Chapter 6: Methodology

6.1 Introduction

This chapter both describes and defends the methodology chosen for this thesis. It investigates the possible ways of solving the research question, by investigating the means by which other research methods have been developed – but found no methods that had been systematically developed. Next, it describes the chosen set of methods, the reasons for making the choice of action research, and the particular type of action research found most relevant. The chapter concludes with a brief section identifying the limitations of the evaluation method and the provisions for rigour incorporated in this research.

Because of the central research question, this thesis is unusual in that it involves two interwoven studies: not only developing the scenario network technique, but also evaluating the success of the development work. Therefore, in principle, two methodologies are involved: the development method and the evaluation methodology. Initially, it seemed self-evident that the most appropriate way to develop a method was to work through a number of widely varying cases, to discover which components worked well, which needed improvement, and what interactions might exist between the different components.

Thus from one point of view, the methodology was developed iteratively, and therefore the methodology used for development should be the same as the methodology being developed; a solution referred to in the computer systems environment as “bootstrapping.” But on the other hand, simply developing the methodology may not be sufficient for a thesis, because there is also a need to justify and instrument the development process itself. Nor was it feasible to separate the development and the evaluation, because they both concerned one process, with one group of people (for each case). In summary, the only feasible solution was to use some form of action research.

6.2 The choice of a development method

This section explores the literature of social research in an attempt to discover criteria for a useful method for developing a methodology.
6.2.1 How other social inquiry methods have been developed

The first issue was how to go about developing the proposed method: not quite a research method, nor exactly a planning method, but a hybrid of the two: the term “social inquiry” (used in the broad sense of Dewey, 1938/1986, chapter 24) covers it well. I began by reviewing the literature to determine how other social inquiry methods had been developed. When setting out on this thesis project, I assumed that many other people would have written a thesis in which some type of social inquiry process was developed. My intention was to find how others had developed inquiry methods for their doctoral theses, and choose the most appropriate of these for developing the Process.

However, after a lengthy search, I found only one relevant thesis: that of David Cooperrider (1986), the developer of Appreciative Inquiry1. Cooperrider did not set out with the intention of developing a research process. His original intention was to evaluate the effectiveness of management in a large hospital, using action research. However, being highly impressed with the hospital’s collegial management system, he developed the germ of the Appreciative Inquiry method (which is now widely used in OD work). Since he had no initial intention of developing a process, the development process itself was not described in his thesis. In fact, the first 61 pages make no mention of appreciative inquiry; they are mainly about action research. Thus the sudden appearance of the term “appreciative inquiry” on page 62 caused me to turn back and check that no pages had been omitted. A single case (management of the hospital) was used in the development. Later writers on Appreciative Inquiry (e.g. Elliott, 1999) developed the method further by exercising it in a variety of situations, particularly in organizations and communities in developing countries.

Since Cooperrider’s thesis did not record the methodological development process in detail, I extended my literature search to include other work by social scientists, published as books or articles – but preferably as books, because of the detail I hoped to find. My search sequence, in descending order of relevance was on (1) futures studies methods; (2) qualitative methods of social inquiry, and finally (3) quantitative methods. Choosing well-known and widely used methods in each case, the specific literature searches were as shown in the following table (which, for completeness, includes Cooperrider’s work). For each method I identified a early and thorough reference, which could have been expected to have included citations to the development of the method.

1. Almost when this thesis was completed, I discovered the thesis of Juanita Brown (2001), in which she developed the World Café method, seemingly without varying it from its initial form, but with a prior gestation period of around five years’ work in developing methods for facilitating dialogue.
### Table 6.1 Tracing the development of social inquiry methods

<table>
<thead>
<tr>
<th>Broad area</th>
<th>Technique</th>
<th>Originator / First reference</th>
<th>Major references</th>
</tr>
</thead>
<tbody>
<tr>
<td>Futures studies</td>
<td>Scenario planning</td>
<td>Kahn</td>
<td>Kahn (1961, 1965)</td>
</tr>
<tr>
<td></td>
<td>Delphi method</td>
<td>RAND, 1953</td>
<td>Linstone &amp; Turoff (1975)</td>
</tr>
<tr>
<td></td>
<td>Futures wheel</td>
<td>Glenn</td>
<td>Glenn (1972)</td>
</tr>
<tr>
<td>Qualitative inquiry</td>
<td>Focus groups</td>
<td>Merton</td>
<td>Merton &amp; Kendall (1946)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>Merton, Fiske &amp; Kendall (1956), Morrison (1998)</td>
</tr>
<tr>
<td></td>
<td>Action research</td>
<td>Lewin</td>
<td>Lewin (1946)</td>
</tr>
<tr>
<td></td>
<td>Grounded theory</td>
<td>Glaser &amp; Strauss</td>
<td>Glaser &amp; Strauss (1967)</td>
</tr>
<tr>
<td></td>
<td>Use cases (in ICT)</td>
<td>Jacobson</td>
<td>Jacobson (1992)</td>
</tr>
<tr>
<td></td>
<td>Appreciative Inquiry</td>
<td>Cooperrider</td>
<td>Cooperrider (1986)</td>
</tr>
<tr>
<td></td>
<td>Survey research</td>
<td>not recorded</td>
<td>Hennessy (1975)</td>
</tr>
<tr>
<td></td>
<td>Latin square design</td>
<td>Fisher</td>
<td>Box (1978)</td>
</tr>
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</table>

In brief, almost all of this research drew a blank. The original sources were often very scanty, when it came to describing the development of the method: seldom more than several paragraphs. As an example, the following section reviews attempts to unearth the original development of the focus group and of action research.

#### 6.2.1.1 Development of the focus group

Even for such an extensively documented method as focus groups, and even given the existence of two versions of a book by Merton, the acknowledged developer of the method (Merton & Kendall, 1946; Merton, Fiske & Kendall, 1956 and 1987) as well as a recent book on the history of the method (Morrison, 1998), the most detailed account I found of the initial development of focus groups occupied only a single page, recounting a 1941 meeting between Merton and Lazarsfeld. Also, this page was about focused interviews, which are not the same thing as focus groups, but from which focus groups were evidently derived. As with Cooperrider’s thesis (1986) it seemed that the origination of the focused interview was almost serendipitous: a discovery, as opposed to an invention.
Merton, Fiske and Kendall (1987) and Morrison (1998) describe Merton’s role in the origin of focus groups, as beginning with his attendance at a group workshop with Paul Lazarsfeld in 1941. Merton criticized the procedures, and Lazarsfeld invited him to “show us how the interview should be done.” Drawing on his experiences interviewing unemployed people in the 1930s for the WPA, Merton then developed the principles of focused interviewing He distinguishes between the “focussed interview” and the “focus group.” The 1956 rewrite of the manual by Merton, Fiske, and Kendall, and the paper by Merton and Kendall (1946) do not distinguish between the focused interview of an individual and a focused group interview. In fact Merton, Fiske, and Kendall (1956) devote no specific section to the group interview, but simply suggest that the methods for interviewing individuals can also be applied to groups of individuals. They do not discuss the contribution added by interaction between participants.

