

PAPER
39

Occasional pieces

Bob Dick (1997-2002) *Occasional pieces in action research methodology*.¹ Occasional discussion pieces appearing on the armnet-l mailing list. Available on line at <http://www.scu.edu.au/schools/gcm/ar/arm/op000.html>

The following occasional pieces were written for ARMNET-L, the action research methodology elist. They appeared there sporadically between 1997 and 1999 and since revised (they may resume again in the future). The reason for writing the occasional pieces is bound up in the reasons for starting ARMNET-L.

My reasons for starting ARMNET-L were mainly twofold. A growing interest in qualitative methodologies generally, including action research, made it timely. In addition, it provided a medium for methodological discussions which might be out of place on the more popular action research elist, ARLIST-L. The “occasional pieces” were intended to be consistent with both of these aims.

I limited the length of each piece to two or three screens only. I also tried to raise issues that are important and will be of interest.

1. Copyright © Bob Dick, 1997-2002. May be copied if it is not included in material sold at a profit, and this notice is shown

1 "Good" research

Speaking only for myself ...

I had occasion recently to talk with some postgraduates about qualitative research. They have included some qualitative components within their thesis research, which is in psychology.

Their undergraduate psychology program has clearly equipped them reasonably well to do quantitative research. They are less well equipped to deal with research of any other flavour.

It also seemed that much of the teaching had treated experimental and quasi-experimental research as *the* way to do research.

This may partly explain why most psychological practitioners don't read much research, let alone do any. They find the research they've been taught is often too limited to be relevant, and too hard to apply.

Many of them throw the baby out with the bathwater. They ignore all research thereafter.

Suppose, instead, they were taught action research. I suspect many of them would find it more relevant and useable in their work situation. In turn, this might lead to more practitioner research.

I fear, though, that as a result they might regard action research as the one way to do research. That doesn't seem to me to be a desirable outcome.

So, what is good research? Following Dewey, I think it's research which achieves the intended outcome within the particular research situation.

It seems to me that sometimes that's action research. And sometimes it isn't.

Experimental research is good for some purposes, but not others. And I think you can say the same about action research, or ethnographic research, or any other flavour.

That would complicate matters, though, wouldn't it? It would make it difficult for us to do research as technicians.

No more following recipes. We would have to understand what we're doing.

2 What is “action research”?

Speaking only for myself ...

What is “action research”?

I hasten to add that, by and large, definitions don't matter much to me. I think that there are more important issues to discuss. How to change the world, for example. In any event it makes more sense to me to talk to people using their own definitions rather than insist on using my own.

I don't experience a world subdivided into boxes labelled “psychology” or “economics” or “engineering”. So I don't see why research has to be labelled as “experimental” or “ethnographic” or “action research” as if those were real categories.

But sometimes it helps to define what I mean by a given label.

Some time ago, Ron Passfield, Paul Wildman and I were writing the introduction for a new publication. We set out to devise a minimal definition of action research. After some discussion we agreed on two criteria:

- true to label, pursuing action *and* research
- a cyclic process, alternating action with critical reflection.

We acknowledged that it was also *usually* qualitative and participative.

As some of you will know, Pam Swepson recently submitted a draft paper to *Action research international*. In it, she argued that there were advantages in treating participation as a matter of choice. Some of the responses expressed strong disagreement. For many people, participation (and preferably emancipation) is clearly not a matter of choice.

If it isn't participative, in some views, it isn't action research. I think it's important to note that Pam wasn't arguing against participation or emancipation.

The responses lead me to ponder. Has ideology replaced pragmatism as the main driver for current approaches to action research?

It isn't my intention to argue that action research is this or that or something else. The issue I'm trying to raise in this and the previous occasional piece is quite different. It is something like: "How do you choose the best research approach, whatever it's called, for a given research situation?".

Or, to put it differently ... In research design how can I see beyond definition or ideology to issues which to me appear more substantial?

(The issues in this and the previous occasional piece are also addressed in the paper <http://www.scu.edu.au/schools/sawd/arr/choice.html>)

3 Critical thinking (1)

Speaking only for myself ...

Action alternating with reflection. That's how you might, simply, describe action research.

In an earlier occasional piece I described action research as a cyclic process. I suggested that it had joint goals of action and research. Within each cycle, action and critical reflection alternated.

In this piece I'd like to raise some critical and not-so-critical thoughts about critical thinking.

First, the cycle.

This same cycle, action alternating with reflection, is the point at which action research and experiential learning touch. It is also characteristic of action learning.

I think of the action as something which draws on a "theory" which is often implicit. The purpose of critical reflection is to make the theory explicit. To put it differently ... It is through alternating action and critical reflection that my tacit knowledge and my explicit knowledge can inform each other.

In this context, what does “critical” mean? I subscribed for some time to a sceptics newsgroup. And it occurred to me that many of them were certainly sceptical about everyone else’s ideas. I didn’t get the impression that they were very often sceptical about their own.

It appeared to me, on the other hand, that there were advantages in turning this on its head — in being sceptical about one’s own ideas, and open to the ideas of other people.

The same perspective, I think, can be taken on critical thinking.

More later ...

4 Critical thinking (2)

Speaking only for myself ...

This follows up the earlier occasional piece on critical thinking. In that piece I suggested that many of us find it easier to be critical about others' ideas than to be critical about our own.

I can observe this in my own behaviour. Once I have begun to form a view or opinion, I seem to notice supporting evidence without difficulty. I have to make a special effort to pay attention to disconfirming evidence.

Observation suggests to me that this is true for others too.

For example, when I was at the University of Queensland, Cindy Gallois and I conducted a postgraduate course in group facilitation. Much of the course was experiential. Each week the class members recorded in a journal their observations, and their interpretation of them. From these journals, they chose a small number of critical incidents for deeper analysis.

One of the criteria we used to give feedback to them was their openness to evidence which disconfirmed their emerging interpretations. Most of them had a lot of trouble with this. I might mention that quite a few of them were already skilled facilitators. This didn't seem to help them all that much.

