

3

The nature of knowledge

in which I outline briefly my approach to philosophy and then describe the style of theory which underpins much of the practical work described in this document; I explain how it differs from some common conceptions of theory

This chapter marks the transition from the background material of chapters 1 and 2 to the description of my own work in chapters 4 to 7.

It shares features of both. In the interests of making explicit some of the assumptions of my work it briefly describes my views of reality and theory. It also introduces some of my own work in the form of four papers on the nature of theory.

The emphasis of the chapter and of the papers is on practitioner theories: their nature, how they can be related to other theories, and how they can be used to improve practice.

Peter Checkland (1992) describes research as a framework **F**, operationalised as a methodology **M**, used to investigate an area of concern **A**. (See also Checkland & Holwell, 1998.) In the course of the investigation the researcher learns about *and modifies* **F**, **M** and **A**.

Checkland's framework appeals to me on at least two grounds. First, Checkland clearly has in mind an *emergent* approach. His expectation is that researchers are open to changing their minds about the underpinning philosophy and the methodology. Second, it is action oriented. The "area of concern", the research situation, is subject to change as a result of the research. I make an additional distinction between methodology (for instance soft systems methodology) and processes (the fine grain processes for collecting and interpreting information, for example).

Checkland seems to have in mind an *epistemological* framework, for he talks about frameworks which specify the nature of knowledge. So to sum up, for me research is

an epistemological framework**F**
 operationalised as a methodology**M**
 further realised as a set of processes**P**
 used to investigate an area of concern **A**

in the course of which one learns about and modifies **F**, **M**, **P** and **A**.

It is time to make use of this by being more explicit about my epistemology.

I am here interested in providing a context in which the discussion in later chapters will be more easily understood. In particular, I wish to say a little about my assumptions about the nature of the world (ontology) and our knowledge of it (epistemology).

The nature of the “real world”

This section will be relatively brief. Most of the literature I read has an influence on my practice and the way I think about it. My reading of philosophy, on the other hand, has had very little effect. I have found little reason to change my practice because of anything I have read in the philosophical literature.

I offer this as evidence that, in my own practice, philosophy has not proven particularly relevant. I shall have something to say about the nature of knowledge and of theory shortly. Where “reality” is concerned I have little to say. My ontological assumptions include the following.

I assume that there is a “real world” and that it is “out there”. Any other position, I think, would not fit well with a methodology which sets out deliberately to help people change that world. (However, see below.)

If I had to label my philosophical position as it appears to others it would be roughly that of pragmatism. I use theories publicly in my work not for themselves. I use them when they aid action or understanding.

Stated coarsely, from this perspective truth is what works. In this respect I claim to be in good company. The philosopher Richard Rorty (1999b: 122) has said:

Scientists who agree with Kuhn are not about to do anything very different from what their colleagues who agree with Weinberg¹ do. Their disagreements come up only in after-hours chat, not during the daily grind in the lab. [...] In neither science nor politics is philosophical

correctness, any more than theological correctness, a requirement for useful work.

I regard my work as belonging approximately in the tradition of the action research begun by Kurt Lewin (for instance 1946), built in turn upon the work of John Dewey (for instance 1933). Richard Rorty (for instance 1999a) might be regarded as a modern representative of this philosophical tradition.

My private position is a little more complex. I accept that I can know the world only indirectly and imperfectly. When it comes to the physical world my knowledge is nevertheless adequate for me to survive day to day and year to year. I assume that our sensory apparatus including the brain has been honed by millenia of evolution.

I accept that for the less tangible aspects of the world — the thoughts and motives of people, for instance — my perceptions are even less accurate. Even here, though, they have so far proved adequate for me to survive for 70 years.

Let me add that I am not entirely a constructivist. For instance, people's assumptions about other people exist in people's minds. They are a construction. When expressed in behaviour, however, they do have tangible and more-or-less predictable effects. The "self-fulfilling prophecy", identified by Robert Rosenthal (1966) in experimenters' laboratories attests to this.

