

Bricolage as an Alternative Approach to Human Resource Development Theory Building

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This article argues for a liberal and pragmatic approach to human resource development (HRD) theory building, which, while retaining academic rigor, celebrates difference and allows learning from more than one ontological paradigm. The author argues that the paradigmatic struggle between positivists and constructivists for supremacy in the HRD field is futile because each ontology draws on such different political positions, discourses, and languages. The possibilities of rigorous and innovative theory building within the constructivist paradigm are both exciting and important for developing new knowledge in HRD, allowing both creativity and imagination to flourish in the research process. The author illustrates the richness of this approach through her own experiences of qualitative theory building from a single case study and suggests that the metaphor of research as bricolage can offer many new and refreshing possibilities for researchers in the field of HRD.

This article takes the view that theory building in human resource development (HRD) is rarely the “neat” process it is often claimed to be. My aim is to raise the credibility of the contribution of qualitative research to HRD theory building by arguing in favor of Denzin and Lincoln’s (1994) bricolage metaphor as an approach to theory building that challenges the dominance of orthodox positivist approaches (e.g., Dubin, 1976), liberates the qualitative researcher from being judged according to these criteria, and more closely represents the active theory-building process in which many qualitative researchers are engaged.

To illustrate my argument, I will draw throughout on my own theory-building experiences and in particular on a specific case study that provided a vehicle for the development of a new theory about the impact of corporate culture change programs on the lives, behaviors, and values of those managers targeted for change. This article is an account of the ontological and methodological journey I took in conducting this study. It focuses on the processes followed and the choices made, but it is not intended as a presentation of the theory itself (for this see Turnbull, 1999).

It is already well understood that theoretical paradigms (Burrell & Morgan, 1979; Denzin & Lincoln, 1994) are responsible not only for differences in approach but also, importantly, for how the communities operating within them make judgments about epistemological issues. What constitutes valuable knowledge, therefore, remains a hotly contested question in HRD to which there are infinite responses and which cannot therefore simply be reduced to a debate between the qualitative and quantitative research communities. Qualitative researchers cannot be considered as the homogenous group they are often labeled as being. A closer look will reveal postpositivists, critical realists, constructivists, social constructionists, postmodernists, poststructuralists, and critical theorists of various orientations including labor process theorists, feminists, and so forth. Clearly then, even to agree on criteria for good quality theory building within the qualitative research community is not straightforward. What is accepted by most, however, is that reliability, internal and external validity, and generalizability are inappropriate success measures for qualitative theory building, as these fail to recognize that much qualitative research aims at generating rich insights into a particular case through attempting to understand the way the subjects of their research see and interpret their own world.

Guba and Lincoln (1998, p. 213) have proposed a number of alternative criteria for judging the goodness or quality of qualitative inquiry. They are trustworthiness, credibility, transferability, dependability, confirmability, and authenticity, each of which is judged according to the declared purposes of the research, whether these are to advance theory, education, or emancipatory ideals. These, however, are designed to map onto the criteria of validity and reliability to satisfy the academy that qualitative research can have the rigor of quantitative research and that this can be measured by equivalent standards. Alvesson and Deetz (2000) acknowledged this need:

Sloppiness, the expression of opinion not grounded in argumentation, arbitrary use of empirical material, reluctance to engage in dialogue with the literature, and careful consideration of alternative interpretations before deciding which one to favour, are all certainly not to be tolerated. Formalisation, procedure and technique may, however, be replaced by interpretive and theoretical awareness and sensitivity as means of achieving "qualitative rigour", and thus avoids problems of relativism and arbitrariness. (p. 69)

Alvesson and Skoldberg (2000) have suggested that good qualitative theory building should be "rich in points," by which they mean interpretively rich. Thus, for them, "research rich in points usually avoids definite statements about 'how things are' and emphasises the importance of looking at things in some particular way, which allows a new understanding of the empirical situation concerned" (p. 278). For this, both imagination and creativity are required, to allow the possibilities that arise from ambiguous empirical material to emerge.

