# Accounting for fieldwork in three areas of geology: A situated analysis of textual silence and salience

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# ACCOUNTING FOR FIELDWORK IN THREE AREAS OF MODERN GEOLOGY: A SITUATED ANALYSIS OF TEXTUAL SILENCE AND SALIENCE

**by**

**Dacia Frerika Dressen**

A dissertation submitted in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
(Linguistics)
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Doctoral Committee:

Professor John Malcolm Swales, Chair
Professor Carol Berkenkotter, University of Minnesota
Professor Jeffrey G. Heath
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*The systemic nature of genre foils formalist studies, because formalism is limited to describing what is "there" in the texts, whereas any generic reading of a text is based equally on what is not there, on what the text does not say, and ultimately on what cannot be done with it. Comparison has been my way of getting at this "non-said".*

Thomas Beebee, "The Ideology of Genre"

*"... rien d'autre qu'obéir au principe de la description circulaire Balzacienne: vous n'entrez jamais dans le coeur de quelqu'un sans avoir tourné autour de son horizon."*

Tariq Ramadan, in *Le Monde* (12/01/2001)

To the tender and sweetly cherished memories of my grandparents, Annie and Nick,
whom I have been ready to know only too late.

Julius A. Nicholsen, 1911-1987
and
Anna Emma Howanitz, 1913-2001

*and to the butterflies at my grandmother's house…*

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# CHAPTER 1

# INTRODUCTION

## 1.1 Scientific discourse and the construction of research activity as instantiated and typified communicative behavior

Much has been written over the years about how scientists construct their research accounts. This has been clearly been the case in disciplines which have been interested by nature in the structure of such social entities as the scientific community and its inner-workings, as in the sociology of scientific knowledge (Latour and Woolgar 1979; Knorr-Cetina 1981; Gilbert and Mulkay 1984; Rudwick 1985; Traweek 1988; Myers 1990; Lynch 1993; Callon and Latour 1993). Here we see that "scientific fact" and activity are conceived of as being mutually constructed, and do not result from the work, however brilliant, of one individual, but from the discussions, dynamic interactions and struggles which take place over time within a community network of individuals. The community's breakthroughs and "paradigm shifts" (Kuhn 1970) thus occur as a result of the terrane having been prepared by the meeting of particular historical, institutional and social conditions, such as has been shown to be the case for Darwin's Origin of the Species (see for example, the discussion in Campbell 1997; see also Paul et al. 2001).

A similar paradigm shift has occurred in domains such as applied linguistics, discourse analysis, genre studies, technical communication and rhetoric, which have traditionally focused on the "product" of the research process, such as the scientific research article. Here also, however, researchers have grown increasingly interested in the social aspects of text construction, leading to hybrids of methodological approach and

new areas of study — or, in Barton's (2001) words, areas of "interdisciplinarity". Accordingly, researchers have entered workplaces in order to gather information and develop descriptions and theories of language and discourse in institutional and organizational contexts by collecting information on both oral and written language from various sources (texts, interviews, site field notes) and by analyzing it within a variety of inductive frameworks (discourse, rhetorical, ethnographic; see for example, Odell, Goswami and Herrington 1983; Gilbert and Mulkay 1984; Odell 1985; Doheny-Farina and Odell 1985; Myers 1990; Bazerman and Paradis 1991, Doheny-Farina 1991; Smart 1993; Berkenkotter and Huckin 1995; Winsor 1996, 2000; Swales 1998; Artemeva and Freedman 2001; Barton 2001, forthcoming).

### 1.1.1 The centrality of 'text' to the scientific research community

And yet, despite the epistemological underpinnings apparent in these different disciplines, which focus on a text's inherently and dynamically socially-constructed nature, the centrality of the unit of "text" to a particular community and its practices cannot be ignored. This relationship is especially noticeable between the scientific research community and its specifically research-oriented genres of text. Here, the binding relationship is only underscored by the increasing importance of publications in the research world, as seen in both the explosion of the number of publications as well as the pressure put on researchers to turn out reports of new research in the endless quest for grants, tenure, promotion and recognition. By some now somewhat dated estimations (e.g., Garfield 1978; Moravcsik 1985), there are between 70,000 and 100,000 journals devoted to science and technology around the world. As a result, we have seen the research article become instrumental in the "manufacture" of scientific knowledge (Knorr-Cetina 1981) and the maintenance of the scientific community.

In this sense, the scientific research article, as a genre of texts, has a primordial role to play in stabilizing the workings and doings of the scientific community — in many if not most of the scientific and academic disciplines. While the contrary has been shown to the be case for such disciplines as botany (Swales 1998) or biomedical automotive crash research (Raïsänen 1998), where the most important scientific "revelations" and contributions take place outside of the scientific research article, for disciplines like geology, the research article remains one key area where scientists publicize their research.

However, it is also abundantly clear that the research article, despite its undeniable importance, is not by any means the only genre of text involved in the research activity, and thus we can appreciate the fact that a good number of genres of texts come into play during the course of research and publication. Accordingly, the current study goes beyond the textual analysis of just one genre, for example, the scientific research article, for the text analyst must also be attentive to the variety of genres researchers come into contact with and must manipulate in order to successfully participate in their research communities (Parkinson 2000). In other words, what we will examine here is the range of genres regulated by a particular community's 'system of genres' (see Devitt 1991, on her description of tax accountancy, and Bazerman 1994, on genre systems of U.S. patent applications). The relevance of this approach is further highlighted in a recent comment by Berkenkotter (2001) who suggests that the activities of the professional workplace are organized by and its work is carried out within genre systems. A system and its constitutive genres are held together by a series of discursive links, what Linell (1998, p. 149) has called "intertextual chains", which both characterize and organize individuals' discursive activity and locate this activity within the disciplinary endeavor.

The stabilizing effect of genres of text on the shared practices within a given community is an aspect of typified communication long ago recognized by Merton

(1968), who noted that many traditional forms of social organization, such as bureaucracies, professions, or the sciences, have gained their persisting structure and function precisely through this typified text and discourse. Therefore, because genres are not reducible to mere textual regularities or text types, but following Miller (1984), are recurrent or typified rhetorical acts, or "symbolic actions", that occur in response to repeatedly occurring rhetorical situations, they can be considered to be a community's "indexes", "archives" (Artemeva and Freedman 2001; Geisler et al. 2001), or archeological artifacts (Foucault 1972). As such, they further reveal the community's shared tacit assumptions and give insight into its commonly-held ideological beliefs and working practices. For their users, genres regulate and help give shape to discursive events by orienting writers in their communicative needs and strategic possibilities, and by providing readers with the means for making sense of the texts they receive by stabilizing, at least "for now" (Schryer 1994), the forms in which information is communicated (Bazerman 1988; Swales 1990, Berkenkotter and Huckin 1995). In short, what I am referring to here are those elements that "make a [genre] coherent to genre-experienced readers" (Swales 1990, p. 190).

### 1.1.2 Recent trends in Genre Theory: The place of actor intentionality

While "genre" can therefore be taken as a site for facilitating and rendering processes of social typification (Bergmann and Luckmann 1994; Schutz and Luckmann 1973), it is also important, in terms of Witte's (1992) plea for a theory of language that would take into account the institution, the social *and* the agent, that genre theory has also come to be reconceptualized and recognized as a site for individual engagement and actor participation, through the embodiment of *habitus* (Bourdieu 1984, 1990), human activity (Leont'ev 1981, Engeström 1988), and structurational reproduction (Giddens 1979, 1984).

The latter theory, which became influential to text and writing analysts in the early part of the 1990's (e.g., Bazerman 1992, Yates and Orlikowski 1992, Swales 1993, Berkenkotter and Huckin 1995), has played a key role in reshaping the concept of genre for it holds as its central tenet that social structures, or institutions, are actively produced, reproduced and altered by human agents in ongoing, recursive interactions. Thus human agency and social structure are considered to be implicated within one another, rather than being in opposition. For Giddens, this dual nature of structure results from the "essential recursiveness of social life as constituted in social practices: Structure is both the *medium* and the *outcome* of the reproduction of practices. Structure enters simultaneously into the constitution of... social practices, and 'exists' in the generating moments of this constitution" (1979, p. 5).

One element that is particularly important here for socio-cognitive text linguistic analysis is what counts as "institutions." For in addition to observing institutional structures in the political, economic or legal spheres, Giddens also considers rhetorical and linguistic rules and resources to be "instances of institutions" (1979, p. 5). By using discourse to draw on and conform to convention, one further reconstitutes it, and as such language in its typified forms also becomes an institution. And, by choosing — or just as important, *not* choosing — to use particular situated genre resources, human agents either enact or modify established genres, thereby reifying and reinforcing them, or challenging and changing them.

Therefore, as an important contributor to recent conceptual shifts in genre theory, structuration theory has made a place for the individual actor and his or her[i] situational dynamics, given the necessary flux resulting from things being different in different circumstances in different places at different points in time. As a consequence, although genre features may indicate past continuity, they may be essentially open to variation through communicative exchanges, as each communicative exchange is the sum of a set of variables produced by the time-space continuum, thereby endlessly creating "new

situations". As such, for every instance of a typified communicative act, there exists the possibility that the genre itself can be modified. We see these modifications in the evolution of scientific discourses over time, of course, but they are equally apparent in the ways in which an individual may choose to challenge or discount discoursal conventions at specific points in his career. As Bazerman notes, "This machine... does not drive us and turn us into cogs. The machine itself only stays working insofar as we participate in it and make our lives through its genres precisely because the genres allow us to create highly consequential meanings in highly articulated and developed systems" (1992, p. 2). Thus, genres as instances of typified language exist, are created, maintained, and achieve institutional force, but only through human "engagement" (Swales 1993).

However, there has as yet been no clear and explicit statement by genre theorists concerning the exact mechanisms by which actors choose or do not choose to reproduce "past regularities of conduct" (Cohen 1989), nor to what extent actors are truly free in their choices. As a first element of response to these issues, we might turn to Bakhtin's explanation for community-generated regularities, and how he views the individual actor's place within the system in relation to the collective.

Thatcher (2001) pertinently notes that there exists a the tension between "diversity" (i.e., dynamic change and human agency) and "generalization" (i.e., the momentary stabilization and institutionalization of generic forms) that is in fact central to Bakhtin's (1990) concept of a "dialogized unity". This dialogized unity emphasizes a dynamic culture of give-and-take between a structure and its participants, each of which mutually "instantiates" and allows for the existence of the other. And so, if, as Bakhtin (1990) argues, discourse structures thinking processes and corresponding cultural patterns, a group of people who share certain discourses also then share the cultural patterns of those discourses, despite the divergences each individual brings to the standardized interaction. Therefore, a group of people brought together by a "dialogized

unity" share a configuration of similarly agreed-upon thought processes and accompanying actions. As Thatcher explains,

> "This unity is situated in a physical context and is constantly being dialogized and subjected to change because it is the meeting point of authoritative and persuasive discourses and centripetal and centrifugal forces; such dialogism is permitted precisely because of the dialogic unity—not the heterogeneity. And only through this dialogized unity can people meaningfully participate in and be answerable for the events in their lives" (2001, p. 463).

In other words, it is the unified and conventional, but "dialogized", interaction, such as we find in each particular instance of genre enactment, that gives voice to the individual's experience by creating and establishing a *forum* for structuring human interaction and cognition. Individuals must retain an active input in this forum, through the events of their daily lives, in order to make the dialogized system operate, and this system in turn regulates and organizes human experience in what come to be expected ways.

However, an adequate description of the individual's actual responsibility and role in the process, elements necessary to understanding the instantiation or enactment of a genre system through its particular genres of texts, is yet to be found, for the discussion thus far does not elucidate how the individual retains "answerability" nor in what conditions his creativity and individuality might be expressed. An enabling mechanism for self-expression might be found within the possibilities provided by the social structure itself, such as it has been described by Bourdieu (1984, 1993), with his "formula" for describing and accounting for human social behavior, "[(habitus)(capital)]+field = practice]" (Bourdieu, 1984, p. 101). For Bourdieu, while the individual may at times *appear* to have a free choice and therefore seem to not conform to "past conventionalized regularities", he is in reality constrained by what Bourdieu calls "habitus". And thus, some key concepts we might add to a discussion of genre here, and which will later be taken as a frame for this study's analysis of textual silence, are what Bourdieu has called "(social) field", "habitus", and "performativity".

Bourdieu (1984) distinguishes "social fields" as dynamically structured systems of social positions in which actors often intensely compete for access to and control over scarce resources. Further, any social field can be seen analogically as "a game" that both presupposes and creates the commitment of those who partake in it. The game's participants invest time and energy, and are disposed and predisposed (through their *habitus*) to see it as meaningful, worthwhile and legitimate.

> "In the social fields, which are games... one does not embark on the game by a conscious act, one is born into the game, with the game; and the relation of investment... is made more total and unconditional by the fact that it is unaware of what it is" (Bourdieu 1984, p. 67).

This is what Bourdieu means by *illusio*, or the "involvement in the game which produces the game" (Bourdieu 1984, p. 86). The objectivity of the game as a social institution and the subjective dispositions of the players are, like in Bakhtin's dialogical unicity, mutually dependent and mutually reinforcing: "The game makes the *illusio*, sustaining itself through the informed player's investment in the game" (Bourdieu 1993, p. 257).

The critical link, however, between a social field and the social practice of its actors is found in an individual actor's *habitus,* a durable set of cognitive and affective dispositions — or in Aldridge's (1998) words, citing de Toqueville, an individual's "habits of the heart" (p. 11) — which are rooted in early socialization in the family and at school. It is a conservative force, linked to what Bourdieu calls *doxa* — "an uncontested acceptance of the daily lifeworld" (Bourdieu and Wacquant 1992, p. 73), through which cultural processes are experienced as though they were natural (Bourdieu and Eagleton 1992). Jenkins (1992), however, effectively challenges the notion that *habitus* is acquired cumulatively only through early childhood socialization in kinship and education systems by pointing to fields which people encounter only as adults (p. 90).

The construction of the self by society takes place "below the level of consciousness," by which the body mimics or "enacts" what it *performs* (Bourdieu 1980, p. 73). The body's knowledge of typical and appropriate performance is acquired through

the sedimentation of the particular socio-historical forces that effect it. Thus, *habitus* is produced according to the social conditions in which one is "raised" or "inculcated", and later, following Jenkins (1992), by the conditions that characterize particular adult social fields, e.g., the academic world. Although habitual preferences are objectively adapted to an actor's social position, they are for the most part not consciously chosen, nor are they a question of simple obedience to a rule. Instead, Bourdieu refers to *habitus* as a practical and seemingly inherent "feel for the game".

What is more, the cultural conditions that produce *habitus* are in no way arbitrary and alterable at whim, for their socially-structured effects are abiding. In fact, the cultural system makes only certain choices available to the actor, and these choices are in effect constrained by the limits of his *habitus*:

> "In reality, the dispositions durably inculcated by the possibilities and impossibilities . . . inscribed in the objective conditions... generate dispositions objectively compatible with these conditions and [are] in a sense pre-adapted to their demands... and as a system of generative schemes, the habitus makes possible the free production of all the thoughts, perceptions, and actions inherent on the particular conditions of its production — and only those. (Bourdieu 1980, pp. 54-55)

Therefore, according to Bourdieu, the options authorized by the objective conditions of the system are hidden from the subject and appear as free options. The hiding of the structures accounts for the illusion of free choice within the cultural framework. There is a sense that things are as they ought to be and as a consequence, the actor's view of the world, albeit constructed, appears to be an unchanging truth because his *habitus* internalizes the history as it is presented to it, and yet immediately forgets that it is the effect of this history. Instead the effects become "second nature ... the active present of the whole past of which it is the product" (Bourdieu 1980, p. 56). And since *habitus* is forgotten once it is incorporated, one tends to view various possibilities or choices as simply the whole range of possibilities proffered by reality. This is what Bourdieu refers to as the "magic of performativity". Therefore, the "strategies" that actors

adopt in a given field are not the conscious decisions imputed by a rational choice, but are unconsciously generated by their *habitus*. *Habitus,* then, is a complex and structurally binding account for one's cultural identity and situated actions, or what others have called a "way of being in the world" (Geertz 1988; Swales 1998).

Bourdieu's of human agency account is therefore an undeniably deterministic and socially structured version of actor identity, and his sociology of practice and his notion of *habitus* portray a subject completely produced by the system in which it functions. As a result, the "subjective actor" emerges as though it were thoroughly determined by its society. Within such a context of "durably inculcated sets of dispositions", there consequently seems to be little actual room for true "free" agency here. And furthermore, there appear to be few real options to resist the system, and we may begin to wonder how it is possible to alter structures and cause change or to resist their productive effects along the lines suggested by Giddens (1984), by Leont'ev (1981) or Engeström (1988), if indeed we can at all. Nor does Bourdieu's description of a habitus-determined and non-rational strategy really correspond to the use of "rhetorical strategy" typically taken by genre and text analysts to reflect an author's choice and intentionality.

Yet, it is precisely through the analysis of the choice to modify, or transgress, the system, and the instances in which this is possible that we might also begin to understand that Bourdieu's position does in fact leave some room for individual choice, although, for him, in an appropriately restricted fashion — and further lacking the connotations dear to genre analysts, rhetoricians and writing specialists, namely, "strategy":

> "There exist dispositions to resist ... [but that we need] to examine under what conditions these dispositions are socially constituted, effectively triggered, and rendered politically efficient." (Bourdieu and Wacquant 1992, p. 81)

In clear terms, Bourdieu posits these conditions as being driven by a need for subversive behavior permitted by rank in the social hierarchy, and it suffices that these behaviors be "implicit" options, or options not openly proclaimed, as long as they are supported by a

part of some social field. In short, these are divergences that are "authorized" by the system, and by the socially determined co-actors within the system.

> "A whole set of socially constituted differences … tends to weaken as one moves up the social hierarchy. The greater tolerance of deviations from the norm ... rises strongly with position in the social hierarchy" (Bourdieu 1984, p. 382).

Therefore, Bourdieu accounts for and gives a socially-structured explanation for the space in which the individual actor may resist and transgress the system, by actively subsisting within the confines of "socially authorized" individuality, creativity and answerability. However, the precise mechanism through which transgressions of the norm occur still remains to be explained, for Bourdieu in fact seems to "gloss over" the instigation for individual subversion and resistance to reproducing conventionalized behavior. He points only to what makes such deviations from the norm "possible", and does not provide the *means* for explaining their origin, nor *why* they occur. We are, in short, lacking the essential elements of the notion of "strategy".

We may get a bit closer in pinpointing the origins of transgressive behavior and in proposing a bridge between the conflictual notions of *habitus* and authorial strategy within the framework of the cultural-historical theory of activity (Vygotsky). According to Vygotsky, human psychology is a mediated, triadic structure consisting of subject (the actor), object (the goal), and a mediating artifact (the means by which the goal is accomplished). Here, human behavior (activity) is taken to be driven by "goal-directed, historically situated, cooperative human interactions, such as a child's attempt to reach an out-of-reach toy, a job interview, a 'date,' a social club, a classroom, a discipline, a profession, an institution, a political movement, and so on" (Russell 1995, p. 53). In other words, the actor (subject), who may be either an individual or a group, uses mediating artifacts (tools, concepts, theories, genres, discourse, signs) within a social group in order to reach, attain, or achieve a particular object or goal (a product, such as a desired toy; the solution of a problem; the completion of a task; the successful instantiation of social

relationships; the transmission of contextually-embedded and context-sensitive knowledge). As a way to elaborate on this idea, we might imagine that an author (subject) effectively makes use of a strategy (mediating artifact) — either genericized or innovative — in order to achieve a goal (e.g., the publication of research, seeking funding, proposing a project).

Such a description of goal-driven activity among a "plures of individuals" (Miller 1993) appears to be especially appropriate for describing the practices of academic and scientific communities, given the underlying motivation driving the research activity in its various textual renditions. However, whether this social account applies to social interaction and activity in general remains to be seen. For example, the basis for need-driven motivation and innovation might be assumed to differ quite a bit for tax accountants or for lawyers, who would not experience the same cultural need as scientists to be constantly, at times contentiously, searching for novelty in order to establish their careers. The scientific research community by definition does not allow its activity to be purely "collaborative" (Engeström 1988), nor does it allow its successful members to be "drones".

However, what will be retained here for the sake of discussion is a key concept for explaining the motor behind individual divergences from the norm: this is the assumption that a subject's or community's actions do not exist without a *motive*, and that there must always be some 'need' to drive the act or activity. Hence, human (e.g., discursive) activity is generated from within a state of need, and the causes for this need rest solely within the bounds of individual actors or their immediate social environments. As explained by Artemeva and Freedman (2001), "the motives of such activities are [further] subjectively or objectively concealed" (p. 167). Therefore, there may very well be a private, unexpected and unpredictable motive or need, which as a consequence may not always be explicitly expressed— nor indeed, even able to be — thereby causing the action to appear unmotivated to an outside observer. However, it is within this private

and transitory need-state that we might find an explanation for instances of linguistic innovation, or deviances from the conventionalized and standardized norm. We can take such individual actions, or "local innovations" (Cole and Engeström 1993, p. 8), to be elements inherently necessary to the system, for they in fact provide a dynamic engine for change by continually working to fulfill "need states" (Artemeva and Freedman 2001, p. 168).

Within the theoretical frameworks I have outlined above, we might thus begin to describe the specific role an individual within his community might play in discoursally constructing and maintaining the systems of genres in which he partakes in his professional realm, both in terms of reduplicating "past regularities of [linguistic] conduct" (Cohen 1989), as well as in the sorts of need-driven linguistic innovations that may characterize and explain the linguistic variation and deviations from the norm characteristic of any genre of texts. It is clear, however, given Bourdieu's convincing structural explanation for the conditions that permit performative transgressions, that one cannot claim a strong position of "transgressive performativity" for the actor, or in other words, an unstructured, free agency, for to do so would be to underestimate the force of the socially-constituted institution, and to further overestimate the ability of subjects to opt out of recurrent behavior patterns. However, while, as Bourdieu suggests, transgressions are authorized by one's social position in hierarchies or other conditions which permit "deviant" behavior, they are also privately-motivated, and therefore "unpredictable". As instances of concealed individual intent, they are clearly driven by occulted personal needs, facilitated by the strategies the actor adopts for carrying out his or her intentions. What exactly the implications of this is for genre theory will be the subject of discussion in Chapters 5 and 6.

### 1.1.3 Underlying objective of the dissertation: Identifying and explaining textual silences

And so, while there may very well be a structural explanation for the conditions which allow such behavioral deviations, in order to provide a full account of linguistic behavior, we also need access to the more private motivations generating such transgressions, and the strategies one uses to achieve goals, in order to better understand and describe the phenomenon. This brings us back once again to Witte's (1992) call for a tripartite theory of language as being the crossroads of the institution and its conventions, the collectivity, and the individual. This very concern structures the methodological choices made in this dissertation, where the underlying intent is to identify and explain "textual silences" (Huckin 1997) in one specific domain of scientific discourse, i.e., modern geological field reporting.

How we treat instances of agency in genre theory, then, is acknowledgedly an important foregrounding issue for this dissertation, which seeks to describe and explain the discoursal saliencies and silences in three subdisciplines of geology where authors give an account of their fieldwork. In particular, in light of this dissertation's explicit focus on field geology's communally constructed instances of textual silence in modern field reporting practices (see section 1.2 below for further discussion; see also Chapters 3 and 5), the theories discussed above pose interesting and complementary bases for addressing discoursal events where agency is inescapably part of the equation. While there clearly are conventionalized and conditioned "silential relations" (Becker 1995) that bind a user to specific field reporting discourses, thereby revealing the discipline's institutional organization[ii] (Dressen 1998; Dressen and Swales 2000), there are also instances when a particular author or group of individuals will chose to say more than is expected or apparently allowed.

Nonetheless, it is clear that agency continues to pose serious theoretical problems to discourse and text analysts. Indeed, in the words of Zdenek (1998), in a recent review

of Gross and William's (1997) *Rhetorical Hermeneutics,* authorial agency remains "a nagging question which will not go away." We will return to a more thorough discussion of agency in Chapters 5 and 6 where, after having looked more carefully into the "silential relations" that link the linguistically marked and unmarked features of field discourse to its users' practices (Chapters 3 and 4), it is hoped that a valid contribution to the ongoing discussion in genre theory, about the place of individual intent and purpose within institutional and social structures, can be made.

## 1.2 The 'said' and the 'unsaid' in scientific discourse

In the following sections, we will look at what might constitute instances of textual silence in written scientific and academic discourse, first by considering that silence holds a fundamentally meaningful and complementary place in a system of discourse. Next, we will review what has been written about textual silence in the literature and will examine its contextual and recurrent features in a handful of disciplines, looking specifically at the field of geology. Finally, I will propose a typology of the silences one typically finds not only in field reporting discourses from geology, but — it is assumed — across written scientific discourse in general. Here I will draw on a variety of sources (Ducrot 1973; Swales 1999; Huckin 2002; Bourdieu 1984, 1993; Gilbert and Mulkay 1984; Leont'ev 1981; Engeström 1988; Bakhtin 1990) in order to establish a typology of silence within its unified system of "dialogized" discourse, which presupposes that a group of "like-minded individuals" are drawn together and cohesively interact within the frame of a structured discourse (Bakhtin 1990).

While scientific discourse has been the focus of intense research activity across a variety of disciplines for some time now, discourse analysts have tended to shy away from investigating its more covert, or linguistically unmarked, features. However, the

fundamental role played by “silences” in communication has been strongly underscored by a number of authors over the years. Hall (1985), for example, has observed that positively marked terms have meaning because of their relation to what is absent and unmarked. Ducrot (1973) explores this relational link between the explicit and the implicit and argues that it is made visible by the presuppositions underlying communicative acts. These implicit presuppositions in fact allow for a set of conventions and laws to be seen within a language that act as the “institutional framework” by which individuals’ interactions are regulated.

Becker (1995) also emphasizes the essential role played by silence in discourse and its “silential relations”, writing that the “stupendous reality that is language cannot be understood unless we begin by observing that speech consists above all in silences. A being who could not renounce saying many things would be incapable of speaking. … Each people leaves some things unsaid *in order* to be able to say others” (Becker 1995, p. 6, original emphasis). The act of communication therefore involves a process of selection, of setting aside certain items “unsayable” in particular situations, either for structural reasons (individual languages’ “interlingual system constraints” in Swales’ (1999) words), or due to the communally-constructed and culturally-determined context of silence (Swales’ “intralingual ritual constraints”). Therefore, the process of selection is highly dependent on the situated context of the communicative event.

As an illustration of this, we might turn to the cultural differences in solidarity-building and elements left unspoken which I have observed between French and American-English, educated, middle- to upper-middle class speakers, resulting in unexpected oppositions and conflictual communicative break-downs. While an American-English speaker might seek to initially build solidarity and establish contact with an interlocutor by “freely” revealing personal and intimate details of his or her life, French speakers would likely find such details irrelevant and embarrassing, indeed would likely “pass over” such details of their own lives in silence. Instead, a similar process of

solidarity and contact building among the French might be established by an intricate exchange of repetitive details, which act to show the other speaker that the exchange is interesting and that he or she is "listening". For an American interlocutor, such information is equally irrelevant and unnecessary. However, failure to enact one or the other strategy, for example, in the case where a non-native speaker is interacting in a foreign environment, thereby inadvertently "silencing" important information, leads to communicative breakdowns and the impression that the transgressor is exceedingly impolite.

Another example of this sort of communicative breakdown that results from the transgression of expected silential norms can be found in Jaworski (1993), who describes the tacit conventions found among speakers of Polish, which regulate the behavior of the different actors involved in an exchange of personal and friendship-based services. Thus, a service rendered to a friend on the occasion of a wedding, for example, must never be monetarily recompensed, and any reference to payment would shock or offend the friend who had simply wanted to "help out" (Jaworski 1993, p. 3).

There are a few, but a growing number of, linguistic and discoursal studies that have examined the effective role silence plays in oral discourse (e.g., Tannen and Saville-Troike 1985; Jaworski 1993; Becker 1995; Scollon and Scollon 1995; Sless and Shrensky 1995; Bilmes 1996; see also the extensive bibliography in Jaworski 1993). To briefly summarize what these studies tell us about discourses and their silences, we can note that within every communicative structure there exists a necessary complementarity between what is explicit and what is not. That silence is not a simple pause or absence of communication, but rather it, like overt discourse, has a functional role with its own meaning and interpretive value. This interpretive value is not immediately apparent, for it appears only after the hearer has "reconstructed" the speaker's intent on the basis of shared knowledge and assumptions. Over time, silences become a 'normalized' and 'anticipatable' part of the conventionalized institutional framework which regulates

communicative interactions. And finally, our capacity to use silences at appropriate moments and interpret the silences of others depends on our acculturation into a particular community. As Pittenger, Hockett and Danehy (1960) fittingly remarked some time ago, “It only takes one person to produce speech, but it requires the cooperation of all to produce silence.”

And yet, despite the manifestation of such culturally-embedded, highly-conventionalized and community-generated instances of implicit communication, the discourse analysis of academic and research genres has to date focused largely on clearly identifiable text-types and visible text features, assuredly because what is most immediately accessible to the text researcher are not impalpable concepts such as “communicative purpose” (see Askehave and Swales 2001 for a recent discussion), “private intentions” (Bhatia 1997) or indeed its “textual silences”, but linguistic form and content. Very little attention has in fact been paid to describing and accounting for the *muted* and *tacit* conventions of textual practices, despite Huckin’s (1997) recommendation that the analysis of content should also include close attention to what is *not* said or written and Swales’ (1998) observation that “genre analysis’ most consistent lesson is the importance of noting elements that are unexpectedly missing from a text or discourse” (p. 151).

It will be noted here in passing that given this dissertation’s defined realm of study (i.e., scientific and academic discourse), the analysis here will concentrate on the “intralingual ritual constraints” within one language (English), rather than on the “interlingual system constraints”, or cross-linguistic differences, noted by Becker (1995), Swales (1999) and others.

### 1.2.1 Previous accounts of silence, or "incomplete information"

In the literature, there are studies that do treat the silencing process of certain aspects of the scientific experience, although they refer to it more in terms of "incomplete information" and have not focused on the phenomenon of textualized silence as the primary impetus for their study, per se. Latour and Woolgar (1979), for example, have looked at "given knowledge" acting as a kind of incomplete information. Because certain types of knowledge are considered to be acquired by all, Latour and Woolgar observe that reference to these "knowledges" never occurs in discussions between members of the same community. In other words, the elocution is not expressed explicitly, but is part of the community's background knowledge (1979, pp. 74-75). Berkenkotter and Huckin (1995) likewise refer to role of shared established knowledge in the relative lack of explicitness of claims and warrants. Their explanation for the missing information is that members of the discourse community not only know about the warrant, but agree with it, indeed "subscribe to it" (1995, p. 52), and thus, it becomes unnecessary to restate it. In this sense, Berkenkotter and Huckin suggest that unstated claims and "missing information" are based on the "tacit presuppositional knowledge" shared by a discourse community.

Gilbert and Mulkay (1984) look at the case of incomplete information in methods sections in the field of biochemistry. They point to the "impersonal style" of their authorial subjects, which minimizes the author's actions, choices, judgments and beliefs (1984, p. 42). A noticeable feature of the methods sections they identified is that the specific (research) actions of the researchers are not described at all in the text, but are instead expressed in terms of general and abstract formulae. Gilbert and Mulkay's authors explain the reduction of the research narrative by pointing once again to its highly localized and situated nature, stating that the "[practical character] of the [research] actions... cannot be properly written down and can only be understood satisfactorily

through close personal contact with someone who is already proficient" (1984, pp. 53-55) (i.e., working within the same local research group).

Discourse analysts have also identified and categorized the discoursal (and rhetorical) means by which authors can imply a reduction of active and agentive research involvement. Again, we can note the contribution of Gilbert and Mulkay (1984), who discuss what they call empiricist (formal) in contrast with contingent (informal) repertoires. The "empiricist repertoire" presents the natural world as agentive in its own right, where "scientists' actions and beliefs ... follow unproblematically and inescapably from the empirical characteristics of an impersonal natural world" (1984, p. 56). This is a formal discourse, such as that presented in the scientific research article, where method and observation take precedence over the little glitches met along the way. The "contingent repertoire", on the other hand, occurring in the more informal settings offered by "bar talk", chats in the hall, or "in the wings" gossip at conferences, allows the researcher to informally acknowledge the imperfect nature of doing scientific research, in that it

> "...enables speakers to depict professional actions and beliefs as being significantly influenced by variable factors outside the realm of empirical (biochemical) phenomena... Scientists' responses are no longer depicted as generic responses to the realities of the natural world, but as the activities and judgments of specific individuals acting on the basis of their personal inclinations and particular social positions" (1984, p. 57).

While the second repertoire is essentially a "community private" discourse, the first type of discourse represents a conventionalized public interaction. Here then, Gilbert and Mulkay (1984) seem to have picked up on a constrast between two forums for communicative interaction for the scientists they studied: one is highly regulated by the structure and conventions of modern scientific reporting, and the other lends them freedom to give voice to private concerns, concerns that the wider community would most likely find irrelevant — and unreportable.

Myers (1990) reports on a similar differentiation of distanced from personalized discourses by using a narrative approach to explain the discoursal differences in two distinct genres. He observes that authorial presence ("persona") comes into play differently in two separate stages of the research publication, namely the scientific research article and its popularization (1990, p. 142). He notes that the "narrative of science" (i.e., the scientific research article) presents a scientific argumentative structure that arranges time into parallel series of simultaneous events which all support scientists' claims, and emphasize the syntax and vocabulary of the discipline's conceptual structure. On the other hand, the "narrative of nature" (i.e., popularizations) presents a sequential narrative structure where plants and animals, and not scientific activity, are the actors. Here the narrative is chronological, and the syntax and vocabulary emphasize the "externality" of nature to scientific practice.

### 1.2.2 Traces of textual silence across the disciplines

One immediate question that arises is whether the process by which human experiential discourse is filtered out from the research account is generalizable to scientific discourse as a whole. As we have seen in diachronic studies of scientific and academic discourse (e.g., Bazerman 1988; Salager-Meyer 1994, 1998, 2001; Dudley-Evans and Henderson 1993; Valle 1993; Atkinson 1999), it is clear that the research narrative has in general become increasingly de-agentivized throughout this century. As noted by Salager-Meyer about her own diachronic work in medical discourse,

> "... en ce qui concerne le discours médical écrit en français et en anglais (XIXème et XXème siècles), ... l'abandon du style personnel, anecdotique, émouvant même, pleins de détails (qui aujourd'hui paraissent superflus, inutiles, voire ridicules) date du début du XXème siècle" (Salager-Meyer, pers. comm., 1999).

The textual de-personalization of the research account also appears to characterize other disciplines as well, such field-based disciplines as mycology or linguistic anthropology,

although the level of its realization is somewhat variable. Therefore, while this kind of textual depersonalization and distillation of the research account is most obvious in certain natural field sciences like mycology or geology, it is less so in field social sciences like anthropology, and seems totally absent in other areas like social psychology where a good deal of methodological detail finds its way into the text. Swales and Luebs (2002, forthcoming), for example, have discussed the reasons why methodology sections from social psychology tend to be "long, tedious and repetitive".

In mycology texts, on the other hand, we can observe a total reduction of the fieldwork narrative in treatments, identified by Swales (1998) as one of the genres of texts making up the systematic botanist's 'system of genres'. Here details of the fieldwork are not mentioned in the main text, but are reduced to the enumeration of geographical and biological details in a "key", whose breviloquency reduces the fieldwork endeavor to a couple of lines.

> **Sporoschisma juvenile** ... Species examined: United Kingdom, on dead Faegus wood, Apr. 1947, S. Hughes... Seychelles, Le'niole, Rivière St. Louis, on submerged wood, Aug. 1996, V. & K.D. Hyde.

Researchers account for this conciseness by attributing it to communal concerns — or lack thereof. "How the fungus was collected, other than where, is probably not of interest or much importance to other scientists" (K. Hyde, pers. comm., 1999). The absence of any need for explicit geological or physical detail, let alone any information about how wet or leech-covered the researcher got in getting his sample out of the water, is further reflected in the words of yet another mycologist I interviewed at the University of Michigan, who quite simply noted that the "specifics are not all that important" (B. Fogel, pers. comm., 1999). However, it is interesting to observe that an apparent "paradigm shift" is under way in modern mycology field research, causing the discourse to tend toward a greater expansion and detail of field specifics in the quest for funding. Today, order to survive academic mycologists are increasingly obligated to "sell" their

services to national forestry conservation organizations who are becoming more and more interested in developing a "global" approach to ecology conservation (B. Fogel, pers. comm., 1999). Herein surely lies a topic which merits further study, whereby we might document the linguistic and rhetoric shift undergone in a discipline's report of its research activities, where social, political and financial contingencies cause the research account to become more detailed over time.

In linguistic anthropology texts, on the other hand, we can discern a semi-exclusion of the fieldwork account, although the extent to which the experience is "silenced" seems to depend more on the amount of experience a researcher has going into the field than on the non-necessity for unambiguous physical description. As it appears in the experience of one recently established anthropological linguist, the less background one has going in to do fieldwork, the less inclined one is to talk about it so as to draw attention away from one's status as novice (R. Simpson, pers. comm., 1999). This particular instance, then, would appear to confirm nicely to Bourdieu's continuum of socially-authorized — or, as the case may be, unauthorized — actor deviance and transgression of expected norms.

It is clear that in today's scientific prose, authors from the areas mentioned above working within the constraints of their established research genres have adopted a style that makes their personal involvement in the research process less visible. In other words, "silencing" certain details of the research account does in one way or another appear to be a significant and generalizable phenomenon across today's scientific disciplines. Nonetheless, it would appear that it is disciplinary *differences* that determine what is reported and what is not. Along these lines, Swales (1999) for example has shown that although there may be some similarities between geology and mycology in terms of how little detail of the research endeavor is considered pertinent, in linguistic anthropology this down-playing of the researcher's activities is much less comprehensive and thus less extreme.

While some information is available about what is or is not considered relevant in, say, mycology, linguistic anthropology, social psychology or even geology (see section 1.2.3 below and following), very little if anything has been said about vastly different domains such as archeology or international finance. This also clearly constitutes a matter for further study.

### 1.2.3 Textual silence in geology

Disciplinary differences, then, do appear to be at least marginally important elements in any discussion of disciplinary convergences, i.e., conventions. Indeed, one might even presume that the manifestation of such linguistic phenomena as textual silence might in fact be closely linked to the discipline's particular socio-historical and ideological background.

Generally speaking, there is some evidence that the textual silences framing the research account, such as those identified above in various scientific and academic disciplines, are also typical of geology as a field-based discipline (Dressen and Swales 2000). Of course, most disciplines to some degree do give boiled-down and "distilled" versions of their research activity, but geology as a "natural" science provides an exceptionally rich window for studying this linguistic phenomenon given modern field geologists' self-declared, unusual and curious relationship to "the field as analytical object" (see discussion below; this topic will also be further addressed in Chapter 2).

However, despite the tremendous number of studies on the nature of scientific discourse, spanning numerous research domains, surprisingly little rhetorical, linguistic or sociological attention has been paid to geology as a discipline. Although it is an important and long established science, it has so far largely failed to attract the attention of discourse analysts and applied linguists, not to mention rhetoricians or sociologists. For this reason, there is to date no established body of textual work on its academic and

research genres, such as exists for physics (Bazerman 1984), biology (Myers 1985, 1990; Dubois 1982, 1988), medical discourse (Adams Smith 1984; Atkinson 1992, 1999; Salager-Meyer 1994, 1996, 1999; Ravotas and Berkenkotter 1998; Barton 1999, 2001; Berkenkotter 2001), astrophysics (Tarone et al. 1981), or economics (McCloskey 1983; Dudley-Evans and Henderson 1993). In contrast, the existent literature on the analysis of geology texts today in text linguistic and ESP research is confined to Cox's (1995) genre study of abstracts in geotechnical writing, Een's (1982) study of past research reporting in geotechnical writing, Love's (1991, 1993) analysis of introductory English language geology textbooks, or Montgomery's (1996) general scholarly review of the stylistics of geological writing. Rowley-Jolivet's (1998) contrastive study of geology, economics, and medical conference discourse provides one further venue for identifying the features of geological discourse, by examining oral geological discourse (i.e., conferences; see also Rowley-Jolivet 1999, 2001). Rudwick (1985, 1996), as a former geologist (paleontologist) and current historian of science, has looked into the controversial and evolving nature of the geological community during the period of 1830-1840, and alludes to the effect this has on interpreting fieldwork, as well as on the reporting and acceptance of research results.

However interesting these studies are in and of themselves, they have not answered wider questions about the specifically discipline-embedded features of today's geological discourses, with implications they have for the divergences and convergences of scientific discourse as a whole, such as those this study will raise and address. One of the most notable features of today's geological field discourses, for example, is the modern practice of minimizing the fieldwork account. While reducing the research account appears to be a generalizable trend in scientific discourse, as mentioned above, the cultural and historical particularities of geology as a field-central discipline, as they will be described below, make the textual silences in geological field writing all the more "telling". Therefore, one further objective of this dissertation, in addition to the

forementioned tasks of developing a framework of analysis for identifying and explaining textual silence and for providing an account of actor intentionality within genre theory, is to give a somewhat "intimate" description of a long-neglected, but extremely fascinating, discipline.

Many geologists today in fact claim that they have a special relationship to "the field as analytical object" that is distinctive in comparison to, say, biologists' or physicists' relationship to their object of research. As one first element, then, geology as a *natural* science has a different relationship with the object of its study than do the physical or hard sciences, whose object of study has been created wholly for the purposes of the study — in the latter case, the research is indeed a construct, sensu stricto. In contrast, however, the analytical object in geology actually exists outside of man's intervention and social history (see French geophysico-chemist C. Allègre's (1988) comments in Chapter 2, section 2.8). This remains particularly true for the field geologist, who by engaging in fieldwork retains an explicit and, in more ways than one, *physical* contact with a concrete natural object, in contrast to the laboratory geologist who effectively "constructs" his research from beginning to end. What further differentiates geology from other natural sciences such as biology lies in the discipline's emphasis on temporality: Geology from the middle of the seventeenth century on has essentially been a "historical" discipline and as such its researchers have been preoccupied by the search for the keys to the Earth's (and our solar system's) geological materialization over time.

The contrast between the field and the laboratory geologist mentioned above reveals yet one other contextual element of geology's disciplinary peculiarities in that there has been a long-standing and often impassioned polemic over the viability of the two respective approaches to studying geology. According to my many informants, all geologists — even the most experimental and laboratory-based among them —must show an evidence of "field culture", or an understanding of the way the Earth works in the conclusions they draw from their research. This is one basic precondition for having

their analysis be considered pertinent and relevant by the wider geological community. However, the extent to which this is true differs depending on "the camp" they work in, and the conditions for establishing such disciplinary divisions reveal basically two discordant and conflictual underlying epistemological assumptions. Thus, in the eyes of the field geologist, any theory or analysis that does not correspond to what occurs in nature is considered by some to be at best "science fiction", and in the worst case, useless. However, a subtle difference can be observed in the comments of my informants who are experimental geologists. On the contrary, they claim that the whole point of their research is not to "recreate nature", which would most certainly make nice samples, although it would be scientifically "uninteresting". Instead, their goal is to construct models and predict what *might* happen. Only in this way do they assume that geologists' understanding of the Earth's functions can advance. Therefore, one view posits that we can only rely on what we can empirically see in the field; the other advances that only by constructing theoretical and natural laws will we be able to explain the irregularities and chaotic variability which characterize the natural world.

Given the liveliness of this still ongoing debate, we can note at least two crucial elements which frame any discussion of the textual silences emblematic of field reporting discourses. The first is that for geology, "the field" remains somehow culturally central despite contemporary analytical and rhetorical tendencies to downplay its importance. Secondly, the persistence of the debate itself shows that the precedence geology thinks it ought to accord to the field is still a live — and hardly resolved — issue (for further discussion of this debate and its historical significance and cultural background, see Chapter 2).

As a final element to the equation, we can also note that geological field researchers today are bound by modern scientific discourse conventions and the contemporary need to downplay their field mission. However, quite interestingly, writers must still — albeit quickly and quietly — evidence their knowledge of the field and bow

down before geology's traditional "positivist eye", indicating to their peers that they have indeed been in the field so as to construct their credibility, authority and competence. We are left with a remarkably confined and muted discourse, needing to say much but most often without the — overt — means to say it. This, then, is a partial description of the frame geology provides for studying the occurrence of silence.

The foregoing description of the "special relationship" that geologists claim to hold toward the field, while it frames this study's choice of discipline as one which is rich in possibilities for learning about how silences and saliencies function within one particular community, also constitutes a first but brief explanation of geology's "disciplinary peculiarities" that Rudwick (1985) so pertinently mentions. As one "in the know"[iii], he thus further warns against empirically and methodologically minimizing geology's relation to the "real" external world of nature at the risk of opening up an unbridgeable gulf between the analyst and the scientists he or she seeks to adequately observe and describe. Thus, I am assuming that it is methodologically consequential to take into account the sorts of historical disciplinary background and "doing-the-work details" that make going out into the field radically different from doing laboratory experiments, studying physical phenomena, or analyzing human social behavior. Very simply, it would seem, field geologists are different and perhaps even unique, given their specific disciplinary history, underlying epistemological framework, and inevitable encounters with nature and the daily field conditions of variously being subjected to wild animal attacks, being held hostage, having little to eat, or doing fieldwork at 3500 meters, come rain or shine, etc. Such stories abound and every field geologist has his own stock; in large part, informants relate that it is the essence of these very stories that in fact privately motivates their desire to do fieldwork.

The details of these stories constitute some of the cultural input for the construction of the geologist's *habitus,* and it is this which sets field geologists

irrevocably apart from the "mere" laboratory, or "drawer-type", geologist — or from other scientists, for that matter. It is here, then, within the background of geological cultural practice, that the "disciplinary motivations" for textual silence in geology's field account can be highlighted. On the one hand, it is in a genre's *omissions* that we may hope to find disciplinary and professional conventions most strongly at work. However, within the boundaries of these discipline-motivated silences, or the contextual reasons for their occurrence, we may also see other types of "silential expressions" at work, as will be discussed in the following section.

### 1.3 A description of textual silence

So far we have been using a rather ambiguous term, 'textual silence', in order to describe the process by which scientists "deselect" certain elements of their possible experiential repertoire in order to focus more closely on those elements that have been communally established as having more 'relevance' to the research community. The following section constitutes my effort to provide a theoretical basis for identifying and discussing this type of rich and meaningful communicative interaction.

Two linguists have concretely addressed the issue of silence by proposing typologies for the different silences one might find in communicative exchanges. Ducrot (1973), for example, has identified two overarching types of silence in language: (1) what is implicit within an utterance ("l'implicite de l'énoncé") and (2) what is implicit within the act of speech ("l'implicite fondé sur l'énonciation, ou les sous-entendus du discours"; see also Ducrot 1969).

In the first of the two, a speaker would say X but in so doing would in fact implicitly say Y. This implicit proposition is signaled by a "gap" within a chain of explicit utterances. However, the existence of the gap is concealed and is instantiated only because the hearer (or reader) is able to fill it in, thus requiring that the proposition

be tacitly accepted and supported by a community of speakers or writers. In the second type of silence, the speaker's planned communicative act is subject to a set of conditions that influences the act of speech. These conditions must be met, for example, in order for the speaker to be granted the right to speak; if not, the speaker must manage to say what needs to be said but indirectly, without actually having 'said' it (1973, pp. 5-8). Thus, language must also possess a range of expressions that play on the contrast between salience and silence, but where the expression of the content may remain "silent" — at least to outsiders, for it is related in an inexplicit manner.

In a recent paper, Huckin (2002) also proposes a typology of what he calls "textual silences" (see also Huckin 1997). Among these, he includes six categories:

(1) 'Speech act silences': the speaker or writer intends for the silence to have communicative import, but the reader or listener can arrive at the intended understanding only because he or she shares a set of expectations (see also Goffman's (1974) "frame of reference").

(2) 'Presuppositional silences': the speaker or writer may achieve greater communicative efficiency by not stating what is assumed to be common knowledge easily recoverable from context.

(3) 'Discreet silences': the speaker avoids mentioning sensitive subjects conditioned by issues of confidentiality, tactfulness, or taboo topics. Ducrot (1973) similarly points to entire subjects (be they activities, feelings, or events) that are protected by 'a law of silence' so that if an individual were to talk about a particular subject, he would be considered to be bragging, complaining, offending someone, or humiliating himself or someone else (1973, p. 8).

(4) 'Conventional silences': some silences are governed by genre conventions, such as those found in the specific subsections of the scientific research article. As an example, Huckin cites scientific research reports that routinely

leave out methodological details. The inclusion of such "unnecessary" information would bring the investigator's role and activities into the foreground, thereby undermining the genre's aura of "machine-like objectivity" (see also Swales 1999).

(5) 'Manipulative silences': the focus of Huckin's analysis, these silences deliberately conceal relevant information from the reader or listener.

(6) 'Incidental silences', or those that occur by accident and appear to have no particular purpose.

In his paper, Huckin defines textual silence as "the omission of some piece of information that is *pertinent* to the topic at hand" (emphasis added), which is clearly a relevant definition for discussing the sorts of manipulative silences that characterize media or political discourses whose silences are meaningful given their underlying and communally "pertinent" propositional content. However, following Becker (1995), one might also make the case that there are silences which are silences simply because they are *not* considered relevant pieces of information, for as he proposes, "each people leaves some things unsaid in order to be able to say others" (1995, p.5). Therefore, some things are not said because they are *not* — or are no longer — pertinent.

As a consequence, I would propose replacing the term "pertinent" by a more general term, *possible*. What is possible is everything that makes up the experiential domain of a speaker or writer that he or she could "possibly" communicate. For instance, it is "possible" for a geologist to talk about a tree next to a site, for it effectively makes up part of the experiential domain of the fieldwork endeavor, and it may even be important to the researcher in some way. Therefore, we might begin by defining textual silence as 'the omission of *possible* propositional content from a discursive event.'

However, as we know, the communication of details is tightly restricted, making only some of them *contextually appropriate*, and therefore, sayable under certain conditions. As Huckin (2002) has very rightly pointed out, textual silence refers to

something the context allows or even invites, yet is not there. And it is this context that defines and establishes what is effectively communicated, or contextually appropriate. As a consequence, we might propose a second, more constrained definition for textual silence: 'The omission of *possible* propositional content from a discursive event, as determined by its *contextual inappropriateness'*.

Although we might seem to be getting back around to Huckin's original definition, the point here has been to bring the reader's attention to a distinction to be made between what may possibly be spoken (or written) but is no longer appropriate content, having been totally deselected from the structural confines of a particular genre, and the dynamic, community-internal process of determining pertinence. Therefore, the omission of methodological details of the research experiment would not seem to me to be the same type of silence as the ideologically-driven omission of details about the condition of being homeless. They each denote a different point in the process of instantiating, and later consolidating, silence.

In this sense, while Huckin's typology effectively captures the socially embedded facets of communicative silence, it also interestingly appears to dichotomize silence, perhaps unavoidably and necessarily so. The first type describes the community conventionalized silences that rely on shared frames of reference and expectations, such as speech act, presuppositional, discreet, or conventional silences, where shared background knowledge is a necessary precondition to successful instantiation. Community members must know and understand the implicit proposition and must themselves be able to manipulate this knowledge in order for their silences to be meaningful.

It would seem, however, that the ability to identify and comprehend 'manipulative' or 'incidental' silences is less straightforward, relying instead on penetrating idiosyncratic purposes. The act of deliberately silencing certain aspects of relevant information conceals the "probable" content[iv] of the original proposition, and

uncovering it would entail gaining access to the ideologically motivated interests of individuals or specific communities, such as the journalistic milieu examined by Huckin (2002). As his corpus shows, images of the Homeless are often constructed in negative terms by the American media, where journalists routinely write about drug use, insanity or laziness as causes of Homelessness, only rarely making reference to other aspects of the problem. In other words, journalists tend, for ideological reasons, to 'silence' certain — and important — bits of the whole story.

This discussion is but a starting point and the task now, following Beebee's (1994) suggested comparative approach for "getting at the unsaid", will be to determine silence's various contextual constraints and features. It is by unveiling its contexts of occurrence that we might hope to isolate and explain instances of textual silence.

### 1.3.1 A three-part structure for silence types in written scientific discourse

As we might very well suspect by now, a genre's various silences are not determined solely by its institutionalized context nor by its users' needs, *but by both*. And thus, it can be assumed that the silences that characterize newspaper articles differ, at least slightly, from those of the specific part-genres of written scientific discourse. In addition, there would appear, in the domain of written scientific discourse, to be "a system of silences" that operate within a given social field and that become consolidated through repeated instantiation within specifically-oriented communicative acts. Therefore, for the purposes of this study, which focuses on making the textual silences of one particular scientific part-genre 'visible' (i.e., those of the Field Account in geology), an alternative descriptive typology of silence will be proposed, building on both Ducrot's (1973) and Huckin's (2002) descriptions of silence. I will also draw from the theoretical intersection between the social structural (Bourdieu) and need-driven dynamics (Leont'ev 1981, Engeström 1988) discussed in section 1.1

It is suggested here that there are three overarching types of silence in written scientific discourse that can be placed on a continuum of what is "most" to "least" accessible to the analyst — and, it is assumed, to users as well. The first, as the most regularly occurring, is the most generally accessible for members of a community, for it is generated by the structure of a particular discoursal system itself ("discursive system constraints"). The elements of this "institutionalized silence" have evolved over time within a set community of users whose thinking processes and cultural patterns have been structured and unified by a process of "dialogized unity" (Bakhtin 1990). Over time, particular elements of the possible repertoire of what might be a topic of discourse have been deselected, and therefore appear to no longer be a discoursal option. However, this deselection process is the result of an unsuspected bias in the world picture painted by the community's and its individual actors' *habitus* (Bourdieu 1984, 1993), and users unproblematically reduplicate it as "the way things are". On the basis of this type of silence's conventionality, it constitutes what is expectedly missing and typically left unsaid in a text, for users do not consider it relevant or pertinent to the community's particular needs. An example would be the proverbial tree that has no significance in the geologist's field account. There are the unnecessary details about methodological procedure and the investigator's role and activities that characterize scientific writing in the natural sciences.

The motivation for the second type of silence is less immediately accessible to the researcher than the first, as it originates from within the discursive system and represents an ongoing dynamic interplay between the various needs of a "plures of individuals" (Miller 1993). These are "innovative and meaningful silences", where silential conventions are manipulated in order to construct new and specifically situational information. "Private community needs" within a closely delimited community (e.g., "field geologists" or "mycologists") motivates such instances of silence, and they contrast with the wider and more general concerns of the larger community (e.g., "the geological

research community", "biologists", "natural scientists", or "scientists"). Those silences motivated by "private individual needs" also fall into this category, and these are moments of unexpected (i.e., non-conventional) linguistic innovation, and although the social structure may allow for its occurrence, it cannot condition its content for it results from the transitory need-state of an individual on a particular, 'situated' occasion. While the undeclared manipulation of the rhetorical intent of these silences may appear to result more in 'discretion' than 'silence' per se, such purposeful silences are characterized by their *lack* of linguistic explicitness. It is the very *act* of not saying, or getting around the not-saying, that carries the propositional weight. To use this type of silence, there exists a range of linguistic strategies which allow authors to make claims more rhetorically present, but without binding them to the necessity of having to overtly say them and take responsibility for them (see Ducrot 1973).

And finally, there are also "unpredictable and accidental" silences, which because they occur randomly and do not appear strictly conditioned by the social structures in which an individual operates, are the most occluded and difficult to identify. We can include here incidental or inadvertent silences (Huckin's accidental silences), which, like the cross-linguistic transgressions of silential boundaries (section 1.2), might occur, for example, when an individual who lacks full disciplinary knowledge about the conventionalized expressions in a given discourse domain, fails to "deselect" certain bits of information, thereby failing to appropriately observe what is typically left unsaid. Or an individual may also inadvertently fail to mention what *should* be made overt, thereby innocently running the risk of being stuck with a label of intentional malevolence.

### 1.3.2 The erasure of possible propositional content: Structural silential constraints imposed by institutional and conventional practices

In the following categories, the silences discussed are characterized by the consolidated and conventionalized "erasure" of possible content, and the move to draw

attention *away* from a particular aspect of this content. Conformity to or instantiation of this type of silence denotes the existence of a social structure that regulates a particular group's linguistic behavior.

**Conventional omissions of research activity**. Given the "discoursal objectivity" it is supposed to represent, under normal circumstances, modern scientific discourse no longer typically allows inferences to the "nitty-gritty details" of the research activity[v]. As we know from diachronic research, scientific discourse has evolved greatly over the course of the twentieth century (e.g., Bazerman 1988; Salager-Meyer 1994, 1998, 2001; Dudley-Evans and Henderson 1993; Valle 1993; Atkinson 1999). The research narrative has largely disappeared from scientific discourse and this is accompanied by an ever-growing increase in "authorial invisibility" (Salager-Meyer 1998). In this sense, one can talk about an evolution of communicative practice which has resulted in a globally-accepted 'silencing' of the scientist's methodological discourse in written texts, such as we have seen for biology (Gilbert and Mulkay 1984; Myers 1990), physics (Bazerman 1988), medical discourse (Salager-Meyer 2000) or neurochemistry (Lynch 1985). Lynch (1985) refers to the "normalization" of the research activity, where such hitches in the road as the "subject" who dies before the completion of the experiment can be nicely tucked away and concealed in conventionalized visual representations. Thus, reporting on the non-empirical, experiential or "contingent" (Gilbert and Mulkay 1984) details of research activities or events is considered irrelevant, and their inclusion would characterize a writer who does not master the genre's silential conventions.

**Conventional imposition of personal modesty**. In addition to the 'law of silence' imposed on reports of research activity, contemporary scientific conventions also allow little or no personal implication on the part of the researcher, instead imposing an "appropriate authorial distance" between the research "experience" and its writer. Thus if

an author were to talk about his "feelings" or "personal experiences" as a researcher, he might very well be considered to be bragging, complaining, or perhaps even humiliating himself (see Ducrot 1973). Gilbert and Mulkay (1984) make the case that the impersonality and silencing of experiential discourse characterizing scientific discourse is generated from within the "empiricist repertoire" which minimizes not only the author's actions, but also his choices, judgments and beliefs (1984, p. 42).

Dressen (1998) and Dressen and Swales (2000) have also pointed to similar omissions made by writers of the field account in the geology research article. Here, geologists textually downplay the difficult conditions of doing fieldwork, as well as their own personal participation in the field mission. In spite of the obvious difficulties inherent in going out into the field, such as the need to fend off attacks by wild animals, to have keen negotiating skills and be proficient in human and animal psychology (Scholz 1997), to be physically able to withstand the rigors of spending weeks or months in the field in extreme climates (N. Arnaud and G. Chazot, pers. comm., 1999) and the resulting field culture of "rugged individuals" (see also Rudwick's (1985) still relevant description of field conditions from the eighteenth century), geological authors must today carefully avoid relating the sorts of 'Traveler's Tales' that were commonplace well into the first part of the twentieth century.

It is obvious that all geologists must be silent about information like 'I got the rock despite the automatic rifle pointed at my back' or 'I slept badly because of the fleas', which travel writers would include. However, I will argue that while such information is clearly inappropriate within the conventions of the scientific article genre and the overarching ideology of the modern scientific community, it is not wholly irrelevant to the field geologist community, as evidenced by the fact that geologists frequently speak freely of their 'field experiences' at conferences, both in paper sessions as well as in the halls (Rowley-Jolivet 2000; M. Rudwick, pers. comm., 1999). Furthermore, on occasion, a geological writer *will* include such personal experiential details in his published field

account, thereby transgressing boundaries of expected conventional silence. This, in turn, leads to another type of silence that will be discussed below (section 1.3.3).

**Economy of expression.** Another type of conventional silence found in scientific discourse is that engendered by shared disciplinarity and background knowledge. Latour and Woolgar (1979), for example, have looked at the omissions of 'given knowledge', which leave the impression that the research report consists of 'incomplete information' (1979, pp. 74-75). Berkenkotter and Huckin (1995) develop an explanation for the role such shared, established knowledge, or "tacit presuppositional knowledge" (p. 52), plays in the relative lack of explicitness in claims and warrants. Because members of the same community share this knowledge, its explicit expression becomes unnecessary. This type of silence is, once again, highly dependent on the shared assumptions and knowledge of a particular community for it results in a sort of "short-hand", enabling the writer to avoid having to go into detail about certain topics. A well-placed reference smartly embedded within a short phrase, for example, largely suffices for summarizing the key contributions a researcher has made and how they relate to the study at hand.

### 1.3.3 "Innovative and meaningful silences"

The categories of silence which follow stem not from the imposition of a social system's boundaries, but rather from a dynamic, goal-driven activity internal to the system (i.e., originating with a system's users). These silences all share defining but intertwining characteristics, and as such are marked by their meaningfulness. In other words, although explicit content remains silent, the silence itself has a tacitly-understood corresponding value, allowing it to imply something that is not overtly said. A range of discreet expressions allows the author(s) to imply the content, but without having to overtly "say it". The available sets of discreet expressions contain implicit value for those

"in the know". In this sense, there is a dynamic movement to draw attention *toward* some aspect of possible content (in contrast to the structural erasure of content), motivated by a particular "system internal" need, which can be either conventionalized or transient.

**Rhetoric of understatement**. Thus, there are instances when a group of authors uses a range of expressions, which may very well be conventionalized and accepted, but that translates the "private" need of a restricted research community to draw special attention to a topic within a circle of insiders. However, this is done indirectly and the content remains inexplicit — at least to outsiders. Therefore, this is a type of silence that is marked by a content that is purposeful, although understated. An example can be found in systematic botany where the usual rhetorical exigency of establishing one's scientific reputation takes a back seat to "communal modesty", since a given research project may well outlast the life expectancy of its researchers. And so, when 'new' and possibly 'important' discoveries are made, they are indicated only discreetly in the research article by using a relatively short paragraph introduced by a small abbreviation — "*sp. nov.*" (Swales 1998). Here we see one first illustration of Ducrot's (1973) range of discreet expressions used to make what is typically omitted known. While researchers cannot overtly declare their research a success, the implied content of *'sp. nov.*' very clearly relates to the insider that the research is in fact a success, i.e., the research managed, despite the river and the leeches, to find a new plant species.

A similar process is seen to occur in geology, where writers of field accounts in research articles must provide proof of physical presence in the field by rhetorically establishing their competence, credibility and authority as geologists. However, given the exigencies of the conventionalized omissions which downplay the account of the field mission, today's authors cannot write explicitly about their field presence and so must instead make use of a subtle and limited set of low-level "linguistic traces" (see Chapter 3) designed to suggest that the field description comes from the eyes of the author

(Dressen 2000). These markers act as implicit propositions that work to make the individual's participation in the research activity more clearly visible to members of his community.

**Omissions of disregard.** Just as there are speech act silences that depend on contextual or pragmatic cures, there are also silences marked by a *nonconformity* to Gricean maxims, in other words, an unconventional or 'unexpected' omission. This reflects a transient need on the part of a writer or group of writers to manipulate expected conventions by replacing them with silence, such as in the case of Huckin's (2002) manipulative silences in their failure to fully report on relevant information. A writer of a scientific article might evidence this type of silence on a more local level by purposefully *excluding* the name of a certain researcher in his review of important work published in an area. These are purposeful silences motivated by personal intent — Bhatia's (1995, 1997) "private intentions" — rather than by conventional expectations of silence. Here, the author intentionally presents a version of the account that manipulates the function of silence in order to "implicitly" communicate new (i.e., non-conventional and unexpected) propositional content.

**The concealed personal story**. There are also instances when individual writers need to draw attention to their own personal story, but for reasons that remain entirely private and unexpected. The conventionalized silences of modern scientific discourse do not permit writers to reveal their story in explicit terms, but we may very well find it in the use of unexpected details. For a geologist to write "During five summers, regional mapping of the entire Central Karakoram from Hunza in the west to Hushe in the east has been carried out" adds little relevant 'scientific' content to the proposition, for knowing how long the field mission lasted is not important for understanding the researcher's results. It will be argued in Chapter 5 that this type of silential expression instead illustrates a

transgression of the conventionalized silential boundary whereby the very discreet "personal and the heartfelt [are seen to] ruffle the smooth rhetorically machined textual surface" (Swales 1998, p. 80).

### 1.3.4 Non-meaningful, accidental silences

As briefly outlined above, accidental silences are wholly random and would appear to represent individual cognition and the "alea" of linguistic performance. They are the least accessible and will not further be dealt with here, given that they depend more on identifying individual cognitive processes rather than the operation of an individual within one particular social system. They may, however, become more relevant to instances where an analyst would wish to do a cross-linguistic or cross-cultural comparison, for in such cases they would take on new significance, such as in the contrastive rhetorical analysis of textual practices (F. Hilal, pers. comm., 2001) or in the discourse analysis of cross-linguistic social groups (E. Axelson, pers. comm., 2001). They may very well become more meaningful, and thus predictable, in the comparison of different socio-cultural systems.

## 1.4 Purpose and methodological approach of the dissertation: Identifying and explaining textual silence

The central and overarching purpose of this dissertation is to identify and to explain textual silence in the various "recontextualizations" (Linell 1998) of the research account in geology where the author is reporting on the results of his fieldwork, by using a combination of case study and participant-observation methodologies, plus a fine-grained linguistic and discoursal analysis of a corpus of written texts. Primarily, it seeks to identify the sorts of "multivoicedness and inner contradictions existing within the institutional setting" (Berkenkotter 2001, p. 332) which give rise to both communally and

privately-motivated, structurally determined and need-driven instances of silential communicative behavior.

Silence, of course, does not have the same 'easily' identifiable features as marked discourse, and therefore the investigation of silence would appear to be 'a study in reverse'. Focusing solely on typically formal linguistic topics such as aspect, tense, verb type, cohesion, discourse boundaries or rhetorical moves cannot get us far in the quest to uncover and explain silences, for these approaches are reliant on the marked, or salient, features of text. Accordingly, a logical point of departure would be to establish a basis for comparison, by looking at the marked features which have established themselves as the apparent and visible linguistic, discoursal, and rhetorical characteristics of today's Field Account. Methodological inquiry must go deeper than this, of course, for the main question to be asked and answered here is how we *know* whether or not particular information has been silenced. And further, how we can best interpret it and classify it. And so, the next task will be to see what might have been written, but was not, in other words, to establish the 'possible context' and content elements that *might* be selected in Field Account writing, but are not. Only in this way can we hope to identify, predict and classify silence.

As we can recall, textual silence refers to something that the *context* allows or even invites yet is 'not there' (Huckin 2002). Therefore any approach that intends to identify textual silences must necessarily rely on defining silence's context. Hence, identifying silence is essentially a methodological problem, where the first and primary task would be to identify the context. The strategy Huckin adopts for doing this entails making a determination — or as he puts it, "often only an assumption" — about the writer's knowledge of the topic and the framework of possibilities available to the author, which he proposes to do using Minsky's (1979) "frame theory". He then compares this framework of assumed knowledge to the actual text in order to see which possibilities the writer chooses to use or not use. In particular, his methodology for determining the

contexts that condition the instantiation of textual silences consists of four steps: (1) to identify the text producer's topical knowledge by examining a corpus of texts representative of the larger body of discourse to which the knowledge belongs; (2) by applying a 'qualitative content analysis' (Altheide 1987, Huckin also forthcoming), to discern thematic patterns and compile a set of subtopics across individual texts; (3) to examine the individual texts that address the discourse topic, noting from the set of subtopics which ones are included and which are not; and (4) to apply a "sociopolitically-sensitive form of discourse analysis" (e.g., critical discourse analysis, genre analysis, rhetorical criticism, or critical rhetoric) in order to establish what are the standard features of the genre under investigation (and indeed, if it is even a genre), and to determine other context-specific elements.

The approach adopted here is slightly different by necessity, for instead of identifying and explaining only one type of silence, as in Huckin's examination of the manipulative silences found in American newspapers, I am seeking to identify and explain "a system of silences" engendered and consolidated within a specific community of practice, and whose production of a system of genres is further marked by these various silences. And so, like Huckin, I need to establish the context that motivates the types of silences I have provisionally identified above (sections 1.3.1 – 1.3.3) in terms of the "social field's" (Bourdieu 1984) institutionalized, as well as individual users', contexts. In order to do this, we must assume a full account of language use is necessary, such as that proposed within Gidden's (1984) social structuration theory that provides a full context for the function of language by allowing us to view human social behavior (including linguistic behavior) as much a part of the institutional, as it is the social and the individual: These elements are all mutually linked in an on-going and recursive intertextual chain (Bakhtin 1986). The institutional provides the overarching structure of instantiated conventions and gives a stabilized shape — at least ,"for now" (Schryer 1994) — to the social field in which the various actors interact. An individual's identity,

as a part of "the plures" (Miller 1993), is constructed by his interaction with this social field, which forms his *habitus* (Bourdieu 1984). And this individual participates in his social field as a contributor to the field's "activity system" (Engeström 1988), in which is found the engine for dynamic need-driven change, on both a group and individual level. Thus, the field's various actors, by initiating need states, enter into conflict with other individuals, groups, and the social structure. And in this way, silences are conventionalized, instantiated, or on occasion, transgressed.