To say this differently, certain beliefs have consequences in the world. Suppose I held certain stereotypic views of (say) migrants. Those views would be likely to colour my language when I talked about migrants and my behaviour in their presence. In turn, my language and my behaviour might influence other people.

1. The reference, I presume, is to Steven Weinberg, whose realism Rorty has elsewhere critiqued. Weinberg is a theoretical physicist at the University of Texas at Austin, Nobel Laureate and author of *The first three minutes*, an account of the (hypothesised) early minutes of the universe.

I also agree with the proposition that there are qualities or properties of complex systems which cannot be predicted from their constituent parts. That is, *emergence* exists. In understanding the whole it helps to understand the parts. But by themselves the parts are not enough.

The philosophical position which seems closest to the one I have described is Roy Bhaskar's "critical realism".² It accepts both that the world is real and that it cannot be completely known. Its view of the world is of a hierarchy with nested levels — physical, chemical, biochemical, biological, social and so on.

Critical realism assumes that higher levels are constrained by but not completely predictable from their constituent levels. That is, critical realism is not reductionist. It also assumes that the higher levels can affect the lower levels. As Collier (1994:47) puts it

To explain what happens to stones in the garden, one must know about the habits of ants; to explain the damage to the ozone layer, one must know about the laws of economics.

Though I have read some of Bhaskar's work, my understanding of critical realism is based mostly on Andrew Collier's reasonably accessible introduction (Collier,1994). I find much of Bhaskar's writing too obtuse to repay the effort it requires for me to understand it.

In short, with one exception the philosophical nature of reality is not an issue I find very relevant to my work or my thinking. The one exception relates to the nature of theory. While it does not much influence my practice it does affect how I think about that practice and how I talk about it to others. These activities —

2. "Critical realism" is the label which others have applied to it, and the label by which it is generally known. Bhaskar's (1978) own label for it is "transcendental realism".

thinking and talking — depend on some model or map of the world, some theory.

The nature of theory

My earlier thinking is shown in an unpublished paper “A model of models”, written in the mid 1970s for a departmental seminar. (See the brief summary below.)

Paper 1 — A model of models ³

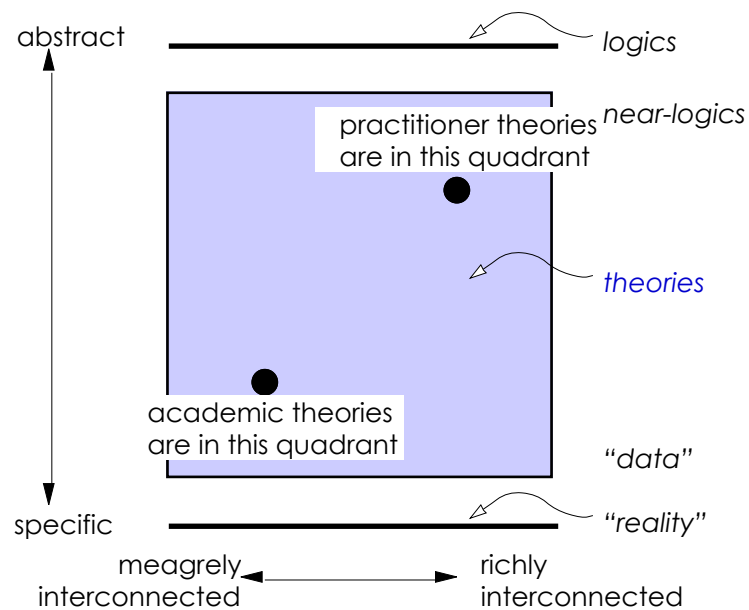
Bob Dick (nd) *A model of models*. An unpublished background paper for a departmental seminar. St Lucia: University of Queensland, Department of Psychology.

The paper identifies two dimensions on which psychological models vary: abstractness, and complexity. It argues that practitioners, to deal with the world as it is, must be able to carry their models around in their heads. To keep the theory small enough to be portable they most often work with theories which are abstract and complex. The abstraction is required to compensate for the complexity. Academics on the other hand favour researchable models. In psychology their theories are in the opposite quadrant of the grid: specific and with relatively few variables.