6.2.1.2 Development of action research

Though the founder of action research is generally acknowledged to be Kurt Lewin, in his 1946 paper, some writers have traced much earlier origins. For example, Hart and Bond (1995) acknowledge the work of Collier and others in the 1930s and early 1940s in the USA, and McKernan (1991) sees action research as having developed from the Science in Education movement in the USA in the late 19th century. However, Lewin’s 1946 paper was the first to use the term, though its focus was resolving problems in inter-group relations; action research is barely mentioned. From the context, it appears that the method was fairly new, but nothing is stated in that paper about how the method was developed, or by whom. The key contribution to action research in Lewin’s 1946 paper is the cyclic concept of planning, action and reflection.

6.2.1.3 Development of the consensus group technique

Given this surprising absence of documentation, I turned (as a last resort) to a qualitative method that I developed myself: the consensus group technique (List, 1997; List, 2001a; List and Metcalfe, 2004). This method, beginning in 1987, gradually evolved because:

(a) In my work as research manager for a broadcasting organization, I was expected to provide audience research data for many more regional radio stations than the research budget could afford.

(b) Many of the regional managers were interested in doing their own small-scale research.

(c) As their education (generally in journalism) had focused more on linguistic than mathematical skills, this indicated that qualitative methods might be more successful than surveys.
(d) However, they had neither the training in psychology nor the experience necessary to moderate and analyse focus groups effectively.

(e) Nor did they have skills in the use of software, except word processing.

(f) They had vested interests in finding that their work was successful.

Thus the need was for a social research method that would use the verbal skills of journalists, could be easily learned, could be done at low cost without special software, and would produce undistortable findings. My position as research manager enabled me to facilitate and coordinate the development of a method that fulfilled these criteria.

Development of the consensus group technique began in 1987 with some experiments combining elements of nominal groups (Delbecq, van de Ven, and Gustafson, 1975) and public meetings, as well as the “new interaction method” of Doyle and Straus (1993). Since part of my work involved qualitative research, I began to vary the moderation and analysis procedures of group discussions I conducted, to better meet the above criteria. Through trial and error, over a period of ten years, the consensus group method gradually emerged. I did not set out initially to create a new research method, originally regarding it as a variant of the focus group. It was not until the 1990s, after at least five years of sporadic development (and around 20 studies), that I realized how different this method had become from standard focus groups. The only development of the method involved noting problems of implementation, and using the next opportunity to try to overcome them. By 1997 the method had become stable enough that I could give it a name, outline it in a manual for broadcasters in developing countries (List, 1997), and train others in its use.

Because the method was developed for practical rather than academic purposes, and I was employed as a non-academic media researcher, no particular attention was paid to documenting the development process. To generalize, that process was:

1. Client has specific need.
2. Review existing research methods, finding none that seem meet the need.
3. Imagine a new method that might do so (based on knowledge and experience of using other methods).
4. Trial that new method.
5. Review the client’s need in the light of the outcome.
6. Reflect on possible ways of making the method more effective.
7. Continue to cycle between stages 4 and 6. If the method does not reach a threshold of perceived effectiveness, go back to step 3.
Step 5 (reviewing the client’s need) turned out to be crucial: it re-examines the function that the method was designed to fulfil. Even if the method does not fulfil the function originally perceived, the perception of need in a large organization is never clear-cut; often it is not initially evident that an expressed need has another underlying purpose. Therefore, the development cycle was not simply a matter of evaluating the new method against fixed criteria. Both the criteria and the method were continuously evolving; by varying the two factors together they were more able to fall into alignment – labelled the “Swiss cheese” effect by Reason (1990).

### 6.2.1.4 Outcome of the review of how other methods were developed

Some conclusions from the methods summarized in Table 6.1 are that:

1. Most of these methods seem to have been developed long before they were first documented.
2. Some of the methods seem not to have been developed intentionally. Rather, the developers realized after several iterations that an inquiry method was being formed.
3. Some of the developers seem to have begun with one or two central ideas, and developed the method from those ideas. Thus the focus group began with the ideas of a group interview and the focusing sequence; action research began with the planning-action-reflection cycle, and the consensus group technique began by combining focus group principles with the meeting procedures of Doyle and Straus (1993).
4. I found no evidence that any of these methods had been systematically developed from the start.

In general, the development seems often to have been opportunistic: a problem presented itself, a researcher improvised a solution based on one or two central ideas, found that it seemed to work, and eventually published it as a new method. Often, others took it up in a modified form, developing it further. However, the fact that the above methods were developed in something of a haphazard fashion is no reason why a more systematic approach would not be more productive – and perhaps enable more rapid development, to a point where the method was settled.

I eventually realized that my own experience in developing the consensus group method, far from being atypical (as I had previously thought), could in fact be the norm. Given the failure to find even one well-documented account of the development of a research method, the inescapable conclusion for the present Process was that I was very much on my own. In other words, my task was twofold:
developing the method of futures studies, and
developing a method for developing that method.

Following Chapter 5, which has covered the first of those two tasks, the second will now be considered.

### 6.2.2 Eight considerations for choice of a development method

The possible ways of developing a research technique can be categorized as a set of choices, of which eight were clearly relevant to the present study. The following table presents these choices, and summarizes the answers obtained. Justification for these answers is provided in the remainder of this section.

<table>
<thead>
<tr>
<th>Issue</th>
<th>Answer for SNM</th>
</tr>
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<tbody>
<tr>
<td>1. Does the development of the method require empirical data?</td>
<td>Yes</td>
</tr>
<tr>
<td>2. If so, can the developer rely on existing (secondary) data – or must primary data be collected?</td>
<td>Mainly primary, some secondary</td>
</tr>
<tr>
<td>3. If empirical data is necessary, is a “gold standard” quantitative method (such as a controlled experiment) feasible?</td>
<td>No</td>
</tr>
<tr>
<td>4. Should a summative or formative approach be used?</td>
<td>Formative</td>
</tr>
<tr>
<td>5. Are formal hypotheses appropriate in these circumstances?</td>
<td>No: use evidence-based approach</td>
</tr>
<tr>
<td>6. Should the method be developed in a single study, or multiple studies?</td>
<td>Multiple cases</td>
</tr>
<tr>
<td>7. If multiple studies are used, should they be sequential or simultaneous?</td>
<td>Sequential</td>
</tr>
<tr>
<td>8. If a quantitative method is not feasible, which qualitative method (or combination) is most appropriate?</td>
<td>A form of action research</td>
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</tbody>
</table>

The following diagram presents the above sequence of decisions in graphical form. For the sake of clarity, the process is shown as a sequence, though in practice some of the decisions had to be made simultaneously.
Figure 6.1 Sequence of Methodological Decisions
Issue 1. Use empirical data?