This view is echoed by Watson (1994), who wrote that good qualitative research should be a craft “which involves imagination, flair, creativity, and an aesthetic sense” (p. 78). Alvesson and Skoldberg (2000) continue by saying, “Good research according to the criteria of interpretive richness thus enables a qualitatively new understanding of relevant fragments of social reality. This represents a break with earlier ideas, at least on certain points” (p. 279).

Here we see an immediate departure from traditional theory builders for whom creativity and imagination may be considered traits that should be minimized at all costs, seeing these as responsible for bias in interpretation and reporting. We know that within this paradigm, researchers are expected to report only that which can be empirically corroborated, in other words, only that which can be “known.”

How, then, can we reconcile these paradigms of theory building? Each clearly draws on different ontological and political positions, discourses, and languages while competing for academic credibility by identifying apparent flaws in the others’ arguments—using the presuppositions of their own paradigms to do so—which as Newton (cited in Alvesson and Skoldberg, 2000) has argued, results in a narrowing down instead of a widening out of the debate.

This article picks up this question and argues that deep entrenchment in a single paradigm can limit the possibilities of building new and exciting theory. I am arguing here for a liberal and pragmatic approach to HRD theory building, which, although still retaining academic rigor, celebrates difference and allows learning from more than one paradigm.

An Illustrative Case Study of Bricolage in Action

The study that I have chosen to illustrate the value of multiparadigmatic HRD theory building was a 12-month project to evaluate the shifts in values, beliefs, and behaviors taking place among a group of managers who were attending a corporate change program designed to “change” culture. Having completed a literature search, it became clear that there was little theory that could account for or explain the responses of those targeted by such programs, the enduring effects of such programs, their emotional impact, the impact of previous management fashions on their responsiveness, the effects on their identities, and ultimately the value for the organization of investing large sums in such programs. The theory that existed on organizational cultural change was almost entirely aimed at the level of the organization and tended to be instrumental in its tenor (e.g., Deal & Kennedy, 1982; Kotter & Heskett, 1992; Schein, 1985).

My aim was to build new theory through the process of “bricolage” designed to understand deeply the various experiences, responses, behaviors, beliefs, and values held by the managers of the organization I was researching.

Process

The research for this work was truly emergent, with its methodology gradually becoming clear as it took shape at the hands of the researcher. Denzin and Lincoln (1994) referred to this craft-like quality of qualitative research as “bricolage”: “The bricoleur produces a bricolage, that is, a pieced together, close knit set of practices that provide solutions to a problem in a concrete situation” (p. 2). “The solution,” they said, is an “emergent construction,” the product of the researcher and the setting: “The bricoleur understands that research is an interactive process shaped by his or her personal history, biography, gender, social class, race, and ethnicity, and those of the people in the setting” (p. 3).

I was constantly aware of my own personal history while scoping this research. Fifteen years as a manager in various industrial enterprises and the impact of living through the delayering, downsizing 1980s and 1990s could not have avoided leaving its mark. Objectivity and neutrality then were not an option, and a positivist position would have disregarded the influences that had created the sense of urgency and interest with which I pursued the chosen topic. Mangham (1987) confirmed this position:

There is no such thing as presuppositionless research, nor does theory simply emerge from data. One of the reflexive features of this model¹ of human beings is the recognition that a researcher also gives meaning to what she sees and hears. (p. 25)

To continue drawing from Denzin and Lincoln’s (1994) metaphor, “The bricoleur knows that science is power, for all research findings have political implications. There is no value free science” (p. 3). It was clear from this analogy that this research would carry political implications, it would almost certainly lead into some difficult and sensitive territory, and it might be quite difficult to read meaning into the words of the respondents, as they themselves might be using the interviews to further their own political motives. Thus, complexity in approach, data collection, and data interpretation was inevitable. As an interpretive piece of research, a clear and definitive result or “answer” would neither be sought nor found. This was not my understanding of the theory-building process. Instead, I was seeking rich interpretation, as suggested by Alvesson and Skoldberg (2000) and echoed by Denzin and Lincoln (1994): “The product of the bricoleur’s labor is a bricolage, a complex, dense, reflexive, collage-like creation that represents the researcher’s images, understandings, and interpretations of the world or phenomenon under analysis” (p. 3).