The relationships suggested in that paper may be summarised in a diagram (based on the diagram in the paper) — See Fig 3.1.

As an example of the evolution of my thought I offer Paper 2. Maslow’s (e.g. 1978) hierarchy of needs, despite a lack of experimental support, is still popular

3. To spare the reader from having to refer to each of the actual papers I provide brief introductions to my own papers in boxes such as this. The papers themselves will be found in attached.



The heavy line at the bottom is reality, which is not directly knowable. There is an inevitable gap between it and our theories. The heavy line at the top represents logics, which are logically consistent systems like mathematics. Theories vary in terms of their specificity or abstractness (the vertical dimension in this diagram), and the complexity of their components and connections (the horizontal dimension). See text.

Fig. 3.1 Dimensions of theories and models

in practitioner circles. In the paper I argue that experimental test may not be the most valid challenge to a theory such as this. More abstract theories are far from the direct evidence, the “fact base” as West Churchman (1971) labels it. A failed test may be as much due to the particular operationalisation of the theory for experimental purposes as to error in the theory itself.

Paper 2 — Maslow revis(it)ed

Bob Dick (2001) Maslow revis(it)ed: Maslow’s hierarchy of needs examined and reformulated. A discussion paper for a seminar. St Lucia: University of Queensland, Department of Psychology. Originally written mid-1980s, revised 1990, 1993, 2001.

The paper examines Maslow's hierarchy. Two main strategies are used to examine the theory: a conceptual analysis, and a comparison to theories which claim to address similar phenomena. This is followed by a recommendation for modifying the theory.

The result is a theory which is conceptually cleaner and is more consistent with related theories. It also implies certain ways in which the theory might be applied in practice.

I argue in effect that if a theory is abstract and logically consistent it is more appropriate to talk about its *applicability* to a given situation than to its correctness. Conceptual analysis may therefore be more relevant. In the "Maslow revis(it)ed" paper the conceptual analysis results in a recasting of the theory. In its modified form it is more consistent with other theories which attempt to cover the same ground.

To the extent that it is true, Maslow's hierarchy has a wide boundary of application. It is intended to apply to most of humankind. In its modified form it agrees with a range of theories which partly overlap it. I take this as support for its wide applicability. It is a theory which deals with concepts which can be used in a wide variety of situations. I will use the term "strategic concepts" for such concepts and "strategic theories" for such theories.

Other examples of strategic theories are Leon Festinger's (1957) cognitive dissonance (1957), and Robert Blake and Jane Mouton's managerial grid (1974). Each of these has strong similarities to a number of other theories. Cognitive dissonance resembles a range of the theories known as balance theories, most notably the earlier theories of Fritz Heider (1958).

The two dimensions of the managerial grid are to be found in a number of other theories. The dimensions are *concern for task* and *concern for people*. Robert Bales' (1950) work on group process identified *task* and *social* dimensions. The Ohio

State Leadership Studies (Halpin and Winer, 1957) named similar dimensions *initiating structure* and *consideration*.

Paper 3 more closely captures my current view of theories. A paper still in draft form, it more clearly identifies the differences between experimental and practitioner theories. Based on this foundation it offers a strategy for relating academic and practitioner theories to one another.

Paper 3 — Practitioner theories

Bob Dick (2002) *Practitioner theories*. An unpublished draft discussion paper. Experimenters theories are mostly cast in the form of hypothesised relationships between a number of "independent variables" and one or more "dependent variables". Of more use to the practitioner are theories which identify which actions are likely to produce which outcomes in a given situation:

situation [actions → outcomes]

Outcomes can be related to dependent variables. Actions are not the same as independent variables. But actions can influence independent variables:

situation [actions → (independent variables) → outcomes]

Practitioners typically learn through experience, often tacitly, which actions tend to produce which outcomes. As they come to understand which independent variables are involved they are also able to relate their theories to those of experimentalists.

A further implication if this account has merit is that for practitioners, theory is more often derived from practice than practice from theory.

Reflection

Reflecting now on these earlier papers I have further comments to make about the varieties of theory and about their use by practitioners.