I've observed related difficulties in other settings. For instance, many experienced consultants recognise that, at any choice point in an intervention, many choices are available. Having made a choice, however, they find it difficult to identify and acknowledge its disadvantages.

I think the implications for action research are important. Many of the advantages of action research can be undermined unless we develop ways of countering our own biases and checking our own perceptions.

There are certain tools which help me be more open to challenge about my own interpretations. I'll talk about these in subsequent occasional pieces.

5 On maps and territories

Speaking only for myself ...

I promised discussion of some of the tools I've found helpful in developing some skill at thinking critically.

In some later sessions I intend to give some space to Chris Argyris' concepts and processes. His work, alone and with Don Schön, has been influential on my own practice. However, in this and the next occasional piece I intend to take up some of the ideas of the "general semanticists". They, too, have had an influence.

My early exposure to their ideas was through reading some of the work of Samuel Hayakawa. Foremost among these works for me was the book *Language in thought and action*.

I've since found that some modern general semanticists downplay Hayakawa's importance. Some of them think he has misrepresented the views of Alfred Korzybski, who was the founder of the discipline. I didn't know this.

In any event, I found Hayakawa's ideas compelling and practical. It was there that I came across the notion that "the map is not the territory". The idea, or concept, or theory, or model, is not the reality. Some of you may have come across it more recently in the literature of neurolinguistic programming, which has made the concept its own.

If I remember that the map is not the territory ...

- I find it easier to remember that several different maps may capture well enough important features of the same territory. Different ideas are not necessarily competitors.
- I find it easier to acknowledge that most (all?) maps are incomplete. (That is their virtue.) I can therefore remain more open to revising my maps in the light of experience.
- I can expect others to hold different maps of our common experience, and not attribute this to their malice or ignorance.

In short, I can think more critically about my own ideas. I can think more critically about my emerging interpretations as I do what I do. I can be more open to learning from the ideas of others.

6 The ladder of inference

Speaking only for myself ...

Last occasional piece, I talked about the concept of the map not being the territory. In this piece, I mention a related concept: the ladder of abstraction or the ladder of inference.

It, too, was prominent in the writing of Alfred Korzybski. (His preferred term for it was "process of abstraction".) In his conception there were three foundation levels:

- The event level consists of the world as it is. Influenced by the physics of the day, Korzybski thought of this as consisting of elementary particles whizzing about
- The object level is what our perceptual apparatus extracts from the event level. We have no way of knowing how complete (or sometimes how accurate) our perceptions are. At the very least, we know they are incomplete.
- The label level consists of the concepts we apply to the object level. We generalise, we categorise. Our language in a sense obliges us to do so, even if that does not well represent the perceptual experience.

As you might suspect, Korzybski recognised that there were many levels beyond these three, consisting of our higher-order abstractions.

As with the concepts of map and territory, Samuel Hayakawa made use of Korzbyski's ideas. In *Language in thought and action* he gives the following example, beginning with a cow:

- 1 The cow known to science
- 2 The cow we perceive
- 3 The word "Bessie", a name given to a particular cow
- 4 The word "cow", standing for the characteristics we have abstracted from a number of cows
- 5 e.g. when Bessie is grouped as a member of "livestock"
- 6 e.g. when Bessie is included among farm assets
- 7 e.g. when Bessie is included among assets generally, not just farm assets
- 8 The word "wealth".

[I've shortened the entries from Hayakawa's example.]

Those of you familiar with Argyris' work will know that he, too, makes frequent use of the concept. His term is "ladder of inference". I won't go into details at this time.

The relevance of this to me is twofold. (Actually more than that, but two will do for now.)

First, I find it helpful to remember that the "reality" I perceive is certainly not the reality which is. The map is not the territory.

Second, much conversation is held at high levels of inference. One consequence is that sometimes we mistake the use of similar labels for agreement, and dissimilar labels for disagreement. This helps me to avoid being stuck in debates which are essentially about definitions, and little else.

7 Abstraction, and theories

Speaking only for myself ...

I wrote in some previous occasional pieces about the “ladder of abstraction”. In this occasional piece I’ll add another dimension and begin to discuss the implications for theory.

To begin with the ladder of abstraction ... C. West Churchman has a similar concept (*The design of inquiring systems: basic concepts of systems and organization*. New York: Basic Books, 1971). He talks about a fact net.

Below the fact net, raw observations are to be found. The fact net at its base has theories closely tied to observations. Gradually more abstracted and more abstract theories are to be found as one moves up through the fact net.

I would add that at the very top of the fact net, or perhaps above it, one might find theories which are entirely abstract. Such theories have more the quality of logics.

Here you might find systems theory, which might be regarded as a set of related categories into which many phenomena can be classified. Here too information theory might be found. It is a metric which can be applied to any phenomena which can be categorised as consisting of signal and noise.

I would now like to add another dimension: the degree of interconnectedness. At one end (the left, let us say) you might find theories which depend upon few variables and low interconnectedness. At the other end (towards the right) would be found the theories which involve many variables which are richly interconnected.

Where would you expect to find most models and theories dealing with the behaviour of people in interaction? In the upper right quadrant. Here are among other theories those that deal with what Peter Checkland (of soft systems methodology fame) calls “human activity systems”.

And now I would make the point which is in a sense the purpose of this occasional piece ... Different parts of the extended fact net contain different theories.

It seems to me that they require very different methodologies to engage with them.

8 Cycles within cycles

Speaking only for myself ...

In an earlier piece I suggested that action research was cyclic — that this was one of its defining characteristics. In particular, action and reflection alternate with one another.

This is a very simple model. Action, reflection, action, reflection ... But already powerful. It allows the integration of the conscious and deliberate thinking and knowing of reflection with the less conscious and deliberate knowing and thinking of action.

The question arises, how long is a “cycle”. Increasingly I am drawn to the conclusion that there are cycles within cycles within cycles, and so on.

The inner cycles may occupy only seconds in time. The outermost cycle may be longer than a particular intervention.

