportunities they open up, not because of any intrinsic research qualities of action research. People mainly persuaded by money and

similar media will of course accept or discard anything depending on its money-generating qualities. Within commissioned research, the persuasiveness of means like these is becoming great due to increased and enforced market exposure among other things. If something called action research is in demand by money and power, it will attract people. If conventional research is in demand, so will it. Hence, this – might we call it research “opportunism” – is hardly an argument for action research as such. It is an argument for anything in demand by people able to demand.

The question is whether research approaches combining A-C or B-C in the table will convince conventional researchers. The A-C combination is the mentioned collaborative approach between established research institutions and “non- or co-research” practitioners in work life. The B-C combination also implies something already happening. This is the current transfer of conventional research competence and activity from protected and insulated research “academies” to “normal” work life organisations. In our late modern, ICT-dominated era, normal work places and normal jobs increasingly include the application of research methods more or less advanced and more or less professionally applied (using statistics, interviews, laboratory testing, field work, etc.). This social transfer and redistribution of research competence and practices challenges the institutionalised social division of labour between producers and appliers of knowledge, of course.

Although both of these (A-C / B-C) too may be called applied research, this is a different application from the one mentioned above. There, it concerned the application of existing theories and research results used as a general background for collaborating on a par with others in practical projects, i. e. without necessarily doing research in the projects. Here, it concerns specific research *methods* used to serve objectives set by others in collaboration or by the practitioners themselves. For researchers who want to use their methodological research expertise in order to promote certain commercial, ethical, or political ends, I am sure the A-C combination will be attractive as long as the researchers are given specific research tasks within a collaborative project. But this is still something motivated by relations external to the research activity itself. Hardly anything needs to change regarding research methods. Researchers only have to use their identical methods for slightly different purposes, serving some business project or some political cause or movement as servants of power, as servants of the people, or in some other servant position. Anyhow, research is subservient.

Within scenarios like these, opportunities will obviously also come up for researchers to work as teachers or tutors for practitioners who want to increase their proficiency in conventional research methods. Many historical variants of action research have certainly worked within an applied horizon like this, not unreasonably ascribed to Max Weber as its originator. But since it merely applies conventional research methods, the term “action research” may seem superfluous and even confusing. For many researchers, whose main wish is to do research, doing research in the conventional way within well established research institutions will be more attractive, if the preconditions for doing it well are provided. Doing research well is the clue, and the A-D / B-D combinations in the table remain to be discussed.

Improving research methods

Basically, the kind of arguments that will really convince conventional researchers mainly interested in doing research of the merits of action research must be research-intrinsic arguments that concern the quality of their research activity as such. For intrinsically motivated researchers, research intrinsic arguments bite the most. All researchers are not obliged, *qua* researchers, to let their research serve specific external causes. They may be *qua* human beings, perhaps, but not *qua* researchers. But in a way, *all* researchers are professionally obliged to heed research intrinsic arguments, unless they want to be charged with doing “sloppy research”. Researchers have to do research in the best possible ways available, adjusted to the subjects studied in the best possible way, studying real challenges at some frontier of knowledge. Whenever research is subordinated to some other purpose, this is threatened. “I’m only in it for the money”, or, “I’m only doing this because it serves this or that political cause”, is hardly a convincing research justification. Demanding whole-hearted, top-quality research from researchers *qua* researchers is both reasonable and necessary (if politicians provide the preconditions). Hence, in a way, such arguments are the only ones that *ought* to convince researchers if our general confidence in research is to be preserved. But does anything reminiscent of action research have any such arguments in its favour? I think it does.

Arguments about using all kinds of research techniques in order to promote all sorts of good ethical, political, or commercial causes may, of course, mobilise many researchers. Probably they will not only receive but actually deserve true praise for taking social and economic responsibility too. But such arguments will not mobilise them for research-intrinsic reasons. Such research *use* could easily be incorporated into mainstream activities simply as ways of applying the existing research resources. They hardly need a controversial term like “action research” in order to do that.

