

Accounting and Organizations: Realizing the Richness of Field Research

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Abstract: Over the past 15 years, literature has seen a considerable growth in field work, motivated by the quest for rich descriptions of accounting in action. Despite a lively methodology debate on the topic, the question of how richness can be realized in field studies has not been directly discussed. This paper seeks to address this gap. It suggests a focus for rich studies of accounting and points to three issues that researchers need to consider to produce rich accounts. It then discusses six studies of accounting, showing how different decisions on these issues can inform different styles of research. It concludes with some observations on the research process itself.

INTRODUCTION

Over the last decade and a half, management accounting research has experienced something of a renaissance. Lamenting the scarcity of empirically grounded studies of accounting in action, Hopwood (1983, 302) commented "how little was known of the accounting endeavor." Similarly, Kaplan (1984, 415) commented on researchers' "reluctance to get involved in actual organizations and to muck around with messy data and relationships." In contrast, just a few years later, Ferreira and Merchant (1992) were able to review 82 field studies published in the period 1985-1992, and more have been published since that time. Although motivated by a common concern for an apparent cleavage between accounting research and practice, Hopwood (1983) and Kaplan's (1983, 1984) much cited calls to the field were advanced with rather different agendas. For Kaplan (1983, 1984), changes in organization and technology were rendering traditional accounting and control systems obsolete. The challenge was to learn about innovative, leading-edge practices in successful firms. Hopwood (1983), in contrast, sought to move research beyond the technical, to illuminate the way in which accounting was intertwined in the fabric of organizational life and how it related to the wider social context in which it was embedded.

Nevertheless, both sought a commitment to study accounting in its organizational contexts. A new, "rich" literature was regarded as the way to deepen our understanding of the functioning of accounting. Hopwood (1983, 296), for instance, referred to the "rich insight" available in studying accounting from an organizational perspective and Kaplan (1986, 445) wanted to read about "rich slices of organizational life."

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Yet, the ways in which researchers can realize the potential of field research to provide rich accounts of the often very complex relationships between organizational contexts and the functioning of accounting have not been discussed in any detail. Our purpose in this paper is to address this issue. We are impressed with the tensions and ambiguities that often characterize accounting in action, with the sometimes contradictory ways it is drawn upon by actors in organizations and how it can constitute organizational life differently in different settings. We believe capturing these ambiguities, tensions and contradictions to be a major opportunity offered by field research methods. Field research yields data and observations which are usually suggestive of theme and counterpoint, of interpretations and counter-interpretations. In our opinion, field studies should be written with a view to bringing out the different voices around accounting in organizations. Too often, we feel, this information is suppressed in published work.

There are many ways to conduct field research. In this paper, we attempt to articulate a rather particular way of researching accounting in complex organizations and writing about it, a manner with which we are comfortable. Our intention is to guide others who may wish to conduct similar research. The paper is structured as follows. In the first section, we propose an orientation for such studies in accounting and point to three issues that need to be considered in producing rich accounts. The second section discusses six field studies in the accounting literature, showing how decisions on these three issues inform different styles of research. In the third section, we address the research process itself and, in particular, the movement from entry to the field, through data collection and interpretation, to the production of publishable material. The fourth section concludes the paper.

RICH ACCOUNTS

As Ferreira and Merchant (1992) discuss in their review, the burgeoning field studies literature is quite eclectic. Researchers have different motivations for doing field research and adopt different research designs. Some set out to describe technical practice. Cooper and Kaplan (1987), for example, documented the cost distortions introduced by traditional product costing systems in three organizations. Patell (1987) documented cost accounting innovations in a just-in-time context. Innes and Mitchell (1990) described management accounting innovations in high-technology firms. Johnson (1990) described innovations in performance measurement. These researchers contribute to an understanding of accounting as technique. Consistent with Kaplan's (1984) challenge to describe and document innovative practices, the dominant emphasis is on technical properties of new systems of calculation and their relevance to actual or theoretical classes of management decisions.

Other field researchers prefer to focus on the linkages between accounting and management or organizational processes. For example, Dent (1987) described some effects of control systems on management behavior. Knights and Collinson (1987) discussed the disciplining effects of accounting on the shop floor. Preston (1986) described the complex informal arrangements that managers initiated to inform themselves and each other.

Keating and Jablonsky (1990) discussed the changing role of contemporary financial management. For these researchers accounting is an organizational phenomenon, embedded in a social system. Consistent with Hopwood's (1983) call to explore accounting's interrelationship with organizational life, the emphasis is less on technique and more on the roles and interpretations of accounting in use.

This distinction between the technical and organizational aspects is often less than clear cut, for many researchers recognize a degree of interaction between the two. But there are certainly differences of emphasis. Some bring accounting technique to the foreground of analysis, leaving the organizational dimension in the background. Others bring the organizational perspective to the foreground, leaving technical properties in the background.

Another distinction concerns sample size and the number of organizations studied. At one extreme, some researchers focus on single organizations or sites. The studies of Patell (1987), Dent (1987), Knights and Collinson (1987) and Preston (1986), mentioned above, fall into this category. At the other extreme, some have much larger samples. Bruns and McKinnon's (1992) study of performance evaluation and incentives covered 12 companies, as did Merchant's (1989) study of 54 profit center managers. Even here though, distinctions are not always clear cut. Simons (1987, 1990, 1991) increases his sample size from one business unit in the 1987 study to 30 business units in 17 companies in the 1991 study. His 1990 study draws on 16 companies, but only focuses on two.

Notwithstanding these rather crude distinctions between different types of field study, one could imagine structuring the literature through a two dimensional graphical array in which studies are positioned according to their focus (technical vs. organizational) on one axis and sample size (from single case studies to comparatively large samples) on the other. The point of this delineation is not to suggest that some combinations of focus and sample size are superior to others, *per se*. Small sample studies typically have different objectives than large sample studies; and researchers with a technical focus aim to capture different aspects of the relationship between systems and organization from those with an organizational focus. Our point, rather, is to delineate our message in this paper. Our interest here lies in small samples addressed from an organizational perspective. It is toward this combination that our comments on richness are primarily directed. Richness presupposes a deeper appreciation of accounting in organizational and social settings and of information more broadly. Small samples typically permit closer engagement with the field than large samples. Rich descriptions of organizational practice build on such closer engagement.

The growth of field studies in management accounting has lead to literature on methodological questions of field research. Among the issues addressed in this literature are ontological and epistemological assumptions underlying fieldwork (Tomkins and Groves 1983), how to cope with threats to validity and reliability in field studies (McKinnon 1988), how such studies complement other research methods (Birnberg et al 1990), how one can generalize from qualitative research as compared to quantitative research (Lukka and Kasanen 1995) and how field studies fit into

the theory generating process overall (Keating 1995). The dominant concerns of this literature are with engineering "reliable" research which can be ordered into the scientific study of accounting.

In a more practical vein, a limited number of papers have addressed the conduct of field research itself. Covaleski and Dirsmith (1990) give some insights into how iteration between different theoretical approaches helped them to order and understand their own, often confusing, field research material. They explain how contradictions and loose ends which emerged during the research process offered opportunities to adjust the direction of their inquiries to learn more about the conflicting meanings and tensions which are almost always found around organizational uses of accounting. Consistent with such iterative method, Parker and Roffrey (1997) provide a tutorial on the application of Glaser and Strauss' (1968) grounded theory to accounting research.

These methodological and practical debates are ongoing. However, conspicuous by their absence are discussions of the goal to be realized by field research itself, the "rich insights," mentioned above, to be gained from "rich slices of organizational life." While a prescriptive definition of a rich study is problematic, because it should bring out the specificity of the very particular contexts in which accounting functions, the presentation of rich studies probably needs a rather more developed concept than that applied, for example, by Ferreira and Merchant (1992, 4). They seem to equate "rich" with "detailed." Detailed on contexts and practices it should be, but the rendition of the multi-volume accounting guidelines of a large firm is not rich. Rich has more to do with making understandable the actions and motivations of often very skillful people who routinely mobilize accounting in their daily work lives.

At its simplest, rich could generally be taken to mean life-like, born out of recognizable organizational contexts. But it is instructive to speculate further about what aspects of accounting in these organizational contexts the researcher ought to try to communicate. We might take a lead from the use of accounting to permit control at a distance. Senior management, from sometimes distant head offices, often intervene in local organizational contexts through accounting (Roberts and Scapens 1985). Conflict may be experienced when local management see themselves accountable to abstract, central accounting measures which appear unjust in the local context. According to Argyris (1977, 116-117) this conflict is inherent in formal management information systems:

The sheer information processing requirements and the costs that would be necessary to assure minimal misunderstanding and injustice may be so high that such assurances [of justice] would not be possible. Employees who are responsible and loyal understand these constraints but, in doing so, place themselves in a dilemma. If they accept the high probability of injustice as necessary, then they have acted to legitimize injustice. If they do not accept the necessity of injustice, they would be seen as disloyal. Those at the upper levels may find it necessary to defend themselves from the dilemma of having to be unjust in order to make the organization effective.

In our own experience, the tensions pointed out by Argyris (1977) are a fruitful source of rich insights into accounting in action. Argyris (1977)

casts this conflict in terms of tensions between local and central concerns. We might equally cast it in terms of the rather traditional distinction between line and staff. Staff, for example, often appeal to generalized frames of reference and/or standardized procedures and systems. These may be in conflict with the very particularized and immediate frames of reference of line managers, at least those concerned with local activities or operations. Or we might cast it in terms of tensions between different functional groups—research and development, manufacturing, sales and service, for example—or between commercial and operating managers. Rich field research could usefully focus on such conflicts that arise in organizations and the interpretive tensions which give rise to them.

The calls for richer research in management accounting mirror similar concerns in the organizational sciences. Some have argued that organization theory, like accounting, has been too loosely tied to actual organizational experiences. Pettigrew (1985, 15), for example, described the organizational change literature as largely “ahistorical, acontextual and aprocessual” (emphasis added) and there have been repeated calls for more qualitative, contextual and interesting research (e.g., Mintzberg 1979; Morgan 1983; Smircich 1983; Van Maanen 1979, 1988). Many issues relating to field research have also been discussed in the organizational literature. Yin (1984), for instance, has analyzed methodological approaches to case study research. Miles and Huberman (1994) have outlined a variety of techniques for analyzing qualitative data (see also Denzin and Lincoln 1994). Strauss and Corbin (1990) and Glaser (1992) have differentiated their positions on the methods of grounded theory.