In this way a very simple conceptual model — act, reflect, act, reflect ... — becomes enormously flexible. Its flexibility allows it to deal better with the complexity of people and social systems.

9 Cyclic critical reflection

Speaking only for myself ...

As I've said, to my mind the cyclic nature of action research is part of its special strength. The use of cycles allows action research to be both flexible and rigorous. Each cycle includes critical reflection, which contributes to both.

To continue the "cycles within cycles" theme ... Embedded cycles constitute a form of process. Even more importantly, I think, they can support a particular mindset. This is what I wish to explore in this occasional piece.

I think of each cycle as consisting of planning, action and critical reflection. This is true whether a cycle occupies seconds or years.

For instance, consider a major action research project.

One may begin by anticipating what will happen, and developing a broad plan. At the end of the project there is typically a period of reflection and review. The result is that, over the course of the project, new light has been cast on the assumptions underlying the plan.

Or move to the opposite end of the continuum: to cycles which occupy only seconds or less. It is here that the nature of action research as a performing art is most evident.

For instance, imagine an action researcher about to query a colleague. She might first be clear about the outcomes she is pursuing with her question. Then she asks it. Then she checks if it achieved the intended outcomes.

It is also in the briefer cycles, I think, that the relationship between tacit and explicit knowledge is more apparent.

I think my natural talents as a theorist exceed my natural talents as a facilitator. Yet, when it matters most, I find that there isn't time to theorise.

There is too much happening, on too many channels. My actions are then driven by my tacit theories. Becoming aware of them is difficult enough in later reflection, and even more difficult during action.

So in planning I draw on my explicit theories. In action, tacit theories often guide me. In later critical reflection I have time to ask if my explicit theories corresponded to the theories implied by my actions.

To return to the theme ... It is when I adopt a particular mindset that this seems to happen most effectively. There are times when I am able to adopt this mindset. It consists in being conscious of my intentions, willing to follow my intuitions, and willing to be open to evidence that my tacit or explicit theories don't work.

And this is when I seem to be most alive. Most open to learning.

I welcome your comments.

10 Emergent methodologies

Speaking only for myself ...

Sometimes I think about the reasons why action research takes the forms that it does. I presume that much of its typical shape arises because of its joint objectives of action and research. Action research is action and research.

Consider the first of these dual aims, action or change. Action research must be responsive. It has to be able to adjust to the demands of the situation. It has to be able to engage with the world as it is, in all its complexity.

The preference for very participative forms arises partly from the same requirement.

To achieve the second of these dual aims it has to be rigorous. You cannot otherwise trust the information you collect. As you would expect, it shares that feature with other varieties of research.

Put these two aims together. The desired result is twofold. On the one hand, understanding (i.e. the research) allows more effective change. In return a pursuit of change enhances the understanding.

It is especially the “cycles within cycles” we’ve been talking about which allow both to be achieved.

Consider further the notion of flexibility and responsiveness. The consequence is that action research is an *emergent* methodology. Its methodological form is fine-tuned on the run to take account of the increasing understanding of the researchers. It takes shape gradually.

Cycles allow emergence. Cycles within cycles allow that emergence to apply both to broad-brush design and fine-grain detail.

In my experience, this is important for people who wish to facilitate change. As I've said before, I often find in complex change situations that wherever I start is the wrong place.

In short, if I don't know where to begin in some change situation, action research nevertheless allows me to get started. I can make up your mind as I go.

This feature allows action research to be used for other purposes. In fact, it seems to me that it might be useful whenever I find myself in an ambiguous research situation.

In particular, consider very exploratory research. Suppose I have a hunch that some situation, or some observation, is worth researching. But I don't know enough to choose a research approach. I might find action research useful for taking those early steps.

The idea I'm playing with here is that action research (or action-research-like processes) might constitute a meta-methodology. I don't know what methodology to use? Then I begin with action research until I have enough understanding to make an informed choice of methodology.

11 Participation (1)

Speaking only for myself ...

That's how I usually start these occasional pieces: "speaking only for myself". I do so because that's how I prefer it. I offer this statement as a sign that I have no wish to persuade you to my point of view. I don't claim my view (thoughtfully held though I believe it usually is) as the "truth". My intention is to raise issues and invite you to think about them.

I think that "speaking only for myself" may have an additional purpose for this occasional piece. It seems to me that the issue of participation is influenced more by ideology than the pragmatism that informs much action research.

By labeling my views as just an opinion I may stir less resistance to thinking about the issues I mention.

I think that where participation in action research is concerned there is a "conventional mythology". It is that participation is somehow "better" than non-participation.

I have a strong preference for participative approaches. I do so both on ideological and pragmatic grounds. Ideological, because that's my preferred position. It's a personal ideology. Pragmatic because the action research is

intended to help people change. It makes pragmatic sense to involve them deeply and directly.

But I don't understand why participation should be a *necessity*. Why not match the extent of participation to the purpose of the research?

I have been more vehemently criticised for this view than for any other view I've offered about action research.

Another of my interests is evaluation. The evaluation methodology I use is action research. Almost always I do it participatively.

And I have been criticised occasionally for that. It appears that in some (not all) evaluation circles, the conventional mythology is that non-participation is "better" than participation.

Interesting? I think so.

Do you think it could be that we are guided by the labels "action research" or "evaluation"? Do we allow the labels to function as a substitute for thought?

12 Participation (2)

Speaking only for myself ...

In the previous occasional piece I mused upon participation, ideology, and the influence of labels.

I mentioned some contradictions I had noticed. That I had often been criticised for suggesting that action research might occasionally be less than participative. That I had sometimes been criticised for suggesting the evaluation might be at all participative.

To continue the theme, it seems to me that supporters of different varieties of action research hold different views. Just as the labels “action research” and “evaluation” sometimes direct our beliefs, so do those different varieties.

For example, I know some consultants who use action research in their organisational change work. Some of them offer more direct involvement to the senior managers than to others.