Research Paradigm and Pragmatic Pluralism

The starting point was to endeavor to define the paradigm (Burrell & Morgan, 1979) or interpretive framework (Denzin & Lincoln, 1994) within which my research could be situated. This convention for neat categorization was one of my first challenges. Indeed, Watson (1997), known for his insightful ethnographic study into management and the substantial contribution to theory of his book *In Search of Management* (Watson, 1994), criticized the “artificial and stultifying” effects of “paradigm closure” (Watson, 1997, p. 5). Watson rejected the either/or choices often imposed by paradigm closure in favor of a multiparadigmatic strategy, which, following Hassard (1993), he suggested is more appropriate to the empirical researcher. Denzin and Lincoln (1994) appear to agree and support the possibility of a hybrid research paradigm: “Clearly, simplistic classifications do not work. Any given qualitative researcher-as-bricoleur can be more than one thing at the same time” (p. 512).

Such an approach has been successfully applied by Law (1994) in his ethnographic study of the Daresbury scientific laboratory. In this study, Law acknowledged the influences of “first, symbolic interactionism (whose patterns tend to be local); second, post-structuralist discourse analysis, whose patterns in some cases seem to be strangely hegemonic; and a third theoretical tradition, that of the actor network analysis” (p. 19). From these three paradigms, Law looked for patterns and areas of overlap to assist in the interpretation of the stories of the people he was studying, and from this he generated rich new theory on the actor networks found in this organization. Much of this theory has since been found to be transferable to other organizational contexts.

Watson (1997) stressed the need for plausibility, coherence, and integrity when following such a hybrid research paradigm. With these criteria in mind, I selected ideas from the following paradigms to assist in the interpretive phase of my research.

Gergen's (1999) social constructionism. This was the dominant ontological paradigm in this research, as I was concerned with understanding how meaning was constructed by the managers in Aeroco and understanding their responses from an emotional perspective. This distinctive theory within the constructivist paradigm emphasizes the process of social exchange in shared constructions of meaning and knowledge, through language and other social processes.

Symbolic interactionism. This movement, which led to the dramaturgical (Blumer, 1969; Mangham, 1986, 1987) school, also offered useful concepts for understanding emotion and identity, particularly in its central notion of social actors and role, its attention to the mediating of meaning through language and symbols, and its self-consciousness in interpreting this meaning and its impact on the self.

The processual or relational ontologies of becoming. These ontologies, as developed by Chia (1996) and adopted by Watson and Harris (1999) in their interpretive work on managerial identities, were helpful in following the constantly evolving identities of the managers. In the same vein, I found Foucault's (1988a, 1988b) work on subjectification and the project of self an extremely helpful interpretive framework.

The radical humanist paradigm. This paradigm (Burrell & Morgan, 1979; Parker, 2000), which seeks to critically interpret empirical data by emphasizing the importance of both "meaning and power" (Parker, 2000, p. 77), was also important in influencing my thinking about power and control in culture-change initiatives. Parker (2000) reinforced the importance of the radical humanist paradigm for studies in this field:

From subjectivist thought it gains a recognition of the importance of actors' meanings and attempts to treat organizations as cultures, as processual arrangements of beliefs, myths, symbols and so on. However, writers within this area also recognize that these local arrangements are inescapably related to wider historical, economic and social forces. (p. 78)

Although the above paradigms are recognized as taking slightly different ontological and epistemological positions (although each of these takes a nonpositivist position and each suggests a qualitative methodology), I judged that each would enable me to examine the complex responses of the managers from multiple overlapping perspectives, thereby increasing the depth and therefore the plausibility, integrity, and coherence of the account.

Choice of Subject: Single Case Study

Stake confirmed that "case study is not a methodological choice but a choice of subject to be studied" (cited in Denzin & Lincoln, 1994, p. 236). This particular study fits into Stake's category of an instrumental case study as it was specifically chosen as an example of a corporate change program aimed at middle managers. Stake denotes this as being when "a particular case is examined to provide insight into an issue or refinement of theory . . . the choice of case is made because it is expected to advance our understanding of that other interest" (Stake, 1994, p. 237).