One exchange of particular interest concerns sample size, or the number of organizations studied and depth vs. breadth in field research.¹ Cast in terms of constructs vs. stories, it centered on Eisenhardt's (1989) elaboration of Yin's (1984) method for generating theory from multiple case studies. Drawing on her own research on strategic decision making in eight firms (Eisenhardt and Bourgeois 1988), Eisenhardt (1989) outlined a structured nine-stage approach for researching multiple cases, complete with *a priori* specification of the research problem, theoretical constructs, research instruments and protocols and methods for cross-case replication and comparison. Her emphasis is on the use of contrasting observations from multiple cases to create theoretical constructs.

Eisenhardt's work with Bourgeois, in 1988, is highly readable, with lots of examples and quotations. But it focuses on theoretical constructs, with rather limited attention to context and the role those constructs played in particular settings. Dyer and Wilkins (1991), in a strong critique of Eisenhardt's (1989) formula method, argued that her approach placed so much emphasis on the constructs that it failed to communicate the rich background of each case. As a result, the reader was denied an exemplar, “a story against which researchers can compare their experiences and gain rich theoretical insights” (Dyer and Wilkins 1991, 613). They contrasted the “thin” case descriptions in her strategic decision-making work with the deep insights of the classic (single) case studies in the

¹ For a related discussion within anthropology see Aunger (1995) and the nine comments from different academics which were published as part of his piece.

organizational and sociological literatures, in particular Dalton (1959), Whyte (1943), Gouldner (1954) and Lipset et al. (1956).

Dyer and Wilkins (1991, 616–617) state:

[B]ecause Eisenhardt argues that the more cases a researcher studies, the better (within certain limits) for generating theory, she seems to lose the essence of case study research: the careful study of a single case that leads researchers to see new theoretical relationships and question old ones....

Those who would attempt to use Eisenhardt's method are necessarily constrained by the number of cases that will be studied and descriptions will be rather "thin," focusing on surface data rather than deeper social dynamics....

However the key issue is not...the number of cases....The central issue is whether the researcher is able to understand and describe the context of the social dynamics of the scene in question to such a degree as to make the context intelligible to the reader and to generate theory in relationship to that context....

Dyer and Wilkins (1991) see theorizing and the presentation of field materials to be closely related:

[T]he classic case study approach has been extremely powerful because these authors have described general phenomena so well that others have little difficulty seeing the same phenomena in their own experience and research. We return to classics because they are good stories, not because they are merely good statements of a construct. Indeed, the very clarity of the constructs stems from the story that supports and demonstrates them....

We can experience vicariously the relationships and ideas presented. We therefore remember them longer and understand them more complexly than had they been presented as a thin description of a construct or as a statistical table.

Eisenhardt (1991) mounts a robust defense, in which she argues *inter alia* that many of the authors of the classic case studies actually drew on broader experiences than those they gained in the focal organizations and that cross-case replication in structured research designs is "simply good science" (Eisenhardt 1991, 624). Further, she argues, the power of deep studies is an illusion. Their striking impact is a result of cognitive bias:

Individuals inappropriately overweigh the information value of a story as compared with more abstract data. So, contextually rich stories lure people into thinking that they know more than they do. (Eisenhardt 1991, 626)

At issue here are at least three themes which are pertinent to field research in accounting. One concerns the trade-off between deep understanding of a particular social or organizational setting and the benefit of replication and comparative analysis. A second theme addresses the process of theorizing, particularly the synthesizing of material from the field into patterns. Thirdly, the exchange between Eisenhardt (1989, 1991) and Dyer and Wilkins (1991) points to the issue of imposing prior theoretical constructs on the materials to be found in the field, in contrast to a more

emergent approach which would allow theoretical constructs to unfold through the research.

Deep Understanding

While we have sympathy for Eisenhardt's (1989, 1991) concern to replicate across cases, constraints of space and time generally dictate that breadth comes at the expense of depth—the more contexts studied, the less contextual insight can be communicated. We simply cannot expect much insight about individual cases when, say, five or more cases are considered in a journal-length article. Is replication across a large number of cases strictly necessary for theory, or can rich stories produce good theoretical insights on their own?

Of course, the answer depends in part on one's definition and understanding of the term "theory." Too easily, we feel, are rich stories rejected as "non-theoretical," because people forget that theory can be speculative. Pertinent here is Weick's (1995, 386) quotation of Runkel and Runkel (1984, 129–130), who suggest that "Perhaps some social scientists yearn for a Theory that Sweeps Away All Others" and thus avoid the word theory for intermediary speculation; but, they continue, "theory belongs to the family of words that includes *guess, speculation, supposition, conjecture, proposition, hypothesis, conception, explanation, model*" (italics in original).

With this broader notion of theory in mind, the evidence of the classic studies suggests to us that rich stories can produce good theory. Whyte's (1943) *Street Corner Society* leaves us with powerful insights into informal social structures and street gang racketeering in Eastern City's "Cornerville." Gouldner's (1954) *Patterns of Industrial Bureaucracy* gives a detailed and thorough understanding of the power and dysfunctional effects of the bureaucratic rules of his gypsum plant. Dalton's (1959) *Men Who Manage* leaves us with an indelible image of informal vs. formal structures in his Milo plant. Although grounded in the authors' lived experiences of particular contexts, these studies are replete with theoretical insights that extend beyond the stories themselves. Moreover, their theoretical insights are often more powerful and memorable than those generated from large-sample field research, because these authors got as close as possible to the field and demonstrated theoretical constructs through their application in ongoing social settings.

Eisenhardt (1989, 1991) cautions against the power of such stories and it surely comes as no surprise that stories are a more immediate way of conveying knowledge about organizations than detached abstractions. But this does not necessarily mean that they lure people into thinking that they know more than they do, at least if the stories are told responsibly. The classic studies are generally cautious in their claims to generality. They have points of ambiguity and leave many loose ends. Their grounding of theoretical constructs in lived experience leads to a more complex understanding of theory, not an illusory one, for abstract theory is often "innocent" in the particular. Moreover, Eisenhardt (1989, 1991) herself wishes to tell a story. Her formula method emphasizes comparison; she would have us construct "comparative grids," as it were. This, too, is a particular kind of story-telling, one which appeals to comparative ideals.

In comparative case research, just as in single case research, the researcher has to make choices about what to study (or what to compare) and what to present. As White (1978, 43) comments:

[N]o given set of events attested by the historical record constitutes a story manifestly finished and complete...we give our lives meaning by retrospectively casting them in the form of stories.

A story, be it comparative or otherwise, must be told.

A frequently raised concern about small sample work is that it is scientifically unsound. Interestingly, Campbell (1988), a methodologist who had otherwise done much to support calls for measurement and quantification in social science, criticized this charge. Commenting on anthropologists' use of case studies in particular, Campbell (1988, 377-380) explains:

[L]et me try to correct some of my own excesses in describing the case study approach. The caricature of the single-case study approach which I have had in mind consists of an observer who notes a single striking characteristic of a culture, and then has available all of the other differences on all other variables to search through in finding an explanation. He may have very nearly all of the causal concepts in his language on which to draw. That he will find an "explanation" that seems to fit perfectly becomes inevitable, through his total lack of "degrees of freedom."

However, he continues,

[I]t now seems clear to me that...I have overlooked a major source of discipline....In a case study done by an alert social scientist who has thorough local acquaintance, the theory he uses to explain the focal difference also generates predictions or expectations on dozens of other aspects of the culture and he does not retain the theory unless most of these are confirmed. In some sense, he has tested the theory with degrees of freedom coming from the multiple implications of any one theory.

Referring to the surprise a researcher usually gets in the field, Campbell comments:

[A]lmost invariably the social scientist undertaking an intensive study, by means of participant observation and other qualitative common-sense approaches to acquaintance, ends up finding out that his prior beliefs and theories were wrong....This is an important fact....It shows that the intensive cross-cultural case study has a discipline and a capacity to reject theories which are neglected in my caricature of the method.

Campbell (1988) points to an implicit rigor in intensive case study research. Discipline and degrees of freedom come through the richness of detail that theories have to explain. A conscientious, sensitive, self-critical researcher discards or modifies unsatisfactory theories. He or she may go on to develop others. The classic case studies, mentioned above, had a profound effect on social science theorizing, introducing new concepts for the interpretation of organizational and social phenomena. By implication, the power of their theoretical insights was attributable, at least in part, to

the inability of extant theories to explain the rich detail observed in each case.

The Process of Theorizing

A second theme that we draw from Eisenhardt's (1989, 1991) exchange with Dyer and Wilkins (1991) concerns the process of theorizing itself in field research and the relationship between theory and data. The use of the term "story" applied to case studies could be taken to imply fiction, in a pejorative sense. That is not intended. Most accept a clear distinction between social science and fiction, at least in principle. Social scientists discover things; novelists make them up. Social science attempts to be faithful to observation of the real world, moving systematically from field material, through interpretation, to explanation. In contrast, fiction relies on an imaginary world. To denigrate case studies because they are stories, as Geertz (1988, 140) points out, is to confuse "making things out" with "making things up."

That is not to deny that case studies are creative texts. A recurrent fear in all field research is the possibility of "drowning in the data." The field researcher searches for a pattern, synthesizing observations into recurrent themes. The story is the product of this patterning of field material. Seeing patterns usually requires imagination and creativity. In this sense it is artistic. But it is not purely artistic, it represents the world observed. The researcher examines and re-examines existing observations and gathers more field material, to ensure that the patterns adequately represent the observed world and are not merely a product of his or her imagination (cf. Whyte's (1943) appendix). Seeing patterns and developing theory is an emergent process in field research, in which the researcher iterates between insights and the field material.

This process of seeing patterns is often the most difficult and least codified part of field work. Miles and Huberman (1994, 2) comment:

We do not really see how the researcher got from 3,600 pages of field notes to the final conclusions, as sprinkled with vivid quotes as they may be.

In extreme cases, one is sometimes suspicious that the researcher moves from theoretical conclusions to the field material, presenting carefully chosen examples to support the points being made, rather than moving from observations to theoretical conclusions. Such practice is inadequate and irresponsible. The researcher is accountable to the reader for the integrity of his or her method. This extends to presenting material in sufficient depth so that links from data to theory can be traced. The researcher has an obligation to convince the reader of the validity of the case description and analysis.