My own consulting work was influenced by the traditions of organisation development. There it was seen as important to involve as many as possible. And to this day, I still try to negotiate with the presenting client that I wish to represent all the stakeholders, not just some.

Some practitioners in this field regard themselves as favouring those in lowly positions over those in more senior positions. Similarly in the field of advocacy, the disadvantaged are well represented. The advantaged sometimes are not.

A further example. In classroom research, it is not all that unusual for the research not to involve the pupils. It is self-research, though there are other stakeholders.

I do some of this myself by reflecting on my own practice and drawing my own conclusions about what worked, what didn't, and why and why not. There isn't always an easy opportunity to involve anyone else.

The meaning of "participation", it seems, is at least to some extent in the eyes of the beholder.

And similar with extent and type of participation ...

Are we to involve stakeholders as informants? Yes, usually. How else are we to get access to the needed data. In data interpretation? Perhaps. In action planning? Often. In designing the research study? Perhaps, perhaps not.

I might add that I don't think there is a "best" form of participation in any absolute sense. Personally, I value high levels. I think most people who know me well would report that my educational work and my consulting work strive to achieve those high levels.

I think it would be presumptuous of me — and in a sense ironically non-participative — to assume that that is right for everyone else too.

13 Rigour (1)

Speaking only for myself ...

In some respects it seems to me that action research sits (sometimes a little uneasily) between two paradigms. On the one hand we find the mostly reductionist experimental and quasi-experimental methodologies. On the other there are the various qualitative methodologies.

On some occasions action researchers appear to be influenced by the current fashion in some of the qualitative methodologies. Part of this is to treat the experimental and allied approaches as dominant, and to be resisted. Resistance sometimes takes the form of arguing that qualitative researchers need not be bound by the criteria by which experimentalists judge their own work and that of their colleagues.

By and large I'm sympathetic to this. I've seen too many action research theses and studies criticised because they didn't sufficiently resemble good experimental designs.

Some of you can no doubt provide criticisms from your own experience. The criticisms are many ... The design was modified on the run. The researcher formed close relationships with informants. The initial research question wasn't precise enough. The variables weren't adequately defined. There wasn't a control group. And so on ...

I think there is reason enough to resist such criticisms as uninformed. Some of them, when you look further into it, might even be regarded as absurd. Let us acknowledge and respect the differences between ourselves and the experimentalists.

(This is in no sense intended as a criticism of experimentation. Only of their habit of thinking that theirs is the only way.)

Let's also acknowledge that action research is not synonymous with "qualitative" either. In particular, I sometimes wonder if we have been too much influenced by deconstructionism. And I wonder if that in turn has been too much influenced by its origins in textual and artistic criticism.

It is sometimes said that in textual analysis the reader places the meaning on the text. The text is just squiggles on a page until a reader fashions the meaning. I don't wish to argue with that. I think it may be an extreme view, but I'm not planning to take that up here.

Assume for the sake of discussion that it is true.

The point I wish to take up is that sometimes the relationship between text and meaning, so defined, is then said to exist between data and interpretation. Interpretation is to data as meaning is to text. In other words, data are equivalent to squiggles upon a page and with no inherent meaning. We devise the interpretation. And one person's interpretation is therefore as good as another's.

What do our data represent? People and objects and activities. The people have bodies and opinions and feelings. Most action research is intended to act upon that world of people and objects and activities.

I don't personally think we are free to impute whatever meaning we wish to our research situation. At least, not if we wish the subsequent actions of ourselves and our co-researchers or informants to achieve the outcomes we desire.

14 Rigour (2)

Speaking only for myself ...

I invite you to imagine this research situation. Your research setting is a community or organisation. You are talking with a member of that community or organisation. Your intention is to learn something about how the community or organisation works.

How likely are you to develop an understanding of the community or organisation?

In situations like this, I find the concepts of “validity” and “threats to validity” useful. In doing so, I envisage that many of the threats to validity reside in the gaps between experience and understanding.

(Yes, I realise there are arguments against using the language of positivism to discuss issues of rigour in qualitative research. Here I am speaking for myself.)

The gaps are numerous. For example:

- What the informant experienced is a subset (and probably a small subset) of what actually happened.
 - What the informant recalls is less (and sometimes much less) than the informant experienced.
 - What the informant is willing to report is less than the informant can recall.
 - What you hear is less than or different to what the informant reports.
 - What you understand may be less than or different to what you heard.
-

- The themes or interpretations you develop from informant reports are a subset of all you understood.
- What you succeed in reporting to others is less than or different to the themes or interpretations you developed.

I don't offer this as a complete list. All I have done so far is to consider a single researcher and a single informant. And I have ignored the possibilities that the informants may also be researchers. Further, there may be deliberate distortions at some of the gaps.

It seems to me that at each of these gaps I can act in ways which seek to reduce the omissions and misunderstandings. To consider just a few of the easiest of them ...

- I can begin the talk in a very open-ended way. The information I am given is then more likely to be determined by the priorities of my informant than by my questions and the preconceptions which suggested those questions.
- I can explain what use will be made of the information, and what access the informant will have to it. I can develop a reasonably close relationship with the informant and be clear about my own motives. In this way, I may build a more trusting relationship within which there will be less censorship.
- I can involve the informant in interpreting information as well as providing it. This may help protect me against some of the mistaken interpretations I might otherwise adopt.
- I can encourage the informant to provide examples, so I am less likely to be misled by abstract and ambiguous terms.

Some threats to validity are less easily avoided. In searching for ways of reducing these threats, I have found the work of Chris Argyris and his colleagues especially helpful. I think he is one of the few writers to acknowledge the extent to which the unspoken rules of interaction distort and filter the information we are willing to exchange with one another.

I expect to return to these topics, and the work of Argyris, in later occasional pieces.

15 Qualitative and quantitative

Speaking only for myself ...

(Which is to say, I'm not trying to argue for a point of view. I'm thinking aloud, so to speak. I don't much mind if you agree with me or not. I'll be delighted if you join the conversation.)