The first issue to address was whether the research question was one that required empirical data, or whether it could be resolved theoretically. Alvesson and Sköldberg (2000) discuss this question, pointing out that empirics are not always necessary for the development of a theory.

Though it would be possible to develop a method purely “on paper,” any method derived solely from introspective sources (not necessarily from a single person) would run a serious risk of being found deficient when put into practice, on the first occasion it encountered an unexpected set of circumstances. After working for some years in media research, my experience was that it is never possible to predict exactly how a study will turn out – either in terms of findings or of methodological issues. Thus it seemed likely that a “thought experiment” approach (Horowitz and Massey, 1991; McAllister, 1996) would be likely to miss important issues and might not uncover some important problems. Also, because the focus of this Process was to be anticipating the future, and the future is notoriously unpredictable, it seemed particularly unwise to try to develop a method without using empirical data. Therefore an empirical approach was chosen.

However, a method cannot be developed using only empirical data, which has no intrinsic purpose. To anticipate a later section of this chapter, an action research approach was chosen because of its cycling between the empirical data and the conceptual framework set out in chapter 4. By beginning with extensive reading, not rushing into the fieldwork phase of the research, discussing my ideas with other futurists, presenting several preliminary papers, and taking part in email discussion groups about the future, I was able to improve the planned technique before beginning the fieldwork.

Issue 2. Collect primary data?

Given that empirical data was necessary, the question arose as to whether it was possible to rely on existing data (either published or unpublished) or whether primary data had to be collected. Because the method I was proposing to develop was one that relied very much on the participation of individual stakeholders, it was clear that the empirics would need to come from primary sources. However, because it was possible that the use of secondary data would reveal different issues from the use of primary data, a decision was made that at least one example should be based on secondary data – but that such secondary data should be very different from the example using primary data.
Issue 3. Quantitative or qualitative approach?

(a) Implications of a quantitative approach

The quantitative approach normally involves deriving hypotheses from theories, expressing the hypotheses in terms of operational variables, and measuring the mathematical relationships between sets of variables. This set of methods was developed over the last few hundred years by scientists studying the physical world, and was labelled “positivism” in the early 20th century, as outlined by Whitehead and Russell (1910/1962) in *Principia Mathematica*.

However, the logical rigour of quantitative research begins with the statement of hypotheses, and ends with the evaluation of the hypotheses based on the data collected. This is only the central part of the scientific process: it is preceded by the selection and generation of hypotheses, and often followed by an attempted generalization to a wider situation. To that extent, quantitative research is positivist only in its core process, as noted by Gephart (1988).

How would a quantitative approach be manifested in the present case? In positivist thinking, the most rigorous possible approach would be to set up a formal experiment, using the methods developed by R A Fisher (Box, 1978). Light, Singer and Willett (1990) typify this position by stating that “to establish a causal link you must conduct an experiment…. Of the three research designs we discuss [descriptive, relational, and experimental], only experimental inquiries allow you to determine whether a treatment causes an outcome to change” (cited by Maxwell 2004, pp243-244).

As a thought experiment, I designed a positivist approach to the development of the Process:

This design would use the “gold standard” (Campbell and Boruch, 1975) RCT method: the random controlled trial, or formal experiment. One set of cases (using the Process) would comprise the experimental group. To avoid any expectancy effect (Draper, 2004) the control group should receive some kind of placebo treatment – which might be some standard method of scenario planning. To maximize measurement sensitivity, cases would be matched pairs (as similar as possible within each pair) and within each pair, randomly allocated to the two conditions.

The minimum sample size for a quantitative study is generally regarded as being around 100: barely enough to detect a difference of around 5%, at a 95% level of confidence – without allowing any breakdowns of the sample. That sample size would apply to both the experimental and the control groups, producing a minimum sample of 200 cases. This means not 200 individuals, but 200 studies using the Process.
To guard against experimenter effects, there would have to be at least two experimenters, dividing their work at random between the experimental and the control groups.

The main hypotheses would be that the Process (in some specified ways) improved the performance of a social entity that underwent the Process. These specified ways would be measured using a combination of performance data and surveys of those involved. The experiment would be preceded by a preliminary survey, which would be used to develop a set of factors from the questionnaire. The factor analysis data would be used to refine the main instruments: two matching questionnaires, known from the pre-test to produce the same results. Each instrument would probably include around 100 variables, producing perhaps 3 or 4 near-orthogonal factors.

Before the intervention, the instrument would be applied to measure baseline data among those to be involved. Because the intervention could not be assumed to have immediate effect, there would have to be a time lag to allow for any implementation.

Since social change is slow, and that the Process is designed for looking at least some years into the future, it would be at least three years before the success (in that positivist sense) of SNM could be evaluated: the length of the evaluation period, plus probably several more years waiting for comparative statistics to be compiled and published. And as I found in my case studies, negotiating co-operation, finding a suitable time, and preparation of a set of scenario workshops can be a lengthy process: in some cases more than a year; it would take longer still to coordinate hundreds of organizations. Thus the time lag could well be 6 years.

After that period, the second instrument would be applied. Two matched instruments would be used, to avoid any problems of respondent memory of the initial questions. The difference in scores between the “before” and “after” measures, after taking into account the effect of the two control groups, would estimate the effectiveness of the Process.

That, then, is the quantitative methodology: the most “standard” method. However, for many reasons it was not feasible in this situation:

1. The scale and cost would be enormous: hundreds of cases – each involving the equivalent of several months’ full-time work with an organization whose future was to be studied.

2. There is no accepted standard scenario method that could be used for the control group. To design such a method would be to create an artificial standard – so the comparison would be between two broadly similar methods. Thus it would be advisable to include a second control group, in a “do nothing” condition – increasing the sample size and cost by 50%.
3. It would be difficult to find hundreds of organizations willing to co-operate with the study, particularly as only a third of them would be receiving the treatment hypothesized to be better.

4. The above description covers only a single study – but it is highly likely that results could be inconclusive, and that a further study would be required to clarify them... and probably several more studies, all on the same scale.

5. The rigid procedures necessary for successful accomplishment of such a large-scale study would make it impossible to introduce minor improvements to the method, except after each round of studies.

6. Because of the long time delay, particularly after multiple rounds, it is likely that social change would have superseded some of the earlier findings by the time the study was completed.

7. Bearing in mind Scriven’s (1967) distinction between summative and formative evaluation, there is one further problem: that the entire exercise is misconceived. The above description is of a study designed to answer the summative question “Is this new method better than the method used in the control groups?” However, in methodological development, a more appropriate question is “How can this method next be improved?”