Social science has traditionally had a commitment to the separation of data from theory. But, as Krieger (1984) notes, in field research stories often create theories, they are not just evidence for them. Case studies deal with specifics—specific individuals or groups in specific settings. Stories are told in highly concrete terms. But in following the stories, we learn not only about the specifics, but about interrelationships and patterns in a broader whole. It is not theoretical language that builds theory here, but

recognition of the patterns and interrelationships across very particular experiences. Theoretical explanations are not usually provided in abstract or theoretical terms—although they may be in the end. They emerge because the stories lead us through a world in which we vicariously experience how individual actions relate to each other and add up to a coherent whole.

This interpenetration of data and theory is sometimes disturbing. The “specificity” of the story and its “suggestiveness” can effectively produce “a world which is explanation for itself” (Krieger 1984, 273). The specificity of the story persuades us of its realism, as if made of fact; and its structure is suggestive of theoretical explanation. The result may be so compelling as to obscure the inescapable truth that the reality conveyed, however carefully researched, is only one of a possible number of interpretations. Crapanzano’s (1986, 74) comment is pertinent here: “There is only the constructed understanding of the constructed native’s constructed point of view.” This realization has provoked much debate, particularly in the anthropological literature (Clifford and Marcus 1986; Clifford 1988). Classic ethnographic stories are charged with incorporating linguistic devices that obscure the uncertain and personal nature of the “truths” presented, in that they produce these “truths” through rhetorical and stylistic persuasion. The researcher’s responsibility to the reader thus extends to presentation and literary style.

Diaries, charts and records of interactions and observations—the documents of field research—are of course crucial in establishing the credibility of stories. So is critical reflection on the pattern of interrelationships the field researcher perceives and its inclusiveness. But the researcher also needs, at a minimum, to distinguish informants’ literal, unedited statements from the researcher’s own opinion and to situate speech statements (who said it? when? why? to whom?). He or she also needs to think very carefully about the structure of the story, for to structure an account is effectively to provide an explanation. The researcher may also want to adopt further rules of presentation. Krieger (1984) refers to two that she followed in her own work. The first, although rather extreme, is suggestive of the type of literary device that can be applied (cf. Tyler 1986). It was to construct her account almost entirely by paraphrasing from her interview and documentary evidence. According to this rule she would allow herself little interpretive comment in the body of her text, her only commentary being in connectives moving the reader from one paraphrased passage to the next. “My evidence, faithfully interpreted, would have to do the work for me,” she says (Krieger 1984, 279). The second rule was that her account should not be partial to any one person or point of view, representing “a particular angle of vision, line of interpretation or a single theory.” Rather, it should capture the complexity of the composite whole, setting out “a pattern in which everything was important” (Krieger 1984, 279).

Thus, in Krieger’s (1979) work *Hip Capitalism*, the story of an underground radio station, her account was written so as to guide the reader through the station’s emergence, legitimization and commercial success. Theory was implicit and revealed through the selection and ordering of her observational and interview data. In Krieger’s (1983) work, *The Mirror*

Dance, about a women's community, she applied her rules even more rigorously, reproducing transcripts almost verbatim and denying herself connectives save to introduce different interviewees. The views of different members of the community are presented in such a way that things that initially seem obvious become more complex as further, sometimes contradictory, evidence is introduced. Readers have almost constantly to revise their understanding of the community and redefine their theory to accommodate layer after layer of material.

Theoretical Constructs vs. Emergence

A third theme that we draw from Eisenhardt's (1991) exchange with Dyer and Wilkins (1991) relates to the extent that theoretical constructs should be used to filter contextual information in field research. Eisenhardt (1989) articulates a research process which starts with a clear research focus and precise and measurable constructs. She describes how she and Bourgeois (Eisenhardt and Bourgeois 1988) identified important constructs from the literature and devised interview protocols and questionnaires to measure them, prior to their field research. Dyer and Wilkins (1991, 617) are critical that this approach will merely "confirm, disconfirm, or build upon existing theories," rather than exposing "new relationships, new orientations, or new phenomena that current theory and theoretical perspectives have not captured." In this regard, Krieger (1984, 284) comments that,

[T]he worlds we describe in social science too often reflect a limited set of currently fashionable views. We tell the same stories over and over; we paint the same pictures. We don't let in many dimensions of the outside world. Although our methods are designed to prevent this, what happens most often is not that our arguments follow from our evidence, but just the reverse. Our models are not only abstract, but also out of touch...."

Eisenhardt (1989, 536) talks of her constructs and research focus as tentative, noting that "preordained theoretical perspectives or propositions may bias and limit the findings." She talks of "taking advantage of serendipitous findings" and the "ideal of a clean theoretical slate." Nevertheless, her strong *a priori* conceptualization stands in some contrast to the theoretical emergence characteristic of the classic case studies, the authors of which allowed their analyses to emerge over time. Taken to extremes, her method forces theoretical constructs onto the data, rather than allowing constructs to emerge from the data.

Of course, no well-informed researchers can deny their theoretical training, nor would they want to, for theoretical sensitivity is essential to good field research. What concerns us here is the danger of losing information by over-filtering rich field material through explicit theoretical concepts. Weick (1995, 387) observes:

Data by themselves are not theory ... [but] theorists also need to be attentive to Starbuck's (1993) argument that, just as the best medical doctors treat symptoms directly *without* relying on diagnosis to determine treatments, the best theorists may make prescriptions based on data alone

without introducing theory between data and prescriptions. In both cases, diagnoses and theories come last and summarize observed relations between treatments / prescriptions and symptoms / data. In both cases, there are more combinations of symptoms than there are diagnoses or theories, which means that translating symptoms into diagnoses discards information. Since there are also more treatments than diagnoses, basing treatments on diagnoses injects random errors. The key links are between symptoms and treatments, with feedback from treatments making these links clear. Once these effects become clear, *then* the theorist knows better what is being treated and can attempt a diagnosis or explanation (italics in original).

Clearly, our ideas apply to some kinds of theory more than others. DiMaggio (1995, 391) distinguishes three types of theories: theory as covering laws, theory as narrative and theory as enlightenment. Theory as covering laws, in the extreme, is only concerned with the demonstration of a high R^2 and not its explanation. Theory as narrative ranges from exploratory hypotheses detailing regularities in relations among variables together with plausible accounts of how action could produce the associations observed, to formally modeled principles predicting distributions of outcomes. Theory as enlightenment inspires new ideas through novel or paradoxical views of the world—the garbage can model of organizations is an example. While field work can support different types of theoretical work, it is clear that we are not talking of theory as covering laws. Rich case studies are certainly congruent with the notion of theory as narrative. They may also be well suited to more speculative or even paradoxical reasoning to generate theory as enlightenment.

In presenting these three themes of depth, theorizing and emergence, we have sought to draw on the organizational literature in order to prepare the ground for a more focused discussion of accounting field work. We find those themes helpful in that they draw attention to issues that must be considered in undertaking small-sample field research and in producing rich accounts: in particular the question of depth vs. breadth, the approach to theorizing and the extent to which theoretical constructs should be allowed to emerge in the field. We lean toward Dyer and Wilkins' (1991) position in their debate with Eisenhardt (1991), in that we feel it offers more potential for generating rich field studies. But we do not want to privilege their position unduly. There are different motivations for entering the field and consequently there are other styles of field research which are valuable.

Drawing on Argyris' (1977) argument on the perceived injustices which arise necessarily from the limitations of accounting systems, we suggest that *rich descriptions of accounting should draw out the tensions which develop around its use in organizations and the interpretive differences that give rise to them*. Concentrating on those tensions offers a practical route into the difficult process of making sense of the particular functionings of accounting in the field. It also facilitates the writing of a story which communicates the often very ambiguous roles of accounting to the reader.

SIX FIELD STUDIES IN MANAGEMENT ACCOUNTING

In this section we consider six studies accounting in organizational contexts. Our objective is not to review the findings of the many field studies that have been conducted in recent years, but rather to draw out features that characterize some of them as rich. Our selection of studies for

consideration is not arbitrary, but we could have selected many others. Four of these studies are drawn from a larger set that we find interesting, both for their method and their content. From this larger set, we thought it appropriate to choose two studies from Europe and two from the U.S. Our choice was further guided by a search for studies that address comparable topics in "matched pairs," as it were, in order to draw out similarities and contrasts. The remaining two studies are drawn from our own work. Whatever their faults, we at least wrote them, so we may be able to bring salient points to the discussion. These six studies are grouped in pairs according to their subject matter. The pairs focus, respectively, on top management control and, in particular, on linkages between strategy, accounting and control, on new technology and accounting for cost improvement, and on cultural dimensions of accounting.

Strategy, Accounting and Top Management Control

Roberts (1990) provides our first study on top management control. With an intent to explore concepts of accountability in control, he focuses on the use of accounting in the structuring of head office-subsidary relationships in "Conglom," a U.K.-based financial conglomerate and on its use within a newly acquired subsidiary, "ELB." Briefly, ELB, a manufacturing company with a distinguished history in its field of operations in the U.K. and abroad, had been overtaken by unanticipated events and acquired by Conglom in a hostile takeover. Conglom, a company dedicated to growth through acquisitions and notorious for its "hard-nosed" management style and indifference to anything except immediate financial returns, appointed new management at ELB. Roberts (1990) takes us through the application of Conglom's "numbers oriented" management style to ELB, the consequent rationalization of its activities in the face of a competitive market and the production and reproduction of the patterns of accountability through which the rationalization was enacted.

Roberts (1990) provides us with a sensitive and reflective account of the actions of the new management, their articulation of Conglom's management philosophy within ELB, the reciprocity implicit in its application and contrasting views of its consequences. He talks of a "divorce" of Conglom's head office from subsidiary operations, a kind of emotional detachment of the center which avoids the contagious effects of commitments to people and products, and of the head office's reliance on centrally approved budgets for control. These budgets are put into action by local management, whose self-interest is "aligned" with the head office agenda through bonus structures, stock option schemes and the ever-present threat of the divorce becoming "absolute" should returns not be adequate. Roberts (1990) explains how Conglom's tough detached style, while in one sense repressive, also offers local autonomy as a reward for meeting central demands for financial results; and how this autonomy makes local management unequivocally accountable for what they do. At the subsidiary level, he talks of decentralization of responsibility within ELB and the use of conferences to bind ELB people into rationalization programs with year-on-year cost reductions which make their colleagues redundant. He also discusses the management of dissent.