### 1.4.1 Identification and explanation of multiple contexts and their engendered silences: Outline of the dissertation

In order to provide as full an account as possible of the system of silences that characterize geological field reporting, it is necessary to draw from the entire context, or set of contexts, which motivate this particular type of language use, which is itself in turn embedded within a particular discursive system. We might provisionally identify the situated contexts as follows:

(1) institutionalized conventions as instantiated in a corpus of texts;

(2) the community's standard of practice shaped by its historical context and disciplinary evolution, in which one can identify the underpinnings for some of the major elements of a group's identity and practices, or its 'field-based *habitus*';

(3) the ways in which conventions act on the transformation of the research account into its various conventionalized recontextualizations;

(4) how and why the agent, as an actor within a dynamic and shifting organizational field/activity-system, instantiates or transgresses these conventions, or in other words, the interaction between the *habitus* and the individual who actively shapes his own needs; and

(5) the more or less hidden structural conditions that allow and/or cause the actor to transgress normative behavior. It is the interaction between these different contexts which will provide one further basis for identifying and explaining silences.

The various elements of this context will be further clarified in each chapter. Notably, I will describe the discipline of geology's mindset, attitudes, and view of the world, as well as its experiential makeup, or elements of its *habitus* (Chapters 2 and 5). I will also establish the boundaries of its organizational field's structure that determine conventions and practices (Chapters 4 and 5). The way in which the context discoursally expresses itself through a stabilized set of saliencies and silences will be the focus of Chapter 3, and how these saliences and silences appear in field geology's 'system of genres' will be the subject of Chapter 4. Finally, we will examine both how individuals work and subsist within their social field and system through performative instantiation (enactment of *habitus*) and transgressions, as well as the motivations for need-based innovation, in Chapters 4 and 5. How we gain access to and establish these contexts thus constitutes the focus of each chapter in this dissertation. In so doing, the analysis reported on in this dissertation leans a bit more into the realm of a "social description of linguistic behavior" rather than a "linguistic description of social behavior", in accordance with recent trends in genre analysis.

Because this dissertation's ultimate theoretical goal is to assess what might be meant by textual silence, Widdowson's (2000), following Hymes (1972), distinction between what is 'formally possible', 'contextually appropriate', and 'actually attested' is also particularly useful for establishing the contexts that frame linguistic behavior. While Widdowson (2000), using these distinctions in his criticism of corpus linguistics, has opposed the 'attested' and the 'possible', by arguing that "it would be . . . mistaken to suppose that what is textually attested uniquely represents real language" (p. 8), Stubbs (2001) has effectively argued that no self-respecting corpus linguist would buy into such

a corpus as representative of real language, and that "a more relevant opposition is [instead] between what is possible and what is probable" (p. 151). In a sense, the latter opposition is also more accurate than the first for the needs of current genre analysis methodology, as well, which has succeeded in moving text linguistics beyond the mere page and back into its users' laps (see Paul et al. 2001). Yet, it is in fact the three-way distinction originally made by Hymes (1972) that is the most helpful for identifying instances of textual silence, for it is only by comparing the three levels of discourse structure that one might hope to see the "unsaid" features emerge in a text.

The first task then, which is presented in the following chapter (Chapter 2), is to establish geology's particular and culture-specific socio-historical background, and some of the elements of the field geologist's *habitus*. To this end, I will document the attitudes geologists have held toward the field over time, as a means for establishing what the discipline currently considers to be relevant detail or valid topics of report. Once these initial cultural boundaries have been established, the second task will be to describe more fully what elements may be included within the realm of "possible" topics to report on. Accordingly, in Chapter 3 we will look more closely at the historically possible topics of field reporting, given past and present practices, before moving on to identifying what the salient features of today's field reporting in geology actually are in the second, and longest, part of the chapter.

Using the elements of the different contexts mentioned thus far (e.g., community attitudes, the field geologist's *habitus*, and fieldwork practices), we can begin to establish a comparative framework for determining what will probably occur in a text by comparing these elements to what is actually found in a corpus of 103 recent research articles (to be described in Chapter 3, part 2). Here, we will see how geologists from three different research domains in geology — petrology, geochemistry, and structural geology — report on their field mission in the scientific research article. By using a "qualitative content analysis" of the corpus (like that described by Altheide 1987 and

Huckin 2002), a set of low-level linguistic traces field writers use today in order to establish their field presence and competence has been identified. Furthermore, it is the at the point of convergence of field geology's cultural-historical context and these textual traces that we can better understand the shape the field account has assumed today (see Bazerman 1988, Salager-Meyer 1998). Indeed, while today's field writers are able to give an account of their fieldwork, they possess a discreetly hidden and undeclared part-genre to do so.

In Chapter 4, we will further add to this cultural and textual context by looking at the actual process of "instantiating the probable", through the transformation of the field observations made by one researcher into the various recontextualizations (Linell 1998) within the researcher's system of genres. This process can be taken as a site for examining the instantiation of contextually appropriate and community-determined conventions which bind actors' discursive habits through text. Here, we will see the emergence of silences during a process of gradual distillation of non-accidental events (i.e., the field observations) into a conventionally accepted 'representation of the same'. In particular, we will see the ways in which what is observed in the field is recontextualized at various communicative stages, and what in the "source text" is set aside or reformulated and distilled to best serve the communicative purpose of each particular genre. However, we may also view this process as a site of contention where the author may actively struggle to make his own voice heard.

In Chapter 5, I will further describe the individual actor's context by using a series of text-based interviews (Swales 1998) with a small set of expert geologist authors working within one Earth Sciences department in France, giving further contextual insight into the motivations for both instantiative and transgressive performativity. The contextually appropriate or relevant aspects of a descriptive fieldwork discourse are revealed by what these informants admit they will or can write about their own field practices, against the backdrop of today's attested field reporting practices. However, an

analysis of the interviews also contributes to an understanding of the individual user's situated context and how this may play out "against the system". Although the overall study has not focused specifically on the erasure of the geologist's personal story as an instance of silence, given the high improbability of finding such details in the recontextualization of the field mission into its various communicative genres, we can nonetheless observe how this story may unexpectedly "reappear" in an unconventional violation of silence (i.e., including irrelevant information as a way of implicitly implying personal intent). Therefore, the context of authorial choice and transient individual needs may act as a site for examining opposition to social structures. Here, we can observe that the individual actor, although shaped in his cultural identify and discoursal practices by recurrent and ongoing interaction within a social system, effectively retains a shifting notion of self by actively engaging the system, thereby both providing the means for institutional instantiations and acting as an engine for on-going generic instability.

Finally, once these contexts have been established, in Chapter 6 I will revisit the typology of silences detailed in the earlier parts of this chapter. I will also discuss the viability of the somewhat complicated methodology used here to establish the various contexts and identify their motivated silences, and will compare the methodology to a broader frame of recent developments in discourse analysis. Given the results of the study, I will also replace the discipline of Geology within its wider implications, comparing it to the perspectives of such analysts as Myers (1990) on biologists, Bazerman (1988) on physicists and Swales (1998) on botanists, and discuss how geologists "fit" in comparison to these other research communities.

Finally, I will address the pedagogical implications of what such a study might mean for the teaching of English for Specific Purposes within the context of French speakers writing in French, French speakers writing in English, and English speakers writing in French.

## Notes to Chapter 1

[i] Because the great majority of field researchers in geology continue to be men, the discussions about geological field practices presented in this dissertation also center around men. For this reason, I will be using the masculine-marked singular, third-person pronoun throughout the rest of the dissertation, for reasons of simplicity. This choice is in no way intended to diminish the fact that a great many scientific researchers are women. Nor does this reflect my personal ideology, but rather the contemporary social reality of geology as a "manly" field discipline.

[ii] Indeed, we have found that field geologists tend to downplay the importance of their field mission, keeping details to a rhetorically useful minimum, despite possible and legitimate motivations for including such detail.

[iii] Rudwick started off his professional academic career as a paleontologist.

[iv] see Widdowson 2000, who following Hymes (1972), contrasts "possible", "probable" and "attested" instances of communication. For further discussion of this trichotomy, see section 1.4.1.

[v] There are exceptions to this statement, for there is evidence that writers from botany, in fact, provide very detailed and extensive methods sections (J. Swales, pers. comm., 2002).

# CHAPTER 2

# THE EARTH AS A "HISTORICAL MONUMENT": MOMENTS IN BACONIAN SCIENCE, THE ATTRIBUTION OF RECOGNIZED FIELD COMPETENCE, AND A NOT UNDYING DEVOTION TO THE PRIMACY OF FIELD DATA

## 2.1 Introductory matters

If we consider that a particular text, or set of texts, is the "instantiation" (Bazerman 1992) of a community's intertext evolving over time, then the linguistic and rhetorical elements which characterize its system of genres find their origins in the events, acts, practices, thoughts, discussions and debates which are recounted and reshaped by the progressive and repeated telling of a community's story. If we further consider that what is textually present — and also silent — in a genre reflects a community's value system and way of operating in its disciplinary world as established over time, then we might also say that a genre's characteristic features are the sum of what past story-tellers have retained as important elements in the transmission of their shared knowledge. We might further add the working assumption that it may be possible to link, in at least a very loose way, the features of a text to the practices and culture of its community of users.

These, then, are the crucial assumptions which orient the historical account of geology given in this chapter. As Bazerman wrote already some time ago,

> "While features of the genre may emerge as individual solutions to various rhetorical problems, the regularities that appear in the genre come from the very historical presence of the emerging genre. ... The genre does not exist

> apart from its history, and that history continues with each new text invoking the genre" (1988, pp. 7-8).

However, it must be stressed that it is not so much the history of disciplinary knowledge creation or the history of geology that interests us here. Rather, it is very specifically *the story* behind the historical shaping of what has come to be called a geological field account to which we will turn our attention in this chapter. As a consequence, what we will examine here are the events, personal acts and group practices which have contributed to establishing geology's "culture of the field" and the attitudes geologists hold toward the field as object of study. In so doing, we might hope to describe and explain the significance the field holds today in the science of geology, and perhaps also the cultural origins of the textual features and characteristics we find in the modern-day Field Account published in the scientific research article.

As one final introductory point, this chapter does not intend to recreate a "historian's analysis" of this story. Instead, through a review of French and Anglosaxon literature, we will see what geologist-historians themselves have had to say about the place of fieldwork in their discipline's development. The principle sources for this account — Gohau (1987), Dalrymple (1991), and Rudwick (1985) — in addition to those of a handful of other geologists, differ from one another both in terms of national origin and disciplinary specialty, a fact not unimportant by any means. Gabriel Gohau finds himself at the tail end of a long and prestigious career as a French field geologist, and is overwhelmingly recognized as an authority on the history of geology, at least in France. G. Brent Dalrymple is an American geochemist and an early leader in the field of the radiometric dating of rocks, who has now retired from a prestigious laboratory in California. His book is the result of a set of notes he had prepared for a court case in the 1980's where, as a specialist in geodating methods, he was called to testify against a group of revisionist-creationists intent on making evolution but "a mere version" among others to be taught in the public school. Martin Rudwick is a British paleontologist-

turned-historian of science, formally cited as an authority on paleontology and geology by both geologists (e.g., Gohau 1987, Dalrymple 1991, or Richet 1999) and sociologists of science. Given their differing national and disciplinary backgrounds, the emphasis each unsurprisingly places on different aspects of the geological story results in a simultaneously complementary and conflicting account of geology and its fieldwork locus. And so we may see played out in their writings the very debates and attitudes which will be recounted here.

The roots of geology as a fieldwork discipline and concomitantly as an observational and empirical science can be found in a number of quests: a quest for the age of the Earth, a quest for unraveling the mysteries of the Earth's strata, a quest for discovering the causes and mechanisms of the Earth's transformations. It is here, strangely enough, that we can see geology, despite the apparent muted and dehumanized overtones of its modern-day "rock-centered" written discourse, as a science which in some sense has been historically central to human experience.

We have without a doubt pondered over the origins and age of the Earth for thousands of years. History makes it abundantly clear that mankind has sought to understand its place in the universe since the first recorded writings. As it is reflected in theological, philosophical and scientific debates, we have sought to answer whether the cosmos was created for man, or whether we are a mere result of natural processes shaping the universe over perhaps infinite time. Yet, as Dalrymple (1991, p. 2) has noted, recorded Western thought has until only relatively recently given overwhelming precedence to the belief that we are central to a grand and purposeful scheme. And even after the discovery that the Earth's, and by extension our own, place in the universe was not geometrically central, many clung — and still cling — to the belief that our timing was.

As a further extension of this human-centered view of the Earth's existence, the chronology of Earth history was solidly restricted by religious tenets until well into the eighteenth century, with Hebrew and Christian calculations giving us values of less than 10,000 years (Haber 1959). It was not until the naturalists of the eighteenth and nineteenth centuries began to formulate methods for calculating the Earth's age based on the physical observation and measurement of the Earth's strata, using scientifically-grounded theory, and began to look more closely at the rocks and living things of the Earth and think objectively about the way they may have formed, that man began to suspect that a much longer time period was probably required for the Earth's history than previously believed. It is through the questions surrounding the age of the Earth, how it was formed and how it works, and attempts to answer them, that the geological endeavor has been most profoundly shaped, establishing some of the principal characteristics of a discipline as one that is essentially *historical, observational,* and *empirical.* For it was first by *observing* the accumulation of sedimentary rocks and the fossils they contained that man could finally see them as the products of "processes operating over vast periods of time" (Dalrymple 1991, p. 13). By taking the quest for the age of the Earth as a framework, we can trace the role fieldwork has played in the field of geology throughout the discipline's history.

## 2.2 The early days: Geology in its nascent stages as a field-based discipline

One of the earliest important contributors to the methodical observation of "natural facts" was a Danish naturalist, Nicholas Steno (1638-1686). Attributed by Gohau (1987) with playing a considerable role in the theoretical and methodological development of geology — indeed, Gohau entitles his chapter dealing with Steno's work "Naissance de la Science" .... of geology — Steno, in careful following with his times, set the beginning of the Earth's history at the time of the original flood, some 4,000 years

before Christ. Despite this limitation, his approach is considered fundamental for defining the ways in which geologists eventually came to look at the Earth and understand it.

As such, Steno is credited with establishing geology fundamentally and methodologically as a *field* discipline. In marked contrast with other sixteenth and seventeenth century philosophers, such as Descartes who had based his principles of Earth mechanisms on *abstract* notions of the globe's formation — what we would now call "closet philosophy" (Rudwick 1985) — Steno demonstrated his principles by relying on the actual, physical field observations he himself undertook in a limited region (i.e., Tuscany). For Steno, one truly needed to go into the field to observe and "gather witnesses of the past", positing field evidence as the necessary basis for any theoretical explanation of Earth history and formation. Conjecture on the Earth's formation was thus giving way to field-based research into its history.

Remembered largely as a biologist and anatomist, Steno's contribution to geology comes from his observation that the fossils found in rocks from different stratigraphical layers may resemble one another. He thus proposed that the identification or determination of a species depends mainly on the morphology of the geological formation in which the fossil is found. This is a critical proposal, given Steno's further proposal that the different layers of the Earth have been deposited at different times, the oldest being that at the bottom, the youngest at the top — the "principle of superposition". Based on such physical, *empirical* evidence, he can therefore argue, and he does (1669, *'De solido intra solidum naturaliter contento dissertationis prodromus'*), that fossilized species which resemble one another, although found in different stratigraphical layers, appeared at different points in time. Therefore, the Earth's layers can be taken as an "archive" of nature, or a *history* of the Earth. Although Steno himself does not use the term, geologists applied his reasoning and currently spoke of the Earth as the physical "monument" of its own history roughly a century later (Gohau 1987, pp. 79-80).

Steno was certainly not alone in proposing such (r-)evolutionary ideas, some contemporaries going explicitly further: Robert Hooke (1635-1703), English physician and naturalist, considered that "new species now existed which hadn't before" and the German philosopher and mathematician Gottfried Wilhelm von Liebniz (1648-1716), codiscoverer (with Newton) of the calculus, maintained that during "the important changes undergone by the Earth, a good number of species had been transformed" (both cited in Gohau 1987, pp. 72-73). Yet, Steno *was* alone for rather than being summed up in a mere sentence or two, his claims rested solidly on a well-crafted argument supported by *empirical* field evidence in a work some 60 pages in length. Nevertheless, the use of Steno's efforts and ideas came into wide prominence in the geological community only a little over a century later, what Gohau (1987, p. 80) takes as proof of Steno's intellectual isolation during his own times, which were not yet ready to integrate such novel ways of understanding the world by focusing on the Earth's evolutionary past.

Despite this, the earlier influence of the Danish naturalist on geology is nonetheless maintained by Gohau, who notes that Hooke's *'Lectures and discourses of earthquakes and subterraneous eruptions'* appeared only posthumously in 1705, and that Liebniz' ideas, while resumed in a few pages in 1693 in his *'Protogaea...*' were only presented to the public in any developed way in 1749, the year in which the French naturalist and geologist Buffon's *'Théorie de la terre'* appeared. This was a time when geological knowledge, ambitions and challenges had already evolved to some extent, although as Gohau rather amusedly points out, Buffon did feel an obligation to define what were soon to be called "evolutionary" concepts and demonstrate once again that fossils really *were* the remains of organic material (Gohau 1987, p. 73).

It is in the latter part of the seventeenth century, then, that we can find one basis for geology's disciplinary peculiarities, setting the stage for geologists to work as empirical field observers of the Earth's stratigraphy in the eighteenth and nineteenth

centuries. For it is precisely through the up-close study of the Earth's layers, and observed processes and rates of change, that the naturalists of the eighteenth century first began to estimate, though they could not yet prove, that billions of years may in fact be required for the Earth's history. One of the earliest refutations of a very young Earth, squarely based on empirical field observation and scientific theory, was that of Benoît de Maillet (1656-1738), French diplomat, savant, and amateur naturalist (Dalrymple 1991, p. 25). As a diplomat, he traveled extensively around the Mediterranean, which provided him with an excellent opportunity to study the geology of the area.

Clearly aware of the power and influence of the Church over theories of Man's origins, however, Maillet presented his theories about the Earth as a fictitious account of a conversation taking place between a French missionary and an Indian philosopher named Telliamed (de Maillet spelled backwards), entitled *'Telliamed ou Entretiens d'un philosophe indien avec un missionnaire français sur la diminution de la mer, la formation de la terre, l'origine de l'homme'.* Even so, Maillet did not publish his work during his lifetime so as to avoid conflict with the Church, and it circulated only within restricted circles as a hand-written manuscript during the 1720's, appearing in print in 1748, ten years after his death (Dalrymple 1991, p. 25; Gohau 1987, p.83).

Like Steno, Maillet based his claims on concrete physical evidence collected during various field studies. His claim that the Earth had once been covered entirely with water was therefore much more than a mere assumption, for he pointed to a number of empirical observations to support it. He had found, for example, that mountains far inland were extensive and varied in type and composition, and especially that they contained sea shells. He took this as certain proof of a formation taking place over a long period of time in an ocean much more extensive than the present one, thereby making the transient version of the original flood incapable of accounting for either the variations found in mountain formations or the universality of marine life.

Maillet, too, had his word to say about the age of the Earth, and this is where we find his primary contribution to the field of geology. Maillet contended that one needed to observe the Earth's "history" through nature itself. Based on his empirical observations of nature, he suggested that geological time by far surpassed the exceedingly short Biblical duration of only a few thousand years. For it was through the very observation of natural processes that Maillet recognized that they in fact required vast amounts of time to form the Earth's rocks and to shape its features. Moreover, these temporal events could be estimated by observing and measuring field structures, and by making reasonable interpretations based on empirically-grounded logic. And so we see that in the practices of yet another early geologist, the ground has been laid a bit further for the later fieldwork practices of a growing discipline, although it is not yet one.

It is during this same eighteenth century, and especially the latter half, that the greater public "discovers" nature. During the first part of the century, already, the French public, at least, is avidly reading such natural history series publications as the *'Spectacle de la Nature'* of Abbé Pluche (1732) or the *'Histoires des Insectes'* series of Réamur published between 1734 and 1742 (Lagarde and Michard 1970, p. 249). A parallel development to this fascination with the natural world is seen in the beginnings of a naturalist movement, crystallized in Rousseau's *'La Nouvelle Héloïse'* (1761), an ode to nature and man's sensitivity to it (Lagarde and Michard 1970, p. 281). It was within this epoque-centered cultural frame of "amateur naturalists" with a love for nature and rural life that another important contributor to the geological story evolved.

This is Georges Louis Leclerc (1707-1788), Chevalier then Comte de Buffon, who at the young age of 32 became the new steward of the Jardin du Roi, which specialized in the study of medicinal herbs, human anatomy, medicine, botany and chemistry (Gohau 1987, p. 101). It is in this capacity that Buffon makes prodigious and fundamental contributions to probability calculus, plant physiology, the "scientific

method", and lays the foundations for what was to become the field of paleontology (Dalrymple 1991, p. 29). He also begins work on an ambitious project, what was at the outset intended to be a 50-volume study of natural history. What was in fact published were 36 volumes completed before his death in 1788 (by Dalrymple's account, there were only 35; p. 31) under the title *'Histoire naturelle, Générale et Particulière',* which grouped together a number of themes: I. his 'Theory of the Earth', II. 'Man', III. 'Quadrupeds', IV. 'Birds', V. 'Minerals', and seven supplements (Lagarde and Michard 1970, p. 249).

The explanations given for this rather strangely "incomplete" account of natural history are not without interest or pertinence for the unfolding of this story. As related by Gohau (1987), and alluded to by Lagarde and Michard (1970), Buffon was highly arrogant and quite disdainful of his contemporaries, who he feared would overshadow him. And thus he blatantly ignored the very popular work done by the famous 'insectologist' Réamur (1734-1742) by claiming very simply that he "abhorred insects" (Gohau 1987, p. 102). He also apparently avoided venturing into domains where it would be necessary to classify, and thus risk being compared to his successful contemporary and arch-rival, Carl von Linnaeus. Namely, Buffon accused Linnaeus of concocting "arbitrary orders of classification", insisting instead on the "infinite diversity" of nature (Lagarde and Michard 1970, p. 249).

Here, Gohau (1987) provides us with an amusing account of the animosity which existed between the two naturalists. Strangely enough, the account is also perhaps a further element in understanding the ultimate place fieldwork has come to occupy in modern-day geological culture. When Linnaeus writes about Buffon "that particularly disgracious plant", Buffon himself retorts by disdainfully criticizing Linnean classification in the introduction to his *Histoire Naturelle,*

> "Il faut aller le microscope à la main pour reconnaître un arbre ou une plante [...]. Ce grand arbre que vous apercevez n'est peut-être qu'une

> pimprenelle; il faut compter ses étamines pour savoir ce que c'est; et comme ses étamines sont souvent si petites qu'elles échappent à l'œil simple ou à la loupe, il faut un microscope" (Buffon 1778, p. 27, cited in Gohau 1987, p. 102).

Hardly much of an insult by today's standards, especially if one considers that Buffon himself dedicated a good forty years of his active scientific research to "dissection and microscopic examinations" (Lagarde and Michard 1970, p. 249). Nevertheless, his attack on Linnaeus is a good illustration of Buffon's attempt to dismiss the systematic organization of *natural* empirical evidence.

The profound divergence between the two naturalists' methods therefore belies differing conceptions of natural history and especially, working style. While Linnaeus classes objects as simply as possible, all the while moving as closely as possible toward a natural order, Buffon openly professes a disdain for "organized natural systems" and works hard to find alternative analytical approaches. Therefore, after studying man, he begins the study of quadrupeds with the horse, because it is man's most "noble conquête"; next comes the dog, "parce qu'il a coutume de suivre [le cheval] en effet", while the zebra "nous est peu connu" (Gohau 1987, p. 102). It is this disdain for systems, and perhaps even more the rejection of his successful contemporaries' use of them, that causes Buffon to move away from empirical natural evidence and the field-based investigation of the Earth's physical "monuments" and toward theorical and experimental explanations.

Yet it is precisely this penchant toward theory and experimentation which is considered to be a weakness in Buffon's contribution to the culture of field geology, and here we can get a first glimpse at the onging debate in geology over the primacy of the field observation in contrast to the laboratory experiment or theory for describing geological objects and events. For Buffon is most criticized by some in the modern geological community for the lack of empirical — or "field"— evidence for his theories. As Gohau (1987) writes,

> "Ses époques successives sont en grande partie reconstituées par la seule pensée de l'auteur. Les observations qu'il fournit sont en nombre limité. Et même leur impact se trouve restreint par le fait qu'il cherche à les expliquer par une loi de la nature" (p. 112).

He goes on to surmise that had Buffon done fieldwork outside of his own native region (Bourgogne), and had he simply taken the time to do *more* fieldwork and amass samples, he would have quickly seen that his theories about strata sequencing were strongly put to the test — and lost — in the face of field evidence. And so Buffon is judged in historical retrospect by modern standards, as having failed as a "true" [field] geologist who should only rely on what he actually "sees" — and prove that he sees — in order for his claims to be credible. For most geologists of Buffon's time, and growing with increasing intensity into the nineteenth century, what came to be called "Baconian science" (Rudwick 1985; see also Bazerman 1988, pp.91-92) was already very much the sole modus operandi: theoretical claims which had no empirical field reality had no validity. Indeed, theorizing was simply, for quite some time, "taboo"[1].

For Buffon, however, the problem was undeniably and largely a *theoretical* one: Understanding the significance of the ordering of the Earth's layers and the corresponding estimations of the age of the Earth did not necessarily come from natural empirical evidence, but was rather to be deduced from the laws of nature. Without reverting to Cartesian methods, Buffon "limited" his research to the quest for a natural law which would explain observations and, once established, allow the researcher to forgo empirical evidence entirely (Gohau 1987, p. 112).

For other geologist-historians, such as Dalrymple (1991), it is precisely Buffon's penchant for experimentation and theory that constitutes his primary contribution to the growing field of geology. As noted by Dalrymple (1991), in addition to Buffon's "official scientific interests", he also built and operated an iron foundry. In clear contradiction with later accusations of having theorized through "mere speculation" (e.g., Gohau 1987), Buffon in fact had his foundry fabricate iron spheres which he heated to white

heat, and then observed the time it took for them to cool. On this basis, he calculated the time it would take a molten mass of iron the size of the Earth to cool to Earth's present temperature. Using this experimental method, Buffon "officially" set the age of the Earth at ... a still very limited 74,832 years (Dalrymple 1991, p. 30), which even then caused him particular difficulties with the Church, given the implications of promoting a time-scale which surpassed the Church's own by nearly 19 times. His exceedingly delicate situation as a "King's man", in addition to the fact that his writings were published by the Royal Press (Gohau 1987, p. 112), kept him from going any farther.

Off the record, however, Buffon did evidence being troubled by certain natural phenomena which were not accounted for by his theories and laws, and was suspicious that his calculations were most likely incorrect. He was notably impressed by questions of "true geological time", as indicated by his preoccupation with the extraordinary thickness of sedimentary rocks found in the Alps and by the exceedingly slow rate of formation of oceanic sediments. As a consequence, he privately revised his estimations faced with the concrete reality of certain field data, and later suggested an age of nearly three million years in a series of unpublished manuscripts which were made public only a century after his death (Albritton 1980, p. 85).

And so, in the earliest instances of the modern debate over the precedence of empirical field data versus theory and experimental evidence, we can already note how inextricably tied the two approaches are. It may be unjustified to speak of a "love-hate" relationship between the field and theory, as if personifying the debates within the developing discipline should somehow make a reviewer's task easier. But it is this very give and take — or tug and pull, as the case may be — between the two approaches which inherently frame the ultimate place fieldwork has come to hold in geological culture. And so, we might take Buffon's public rejection of the centrality of empirical field evidence at a time already dominated by the primacy of this very evidence as a

precursor to later conflicts between "theorists" and "empiricists", or in later terms, "experimentalists" and "field geologists".

At the time Buffon was working out his theories, however, the sway of geophysical theories, such as that found in the "geological dynamic theories" of Lyell and Elie de Beaumont with their issues of "causality" in the 1830's, over physical field evidence was still some time in coming, in fact not reaching its culmination until the middle of the twentieth century[vi]. Instead, most geologists at the start of the nineteenth century were involved primarily in "descriptive geology", or the minutely-detailed investigation of the sequences of the Earth's crustal layers, motivated by the needs of national mining industry pursuits — that "mainstream business of science" (Rudwick 1985, p. 233).

### 2.3 The standardization and institutionalization of a growing discipline (1780-1840) and the rising important of the practice of visuality.

We *can* begin here, however, to trace the edification of some sort of common practice and set of beliefs during the early part of the nineteenth century, a time when geologists already widely spoke of the Earth as a historical "monument". It was more or less expected practice that any true contribution to geology could only come from the examination of physical evidence in the field. By examining this evidence, geologists gradually were beginning to suspect that Earth's history ranged over an exceedingly long time-span.

The establishment of these commonalities is perhaps further reflected in the increasingly widespread appearance of the word "geology" during the latter half of the eighteenth century. Gohau (1987) traces the first appearances of the term in various late-eighteenth century works with explicitly geological interests, found for example in the *'Lettres physiques et morales sur l'histoire de la terre et de l'homme'* (1778) of the Genevan, Jean-André Deluc (1727-1817), a traveling naturalist. Deluc argued that the

term was better adapted than "cosmology" for designating knowledge specifically concerning the Earth. The following year, another Genevan, Horace-Benedict de Saussure (1740-1799), famous for having organized the first climbs of Mont Blanc, publishes *'Voyages dans les Alpes, précédés d'un essai sur l'histoire naturelle des environs de Genève'* (1779-96) where he uses the word "geology" in his introduction. The word itself, of course, has important precedents, not suddenly appearing one day out of thin air. Famous for his neologisms, Diderot is attributed with using the word "geology" in his *Encyclopédie* (1751), which he himself most likely picked up from an earlier English author (Martin 1735), with the Latin equivalent having come long before (Gohau 1987, p. 9).

However, as Gohau so correctly reminds us, "Un mot, bien sûr, ne fait pas une science" (1987, p. 9). And thus we see that in 1795 a contemporary of Deluc, James Hutton, considered by geologists as no less important for his contributions to the development of the discipline, is still writing about a 'Theory of the *Earth*'. And thus, despite certain evidence of common knowledge and shared practice in the still yet nascent stages of geology, we can see at the close of the eighteenth century that it is not yet firmly established as a cohesive discipline, nor has its terminology been firmly anchored. In fact, it is not until the first part of the nineteenth century that we begin to see the "institutionalization" of the discipline, the "standardization" of its practices, and the "internationalization" of the issues that were to determine geological research for more than a century. Indeed, it was at this time that geology began to emerge as an autonomous line of scientific enquiry from a heterogeneous set of earlier traditions that included cosmogony, mineralogy, physical geography, natural history, 'Géognosie', and mining practice (Rudwick 1976, p. 177). It was in the process of solidly acquiring its own intellectual goals and institutional structures.

The growing institutionalization and establishment of the discipline is accompanied by a series of important changes which took place in the quantity and

quality of visual representations used by geologists at the end of the eighteenth and beginning nineteenth centuries. A survey by Rudwick (1976) of a broad range of journals on topics relevant to the future science of geology points especially to the poor quality and overwhelming paucity of illustrations at the close of the eighteenth century. The poor quality of what were largely topographical maps made it impossible for geologists to use them for explaining complex and abstract types of geological information (Rudwick 1976, p. 160). Moreover, Rudwick also points to an attitude of the time which hardly appreciated the potential of maps as a valid form of visual communication, which was highly undervalued (op. cit.).

This quickly changed, however, during the first decades of the nineteenth century, no doubt due to major technical innovations in illustrations — aquatints, wood engravings, steel engravings and especially, the less-expensive lithographs — which became widely available at that time (op. cit., p. 151). Indeed, the period ranging over the first decades of the 1800's, during which time geology was emerging as a "self-conscious new discipline with clearly defined intellectual goals and well established institutional forms" (op. cit., p. 150), was marked by a similar increase and standardization of geological illustrations (e.g., maps, traverse and columnar sections, and block-diagrams). As Rudwick suggests, the possibility to make use of a new range of more or less economically accessible illustrations within the texts most likely "spurred [geologists] towards a closer and more conscious integration of verbal and visual communication" (op. cit., p. 158), effectively transforming geology into a discipline undissociable from the visual accounts of its field data. The 1810's, then, were a time of visual emergence, with the first appearances of color-pointed geological maps, traverse and columnar sections; the 1830's saw the general standardization and widespread use of these forms of visual communication, and the 1840's were marked by an ever-increasing abstraction and theory-laden approach to integrating visuals into field descriptions.

### 2.3.1 English and French geologists (1830-1840): The emergence of a "culture of practice"

The early part of the nineteenth century was a time when, according to Rudwick's historical study of the English geology scene of the 1830's and 40's, much of the best scientific research, in England and elsewhere, was in the hands of a "gentlemanly social group of specialists" (Rudwick 1985, p. xxii), who were intensely concerned with building their careers and enhancing their social status through the practice of science. Yet theirs was an "amateur" effort, in the true sense of the term, for not only were English geologists of this time not dependent on the practice of science for their livelihood, but it was moreover their "gentlemanly" social status determined by inherited, or otherwise acquired, wealth that enabled them to carry out their scientific pursuits.

As Rudwick (1985) describes it, geology of the early nineteenth century was "a new, exciting, and fashionable science ... experiencing its first and greatest boom in conceptual innovation, empirical expansion, and public approval and interest" (p. 3). It attracted some of the most bright and talented scientists of the time, particularly those with a penchant for the outdoors and travel, but nonetheless leaving space for those with less talent or more limited "social opportunities" to contribute to the development of the science on a more local level through their expert knowledge of local terranes.

By the 1830's, as well, geology, using empirical observation, had by and large done away with the restrictive time-scale of Biblical chronologies, disclosing a vast history of the Earth and of the life found on it. A tacit agreement now circulated among geologists that the maximum thicknesses of successive groups of strata roughly represented the relative duration of the periods during which the strata had accumulated, and that the timescale represented by the strata as a whole indicated a geological time which far exceeded their wildest imaginative understanding, as it was by this time estimated — by geologists — in tens of millions of years. All this was vouched for by the leading geologists of the time, many of whose "impeccable piety" lended further

credence to this new understanding of the Earth's history and our place in it as but the "merest [of] newcomers" (Rudwick 1985, p. 4).

At this time then, the field had understatedly become "the primary focus of encounter between the geologist and the phenomena of his science, and therefore an indispensable part of his activities" (op. cit., p. 37). The primacy of fieldwork for geology and its empirical focus in the field were further embodied by geology's intrinsic spatial dimension, translatable by maps, traverse and columnar sections, the use of which was well-established by the 1830's. These visual representations of the field constituted an indispensable "visual language" for the geologist, used for effectively communicating his field findings to his peers (Rudwick 1976). Indeed, the visual aspects of geology were already deeply engrained in practice, and so it was essential that the field geologist have a "good eye" for interpreting the topography, soil, vegetation, rock types and fossils and for translating them into sketched maps and sections made in the field, and then into completed maps and sections used in communication with others. In this sense, geological practitioners would indeed get "nowhere, in all senses of that phrase, if they remained 'closet philosophers'" (Rudwick 1985, p. 41). The essence of "doing geology" was to be found in the field.

However, the centrality of the field to geological practice had more significance than simply allowing for "a more rational assessment" of the subject. Doing fieldwork was also loaded with cultural connotations, even then, of rugged individualism, for walking twenty miles on foot a day, going from first light to dusk was not considered unusual, "at least by the English" (op. cit., p. 37). Rudwick at this point refers us to a footnote proposing national differences, for apparently two young French geologists (i.e., Dufrénoy and Beaumont) who surveyed their country on government service were each recommended for the 'croix d'honneur', on the grounds that their fieldwork had been arduous and entirely on foot! Given the absence of context surrounding this incident, we

must at least temporarily stick to Rudwick's contention that more was made of the rigors of fieldwork by the French than the English, although this is hardly the case today.

Finally, in Rudwick's (1985) description of the period's fieldwork practices, we also find sentiments of romanticism as well as a "tacitly pantheistic religion" with elements of "robust, manly Christianity and the gentleman's love of the countryside and its sporting pursuits" (p. 41). It was marked by elements of "liminality", where a "liminal pilgrimage" away from civilization and set ways of thinking about the world into a closer communing with rural nature allowed for geological innovation and theorizing outside of the established ways of understanding natural phenomena (Rudwick 1996). Also, given the close contact established with nature, fieldwork was considered to be initiation and ordeal, "the mark of the true geologist" whose "arduous nature was the test of his apprenticeship and the badge of his continuing membership in the 'brethren of the hammer'" (Rudwick 1985, p. 41).

Just as important for our purposes of tracking the historical precedents to modern fieldwork practices is Rudwick's (1985) emphasis on the nineteenth century geological practices of a *group* of geologists rather than the isolated activities of only *one person,* as has been the case thus far (e.g., Steno, Maillet or Buffon). For the early part of the nineteenth century was no longer a time, in Rudwick's words, of the "universal virtuoso or the polymathic savant" (1985, p. 18), but a time when scientific practice had already developed into discrete fields — or, like geology, was still developing — each with its own group of practitioners, institutions, and corresponding journals. According to Kronic (1976, p. 89), the number of active, substantive journals in Europe increased exponentially over the course of the eighteenth century, going from 7 in 1710, to 27 in 1750, to 118 in 1790. This was certainly true for geology in the 1830's, as well, for even as a relatively new discipline it already possessed a dozen different specific journals for

publication[vii] (Yoder 1993; see also Bazerman 1988 and Salager-Meyer 1998 for corresponding accounts in physics and medicine).

While we may also come across a good number of practicing geologists from this time, it is only their undertakings as an ensemble that give meaning and substance to the advancement and development of the field, and context to geologists' practices. Following Rudwick's "read" of the historical documents coming from this period, geological science at this point in time resulted from the ongoing debates and communal shaping of what he terms "natural knowledge" (p. 429) in the forum of national geological societies. Importantly, it is in these national geological societies that disciplinary practices made a large leap forward in standardization.

These national societies also embodied distinct cultural inclinations, in principal making it possible to provide at least two different narratives of geology during this time, one of the British and another of the French geology scene. We might, of course, also talk about the German geology scene which was not unimportant by any means, as reflected by an extensive list of important contributors[viii], but it will purposely not be dealt with here given that it is especially the first two national research scenes which are central to any in-depth examination of the sets of issues the French geologists interviewed for this study might currently face.

The most influential of these national societies at the time of the "Devonian controversy", which in short centered around a quest for determining the age of older and older crustal strata, was the Geological Society of London (GSL), the "very first" geological society (established 1807). According to Rudwick, quick to disavow any personally nationalist pretensions, it was internationally regarded as the most active center of geological research (1985, p. 18).

Originally intentioned in a utilitarian function as a mineral resource center, in service of the country's economic and mining needs as a sort of national repository for geological data and specimens, the GSL was quickly taken over to serve the ends of a

"gentlemanly dining club" and learned society. As a consequence, those who had the most to contribute in terms of real concrete and expert information, in other words, the land surveyors and professional mining experts, were excluded early on from membership on social grounds. The president of the London society, Greenough, as the supervisor of a national project entailing the geological mapping of all of England so as to determine the country's economic resources, in fact took over the mapping as a personal research project intended to further his own career (op. cit., p. 20). And even further evidence of the domination by the elite is to be found in the "snubbing" of William Smith, accomplished land surveyor, considered by Rudwick to be the father of stratigraphy (op. cit., p. 62) and who published the first geological map of England (Yoder 1993, p. 448). However, Smith was refused admittance into the Society for want of status. Society members, some twenty years after the publication of Smith's (1815) very influential geological memoir, eventually recognized his importance and rather contentiously granted him a medal of honor to award his being "a great original discoverer in English geology, and especially for his having been the first in [the] country to teach the identification of strata, and their succession, by means of their embedded fossils" (Rudwick 1985, p. 63).

One enduring legacy of the originally-intentioned mineral resource center, however, was the policy, or even ideology ,of favoring empirical facts over theoretical generalizations. And given that those with effective power in the early years of the society rigorously excluded theorizing in favor of purely "fact-oriented, Baconian science" (op. cit., p. 18), allowing only field evidence as the basis for making claims, this "antitheoretical" stance was the official policy of the Society long after actual practice had discounted it. It was not until the 1820's that this position began to undergo a subtle and gradual change, as a new, younger generation entered into the society, mostly participants themselves in the Devonian controversy and endorsers of the sort of theorizing required for its resolution. Although geology publicly bowed to the weight of

empirical evidence and Baconian science, it had in practice already become highly interpretive as the geologists of the nineteenth century, moving deeper and deeper into the Earth's crust, were already having to work with what they could not always "see". These younger geologists eventually gained power from the older members, and space was thus made for public theorizing after paper readings which permitted "competent geologists of any kind (field or theoretical)" to openly state their theories about the significance of their observations. Because the debates were not recorded, the society could thus tolerate the private disagreement inherent to the process of theory-working while publicly maintaining its "atheoretical neutrality" (Rudwick 1985, p. 25).

A Continental precedent closely comparable to the original utilitarian purpose of the Geological Society of London (GSL) is the Ecole des Mines in Paris (founded in 1788 during the last years of Louis XVI's reign). This was a state institution which, in addition to surveying mineral resources, also provided advanced training for land surveyors (Rudwick 1985, p. 19).

The practicality and nationally-centered interests which surrounded French fieldwork missions at this time is reflected in part by the fact that much funding for fieldwork came directly from the state — in contrast to the English practice, where it came rather out of private individuals' own pockets. In addition, geology in France had undergone a relatively long-lasting institutional subordination to other disciplines, such as mineralogy which was considered more relevant to the mining enterprise. And so, for example, while geology was originally included among the 12 chairs established at the Muséum National d'Histoire Naturelle in 1793, it remained absent from other Parisian institutions for some time. At the Université de Paris created by Napoleon in 1808, for example, geology is still considered a part of mineralogy. It is not until 1831, and the arrival of such up and coming young geologists as Elie de Beaumont on the research scene,[ix] that geology is finally taught independently of mineralogy at other important

Parisian institutions, such as the Académie des Sciences in Paris and the Collège de France (Gohau 1987, p. 10). This, then, reflects an effective change in status of geology in France, as a pursuit that had finally moved beyond the mere "harvesting" of mineral resources by the state and had blossomed into a full-fledged academic science in its own right.

This change is reflected in other ways as well, for in the early 1830's France is at last endowed with its own geological society, the Société Géologique de France (1830), cofounded by Ami Boué and Constant Prévost (Perrodon 1980). Modeled closely after the London society, it grouped together, despite the "national" proclivities of its name, a largely inner circle of active, Parisian geologists (Rudwick 1985, p. 28). In this sense, it acted mostly as an arena for informal debate among the "salaried *professional* geologists" of the Ecole des Mines, the Muséum, and the Académie des Sciences. However, it differentiated itself from its Londonian counterpart by markedly insisting on *égalité;* as such, it allowed no distinctions between different classes of membership, and foreigners could become members on the same terms as the French by paying the same entrance fee and subscription (op. cit., p. 28). However, given that it had fewer than half the number of members of the Geological Society of London (in 1836, it had a mere 302 members in comparison to the GSL's 810), the insistence on open access to all could also be taken as a ploy to bolster its numbers and its significance. There are, in fact, exceedingly few big name French geologists who stand out from this era. Even the Parisian society was at this point in time made of up nearly one-third of non-French nationals.

However, this is not a point whose interest lies only in describing national issues, for it was in fact the very overlapping of the two societies, Londonian and Parisian, that accounted for roughly two-thirds of all minimally competent geologists in Britain and continental Europe at the time (op. cit., p. 419), thus pointing to the increasing "internationalization" of geological research problems and methods.

## 2.4 The development of recognized field competence

In the expansion of geological science into nationally discrete and distinct, but also internationally linked, research scenes, it becomes tempting to want to speak of the emergence of a "geological community". Rudwick makes a strong disclaimer, however, to using the term geological "community" at the time of the Devonian controversy (1834-1842), for it "anachronistically [suggests] a strong-boundaried professional group, marked by standardized training and certification, differentiated from an uninitiated lay public on the outside" (1985, p. 418). Pertinent to current debates over discourse community, Rudwick further goes on to question whether the image of "disciplinary community", with similarly strong boundaries between the initiated and the uninitiated, can even be taken as a valid concept for modern science, and insists rather on the necessity of considering a "minimum [level of] competence" as the only binding, concentric value accorded to modern members of a scientific community. This minimum competence is bestowed on modern members of disciplinary and academic communities as holders of a Ph.D. (op. cit., p. 418).

It is this notion of "minimum [field] competence" that Rudwick extends back to the early 1800's as a defining characteristic of the emerging discipline, when geological practice was still largely characterized by a *lack* of formal and standardized training. Rudwick thus suggests, rather than proposing the idea of a fixed community with rigid boundaries between initiates and non-initiates, that it would be more appropriate to speak of acknowledged participation in a "circle" of gentlemen geologists with a Londonian elite in the center surrounded by "outlying locals". One's participation in the circle depended on gaining recognition of demonstrated competence as a field geologist among one's more or less equal peers in national geological societies (op. cit., p. 419). Thus, the notion of "field competence", both demonstrated and attributed, is particularly important

for describing the place the geological terrane had come to occupy in nineteenth century geology, retaining its importance for describing geology field practices today.

As we can recall, at the start of the nineteenth century geological practice was still largely concerned with the *empirical* description and interpretation of the Earth's layers. According to Rudwick (1985, p. 49), the procedures and techniques for describing a sequence of formations reach back into the eighteenth century, but as we have seen, its historical roots may also very well reach back into the seventeenth century as well. The mapping done by Cuvier and Brongniart (1811) around Paris and by Smith (1815) around Bath, however, was instrumental in establishing a "standard" for describing simple stratigraphical structures. These regions were considered relatively simple to analyze given that the strata there had retained their horizontal position and were easily accessible, having been excavated deeply into valleys (Rudwick 1985, p. 49).

Comparing local geological formations to these two early works, "standardized" memoirs describing local geology had become common by the 1820's. Starting with simple structures or even those with good exposures, the field geologist could gradually learn, on his own, to recognize more or less ambiguous or well-exposed examples. As a result, his confidence in "the reality of inferred structures grew accordingly" (Rudwick 1985, p. 49), as well as the belief in his own expanding competence domain. Thus, learning through imitation and comparison, given the continuing lack of standardized institutions for knowledge transmission, geologists had by this point in time learned how to cope with areas of less simple structures, and with increasing success were extending the procedures to deal with more extensive regions.

Nonetheless, the structures which were considered difficult at the time, such "deceptive features" as jointing, slaty cleavage, highly tilted, folded or otherwise disturbed terrains, fractures, dislocations or faults, and non-conformities, not to mention the interpretive problems engendered by interpreting the older rock formations of the

Paleozoic, raised a good deal of skepticism among the in-group of leading Londonian geologists when those considered “less competent” dared venture an interpretation (Rudwick 1985, p. 52). The elite geologists considered that these were features learnable only by a long and especially “wide” field experience, such as their own, with an arduous “apprenticeship in the field” posited as necessary for the development of “practical competence” in fieldwork (op. cit.). It was the basic condition sine qua non anything but the lowest levels of contribution to the science could be made.

It was, however, also a condition determined largely by social status and it is here that Rudwick fully advances his notion of a “gradient of attributed competence” (Rudwick 1985, p. 419), with some scientists recognized as being more equal — and meritorious — than others. Here we see that attributed competence in fact operated as a “commodity” in geological circles, assuredly gained through vast field experience, but also to be granted or taken away by the Londonian elite, who exercised effective control over how much acknowledgment was to be given to those “lower down” on the — social — competence scale. A vivid illustration to this can be found in the words of one geologist of the time, A. Sedgwick, who patronizingly remarked about a dangerously competent local geologist, at first treated as “a mere local geologist with moderate competence”, that he was someone who “[wanted] nothing which a little practice [would not] give him” (op. cit., p. 423).

It is also this notion of “attributed competence” that might explain the relatively limited number of non-English participants in the Devonian controversy, as well as the impression that there were relatively few important francophone geologists of the day, an impression quickly corrected by a perusal of the period’s volumes of *Bulletin de la Société Géologique de France,* Yoder’s (1993) reconstruction of important geological contributions, or by Gohau’s (1987) account of the period. This interpretration is actually further supported by other historical documents used by Rudwick (1985), namely one in which he cites the case of a Belgian geologist, Dumont, who was grudgingly accepted as

a competent geologist, but only once his complete mastery of the complex folded strata in his native Liège province had been demonstrated "on the spot" to two of the leading English geologists of the day, Murchison and Sedgwick (op. cit., p. 423). And so, through the proof of field evidence coupled with the weight of interpretation, an outsider could gradually gain recognition of competence, but without ever necessarily attaining the ranks of the elite.

Thus, the legacies left by this period of geology's development are many. As eloquently summarized by Rudwick,

> "The Devonian controversy exposed the procedural roots of geological practice and subjected them to more probing scrutiny than ever before. The successful resolution of the controversy endowed geological practice with a new confidence in the reliability of its empirical conclusions — a confidence that it retains to the present day" (1985, p. 5).

And so we have seen an ever increasing centrality accorded to the field, in terms of empirical locus, standardization of method and practice, scientific inspiration, social bond, and source for the attribution of personal and professional qualities. Fieldwork was also a highly practical enterprise, based on a subtle and largely tacit body of rules and precedents learned largely through unsupervised, practical experience in the field. A geologist's skilled knowledge of the trade came from self-made interaction with nature.

As one last note about the period, it is of extreme interest for our understanding of modern-day geological field writing practices to note that fieldwork descriptions by this time, communicated through the medium of intensive letter writing and public paper readings, had already become far more than a "simple description", having evolved into a rhetorical tool which geologists could wield in order to gain acceptance, credibility, attributed competence, and maintain authority,. It entailed the skillful marshaling of relevant evidence in ways to make the most persuasive impact. The rhetorical handling of this evidence was not just "a stylistic 'extra' tacked on to content for good measure; it

*was* the content" (Rudwick 1985, p. 423), and as such was inseparable from the communication of field observations.

## 2.5 The growing polemic between empirical field practitioners and experimental method and theory

Of course, the review of geology's developments as a field-based discipline would not be complete unless we also examined the on-going conflictual issues between field-centered empiricism and experimental theory. While Rudwick points to the non-confrontational, even complementary, relationship that existed between "theorists" and "empiricists" of the middle 1800's (1985, p. 54), this complementarity did not continue indefinitely, especially once other "non" geologists, largely physicists and mathematicians, soon thereafter began making "incursions" into what geologists considered to be their privileged domain (Dalrymple 1991, p. 13).

In his extensive bibliography, Yoder (1993) cites the first issues of the *Journal of Geophysical Research* appearing as early as the 1860's, and it was during the latter half of the nineteenth century that the Earth's age became one of the most hotly debated subjects, not just of geology, but of science (Dalrymple 1991, p. 13). Resolution to the questions sourrounding the age of the Earth could not occur until the contributions from chemical radiometric and isotopic methods, field geological, and physical methodologies converged to provide appropriate tools for determining the age of the Earth. Made possible only by this multidisciplinary convergence, the means for providing answers to the questions hereunto left unresolved by geological science were moving beyond what even the most competent and talented field geologists could themselves see in the field.

Field geologists of the late nineteenth century, however, resolutely maintained their preference to empirical field data over mathematical calculations, physical theories or chemical analyses, as reflected in the following passage written by, in Dalrymple's (1991) words, "a highly respected professor of geology at the University of Chicago":

> "The fascinating impressiveness of rigorous mathematical analyses [NB. making reference to Lord Kelvin's calculations of the age of the Earth], with its atmosphere of precision and elegance, should not blind us to the defects of the premises that condition the whole process. There is perhaps no beguilement more insidious and dangerous than an elaborate and elegant mathematical process built upon unfortified premises" (Chamberlain 1899b, p. 224, cited in Dalrymple 1991, p. 44).

Many nineteenth century geologists were simply ill-at-ease with the application of physics and chemistry to what were considered to be "geological problems", such as the age of the Earth and determining geological time. Most in fact preferred to rely on the results they themselves accrued though their own field observations. As tellingly recounted by Dalrymple, late nineteenth century geologists'

> "... reaction to the physicists' intrusion into what they considered to be their domain ranged from the defiance of Sir Achibald Geikie (1903: 77): "Until it can be shown that geologists and paleontologists have misinterpreted the records contained in the Earth's crust, they may not unreasonably claim as much time for the history revealed in these records as the vast body of accumulated evidence appears to them to demand" to the apologia of T. Mellard Reade (1893: 97): "Geologists can hardly be blamed if they attach greater weight to their own observations and data and to reasoning that is more familiar and appears more certain and satisfactory to their minds" (1991, p. 59).

However, experimental contributions were already inextricably making their way into the foreground of geological debates. Radioactivity was discovered by Henri Bequerel in 1896, and two years later Marie Curie discovered the radioactivity of thorium, polonium and radium, "susitant désormais un intérêt qui gagne le grand public mais dont personne ne peut encore deviner la pertinence pour la géologie" (Richet 1999, p. 88). In 1902, the physicist E. Rutherford, in collaboration with F. Suddy and P. Curie, discovered radioactive transmutation, where each radioactive element was observed to disintegrate at a constant and particular rate, independently of temperature, pressure or chemical compound, thereby constituting an "absolute measure of time" (op.cit., p. 89).

Rutherford's early uses of radioactive decomposition in geological dating (1904-1906) were wrought with imprecisions, however, and the great fluctuations in age estimates were unconvincing to geologists, to say the least. As Richet (1999) explains,

> "Si Kelvin, la plus haute autorité en physique de son époque, a pu nous méprendre par ses calculs[x], sont enclins à penser les géologues, comment pourrions-nous être certains que ces nouvelles méthodes ne sont pas également fallacieuses?" (p. 89).

Only a few years later, however, we can already begin to see the increasing interplay between field and experimental methods, and acceptance of the latter in the work and conclusions of such well-known and respected geologists as Arthur Holmes, who also happened to be "doted with a solid training in the physical sciences" (Richet 1999, p. 90; Gohau 1987, p. 221). As Holmes argued,

> "Wherever the geological evidence is clear, it is in agreement with that derived from lead as an index of age. Where it is obscure, as, for example, in connection with the pre-Cambrian rocks, to correlate which is an almost hopeless task, the evidence does not, at least, contradict the ages put forward. Indeed, it may confidently be hoped that this very method may in turn be applied to help the geologist in his most difficult task, that of unraveling the mystery of the oldest rocks of the earth's crust; and, further, it is hoped that by the careful study of igneous complexes, data will be collected from which it will be possible to graduate the geological column with an ever-increasing accurate time scale" (1911a, pp. 255-256, cited in Dalrymple 1991, p. 74).

Indeed, the viability of experimental evidence *was* clear for Holmes (1911) who, by measuring lead levels in minerals from the same rock sample, had obtained conclusive results allowing him to offer radiogenic ages in fact similar to actual rocks from the same geological period. Thus, he found 340 Ma for base of the the Carboniferous, 370 Ma for the beginning of the Devonian and 430 Ma for that of the Silurian, in remarkable agreement with the modern ages established at 355 Ma, 408 Ma, and 435 Ma, respectively (Richet 1999, p. 90).

Nonetheless, Holmes' was an achievement met with disinterest and skepticism in geological circles, as illustrated by the comments made by Pierre Termier in 1919:

> "Le sentiment général, parmi les géologues, est que cent millions d'années [pour un total des périodes géologiques] est un minimum. Depuis la découverte des corps radioactifs, une nouvelle méthode de calcul a surgi, en effet, dont les résultats, assez concordants dans leur ensemble, conduisent à allonger beaucoup les périodes, jusqu'à attribuer à quelques unes d'entre elles cent millions d'années; mais la méthode repose toute entière sur un postulatum invérifiable, qui est la constance absolue de la vitesse de désintégration de l'atome instable, de plus, les causes d'erreur, dans de tels calculs, sont nombreuses... Tout cela est vraisemblable, et cependant très incertain" (cited in Richet 1999, p. 90).

But change was already underway, and Francis Aston "ups the ante" even further in 1919 when he invents the mass spectrometer, allowing for an even greater precision in dating through measurements of isotopes. As methods now used three separate elements to pinpoint time, precision in dating eventually allowed for accuracies of up to 1/10,000 (Allègre 1980). Moreover, these radiometric findings were ever more consistent with the geologically-determined ages found for the Earth's rocks, based on physical evidence, thereby making "the claims of each more credible" (Dalrymple 1991, p. 77).

And so, for the first time, investigations surrounding the Earth's age were beginning to be grounded as equally in adequate theory as they were in empirical evidence, thus indicating the beginnings of a Gestalt shift in geological reasoning, with claims based more on "quantitative *reasoning* rather than scientific *intuition*" (Dalrymple 1991, p. 75; emphasis added). These new methodologies provided the theoretical basis for radiometric dating, and had a profound impact on how geologists eventually came to judge geological time. However, once again, this was research still taking place largely outside the domain of geology, and geologists resolutely continued to ignore non field-based advances until the middle of the twentieth century when these new methodologies (e.g., electron microscope, mass spectrometer, isotopic methods), greatly improved as a result of the war effort in the 1940's, took over by storm. In was also at this time that at long last one saga was put to rest, when the accepted estimation of the age of the Earth had rapidly increased to 4.5 Ga (Seuss 1949), from the "bit more than 1.46 Ga and probably a lot less than 3 Ga" of Arthur Holmes in 1931 (Perrodon 1980, p. 19)[xi].

The seeds for one other major shift in geological methodology are to be found at the start of the twentieth century in work by the physicist Wegener (1912) who, using theories from physics, came very close to altering the face of geology as early as the end of the nineteenth century. One of his theories was the geophysical discovery of the interior structure of the globe through predictions about the speed with which seismic waves would propagate through the Earth. The second important proposal he made was a theory for continental movement, which posited that one primary continent, Pangea, was divided into the seven continents known today, thus explaining similarities found between terranes and fossils on different continents.

While the value and importance of their contribution to the discipline has since been overwhelmingly recogntized, these theories were rejected by both geologists and geophysicists at the time (Allègre 1995), for the problem with their immediate geological application lay in the absence of a "motor": an adequate explanation and veritable cause for the Earth's movement. As recounted by Gaudant (1995), Wegener's theories in fact met with fierce opposition in France, where as early as 1923, the Société Géologique de France had dedicated one of its sessions (23 April, 1923) to the problem. Apparently, L. Joléaud, a staunch supporter of the theory of continental bridges, "severely criticized [Wegener's] theory" (Gaudant 1995, p. 133), which

> "rend compte en partie des curieuses anomalies apparents que réflètent les données de la paléoclimatologie, mais [...] ne permet pas d'interpréter la cause de l'un des points essentiels de ce domaine scientifique. ... Elle peut contribuer à élucider certains énigmes de la tectonique mais [...] ne constitue pas une base d'interprétation générale de l'orogénie terrestre" (Joléaud. 1923. Bull. Soc. Géol. Fr., 4(23), pp. 205-257; cited in Gaudant 1995, p. 133).

In the end, however, the essential discoveries from these two research domains outside of geology did weigh out against the sole primacy of field data, and have as such moved the locus of geological research beyond what one can perceive and interpret by oneself in the field and into the laboratory, as newer generations of geologists trained in

the new methodologies and theories began to compete heavily with an older generation of fieldworkers.

In the late 1950's and 1960's, the field of geology in effect underwent a veritable "revolution". At this point, what has been an essentially nineteenth-century field discipline, such as that locally situated in France, comes almost brutally into contact with the outside. It undergoes an impact with a multitude of physical and chemical studies whose methods began to be applied for the first time to the study of rocks after WWII, resulting in a veritable scientific — and geologic — boom. The resulting explosion in the number of corresponding studies is further reflected in the exponential increase in the number of scientific journals and articles repertoried in the database GeoRef after these years.

At that time, geology finally consolidated the long-wrought work of now centuries of fieldwork with the more recent, experimental innovations introduced at the very tail-end of the nineteenth and start of the twentieth centuries, the effects of which are so easily forgotten. As remarked by Allègre (1988), commenting on the development of geology as a discipline over the course of the last century,

> "Scientific theories are like talented artists: once recognized their merits seem so obvious that their success is assumed to be due only to their excellence. In science especially, new ideas are seen as an inevitable and unshadowed enlightenment, and the fact that the process of discovering them was slow and chaotic is forgotten" (1988, p. vii).

Disciplinary crossovers due to the increasingly acknowledged contributions of physics and chemistry effectively moved the point of focus away from the field to the laboratory, and many seemed to have left behind geology's descriptive mission — or were told to do so — in order to begin work on the Earth's motor and mechanisms. As a consequence, today's "pure" field geologist is considered to be a "has been" by his more "up-to-date" experimentalist colleagues. And, in current literature, we can find echoes of this shift in decreasing prestige accorded to field evidence and the fieldwork endeavor where certain

geologists disapprove of geology's descriptive and classificatory mission as having remained in force far too long. They explain this perceived disciplinary shortcoming as their misguided colleagues having "blindly" continued to focus on the Earth's crust by relying on "pure descriptions" made in the field rather than on obvious theory or physical principles (Allègre 1988).

## 2.6 The "revolution" in France: the effects of Wegener's theories on the French geological community

Gaudant (1995) provides us with a compelling account of the reversal of power resulting from what we can identify in retrospect as an inevitable methodological and theoretical shift in the geological community. In his article, he shows us that field geologists in France put up a good deal of resistance to the change underway, with the struggle lasting well into the 1960's. As he describes it, however, the shift was insidious, creeping its way into the community, slowly but surely.

Gaudant links the early dispersion of the growingly accepted theory in France to the way in which scientific ideas are diffused within the scientific community in general. French doctorates, such as C. Allègre and X. Le Pichon, left for post-doctoral study in the United States in the late 1950's and early 1960's, learned the new methods and theories, and then returned to France and started their own laboratories. Conferences and symposiums allowed for the rapid diffusion of new ideas, such as those held in Berkeley in 1963 and Cambridge in 1970, where Le Pichon gave a talk on his recent work in plate tectonics. Important and prestigious journals also spread the word, such as Dietz' (1961) article published in *Nature,* and Dietz and Holden (1970) in *Scientific American.*

Yet rigid opposition to Wegener's theories persisted in France into the late 1960's. Gaudant (1995) reports on a series of interviews he conducted with 26 geologists representing the "old school" of French geology, where they were asked to recount their experiences during the "revolution"; those interviewed had all received their doctorate

prior to 1960. Their influence on field geology was such that it is said that all remaining field geologists still active today in France have at one point either studied with one of them, François Ellenberger, or one of his immediate students (L. Latouche, pers. comm., 2001).

What is revealed from the interviews about the long-lasting opposition to the new theories is multiple. Some of the opposition was practical in nature, as we see in the words of R. Dars, for whom "les travaux sur le terrain (à terre comme sur mer) demeurent indispensables [car la théorie] ne rend pas compte de plusieurs faits naturels" (Gaudant 1995, p. 135). But of course, many of the comments tended toward greater virulence, such as that found in the criticisms of J. Debelmas who disapprovingly pointed his finger at "les abus des autres modernes qui, au prix de plus de subduction, microplaques, tranformations et autres, créés pour le besoin de la cause, sont capable d'expliquer n'importe quelle structure" or A. Cailleux who simply rejected the 'new doctrine' by doubting the younger generation's intellectual capacity: "Compte tenu de la diminution générale de l'esprit critique et de celle de l'information, je comprends que mes cadets, qui lisent peu, y croient ferme" (both cited in Gaudant 1995, p. 135).

Of course, it is far easier to formulate such attacks when oneself is under attack, and this the old school was. C. Pomerol was quoted as having been "choqué par le triomphalisme de certains: avant le néant, après la lumière. Quelle fatuité et quelle naïveté!" Indeed, Gaudant takes great pains to point out how the elder geologists considered the converts to the "new doctrine" to have displayed an excessive rejection of past descriptive fieldwork practices, with an undeniable propensity toward intolerance of opponents and sceptics. As P. Burollet describes it, an inhospitable atmosphere was quickly generated by "la terreur intellectuelle qu'ont fait régner les tectoniciens américains, puis français, les plus sévères étant comme toujours les convertis de fraîche date." F. Ellenberger, in a way which captures the very essense of the debate, describes

how his and others' life-long contributions to geology were lightly cast aside, like the proverbial baby thrown out with the bath water. He stigmatizes

> "le dogmatisme d'une "théorie"..., l'intolérance de ses adeptes; la prise de pouvoir des dits avec accaparement des crédits; leur inintérêt pour tous les points où la théorie "coince", l'extrême spécialisation à dominante technique excluant toutes les visions synthétiques et historiques; le désolant réductionnisme du vocabulaire..."

The reponses obtained during the interviews overwhelmingly show that the phenomenon at the time was considered by those in the "old school" of field geologists to simply be an "effet de mode". Yet those who did not comply with the new doctrine were treated as "retrograde", and were eventually deprived of status and even prior achievements[xii]. Ellenberger denounces, for example, the repeated stealing of credit for past work he had observed on numerous occasions, citing in particular the case of one group of Anglosaxon researchers who, in a 1965 *Philosophical Transactions of the Royal Society of London* publication, published a map which was the exact replica of a map published in 1935 by a French geologist, but without a single reference to their Continental predecessor (Gaudant 1995, p. 135).

These repeated attacks ultimately led to a deplorable research climate in France during the 1970's, characterized by a lack of "freedom of thought" and what B. Gèze and P. Routhier have referred to as "sclérose en plaques". In Gaudant's conclusion, he points to the emergence during the 1960's of a

> "nouvelle foi... avec la constitution d'un cortège de grands prêtres. Cette religion repose évidemment sur l'acceptation de certains dogmes qu'il est impossible de remettre en question sous peine d'être considéré comme un parjure ou un renégat" (Gaudant 1995, p. 135).

What remains from this difficult period in French geological history is an image of ailing health, and it is questionable whether the geological community has ever truly recovered from the experience of its "revolution". For one, these internal struggles have taken their toll on the boisterousness of the French academic geological community, by at

least momentarily stifling the liberty of thought so central to the research mission of the C.N.R.S. ('Centre National de Recherche Scientifique'), founded in 1935 , which in 1980 employed some 500 geologists (Perrodon 1980, p. 14). Further, the work of a generation of geologists was cut short as they were pushed out the doors. Ellenberger, of course, has had strong thoughts about the effects of centralized planning by only a few on the research initiatives of the whole community:

> "... le dirigisme en science est, de tout temps et en tout lieu, un péril mortel pour la recherche fondamentale, en Géologie comme ailleurs. « Sire, pour être florissants, les sciences ont besoin de liberté » : ainsi s'est exprimé Constant Prévost en présentant au roi Louis-Philippe, le 20 août 1830, la Société géologique de France toute nouvellement constituée, et son apostrophe est plus d'actualité que jamais. Ce n'est pas le lieu d'évoquer davantage ce pénible sujet du fléau d'une science dirigée autoritairement vers le corset stérilisant des « bons choix » supposés." (Ellenberger 1980, p. 35).

Some today readily recognize that with all the in-house bickering and difficulties in imposing the methodological shift from field to experimentation, French geology has largely, apart from the likes of X. Le Pichon and C. Allègre and their laboratories and followers, simply "missed the boat" and has never been able to catch up with the extraordinary delay in innovation and cutting-edge research it has experienced compared with Anglosaxon researchers. In some respects, the centralized system for allocating research funds, originally crucial for encouraging freedom in research initiative, has led to atrophy within the research community, as only a minimal level of output is required to assure the yearly research budget, resulting in little motivation for research innovation and production.

Further, budgetary restrictions have continued to increase, as successive conservative governments in France from the middle 1980's to the middle 1990's have repeatedly cut funding for scientific research. While funding levels have remained theoretically stationary since the start of the most recent cohabitation period, bringing a socialist government to power in 1997, conditions have continued to worsen as funding

"credits" for the C.N.R.S have in fact been transferred to other areas of national education. The prospects of the right returning to legislative power in 2002 bode ill to the stability of budgetary research credits in France, especially in the context of important funding cuts having recently been made into the research and education budget by the socialist Jospin government in order to finance the demands of law enforcement and heightened security measures (N. Arnaud, pers. comm., 2002). And finally, field geologists continue "symbolically" to lose ground, as the Ministry of Education and Research has recently (1999), under the guidance of C. Allègre, decided to phase out the "historic" geology department at the Muséum National de l'Histoire Naturel — which is, as we can recall, geology's first institutional establishment in France.

1980 was the year in which the Société géologique de France celebrated its 150th anniversary. That year's session was a time for reflection on the "state of the art", as can be observed in the collection of articles published in the *Bulletin de la Société géologique de France's* Jubilee edition (1980). F. Ellenberger sums up the travails of the period which French geology had just crossed, and offers an apologia for the French community's collective faults: for not having seen the tides turning, for being overly rigid, and for having almost irrevocably isolated themselves from the international research world.

> "Notre tort à nous français est double: nous méfier si souvent des nouveautés, tant que leur rentabilité ne paraît pas démontrée, mais aussi dénigrer volontiers nos travaux, au lieu de mieux les faire connaître. ... Il est facile en 1980 de remâcher avec une certain amertume devant notre manque probable de clairvoyance et d'audance au cours du demi, ou du quart de siècle passé, et d'avoir été mal à l'écoute des grandes innovations en train d'éclore ailleurs (quitte à se ruer ensuite en troupeau dans le train en marche)… L'innovation d'aujourd'hui est vieillerie ou routine de demain. Doctrines et modèles se succèdent; d'être à la mode un jour garantit d'être démodé bientôt. Il faut se méfier surtout des révolutions: elles se sclérosent trop souvent en un nouveau conservatisme…" (Ellenberger 1980, pp. 34-35).