It was fashionable for a while (and still is in some circles) to rail against the mixing of qualitative and quantitative methods. The claim was that they are based on what Kuhn called incommensurable paradigms. Different philosophies.

I should acknowledge that there has also been a counter view, for instance in the work of Jick. Mixing methods is good: the prejudices of each cancel each other out to some extent.

My own view is that it isn't always methodologies that necessarily presume particular worldviews and ways of knowing. Often it is researchers who do so. If a researcher can find ways of using other methodologies and techniques within her or his own worldview, then why not?

And perhaps it is no longer an issue since the book of readings by Reichardt and Rallis. I think the papers there make a pretty good case for mixing qualitative and quantitative. But then, some find the arguments in such books as Egon Guba's *The paradigm dialog* convincing.

In any event, that isn't the thrust of my musings. I intend instead to offer four reasons why I don't plan to get very excited about the issue.

My first reason is a suspicion that most people don't let their espoused philosophy interfere much with their daily business of driving cars, eating breakfast and the like. Philosophising takes place at more rarified heights.

And why should they? If the best of philosophers can't agree then I'm happy to remain a fence-sitter. It seems to me that we can't depend upon a certain philosophy. Best, then, that we don't wait for one before trying to understand how the world works.

My second reason is to be found in a particular view of methods and techniques. I don't personally value any one methodology as a best way of doing research. I prefer to define what I want to achieve, and then work out some way of achieving it. I'd rather not limit my choices by drawing a philosophical boundary around some of them. I like to mix and match.

Third (and this is related) I like to reserve the right to change my mind. In the situations I work in, my understanding slowly grows with my experience in those situations. To me, it makes sense to design methods and techniques that make the most of that understanding.

And I find that some of those methods resemble those from within one paradigm. And sometimes another.

Fourth, I find it hard to view different philosophies as competitors. Different perspectives on the same reality perhaps. It seems to me that I can often improve understanding by deliberately moving between different perspectives.

To pick up what is a recurring theme for me, the map is not the territory. The view through a given window is not the only view or the whole view. I think one can as usefully apply this to philosophies and paradigms as to more limited models and theories.

16 Grounded theory (1)

Speaking only for myself ...

These days, “grounded theory” is usually taken to mean the approach of that name initiated by Glaser and Strauss. Or, more often, the later elaborations of Strauss and Corbin.

There is a sense, however, in which you might talk about theory grounded in data or experience. I think “grounded theory” is an appropriate term for that, too. It is in this more generic sense that I wish to talk about grounded theory here.

Action research is well placed to develop this form of grounded theory.

In my view, an important purpose of action research is the development of understanding and action. Understanding which derives from action. Understanding which in turn informs action. Within each “intend → act → review” cycle the understanding develops further. In each “act” the growing understanding is tested in action.

In this form, theory development observes well the intention of the earlier Glaser and Strauss work. Theory is developed progressively as the data are collected. It has local relevance and is practical.

Glaser, Barney G. and Strauss, Anselm L. (1967) *The discovery of grounded theory: strategies for qualitative research*. Chicago.: Aldine.

Strauss, Anselm, and Corbin, Juliet (1990) *Basics of qualitative research: grounded theory procedures and techniques*. Newbury Park: Sage.

17 Grounded theory (2)

Speaking only for myself ...

This is one of a sequence of occasional pieces on the style of “grounded theory” that action research might produce. In the previous piece I suggested that action research processes were good at developing understanding from experience. Theory grounded in experience. One might reasonably label this “grounded theory” too.

Within each action research cycle practice informs theory which in turn informs practice. It seems to me that this cyclic building block is able to offer many advantages.

If you look specifically at the way it develops theory, you may well decide that it isn't all that different from experimental research. Experiments take place in the context of the theoretical literature, which they add to or challenge. You might say that there, too, theory informs the practice of experimentation which modifies the theory.

In effect, each experiment is a cycle. One of the differences in action research is that the cycles are smaller. A given action research study contains many cycles. And they in turn may include other smaller cycles. One of the earlier occasional pieces addressed this: there are cycles within cycles.

There is another difference which also appeals to me as important. It is that the theories tend to be different in at least two respects.

First, and most obviously, the theories in action research tend to be about practice. They inform practice, thus supporting the “action”. They are immediately tested in practice, thus enhancing the validity of the “research”.

Second, the theories tend to be more general. They tend to deal with complex situations. They are expected to guide action. I think, therefore, that one appropriate form for them (as Argyris and Schön² have suggested) is:

In situation S, to produce outcomes O1, O2, ..., try actions A1, A2, ...

This form of theory clearly and directly attempts to inform action. It is immediately tested in action. In specifying the important features of the situation it also allows its generalisability to be tested in other similar settings.

2. See Chris Argyris and Donald A. Schön (1974) *Theory in practice: increasing professional effectiveness*. San Francisco, Ca.: Jossey-Bass.

18 Grounded theory (3)

Speaking only for myself ...

We've been exploring the notion that action research generates a form of grounded theory: theory grounded in experience. The previous "occasional piece" addressed a number of elements of this:

- the alternation between theory and practice
- the resemblance of each action research spiral to a sort of experiment, and
- the nature of the theory in addressing action.

Following Argyris, I suggested as a possible form of such a theory:

In situation S, to produce outcomes O1, O2, ..., try actions A1, A2, ...

In this occasional piece I'd like explore further some of the features of this form of grounded theory.

First, it seems to me that in this form, theory can to some extent integrate the local and the global. It emerges from the local, because it is often local evidence and understanding the builds it. Yet it is stated in general terms. It is framed in a way that allows it to be tried and tested in other settings.

Second, it can to some extent integrate the subjective and the objective. The participants invest the theories with their own values and meanings. At the

same time the theories gain objectivity in its most pragmatic sense by being tested against reality through action.

19 Grounded theory (4)

Speaking only for myself ...

I thought it might be useful to describe one of the ways in which I develop theory from experience. I use a set of questions to help me prepare for action, and a further set to analyse, later, what happened.