A key device in Roberts' (1990) exposition is his reference to an event early in the post-acquisition period. To recover approximately one third of

its outlay cost, Conglom sold ELB's continental European operations to its major international competitor. The deal was handled at the center, without reference to ELB and it included valuable patents to an energy-saving technology developed by ELB which would have provided the company with a distinct competitive advantage. Roberts (1990) refers to this repeatedly. It is a defining event around which he crystallizes various tensions in his story: the contrast between the center's detachment and the ELB people's commitment, the tension between the sanction of the "decreto nisi" and the reward of local autonomy, and the contrast between public declarations of faith at management conferences and the doubts which spring from plant closures and redundancies.

Simons (1990), like Roberts (1990), is motivated by the question of how managers use planning and control systems to achieve organizational goals, but his study, our second example, provides us with a rather different analysis of top management control. Earlier work by him and others had suggested the possibility of a systematic relationship between firms' strategies and their use of control systems, and his 1990 field research set out to explore this relationship further. Part of an ongoing project, his 1990 study drew on research in 16 companies, but only focused on two: companies "A" and "B." Company A's strategy was akin to cost leadership. It competed on the basis of high volume and low price, had the lowest research and development expenditure in the industry, as a percentage of sales, and introduced few new products. Company B's strategy, on the other hand, was an example of differentiation. It competed on the basis of product innovation and marketing, and its products were highly priced with advanced features. It had the highest research and development expenditure in the industry, as a percentage of sales, and sought to develop new markets and aggressively market new products.

Simons (1990) notes striking differences in these firms' use of their control systems. Company A's long-term planning was sporadic. Financial goals were set by top management and communicated down. Supporting budgets were coordinated by the finance department and not revised during the year. Top management attention was more closely focused on product- and process-related programs, the progress of which was intensively monitored. Company B's long-term planning was a recurring annual event. Financial goals and budgets were set at the business level and subjected to intensive analysis and debate as they were communicated to top management and revised three times during the year. Top management attention was not focused on product- and process-related programs, but on budgetary goals and their achievement.

Simons (1990) is stimulated by these differences into developing a model explaining why managers personally involve themselves in certain controls, that is, they use them "interactively," while others are "programmed" or, as he later said, used "diagnostically." Briefly, he argues that the strategy the firm adopts gives rise to key strategic uncertainties. Top managers use interactively those control systems that relate to, or provide information about, the strategic uncertainties (using others diagnostically). This guides an emergent learning process, through which the strategy is recursively enacted. Thus, the key strategic uncertainty facing cost

leadership firms is the development of new product or process technologies that may preserve or threaten their competitive advantage. By personally and frequently involving themselves in program reviews, top managers signal the importance they attach to capturing new product and process technologies. This focused attention, in turn, promotes new initiatives consistent with the cost leadership strategy. Similarly, the key strategic uncertainty facing firms competing on the basis of product innovation is the timing and success of new product introductions. By personally and frequently involving themselves in planning and budgeting, top managers engage in debate about underlying product release dates, marketing strategies, competitor responses and product life cycles. This focused attention promotes actions to accelerate product introductions and new product innovations.

These rather different accounts of top management behavior both shed new light on the process of control in large organizations. Both draw on and develop a notion of emergent strategy. Top managers may have strategic goals, but strategy itself is enacted; if not at the "grass roots" level, then at least through local management. Top management guide the process of emergence by adopting a style of control and allocating their attention in a manner which signals corporate priorities. In addition and importantly for Roberts (1990), control comes through the structuring of interpersonal relationships and reciprocal obligations. Further, to varying degrees both accounts draw out the intrinsic tensions and ambiguities of control. In Roberts' (1990) case, we see tensions between the central and local agendas, between strategies for immediate returns and strategies for jobs; tensions which are not completely resolved, even by skillful actors. In Simons' (1990) case, we see tensions between efficiency and cost reduction on the one hand and innovation and creativity on the other; tensions that are resolved differently in his two firms in a manner consistent with their different strategies.

These papers adopt very different modes of presentation and rhetorical styles. Roberts (1990) draws us into the world experienced by managers in his organization. His story is immediate and grounded in concrete events and interactions (such as the sale of the patents) and it is substantive. His treatment is suggestive of theoretical constructs and his details fill and develop the constructs, so that we can understand them rather more complexly than we otherwise might. But the theory, for the most part, is implicit. In contrast, while appealing to such concrete events, Simons (1990) offers us a less grounded account of his organizations, preferring instead to reveal his theory more explicitly. He describes differences between his two companies and then presents us with a rather stylized, but instructive, theoretical model to explain these differences. His account of the field provides less substantive evidence for his theory. Instead, it serves more as a motivation for his theory. In the absence of deep contextual grounding, the theory comes across very clearly, but it is not filled out as it might be; it is understood clearly but not complexly. Despite these different modes of presentation, both studies are convincing. Both demonstrate a depth of knowledge about their respective organizations. Both show a sensitivity to prior theoretical constructs and both provide theoretical insights suggestive of theoretical development.

New Technology and Accounting for Cost Improvement

Anderson's (1995) study of the implementation of activity based costing (ABC) at General Motors is the first of our studies on accounting for cost improvement. Her retrospective account, relying on only ten informants (in addition to her own experience at GM), provides a remarkably detailed report of the "glacial pace" of the ABC venture over an eight-year period. Locating the origins of GM's interest in ABC in autonomous initiatives stemming from new manufacturing technology, threats of outsourcing and a "product costing problem," she traces the "ebbs and flows" of enthusiasm for the project.

Anderson (1995) organizes her data around a model drawn from the Information Systems literature which distinguishes different stages of implementation—in particular, initiation, adoption, adaptation, acceptance, routinization and infusion. (Her study only deals with the first four stages.) She then analyses the effects of various socio-technical factors on transitions through these stages, commenting on individual, organizational and technological factors, task characteristics and environmental influences. Finally she reports on the lessons learned.

Anderson (1995) raises many interesting points, which tell us much about the reactionary influences at work in GM and, by implication, other large organizations. For example, she talks of local initiatives striving for corporate recognition, their advocates competing with one another for dominance. She documents success in achieving corporate endorsement through the "Cost Systems and Measurement Council" and the Chief Financial Officer's "wholehearted support," this latter individual being nominated "official Corporate ABC Champion." She shows, in turn, how the adoption of ABC as a "corporate policy" led to demands for standardization around common ABC designs, undermining the autonomy, individuality and challenge of prior initiatives and diluting local enthusiasm. And she goes on to note that local initiatives, once championed through a "push" system, then had to be demanded through a "pull" system, the corporate now requiring information prepared through ABC.

Similarly Anderson (1995) talks of the importance of championship and local entrepreneurship, referring to the "missionary zeal" of her plant controller, who was later to become the "ABC Sponsor." But she notes that, while ABC had support from people with strong operational ("process") knowledge, this championship also led to silent opposition, a "resistance movement," within financial accounting ranks. As ABC costs became available, this resistance was compounded by local fears "of losing competitive advantage" if plants communicated costs to one another and fears of the center appropriating of benefits—"you saved \$100,000, give me the \$100,000 back." In addition, while she notes that the initial pilot study teams included few accountants—accountants might not be able to "approach the problem with a clean slate"—she also notes that the project suffered by its isolation in the finance function, not in operations.

Continuing her account, she refers to managers' frustrations at the difficulty of attributing gains to the new costing systems and comments that improved product costs were not a sufficient motivation for implementing ABC. Instead, in the quest for relevance to users' needs, the project was refocused toward activity or process costs, to be keyed into local

business process re-engineering efforts with the labeling of costs as value added / non-value added in the manner of activity-based management. Despite this, however, managers were reluctant to commit, or to continue to commit, resources to ABC. Even the initiating teams were largely disbanded and their systems not adequately maintained. Notwithstanding the 150 ABC implementations in GM at the conclusion of the study, many perhaps introduced under duress, proponents lamented the lack of use of ABC information in substantive operational decisions, and only two plants were identified as using ABC data "to identify non-value added activities as candidates for business process re-engineering."

Anderson's (1995) account of ABC implementation demonstrates the tangible difficulties of systems introductions. While these difficulties might seem familiar to those schooled in organization theory and organizational change, they have not been rehearsed in any depth in the accounting literature and her study makes a substantive contribution in this regard. Interestingly, though, while ABC is implicitly grounded in a technological imperative—the substitution of indirect costs for direct costs that comes with new manufacturing technology—it is significant that her discussion is oriented around ABC itself, not the management of new technology. In this it stands in contrast to a comparable study of a Swedish firm. Jönsson and Grönlund (1988), our second study on accounting for cost improvement, address the issue of new technology from a rather different standpoint. It refers to the problem of managing ever-changing technologies. With a continual flow of investment in new machinery, there is always something new to engineers and operators, and stoppages occur, Jönsson and Grönlund (1988) observe. Thus, "an important part of the competitive struggle is...fought on the shop floor," where operatives make technology work and realize improvements.

Jönsson and Grönlund (1988) draw a distinction between central and local logics. From a central perspective, the problem is designing and installing production technology; this calls for engineering competence. At the local level, on the shop floor, the problem is assimilating a stream of new technology; this calls for a capacity to learn to use it. At the center, the control problem is setting performance goals to be realized through new technology—goals for quality, flexibility, timeliness and productivity—and, in an accounting sense, allowable product costs (on the basis of marketing, customer and competitor analyses) and required cost improvements; it is focused on outputs. At the local level, the control problem is realizing the desired performance improvements within the parameters set by the overall design of the technology, through action on items controllable at a local level; it is more focused on tacit behavioral routines built on skill and experience, and the development of causal knowledge. At the center, learning is conceptual and structured. Data is "hard" and standardized. At the local level, learning is experiential. It takes place through "solving puzzles." Data is "soft" and unique.

Against this backdrop, Jönsson and Grönlund (1988) describe experiments in a Swedish auto-components subcontractor. As in GM, the origins of these experiments lay in the ever present threat of the customer sourcing components elsewhere, but unlike the initiatives Anderson (1995) describes, the Swedish company's response was to operate at the

shop-floor level in an attempt to improve the organization's competence to learn from operative experience. In the context of a decentralization program, Jönsson and Grönlund (1988) describe the emergence of local information systems. They illustrate their story with several useful examples. Three are noted here.

A work group machining drive shafts identified a tool consumption problem. It set up a local information system to monitor the consumption of tools at each machine. (Data was caught with a pencil and paper system and subsequently presented graphically on a personal computer.) The work group tracked the problem down to the hardness of a jointing material on the shafts being turned. After discussions with the supplier of the shafts, the tool was changed for one of harder construction and the local information system showed the problem had been solved. In a follow-up episode, the work group experimented with different tools and found that a more expensive one more than paid for itself through its longer life. Adoption of this tool led to a reduction of 50 percent in the tool cost per shaft produced. Another work group, this time machining brake drums, identified a rejects problem. It narrowed the problem down to the design of the casting and initiated a discussion with the foundry. As a result, the casting was redesigned and the reject problem solved.