Thus the positivist approach was simply not feasible for developing the Process. Having considered and rejected the positivist model, I next considered a qualitative approach.

(b) Implications of a qualitative approach

The qualitative approach, which until recent years has not been generally regarded as “scientific,” tends to be used mostly in the social sciences, while the positivist approach is dominant in the physical and natural sciences. In contrast with the quantitative approach (in which “variables” are tightly defined, do not overlap, and can readily be measured) the qualitative approach deals with concepts which are often not clearly defined, or for which there exist a range of interpretations. As the analytical tool used by qualitative researchers is words rather than numbers, verbally oriented research techniques are normally employed – though not always; for example, content analysis, essentially a quantitative technique, has been widely used by qualitative researchers.

Qualitative research has been more concerned with identifying and distinguishing concepts, rather than measuring them. When a variable can be precisely defined, it can be measured and if its occurrence is frequent enough, it can be usefully studied using quantitative methods.
However, in the social sciences, where many factors may simultaneously apply and overlap, the selection of relevant variables is a much more subjective assessment than in the case of the physical sciences. The difference arises because the social sciences have to deal with human language, while the physical sciences are confined to observation and physical measurement. If objects such as the moon employed publicists, to whom all questions had to be referred, physical scientists would have a much more difficult task, because of the human characteristic of reflexivity (Steier, 1991). What if one H in H₂SO₄ lied when asked about its local molecule, because it perceived sulphur dioxide as undesirable? And what if the moon gave an evasive reply, when asked “Where were you on the night of the 28th?”

Though it is possible to observe and measure human behaviour at the individual level without involving the subjects of the research, and even to formulate law-like generalizations about the behaviour of individuals en masse (such as the use of Dirichlet models in consumer behaviour: cf. Goodhardt et al, 1984), the value of observational measurement becomes more problematic as the scale of observed behaviour grows. Behavioural patterns may be established, but without knowledge of the purposes of those involved, prediction is fragile. I thus incline to support the view (frequently expressed by hermeneutic philosophers) that social science requires a different research approach from the traditional sciences – but on empirical rather than a priori grounds.

Some evidence for this is that 100 years’ research in psychology and related areas using the positivist approach has failed to produce a general theory that can be used to successfully predict human behaviour (cf. the review of forecasting in chapter 2), in the same way that (say) in the 17th century Newton developed laws to accurately predict the behaviour of objects in motion. As stated by Nagel:

In no area of social inquiry has a body of general laws been established, comparable with outstanding theories in the natural sciences in scope of explanatory power or in the capacity to yield precise and reliable prediction. (Nagel 1961, p447)

More than 40 years later, this statement is still true of the social sciences. The continuing lack of usable “laws” for predicting human behaviour can be viewed as a rebuke to positivism as the dominant tradition in social science. I am not suggesting that a qualitative approach would somehow uncover such laws; rather that general laws (in the sense of those of Newton) are not likely to yield useful predictions, because they would be so general, and because human decisions result from the interplay of many considerations. Thus the concept of “forces” (borrowed from Newton) may not be a useful analogy to apply to human futures.
(c) Choice of method

Eisner and Peshkin (1990, p11) discuss various perspectives on the state of research methodology. They distinguish four types of approach:

1. Those who regard conventional [i.e. positivist] and qualitative approaches to research as complementary. They argue that each approach is good for a particular class of problem. They cope with methodological pluralism by holding that it is the question that should drive the method, not vice versa.
2. Others...hold that qualitative research is basically a soft and less trustworthy version of the real stuff...Their view is that qualitative research might be good for exploratory work...
3. Another group holds that it is conventional methods that are suspect... Methods based upon a deterministic causal model simply do not fit the arenas in which human action takes place.
4. Another group denies that there are any differences between qualitative and conventional research. Epistemological differences are, in their view, grossly exaggerated.

The position adopted for this thesis derives from the first of the above categories: that the nature of the problem determines the approach. Given the nature of the present Process, the third approach applies in this particular situation. For developing a futures studies methodology of this type, a positivist quantitative approach was untenable, and a qualitative approach was chosen.

Issue 4. Summative or formative?

Scriven (1967) makes a distinctive between summative and formative evaluation, which has come to be widely applied in the field of evaluation (particularly for educational programs) and is highly relevant for the present Process. Summative evaluation sums up the accomplishment of a program on its completion, while formative evaluation is a continuing process during development. Though Scriven’s original definition of formative evaluation was quite narrow: “outcome evaluation of an intermediate stage in the development of the teaching instrument” (Scriven, 1967, p51), more recent usage has extended Scriven's definition. Chen (1996) separates Scriven’s summative/formative distinction into two dimensions: process vs. outcome, and improvement vs. assessment. In Chen’s terms the present study would be of process improvement. Flagg (1990, p5) defines formative evaluation as “any kind of feedback from target students or professional experts that is intended to improve the product during design, production, and initial implementation” – and later as “the art of making mistakes obvious, by hindsight” (Flagg, 1990, p63). Patton (2002, p220) notes some emerging variants of formative research, for developmental evaluation (Preskill and Torres, 1999) and continuous improvement (Mizuno, 1988), both of which are relevant in this case. Though formative research does not rule out the quantitative, its mainstay is qualitative data. Conversely, summative research frequently uses quantitative methods (Tessmer, 1993).
Thus a summative evaluation of a Process would answer the question “How good is this process?” – in comparison either to an absolute criterion, or to other comparable processes. In contrast, a formative evaluation would answer the question “How can this process be improved?” When a method is being newly developed the formative question is more relevant. Also, in the case of the current study (as demonstrated by the preceding design for a summative study using the hypothetico-deductive paradigm) a summative approach would simply not be feasible at this early stage of the development of the process. Similarly, Rowe and Wright, discussing research into the Delphi technique, state that:

We believe that recent research has been somewhat misdirected, with too much emphasis on Technique-Comparison studies at the expense of Process studies (Rowe et al. 1991, Rowe and Wright 1999). Studies of the former type tend to compare Delphi to other procedures to answer the questions “is Delphi (relatively) good or bad?” while studies of the latter type ask “why is Delphi good or bad?” (Rowe and Wright, 2001, p139)

In those terms, the present research was a Process study. Its purpose was not simply to determine whether the scenario network method was “good” or “bad” – particularly given the difficulty of developing testable criteria – but rather to detect weakness and strengths and to iteratively improve the method. Therefore, a formative approach was chosen.

**Issue 5. Hypothesis-based or evidence-based**

**Hypothesis-based.** Though a hypothesis-based approach is seen by many as synonymous with a positivist approach, this need not be so. Many scientific discoveries have been made not with a formal hypothesis, but with serendipity – a surprising finding, which has later led to the production of hypotheses and the measurement of their applicability. For example, in the case of the discovery of stainless steel in 1913, Harry Brearley (who was seeking a steel suitable for a gun-barrel, with resistance to erosion rather than corrosion) noticed that in a heap of metal scrap, the result of previous unsuccessful experiments, one piece had not rusted. There was no hypothesis: it was obvious to the naked eye (de Bono, 1974). Nor was there a need to observe a large sample: as a particular alloy will have invariant properties, a single case was sufficient, and generalizability was taken for granted.