Forms of theory

I would now distinguish theories (like Maslow's hierarchy) which link high-order concepts from theories of action such as those proposed by Argyris and Schön (1974).

The former may *imply* certain actions to achieve certain outcomes. For example the revised hierarchy in Paper 02: *Maslow revis(it)ed* identifies unconditional self-esteem as more robust than conditional self-esteem. It proposes that the esteem of others can be one of the pathways to self-esteem. This implies that to increase a person's robust self-esteem one might provide unconditional support to that person in lieu of conditional support. Some theories of this type may be converted easily to theories of action.

Theories of action, on the other hand, *directly* specify what actions might be expected to produce certain outcomes. For example, Argyris's Model I specifies that actors who unilaterally design and manage the situation will be seen as defensive. Their behaviour will lead to self-fulfilling ("self-sealing") prophecies (Argyris and Schön, 1996:93).

It is usually held in academic psychology (and, I suspect, in some other professions) that practice can only be built on a prior understanding of theory. I have concluded (in Paper 03: *Practitioner theories*) that in fact practice more often *precedes* this type of theory. This position is consistent with the views of Argyris and Schön (1974) on practitioner theories and Schön (1983) on the way practitioners learn.

Dennis Howitt (1991) reaches similar conclusions in his book subtitled "psychology applied to social issues". Psychology mostly isn't so applied. My own experience is that my practice has very often preceded my theorising. The learning processes described in the following chapter are consistent with this conclusion.

If this is so it has implications for the current emphasis on evidence-based practice. When practice leads theory, practice-based evidence is also required. Among other writers on the topic, Scott Miller and his colleagues reach similar conclusions for clinical psychology (Miller, Duncan and Hubble, 2003).

It is inevitable, I think, that much experimental behavioural science will research what is researchable. This implies the use of theories close to the fact-base, with a limited number of specifiable and measurable independent variables. Such theories clearly add to knowledge. They are not easily converted to action. When those who do the research are not those who apply it the separation is further enlarged.

Presently, experimentalist theories and practitioner theories are seldom mapped on to one another. If they were, there could be useful cross-fertilisation of benefit to experimentalists and practitioners alike. I suspect there is more motivation for practitioners to do so than for experimentalists. In “Practitioner theories” (paper 03, above) I offer a way in which this might be done.

Practitioner theories

Consider a practitioner during a consulting or facilitation activity. She⁴ may decide that some particular outcome is to be desired. Note that at this point in time the outcome is hypothetical. Her task is then to decide which actions might achieve those outcomes.

In fact, with one important difference the task bears a close similarity to what the philosopher C.S. Peirce (1940) called “abduction” or sometimes “retroduction”. Given an observed outcome not completely understood, abduction is the process by which a person decides how it might have come about.

4. Unless the context dictates otherwise, feminine pronouns can be regarded as applying to both male and female.

Fred and Merrelyn Emery (1997) apply James Gibson's concept of ecological learning — that we've evolved to make sense of experience — to their analysis of abduction. The Emerys have argued that everyone does research, in that they make sense of their experience. To do so they use a process of abduction, explained further below.

As Tomis Kapitan (2000) describes it in the *Digital encyclopedia of Charles S. Peirce*, the logic of abduction is as follows:

- (1) The surprising fact, **C**, is observed;
- (2) If **H** were true, **C** would be a matter of course.
Hence,
- (3) There is reason to suspect that **H** is true.

In abduction as Peirce describes it an actual outcome is observed. In facilitation or consultancy a *desired* outcome is *imagined*. The task of the facilitator is then to identify what actions seem most likely to produce those outcomes.

Applying Peirce's logic to the design of an intervention by a facilitator or consultant, we obtain:

- (1) In situation **S** the non-present outcome **O** is desired;
- (2) If action **A** were done in this situation, based on past experience **O** seems likely to occur.
Hence,
- (3) There is reason to believe that **A** is an appropriate action in this situation.

One might call this *prospective abduction*: abduction before the event. Applied mindfully (as Valerie Bentz and Jeremy Shapiro, 1998, might say) it would help the practitioner be more aware of the assumptions that guide her actions. She

might then be more likely to notice any evidence which violates those assumptions.