It is contended that considerable learning and contribution to knowledge can emanate from case study research as illustrated in this study, although the limitations of case study research in seeking to claim broader connections and generalizations beyond the case itself should be acknowledged. Indeed, Stake (1994) reminds us that qualitative research does not hold generalizability as its goal and therefore "damage occurs when the commitment to generalize or create theory runs so strong that the researcher's attention is drawn away from features important for understanding the case

itself" (p. 238). Stake also reminds the case study researcher that meaning will always be mediated by both author and reader in light of their own experiences: "Knowledge of the case faces a hazardous passage from writer to readerConceptually for the reader the new case cannot be but some combination of cases already known" (p. 241).

Control and influence over the interpretation of the data is therefore always shared, mediated by the researcher's own past experience and offered to the reader as such, and then by the recipient of the research who inevitably adds his or her own life experience to the interpretation: "In private and personal ways, ideas are structured, highlighted, subordinated, connected, embedded *in* contexts, embedded *with* illustration, laced with favor and doubtThey know that the reader too will add and subtract, invent and shape" (Stake, 1994, p. 240).

In acknowledging some of the limitations associated with a single case study, as outlined by Stake (1994), I undertook comparative analysis of a number of previously published case studies of organizational change programs and studies of managerial behavior. This enabled the triangulation and comparison of data, to provide further insights on the case itself and its potential for transferability.

Ethics of the Research Methodology

The research followed a broadly ethnographic approach. Hammersley (1990) defined this as studying people's behavior in everyday contexts rather than in experimental conditions; gathering data from a range of sources, with observation and/or relatively informal conversations being the main ones; and focusing on a single setting or group. The analysis involves interpretation of meaning.

A number of other ethical considerations had to be taken into account:

With much qualitative work, case study research shares an interest in personal views and circumstances. Those whose lives and expressions are portrayed risk exposure and embarrassment: loss of standing, employment, self-esteem. It is imperative that great caution be exercised to minimize risks. (Stake, 1994, p. 244)

Great care was taken to protect all parties in the research from any risk that could occur as a result of their views given directly or indirectly during the research.

According to Czarniawska-Joerges (1992), there are some key ethical considerations for the qualitative researcher. First, "the social investigator must sort his values and obligations and weight them repeatedly throughout the research process" (p. 112). Second, "the only route that should be absolutely forbidden is the escape route—pretending these questions do not exist" (p. 112). Czarniawska-Joerges also warned that "by conducting one

formal interview, we are already meddling with the social tissue of an organization, and we must take this into account” (p. 112).

I was constantly aware of the potential impact of the questions I posed on the lives of the respondents and was careful not to unsettle them by any deliberately leading questions.

Data Collection

Returning to Denzin and Lincoln’s (1994) metaphor, a bricoleur is “adept at performing a large number of divergent tasks, ranging from interviewing to observing, to interpreting personal and historical documents, to intensive self reflection and introspection” (p. 3). This research used a range of data collection methods.

The research took place over a 27-month period and was divided into five phases. The empirical work spanned 20 months. The phases were the following:

- Phase 1: understanding beliefs and values prior to the change program and initial responses to the program;
- Phase 2: analyzing the data collected and starting to interpret this using previous research and available literature;
- Phase 3: conducting second interviews;
- Phase 4: interpreting the data, studying previous cases, and generating theory; and
- Phase 5: writing an account.

Interview Design

The design of the research questions was intended to be as open as possible. Although I was conscious of having already framed a number of questions and possible preconceptions about the value and meaning of the program, it was important that these were not brought into the interviews through leading or skewed questions. Determined to capture the voices of the managers themselves, I asked only a small number of very high-level questions to start the process of inquiry, with my secondary questions being framed in response to their answers. It was intended that during the analysis certain questions would be addressed concerning the differences in responses to the program and that there would be a close look at these differing responses according to the business unit and site, length of service, age, function, professional background, and gender. However, an inductive approach to data analysis was the primary mechanism for identifying the themes emergent from the interviews themselves.

Interviews were tape-recorded with the permission of the interviewees (permission was granted in all cases, with only one exception). These were then transcribed for further analysis.