As indicated already, the A-D and B-D combinations in the table have not been discussed yet. In a sense, they presuppose that there are some important forms of knowledge generating activity still not integrated into normal research procedures. Seen from the perspective of conventional research, people in both the D categories seem to be doing something “completely different”. But are not all social research activities just various ways of observing, questioning, or experimenting – including reading, thinking, etc. already implied in observing, etc? I think the best way to illustrate is by showing how such non-integrated but still essential activities are internal, like a Trojan horse, to normal ways of doing research “in spite of themselves”.

Normal research methodology recommends variants and combinations of the approaches just mentioned for empirical research. But there is an important field not covered by these: the methods of methodology itself. Methodology as a discipline is not primarily based on questioning, observing, or experimenting on other researchers in any conventional senses. Anyone trying to base methodology on things like this would, of course, have to learn to do research properly *first*, since these are research methods. But from whom, if we need to *know* how before *learning* how? They say methodology is normative. And so it is, but I would claim it is still empirical.

Making a long story short, my contention is that, basically, methodology is built on the self-reflections of the research community, founded on the community members’ long-term,

practically acquired experience from doing research, i. e. as research practitioners. Over time, this practical selfconsciousness is produced by an enduring learning process that gives identity to the research profession. Methodology, then, is knowledge developed “inside-out”, “bottom-up” by practitioners within a certain community of practice by sifting and sorting similarities and differences in their own acquired *habitus* and experience. Methodological knowledge is “actionable”, practical knowledge produced as such by practitioners, not “translated” or conveyed back to practitioners after having first been produced by spectators, visitors, or manipulators as non-actionable knowledge. Any “translation” within the community of practitioners is between peers alternating as masters and apprentices whose practice is either closer to or further away from common standards of reference in “perfect” performance. The saying that those wearing the shoes know best where they squeeze and gnaw becomes even truer through procedures like these. Simultaneously, this method of methodology is not subordinating research to any practice. It is putting the research practice at the very centre of attention and at the very centre of every practice.

Strangely enough, although clearly empirical (if empirical still has anything at all to do with its roots in experience), this is not normally included among the recommended empirical methods of mainstream empirical research. This strangeness is increased by the fact that methodology is not an incidental or peripheral discipline. It is actually what gives public legitimacy to the whole business of professional research. But still, this crucially important and powerful discipline is not recommended for other professions who are required to make their professional practices so-called “research-based” in order to increase their legitimacy. Research is a practice among other practices. But the profession of the social research practitioners is hardly research based in the required sense. Hence, the question is: Why cannot other professions do as the research professions; justify their professionalism through systematic and collective self-reflection based on native or practitioner experience and analysis?

Implicitly or explicitly, this is the question raised by the A-D and B-D combinations in the table above. At least, this kind of self-reflective activity is what characterizes these approaches. There is something crucial not included in the methods of normal empirical research, then. This kind of selfreflection is missing. And this, I claim, is also the “hard core” of action research. The methods of mainstream research methodology are action research methods as good as any. As long as mainstreamers and action researchers of these kinds do not realize that they have the methods of methodology in common, the A-D and B-D combination appear to be “cut off” from the normal business of empirical research, i. e. from the externally recommended methods of empirical research. They’re apparently simply not doing the same sort of things, and action research appears to be out of touch with ordinary research. But this is hardly true.

The method of methodology is hardly “cut off” or “out of touch” in this sense, although what it does is quite different from ordinary empirical research. But it coincides with action research. The method of methodology – i. e. hard core action research – is the heart and brain of all research. Action research of the B-D combination is developing the methods of research methodology from within the methodologies of other professions or activities, that’s all. They’re doing what the research profession has been doing for themselves all along: analysing their own activity experientially and as natives from within in order to improve it, i. e. practical, actionable knowledge produced directly from practice by practitioners.