## 2.7 A framework for analysis

This, then, is our background, the frame within which we must begin to pinpoint the sources of today's geological field writing, such as it is practiced in today's major research journals as well as in the small geology department in France where this study's primary informants now work. The context for these written practices is further framed by the ongoing research tendencies described here: One promotes a disciplinary practice whose source of inspiration and break-through comes from observations made in the field, while another, whose primary research concerns have been relocated from the field into the laboratory, seeks to document the conditions of the Earth's genesis by recreating them in the laboratory, thus making it possible for some geologists to avoid all direct contact with the field altogether. We are reminded of geology's earliest mission of explaining man's history and place on Earth in the following divergent description of the contemporary essence of current disciplinary practices:

> "Geology is distinguished from history, in the traditional sense of the term, in that it is a natural science. The messages that have been "written in stone" were written not by man, but by nature. The laws that we seek to discover *are not those of human behavior, but of nature"* (Allègre 1988, p. 249, emphasis added ).

And thus we can fully appreciate that the break between man and nature has been ultimately consummated over time, making way for a discourse today in which rocks may effectively speak for themselves.

Today, the descriptive task of relating fieldwork results in the scientific research article is often looked down upon and dismissed as less important than experimental results, yet at the same time authors remark on the fact that it is hard to do well (N. Irvine, pers. comm., 1996). Ironically, the full importance of the location of minerals or rocks is often lost without an accompanying discussion of the surrounding geological context. As a consequence, this descriptive section of field results, far from being a "simple description", is today considered by geologists, as both writers and readers, to be

one of the most difficult and painstaking sections of the geology article to write. As Irvine and Rumble comment: "One of the frustrating truths of petrological studies is that many detailed measurements and observations commonly have to be made on rocks and in experiments in order to define pertinent general relationships, but the details themselves have only limited interest" (1991, p. 27). One geology informant echoed this same frustration when he noted that Geological Setting sections where field results are often related were "boring to write, and boring to read. In fact, I have to admit that I often skip over them." The reasons for this difficulty become apparent in the words of Irvine and Rumble once again, who have remarked that "commonly there is no indication or explanation of why the data are significant or where the description or discussion is leading. Faced with so much uncoordinated information, even the most dedicated readers soon bog down and lose interest" (1991, p. 28).

To insist, then, that geology has become a purely experimental science, having once and for all moved beyond the need to go out into the field and witness the processes of Earth's features and transformations for oneself, would be sorely misleading. For by all accounts, geology would not be geology without its "soul": the field. As captivatingly explained by one of my informants during the course of our interview,

> "Je crois quand même que, si on se détache totalement, du terrain, ce que la science pourrait nous faire faire à terme parce qu'on a tendance à faire des études de plus en plus pointues, mécanique, etc. J'ai peur que, en abandonnant progressivement le terrain, euh, alors, d'un point de vue purement scientifique, on finit par inventer des modèles qui n'ont plus rien à voir avec la réalité. On finisse par faire de la science fiction et l'on ne s'en rend même pas compte. Si tu veux, on peut dire mon modèle a prévu ça, même si ça n'a jamais été observé sur Terre, c'est grave. Euh, ensuite, d'un point de vue psychologique, je dirais que si on arrête complètement d'aller sur le terrain, j'ai peur qu'on y perde notre âme. Ce qui fait la géologie c'est aussi alors, il faut dépoussiérer un peu la science pour ne pas rester à ce qu'on faisait au dix-neuvième siècle, mais il y a quand même une façon d'avancer dans la compréhension de la Terre qui dépend de notre culture, et dans cette culture il y a le terrain" (N. Arnaud, Interview, October 1999)

And so, despite the ongoing "subsurface" polemic which to this day continues between a new generation of experimental methodologists and those who do fieldwork, long considered to be the "real work" of the geologist embodying a certain pride in being a "rugged scientist", in reality many modern day field geologists, such as those interviewed for this study, find themselves somewhere in the middle. These geologists thus respond to a late twentieth century need for analytical accuracy and experimental proof, while simultaneously needing to replace these findings back into a historical, and necessarily field-based, framework of recognized field competence and culture.

It will be argued here that the depiction of such necessary disciplinary practices has, over the years and decades, become largely conventionalized in the form of a "part-genre" (Ayers 1994): the Field Account. In this part-genre, we can observe that geology to this day remains a discipline that intrinsically needs to connect the micro and the macro, and to re-place a particular or aspectual technical investigation within the context of broader, more physical and geologically-historical phenomena. We will also see in the geologists' writings represented by this study's corpus that there exists a back and forth pull between the need for scientific objectivity and authorial discretion, and a corresponding need to demonstrate a return to roots by signaling field presence.

And so, we may have a first explanation of the textual silence which marks contemporary Field Accounting practices. On one hand, field researchers are bound by the contemporary practice of downplaying their field mission, for geologists are no longer supposed to care about the "nitty-gritty" of what was actually seen over weeks or months of intense fieldwork. Instead, they need to "hurry up and get the boring stuff over with" in order to get on with "the more important work of analyzing" (N. Irvine, pers. comm., 1996) their samples, and proposing theories to explain processes larger in scope than what they can humanly see. On the other hand, however, researchers must still "kowtow" to the positivist eye, indicating that they have indeed been in the field so as to

reinforce their credibility, authority and competence. What we are left with is a confined and muted discourse, needing to say much but without the — overt — means to say it.

The geologists interviewed for this study fit into this frame, and their domains of geological research reflect a broad geological culture and a marriage of different methodological approaches. We have a geochemist who spends a good deal of time nearly every year going to the far-reaches of desert regions to gather his own rock samples. While his primary disciplinary focus is the chemical composition of his bulk rocks coming from the mantle, which gives an indication of the rocks' formation conditions and hence, the Earth's genesis and evolution through time, he places his results into another frame of geological perception by also giving them a tectonic interpretation. Another geologist interviewed for the study is a structural geologist with an interest in tectonics, who studies the deformation of terranes due to surface movement, but once back in the laboratory creates models to predict and explain this movement. And finally, we also have a geochronologist also trained in structural geology, and who collects samples of rock in order to analyze and establish their relative ages according to their chemical composition. An eclectic collection of approaches appears here, each researcher with his own input and separate area of competence, further tempered by the particularities of being a geologist in France today.

The different sub-disciplines which reflect the informants' specialties are represented in the corpus analyzed for this study. The linguistic and discoursal analysis of this corpus works diligently to establish the general trends of the tacit and subdued discourses of fieldwork (non-)presence, as we will see in the next chapter.

## Notes to Chapter 2

[vi] As Rudwick (1985) explains, the restrictions placed on theorizing remained "officially" in force until around the middle of the 1820's, by which time more and more geologists were already doing a good deal of "unofficial" theorizing (p. 24)

[vii] At the time, there were a number of French journals, such as *Compte Rendu de l'Académie des Science, Bulletin de la Société Géologique de France, Journal des Mines, Annales des Mines, Annales Muséum National de l'Histoire Naturelle,* a number of British publications such as *Transactions of the Royal Society of London, Transactions of the Royal Society of Edinburgh, Edinburgh Philosophical Journal,* one repertoried German review, *Magazine Gesellschaft Naturehorscher Freunde,* and three American reviews, *Smithsonian Report, Carnegie Institution of Washington Pulication,* and *American Journal of Science.*

[viii] Not fewer than 11 during the period 1810-1840 were cited by Yoder (1993) in his 'Timetable of Petrology'

[ix] We learn from Gohau that Elie de Beaumont became professor at the Ecole des Mines in 1829 (Gohau 1987, p. 171)

[x] Lord Kelvin in fact gave what geologists considered unreasonably short estimates of the age of the Earth, in direct conflict with what field observations were then suggesting.

[xi] Current estimations of the age of the Earth have settled on 4.64 Ga (Dalrymple 1991). The reader might refer to the table of geologic time scale shown in Appendix A.

[xii] It is interesting to place this heated debate into one further context, as has been suggested to me by J. Heath (pers. comm., 2001), namely that geology's "revolution" also coincided with a pivotal point in recent French political and social history, during the turbulent 1960's and the culminating "mai '68".

# CHAPTER 3
# DETERMINING THE "SILENTIAL BOUNDARIES" OF THE FIELD ACCOUNT: A DISCOURSAL, LINGUISTIC AND RHETORICAL DESCRIPTION OF AN EMBEDDED PART-GENRE IN THREE SUBDISCIPLINES FROM GEOLOGY

## 3.1 Fieldwork reporting in geology

In order to account for what does not appear in today's field account, we first need to establish the "context" for textual silences, such as they have been discussed in section 1.4.1, and this will constitute the essential tasks of Chapters 4 and 5. Part of this context has already been established in the preceding chapter, which examined the attitudes the geological community has held toward "the field" and its practitioners over time. What we will be principally concerned with in the present chapter is determining another part of the context for understanding textual silence, or the *attested* textual features of the contemporary field account as they appear in the scientific research article. In order to better discern the context for modern writing practices, I will first briefly describe the field writing practices of times past, which will be taken as a first basis of comparison to what we currently find in modern field writing practices.

### 3.1.1 Identifying 'the possible': Geologists and their field activities

How exactly do we go about identifying what is textually *absent*? One way of doing this is to examine and comparing its multi-faceted contexts — on the one hand, what is 'possible', 'probable' and 'attested', and on the other, what is 'institutionalized'

in contrast with the dynamic workings and doings of a community, and the contributions individual actor.

As we have learned from Becker (1995), Jaworski (1993), Ducrot (1973) and others (see section 1.2), silences in oral discourse are culturally, and so communally, determined. To this end, we can recall that Pittenger, Hockett and Danehy (1960) very fittingly remarked some time ago, “It only takes one person to produce speech, but it requires the cooperation of all to produce silence.” This is certainly also the case with the types of silences we are interested in investigating here, given that what we are looking at in scientific research article trends are the practices of a group of writers, institutionally bound together by shared disciplinary background knowledge, and standardized ways of both working in the field and reporting on field results, all shaped over time.

While relatively little has been written specifically about geological culture and its field practices of the twentieth century, a fair amount has been written dealing the 1800’s, and so it would seem fitting to begin here. As we have seen in Chapter 2, geologists in the early part of the eighteenth century had already developed a standard for doing, and then reporting on, fieldwork. These activities were further standardized through memoirs (Rudwick 1985) and in popularized geological handbooks. In this respect, Ami Boué’s (1836) ‘Guide du géologue-voyageur’ is particularly important for the task of describing what field geologists have done in the past, in that it establishes the following list of “to do’s” for the traveling geologist (numbers have been added for ease of reference). As we might recall, Ami Boué played a leading role in French geology in the middle part of the nineteenth century, and was one of the co-founders of the Société Géologique de France (Perrodon 1980; see also section 2.3.1).

In this outline of what geologists ought to *have been doing* in the field we can also begin to think about what it might *have been possible* for them to write about a century and a half ago. This document is also interesting for us here, for as we shall see, field practices have in fact changed relatively little over the course of time.

Chapitre XI. ‘Règles générales sur la manière de faire des observations’ (pp. 97-98)

1. “ Le but du géologue est de connaître dans le plus grand détail possible, la nature du sol, de tout ce qu’il contient et le traverse, et de rechercher les causes de sa formation.

2. ... [On doit commencer] par l’étude de la géographie et de la configuration du pays: la distribution des plaines et des plateaux, des collines et des montagnes, des vallées et des rivières, les directions principales des arrêts et des sillons, et sur leurs rapports mutuels. Puis on tâchera d’acquérir une connaissance générale de ... ses volcans et des phénomènes volcaniques dont le pays a été témoin, etc.

3. Ce premier travail fait, on procédera à l’analyse de la constitution intérieure de son sol et des accidents de divers genres qu’il a pu éprouver. C’est là que viendra s’appliquer tout ce que je dirai sur l’examen soigné des roches, de leur composition, de leurs passages les unes aux autres, de leur altération ancienne ou récente, de leur décomposition à l’air... Il faudra aussi les étudier en grand dans leur stratification, dans leurs joints de séparation ou divisions naturelles ou secondaires, dans leurs rapports de gisement...Enfin on observera les nids, les amas et les filons, les cavernes, les éboulis, les écroulements, les ruptures, les fendillements, etc.

4. Pour voir les superpositions, des roches et des dépôts, ou leur relatés, il faut se munir de beaucoup de patience, rarement les series sont complètes, ou bien des couches plus récentes, des alluvins, des failles, des bouleversements en cachent certains membres; c’est pour cela qu’on est obligé le plus souvent de rattacker [sic] ensemble par la pensée certaines masses, dont la liaison immédiate reste cachée au voyageur.

5. Les voyages géologiques se divisent naturellement en reconnaissances locales, en relevés de coupes, et en relevés de limites de dépôts. A cet effet, il faut examiner la contrée à relever en plusieurs points, la couper en différentes directions, et suivre, autant que possible, le prolongement des couches... Il faut déjà être bien versé dans la géologie pour ces sortes de travaux, ou du moins connaître les roches et les fossiles pour pouvoir décrire fidèlement, sans se sentir toujours les connaissances nécessaires pour classer les dépôts. Ce sont les géologues stationnaires qui nous donnent le plus de ces descriptions; malheureusement, trop souvent, leur imagination les trompe au point de leur faire construire tout le globe comme le coin de terre où ils habitent.

6. Les relevés de grandes coupes conduisent à des conclusions plus certaines... En général il faut faire plusieurs coupes dans divers sens, et les répéter; car ce qu’on voit dans un profil, peut avoir échappé dans un autre, ou on peut du moins changer d’opinion sur des apparences douteuses qu’on a vues ailleurs.

> Dans les relevés de coupe, il est bon de marquer soigneusement ses observations sur les cartes, et de tâcher de les représenter par des dessins. Mais il faut avoir bien soin de distinguer les représentations de coupes naturelles, c'est à dire dont toutes les masses sont exposées dans la nature, d'avec les profils théoriques construits d'après les idées qu'on se fait de la position des couches.
> ... On se laisse aisément séduire par un joli dessin... où il ne manque rien autre chose que d'être vrai ... Cependant, la science ne doit se baser que sur des coupes naturelles, tandis que les coupes idéales les mieux faites ne seront toujours que de mauvaises caricatures de la réalité.

7. Les relevés des limites des dépôts sont le travail géologique le plus complet; ils exigent toujours beaucoup de temps, c'est ce qui fait qu'ils sont toujours précédés de profils et d'un grand nombre d'observations locales."

Of course, it is clear that what we have here is a text firmly situated in the concerns of its own time. We see echoes of Rudwick's (1985) description of the "inner elite" and the "outlying locals", and issues of "attributed field competence" in section 5, lines 4-10. And nowadays, quite often all the topographical pre-work described in section 2 is done before geologists ever go out into the field, with most regions of the world now having been geologically mapped over the years, and geologists benefitting from new technologies such as GPS and satellite imaging to help them get a pretty clear idea of the topography of the area they will be covering. We can also see Boué's devotion to the prevailing belief of the time (i.e., Baconianism) that the only way to do geology was to stay close to the actual, physical data found in the field (section 6, lines 6-13).

But we do also get a sense of some of the things geologists might still do today. Of course, recommendation number one still holds true: Geologists must still know in great detail the region they will be covering, and as they walk they must "do the geology", marking measurements and specific locations of particular points, noting mnemonic devices for remembering specific sites ('next to the river with the little bridge'). However, depending on the researcher's particular objectives, observations might then diverge. A petrologist or geochemist who goes into the field today might be

interested in gathering samples, and in situating these samples within the context of their immediate surroundings. And so in addition to sampling rocks, such a geologist might also textually describe the rocks in his field notebook, describing relations with other surrounding rocks, and in all likelihood, do a fast petrographical study, make a quick sketch or take a picture. Or, like a structural geologist, he might also be interested in more regional formation/deformation processes, and so would need to walk from point to point, trying to see the relations of succeeding structures.

Many of the recommendations Boué made over 150 years ago do in fact still ring true: One needs to have much patience (section 4, lines 1-4), be willing to follow a structure as far as one can see (section 5), even when these structures disappear from view on the other side of a river, and draw schemas but be willing to correct them as one gets a better idea of the layout of the land (section 6). A field geologist must be willing to use his imagination (section 4, lines 4-5) in order to fill out the things he cannot actually see, and also need from time to time to make best educated guesses based on extensive, past experience. And, of course, any field geologist must make drawings in the field, but take care not to stray too far from "reality" (section 6). While in some sense, this last point is strongly historically situated, for shortly after the publication of Boué's guide, visual representations were already quickly becoming more and more abstract and standardized during the period between 1840 and 1850 (Rudwick 1976), closeness and fidelity to natural reality does still remain a truism for beginning geologists today, who are taught in map-reading classes and on field trips that if their drawing is not correct, it is because they have not properly observed.

### 3.1.2 What happens to the geologist in the field?

Of course, another part of the contextual story is to be found in what actually happens to the geologist while in the field, and it is important to note here that fieldwork

conditions really have not changed all that much over the course of the past 150 years. These early conditions are tellingly described by Rudwick (1985), who analyzed a series of letters exchanged by a group of English 'gentlemen' geologists during the period of 1830-1845:

> "[S.] had to do his fieldwork on foot, but this was no unusual hardship for him. He walked through the Eifel on a long traverse by Prüm in the west, being "half eaten with fleas" at one overnight inn and arriving "in a miserable wet condition" at another. He reached Gerolstein "dead tired and quite worried" (p. 321).

In comparison, although field geologists today may very well drive all-terrain vehicles, or if financially well endowed, employ helicopters to reach out-of-the-way areas, their work remains essentially the same. Fieldwork remains quite *physical,* to say the least, and many geologists must cover large areas of a region on foot, at times accessible only by foot due to a lack of serviceable roads (such as in parts of the Himalayas, the Alps, or desert/volcanic regions in the Middle East), go without eating all day or eating little so as to leave room in their backpacks for precious samples (sometimes weighing 9 to 18 kg. each), which they will then lug around all day until returning to camp in the evening, and engage in extended field missions ranging from a week to a couple of months, during which time they may typically lose 7 or more kilos in weight.

Other difficulties encountered in the field call for efforts of a psychological sort, such as that described in one field researcher's memoir (Scholz 1997). The success of his field mission, undertaken in the Kalahari region in Botswana in the early 1970's, depended on his skill in deciphering elephant psychology and resolving a mutiny among members of his research party due to the repeated attacks of wild animals. Sometimes the field geologist must also demonstrate keen negotiating skills, such as in the tale told by a former doctoral student who, while doing his field research in Peru, was held hostage at gunpoint for several hours by local villagers who were convinced he and his work-mate

were members of the Shining Path (LeGros, pers. comm., 1998). These types of stories abound and geologists seem happy to get the chance to talk about them, as evidenced by the fact that geologists frequently speak freely of their 'field experiences' at conferences, both in paper sessions as well as in the halls (Rowley-Jolivet 2000; M. Rudwick, pers. comm., 1999), as well as in memoirs or special articles in standard journals.

### 3.1.3 Boué's recommendations for keeping a field notebook

What is also interesting to us, in our quest for establishing current field practices as a context for today's field writing, is what Boué (1836) suggests as the necessary content for the indispensable field notebook. We might assume that these are still the sorts of things that get marked down in the geologist's own notebook today, and which may eventually find their way into the research article.

The following excerpt is from another chapter from Boué's (1836) geologists' guide on keeping a travel log, or in more explicitly geological terms, a 'field notebook'. We can note here how central the field notebook is to the field geologist, as the centerpiece to his field findings (emphasis has been added).

Chapitre XII. 'Journal de voyage' (pp. 99-100)

> "*Le journal de voyage est la partie la plus importante* des tournées géologiques. *Pour qu'il soit un miroir fidèle de la nature, il faut écrire ses observations sur les lieux même et mettre chaque soir ses notes au net,* en y ajoutant *les détails commemorables* et en consultant soigneusement les cartes pour *les localités et leur dénomination*. *On ne saurait trop y mettre de détails,* car souvent une observation insignifiante au premier abord peut être utile plus tard, et une fois consignée elle est toujours à la portée du géologue...
>
> Dans les pays à couches redressées, *il faut souvent noter la direction et l'inclinaison des couches,* et dans les plaines il faut donner plus d'attention aux moindres *affleurements* que dans les pays de montagnes.
>
> Enfin il est extrêmement important de *désigner très exactement les lieux d'observation et de ne pas se contenter d'à peu près ou d'expressions*

> *equivoques,* telles que celles de près de, loin de, dans le voisinage de, etc. *Il faut préciser la distance en myriamètres, lieues ou heures, et le lieu* par l'indication du point de l'horizon, de côté de la montagne ou de la vallée, du bord de la rivière, etc. ... Pour ceux qui ont voyagé et comparé des descriptions géologiques avec la nature, ils connaissent tous les inconvénients de ces éllipses de langage... qui peuvent être très désagréables pour le géologue-voyageur, dont le temps est précieux et dont chaque course a un but.
>
> La géologie n'est pas une science spéculative mais pratique...."

While Boué does not give a great amount of detail about what actually went into a field notebook, we do get a sense of some of the things that might be there, measurements, distances and the like, as well as the care necessarily taken in producing such a valuable document. This is a record of field results which is invaluable not only to the field researcher, who must at some point leave the field behind in order to go back home, write up the report and get on with other things. The precision required for such an undertaking is indispensable for other geologists, as well, for they might wish to visit the site themselves or use the description of the site to advance their own understanding of geological structures or processes. It is during this visit to someone else's site, and efforts in getting the description and the site to match, that these other researchers might be led to vouch for or altogether dismiss one's claimed competence as a field geologist.

And so we can begin to talk about some of the possibilities for field writing, with the view adopted here being that writing is inherently centered within disciplinary practices and activities. The possibilities of what a geologist *might* write about are set down in the following list, which sets the stage for a later discussion of 'contextual probability' for it is obvious, of course, that geologists will not write about everything they do in the field given that their disciplinary "frame" (Minsky 1979) constrains most of the choices they may make in their writing.

The following list has been compiled using various sources: what we have seen from Boué (1836), Rudwick's (1976, 1985, 1996) excellent and insightful analyses of nineteenth-century geological practices, as well as interviews with various geologists.

**What the field geologist "does":**

1. Works toward obtaining the most detailed knowledge of his territory, its structure types, rocks, deformities and their causes.

2. Learns about the topography, if possible using existing geological maps or nowadays satellite imaging.

3. Identifies, observes and analyzes distinct exposures, outcrops, and formations.

4. Observes and analyzes the area's rock types, their composition, their bedding and relation to surrounding rocks, any alteration.

5. Identifies, observes and analyzes deformities[xiii]: stratification, jointing, faults, folds (anticlines and synclines), outliers and inliers, unconformities, etc.

6. Makes correlations with other regions and known scenarios for geological activity or processes (see Rudwick 1996).

7. Makes drawings and other schema: maps, traverse sections, columnar sections, relationship schema, block-diagrams, etc. (see Rudwick 1976).

8. Keeps a field notebook in which he specifically notes:

   - The date

   - Place/location of observation (+ GPS reading)

   - Measurements
     a. Directions of lineation, foliation, schistosity
     b. Degree of strata inclination
     c. Size of rocks, minerals and structures in situ
     d. Distances

9. Gives a description of the field's features.

10. Includes his drawings, sketches, notes for photos.

11. Jots down other observations or first interpretations.

12. Gives the relation of the site to surrounding localities: establishes a "road map" for getting back to the site in the most ideal conditions (e.g., best time of day for viewing structures), also allowing for "reproducibility" (Popper 1978) so that other geologists can get there and see for themselves.

13. Includes mnemonic devices (ex. "It rained all day; cold and icy" or "Today we actually had a bite of supper") to help jog visual memory of sites visited during the mission.

14. Carries around a geologist's tool kit, full of hammers, picks, and other tools.

15. Samples and takes away rocks.

16. Walks around all day to survey general landscape, to specifically follow outcrops and other structures as far as possible; must bring sturdy shoes or boots.

17. Must be willing to endure all weather extremes as the entire day is spent outdoors.

18. Patience and perseverance needed in searching out clues.

19. Puzzle-solving, interpretive skills and educated guesses necessary for filling in the gaps.

20. Knowledge of human and/or animal psychology possibly needed.

21. Knowledge or awareness of regional or political context likely necessary.

22. Negotiating and diplomatic skills a plus.

23. Countless others, depending on individual field stories…

As we can see from this list, there seem to be three main groupings of 'geologistic' activities. In items 1-8, we find those intellectual activities resulting from explicit field and geological training. In items 8-16, we see the geologist's professional activities, much as we might see him doing if we were to spy at him while perched in some nearby tree or peering out at him from behind the rocks. And then, items 17-23 seem thrown in with a different lot. These seem to be some of the difficulties the geologist might face in

the course of 'simply doing his work'. Not quite a day at the office as it would be. And of course, the further we go down the list, the more extraneous the various possibilities seem to become. However, these are exactly the sorts of 'doing-the-work details' that give us the full range of possibilities to explore in possible accounts of geological writing.

### 3.1.4 The field geologist as writer in times past

Let us now take a look at what geologists themselves have written about their field mission. To begin, and as one further basis of comparison, we will look at a couple of texts from an earlier time, when field writing more commonly contained an account of the sorts of things that actually happened during a field mission, but have since disappeared from field writing practices. In geology articles published at the beginning of the last century, for example, it was typical to find a description of the field framed within a 'Traveler's Tales' narrative about the field mission, as we can see in the following excerpt (emphasis added[xiv]).

> "La région *que j'ai parcourue* est entièrement formée par des tufs volcaniques avec dykes et coulées de labradorites.... *En plusieurs points j'ai observé* une stratification nette dans les tufs volcaniques et, en même temps, le mélange en proportions plus ou moins grandes d'éléments calcaires... *J'ai observé les mêmes intercalations de calcaire sur la route du Marin au Vauclin, près de l'habitation Puyferrat et, plus au nord, dans la presqu'île de la Caravelle, à l'ouest de l'habitation Spoultourne...*" (Giraud 1902)

In this first excerpt, we can observe the use of a chronological, narrative form — an action which has taken place in the past (fieldwork) is in fact represented as having occurred in the past. This we can see in 'La région que j'ai parcourue...', or 'En plusieurs points j'ai observé...' This chronological account, of course, contrasts quite distinctly with modern practices, where the scientific research account has now become generally "detemporalized" (see Myers 1990). We also have a very nice roadmap, as in 'sur la route du Marin au Vauclin, près de l'habitation Puyferrat et, plus au nord, dans la

presqu'île de la Caravelle, à l'ouest de l'habitation Spoultourne'. As a first estimation, this is a text that seems to conform to the standard of practice represented by Boué's (1836) field geology handbook, apart from the vagueness and lack of precision apparent in the expression "en proportion plus ou moins grandes".

Let us now turn to the second excerpt.

> "La Martinique est essentiellement de nature volcanique... Aucun sédiment n'a été jusqu'ici rencontré dans le massif de la Montagne Pelée, *aussi me paraît-il utile de signaler à l'Académie une découverte que j'ai faite, il y a deux ans, et dont je poursuis l'étude...*
>
> L'anéantissement de toute végétation au cours de l'éruption de 1902 a depuis lors rendu particulièrement destructives les actions torrentielles... Les falaises de certains ravins, affouillées par les torrents, s'éboulent; les matériaux fins sont entraînés à la mer, alors que les blocs volumineux restent sur place ou se concentrent dans des points favorables. *C'est grâce à cette particularité qu'il m'a été possible de trouver les calcaires,* qui font l'objet de cette Note.
>
> *En remontant, non sans peine et parfois non sans danger, le lit de la petite rivière Paillacard... j'ai rencontré tout d'abord des blocs calcaires, que j'ai retrouvés ensuite dans les profonds ravins du Morne Fortuné sur la rive gauche de la rivière de Céron...*" (Dublancq-Laborde 1912)

In this second text, we can once again note the narrative structure of the research account, as in '*aussi me paraît-il utile de signaler à l'Académie une découverte que j'ai faite, il y a deux ans, et dont je poursuis l'étude*' or '*j'ai rencontré tout d'abord des blocs calcaires, que j'ai retrouvés ensuite dans les profonds ravins du Morne Fortuné*'. But what is also remarkable in these older texts, in contrast to modern ones, is the way they are marked by a personal narrative — indeed, the author seems to be telling us the story of his own research mission. This becomes clearer when one considers the sheer number of linguistic strategies observable in these excerpts, available to the author for establishing his physical presence in the field, thereby establishing his authority over his territorial domain of work. This is clearly achieved through the use of the first person

pronoun (i.e., 'Je'), as well as in the choice of verbs that semantically describe a human activity, both physical and intellectual (e.g., parcourir, trouver, observer, rencontrer...).

We can also note with a bit of surprised amusement that the author actually tells us his work was *hard,* as we might very well guess that this is precisely the sort of information that has lost relevancy in today's cut-throat research world*:* '*En remontant, non sans peine et parfois non sans danger, le lit de la petite rivière Paillacard*'. Finally, we can also notice some highly textualized indicators of physical presence and authority establishment in the way the physical description of the locale is given. Here, descriptive geographical markings not only situate observations for other geologists, but dually act as a further manifestation of the field worker's physical field presence (e.g., 'que j'ai retrouvés ensuite dans les profonds ravins du Morne Fortuné sur la rive gauche de la rivière de Céron').

We continue to find sporadic traces of these Traveler's Tales quite late into the twentieth century, in fact, as we can see in the following excerpt that is an interesting and even somewhat peculiar example (emphasis added).

> "*Lors d'une mission d'établissement* de la feuille de Timimoun... pour le compte du Service géologique de la République algérienne, *l'un de nous remarqua le 25-10-1967* d'assez nombreux fragments d'une roche... *La présence de cette roche... fit immédiatement penser à une météorite, hypothèse qui fut confirmée par l'examen de deux échantillions rapportés a Paris. Au cours d'une mission suivante, on retrouva le 12 mars, 1968, après deux jours de recherche, l'emplacement de la météorite... Trois journées furent nécessaires pour rechercher les fragments répartis en 71 points de chute...*" (Michel-Lévy, M., Lévy, C., Lefranc, J.-P., and Wiik, H. 1970)

This passage, the first paragraph of the introduction to the article, is immediately striking by its resemblance to late nineteenth-century French prose — although published in 1970. First, one might notice the abundant use of the passé simple to relate the events, as seen by 'remarqua', 'fut confirmee', 'retrouva', or 'furent'. The use of the passé simple in contemporary French scientific texts has become very rare, and its use would appear to

be a marker of an older generation of scientists and academics — perhaps as an indicator of class, or even of the 'old school' of field geologists who were booted out from French intellectual life after the 1960's revolution (L. Latouche, pers. comm., 1997). We also have a chronological description of the research event, as in 'L'un de nous remarqua le 25-10-1967' or 'fit immédiatement penser à une météorite', then 'Le 12 mars, 1968, on retrouva, *après deux jours de recherche,* l'emplacement de la météorite, and 'trois journées furent nécessaires pour recherche les fragments'.

Further striking is the absence of any of the introductory rhetorical moves identified by Swales (1990), which by this time we might imagine would already have been used to situate the paper within the concerns of the research community. Quite to the contrary, the entire introduction appears once again to be a 'narrative of discovery', thereby further resembling nineteenth-century French scientific prose by relating the unexpected discovery of an entirely new rock, something which at this point in time was quite rare given both the extensive classificatory work already accomplished and the paradigm shift that had seen non-field geological methodologies push their way into the forefront. In other words, the rhetorical effect of the account implies that the importance of the discovery seems to simply speak for itself.

Interestingly enough, a geology informant has reported having come across a similar narrative account in a much more recent article (published in 1996) that reports on the fieldwork undertaken in Vietnam, which at the time had only recently been reopened to westerners (T. Hammouda, 1997). Bazerman (1988, p. 90) has also documented a similar use of narrative in early articles of the Philosophical Transactions of the Royal Society of London, as well, where in issues falling between 1760-1780 he noted the use of a 'discovery account' to explain and explore the meaning of unusual events. One might therefore wonder if recourse to such a narrative of discovery is not simply a natural human reaction to a situation in which extremely special or novel circumstances must be described, causing the writer to forego a more standard rhetorical representation of the

event, which may be insufficient for the occasion. This is a subject that is currently being investigated in early reports on the AIDS virus (F. Hilal, pers. comm., 2001).

Finally, one can also situate this article historically and politically, following Latour's (1984) argument that science cannot reasonably be separated out from the context of politics and the contemporary state of society. At the time of its publication, it is eight years after Algerian independence and France is still quite politically and economically implicated in the internal affairs of its former colony which it had ferociously fought to keep, especially in the context of maintaining oil and mining rights. This excerpt can therefore also be situated in what has been termed a 'colonialist rhetoric', which assumes that the world is a place yet to be discovered by the European, western explorer (D. Caron, pers. comm., 1997).

### 3.1.5 Evacuated authorship in times present

As might be expected, of course, ways of talking about the field have changed quite dramatically over the years, and so the contemporary fieldwork account is very different from its counterpart of the preceding century, as diachronic research has also demonstrated. Given the results of an unpublished study (i.e., a diachronic study of research articles in geology, 1870-1995; Dressen 1997a) from which the above excerpts are taken, it is apparent that conventions for reporting on fieldwork have changed over the course of the past century, leading to a greater 'textual discretion' on the part of the researcher (Dressen and Swales 2000).

This increasing authorial invisibility is a feature also noted by researchers in other domains of scientific discourse such as Physics (Bazerman 1988) and Medical discourse (Salager-Meyer 1998). Indeed, Salager-Meyer (1998, p. 47) has described medical discourse's increase in "authorial invisibility" over the years. It is in the diminishment of overt indications of authorial activity in the research process that we can begin to identify

what today has become 'contextually appropriate' in the particular socio-cultural situation of late twentieth-century geology.

As we saw in the preceding chapter, the centrality that fieldwork had long enjoyed underwent a shift toward the periphery in the middle of the twentieth century, moving to a far less prestigious position as geologists began to turn away from purely physical, empirical field evidence to alternative methods for investigating the Earth's structure. And so, we would expect for geologists today to be somewhat more restricted in their recounting of the field mission. This, is fact, is what has happened, as we can see in the following excerpts taken from the early part of the Geological Setting section.

> "The Karakoram terrane, along the northwest frontiers of Pakistan and India, forms the southern continental margin of the Asian plate (Desio 1964). It lies immediately north of the Tethyan suture zones which mark the zone of collision between India and Asia (Fig. 1). The Shyok suture zone (SSZ) separates the Kohistan arc-microplate from the Karakoram terrane in the north and the Main Mantle Thrust (MMT) places the Kohistan arc-microplate southwards over upper and mid-crustal rocks of the Indian plate. Sedimentology along the Indus suture zone (ISZ) and north Indian plate margin in Ladakh and south Tibet suggests that closure of Tethys along the ISZ, and collision of India and Asia occurred between the early and mid-Eocene at ca. 50 Ma..." (Searle et al. 1992)

> "The orebody is divided by an irregular transition zone into a western massive sulfide body and an eastern body of bedded sulfides and intercalated clastic metasedimentary rocks (Fig. 1)." (Jiang et al. 1999b)

Indeed, the modern fieldwork account, as we see it here, would seem to be best characterized by an evacuation of human agency, where a sort of "rock-oriented discourse" (Dressen and Swales 2000) has de facto taken over.

Upon the first reading of these modern field accounts, we might be struck, for example, by the complete absence of human researchers, and be tempted to believe that rocks and formations truly do act independently of the human hand despite the obvious necessity of such inherently *human* activities as observation during fieldwork, geological mapping, or the analysis and interpretation of structures. This first impression has been

corroborated by the analysis of a small, preliminary corpus of twenty petrology articles[xv] where 82% of all grammatical subjects refer to *rock* formations and geological *structures* (i.e., 605 from 734), and a mere 3% (i.e., 19) to human agents. The main grammatical subjects of the account are seen to be the geological structures encountered by the researcher in the field, and these structures are represented as acting on their own. Although most tend to associate "author-evacuated" (Geertz 1983) writing styles with the passive voice, even the accompanying verbs here are seen to be overwhelmingly *active*. Instead, we might qualify these excerpts, then, as instances of what seems to entirely "human-evacuated" writing.

And so, what we have in modern geological field writing is a "rock-centered discourse", where the grammatical actor-role has effectively been shifted from the researcher to the geological structures being studied, resulting in the apparent disappearance of the field geologist from his account. The trend toward tight authorial discretion is further confirmed by an analysis of the verbal repertoire from the preliminary corpus, one which paints a picture wherein the field experience is related as non-narrative, synchronic and static, as analytical rather than narrational, and as more existential than experiential.

A complex process does indeed seem to be at work here, and we might suspect that contemporary field reporting discourses are the product of a fine-tuned rhetorical maneuvering game, where today's authors in geology are constrained by the need for a succinct reporting style imposed by shifting — or "shifted" — discourse community norms and by modern journal conventions. Indeed, an additional argument lending credence to the assumed existence of linguistic traces of textual silence can be found in the demands of the market place. Today's scientific writer must always have at the back of his mind the tight space constraints imposed by publication costs, where surpassing the page limit can entail a penalty of $70 per additional page (R. Van der Voo, pers. comm.,

2001). It appears clear , in Becker's (1995) words, that today's field geology community has chosen, by sheer necessity, to silence certain details in order to be able to say others.

However, while Searle or Jiang and their cohorts cannot *openly* demonstrate their fieldwork endeavors, discreet rhetorical and grammatical strategies are nonetheless available to them to indicate their physical presence in the field, thereby allowing them to construct empirical support for their claims, establish their credentials as authorities in a region, build their credibility and gain competence recognition from their peers. What exactly these discreet strategies are will constitute the focus of the remainder of this chapter.

The texts serving as the basis for this brief illustration of *modern* textual silence have been in English, giving an English-language context for the study and some preliminary results, as well as a glimpse into enduring textual practices and contexts for writing in French. The choice for focusing on only English as a research language is motivated by the results of an earlier study, which have shown that the way in which fieldwork in geology is reported on in French and English is roughly equivalent (Dressen and Swales 2000). As a consequence, the current study will not be a full-blown contrastive analysis study.

Further reasons for not pursuing a contrastive study of this nature are explicitly outlined by Crosnier (1997), who has admitted that her own contrastive analysis ambitions have had to be curtailed since such studies lose their meaning in an era of quickly falling numbers of French language scientific journals. In other words, French researchers today are very simply, by necessity, needing to publish directly in English in order to gain access to and acknowledgment from the international research and publishing community. Using English as a research language is an issue we will return to in Chapter 6, in terms of the implications of this study for teaching geological writing in

English to speakers of French. The geologist-authors I have interviewed, the details of which are presented in Chapter 5, are all French but publish primarily in English.

## 3.2 Corpus description

To examine how authors do — or do not — talk about the fieldwork expedition, and to describe the linguistic, rhetorical and discoursal features of textual salience — and of silence — in field reporting, this study uses a corpus of 103 recent research articles (1996-1999)[xvi] from three subfields in geology (geochemistry, petrology, and structural geology). A total of nine journals, three for each field, were chosen to constitute the corpus, based on their centrality to the three research communities, as indicated by geologist-informants.

### 3.2.1 Description of the subdisciplines

While remaining closely linked to the study of rocks and their origins, clear disciplinary differences exist between the three areas of geology. Structural geologists necessarily spend the most time in the field since the aim of their study is to establish the history of a region given the surface evidence and structural relations of the various folds, uplifts, dykes, etc. The focus is thus on the field itself, rather than just a sampling of rocks they pick up and carry away back to the lab with them. Petrologists, who principally pick up particular rock samples, are concerned with establishing the history of particular rocks and focus their analysis on the rocks themselves, with little concern about their environment other than immediate field relations. And finally, geochemists attempt to 'chemically' re-create the history of the earth by studying the isotopic interactions within a bulk rock mass of minerals types. They therefore will sample large quantities of rock, often weighing 20-40 lbs each, in order to analyze the interactions between the rock's different minerals or crystals.

The 'pure' structural geologist, petrologist, or geochemist is, however, something of an anomaly and there is quite a bit of interplay between the different subdisciplines. Petrologists, for example, may engage in structural or chemical studies to better explain observed phenomena; structural geologists may also be petrologists; geochemists may also be structural geologists, making geology fundamentally appear to be an "interdiscipline" (C. Berkenkotter, pers. comm., 2001).

This intermingling of interdisciplinary specialties is reflected in the journals chosen for the corpus. As such, one may find geochemists or petrologists publishing in structural geology and tectonics journals, geochemists publishing in petrology journals and vice versa.

The following journals have been used:

| Geochemistry | Abbreviation used |
|---|---|
| • Contributions to Mineralogy and Petrology (1998) | (CMP) |
| • Chemical Geology (1998-1999) | (CG) |
| • Geochemica et Cosmochimica Acta (1997-1998) | (GCA) |
| **Petrology** | |
| • Journal of Petrology (1996-1999) | (JP) |
| • Mineralogical Magazine (1998) | (MM) |
| • Lithos (1998-1999) | (LI) |
| **Structural Geology** | |
| • Journal of Structural Geology (1997-1998) | (JSG) |
| • Tectonics (1997-1998) | (TECT) |
| • Geodinamica Acta (1997-1999) | (GA) |

Throughout the remainder of the chapter, articles from the corpus will be referred to by an abbreviation of the journal and of the author's last name. Thus, an excerpt cited from "CMP-Kr" would refer to:

> **Kröner**, A., Willner, A. 1998. Time of formation and peak of Variscan HP-HT metamorphism of quartz-feldspar rocks in the central Erzgebirge, Saxony, Germany. ***Contributions to Mineralogy and Petrology,*** 132: 1-20.

A full list of the articles constituting the corpus is provided in Appendix B, part III.

For obvious reasons, I have deliberately chosen articles that report on fieldwork. As a consequence, all articles in the corpus do contain evidence of the actual field presence of at least one of its authors, although determining their field presence has not always been such a clear-cut matter. Field presence can be guessed at with a reasonable amount of accuracy, however, by using a combination of textual clues. These clues are general in nature and include whether: (1) the region studied is indicated in the title; (2) authors make reference to their fieldwork mission in the acknowledgments; (3) they have cited their own prior fieldwork in the region; (4) they credit themselves for the published geological maps and other visual support; (5) the article contains a number of explicit linguistic elements within the field description that point to the author's presence in the field.

## 3.3 Today's field account

How we might see a geologist reporting on his field results today is shown in the following passage, taken from the first three sentences of a field results section from a 1997 article in the journal *Tectonics.*

> "The Koolen Lake-Lavrentiya Bay region (Figures 2, 3, and 4) exposes a sequence of sillimanite-grade to second-sillimanite-grade granitic gneisses, paragneisses, schists, amphibolites, and marbles that are intruded by a heterogeneous suite of plutons, stocks, dikes, and sills. The metamorphic rocks constitute the core of a large (100 km across) structural culmination flanked on all sides by lower-grade Paleozoic and Mesozoic metasedimentary rocks and Cretaceous volcanic rocks; all are intruded by Cretaceous granitic rocks (Figure 2)." [TECT-Be]

This three-sentence passage is representative of what one typically finds in the fieldwork account. Such written accounts, as we will see more clearly in later chapters, are the end result of a long process of exploration and observation, annotation, analysis, synthesis, interpretation, research activity "distillation" and discoursal "recontextualization" (Linell 1998). However, how one should classify this apparent genre's features and its discoursal

structure is not immediately clear. Notably, one might question whether or not a specific part-genre exists for organizing an account of such field details.

We can note, for example, that each of the three sentences has a relatively simple subject and verb, but lexically and syntactically complex predicates, consistent with an earlier observation about the lack of technical explicitness in field reporting verbs:

1. Subject and verb: "The Koolen Lake-Lavrentiya Bay region… exposes…"
   Predicate: "…a sequence of sillimanite-grade to second-sillimanite-grade granitic gneisses, paragneisses, schists, amphibolites, and marbles that are intruded by a heterogeneous suite of plutons, stocks, dikes, and sills."

2. Subject and verb: "The metamorphic rocks constitute…"
   Predicate: "… the core of a large (100 km across) structural culmination flanked on all sides by lower-grade Paleozoic and Mesozoic metasedimentary rocks and Cretaceous volcanic rocks."

3. Subject and verb: "all are intruded…"
   Predicate: "… by Cretaceous granitic rocks (Figure 2)."

We can also see that all verbs (both active and passive) are in the present tense; further, any qualifications or hesitations are absent. And so we find no instances of "may" or "might", or of "seems" or "tends", nor any qualifications such as "for the most part" or "in general".

The features identified so far might suggest that the passage comes from a textbook, since these very features have been closely associated with the genre in the literature. Surely the authors of the passage might have, like a textbook author, tried to "arrange currently accepted knowledge into a coherent whole" (Myers 1992, p. 8), therefore presenting facts as accepted knowledge, separated from any researcher activity (p. 13). Further support for a textbook status is the obvious absence of reference to any previous work, just as textbooks intend to "represent a broad area of available

knowledge, to offer a "vision", and to incorporate new findings emerging as a result of the exigencies of textbook writing" (Swales 1995, p. 15).

However, the complex and unglossed technical terminology ("second-sillimanite-grade granitic gneisses") would imply that we are at the very least dealing with an advanced textbook here, rather than one designed for an introductory geology course. Moreover, there is no indication of why this region is even being described, such as we might expect in a textbook. There is no contextualizing onset such as:

> "One area where these processes can be clearly seen at work is the Koolen Lake-Lavrentiya Bay region."

Similarly, there is no background given to explain the study area's name: "Koolen Lake-Lavrentiya Bay region". This might be an already established geographical name, well known to geologists such as the "Harney Peak granites" or the "Karakorum fault", but it might also be the author's attempt to create a new combination of common words like 'bay' or 'lake' with his study area, thereby setting claim to his explored territory. Moreover, we have no immediately apparent way of knowing whether the description of the region is based on the author's own fieldwork or is found in the literature.

In effect, the characteristics of this short text do not easily or intrinsically comply with the genre features of textbooks, which are written with a "complex audience configuration" in mind (Swales 1995, p. 15). Here, it is as yet unclear whether this text was written for a "wide audience" or for an exceedingly reduced number of readers. As we might suspect, there is most likely more going on here than what immediately meets the eye.

While textual accounts of field research and indications of researcher presence and expertise clearly do not occur today as agent-marked 'Traveler's Tales', we must wonder whether there is a way to determine that the account is the author's own, and whether there are linguistic and discoursal features that would in fact allow us to definitively conclude that the authors have truly been in the field. Certainly, the account

given above is quite general. The authors of the text are writing about an entire region, and give very global details: its whole structures ('plutons, stocks, dikes, and sills'), the size of the main structure studied ('100 km across'), as well as the estimated age of the rocks. As a consequence, the means for determining whether this account is the end result of the authors' own field mission and not merely a review of the literature may appear elusive. And so, the main question to be addressed in this chapter is exactly how the researcher establishes his own field presence in the text, given the conventionally discreet manner with which researchers must present field results and the communally imposed silences surrounding field experiences.

Given what geologists themselves have said about the space constraints placed on the reporting of field results (e.g., Irvine and Rumble 1991; R. Van der Voo, pers. comm., 2001), we might well imagine that expertise in the locale and the authoritative voice of the expert is located in a "quick" description of the data. It is in fact in this 'quick description' that the types of linguistic markers which proclaim to the reader "We were there" are to be found.

Because verbs in geological field reports tend to avoid technical details (Dressen and Swales 2000), as seen in the following example,

> "The zones of enhanced deformation **surround** rocks with a weaker development of foliation (Fig. 1b and c), variably-oriented and generally not parallel to moderately-dipping compositional layering, but which **have** a well-developed moderately-to-steeply NE-plunging mineral elongation lineation." [LI-Pr]

geological writers will instead place markers of professional expertise and field presence into instrumental noun phrases, process nouns, non-finite verbal (participial) phrases, adverbial movement or other types of verbal modifiers, rather than in the type of agentive narrative common to earlier geology texts (see earlier examples from section 3.1.4). In the following examples, we can see what might tentatively count as 'field presence' markers.

> "The **intrusions** have domal structures which grew **laterally by continual emplacement** of numerous sills and dikes, **indicating extraction on** small batches of melts from the sources." [JP-Na]

> "The peridotite **over**lies high-grade gneisses and marbles… **along** an **essentially low-angle** brittle **thrust marked by** extensive **brecciation discernible over** a distance of up to 100 m **away from** the context." [JP-Vd]

These markers constitute a complex and skillful description made by the geologist in the field. Because convention no longer allows geologists to come out and say "After climbing up a steep incline, *we* got to the site, *we* picked up some rocks, and *we* saw that they were situated in a certain manner, which suggests to us that....", these types of constructions serve as a notice to readers that there has been an actual 'taking in' of the terrain with a specialist's eye, rather than an account which has merely been gleamed from the literature. With this in mind, the corpus has been closely analyzed in order to identify a possible set of textual indicators that may systematically reveal field presence and researcher expertise.

### 3.4 The geology research article

Of course, what we now know about the structure of the scientific research article in general goes well beyond the somewhat oversimplified, but useful, schema of the 'IMRD' (Introduction–Methodology–Results-Discussion) model. There are important contributions reflected in the now-extensive literature on the schematic structuring of the scientific research article into "part-genres" (Ayers 1994), each characterized by a specific discoursal purpose and the particular rhetorical and linguistic features embedded within its frame of use. This we have seen for Introductions, Methods, Results, Discussions sections, and the overall article (e.g., Swales 1981 and 1990, Cooper 1985, Hopkins and Dudley-Evans 1988, Crookes 1986, Thompson 1993, Belanger 1982, Peng 1987, Hill et al. 1982, Nwogu 1997, Williams 1999).

Looking at each journal's typical article structure, I have distilled the following global structure across the three disciplines, which as we can see generally adheres to the 'IMRD' structure format (see Table 3.1). We can quickly note, however, that there are also some peculiar, discipline-based differences. The general names given to the following subsections have been determined using both the frequency with which subsections were so or similarly denominated throughout the corpus of 103 articles, as well as their evaluation as being "functionally viable" categories by a group of geologist-informants.

Table 3.1 Geology research article subsections

| **Geochemistry** | **Petrology** | **Structural Geology** |
|---|---|---|
| • Introduction | • Introduction | • Introduction |
| • **Geological Setting Sampling/Methods** | • **Geological Setting** | • **Geological Setting** |
| • **Petrography** | • **Field relations Samples/Methods** | • **Autonomous fieldwork report Samples/Models** |
| • Methods | • **Petrography** | • Chemical, Seismic, or Experimental Results |
| • Results | • **Mineralogy** | • Discussion/ Conclusion |
| • Discussion/ Conclusion | • Results | |
| | • Discussion/ Conclusion | |

As we can note in Table 3.1, in addition to its adoption of the standard schematic structure for modern research reporting, the field of geology also possesses a couple of discipline-specific subsections that respond to the research community's particular needs.

The first of these is what has been identified as the "Geological Setting" (GS) section, which is a disciplinary scene-setting, introductory part-genre that directly follows the Introduction. Occurring in a majority of research articles in geology, it establishes what is 'geologically' known about the region under study (Dressen and Swales 2000) and therefore consists essentially of a literature review of past publications. Moreover, it

responds specifically to geology's epistemological underpinnings as a field-based discipline, in that it sets the context for a later discussion of how the paper's results relate back to the current state of disciplinary knowledge about terrestrial structures and mechanisms.

Given that geology is a "natural science" whose research objectives are centered around the study of natural phenomena observable in the field, even when these phenomena are reproduced in the laboratory, it has therefore been argued that the Geological Setting reflects an alternate structure for framing research as one which continuously needs to relate the micro– and macro– issues proper to geological research (Dressen and Swales 2000). As a consequence, it is discoursally characterized by a series of background-setting, general-to-specific descriptive statements and sub-moves that describe the topographical, historical, and physical features of the sampled area or investigated region. However, if little or nothing has been published on the region, the GS may also consist of a number of specific field details pertaining to the authors' field mission, who use their own field data to establish the region's geological context. Hence, this is one first place where geologists may actually report on their field results.

Other differences in article structure pertain to the particular research imperatives of each, individual subdiscipline. As we can recall, geochemists study the isotopic interactions between different minerals within rock types. Therefore, the field mission itself centers principally around sample collecting and it is generally not important to relate information about the mission other than where the sample was picked up for the analyst's main concern is to examine the "bulk chemistry" of sizeable chunk of rock. And so, we can see that in geochemistry, authors will often attach a very short description of their sampling site at the end of their Geological Setting section, and spend little time actually talking about the field. A typical passage relating aspects of the field mission is generally only about 500 words in length.

Petrologists, on the other hand, generally place specific information about the field into a separate subsection that they will typically call “Field relations”. This subsection reflects their collective need to establish the relations observed in the field between the different rocks of an area, noting specifically which rocks or minerals occur together and eventually what the immediately visible features of their interactions are. These field-reporting sections are also generally quite short (nearly 600 words), and most authors put details about their sampling sites here, as well.

Quite often, both geochemists and petrologists will then also give an account of the petrographical and mineralogical features of their samples, which consists of providing a physical description of the rock based on features visible by the researcher in the field. An example is given in the following excerpt taken from a petrography subsection:

> “There are rapid lateral changes on a decimetre or metre scale between granular and stellate fabrics, with grains varying from medium grained to pegmatitic…. In the southern part of the complex, in particular, the annular remnant of sövite is rich in xenoliths of ijolite, ranging up to 30 m in diameter. Within this zone, and at other localities around the complex (see Fig. 1), pale biotite-sövite encloses or intrudes an earlier grey variety of sövitic carbonatite, which has either an even, equigranular texture or a diffuse spotted appearance because of dispersed euhedra of nephaline. In turn, nephaline soviets contain occasional and greatly subordinate centimeter to decimetre thick bands and lenticular patches rich in nephaline and pyroxene…” [JP-Co]

And, they will report on the mineralogical features perceivable only under microscopic examination, as well:

> “The spinel tectonite microstructure is dominated by large elongate olivines (1–2 mm) surrounded by small olivine neoblasts (200–400 μm) with straight to curved grain boundaries. Deformation-induced undulatory extinction and deformation bands (sub)-parallel to olivine (100) are common. Elongate orthopyroxene (enstatite) clasts with clinopyroxene exsolution lamellae (Fig. 4b) are surrounded by polygonal orthopyroxene and clinopyroxene (diopside) neoblasts, suggesting deformation-induced dynamic recrystallization of the pyroxenes.” [JP-Vd]

The details contained in all of these early subsections (e.g., the Geological Setting, Sampling, Field relations, Petrography) are considered to be a part of the contextualization for the study's analytical results; these results are presented separately in a subsection following this preliminary detail. It is the description of these details, both field and mineral descriptive, which are the most "localized", often pertaining solely to the study at hand. As a consequence, some have noted that they are also the most tedious and difficult to write, where authors have a difficult time deciding between "too much" and "not enough" detail (Irvine and Rumble 1991), and many informants identify them as being the most boring to read. Fittingly, these are also the sections that may also be published in smaller print.

One final detail yet to be discussed are the subsections typically seen to occur in structural geology. As we can recall, structural geologists' primary focus of study is the field itself with all of its various structural relations, rather than its rocks or minerals. As a consequence, we can find a significant subsection, or series of subsections, in structural geology research articles of a considerable length (nearly 1900 words on average) where the author gives an explicit — and extensive — account of his fieldwork results. Moreover, it occurs alone as an autonomous subsection, i.e., independent from the Geological Setting or Methodology sections. The name I have given it here, autonomous fieldwork report, reflects not so much the actual name of the subsection itself as indicated in the corpus, as it does the purpose assigned to it by my geologist informants. While the actual subtitle may be the name of a region, a geological structure or process, my informants have consistently referred to it as a 'field report' (or, a 'rapport de terrain') in indicating to me that this is where they give an account of their field data. Of course, geochemists and petrologists also talk about 'field reports', and the discoursal reasons for this will become clearer shortly. For mostly superficial and classificatory reasons, then, I have designated the subsection relating field data in structural geology 'autonomous fieldwork report' whereas in petrology, I have named the field account 'field relations'

and in geochemistry ‘sampling’, thereby seeking to reflect the distinct purpose each community overwhelmingly gives to the report of its field data.

And of course, just as one might expect, given the methodological and theoretical shift the field of geology underwent during the 1960’s (see sections 2.6-2.7), all of the articles from the corpus also supplement their field results with some type of laboratory results: Chemical analysis, calculations, modeling, seismic profiling, and various other “experimental” results. As remarked by one of my structural geology informants, a field geologist just simply cannot “do geology” anymore without also having the analytical or mathematical data to back it up (O. Merle, Interview, May 1999), thereby showing an obligation to adhere to the general trends of the modern scientific community.

It is precisely in geology’s specifically “geological part-genres”, such as they have been described above, that details of the field mission are to be found, as we shall see in a moment.

### 3.4.1 Frequency and location of field reporting in the research article

One next question to be logically asked is how often geologists who write articles based on field data give actual textual descriptions of their data in the article. While we expect these descriptions to be “quick”, must they be so quick that the author could decide *not* to dedicate any descriptive textual space to these results whatsoever? There are a small handful of such instances in the corpus (8 from 103 articles) where no explicit textual indications could be found within the *written* text (i.e., field account) to ascertain the authors’ field presence. In these cases, the actual field presence of the author was determined using ‘extra-textual’ information: whether I knew that the author had an established reputation for doing fieldwork in the region, as related by geology informants, or as indicated by the author’s own previous publications cited in the article. I also used a

number of other 'in-article' indications, such as the author's use of acknowledgements where clear evidence for field presence was given.

> "The support of NSF grants EAR 94-18105 and EAR 93-15844 were vital to this work. Both authors relished the help and support of several undergraduate assistants during several field seasons in Mexico." [CMP-Bl]

> "Many thanks also to O. Krüger for help and discussions especially during the field work." [CMP-Ge]

> "We thank Prof. K. Lydka for kindly giving us a specimen sample of barite hardly available in the field." [CG-Le]

Such conclusive evidence may also be found in the captions accompanying visual representations, where according to expected practice and established traditions, the author must acknowledge other researchers if the map is taken from their publication. An absence of citation therefore indicates that the map is the author's own, even more so when it is coupled with sampling site information[xvii].

> "Fig.1: Geological sketch map of Connemara, Western Ireland showing the Galway Granite and its satellite plutons of Roundstone, Inish, Omey and Letterfrack (L). B, Illaunacroagh; D, Deer Island or Croaghnakeela Island; E, Errisbeg Townland; I, Inishlackan; M, Murvey; MS, Mason Island; S, St Macdara's Island. Circled spot is location of specimen BL4878." [MM-Le]

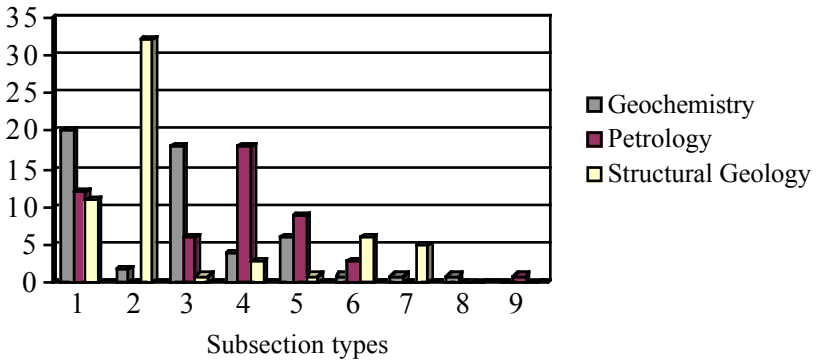
Generally speaking, however, most articles (92%) do contain at least a brief textual description of the field, as seen in the following table (Table 3.2). Moreover, as we can see in Figure 3.1 below, geologists have potentially nine different locations where they can report on their field data in the research article. The so-named Geological Setting (GS) section such as it has been briefly described above, is the most common location of field reporting across the disciplines. with slightly more than a quarter of all field accounts occurring in the GS. Other subsections, such as the Autonomous field account, Sampling, Field relations or Petrography, instead reflect disciplinary tendencies.

Table 3.2 Number of articles containing an explicitly identifiable *textual* description of the authors' own fieldwork

| Geochemistry | Petrology | Structural Geology | Total number of articles: |
|---|---|---|---|
| CMP : 13/13 | JP : 13/13 | JSG : 13/13 | |
| CG : 13/13 | LI : 4/5 | TECT : 12/13 | |
| GCA : 12/13 | MM : 10/13 | GA : 5/7 | |
| 38 | 27 | 30 | 95 from 103 |

Thus while slightly more than one-fifth of field reports occur in an autonomous fieldwork report section, the overwhelming majority of these are found in structural geology. 'Sampling' and 'Field relations' subsections can be cited as the next most frequent location for reporting on field data, each constituting roughly one-sixth of all field-result reporting subsections.

Figure 3.1 Textual location of field data across disciplines (from 103 articles): (1) Geological Setting section: 43 total occurrences; (2) Autonomous field report: 34 occurrences; (3) Sampling: 25; (4) Field relations: 25; (5) Petrography: 16; (6) Introducction: 10; (7) Methods: 6; (8) Results: 1; (9) Mineralogy: 1.




And finally, the 'Petrography' subsection makes up for one-tenth, with the remaining — the Introduction, Methods, Results and Mineralogy sections — each

accounting for 6% or less. This low frequency for the final four categories suggests that these final sections are not conceived of as the typical place for reporting on fieldwork.

While the frequency of field reporting is relatively equivalent across disciplines (see Table 3.2 above), there are nonetheless a number of discipline-specific differences, as further illustrated in Figure 3.1, which we have seen above. Thus, we can note that in structural geology authors tend overwhelmingly to put their field results into autonomous fieldwork report sections. They will, however, also put field data in the Geological Setting section. Geochemists also tend to use either the Geological Setting, but also Sampling sections, for field reporting. Petrologists, on the other hand, tend first to put their field details in a subsection specifically describing field relations and only then in the Geological Setting or Petrography sections. Thus, while we can observe that the three disciplines clearly have different locational preferences for the report of their field mission, we can observe that they do also put a good deal of their own field description into the background, scene-setting Geological Setting section, thereby corroborating earlier claims about the function of this part-genre (Dressen and Swales 2000).

### 3.4.2 Textual descriptions of the field

Within these various subsections, two distinctive discoursal types for reporting on field data can be found. One is a “Sampling Discourse”, which is a very brief account limited mostly to indicating that samples were in fact collected. Unsurprisingly, we most often find this type of field discourse in ‘Sampling’ subsections. The other type of field reporting, the “Field Account”, provides a more or less elaborate description of the field based on actual field observations. It is further characterized by a number of linguistic elements that intricately indicate field presence. While it may appear logical that Field Account discourse occurs within the autonomous fieldwork report section, in fact it is very frequently found to be embedded within other subsections, suggesting that all

subdisciplines to some extent use this type of discourse for reporting on their field research. Examples of each discoursal type are given below so that, if we were to read a geology research article using field data, we might very well find something like this:

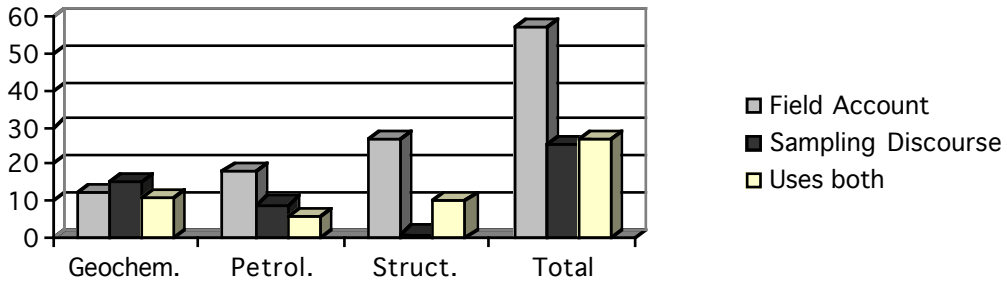
> Sampling Discourse
>
> *"Sample E41* — This is a HT mylonite of granitic composition collected at an outcrop along a gravel road leading from Niederlauterstein to the Burgberg hill (Fig. 1)." [CMP-Kr]
>
> Field Account
>
> ".... The Kanawa vein is confined to an N–S-trending brittle-ductile shear zone about 20 m wide and which extends well over 1 km along the strike (Fig. 3). At the centre of the vein are narrow anastomosing mylonitic bands with a subhorizontal mylonitic foliation and a horizontal stretching lineation, consisting of aligned quartz ribbons and a few sigmoidal feldspar porphyroclasts. Where the ductile deformation was very intense, ultramylonites with a high haematitic silica content developed. This central ductile zone grades outwards into a brittle zone comprising cataclasites which give way to fault breccias towards the margins of the vein. ..." [JSG-Su]

In the first example of data reporting, the author quickly signals some specific information about the sample, e.g., what it is, that he collected it, and more or less explicitly where it is from. It is a terse — but as we will see, conventionalized — formulation of information valuable essentially to other geochemists but also petrologists.

The second example of data reporting appears in striking contrast, for in comparison it literally contains a wealth of information. We have various measurements ('N–S-trending' or '20 m wide'), information about structure type ('brittle-ductile shear zone'), size, and densely packed noun phrases. Sentence 2, for example, consists of 32 words; of these, there are no fewer than five complex noun phrases with verbal modifiers to describe one word, "vein", all held together by a very plain little relative verb, "are". Here, we seem to have scientific writing at is very best, such at it has been described by Dubois (1982) or Vande Kopple (1992), among others.

Figure 3.2 (see below) gives us some idea of the type of field discourse each discipline favors. We can note that the Field Account is the preferred reporting strategy for petrologists and structural geologists, while geochemists make frequent use of both. Field details for the structural geologist are related almost always in terms of a Field Account. Sampling Discourse, on the other hand, appears more frequently in geochemistry than it does in the other two disciplines, and this is tied to disciplinary issues. Since geochemical methods require an analysis of "bulk rock", which includes not only one specific mineral or rock but 'all' the elements contained in the entire sample, some information about where the sample was collected becomes extremely important, although we often get little more than an indication of location; sometimes, we do get an indication of the size of the sample (e.g., "8 kg"), and hence quantity of bulk rock.

igure 3.2 Discoursal for petrologists preferences across disciplines




In comparison to geochemistry, the disciplinary interest in petrology lies not so much in the bulk rock sample as in the particular rock or mineral and its surrounding geological relations. And so we can see in Figure 3.2 that petrologists tend to relate their fieldwork details using Field Account discourse — either alone or in combination with Sampling Discourse, so as to account for this necessary field description. However, because they are especially interested in establishing the "petrographical" relations, their specifically field-based account tends to be quite short as they quickly move on to

petrographical description. As a consequence, one impression the analyst may have — and this one most certainly did — in reading through the corpus of texts is that it is the field petrologist who gives the most muted account of his field mission; one might even say that he appears to be the most textually discreet. He has really neither the more explicit 'Sampling Discourse' of the field geochemist nor the elaborately more detailed field descriptions of the structural geologist, for his discipline-imposed textual space allotment is quite short (averaging about 570 words). And yet the actual amount of time spent in the field by a petrology is as much if not greater than for geochemists, and probably can be nearly as much as some for structural geologists.

Despite these discernible differences, however, which are tied to issues of disciplinary practice and research orientation, there are also immediately apparent shared features of field reporting that hold across disciplinary boundaries, notably, in the ways of talking about the field as reflected in the two discoursal types introducted here. In the following sections, we will examine these discourses' shared features in greater detail in order to determine the extent to which these discourses are similar across disciplines, and whether there might not in fact be a shared part-genre to be found for all field geologists, allowing them to systematically organize and communicate their findings in rhetorically conventional and discoursally structured ways.

### 3.5 Sampling Discourse and sites description

While we may in fact initially identify both of the discoursal types as simply "talking about the field", practical differences do of course exist and geologists will chose one of the two to better suit their purposes. As we have seen in a previous example of Sampling Discourse, the author signals that he has collected his own samples, and where he has done this. Indeed, the emphasis is on the sample itself. In the Field Account, however, the author further specifically describes the field since such a description is

necessary for establishing a context to frame the sorts of claims he will be making. Thus, placing these purposes into another sort of conceptual frame, we might say that while Sampling Discourse can be described as similar to giving the 'provenance' of a work of art, the Field Account more resembles giving the 'circumstances' of its origins.

Sampling Discourse, therefore different in purpose, is also clearly linguistically distinct and textually separate from the Field Account; in other words, they will not co-occur as one embedded within the other. The task of identifying Sampling Discourse was relatively straightforward since it was contingent on noting instances where the research account essentially related that "the authors [had] sampled" but without giving any further field description. From the corpus of 103 articles, 52 articles evidenced having Sampling Discourse. From these, 73 distinct sampling statements were counted. Nearly 80% of these statements occur in geochemistry (58 from 73); the remaining instances occur next most often in petrology (12) and finally only 3 such sampling statement tokens were identified in articles from structural geology.

Three main discoursal elements systematically surface in Sampling Discourse. These are:

(1) the rock type investigated, as indicated either in the immediate text or in the subtitle;
(2) a verbal indication of research activity, such as "The sample was ... collected, found, discovered, observed," etc., and
(3) the general location of the sample given through a geographic description of the sampling area, in metric size or by its physical features, e.g., "By the bridge", "North of the road leading to".

The following illustrations of this discoursal type, taken from the corpus, highlight these three elements in context.