Participant Observation

One of the early exponents of participant observation through his own style of dramaturgical sociology was Goffman (1959). According to Adler and Adler (1994), "By studying how people act, interact, and form relationships, he sought to understand how they accomplish meaning in their lives. He was particularly interested in how people construct their self-presentations and carry them off in front of others" (p. 383). As already indicated, all modules were attended as observer-participant (Gill & Johnson, 1996) during the course of this research. A great deal of useful data was collected not only during the formal sessions of the program but also informally in conversations in the bar and dining room and between formal sessions.

Interviews With the Research Sponsor

Although it was never the intention to interview senior management about the ideological intent of the program, given that the focus was on the responses to it irrespective of its intentions, the opportunity presented itself at different stages in the research for two in-depth interviews with the senior manager responsible for the implementation of the program. In both cases, he was seeking feedback from the research, and this led to the opportunity to question him further on his own perspectives. These perspectives were used as background data to the program only. The work of Easterby-Smith, Thorpe, and Lowe (1991) warned the researcher to be aware of possible "contamination" (p. 57) of research as a result of the political interests of the sponsor. Clearly his agenda was to find positive messages about the program to present to his colleagues. However, this agenda never presented any threat to the integrity of the data, and the program's momentum seemed unaffected by these discussions.

Secondary Data

Many other insights about the program and the company were also provided through large amounts of documentation about the program and the values collected during the research. Communication videos, company magazines, and other printed material were often offered by the respondents themselves to illustrate a point they were making. This was specifically used to provide contextual data and a deeper understanding of the discourses to which the managers were exposed in their work. This came to be helpful when making sense of their own texts and in understanding the dynamics of the production, distribution, and consumption of these discourses (Fairclough, 1995).

Interpreting the Data

Coding

The themes and subthemes that emerged after a number of iterations through the data contained in the transcripts and interview notes were partially derived from the interview structures but were largely inductive, found in the responses of the interviewees.

Clearly at this stage in the research, I had already identified a number of theoretical areas that would be crucial in the later interpretation of the data, but it was a conscious decision to put these aside (as far as this is possible) during the coding stage.² The coding was a continuous and emergent process. Having completed this inductive process, the data were then rescrutinized to achieve a deeper analysis, applying some of frameworks from the literature on ideology, emotion, and the self. Finally, Fairclough's (1995) framework for analyzing discourse and social change was very useful for taking a broader perspective on the texts of the managers.

Interpretation

Altheide and Johnson (1994) remind us that "our experience suggests that researchers should accept the inevitability that all statements are reflexive, and that the research act is a social act . . . The nature of meaning and its unfortunate location between language and experience produces an imperfect fit" (p. 492). They go on to address the problem of how to deal with deception by interviewees, so that contradictory evidence and hesitancy in responding all became part of the data for analysis:

Consider problems of communication with informants: misinformation, evasions, lies, fronts, taken for granted meanings, problematic meanings, self-deceptions. We do not claim that attending to the relevance of these issues in a study makes the study more truthful, but only that the truth claims of the researcher can be more systematically assessed." (p. 494)

I became very conscious throughout the data interpretation phase of crafting a story (Law, 1994; Mangham & Overington, 1983, 1987; Watson, 1994) to make sense of the vast amount of data: "Interpretation is an art; it is not formulaic or mechanical. It can be learned, like any form of storytelling, only through doing" (Denzin & Lincoln, 1994, p. 502).

Denzin and Lincoln (1994) concluded that storytelling is a legitimate and rigorous form of data analysis, but they remind us that "the storytelling self that is presented is always one attached to an interpretive perspective" (p. 502). Thus, Denzin and Lincoln (1994) confirmed that such storytelling can never be neutral in its outlook and will always reflect the dominant paradigms of the research. In his ethnography of the Daresbury scientific laboratory, Law (1994) defended the

value of stories in leading to the identification of patterns, and from these “to find some common space or area of overlap” between ideas from different disciplinary strands:

Thus people in the laboratory formulate and they tell stories of themselves and one another—layer upon layer of stories. Then *I* formulate and *I* tell stories of them: my stories, too, are just a further moment of productive but parasitic story tellingSo what is the justification of my story telling? The answer has to do with patternsLook upon it this way: the search for pattern is an attempt to tell stories about ordering that connect together local outcomes. (p. 19)

My study drew on Law’s (1994) ideas, endeavoring to avoid reductionism but to identify patterns in the stories told and repeated by the managers of their experiences of the program and to find the areas of overlap of a number of different disciplines and theoretical ideas to interpret these patterns.