Conversely, qualitative research can take a hypothesis-based approach: clearly stating initial hypotheses, and gathering evidence that serves to either confirm or disconfirm the hypotheses. Thorough research of this type often uses “judges” – not relying on one person’s (i.e. the investigator’s) view of the evidence, but combining the views of others who have less ego-investment in the successful confirmation of the hypotheses. The most rigorous qualitative content analysis is conducted in this way (Deacon et al, 1999).
The main use of the hypothesis-based approach in decision-making is when there exists a single relevant variable, the value of which can be precisely determined. But when does this apply? Some possible examples include evaluating the success of companies in terms of the aggregate value of their shares, and choosing as Olympic winners those fastest in their chosen competition. Even in those two arenas, however, the definition of variables is problematic. For example, the company could choose to maximize its market capitalization (as for public companies in the USA, following Milton Friedman, 1962), its return on capital invested, its profit level, its market share (as often in Japan), or some broader measure of success such as the Triple Bottom Line (Ellkington, 1997) or Balanced Scorecard (Kaplan and Norton, 1996). Even in the Olympic Games, the shortest-time rule excludes competitors with traces of certain substances (but not certain other substances) in their blood, and arguments have been raised concerning natural threshold levels for some substances. So what at first seems to be a simple measure, with one clear variable, is on closer examination more complex, with a large number of potential variables, many of which are complex social constructions. In these circumstances, there is no clear, single hypothesis, but many conflicting possibilities.

Evidence-based. For an evidence-based method, the model is the courtroom rather than the laboratory. Evidence is weighed up in all its detail, and a verdict arrived at. An implication of the thinking of Kuhn (1970) is that all science is consensus-based, and that consensus is largely based on generally-known evidence. Evidence of this type is usually in verbal form, but it need not be confined to that. For example, in a situation which requires a Yes/No decision based on a set of measures, there may be no agreed objective basis for the relative weighting of those measure, and to change the weightings in a preference matrix will change the decision – as with the examples in the previous paragraph.

In health and education, the use of “evidence-based medicine” and “evidence-based practice” has recently become popular, often using meta-analysis of findings of multiple studies. According to Pawson (2002a, 2002b) this can be either quantitative, or a narrative review, more theoretically based, using qualitative tables in the style of Miles and Huberman (1994). (Appendix 1 of this thesis, in reviewing scenarios for 2000, uses such a narrative evidence-based approach.)

Scriven’s (1974) “goal-free evaluation” is another example of an evidence-based approach. Unlike the classical evaluation method, which treats the objectives of a program as hypotheses

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2. The best known example in the health field is perhaps the extensive work of the Cochrane Collaboration (www.cochrane.org),
and tests the success of those objectives, Scriven’s method involves studying a program and asking “What have been the actual effects of this program?” Scriven maintains that it is not necessary for the investigator to know the intended effects during this research. Such an approach is close to grounded theory, the tools of which could be used to determine the actual effects. Logically, the more diffuse the likely effects of a program, the less likely it is that these will be captured by a hypothesis or a logic model, and the more likely that an approach such as goal-free evaluation will discover the existence of unintended consequences.

In the present case, the evidence-based approach was judged more appropriate, for two main reasons: (a) testable hypotheses could not be formed at the outset of the development process, and (b) evaluation of the process came through a wide variety of sources, both formal and informal, and an evidence-based approach was more suitable for integrating this information and deciding how to modify the nascent Process.

**Issue 6. A single case study, or multiple cases?**

The above sequence of decisions (empirical, mainly primary research, qualitative, formative, and evidence-based), combined with the relatively large scale of the Process, clearly required using the case study method. Also, because the Process is one that involves whole social entities, it would have been almost impossible to carry out this project without using case studies. It is now well established that the case study method produces knowledge of high quality, as documented by writers including Bennett and George (1997), Brown and Gerhardt (2002), Eisenhardt (1989), Guba and Lincoln (1992), Kvale (1996), McGuire (1998), Pozzebon, Freitas and Jenkins (1998), Stake (1983, 1995), Walsham (1995), and Yin (1994). The question here is whether the method should be developed in a single case study (as when Cooperrider (1986) developed Appreciative Inquiry), or a number of cases. For several reasons, the multiple-case approach was more appropriate in this situation:

**Reason 1.** Using only a single case study, by providing no basis for comparisons, would make it dangerous to produce any generalizations. Instead of being able to say “this method works in a variety of situations” the claim could only be “this method worked in one particular situation” – but only if it *had* worked in that situation (Kennedy, 1979; Donmoyer, 1990; Becker, 1990; Marianne Lewis, 1998).

**Reason 2.** A multiple case study allows for more speedy recovery if one case fails to be completed or for some reason is unusable.
Reason 3. One of the objectives of the Process was that it should be feasible for smaller entities, and should not (unlike the scenario planning of Shell Oil) take a year or more. But by working with a single case, one of two problems would arise: either the fieldwork would almost inevitably become highly detailed, involving multiple interviews and meetings – or else, if restricted to the planned set of four main workshops, a single case would be too perfunctory for doctoral fieldwork: it would not provide enough data to illuminate an entire thesis.

Issue 7. Sequential or simultaneous development

On close examination, this issue turned out to contain two sub-issues:

(a) Given that multiple cases were to be used, whether each case should be studied sequentially, or all cases should be studied at once.

(b) Whether the entire Process should be developed as a whole, or separate components of it should be developed sequentially.

Concerning point (a) it was simply not feasible to conduct all cases simultaneously. Even if this had been possible, it would not have been as useful as sequential development of cases. Pope, Ziebland, and Mays, in the context of qualitative health research, articulate this well:

In much qualitative research the analytical process begins during the data collection phase as the data already gathered are analysed and fed into, or shape, the ongoing data collection.... It allows the researcher to check and interpret the data she/he is collecting continually and to develop tentative conclusions based on the data already collected, or hypotheses for subsequent investigation in further data collection. Compared with quantitative methods, this has the advantage of allowing the researcher to go back and refine questions and to pursue emerging avenues of inquiry in further depth. Crucially, it also enables the researcher to look for deviant or negative cases; that is, examples of talk or events that run counter to the emerging propositions or hypotheses, in order to refine them. This type of continuous analysis is almost inevitable in qualitative research; because the researcher is “in the field” collecting the data, it is impossible not to start thinking about what is being heard and seen” (Pope, Ziebland, and Mays, 2000:114).

As the process of iteration is itself highly valuable, allowing as it does the continuous comparison of each case with each previous case, sequential development was used for the cases.