1. “Our youngest sample was collected at Dolomieu crater, in the central zone (PF92-10, altitude ~2500 m).” [CG-Bu]

2. “Fig. 1 shows the localities in central Telemark where samples, 2-5 kg in weight, were collected.” [CG-Bre]

3. “Petrographical and mineralogical (~10 kg) samples were collected at selected stations along traverses oriented perpendicular to the general strike of the investigated granitic masses to detect any aerial variations.” [MM-Aa]

4. “The glass spherules studied were collected from the K/T boundary outcrop at Beloc, Haiti, described in detail by Hildebrand and Boynton (1990) and Jehanno et al. (1992).” [GCA-Ho]

5. “*Sample CS 16* — This is a light grey, fine-grained, dense quartzo-feldspathic gneiss that belongs to a sequence of metasediments and metavolcanics exposed along the southern margin of the Erzgebirge in northern Bohemia and was collected from a roadcut near a bridge across the Luznica River southeast of Blahunov (Fig. 1).” [CMP-Kr]

6. “The sample belongs to a group of garnet-biotite gneisses which predominantly occur toward the eastern part of the Ponmudi Unit.” [CG-Bra]

7. “Th-rich loparite examined in the current study occurs in foyaite pegmatites exposed on the southern slope of Mt. Eveslogchorr, south Khibina.” [MM-Mr]

8. “Specimen BD4421 (wollastonite nephelinite) is from the most northerly of a chain of small scoria cones on the lower northern slopes of the volcano.” [JP-Da]

Indications of sampling locale rarely get very specific. At times, the sampling site location is cited using GPS measurements or other somewhat specific geographical references (e.g., ‘from a roadcut near a bridge across the Luznica River southeast of Blahunov’), but most often authors content themselves with “general” information, as we can see below:

> “ ... was collected **at Dolomieu crater**, **in the central zone** (PF92-10, altitude ~2500 m).”
>
> “Fig. 1 shows **the localities in central Telemark** where samples [..] were collected.”

> "Specimen BD4421 (wollastonite nephelinite) is **from the most northerly of a chain of small scoria cones on the lower northern slopes of the volcano**."

Clearly, then, these are not instructions for other geologists on how to get to the sampling site, per se. Information on where to go to find similar samples would likely need to be gathered from a more explicit exchange with the author — today probably over email.

At times the locational information also has a methodological slant, intended to justify the choice of sampling sites in terms of their usefulness:

> "... samples were collected at selected stations **along traverses oriented perpendicular to the general strike of the investigated granitic masses to detect** any aerial variations."

This example of methodological justification in Sampling Discourse, while it was not noted to occur with any significant frequency in the corpus, is striking in the sense that it provides more information than is typical not only about the sampling site, but also about the researchers' own actions and motivations. In this sense, it is similar to the types of "principled methodological narratives" found in psychology methods sections. Although comparatively speaking, Sampling Discourse is the diametrical opposite of what Swales and Luebs (2002 forthcoming, p. 143) have described as the "comprehensive narratives" they found in psychology methods sections, it is interesting to note that geologists, even in tight quarters, may also use such 'preemptive' strategies to imply to the reader that the reasons for the methodological decision-making process were made *before* the research was carried out, although we know the contrary to be true in actual practice.

And at other times, given the need for accommodating a research journal's need for economy of space, authors will simply tell the reader where the sample was globally picked up, but will then refer the reader to a more explicit description of the sampling site found elsewhere, for example, in the author's own prior publications:

> "... were collected from the K/T boundary outcrop at Beloc, Haiti, described in detail by Hildebrand and Boynton (1990) and **Jehanno et al. (1992)**."

As a first point, we might then say that while it seems to be important for writers of Sampling Discourse to provide at least *some* indication of where the sample comes from, great precision in site location is not typical. Indeed, they do not seem to be "road map" indicators as much as they are simple "sign posts". As one possible explanation for this common vagueness, it can be assumed that most geologists reading the article will in fact not be interested in the specific details of the site. Rather they are most likely reading for analytical content or methodology. Those who *are* interested in the "details" because for instance they are working on the same rocks or the same phenomena will typically contact the author (P. Goncalvez, T. Hammouda and N. Arnaud, pers. comms., 2000).

In the end, while the purpose of the locational elements in Sampling Discourse has been clarified somewhat, its actual role remains a bit mysterious. We are led to ask what the true scientific purpose 'sites locations' have for geologists, given the few actually helpful details and the vagueness that persists in the lack of explictness of valuable detail. And so we are led to ask whether they are not little more than simple markers of researchers' physical engagement in their fieldwork. This, however, will be a question for another time.

We will now turn to the ways in which the author signals his own participation in the sampling endeavor, which is of course what we are mostly interested in here, in terms of what we suspect field geologists' rhetorical needs are: to signal field presence so as to build credibility in the field account, to demonstrate competence and to establish authority over an area. In Sampling Discourse, there appear to be two main ways of doing this. In the first type, researcher agency is clearly implicated, either through overt pronominals or in statements in which researcher agency appears only semi-overt. And

so, while the researcher may at times seem to have been somehow evacuated in favor of some impersonal, scientifically objective *act,* he is in fact still present:

> "**Our** youngest sample **was collected**"
>
> "**We collected**…"
>
> " … samples **were collected**…"
>
> *"Sample CS 16* — This is a light grey, fine-grained, dense quartzo-feldspathic gneiss **that … was collected**…"

And then, there are those instances where even in something so seemingly concrete as "Sampling Discourse", the author-as-fieldworker appears to entirely disappear from his text and the sample becomes the transient doer of deeds:

> "The sample **belongs to** a group of ..."
>
> "Th-rich loparite [..] **occurs in** ..."
>
> "Specimen BD4421 (wollastonite nephelinite) **is from** ..."

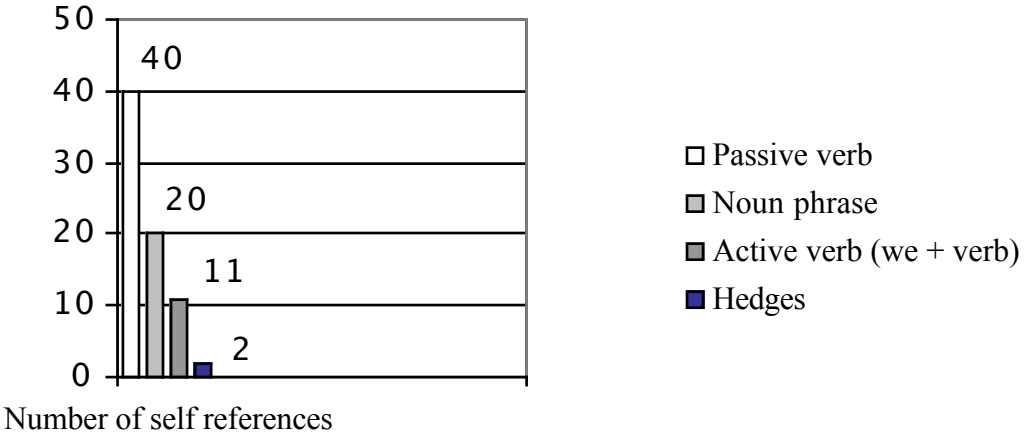
Incidentally, in this last group of sentences, we seem to no longer have any clear assurances that the author has sampled his own rocks. There *are* clear indications to be found, however, in the context. The context for the third sentence "Specimen BD4421", for example, comes from a caption the author has included with his geological map. In the caption, he informs us that he has named all his samples "BD_number_", with "BD" being the initials of his name: Bob Dawson. As he explains, thereby providing the reader with all the necessary context for establishing 'BD' as the collector of his own samples,

> "The author's BD collection prefix will be dropped..." [JP-Da]

### 3.5.1 The author as collector of his own samples

In this next section, we will continue to examine how authors of Sampling Discourse indicate that they have collected their own samples. Of course, going out into the field to collect one's own data and samples — and especially, indicating this in the text — are crucial for the process of building credibility, competence and authority. Whether or not one can admit to needing to do this in a context where the results of the lab analysis are admittedly the most important remains to be seen. Nonetheless, the corpus shows that the author's activity is indicated in a number of concrete ways (see Figure 3.3 below).

Figure 3.3. Strategies for indicating researcher activity with Sampling Discourse




The most frequent strategy is with a verbal construction, either with a passive verb phrase and linguistically unmarked agent (65%), where human activity is indicated but the agent himself is merely implied, or by the use of more 'overtly' author-implicated strategies, as in "we sampled or collected... " or "we could find no such distinct groups in our area of investigation" (15%). Although it may be argued that these two verbal strategies essentially amount to the same thing (i.e., drawing explicit and clear attention

to researcher activity through a verbal strategy), the overall greater tendency to use the passive rather than the active voice to indicate researcher activity would seem to suggest that geological writers consider the use of the first-person pronoun too overt or bold. It may even indicate that authors are conforming to perceived conventions for appropriately muted and indirect self-presentation in the research article (O. Merle, N. Arnaud, G. Chazot, pers. comms., 1999).

We can also notice how clearly authors implicate themselves in their research activity through their choice of verb. The list below details all the passive verbs identified in the 73 instances of Sampling Discourse, and all describe inherently "human" activities by variously making reference to the types of concrete, physical activities researchers do in the field (e.g., sampling, breaking and observing) as well as the necessary brain work (e.g., interpreting) the goes along with doing fieldwork. These are events that are always noted as occurring in the past.

| # of tokens each | Physical activities: |
|---|---|
| 11 | Collect |
| 5 | Sample |
| | Find |
| 3 | Take |
| | Select |
| 1 | Break, Obtain, Amass, Split, Map, Keep |
| | Investigate, Examine, Observe |

| # of tokens each | Intellectual activities: |
|---|---|
| 1 | Interpret, Think, Distinguish, Want |

Note that even the two most frequent numbers are quite low. This suggests that this strategy is not a standard option for indicating researcher activity, such as we might find in the references to methodological activity in the undergrad lab report (Swales, pers. comm., 2002).

Authors will also imply their own research activity through the use of a noun phrase (32%) that occurs with some active, mostly intransitive verb. Only very marginally do they indicate their activity with other strategies such as hedging, where modality in fact serves to modify objective natural reality, imperfectly filtered through the human eye, as in "the location *appears* to be correlated with …"[from CMP-Sh]. Therefore, this would appear to be a largely straightforward discourse, where hesitations or qualifications are mostly absent.

Finally, subject-position nominal phrases also clearly indicate researcher participation, as in "a typical *sample* [*weighed 20 kg*]", "most of *our samples* come from" or "*Sample 91* is from...", where "91" refers the reader to a specific point on a geological map indicating the researcher's sampling sites. Geologist informants further report that if an author uses the processual noun 'sampling', it is implied that the author has picked up his own samples (G. Chazot, pers. comm., 2000).

At this juncture, it would be appropriate to reiterate the ultimate aim of the present chapter, and this is to identify the "textually attested" instances of field reporting and to describe how geologists talk about the field and themselves in this field. Thus far, we have seen that geologists have two types of discourses to relate what for them is essential and contextualizing field data. The first discoursal type, Sampling Discourse, indicates whether or not they have sampled their own rocks. The strategies authors may choose from to do this range from the explicit to the rather obscure (e.g., "Specimen BD4421 (wollastonite nephelinite) is from..."). It is in the manipulation of these textual and linguistic cues that we can find one premise for credibility building within the field

report. In part, this task lies in the mutual trust established between the author-as-fieldworker and the reader, which hinges on the use of a codified set of markers such as those we have already seen, whereby the author signals that he is the collector of his own samples, the do-er of his own fieldwork, and hence, a valid member of the "brethren of the hammer" (Rudwick 1985). It would be simply unheard of, for example, for a field geologist to put his own initials on a sample if he were not the one to have picked it up.

While geologists can no longer give us a travel narrative, a vestige of the more "contingent repertoire" (Gilbert and Mulkay 1984, p. 57) of days gone by, writers of Sampling Discourse do nonetheless succeed in telling us quite a bit about themselves and their work in a mere sentence. Through their choice of verbs, nouns, and sites descriptions, we get a clear sense that "they were there". However, these are short and scattered accounts and although they appear with a good deal of regularity, their discoursal structure does not appear all encompassing, perhaps leaving room for pleasingly unexpected appearances by the author. We can readily recall "BD's" explanation for the change in sample nomenclature, reminding us that although he has dropped the BD-prefix, his samples remain his own.

We must at this point then perhaps be asking ourselves about the binding strength of conventions. Discourse analysts and language theorists alike have so whole-heartedly adopted the notion that scientific discourse is rigorously regulated by its "conventions" that we tend to forget that conventions do not forever bind the researcher-agent in each instance (Cohen 1989). Such a rigid notion of scientific discourse, by unduly drawing attention to stabilized patterns of discourse rather than to its dynamism, also neglects those instances where conventions may not in fact be so strong. In the case of Sampling Discourse, for instance, discoursal conventions do not seem strong enough to regulate everything that a geologist may want to say about his fieldwork, for the imposed limitations amount to only a handful of pieces of relevant information but do not entirely

'censure' the author, who like Bob Dawson, may draw attention to himself through indirect strategies.

We are thus left with the impression that while geologists may amply use the linguistic strategies offered by Sampling Discourse for objectively marking their participation in the research project, it is also a reporting frame that from time to time leaves room for the contingencies inherent in every research story. It may therefore be more appropriate to regard how geologists organize the report of their fieldwork in terms of "constellations" of authorial strategies (Schryer 2001), granting a shifting authorial space for the writer to manipulate, depending on the circumstances. We shall say more about this in the following parts of this chapter, and will return to a more in-depth discussion of the topic in Chapter 5.

## 3.6 The Field Account

From the general layout and few linguistic features we have seen in Sampling Discourse thus far, we are already beginning to get at some of the "story" behind fieldwork and geologists' reporting practices. It has so far been a story marked by tight space-constrictions, so much so that some of the field details have seemed at times to "trickle over the edges" into other areas of the article (e.g., captions, acknowledgments).

We will now turn our attention to the features of another type of field discourse. While in Sampling Discourse we were looking at the report of field data that is contained very often within a single sentence, here we will examine the Field Account, which will be shown to be an extended and discrete functional category. It will be argued that this type of discoursal strategy is in fact organized over several sentences, resulting in a concrete discoursal 'unit', with clear onset and offset criteria. Its length permits a more detailed account of the field mission, and it is characterized by a number of more or less explicit discoursal traces of researcher presence in the field. In the corpus, it ranges from

515 to 1686 words in average length, the shortest being a mere 83 words and the longest, 4905.

As we can recall (Figure 3.2), the Field Account occurs more frequently than Sampling Discourse (63% of all articles use it, or 65 from 103 articles, while only 48% have Sampling Discourse). There are, however, some marked discipline-specific tendencies. For example, 46% of all field-reporting articles in geochemistry have a Field Account; in petrology, the frequency is 61% of all articles and in structural geology, it is 90% (Table 3.3 below). Furthermore, while overall article length tends to be quite similar among the disciplines — on the whole, 5845 words in length — the length of the Field Account (FA) in relation to the overall article varies quite a bit from discipline to discipline (Table 3.3). And thus, the amount of time authors spend talking about the fieldwork mission in comparison to other subjects (e.g., chemical analysis, numerical modeling, etc.) differs markedly in each subdiscipline, on at least a visual level.

Table 3.3 Average Field Account (FA) length, in number of words

| Number of articles with an explicitly identifiable textual description of the authors' fieldwork (see Table 3.2) | Average article length (in # of words) | Average FA length (in # of words) | Amount of total article dedicated to the FA |
|---|---|---|---|
| **Structural Geology – 28 of 33** | **5978** | **1686** | **28.2%** |
| Journal of Structural Geology (13 of 13) | 5258 | 1949 | 37.1% |
| Tectonics (12 of 13) | 7302 | 1825 | 25% |
| Geodinamica Acta (5 of 7) | 4531 | 854 | 18.8% |
| **Petrology – 19 of 31** | **5242** | **570** | **10.9%** |
| Journal of petrology (13 of 13) | 5518 | 745 | 13.5% |
| Lithos (4 of 5) | 6859 | 705 | 10.3% |
| Mineralogical Magazine (10 of 13) | 3350 | 309 | 9.2% |
| **Geochemistry – 18 of 39** | **6314** | **515** | **8.2%** |
| Contributions Mineral. Petrol. (13 of 13) | 6547 | 474 | 7.2% |
| Chemical geology (13 of 13) | 5569 | 444 | 7.9% |
| Geochimica et Cosmochim. Acta (12 of 13) | 6883 | 660 | 9.6% |

Not surprisingly, structural geology not only typically has the longest Field Account (1686 words on average), but it is also the discipline in which the author devotes the most textual space to talking about the results of the field mission — indeed, nearly one-third of the entire research article is reserved for this purpose. While the Field Account is shorter in overall length in the two rock-based disciplines, it tends to be a bit longer in petrology than in geochemistry (with an average of 570 words in petrology and 515 words in geochemistry). This might appear to favor the assumption that field petrologists are more "textually visible" than geochemists because they seem to spend more time talking about their field endeavors. However, this is where we return to the "impression" mentioned earlier that petrologists are actually *less* textually present as field reporters than others. While 11% of the research article in petrology reports on field details, and 8% of the research article in geochemistry is reserved to this end (see Table 3.3), in fact petrology Field Accounts are characterized by a greater detailing of minute petrological and mineralogical features, rather than actually talking more about field structures, per se and, of course, using the corresponding field presence indicators to match, as we shall see in a moment.

Figure 3.4 Location of the Field Account in 111 subsections

As one final point in this broad overview of the general discourse tendencies of the Field Account, we might also wonder where exactly we are to find it. I have noted, from a total of 111 subsections where a Field Account was identified, that while it will never occur in the 'Sampling', 'Methods', 'Chemical Analyses' or 'Mineralogy' subsections, it is nonetheless most frequently embedded within one subsection or another (68%). Thus, we find it in such locations as the 'Geological Setting' (40%, or 44 from 111), 'Field relations' (15%, or 16 occurrences), or 'Introduction' (8 times) and 'Petrography' (6 times) sections (see Figure 3.4 below). In contrast, it occurs as its own, autonomous subsection 32% of the time, and the overwhelming majority of these instances appear, of course, in structural geology (92%; see also Figure 3.2).

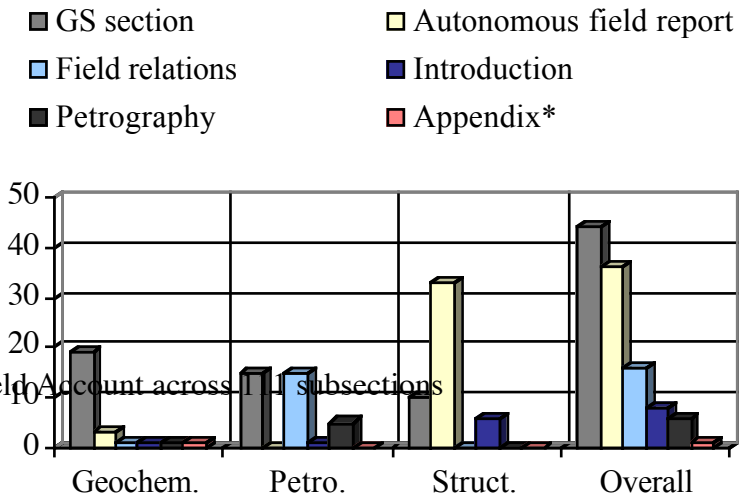



Figure 3.4. Location of the Field Account across 111 subsections

The figure above (Fig. 3.4) details the textual location of the Field Account in the geology research article. While it is most frequently embedded within another subsection, it will be argued in the following sections that the Field Account is a functional category, and acts as a discrete discoursal unit. The possibility that a "stand-alone part-genre" might be found embedded within another part-genre has already been suggested by Bhatia (1997), who has examined promotional statements found within academic book introductions.

To argue, however, that any specific unit can be characterized as a 'genre' is quite another story. Certainly, I would not attempt to argue, for example, that 'Sampling Discourse' is constitutive of a part-genre, although there is clear discoursal homogeny between textual exemplars. In scope, however, its level of detail is local (sentence-length), as it seeks only to provide very brief and general information about the site. Furthermore, not all field geologists use it.

The Field Account, on the other hand, as an extended and more detailed version of a field description, seems to me to be operating at a different level, one which draws together the concerns of an entire community of practice, as evidenced by its occurrence

across all the disciplines investigated here, regardless of methodological approach. In addition, there has also been preliminary evidence (see Chapter 2) that it embodies a standard of practice that has been transformed over the years, as such constituting the modern-day remnants of the rhetorical field descriptions that at the outset were communicated during intenstive letter-writing exchanges in the middle 1800's (Rudwick 1985). The results of the transformational analysis of the following chapter (Chapter 4) further suggest that it is one of the "recontextualized" (Linell 1998) sites geologists use today for reporting on their fieldwork, reflecting the findings indicated in the geologist's field notebook and his resulting interpretations.

Certainly, its apparent ties to other historical or antecedant genres, "Un nouveau genre est toujours la transformation d'un ou de plusieurs genres anciens: par inversion, par déplacement, par combinaison" (Todorov 1978, p. 47; see also Jamieson 1975), as well as its existence as a "report" of research carried out in the field, as such being a link in the constitution of an intertextual discursive chain (Linell 1998, Fairclough 1992), lends some early support to the claim being made here, that the Field Account does in fact constitute its own part-genre. Further evidence for this claim will be discussed in the following sections.

### 3.6.1 The autonomous Field Account

Because it will be argued that the Field Account may in fact constitute its own, independent "part-genre", occurring not only as an autonomous subsection but also embedded within other subsections, we will here glance briefly at the specific conditions characterizing the autonomous Field Account in structural geology as a way of framing the ensuing discussion.

Slightly more than half of the subsections containing a Field Account in structural geology are found as "autonomous" textual units, with their own subtitle (32 from 59, or

54%; see Figure 3.4). The titles of these autonomous fieldwork reports reflect two main types of organization for field reporting, using

1) the name of the geographical area whose structure is being studied,

or

2) the name of specific structure types

Sometimes, there is a combination of both; these types are illustrated in the following lists. Each number refers to a single article, a 'semi-colon' to the end of a section.

"Geographical titles"

1. The Seve Nappe / Köli Nappe Complex
2. The Rödberget-Bunnerviken; Västerån synform; Tälijstensvalen-Handöl; Mount Snasahögarna; Storvallen and Storlien-Skardøra Window areas
3. The Kangaroo Island domain – Oblique wrenching; Southern Fleurieu domain – Reactivated growth faults; Carrickalinga section – Cambrian platform shortening; Central section – Footwall shortcut faulting; and the Karinya domain – Homogeneous shortening
4. The Carboneras fault system
5. Examples of lateral extrusion in transpression zones: The Troodos-Mamonia suture zone, SW Cyprus; Mid-Devonian upper crustal deformation in central Scotland
6. The Alpujarride complex in the central Betics
7. Examples in the Eastern Appalachian Piedmont
8. The South Mayo Trough
9. Structure: Orocopia schist; Upper plate; Chocolate Mountains Fault
10. Southern Yadong-Gulu rift; Zherger La Detachment
11. Contacts of Koolen Gneisses and surrounding rocks
12. Structure: Corocoro-Corque region; Eastern limb of the Corocoro-Corque

syncline; Western margin of the eastern Cordillera; Tambo Tambillo region

13. Structural Geology: Skagit-Skymo contact; Skymo complex; Skymo – Little Jack contact and deformation of the Little Jack Terrane

14. Beardmore group structural relations; Cobham Range; Kon-Tiki Nunatak

15. Structural details in the Garm segment of the Pamir region

16. Comparisons with other Tethyan sediment series: Polish Carpathians

17. Structural data: Ampanihy shear zone

18. The studied shear zones: Autochthonous Cadomian Thaya basement; Upper Moravian nappe

19. Superposed structures in the Adra extensional unit

20. Criteria for the sense of movement on the slickenside surfaces of the Jaloche detachment fault

21. Late Miocene Awatere basin; Late Miocene conglomerate to the north and south of the Awatere fault; Late Cenozoic strike slip on the Awatere fault zone

22. Fault-slip data from the Magallanes Fault zone: Fault populations; Kinematic analysis

"Structural titles"

23. Mylonites

24. Description of the shear zones / Shear strain estimates

25. Displacement profiles

26. Lateral tip geometry

27. P-shears

28. Gouge fabrics

29. Lithostratigraphic sequence

30. Stratigraphic constraints on fault motion

31. Folds and transport direction

32. Metamorphism / Deformation / Magmatism.

33. Structural analysis: D1-2: Early thrusting; D3: Steep folding; D4: Detachment tectonics and kinematic significance; D5: Wrench tectonics; D6: Permian extensio

34. Pre- and synaccretionary structures, Group I / Post-accretionary structures, Group I

35. Metamorphism and deformation

36. Tectonic and metamorphic evolution: D2 deformation and metamorphism; D3 deformation and metamorphism

Because we now know that geology tends to downplay the report of its field data in general, these subtitles somehow contradict what we might expect from the Field Account as something demoted, muted, and spatially constricted. Instead, here we have geologists overtly announcing their intended and upcoming fieldwork presentations. Such 'titled' announcements of fieldwork-to-come are not, however, "the norm" for all geologists, as we might guess. In fact, the Field Account is most often embedded within another subsection, making it a text "hidden" from view. However, the very presence of such field-centered subtitles in structural geology lends some early support to the argument being made here that field reporting practices have evolved over time into a specific, discoursally organized unit that we find in the contemporary research article, a "Field Account part-genre". What these titles seem to suggest is that this discoursal structure may simply be more overtly delimited in structural geology than in the other subdisciplines examined here.

Even though the Field Account in structural geology is longer and perhaps even more 'visually overt' than it is elsewhere, we will see, however, that the researcher may not talk more freely about his field endeavors here than in other disciplines. In fact, the Field Account in structural geology is just as "muted" as it is in other areas of geology,

and therefore we do not necessarily have a sense that its authors have more of a stake in his territory than do their colleagues.

### 3.7 The rhetorical construction of field competence, credibility and authority.

As we can recall from Rudwick's (1985) account of the field reporting practices in England during the nineteenth century, it was already abundantly clear by middle 1800's that the practice of field description in geology went far beyond a "mere" description of the field. By studying a series of letters exchanged between members of the Geological Society of London, Rudwick (1985) relates that field description had already evolved into a complex rhetorical tool whereby, through the skillful handling of relevant natural evidence, geologists could establish their credibility, maintain authority, and gain acceptance and attributed competence from their circle of peers. As Rudwick puts it, the rhetorical handling of this evidence was not just "a stylistic 'extra' tacked on to content for good measure; it *was* the content" (Rudwick 1985, p. 423).

Along these lines, our objective here is to identify the linguistic and rhetorical strategies by which the geology author today reconstructs his encounter with nature in the scientific research article, and how he reports this encounter to his research community in order to establish himself as a competent field researcher. We will constrain the focus of our investigation to the principal subsection in which this occurs in the research article, namely, what has been identified as a "Field Account part-genre".

"Move analysis" (Swales 1984, 1990, Dudley-Evans 1986, Hopkins and Dudley-Evans 1988, Bhatia 1993, Nwogu 1997), or the analysis of the different rhetorical patterns or discoursal acts within a text, has long been considered by EAP practitioners to be a useful way of uncovering the author's conventionalized rhetorical plans and the linguistic strategies for carrying them out within a genre of texts. Occurring at the level of

discourse, ‘moves’ group together as a series of utterances all intended to achieve one central effect, or purpose. Through the identification and analysis of a set of “strategic moves” in a genre, one can identify and explain the way in which the author — conventionally — constructs his text in response to a set of strategic objectives. These objectives must, of course, be understood and met in order for the text to be considered successful by readers in his research community; moreover, a well-written text may in fact be so considered due to its success in sailing the reader over the various move boundaries (Swales 2000b).

Some analysts, however, criticize the approach’s failure to directly account for linguistic data. Paltridge (1997), for example, has argued that move analysis, such as that demonstrated in the ‘CARS (‘Create A Research Space’) model’, does not rely on *linguistic features* in determining textual boundaries and rhetorical strategies as much as it bases the identification of these boundaries on *content.* As a consequence, move analysis such as it is typically practiced would necessarily be more ‘content-based’ than ‘text-based’. Of course, determining content is crucial for identifying and classifying moves, but it is likely that it is linguistic form in the first place that enables us to identify moves. We can be usefully reminded, such as we are in a recent article by Parkinson (2000), of Halliday’s (1993) suggestion that “scientific English”, and more specifically, its related genres and part-genres, is in fact recognizable as such because it contains “clusters of features” (p. 56) which are relationally organized throughout the text.

As one working premise, then, we will here take linguistic form and content as intimately intertwined. In other words, a text’s linguistic form, occurring as “clusters of features”, is relationally organized around strategic “moves”, or conventionalized communicative intentions. Assuredly, it is because particular strategies or moves have specific linguistic features that we, as analysts, and they, as scientific readers, can identify them as such.

And thus, despite the success with which the CARS model's "Introductory moves" have been identified at the level of discourse, one might also work from the assumption that it is also possible to describe rhetorical strategies by examining and analyzing the specific clusters of linguistic features that occur at a level below that of discourse structure, and by determining how they might purposively cluster together so as to achieve an intended effect. As a consequence, even the most minute linguistic features in a text should not be neglected in establishing the complex process of text construction, for it is the very way in which these features cluster together that creates "meaning", or in the specific case of the Field Account, "authority as convention". Identifying the clusters of features characteristic of the Field Account and their strategic relational organization throughout the text will therefore be one of the principal tasks of this chapter.

However, one difficulty that quickly arises in a move-based analysis of a corpus of texts is exactly how to identify and categorize "the move", and here one can recognize that Paltridge (1997) indeed raises an important objection to the move analysis approach and its focus on "discoursal content", a point to which we will return shortly. But first, for the purpose of discussion and establishing how the identification of moves has largely been based on discourse-level structures, we might take as an example the set of moves which occur in research article introductions (Lopez 1982, Cooper 1985, Crookes 1986, Jacoby 1987, Swales 1990, Ahmad 1997)**.** Here, we effectively see, as in Swales' (1990) 'CARS model', an effective explanation for content organization in terms of the authorial strategies that occur at various, but specific, points throughout a text. These strategies (or moves) in the scientific research article are further characterized by a number of ordered or alternative discoursal steps which progressively build upon one another in order to achieve the ultimate intended effect: "creating a research space" for the particular author within a particular manuscript. To remind ourselves of these steps more clearly, we can consider the following oft-cited page from Swales (1990):

'Create A Research Space' (CARS) Model

Move 1-- Establishing a territory
- Step 1 Claiming centrality
  - and/or
- Step 2 Making topic generalization(s)
  - and/or
- Step 3 Reviewing items of previous research

Move 2 -- Establishing a niche
- Step 1A Counter claiming
  - or
- Step 1B Indicating a gap
  - or
- Step 1C Question-raising
  - or
- Step 1D Continuing a tradition

Move 3 -- Occupying the niche
- Step 1A Outlining purposes
  - or
- Step 1B Announcing present research
- Step 2 Announcing principal findings
- Step 3 Indicating RA structure

(Swales, 1990: 141)

Even if this rhetorical structure has often been validated, albeit with some minor omissions and extensions, it is quite telling that comparable success has not been achieved with other part-genres, apart from abstracts (Santos 1996; Yakhontova 1998). One reason for this is that the methodological approach of move analysis, by focusing on the discoursal organization of the CARS model as a 'stepped' move analysis, as cognitively neat, successfully explanatory and pedagogically useful it has been seen to be, tempts us into thinking about the structure of part-genres in purely straight, linear terms and this especially at the level of discourse — or in Paltridge's (1997) terms, at the level of "content". However, as Paltridge (1997) rightly points out, genre analysts would do well to pay more explicit attention to the *linguistic* features of a genre, such as the low-level clusters of features that will be examined here.

One related point that makes the CARS and similar models difficult to use in the description of the low-level clusters of features that characterize certain part-genres lies in the reasons for the model's success. The CARS model has been considered successful not only because it usefully describes and explains the rhetorical strategies found in the

research article introduction, but also because it picks up on and accounts for elements which have consequently been shown to relate to how scientists actually read articles. As we know from such studies as Bazerman (1988), Myers (1990) or Pinelli, Cordle and Vondran (1984), scientists read only a small number of articles completely, structuring what focused reading they do in ways that gather the most pertinent information in the least amount of time. The introduction section, with its discrete set of explicit strategies, does not escape this selective reading practice. Paul and Charney (1995), for example, have investigated how introductory strategies are intended to affect readers, given that scientists will read introductions mostly to decide what not to read or when to stop reading (p. 402). Reviewers and editors, of course, further reinforce the presence of a strong set of rhetorically visible intentions in the introduction, by evaluating the introduction of a submitted article as being appropriate, interesting, or comprehensive during the review process (Burrough-Boenisch 1999). Thus, a strong connection can be made between the tangible set of strategies found in the introduction, the predictability with which they occur (i.e., their conventionalization), and readers' strategic use of them to navigate through the text.

However, it might very well be the case that not every genre or part-genre is so explicitly rhetorical or linearly well endowed as the research article introduction or abstract. Quite a revealing contrast can be found in other genres of texts, such as the Field Account in geology. One might very well find identifiable "moves" in this genre of texts, but often these moves remain overly vague and general, and do not seem indicative of any progressive construction. In such a text, one has the idea that something is assuredly going on, but whatever it is is not wholly clear at the level of discourse structure. It is only upon closer examination of the minute linguistic and other textual elements embedded within various parts of even a single sentence that we may first begin to get a sense of what is really happening: how the text has been constructed and what it is doing.

The obscure nature of its discourse is perhaps explainable by the fact that, in contrast to article abstracts and introductions, comparatively few geologists ever actually *read* Field Accounts. Geologist informants have reported that it is written essentially for those few who have a specific interest in the area and more general readers will simply most often skip over them. This changes the tune quite a bit, and so we might perhaps suggest that the article introduction and other strongly move-inclined part-genres, such as the abstract, have such clearly identifiable and recognized, step-by-step-constructed strategies because they are read and used by so many people. This extensive use over time may have resulted in the establishment of concrete and explicit conventions at the level of discourse and rhetorical content, for the purpose of regulating a widespread and complex communicative interaction. This is clearly a matter for further investigation.

For now, however, it is suggested here that the lower-level status of the Field Account, as a less-frequently read and historically 'down-graded' document, which is most often hidden from view and embedded within other part-genres, may have resulted in the establishment of this part-genre's rhetorical strategies as occurring at a level 'below' that of discourse. In order to better account for this 'low-level' linguistic detail, one would therefore need a model of a genre which, while retaining the descriptive power of authorial intention and the strategic move, would also remove the content-based constraints imposed by discoursal linearity and the step-by-step construction of the CARS model on the description of linguistic data.

A recent and very useful reconceptualization of genre allows us to get around the problems of linearity and progressive construction in the linguistic description of text. In Schryer (2001), taking inspiration from Lemke (1995) and Bourdieu and Wacquant (1992), we find the suggestion that genres may be taken to function as a *constellation* of "regulated, improvisational strategies" or sets of strategies. Authors may use this constellation of strategies to mutually negotiate and improvise the construction of their text through space and time. Therefore, the instantiation of a particular genre of texts

entails the selection of elements from a set of strategic options. If we further accept that these strategies are recognizable as such given their organized relationship to particular clusters of linguistic features (i.e., Halliday 1993), then we may take many genres to be a constellation of strategies constructed and played out in its clusters of relational linguistic features.

It seems clear, given the characteristics of the Field Account as depicted by the corpus, that a description of genre which focuses solely on the level of discoursal content cannot explain how clusters of features typical of the Field Account act together in any organized way to produce coherent meaning or to crystallize authorial strategy. A conceptualization of genre as a 'constellation' of strategic options, on the other hand, would free up the board for developing a detailed, explanatory account of low-level features whose only immediately apparent connection is that they occur together within one discourse unit, while not seeming evidently related to the text's overall rhetorical and discoursal organization. In the following sections, we will examine how the link between the enactment of multiple strategies and clusters of features in the Field Account's might coincide.

## 3.8 The Field Account: An embedded part-genre

As we have seen previously, the Field Account occurs in a wide variety of subsections in the geology research article (see Figure 3.4), such as in the 'Geological Setting' section, a disciplinary scene-setting, introductory part-genre which establishes what is geologically known about the region under study (Dressen and Swales 2000). It is also found in 'Field relations', 'Petrography', 'Sampling' and even in 'Introduction' sections.

With the exception of field reporting practices in structural geology, it occurs only nominally as an autonomous field reporting subsection. In geochemistry, in contrast,

since geochemists' primary analytical contributions take place in the laboratory, their fieldwork would appear to take a back seat to geochemical results and therefore, reports of their fieldwork are overwhelmingly found to be embedded within the general, scene-setting Geological Setting section. To a lesser extent, the same is true for petrology, where slightly less than half of the reports of field data occur within the Geological Setting; a sizable quantity (nearly a quarter) of field results are also to be found in 'Field relations' sections.

Therefore, the Field Account generally does not constitute its own, textually autonomous "part-genre" (Ayers 1994) in geology, but is found to be embedded within other discoursally and textually delimited part-genres. It is precisely this embedding which interests us here, for without the "pre-modifying nominal of purpose" (Swales 1990, p. 55) or the subtitle typical of other part-genres in the scientific research article, which works to orient the reader's expectations about up and coming text, such embedded occurrences offer the best opportunity to study the Field Account in all its distinctive 'occultness'.

Furthermore, if we are to assume, like Gunnarrson (1994), Atkinson (1992) or Salager-Meyer (1998), that text and context are closely related, by which relationship authors' lexical, grammatical, rhetorical and discoursal choices are conditioned and constrained by their sociolinguistic environment (Régent 1994, Schramm 1996), then we might very well accept that the embedded status of the modern Field Account, with its low-level, rhetorically oriented, strategic clusters of features, may in fact be the end-result of a long, drawn-out disciplinary struggle, where the centrality "the field" has enjoyed in times past in geological practice has ultimately been displaced to the sidelines as other methological priorities have taken center stage.

Because the Field Account occurs most frequently within the Geological Setting section, and also because an earlier study (Dressen and Swales 2000) has previously documented the rhetorical and linguistic features of this part-genre, the interaction

between the two part-genres constitutes an appropriate subject for analysis here. As a consequence, in the following sections I will first seek to establish that the Field Account can be identified as an independent discourse unit when it occurs within the frame of

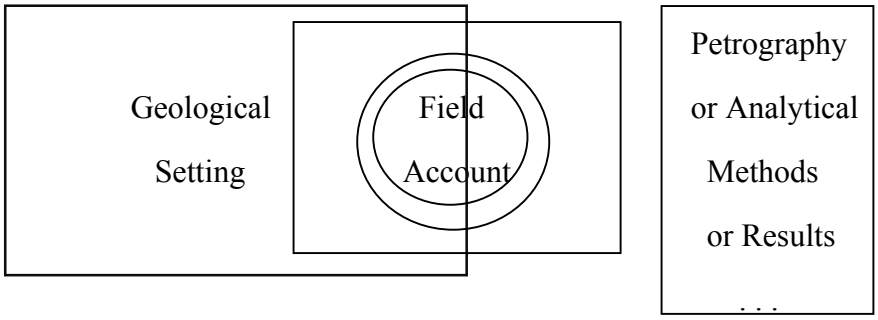



Figure 3.5 Field Account embedding with the Geological Setting

another part-genre, here, the Geological Setting. It will further be argued that although the Field Account does not usually stand on its own, with a special, nominally dedicated subsection, it *is* clearly delimited, both rhetorically and linguistically, from its surrounding part-genre context, as represented in Figure 3.5 above, showing onset and offset boundaries .

Next, I will show that the Field Account consists of a small handful of identifiable moves. And lastly, that these moves remain very general in nature and do not reveal all there is to say about the underlying intentions or motivations of the text's users, nor about how they construct their account of the field. Instead, we must turn to a description of the discoursal unit's low-level features. The recurrence of a fixed set of strategically-oriented "clusters of features" (Halliday 1993) acting as "discoursal options" throughout all 111 identified instances of Field Accounts from the corpus lends further credence to the claim that it is truly a specifically designed and oriented part-genre. This contention is additionally supported by the similarities found in field reporting styles across disciplines. For while the Field Account is structurally autonomous in structural geology,

its discourse appears just as 'muted' and 'agentially devoid' as that of geochemistry and petrology, making use of the same set of strategic discoursal choices.

### 3.8.1 Evidence for a Field Account part-genre: Generic integrity within the Geological Setting section

We will now turn to the Geological Setting section in order to examine the contrast between the two part-genres, as indicated by shifts in linguistic features and rhetorical purpose. As mentioned above, the Geological Setting section was shown to be an introductory and scene-setting part-genre that outlines disciplinary background knowledge about the region and known Earth mechanisms, reporting on geological history, emplacement processes, structure and composition of the region under investigation (Dressen and Swales 2000). It is characterized primarily by a series of general-to-specific rhetorical moves that describe the topographical, historical, and physical features of the terrain under study. Reflecting these different rhetorical purposes, it is composed of a number of scene-setting moves which are shown in the following excerpt , taken from CMP-Ge (see Text 3.1 below; see also Appendix C for the entire text; sentence numbers have been added).

Text 3.1 Field Account embedding within the Geological Setting

| | | Geological setting |
|---|---|---|
| Para. 1 | Localization | [1]The peninsula of Ardnamurchan is the most westerly point of the British mainland and belongs to the British Tertiary Volcanic Province (Fig. 1). [2]Intensive magmatism occurred in the region about 60 Ma ago (Wells and Mac Rae 1969; Mitchell and Reen 1973; Mussett et al. 1988) in connection with the opening of the North Atlantic ocean. [3]The Ardnamurchan igneous complex is one of a number of intrusive centres in this province and lies at the westernmost point of the peninsula. [4]The igneous rocks intrude into the Proterozoic metasediments of the Northern Highlands, the so-called Moine schists, and thin overlying Mesozoic sediments (Fig. 1). [5]Although the thickness of the Moine schist is unknown (several kilometres has been suggested; Morrison et al. 1985) it is assumed that the boundary between the Moine schist and the Lewisian gneiss beneath the region lies at relatively shallow levels. [6]Seismic data from central Scotland, 150 km east of Ardnamurchan, indicate that the transition between amphibolite- and granulite-facies rocks may lie at depths between 6 and 14 km (Bamford et al. 1977). |
| | Visual reference/Age | |
| | Emplacement process Nomenclature | |
| | General structural descriptions | |

| | | |
|---|---|---|
| Para. 2 | Nomenclature | [7]The ring-shaped igneous intrusions in Ardnamurchan and neighboring complexes have been named ring-dykes by Richey et al. (1930) and were divided into three different centres with decreasing intrusion age (Fig. 1). [8]The central complex of Ardnamurchan was |
| | Emplacement process | intruded by numerous basaltic cone-sheets forming the latest stage of magmatism apart from several northwest-striking dykes. |
| Para. 3 | Field description | [9]The thickness of individual cone-sheets varies between 10 cm and several metres. [10]Individual cone-sheets can occur side by side and occasionally cross or unite to a thick sheet. [11]The cone sheets of Centre II can be grouped into an inner and an outer suite relative to a large gabbroic ring intrusion which cross-cuts the latter (Fig. 1). [12]The outer cone-sheets are inclined at angles of about 30° in the direction of the focal point and intruded into Proterozoic Moine schists, Jurassic sediments and Tertiary plateau lavas. [13]In contrast, the inner suite dips with angles of the order of 70° and cuts the igneous ring-dykes of Centre II. .... |

One of the primary avowed rhetorical purposes of the Geological Setting is to reconstitute the history of the region by giving a review of what is often established — and accepted — knowledge within the geological community. It is therefore a largely neutral terrane, and authors here generally avoid raising debate or criticizing prior claims (Dressen 1997b). As a consequence, it is discoursally and rhetorically marked by the occurrence of multiple citations, serving to attribute ownership of prior knowledge claims. Nonetheless, there is a relatively good deal of human agential participation and implication in the discussion, albeit discreet, as illustrated in the verbal repertoire. While the 'intrusive igneous complex' variably 'is', 'occurs', 'lies', or 'intrudes', the researcher's and his community's, at times merely implied, agential role is indicated by the verbs 'suggest', 'indicate', 'name', 'divide', and 'know' (sentences 5-7).

It also incorporates a rich range of verbal modes and tenses for reporting on geological processes and emplacement processes, prior fieldwork results, discussion of prior publications, and the current state of knowledge (Dressen and Swales 2000). These differing rhetorical tasks are reflected, for example, in the use of the present tense for giving a general description of the area (sentences 1-3), thereby setting up the evidential support which has been previously confirmed by the scientific community as 'truths' about the Earth: the present tense for referring to unsettled knowledge claims (sentence

5), and by the past perfect tense, used to describe geological time, emplacement processes (sentences 2 and 8) and prior work (sentence 7).

Moreover, this particular text exemplar contains a number of discoursal elements specific to the Geological Setting, called "opening sub-moves" (Dressen and Swales 2000), that have been identified as follows:

1. **'Localization'**, which indicates both the general geographical location of the study area as well as a geologically composed-name (e.g., 'Koolen Lake-Lavrentiya Bay region' which gives 'Koolen gneiss'):

   s. 1: 'The **peninsula of Ardnamurchan** is the **most westerly point of the British mainland** and belongs to the **British Tertiary Volcanic Province**.'

   s. 3: 'The **Ardnamurchan igneous complex** is one of a number of …'

2. **'Visual reference'**, where authors provide some map or figure to help the reader visually determine the exact geographical location of the formation, as well as how it appears on the terrain in relation to surrounding geological features:

   s. 1, 4: '**Fig. 1**'

3. **'Geological age'** of the region and its structures, introduced by the use of the past perfect:

   s. 2: 'Intensive magmatism **occurred** in the region **about 60 Ma ago**…'

   s. 4: '**Proterozoic** metasediments of the Northern Highlands, the so-called Moine schists, and thin overlying **Mesozoic** sediments…'

4. **'Emplacement processes'**, or a brief description of the geological activity that has occurred in the region:

   s. 2: '**Intensive magmatism occurred**… **in connection with the opening of** the North Atlantic Ocean.'

s. 8: ‘The central complex of Ardnamurchan **was intruded by numerous basaltic cone-sheets forming the latest stage of magmatism** apart from several northwest-striking dykes.’

5. **‘Nomenclature’**, where the studied rock– or formation–type is identified, often occurring in close conjunction with the given location:

s. 3: ‘**Ardnamurchan igneous complex**’

s. 4: ‘The **igneous rocks** intrude… the **so-called Moine schists** …’

s. 7: The ring-shaped **igneous intrusions** in **Ardnamurchan** …’

6. **‘Composition and/or structural description of the area’**, using established knowledge, prior studies, or the author’s own fieldwork data:

s. 4: ‘**The igneous rocks intrude into the Proterozoic metasediments** of the Northern Highlands, the so-called Moine schists, **and thin overlying Mesozoic sediments** (Fig. 1).’

s. 5: ‘…it is assumed that **the boundary between the Moine schist and the Lewisian gneiss** beneath the region **lies at relatively shallow levels**.’

s. 6: ‘**Seismic data** … indicate that the **transition between amphibolite- and granulite-facies rocks may lie at depths between 6 and 14 km (Bamford et al. 1977).’**

In short, the author is telling us the general story of the region, as it is cautioned by his own and other geologists’ current understanding of this story. It is an act which this particular author accomplishes reasonably adroitly, as exemplified by the complexity of his sentences. Indeed, while a loose thematic thread runs through the two first paragraphs, a rich and intricate rhematic construction adds a wealth of information to his account by tacking on a number of complex qualifier phrases, or “elaborative attributes” (Paltridge, 1994). Namely, we learn that

s. 1 the ‘peninsula’ is the most westerly of the British mainland and belongs to the British Tertiary Province

s. 2 that intensive magmatism occurred [there] a certain time ago in connection with the opening of the North Atlantic Ocean

s. 3 that [the complex] is one of a number of intrusive centres in the province and lies the most to the west

s. 4 that [it] intrudes into both the Proterozoic metasediments of the Northern Highlands, which we learn have previously been called 'Moine schists', and into thin, overlying Mesozoic sediments

s. 5 and that there is some uncertainty concerning the boundary between the different structures: 'it is assumed that … the boundary between A and B beneath C lies at D'.

Finally, we can further see the unfinished aspect of knowledge establishment in sentence 6, where epistemic modal 'may' and the verb 'indicate' suggest that consensus is still lacking in the interpretation of the structural layout discussed in sentence 5.

On the other hand, a striking shift in communicative and rhetorical objective is apparent beginning in paragraph 3, sentence 9, which has been identified here as the onset of the Field Account. Although one might argue that the Field Account is simply an extension of the Geological Setting, I would respond to the contrary, instead proposing that it acts as a "stand-alone part-genre" (Bhatia 1997) for two reasons. First, it is an independent discoursal unit that occurs across a variety of different subsections, and is not inherently tied to the Geological Setting alone. Second, its determining linguistic features are coherent across all its manifestations, regardless of the embedding environment. We will next examine the elements that make the Field Account a distinct — albeit embedded — part-genre.

A number of features distinguish paragraph 3 (Text 3.1) from the more general background of the Geological Setting in the preceding paragraphs. When the author begins to report on his own fieldwork results, there is a marked shift in focus from general-to-specific, moving from the general, established background knowledge of the

Geological Setting to the very specific particularities of field description. Accordingly, the rhematic sequences here immediately become less complex:

s. 9 [the thickness simply varies] between 10 cm and several metres
s. 10 [cone-sheets] can occur side-by-side, etc.

At the same time, the thematic links between sentences become tighter, as seen for example by the strict and overt repetition of 'cone-sheets' throughout sentences 9-12.

s. 9: 'The thickness of individual **cone-sheets**'

s. 10 'Individual **cone-sheets**'

s. 11 'The **cone sheets** of Centre'

s. 12 'The outer **cone-sheets**'

In the corpus, another cue for onset is found in the fact that relational verbs suddenly and significantly increase in frequency. What is more, verbs, such as those here, all occur in the present tense, and save one instance ('can be grouped' in sentence 11), there is no other verbally implied human agency. Instead, the geological structures go about their business well outside the realm of human intervention, variously varying, occurring, crossing, uniting, cross-cutting, inclining, intruding, dipping or cutting. Moreover, an absence of prior citations implies — explicitly to members of the scientific community — that these are observations made with the researcher's own eyes.

### 3.8.2 Generic integrity within the Field Account: "Discoursal moves" as further evidence for part-genre status

After examining one case of Field Account embedding, it is hoped that a picture of the part-genre as a cohesive, discourse unit is beginning to emerge. As we have seen thus far, it has a clearly defined onset, announced by a shift in general-to-extremely

specific thematic focus, changes in verb tense, mode, and the emergence of what are apparently field-related linguistic markers. Moreover, once the author has moved out of the scene-setting introductory Geological Setting section into field results, he never returns to the general background of the preceding subsection. This would further imply that the presentation of field results is not merely backgrounding 'introductory material', but possesses its own distinct set of rhetorical purposes, and thus, its own generic integrity.

The Field Account also has a clear ending, leading into other rhetorically distinct subsections, here 'Sampling and analytical techniques' (see Text 3.2 below). In other articles of the corpus, the cut-off is also signaled by the onset of a petrographical description, chemical analysis or experimental results. However, while the Field Account does have relatively clear beginnings and ends, we must also ask whether it is also cohesively topical in the middle, and whether the field details contained within the Field Account are systematically organized into "discoursal moves" or other conventionalized structures.

What I have observed in the overall corpus is that authors typically use their field data to discuss and evaluate the prior and present interpretations of the regional make-up, structure, and emplacement processes presented in the Geological Setting, as a way of setting up their own 'niche'. In addition, we can observe a back-and-forth movement between the description of their field data and corresponding interpretation. The following passage is the continuation of Text 3.1 above, and begins at the onset of the Field Account (paragraph 3), which here has been renamed 'sentence 1'. Here, sentences 1-5 constitute a description of the field (the full text, containing both Geological Setting and Field Account, can be found in Appendix C).

> "[1]The thickness of individual cone-sheets **varies** between 10 cm and several metres. [2]Individual cone-sheets **can occur** side by side and occasionally **cross** or **unite** to a thick sheet. [3]The cone sheets of Centre II

> **can be grouped** into an inner and an outer suite relative to a large gabbroic ring intrusion which **cross-cuts** the latter (Fig. 1). [4]The outer cone-sheets **are inclined** at angles of about 30° in the direction of the focal point and **[are] intruded** into Proterozoic Moine schists, Jurassic sediments and Tertiary plateau lavas. [5]In contrast, the inner suite **dips** with angles of the order of 70° and **cuts** the igneous ring-dykes of Centre II."

The move to interpretive work in the following sentence (s. 6) is signaled by a number of linguistic elements. We can note, for example, an increase in the number of argumentative linking words as well as a shift in tense:

> "[6]**Thus**, the inner suite post-dates the ring-dykes and **therefore** both cone-sheet suites represent different ages; [7]an older suite with shallower inclination and a younger inner suite with a steeper dip. [8]**Because** the inner cone-sheets dip more steeply towards the common centre than the outer cone-sheets, **it was suggested that** all cone-sheets originate from a centre at one defined depth (Anderson 1936; Phillips 1974), possibly 2 to 3 km below the present land surface in Ardnamurchan (Richey et al. 1930)."

In addition to increasing the number of textual clues to indicate he is evaluating the evidence, the author of this text also uses a corresponding set of field results to support these interpretations and claims. There is, on the one hand, some field evidence that was gleamed from prior literature (e.g., Anderson 1936, Philips 1974, Richey et al., 1930). But especially, as we can see here, this 'borrowed' field data in fact adds to the author's own original field results (s. 1-5), which act as cumulative support for the contention made in sentence 6:

s. 6 'Thus, the inner suite post-dates the ring-dykes and therefore both cone-sheet suites represent different ages…"

In sentence 9, the author continues to evaluate the various evidence by indirectly making reference to his own field research. He insinuates that other researchers' interpretations

are not plausible given ‘a lack of field evidence’, or in other words, that the author has not himself seen this evidence, although he was ‘there’.

> “[9]**There is no evidence in the field for** either spiral or lateral emplacement of cone-sheets as has been assumed by several authors (Jeffreys 1936; Durrance 1967; Hills 1972).”

Finally, in the final paragraph of the Field Account, which begins with sentence 10, the author once again shifts back to a description of the field, this time with even greater geological and petrographical detail, and a justification of his sampling site (s. 12), in order to better frame the following results and understanding of the geochemical analyses. We can note, in s. 14, the curious and perhaps unexpected adverb ‘anomalously’, which would seem to imply a pointed authorial discernment:

> “[10]In the outer suite two composite cone-sheets contain both basic and acidic magmas side by side, with the basic rocks forming the rims and acidic rocks the core regions of each intrusion. [11]A large composite sheet at the eastern coast of Kilchoan Bay (Fig. 1) near Port na Luinge was selected for more detailed investigation in this study. [12]The intrusion has a 40 cm wide dolerite rim on both sides which corresponds in form, mineralogy, dip and elongation to a normal cone-sheet of Centre II. [13]Within this follows a ca. 50 cm wide intermediate transition zone chemically classifiable as an andesite. [14]The felsic core of the intrusion is **anomalously** thick, reaching more than 60 m at its widest part.”

And so, we can appreciate the circular rhetorical interplay between description and interpretation, which are the two primary and overarching discoursal acts that characterize and organize the Field Account.

While in a report of fieldwork, one might expect for the Field Account to contain nothing but descriptive evidence, it can be argued, on the basis of the textual practices seen in the corpus as well as interviews with geologists, that a second move is indeed present in the Field Acocunt. Interpretation is as equally a fundamental and vital part of the observational enterprise, and by extension, the Field Account, as is description, and it

is here that we can begin to understand how geologists might rhetorically and intentionally marshall their field evidence in what they intend to be convincing ways.

While geologists strive to portray an objective and empirical account of what they have seen in the field, the geologists interviewed over the course of this study are quick to point out a number of empirical, but unavoidable, anomalies. First, no geologist will observe and understand natural phenomena in quite the same way as another, and no two geologists will pick up on the same features. Thus, even a 'pure' observation is but a slightly subjective 'interpretation' in the eyes of the beholder and a rhetorical interpretation becomes a means of mitigating conflicting observations. Second, in order to give field observations explanatory value and to avoid accumulating what may otherwise appear as simply trivial facts, the field geologist must also replace his observations into the frame of current disciplinary understandings of terrestrial mechanisms, a shared set of possible "scenarios" to explain the Earth's formation and continuing deformation. He must thus position his findings within a more local, community network of claims, or against what others have said, either about the same region, or about similar structures in other regions, and discuss how they confirm or disconfirm what the author is claiming based on his field observations. Therefore, the selective presentation of findings is, itself, constrained by interpretation. As a consequence, the descriptive task of the Field Account is not mere description, but constitutes a powerful rhetorical tool for persuading peers that the researcher is not only making credible observations about the field, but also knows how to frame these observations within reasonable frames of interpretation and communal knowledge. In other words, he must demonstrate that he merits the "attribution of field competence" (Rudwick 1985).

### 3.9 The construction of the Field Account: A constellation of strategic options as 'traces of professional field presence'

To this point, then, we have seen the overarching discoursal structure of the Field Account in terms of its intertwining and inseparable descriptive and interpretive tasks, or basic discoursal 'moves'. But we might also ask whether there are not other tasks that need to be accomplished within the Field Account, a set of 'sub-tasks', as it were.

To answer this question, we might turn to what geologists themselves have to say about the field-writing task. As geologist informants have remarked, a well-written field description provides a view of reality that makes the account appear as if no other 'reading' could be possible (O. Merle, pers. comm., 1999). And thus, while the Field Account intends to be a 'straightforward' description of the field, it is in which the way the author handles this description that he does or does not succeed in convincing his peers that his interpretation of events is the most plausible. Therefore, while the Geological Setting presents confirmed discourse community truths, the Field Account provides a different sort of truth, or one that hinges on the empirical observations of geological features made by the geologist in the field which, because they have been 'seen', are proffered as categorical, non-negotiable assertions.

Because the most important details of this account must be presented quickly while retaining the most persuasive impact — geology writers have little textual space for constructing this account, consisting of somewhere between 8% and 11% of the entire article — we would anticipate that field writers draw on a set of "discreet strategies" (see Ducrot 1973) that economically allow the author to assertively present his 'definitive' version in a short amount of time. To do this, the Field Account in fact possesses a discreet set of traces found concomitantly within several sets of feature-clusters and moves, all jam-packed within even a single sentence.

This space crunching illustrates fieldwork's demoted status in the field of geology, but does not erase the complex back-and-forth movement between authorial

positioning and the descriptive task, nor the ongoing tension between acknowledging community concerns and the giving of results. It is here then that we can find a number of rhetorical 'sub-tasks' realized by a set of discreet and strategic, low-lying clusters of features, which work to concretely set the author in the field, respond to suspected peer concerns, place the field on center stage, confirm community membership, persuade colleagues of particular "truths", and perhaps even do a bit of self-promoting.

While the primary tasks of describing and interpreting are perhaps, then, something one might teach to geology students as their way into the discipline, it is clear that for senior and more experienced researchers, the rhetorical construction of the Field Account also depends on the presentation of this data in a way which anticipates how readers may react. The success of a Field Account would, at least at first glance[xviii], therefore appear to be contingent upon the writer taking up these "internal" issues by strategically manipulating the features of the discourse occurring 'below the surface', so to speak.

### 3.9.1 Methodology

Within the corpus, I have identified a number of elements, or clusters of features, that appear to tacitly foreground the task of competence building. Because having physically been in the field is a basic precondition to gaining recognition, these clusters of features allow the author to show he was there and to present himself as the "doer" of his own fieldwork. By giving the author the means for describing his field data as a straightforward representation of natural reality, they also enable him to make it appear "obvious" that this is the way it works. Moreover, these features respond to the author's need to demonstrate his membership to his research community by displaying his knowledge of its concerns, to persuade the research community of the clarity of his interpretations, and to sometimes even give himself a promotional boost or say something

about himself. Because the frequency of the textual activities that materialize within the Field Account hinge upon whether or not the author was truly in the field, and because he must also always work within a frame of collective field practices, these clusters of features will be called 'traces of professional field presence'.

A delimited set of such traces has been identified using a "qualitative content analysis" such as it has been described by Altheide (1987), Huckin (2002), or Salager-Meyer (1998), whereby I have manually examined each of the texts from the corpus of 103 field-based research articles. From this, I have constituted a targeted corpus of 65 articles that effectively contain a Field Account. This targeted corpus of Field Accounts consists of 67,758 words. I have counted only those words that appeared to functionally act in the sense outlined above (i.e. the rhetorical construction of the field observation), therefore excluding non-content based or contextually unmeaningful words, such as articles, connectors, or conjunctions. I also did not include metadiscoursal comments about the argumentative structure of the paper, nor did I count verbs, except for those that demonstrate researcher discernment (e.g., 'the rock *appears'*, 'the structure *indicates'*). The reason for not having focused my analysis on verbs is that the technical details of the geological field endeavor are contained elsewhere than in the verbal repertoire, which consists essentially of relatively general, non-technical verbs (Dressen and Swales 2000).

In the corpus, I have discerned a set of systematic thematic patterns whose occurrences extend throughout each text across the entire targeted corpus. In contrast to 'quantitative' content analysis, which uses statistical models based on key words, the 'qualitative' content analysis (Altheide 1987, Salager-Meyer 1998, Huckin 2002) adopted here uses a more interpretive identification process. The explicit choice of this somewhat old-fashioned approach to text linguistics, in a time of corpus linguistics and statistical programs, is motivated by the very nature of the texts themselves. Because the categories of traces identified here are inherently thematic and functional in nature, classified best by what they accomplish in the text rather than by their grammatical, linguistic or

discoursal features, it is uncertain whether a complex search for key words or concordances would have been able to identify them, for the identification of their function at times depended on situating the traces within their surrounding contexts and comparing their use across a number of texts and contexts.

The features of these traces have therefore been variably characterized as grammatical and linguistic categories (e.g., noun phrase constructions, verbal modifiers and adverbs, pronouns), some types of metadiscourse, mathematical representations (e.g., measurements), or sentence-level structures, and these are variously organized into three overarching categories (see Table 3.4 below).

Finally, I have established the normative trends of these functional traces of professional field presence and description, by (1) calculating the token sum of each trace within each article, based on the total number of words of each article. This was done so as to establish a basis of comparison between and across different text exemplars, which are of widely different lengths. Next, (2) I have calculated the traces' average frequency of occurrence across all journals, so as to establish conventional patterns of use within and across disciplines. And finally, (3) since my ultimate goal here is to provide a genre account of what is 'typical' about these texts, I have also established the standard deviation of use for each trace. All results from this analysis can be found in the tables in Appendix D.

By determining what falls outside the range of standard variability, we can identify what are 'atypical' (i.e., unconventional) uses of a trace within a particular text. In this way, it is proposed that the comparison of the habitual and less-habitual uses of these traces can be used as a method for describing differences in authorial strategies seen across different text exemplars, either for the same author or among different authors, perhaps revealing fleeting moments of self-representation or of particular research stories. This will be discussed in more detail in Chapter 5 (see section 5.7).

As discussed above, the conditions of the "discreet context" imposed on the Field Account's particular writing task seems to cause the writer to draw simultaneously on a variety of strategies at various levels to accomplish a complex set of purposes within his text. The Field Account, thus conceived of as a "constellation" (see Schryer 2001) of such subsurface rhetorical and persuasive needs, would be a shifting space whose strategically oriented clusters of "discreet optional traces" could be manipulated by the author in the written construction of his particular research story. A first attempt at accounting for the elements of such a "constellation" of strategic intentions and their accompanying discoursal options can be seen below in Table 3.4.

Table 3.4 'Traces of professional field presence': a set of strategic discoursal options used in the Field Account

| | |
|---|---|
| **I.** | **Strong authorial implicature in the Field Account** |
| a. | Agential statements of activity in the field |
| b. | Evaluative adjectives and adverbs |
| c. | Interpretative comments coupled with field observations |
| **II.** | **A disguised account of research activity in the field** |
| d. | Nominal indications of activity |
| e. | Verbal indications of activity |
| f. | Metric, angle and direction measurements |
| g. | Locational adverbs and prepositions of researcher movement in the field |
| h. | Metadiscoursal references to visual data |
| i. | Geographical location markers |
| j. | References to own prior field publications |
| **III.** | **Demonstration of research community-based professional expertise** |
| k. | Nominal and adjectival descriptive qualifiers of the field |
| l. | Geological age |
| m. | Petrological, structural and laboratory qualifiers |
| n. | Technical verbal adjectives and participles |
| o. | References to others' fieldwork |

These strategic optional traces group together into three overarching categories. We have, from least to most frequent: (1) a strong authorial implicature in the research

account whereby the author draws overt attention to his role as the field researcher, (2) 'disguised' indications of researcher activity in the field whereby the author signals his field doings but without necessarily drawing explicit attention to himself, and (3) a demonstration of research community-based professional expertise, which allows the author to "display" his detailed disciplinary and "community-based" knowledge by using appropriate metadiscoursal cues as well as an explicitly field descriptive and disciplinary terminology.

### 3.9.2 Strong authorial implicature in the Field Account

The first category of strategies we will examine here are those instances where the author draws the most explicit attention to his research endeavors, intellectual engagement and activity.

**a. Agential statements of activity in the field**

The author draws attention to himself in part by 'fronting' references to his person and his research group in the sentence. This is found in the use of more or less overt traces of researcher activity, as seen for example in the first person plural pronoun 'we' (examples 1-4 below) or in the plural possessive 'Our + [a noun]', as in 'our study' or 'our samples' (example 5 below). I have also included more muted, yet still unequivocal, instances of self-reference where the author refers to his research activity in a passive voice, main verb as in 'The region was mapped in detail' (example 6) or as the hidden but obvious recipient in a phrase often introduced by expletive 'it' (example 7). Interestingly, it is in this last type of trace that the authors from the corpus may most frequently signal difficulties encountered in the field.

This first category, then, attests directly to the actual presence of the researcher in the field, with the author representing himself and his co-workers as the grammatical

subjects, subject possessors, or as the undeniable demoted or hidden agents of the main verbal activity.

1. "**We collected** samples at two sites: a recent road-cut through a small hill at the village of Malemba in the southwestern Congo (4°20'S, 12°25'E; 300 m altitude) and a gold prospecting trench cut into a lateritic surface at Larafella, Burkina Faso (11°32'N, 2°47'W; 250 m altitude)." [GCA-Br]

2. "In this study **we collected** samples from B-horizons of fossil soils with evidence of strong chemical weathering (iron and calcite mobility) because **we wanted** rocks which would most likely contain clay minerals of pedogenic rather than detrital origin." [GCA-St]

3. "At outcrop scale **we observe** contradictory shear sense indicators (i.e., both dextral and sinistral shear criteria). **We also find** cylindrical folds, sometimes strongly transposed, with subhorizontal (Fig. 9B) or vertical axes associated with steeply plunging stretching lineations." [GA-Ma]

4. "Because the block is exposed high in the near-vertical wall of a glacial cirque… **we have not been able to examine it closely**" [JP-So]

5. "However, **our** synthetic secondary fractures make a greater angle with respect to the fault-zone boundary (approximately 30-60°)." [JSG-Do]

6. "An additional 11 faults **were surveyed** over approximately half their trace lengths, and a further 22 faults **were** only **surveyed** in the region close to the tip." [JSG-Ca]

7. "This depositional system was actively degrading, so **it is often difficult to distinguish** individual soil profiles in a sequence of pedogenically altered rocks." [GCA-St]

**b. Evaluative adjectives and adverbs**

Another way in which the author is directly implicated in his text is through a set of evaluatives, such as judgment-marking adjectives and adverbs, by which he marks his personal discernment. In a word, this is a type of evaluative trace that further clearly signifies that it is the researcher alone who has made the observation *because* he has been in the field. We thus find "on-site enthusiasms" occurring as adjectives or adverbs of opinion (examples 8-9 below). We often also find here unusual or unexpected words,

which set the author's strategy apart from standard norms of use (as in 'fortuitously' or 'dramatically'; see also example 10). These, then, are cues that imply that only the researcher could make such an observation (examples 11-12).

While these traces can be true personal opinions, as in "The footwall exposes a *superb* or *spectacular* fold axis...", they attest especially to the actual physical observation of the field, the researcher's own evaluation and discernment, and his own judgment in describing the field. Through these traces, field structures and rocks are directly subjected to interaction with the researcher, resulting in individual descriptive variations. We might therefore say that the "presentation of natural reality" is here most strongly seen as something that is undeniably human constructed.

8. "The CFS is **extremely well exposed** along most of its strike, but **exceptional exposures** occur along the south and east sides of the Sierra Cabrera basement high, to the west of the town of Carboneras and in the La Serrata ridge (Fig. 2)." [JSG-Ke]

9. "The greenstones are massive, but **excellent** in situ pillows were recognized in the northeastern par of the area, and **less obvious** pillow structures were noted as float along the southern edge of the map (Fig. 3)" [GCA-Er]

10. "Most ijolite outcrops reveal such a **bewildering** range of textures that there is little doubt..." [JP-Ha]

11. "The faint internal layers are **apparent only** when seen from a distance and in the **most favorable** light." [MM-Na]

12. "Other chemical features... point to an **undoubted** affinity with A-type granitoids..." [JSG-Ch]

Another type of evaluative identified in the corpus are those descriptive adverbs that, while they lack the overtly judgmental weight and explicit intellectual engagement of the previous category, undeniably indicate a process of authorial discernment, a fortiori because they co-occur with the other traces identified here. It is in this context that they take on a clearly significant status of indicating field presence, for one

understands the author to be describing what he has seen in the field *in comparison to* other features. These traces thus constitute further 'proof' that the author is fashioning a description based on what he, himself, has seen.

Similarly, these are adjectives and adverbs of authorial discerment that may offer mitigating circumstances, such as 'comparable', 'usual', 'abundant' or 'pervasive', and often summarize a highly visual description in one discerning phrase such as "*intensely* fractured" or "the gneisses are *riddled* by little-deformed veins". While these traces are clearly not personal opinions, per se, they are nonetheless overtly cognitive discoursal options as they indicate a process of *evaluation*.

While this set of seemingly 'banal' traces might appear to be a more marginal member of this first category, which is supposedly marked by strong authorial implicature, they occur approximately three times more often than judgment-marking adverbs and adjectives. This suggests that the strategy of comparing features and evaluating the nature of the evidence, as a more discreet approach than boldly stating an opinion, is more frequently adopted by authors as a means for constructing their credibility through description.