The questions asked beforehand³ are:

- 1a What do I think are the salient features of this situation?
- 1b Why do I think those are the salient features?
- 2a Given that situation, what do I think are the desirable outcomes?
- 2b Why do I think those are the desirable outcomes?
- 3a What actions do I think will achieve those outcomes in that situation?
- 3b Why do I think those actions will achieve those outcomes in that situation?

It will be seen that these derive two “theories”. One is defined by the “what” questions. It is in the form I’ve already discussed: In situation S, to achieve outcomes O(1), O(2), etc., try actions A(1), A(2), etc.” It captures, in action form, my assumptions about the action to follow.

The “why” questions help me to make explicit the assumptions which underlie the “what” questions. The resultant “theory” may take a variety of forms.

3. Some of this material emerged in conversation with Stephanie Chee, Alan Davies, Goh Moh Heng, Richard Kwok and Shankar Sankaran. It also builds on the “theories of action” postulated by Argyris and Schön.

Both “theories” are then tested in action. If the action achieves the desired outcomes, this provides some support for the “situation, outcomes, actions” theory, and for its underpinning assumptions. Any changes in actions or outcomes draw my attention to my incorrect assumptions.

In the critical reflection afterwards, these are an example of the questions which might be asked:

- 2a Were the outcomes achieved?
- 2b If so, now that I’ve got them, do I still want them?
- 2c Why / why not?

If not...

- 1a Was I mistaken about the situation?
 - 1b If so, in what respect?
 - 1c What led me to that mistake, and what have I learned from it?
 - 2d Was I mistaken about the desirable outcomes?
 - 2e If so, in what respect?
 - 2f What led me to that mistake, and what have I learned from it?
 - 3a Was I mistaken about the desirable actions?
 - 3b If so, in what respect?
 - 3c What led me to that mistake, and what have I learned from it?
 - 3d Did I produce the actions?
 - 3e If not, why not?
 - 3f What have I learned from that (about the situation, about the desirable outcomes, about the desirable actions, about systems, about people, about myself ...)?
-

20 Grounded theory (5)

Speaking only for myself ...

We've been exploring a form of grounded theory — theory grounded in experience — that suits action research. One form in which its outcomes might be stated has three terms: situations; desired outcomes; and intended actions:

In situation S, to produce outcomes O1, ..., try actions A1, ...

Situations are specified on the assumption that what works in one situation will not necessarily work in another. Outcomes are specified, for they are what is being pursued. In addition, they provide the criteria for defining success. I've said "try" to imply that this is one possible set of actions. They may or may not work. There may be other actions which would be as effective.

Intentions are built on assumptions. I assume these actions will give me the desired outcomes. Therefore I intend to try those actions.

In the previous occasional piece I added the thought that behind these assumptions lies a further set. This second set tends to be about people and systems and change. And (importantly) about the facilitator or action researcher.

Before moving on to other issues, I'd like now to address the way in which such a grounded approach to theory can integrate the subjective and the objective.

The subjective component arises because action research is usually participative. *People* develop the intended actions. The participants invest their intentions with their own values and meanings. Out of all possible actions they limit their intentions to those that fit their ethical framework. Even in less participative forms the assumptions and values and meanings of the researcher are brought into play.

The intentions of researcher and or participants are also likely to be unique to the individuals in another respect. The participants are most likely to intend feasible actions: actions which they believe they will be able to carry out. This is partly situational, partly a matter of their skills.

The objective component arises because their intentions are tested against reality in action. I'm not sure that objective is the right word. "Generalisable", perhaps. I take this point up in further occasional pieces.

In any event, success means that actions A (etc.) *did* produce outcomes O (etc.) in situation S, at least on this occasion. It is therefore possible (and sometimes even likely) that similar actions will yield similar outcomes in other similar situations. The actor can try these same actions on other similar occasions and note the results.

21 Generalisation (1)

Speaking only for myself ...

I touched on the issue of generalisation in the occasional pieces on grounded theory. There I made the point that there are advantages in expressing theories in terms of situation, desired outcomes, and intended actions. Very briefly:

in situation S (actions A → outcomes O)

I said that one reason for doing so was that participants might then experiment with using the same actions to produce similar outcomes in similar situations. When this has been done several times then participants may develop some confidence that their theory is generalisable, at least tentatively. This may occur several times within a single study, or across several studies.

The resulting generalisation may be peculiar to a particular person or group, of course. I don't do facilitation or consulting or action research in quite the same way as my colleagues. If I did I would be ignoring my own skills and strengths and experience.

Each person is different. Each situation is different. For that matter we seldom produce *precisely* the same sequence of actions on different occasions. I expect this is why it is commonly held that you can't generalise from action research.

But I think that is unnecessarily defeatist.

Suppose other people try similar actions in similar settings. And suppose they achieve similar outcomes. Then it may be argued that generalisability has been demonstrated.

Beyond that, I may also test my personal generalisations against the literature. I may note there where others have carried out similar actions, with similar results.

For these reasons I often avoid the relevant literature when I am experimenting with a new skill or a new process. I decide by trial and error what works for me.

I can then use the literature for several purposes. I can note the similarities and differences to my generalisations. I can refine my own actions. I can note which generalisations do tend to apply more generally.

22 Generalisation (2)

Speaking only for myself ...

A detour. I was about to address the commonly-held fiction that action research usually does not permit generalisation. In the meantime some interesting communications from Vic Wooddell and DP Dash have suggested that there are some other issues to address first.

In addressing the issue of generalisation in its various forms, and its relationship to action research, I think it is useful to differentiate two forms of action research. I might characterise them as “*action* research” and “*action research*”. I think they have different requirements for generalisation.

By “*action* research” I mean the form where action is obligatory, or at least the primary reason for conducting the activity. The research is desirable but can be jettisoned if necessary. Programs for community or organisational change are often like this. A client or client group is paying for certain results and expects to get them.

In situations like this the researcher will probably achieve some research outcomes. At the least, it is likely to take the form of an improved understanding of what was done. Beyond that, the client may or may not develop better understanding. The client may or may not have better understanding as an explicit goal.