Concerning point (b) above, when it comes to the development of elements of the process, each method has advantages and disadvantages. The advantage of sequential development is that the effect of each component can be separately assessed. The disadvantage is a potentially severe problem with sequential development: the possibility of interactions between components. It is possible that two components, each of which worked separately, might not work when combined. A further disadvantage of sequential development of components is that
there can be so many variables that it might take hundreds of cases to resolve these issues – as has occurred with the study of response rates in surveys (as noted by Dillman, 1978). Thus the components were developed simultaneously, with two exceptions. One exception was the pilot study, which was conducted purely to assess the feasibility of the envisaged Process, and was thus incomplete. The other exception was the final case study, which developed one component in detail. The rest of that study was delayed due to funding issues. It is not due to recommence until after this thesis is completed.

To summarize the outcome of this seventh issue: cases were studied sequentially, but (with two exceptions) all components of each case were studied simultaneously.

**Issue 8. Which qualitative approaches?**

The final question for setting up the research was which particular qualitative approaches to use – expressed in the plural, because qualitative approaches overlap a great deal: it is all but impossible to use a single qualitative approach. To some extent, the choice of qualitative approaches had effectively been decided by choosing the sequence of decisions summarized in Figure 6.1 above.

Qualitative research offers an enormous array of potential approaches. As noted by Patton (2002, pp131-134) different writers on qualitative research have produced different epistemological categorizations of qualitative research. Crotty (1998) lists five “perspectives,” and Creswell (2003) names five “traditions” (different from Crotty’s five). Schwandt (2000) lists three “epistemological stances,” and Denzin and Lincoln (2000) offer seven “paradigms/theories.” Pepper (1957) distinguishes four “world hypotheses” which underlie the major philosophies and corresponding research paradigms. Patton (2002) lists several more such categorizations, then offers his own set of 16. These various groupings are not clear alternatives to one another, but overlap in various aspects, and address different issues: in the judgement of Miles and Huberman (1994, p5) these sets of paradigms are “basically incommensurate.” It is therefore not a question of choosing one or another, rather a matter of choosing (whether explicitly or implicitly) a configuration of qualitative approaches: a toolbox, rather than a tool.

One way of making sense of all these approaches is to sort them into a chronological sequence of choice decisions: ontological, epistemological, methodological (including praxiological), and analytical. Using that sequence, the following qualitative approaches were applied:
TABLE 6.3 QUALITATIVE APPROACHES USED IN THIS STUDY

<table>
<thead>
<tr>
<th>Sequence</th>
<th>Approach</th>
<th>Key references</th>
</tr>
</thead>
<tbody>
<tr>
<td>2. Epistemological</td>
<td>Pragmatism</td>
<td>Dewey (1910/1991), Peirce (1931)</td>
</tr>
<tr>
<td></td>
<td>Action research</td>
<td>Lewin (1946), Reason &amp; Bradbury (2001a)</td>
</tr>
<tr>
<td></td>
<td>Analytic induction</td>
<td>Znaniecki (1934/1968), Robinson (1951)</td>
</tr>
</tbody>
</table>

These four categories are not mutually exclusive, and the boundaries between them are not widely agreed. For example, many writers classify constructionism as an epistemology rather than an ontology; and action research, since its original articulation by Lewin (1946), has developed two aspects: it is not only a methodology, but also an epistemology, as noted by Peters and Robinson (1984). Thus the following categorization is not as tidy as it might seem: it is simply a means of classifying the range of approaches used in this study.

**Ontological.** In terms of ontology, viewed in this context as the nature of reality, the focus is on human groups and events as systems, which form the central unit of inquiry for this project. Thus a constructionist viewpoint was taken. This is almost a requisite viewpoint for qualitative futures work, because a central focus of futures studies is to understand human meaning, and what is the future but a social construct? There are two streams of constructionism: the social constructionism of Gergen (1999) and the radical constructionism exemplified by von Glasersfeld (1995). The latter is sometimes referred to as constructivism, by writers such as Crotty (1998, p58). In this study, the social stream is most relevant: though our world is but a social construction, many aspects of it are widely shared. In terms of the hemispherical model developed in chapter 4, the social element of construction is most pronounced at the top layer (events), but lower layers of the hemisphere, less able to be shared, are less socially constructed.

**Epistemological.** The development of any method perhaps implies a pragmatic viewpoint, because of its focus on the practical effectiveness of the method. Such thinking is expressed in the pragmatism of Peirce (1900/1955) and Dewey. In an apposite quotation, the latter states:
There are two ways in which man moves towards the future: by blind trial-and-error, or by consciously planning his future on the basis of his past experience and his projected desires. The Experimentalist hopes to construct methods which mankind can use...to control the controllable part of the present, and to make intelligent plans for conscious movement forward, step by step, into a partially foreseeable future. (Dewey, 1929/1960, p119)

The Experimentalist’s (i.e. pragmatist’s) hope could almost be a description of the Process being developed in this thesis. The pragmatists’ position was that objective truth is not knowable, so propositions should be judged by the outcomes they produce. Action research thus implies a pragmatist epistemology.

A secondary epistemology relevant to this study is critical realism (Sayer, 1984). Despite its major differences from constructionism, Mir and Watson (2000, 2001) show how the two are compatible. Also, critical realism, as argued by Bell (2003) and van der Heijden (2000, p31) is almost inherent in futures studies. A related approach is the “realistic evaluation” of Pawson and Tilley (1997), which abjures the so-called “black box” methods used in formal experiments in favour of probing into the antecedents of any outcome. Rather than simply asking “Was the null hypothesis rejected, and the intervention thus found effective?” it probes: “In what specific ways did the intervention achieve its effects?” These ways are grouped using the mnemonic CMO: what Contexts, what Mechanisms, and what Outcomes? The practical application of the layered hemispherical model developed in chapter 4, section 4.7 was much influenced by the thinking of Pawson and Tilley.

The third relevant epistemology is critical theory. To drastically summarize a wide range of thinking, in so far as it applies to this study, the contribution of critical theory is a focus on the negotiability of knowledge: in particular the relations of knowledge to power, with the powerful circumscribing the knowledge of the relatively powerless. A purely constructionist approach, as Pawson and Tilley (1997, p20) point out, implies a negotiation between stakeholders of equal power. Because the Process involves social entities, which are clearly not of equal power, critical theory is useful in exposing assumptions and unrecognized worldviews. The contribution of critical theory to the Process includes the exploration of embedded systems (as explained in chapters 4 and 5) and the effect of various actors’ conflicting goals on the futures of a human entity. Though Churchman (1971) is not viewed as a critical theorist in the Frankfurt School sense, his philosophy is relevant in this context, as is that of his intellectual descendants Ulrich (1994) and Midgley (2000).