13. "Thin, wispy (~2 cm in thickness) dykes that have a gradiational contact with leucogabbro are located stratigraphically below the pegmatoid… Oxide-rich ferrodiorite outcrops are **commonly** banded…" [JP.Mi]

14. "The shape and 'taper' of the displacement profiles... is **highly variable**. In some cases, the shape is **approximately** linear..." [JSG-Ca]

15. "The granodiorite **usually** occurs between the gabbro and the granite, and the quartz diorite is **mainly** scattered as enclaves within the granite (Fig. 2e, f)" [LI-Xu]

**c. Interpretive comments based on field observations**

Personal discernment also occurs in whole interpretive phrases where the author takes a stand on his field description by working a plausible and self-evident

interpretation from it. Here he exposes his interpretation of reality by using natural facts collected in the field as support for his claims, and presents his interpretation as the natural and evident prolongation of this field description. The onset of this discoursal move is quite often signaled by a cluster of coinciding features, such as existential 'it' or 'there' coupled with a judgment-marking adjective (e.g., 'enigmatic', 'evident'), by argumentative markers (e.g., 'although', 'however'), and by a marked increase in what is an otherwise non-existent modality in the straightforward Field Account (e.g., 'may', 'might').

It is also at times indicated by a shift in verb tense (from present to past), as well as by a set of verbs that imply human intellectual engagement (e.g., 'imply', 'suggest', 'base', 'interpret', 'indicate', 'argue'), introduced most often by the field object acting as grammatical agent. Thus, the interpretation is posited as if following directly from natural objects observed in the field. Yet with this type of trace, the author still moves away the expected objective and empirical observation of the natural world and into the realm of argumentation and of convincing one's peers of "observed truths".

16. **It is enigmatic that if the pyroxenite** was indeed the earliest intrusive component, **it is never seen intruding** the mafic fenites that are extremely well-exposed along the..." [JP-Ha]

17. "**It is evident, therefore, that the growth of graben-bounding pairs of faults** is not always coupled in such as way as to maintain complementary displacement profiles." [JSG-Ca]

18. "**The uppermost mantle section** (depth 0-2 km), defined by a marked change in the high temperature stretching lineation from oblique to normal to the ridge, **was interpreted on the basis** of geological, microstructural, and mineral chemical data to contain a **significant proportion of trapped melt of minerals** formed from migrating melts (Suhr, 1992, 1993; Suhr and Robinson, 1994). **This is seen in the field as** cpx-rich harzburgite (sample TM 1454), lherzolite (TM 599), or, in one observed case, several meters of plagioclase lherzolite (sample TM 1524)." [GCA-Ba]

19. "**Based on these top indicators, and stratigraphic coherence** displayed by the mapped lithologies, **the region is interpreted to consist of** major, upright,

greenstone-cored synform on the east, and westward, overturned, west-verging metasediment-cored antiforms and greenstone-cored synforms." [GCA-Er]

20. "**These pegmatitic granites imply** filling of late-stage fractures by residual melt drained from the largely crystalline leucogranite by percolative flow. **Although the pegmatitic granites may suggest** local volatile phase saturation, **we have not observed miarolitic cavities to confirm this.**" [LI-Pr]

21. "**Preservation of the euhedral calcite rhombs argues** against this **interpretation** and **suggests that** the calcite was..." [JP-Co]

22. "In fact, **D3, D4, and D5 stretching lineations and fold axes**... **refute** core-complex models." [TECT-Ae]

### 3.9.3 A disguised account of research activity

A second category of traces is thematically organized around giving indications of the research activity itself. Here the author tells us about his activity and begins to give us more explicit descriptions of the field, but in a way which 'disguises' his own participation in the observational and descriptive task. As references to the author become more discreet, the optional traces themselves become more neutral and begin to take on their aura of appropriate authorial distance from the object of study, given their focus on concrete activities, their non-frontal placement in the text and their corresponding association with other clusters of features. In a sense, then, these are the research account's "doing the work details" and are generally presented without personal attachment.

**d. Nominal indications of research activity**

There is, for example, a whole range of activity-based nouns, often processual, that refer to the author's own research endeavor, but that remain largely 'unclaimed'. Here we can find such nouns as 'data', 'evidence', 'mapping', 'assessment', 'interpretation', 'discrimination', 'observation', 'examination', 'determination', or 'investigation', or 'implication', etc.

23. "Clear **field evidence** indicates that..." [JP-Ha]

24. "Taken together, the **field observations** indicate that..." [CMP-Bl]

25. "The high relief in the area of up to 900 m allows **assessment** of...." [MM-Gr]

26. "The ubiquitous **evidence** for extensive brittle deformation... may have two important implications." [JSG-Ke]

27. "In zones lacking D4 strain, **discrimination** between D3 and D5 is not possible." [TECT-Go]

28. "The faults in **the study area** are all segmented and are organised into a densely faulted array with an average spacing of a few hundred metres." [JSG-Ca]

29. "**During field studies** in the summer of 1990, **two of the authors** (**J.A. Dons and J. Naterstad)** identified a series of melt-bearing breccias overlying the Gardnos Breccia." [GCA-Fr]

**e. Verbal indications of research activity**

There are also a number of verbal strategies which disguise the researcher's activity, some tucked far away into the sentence as seemingly 'non-essential' verbal adjectives far distanced from the main verbal phrase (examples 30-31). Another type of discreet verbal strategy consists of a passive verb, where the demoted agent might be 'imagined' to be the researcher, but whose true identity remains ambiguous (example 32). The field geologist author further disguisedly indicates his participation in observing and interpreting field structures and rock features with epistemic modality, or the oft-discussed 'hedge' (e.g., Myers 1989; Salager-Meyer 1994 1996; Hyland 1996).

While it is commonly argued that hedges are a tool for indicating 'politeness' or are used as 'face-saving' tactics (Brown and Levinson 1987), one can also consider them to be indirectly indicative of researcher activity in a discourse characterized most by its "researcher invisibility" (Salager-Meyer 1998). As such, they can be read as additional proof of having been there, for they constitute a softening of the impersonal, empirical fact (examples 33-35).

One final verbal strategy is to present oneself as some elusive actor or 'invisible' demoted agent-recipient only remotely alluded to by the choice of words (examples 36-37). While it is clear that the demoted agent in example 32 is human, although we cannot discern whether it is the researcher himself or someone else, in this final type the geological features seem to supplant the human observer.

By using these types of traces, the researcher signals his activity, but does so more or less "in passing".

30. "In pavement outcrop within a discrete body of leucogranite NW of the town of Phillips (Fig. 2, locality 1), the leucogranite exhibits a sheeted structure in which structurally concordant screens of pelitic schist occur (Fig. 3a–c), and sheets of leucogranite and pegmatitic granite occur within the pelitic schist outside the **mapped** contact of the body." [LI-Pr]

31. "At the scale of the shear zone, as well as in 80% of the **studied** outcrops, the sub-vertical plane lineations are gently dipping towards the south or the north (Figs. 6, 8)." [GA-Ma]

32. "No feldspar, magnetite or mica **is ever found** in these ijolites." [JP-Ha]

33. "Tourmaline commonly is a minor phase in pegmatites of the southern Tin Belt. Locally, however, relatively troumaline-rich assemblages in different zones of the pegmatite bodies can be observed.... In some places (e.g., Davib-Ost), tourmaline crystals **appear** fractured and partially replaced by quartz (Fig. 2d)." [CG-Ke]

34. "To the north of the shield volcano there are several small basaltic cinder cones which **appear** similar in age and size to the [MGVF cinder cones] (Hasenaka & Carmichael 1985, 1987)." [CMP-Bl]

35. "Metre-scale, low-amplitude, lineation-parallel ridges and grooves — megacorrugations — are also observed (Fig. 2), and these **seem** to be typical features associated with other normal faults." [JSG-Do]

36. "The good three-dimensional exposure **afforded by** the incised banks of the Tshweneng River shows that..." [CG-Ja]

37. "Locally, several centimeter thick late pegmatite veins consisting of aenite, perthitic alkali feldspar and eudialyte and other Na-REE-Zr-silicates **trace** the main fluid pathways..." [JP-Mi]

**f. Metric, angle and direction measurements**

We also find references to the author's explicitly disciplinary, 'geologistic' activities in the various metric, angle and direction measurements he has made in the field:

38. "The structure is pipe-like with near-vertical sides on the **north** and **west**, plunging outwards steeply on the **south** and **south-east**..." [JP-Ha]

39. "Two preexisting joint sets occur in this area, oriented **N040°** and **N135°**." [JSG-Ca]

40. "Vague schistosity in the metavolcanic rocks and more distinct compositional layering in the metaclastic units coincide in attitude, strike **north-south** to **N30°E** and dip steeply, predominantly **to the east**." [GCA.Er-97]

41. "Fresh pyroxenite is only found as xenolithic blocks **<1 m in diameter**..." [JP-Ha]

42. "Maximum displacements on the mapped faults range from **1.5 m** to **150 m**, and their trace lengths range from **108 m** to **6584 m**." [JSG-Ca]

43. "The most recent volcanic activity (10,000–300,000 a) in the plains sub-province produced small (**< 100 m** from base to summit and usually **< 1 km** in diameter) cinder cones, maars, and lava shields surrounded by aprons of overlapping basalt sheets which are circular to elliptical in plan and commonly approximately **10 km** across." [GCA-Pr]

**g. Locational adverbs and prepositions of researcher movement in the field**

There are also 'locational' adverbs and prepositions that indicate where and how the structures occur in the field. They denote an actual 'taking-in' of the field with a specialist's eye, and translate either the researcher's visual and microstructural appreciation of the location of features in relation to others (example 44), or the researcher's own movement from point to point in the field (examples 45-46).

44. "Drag effects **along** the margins of the intruding magma have given rise to small-amplitude (~10-30 cm), asymmetric folds **in** the foliation..." [JP-Ha]

45. “The peridotite **over**lies high-grade gneisses and marbles… **along** an essentially low-angle brittle thrust marked by extensive brecciation discernible **over a distance of** up to 100 m **away from** the context.” [JP-Vd]

46. “The metamorphosed WTrPz mafic-ultramafic suite + associated coeval sedimentary strata in the Sawyers Bar area can be traced without interruption **along** strike both to the northeast and south.” [GCA-Er]

**h. Metadiscoursal references to visual data**

The researcher’s active participation in the descriptive and observational task is also implied through metadiscoursal reference to his maps, cross-sections and the like, which he has drawn based on his own field mission. Field geologists, of course, must necessarily visually communicate what they have observed (cf. Rudwick 1976). As such, in-text references to visual representations and these visual representations are an important form of field description, where the author’s involvement is assumed. An absence of citation of other authors would indicate “I drew this.”

47. “Carbonatite is the youngest component of the complex and forms a heart-shaped outcrop some 1.1 $km^2$ in diameter, eccentrically positioned, somewhat east of centre in the complex **(Fig. 2)**.” [JP-Ha]

48. “A representative structural map of a Type B tip **is presented in Fig. 6**.” [JSG-Ca]

49. “The pluton is about 6 km in diameter and consists of several roughly-concentric instrusive phases of hypersolvus, subsolvus, and transsolvus granites (Nassif and Martin, 1991), most of which contain large xenoliths and roof pendants of the host rock **(Fig. 1)**” [GCA-Sa]

50. “The most spectacular slickenslide surfaces **have been schematically represented in Fig. 2** and **they are shown in the photographs of Fig. 3**” [JSG-Do]

**i. Geographical location markers**

Then, there are also indicators of the geographical location where the author has done his fieldwork. These traces locate the structures in the field through a specific geographical reference point the author has identified in relation to the structure, and so,

we might have the name of a nearby town or well-known geological structure. There is evidence that this type of trace frequently co-occurs with other types of traces, and as a result authors use them to construct complex noun phrases where, by naming a structure in relation to a geographical location (we can recall the "Koolen gneisses" of the preceding section), they appear to lay territorial claim to the structures studied in their region:

51. "From the **town of Rjukan** to the **village of Tuddal** (Fig. 1), an excellent section (ca. 1000 m) is exposed." [CG-Br]

52. "**Bushveld** gabbros underlie the hills of **Spitskop** and **Mare**." [JP-Ha]

53. "This set of mapped faults forms the basis for compilations of displacement and length statistics for the **Canyonlands** fault system." [JSG-Ca]

54. "Due to deep erosion, the **Gardnos** structure exhibits no obvious circular topographic features." [GCA-Fr]

**j. References to own prior field publications**

Finally, there is also a muted "textual" confirmation of this research activity in the field, through sentence-final, "parenthesized" references to the author's own prior fieldwork missions, as seen in the following examples from the corpus (examples 55-57). Not only do these references as published documents act as a short-hand for field description, allowing the author to say much while saying little by relying on past discourse (examples 55-56), they also seem to present him with an increased authority to counter-claim (example 57).

According to one geologist, reference to one's own previous fieldwork constitutes definite proof of field presence, although the present article may not make direct reference to it. Because one fieldwork mission may inspire three to five separate articles, some more closely focused on the field mission than others, not every article written will have the same amount of explicit field detail. If questioned about the curious "absence"

of explicit fieldwork markers in a particular article, although the analyst knows full well that the author "was there", the author might very well respond that "yes, it [the field report] is there, but it's in the bibliography" (N. Arnaud, pers. comm., 1999).

55. "To the south of the Sierra Cabrera block the CFS branches out and forms three separate fault zones; the Polopos, Sopalmo and Colorados faults (Fig. 3) **(Hall, 1983; Keller et al., 1995)**. [JSG-Ke]

56. "The structure is currently defined by a roughly circular area, about 5 km across (Fig. 2), within which discontinuous bodies of Gardnos Breccia occur within fractured but more coherent crystalline basement rocks (**Dons and Naterstad,1992; Naterstad and Dons, 1994**)." [GCA-Fr]

57. "Strauss & Turner (1950) considered the pyroxenite to be the earliest intrusive component of Spitskop and presented a photograph. However, Strauss & Turner provided no petrographic description of this dyke, and as this exposure no longer exists **(Harmer 1992)**, the relationship... remains unclear" [JP-Ha]

### 3.9.4 Demonstration of research community-based professional expertise

Finally, turning to those discoursal options that occur most frequently in the corpus, we can also note that it is in this set of optional traces that the field and the disciplinary community who frames its study make their most concrete appearance. The field is described here in total abstraction from the "actor researcher" who is implicitly the most distanced from the object of research, for nowhere in this set of options does he make his field presence visible. Therefore, these traces are less traces of field *presence* as they are of field *professionalization*, as such allowing the author to paint a picture of himself as a competently trained field geologist who knows when and how to wield the appropriate terminology and discourse. In other words, that the terminology he is using belongs to the appropriate professional domain.

**k. Nominal and adjectival descriptive qualifiers**

The majority of these are the basic optional traces around which the description of the field hinges, for they relate the observed 'geological rocks and structures' whose description conforms to the norms of a specific research community terminology. This is seen for example in the use of nominal and adjectival descriptive qualifiers of the field, as well as in modifying complex noun phrases, all of which use a stock set of terms one might easily find in any geology dictionary.

58. "**Ijolites** form the **bulk** of the **silicate phase** of the **complex**." [JP-Ha]

59. "**Dips** of the (steepest) **part** of the **monoclines** vary along their **fold axes**, with the **maximum bedding dip** usually occurring close to the '**brittle' tip**." [JSG-Ca]

60. "Above the **opx**-bearing **mantle rocks**, poorly to unlayered **dunites**, **cpx-dunites**, **wehrlites**, and **plagioclase**-bearing **wehrlites** occur with a **thickness** up to 500 m." [GCA-Ba]

61. "Most **ijolite outcrops** are extremely **heterogeneous**..." [JP-Ha]

62. "Beyond the **tip**, the **main fault plane fissure** usually links into a **series** of **smaller vertical fissures** via **networks** of **extensionally** reactivated **joints**, or more rarely, **newly** formed **fracture surfaces**." [JSG-Ca]

63. "In addition, a **stone-line** of **quartz cobbles** extends downslope from the **quartz vein**, essentially following the **interface** between **soils** and **saprolite**." [GCA-Br]

**l. Geological age**

The author also appeals to the shared background knowledge his readers will be assumed to possess, making it more relevant to a wide geological audience by using both adjectival qualifiers of geological age (example 64), as well as verbal and adverbial indications of geological activity, quite often coupled with the past tense (examples 65-66).

64. In the study area, there are three principal **early Devonian** granite plutons." [LI-Pr]

65. “**Miocene-Recent** left-lateral oblique-slip (transpressional) was accommodated along the CFS (Hall, 1983; Rutter et al., 1986). ... The CFS **has been active from** the **Burdigalian to** the **present,** with fault movement being laterally transferred, and accommodated, by different branches of the CFS **through time** (Keller et al., 1995).” [JSG- Ke]

66. “In the Potwar Plateau, the Siwaliks contain abundant paleosols, **spanning a wide age range**.” [GCA-St]

**m. Petrological, structural and laboratory qualifiers**

He also appeals to a more methodologically-restricted, domain-specific audience by using petrographical rock composition qualifiers (examples 67-70), structural and tectonic qualifiers (examples 71-72) or nominals and qualifiers of laboratory-based activity (examples 73-74). It is a further demonstration that the author is well versed in specific community norms and that he knows how to play the basic “disciplinary game” by using its key terms; these traces further enable him to display his disciplinary knowledge and community membership to his peers.

67. “Ijolites from these occurrences have **euhedral, square to hexagonal-sectioned grains of nepheline set in compact aggregates of fine- grained, prismatic to acicular aegirine-augite**.” [JP-Ha]

68. “The sample locality is situated in the Torome river bed (Fig. 1b), which presents on an outcrop length of several hundred meters, an interlayering of **amphibolite, garnet-bearing leucosome, granitic gneiss, migmatitic garnet-bearing paragneiss and migmatitic orthogneiss**.” [LI-Ch]

69. “In the field, the granites are distinguished based on **the darker grey color, finer grain size and higher modal biotite content of the granodiorite**.” [LI-Pr]

70. “Disseminated accessory minerals are **fluorite, topaz, columbite, Zn-Mn-Nb-rich ilmenites, zircon, xenotine, thorite, cassiterite, wolframite,** and **monazite**. **Violet** and **colourless fluorite** veinlets are locally abundant. Local degassing breccias and pegmatite nests are common.” [MM-Ab]

71. “**Riedel-oriented second-order** faults (NE–SW trending) also occur, and can be as common as the **P-oriented** ones.” [JSG-Ke]

72. “These folds and the presence of syntectonic veins in both **XZ** and **YZ sections** of the **finite strain ellipsoid** indicate that the shear zone was submitted to an east-west subhorizontal shortening.” [GA-Ma]

73. “Keller et al. (1995) have proposed that P-oriented shears are characteristic of oblique-convergent transpressional fault zones, and may be used as diagnostic of this deformation regime in the field. **Physical clay-box experiments (see below) were carried out to test this assumption.**” [JSG-Ke]

74. “Granites are the dominant rock type in Pingtan complex, and consist of **quartz (30-33%), K-feldspar (50-57%), plagioclase (An 30-36; 10-15%) and minor amounts of biotite**.” [LI-Xu]

**n. Verbal adjectives and participles denoting technical relationships**

Introducing another type of trace, we can recall that the verbal repertoire used to describe the field is mostly general and non-technical in nature (Dressen and Swales 2000). Instead, the geologist demonstrates his ability to describe the field’s *technical* relationships by using a set of verbal adjectival and participial qualifiers found in the text that describe the interaction between the different rocks and structures observed in the field. These traces indicate both the relationships between the different geological objects as well as their processual state (i.e., how they came to be, or their geological history). The agents and demoted agents of these verbal qualifiers are necessarily geological structures.

75. “[Ijolites] form well-**defined sheeted** intrusions sometimes **having** finer-**grained**, **‘chilled’** margins.” [JP-Ha]

76. “The **stepping** sense of the fissures preserves the *ca* N020° strike of the main fault, whilst fully **exploiting** those joints **oriented** favourably for reactivation...” [JSG-Ca]

77. “On the northern side of the structure, these melt-**bearing** breccias are, in turn, **overlain by** a tick sequence of **alternating** coarse- to medium-**grained**, siliceous clastic sediments.” [GCA-Fr]

78. “[The Pickle Crow assemblage] comprises **pillowed** and massive basalt flows (sampled for this study) with **intercalated banded** iron formation.” [LI-Ke]

**o. References to other researchers' field publications**

Finally, the author responds to his research community's concerns and practices by acknowledging what other researchers have reported and contributed to the bed of communal knowledge, and by situating his own field research within a myriad of references to others' published field research results.

79. "Extant geologic maps (e.g., **Moench and Pankiwskyj, 1988**) show the Philips pluton to comprise northern and southern lobes, in which granite contains screens of metasedimentary rocks." [LI-Pr]

80. "Many faults in the study area developed as one of a pair of graben-bounding structures **(McGill and Stromquist 1979)**." [JSG-Ca]

81. "Since this entire section is plastically deformed, their origin by cumulate processes or by in situ transformation of mantle rocks (e.g., **Nicolas 1989**) is difficult to resolve. The rocks are referred to as transition zone rocks (**Nicolas and Prinzhofer, 1983**)." [GCA-Ba]

### 3.9.5 Concluding evidence for part-genre status: Generalized tendencies of professional field traces across disciplines

By showing us the discoursal traces presented above in a descending order of frequency, Table 3.5 below also gives us some idea of the quantity of text in the Field Account that is typically allocated to field descriptions in the three disciplines, as well as to each type of trace (for the specific analytical results for each article, see Appendix D). The first set of numbers (Column A) presents the average number of occurrences of each trace, while the second set (Column B) posits that number against the overall average number of words from each discipline's set of texts, so as to facilitate comparison between disciplines.

As we can see in Table 3.5 (row 1), all the traces together usually occupy only about half of the Field Account (55%, 47% and 48% for Geochemistry, Petrology and Structural Geology, respectively). Among these, the most common are the field and

professionally descriptive Category III-traces (33%, 32% and 31% for the three disciplines, respectively), while Category II-traces marking researcher activity in the field have a frequency on the order of 12-14%. And finally, Category I-traces are the least frequent, accounting for only around 5% of the entire text. First and foremost, these numbers suggest that geologists use the professionally-situated field descriptive adjectives and adverbs from Category-III as the centerpiece for their accounts, in effect making it a "rock-centered discourse" (Dressen and Swales 2000) where the principal movers and shakers are the rocks and geological structures being studied.

What these numbers also suggest is that it is at least somewhat important for the author to signal his own participation in the field mission, but he does so mainly by muted references to his research activity (Category-II traces) rather than by drawing overt attention to his person or to his thoughts (Category-I traces). Importantly, Category-II traces allow the author to make reference to his own methodological activity as further elements in the construction of his descriptive account, and the traces of this activity provide undeniable empirical support for his observations, thereby further demonstrating that the work has been done — by the author.

And finally, quite peripherally, given its overwhelming 'infrequency' as a strategy, geological writers will use Category-I traces to establish an explicit and active ownership of their research activity and intellectual investment. It is in the frequency variations sometimes apparent in this first category that one may have the distinct impression that the author at times seems to be emphasizing his personal role in his research endeavor more strongly than other writers. Given the veritable paucity with which they occur in the text, even a small handful more than what is typical for these traces is sufficient for making the text appear to be operating outside of the norm. We will return to a more detailed examination of this observation in Chapter 5, when we will view the results of a series of text-based interviews, where authors talk about themselves and their textual practices.

Table 3.5 Trace frequency among the disciplines

| | A. Average number of tokens per text | | | B. Amount of text typically occupied by the trace | | |
|---|---|---|---|---|---|---|
| Overall number of traces | 279.78 | 269.89 | 814.14 | 0.55 | 0.47 | 0.48 |
| Category I traces | 26.94 | 22.21 | 64.86 | 0.05 | 0.04 | 0.04 |
| Category II traces | 63.5 | 65.84 | 230 | 0.12 | 0.12 | 0.14 |
| Category III traces | 170.89 | 181.84 | 519.29 | 0.33 | 0.32 | 0.31 |
| Overall number of words in FWR | 515.39 | 569.63 | 1686.14 | | | |
| Optional field traces | GC. | P. | SG. | GC. | P. | SG. |
| CATEGORY I TRACES | | | | | | |
| a. Agential statements in the field | 0.22 | 0.16 | 1.75 | 0.00 | 0.00 | 0.00 |
| b. Evaluative adjectives and adverbs | 22.06 | 17.47 | 46.29 | 0.04 | 0.03 | 0.03 |
| c. Interpretive comments | 4.67 | 4.58 | 16.82 | 0.01 | 0.01 | 0.01 |
| Category II Traces | | | | | | |
| d-e. Nominal and verbal activity | 11.72 | 13.05 | 33.11 | 0.02 | 0.02 | 0.02 |
| f. Metric, angle, direction measures | 12.22 | 11.74 | 49 | 0.02 | 0.02 | 0.03 |
| g. Locational adverbs and prepositions | 19.22 | 19.05 | 68.61 | 0.04 | 0.03 | 0.04 |
| h. Metadiscoursal refs. to visual data | 3 | 5.21 | 24.14 | 0.01 | 0.01 | 0.01 |
| i. Geographical location | 13.28 | 13.53 | 47.61 | 0.03 | 0.02 | 0.03 |
| j. Own prior publications | 4.06 | 3.44 | 7.54 | 0.01 | 0.01 | 0.00 |
| Category III Traces | | | | | | |
| k. Nominal & adjectival field qualifiers | 150.39 | 144.74 | 406.39 | 0.30 | 0.25 | 0.24 |
| l. Geological age | 10.39 | 5.42 | 21.04 | 0.02 | 0.01 | 0.01 |
| m. Laboratory & Petrology and | 3.33 | 3.63 | 5.79 | 0.01 | 0.01 | 0.00 |
| Structural/tectonic qualifiers | 0 | 0 | 9.11 | 0.00 | 0.00 | 0.01 |
| n. Verbal adjectives and participles | 18.83 | 23.05 | 63.46 | 0.04 | 0.04 | 0.04 |
| o. References to others' fieldwork | 6.39 | 5 | 13.5 | 0.01 | 0.01 | 0.01 |

One other crucial point illustrated in Table 3.5 is the overall relative homogeneity in trace frequency between disciplines, based on the average overall length of the texts (Column B). This suggests that geologists, even across different domains, share similar

reporting practices, further supporting the contention that there exists an overarching 'part-genre' that governs field-reporting practices in geology. Modern-day genre conventions — here, seen in the Field Account —would thus seem to create a mould for researchers to work in.

Results of the interview data from this study further confirm the empirical observation that field geologists appear to use and globally recognize a set of traces which concretely sets the author in the field. These traces may even sometimes occur as a set of acceptable "coquetteries" (i.e., Category-I traces), as one of my informants put it (N. Arnaud, pers. comm., 1999), in order to give voice to the personal implications of researcher experience.

Nonetheless, we might be particularly struck and even troubled here by the relatively small quantity of each trace at play in the Field Account, and the relatively little room they take up in the text. It will be argued in the next section, however, that while their low incidence underlines the geologist's need for discretion and the 'silencing' of the fieldwork mission, the constellational use of these strategic traces *does* fully confirm the researcher's physical field presence, although occurring but even a few times in the Field Account.

And so, despite the confirmed existence of these traces, it remains obvious that the time spent in the field, with its accompanying labor and hard work conditions, and the human part of the research experience, so to speak, are clearly minimized in today's written Field Account. Much of this experience seems to 'disappear' behind a maze of discrete discoursal traces, in which many of the details of the field expedition appear in reduced form. And yet, although these strategies may at the start seem less apparent than the narrational and author-oriented field reporting practices of times past, we can see that they do concretely exist in a modified form for contemporary field workers who skillfully use them to their advantage in responding to the various tasks set out by this field

descriptive and expertise demonstrating part-genre. The next section looks more closely at how this is accomplished.

### 3.10 A textual analysis of the Field Account

In this final section, we will examine how one author from the corpus uses the sets of optional traces outlined above to construct his Field Account. If we are to take genre to be a constellation of strategic optional traces, such as it has been outlined in section 3.9, it might be useful to first summarize the multiple goals found to be associated with the Field Account. These can be seen as follows:

Table 3.6. A set of goals associated with the Field Account

1. Describe the field
2. Interpret the structures
3. Use field description as an obvious basis for interpretation, as if to say "These are the facts"
4. Position findings within communally shared knowledge, citing prior publications and using appropriate terminology and concepts
5. Build credibility by proving that the researcher has been in the field, and knows how to carry out appropriate and viable fieldwork
6. Take possession of the field by laying territorial claim to it
7. Create a 'personal niche' by confirming and disconfirming current beliefs using own field observations and interpretations
8. Sometimes, self promotion
9. Establish a basis for competence recognition

The strategies used for accomplishing the Field Account's goals are enacted in the list of discoursal options seen in Table 3.5, the "traces of professional field presence". These traces variably cluster together with other traces, so as to achieve the author's intended rhetorical effect. The fact that an author strategically picks and chooses from these traces to construct his account, depending on his particular situated needs, would explain why exemplars of a same genre of texts are marked by linguistic variability and why there can be no superficially imposed "template" on the writer of a particular genre.

In the handling of these trace clusters, we can see the author position himself in relation both to his own concerns as well as his research community, resulting in an account which moves in and out, and around the description of the field. Equally, we can see how the author's field observations are essentially fashioned into the rhetorically sensitive description and community embedded interpretation we find in the end account, where each word has its role to play in the construction of the Field Account as a constellation of strategic optional traces.

Let us now examine the Field Account published in "TECT-Wu", some 2810 words in length. The title of the article is "Yadong cross structure and South Tibetan Detachment in the east central Himalaya (89°-90°E)[xix]". The following excerpt from the Field Account (286 words in length) is taken from a passage where the authors relate the results of a prior field mission, before moving on the presentation of new field results in the following section.

The situated context for the Field Account is provided by two sentences taken from the Introduction as well as a segment from the Geological Setting section, entitled 'Description of the Yadong Cross Structure'. As one introductory element to the story, we might note that 'STDS' refers to 'South Tibetan Detachment Structure', and that 'YCS' refers to 'Yadong Cross Structure', the two main structures the authors of TECT-Wu have investigated (see the final sentence of the Geological Setting).

In the introduction, the authors situate the importance of their findings for us within the framework of what has previously been found in the region, and what they assume their findings will contribute to the bed of collective geological knowledge. Notably, we learn that they are the first to describe the geology of the area, and that in their estimation, this description has important implications for understanding some of the major structures in the *entire region*.

**Introduction**

> "… **In this paper, we describe the STDS** in the vicinity of 89° east longitude in the Himalaya, together with a related feature in the area called the **Yadong cross structure (YCS)**. **The bedrock geology of this area has not previously been described but potentially provides several new insights** into the nature and along-strike variability of the STDS, the nature of along-strike discontinuities in the deep structure of the Himalayan orogen, and possibly the temporal relationship between granitic magmatism and slip on the STDS.

Further, in the Geological Setting we learn that the door for making this contribution was left open by previous researchers (i.e., Burchfiel et al. 1992), who failed to adequately define the research area, doubtlessly due to the political and diplomatic problems encountered at the time with Chinese officials in the region (Tibet), making access by non-authorized, especially non-Chinese, researchers to certain research sites nearly impossible. Nonetheless, one geology informant, whose domain of expertise is also on Tibet and South China, has indicated that Burchfiel et al. (1992) is considered to be an edifying article of significant importance on the region's geology (N. Arnaud, pers. comm., 2001). For the authors of TECT-Wu to have new information, then, is clearly not negligible.

**Geological Setting**

> "… **Burchfiel et al. [1992]** … suggested that the strike separation across the YCS might be as much as 150 km. They identified and described the STDS in the field a short distance east of the YCS at Wagye La. **However, because of border-access restrictions in existence at the time, they were unable to examine the YCS directly in the field, nor were they able to determine directly from field observations where the STDS projects into the TCS from the west. Thus, while noting the regional**

> **significance of the YCS, they were unable to determine whether** it was a strike-slip fault cutting the STDS, a transfer fault on the STDS, or some other structure or combination of structures. Similarly, the actual strike separation across the YCS could not be determined.
>
> In 1992, 1994, and 1995, **the International Deep Profiling of Tibet and the Himalaya (INDEPTH) project undertook geophysical investigations along the Yadong-Gulu rift**, aimed principally at characterizing the deep structure of the crust beneath the region [**Zhao et al., 1993; Nelson et al., 1996**]. As part of this effort, reconnaissance field geological investigation of the bedrock adjacent to the southern Yadong-Gulu rift was **undertaken in hope of locating and characterizing the STDS in the area and determining the nature of the YCS**."

The lack of access to the area that hindered Burchfiel and al. is an obstacle that the authors of 'TECT-Wu' were able to get around, thanks to their collaboration with a team of Chinese geologists. The effects of having local Chinese contacts on the field mission's successful outcome cannot be underestimated, and we get a hint of the significance of this collaboration in the following statement from the Acknowledgments:

> "We are particularly grateful to **Zhao Wenjin**, leader of the Chinese INDEPTH team, whose many efforts in China have made INDEPTH possible."

This recognition is further indicated in the second paragraph of the Geological Setting where we find a description of the corresponding field missions, as well as a reference to a resulting co-authored paper written by the Chinese team leader of the effort, Zhao Wenjin, and an American counterpart, K. Nelson, which appeared in the journal *Nature*. The second citation refers to a paper published on the same topic in the journal *Science*.

> "In 1992, 1994, and 1995, **the International Deep Profiling of Tibet and the Himalaya (INDEPTH) project undertook geophysical investigations along the Yadong-Gulu rift,** aimed principally at characterizing the deep structure of the crust beneath the region [**Zhao et al., 1993**; Nelson et al., 1996]."

It can be noted in passing that even the choice of journal for these first publications implies that the authors — as well as the journal editors and reviewers — consider the importance of their field findings to warrant a prestigious recognition from the geological and even scientific community:

The collaboration between Western and Chinese geologists is not simply a one-way street in favor of the American geologists K. Nelson, W. Kidd and M. Edwards (see endnote 7 in Notes to Chapter 3 for a list of all authors), whom my informant has identified as being an internationally recognized and influential research team in the region. As he described, it has become a typical practice for the Chinese geologists to then be invited to the United States for a certain length of time, hosted at the American (or other Western) geologists' universities; we can thus note with some interest that of the three Chinese authors of the paper, Y. Yue and J. Li are from the Chinese Academy of Geology Sciences in Beijing. However, at the time of publication, J. Li is cited in a footnote as already having relocated to the Department of Earth Sciences in Syracuse, New York. This is also where the first author, C. Wu, had set up house at the time of submission, and is furthermore the home university of K. Nelson. It is likely that C. Wu is also a visiting scholar for the time being, and will probably return to China (N. Arnaud, pers. comm., 2001).

All of the contextualizing information presented here is important for understanding the strategic choices the authors make in the presentation of their Field Account.

The excerpt of the text we will see below is a part of the Field Account that describes the southern Yadong-Gulu rift, which the authors consider important because its features allowed them to both locate and characterize the elusive "STDS" as well as to determine the nature of the "YCS". It occurs as one unit, spatially organized as a subsection separate from the description of the other studied areas, such as the Zherger La Detachment (sentence numbers have been added).

**3. Southern Yadong-Gulu rift**

[1]The southern Yadong-Gulu rift is composed of the Pali and Duoqen valleys, which together extend approximately 90 km in a north-northeast direction across the southern Tethyan Himalaya (Plate 1). … [2]Both valleys are bordered on the east by a rugged, anomalously north-northeast trending segment of the High Himalaya dominated by Mount Chomolhari (7313 m),

> referred to subsequently as the Chomolhari range. [3]The Chomolhari range is the geomorphologic expression of the YCS. [4]The western foot of the range, bordering Duoqen and Pali valleys, is marked by an en echelon set of active high-angle normal faults which, in aggregate, we refer to as the Chomolhari fault system (CFS) (Figure 2). [5]The CFS is evidenced by conspicuous scarp cutting moraines, hanging glacial valleys, and triangular range-front facets [Armijo et al., 1986]. [6]These features are evident both in the field and on thematic mapped images. [7]West of Duoqen valley, generally east-west striking Paleozoic and Mesozoic sedimentary strata of the Tethyan belt are exposed. [8]These strata are succeeded southward by a diverse assemblage of granite, granite gneiss, schist, phyllite, and locally marble, the bulk of which we assign to the Greater Himalayan belt. [9]These strata are readily observed along the west side of Pali valley and along the two principal north-south roads through the region, which converge near the southern edge of the map area at Yadong (SW corner of Plate 1). … [10]Our field observations together with the regional mapping by Gansser (1983) suggest that the crest of the Chomolhari range is underlain by high-grade metamorphic rocks and granites of the Greater Himalayan belt. [11]To the east in Bhutan, the Himalaya are similarly underlain by greater Himalayan belt strata, with local outliers of Tethyan belt strata preserved above [Gansser, 1983].

Upon reading, this text appears to be a relatively typical example of a Field Account in structural geology. However, while this text exemplar is relatively standard, one does also have a clear sense that the principal author, or authors, is authoritatively describing his field research, in order to firmly situate this research against what others have done.

In our first reading of this Field Account, we can see that it effectively identifies and localizes the general structures of the area (sentences 1-3), and introduces the “CFS”, or the Chomolhari fault system, (sentence 4), which is the principal structure they then generally describe using their field observations (sentences 5-9). Crucially, it is the field observations made along this fault system that allow them to posit an important “finding”, with implications for understanding the whole regional geology (i.e., the greater Himalayan belt strata). Not an unimportant grouping of field data, by any means then.

But what allows us to consider that this text is ‘authoritative’? In addition to this primary level of analysis, it is suggested that another level of rhetorical and linguistic analysis is also possible here, which will allow us to uncover the strategies by which the authors of this text accomplish yet “another” set of goals, which are undeclared. In effect,

this secondary, or underlying, set of goals is communicated only implicitly to the inner members of the community of field geologists, using the constellation of strategic optional traces (Table 3.5) to carry out their "hidden agenda". Namely, this agenda is to imply to readers that the authors are also competent, professional field researchers, which in turn serves to further bolster the strength and acceptability of the new claim being made here.

I am therefore proposing that there is a duality of discoursal structure in the Field Account, as seen in the interplay between salient and silent markers. One part of this structure aims to overtly achieve a number of "publicized" goals: to describe, interpret, claim and counter-claim. The second part of the structure, on the other hand, works 'behind the lines', or 'under the text', so to speak, in order to influence the way in which the first set of goals is received, and ultimately, whether or not competence recogntion is to be granted.

While the acts of describing, interpreting and claiming are textually salient acts, the underlying agenda, which entails rhetorically establishing one's competence, credibility and authority, is less so, and we can thus consider to be hidden, or silent, act. It is silent because its expression involves the use of a range of low-level linguistic and discoursal traces that are organized in rhetorically discreet and undeclared ways. It is striking that the way in which these traces play out as an ensemble is the very way in which they do *not* immediately appear to the reader of the text. Instead, they seem to constitute a series of "background cues", by which the author's message is ultimately communicated, although the reader may not always be cognitively aware of the strategy, nor of the author's intent to persuade him of his credibility and competence.

It is here that we can see one illustration of the 'rhetoric of understatement' outlined in section 1.3.3. It is a complex strategy whereby the author may choose from a range (constellation) of expressions to imply a particular proposition, e.g., his 'undoubted' competence and authority, to insiders, but without having to make his

intentions overt and take responsibility for them (Ducrot 1973) in a time where drawing explicit textual attention to oneself as a scientific researcher has become taboo ('conventional impositions of personal modesty', section 1.3.2). Nonetheless, given that the ultimate rhetorical goal of the Field Account is generated by the historically documented need for gaining "attributed field competence" (Rudwick 1985), this is a task that has become as widespread as it is necessary, resulting in the conventionalization of the strategies identified in the Field Account part-genre.

Turning to a pointed and detailed analysis of the text, we can see that Category III– traces are the most common and are of standard frequency, making up 0.28 of the paragraph (80 tokens from 286 total words; the average for category III traces in structural geology is 0.31, with a standard deviation of 0.04; compare with Appendix D). These traces, then, constitute the core around which the author constructs his account, and are densely packed throughout sentences 4-8, as well as in sentence 10.

However, the author also keenly demonstrates an active field activity using Category II– traces, and here the use is somewhat atypical, making up 0.23 of the paragraph (66 from 286), while the norm for structural geology is 0.14 (with a standard deviation of 0.02). We can note that he principally uses 'Geographical location markers', to do so (29 tokens from 66). While these traces alone do not constitute clear evidence that the author has been in the field, it is noteworthy that he often attaches them to 'Nominal qualifiers of the field', which suggests that he is somehow taking possession of the field, especially when this construction occurs in conjunction with a number of quasi-metadiscoursal statements (s. 2, 4, 8 below). I have taken these to represent agential statements of activity in the field, given that they directly describe the authors' field mission and that the naming of these geographically marked geological structures therefore seems to procede the writing of the article, on the assumption that they are not mentioned in the literature.

s. 2: "… dominated by Mount Chomolhari (7313 m), **referred to subsequently as the Chomolhari range**."
s. 4: "The western foot of the range, bordering Duoqen and Pali valleys, is marked by an en echelon set of active high-angle normal faults which, in aggregate, **we refer to as the Chomolhari fault system (CFS)** (Figure 2)."
s. 8: "… the bulk of which **we assign to the Greater Himalayan belt**."

Although the last example most likely refers to a geological structure named long before the article was written, the authors have clearly coined the terms 'Chomolhari range' and 'Chomolhari fault system', especially given that, following standard practices, they replace the term by the acronym 'CFS', which they then use throughout the remainder of the Field Account.

In addition to the high number of 'Geological location markers', the authors also give clear indications of their field activity through a variety of 'Direction measurements' (s. 4, 7-9), a couple of locational adverbs (s. 9) and references to visual data, as well as a very small handful of nominal and verbal indications of fieldwork:

s. 5: "The CFS is **evidenced by**…"
s. 6: "These features are evident both **in the field** and **thematic mapped images**."
s. 10: "**Our field observations** along with the regional mapping by Gansser (1983)..."

And so, it would seem obvious thus far that the authors have already situated themselves in the field, and that consequently, one crucial piece in the construction of their credibility has been laid. However, it is hardly sufficient to have merely been in the field, for one must also use the description of the field's features as an obvious basis for interpretation. This is a task the author spends the majority of his time constructing, and we can see that the central part of the paragraph (especially sentences 4-8) contain most of the Category III-traces, using primarily 'Nominal field qualifiers' and 'Technical

verbal adjectives'. Here, the author seems to be 'presenting the facts', as further suggested by the use of the relative verb "be".

s. 4: "The western foot of the range, bordering Duoqen and Pali valleys, **is marked** by an en echelon set of active high-angle normal faults which, in aggregate, we refer to as the Chomolhari fault system (CFS) (Figure 2)."
s. 5: "The CFS **is evidenced** by conspicuous scarp cutting moraines, hanging glacial valleys, and triangular range-front facets [Armijo et al., 1986]."
s. 7: "West of Duoqen valley, generally east-west striking Paleozoic and Mesozoic sedimentary strata of the Tethyan belt **are exposed**."
s. 8: "These strata **are succeeded** southward by a diverse assemblage of granite, granite gneiss, schist, phyllite, and locally marble, the bulk of which we assign to the Greater Himalayan belt."

These facts are further embedded within a disciplinary frame of shared communal knowledge, as seen by the author's use of other professional-based traces, such as 'Geological age' (s. 7), and references to other researchers' publications (s. 5, 10, 11).

Using a number of Category-I traces, the author mitigates the presentation of natural reality, making it more amenable to being believed by qualifying this straightforward description with well-placed comparative adjectives and adverbs of evaluation (s. 1, 7, 11 below). He also strengthens the force of his descriptive claim by using marked judgments (s. 2, 5 below). These traces of the author's moderation of natural facts accompany both his descriptions of the field as well as the indications of his activity.

s. 1: "… which together extend **approximately** 90 km in a north-northeast direction across the southern Tethyan Himalaya (Plate 1)."
s. 2: "Both valleys are bordered on the east by a **rugged**, **anomalously** north-northeast trending segment…"
s. 5: "The CFS is evidenced by **conspicuous** scarp cutting moraines…"
s. 7: "West of Duoqen valley, **generally** east-west striking…"
s. 11: "To the east in Bhutan, the Himalaya are **similarly** underlain by…"

Furthermore, the author personally vouches for these observations, making them appear more real by saying they are 'evident' or can be easily observed' in the field:

s. 6: "These features are **evident** both **in the field** and **on thematic mapped images**.
s. 9: "These strata **are readily observed** along the west side of Pali valley and along the two principal north-south roads through the region, which converge near the southern edge of the map area at Yadong (SW corner of Plate 1)."

And finally, the author finishes the section by posing his interpretation of events, resting squarely on the concrete field observations he and others have already described:

s. 10: "**Our field observations** together with the **regional mapping by Gansser (1983) suggest that** the crest of the Chomolhari range is underlain by high-grade metamorphic rocks and granites of the Greater Himalayan belt."

In accordance with the research reporting conventions of modern scientific discourse, we have thus seen a text where the field appears as the centerpiece of the account, here reinforced by use of the most frequent traces from Category III (see Figure 3.6 below). Gathering around this "description of the field" (sentences 4-8), we find other clusters of features at work (Category II traces), showing us that the author is the master of his domain: he possesses the description, he was in the field to see for himself. Therefore, he is the best describer of the field. He demonstrates the veracity of his description with unquestionable field data such as measurements, sketches, locale names, descriptive relationships, indirect references to himself and his research team, newly constructed interpretations, all lying upon the support of references to prior field missions. He also takes pains to evidence his knowledge of his professional community, fully versed in its conventions such as when to cite others and what this means, how to wield the terminology, how to imply to others that he did his own fieldwork, when to add in all the descriptive "tidbits" that could only come from his own eye so as to bolster a claim, and also at times draws attention to himself by simply and strategically *not* referring to anyone else's work. And finally, he does manage from time to time to say in an outright manner "I was there" and "what I saw was truly exceptional".

The ways in which these markers cluster together to create the intricately crafted construction of the Field Account thus lend support to an earlier claim made at the beginning of this chapter, namely that the Field Account, with its inherent overt and hidden goals, is rhetorically constructed not so much by a series of progressive moves, but through the manipulation of a non-stepped constellation of mostly sub-discoursal clusters of optional traces, which do not occur in any fixed order. Instead, we have seen an "interweaving" of the different categories of traces, demonstrating a continuous movement between different strategic intentions: establishing one's field presence through description, interpretation, drawing attention to one's research activities, judgments and interpretations, and demonstrating professional expertise, such as how to prioritize findings and how to present them in terms of community concerns and its discourse. Only when the sub-tasks embodied within the "hidden agenda" have been completed may the author hope to provide the rhetorical basis for being granted credibility, competence and authority recognition from his peers, at both the editorial and review stage, as well as after publication.

The way in which these various traces of field presence, description and professional expertise might structurally and constellationtionally cluster together, such as we have seen in Table 3.6, might be conceptually represented in Figure 3.6 (below).

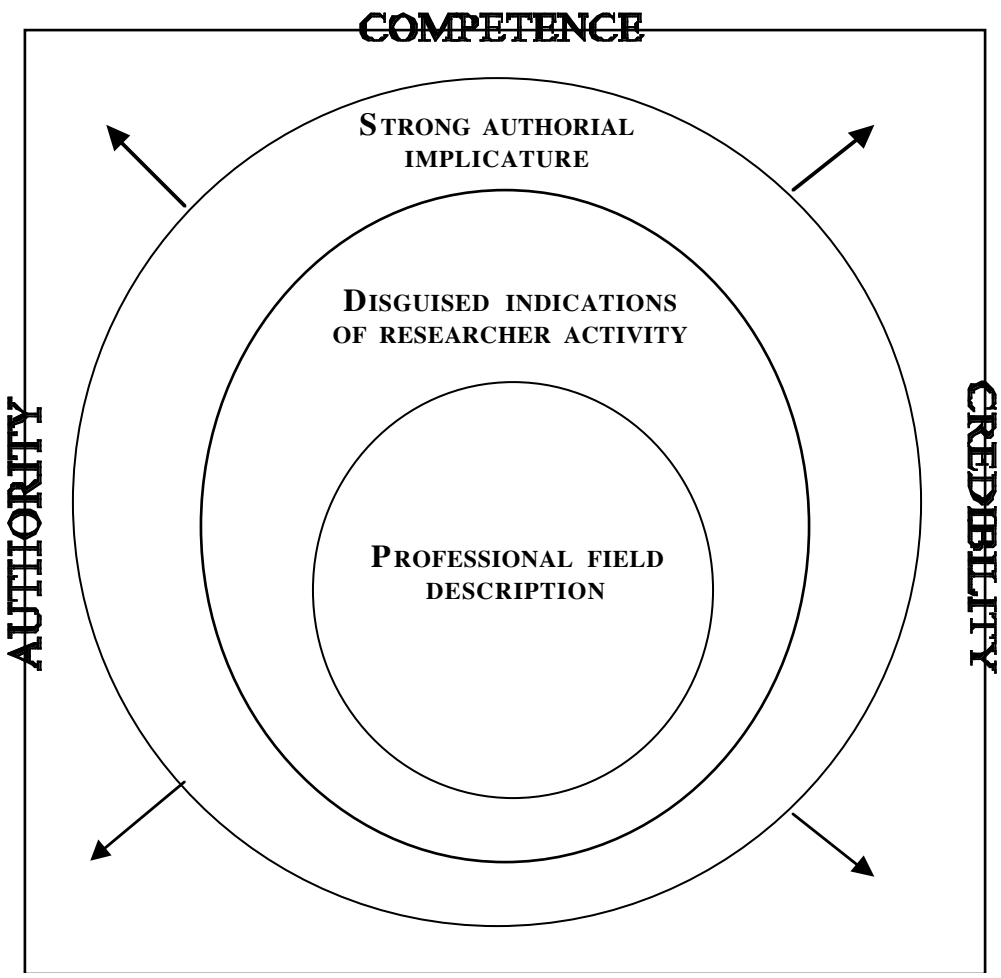



Figure 3.6 The Field Account as a constellation of strategic optional traces, allowing for the construction of competence, credibility and authority recognition

### 3.11 Textual expressions of silence

And so, we might ask, what do the textual saliencies identified here tell us about textual *silence* in geological field writing? The beginning of a response has been attempted, by elucidating our understanding of the grammatical and discoursal variations in authorial discretion and expansiveness, and by defining what have today become the typical and conventionalized expressions of the field geologist writing in his discipline. Given the context for field writing, such as it was described as a socio-historical analysis

in Chapter 2, it is proposed here that these salient textual strategies in fact constitute an elaborate range of expressions and strategies available to the writer in order to imply what he can no longer overtly say: “I was there”. As a conventionalized but ever-dynamic instance of a rhetoric of understatement, the traces described here play a meaningful role in the system of silences described in Chapter 1 (section 1.3), by allowing writers to “privately” signal to others in a restricted circle of field geologists, the sorts of things modern scientific discourse supposedly no longer allows for. As such, it constitutes a community-internal need to ‘transgress’ certain institutionalized silential boundaries: (1) Do not talk about the research activity and methodology, and (2) Do not talk about oneself. As we have seen, this transgression of institutionalized silential norms is an imperative for the community of field geologists, for this range of implicit strategies allows them to provide the arguments they need to build and obtain recogition of their competence, authority and credibility among their immediate peers.

However, this is but a starting place, and in the quest for describing geology’s system of silences, we must now turn once again to the establishment of geological field writing’s ‘context’. While it is clear that studying the occurrence of saliencies is one starting place for identifying and explaining their contextual absences, focusing attention entirely on written texts as a source of information about silences cannot account for other silences whose traces *do not* appear in the text. These are the disciplinary motivations for textual silences, the conditions of which are unclear to the disciplinary outsider but are certainly not invisible to the accomplished field researcher. For example, the time actually spent in the field, at times measured in months or even years, is reduced to a handful of sentences in the research article. The mound of measures and observations made over weeks and months often find their sole outlet in some visual or table, or are eliminated entirely. Conventional practices silence the various obstacles met in the field, such as the fact that a cut can disappear in a river or behind a tree, as well as obvious difficulties in logistics or funding. A hierarchization of results is imposed, and for reasons

of economy, authors are forced to relate only those results which are capable of 'convincing' peers, even if in the researcher's own eyes, they are hardly the most important. As a consequence, not only do the majority of field results never make it into the research article, but the supposedly empirical representation of natural phenomena is reduced to authors' publicity needs. All this translates into varying levels of textual authorial presence – and silence – in the Field Account.

We have seen in this chapter some of the rhetorical and discoursal constraints within which the field geologist must maneuver when writing up a report of his field data, and we can very well imagine that this is a framework where it has become largely unnecessary, indeed even irrelevant, to recount the joys and woes of doing field research. This, in part, causes the mutedness and discretion discernible in today's published version. The other factors that play a role in the distillation of the research account is a subject we will investigate further in the next chapter, which looks at the transformation of the fieldwork observations made during one fieldwork mission into a number of "recontextualizations" (Linell 1998).

## Notes to Chapter 3

[xiii] Definitions of geological terms may be found in the Notes to Chapter 4.

[xiv] Details of all the geology articles cited in this illustrative discussion may be found in Appendix B, part I.

[xv] The details of this corpus can be found in Dressen and Swales (2000).

[xvi] The complete corpus is given in Appendix B, part III.

[xvii] We can recall from section 2.7 the dismay expressed by a prominent French field geologist, François Ellenberger, in response to an Anglo-Saxon research group's failure to acknowledge the (French) author they took their geological map from. Publishing a map as one's own, without giving proper recognition, is tantamount to credit stealing.

[xviii] Of course, only practicing field geologists can truly answer the question of what makes a text successful, or rhetorically convincing. While a first attempt has been made

here to gleam background information about what my informants consider to be good writing, and to associate linguistic features with various rhetorical tasks, it is clear that there is more work to be done here. One might, for example, extend the study to include an examination of reader response to a set of text features, in order to determine whether geologists use the features in significant ways to assess the effectiveness of a particular Field Account (see Paul et al. 2001).

[xix] The complete reference is: Wu, C., Nelson, K., Wortman, G., Samson, S., Yue, Y., Li, J., Kidd, W., Edwards, M. 1998. Yadong cross structure and South Tibetan Detachment in the east central Himalaya (89°-90°E). Tectonics, 17(1): 28-45. The entire field account can be found in Appendix E.

# CHAPTER 4

# FROM FIELD OBSERVATION TO NOTES TO TALK TO TEXT: RECONTEXTUALIZATIONS OF THE FIELD ACCOUNT IN GEOLOGY

## 4. Introduction

As we have seen previously in the Fieldwork Reports discussed in Chapter 3, writers of field accounts in Geology research articles make use of a subtle and limited set of linguistic "traces" designed to suggest that the field description is based on the investigative activities of the author. These traces provide "proof" of physical presence in the field, at least to geology insiders. Physical presence in the field is one basic condition for the rhetorical establishment of the author's competence, credibility and authority as a field geologist.

It is precisely through examining the conditions surrounding the strategic re-construction of the field account that we may begin to have an idea of what is actually absent from its later more public versions; in other words what it might be "possible" for field geologists to report on in the research article, but do not (see section 3.1), as well as the contextual background which shapes what actually does and does not get reported. It is precisely these background, "contextually appropriate" features of a genre as they are shaped by their time-and-space situatedness (Giddens 1984) that may provide us with further information about the silences whose traces *do not* appear in the text. These are the "disciplinary motivations" for textual silence, as reflected in institutionalized discourses.

While what appears as the final field account may seem to have been immutably distanced from the original field mission through a Bakhtinian process of "hidden dialogicality" (Wertsch 1999), there is evidence that expert-insiders to the discipline do reconstruct these tacit reformulations by drawing on their background knowledge, including their own comparable field experiences as well as their command of discourse community practices, rhetorical positioning and genres. And so, we can gain access to at least part of the dialogical process by pinpointing the knowledgeable users' background assumptions about the contexts for genre production. We can gain further access to this process through insiders' knowledge about and compliance with what is considered contextually appropriate in a research community, shaping the published research account and making it conform to "stabilized-for-now" conventions (Schryer 1994).

Therefore one of the main points of focus in this chapter will be to delineate by means of an extensive, longitudinal case study how, where and why pieces of the field account have been omitted. The focus of the analysis here is therefore on how and where, in the telling of the field account, bits of the story have been left out, leaving us with the muted and human-devoid discourse of the contemporary field account. In so doing, we will also examine the points in the process from field account to publication where we see the contextually appropriate being clarified through explicit discourse community input at its various stages.

These stages are what Linell (1998) has described as "recontextualizations", which occur when some part or aspect of a genre of texts or discourses is taken out from its original context and fit into a new one, namely into another text or discourse genre replete with its own particular use and environment (p. 145). In particular, recontextualization refers to the processes through which a speech event or a text is produced and mediated through its relationship with prior discourse (see Fairclough 1992) notably by an intentional, and by extension, strategic embedding of text within new contexts. Therefore, recontextualization is to be taken not as mere 'representation', but

instead as ‘re-presentation’ and ‘re-production’ with inherent connotations of individual creativity (Bourdieu 1991).

When parts of one genre are relocated within another through recontextualization, they are most often subjected to various textual changes, such as simplification, condensation, elaboration or refocusing (Bernstein 1990), shifts in self-presentation, role-relationships or legitimization of authority (Sarangi 1998), or reversals of figure-ground relations: what is central in one text may become peripheral in another, and vice versa (Goodwin and Duranti 1992). It is precisely these textual changes and the possible reasons for the changes at various points in the transformation process that will be the focus of the inquiry here.

### 4.1 The transformation of one field study from Geology into text and image.

In order to illustrate how the final fieldwork account can become a “distilled” version of the fieldwork activity, we will look at the transformation of one fieldwork study into its conventional textual and visual forms. All fields to some degree do give boiled-down versions of their research accounts, of course, but geology provides an exceptional window for studying this process in that the published account of the geological fieldwork report is marked particularly by its “silences”, or by its dehumanization and deagentification of man’s encounter with nature. Although Swales (1999) shows some similarities in this regard between geology and mycology, he also shows that in linguistic anthropology this down-playing of the researcher’s activities is much less comprehensive and thus less extreme.

The following analysis examines a number of discipline-embedded generic tasks that provide us with a particularly good opportunity to see explicit community conventions most strongly at work. What will be presented here are the results of a longitudinal examination of the different genres produced by a doctoral student in

Geology, based on his field mission. The four documents come from a French doctoral student, Philippe Goncalvez (see Appendix B, part II), who is from an Earth Sciences department at a French university. Philippe spent two months in the field in the northern part of Madagascar gathering data with his dissertation advisor as part of his doctoral research during the summer of 1999. As the first document, we will see excerpts in French of his field notebook from the field mission, which constitutes the basis for the following transformational analyses.

As a second document, we will look at the conventionalization of his field results into visual representations. Using his many field results, which are found in the form of notes, measures, photos, or different schemas in the field notebook, Philippe has worked and reworked a block diagram, a rectangular "cube" which synthetically and geometrically represents the earth's crust in three-dimensional perspective. It is conceived to show a "bird's-eye view" of the ground from the top of the cube with its sides giving the underlying geologic structure (Bates and Jackson 1984, p. 58), thus displaying the sorts of visual information on which field geologists resolutely depend. This visual representation can also be taken as a generic "text" in the sense that it represents an entire discoursal "unit" and encompasses a set of visual conventions involved in the production of a standardized visual communicative event (see Rudwick 1976).

As a third document, we will look closely at excerpts of an abstract Philippe submitted to a conference about six months after his field mission. In so doing, we will examine how textual conventions, with their visible and not-so-visible features, play on what gets reported on and how in the fieldwork account. Finally, we will also examine a recently submitted journal article, and especially, its Field Account. The chronology of these texts is shown below in Table 4.1.

Table 4.1 Chronology of different texts produced by Philippe

| Time frame | Visible, written disciplinary tasks |
|---|---|
| August 1999 | 1. Field notebook: northern Madagascar |
| September – October 1999 | 2. Reworked version of the block diagram |
| March 2000 | 3. Conference abstract on field results |
| April 2001 | 4. Field Account in published research article |

Through these different documents, we will observe both the transformation of Philippe's field results through its various stages and the role the institution and its community plays in shaping exactly which results are — or are *not* — related to the scientific community.

## 4.2 The field notebook: "Dimanche 15 août, 1999"

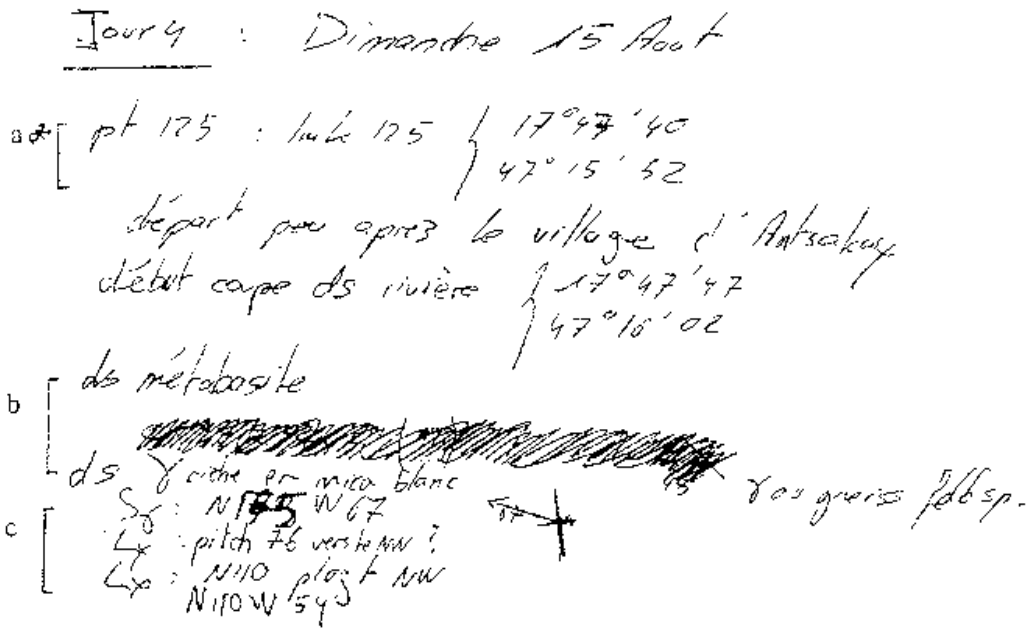



Figure 4.1 Excerpt from the field notebook. Outcrop 125 (August 1999).

The field notebook excerpts which are presented here include the notes and drawings Philippe made during one particular day in the field that proved to be extremely fruitful for his and his advisor's understanding of the regional structures in northern Madagascar. On this day, Philippe and his dissertation advisor began their work on what is noted in the field notebook as outcrop 125 ("Affleurement 125", Figure 4.1a). The position of this outcrop is indicated, as are all of the outcrops noted, using GPS (Global Positioning System), which situates them at 17°47'47 latitude and 47°16'02 longitude. They began their day not far from the village of Antsakay, and worked their way on foot toward the northeast.

Upon arriving at an outcrop, they always proceed according to a conventionally fixed methodology, first observing what types of lithologies are present. At outcrop 125, for example, they found *metabasite, mica, feldspathic gneiss,* and *granite* (Figure 4.1b). Next, they observe whether or not such geological structures as *folds* ('plis'), *boudinage,* or *shearing* ('cisaillements ') are present[xx]. Finally, at each point of observation, they measure the orientation of the planes of *lineation* and foliation, or *schistosity*, noted "Lx" and "Sx", respectively, in the field notebook (Figure 4.1c). Particular attention will be paid to 'boudinage' throughout the account and for this reason, a simple schema of the structure is presented in Figure 4.2 below.

After making these initial observations, they next set out to find the surrounding sections parallel and perpendicular to the first lineation in order to construct a three-dimensional image of the region's tectonic structure. It is this part of the fieldwork which, according to Philippe, "n'est pas toujours un mince affaire." As we shall see, the typical manifestations of these sought-after exposures greatly complicate the task for the field researcher, by disappearing behind a tree, or under a river. The field geologist must thus often be "crafty" in order to reconstruct regional structures.

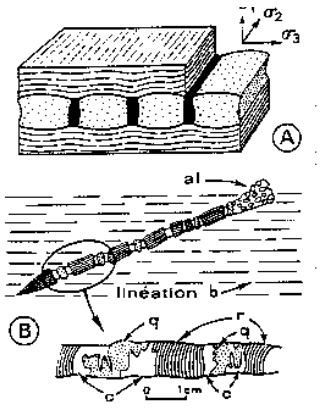



Figure 4.2 Boudinage: A sectioning of rigid strata caused by their being stretched into 'boudins', or elongated and slightly convex prisms which resemble sausage links.

During this day of fieldwork, they noted different outcrops; the first, number 125, very fittingly begins in a river ('début coupe dans rivière', see Fig. 1a). A bit further on, at point 129, things began to fall together a bit more (Figure 4.3a, below). In a "gros niveau de *gabbro*", which is a type of magmatic rock, they discover an outcrop characterized by numerous folds and note the outcrop's lineation, N120, as well as the orientation of the different folds, at one point parallel (N120) to the lineation and at another perpendicular (N50) (Figure 4.3b). In addition, they observe *double boudinage*[xxi], indicated as "boudinage syn aplatissement" in the notebook (Figure 4.3c), which is once again parallel and perpendicular to the outcrop's lineation.

According to Philippe, for these types of structures to be fully observable within one exposure is very rare, and this points to a very specific form of tectonic deformation in the region. Philippe and his advisor have therefore made an extremely important discovery, even if during the course of one of our interviews Philippe confessed that he recognized its true significance only later, as we shall see in the series of comments he makes during our first interview. While Philippe's advisor may very well have understood the importance of the outcrop's structures on the spot, Philippe himself reported having been especially struck by the rarity and "beauty" of the exposures. He

meticulously sketched everything that he saw in his notebook, and complemented these drawings with photos, measures, and a few notes.

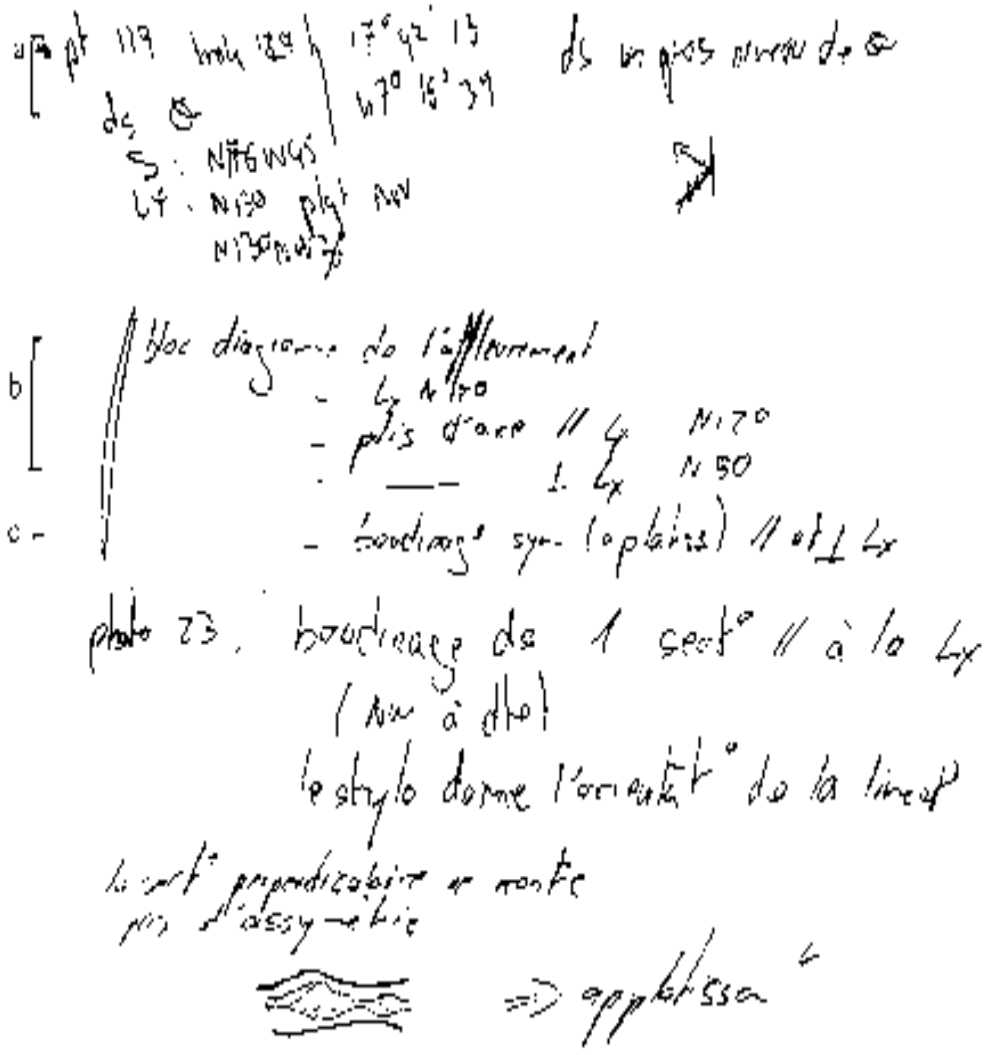


Figure 4.3 Excerpt from the field notebook. Outcrop 129 (August 1999).

So much, then, for a first look at the field notebook. As we can see, its contents are really neither textual nor literary. Quite to the contrary, it is a type of document which brings together a mass of "private" information, exploitable only by its researcher. How to exploit this data is one of the tasks Philippe must learn on the way to his dissertation.