Taken comprehensively over time the discipline of methodology has even been doing it “paradigmatically” or in exemplary ways from which all action researchers could and should even learn. To the extent that specialised action researchers of the A-D combination are doing similar things, they either work openly as facilitators in the self-reflective processes of others, or they are refining their so-called “intervention methods” more clandestinely through internal self-reflection in narrower circles for their own benefit or for that of novices in their profession. But the most essential and “hard core” part of action research is not based on intervention by outsiders into the lives and practices of others. Intervention cannot rid itself of a manipulative taint. Hard core action research is not intervention but collective self-reflection. Anyhow and ultimately, then, the action research in both the D categories is both totally different and cut off from mainstream research, and still, at heart, common and exactly the same. Mainstreamers are not always as meticulous as they should be in their own discipline of methodology, however, to say nothing about their accuracy in the *methods* of methodology.

Inherent mainstream insufficiencies

In general, the approaches mentioned – observing, questioning, experimenting – are recommended as empirical by the discipline of research methodology. But most conventional social research within major disciplines like sociology, political science, and anthropology no longer really count the experiment within their methodological repertoire (if they ever did?). Some psychologists still do. But the disciplines mentioned mostly stick to questions and observations as empirical methods. But are these methods the best available? The necessary self-reflection presupposed even by mainstream research – the method of methodology – indicates they may not be. Also, this is where a consideration of the second argument (II) supporting action research above, becomes particularly relevant.

At least since Francis Bacon, 400 years ago, experimentation has generally been seen as the most basic and reliable method of research. Bacon’s famous idols (*idola tribus, idola specus, idola fori, idola theatri*) were invalidating distortions bothering mere receptive and inactive observation from afar through the senses in particular. Bacon knew quite well that everything taken in perceptually through the senses is thoroughly interpreted and saturated by particular prejudiced perspectives – the idols – a condition generalised today into a kind of post-modernistic stalemated absolutism and simultaneous suspension of all perspectives indifferently.

If we distinguish in the conventional way between 1) what we use in order to explain and interpret “something” as the *explanans* or *interpretans*, from 2) the “something” itself in need of explanation or interpretation as the *explanandum* or *interpretandum*, the 1) *explanans* or *interpretans* always has to be something we already understand. Since we don’t understand the *explanandum* itself, we make sense of it by seeing it as if it was similar to something we already understand. This goes for observations, and for the answers to questions posed, as well. We necessarily explain and interpret, i.e. understand at all, by means of whatever knowledge and understanding we already have. The opposite would be absurd, i.e. explaining something obscure by something even more obscure, although this has been common for

centuries, receiving its designation – *obscurum per obscurius* – by the medieval scholastics referring to a passage in Aristotle’s *Physics* warning about this possibility. For some, the necessities of explanation seem to make the universe thoroughly metaphorical. Everything seems to be understood by means of something else transferred, ending up in an endless regression.

But quite basically, our understanding of any explanation or interpretation is ultimately referred back to and founded in our acquired and accumulated experience (*Erfahrung* not *Erlebnis*). This experience is not simply something else transferred from somewhere else. It is basically formed and generated over a certain time and somewhere specifically, repeating things over and over. This does not produce another *explanandum* needing explanation. It produces new basic understanding, i. e. a new *explanans* that can be used to understand or interpret new observations. Hence, equally basically, experimenting with nature, or more generally with whatever subject is studied, deliberately intervening and changing it in controllable ways, was and is an attempt at moving beyond the “idolatry” of arbitrary spectator interpretations by non-participants from afar. Experimentation – trying things out again and again – is gaining new experience first-hand with whatever our knowledge, interpretation, or explanation is about.

The insufficiencies of conventional social research methods – mainly variants of questions and observations – are also indicated more specifically by a number of objections well-known within methodology and the philosophy of science. I will mention some without delving deeply into them. There are two sets of arguments, one concerning the quality of data or relevant experience, another one concerning the quality of explanations or interpretations.

General data insufficiencies and weaknesses

It is a fact well-known and often repeated in post-positivism that data collected depend on theories in at least two different ways. Such theories do not have to be explicitly formulated, of course. More often, they are implicit basic assumptions. Data collections are selections of data. What are considered relevant data to collect depends on some implicit or explicit theory about relevance. Explaining or interpreting social conduct, most social researchers ignore the positions of the planets and stars as irrelevant data. For astrologers these are the most relevant data, however. The point here is not who is right. The point is that their theories of relevance differ widely.