Methodological. In terms of methodology, the criteria set out in chapter 3 effectively dictate the use of a participatory approach. Issue 5 above demonstrates the value of case studies in
the present context. Given the iterative nature of methodological development, action research was the only qualitative method that both explicitly uses iteration and can involve a high level of participation. Many writers, particularly on information systems and operational research, support the utility of action research in such a situation. For example, Baskerville and Wood-Harper argue that action research is the most suitable method for studying purposeful human activity:

> We suggest that action research, as a research method in the study of human methods, is the most scientifically legitimate approach available. Indeed, where a specific new methodology is being studied, the action research method may be the only relevant research method presently available. (Baskerville and Wood-Harper, 1996, p240)

Eden (1995) concurs, stating that an action research approach is the most relevant for evaluations with complex goals. As no more appropriate method could be found, action research thus became a key element of this study. The following section (6.3) provides more detail on the specific action research approach selected.

**Analytical.** In terms of data analysis, the visual approach outlined in chapter 5 indicated the value of using cognitive mapping. The layered hemispherical model required the use of critical methods, to probe beneath the surface of participants’ beliefs about the future. To enable better understanding by participants of the layered model, a variant of cognitive mapping was developed. This is not a method of analysis in the same sense that statistical methods are; rather, it facilitates qualitative analysis through critical questioning of initial perceptions. Though it is possible to derive numeric data from cognitive maps, this was not appropriate in the present study, because commensurability of concepts could not be established.

For formative evaluation of findings, analytical induction (Znaniecki, 1934/1968; Robinson, 1951) was judged to be the most appropriate method for this study, given the use of multiple case studies and the need to develop a workable Process. Though seldom used, analytic induction is acknowledged to be a rigorous method for establishing patterns in any form of qualitative data (Frankland and Bloor, 1999; Katz, 2001). It develops generalizability by attempting to re-form tentative propositions in such a way that they are able to cover all cases; similar to a non-participative equivalent of the consensus group process described above. When a proposition cannot be restated in such a way that it covers all cases, the minority of cases that it does not cover are separated from the population to which the proposition applies, and a different proposition may be developed for that sub-population.
6.3  Action research as a development method

Because action research was judged to be the most appropriate methodological approach to the development of the Process, this section considers it in detail, and determines which form of action research was most appropriate to use. Since its original articulation by Lewin (1946), action research has developed into a variety of related streams. Peters and Robinson (1984) surveyed 11 early writers on action research, including Lewin, Argyris, and Kemmis, and compiled a “consensual summary” of 18 characteristics of action research. On exploring commonality between the methodologically and the epistemologically focused writers, Peters and Robinson distinguished three shared groups of characteristics:

1. Involvement-in-change characteristics – i.e. they are problem focused and directed toward the improvement of some existing social practice;
2. Organic process characteristics – i.e. research consists of a series of systematic cyclical or iterative stages of fact finding, reflection and planning, strategic action, and evaluation;
3. The collaborative characteristic – i.e. research is carried on as a joint, cooperative endeavor among the participants. (Peters and Robinson, 1984, p121)

If these are the key characteristics of action research, an implication is that if any of the three is lacking, the process being used is not action research. For example, concerning the second characteristic, Dick points out (Williams, 2004) that after the concept of action research has been explained to managers, some claim to be doing action research already, but often this is a misconception, as the component of explicit reflection is lacking.

Some variety of action research seemed the most appropriate method to use, because the development of a method would clearly require an iterative approach, and because the development would entail working closely with staff in the target organizations. The questions addressed at this point were thus:

1. Which varieties of action research are most relevant for the Process?
2. If no single variety is fully applicable, can elements of several varieties be used in developing the Process – and is it defensible to combine elements in such a way?
3. Can the development of a research method qualify as action research?

6.3.1  Action research and futures studies: similarities and differences

Action research, as well as being an appropriate method for development of the Process, is well matched with futures studies. Ramos (2002) found eight intersections of futures studies and action research: in their focus on participation, social change, engagement in creation of knowledge, systems thinking, holistic complexity, visions of the future, commitment to democracy, and social innovation. I suggest adding a ninth: an ongoing probing of assump-
tions, and reinterpretation of the system under study. Ramos includes this under democratic commitments, but the former need not entail the latter: for example, developing a root definition using Soft Systems Methodology can be done solely by experts (Checkland, 1981).

Despite their similarities, futures studies and action research also have several clear differences. The first, which arises almost by definition, is that futures studies looks well into the future, while action research, being action-oriented, focuses on the present. The second difference is one of scale. Action research is internally focused: it generally involves working with a single organization. In contrast, futures projects, even when commissioned by one organization, look beyond the confines of that organization, considering social trends and other external forces. A third difference is that action research makes explicit use of iterative cycles. This is not a normal component of any futures studies method, except Delphi (though CLA may do so in practice, by sequentially considering a series of layers).

Despite those differences, both participative futures and action research clearly fall under the label of “transformational inquiry” (Hart and Bond, 1995). By combining the two approaches, both might be improved. Participative futures methods could be enhanced through the use of iterative cycles, while action research could be enhanced in two ways: firstly, by viewing the focal entity as one component of a larger world, at a particular moment in time; and secondly, by incorporating futures studies’ central concept of alternative futures into the practice of action research. These similarities between futures studies and action research allowed some simplification of the task ahead: not only could the development of the Process be done using action research, but the use of the Process could also be regarded as a form of action research.

6.3.2 Forms of action research

Since Lewin (1946) first put forward his concept of action research, numerous variants have arisen. Peters and Robinson (1984) discuss 15 forms of action research, and the handbook of Reason and Bradbury (2001a) adds more still. Despite their differences, all of these methods are similar enough that they can still shelter under the broad umbrella of action research. Some major varieties of action research (and related methods) are listed below in approximate chronological order; these are key references rather than initial publications. The list is not exhaustive, as it excludes some offshoots not relevant to the development of a method.
### Table 6.4 Varieties and derivatives of action research

<table>
<thead>
<tr>
<th>Variety</th>
<th>Major references</th>
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<tr>
<td>Participatory action research (PAR) – two varieties:</td>
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None of the above approaches was designed with the development of a social inquiry method in mind. However, two methodologies explicitly use the concept of “double loops” – the action science of Argyris, and the soft systems methodology of Checkland. The relevance of this is that an inner loop can apply to the use of the method within a case, and an outer loop to the development of the method between cases – corresponding to the revision of development criteria for the consensus group technique, as described in section 6.2.1.3 above.

Checkland’s FMA model, derived from Soft Systems Methodology, was the most relevant model that could be found. However, Hindle et al (1995) on discussing the FMA model do not imply that the F (Framework: as covered in this chapter) would change, even though Soft Systems Methodology, of which FMA is a component, is a form of action research. The later LUMAS model (Checkland, 2000), effectively a more flexible version of FMA, though specifically not designed for generalization, does allow for revision of the initial framework. However it is not stated specifically enough to be useful in practice.