Then there is “*action research*”. The research outcomes may well be crucial — for thesis purposes, for example. It may be good to achieve some action outcomes too. That may even be an explicit goal. But if necessary the action can be abandoned.

It seems to me that in *action* research generalisability may not be a great concern. There is likely to be some, at least for the researcher. Without generalisation each situation is so different that no learning can carry from one situation to another. However, the main emphasis is likely to be on learning how to deal with specific situations.

Now consider *action research*. Here, unless there is a contribution to knowledge, there *are* no research outcomes. If it is a thesis study the examiners may decide there is no thesis. Generalisability is a requirement.

23 Generalisation (3)

Speaking only for myself ...

We've been talking about generalisability. But what is it?

In experimental and quasi-experimental research I think it is relatively easy to define. Suppose the research identifies a relationship between two variables. Then generalisability is the extent to which that relationship applies to the same variables in other situations. In other words, the relationship can now be regarded as approaching universality.

But think a little more closely than that. How often, in fact, will the studied variables be the only ones operating in a given situation? If those *are* the only variables, then it seems to me that generalisability has real and useful meaning.

Otherwise, I have my doubts.

Now consider the situation which applies to action research and similar approaches. Very often it's hard to know what is true about the situation until it is studied.

For example, I may have learned that in a given situation, certain actions were followed by certain outcomes. As situations differ it would be rash of me to assume that the same actions will always be followed by the same outcomes. I assume this is why it is often said that you can't generalise from action research.

But consider the two situations. Let's say I've conducted an experiment in which I have demonstrated that a certain treatment resulted consistently in a given change in some variable. And let's say I've carried out an action research study in which certain actions were followed by certain outcomes.

The key question ... What happens when I now take those two sets of findings into a different situation? Can I have faith in either of them?

It seems to me that if it is a typically complex field situation involving people, the answer is a qualified "no".

In short, the generalisability which experiments provide does have some universality. But only when the researched variables are the only variables operating. In the field settings where action research is typically conducted, that is seldom likely to be true.

24 Varieties of action research (1)

Speaking only for myself ...

(Let me begin by saying that these occasional pieces are intended to be a starting point for discussion.)

Over the next several months I would like to explore some of the many varieties of action research and related methodologies. There are quite a few, and they seem to be multiplying.

Here, to set the scene as it were, I'd like to explore some of the features that most of them share.

For present purposes I'll speak of six such characteristics. Most of the varieties are ...

- change oriented; they intend to bring about some change in the person, or in a social system such as a community or organisation
 - related to the first, action oriented; the change is intended to be the result of action
 - in some sense of the term, data-based; the decisions about action are informed by information which is usually collected for that purpose
-

- emergent; they take shape slowly as they respond to the situation in which they are located and the information they collect
- cyclic; action is preceded by planning and followed by evaluation or review; further, most of them consist of cycles within cycles (within cycles...)
- participative; those who are affected by the intended change are most often involved in deciding which actions are to be taken.

My intention in this occasional piece is to offer two suggestions about how we might use these differences and these characteristics.

First, how might we think about these characteristics? I think it is most useful if we think of them not as obligatory, but as design choices.

It is my belief that sometimes it is true that some variety of action research best suits a given situation. And sometimes some quite different methodology is more likely to be appropriate.

Second, how might we view the many varieties? Here I prefer not to regard them as recipes to be followed. Rather, I think of them as examples of the design choices that may be taken and put into action.

I suspect that in the best of all possible worlds we wouldn't require these varieties. We would sufficiently understand what we were doing that we would design each approach from scratch, as it were, in response to the situation.

25 Grounded theory revisited (1)

Speaking only for myself ...

In piece 24 I foreshadowed a series of pieces on different versions of action research and related methodologies. My purpose is so that, through a comparison, we expand our choices of ways of doing effective research. I've now chosen "grounded theory" as the first of these methodologies.

You may recall that in an earlier sequence in this series (opium 16 to 20) I explored action research as a generator of theory which might be described as grounded.

From time to time I deliberately explore fields which are up until then new to me. Grounded theory was such a field.

It has long been my practice that at these time I initially mostly avoid the existing literature. I try to develop my own ideas first. This allows me to read more critically and with greater understanding when I do approach the literature. I revise my ideas when I read the literature.

Since the earlier sequence on grounded theory I have been exploring the literature. And there I find (particularly in Barney Glaser's work) some enticing parallels with action research.

(For example, he recommends not reading the relevant literature until later in a study.)

One of the key distinctions which Glaser makes is between hypothesis-testing and emergent methodologies. He sees most sociological research as really being hypothesis-testing, even when researchers might not perceive them as such. He sees them as *forcing* the data to fit the existing theories.

The alternative is to let the theory *emerge* from the data. This is what Glaser's form of grounded theory does.

And it is one of the important features it shares with action research. Both are emergent methodologies.

(I've posted a summary of my understanding of Glaser's form of grounded theory on the web at

<http://www.scu.edu.au/schools/gcm/ar/arp/grounded.html>)

26 Grounded theory revisited (2)

Speaking only for myself ...

In piece 25 I foreshadowed a discussion about grounded theory and its similarities to action research. In particular I raised Glaser's distinction between hypothesis-testing and emergent methodologies. I said that action research and grounded theory were emergent methodologies.

It is my intention in the next occasional piece to take the comparison further. Before doing so it is useful if I provide a thumbnail sketch of grounded theory, Glaser style.