But the selection of data is not only theory dependent. The data are also theory-impregnated or saturated. Sticking to the examples mentioned, hardly anyone considers stars and planets holy or divine in any sense anymore. 2000 year ago, many even among the most educated, definitely did. They were not stupid. Many of these understood logic and basic principles of knowledge much better than most modern social researchers. Generally, the point about the thoroughly interpreted nature of all observations is emphasized. All observations are interpreted relatively to what we as interpreters bring along. There is no rock bottom made of un-interpreted data the way the logical positivists presupposed. Data are not given. The question is *how* we interpret and how some interpretations may be said to be more

adequate than others. Merely “unbiased” sense-perception from any distant position is insufficient, and so are interpretive positions arbitrarily chosen, of course.

Another difficulty bothering conventional data collection is the reactivity of the collection procedures themselves. Not only are data “non-given” and thoroughly interpreted, i. e. hardly data at all. The way they are collected is a socially defined activity in itself. Different social contexts influence people in different ways. There are hardly any “neutral” contexts, since eliminating everything social is impossible and at least not at all neutral. The very consciousness that something called data-collection is going on will, of course, influence those involved, either as collectors or as providers. But what is sought for are not data influenced and contaminated by the abnormal activity of data-collection. The researchers want to know how things are when they are *not* present and influencing, i. e. “naturally” or normally. Different selfobliterating ways of eliminating research effects have been suggested, like “unobtrusive measures”, etc. But conventional research stumbles in its own feet, since it is literally standing in its own way. Since researchers are hard to eliminate from the research processes, different ways of utilising the interactive effects as relevant information instead of trying to eliminate them have been suggested. Since these involve self-reflection, they move conventional research closer to hard-core action research as this was depicted above.

Finally, concerning data and the realities studied by social research, there is the challenge of what might be called indicator-research. The trouble is that hardly anything studied by social research can be observed perceptually the way e. g. stars, rocks, plants, and animals can. Neither “a state”, “an organisation”, “the soul”, nor “power”, etc. can be seen, heard, smelled, tasted, or touched. Instead, we have to agree on what is to count as signs indicating activities of the state, organisation, soul, or power. The emergent current orthodoxy sees this as indicating how almost everything is constructed and “unreal”. That organisations, states, etc. are unreal is, of course, totally absurd for practitioners who have to observe the rules and regulations of these very real powers *practically*, every minute of their lives. For researching but non-participating observers or visitors these things may seem unreal, although they observe practically other rules and regulations that seem equally invisible and quite incomprehensible for those visited or observed. The point is, of course, not that any of this is unreal but that you have to participate fully as a native in order to experience their reality. “Going native” is not a distortion, it is a precondition. If you really are a “native” employee in a specific company or public bureaucracy the things you do and how you have to do them are as real and influential as the rules of research are for those insiders “native” to the research processes who really try to do research. In either case the basic rule is: Drop the rules and you are out of the game. *You* become unreal.

Problems of explanations and interpretations

In addition to problems like those above concerning data or experience, there are problems concerning explanations and interpretations. I will mention a few. The first may be called the challenge of theory pluralism. For the last 2000 years this has been known as the explanatory principle of “saving the phenomena”. For ancient astronomers saving the phenomena was what mattered by means of any logically consistent model able to predict the movements of

the light spots on the heavens. Theory realism was out before Galileo. Theory instrumentalism was in. Inspired by Akira Kurosawa, it has entered social anthropology as the Rashomon syndrome: The same phenomenon can be described and interpreted in totally different ways. It is really the challenge of Bacon's idols. Any given set of data can be explained in logically valid ways by an unlimited number of true and false premises, i. e. theories, if you like. In logic, it is well known that true conclusions – e. g. true descriptions – can follow from utterly false premises. If all “fishes” are warmblooded (false premise), and all whales are “fishes” (false premise), it follows logically that all whales are warm-blooded (true conclusion). Exchange fishes with Xs, and you have your logical proof of theory pluralism. Any X will explain that whales are warm-blooded, and any Y will explain anything, provided the right changes are made in the model.