Assessing the Validity of Interpretation

Despite the denial by many qualitative researchers (as discussed earlier) that validity is not an appropriate construct for their research, most strive nevertheless to find alternative forms of evidence that their research is trustworthy and credible.

Fontana and Frey (1994) raised the issue of validity in interpreting data: “More recently sociologists have come to grips with the reflexive, problematic and, at times, contradictory nature of data and with the tremendous, if unspoken, influence of the researcher as an author” (p. 372).

There were many issues to be addressed in interpreting the data. The major question was one of voice, that is, the extent to which the interviewees would feel confident in expressing their beliefs and feelings, particularly given that the company had sustained a strong fear culture over many years. This was a good reason for meeting the participants in the relaxed setting of the bar after the courses, as well as during the workshops and in the privacy of their own workplace. It was vital to reassure the respondents that their responses would remain confidential to encourage them to express their beliefs and feelings as freely as possible. It became clear that the level of empathy reached in the second interviews was considerably higher than in the first interviews, as trust had already been established. This was reflected in the deeper quality of data gathered in the second phase.

Altheide and Johnson (1994) offered a useful definition of *validity* for the interpretive researcher:

All knowledge and claims to knowledge are reflexive of the process, assumptions, location, history, and context of knowing and the knower. From this point of view, validity depends on the “interpretive communities”, or the audiences—who may be other than researchers and academics—and the goals of the research. (p. 488)

This is a more compelling definition than that of Hammersley (1990), who simply said, "An account is valid if it represents accurately those features of the phenomena that it is intended to describe or theorize" (p. 69).

For Altheide and Johnson (1994), the focus is on the process of the ethnographic work, which they labeled *analytic realism*: "The process by which the ethnography occurred must be clearly delineated, including accounts of the interactions among context, researcher, methods, setting, and actors" (p. 489).

I was used all aspects of context to interpret each interviewee's responses. Their environment, demeanor, reactions of colleagues, reaction to the researcher, and many other factors were taken into account when interpreting and sense making from the interviews. Field notes were taken immediately after the meetings to ensure my thoughts and interpretations at the time of the interviews and discussions were not lost. At times, the transcripts were listened to orally as well as read, to search for nuances that might have been lost in the written text.

Central to the ethic of analytic realism "is the renewed realization that all knowledge is perspectival, so the ethical practice of ethnography demands that the author's perspective be identified" (Altheide & Johnson, 1994, p. 490).

As Czarniawska-Joerges (1992) so lucidly put it:

It is quite obvious that none of us is a tabula rasa in any sense of the word, and the only way to counteract these biases and initial expectations is to self account for them, as deeply and honestly as is possible without boring the readers too much. (p. 193)

I was very aware of my own perspective throughout the research, although in my line of questioning I consciously did not allow it to be revealed so as not to influence the responses.

In discussing the interpretation of the participants' voices, Altheide and Johnson (1994) went on to suggest that

a key feature of this knowledge, of course is its incompleteness, its implicit and tacit dimensions. Our subjects always know more than they can tell us . . . the key issue is not to capture the informant's voice, but to elucidate the experience that is implicated by the subjects in the context of their activities as they perform them. (p. 493)

This is supported by Czarniawska-Joerges (1992): "What we can reach is ultimately only our representations of their representations, because our representations are the last link in a chain of perceptions, opinions and concepts" (p. 195).