The pattern that Jönsson and Grönlund (1988) observe is episodes of experimentation and learning. Pressure to improve costs originates from the center and is transmitted through the organizational hierarchy. Work groups initiate action in response. They work on problems one at a time. The stimulus for action is the recognition of something "abnormal." The thousands of possible causes are narrowed down. Data is collected and analyzed. Actions are taken and results ascertained. If costs are improved, the team refocuses on another problem. In this way, the capacity to realize cost improvements is built into behavioral routines and migrates across work groups. Standardized management accounting systems are not helpful at this local level; they report too infrequently, their generalized frames of reference lack the requisite specificity and they do not assist in the development of causal knowledge. Instead, work groups collect data through local "ad hoc" information systems focused on the unique problem at hand. Critical to the success of this pattern is "the mobilization of the will to bother about changing something that is not as good as it could be."

These two studies, each in their own way, shed light on new technology and cost accounting. Anderson (1995) advances a systems perspective. Her champions are those who promote ABC as a solution to the problems of new technology. Jönsson and Grönlund (1988) adopt a local perspective. Their champions are the work groups that improve performance on the shop floor through episodes of puzzle solving. In their different ways, too, the studies reveal the tensions between local unique solutions and central standardized solutions. Anderson (1995) shows how corporate demands for "commonization" of disparate ABC initiatives led to disillusion at the local level. Jönsson and Grönlund (1988) argue that there are different logics in operation at central and local levels, and show how learning can be advanced on the shop floor through temporary information systems.

Like the studies of top management control discussed above, these two studies also differ appreciably in their style. A distinguishing characteristic of Anderson's (1995) story is its detail. In contrast, notwithstanding their intimate knowledge of the decentralization experiment in the Swedish organization, Jönsson and Grönlund (1988) cast much of their discussion in conceptual terms. Anderson (1995) lays her case material out in a chronological format, with minimal running commentary, as if to provide a historical "document of record." While this format allows readers to assemble information in ways that suit them, it is quite distinct from the more reflective account provided by Jönsson and Grönlund (1988), who use their knowledge of the field rather selectively to illustrate their points. Anderson (1995) clearly worked hard to present all her information in a digestible way. Nevertheless the patterns are quite difficult to discern. Arguably, the detail only comes together in her subsequent analysis of the field material, where she draws out the influence of various socio-technical factors.

In addition, following Eisenhardt (1989) Anderson (1995) imports a theoretical framework and uses this to structure her account of ABC implementation. This framework, it will be recalled, distinguishes different stages of implementation and analyzes the effects of various socio-technical factors. She claims "the study uses case study research to develop a framework for assessing ABC implementation" and, she continues, "the theory of implementation that emerges is one of an evolutionary sequence of implementation stages that are influenced by socio-technical factors" (Anderson 1995, 3). Yet, there is some ambiguity here, for in her study the framework precedes her case material and structures her story; it seems that there is little question or possibility of its emergence. This is illustrative of a problem with Eisenhardt's (1989) method. Arguably, Anderson's (1995) story calls for an interpretation grounded in the politics of organizational change. Championship, competition for dominance, the quest for central support, the problem of attributing gains to the new costing systems, the disillusionment that follows and the subsequent abdication of responsibility—all these speak of a political model. Yet the *a priori* theoretical categories of systems implementation that she imports somehow foreclose on this option. While convenient for exposition, the framework relegates the central political issues to the background of her analysis. In contrast, although much of Jönsson and Grönlund's (1988) exposition is cast in conceptual terms, one senses that their theory emerged in the field.

Accounting and Culture

The two studies that we examine on accounting and culture are our own. The first, Ahrens (1996), is a cross-cultural study of styles of accountability in Britain and Germany. Earlier comparative work had been suggestive of differences in management styles and management control in these two countries. Drawing on observations of management practices in five brewing firms and interviews with accountants and operations managers in a further eight brewing firms, Ahrens (1996) studied the different ways in which accounting is involved in organizational action in each country. He introduces his case material with a story of a leaking

roof he found in a British brewer's warehouse, a roof which had been leaking for some time, a situation on which he asks British and German managers to comment. He shows, through their responses, how financial information is seen to reflect organizational reality in different ways in each cultural environment. Managers are held accountable to different notions of good economic management.

Briefly, Ahrens (1996) talks of British managers operating in a "return-risk" framework. The firm is accountable to its investors for a return: "Come hell or high water, we have to deliver" against investors' profit expectations. This translates into operational budgets for which individual managers are accountable. Financial discipline, that is pressure to reduce costs, takes precedence over operational requests. At the corporate level, the leaking roof is unremarkable and fades into insignificance in the context of the firm's accountability to its investors. Besides, priorities are for getting revenues and in the face of resource constraint, "I'd rather spend (money) on my brands than (money) on a leaking roof." At the brewery level, requests for additional maintenance funds would be an admission of failure, for there exists "a certain macho image of managers who can operate on shoestring budgets and still deliver." The maintenance manager's "challenge" is to operate on a constrained budget, to "balance the books for the good of the company," while "handling" the risks to beer production involved. If there are more pressing maintenance priorities than repairing the roof, so be it.

For the German brewers, in contrast, accountability operates along the lines of functionally separated expertise. The German managers in the study could not conceive of operating a warehouse with a leaking roof. For accountants and operations managers alike, "there can be no two opinions." The situation is "unambiguous": a leaking roof would conflict with good practice. Besides, delaying repairs could ultimately be more costly, offending principles of "economy." Operations managers must meet their budgets, prepared on realistic bases, but if the roof needs to be repaired unexpectedly there will be an overspend. Operational integrity is more important than reported earnings. Repairing the roof is not a matter of resource allocation. Rather, it reflects managers' responsibilities for the physical integrity of production and the overriding concern for quality and operational capability.

Against the British backdrop of operations managers' financial accountability, British accountants have "commercial" responsibilities to look beyond the numbers, to share their "wider awareness" of the trade-offs between investing in brands and the "supply-side" facilities that produce the beer and to understand the "business risks" implicit in "squeezing the assets as much as possible." They perceive themselves as "generalists" who challenge operational management in a "cross-functional debate" about optimal allocation of limited resources. In Germany, accountants and operations managers occupy different spheres of accountability. Operations managers decide operations policy. They know best how to cut costs or increase revenues. Accountants are not competent to make such decisions. The intricacies of the production process are beyond their grasp. Accountants perceive themselves as "providers of management information," but operational expertise provides compelling reasons for action. "Of course, we can make suggestions but we have to

acknowledge their expertise." Otherwise "the accountant would run the business," not the "experts" and "that doesn't work...that simply doesn't work."

Ahrens (1996) draws characteristic distinctions between the management practices in the British and German firms which he studied. The British seem to privilege accounting criteria in judging proposals for action, whereas in German practice accounting is seen as only one of a number of possible and credible perspectives. No national style is monolithic, of course, rather, each contains variation. Nor is organizational practice static. Our second study on accounting and culture, Dent (1991), focused on one organization in a process of change. The organization, a railway, was then in public ownership and Dent's (1991) longitudinal study traces the transformation of the culture among the senior management élite from an operations orientation, in which "the railway was a public service" and "the accepted professional concerns were to do with railway engineering and the logistical problems of operating trains," to a "business" orientation, in which "professional management was about making the railway profitable" and "engineering and logistical operations were essentially a means for extracting revenues from customers." Dent (1991) shows how accounting was implicated differently in the "railway" and "business" cultures, and traces the dynamics of change.

The railway was under threat from the government. In response, it introduced new managers with business responsibilities. Appointed at the margins of the organization, these managers gained influence during the course of the study, gradually converting others to their image of a business railway. The story Dent (1991) tells is one of evolutionary change and organizational acclimatization. First, the business managers secured new accounting representations of the railway as a series of profit and loss accounts. Then, gaining access to the organization's planning and decision-making bodies, they translated dialogue on operational and engineering concerns into the new profit calculus. They asked, and kept asking, what were the options, were they affordable, what implications did proposed actions have for customers, for journey times and train reliability and for costs and revenues. Moving from remote concerns of long-term planning, through capital investments, to immediate issues of train scheduling and maintenance programming, they recast management debate into a language of the "bottom-line."

As others came to share their perspective, the business managers staged contests to consolidate the emerging appreciation of the railway as a business. For example, a main line was being upgraded to take faster trains. Consistent with "good practice," the engineers proposed to upgrade the signaling at the same time. A business manager challenged this proposal, arguing it was unnecessary and unaffordable and that there was a lower cost option. The decision was referred to the chief executive who supported the business manager. Collectively, these contests came to symbolize the supremacy of the business culture in relation to almost all aspects of railway management. An operations manager commented: "Five years ago, it would have been revolutionary to challenge what an engineer wanted to spend money on. Now it happens frequently." Senior engineers thought the emerging decisions "unprofessional" and feared for the integrity of the railway infrastructure. But their protestations "fell on deaf

ears." The engineers were characterized as "reactionary, protective and old-fashioned" and most left the organization.

Dent casts the changes he observed in a language of "coupling and uncoupling": that is, uncoupling organizational action from one culture and coupling it to another. Organizations, he argues, have underlying "theories of subsistence": beliefs about the relationship between organizational action and environmental rewards. In the "railway" culture, the belief was that if the organization provided a transport infrastructure (with thrift) then, in the spirit of public service, sustenance would be forthcoming from government. The "bottom-line," in contrast, constructs a quite different belief of looking to product markets for sustenance. The challenge of the business culture is to extract resources from customers, not government, for revenues from customers, not government, must cover costs, and revenues are earned in the market place in competition with other service providers. These different beliefs provide different logics for action and organizational change involves uncoupling action from one logic and recoupling it to another.

Such changes are not accomplished instantaneously. Rather, Dent (1991) talks of sequencing, momentum and cumulation. In the study, separate classes of activity were uncoupled from the railway culture and recoupled to the business culture, as if in episodes. Further, this shift from looking to the state for sustenance to looking to the market has rather fundamental implications. It had changed appropriate forms of organization, relevant patterns of influence and concepts of "good" action. In the "railway" culture, the key task was running trains. The appropriate form of organization was built around operations. Since engineers understood how to do this, they had influence and action was "good" to the extent that it facilitated cost efficient transport provision. In contrast, in the "business" culture, the key task was extracting revenues from customers. The appropriate form of organization was one that reflected markets, not operations. Since business people understood how to do this, they gained influence. Engineering became "mere operations" and action was only "good" to the extent that its value in the market place exceeded its cost.