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3. Though Critical System Heuristics shares many similarities with action research, Ulrich (1996, p9) points out a major difference: that action research avoids conflict, while CSH deals with it.
Having thus failed to find a “ready made” action research variety specifically suited for the development of a social inquiry method, the next step was to examine whether generic principles of action research might be usefully extended, to form yet another variety of action research more specifically usable for the development of a social inquiry process.

### 6.3.3 Elements of action research used in this project

On reviewing the literature of action research, four key elements, gathered from various forms of action research, were identified as essential for this project.

1. From PAR, in both its forms (Greenwood and Levin, 1998, Fals-Borda and Rahman, 1991): participatory development, in which all stakeholders in an entity are invited to consider its futures.

A potential danger with combining components of various methods is the risk of adverse interactions: that elements which in their original context work well individually do not work well when combined. In the present case, this did not appear to be a danger, for two reasons:

- because the various approaches to action research already shared those key characteristics, to varying extents, and
- because no clear contradiction was evident between any pair of the four components; on the contrary, each taken out of its context would be less useful in isolation.

It was therefore decided to combine the above four elements for the purpose of developing the Process.

### 6.3.4 Is this action research at all?

To determine whether the selected method still amounted to action research, its elements were compared with Peters and Robinson’s (1984) three characteristics shared by the methodological and epistemological emphases in action research.
Characteristic 1. To what extent is it problem-focused and involved in change?

In three ways:
(a) participants come to know likely futures for their social entity, and can develop ways of achieving their shared goals, and also
(b) having learned the Process through their immersion in it, participants can apply it to other social entities that they belong to.
(c) For development of the Process: using the outcome of a case to improve the method in later cases.

Characteristic 2. To what extent does it possess organic process characteristics?

The method is designed to make explicit use of the iterative cycle of action research, on two levels. At the case level, there is an iterative cycle of multiple workshops, generally a week apart to allow time for reflection. At the level of development of the Process, there is a larger cycle, in which the unit is the case itself. After each major or minor cycle, there is an opportunity to change the Process; this is the key “organic” characteristic.

Characteristic 3. To what extent does it use participatory, democratic processes?

Participants would be considering the futures of their own entity, and would offer advice on the Process, but because of their lack of expertise in methodological development, they would not be able to participate fully in the development of the Process. In relation to the development of the Process, the form of participatory action research used here would resemble the less-participatory Northern form derived from Lewin (as in Greenwood and Levin, 1998), rather than the more-participatory Southern form (as in Fals-Borda and Rahman, 1991). In relation to the use of the Process after its development, the Southern form could more closely apply, provided that if experts or consultants were used, boundary critique (Ulrich, 1996; Midgley, 1998) was applied.

Given those instances of the defining criteria, the development of the Process appears to qualify as action research, though it did not exactly correspond with any of the 13 varieties listed in Table 6.4. The method used differed from generic action research practice in three respects: collection of detail, degree of involvement, and more explicit use of cycles:

Differences of detail: In most published reports of action research projects, the researcher has long and repeated contact with the social entity being studied. The typical process is almost ethnographic, often extending for a year or more. In this project, with less contact (no more than 15 occasions, over several months, for each case study), less detail was collected.
Differences of involvement: In the more participative forms of action research, participants are highly involved with the process, because they are researching their own social entity. In this project, that was true at the inner (case) level, with participants considering the possible futures of their own social entity. My own involvement at both levels was more that of an outsider, because my major purpose was to develop the Process.

Differences of cycling: A surprising discovery made while reviewing the literature of action research was the paucity of accounts of the explicit use of cycling; the only detailed reference found was Dick (2000). This might be due to the differences of detail and of involvement: because I was not a member of the entities I worked with, the occasions on which I worked with the social entities themselves became components of a cycle. In a more typical situation, when participants are highly involved with the organization being studied, they work so continuously with that organization that it becomes difficult to delineate specific cycles. A paper of mine to be published in Futures (List, in press 2005) covers this topic in more detail.

To establish whether the proposed method could be regarded as action research, I conferred with a highly experienced action researcher, Dr Selva Abraham (Abraham, 1994) and director of the Gibaran Institute of Action Research in Australia. After a long discussion, he formed the opinion that the method I was proposing in fact was action research – but, because of the limited decision opportunities available to participants, it did not fully qualify as participatory action research. Rather, it was a generic type of action research, in Lewin’s sense.

6.4 Review of this chapter

This chapter has outlined the methodology selected for this thesis. It began by looking for precedents in the development of a methodology of social inquiry. However, nothing found was detailed enough to be usable. The next step was thus to derive a set of criteria for the development of a process – specifically for exploring the future, but probably also usable for the development of other social inquiry methodologies. The third section of the chapter investigated a range of approaches, and settled on a form of action research.

The approaches chosen as most suitable for this research included a wide range of methodological components, drawn from a broad variety of social research methods. Though this pluralistic approach might be criticized as bricolage (Lévi-Strauss, 1966) or postmodern adho-

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4. Now renamed the Gibaran Business School; details at www.gibaran.edu.au
cism (Jencks and Silver, 1972), other writers strongly support the use of mixed methods. For example, Mingers and Gill (1997) edited a book on this concept of *multimethodology*, as they labelled it. Several contributors to that book (M. Jackson; Taket and White) argue that methodological pluralism is not only a valid approach, even when paradigms are being mixed, but a preferable one, due to its encouragement of multiple perspectives. Thus Jackson states “Pluralism can provide its greatest benefits only in the context of paradigm diversity” (in Mingers and Gill, 1997, p367). Mindful of such support, I strove to assemble a methodological framework that would address the central research issue as closely as possible. To recap, the methodological toolbox comprised:

(a) Selection of development method

1. Is empirical data needed? (Yes.)
2. Collect primary data, or use secondary? (Mainly primary, some secondary.)
3. Standard quantitative method (e.g. field experiment) feasible? (No.)
4. Use summative or formative approach? (Formative.)
5. Use hypothesis-based or evidence-based approach? (Evidence-based.)
6. Study single case, or multiple? (Multiple cases.)
7. Develop method sequentially or simultaneously? (Sequentially.)

(b) Selection of a coherent set of qualitative approaches:

Ontological basis: social constructionism.
Epistemological basis: pragmatic, with elements of critical realism and critical theory.
Methodological basis: an iterative approach, using multiple cases (in other words, action research), paying particular attention to the use of cycles and reflection, with a high level of participation during the central stages of each cycle.

Analytical bases:
8. For use within the process: cognitive mapping.

Having decided on a suitable form of action research, the next step was to design the research program in detail. This entailed two groups of decisions: on the sampling method, and the specific steps to be used in the Process. These are detailed in the next chapter.