Other authors have recognised the applicability of abduction to change work. For instance Miri Levin-Rozalis (2000) has recently made similar points about the relevance of abduction to the field of evaluation. Fred and Merrilyn Emery (1997) in the article mentioned above have applied it to change-oriented research on human systems (see also Strand, 2005). Brian Haig (1995) has discussed its application to grounded theory, which like action research is an emergent⁵ methodology.

It was Peirce's belief that abduction was the source of creativity in science. It seems clear that devising an intervention is also a creative act. There is some virtue, I think, in making more explicit the often-tacit planning that precedes action.

Making assumptions explicit

Paper 04: *Questions for reflection* describes in some detail a set of questions which may assist a practitioner in identifying assumptions before acting. Abduction is then more likely to guide the practitioners decisions.

Paper 04: Questions for reflection

Bob Dick (2002) *Questions for reflection*. A resource paper for a public on line course in action research. Available on line at <http://www.scu.edu.au/schools/gcm/ar/arp/reflques.html>

-
5. That is, one where theory emerges slowly and gradually as data are collected and interpreted.
-

The paper describes two sets of reflective questions. One set is for debriefing after action. The other set, more pertinent to the present discussion, is for reflection before action. The questions are consistent with Argyris's theory of action.

You will recall that Argyris's theory of action required the specification of the situation, the desired outcomes, and the actions to achieve the outcomes. (In its full form it also required specification of any relevant assumptions about the situation, the actions and the outcomes: Argyris and Schön, 1974: 29.)

Paper 04 list three pairs of questions. The first in each pair requires the intending facilitator to specify in turn situation, outcomes and action, in that order:

- 1a What do I think are the *salient features of the situation* that I face?
- 2a If I am correct about the situation, what *outcomes* do I believe are desirable?
- 3a If I am correct about the situation and the desirability of the outcomes, what *actions* do I think will give me the outcomes?

The order is logical. Understanding the situation allows a better choice of outcomes. It is hard to identify appropriate actions until it is known what outcome is wanted.

A second "why" question in each pair invites the facilitator to identify the reasons for her answer to the first in that pair. That is, it surfaces the relevant assumptions, often making explicit what would otherwise be tacit.

The complete set of six questions then is:

- 1a What do I think are the *salient features of the situation* that I face?
 - 1b Why do I think those are the salient features? What evidence do I have for this belief?
-

- 2a If I am correct about the situation, what *outcomes* do I believe are desirable?
- 2b Why do I think those outcomes are desirable in that situation?
- 3a If I am correct about the situation and the desirability of the outcomes, what *actions* do I think will give me the outcomes?
- 3b Why do I think those actions will deliver those outcomes in that situation?

My experience in supervising action research theses is that this form of planning before action leads to better observation during action and better reflection afterwards.

In partial summary, then, I have maintained the following ...

- For much practice, and especially the more fluid varieties, practice develops through experience. Theory is often implicit, and if explicit tends to come later. Contrary to some views, practice often leads theory.
 - In any event, many of the theories practitioners learn during academic training do not translate easily into practice. The theories specify relationships between “variables”. Practitioners, on the other hand, wish to know what actions will yield which outcomes. Experimentalist theories and practitioner theories often differ importantly.
 - There are potential advantages if experimentalist and practitioner theories can be related to each other. This can often be done by identifying the variables by which actions influence outcomes.
 - There are questions which a practitioner can use before the event to make implicit assumptions explicit. These assumptions will then often identify the variables which exist between the actions and the outcomes. Reflection *before* the event can enhance reflection during and after the event.
-
-

There is one more point I wish to make before identifying the contributions of this chapter to the overall thesis — that facilitation is often more effective when the facilitator *overdetermines* the desired outcomes.

Overdetermination

I have maintained that explicitly or (probably more often) implicitly, facilitators use an abduction-like process to determine which actions to carry out. Don Schön (1983) has argued that professional practice is partly an artistic endeavour — a performing art, one might say, as I mentioned earlier. Part of the reason for this is that few situations repeat themselves exactly. There are different participants, or the same participants after the passage of time, or a different environment. Other events have intervened. Often the facilitator cannot be certain that actions which in the past produced certain outcomes will do so on this occasion.