Ahrens' (1996) study emphasizes the different styles of accountability to be found in brewing firms in Britain and Germany. Dent's (1991) study focuses on cultural change in a railway. Nevertheless, although our work was conducted independently, there are many points of contact. Ahrens' German managers share their concern for operational integrity with Dent's operations managers. And Ahrens' British accountants share a concern with Dent's business managers for the optimal allocation of resources. We have also independently drawn out contrasts between the roles of accounting in different styles of management. In Ahrens' case, the contrast is between British and German practices and in Dent's case it is between a railway and a business culture. We use the contrast between the styles to make each meaningful. Each becomes meaningful in relation to the other.

In terms of aspirations for our work, there are also parallels. Both of us sought to get as close to the field as possible and to structure the mass of data we gathered into intelligible themes. Both of us sought not only to bring theory to the interpretation of our data, but to theorize from the data

in an emergent process. And we both sought to add substance to our theory by grounding it in the experiences of participants in our respective organizations.

Our stories are constructed in rather different ways, however. Ahrens used his observation of the leaking roof as the basis for his initial enquiries in the field. German and British managers' responses to this highly specific situation, along with more general observations in the research sites, provide his field material. In the text, he probes deeper and deeper into this material, developing the reader's appreciation of its significance; until ultimately he pulls it together in an exposition of the styles of accountability to be found in Germany and Britain and their different patterns of coherence. Ahrens' focus on one event to build his theory is rather notable. The event becomes more and more suggestive as he progressively introduces responses to it and elaborates on them.

Dent's study is focused not on one event, but is longitudinal. His field material relates to events that took place over an extended period and he uses his observational and interview data to trace the process and dynamics of change. This story is punctuated by key events, like the signaling upgrading decision, but it is the process of change itself that is suggestive, that is, the gradual coupling of accounting to organizational activities. Dent uses the process of change, rather than any one event, to develop his theory of accounting and culture, showing how accounting can ultimately permeate into ideational systems to construct rationales for action and organization.

Commentary

Earlier, we talked of the closer engagement with organizational worlds that field research fosters and its potential to make understandable the actions and motivations of skillful people who draw on accounting in their daily work lives. We suggested that rich accounts should be life-like, born out of recognizable organizational contexts. More specifically, we suggested that field research should explicate the tensions which develop normally as a consequence of the uses of accounting within organizations.

Our brief summaries of the six studies above hardly do them justice, for each is a carefully crafted paper with subtlety and nuance. None of these studies is perfect. Each has its strengths and weaknesses. However, even from our brief descriptions, it should be clear that each is life-like, conveying recognizable organizational experience. And each in its own way draws out tensions in the use of accounting, tensions between central and local perspectives, or between line and staff or different functional groups, or between different management styles.

We also talked of the value of depth over breadth and the potential of small sample field research to demonstrate theoretical constructs through their application in ongoing social settings, and to deliver theoretical insights that extend beyond the studies themselves. Each of the studies above is grounded in very particular organizational contexts and tells a very specific story. There are few claims to generality. For the most part, the clarity of theory stems from the distinct story that supports it. Nevertheless, the stories are exemplars, in the sense that researchers can compare their experiences with them and gain theoretical insights of more

general relevance. They leave the reader with theoretical residues which have broader application.

We talked too of the relationship between theory and data and the process of theorizing in field research. In this regard we noted the importance of presenting data in sufficient depth so that links from data to theory can be traced and the importance of distinguishing the researcher's own opinions from participants' literal unedited speech statements. We also referred to a concern to capture the complexity of the composite whole, setting out a pattern in which point and counterpoint are both important. The papers above differ quite significantly in their presentation of data. Some rely very heavily on participants' statements, allowing the data, to a large extent, to tell the story. Others place more explicit emphasis on interpretation. Nevertheless, all attempt to convey a holistic story and present accounts which are credible.

In addition, we noted our concern that overly strong *a priori* conceptualization could filter contextual information to such an extent that it would deny the emergence of relevant theoretical interpretations. Again, the studies we cite above differ in their degree of prior theoretical framing of the issue under consideration. But in most, one's impression is that the theorizing is the product of conscientious, self-critical analysis in which unsatisfactory theories have been discarded. Coherent as the final presentations may be, one senses, for the most part, that the patterns traced have been allowed to emerge in the course of the field research.

We have said much of the ambiguities and tensions of accounting in action and of our concern for inclusiveness in field research, in the sense of capturing the different views and constructions placed on accounting in organizational settings. This stems from our understanding of management itself as a complex endeavor—after all, even very skillful actors find management difficult. People in organizations have different backgrounds, sets of experience and motivations. They interpret problems differently, have different frames of reference and see different options and solutions. Representing this complexity in text is not easy. In this regard, Czarniawska-Joerges (1995, 25) refers to the problem facing all researchers returning from the field: "how to avoid smothering the variety of voices in one sleek version as well as the kind of fragmentation that occurs when all the voices are reported simultaneously." In the quest for a coherent account, it is tempting to suppress variety. Taken to extremes this may emasculate the story, making it seem unproblematic or too easy and denying it credibility. On the other hand, an account which contains too much variety or detail and presents this in an unstructured way may be incoherent, or at least difficult for the reader to follow.

There are no standard solutions to the problem of capturing complexity in a coherent way. Researchers have to develop their own style. Some present one perspective and then another in a sequential account: for example, central and local perspectives. Others juxtapose representations one with another, alternating between them. Some leave "loose ends" at various points in their presentations, as if setting "puzzles" for the reader, only pulling these together at the end of the presentation. All of these approaches are used in the studies above and they are mostly effective. By and large, the stories come across well and most are a stimulating read.

FROM FIELD WORK TO PUBLICATION

The previous section discussed the ways in which accounting in action has been captured in a few field studies, but we have said very little about the actual processes of researching and writing. These are only discussed in general terms in the methodological literature to which we referred. In this section, we feel that it is worthwhile teasing out and developing certain aspects of researching and writing up field studies in more detail. Given the very different agendas, approaches and styles which our discussion of only six examples has already brought to light, we can only give a very partial and somewhat personalized account of how best to develop material from the field into published work. Yet, we feel that the common goal of weaving the complexity surrounding accounting in action into a well-founded, coherent and illuminating story makes certain demands on the researcher, and this allows for some generalization.

Our own experience suggests that one can usefully organize the process from field work to publication in terms of three kinds of activities: ascertaining detail, pattern making and writing. We suggest that those activities, while interdependent and overlapping in time, are a good way of thinking about the conduct of field research, because they highlight the different pressures which researchers face between arriving in and opening themselves to the field and tidying up the manuscript.

Ascertaining Detail

Whichever aspect of accounting forms the focus of field research, ascertaining very detailed information is essential to rich field studies. Bearing in mind our interest in the tensions surrounding the preparation, interpretation and use of accounting information, researchers will benefit from obtaining information and views from a wide range of organizational members. "Spreading the net," talking to organizational members in different functions and locations, the researcher may be able to obtain a more and more complex overview. In this way, breadth can add to nuance, until a focus is chosen and further detail is ascertained through the intensive study of, for example, a particular department or location, or a particular issue.

The initial collection of detail is often connected with the negotiation of research access to the organization. Few organizations just agree to let outside researchers observe their processes without an agenda. Typically, access follows from interviews initiated by the researcher to discuss specific management or professional issues thought to be of relevance to the organization. These may have been discovered through commentary in the press or publications of regulatory agencies, or through indirect "hearsay," or even through research on comparable organizations. The researcher then uses these initial interviews to formulate a credible proposal for prolonged study and contact.

However, while an initial research issue may be needed to negotiate access, the researcher need not, and probably should not, be limited to the initial agenda. Instead, as he or she "spreads the net" broader issues are likely to emerge. The researcher should take every opportunity to follow up loose ends. Accounting "problems" are usually indicative of organizational "problems." Sensitive inquiries about specific initiatives often

lead to much deeper discussions about underlying management tensions, and the research may subsequently be guided by the concerns of organizational members themselves.

Once access is granted, the task of gaining interviews with busy managers, for whom time is at a premium, is nontrivial. Moreover, the process of interviewing managers itself calls for sensitivity and interpersonal skills. Interviewees must be put at ease so that they will speak freely, as it were "off the record," notwithstanding that the researcher is taking notes and openly tape-recording the conversation. Rather like a therapist, the researcher has to have a capacity to listen, to understand and to tolerate pregnant pauses without discomfort, for these serve to precipitate further elaboration by the interviewee. At the same time, he or she has, at points, to intervene to bring the interviewee into direct contact with issues that are being skirted around or avoided.

Maintaining access in the light of a shifting research agenda requires a constant "people management" effort. Trust is a necessary condition for gathering information that is valuable: trust both from those that granted access for the research and from people who grant interviews. The "sponsor" of the research has to be persuaded of the continuing value of the shifting research agenda and interviewees have to be persuaded of the value and safety of articulating their opinions honestly. To this end, we have sometimes found it useful to intimate to the sponsor, during the research, that his or her interpretation of the initiative under discussion is not shared by others and that better implementation might follow from a deeper understanding of alternative interpretations. To interviewees, we have sometimes offered ourselves as an impartial conduit of opinion, with a guarantee of the anonymity of participants comments, unless they wish it otherwise.² Earning trust is critical, honoring confidence is absolute. To this end, we have also guaranteed the anonymity of "sponsor" organizations, unless given express permission otherwise, usually at a later date. This principle of confidence applies, *a fortiori*, to inter-company research.

This emergent approach to field work accepts that the researcher is principally engaging with the field to learn what cannot yet be found in the library. As a corollary, in the early stages of a project, researchers should expect to be confused by what they learn, for answers to worthwhile research questions are almost always difficult to find. Indeed, if researchers find that understanding is effortless and quick, perhaps they have judged prematurely what is going on, when in fact they have insufficiently explored the various meanings which different organizational members attach to the use of accounting. Alternatively, if they really have understood very quickly how accounting in a particular setting functions, they are probably studying a trivial problem which offers no research potential, save to restate extant knowledge. In this case, they should move on to a more interesting research site.