So-called “abduction” has been suggested as a solution for developing good explanations, inspired by Charles S. Peirce. But abduction works by means of confirming the consequent in logical inferences, i. e. by confirming observables like “all whales are warm-blooded” or “the sun always rises in the east and sets in the west”. But confirming consequents like these says absolutely nothing about what the antecedents look like, i. e. it says nothing about whether e. g. geocentric or heliocentric theories are best for explaining the observed movements of the sun. In addition, it says absolutely nothing about whether the sun is divine or not, etc. Theories are radically underdetermined by the data has been another way of putting it by W. v. O. Quine. There are other similar challenges, impossible to delve into here, confronting all models of explanation and interpretation based on a radical separation between the knowers and the known.

Experimenting on the others, or experimenting together?

The question, then, is: Why have only the mentioned approaches – observation, questioning, and a certain kind of experimentation – counted as empirical in social research? One important reason is that the tacit presupposition for modern social research has always been that we – the researchers – have to study the others. The “field” is always where the others are. “Othering” is at the root of the trouble, in allocating performance or execution on the one hand and reflection or interpretation on the other to widely different kinds of people, not sharing experience. This, then, has to be radically changed. Conventional empirical research is part of this institutionalised division of labour.

Although action research is not anti-scientific, in its dominant present form, conventional research is part of the problem, not part of the solution. In order to improve research methods, conventional social research has to change.

So, why have most conventional social research disciplines abandoned or never even considered experimentation before getting lost in the labyrinths and quagmires of post-modernism? If experimentation is simply transferred from how natural scientists experiment with objects in nature, i. e. modelled on a division of labour between experimenters and those experimented on, there are some obvious reasons and a few less obvious. The purely practical difficulties in arranging these kinds of experiments in real life social settings involving large numbers of people are, of course, striking. In addition, of course, many people simply do not want to try out different social arrangements openly in order to learn and improve, for ideological or religious reasons or for fear of exposing illegitimate power structures and

positions. The ethical scruples about subjecting people to interventions in their lives that they may be completely ignorant of and neither understand nor control, are salient too. Segregated and protected laboratory experimentation has the same shortcomings in addition to the difficulties of external validity, i. e. of transferring results from artificial laboratory conditions to real life settings. Finally, I would add that normal concepts of efficient causality, mostly overlooking how the interpretations of the actors involved intervene between causes and effects, are much too simple. Similar efficient causes, or even one identical cause, necessarily have a multitude of widely different effects depending on the experience, maturity, cultural background, gender, age, psychic condition, available space for autonomous action, etc. of those affected.

In spite of scruples like these, action research sprang from researchers and philosophers like Kurt Lewin and John Dewey, committed to experimentation. Both men favoured the re-positioning of experimentation moving out from insulated laboratories to enter every local community, work place, school, and even family. How, then, did the offspring of experimental social research – the spearhead of science – end up discredited in a research orphanage charged with “sloppy research”? Even action researchers and many variants of action research have denied their relationship with experimentation, appropriating the derogatory definition of them by mainstream research instead as “extremely applied research”. The major difference, emerging from the initiatives of both Dewey and Lewin, was that the division of labour between researchers and research subjects – treated as objects – began to disintegrate. This was anathema to established research. Heedless of what they thought they were doing, what the action research initiative started was a movement where ordinary people, workers, managers, teachers, families, etc. would start to experiment together, openly, sharing their interpretations critically in order to improve their own practices, just like researchers through their discipline of methodology. This was and is a sore and sensitive blind spot for conventional research.

Action research soon stopped being an attempt to administer manipulative and clandestine treatment to others in order to observe how they reacted to stimuli. Quasi-experimental research, springing from the same sources, continues in this vein. But in transferring the experimental attitude and practice from laboratories to real life settings, it was also transformed into action research, placing experience-forming experimentation into the practical settings of practitioners as open action learning and action research cycles of repetition, trying things out, evaluating the old and adjusting the new attempts at achieving improvement and perfection in whatever practices were central to them.

Do we really have to change things in order to understand them?