Amongst other things, Philippe must learn to manage and exploit not only his field notes, sketches and photos, but also his visual memory of the site, which supplements the somewhat cryptic notes that have been taken and the few drawings sketched here and there. Over the course of time, he must also learn to "nurture along" the interpretations he brings to his field observations, as well as gain knowledge about the conventional means for communicating field results to his research community. It is therefore very much his own intellectual involvement and growing professional fruition that will allow his field results to be "transformed", so to speak, into a form which is recognizable and accepted by his research community. The traces of this personal, physical, financial and intellectual investment by the researcher are "locked into" the raw field data and they later disappear from the visual representations and written text, which are governed by the conventional omissions characteristic of modern scientific discourse, as we shall see shortly.

### 4.3 From field observation to visual representation

Visuals are highly discipline-specific and as such are embedded within a frame of scientific practice. For Kress and van Leeuwen (1996), they are above all a product of "culture", constructed by a particular discourse community in order to fulfill its communicative needs. Thus, the visual representation form is not truly representative of "reality" for it reflects above all a discourse community's cultural specificity, ideology, and intentions. The form this "cultural reality" takes on is therefore influenced by the needs and interests of the community's social institution within which the images are constructed, communicated, and received.

Lynch (1985) observes this conventionalized communicative function in visuals from Neurobiology, which act in ways very similar to conventionalized textual omissions. Although laboratory experiments are clearly done on living animals, through

various "rendering processes" (p. 38) the researcher purports to paint a picture of reality. Yet, in fact he is reorienting the research account to retain only what he and his research community consider to be the most important aspects. Because the visual "normalizes" the properties of each subject, it is therefore effectively indifferent to and "silences" the fact that

> "a particular animal may have struggled fiercely before going under anaesthesia, that a crisis occurred on whether the perfusing fluid would 'take' during the preservation of the brain during dissection, or that staining of thin sections of the animals' neural tissues created an unusual grainy texture that *almost* led to its being discarded from the corpus of analyzed specimens. The lines on the graph no longer represent rats in their ordinary, familiar mode; they represent measurements performed on methodically processed extracts of the animals' dissected brains" (p. 58).

Geology's own cultural specificity can be found in the way geologists relate to their visuals, what one might call their "conventionalized visuality" (see section 2.3). Observations made in the field are immediately framed within a conventionalized "visual language" (Rudwick 1976), since what geologists see in the field has already been filtered through the "visuality" of communally-shared observational techniques and through their previous study of the visual devices of others. Such "visual" practices are today so commonplace that geologists tend to deceptively treat their geological maps, sketches and diagrams as natural and unproblematic, leaving one with the impression that they are straightforward representations of reality. Yet, in truth the process is tacitly accepted as one that can paint only a certain version of reality. In order to avoid hiding critical information in a fog of detail, the geologist must necessarily communicate a selective, incomplete view of the field (Monmonier 1996). Therefore, visuals such as geological maps, cross- and traverse-sections, block diagrams, projections or satellite imaging are compact, *synthetic* constructions that pull together a multitude of information in an extremely abstract, formalized and theoretical way, allowing geologists to convey

configurations that cannot be adequately expressed through words, numerical or mathematical symbols.

Geology's visual language, by permitting the economical and efficient communication of geological concepts and results, also consists of a certain number of tacit rules and conventions which not only govern the form the visual representation may take but are also strictly regulated by discourse community users, who accept and share an understanding of its conventions. These rules and conventions, of course, must be learned by the newcomer to the discourse, and it is here that we see the active input of the discipline's "culture" and regulating discourse practices. Kress (2001), for example, in examining the high school biology classroom, points to the enculturation process as essential for enabling students to learn not only how to *draw* microscopic views of onion skin like the teacher, but especially, how to *see* like the teacher who in fact works actively to shape their observations into expected frames of reference by warning them in advance at the beginning of a lesson (ex. "[The onion skin] may look like a brick wall").

This same process is found in geology, as well, where students spend countless hours learning to draw maps and especially, to visually describe their field observations in conventionally appropriate ways. So engrained is the visual in the observational process that students are trained from the outset to relate their findings in visual form. The pedagogical consequences of geology's focus on its visual language are two-fold. First, the student's success in replicating an observation made in the field depends on how closely he can make it resemble the conventionalized model. However, failure to do so is explained less by the student being a poor artist than a poor observer, for a poor drawing results from having not having observed "correctly". Second, the advantages of drawing a schema with a only few choice words of commentary, rather than writing a five-page descriptive text, become immediately apparent in the sense that the geologist would have a difficult time achieving the same communicative effect with a long, written

"text", where many important details would paradoxically be lost in the process of "writing it out".

### 4.3.1 The block diagram

Based on the observations made at outcrop 129 (Figure 4.3), we can trace the visualization of Philippe's observations and first attempt at understanding what he has seen in the field as it is expressed in a block diagram. Using the different types of information he has noted in his field notebook, Philippe drew his first schema (Figure 4.4) while still in the field, indicating the various field data acquired so far: fold axes perpendicular (N50) to the outcrop's lineation, double boudinage and the various directional measurements they have made (see Figure 4.3). This first schema is not only a visual description of one of the more important folds he has observed, but it also represents the effort involved in trying to work out the various relations in the field.

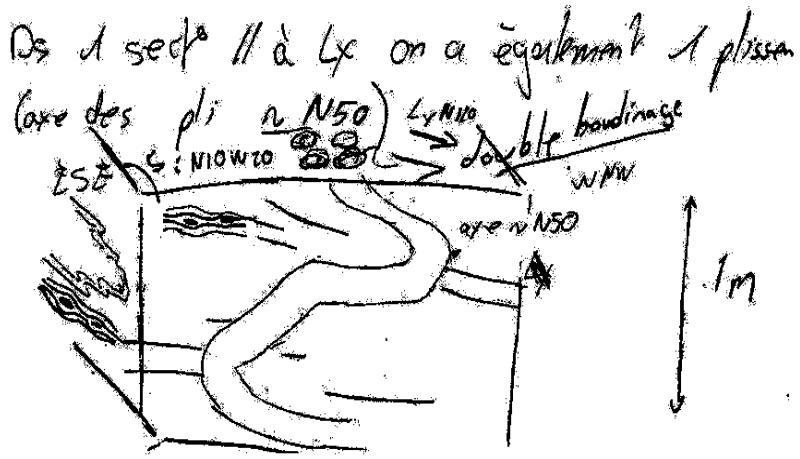



Figure 4.4 First block-diagram, drawn in the field (August 1999)

To be sure, the sort of work required for this visual transformation puts the researcher under the obligation of having actually understood what he has observed in the

field. That same evening as Philippe set to work at his advisor's suggestion, he was anything but sure about how the different structures actually fit together. Instead, he approached the task by putting his observations together in a simple schema with a good deal of textual support (ex. "Dans première section parallèle à la linéation, on a également un plissement (axe des plis environ N50) ou perpendiculaire"). He textually noted the double boudinage and sketched some structures.

As he noted during the course of one of our first discussions,

> P: Ce n'est qu'après avoir fait toutes ces observations (mesures, photos, schéma) que j'ai fait un bloc très synthétique... En fait, au début je ne voulais pas dessiner ce bloc mais j'ai simplement fait un schéma d'une partie de l'affleurement (couche plissée d'axe N50) et c'est là que je me suis aperçu que tout était cohérent ce qui m'a permis d'avoir une vision en 3D (Interview, February 2000).

As a result of the process of having sketched these different elements in a first schema, Philippe realized that he had in fact acquired a conceptual understanding of the regional structure and then proceeded to draw a more refined block diagram (Figure 4.5).

This second block diagram captures their field observations through a "visual" and "subtle" synthesis of the different results noted, such as the directional measurements of the lineation, foliation, the particular characteristics of the folds, and boudinage. Philippe has represented here the peculiar and exceptional nature of the region's tectonic regimes, unveiled to them by the structures at outcrop 129. Importantly, he has also begun to "conventionalize" this representation by using various standard visual cues, thus causing a perceptible change in form in contrast to the first version, which was rather "rough" in comparison, given its accompanying textualizations.

The general framework of this new version of the block diagram will stay with Philippe until the end of the process, although he pointed out during our interview that it is quite rare for an on-site block diagram to be easily used later, and this is essentially due to the wealth of information that characterized the site. Philippe and his advisor were

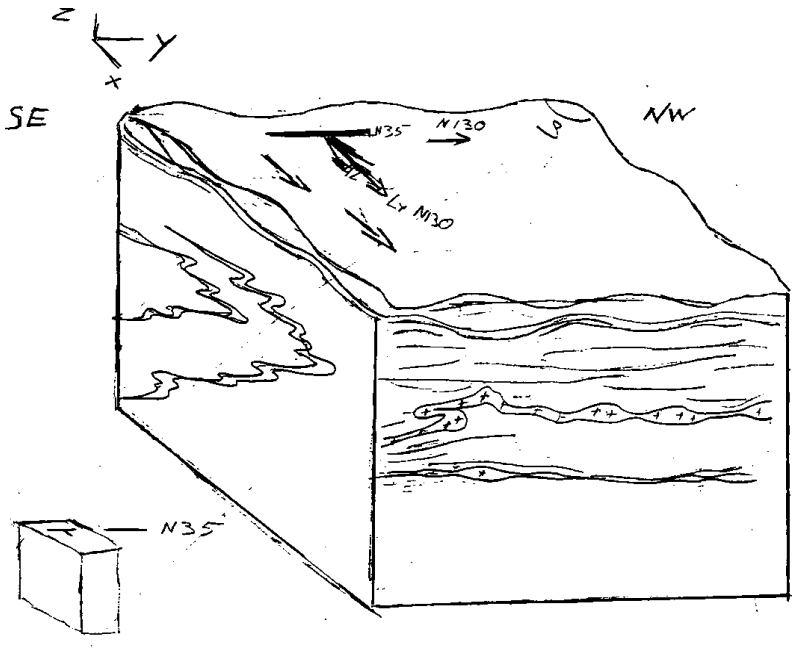



Figure 4.5 Second block diagram (August 1999)

quite excited by their discovery and celebrated the event with a large and well-earned meal. In his field notebook at the end of the day Philippe wrote, "Après cette journée où nous avons trouvé l'Affleurement [i.e., Outcrop 129], nous allons prendre un petit sacafé avec du cassoulet!!!"[xxii]

Upon returning to the laboratory at the end of the field mission, Philippe gets to work and over the next couple of months recrafts the communicative impact of his block diagram sketched in the field, putting it into submittable (i.e., electronic) form. Keeping the same general structure, he adds a few textual notations (Figure 4.6, below). In bits of "text", Philippe once again indicates the double boudinage he has seen in the field, as well as the different lithologies observed at different outcrop points (e.g., gneiss à bio, pegmatites, métabasite), and orientational measurements (e.g., lineation N120). The directional orientations of the different folds, both perpendicular and parallel to the lineation, are also indicated, as well as information about their type: down-folds (*synforms*) or up-folds (*antiforms*)[xxiii]. These bits of text accompany the block diagram,

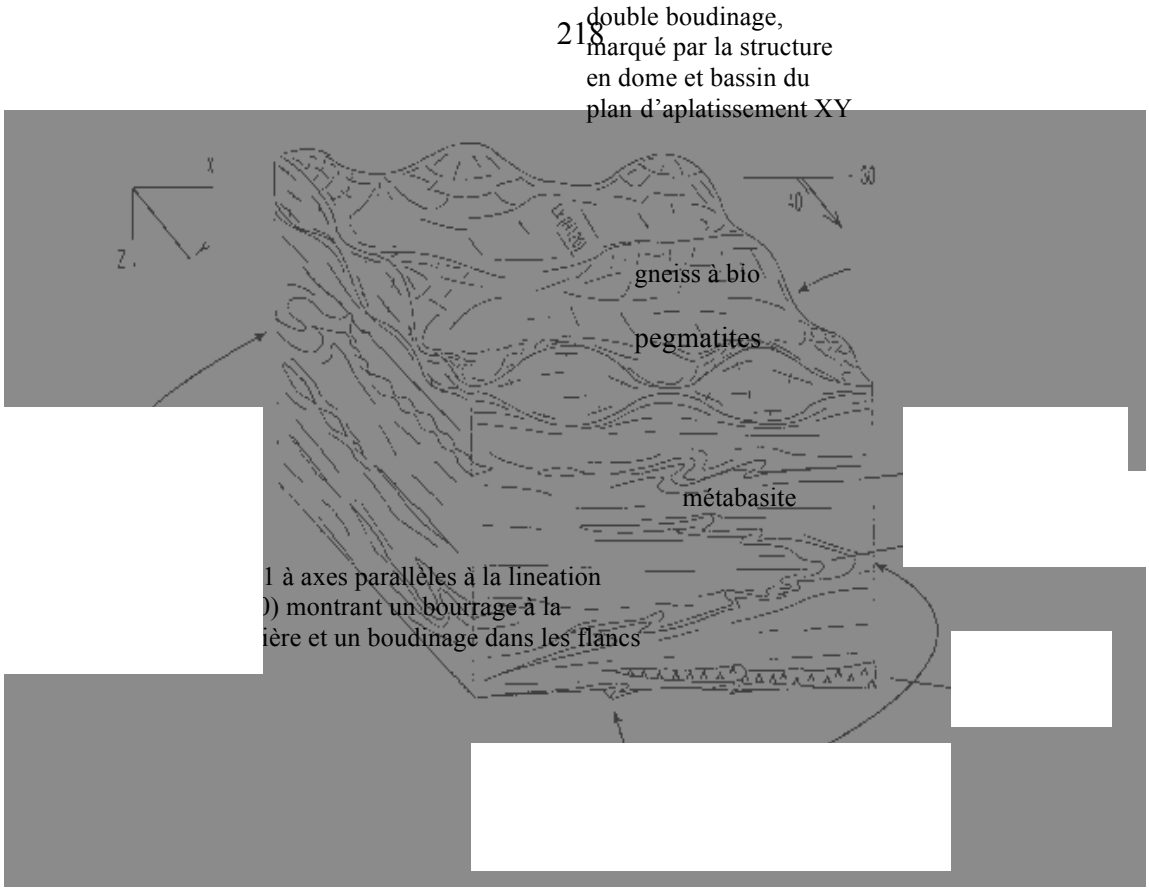



Figure 4.6 Computerized version of the block diagram (September – October 1999)

which has been taken out of its immediate contact with the field and recontextualized as a working document he will use as a basis for future reports of his field data. The schema reproduced in Figure 4.6 (above), as a more "polished" version of his block diagram, represents a visual, abstract and conventional synthesis of Philippe's most important field results, and in this form economically and conventionally communicates them to other members of his community.

And this point, a few comments can be made about the process of erasure involved in generating institutionalized textual silence. With this polished visual representation, we can observe the "distillation" of the results textually indicated in Figure 4.3. In their visual form, the various measurements, textual descriptions and structures have all been reduced to one highly theoretical and abstract image, which is linguistically and cognitively economical. The specificity of the natural "facts" observed

in the field is reduced and rendered fuzzy here, for this abstract and synthetic visual captures and 'homogenously' communicates a set of heterogeneous measurements ranging from 30 cm to 2 to 3 meters. As a consequence, we have lost the specificity of the day in the field when everything fell into place, in spite of the fact that the later "success" of the block diagram[xxiv] and Philippe's developing interpretation directly depended on the discovery of this prized outcrop. The day's crucial discoveries are no longer indicated by any special clue which would give the reader of the block diagram a particular sense of the finding's importance for the researchers.

We also, of course, have lost the "trace" of the evident difficulties gone into the work, efforts that are at once physical, intellectual, interpretive, and observational. And we have lost any indication of hesitation or doubt on the part of an apprentice geologist, who in addition to learning the "trade", is also learning to decipher the characteristics and nature of a new region, northern Madagascar, to date little explored. Thus, perhaps even less so than written conventions, visual conventions leave little room for accounts of the research activity. Instead, what we have is an illustration of the way in which scientific reporting conventions 'synthetically' mitigate findings and their significance (i.e., through conventionalized omissions and imposed modesty). The success with which the block diagram conforms to the conventional omission of such information only underscores the fact that Philippe was already "taking on" the voice of the professional geologist (see Schryer 2001).

The block diagram, a three-dimensional conceptualization of the regional structure studied by Philippe, is one step in "domesticating" and "distilling" field results. The imposition of the (visual) conventionalized silences we have seen so far in geology likely holds true throughout other domains of scientific discourse, as well, much in the way we have seen in Gilbert and Mulkay (1984) or Lynch (1985). However, while some of the process of erasure has been documented here, the examination of such institutional silences further reveals other types of silence. For example, the rhetorical effect of such

polished images of the field is to *implicitly* leave the impression that the field researcher is also inherently intelligent, having immediately made sense of the maze and mess of nature (i.e., another expression of a rhetoric of understatement). We know, however, that the truth is most often the contrary, an observation echoed by the many field geologists interviewed for this study. Furthermore, the adventitious nature of scientific discovery is widely confirmed in the literature, especially those concerned with laboratory studies such as Knorr Cetina (1981).

### 4.4 Textual Silence and the Conference Abstract

That said, of course one can hardly consider that the academic field mission is finished without also having "verbally" communicated the results, in either oral or textual form, to the scientific community. As discussed by Bazerman (1988), although the "printed statement" (e.g., the scientific research article) is crucial for later situations where its author is held accountable and its message becomes the point of reference for further discussion, very often one's peers have already necessarily passed judgment on the work long before the research article has become an indexed representation of communal generic knowledge, through such instances of standardized communicative events as "preprints, letters, and chalk talks" (1988, p. 22), or, as the case may be, through conference papers. These "prejournal" forms of scientific communication play a pivotal role in shaping the research account, by forcing its author to rhetorically adjust to the essentials of public opinion in order for it to gain acceptance as an "archivizable" text. In Bazerman's words, "the core of the argument must be inspected and approved by the relevant others" (1988, pp. 22-23). As a further context for recontextualization, we will therefore next look at the transformation of Philippe's field data into written form, as the Conference Abstract he submitted for oral presentation to a local French geologists' conference in March 2000.

As we know from the literature, the Conference Abstract*,* in contrast to the abstracts that preface research articles, operates as a form of "independent discourse" (the term is from van Dijk 1980), or a full stand-alone genre. It is also perhaps one of the rhetorically "stronger" genres of scientific discourse (cf. section 3.6.2), thus having more overtly identifiable rhetorical strategies. It has been successfully shown to be consistently structured into a five-move pattern (Bittencourt-Santos 1996, Yakhontova 1998). Yakhontova (1998), for example, has suggested the following five-part structure for the Conference Abstract:

1. Outlining the research field
2. Justifying a particular piece of research
3. Introducing the paper to be presented at the conference
4. Summarizing the paper
5. Highlighting the outcome or results

These five elements may also be further broken down into two essential tasks. Because the Conference Abstract is essentially a "freestanding document", submitted months ahead of the actual conference and which must work to "impress" a review committee (Swales and Feak 2000, p. 32), it often spends the first half of the abstract simply justifying the topic (moves 1 and 2) for it would seem that in a majority of cases the author's research position in his or her field would need to be clearly and strongly established before moving on to the results. The second half of the abstract (moves 3 – 5) is dedicated to describing the research and relies heavily on metadiscoursal strategies to draw the reader's attention to the results of the study; thus we might for example find a high occurrence of demonstrative "this", which analysts have interpreted as an attempt to draw the reader in by producing an impression of closeness and solidarity between reader and writer (Mauranen 1993). Further, given rigid space constraints placed on the

abstract's length, the abstract's title becomes quite important for conveying information and thus can be quite long (Swales and Feak 2000, p. 55).

Berkenkotter and Huckin's (1995) study of the approval rates of abstracts submitted to the College Composition and Communication Convention further highlights what are considered conventional features of successful abstracts, by contrasting the features of high-rated and low-rated abstracts. As they found in their study, high-rated abstracts have a clearly-defined problem, use current "buzz words", and use several explicit and implicit references to the literature. Low-rated abstracts, of course, do the opposite: they lack a clearly-defined problem or the problem is not presented in the most interesting light, their terminology is standard, and they use far fewer citations or allusions to ongoing debates (Berkenkotter and Huckin 1995, p. 102). In short, they demonstrate a lack of finesse in managing the tacit conventions of a community's discourses.

Equally as important, however, successful abstracts must also necessarily include an element of novelty for the scientific community, what Huckin (1987) has called "news value". Its purpose is not only to "create a research space" for the researcher (Swales 1990), but perhaps more importantly to persuade the review committee that the study is a valid one, one which will succeed in addressing the general community by effectively responding to the types of questions currently raised by the discourse community ("interestingness and timeliness, or kairos", as discussed by Berkenkotter and Huckin 1995, p. 115).

Even more so than is true for the research article abstract, then, a Conference Abstract presupposes a strong valorization of the study, what Swales and Feak have termed "a selling job" (1994, p. 214). The selling aspect of this particular writing task is one that Philippe recognizes immediately, when during the course of one of our discussions (March 2000), he pointed to the fact that even if his field data and structural interpretations were extremely interesting and valid by themselves, they were essentially

devoid of interest for the general geological community, due to their constrained "regionality". As a consequence, Philippe was bound by the strategic obligation to reset the problems raised by his data into a larger context in order to attract the interest of the greatest number of researchers from his research community.

**La Tectonique Néoprotérozoïque du CentreNord de Madagascar: interaction entre forces aux limites et forces de volume**

[1]Le Protérozoïque correspond à une période de transition au cours de laquelle le refroidissement de la Terre se poursuit, tandis que s'installe peu à peu la tectonique des plaques modernes. [2]On associe à la tectonique Archéenne des mouvements verticaux de type diapirique liés à des instabilités gravitaires, plus ou moins indépendamment des forces aux limites. [3]Ceux-ci seraient liés à des conditions thermomécaniques particulières de la croûte telles qu'un important flux de chaleur, des gradients de densité et une rhéologie plastique. [4]Au contraire, la tectonique actuelle (Phanérozoïque) est essentiellement contrôlée par les forces aux limites. [5]La chaîne Mozambicaine, à laquelle appartient Madagascar, correspond à une chaîne de collision moderne au Protérozoïque. [6]Néanmoins, peut-on trouver dans les parties profondes et chaudes, une composante verticale diapirique?

[7]Le CentreNord de Madagascar est formé d'une croûte archéenne composée de granitoïdes et migmatites surmontée par la formation basique d'Andriamena. [8]Cette croûte aurait subi deux épisodes magmatiques au Néoprotérozoïque moyen (790 et 630 Ma), puis un événement tectonométamorphique majeur correspondant à l'orogenèse Panafricaine (580 – 500 Ma). [9]Cette tectonique Panafricaine est marquée par un raccourcissement horizontal Est-Ouest. [10]Celui-ci est accommodé par des structures subméridiennes tel que des couloirs à fort gradient de déformation et un plissement de longueur d'onde variable correspondant à une succession d'antiformes granitoïdes et de synformes basiques (formation d'Andriamena). [11]Une composante diapirique est à l'origine de structures en dômes et bassins similaires à celles décrites dans les terrains Archéens et pourrait également intervenir dans le plissement à l'échelle régionale. [12]En effet, la structure en synforme pourrait être accentuée par la "sagduction" des formations basiques le long d'un décollement marqué par une zone mylonitique visible à la base de la formation d'Andriamena.

[13]En conclusion, nous suggérons que la géométrie et le champ de déformation fini panafricains sont compatibles avec un raccourcissement horizontal Est-Ouest contemporain d'un régime de type diapirique. [14]Ceci traduit localement le rôle des forces de volumes et leur interaction avec les forces aux limites dans la croûte inférieure au cours de l'orogenèse Panafricaine. [15]La migmatisation, la déformation diapirique, le raccourcissement horizontal atteste d'un amollissement de la croûte inférieure, qui pourrait favoriser des mécanismes convectifs dans la croûte continentale inférieure.

Figure 4.7 The Conference Abstract (March 2000; original emphasis)

The title of the abstract is moderately long (see Figure 4.7 below; sentence numbers have been added; the complete citation of the resulting paper may be found in Appendix B). We can note the geographical region where the fieldwork was done ('Centre Nord de Madagascar') and the chronological constraints Philippe has imposed

on the region's geological activity ('tectonique néoprotérozoïque', or 2.5 billion years ago)[xxv], essential for his interpretation. In the second part of the title, Philippe has included his interpretation of tectonic events ('interaction entre forces aux limites et forces de volume'), the significance of which will become clearer later.

In his abstract, we can straightaway note that Philippe uses a number of the specifically geological discourse markers identified in other areas of geological writing, such as the Geological Setting (Dressen and Swales 2000). These are discipline-motivated and generated "submoves", which we can find, for example, in the subsection where the geological writer sets up the study's relation to background, general knowledge. This geological backgrounding task is accomplished by describing the geographical locale of the research site, the formation's lithological composition, the age of the region, and the emplacement processes of the regional structures, to name just a few. Specifically, these geological submoves serve to situate the study within the background knowledge of the geological community, and define its purpose and interest in accordance with what other geologists understand about the functioning of the Earth.

The abstract, written in French, is relatively "standard" and conforms nicely to Yakhontova's (1998) proposed move structure for the Conference Abstract. The first paragraph outlines the field's current geological knowledge concerning the tectonic events typically associated with the Proterozoic, Archean and regional Phanerozoic periods (sentences 1 – 4) and it justifies Philippe's research (sentences 5 – 6). In the 15 sentences that he can cram into the available limit, it is significant that he postpones any mention of Madagascar until sentence 5. This postponement clearly indicates that he is attempting to place his dissertation research within a much wider context and within a set of issues that (hopefully) will appeal not only to "East-Africanists" but also, and perhaps even more so, to those whose primary interests lie elsewhere.

In particular, then, he poses the interest of his research in terms of having possibly identified a particular tectonic process (see rhetorical question posed in sentence 6),

*diapirism*[xxvi] which although it is specifically associated with the Archean (4 – 2.5 billion years ago) may have occurred during much more recent times — i.e., the early Phanerozoic, 580-500 million years ago, corresponding to the orogenesis of the Pan-African plate on which Madagascar sits. This diapirical element underscores the importance and novelty of the research contribution for such an occurrence would be quite unexpected and unusual, thus giving reason to believe that something radically new is to be learned about diapirism in general.

The second paragraph focuses primarily on the presentation of Philippe's field research, and uses a number of sentence-initial demonstratives to 'draw in' the reader (e.g., '*Cette* croûte' in s. 8, '*Cette* tectonique' in s. 9, and '*Celui-ci*' in s. 10). The principal task here consists of presenting the field data collected in the study area, the 'Andriamena unit' (s. 7), as the demonstrative proof for his interpretation. He quickly summarizes the most important (i.e., the rhetorically most convincing) field results, all the while setting them into a larger context where their novelty will be rhetorically highlighted (s. 8 – 9). We will return to this section shortly.

Finally, the third paragraph outlines the researchers' interpretation[xxvii], and it is here that we return to the third set of elements from the title: 'Interaction entre forces de volume et forces de limite'. Namely, Philippe suggests that the field's geometry and deformative features, described in paragraph 2, are elucidated by an East-West compressional event consistent with diapirism. This diapirical process would be explained by the interaction observed between the study area's boundaries, solid and immoveable granitic cones ('forces de limite') and the less rigid crust between the boundaries ('forces de volume'). Because of the softening of the interior crust due to the arrival of less dense magma from below ('diapirism'), the region was easily compressed by the surrounding granitic blocks

Returning specifically now to paragraph 2, we reencounter the first elements from his field notebook and block diagram (Figures 4.1, 4.3 and 4.6). The following list

summarizes the elements of Philippe's fieldwork which we might then compare to the abstract.

1. Particular rock types: granites, migmatites, basalt
2. Intense folding with axes variously perpendicular and parallel to the lineation
3. Boudinage and double boudinage

In sentence 7, Philippe notes the different lithologies observed in the field ('*granitoïdes*' and '*migmatites*' see Figures 4.1b, 4.6) and includes the observation that the Andriamena unit consists of both volcanic basalt ('basique') and a hard granitic, migmatic crust.

s.7 Le CentreNord de Madagascar est formé d'une croûte archéenne composée de **granitoïdes** et **migmatites** surmontée par **la formation basique** d'Andriamena.

And in sentences 8 – 9, Philippe notes two magmatic (volcanic) events and another major tectonic event that resulted in the geological features observed in the field. This event is the horizontal compression ('raccourcissement horizontal Est-Ouest') interpreted to have occurred on the basis of field evidence and which is a key element in support of his 'novel' proposal of recent diapirism.

s.8 Cette croûte aurait subi deux épisodes magmatiques au Néoprotérozoïque moyen (790 et 630 Ma), puis un événement tectonométamorphique majeur correspondant à l'orogenèse Panafricaine (580 – 500 Ma).

s.9 Cette tectonique Panafricaine est marquée par un **raccourcissement horizontal Est-Ouest**.

It is from this point of the paragraph on (i.e., s. 10 – 12) that we can identify other geological details pertaining directly to the field mission; these details provide observational proof for the interpretation of horizontal compression.

s.10 Celui-ci est accommodé par des **structures subméridiennes** tel que des **couloirs à fort gradient de déformation** et **un plissement de longueur d'onde variable** correspondant à **une succession d'antiformes granitoïdes et de synformes basiques** (formation d'Andriamena).

Horizontal compression is indicated here in sentence 10 in particular by the intense *folding* ('des couloirs à fort gradient de déformation') that has occurred in the strata, resulting first in *fold axes* oriented nearly due north-south (*'sub-méridiennes*[xxviii]*',* with an orientation of N120-N130; see Figure 4.3b) and in *synclinal* and *anticlinal* folds ('un plissement de longueur d'onde variable'; see also Figure 4.6, 'plis d'axe' or fold axes oriented variably N50 and N120'). The silent implication here is that both ancient strata (granite) and more recent strata (magmatic, or basic) occur together ('une sucession d'antiformes granitoïdes et de synformes basiques [formation d'Andriamena]'). Finally, in sentence 13, Philippe describes a 'décollement marqué par une zone mylontique' which he has observed at the base of the Andriamena unit ('visible à la base'). This last sentence represents some field data which was collected at a different point during the mission. Philippe takes these field attributes as proof for his claim for diapirism, which made the horizontal compression possible.

Significantly, however, Philippe's crucial observation of "boudinage" and "double boudinage" at outcrop number 129 (Figure 4.3b) receives no explicit reference here, although these structures played an equally important role in tipping Philippe and his advisor off that the particular tectonic event (East-West compression) had in fact occurred. Because the amount of field details is restricted by severe space limitations, pertinent results are thus chosen carefully to present the most convincing picture. As Philippe explained (July 2001), the boudinaged structures ended up being yet another

piece of data among others, tied more to the particularities of his study site than generally representative of globally-occurring features. As we can recall, all the features occurring together at outcrop 129 were "rare", and thus although interesting not truly *relevant* for the geological community as a whole. Instead what a well-applied field observation seeks is to establish are "mechanical laws," by relating the specific details of particular regions to generalized occurrences. There is thus a "hierarchization" of field results where less convincing (i.e., pertinent and relevant) field features are put aside.

The silence resulting from the selection of the most relevant field data raises yet another issue. Indeed, we can observe here that the marshalling of field data in such rhetorically precise ways in the Conference Abstract works less to establish Philippe's field presence and field competence, which has been identified in earlier parts of this and the preceding chapter as an essential task in geological writing — especially, the Field Account—, as it does to prepare his audience for accepting the validity of his claim. As we can recall, this is considered to be one of the principal communicative purposes of the Conference Abstract. And thus, by complexly layering the different levels of evidence within the frame of a new recontextualization, Philippe in fact prepares the way for making his singular, novel claim easier to accept within the particular genre context which is the Conference Abstract.

As we have seen, the notes taken from the field notebook and the details contained in the block diagram are strictly limited to a minimum of rhetorically "useful" and "permitted" information here. From an abstract about 400 words in length, then, hardly a seventh (roughly 60 words) make reference to the results of the field mission. The two months that Philippe has spent in the field are thus reduced to a mere handful of words, thereby showing the effects of genre-specific institutionalized silence on the shaping of this recontextualization. In particular, we have seen a silence imposed by the need to explain something 'novel' in a strictly limited number of words. However, like

'*sp. nov.*', this is a rhetoric of understatement where a mere word or two to the wise suffices.

### 4.5 Two years later: The scientific research article

> "Les données restent, l'interprétation change beaucoup... et à mon avis si je devais le réécrire, le papier changerait encore..." (from an interview with Philippe, 17 July 2001)

At the time of this writing (summer 2001), it has now been two years since the field mission in Madagascar, August 1999, and a journal has accepted an article Philippe wrote in April 2001. It is to appear in a special issue of *Precambrian Research,* an important and well-respected journal for reporting on structural research[xxix] of the Precambrian period, or early geological time stemming from the Priscoan (4.55 – 4 Ga)[xxx], and all the way through the Archean (4 – 2.5 Ga) and Proterozoic (2.5 Ga – 570 Ma) periods. This particular issue gathers together some of the most recent structural research on Madagascar and the East-African *orogenesis*[xxxi].

As one might expect, this recent paper was written in entirely different circumstances than the abstract we have just examined. While the abstract was written only six months after the field trip, Philippe's reasoning and interpretations had since had two years to mature through various other writings and presentations, further bolstered by petrological and geochemical results. Moreover, Philippe has made a necessary switch to English, so as to make his research accessible to a wider, international audience.[xxxii] This linguistic shift underscores the types of issues raised by contrastive rhetoric analysis, such as in Crosnier (1997), who notes that French and English contrastive studies have increasingly "lost their meaning" as the number of French language scientific journals has significantly decreased since the 1970's. Although bound by the obligation to use English in order to gain access to the international research community, thereby bowing before the apparent domination of English as a research language (Swales 1995, 1997;

Phillipson and Skutnabb-Kangas 1996), Philippe nonetheless remains sensitive to the issue of national identity and in this paper works in his own way to bring recognition to the French geological community.

What was immediately apparent upon reading the abstract and introduction, however, was that an important conceptual shift had occurred — nor, especially, was Philippe still talking about "diapirism" or "forces de limite et forces de volume". Nor did he make any reference to his "double boudinage" (or, in English, "chocolate-block boudinage"). I began to question whether Philippe still thought that these were key or "newsworthy" features of his findings, but as it were, there was the one detail that kept pulling on my shirt-sleeve. This was the field notebook entry "boudinage syn aplatissement", parallel and perpendicular to the lineation (Figure 4.3c), which as we know, provided a large part of the key for interpreting the specific tectonic regime during which diapirism would occur. Indeed, one might think that having more room to discuss specific field details in the greater space afforded by a research article in structural geology would allow for important structural features such as "double" or "chocolate-block" boudinage to re-emerge. What could motivate these changes in interpretation?

As Philippe revealingly explained in retrospect, the earlier abstract (March 2000) had been stuck into a rather unpromising slot late in the conference, and so he and his advisors decided to emphasize what the largest number of people would find exciting and "novel", going beyond the regionally-constrained context of Madagascar and onto the world scene. Using their field evidence, they made the very unexpected claim in the conference paper that diapirism, a tectonic regime characteristic of very early geological history (the Archean period, 4 – 2.5 billion years ago) could occur during more recent geological times (Pan African orogenesis, 580-500 Ma).

But when push came to shove, and Philippe had finished his petrological and geochemical analyses on the samples he had collected while in the field in Madagascar, difficulties in maintaining such a bold claim began to emerge. His geochemical and

lithological data were simply not confirming his hypothesis that Andriamena had been formed during the Archean. And the structural data he had collected in the field were not constrained enough to be more definitive on the subject.

While the purpose of the Conference Abstract is to get the audience to buy one's claim, further highlighting the claim's novelty in order to woo the committee, in the Research Article the author must establish his field presence and competence, and construct strong claims based on substantiated evidence — if this is evidence collected during actual fieldwork, the case can only be made more convincing. However, his field data was "interesting enough", as he put it, but had not the opportunity to contribute to the special issue come up, he would likely have never gotten the chance to publish his field findings — at least, in a journal with the standing of *Precambrian Research.*

And so, his interpretation has changed to fit the rhetorical expectations of the new recontextualization, in part characterized by a need for strongly substantiated evidence. While the "forces de limite, forces de volume" idea and 'diapirism' of the original abstract is in fact maintained, it persists peripherally and indirectly, having been downplayed given a lack of concrete evidence.

Moreover, the field's structural features are no longer considered a result of East-West compression, but of an earlier vertical shortening. Consequently, Philippe proposes here that Andriamena is in fact a 'nappe' placed on a granitic 'basement'. This basement was formed during an original deformation period (D1) of 'subduction', which occurred when one plate slid under another causing the formation of basement granite due to fusion. Andriamena itself is considered to be the by-product of crustal fragments from a volcanic arc. During a second period of deformation (D2), when tectonic convergence caused the widespread east-west horizontal shortening we have seen previously, this volcanic arc "slid up" onto the original granitic basement, thus explaining the presence of Andriamena, which consists essentially of basalt and other magmatic (volcanic) rocks, set upon an older, granite basement.

Philippe has thus shaped a new, more cohesive model, crafting an interpretation which would be considered more relevant, reasonable and pertinent for geologists working on the same part of the world, in terms of communally-shared understandings of the regional geological processes and their accepted timelines. After all, in this particular recontextualization of his fieldwork, he was writing specifically for the regional specialists whose work was also to appear in the same issue, and was thus "fitting in" (see Rudwick 1996) so that his structural field discoveries would not be lost.

### 4.5.1 The block diagram as the report of field data

However, while it is quite expected that one's interpretation will change over time to fit the genre expectations of different audiences, it is of utmost importance here that the block diagram itself has changed little. Quite typically, it is designed much later in the process as a way of synthesizing a group of data, long after the end of the field mission. In this case, however, the block diagram continues to directly translate Philippe's field observations which crystallized at outcrop 129. As a consequence, before turning our attention to the features of the text itself, it might be useful at this point to recall the particular relationship that exists between the visual representation and fieldwork practices in geology, for this opens up a very different issue, as we will see from the following excerpts of one of our interviews (July 2001).

As we can recall from Rudwick (1976) and from the discussion of the "bloc diagramme" in the earlier part of this chapter, the visual representation in geology is indissociable from fieldwork and the field description for it has, over time, evolved into a complex and conventional "visual language" geologists economically use for encoding and communicating field observations. The centrality of this visual language and visual practice to the practices of fieldwork has been echoed time and again by the geologists interviewed for this study. However, the terms of this tight and binding relationship

between the visual and the field is blurred somewhat in the following exchange with Philippe. In the following excerpt from our interview, he was very simply explaining how he wrote his article when something struck me, as we can note by my confusion and resulting surprise:

PG: Je décris les structures à très grande échelle, *c'est à dire à partir de l'image satellite...* [emphasis added]

DD: Mmm, tu parles du texte ou de l'image? Tu parles du texte, là...

PG: Ben, le texte, décrit les images en fait, il décrit les figures, euh... je

DD: Hmmm. C'est marrant [I'm seeking to understand], donc tu ne décris <u>pas</u> le terrain ici. Tu décris les images

PG: Je décris les figures

DD: Ah

PG: Ouais

What was revealed to me here, within the specific context of how Philippe makes use of the various types of visual representations, be they satellite images, block diagrams, or the other types of visuals that appear in his article but are not discussed in this study, is that Philippe in fact uses them to construct his field account, setting aside the field data contained in the field notebook. And so, although we were talking specifically about satellite images in this particular instance, a generalization may be made about the role of visuals in general.

The construction of this field account is quite obviously a multi-layered process, working its way through the various recontextualizations that make up the field geologist's "system of genres" (Devitt 1991; Bazerman 1994). But also, and more specifically, it is a process that is filtered through geology's inherent and conceptualized "visuality", for the field account such as we find it in the research article is not the direct reproduction of a field worker's field notes, per se. Instead, in practice the textualized

field account is written based on the field data recounted in the various *visuals*. Consequently, the field observation does not remain the sole source of subsequent recontextualizations. On the contrary, it is only in its "reformatted" form as a visual representation, based on the discipline's conventionalized visual language which represents the raw field data, that the details of the fieldwork mission persist. The visual, by being the original recontextualization and discoursal link back to the field observation noted in the field notebook, itself *becomes* the field observation and quickly replaces it.

However, the relation between the two communicative strategies (i.e., the textual and the visual), or indeed, the different recontextualizations, is hardly clear or straightforward, and Philippe himself appeared to have a difficult time in determining when the one has ended and the other begins as we continued to talk about the features of his article spread on the table before us:

DD: ... J'ai posé la question parce que je me souvenais que tout à l'heure tu disais qu'en fait, tu ne donnes pas de description de terrain, tu donnes une description des figures...

PG: [exasperated] Oui mais c'est parce que les figures c'est de la description de terrain! C'est euh...

DD: Oui c'est euh c'est quand même curieux, ça, ça m'a frappé parce que c'est la première fois que j'entends ça

PG: mmm...

DD: Disons que, bon c'est clair... tu n'écris par ton texte à partir du carnet de terrain

PG: non pas du tout.

DD: Ok donc l'acheminement depuis ton carnet de terrain, l'image, etc., et puis ton article où la description de terrain est faite à partir de l'image, bon mais pourtant regarde, tu vas écrire ça [I read from his article] "Structures related to the D1 deformation can be observed more easily outside the high strain zones D2". Là tu me donnes l'impression qu'en fait tu décris ton carnet de terrain!

PG: Oui. Oui oui. Euh, c'est tout à fait ça en fait. J'ai mes données de terrain, mon carnet de terrain, je les synthétise, j'en fais des figures où tout est synthétisé. Ces figures vont être utilisées comme support visuel mais après il faut que ça apparaisse, voilà sur le terrain on a observé des linéations de telle direction, des structures de tel ou tel et c'est vrai qu'en fait, je décris mes figures tout en faisant ce lien avec le terrain en disant que on a observé ça sur le terrain... Tu me prends un peu de court, mais je ne sais pas trop comment faire autrement.

This unexpected account of how Philippe transforms and makes knowledge through image into text in fact represents a key element in the transformational process in geology, however recomposed, reconstructed, transformed or "recontextualized" (Linell 1998) the source text becomes. An essential part of the revelation lies in the fact that here the visual representation actually *is* the field description. As Philippe says, "les figures c'est de la description de terrain!"

The role visual representations play in the process of creating the final rhetorical reconstruction in the published account thus only underscores the vital importance that Philippe's original block diagram grows to take on during this process, even though the explanation for the phenomena observed is now entirely different. As we can recall from the beginning of this section, while one's interpretation is expected to change quite a bit over time, especially, one would expect, for a novice researcher, what remains are the "hard facts", or in other words, the field data. And this field data, we now know, is contained within the visual representation.

In Philippe's paper, we see the use of satellite images that give a global notion of the field, which Philippe subsequently supplements with his own field measurements. These field measurements and observations are also reported in the block diagram, which directly translates Philippe's field observations and reflections that crystallized at outcrop 129 that very evening of August 15, 1999. Indeed, as we will shortly see, the centrality of the block diagram to constructing a field account based on his data is, in Philippe's eyes, motivated by more than one reason, and this is what causes him to fight for its retention

as the transformation process goes forward. The strength with which he takes the block diagram to represent this field data would very simply appear to frame his later struggle to have it included in the published article.

### 4.5.2 The Field Account

The very title of the paper, "Late Neoproterozoic strain pattern in the Andriamena unit (North-Central Madagascar): Evidence for thrust tectonics and cratonic convergence", draws attention to the fact that this is a study likely premised on or centered around fieldwork — we have what would be an obvious reference for an insider reader to a particular geographic area, 'the Andriamena unit (North-Central Madagascar)', but equally important is the element 'strain pattern,' which refers specifically to a methodologically 'structural' analysis of the field. This further implies that the researcher has himself been in the field, rather than having simply relied on the use of satellite images to construct a description of the field.

The article has a relatively standard Introduction in terms of rhetorical content, but is relatively short at roughly 330 words in length. The Geological Setting (GS) section follows, slightly longer, nearly 790 words, but recounts only fieldwork accounts published in the literature, and not Philippe's own results. Philippe took great pains to develop the GS, for him a task that went far beyond a simple "literature review". Much was at stake in his success in giving the appropriate background in the journal's special issue on the geology of the East-African rift, for not only is Andriamena an area of Madagascar that is poorly known, but Philippe is currently the only researcher thought to be working on the area. He wrote the GS with his audience in mind, knowing that while he was writing for 'regional specialists', he also had a personal stake in using material little-known or hardly accessible to the general — and especially non-francophone — geological readership.

While there are quite a few recent papers by French geologists published in English, the various French geologists who were consulted as informants during the course of this entire study have related that these studies tend to be passed over by those whose geological language is English rather than French. Philippe then has an additional education task in front of him here, and perhaps one not untinged by a sense of national identity and pride. As a consequence, from a total of 40 citations used in the article, over half of these (i.e., 24) refer explicitly to research undertaken by French-speaking geologists. Of these, 15 references belong to French researchers who have published in English (14 journal articles and 1 book chapter). To further break down these 15 citations, interestingly, 12 of these come from Philippe's own inner research circle — 4 belong to his two advisors (3 and 1 references, respectively); 5 make reference to the work of a fellow student who also worked with the same advisors, but on southern Madagascar; and an additional 3 refer to the publications of a fellow co-worker from the same Earth Sciences Department who has also done quite a bit of work on the geology of Madagascar. The remaining 9 citations refer to other French research, mostly inaccessible to the general geological public either because it refers to conference presentations (2: one belonging to Philippe's advisors as well as one of his own given at the Geological Society of America in 2000), or is published in French (3), or is an unpublished dissertation in French (4).

There are a series of lesser known 1960's French publications in the *Comptes Rendus de l'Académie des Sciences,* when Madagascar was still a popular traveling site for French geologists, that Philippe would have liked to have included but did not, for reasons of space constraints. Instead, he cites the one principal work of the period (Besaie 1963), who supervised a mission, in cooperation with the French army, to go over the whole of Madagascar during the 1960's with a fine-toothed comb. As an early (i.e., Archean) cratonic region and former French colony (independence obtained in 1961) with enormous prospects for mining precious metals, Madagascar drew the attention of

academic and technical geologists, especially in terms of the mining possibilities it offered. As Philippe put it, he could easily have continued for three or four pages, but he describes how his ambitions had to be curtailed in the face of "community customs". He did have to keep the background down to a relative minimum, however interesting. And thus, he had in effect to make a compromise between showcasing a long tradition, if now largely unrecognized, of French work on Madagascar within the exigencies of the typically GS, minimalist account of what is currently known of the region.

The Field Account follows the Geological Setting and is entitled "Strain pattern and related structures". It is of very typical length for an article in Structural Geology (roughly 1850 words). Philippe begins with a short, 267 word introductory section where he describes his methodology (strain analysis) and introduces the areas of focus of his field studies:

> "The Andriamena unit (Fig. 3a and 3b) and the gneissic-granitic basement, which is composed by the Andriba area in the western margin (Fig. 7) and the Ambakireny area in the eastern part (Fig. 8)."

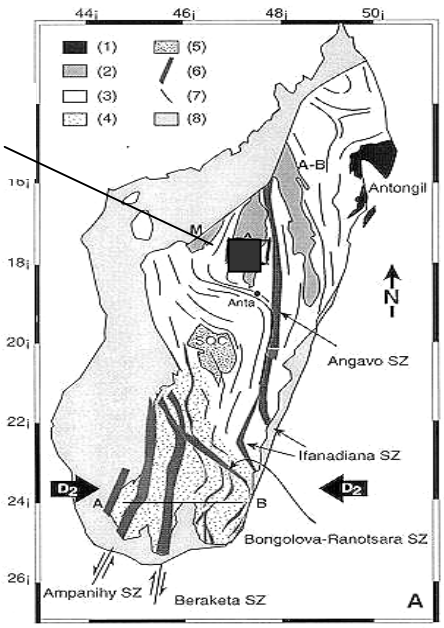



Figure 4.8 Location of study area within Madagascar (Philippe's Fig. 1)

For the location of his study area, refer to Figures 4.8 (above) and 4.9 (below), containing Philippe's original "Fig. 1" and "Fig. 3b", as indicated in his text.

What follows this introductory section is the field description of four distinct study areas: (1) '3.2 The Andriamena unit', (2) '3.3.1 The Andriba area: kilometric fold interference pattern', (3) '3.3.2 The Ambakireny area: dome and basin structures' and (4) '3.4 The western Andriamena/basement contact: a major mylonitic zone'. We will focus our attention on the first of these subsections, entitled '3.2 The Andriamena unit' (Figure 4.10, sentence numbers have been added), for this is where we find reference to the field

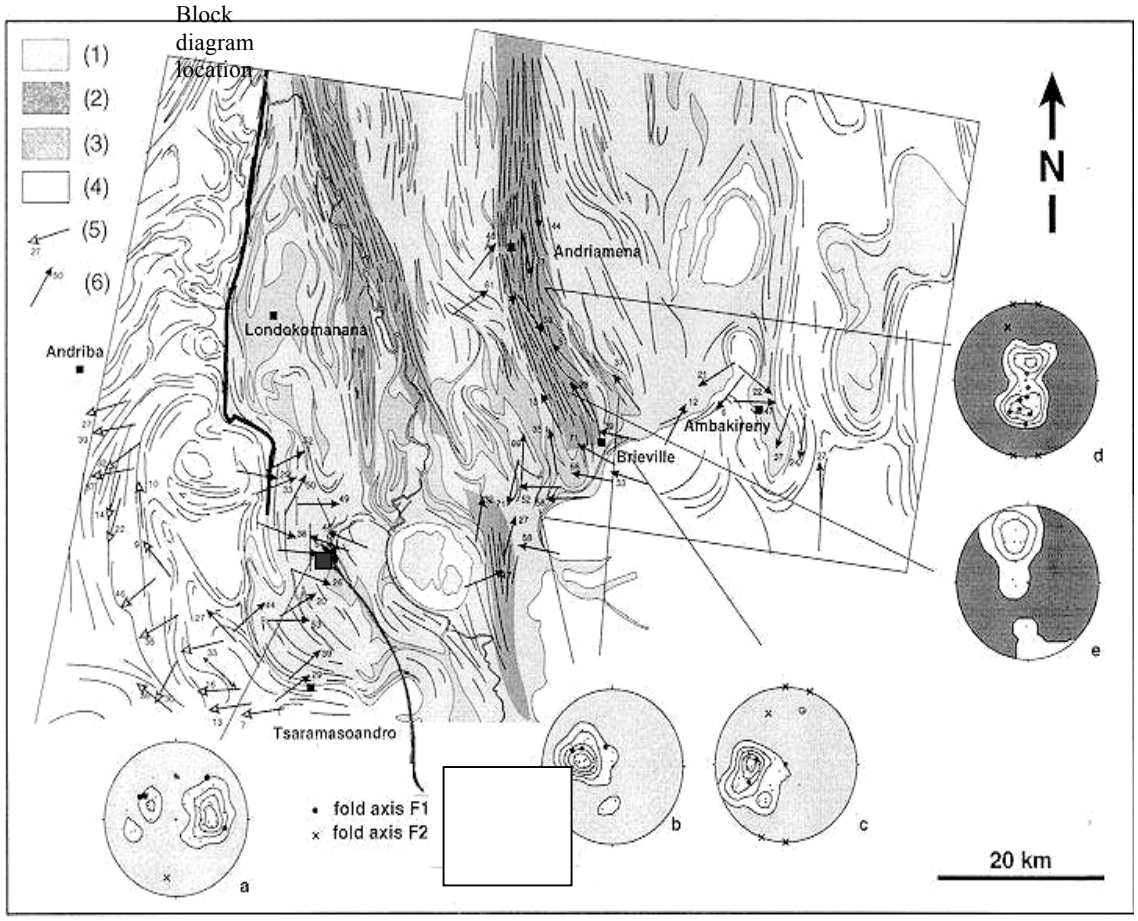



Figure 4.9 Location of outcrop 129 where the block diagram was sketched (Philippe's Fig. 3b). The light gray shading denotes areas of some folding, while the dark gray denotes areas of intense folding.

notes taken on Sunday, 15 August, 1999, of which we have seen the excerpt in the early part of this chapter. This was the day in which Philippe and his advisor found 'THE' outcrop, or in Philippe's words, "l'Affleurement." The area where this exceptional outcrop was found and which inspired the block diagram is found on the western side of the Andriamena unit (see Figure 4.9).

In the following text we will focus especially on the final paragraph (sentences 12-17), which is where Philippe refers to outcrop 129 and the block diagram. It is significant that in this paragraph we find the only verbal cues throughout the entire field report that explicitly signal the researchers' field presence, a feature we will return to below.

*3.2 The Andriamena unit*

[1]The foliation in the Andriamena unit is a transposed composite plan mainly com-posed by the parallelism of mafic, quartzofeldspathic gneisses and mafic-ultramafic bodies. [2]At the regional scale, the foliation plane, denoted as S1, is dominantly oriented N160 –N180 (Fig. 3a) and defines a kilometre-scale synform, with a north-south axial trace (Fig. 4).

[3]The S1 foliation is folded on various scales by post-schistosity folds F2 with a steeply dipping north-south axial plan and subhorizontal axe (Fig. 3b-stereo a, c, d and Fig. 4), coherent with east-west horizontal shortening (D2). [4]The D2 deformation is heterogeneous and shows a strain partitioning between large low strain zones (zones in light grey in Fig. 3a and Fig. 3b) limited by an anastomozed network of high strain zones globally oriented N160–N180 with a width up to 10 km (zones in dark grey in Fig. 3a and Fig. 3b). [5]In the low strain zones, the S1 foliation as the mafic-ultramafic intrusions are gently folded by F2 kilometric open folds, without any related axial plane foliation (Fig. 3a-b and Fig. 4). [6]Locally, some leucosomes can underlie F2 axial planes. [7]In the high strain zones, foliation is subvertical (Fig. 3a-stereo d, e) and can be interpreted as the transposition of the previous S1 foliation into a new penetrative north-south vertical S2 foliation or as the verticalization of S1 related to the upright F2 folding. [8]Mafic-ultramafic intrusions located in these zones are characterized by high aspect ratios (10<H/L<40) consistent with a strong tectonic transposition in this zone (Fig. 3a).

[9]In the low strain zones, where the D2 strain is moderate, the L1 stretching lineation, marked by biotite or amphibole, defines a regular east-west trend perpendicular to the Andriamena/basement contact, with a pitch around 90° and variable plunge due to F2 folding (Fig. 3b-stereo a, b, c). [10]In the high strain zones, where S1 foliation is verticalized, L1 lineations plunge steeply due to their passive rotation during F2 folding (Fig. 3b stereo d). [11]Near Brieville, where transposition of S1 into a new S2 occurs, L1 lineation seems to be replaced by a new L2 subhorizontal lineation broadly oriented N170 (Fig. 3b stereo e).

[12]Structures related to the D1 deformation can be observed more easily outside the high strain zones D2. [13]At the outcrop scale, we observe numerous isoclinal intrafolial folds with a hinge parallel to the L1 lineation and a sub-horizontal axial plane (Fig. 3b stereo a, b, c and Fig. 5). [14]The initially horizontal S1 foliation is also affected by boudinage structures compatible with the E-W stretching lineation direction (Fig. 5). [15]All these structures suggest that the D1 event underwent a significant amount of vertical shortening. [16]The D2 high strain zones are characterized by numerous upright F2 folds, which can locally interfere with the previous F1 isoclinal folds. [17]The lack of asymmetrical

structures in these zones characterized by an intense transposition, as shown by the very high aspect ratio of the mafic-ultramafic bodies, is consistent with a strong component of coaxial strain associated with the horizontal east-west shortening during the D2 event.

Figure 4.10 Excerpt from the fieldwork report: "The Andriamena Unit" (April 2001)

Although Philippe related that he most often skips over the field results sections in day-to-day reading practices — he has, in fact, been trained principally as petrologist — when finding himself faced with the challenge of writing this article he decided to go back in order to dutifully and methodically read over a whole series of field accounts so as to figure out how they should be written. He discovered, on his own, the following global structure and tried to follow it systematically while writing the field report section:

1. the foliation and its description
2. the lineation and its description
3. a synthesis of the two and interpretation

We can see that Philippe roughly follows this structure here, where paragraphs 1 and 2 describe the foliation related especially to the second event D2 (sentences 3-4). He notes that F1 (i.e., foliation related to the first regime of deformation, vs. F2, that having formed during the second regime of deformation) is difficult to identify given the intense deformation of the region that occurred during the second tectonic event (D2). Where D2 deformation is intense in the high strain zones (dark gray areas in Figure 4.9), F2 features seem to have overtaken F1 features (sentences 5-8).

Moving onto paragraph 3, the low strain areas (denoted by the lighter gray areas in Figure 4.9) make identifying D1 features easier, although once again Philippe notes the interference of the second regime related lineation (L2) on the first (L1). But now, let us recall that it is precisely in these low strain areas, especially on the western side of the

Andriamena unit, that Philippe and his advisor found “l’Affleurement”, whose location Philippe marked for me with an ‘x’ in Figure 4.9.

Keeping this in mind, we will now turn our close attention to the final paragraph, which I have identified as the only place in the entire field report where Philippe talks explicitly about l’Affleurement *and* the block diagram. The only textual description of outcrop 129, which Philippe has repeatedly indicated as one of the most important for understanding and inferring the region’s different tectonic regimes, further accompanies the unique verbal expression of researcher activity throughout the entire field report. This we can see first through the use of the passive (‘can be observed’) coupled with a set of evaluative adverbs (‘more easily’) in sentence 12, and then again in sentence 13 with the first-person plural pronoun (‘we observe’), albeit with a slightly unusual and atypical use of the present tense. The ‘we’ here refers either to the research team, or more likely, to a neutral, non-agentive “one”, as in “on observe”, which is a common strategy in written French. In addition, in sentence 13, we find the first reference to the block diagram (“Fig. 5”).

s. 12 Structures related to the D1 deformation **can be observed more easily** outside the high strain zones D2.

s. 13 At the outcrop scale, **we observe numerous isoclinal intrafolial folds** with a **hinge parallel to the L1 lineation** and a **sub-horizontal axial plane** (Fig. 3b-stereo a, b, c and **Fig. 5**).

In terms of explicit field data, in sentence 13 we also find textual reference to the many folds that characterized the outcrop (‘numerous isoclinal intrafolial folds’). The term “isoclinal”, which refers to a fold whose limbs are nearly parallel, is vividly illustrated in the very first block diagram Philippe drew in the field (see Figure 4.4). The hinges of these folds are here once again noted to be parallel to the L1 lineation (‘plis d’axe // Lx N120’ in Figure 4.3b). In this same sentence (s. 13), Philippe also refers the

reader to the block diagram (his Figure 5) in order to illustrate these structures (see Figure 4.11 below). While it is true that the block diagram has changed relatively little in this new version, Philippe has nonetheless inverted its XZ and YZ planes and slightly inclined the figure to reflect certain aspects of his new interpretation, such as the effects of folding during the second tectonic event.

In sentence 14 we at last find the only reference to the key term 'boudinage'. However, we will note here that the existence of the boudinage and double ('chocolate-block') boudinage has been reduced in the text to the expression "boudinaged structures".

s. 14. The initially horizontal S1 foliation is also affected by **boudinaged structures** compatible with the E-W stretching lineation direction (**Fig. 5**).

This is the only overt, in-text reference to boudinage which occurs throughout the entire fieldwork report, 1850 words in length. There is, of course, the metadiscoursal reference to his block diagram ('Fig. 5'), which we can see below (Figure 4.11).

This is certainly not as much as we might have expected, given the importance the structure has represented to deciphering the regional formational process. Instead, one must turn to the block diagram's caption, for it here that we find the only real textual description of the structure:

> "Schematic block diagram showing the different types of structures related to the D1 event, at outcrop scale. In the YZ section: isoclinal folds with axes parallel to the L1 lineation; in the XZ section: boudinage structures associated with scarce folds perpendicular to the L1 lineation; in the XY section: chocolate-block boudinage surface with a lineation L1. All these structures are consistent with a vertical shortening. The actual orientation of the block diagram is related to the later D2 folding. (1) biotite gneiss; (2) pegmatite; (3) metabasite."

Here, in a nutshell, then, are Philippe's field data.

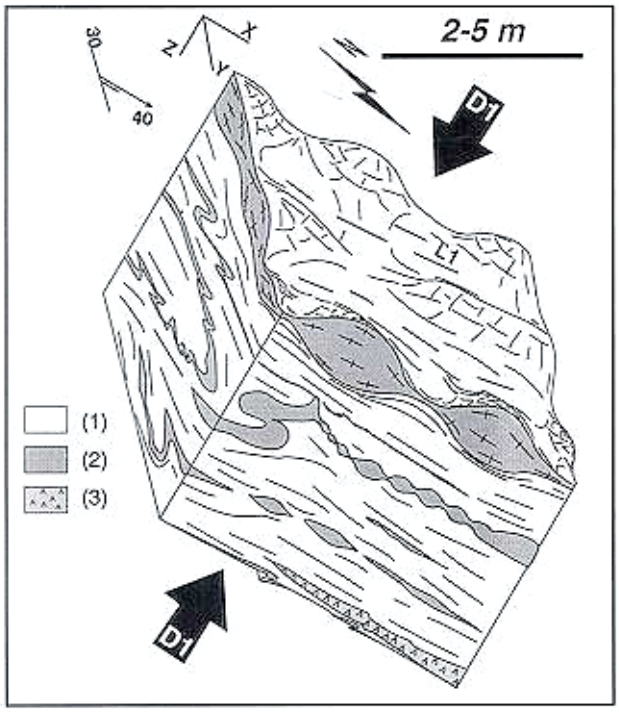



Figure 4.11 The block diagram, as it appears in the research article (April 2001)

In sentence 15, Philippe uses these structures to establish his interpretation of the D1 event. Using the verb 'suggest + that' is a frequent strategy identified throughout the corpus of 117 research articles for indicating a switch to interpretation.

s. 15 **All these structures suggest that** the **D1 event** underwent a **significant amount of vertical shortening**.

This vertical shortening (D1), and its effects, are schematically — and rather primatively — represented by my own efforts in the top part of Figure 4.12 (see below).

In sentence 16, Philippe returns to the difficulty in observing D1 features in the field, due to interference by the D2 deformation event.

s. 16 The D2 high strain zones are characterized by numerous upright F2 folds, **which can locally interfere with** the previous F1 isoclinal folds.

This interference once again highlights the importance of outcrop 129, where the D1 features were particularly visible. He then summarizes in the final sentence of the section

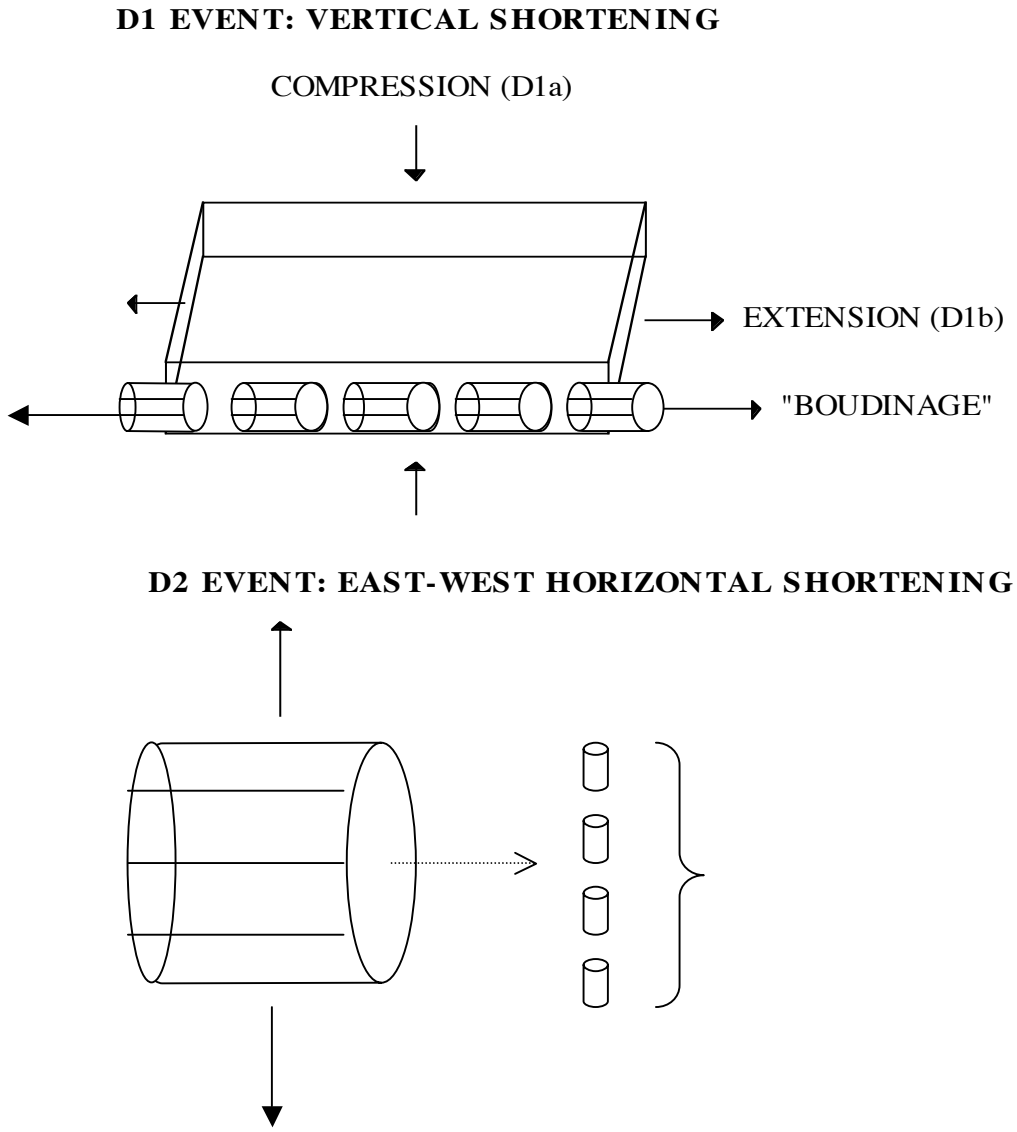



Figure 4.12 Compressional and extensional tectonic events and the formation of boudinage (D1) and chocolate-block boudinage (D2)

(s. 17) how he arrived at the conclusion that the Andriamena unit had also undergone an important D2 East-West shortening, which caused blocks of foreign mafic matter ('mafic-ultramafic [or volcanic] bodies') to slide up onto the granitic basement ('a strong component of coaxial strain associated with the horizontal east-west shortening'; see the bottom part of Figure 4.12).

s. 17 The lack of asymmetrical structures in these zones characterized by an **intense transposition**, as shown by the very high aspect ratio of the **mafic-ultramafic bodies**, is consistent with **a strong component of coaxial strain associated with the horizontal east-west shortening during the D2 event**.

Thus, from the former list of physical features Philippe has used to interpret the region's formation and deformation processes, we have nearly a complete account. In Philippe's words, "les données restent, l'interprétation change… beaucoup". However, it is anything but obvious where we are to find them. We have seen that Philippe has been able to keep his field data largely intact, albeit in a "distilled" form. In reality, what remains are two crucial representations of this field data. The first is a brief description of the outcrop Philippe and his advisor observed at point 129, "l'Affleurement". The other is the block diagram which recounts the observation of this outcrop.

The presentation of the field data is muted and cryptic, marked by many conventional omissions. In conforming to genre expectations, the new interpretation here has been forced to downplay certain aspects of the field account, such as the 'forces de limite, forces de volume' or the 'boudinaged structures'. While the regional situatedness of the field structures makes the observations an appropriate description of the area's particularities, they cannot be considered typical of this type of process everywhere in the world. The textual reduction of the field account in the research article is thus attributable to the particular frame of this new recontextualization, which requires the author to shape

and substantiate his claims more rigorously than in the conference abstract in order to fulfill the research article's purpose and its audience's expectations.

Finally, the reduction of the field account is also further explained by the fact that the block diagram here *becomes* the field data, making the 'textual' inclusion of details superfluous. As Philippe explains (July 2001),

PG: Il y a des choses qui restent qui sont fortes, il y a l'Affleurement. Les données restent, l'interprétation change, beaucoup. ... et pour garder un bloc-diagramme comme ça, je ne sais pas, à mon avis ça doit être assez rare. Souvent, ce qui apparaît sous forme d'article c'est après une grosse réflexion, une synthèse de toutes les données, et c'est après qu'on commence à faire des schémas synthétiques. Là ce qui était bien avec cet affleurement, c'est qu'il était extraordinaire. Il était tout simplement extraordinaire. ...

DD: Tu as décidé d'inclure le bloc-diagramme parce que c'était une belle illustration de tout ce que tu avais vu ailleurs?

PG: Ça représentait assez bien, ouais. Il y avaient des choses qui revenaient très regulièrement, dans toute cette zone là qui appartient au même endroit [NB. autour de la localité du bloc-diagramme, Figure 4.9]. C'était assez, euh, c'étaient pas les même affleurements évidemment mais on retrouvait des structures assez semblables.

But it is questionable whether Philippe really even *needed* to include the block diagram to get his point across and to convince his readers of his interpretation. He explained that he had decided to include the original block diagram in the article because it "beautifully" represented the relative structural homogeneity found throughout the area. However, this explanation already underscores an attachment Philippe seems to have developed toward the block diagram and the field data it recounts, signifying yet one other type of silence, through the intentional and purposeful manipulation of implicit expression.

Here, we may in fact have evidence for a concealed personal story for it is precisely over the inclusion of the field details contained in the block diagram that

Philippe has had to struggle. According to his advisors, this was “too much field talk” and they wanted him to get rid of it. When I asked Philippe to describe the importance of the block diagram in terms of his field findings, he answered by telling me why he had decided to include it in the article.

PG: C’est très proche [de la réalité], à mon avis ce genre de choses c’est assez rare, mais il faut être assez confiant que, je, euh

DD: Qu’est-ce qui est assez rare?