² Anonymity is not just a question of names. It extends to the possibility of the "sponsor" speculatively linking opinions to interviewees through peripheral details, like margins in the interviewee's business, their growth or retrenchment plans, customers, or even business objectives that can be uniquely identified to a particular manager. The researcher has always to be conscious that, unlike university professors, few managers have tenure.

Bearing in mind the initial difficulty of making sense of observations, the researcher should continually reflect on the observational and interview data. In addition, "quiet periods" in the field should be used to take notes on anything within reach of the senses. Notes can be taken on people's appearance, spatial arrangements such as office layouts, proximities between people and facilities, flowers and decorations, views and even smells (coffee, food, stale air, dust), individual noises, general noise levels, temperature etc. The point is that such detailed notes allow a better recollection at a later time and permit a fuller recall. Information regarding context will be helpful, too, in reconstructing meanings which were not obvious at the time: for example, was something said jokingly, or in a rash response to a covert insult, or after a thoughtful pause, maybe to impress a particular audience?

Since the meaning of observations will change in the light of an emerging understanding of the field, researchers must make every effort to avoid premature judgment of what they hear and observe. Sometimes this is taken to mean that they should be free of preconceived theories, but this is counter-productive and impossible. All field researchers have preconceived notions of how management accounting functions in organizations, be they informed by economic, behavioral or organizational theories. Moreover, they need to rely on the analytical lenses which those theories suggest, else they could not identify an initial purpose for their research. However, in trying to learn from the field, judgment should be deferred as to which theories, if any, can contribute to explanations of which situations.

Awareness of multiple theoretical frames helps keep alive competing interpretations of organizational action. This is not only useful to defer researchers' judgment until they are more familiar with the context, it also supports the recording of ambiguity. Ambiguity may arise for different reasons. Organizational members may profess ambiguity on topics which are genuinely of no interest to them. Alternatively, they may profess ambiguity in situations where it is unwise to have an opinion, waiting instead for circumstances to change in favor of a more definitive position. Or ambiguity may follow from a failure to resolve organizational discussions, so that participants cannot agree on a shared interpretation or action plan. Whatever its source, ambiguity has an information content for the researcher, which could be destroyed by the forcing of definite position.

In getting so close to what organizational members think about accounting, the researcher's understanding of accounting in action approaches what Polanyi (1958) called "personal knowledge." If we want to present "rich slices of organizational life" to our readers, we must use our subjectivity as researchers in an attempt to understand what organizational actors have in mind. Only a researcher who has some feeling for how different organizational members think can gain an understanding of how accounting in action functions.

Pattern Making

Gaining intimate knowledge, based on interviews and observations in the research site, is challenging and time consuming. Nevertheless, while

mastering detail is the bedrock of good field research, it is but a first stage. Pattern making follows. Here the researcher seeks not merely to appreciate participants' different perspectives. He or she seeks also to understand how these different perspectives relate to each other and fit together to form a whole—perhaps coherent, perhaps not—that provides explanations of observed outcomes. Pattern making is based on detail. But it abstracts from detail to concepts, grouping like with like and tracing interconnections.

Campbell's (1988) reminder of the researcher's prior beliefs and theories is relevant here. Pattern making is initially guided by prior theoretical knowledge. This is confronted with personal knowledge gathered in the field. If the personal knowledge does not fit, the prior theoretical knowledge is reformulated, or even discarded in favor of new theoretical knowledge. Subjective insights help to start this process and they are important in developing multi-layered understandings of how accounting functions in organizations.

Pattern making starts almost as soon as field work begins, for it would be a strangely disinterested researcher who could withhold from at least tentative pattern making at an early stage during the research process. In fact, field work is a constantly evolving dynamic between observation and pattern making, in an attempt to make sense of information obtained. However, a challenge for the researcher in this phase is the avoidance of premature closure on the conceptual categories employed. Premature closure results in "thin," not rich, case studies. Once the categories are closed, the "window" of inquiry, if you will, is defined and it becomes very difficult to appreciate the subtlety of the experienced world of organizational members.

In our research, one way of maintaining interpretative openness was to concentrate on events as relatively open and flexible categories of inquiry. Concrete events, such as the leaking roof in a brewery (Ahrens 1996) or the signaling upgrade decision in the railway company (Dent 1991), allowed us to communicate to organizational members our specific interests in aspects of accounting without having to specify particular theoretical categories. Thereby, participants were free to explain to us the significance which they perceived in those events. This allowed the multi-layered and conflicting meanings of accounting to unfold in the field.

Pattern making can thus develop, in part, around concrete organizational events without much articulated theorizing from the researcher. Instead, the pattern is woven out of participants' responses to events and interesting and important lessons they draw from these for their work. During much of the pattern-making phase, the researcher can therefore conceptualize the field of inquiry around the various emerging story lines into which organizational members organize the events under discussion.

These events are not context-free, nor typically are they isolated or independent. Thus pattern making may involve the construction of a narrative which connects several illuminating field events. Two points are worth mentioning about the narrative. First, it endows the events themselves with meanings that go beyond the obvious. For example, in Dent's (1991) study of the railway, the decision to defer upgrade of the signaling equipment, despite the electrification of the mainline, could have been just

another capital investment decision. After all, the signaling equipment had not reached the end of its useful life. But in the light of the narrative about the change from an engineering to a business culture, it became a key event symbolizing the progress of deeper organizational reform. The decision offended professional engineering standards and *stood for* the emerging supremacy of the business culture over the engineering culture.

A good narrative thus connects key events with symbolic meaning. Indeed, such symbolic events may be cornerstones or turning points in the story. Events and narratives mutually constitute each other. But, and this is our second point, these events are not necessarily sequential. That is to say, while the text of a field study is, of necessity, tied to linear form, the story and the events which give it meaning do not necessarily unfold sequentially, one event following another, in the form "and then, and then, and then..." Organizational life does not proceed like this. Instead, organizational actors start different initiatives simultaneously. Projects run into brickwalls or get changed, disrupted, colonized or merged. Actors change departments or divisions. New actors enter. Market forces, competitors and regulators appear from the outside. Actors disappear from view. All this may happen in many places at varying speeds and events may overlap. The pattern which the narrative makes visible through the skillful ordering of key events is thus a complex pattern, not a sequence.³

As pattern making proceeds, the researcher will gradually gain the ability to predict the responses of certain organizational members to certain types of issues. Such an understanding approaches saturation (Glaser and Strauss 1968; Glaser 1992; Strauss and Corbin 1990). Here, by "certain organizational members" we refer to the group that has been studied, or subgroups defined, for example, by reference to different sites, functions, levels of seniority or organizations. By "certain types of issues" we refer to the conceptual agendas which emerged from the key events discussed with participants. Saturation is therefore highly context-specific. It is achieved by working through the field material with preliminary concepts and theories, seeking supportive and contradictory evidence and refining the theories in the process.

It is worth pointing out that inference in this type of pattern making aspires to being strictly analytical and not statistical (Silverman 1993). For example, if one finds that an event elicits response A from 13 out of 14 managers thought to be of a certain category and elicits response B from the fourteenth, it is not permissible to disregard B as a statistically insignificant outlier. The concepts and theories which inform our understanding of the field must account for all observations. Thus, unless further information shows that the fourteenth manager does not in fact belong to the same category, the theory must either be refined or discarded.

Saturation is reached when the reworking of patterns and the reclassification of evidence is concluded such that all observations relating to the area of inquiry are parsimoniously accounted for. Earlier we referred to the tensions which exist around the interpretation of accounting. We

³ It is worth pointing out in this context that there are no generally accepted rules of narrative construction. It is not even clear what constitutes an event in general terms. Events and stories are context-dependent (Abbott 1992; Pentland, forthcoming).

are aided in judging when saturation is reached when interviewees' articulations of those tensions cease to surprise us. In our experience, agreement on many *supposedly* general arguments or rules of action breaks down at some point during interviews or observations, because organizational members have a limited capacity to exclude the surrounding tensions. Capturing and understanding the point of breakdown, when a seemingly straightforward and sensible argument or rule is disputed and your interlocutor claims "it's not like that," is important. By searching for such breakdowns and finding out which of the supposedly general rules in the organization seem most vulnerable to give way under the weight of empirical evidence, researchers can provoke key interview sequences which shed light on the cohesion of interpretations of accounting. When such breakdowns cease or become predictable, the researcher may have reached saturation.

Writing

Exploring the limits of particular views of accounting which exist in the field serves to focus the research. But exploration in the field also leads to vast quantities of observational and interview data. Pattern making reduces the complexity of this data by structuring it and revealing interrelationships. Saturation ensures that the patterns are replete. These two stages of ascertaining detail and pattern making are essentially for the benefit of the researcher. In the third stage, that of writing up field research for publication, the researcher aims to make the rich palette of his or her understanding of the research site accessible to the reader.

In an analytical sense, the challenge for the researcher, in this stage, is to shift from what Geertz (1983) describes as an "experience-near" understanding of the field, to a reinterpretation through the "experience-far" concepts of the reader. An experience-near concept is one that participants might naturally and effortlessly use to describe their world.

People use experience near concepts spontaneously, unselfconsciously, as it were colloquially; they do not, except fleetingly and on occasion, recognise that there are any concepts involved at all. (Geertz 1983, 58)

An experience-far concept, on the other hand, is one that specialists—scientists, organizational theorists and so on—use to discuss and progress their understanding of their subject. The major analytical task at this point is, thus, to develop a theoretical appreciation of the field study patterns that will be valuable to the reader: one that extends the reader's prior theoretical knowledge, or leads to a more complex appreciation of theory.

With this in mind, the practical challenge is to craft a manuscript which links events and narratives from the field with the reader's theoretical frames. The themes which structure the write-up will have emerged during the process of pattern making. But even during the writing phase it is usually necessary to re-examine the data again and again to check the consistency of the theoretical argument with the nuanced patterns observed in the field.

As we discussed in the previous section, many different modes of presentation of field material are possible. In part, this reflects the chosen

balance between theory and field material. At one extreme, the text may be biased toward field material. Anderson's (1995) study is illustrative of this bias; she offers comparatively little in the way of a theoretical account. At the other extreme, the text may be biased toward theory. Simons' (1990) presentation verges in this direction, offering relatively little field material. Other authors attempt to bring theory and field material together into a more integrated presentation. In Roberts' (1990) text, for example, while the theory is, for the most part, implicit, his substantive treatment of the field material is infused with theoretical concepts. Similarly, while Jönsson and Grönlund (1988) cast much of their discussion in conceptual terms, their very selective use of field data rather effectively grounds their theory. In these studies, one has a sense of theory and data interpenetrating each other.