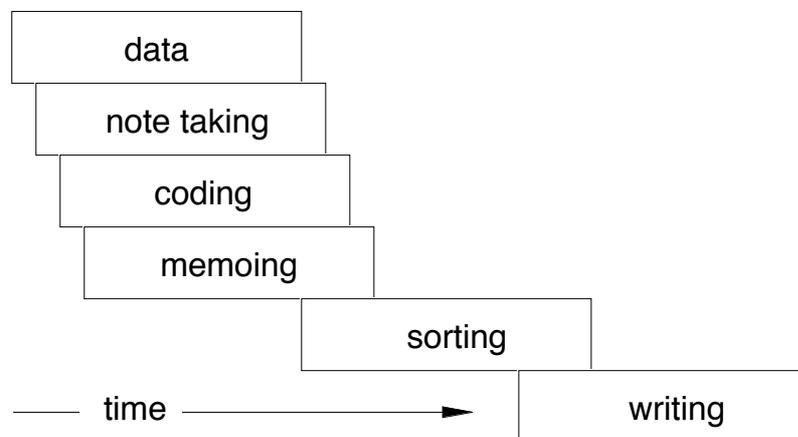
Grounded theory can be described as a set of overlapping stages:

- data collection, in which the researcher observes, talks to people, and engages in whatever other data collection seems appropriate
 - note-taking, capturing the key elements of each data collection immediately afterwards
 - coding, in which the researcher writes in the margin of the notes the *categories* and *properties* contained or implied by each sentence of the notes
 - memoing, in which the researcher writes memos to herself or himself on the theoretical hypotheses arising from the coding
-

These four stages are almost simultaneous. The memos progressively build the theory from the categories and properties of the coding, and the links between them. The data are noted and coded almost as they are collected.

Sorting begins as soon as further data add little to the emerging theory. The memos are sorted to an order which allows the theory to be communicated clearly. Writing the report is guided by the sorting.

This may be diagrammed as follows:



As I mentioned in the previous occasional piece, there is a slightly more detailed description of this process on the web at

<http://www.scu.edu.au/schools/gcm/ar/arp/grounded.html>

27 Grounded theory revisited (3)

Speaking only for myself ...

As you may have guessed from the last couple of occasional pieces, I've been reading some of the literature on grounded theory. In observing its similarities to action research I think I've come to appreciate more the emergent nature of action research. I've broadened my understanding of some of the ways in which that emergence can be achieved.

I've been further helped in this by reading some of Peter Checkland's and Sue Holwell's book *Information, systems and information systems: making sense of the field* (Chichester, UK: Wiley, 1998). This book more firmly places soft systems methodology (Checkland's methodology) within an action research paradigm.

I don't know if Checkland explained it more clearly this time. Perhaps I was more ready for it. Checkland and Holwell describe research in a way which makes a lot of sense both for grounded theory (the form described by Glaser and Strauss) and action research. According to them, in research one uses ...

a[n epistemological] framework..... **F**
 operationalised as a methodology..... **M**
 to investigate an area of concern **A**

in the course of which the researcher learns about *and may modify* each of **F**, **M**, and **A**.

To be honest, in the past I have been a bit cavalier about epistemology. I didn't think we could wait for the philosophers to agree before we decided how to do research.

For that matter, I'm not yet persuaded it counts for all that much. On balance I'm probably somewhere between a pragmatist and an agnostic, philosophically speaking. I think about my philosophical assumptions. (And about my ethical assumptions too.) I think, though, that I would do action research in pretty much the same way whatever philosophical hat I was wearing.

But now I can change my mind as I proceed.. With this extra flexibility, I find that the notion of starting with at least a few explicit philosophical assumptions appeals.

The preceding paragraphs are by way of background. I can now consider the emergent nature of action research and grounded theory as having three dimensions: epistemology, process, and situation. I'm thinking of these as equating respectively to Checkland's **F**, **M** and **A**.

How might I apply this? I can try to spell out my assumptions about all three before I act. I can check my experience against those assumptions during and especially after action. As I accumulate experience I can seek out disconfirming evidence for assumptions under all three headings.

Emergent understanding of philosophy. Emergent understanding of research processes. Emergent understanding of the situation.

That appeals to the empiricist in me.

28 Grounded theory revisited (4)

Speaking only for myself ...

My initial reasons for exploring the grounded theory literature were threefold. I try to choose some of my reading so that it takes me into unfamiliar territory. I supervise some theses where people want to be more open to the situation they are researching. And I thought I might make more explicit my understanding of action research by exploring a different methodology.

It is the third of these which now occupies my attention. I was surprised by the extent of the parallels between action research and grounded theory. As a consequence I think I now have clearer ways of explaining some aspects of action research.

Let me mention a few of them.

First, the distinction between emergent and hypothesis-testing research. Or, to use the terms I prefer (as more transparent): data-driven and theory-driven research.

I had formerly thought of much qualitative research as essentially data-driven. I have followed some of the debate between the recent Glaser and Strauss. It has led to a realisation that much qualitative research is less open to the situation than I would previously have imagined.

In other words, there is a theoretical tradition which is taken more or less as given. The researcher uses it to make sense of the data. Qualitative research may

not be as explicitly focussed on specific hypotheses as experimental research. But there are hypotheses, often implicit, which place boundaries on what one can understand of a situation.

(In case you are wondering, I do accept that we bring all sorts of presuppositions to our research. There are theories and concepts and assumptions which colour our perceptions. I think this is potentially true whatever the variety of research we engage in. However, I think there are effective ways of protecting oneself against allowing these assumptions to colour the interpretations we develop.)

Second, what is the place of literature in research? I have argued for some time that in some forms of research it didn't make sense to read the relevant literature beforehand. (This was before I realised that Glaser makes the same argument in the grounded theory literature.) In much of the practitioner work I do, I often do not know enough to judge which literature will be found to be relevant.

So I encourage thesis candidates I supervise to regard literature as something to which they can compare emerging explanations.

Grounded theory encourages the treatment of literature as data: as just more data, with similar status to the data collected in other ways during a study. This will help candidates to provide additional justification for not engaging in a traditional literature review, I think.

Third, there is a strong emphasis in grounded theory on the *simultaneous* collection and interpretation of data. This better allows the data to guide interpretation *and methodology*. There is a clear parallel here to the tight spiral of action research, with the same advantages.

In all, I find it confirming of action research methodology that a different methodology, developed for different purposes, demonstrates such a clear parallel to action research processes.