PG: C’est, dès sur le terrain on dessine quelque chose, et que ça apparaisse après sous forme de, de publication. C’est euh, moi je l’ai mis parce que je trouvais que c’était un bel exemple, on en a même un peu discuté avec mes directeurs de thèse, et ils se demandaient, euh, quel était le réel, intérêt, si vraiment ça ça apportait quelque chose... ils se demandaient s’il ne fallait pas l’enlever, et puis bon finalement moi je

DD: Et qu’est-ce que tu as présenté comme argument pour le garder?

PG: Pour le garder? Ben ça... c’était un très bon support pour euh ma ma descripton que je fais dans le texte, quoi. Sinon j’allais faire une description dans le texte sans support visuel, et euh, bon.

There were other things Philippe’s advisors wanted to “censor”, as well, such as the details of private conversations they had had which helped Philippe draw the interpretation together. These details were relevant for him, but his advisors considered them to be unnecessary and did not feel they added anything to the argumentative structure of the paper. Some of the suggestions Philippe accepted, bowing to his advisors’ greater experience, recognizing their good sense in noting that a deeply detailed discussion of the field does nothing but lose the reader given that local field details are not immediately relevant.

PG: Le lecteur quand il lit ça il ne va pas pouvoir replacer ça dans son contexte donc il va lire ça, très bien, et puis il va l’oublier.

Despite his advisors' success in convincing him to minimize the overt discussion of the field mission's importance, Philippe held steadfastly, however, onto the block diagram which had been with him since the start. At the outset, it had allowed him to make sense of the incoherent lineations he was noting in the field, going every which way due to intense D2 deformation. In addition, the recurrently demonstrated strength of the block diagram was that it took all other similar structures into account, allowing Philippe to make out a first part of the whole story. And of course, it was "the story" of that day spent in the field on August 15, 1999, when Philippe was learning to decipher the puzzles and mess of nature. And last, although hardly least, it was "his" creation, what was originally a homework assignment for the evening. As we can recall, Philippe's advisor had asked him to sketch out a block diagram of their findings, and he hesitantly set to work. But through the process of recontextualizing what he had seen into a visually conventional form, Philippe himself understood what was going on and from that point on became an active owner and shaper of his field observations into suitable interpretation, moving yet a bit higher up on the novice-expert continuum.

And so, in the solidly and communally constructed silences of the final recontextualization reported on here[xxxiii], we can, "in the wings", find the trace of Philippe's attachment to his block diagram and to his fieldwork, for its inclusion here points to yet another type of silence: an unexpected, non-conventional and purposeful silence, in other words, a concealed personal story. Indeed, there are times when the inclusion of what is conventionally omitted points to a silential expression whose sole purpose lies not in the communication of some 'relevant' information to the research community, but rather in giving voice to the author's personal experience. What might be considered a lack of modesty, according to Ducrot (1973), has here been entirely mediated by the economical framework offered by the block diagram, where such private intention remains unthreatening, for it is "unseen". At most, the inclusion of the block diagram would be irrelevant and superfluous, but here the research community has

granted its presence, and it will appear in the final, published version of the article. And thus, while Philippe's attachment to the block diagram remains wholly silent in his text, its mere presence is the key to unveiling its implied communicative content.

## 4.6 Conclusion

This is, of course, but a very small glance into a story about pedagogical break-throughs and the successful transformation of a student into a self-responsible researcher who "knows the ropes" of the discipline. We now have an idea of some of the different genres a member of the geological research community must become proficient in, such as the field notebook, various visual schemas, the Conference Abstract and the Field Account in the research article (see Parkinson 2000). We have also had a glimpse into how the researcher must effectively recontextualize his field research at all the various stages against a backdrop of disciplinary practice, which includes knowing what aspects of the research to silence and how, as well as when to manipulate silences so as to achieve implicit intent.

It is undeniable that part of learning to recontextualize the research account involves "taking on" the community's voice (see Schryer 2001). And thus we may see junior researchers adopting a more standard authorial voice in order to demonstrate their desired recognition and membership to a new community, replete with an appropriate discoursal distance between the researcher and his research. Nevertheless, we may also see them struggle with the decision to give up parts of the research account they consider important, but which are not validated by senior researchers as being pertinent or relevant for the construction of community knowledge. This is doubtlessly the case for any new-comer to the game, whatever the disciplinary walks he or she has chosen. The shifts authorial space undergoes over time in an individual's writing and what this may say about what is appropriate or inappropriate content is a subject we will examine more

closely to in the next chapter, especially as it relates to the contrast between what is textually attested — and expected — and what occurs as concealed personal stories and unsuspected omissions.

Finally, we may at last begin to concretely compare the two types of silence outlined in Chapter 1. The first type is a conventionalized and institutionalized "distillation" of the research account, i.e., occurring in a way that is both expected and shared. Concretely, the visual and textual conventions acting within the various "recontextualizations" of the field account render the majority of research details and authorial participation in the research account "invisible". Of course, this apparent invisibility is what we would have expected in a restricted and space-constrained part-genre such as the Conference Abstract. Yet it has been somewhat more surprising to have also seen the mutedness of certain key aspects of the field results in the research article. The field details are there, but they are concealed. Some are implied by the technical terminology; some are stashed away within the visual representation that has become the field data, and others have simply vanished.

The other type of silence is purposeful and unexpected, although it, too, can sometimes become conventionalized (i.e., a rhetoric of understatement). It translates the private needs of either particular communities or of an individual by embedding one silence within another. Purposeful silences are here taken to be instances of textual silence precisely because of their lack of linguistic explicitness, but they nonetheless have concrete communicative intent that transgresses what is conventionally expected. Some of these silences can be decrypted within tightly constrained communities, where a mere "word to the wise" speaks volumes. Evidence from Chapter 3 has indicated that the geologist draws attention to his presence in the field by using a 'rhetoric of understatement' in which a set of discreet linguistic traces is framed. Despite the strong constraints on its content, the strategies of "silent" rhetorical reconstruction present a

powerful tool for establishing and maintaining credibility, similar to the journalist's recontextualization of communicative practices described by Forstorp (1998).

Further, we have also seen the geologist make implicit references to "unspeakable" parts of his research experience, such as the contention that his research is 'novel' or that he has found a particularly good exposure. Still others, such as Philippe's motivation for including the block diagram in the research article field account, require a complicated methodology to reveal them — and especially, a good deal of luck, since one cannot expect to be looking for such a "silential expression" given its unique relationship to the experiences of but one person. It remains to be seen whether this type of silence can be accounted for with a deterministic framework of structurally limited agency, such as that proposed by Bourdieu (1984, 1990), or whether it will be necessary to propose a model which, by focusing on the basic "need state" driving linguistic innovation (see Engeström 1988), would account for such instances of "unpredictable" linguistic variability. This will be a matter taken up in more detail in the following chapter.

Through the examination of the features of each recontextualization, as well as the process of genre-knowledge acquisition, we have been able to consider some of the forces which have shaped the research account, thus giving us a new understanding of the contextual framework which produces textual silence. We have seen the effects of this force as an "institutionalized" discoursal silence, in the way the conventions of geology's visual language and written genres are repeatedly played out in the instantiation of each new genre text, amounting to a drastic reduction in the overt 'textual' importance accorded to the field mission. Moreover, these mechanisms inform us about the systematic nature of institutional silence, for "silential rules" indeed appear to act in a conventional manner, repeatedly excluding the same types of field information. Textual silence, then, occurs within the complexly-embedded recursivity of institutional discourses, whose expression is maintained by a community of users in a way which is 'regulating' and 'normalizing'. The "silential frame", consisting of an intricate and

complex web of interweaving silences on various levels, may seem devoid of meaning to group outsiders, but it clearly remains implicitly rich for insiders and thus governs the communication of results in much the same way as "marked" discursive frames. Indeed, geologists may not be "saying" things in a way which we can textually see, but all geologists who read their accounts can, on the basis of their own prior experiences, genre knowledge and *habitus*, "intuitively" reconstruct much of what has been silenced.

The "situated" institutional nature of silence in the recontextualization process must also be emphasized, for we have seen that the motivations for textual silence are imputable to constraints that are specifically disciplinary (or "cultural") in nature. Indeed, Sarangi (1998) questions which elements the analyst is to focus on in the recontextualization process, but in response points to the highly context-dependent nature of situated discourse. Likewise, it has been found here that the ways in which fieldwork is carried out, its working conditions and disciplinary concerns are clearly different from what goes on in the laboratory, and that the imposition of a visual field language as a communicative form further influences how the account is reformulated. The culturally-situated place the field has come to occupy in the field of geology further determines which details of the field account become silenced in the final version. This study therefore lends further support to the idea that the determining elements of a discourse are intimately linked to its cultural, historical and social context (cf. Bazerman 1988). Salager-Meyer (1998) has further suggested that scientific discourse, with its concurrent structure, values, and needs, is but a representation of our modern society.

A similar process of recontextualization and resulting "silence" has been described for other disciplines, as well. In reflexive ethnography, for example, recontextualization or "textualization" is regarded as a rhetorical device for blurring the distinction between description and interpretation in the construction of social reality. Thus, the transformation of fieldnotes into ethnographic texts is directly constrained by the assumed relation which exists with an ideal audience (Clifford and Marcus 1986,

1988; Marcus and Fischer 1986) and what kind of identity the author wants to project, however covertly. This process is also similar to the psychotherapist's practice of transforming casenotes into institutional records (Ravotas and Berkenkotter 1998; Berkenkotter 2001). In the process of producing a new text for a specifically targeted audience, the identity of the source text and its surrounding context are likely to be "sacrificed".

## Notes to Chapter 4

[xx] Fold: Deformation resulting from the flexion or torsion of rocks.

Boudinage: see Figure 2.

Foliation (F) and schistosity (S) can be used interchangeably, but in particular, foliation refers to a planar arrangement of textural or structural features in any type of rock that results from flattening of the grains of a metamorphic (as opposed to sedimentary) rock.

Schistosity, on the other hand, refers to the foliation in schist or other coarse-grained rocks due to the parallel arrangement of mineral grains (usually mica) and is considered to be a type of cleavage.

[xxi] Double boudinage: Simultaneous stretching in all directions due to even pressure applied during widespread vertical compression. This causes boudinaged prisms to separate into further boudins of the original prisms. Typically called "chocolate-block boudinage" in English (see also Figure 4.12)

[xxii] Geologists on extended field missions typically lose 7 – 8 kilos (15 – 18 lbs).

[xxiii] Antiform: Up-fold in which the oldest strata are observed toward the top.

Synform: Down-fold.

[xxiv] From field to publication, Philippe's block diagram undergoes very little change, something which, according to Philippe, is extremely rare.

[xxv] The age of the Earth is currently estimated at 4.55 billion years, so in comparison, Madagascan original geological formation is quite ancient.

[xxvi] Diapirism: Archean mechanism based on a density differential, by which comparatively less dense magma rises through denser crust by a process of convection.

[xxvii] Doctoral students in France typically present conference papers with their advisors, although they most often write them themselves; at the very least, the advisor's name is included in the heading. Therefore, the interpretive comment "nous suggérons que" most likely refers to both teacher and student.

[xxviii] Submeridian: Describes a fold axis that lies slightly less than a due north-south orientation.

[xxix] The term 'structural' refers to the study of a terrane's larger structures at the 'outcrop' scale, and contrasts, for example, with the microscopic study of the chemical composition of rocks (geochemistry) or the study of the mineral make-up of rocks (petrology).

[xxx] or, "billion years"; "Ma" refers to "million years"

[xxxi] A term which refers to the formation of mountain ranges.

[xxxii] The entire Field Account, Geological Setting and Introduction sections may be found in Appendix F.

[xxxiii] The process of analyzing recontextualizations of the field mission should also include having a look at Philippe's dissertation, which is projected to be defended sometime in the Spring of 2002. By his report, the telling of his field data in the dissertation is also minimized, perhaps unsurprising so given the current practice of using research articles as chapters of the dissertation (see also Swales and Lindemann (2002) for a similar account of practices across various disciplines). This rather recent shift in practice results in an extremely distinctive contrast with dissertation writing in times past, where volumes of detail were written about various *irrelevant* field details, such as "The site down the road *from the local doctor's hut*" (J. Bouloton, pers. comm., 1999). One is thus led to ask whether changes in reporting conventions, and especially attitudes toward the inclusion of such detail, have not in some sense trickled down into today's standards for writing a dissertation in field geology.

# CHAPTER 5

# "FROM SILENCE INTO SALIENCE AND BACK…": RETHINKING THE RELATIONSHIP BETWEEN THE INDIVIDUAL, THE COLLECTIVITY AND THE INSTITUTION

## 5.1 Introduction: Issues of authorship and linguistic variability

It was argued in Chapters 3 and 4 that the researcher's physical presence in the field and a fair amount of field details are typically 'silenced', or exist only mutedly; authors can indirectly refer to them, but only via a complex maze of discreet linguistic traces. By discreetly demonstrating an unmistakable field presence and their professional expertise, authors may also circuitously construct and establish their authority and believability.

There are also times, however, when explicit reference is made to a particular 'story' of some kind and this is when individual authorial action becomes more textually visible through the transgression of what is considered the norm. By referring explicitly — at least, for those in the know — to the somewhat "intimate" details of his research endeavor, it would appear as though the author seeks to shift the balance in his favor by manipulating convention and by transgressing the expected norm in what are unexpected (i.e., non-conventional), but nonetheless often communally-accepted, ways. By investigating the ways and means by which authors manipulate silences and saliencies — here, specifically, through text-based interviews and the targeted analysis of informants' texts — one can identify the author-centered motivations for textual inhibitions or exuberances, as well as explore a genre's inherently shifting agential space.

To be sure, there have been instances in the examination of the corpus of texts used for this study where the Field Account has not appeared to *exactly* conform to the norm, when authors haven *chosen* to be more loquacious than the usual muted stance. We must question why this is so for it is precisely in these odd moments of "heart-felt ruffles" (Swales 1998, p. 80), that we do get a clear — or clearer — sense of the "We were there!", where the researcher persona of the field geologist shines through the veil of conventionalized silence. That this sometimes happens, albeit in a more or less muted manner, can be seen in the following very short excerpt from an article written by world-renowned petrologist, Michael Searle, and cohorts (1992):

> "During five summers, regional mapping of the entire Central Karakoram from Hunza in the west to Hushe in the east has been carried out."

In such instances, we are led to suspect that researchers may in fact be sentimentally — or at least authoritatively — attached to their regions. For example, the temporal duration of Searle's field mission, "During five summers…" and its strong quantifier "entire" begin to take on new meaning when one learns that Michael Searle, in his circle of friends and acquaintances, has a reputation for being very thorough in his work, yet is considered somewhat eccentric, that within his inner professional circle he is known as somewhat of "a character".

According to one of my geology informants who worked closely with him during his post-doctoral years in England, Searle's personal singularities are translatable in his research style, both in the field and in the text.

NA: "Ce qui le rend intéressant c'est d'abord parce qu'il va dans des endroits où on ne va jamais, et c'est parallèlement un montagnard qui fait des trucs que le géologue ne fait pas d'habitude, eh? il monte sur des rochers, il traverse des chaînes, puis il est extrêmement précis. Et quand on discute avec lui, on peut être en désaccord sur ce qu'on voit, sur certaines photos, et encore parfois sur l'interprétation, mais là on doit reconnaître qu'il a une <u>connaissance</u> du terrain fantastique. … Et par exemple, il refuse depuis des années d'avoir un poste permanent. Il est sur un poste temporaire qu'il reconduit toutes les années, euh, il <u>aime</u> ça. Il n'a pas de

> maison, il campe... C'est aussi un très grand chercheur sur le terrain, et si je dois comparer son style, il y a toujours quelque chose dedans de très proche des Anglais qui faisaient des découvertes autour du monde au début du siècle... Il y a toujours une partie, comme la description géographique, qui est toujours très emprise des descriptions type début du siècle qu'on faisait, c'est très bien pesé. C'est en fait très agréable à lire" (N.A., from an interview, October 1999).

We also read the excerpt of Searle et al.'s 1992 article differently when we consider that "Searle" and "Karakoram" have been referenced together a total of 58 times in GeoRef as of 2001, and that "Mike" Searle is widely known in geological circles as a long-established geological expert of the politically explosive Karakoram, a mountainous region nestled among the shared borders of Afghanistan, Pakhistan, India and China and containing the hotly-contested Kashmir province. We can further consider the sheer size of the Karakoram, 150 km wide and 1000 km long, and imagine that in all likelihood Searle has covered a good part of the region on foot, given difficulty of access, either through lack of serviceable roads or political exclusion. In this case, one might argue that the choice of such apparently 'discreet' temporal and size quantifiers takes on a whole new — and a good deal of — meaning, especially for the insiders of the tight, inner-circle of the research community to which Searle belongs. Certainly, Searle's "During five summers" is hardly brash nor brazen. Yet, it is there — an 'oddity' in and of itself, given the rarity with which such expressions have been seen to occur in the corpus — and its mere presence would thus suggest that the author has something *else* to say.

Given what we know about geologists' concerns for preserving the scientific and the objective in modern field results reporting, one might be led to question what exactly it is that pushes Searle to bring notice to how much time he has actually spent in the field. Indicating the duration of his fieldwork surely has no noticeable effect on the *scientific* validity of his analysis and interpretation. Might this in fact be intended as a territorial sign-post to other researchers, "Private property: Keep off!"? Is it simply the expression of Searle's personality? Or, in Nick's words, a "coquetterie"? A thrown-in tidbit that may

do little else save to signpost the researcher's endeavors or perhaps even to mark the author's person? It is in these instances of "qualification" that we find an unexpected appearance by the author, where details of the authors' endeavors appear as the occasional elements that belie the smooth and reglemented surface of the silential fieldwork discourse we have seen.

Importantly, however, we must also ask at this point what knowledge of the author's contextual background adds to our reading of his text. Indeed, the principal question to be asked in this chapter is, how *should* it influence our reading? Should this background personal context influence our methodology for analysis? And what should its place be in our descriptions and understanding of a theory of genre? These are some of the central questions to which we will be returning, as they will be the focus of this chapter.

At this juncture, it might be useful to establish exactly what I take "agency" to mean, for as with any widely used term (e.g., "genre" or "rhetoric"), there is a good deal of divergence in definition. As a consequence, some might possibly argue that the very use of the word "agency" reveals a belief that true linguistic and genre change occur at the level of the *individual* rather than at the level of collectively conventionalized structure over time, a notion which has concretely and repeatedly been shown by various linguists to be a misguided understanding of language variation and use (e.g., Milroy and Milroy 1985, Labov 1994, Chambers 1995).

In this respect, many "modernist" and culturally-situated uses of individual and human agency are empirically untenable, such as that contended by Judith Butler (1997) in her criticism of Bourdieu, where shere takes the "modern, liberated actor" to be totally free to determine the outcome of his and her speech act.

> "I would insist that the speech act is one whose contexts are never fully determined in advance, and that the possibility for the speech act to take

> on new meaning… is precisely the political promise of the performative" (1997, p. 161).

Or by Greg Wilson (2001), who although he defines agency as "the ability to act in one's own interest," misleadingly orients this notion as a commodity that can simply be transmitted to others (e.g., students) in order to "empower" them.

These views invariably ground agency within such culturally biased attributes as "self-fulfillment and personal pleasure" (Fox and Fisher 2001), and indicate that as a product of their own times, Butler and Wilson have rooted their vision of agency in modernist, western notions of individualism (see Miller 1993, and relatedly Salager-Meyer 1998, for a discussion of the socio-historical and cultural embedding of linguistic practice and social thought). It is doubtful whether one individual, such as a teacher in the classroom, truly has enough 'invested' structural power to grant such agency, or full freedom of action, to others. Instead, using Bakhtin's (1986, p. 7) discussion of dialogicality and creative understanding, it would seem a more fruitful approach to teach students about the nature of *structure*, for only by intimately knowing the structure may one hope to manipulate and usefully transgress its boundaries. This is an idea that has also found an echo in Bourdieu (1982), who in his inaugural lesson at the Collège de France, stated that

> "Le rapport pratique ou pensé que les agents entretiennent avec le jeu fait partie du jeu et peut être au principe de sa transformation."

However, there are constraints imposed on the nature of this transformation, since the individual is shaped by his *habitus.* Indeed, Bourdieu makes a convincing case for describing what appears to be free agentive action and the transgression of convention as something that is ultimately accorded by the leeway present within the social structure (see section 1.1). As we can recall, Bourdieu argues that the cultural and institutional system makes only certain choices available to the author, and that these choices are constrained by the limits of his *habitus*, which, as a "system of generative schemes," structurally pre-adapted to the specific non-arbitrary and binding conditions posited for

action, "makes possible the free production of all the thoughts, perceptions, and actions inherent on the particular conditions of its production — *[but] only those*" (Bourdieu 1980, p. 54-55, emphasis added). Thus, the options authorized by the objective conditions of the system are effectively hidden from the subject and appear as free options; the fact that these structures are hidden offers the "illusion" of freedom. There is consequently a sense that things are as they ought to be and our view of the world, albeit constructed, appears to us as "an unchanging truth."

Bourdieu would argue that linguistic innovation and variation — or any deviance from or transgression of normalized social behavior — is made possible by "the meeting of the subversive intentions of a fraction of producers with the expectation of a fraction of the audience, thus by a transformation of the relations between the intellectual field and the field of power" (Bourdieu and Wacquant 1992, p. 105). The notion of 'relative power' here is quite important, for Bourdieu further argues that actors may transgress limitations of one social field through performativity — but only if another field allows it. Therefore, there exist a set of binding and non-arbitrary conditions that permit performative transgressions of social norms, and these conditions are communally established and activated as the actor/subject changes fields or place within a field by a relative increase in power. Hierarchical positions of power and advantage are replicated from one field to another, and although there may be barriers to entry ("gate-keepers", Berkenkotter and Huckin 1995), fields are not hermetically sealed, but permeable.

Therefore, "agency" is not to be taken as some hidden "trap door" by which an individual may simply opt out of an expected behavior and liberally innovate, but rather, as a complex interaction that includes the investment or lack of relative power, and the more or less intimate knowledge of structure, or 'the game', by which the individual may performatively construct his experience in reference to the collective and its institutionalized structures. Agency is the individual's space for action in a pluralistic

community, and as Giddens (1981) has also suggested, is the bridge between collective human action and the institution.

## 5.2 A further basis for the identification and explanation of textual silences: The place of agency in a theory of genre

While some explanation for textual silences and the reduction of the fieldwork account has revealed itself in (1) the comparison between what is possible for geologists to write about, as part of their socio-historical background, ideology and past practices (Chapter 2 and section 3.1), (2) the conventional and textually attested features of geological field reporting (sections 3.6-3.9), and (3) explanations for why certain details become silenced during genre-situated interactions between the individual researcher and his dynamic community (Chapter 4), yet one other avenue of investigation offers an opportunity to identify and understand silential structures, namely, in the instances of *overt* authorial presence where authors choose to transgress conventional silences, at times perhaps even erring on the side of unexpected and unconventional "exuberance".

A bridge between textual salience and silence may thus be found through the manipulation of textually salient linguistic traces, which in fact act as an overt and communally recognized "boundary" between the silent and the verbal. We can see, on one side of this boundary, a movement from salience into silence, where the institutionalized and codified language of the various recontextualized field reports determines the way in which the geologist can say, "I was there" without actually having to come out and "say it" (Ducrot 1973 and a "Rhetoric of understatement", and "Concealed personal stories" (section 1.3; Chapters 3 and 4, respectively).

But we can also observe a movement in the opposition direction, from silence into salience. This is the occasional breach of generic silential rules observable on specific occasions for either communitarian or personal reasons. The interpretation of an area, if highly controversial, contested or simply unknown, necessarily calls for a more extended

fieldwork account with a multitude of details in order to garner support for one's point of view and to provide substantiating evidence; or, having worked on a region for decades and being one of the foremost authorities on the subject may give one the right to transgress the silential boundary by including "unnecessary tidbits" that have no apparently real scientific bearing; or, transgressions of expected silence might also occur if a researcher considers the work of his national research community to be neglected by researchers from other areas of the world.

However, given the overwhelmingly impersonal and discreet reporting style of the Field Account, it is startling and quite noticeable when such elements occur, disconfirming the trend by allowing the author's persona to appear, letting the reader "walk along" with the researcher as he recounts his field experience. The analyst has a difficult time dismissing such cases as mere and inconsequential examples of "aberrancy", especially given what such deviances from the norm might actually reveal about the more covert workings of professional practice, i.e., actors' intimate knowledge and manipulation of its hidden structure (e.g., Bakhtin 1986, Bourdieu 1982).

Nonetheless, accounting for agency in written discourse remains somewhat of a thorny issue, to say the least. Since the early 1990's, text and genre analysts have attempted to rethink the relationship between the individual and the collectivity, which Giddens (1981) has described as "the gap between action theory and institutional analysis" (p. 161), and which Douglas (1986) has referred to as the relationship "between minds and institutions" (p. 7). However, the principal research trends that have recently treated the problem of agency in discourse have thus far not succeeded, at least in my mind, in effectively providing a concrete and explicit account of the agent's role in text construction and maintenance, nor indeed whether such an account is even necessary.

One approach is elaborated by the debate recounted in Alan Gross and William Keith's (1997) *Rhetorical Hermeneutics,* which is particularly interesting here for its

focus on scientific discourse and the study of its rhetorical practices. In this volume, D. Gaonkar takes the movement in Rhetoric to "thicken" the classical lexicon to task, for he charges that rhetoricians of science have misused the term 'rhetoric' by maintaining an ideology that remains rooted in a classical — and socially isolated — notion of the agent:

> a view of speaker as the *seat of origin* rather than a *point of articulation,* a view of strategy as identifiable under an intentional description, a view of discourse as constitutive of character and community, a view of audience positioned simultaneously as "spectator" and "participant," and finally, a view of "ends" that binds speaker, strategy, discourse, and audience in a web of purposive actions (p. 32, emphasis added).

In short, Gaonkar criticizes classical rhetoric, and its modern counterparts in the Rhetoric of Science, as being too intentionally agent-oriented to account for the profoundly communitarian nature of scientific communication. For Gaonkar, such an "intentionalist" strategy can neither "unlock the grammar of massive social formations such as 'modern science' that are propelled by 'system imperatives'" (pp. 337-38), nor show how discourse "is produced and populated with signification within a matrix of technologies — literary, social, and material — that elude the reach and the imprint of the subject" (p.337). Therefore, to take Darwin as "a Super Rhetor, bestriding history like a strategic colossus" (Campbell 1997, p. 128, same volume) is to disregard the particular historical, institutional and social intersections from which *The Origin of the Species* is issued. To replace this socially isolated, "intentionalist" agent-centered view of discourse, Gaonkar proposes a Bakhtinian – Burkian – constructionist "intertextual" view. Thus, his criticism of critical rhetorical studies stems mostly from the use of a cumbersome classical, agent-centered vocabulary that is "primarily fashioned for directing performance [including the rhetorical critic's own] rather than facilitating understanding" (p. 32).

Gaonkar can himself be criticized on several points, however, namely that he gets bogged down in a binary — and in Miller's (1997) words "artificial and unnecessary" (p. 159) — distinction between intentionalist and intertextual agential strategies, since elements of both "strategies" inevitably persist in contemporary rhetorical accounts by

nature of the rhetorical endeavor and rhetoric's epistemological underpinnings. And thus, what Gaonkar reduces to the mere "ideology" of the agent is in fact the basic but "necessary background assumption under which communication, including [Gaonkar's] own, is intelligible and perhaps possible" (Campbell, 1997, p. 121). One might also add that portraying the agent as a mere "point of articulation" of the communicative event, as Gaonkar seems to suggest, leaves us with the strangely puzzling image of an agent reminiscent of Reddy's (1979) "conduit metaphor", whereby the social actor is but an 'empty container' through which social interactions pass, implying that he unthinkingly and passively reduplicates conventionalized communicative norms.

In her earlier work, Miller (1989, 1993) has already complicated the facile relationship tacitly assumed by Gaonkar to exist between the agent's background "ideology" and the collectivity, crucially leaving space for individual *dissension* in discourse processes. Finding her source in the Greek 'agon', Miller presents a rhetorical community that is "most centrally a site of contention. … Because there are many citizens, there are differences, because there is one *polis* they must confront these differences. Confrontation is equalizing" (1989, p. 28; original emphasis). This is a point we shall return to shortly.

Nevertheless, one must take note that in the end, Gaonkar's original critique of agency in modern rhetorical theory — that accounts of it are disjuncted and 'a-communal', and not properly situated — clearly remains without a firm answer. In effect, the other contributors to the volume, in response to Gaonkar's criticisms, fail to *explicitly* replace the classical vocabulary within more currently relevant social implicatures, arguing that it is unnecessary because it is already basically inherent in their understanding or approach (e.g., Miller), although *not explicitly defined*. Nor do they adequately articulate what exactly the theoretical place accorded to the agent *should* be in a theory of society, contenting themselves with stating that it is part of the equation. As a consequence, such an approach to accounting for agency ends up being unbounded, and

ultimately, unusable for an analyst who is interested in providing a grounded and explicit account of agency.

To a certain degree, a similar inability to advance effectively on incorporating an account of agency has occurred in other social accounts of text construction and genre analysis, as well. While text analysts, such as myself, have overwhelmingly found clearly useful social constructivist approaches like Latour's limiting in its treatment of human agency, which willingly demotes "human actors to counters that can be cumulated, aggregated, or shuffled like a pack of cards" (Gross, 1997, p. 145), current approaches to text analysis largely fail to concretely take heed of the human actor in their descriptions of text. These analyses, despite lip-service to the contrary and the admitted influence of Gidden's (1979, 1984) structuration theory and the supposed role of the actor in the engaged instantiation of the institution, continue to downplay the role the individual actor plays in text construction, perhaps as a consequence of the constraints imposed by the study of texts themselves.

To be sure, it has become widely unacceptable — and one might even go so far as to say that it has become uninteresting — to study and analyze texts outside of their socially-constructed context, and in this respect efforts that focus on social structures as determinant for genre construction and maintenance remain useful insomuch as, in Gross' (1997) words, "they underline the limitations of individual wills and the degree to which those wills are constituted by cultural imperatives" (p. 145). Yet social theories of language have also resulted in the individual actor having been largely ignored in our quest for understanding and determining socially structured linguistic conventions.

One might argue that our collective dismissal of the agent's importance has also been shaped by a historically-situated cultural attitude. The theoretical and effective weakening of the agent is something that has been elegantly described some time ago, for example, as noted in Miller (1993): "Little by little the elements lose their peculiar strength; the many colors blend into one [in Latin, *color est e pluribus unus*]" (Virgil,

cited in Miller 1993, p. 80). Indeed, in light of this, and also given the success and widespread acceptance of "social approaches" to the study of text (see introduction, section 1), one might be tempted to ask why we ought to even bother with agency. It will be argued here, however, that the individual actor *does* have a useful and even necessary role to play in a theory of genre, as well as in the pedagogical implications we draw from textual analyses, especially in terms of what adherences to or deviations from the norm may reveal about social structures and the more covert aspects of professional practices.

One claim made here, then, is that social approaches to text analysis have failed to concretely address whether and how we ought to make room for agency in our accounts of text construction, despite the fact that some theoretical and practical evidence suggests that we very well ought to. Halliday (1985) for example, while speaking of a "deformable metastable structure", implies that while things remain globally in the same overall structure, the "metastable" nature of the structure allows for internal dynamic instability and change. But it is precisely in the "deformability" within the metastable that we find the trace of the individual actor.

It may be this very deformability that is visible, for example, in the micro-analysis of a corpus of texts, such as that undertaken in Chapter 3, where the linguistic variation that occurs among exemplars of the same genre, despite the centralizing tendency of reviewers and editors, would point not only to a genre's dynamic and shifting nature as the instantiated reflection of a community's ever-changing needs, but in all probability would also indicate that each instantiated text in fact very simply has *an author and a story behind it*. This "story" would be implied, for example, by Searle's use of "during five summers", or by other shifts that occur in authorial style over time. We have seen further evidence for unconventional and seemingly unpredictable variability in genre instantiations in Philippe's motivation for including what some consider unnecessary details of his work, although they have been accepted by others, (i.e., the block diagram).

It is the presence of the story, and the author behind it, that provide an explicit explanation for the well-noted "dynamism" and ongoing instability of any genre. Furthermore, as I will argue shortly, the very presence of this story is necessary and unavoidable as a basic element in the system.

However rare or covert the instances of "qualification" are, the point is that this type of linguistic variability occurs with enough frequency to skew at least slightly the best-laid establishment of general linguistic occurrences and trends (see, for example, Swales' (1990) variable "Move 2", where every step has been posited as a set of options rather than as a typical mode of operation). And thus, it is clearly justifiable to examine what variability in a theory of genre has to tell us about social structure and language use in general.

### 5.2.1 Current genre studies of agency

And yet, apart from a few researchers, theories of genre have still to take into account — in an explicit way, at least — issues of agency and writer identity. This despite the suggestion made some time ago that what we are missing in a theory of language is a synthesis not only of textual and social perspectives, but also of cognitive and agentive initiatives (Witte 1992). A first move to taking social, institutional and agential initiatives into consideration, under the influence of Giddens' "structuration theory", can be found in work by Bazerman (1994), Miller (1992), Yates and Orlikowski (1992), Swales (1993, 1998), or Berkenkotter and Huckin (1995). Continuing this trend, more recent work (Ongstad 2001) purports structuring research within "triads" so as to "bring together and problematize theories of self, world, and society" (citing Habermas 1988), thereby aiming to avoid the oversimplifications and complications which stem from leaning too heavily into only one aspect of a theory of language. However, we can only observe that a concrete account of genre that would include the multimodality of

human discursive existence is still but in its nascent stages, as Ongstad (2001) even here positions his attempt to account for all modes of text construction not as a "solution, but a provocation".

While a growing number of researchers (e.g., Askehave and Swales 2000, Bhatia 2001) promote a complex, or "integrative", approach to genre analysis occurring simultaneously at several different levels, few researchers, save Swales' (1998) "Textography" or Prior's (1998) and Ivanic's (1998) studies of the development of writer identity, have to date examined in depth how agency and writer identity might be explicitly tied to text construction in any real way, thus further failing to "bridge" the gap between micro- and macro-levels of discourse processes. This is the same gap identified by Giddens (1981) as lying between action theory and institutional analysis, whose adequate resolution would seem to depend on the elaboration of a theory of language in which the agent would play more than simply a passing role.

One other contribution to the place of agency in a theory of genre can be found in Bhatia (1993), and the association he has identified between the mastery and knowledge of genre and institutional power and authority. Although this association has long been recognized (Foucault 1972), and has been also taken up by other contemporary social philosophers such as Bourdieu (1984b),

> "… que les jeux de vérité sont des jeux de pouvoir et que le pouvoir et le privilège sont un principe même des efforts pour découvrir la vérité des pouvoirs et des privilèges."

and more recently by Fairclough (1992), the idea that a genre may be manipulated in order to promote tactical advantages has been commonly considered a element of genre knowledge only since Bhatia (1993). Here, Bhatia shows us that a genre as a professional tool, both in the business as well as the academic world, invests the individual with institutional authority, and that this individual may use, interpret, exploit and innovate new generic forms (Bhatia 1997a) through the mixing and embedding of different genres

(Bhatia 1997b; see also Fairclough 1992). In this way, we can view the writer as maintaining a space in which he maintains public relations, by saying what he 'must', while personally promoting himself through the manipulation of different genres — the interplay between Bhatia's (1995) "private intentions and socially-recognized purposes".

One further exception to this theoretical and empirical lacuna is Schryer's (2001, forthcoming) recent work on issues of agency and structure. Describing how young interns move into a new agential space when they take on the power of the 'doctor' discourse, Schryer works within a framework inspired by Bourdieu and accounts for the dynamic and parallel concepts of social structures and agency by using Bourdieu's terms "social field" and "habitus" (Bourdieu and Wacquant 1992), respectively. Thus, as Schryer (2001) argues, because genres function as "constellations of regulated, improvisational strategies triggered by the interaction between individual socialization (habitus) and an organization (field)" (Schryer 2001), they act as "trajectory entities" (Lemke 1995, p. 12), or sets of strategies that agents may use to mutually negotiate and improvise their way through time and space (see section 3.6 for an application of a "constellational" genre framework to the set of optional linguistic traces that characterize the Field Account).

### 5.2.2 An evaluation of recent theoretical contributions to genre theory

However, one might have at least a few reservations about using only notions of power, social field or *habitus* to explain and account for agency, especially in light of the highly socially-structured and binding nature of *habitus*, as it has been discussed in section 1.1.2, where any intentionality on the part of an author is considered to have been previously authorized by the social system in which he operates. As further suggested in this same section, such a structurally deterministic account of agency fails to provide an explanation for the very presence of transgressions, such as it would be explained by the

*motivations* and the *necessity* for individual engagement in a discursive interaction. While Bourdieu's macro-level concepts of 'field', 'habitus' and 'performativity' provide a useful explanation of *how* instantiations and transgressions are possible, by emphasizing the interaction between the individual and society and its outcome, they do not account for the micro-level, internally-driven motivations for action originating from within the individual, which, as Giddens (1979, 1984) has argued, is an essential element to the very existence of institutions.

Therefore, while there is clear argumentative evidence that an individual does not have "totally free agency", it seems as equally an extreme and untenable position to posit that any explanation of the impetus for individual action resides solely in society, rather than also searching for it in the privately situated motivations that lead individuals to instantiate, to transgress, or to innovate. As a consequence, despite the clearly useful structural description of a socially-authorized and regulated agential space that Bourdieu's *habitus* and social field bring to genre theory, we are still left without an adequately complete theoretical description of the driving force behind the individual actor's own role. Nor does it account for the *necessity* of human engagement, or "answerability" (Bakhtin 1990), in discursive processes.

It is therefore suggested that the failure to more actively take agency into account in a theory of language has stemmed from what Prior (1994) points to as a shortcoming in many socially based text studies. Namely, he advises that our reflections should center not around "whether writing is a social activity… or whether social formations of some sort exist. The issue is how we conceptualize social formations" (p. 485). This is seconded by Cooper's (1989) suggestion that notions of "community" are in fact not the only way to conceive of social formations. Therefore we might question here whether it is to do justice in text analysis to work solely within a socially bound framework, where the human agent is considered to be constructed purely in relation to others, and to his or

her time and place in society. In short, that the individual actor is solely the creation of his own social environment.

### 5.2.3 Pluralism as a description of the relationship between the individual and the collectivity

Returning to descriptions of the collectivity that are more strongly inclusive of the individual actor will be taken as a starting point, for they may better allow us to argue for the necessity of providing an actor-centered account of the linguistic variation found within a genre. We see, for example, in Miller's (1989) use of the Greek 'agon', "the simultaneous existence of one and many, the cooperative nature of competition, the inclusion of the outsider, [and] the continual existence of opposing arguments" (p. 28). Justification for an account of agency is also to be found in the type of "pluralism" inspired by our own occidental, contemporary, social and political convictions that make "the individual autonomous and prior to the communal and favors the *plures* of individuals over the *unum* of a community" (Miller 1993, p. 86). The case for pluralism is eloquently made by Berlin (1984), who writes that communal pluralism "seems to me a truer and more human ideal" than communal monism; truer because it recognizes "the fact that *human goals are many*, not all of them commensurable, and in perpetual rivalry with one another." It is also more human because "it does not deprive men, in the name of some remote, or incoherent, ideal of much that they have found to be indispensable to their life as unpredictably self-transforming human beings" (pp. 32-33, emphasis added).

The elaboration of a pluralisitic community, or a "plures of individuals", undeniably finds an echo in Bakhtinian theory, namely in its "intertextuality" and "dialogism" (1986), as well as "dialogized unity" and "answerability" (1990), all of which emphasize a dynamic culture of give-and-take between a structure and the discursive activities of its participants[xxxiv].

Bakhtin takes as an inherent part of the interactional system the very *divergences* each individual brings to the standardized interaction. These divergences are characterized by each actor's individuality, creativity, and need to be answerable for the events in his or her life. And thus, the fact that an individual actor may participate in one or several communities does not erase the key aspects of his or her personal life, nor the fact that this person makes decisions on a daily basis that are relative *only* to his or her own particular interests. The tensions invoked by these individual divergences are mitigated by a community's "dialogized unity", made possible by a set of shared discourses and shared cultural patterns established over time, which, as a meeting site for centripetal and centrifugal forces, as well as actor–centered and privately motivated discourses, smoothes out individual actors' divergences into overarching discoursal trends.

### 5.2.4 A point of convergence between the actor and the institution: Need-driven, goal-oriented instantiations of convention and innovational transgressions of the norm

And thus, the description of these divergences as markers of individuality, creativity and anwerability cannot be ignored or passed over in a conception of community that views social structures as actively instantiated and maintained by the ongoing activities of a group of individuals (Giddens 1984). Accordingly, elements of social psychology and Activity Theory may provide a key for linking the divergent activity of individuals with the concretization of structures, stratified over time.

While many important extensions of Activity Theory emphasize the *collaborative* and *cooperative* aspects of human interactions (e.g., Engeström 1987, 1988; Russell 1995), it is clear that this description is inconsistent with the account of the academic and scientific world that has been developed here, which has, following Bourdieu (1984), Berlin (1984) and Miller (1993), instead emphasized the perpetual rivalry between

members of the collective. This is a view that is also consistent with that presented in the literature (e.g., Latour and Woolgar 1979; Knorr-Cetina 1981; Latour 1984; Rudwick 1985, 1996; Myers 1990; Miller 1993; Fredrickson and Swales 1994; Berkenkotter and Huckin 1995; Salager-Meyer 1998, 2000).

What *is* interesting and complementary from Activity Theory to the theoretical framework developed thus far, namely the notion of "*goal*-directed, *historically situated*… human interactions" (Russell 1995, p. 23; emphasis added), results from the original work of Vygotsky (1978), which emphasizes a mediated, triadic structure between the subject (actor), an object (goal) and mediating artifact. What is especially crucial here is the notion of an individual's *goal* as the driving force behind all human activity. As a way of elaborating on this idea, we might imagine that an author (subject) effectively makes use of a strategy (mediating artifact) — either genericized or innovative — in order to achieve a goal (e.g., communicating findings through a public paper presentation or the publication of research, seeking funding, proposing a project, etc.). This activity is set in motion by the presence of a particular *need*: the need to provide for oneself, the need to find a life partner, the need to be answerable for the events in one's life.

Such "need-states" (Leont'ev 1981) are a basic psychological condition of human existence, and the need to fulfill a particular goal would thus appear to be the driving force behind most, if not all, instances of human linguistic interaction. Because needs originate with the individual who is "one of the many" (Miller 1993), there are many, often conflicting, needs, accounting for the existence of divergence, rivalry and confrontation. Because needs at various levels are a principal element in a person's life, that person is always driven to act, indeed, must act by necessity in order to ensure his well-being.

The individual must work to fufill his need, and does so by strategically setting a goal within the framework of a pluralistic community. The individual's participation in

the community, which we might also call a "social field", following Bourdieu, results in the smoothing out of individual divergences, due to the constraints of one's social group embedded identity, or *habitus*. Bakhtin would further say that that these differences are also minimized through a process of ongoing dialogicality, in other words, that an individual's identity is constructed through his linguistic interactions with his social group. Thus, partipants in the same community, by sharing a similar *habitus* and similar dialogical processes, come to share attitudes and ways of seeing the world, while retaining individual differences, motivated by private needs.

The choice of strategies by which an individual's need-driven goal can be met is linked to the individual's knowledge of the stratified social interactions embedded within the contextual frame of a particular social field, or its 'genres' (e.g., a job interview, asking for a date, getting one's research published, the act of voting; for other examples, see also Russell 1995). At times, the strategies of a group of like-minded individuals have become conventionalized, through a Bakhtinian process of "dialogized unity", leading to the momentary stabilization (Schryer 1994) of the communicative interaction. Thus, genres as instances of typified language exist, are created, maintained, achieve institutional force and are "instantiated" (Giddens 1984; Bazerman 1994), but only through human "engagement" (Swales 1993). A group of actors effectively reifies and, over time, contributes to the consolidation of typified genre structures, but crucially, they do so because they are driven by necessity of meeting their own, particular needs.

The enactment of a goal-oriented strategy thus accounts for repeated instantiations and the maintenance of convention; likewise, individuals' intended need-driven goals may also cause them to choose a strategy that *transgresses* one norm to enact another, for example by using a strategy that has, itself, become conventionalized over time (e.g., a rhetoric of understatement). However, one might also suppose that there are instances where no conventionalized structures exist for obtaining a goal. In order to fulfill the particular need, an individual must therefore devise an innovative strategy that depends

on the situatedness of the moment, seen for instance in ‘concealed personal stories’ or the other types of linguistic innovations the characterize genre’s inherent linguistic variability and instability, as reflected in the stories we will see in the following sections of this chapter.

Using Cole and Engeström (1993), we might refer to these as “local innovations”, that is, the enactment of strategies that, while they do not *influence* or *change* the system in an immediate way, are nonetheless a reflection of the particular needs of a particular individual at particular points in his professional career. What is interesting about such personal strategies is what they may further reveal about the hidden practices of a community and its hidden structure, e.g., why a young researcher might be pushed to maintain his personal voice in a context where his act is a clear transgression of some silential norm, where and why his community may allow it; or why a researcher over time might be invested with the authority, or the power, to widen his perceived agential space.

It is furthermore in these privately, need-driven instances of strategic innovation, where the possibility for the transformation of the system does arise. The system is a hierarchical social structure based on the organization of power, where the manipulation of its structural elements may result in the transformation of power distributions (Bourdieu 1984). As suggested by Bourdieu (1984b), Foucault (1972), Bakhtin (1986) or Fairclough (1992), it is in the knowledgeable manipulation of a structure, in all of what it offers and does not, in other words its presences and its absences, its saliences and its silences, that one gains the power for change, and may effectively transform the system. However, the scope of the transformation driven by individual action is tightly constrained by the conditions of interaction between self and society and the binding constraints of identity, or *habitus* (Bourdieu), and thus the individual alone may not easily transform the power structure; but he does offer the *possibility* for change.

The foregoing "interweaving" discussion and synthesis of the various theories currently being used by various analysts in genre theory , all of which in one way or another address some particular aspect of agency, provides a theoretical framework for better understanding the tripartite relationship that has been assumed to exist between the individual, the collective and the institution (Witte 1992). Unsurprisingly, these various theories taken together give a vision of the relationship that is based on activity, motivated engagement, divergence, dynamism, and change. It is in the places of convergence between these different theories of society and cognition that we can better understand not only the author's active place and role in his community, and his motivations for instantiation, transgression or innovation, but also how his field of action is effectively constrained by his unconscious cultural identity. His cultural identity, contained in the theoretical notion of *habitus*, leads to effective constraints not only on the extent to which he is free to implact or influence the system, but also explains why a group of individuals may become "like-minded", and share similar strategies for enacting goals.

The elements of a "need-driven goal" seem as appropriate to the epistemological underpinnings of genre theory as do social approaches to text, for while genre theory has accepted and adopted the notion that the construction of a text is made possible only by a complex interaction between an individual, or group of individuals, and their community, it has also for the past twenty years been largely concerned with identifying and classifying the conventionalized rhetorical *strategies* (embodied by genres) by which a group of individuals fulfill a set of goals. We can see the enactment of a goal demonstrated in the various descriptions of research genres, such as how researchers may use the forum of the article introduction in order to establish their research space (Swales 1990), how researchers may convince a review community of the interestingness of their research (Yakhontova 1998; Berkenkotter and Huckin 1995), or similarly, how geologists may use the forum of the Field Account as one means for gaining recognition of their

competence, credibility and authority as field geologists within their community (Chapter 3).

While Chapter 3 has focused on the identification of these conventionalized strategies and, along with Chapter 2, has worked to establish the "*institution*" of geological field reporting, and Chapter 4 has examined the emergence of conventionalized strategies during the various generic recontextualizations of a research activity, showing us how and where certain details of the activity are marked for distillation or removal as determined by the *community's* frame of relevance, the present chapter has a different aim, namely, to examine the formation and maintenance of *agential* space through text, in order to highlight what this shifting agential space adds to our understanding of the system of textual saliences and silences outlined here.

## 5.3 Outline of the chapter

Building on Bourdieu's concepts of "field" and "habitus", Foucault/ Bourdieu/Fairclough's "power and transformation", Bakhtin's "dialogized unity", Miller's "plures of individuals", Berlin's "pluralistic rivalry", Vygotsky's "triadic goal-mediated structure" and on Leont'ev's "need states", the remainder of this chapter will explore how the intersection between these concepts provides an effective site for the agentive transgression of social norms, or "need-driven performativity" within the text, thus developing an explanation for the linguistic variation that one can observe between exemplars of one same genre. In so doing, we will observe which identity-shaping elements constitute a field geologist's *habitus,* if we are to assume, like Jenkins (1992) that it is possible for *habitus* to be acquired through contact with certain fields beyond childhood as an adult (section 5.5). Furthermore, we will look at the way the organizational "field" operates within geology, as a dynamic "community of practice" that is governed and regulated by overarching, institutional norms (section 5.6). It is

within the intersectional framework of the organizational field, replete with its conventional structures and sets of norms of behavior, and the actor's socially-structured *habitus*, that we can view the "dialogically unicized" text as a site for performativity, where variability (e.g., details about the research account) is driven by need-based and goal-oriented linguistic innovation (section 5.7).

Such an account of genre which affords a working and breathing space for the individual author may help better describe and explain the instances of genre variation observed in my corpus of geological Field Accounts. As pertinently noted by Bhatia (2001) in reference to his own corpus analyses of genres of text, "There are no pure genres." However, while he relates the need to take "50 or so samples" to find the perfect illustration of a genre for teaching purposes to genre's "ever-changing nature", one might also be tempted to ask exactly what it is that makes a genre ever-changing? Of course, as we know, genres change over time as a result of "instantiation" (Bazerman 1992) and "engagement" (Swales 1993). But we might also be well advised to remember that the variations found within a set of genre exemplars are perhaps simply explained by the specific occasions of genre instantiation as they are tied to issues of author identity, life experiences, status, specific community values and practices, "ways of being" in the world (Geertz 1988), both professionally and personally.

Regarding genre as a constellation of negotiational and improvisational sets of strategies which are available to individuals (Bourdieu and Wacquant 1992, Lemke 1995, Schryer 2001), but which arise in effective response to a particular "need state" (Leont'ev 1981) and which, when they are not available to individuals, result in locally innovative linguistic strategies (Cole and Engeström 1993), may effectively provide a framework for investigating the types of generic variations which are found to typify textual "exuberances and deficiencies" (Becker 1995), or the movement in and out of silence characteristic of field reporting practices in geology. It is the author's personal 'story', along with issues of motivation, conditions and means for telling the story that may

further elucidate silential transgressions by uncovering the tacitly accepted and occulted elements of a community's organizational structure.

### 5.3.1 Methodology and research objectives

In this chapter, I report on the results of a series of text-based interviews I carried out over an initial period of six months (May 1999 through November 1999) with three French field geologists (one structuralist/tectonician, one structuralist/ geochronologist, and one geochemist), all working and teaching within the same Earth Sciences department in Clermont-Ferrand, France. The interviews consisted of two parts, namely a set of questions about geological practices, and the other, a set of questions and comments prepared after reading a choice set of their field-based publications. The interviews revolved around a set of three-to-five articles the informants had chosen to give me in advance, as a sample of their field reporting in English, of which the earliest sample is the first article they wrote based on their dissertation research[xxxv]. For this reason, the informants may have had more to say about their own process of inculcation into the practice of doing and writing about field geology.

Each interview, conducted in French, lasted about an hour and my informants responded to a series of questions I had prepared to orient the discussion, inquiring about their area of specialty in relation to the rest of the discipline, their professional background and how they came to be geologists, the nature of doing fieldwork, and how fieldwork is reported on in the research article and what sort of information is put aside. The list of these questions can be found in Appendix G. I have recontacted them periodically in the now nearly two years since our original interviews, with small-detail questions, and more in depth as I have asked them to read and respond to my accounts of our interviews and their texts.

Each interview was tape-recorded and has been transcribed in its entirety, and in this chapter I have looked for ways of allowing the researcher to tell his own story, both orally through the content of the interviews, and in his published articles where he reports on the fieldwork he has done. The tack taken here has been to let the geologists and their texts "speak for themselves", in terms of their overwhelming features, and also, to give full voice the geologist-informants who do not always agree with my interpretations of their authorial intent. In short, I have tried to adopt a strategy that very loosely attests to the "way of being" of the authorial field geologist — doubtlessly not as fully as one might do this, such as with a 'Textography' (Swales 1998), where the "discursive lives of individuals made within the complexes of organized communication and social relations, mediated through writing" (Bazerman 1998, p. x) have been uncovered and recreated. Instead, it is one that attempts to establish the professional sphere of the researcher, where his own desires, ambitions, and personality inevitably come to play in his day-by-day written work as a field scientist. Clearly, for field geologists, their professional persona operates as "a way of being in the world" (Geertz 1988), influencing who they are beyond the doors of their offices and the confines of the field mission. This way of being in the world constitutes their field geologist's *habitus.*

It goes without saying that field geology engenders a 'certain' type of geologist — and individual —, perhaps some vestige of such cultural values as the 'explorer' and 'traveler' (J. Giltrow, pers. comm., 2001) or the 'rugged individual' (Rudwick 1985). Here, in marked and purposeful contrast to the traditional skepticism and distrust involved in social constructivist accounts of the individual author, I am working from the very basic assumption that these authors can be cajoled into saying valuable things about their fieldwork experiences and their texts, which would not only enlighten us about the reasons for their own agent-centered — or at times, agentless — discourse, but by there doing would also inform us about social practices and structures. Therefore, this chapter

attempts to further delimit and identify the social boundaries that cause people to write as they do.

Accordingly, the interviews reported on here and the accompanying published articles have been structured in terms of three main lines of inquiry. The first theme works to establish the field geologist's *habitus* (section 5.5). And so we might ask, who exactly is the author? How does he 'get by' as a geologist? What does the field geologist-author need to tell us about himself? Persuade his community of? Authority? Credibility? Funding? The quest for truth? And how does he do this? We must also ask whether issues of writer identity can be identified and explained. Can certain markers be taken as a point of departure to talk about authorial presence in a text? Can we expect marker frequency to indicate an author's position and relative status within the research community? Does the author mark his presence more overtly as time goes on and he becomes established and weighted within the community? Does he mark his persona differently depending on the circumstances?

The second theme of inquiry relates to issues of the author's immediate community and its practices (section 5.6). Where do discoursal conventions, with their silential boundaries surrounding the fieldwork expedition, begin to take hold? In the composition of his text? In the field? In the collective geological mind? How and when may authors transgress silence? Relatedly, how do experienced researchers know to maneuver within silential areas? And why can certain experienced researchers "get away with" using apparently non-essential details in field reporting?

Finally, as a way to link the micro and the macro, we can, as Miller (1992) suggests, take the structure of genre as a link between community practices and authorial space by examining how the 'textual' intersection between the social field and habitus may act as a site for identifying and explaining the ways in which the author may create, maintain and sometimes yield his agential space within the conventional silential zones surrounding the Field Account (section 5.7). Here, we might ask what exactly is the story

the text is telling us. Can we expect that individual style and other text features may not only indicate parts of an intimate "story", but also inform us about the place of the individual within the plurality?

In the answer to these questions we may find the motivation for the move from textual silence into salience, and at times, silence into deeper silence still. This part of my work departs then with the premise that all scientific texts have a story to tell. Thus going beyond what social construction has very usefully added to our understanding of text construction, we will focus here on individual stories and the personal needs they recount as an additional source of information.

At times, it is in the interplay between an adherence to silential conventions and the manipulation of salient and silential transgressions that the story is to be found. For others, their *silence* is the story. What follows are the accounts of three geologists who talk about their field experiences, their writing tasks, and in the process, a little bit about themselves, prefaced by a short description of my own role as a researcher and author.

## 5.4 Dacia: The observer amateur geology neophyte and a qualitative description of the interviews with a group of geologist-informants.

It seems only fair to begin the tale with an account of my own role in the following descriptions of authors, for it is a story which I believe has had a non-negligible impact on the sorts of information I was able to elicit from my group of geologists.

I have been hanging around with geologists for some time, for nearly the past 13 years, in fact. My husband is a geologist, and my first introductions to the geological community came during a time when he was doing his graduate work at a university in Paris in the late 1980's and early 1990's. Going to parties with his classmates, going bowling with his friends from the program, sitting in on their Master's and Doctoral oral defenses as they became my friends, too. And later, spending a good deal of time with his

friends from his doctoral and post-doctoral years, playing soccer, going for dinner or out to the pub, some with whom we have kept close contact. Although I have had no formal schooling in geology, or even really the natural sciences for that matter, over the years I have learned quite a bit about the discipline "through osmosis", as I have described it. And, perhaps inevitably, I have developed a real interest in their debates, subjects, and work, both in the field and in the lab. My turning point as a neophyte geologist was my husband's oral dissertation defense in experimental petrology, where by straining every cell in my brain to follow his presentation, I realized I was actually understanding something of his subject: Osmosis had begun.

While my husband's work does not directly take him into the field, being an experimental petrologist, he does nonetheless go into the field at various times, either as instructor with a group of students, or invited by a colleague friend who has a specialty in a particular region. I myself have accompanied him on a couple of field outings; to Japan where in the periphery of a conference, we visited a very recently formed volcano in Hokkaido (the 1940's) which has since re-erupted. I was elected "honorary geologist" for the day by some in the group and got to wear a yellow hard-hat like all the other geologists. Another field outing took us to the Black Hills in South Dakota, where a good friend of my husband's, an experimental petrologist but also a world-renowned authority on the area's Harney Peak granites, took us around for a couple of days with two of his doctoral students on a more substantive field trip. Their detailed and very technical discussions went quickly over my head, and I largely spent my days climbing around the rocks with my dog, waiting for our evening swim and marshmallow roasting. Osmosis was not complete, but was in the process. But I have learned to always watch for details and pay attention to surroundings, a way of looking at the world that sees its overwhelming network of details placed within their proper places.

And so, geology, while it may not be the sole "way of life" at our house, is nonetheless part of its foundations, and thus I have not come to this study as the purely,

or attempting to be, objective geological outsider as one might hope for the sake of scientific objectivity. It is questionable whether such outsider objectivity is even truly desirable, for closeness to my object of study may in fact be a necessary precondition for rendering an account in which even geologists may find themselves. Instead, I have brought to the task my own "creative understanding" (Bakhtin 1986, p. 7) of geologist's work and lives, doubtlessly shaping my account in different, although perhaps not irrelevant, ways.

So much for myself. However, there is also one other crucial point to be raised, in terms of how the interviews were conducted. As is often said in discourse analyst circles, the best "in" to a discourse community occurs through a spouse. And so I have held a privileged position in this sense, knowing the geologists I have interviewed for this study in a personal capacity before interviewing them, as they all work in the same laboratory as my husband. While the doctoral student, Philippe, was someone I contacted directly after having known him for some time, the other three geologists were first approached by my husband, their colleague, who handed them each a brief written account of my research project that I had prepared in advance, and at the same also gave them a rehearsed (with me) verbal context for the study, asking them if they would mind if I came to talk to them about their fieldwork practices. He also at that point asked them if they could give me some samples of their professional writing, or research articles in which they had recounted fieldwork missions.

They therefore knew a fair amount about me, the linguist interested in geology, and our personal lives, before I ever sat down to talk to them. One geologist, Olivier, I interviewed when I was nine-months pregnant with our second child — not really a trivial detail, as I will argue shortly. They have all met our children, and have children of their own, one of whom is about the same age as our oldest child and who have played together on several occasions. Another, like us, has three children, making for a common subject of conversation at work with my husband. And the other shares a similar story for

difficult times of uncertainty trying to secure a permanent job after the dissertation, with children already in tow. And so our lives cross other social paths outside of the offices and rooms where our interviews took place.

Although it is considered highly objectionable in academic quarters to include such seemingly, irrelevant "personal" details, I would strongly argue that this background context is in fact extremely important — and contextually appropriate — for establishing and understanding the methodological conditions in which my interviews were conducted: a level of trust had already been established, for we knew each other, and through my husband's intervention, they were familiar with my research and had agreed to be interviewed. I went into the interview knowing that the hard work of 'proving myself' had already taken place. The trust that had been established prior to the interview is crucial for understanding the informality of our interviews and perhaps the quality of information I was able to elicit. I was thus able to avoid the down time, or time it would have taken to get them interested enough to want to talk to me, not to mention "bare their souls" to a certain degree; in particular, to talk about instances where silence comes into play in their writings and how transgressing this silence on certain occasions might require a certain level of trust.

Just as Bhatia (2001) has recently described in his depiction of his own difficulties in interviewing a group of lawyers, it took some time to establish meaningful contact with them given that to start, their response to his interview requests was typically "We don't have the time." The group of what I have come to call "my geologists" has always very generously had the time, even in moments when I've interrupted important work with the "I promise, just one quick question" starter to a half-hour discussion. I have without a doubt been able to use my personal connection to work them hard for information, but hopefully, have not been overly abusive of their generosity. Given that I have many geologists for friends, I have throughout this study

striven to provide a fair, accurate yet heart-felt description of their "ways of being in the world".

## 5.5 *Habitus* and authorial identity: A sense of the field geologist's "way of being in the world"

For many geologists, I would imagine, understanding their textual place as an author also involves placing them in the historical context of their disciplines (Bazerman 1988) and getting a sense of their professional identities. This approach to situating the author is not unique (see Swales 1998), but the approach that is adopted here is perhaps less prevalent than it should be in text-oriented genre analysis and ESP circles. As Clifford Geertz (1988) writes, "I am emphatically not one of those who believe in wholly autonomous 'ontological' texts, and doubtless biographical and historical matters are far from irrelevant to the interpretation of [anthropological] works" (p. vi). In this, my three specialist informants are no exception, as they have intimately linked their personal and professional selves by firmly entrenching themselves in their "field geologist cloaks", as we will see in the following sections.

### 5.5.1 Olivier: "Le terrain sans fantaisie"

Olivier is a French structural geologist in his now middle 40's currently working in the Geology department at the Université de Blaise Pascal in Clermont-Ferrand, France, where he has held the status of professor since 1995, an impressive achievement by French standards for someone so young. He has been an active researcher since the late 1970's, and his first publication of fieldwork dates from 1981. While he moved quickly from the status of student to that of permanent established researcher, finishing his dissertation in 1982 and landing his first permanent job in 1984, he did take time for a post-doctoral fellowship, but this occurred much later in his career, after he had already

been ‘institutionally’ established for six years. In 1990, he left for Arizona with his wife and first child, to spend two and a half months in the field.

Olivier the researcher still publishes quite a bit, as much as, if not more, than he has throughout his now 20-year career, especially when we take into account his joint publications with his doctoral students, in addition to his own research. The scope of his publications has changed somewhat over the course of time, however, in that for the time being at least, he no longer publishes articles based on his fieldwork, which is no longer the primary emphasis of his research, but rather on the analytical work he does in the lab. His most recent article reporting on field research dates from 1993, where he reports on the fieldwork he did while in Arizona in the early 1990’s. He now shares his time between developing analogical models to explain geological structures and working with his students, one of whom has recently defended his dissertation fieldwork on the Massif Central Rift of the Limagne plain on which Clermont-Ferrand is situated.

One way of unveiling the ways in which a professional considers himself and his work, in other words, his personal estimation of his professional identity, is by having him talk about his work in all of its details. When I asked Olivier to describe what could be considered to be a typical “day in the life of a field geologist”, he responded, speaking as an experienced and established field geologist, that in his opinion there are in fact two kinds of field research. There’s “real” fieldwork, a solitary endeavor where the geologist finds himself alone in the field for long stretches of time, a comment vaguely reminiscent of Rudwick’s (1996) analysis of field work dynamics in terms of what the researcher is able to develop as novel ways of interpreting data, far removed from the constraints of the community’s “way of seeing”. Then there are what he calls “group expeditions”, where little productive fieldwork is actually done.

OM: “Ça ressemble plus à mon avis à des excursions, qu’à du travail de terrain, euh, c’est ce que moi j’ai connu, c’est à dire qu’on a un véhicule, on se déplace, on fait plus du bord de route qu’autre chose, on s’arrête, tiens il y

> a un affleurement, on s'arrête, bon. Eventuellement, on peux s'éloigner de la route un petit peu mais c'est pour voir une falaise qui est un peu plus loin, on prend la voiture, on repart, etc. Ça ne m'a jamais paru, euh, très très productif. Ça peut être intéressant, ça pourrait être un travail du départ, en fait, avant de travailler sérieusement, mais c'est plus du débroussaillage qu'autre chose. Et c'est plutôt le genre de terrain que je fais maintenant, comme j'ai moins de temps, je vais plutôt sur le terrain avec des étudiants, soit ils m'amènent là où ils ont travaillé plus sérieusement, soit on part avec quatre, cinq, ou six du labo et puis on essaie de voir des choses. A mon avis, c'est pas le vrai travail de terrain…"

But the kind of fieldwork that Olivier has engaged in until recently, the kind that has the makings of memoirs, has today become impracticable for him as his commitments at work and with three kids at home make it difficult for him to be absent for long periods of time. As he explains, the conditions of doing this type of fieldwork are quite different, and in fact quite demanding, requiring a firm commitment of both time and energy. Earlier in his career he engaged in this "real" fieldwork, what he calls "le terrain de montagne", and seems to have been intimately shaped by these experiences, although he no longer does fieldwork in this fashion.

OM: "Il y a le terrain tel que je l'ai pratiqué pendant longtemps, qui est le terrain de montagne, où tu pars pour longtemps… Un mois, ou plus. Mais après un mois, déjà, t'as envie de rentrer (we laugh), et euh… je ne parle que pour moi, je ne sais pas comment travaillent les autres, je connais un tas de gens qui travaillent différemment. Mais moi quand je vais sur le terrain, quand j'allais sur le terrain parce que je n'y vais plus de cette manière là, j'ai fait ce genre de terrain jusqu'en… ben, en Arizona [NB. 1993], j'en faisais encore, mais depuis je ne fais, je ne fais plus ce genre de terrain. J'y vais pour une période assez longue, je reste sur le terrain pendant un mois, un petit peu plus, en Arizona c'était pendant deux mois et demi, mais c'était déjà différent parce que j'avais ma femme, j'avais déjà un enfant…"

What I understood Olivier meant by "real" fieldwork is essentially a solitary endeavor, where the researcher is left alone or seeks to isolate himself from his community, with his observations and his interpretations, living with them and only them

from sun–up to sun–down for an extended period of time. This process of communing with nature and the researcher's own private thoughts without any interference from others is what Rudwick (1996) has described as a "liminal" experience, whereby true theoretical innovation is made possible only by freeing oneself from the constraints of the community's way of seeing and understanding. The conditions for having access to this "liberty of thought" are obtained at a physical and emotional cost that is not negligible for the researcher, who engages in intense intellectual and physical effort every moment of each day spent in the field. While some have described fieldwork as an endeavor marked with "romance" (e.g., Rudwick 1985, p. 41), in reality it is one that can grow in affective importance only at the end of the field mission. While in the field, Olivier describes being simply too busy to do anything but the "nitty-gritty" of the task before him:

OM: D'habitude, quand je vais sur le terrain en montagne, j'ai une tente, je vais dans un camping, je me lève le matin à 7h, à 8h je quitte le camp, et j'amène ma voiture le plus haut possible en montagne, après je pars avec mon sac à dos, et c'est partir pour toute la journée, rentrer le soir vers 6h, manger, se coucher, pour se lever le lendemain, sans samedi, sans dimanche, pendant un mois, un mois et demi... c'est pour ça que t'as envie de rentrer. Je connais des collègues qui font ça plus cool, et qui restent sur le terrain deux mois, trois mois. Pour moi, une journée type de terrain c'est ça, c'est partir tout seul... Donc ça c'est [le type de terrain] que j'ai pratiqué pendant des années, seul, pendant un laps de temps assez long et euh, de façon très rythmée, sans fantaisie, sans samedi ni dimanche, réveil à 7h, tu es tout seul dans ta tente, et à 8h je me couche parce que je suis crevé, voilà...

However, Olivier's vision of doing fieldwork also tends to be somewhat "old-fashioned", and in practice, few geologists actually "do" fieldwork in this way any longer. This difference is perhaps driven by two personal variables. First, the areas of his principal field studies, while they included the remote outbacks of the French Alps and the Colorado Rockies, kept Olivier relatively close to home in contrast to the "wildly exotic" destinations sought by many in their choice of field missions. Moreover, given

that his most recent field mission dates from the early 1990's, his temporal distance from the more recent practices of the community, which today require that several researchers with various specialties group together on field missions, given the high cost and at times danger involved with doing fieldwork, may also leave us with the impression that Olivier has worked in by-gone times. Nevertheless, the way in which Olivier recounts what for him is a typical "field experience" gives us a good sense of the momentous and serious undertaking he considers fieldwork to be, its long, hard physical hours, its solitary nature, and its intellectual demands.