For example, imagine that a facilitator wishes to give an important set of instructions. From previous experience she may have learned that it helps to get the attention of participants. She may also know that providing the instructions both verbally and visually is beneficial. Providing an opportunity for people to ask questions for clarification also helps. If she carries out all of these actions (and perhaps others as well) then she is more likely to achieve the outcome of better attention by participants to the instructions.

Symbolically, if

$$\mathbf{a}_1 \rightsquigarrow \mathbf{O} \text{ and } \mathbf{a}_2 \rightsquigarrow \mathbf{O} \text{ and } \mathbf{a}_3 \rightsquigarrow \mathbf{O}$$

where \mathbf{a}_1 , \mathbf{a}_2 , \mathbf{a}_3 are actions, “ \rightsquigarrow ” indicates “tends to”, and \mathbf{O} is the desired outcome then \mathbf{a}_1 , \mathbf{a}_2 and \mathbf{a}_3 in combination increase the likelihood of achieving \mathbf{O} .

From an experimentalist's viewpoint overdetermination might appear to hinder learning from experience. How can you tell which actions produced the outcomes? Yet clearly practitioners do learn how to act. In addition to those writers already mentioned, several writers who are practitioner-academics agree. I include here Daniel Fishman (1999) and Peter Jarvis (1999). Experience does count even when most of the learning is tacit. When practice precedes or accompanies theory the important question is this: how can the learning of the practitioner be further enhanced by making learning more explicit?

The time has now come to relate the material in this chapter to the theme of the thesis, the design and facilitation of robust processes.

Contributions to process

Novice cooks follow recipes. If an ingredient is missing or unsuitable they are often unable to use the recipe. Experienced chefs can instead seek out the best ingredients available, and modify the recipe (or invent one) to suit. Facilitation too has its recipes.

Novice facilitators

Some novice facilitators learn particular facilitation processes by observing more experienced colleagues. Others learn by reading "recipes" in books. In both instances they are likely to stay close to what they have learned. Their understanding isn't sufficient to allow them to modify the recipes.

In effect, they are working fairly mechanically, as technicians. As they develop more experience the amount of artistry in their facilitation is likely to increase.

The more explicit their understanding of the processes they use, the faster this process is likely to be.

Theory can assist the novice's learning if it is in a form that translates easily into action. I have earlier identified a form of theory that I think is suitable for this purpose. It consists of action theories (Argyris and Schön, 1974) and other general theories where concepts relate more or less directly to action.

Much of my own working time in the past 30 years has been spent helping novices learn to facilitate process. Some of the attached documents and monographs contain descriptions of robust processes — processes which perform well in inexperienced hands. To help novices learn more effectively, in most of that documentation I try to provide a rationale for each description.

In recent years I have had more opportunity to provide coaching for experienced facilitators. For them, too, explanatory theory is helpful.

Experienced facilitators

As the earlier accounts imply, experienced facilitators are more likely to respond intuitively to a situation. They are likely to draw on theory (or understanding, if you prefer) which is often implicit. Donald Schön (1987) has said much the same.

When actions are successful this is fine. Nevertheless, there are some tendencies which threaten success. Gary Klein (1997) has shown that more experienced experts have a tendency to seize upon the first solution that comes to mind. As David Snowden (2002) has argued the result can be "entrainment", a reluctance to admit that the solution hasn't worked or that the intuitive understanding is wrong.

If the expert is conscious of the assumptions acted on, there is more chance that those assumptions can be challenged by a lack of success. As I shall later claim,

this is most likely to be true when the expert seeks out disconfirming evidence. The six reflective questions above help the experienced practitioner surface assumptions. It is then more likely that it will be noticed when they are not supported by what actually happens.

This chapter provides a useful background to a consideration of some of the processes that practitioners use. Those processes are considered in the following three chapters. Chapter 4 examines learning processes.