As these stories illustrate, there are many research-intrinsic reasons why conventional social researchers should be interested in action research. The examples taken from conventional research methodology are examples of immanent critique. This is, in fact, an action research method starting out from within *any* existing practice. It does not have to be a research practice. Immanent critique is nothing separate or extraneous to these practices. It is only making explicit tacit knowledge, and inner tensions and contradictions in such communities of practice or discourse formations provoking and promoting the development, the explication, and the actualization of inherent potentials in the practices. The examples from conventional research

practices are just that: illustrative examples. Although immanent critique springs from critical theory, from Marx and Hegel, and ultimately from Socrates, Plato, and Aristotle, as a practice – or as a “meta-practice” if you like – it has, of course, absolutely nothing to do with the hypertrophied household economies misnamed “communism” during the 20th Century.

The practice of immanent critique is fundamentally dialogical. It is a learning process of improving practices through open examination and criticism comparing ends and means and how they agree or disagree, making everything explicit that needs to be explicit in order to reach the ends. Immanent critique changes things but not as external things, technically. The point is not to change things technically (*tékhnê* / *poiêsis*). It is to think through practically formed concepts (= summarizing grasps), i. e. practically acquired experience (*empeiria* = *Erfahrung* not *Erlebnis*). Thinking through this is reflective thinking as dialogical thinking. The practice of immanent critique, critically examining any practice from the inside for inconsistencies of any kind in order to improve it, is nothing other than the basic principle of apprenticeship learning. Apprenticeship learning is a process of formation and perfection of a practice. It is a *praxis*, as this was understood by Aristotle. The meta-practice of masters and apprentices in a community of practice, inquiry, and learning is, of course, dialogical immanent critique enacting the method of methodology.

Since Socrates’ practical turn to the practice of the crafts – and learning from the practice of ancient medicine – in order to understand what knowledge was, instead of speculating and “metaphorizing” about what moved the stars and outer nature, this has been the common wisdom of all critics of similar research practices. As I have tried to show in other places (see endnote 1), this Socratic, immanently critical, apprenticeship learning is also intrinsically connected to a concept of, not a general public sphere where rhetoric dominates, but a counter-public sphere or a leisured free space (*skholê*) for dialogical reflection among practitioners. All of this, again, provides the tools for a form of organisational learning as action research or practitioner research approximating the method of methodology, alternating systematically between performance and reflection. Organisations are communities of practice in need of becoming communities of inquiry and learning as well. Collaboration between existing research institutions and work life organisations should promote, support, and cultivate this internal transformation of existing communities of practice.

The important thing, then, is not to intervene in the lives of others, changing them as some external objects in order to understand them. This may be the only way to learn experimentally and experientially about dead external materials with which we cannot communicate or share any practices and experiences. In order to influence dead matter, we have to manipulate it. When it comes to social realities, however, the important thing (in order to understand) is to go native on the right levels, to participate fully, trying to improve whatever we are involved in doing, changing and improving ourselves individually and collectively, just like the community of researchers have been doing for decades through their discipline of methodology. This goes for everyone. The motto from experimentalism has to be modified. You do not have to change *them* in order to understand *them*. In order to understand anything, you have to practice. You have to go native, or realize that we are all always immersed as natives into some practices already, and provide the conditions for qualified participation by the natives in generating the necessary knowledge. That’s where we have to start, in our own “nativeness”, whether we want to or not, and whether we know it or not.

This, then, is a form of action research that whole-hearted conventional researchers are obliged to be interested in for their own sake (as “natives” to the research processes), for the

sake of the quality of research as such, for the sake of the quality of organisational life, for the sake of their collegial relations, for the sake of participatory citizenship, and for other reasons I have tried to present in the foregoing text. This form of action research is also a form that will satisfy the subsidiary arguments provided on the way even better than the variants motivated by research extrinsic reasons. It is the method of methodology, i. e. a self-reflective practitioner-research.

But everything implied and involved in an approach like this cannot be dealt with here. Its full range cannot be described. There may, of course, also be limitations to what can be studied through methods like these. But for the practical purpose of writing this text, this will have to suffice. Further research will have to wait for other opportunities with even more leisure, relieved from the constant exigencies of practice. That's how it is in the so-called "real life".

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