Our own preference is for a balance between theory and field materials, in that field material presented without theory is often, to be blunt, just boring. Equally, theory presented without contextual data is often rather empty. As will have become obvious by now, we find presentations in which theoretical explanation emerges through the field story appealing: that is, those presentations in which the story is structured to provide an explanation, so that the reader is taken through specific events and interactions in a sequence that is suggestive of quite particular theoretical implications. The substance of the theory that emerges can often be impressive.

In our own work, we make quite heavy reference to field material, such as observations and quotations. This is partly a reflection of the way we have developed manuscripts. Early in the writing process we have found it helpful to identify key events, quotations or issues, based on our knowledge of the research site and then weave a story around them. We try to assemble the story in a way that is likely to be meaningful to the reader at a theoretical level. In order that the reader may appreciate the theoretical import of the story, we then frame the field material by setting out prior conceptual reference points early in the manuscript and later spell out our theoretical conclusions.

We have also used "dramatic scenes" in our studies. The dramatic scene draws the reader into the world experienced by organizational participants. It is akin to anthropologists' classic "arrival scenes," as they make contact with "their" people. We use these to communicate the places in which accounting information is discussed and negotiated. A description of a setting, an office or a board room, say, can draw on the reader's experience of similar settings and need only point out its particular features to be powerfully evocative. Wood paneling, Persian rugs and antique furniture suggest a context quite different from, say, the glass and stainless steel architecture and furniture to be found in a modern "hi-tech" building.

We go on to provide the reader with a feel for the people in those places and their dialogue, their dress and status, their rules of interaction and the priority they accord to accounting information over other aids for judging the situation of their organization. Dialogue not only enlivens scenes, but it contributes to a more refined understanding of how organizational actors conceptualize matters of accounting, for example, as personal issues, strategic problems or matters of routine administration.

During a scene, organizational members can adopt conflicting or complementary frames; they refocus, escalate, or play down tensions; and they may align financial knowledge with operational, administrative, or marketing expertise. They act out Goffman's (1959) notion of "performance." This refers to the integration of signals often imperceptively "given off"—gestures, facial expressions, dress, posture, spatial position relative to others and so on—with actors' purposes in day-to-day interactions. Importantly, it needs to be carried off with ease. Actors seek to avoid appearing socially incompetent and the reader can learn from dialogue and performance what "socially incompetent" with regard to the use of accounting information means in that particular organization. With additional information on actors' histories and experience, their present circumstances, conditions and their interrelationships, further clues can be provided as to how accounting is drawn upon and supports the performance of different participants.

As an aside, it is perhaps worth pointing out the rather obvious fact that, while we make heavy use of field material in our text, the mass of it is not reproduced directly, but instead informs the narrative. Equally, and perhaps less obviously, we would not like the reader to think that the power of the manuscript relies on the quotations and scenes presented. It is the story that makes those quotations and scenes significant to the reader. The text draws on much wider patterns to lay claim to the typicality of the quotations and scenes presented. Scenes and quotations generally serve as illustrations, rather than as evidence.

It is also worth pointing out the rather obvious fact that field research will not yield a unique story. Our earlier quotation of White (1978), to the effect that no set of events attested by the historical record constitutes a complete account, is again pertinent. Field research yields data that can be analyzed in different ways. Different themes can be drawn out. It is quite possible for researchers to structure their observations in the field into complementary conceptualizations, telling the story from the field in different ways. For example, Dent's (1991) story of the railway company was framed against a cultural metaphor. This was not the original research intention, rather the cultural metaphor emerged in the course of the field research as the most interesting way of interpreting the events observed. But the events could have been interpreted through a contingency metaphor: an organization responding to its environment, as it were. Equally they could have been interpreted through an institutional metaphor, the organization conforming to external beliefs in particular manifestations of competent management. Indeed, rich case material lends itself to diverse theorizing. Ahrens' (1996, 1997a, 1997b) research on brewing firms has led to papers on styles of accountability, the language of accounting and strategy. These elaborate on different themes, all of which contribute in different ways to an understanding of accounting in the organizations observed. Being open to different interpretations of field materials does not mean atheoretical research, but quite the reverse. It calls for a sensitivity to multiple theoretical metaphors.

In this section, we have given a rather personalized account of field research and, in particular, the processes of ascertaining detail, synthesizing material into patterns and writing up field research. Much of this

will seem extraordinarily pragmatic and in our experience it often is. Emergent research agendas, opportunistic data collection, the processing of ambiguous data into patterns, the constantly evolving dynamic between theory and observation and the possibility of interpreting the data through a multiplicity of theoretical perspectives—all these are a feature in the production of rich accounts.

Despite its pragmatic element, or perhaps precisely in view of its pragmatism, the demands of field research are extraordinarily rigorous. A statistical methodology, which crunches inputs into outputs, effectively absolves the researcher of errors in inference. In field research, the qualitative nature of the data and the absence of such a methodology imposes the burden of inference on the researcher. We like to think of the researcher's subjectivity as a rather more sophisticated tool for making inference than a statistical package. But he or she does not have the option of hiding behind the statistical results. The challenge of capturing complexity in a coherent way, such that it faithfully represents the world observed, falls entirely to the researcher.⁴

As the researcher moves through data collection and pattern making to the production of a manuscript, different theoretical demands are made on the researcher. Ascertaining detail may not sound as if it depends on theoretical insights, but it does. Implicit in the search for potentially interesting details are judgments about where to look, what loose ends to follow up and what to record. The researcher has to engage in preliminary guess-work about relationships between potential observations and different fields of enquiry. These judgments are shaped by theory. In the second stage of pattern making, the challenge is complexity reduction. The theoretical demand is the processing of detailed information into patterns that are replete, in that they account for all observations and are holistic, in the sense that they explain observed outcomes. Finally, in the third stage of writing, the emphasis shifts to the reader. The key concern is to capture the complexity of the field in a coherent way, such that it contributes to theory. The researcher has absorbed the field material. He or she has now to construct a story that will "travel" to the reader, one that will represent the world observed and stretch his or her theoretical knowledge.

CONCLUSION

If organizations followed the predictions of single theories, nobody would demand rich field studies. To obtain better understandings of how management accounting functions in practice, field studies that bring the messy world of organizations closer to the reader are needed. We suggest that this should be done by focusing on the tensions which often develop around the use of accounting and the conflicting interpretations that give rise to them. This does not privilege a political perspective on organizations. It simply realizes the limitations of accounting in satisfying the multiple and often conflicting demands made upon it in complex organizations.

⁴ This is not to deny the usefulness of software packages for analyzing qualitative data, such as ATLAS/ti, The Ethnograph v5.0™ and Q.S.R. NUD*IST®. We see these as an aid to the researcher's subjectivity.

From a focus for richness, our argument proceeded to three theoretical issues specific to field research: in particular, deep understanding of the field, the process of theorizing and the emergence of theoretical constructs from the field material. Common to those issues is a concern with the identification of observations within organizations and their ordering in ways that inform theory. The relationship between data and theory is never straightforward. But in field research, particularly, the flood of observations makes the task of selecting, ordering and presenting key observations and events all the more difficult.

Subsequently, we showed how those issues have been resolved in six different studies of accounting in action. The examples illustrated different ways of combining detailed observation with theory development in a narrative structure. Our main point here was to emphasize the balancing of theory and observation in order to substantiate the argument. Theory without observation is empty—observation without theory dull. The most accomplished of the studies manage to weave observations into a theoretically interesting story.

Finally we sought to make concrete suggestions on the process of moving from entry into the field to the production of publishable material. Drawing on our own experiences we discussed the ascertaining of detail, the patterning of observations and interview material and writing. Some of the principles which field researchers might usefully follow are summarized in the appendix.

There are many different objectives for field research. Our concern in this paper has been with the production of rich studies of accounting in action. In the spirit of practical reflection, we have sought to encourage others who might wish to engage in similar research. We are aware that there are deeper ontological and epistemological issues underlying field research that we have not considered. But these have not been our concern in this paper.

We have appealed to the messiness of organizational life—not out of unkindness to the practitioners who work in it. Quite the contrary; if organizations were not messy, less skillful people would be needed to work in them. Practitioners work under conditions of time pressure as well as shifting and competing perspectives to frame particular issues. If they were not to work with ambiguity, but instead privileged rigid frames of reference, complex organizations would not function like in textbooks, but rather not at all. In trying to bring out the particularities of accounting as it happens in the field, we need to approach organizations with modesty. Rich studies are a way of presenting field material which brings theory to life through grounded examples. Sometimes such studies refute existing theory. At other times they positively state new theory. What may sound bland or even crass if stated in the abstract, assumes meaning by being connected to lived experience in organizations.

APPENDIX

Do's

Activity 1: Ascertaining Detail

- record as much detail as you can
- concentrate your observations on opportunities with the potential for tensions
- cast your net wide at the beginning
- be alert to new and interesting questions
- adopt a modest attitude—you are there to learn from the field, not vice versa
- be in regular contact with your informants in the organization to maintain access and keep in touch with the latest news
- be aware of your theoretical “lenses” and their limitations; try to develop multiple lenses
- compare your observations to your previous assumptions
- try to see things through the eyes of different organizational members
- be sensitive to shifting meanings

Activity 2: Pattern Making

- find an illuminating kind of complexity reduction
- use concrete events as open categories of inquiry
- seek to identify events which are key
- think of patterns as the bringing together of complex narratives—not simple causal links

Activity 3: Writing

- seek out diverse audiences to test your ideas of how your field material fits the theory
- use dramatic scenes and quotes to bring together observed detail with a sense of the actors' skillful use of accounting
- select as central to the write-up those events which most clearly illuminate the progression of the story

Don'ts

- do not mix observation with interpretation
 - do not jump to conclusions on what your observations mean
 - do not increase the influence of your presence on observations by giving your opinion without being asked for it
 - do not be discouraged if your observations initially seem mundane: there's probably more to them than you think!
 - do not spend too much time trying to think up complex answers or explanations for your observations too early
 - do not be frustrated by the slow speed with which you negotiate access and gain understanding
 - do not let ambiguity make you impatient for “clearer” insights
-
- avoid premature closure of conceptual categories
 - do not treat exceptions as statistically insignificant outliers—learn from them to refine your analysis
 - do not simply accept the explanations from informants—seek out the boundaries of their explanations
-
- do not aim for too neat a story which seemingly effortlessly ties together all loose ends—it will smother your insights
 - do not generalize from single quotes or observations

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