Both comments were very true of this research. The researcher's voice must therefore always be acknowledged, as asserted by Stake (1994), who returned to the theme of research as storytelling:

Even though committed to empathy and multiple realities, it is the researcher who decides what is the case's own story, or at least what of the case's own story he or she will report. More will be pursued than was volunteered. Less will be reported than was learned . . . Many a researcher would like to tell the whole story but of course cannot; the whole story exceeds anyone's knowing, anyone's telling. (p. 240)

It was certainly a frustrating experience selecting from the numerous pages of coded transcripts those quotations that might most fairly and interestingly represent the voices of those interviewees. Categorizing the issues identified by them helped to some extent, but there is no doubt that another researcher might well have selected different quotations and thus placed a slightly different nuance on the story. It also has to be admitted that some interviewees' voices were heard more than others in the interpretive chapters of my report. This was not a deliberate decision, but it is inevitably the case that some participants will be more quotable and more interesting in their observations than others. As in storytelling, some orators bring stories to life more than others do, and the researcher is inevitably drawn to such accounts. On reflecting on this inadvertent selectiveness to check that I had not inadvertently selected accounts that reflected my own preconceptions, I was confident that those most extensively cited were not representative of any particular perspective nor of any specific type of manager but appear to be more a reflection of these managers' ability to articulate their thoughts.

Qualitative researchers are constantly seeking ways of validating their findings and reinforcing the interpretations they are making. Therefore, Stake (1994) suggested that "to reduce the likelihood of misinterpretation, we employ various procedures, including redundancy of data gathering . . . for qualitative case work, these procedures are generally called *triangulation*" (p. 241).

In this case study, a surplus of data was gathered, until it reached the point at which I felt I was not hearing new insights but repetitions of the previous ideas. The interview texts were triangulated with the notes taken by the researcher during the participant observation at the modules, to identify patterns, contradictions, or anomalies, particularly when tracking the views of particular individuals over time.

Until this stage, I had consciously tried to avoid being explicitly influenced by previous studies of corporate change programs and the lives of middle managers. Once the coding of the Aeroco data had been completed, however, the emergent themes were then compared with the findings of a number of previous studies. It was fully recognized that valid comparisons could not be made between the cases, owing to important differences in research paradigms, time scales, industries, methodologies, and numerous other dimensions. Nevertheless, it was considered that an understanding of the similarities and differences between these studies might offer additional

insights into the “Worldclass” program and might assist in illuminating the researcher’s understanding of the Aeroco managers’ responses to it.

Cassell’s *New French Dictionary* defines *bricoleur* as a “jack of all trades.” Denzin and Lincoln (1994) have extended this metaphor of bricolage to the “piecing together” of a “close knit set of practices that provide solutions to a problem in a concrete situation” (p. 2).

In the process of this research, I was conscious of undertaking a “piecing together” of scholarly insights from a range of paradigms and methodologies, as though building a new but unknown construction, and therefore doing exactly what Denzin and Lincoln (1994) have referred to as bricolage. As this bricolage evolved, it provided a variety of insights during the process, and as each piece was added to the construction, a new and interesting perspective invariably evolved, providing fresh insights into the case as each “block” was added.

Reading the case through the ontological lenses of social constructionism, which foregrounds the social construction of meaning, followed by symbolic interactionism, which focuses on the process of social exchange through language, provided different but rich readings of the case. Foucault’s (1988a, 1988b) poststructuralist work on subjectification and the project of self added an extremely helpful interpretive framework that offered new insights into the ways that self-identity is shaped in and through organizations. Finally, the radical humanist paradigm (Burrell & Morgan, 1979; Parker, 2000), which seeks to critically interpret empirical data, added to my understanding of meaning, power, and control in Aeroco. Although a purist might argue that applying different ontologies to produce different readings is not defensible, my own experience was that each of these lights shone on the case from different angles, illuminating it in new and exciting ways and therefore adding to the robustness of the emerging theory.

Researching Ideology, Emotion, and Identity

Having taken the view that it was the middle managers as consumers of change programs who had been most neglected by the existing theory, a decision then had to be made as to where to enter the vast field of existing relevant literature. Initially, the most obvious literatures to engage with appeared to be those addressing organizational culture and change, yet it was felt that these had been so extensively researched that they offered little scope for original thinking. Managers as catalysts of change, change agents, and resisters of change had already been well explored, but all of these appeared to analyze managers at the behavioral level. What appeared to be missing was an account of their experiences as recipients of these programs, their varying responses, and the emotions they experienced when immersed

in such programs. What, for example, led one manager to resist but another to embrace a program wholeheartedly? How much of the managers' behavioral change was deep rooted and how much was superficial playacting? It was clear that to find fresh insights into these questions I would have to look beyond existing theories of change and culture.

The fields of ideology, emotion, and self-identity, combining to produce meaning in organizations through their interaction, seemed to offer opportunities for new and original insights into the already well-trodden research into corporate change programs. In addition, they seemed to focus more adequately on the areas of concern I had already highlighted, such as power, discipline, and control, and, it was felt, would assist me in applying critical thought to the issues that might emerge from the research.

It is clear that the study of emotion in organizations is one of the most difficult areas for empirical research, as it is so well hidden and multilayered: "Capturing emotion in process requires some methodological ingenuity" (Fineman, 1993, p. 222). However, it was also clear that we can no longer continue to avoid the subject. Fineman (1993) stated,

Emotions are central to the constitution of the realities we so readily take for granted in our working and organising. Once we strip the facade of rationality from organizational goals, purposes, tasks, and objectives, a veritable explosion of emotional tones is revealed. (p. 1)

Researching emotion in an organizational setting was clearly best approached using an ethnographic methodology, owing to the requirement of building up high levels of trust with the participants of the research. This worked well during the participation at the modules, when trust could be built up with other delegates over several days. Researching the concept of self-identity was equally problematic, as participants are not always aware of how their identities are constructed or of the influences on them. This puts considerable onus on the researcher to act with integrity in interpreting and writing up the stories she or he has heard.

Researching ideology in organizations is also problematic. Ideologies have always been accepted as the most deep-rooted and inaccessible aspects of organization life (Hofstede, 1980; Schein, 1985). This was further complicated by the multiple interpretations of the term and by the hidden archaeologies of power inherent in ideologies (Fairclough, 1992; Foucault, 1977). Yet focusing on organizational ideology, however elusive a construct, is also considered to offer one of the most rich and important interpretations of organizational life today because, as Fairclough (1989) reminded us, "ideology is the prime means of manufacturing consent" (p. 4), or, in other words, "ideological power, the power to project one's practices as universal and 'common sense', is a significant complement to economic and political power" (p. 33).

Despite the complexity associated with the constructs of emotion, self, and ideology, the field and topic chosen for research was considered to offer rich opportunities for uncovering important qualitative insights into the way the managers in the study understood their lives, relationships, and values and an important contribution to the theory of change management in HRD programs.

Conclusions

The insights that emerged from the case were rich and varied, demonstrating the value of theory building through bricolage. A typology of managerial responses was constructed that demonstrated six predominant response types, some of which shifted considerably over the period of the study. A number of important influences on the managers' responses to the program were found, many of which were related to their social environments and relationships, often being described in emotional rather than cognitive language. The more extreme responses to the program of evangelism and cynicism were found to be rarer than the responses of those who described themselves as "sitting on the fence," but these extremes were most prone to change, with a number of evangelists becoming cynics when trust was broken through inconsistent behaviors. Conversely, some cynics described conversion experiences using highly emotive quasi-religious imagery. More detailed accounts of the findings are contained in Turnbull (1999, 2000).

This article has argued that qualitative research in HRD can make a valid and useful contribution to theory building and can be complementary to more quantitative approaches, particularly when researching the affective and political domains in organization. I have argued in favor of Denzin and Lincoln's (1994) bricolage metaphor as a legitimate way to develop a methodology and have demonstrated that rigor of approach is vital for a qualitative researcher seeking credibility for her or his research. Qualitative research is not the easy option that some of its critics have suggested. It demands considerable thought, reflexivity, and close analysis of data to generate the themes and develop convincing and robust theory from the data. It demands political and cultural sensitivity and empathy and is time-consuming, but it offers an important and complementary approach to HRD theory building.

Notes

1. Mangham (1987) referred here to the phenomenological or social constructionist perspective.

2. This follows some of the advice of Glaser and Strauss (1967) and Corbin and Strauss (1997). Grounded theory as a research paradigm was a highly influential component of my research bricolage, particularly in its data-gathering and coding stages. It diverges from my

own research in its positivist goals of verification and causality, which differ from my constructivist purposes of building theory to understand the construction of meaning and identity by the managers in this case.

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