We further get a sense that a field geologist, trained as he has been, has also necessarily been duly forged along the way through his solitary interaction with nature, in a way similar to what Rudwick (1985) has described for learning to "do geology" in the nineteenth century, at a time when there was not yet an institutional structure for transmitting disciplinary knowledge. The geologist learned by "doing" and by comparing this with what his more experienced colleagues were capable of doing. In current procedures of training new geologists, this practice has in fact changed little, according to other the geology specialists from the same department as Olivier, responsible for training students in the field. Olivier describes the process, at least for himself, as follows:

OM: "Oui, ... mais si tu veux, quand moi j'ai démarré, on m'a dit, tu vas aller travailler dans les Alpes, à tel endroit, tu vas prendre une carte géologique en bas à la bibliothèque, et tu y vas. Donc (he chuckles), j'ai pris ma voiture, je me suis planté quelque part, j'ai pris ma tente, etc., puis le premier jour, j'ouvre ma carte géologique, et j'ai regardé autour de moi, il y avait des sommets à trois mille mètres, et euh, débrouille-toi, bon! (he laughs). Je pense que ça a du bon, et ça a du mauvais. Ça a du bon parce que ça t'apprend à te débrouiller tout seul, … mais en même temps tu perds du temps parce qu'il y a des choses qui seraient mieux qu'on te dise tout de suite, alors tu perds du temps à comprendre ce que c'est."

And so, given already these few comments, we have some idea of the formative experiences that have given shape to Olivier the field geologist, framing the situatedness of his practice and providing the elements of his *habitus*. Although it is difficult for a non-field geologist to fully describe the field geologist's identity, I *can* relate what all the field geologists I have spoken to have said about this identity. Notably, they report having a strong sense of belonging to a very particular social group, where individuals most frequently run into "contingent" difficulties in the carrying-out of their daily research (e.g., Scholz' 1997 memoir on his 1970'a fieldwork in the Kalahari, or F. LeGros' account of being held hostage at gunpoint by local villagers). These difficulties are further highlighted within the context of what Olivier himself has said about his own experiences, and using these experiences, we can begin to draw together an image of this field identity.

One element in defining the field researcher's collective *habitus* is to be found in the early days of a geologist's participation in the community, where as a student who must go out in the field to learn to map, he gets a first taste of what fieldwork is really like, for example, the fact that when it rains, one gets *wet*. That at the end of the day, one is simply tired from walking around. There is therefore a pre-selection that occurs even at the earliest stages, between those who bring a previously established "taste for travel and life in the outdoors" (Rudwick 1985, p. 1) to the endeavor and those who "don't much care for it", thus orienting some back into the warm shelter of the lab. In other words, in order for the field geologist to succeed in his mission, he must very simply already be pre-disposed to accepting the sorts of conditions that Olivier has described, in order to even get through the training.

As a second element to this identity, we can see that geologists consider fieldwork to be an accomplishment, both physically and intellectually. In addition, these difficult and daily conditions imbibe the field geologist with a sense of "difference" from others, who, like the anthropologist or field linguist, spends an extended period of time cut off

from his or her own social circles and is surrounded instead by an entirely new and unstable, for unknown, environment. During the time of interaction between nature and man, to be sure, the geologist seeks to tame the puzzles of nature, making them more coherent and understandable. However, it is clear that it is also Nature that forges the geologist. Field geologists therefore have described coming back to "civilization" and "the office" as having been somehow altered by their "otherly" experiences, underscoring their sense of difference from their circle of fellow peers, and especially from those who have not been initiated into the "brethren of the hammer" (Rudwick 1985, p. 41).

### 5.5.2 Nicolas: "Un reste de mes amours enfantines…"

In his middle 30's, Nicolas is a young and, as some have said to me, brilliantly successful and fast-rising geologist, holding a key structural position in France's nationalized scientific organization, the C.N.R.S, and having an already internationally-established reputation given his numerous publications in English-language journals and books (33 overall). His way of doing fieldwork differs significantly from Olivier's, and he falls into that category of field geologists who still go off at least once a year to exotic and far away places to do fieldwork. Like Michael Searle, Nicolas's primary terrain of predilection is centered near the Karakorum fault, although he works primarily on the Kunlun, the only practicable passage between the Karakorum and the Himalayas. He focuses his field research far to the north of Searle's research sites, working at 3500 meters altitude in Tibet's Himalayan, snow-covered glaciers.

For Nick, fieldwork is a "group endeavor" for not only is there a personal risk involved in going off by oneself in such areas of the world, but field missions of this sort are exceedingly expensive and nearly impossible to pull off today without the contribution of a group of people actively seeking funding. The group nature of the mission is true even to the extent that the final interpretation of the study area is mutually

constructed as the synthesis of the various participating researchers' specialties. As Nicholas explains,

NA: "Lorsqu'on va dans des pays lointains et disons dans des zones difficiles d'accès ou dans des coins dangereux, on ne part jamais tout seul. [Confidingly] ou toujours il faut être au moins deux… pour des raisons de prudence, mais aussi d'efficacité, on y part toujours à plusieurs spécialités, parce que ce sont des missions qui sont assez chers, et qu'on essaie de mettre des gens qui auront des vues très différentes sur les mêmes cailloux, pour qu'ils soient complémentaires … avec l'idée qu'on parte avec un groupe où il y a suffisamment de similarités pour que les gens puissent se parler, donc qu'il y a un terrain commun, mais qu'il y a aussi suffisamment de différences pour que les gens puissent apporter quelque chose…"

Because of the financial and logistical difficulties inherent in organizing such field missions, Nicolas's field experiences do resemble Olivier's in the sense that they lead to social separation and isolation. While he is not "socially isolated" as is Olivier, alone in his tent for weeks at a time, he and his fellow co-workers are isolated from their familiar surroundings for a period of a week to several weeks, leading to the development of the same notion of "difference" from their familiar surroundings and social contacts upon their return.

At one point, Nicolas spent a period of three months in the field in Tibet during his doctoral research and, in describing this period, echoed Olivier's own description by confessing that it really was difficult to be away from home and the familiar for so long. However, rather than pointing to a necessity for theoretical innovation and "seeing more clearly", Nicolas clarifies the need to engage in this "otherly separation" as something motivated purely by practical concerns. As he describes,

NA: "… c'est très difficile de l'atteindre. Quand tu vas au milieu du Tibet, c'est que tu as déjà pratiquement une semaine et demi de transport. T'as un jour pour voler à Pékin, un jour pour voler de Pékin à une ville pas trop loin du Tibet, tu prends la voiture et tu vas là où tu peux, et ensuite t'es encore à … Mais trois mois, c'est beaucoup trop long. Ça va quand on est

> en thèse et on n'a que ça, mais euh, je crois que c'est aussi par période dans la vie. Il y a des périodes où tu peux partir beaucoup plus facilement et puis tu deviens un peu casernier avec l'âge euh, avec la famille, euh. Donc, j'aime toujours parce que ça fait vraiment partie de ma façon de voir la géologie, mais je pars beaucoup moins longtemps. Classiquement, je dirais, maintenant je pars sur des périodes de deux ou trois semaines."

Interestingly, Nicolas places the development of his professional identity, or the accumulation of the cognitive and affective dispositions making up his adult *habitus*, squarely within the confines of doing geology in France, such as it was described in Chapter 2 (see sections 2.7 – 2.8). The ambitious and hard-working young geologist was thus very quickly aware of the complex problems facing geology as a discipline in France in the latter part of the 1980's. As such, he had very early on a clearly-defined idea of what would be the best route to becoming a 'good' geologist, and so ... first specialized in a Mathematics program, thereby showing a keen awareness not only of the internal debates that had been raging in French geology for the past 65 years, but also an acceptance of what the ultimate outcome was to be. While university students in France have the option of choosing their specialized areas as early as their first year, Nick chose to specialize in geology starting only in his fifth year, figuring it was "now or never".

As he explained to me, when he began his studies about 15 years ago, many geologists in France still worked within old paradigms of practice, which certainly were on their way to becoming obsolete and were changing, but hadn't quite been replaced yet. In the last century and in the early part of this one, as was discussed in Chapter 2, geological 'facts' were taught in France by what Nick calls "geographer-geologists" until well into the 1960's in fact, as something necessarily encyclopedic and taxonomical, a practice which, as he pointed out, was considered normal

NA: "… parce que la géologie semblait quelque chose de pas très importante, qui accompagnait les paysages, sans plus. Et pendant très longtemps elle a continué d'être enseignée, même par des géologues, de manière très qualificative, très naturaliste, on classait des choses, c'était la taxonomie des cailloux, et encore il y a une quinzaine d'années lorsque j'ai

> commencé mes études d'université, on avait tendance souvent encore à penser que la Terre était certes une machine, mais qu'elle était tellement complexe que de toutes façons on ne la comprendrait jamais, et donc on allait d'abord bien la décrire avant de la comprendre. Alors, il y a du vrai là-dedans. Il faut bien observer, mais si tu n'essaies pas d'ajouter à tes observations des explications physiques, et que tu ne peux pas comprendre quels sont les mécanismes de formation, c'est un peu limité."

However, as we know, geophysical and experimental theories have since succeeded in imposing themselves on pure field observational and descriptive practices in France and elsewhere, although this has occurred to the detriment of the latter — and older — field-based methodology. Nicolas thus found himself in the 1980's to be a member of one of the "newer generations of geologists" in whose epistemological make-up was found the basic notion that geology was about much more than simply picking up rocks, describing and classifying the field. Therefore, in addition to doing a structural analysis of a given terrane, Nicolas also samples rocks he brings back to the laboratory in order to chemically analyze them and establish their age by radiometric dating (see section 2.6 and Arthur Holmes' contribution to the perfecting of a dating method using radioactivity).

NA: "Je date les roches. Je donne un âge aux roches, je donne un chiffre qui est un âge absolu du nombre d'années écoulées entre la formation de la roche et maintenant. J'utilise le fait que la radioactivité naturelle dans les roches, donc la désintégration naturelle d'un certain nombre de noyaux d'atomes se fait de façon très régulière, et que ce sont donc des horloges. Et l'on peut, en comptant la quantité de certains éléments dans la roche, savoir depuis quand la roche s'est formée. Donc sur beaucoup de types de roches différents, j'apporte des âges, qui mis dans le contexte géologique, nous donnent des points de repère dans l'évolution d'une région ou sur les mécanismes qui ont formé cette région. … C'est comme un homme qui marche sur une route, qui va d'une ville à une autre. Il sait qu'il est entre les deux, mais s'il n'y a pas de bornes au bord de la route, il sait qu'il va vers la ville, mais il ne sait jamais s'il est loin de la ville de départ ou sur la ligne d'arrivée. Donc nous on donne les bornes, et ma spécialité, c'est ça."

While certain key elements of his adult *habitus,* or personal and professional cloak, such as the ability to endure the harshness of fieldwork conditions, the transient dislocation of self from society, the unproblematically accepted "new" philosophy that takes geology to be an analytical and experimental science with corresponding like methodologies, are to be found in his doctoral and post-doctoral encounters with the discipline, one also clearly has the sense given what Nicolas says and how he says it, that the shaping of his worldly "way of being" has been an on-going and lifelong process. In this sense, his "habits of the heart" are undoubtedly the accumulation of elements dating also from his early childhood socialization — as an only child. As Nicolas describes, being an only child influenced his relationship to the rest of the world, and in early childhood he tended to isolate himself from others — but this isolation brought him closer to rocks:

NA: "J'ai toujours été intéressé par la Terre. Euh, disons par, euh, par la façon dont… J'aimais les cailloux étant petit. J'aimais pas, disons, il y a des enfants qui aiment les chats, d'autres qui aiment les chiens, et moi j'aimais les cailloux, sans doute parce qu'il y a moyen de les contrôler, et moi j'étais un enfant assez seul, et assez renfermé. "

This ease of contact with rocks and the pleasure Nicolas had in interacting with them reveals an almost inborn and ingrained "geologist's nature", and rooted as he was in his childhood loves, made the decision later on to become a geologist as he discovered within himself a natural aptitude for finding beauty within the natural symmetry and order of the rocks he picked up:

NA : "Donc, j'aimais bien les cailloux et puis j'aimais bien les couleurs. J'aimais également dans les cristaux la symmétrie. Cet ordre qui semble dire que la nature ne se fait pas n'importe comment. Et à travers ça, progressivement, j'ai eu la sensation que la Terre était une espèce de machine, et qu'il y avait le moyen d'en connaître les rouages. … J'ai fait mes études à Marseille, et puis je suis venu l'année avant le doctorat à Clermont-Ferrand, parce que je savais qu'on y étudiait les volcans, et

parmi les phénomènes géologiques, ce qui m'intéressait était ça, un reste de mes amours enfantines. "

This early childhood experience of contact with rocks and the "strong sense of self" resulting from a period of isolation during his childhood also seems to have carried over into the specialty he chose for himself and which ultimately defined his relationship to the rest of the discipline. His sense of finding himself in the thick of a personal experience comes out in the description he gives of his own specialty in geology — Geochronology — which he places at the center of all geological analysis.

DD: "Comment est-ce que [la géochronologie] se place par rapport à d'autres domaines de la géologie? Parce que j'ai remarqué que tu publiais aussi bien dans des revues de pétrologie que dans des revues de tectonique.

NA: Alors, en fait, il y a plusieurs éléments de réponse. Tout d'abord, je suis un iconoclaste. J'aime des tas de choses et j'ai horreur de m'enfermer dans un sujet particulier. Quand même il n's'agit pas de toucher à tout, j'essaie quand même de focaliser mon attention pour avancer sur certains problèmes, mais j'aborde un certain nombre de problèmes. Le deuxième élément, c'est que dans la géologie, le paramètre temps est fondamental. Si tu n'as pas de paramètre temps, tu n'as rien. Donc on applique la connaissance de l'âge des roches à des tas de problèmes, par exemple, quel âge a une coulée de lave sur un volcan. Problème particulier pour étudier les volcans il faut savoir quel temps s'est écoulé entre deux éruptions pour savoir s'il y a un risque prochainement, il faut dater le volcan. Dans l'Himalaya, il faut qu'on arrive à dater quand les roches se forment et quand elles se déforment pour savoir comment l'Himalaya s'est faite. … Donc, en fait, si j'ai choisi la géochronologie, … il y a certainement dans mon choix la tentation que c'était une spécialité au cœur de toutes les autres. On part forcément du paramètre temps, ça me permettrait comme ça d'avoir une vision plus large. "

However, the embedded nature of Nick's professional persona within his personal sphere, the strong and affirmed "self" which directs all this activity ("J'aime des tas de choses et j'ai horreur de m'enfermer dans un sujet particulier"), and the centrality and embeddedness of geochronology in relation to the rest of the discipline of geology cannot overshadow the fact that as a "modern geologist", Nicolas also clearly recognizes that his

place is as but one in a plures of individuals. Part of this may come from Nick's tendency to position himself in reference to others, in a way similar to John Swales' (1998) description of C. Madden, who has invested her authorial self in group work, apparent in the personal and professional interweaving of her textual practices. Likewise, Nicolas does not work as a "lone ranger", but throughout our discussion, positions himself as a researcher in terms of other writers and as researchrs, forever working toward what is "good for science", both in the fieldwork enterprise and in the process of knowledge creation. Indeed, for Nicolas, positioning research within a group endeavor, I would say, seems to be simply the way things are done. But it is also an element of how Nick is, and the identity he has taken on.

In particular, this plural positioning allows Nick to provide a better and more "exact" account of what he sees in the field, with the idea that an account that is the negotiation of two or more researchers' visions is more likely to be 'realistically' accurate than one:

NA : "Le plus souvent tout de même, j'aime pas aller seul sur le terrain parce que je trouve que rapidement on a tendence à tourner en rond. Ce qui est difficile lorsqu'on est seul sur le terrain c'est qu'on peut facilement se persuader qu'on voit des choses. On regarde le contacte entre deux types de roches, et on a parfois tendence à y voir un peu ce qu'on a envie de voir, tu vois, on commence à faire une interprétation en même temps qu'une observation. Et être au moins deux c'est la possibilité que l'autre ne voit pas pareil, qu'on peut voir tous les deux différemment la même chose, donc c'est que chacun est déjà parti sur une interprétation, et là on peut se recentrer sur une observation. Ça veut dire, ok, c'est pas possible, regardons réellement ce qu'on <u>voit</u> <u>tous</u> <u>les</u> <u>deux</u> [he pounds on the table]. Donc, je préfère ne pas y aller tout seul. Euh, mais ça m'est arrivé quelques fois. C'est beaucoup plus enrichissant à plusieurs quand même. … Chacun, chaque individu va, je pense, se focaliser sur quelque chose de différent. Et c'est pour ça que c'est important d'être plusieurs parce que c'est plus riche. On se focalise chacun sur quelque chose de différent, et puis on va voir le tout ensemble, où chacun aura sa part."

This rhetorically plural positioning of the self in reference to others also comes out in the writing process, which as Nick describes, is not only an effort of coming together to work as a group in order to hammer out a cooperative interpretation, but is also necessarily geared toward creating an "expansionist rhetoric," whereby the authors work to make their findings more accessible to the wider community. For Nick, this expansionism is already the natural consequence of succeeding in getting a group of researchers with differing specialties to work together.

NA : "Si on a la chance d'avoir été plusieurs, on va confronter nos résultats. D'abord on a des données qui sont complémentaires, on a tous fait des choses différentes, il faut donc les comparer, et ça met longtemps parce qu'on s'assoit tous autour d'une table et puis on va comparer nos données alors qu'on est pas toujours spécialistes. Donc il faut qu'on explique, ce qui est tout un travail de vulgarisation pour leur expliquer ce que ces données personnelles signifient dans le temps, puis on arrive tous à une synthèse. Ensuite il y a l'écriture, en général le plus efficace, c'est que quelqu'un, le premier auteur de la publication, en général va écrire en gros l'intégralité. … si chacun écrit sa partie spécialisée, le défaut c'est que ça peut devenir rapidement illisible pour quelqu'un qui n'est pas spécialiste. Or on peut avoir vocation à la spécialisation mais aussi à la présentation plus large. Il faut que les gens qui sont spécialistes sur un côté trouve matière à vouloir lire notre article, mais il faut aussi que la communauté géologique qui peut être intéressée sans être absolument spécialiste puisse comprendre aussi. Et je trouve que ça marche mieux si quelqu'un se charge de l'écriture. … Et puis il va le faire lire à tous les autres pour leur demander leur avis. Voici moi ce que j'ai compris du problème, est-ce que vous partagez mon avis? Alors on va tous interagir, on peut être plus ou moins d'accord avec certaines des hypothèses, oui mais j'ai d'autres données, etc., on va regarder ça ensuite."

And so, we might describe the particular elements of Nicolas' accumulated child–and–adult *habitus* as follows: As a firmly-entrenched member of the new generation of geologists and a whole-hearted student of the post-1960's geology revolution in France, perhaps even "predisposed" in some sense by his early love of rocks to succeeding in the field endeavor, Nicolas has thus been shaped by his encounters with these various milieus in the way he approaches his professional task. He is a personal-professional geologist

who willingly takes on the challenge of the field, while constantly defining the experience in strictly disciplinarily, structured terms. For him, geology is "about the field", but it is also necessarily an analytical discipline inherently connected to the laboratory, and modern prerogatives make him equally concerned about securing funding for research, engaging in group collaborations, and getting French geology to move ahead so as to finish modernizing itself. This modern positioning makes him aware of the logistical and financial organizing necessary to setting up a field mission; modern research isn't just about "getting the data and reporting on the results," but it is also a question of seeking and securing funding, submitting projects, and making the most of scarce resources. This recent shift in how modern geologists in France "do geology" has had for another partial consequence to shape the adult Nicolas into an inherently social being, for given these particular working conditions, he cannot allow himself to "get away from civilization" in order to make innovation happen; instead, he is always carrying it along with him, as his work group who is not only in his head, but meeting up with a him a few times a day to compare notes.

However, although he belongs to this "new-fangled" analytical generation, he is also equally a "*field* geologist" and all that this entails. He therefore knows what "real" fieldwork is like, that it can be dangerous doing fieldwork in Tibet, that it can be tricky getting valuable samples back to his lab for analysis, past the Chinese authorities who object to anything being taken away out of their "national heritage", that he does fieldwork at 3000 or 3500 meters on snow-covered glaciers, has been in the field for 3 months at a stretch, and uses the daily conditions of the field experience (e.g., violent downpours, etc.) as stimuli for recalling important sites.

He has also admitted to having an intimately professed affinity for the field and for being in contact with it, for it influences how he views his own research and the direction he believes his discipline should take. As he warns, without the field, geology may in the end no longer be "geology".

NA : "Je crois quand même que, si on se détache totalement, du terrain, ce que la science pourrait nous faire faire à terme parce qu'on a tendence à faire des études de plus en plus pointues, mécanique, etc. J'ai peur que, en abandonnant progressivement le terrain, euh, alors, d'un point de vue purement scientifique, on finit par inventer des modèles qui n'ont plus rien à voir avec la réalité. On finisse par faire de la science fiction et l'on ne s'en rend même pas compte. Si tu veux, on peut dire mon modèle a prévu ça, même si ça n'a jamais été observé sur Terre, c'est grave. Euh, ensuite, d'un point de vue psychologique, je dirais que si on arrête complètement d'aller sur le terrain, j'ai peur qu'on y perde notre âme. Ce qui fait la géologie c'est aussi alors, il faut dépoussiérer un peu la science pour ne pas rester à ce qu'on faisait au 19e siècle, mais il y a quand même une façon d'avancer dans la compréhension de la Terre qui dépend de notre culture, et dans cette culture il y a le terrain. … Donc moi, si tu veux, je suis un peu cette mentalité générale qui est de dire que les géologues doivent continuer à aller sur le terrain, s'ils en ont envie, que c'est important."

And so, while Nicolas believes in the direction his field is taking, in its move toward more analytical rather than purely descriptive contributions, he also perceives the impossibility of doing geology without the field, for in his mind, geologists must always come back to it as a resource and a disciplinary "raison d'être". This therefore places him, more or less resolutely, in the camp of those who might reservedly chide experimentalists, pure geochemists and geophysicists, for not having more realistic links to described natural reality.

### 5.5.3 Gilles Chazot: "Tu risques pas d'y aller en 4X4, c'est pas possible…"

And finally we turn to our geochemist, Gilles, who like Nicolas is in his middle thirties. The object of study during his dissertation was a series of volcanic rocks which he himself sampled in northern Yemen in the early 1990's, but since his post-doctoral days has begun to spend more of his time studying mantle rocks, which one finds as enclaves or inclusions within these same volcanic rocks.

His specialty differs quite a bit from that of Olivier or Nicolas, who as structuralists do spend a good deal of time in the field, as well as in the laboratory. As a geochemist, Gilles is very closely tied to his laboratory, where he studies the major and trace chemical and isotopic composition of rocks. While it is possible to understand a geological problem in a structural approach by comparing one point to another 100 meters further along while still in the field, Gilles cannot, by simply looking at a mineral with the naked eye, discern which basalt or peridotite will have the isotopic characteristics that will shed light on geological processes. And so, he must take these rocks back to his laboratory where very specifically he studies the isotopic and chemical compositions of the minerals found in mantle rocks, as well as the interactions between fluids circulating in the mantle and the mantle's rocks.

As Gilles explained, these interactions in turn have important implications for explaining the long-term evolution of the Earth's mantle, as well as the genesis of magmas found at the surface, which for the most part result from fusions taking place within the mantle at depths of between 70 and 2900 km. By studying the chemical composition of the minerals found in magmas brought to the surface by mantle plumes[xxxvi], one can reconstitute the origin of the magmas and material found in the mantle, what the sources of its volcanism are, the composition of the mantle plume, what sort of surrounding material it carried with it while moving up to the surface, and whether the magmas themselves have been 'contaminated' while crossing through the continental crust. Thus, this particular geochemical side to geology allows geologists to understand what is happening at depth, and to reconstitute geological processes in places we cannot 'see'. We indeed seem to have left the field far behind. And yet, Gilles is a geochemist who does still goes into the field to sample for himself. And although Yemen is now almost completely inaccessible to outsiders, he is involved in similar projects in Ethiopia and Djibouti and is able to organize a field mission every two to three years, or so.

Gilles describes his route to becoming a professional geologist-researcher as hard, most likely character-forming, perhaps even something which did not come as easily as it might have for others — if easiness is possible on the way to a Ph.D. dissertation. This road to his professional realization is characteristic for many of today's geology students in France. Given that too many Ph.D.'s are produced today for the university system to absorb, many doctoral graduates must engage in a multi-year search for a permanent position, often going out the country for an often temporary post-doctoral position, recycling oneself into a different domain, or simply remaining unemployed.

Gilles talks about how his studies were interrupted for a year after obtaining his 'Maîtrise' in order to do his military service (1988-1989), how he did his 'Diplôme d'Etudes Approfondies' or 'D.E.A.' the year following, and his Ph.D. thesis between 1990-1993, defending in March of 1993, thus doing his entire dissertation from start to finish in fewer than three years. One could further contextualize by pointing out that although doctoral students in France receive three years of funding, in is not uncommon for students to 'spill over' a few months beyond the three-year limit, some even clearly going into a fourth year. Those who finish squarely 'on time' are considered hard and diligent workers; it can therefore be said that if someone finishes in less than the allotted time, it is truly an accomplishment. Gilles finished nearly a whole six months early. And he was married and already had two small children at defense time.

His hard-working, tenacious character served him well through the next few years. After finishing his Ph.D., Gilles was awarded a two-year post-doctoral fellowship in England, which began in July 1993; his wife took a 'congé parental d'éducation' for a year, and the couple moved to England with their children. After a year, they returned to France, where Gilles was able to set up shop in the laboratory where he had done his dissertation, in Lyon. But he also continued to commute, spending about one week a month at the University of London, occasionally going to Edinburgh to work with colleagues there.

At the end of the two-year period, still without a permanent position, Gilles received a grant from the 'Société de secours des amis de la science', a private French foundation which grants living money to essentially young scientists who are still trying to make their way through the system, in order to allow them to keep afloat just a bit longer and stay 'in the system'. This smallish grant in combination with a new part-time post-doctoral position in Lyon from September through December 1995 kept Gilles on the research scene until he was able to land another, more stable post-doctoral position with the French equivalent of the Geological Survey, the B.R.G.M. ('le Bureau régional de géologie et de minéralogie ') in Orléans, a post he held from January through July 1996. At this point, nearly three and half years after defending his dissertation, he learned that he had finally been granted a permanent assistant professor position as 'Maître de Conférences' in the Earth Sciences department at the Université de Clermont-Ferrand II, Blaise-Pascal. A long enough period of time to spend in and of itself without knowing what one's professional future would bring. But with the added frustration of having come close to landing a permanent job many times, and not quite getting there — having been twice classed second on the waiting list for the C.N.R.S. ('Centre national de recherche scientifique'); classed second once for a Maître de Conférences position in Brest, and once second in Toulouse.

At the start of our interview, Gilles appeared to me as someone who was cautious in his choice of words, carefully guarded, as though he were unsure of how much he was to reveal of himself. Before going in to talk to him, however, I knew that he was a scientist a bit out of the ordinary, as are, in fact, all the geologists interviewed for this study, in that they are highly cultured and read extensively outside of their own domain of specialty — sometimes even into my own. Gilles has read Thomas Kuhn, and is an avid reader of Bruno Latour, including his most recent novel which I myself have yet to read. He thus perhaps thought I was one of those "tricky Latourian scholars", but appeared ready to play 'my game', although without quite knowing what the rules were

going to be. However, once he heard that what I especially wanted to hear about was his fieldwork, what it was like, whether it was hard, what he wrote about in his field notebook, he loosened up and starting talking quite a bit. Indeed, he seemed to delight in giving the sort of details that would amply illustrate what fieldwork was truly made of, making it seem to be a truly heroic adventure.

Here, we can be reminded once again that the culture of the field in geology is made up not only of a tacit "intuition" of how the Earth works, based on the physical observation of nature and grounded in disciplinary knowledge and shared practices, but that there is also a shared attitude of rugged individualism among field geologists, forged by field experiences. These experiences very clearly set field geologists apart from those geologists who have little or no contact with the field, variably called 'closet philosophers' (cf. Rudwick) or 'drawer-type geologists'. This mentality has been acknowledged by many of the geologists I've come into contact with over the years, not excluding those geologists interviewed for this study. We can also recall Nicolas' description of Michael Searle, who refuses to hold down a permanent position and house, preferring instead to have his position renewed year-by-year, and to camp in a tent. We also think of Olivier in his portrayal of the daily life of a field geologist 'sans fantaisie', forged by his own experiences at 3000 m left alone to uncover the mountains. Or Nick, who describes the difficulties inherent in doing fieldwork in Tibet, with its work going well beyond the competencies normally expected of geologists, in terms of political uncertainties, the physical ardor of doing fieldwork at 3300 m altitude in icy downpours, or his avowed sentimental 'soul' connection to the field.

In this, Gilles is no exception, and he relishes in giving almost gory details of the bits of the field mission one may not talk about in the research article — although there is quite a bit of evidence that geologists will talk about them freely at conferences (Rudwick, pers. comm. 1998; Jolivet, pers. comm. 2000). This we can see in the following 'anecdotes' quoted from our first interview. Indeed, doing fieldwork in Yemen,

where Gilles did his dissertation fieldwork, is anything but a walk in the park. The political atmosphere and the harshness of the terrane make even the 'just doing the work' part nearly impossible.

GC: Par contre maintenant c'est quasiment impossible d'y aller faire du terrain [NB. Au Yémen].

DD: Ah?

GC: Ben, il y a eu pas mal de prises d'otages,

DD: Mmmm, mmhmm..

GC: euh, des enlèvements, des trucs comme ça, alors jusqu'à l'année dernière, les prises d'otages se passaient plutôt bien, enfin, je ne sais pas tu as entendu à la radio, en fait les gens ont été ravis, c'est à dire, tu avais un groupe de rebelles dans les montagnes qui prenait un groupe de touristes en otage, réclamait quelque chose, ils les soignaient comme des princes, leur donnaient à manger, c'était sympa et tout, ils les relâchaient au bout de dix jours, les touristes étaient enchantés,

DD: [I laugh]

GC: [he smiles] jusqu'à ce que l'année dernière [NB. 1998], il y a eu des Anglais qui se sont fait prendre en otage, et euh, il y en a trois qui se sont fait tués.

DD: Ah oui...

GC: Dont un qui s'est fait tranché la tête je crois. Donc là depuis euh, il est devenu quasiment impossible de quitter la capitale. Il aurait dû y avoir un congrès l'année dernière à Sana'a organisé par les Anglais qui n'a pas eu lieu ...

C'est en fait une zone hyper militarisée, là, qui est très très difficile d'accès, euh, pour échantillonner là, c'est ce qu'on appelle le […], euh, j'ai eu le droit de descendre du 4X4 cinq minutes avec, euh, le fusil mitrailleur dans le dos,

DD: Ah ouais?!

GC: ... j'avais pas le droit de lever la tête pour regarder, parce qu'il y avait un paroi rocheux et puis une fortification avec des militaires en haut, il fallait que je casse mon caillou, que je prenne mon caillou, que je remonte dans le véhicule et qu'on parte,

DD: [I laugh, whistling]

GC: Donc euh [he laughs in return]. Le long ici, il y a plein de chars de la guerre de 40 qui sont enterrés, juste une tourelle qui dépasse en direction de la mer, et,

DD: Donc en fait, tu devais savoir exactement ce que tu es venu chercher. Euh,

GC: On savait où on voulait aller, on savait ce qu'on voulait chercher. On est remontés ensuite jusqu'à une ville qui s'appelle […] Tout ça sont des coins, ils n'avaient pas vu d'Occidentaux depuis, depuis dix ans. Les derniers qu'ils avaient vu c'étaient des Tchèques qui arrivaient en hélicoptère euh, faire la carte géologique. C'est tout. C'est inaccessible sinon, eh? Il n'y a pas de route. Donc d'Aden tu pars à Little Aden, après il n'y a plus de route, et puis ensuite, c'est, sur la plage. Jusqu'à, ensuite on est monté dans la montagne ici [shows me on themap]. L'accès est assez difficile, ouais.

The unsettling political climate is coupled with logistical difficulties, such as figuring out how exactly to get to certain volcanic fields that are fifteen or twenty kilometers from the nearest serviceable road; these fields are at times inaccessible even on foot given the extreme conditions of the terrane. Or the difficulty in obtaining even basic topographical maps on which the latitude and longitude indications have not already been erased for reasons of military secrecy. Or the difficulties raised by monetary woes, for this type of field mission, given the inaccessibility of the sampling sites, is extremely expensive. French state-employed geologists often lack the funding enabling them to hire a helicopter to take them to out-of-reach places. Instead, they have to rely on regional connections established by the B.R.G.M. and the Saudi Geological Survey whereby they might hope to 'borrow' one. And finally, one can consider even the extreme field conditions surrounding the mission itself in order to get a sense that any geologist could not possibly remain personally 'unaffected' by the travails of doing fieldwork in such regions.

GC: Donc euh, c'est pas évident de faire du terrain là-bas. ... parce que tout ça c'est le désert, ... c'est le désert le plus, désert du monde. Il n'y a rien. Rien. Il pleut jamais, il n'y a pas de pistes, il n'y a rien du tout, et ces champs volcaniques quaternaires ici sont aussi en plein désert, avec quelques pistes qui traversent, et, bon les pistes tu peux y aller en 4X4 sans problème, seulement parfois les volcans qu'on veut atteindre sont à dix, quinze kilomètres de la piste, et c'est dix, quinze kilomètres de coulée de lave récente, des laves quaternaires, donc c'est des champs de gros blocs chaotiques, donc il faut faire dix kilomètres à pied, quinze kilomètres à pied pour atteindre le volcan, se charger vingt-cinq kilos de peridotite sur le dos, et puis revenir ces quinze kilomètres. ... Tant que tu as des pistes tu peux y aller en 4X4, même s'il n'y a plus de pistes, que c'est roulable, tu peux quand même passer, mais les champs de lave récente, c'est pas possible. ... Comme c'est désertique, il n'y a pas eu énormément d'altération, donc les coulées sont encore restées très fraîches. Marcher sur une coulée récente, c'est infernal. C'est très coupant, ça fait des blocs donc euh, ça fait des creux, ça monte, ça descend, c'est complètement chaotique, euh, c'est très coupant, c'est noir, et quand tu sais les températures qu'il fait dans ces coins-là ... tu y vas en janvier, février, au pire mars mais on ne peut plus après, et tu risques pas d'y aller en 4X4, c'est pas possible.

DD: Et même, il doit faire chaud en janvier, février.

GC: Ecoute, moi j'y suis allé, … ici, c'est à une altitude d'environ 30 centimètres, c'est tout plat, très salé parce que c'est juste à côté de la mer. Au mois de novembre, il faisait un peu plus de trente à l'ombre. Donc euh, tu y vas au mois de juin, c'est... Tu ne peux pas faire de terrain en tout cas. Enfin, et quand je dis trente à l'ombre, il n'y a pas un point d'ombre ... il n'y a pas de végétation, donc ch'ai pas, l'été au soleil il doit faire cinquante. Donc tu risques pas de faire du terrain dans ces conditions-là, c'est pas, c'est pas possible. ... Donc passé mars, ça devient difficile.

DD: Mmmhmm. En tout cas, ça t'évite d'avoir à passer l'hiver ici [I laugh].

GC: [he chuckles] Ouais. Mais le retour est dur, eh? Quand tu passes trois semaines, dans ces coins-là et tu rentres le quinze décembre, [he laughs]. Les fêtes sont un peu déprimantes. Tu ne peux rien manger car après n'avoir quasiment rien ingéré pendant trois semaines, tu ne peux pas manger normalement avant quelques temps...

Donc les conditions de travail euh, ici, c'est relativement dur. C'est à dire une journée de travail au Yémen, les deux premières journées qu'on a passées, ce sont des journées où tu manges pas, parce qu'il n'y a rien [said very matter of fact], donc tu pars tôt le matin, pour profiter au maximum de la journée, donc c'est départ six heures six heures et demi du matin, avec un verre de thé comme petit déjeuner..."

Over the course of our interview, Gilles delighted his very captive audience with loads of other tantalizing details of his various field missions, such as the time when, after having gone without eating hardly a thing for a few days, they drove over a hill only to discover an out-of-the-way village not marked on their map, appearing as if out of a mirage. Fully acting out the mirage, they stopped and were promptly and unbelievably served an ice-cold Coca-Cola.

These sorts of field details are intimately embraced within Gilles' geologist's persona, and they pour out when given a chance. And so, given the extremely particular and difficult conditions for doing fieldwork, as well as Gilles' very quick acknowledgment of having been profoundly marked by them, we might expect some of this 'rugged individual' persona to come through in his writing. Yet Gilles' is a discreet nature, one who resolutely refuses to let his research article be anything but an entirely "transparent and objective" scientific account. And so we must ask, can the authorial discretion we find in the Field Account also be a personal choice? In other words, can more silence in fact be imposed on the conventional silences already characterizing the report of the fieldwork mission than is typically expected? The answer to this question must appear quite trivially obvious, as numerous analysts have clearly pointed to such elements as the "private intentions" (Bhatia 1997) of the writer. But the question to be asked here is whether Gilles as the scientific writer in fact goes *beyond* the silential limits normally imposed by institutional norms and community practice. In such cases, it becomes difficult to differentiate between standards of imposed silence and the silences which are self-imposed. As Gilles confided, while some field geologists do in fact show themselves — at times unnecessarily, in his mind — by publishing pictures of themselves posing next to particular rock formations, this is something that he himself would never do. Gilles, in all truthfulness, does not like to show himself.

This is an issue we will return to in section 5.7, after having examined the field geologist circle as a community of practice in section 5.6, or the field geologist's "organizational field" (Bourdieu 1984). It will be argued that the intersection between the author's *habitus,* as the accumulation of engagements with various life conditions and social milieus over time, and his social field can be taken as a site for the instances of divergence, of "need-based performativity", where the author makes an appearance on the page, thereby resulting in unexpected linguistic innovations.

## 5.6 A community of practice: The field as a site for the social construction of disciplinary "ways of seeing"

The following section constitutes the descriptions and appreciations of the three geologists I have interviewed about the place "the field" holds in their personal practice as well as within the discipline of geology. Here, they also talk about the interface between the individual and the community, or how geologists learn to be geologists, indeed how they themselves have learned the trade. In describing the epistemological place and importance of the field, they also reveal quite a bit about what they perceive to be the essential and key points of the discipline.

### 5.6.1 Conventionalized and collectively shaped visuality as a filter of natural reality

One of the first questions to be asked is exactly what field geologists do, and how they operate. Of course, by this time, we already have a pretty good idea of much of how things work in geology. There are on-going tensions between those who aim to remain close to the field and keep it as a driving-force behind their research and those who believe that one doesn't necessarily need to go out into the field in order to do geology. The spaces reserved for talking about the field in the research article have been reduced over the years, and authors retain a hidden and undeclared part-genre for relating their

field accounts marked by a discrete set of low-lying linguistic traces which rhetorically, but squarely, place the author in the field as the doer of his own work and seer of his own structures. The visuality of the field observation is a conceptual methodology and a principal part of the field endeavor. As such, field researchers translate their observations and data through geology's conventionalized visual language, allowing them to give details of the field observation that a restricted textual space and a conventionalized de-emphasis of the research account do not allow for. But geologists equally filter what they see through this conventionalized visuality.

We know that field geologists walk quite a bit as they survey the land, and that to keep track of what they have seen they keep field notebooks. We have already seen in Chapter 4, for example, the sorts of details that one apprentice geologist kept in his notebook while on a field mission in northern Madagascar. One question I have asked the other geologists interviewed here is what they themselves put in their notebook, and overwhelmingly the response has been "drawings!" This further highlights the importance the image plays in geological practice, forming a central aspect of geological field culture, activities and 'ways of doing', the research community's functions and functioning. In fact, the main content of the field notebook is visual, and this becomes even truer as time goes on. The more experienced the field researcher becomes, the more accustomed he becomes to thinking and seeing like a field geologist, and the less he needs to depend on actual words to translate what he sees. As Olivier describes his field notebook,

OM: "Non c'est pas un journal, non. … J'étais plus littéraire au départ, si je peux dire, moins à l'arrivée… Je pense que plus on est du métier, moins on met de texte. Le texte, en fait, il n'a pas d'intérêt, tu as ton modèle, tu l'as dans la tête, c'est pas la peine de l'écrire… Si je prends ça, c'est relativement récent là [he shows me a page from his notebook and then continues to turn the pages]… Il y a un peu plus de texte, mais c'est pas des, bon, [he reads] "le granite est beaucoup moins déformé qu'ailleurs et la déformation se fait avec des [...] conjugués"… Bon, on voit qu'il y a

> plus de texte là. Ah regarde j'ai mis quand même des [he reads from his notebook again] "on peut conclure à une déformation sur axe sud-ouest... de type..." J'ecrivais à l'époque. C'est vrai au départ, ça, ça date de 1983 ou 1984, je sentais plus le besoin au départ d'écrire, en fait, tu vois, et plus le temps passe, moins il y a d'écriture. J'ai pas besoin de l'écrire, je l'écrivais pour me rassurer, ou que j'avais peur d'oublier. … et puis j'ai peut-être relativisé aussi l'importance d'un affleurement unique. Au départ quand tu débutes, un affleurement paraît très important etc., donc à chaque affleurement tu écris des choses sur cet affleurement. Après tu vois les choses plus globalement. Tu comprends qu'en fait, la nature se contredit, hein? C'est à dire que tu rates toujours des faits qui ne vont pas rentrer dans le modèle. Ça tu le comprends assez vite en fait, enfin, au bout de deux ou trois ans si t'es pas trop con tu le comprends, c'est pas donné à tout le monde..."

As we can see, Olivier equates this early need for textual loquacity with his inexperience, both in terms of his concerns about not getting his interpretation right, and about not yet being able to distinguish what is important to write down. The need for the less experienced geologist to physically textually describe what he sees, although the same can be represented visually, also came out in Philippe's fashioning of his block diagram (see Figure 4.4, "Dans première section parallèle à la linéation, on a également un plissement (axe des plis environ N50) ou perpendiculaire"), as if somehow it takes time for the researcher to trust in this new non-linguistic visual language, that it will indeed succeed in saying what it needs to. And thus what replaces the linguistic "text" in the field geologist's notebook as time goes on is the image:

OM: "Alors, d'abord, on fait des desseins. Il y en a même [NB. other examples of his field notebooks] qui sont plus dessinés que ça. Il y a différents types de desseins. Il y a des desseins à très grande échelle, hein, ça c'est des sommets de montagne. Ça on le fait quand on a vraiment compris la structure… Il y a l'aspect représentation visuelle des choses, qui passe par un dessein…"

Nicolas also emphasizes the visual in his field notebook, and he relates how he embeds the use of the visual not only within the need to transmit what he has seen, but also within a framework of cues used to recreate his visual memory later on down the road:

NA: “Je dessine des paysages. Egalement, ce qu’on aimerait avoir c’est un Polaroïd, et tu peux dessiner directement sur le Polaroïd. Mais bon, c’est pas toujours très facile. Donc on prend des photos, donc je, dans un premier temps, je prends des notes toute la journée, et caractérisées par un certain nombre d’éléments, extra-géologiques, comme par exemple, je vais noter l’heure à laquelle on part le matin, toujours l’heure à laquelle on revient, je mets le jour, la date, et je mets globalement l’endroit où on est. La région où on est. Ça c’est très important parce que, et je vais raconter éventuellement, sur un petit carnet de bord, je vais raconter quelques éléments de la journée, au fur et à mesure. C’est important parce que, lorsqu’on doit, avec les autres, reprendre les notes et comparer les notes, plusieurs mois ou plusieurs années après, c’est pas toujours suffisant de se dire, euh, c’est quand on était à tel endroit. Surtout si on y est allé plusieurs fois éventuellement. Mais dès que quelqu’un arrive à dire si! c’est le jour où on a eu la douche froide, là tout le monde se rappelle. Et on va tous attacher une autre vision de la journée à ça. Donc je vais toujours marquer un certain nombre de détails au cours de la journée. Ce sont des détails extra-géologiques.”

This “extra-geological” detail is, in fact very important in that it gives us the key to understanding how the geologist manages to stimulate his visual memory — presented here as just a ‘tiny’ practical detail in passing, but in fact it is crucial and makes up part of the essence of “the craft of the trade”. As Gilles further pointed out, these are the things that one learns how to do “the hard way”, that is, after having made a few mistakes:

GC: “Il faut être attentif au fait que tu auras peut-être envie de revenir, pour que tu te souviennes cinq ans après de quelle sortie de l’autoroute il faut prendre. Même avec les coordonnées GPS, si l’affleurement est à 200 mètres de l’autoroute, tu as beau être sur l’autoroute, c’est pas bon quand même. Ou si tu veux emmener des étudiants sur un affleurement, il faut savoir que tu dois revenir l’après-midi et pas le matin parce que le matin, tu as une falaise dans laquelle il y a des choses intéressantes avec le soleil derrière et tu ne vois rien du tout. Donc toi tu y vas l’après-midi, c’est impeccable et tu ne fais pas attention. Tu reviens avec vingt-cinq étudiants le matin, et tout le monde est devant la falaise dans le noir ébloui par le soleil… Donc en fait on se rend compte a posteriori qu’on ne prend jamais assez de notes. Tu te dis toujours, ah bon ça je m’en souviendrai… Même si je me souviens de l’affleurement, je ne me souviens pas toujours de l’orientation.”

In Chapter 4, the analysis of the transformation of Philippe's field notes into conventionalized text and image has also shown us that this visual memory plays a crucial role in transforming the mass of 'private' field details contained in the field notebook, characterized by what Nick calls a "personal jargon" exploitable only by the researcher, into something which other researchers can exploit as knowledge about the region and its structures.

And thus we can appreciate the field notebook as a fundamental contribution to the research process, for it contains the "private details", or the raw data, the researcher uses to construct his account. These are aspects of the author's personal experiential account that are meaningful only to him and are filtered out from later recontextualizations, the means by which we will see in a moment. The field notebook also holds the first traces of the conventionalized visual language and images which geologists use for understanding and working within established geological scenarios, organizing data and transmitting field details to the wider geological community. Given the practical emphasis on visuality, rather than on textuality, we thus see that the field geologist quite naturally works very much within a discipline-established frame of "multimodal communication " (Kress 2001), where 'text' and 'image', in a broad sense, are equally essential to 'doing geology'.

NA: "Un petit dessein vaut toujours mieux qu'un long discours. Si j'étais très doué en dessein, je pourrais pratiquement éviter d'écrire. C'est à dire au lieu de dire "je vois deux roches différentes qui sont en contacte", je pourrais tout de suite faire le dessein. Je ne suis pas très doué en dessein, donc j'écris et je fais des desseins à côté. Euh, les contactes particuliers, je vais faire des desseins parce que j'essaie de faire mon dessein de ce que je prends en photo. Comme je ne peux pas marquer directement sur la photo, donc je fais le dessein le plus précisément possible, et puis une fois que j'aurai la photo je pourrai remarquer sur la photo. ... [mais] le problème de la photo c'est qu'elle est très tardive par rapport à, au moment passé sur le terrain…"

And so, even given the advent of 'new technologies', the geologist must still rely on what has traditionally been there — paper and a pencil — perhaps keeping him in the past as an "old-fashioned" type of researcher, but he has to stay with what works. As Nicolas confided, he hopes that one day it will be possible to take numerical photos and write directly on the screen, keeping everything in electronic memory. But that sort of technology isn't available yet.

### 5.6.2 Physical closeness to the field, exactitude of description, and the relationship of the field geologist to his *habitus* and the collectivity

In addition to the various "extra-geological details", schemas and drawings that the field geologist invariably puts into his field notebook, geologists are also pressed by the need to provide a faithful linguistic description, albeit brief, of what they see to help provide an more accurate context for their later interpretations. What is important here for Nicolas is that he strives to exactly represent what he has seen in the field by actively working to separate the empirical and the theoretical, thereby restraining himself to giving only a "description" and not an "interpretation" which is a way of "seeing" already tempered by "knowledge".

NA: "Les deuxièmes éléments que je marque [dans le carnet] sont des descriptions, rapides, de chaque endroit où je m'arrête, par exemple je m'arrête, je regarde un endroit où il y a des roches à côté d'une petite rivière, je vais mettre "arrêt à côté de la petite rivière" et éventuellement je vais mettre un numéro qui sera reporté sur une carte si j'en ai une. Donc j'ai une petite description et ensuite je décris ce que je vois. Alors je décris et théoriquement je pourrais faire la même description que la photo que je viens de prendre. Donc je prends la photo avant ou après et je décris très précisément ce que je vois et j'essaie de décrire sans interprétation à vrai dire donc je vois une grosse roche sombre qui est tel genre de roche et puis qui est à côté de l'autre roche. Je vais pas mettre, je ne vais pas employer par exemple le mot en contacte, sauf si je vois un contacte. Si je vois que les deux roches se touchent, là elles sont en contacte. Si j'ai un arbre devant moi et je ne vois pas, je peux tourner autour de l'arbre [said teasingly], mais disons que s'il y a beaucoup de buissons, je vais mettre que je vois deux types de roches qui sont l'une à côté de l'autre, mais je ne

> sais pas si elles sont en contacte. Je vais m'approcher, je vais essayer de voir si elles sont en contacte, mais je ne vais pas mettre une phrase sauf si en fait elles sont en contacte."

A geologist friend of mine, who was for a long time the general secretary to the Société Géologique de France with extensive fieldwork throughout North Africa to his credit, explained the relationship between the geologist and the field in his own terms. Taking inspiration from Lévi-Strauss's 'Tristes tropiques', he interpreted the basic field geologist's modus operandi as follows:

> "Plus l'observation immédiate est subjective, plus la description finale est reproductive et objective" (L. Latouche, pers. comm., 2001).

The field geologist must therefore be close to his data, so as to be a bit further away from the community, and thus not wholly prisoner to its conventionalized visuality. He must be able to "see" what is in front of him. However, the geologist in truth appears in the end to be confined by a particular disciplinary reality, for everything he does see is ultimately shaped by the collective vision the field of geology has developed over time. This is a vision the geologist has unproblematically accepted and adopted as workable and "normal", conditioned as he is by his community's way of doing and his own *habitus*.

And thus, despite an understandable quest for "objectivity" and concern for being true to nature, it is questionable, as we might surmise from Olivier's following comments, whether the field geologist can truly provide an objective description of what he sees, for he himself has been filtered as a student and throughout ensuing interactions with the geology milieu through endless exercises of learning to perceive and think like 'the plures'. Fieldwork observations are thus inevitably the result of a preconceived idea of what the field should look like, and this is a skill that the student in geology must acquire. As explained to me by one particularly oft-consulted geologist,

> "On leur apprend, en T.P. [travaux pratiques], à dessiner, à faire des schémas — bien sûr, ils disent qu'ils ne sont pas artistes — pour qu'ils nous rendent un schéma annoté, au lieu de cinq pages de texte qui n'ont pas d'intérêt. Dessiner, ça leur apprend aussi à observer. S'ils voient que

> leur dessin ne correspond pas à ce qu'ils voient, c'est que c'est mal observé. Dessiner oblige de comprendre ce qu'on a observé" (T. Hammouda, pers. comm., 2000)

Rudwick (1996) has suggested that the field geologist may entirely free himself from the ways of seeing imposed by the community in order to allow for true theoretical innovation, with his concept of "liminality", which, just as a pilgrim's voyage, takes the geologist out of his community structure and places him into a new environment thus breaking with the structural social conditions which determine his perception of the world. However, given what my group of geologists has told me, it is hard to conceive of the geologist as ever totally freeing himself from the confines of his *habitus* and its constructed ways of seeing. After all, he has been with these practices since the early days of his inculcation into the community, where he first began learning to "see" like a geologist. Although many geologists, like Nick, openly profess a sincere concern for empirical objectivity, trying in all modesty to exactly reduplicate *what he sees* and only that, even this way of seeing is already shaped by the field geologist's habitus and filters out a good part if not most elements making up the natural world.

OM: "Quand on fait du terrain, enfin, … on fait de la modélisation de la réalité à un moment donné. Sur le terrain, on a l'impression d'être plus proche de la réalité parce qu'effectivement on la touche, on la touche vraiment, on est sur le terrain, on la touche mais seulement à mon avis on ne voit que ce qu'on connaît, on ne voit que ce qui a déjà été interprété, comme étant telle ou telle chose, et on se raccroche toujours à ça, si tu veux. On passe très certainement à côté de tout un tas de choses qu'on ne connaît pas, qui n'a pas été interprété, et de toutes façons on ne voit que les interprétations. Je ne crois pas aux faits, moi, enfin si, mais les faits passent par un crible de l'interprétation. Il y avait des gens qui interprétaient les structures d'une certaine manière, et maintenant si on voit ces mêmes structures d'une façon complètement différente, c'est parce que l'interprétation a changé."

In addition, the field geologist must always come back to his community, and as a result is always repositioning himself in terms of the plures. And so, it would appear that conventions of practice, as constitutive of the *habitus* package that comes with

participating within a given social field, with its "dialogized unity" (Bakhtin 1990) of conventionalized visual expression and conceptualization, begin to take hold far before the researcher sits down at his desk a few months after the end of the field mission when all the data is in and he has once again returned to "normalcy". Instead, it has taken hold long before the first stroll, and is enacted on the first day, with the first and each subsequent observation, which as Olivier describes, is necessarily intertwined with "interpretation". Observation and interpretation, in Olivier's mind, are inseparable activities, and as a consequence the researcher is always actively seeking to replace what he sees into a frame of communal knowledge.

OM: "Donc sur le terrain on n'arrête pas de regarder et d'interpréter, de regarder et d'interpréter, de regarder et d'interpréter. En fait je ne peux pas m'empêcher, dès que je vois une structure, de l'interpréter. Je la vois, je me dis tiens c'est ça, c'est... j'essaie tout de suite de l'interpréter, de l'intégrer dans quelque chose de général, ce qui fait que toute la journée je passe mon temps, quand je ne suis pas en observation, et que je marche, que je me déplace, à réfléchir, à réfléchir sur est-ce que ce ne serait pas ça? J'aborde un modèle."

### 5.6.3 The model as a site of tension between the community's 'ways of seeing' and the researcher's interpretation

For Olivier, "a model", or in Nick's words, "a scenario", is above all else a community-motivated simplification or idealization of what he has seen in the field, and the whole point is to make what one sees "fit" into the frame, so as to convince one's audience that what was seen in the field was accurately observed.

NA: "Il y a un certain nombre de scénarios classiques, parce que la Terre contrairement à ce qu'on croyait avant, c'est compliquée mais c'est pas du hasard. Il y a des scénarios mécaniques et physiques qui sont reconnus. Alors, [dans un scénario], il y a un certain nombre de critères, c'est comme un diagnostique médical. Il y a un certain nombre de critères, que la très grande majorité des géologues va accepter comme caractéristiques d'un scénario. Alors, si tu a l'impression qu'il y a un certain scénario qui

> se développe suite à tes observations, c'est vraiment comme un diagnostique médical, eh? Tu as une maladie, tu as un certain nombre de critères externes qui te permet de dire que tel patient est peut-être atteint de ça, tu sais qu'il y a des testes médicaux particuliers qui peuvent le prouver, tu fais des testes médicaux. Tu as raison ou t'as tort. Mais si ces testes médicaux vont dans le sens de ce que l'ensemble de la communauté pense caractéristique d'une certaine maladie, je traite comme cette maladie. Le géologue fait exactement pareil. Il va les comparer. Il se pose des questions, il imagine qu'un certain nombre de scénarios puissent se développer, il va échantillonner un peu progressivement pour tester ces scénarios. Donc, dans ce cas-là, si les données sont bonnes et concordantes sur un scénario qui est par ailleurs accepté par la communauté, si la région est inconnue, la communauté entérine l'ensemble de tes données, comme confirmant le scénario. Encore une fois, c'est jamais la vérité. On la cherche mais on la trouve jamais. On trouve un scénario probable. Et parmi plusieurs scénarios on cherche le plus probable."

In truth, the link between this community-constructed idealization and actual practice is made through the individual researcher's somewhat idiosyncratic construct, which despite "conventions" that work to mitigate what the individual sees in the field so as to make it "appear" as if everyone is seeing the same thing, is reduced to fit the need of describing the expected.

OM: "... [Les structures qu'on voit sur le terrain], on les idéalise, c'est à dire qu'elles sont censés représenter quelque chose. Et de fait si quelqu'un va sur le terrain, il pourra peut-être chercher bien longtemps avant de retrouver ça... donc il y a aussi une réduction. Il y a effectivement un, il y a une idéalisation pour faire ressortir des faits qui ne sont pas si évidents que ça. C'est à dire que les choses ne sont pas aussi claires sur le terrain qu'on essaie de le faire croire. Ça c'est évident. Et le but c'est de faire croire que c'est particulièrement évident."

For Nicolas, it is precisely the intersection between the community-shaped scenario or idealization and the idiosyncratic individual's transient needs for innovation that shows that "structure" and "convention" is not so easily mitigated by a group of individuals who must confront their differences:

NA: “Et puis, la situation est encore beaucoup plus compliquée si les scénarios classiques ne marchent pas, et tu inventes un scénario. Et, c’est compliqué si tu inventes sur une région connue ou sur une région inconnue, les deux. T’as un certain nombre de données, elles collent pas aux scénarios classiques, tu inventes un scénario. ... Et puis ensuite, pour persuader les gens de ton modèle, alors, d’abord tu peux avoir une communauté totalement incrédule qui croit pas du tout, et on connaît aussi bien le cas des modèles farfelus qui se sont révélés vrais, que les modèles qui ont l’air très bien mais qui ont des défauts, donc ensuite c’est un problème de mode dans la communauté euh, une façon de penser euh, de distinguer les éléments, originale, parmi les autres, etc. Et puis on peut essayer de persuader une communauté incrédule en montant un modèle explicatif et prédictif qui va confirmer les prédictions sur le terrain. Et là, là, c’est un argument très fort. Tu dis bon, mon modèle dit ça, je devrais pouvoir alors, s’il est vrai, ça m’impose telle évolution par la suite, soit tu as déjà les roches ou tu retournes les chercher si tu ne les as pas encore, et tu comptes effectivement que tu peux produire ça ensuite, et seul ton modèle va te permettre de le prédire, ou en tout cas ton modèle te permet de le prédire, enfin, voilà comment ça se passe au niveau de la communauté. [Mais] ça impose, ça impose aussi une censure, notamment chez les jeunes scientifiques, um, parce que la recherche, même quand elle n’est pas impliquée, a toujours une, um, une part de pouvoir en elle. Eventuellement une part financière. Et en tout cas toujours une part importante pour les jeunes chercheurs qui la font parce que c’est cette recherche qui va permettre ensuite de trouver une place. Et parce que les modes d’évaluation de la recherche sont basés actuellement presqu’exclusivement sur les publications. Il est très facile de faire passer un modèle classique, qui marche pas trop mal avec les données, même si on ne croit pas que c’est le meilleur, qu’un modèle très original qui marcherait mieux mais qui passerait beaucoup plus difficilement au niveau de la communauté.”

While it is one task to sufficiently disassociate oneself from the community’s way of seeing in order to determine that current scenarios do not fit the data, it is quite another convince other members of the research community that such a conceptual transgression is warranted. The means by which this is done will be examined in the subsequent discussion.

At this juncture, we can briefly summarize the key elements of geological field practice raised thus far. As we have seen, the geologist over time learns to keep a field

notebook by including "appropriate and relevant" information so that the field notebook can be used as a tool for exploiting the data and for constructing later recontextualizations. He also learns to use image in conjunction with linguistic text for documenting observations and over time, the use of this image outweighs the use of text as the geologist begins to gain familiarity and ease with the visually conventional way of conceptualizing what he sees. He also learns to provide "accurate" descriptions of the field, by learning to "see like a geologist". This, in turn, raises another aspect: This way of seeing becomes so engrained within community knowledge and practice that it effectively acts as a filter, and thus geologists no longer see "everything", but they see "selectively" only that which has already been accounted for by a particular scenario or by a pre-determined set of interpretations. Therefore, observation becomes and cannot truly be disassociated from a conventionalized interpretative 'frame' (Minsky 1975), which is "a [cognitive] data structure for representing a stereotyped situation … a network of nodes and relations. The 'top levels' of a frame are fixed, and represent things that are always true about the supposed situation. The lower levels have many *terminals*—'slots' that must be filled by specific instances or data" (p. 212). The elements of the scenario, or the 'cues' conventionally perceived in the field, encourage the geologist to construct an interpretation based on a particular frame. In this way, he may fail to consider other plausible ways of thinking about the phenomenon, indeed, may not even see other options.

### 5.6.4 The negotiated rhetorical construction of the Field Account

The link between observation and interpretation, however, remains embedded within the individual actor, and clearly what he describes he sees is not perceived in the same way by all. Thus, in the process of conforming to what is expected, there is a deformation of the natural object, and a reduction of the field account so as to make the

details of the account match a particular scenario. The structured account is shaped by the idiosyncrasy of the researcher as he decides, for rhetorical reasons, which details are the most important to retain in order to convince others of what he has seen. As a consequence, the "scientific reproducibility" and "refutability" described by Popper (1978) in his description of the logic behind scientific methodology, is specifically lost in geology and it is unlikely that any other geologist would be able to find the described structure, on his own and without guidance. As argued by Gay (1995), geology in this sense is a "cultural science", and this culture

> "permet de préciser le contexte [N.B., scenario or frame of interpretation], avec des risques d'erreur parfois importants. On sort de la problématique de Popper: pour cet autre, l'acquisition des faits est considérée comme préalable à la confrontation entre théorie et test expérimental. Mais en géologie, la théorie se réduit à un modèle… dont le statut n'est pas très clair… [La géologie] hésite entre la description simplifiée de la réalité et des modèles abstraits, normatifs et autonomes, à support mathématique, physico-chimie, ou un mélange des deux. Les lois géologiques sont *synthétiques* et *universelles,* donc *non vérifiables*."

And thus we can note that the creation of a workable model necessarily calls for discarding a good deal of raw data, thereby moving away from what is actually seen in the field. In this sense, one first level of silence has been imposed on the fieldwork mission as the researcher works to fit his observations into a general pattern because of course everything that he writes to himself in his field notebook doesn't make it into the final stages of his interpretation (see Becker 1995). An accomplished "communicator" knows how to "finesse" the telling of his fieldwork, in other words to pull down a veil of silence over various, "less convincing" aspects of the results. Or as Olivier puts it, determining what is of first order importance, second order, third order, and so on, and elaborating the model from this hierarchy.

OM: "En fait la science, c'est de hierarchiser les choses. Essayer de comprendre qu'est-ce qui est de premier ordre, de deuxième ordre, de troisième ordre, de quatrième ordre, et de prendre en premier le premier ordre, vraiment les

> choses les plus importantes, et d'élaborer un modèle à partir de ce premier ordre. Alors, si ton modèle s'établit, tu peux ensuite faire entrer le deuxième ordre, peut-être le troisième ordre, mais tu ne pourras jamais les faire entrer tous. Un modèle, c'est avant tout une simplification, c'est une idéalisation, et, uh, à mon avis, un bon chercheur scientifique c'est celui qui arrive justement à hiérarchiser ses donnés, à éliminer ce que j'appelerais, à ce qu'on appelerait un bruit de fond, les choses qui sont moins importantes que d'autres. Il faut savoir, uh, comprendre les choses très importantes, et les choses moins importantes. … Bon, alors, tu es toujours critiqué, il y a toujours des gens qui disent oui mais vous ne parlez pas de ça, il faut leur faire comprendre que c'est moins important qu'autre chose. Tu as même parfois des chercheurs qui s'acharnent sur des choses peu importantes, qui sont manifestement du troisième ordre, ils ne regardent que ça alors qu'à côté il y a des choses qui crèvent les yeux... ils ne voient pas l'importance. C'est un problème d'hierarchiser les choses. Moi je pense que la barrière, la limite entre bon et mauvais chercheur, elle ne passe pas dans la technique. Il y a des gens qui peuvent être très bons techniciens entre guillemets, c'est à dire savoir faire des choses, avoir un savoir-faire, vraiment sur le terrain voir tout, te montrer un tas de choses, te dire tu vois là? ça ça veut dire que ceci, que cela, etc. Ce ne sont pas forcément les bons chercheurs. Ce sont des gens qui voient tout, parce qu'ils l'ont bien appris, qu'ils savent bien le faire mais après ils te mélangent le tout sans parler de la différence entre ce qui est important et ce qui ne l'est pas. La science c'est pas un problème de savoir-faire ni de technique, je crois. C'est un problème d'hiérarchie des données."

And thus we can see that the idiosyncratic link between natural reality and community convention is found in the "communication" of this reality, in other words, the rhetorical and often personally motivated representation of this reality. In this sense, the question to be asked here, then, is exactly how the researcher goes about picking out which details to include, which to discard. As might be expected, this is done in anticipation of the response his vision will receive from his peers and thus the rhetorical representation of natural reality is a tacit, ongoing co-construction between the individual and imagined others in his social group, within the framework of its dialogized unity. As so, it sometimes happens that an article that will see the light only a year or two down the road already begins to take shape while Olivier is, in fact, still in the field. Here we see that the influences of his discourse community are already at work, as Olivier begins to mold his interpretation into communally acceptable "fact".

OM: "En fait il m'est arrivé sur le terrain après avoir trouvé un modèle tu vois, assis quelque part en train de manger tout seul à trois mille mètres d'altitude, de penser au titre du papier, alors que j'en étais encore à un an et demi du papier etc., et de me dire tiens ce titre-là est joli et même de le griffonner sur un bout de papier pour ne pas l'oublier. Tu vois donc, ça n'a pas de, on peut penser à des choses, on peut commencer à penser à un papier alors que le modèle n'est pas encore clair et qu'il n'est pas fini. Tout ça, on apprend à le faire [he laughs], on s'interdit rien en fait, si on se fait plaisir on se fait plaisir, on pense à un titre d'un papier. Finalement c'est peut-être un papier qui ne verra jamais le jour sous cette forme-là, mais… Ça permet aussi parfois de voir si le modèle, où il en est, est publiable. Et ça fait avancer les choses aussi parce que tu te rends compte lorsque tu commences à penser à un papier dans ta tête alors que tu es toujours sur le terrain, au bout d'un moment que non, je ne vais pas pouvoir présenter ça… Ça te fait aussi progresser. Et quand tu écris le papier, alors tu infléchis ton modèle, parce que mettre des choses sur papier, vraiment noir sur blanc, ça, là tu te rends compte que, parfois ça te fais progresser, hein? En fait, quand je l'exprime, l'exprimer ne donne pas la même chose, et c'est mieux comme ça. Tu t'aperçois plus des points forts et des points faibles."

And thus, even by playing around with possible titles, we can already see the effects that working within a particular community has on what Olivier permits as acceptable, and throws away as unacceptable, arguments for his interpretation, regardless of whether or not he himself is convinced by those same arguments. In this sense, we see that although fieldwork for Olivier is necessarily a solitary endeavor, he doesn't disassociate his work from his peers and the work they are doing. Instead, he continually works within the confines of future interactions and possible reactions, rhetorically constructing the communication of his observations.

The question now is how these idealizations, models and conventionalized ways of seeing the natural world get translated into written practice. Olivier provides us with one account of this, and here we can begin to understand how the construction of the field account is rhetorical and purposeful. Indeed, there is in fact much done and thought about over the course of a fieldwork outing that gets left by the way-side. And the reduction of

this endeavor's "details" is variously motivated not only by the need to simplify nature in order to better understand it and fit it into some generalizable pattern and through interpretation, where what the geologist "sees" is tempered by what he "knows". But also, the reduction of field details is also moderated by the need to fit results into a pre-established and constrictive frame for acceptable field reporting and to build credibility within one's research community, in short, to sell one's interpretation of the facts.

OM: "... La publication c'est vraiment un truc où tu ne dis pas grand-chose. Tu as quand même recueilli énormément de données pendant un mois, un mois et demi, et comment dire, la, la publication qui vient du travail de terrain est vraiment un tour de force. C'est donner l'impression que tu vas montrer les <u>données</u>, alors que tu n'as manifestement pas la place, et que tu vas conclure un modèle, présenter un modèle qui vient de ces données, que tu ne présentes pas vraiment. Donc c'est vraiment un tour de force. Alors il y a plusieurs façons de s'en sortir... sous forme de tableaux, de figures synthétiques on peut en mettre beaucoup quand même...

DD: Et avec toutes ces données, comment est-ce qu'on choisit celles qu'on va utiliser?

OM: On choisit celles qui vont convaincre les gens... On sait que dans le métier, des mesures de failles, comme ça, si tu veux ça convainc mieux parce que ça fait plus statistique, et puis ça fait plus scientifique parce que les données ont été traitées par ordinateur. Manifestement ça convainc mieux que si j'avais dessiné une figure synthétique schématisant en fait ces données. C'est clair. En disant voilà, c'est comme ça sur le terrain. C'est comme ça sur le terrain et les gens ne sont pas forcés de me croire. En fait j'ai doublé moi avec une photo montrant les structures, c'est simple, donc je montre une photo à un endroit où les structures sont les plus beaux et les plus démonstratives, et puis tac, ça c'est le mesure partout, un deux trois quatre, ce sont les endroits où j'ai mesuré. Donc c'est un vrai tour de force, où il faut réussir à mettre quand même le maximum de données en sachant que, hein, on peut pas les mettre toutes, et qu'il faut extraire celles qui vont convaincre les gens..."

Of course, there is nothing truly surprising in what has been recounted here about the geological field, and many elements of this have been amply described by other analysts in terms of the communal construction of the milieu and way of looking at scientific

phenomena (Knorr-Cetina 1978). What is especially interesting here, however, is l how the community shaped habitus and boundaries of the social field act as a site for text construction, and ultimately as a site for performative instantiations and transgressions of norms. We will turn to this in the next section.

## 5.7 The text as a site of examination for the intersection between social field, *habitus*, and conditioned need-driven local innovations

In this section, we will examine a number of research articles from each informant, spanning a range of a few years to ten years, depending on the duration of the researcher's active career. These are articles published in English, of which the earliest were based on their dissertation field research. By comparing their various writings that have been published over a number of years, we can look at the changes in their writing style over time as reflected in variations in the frequency of the linguistic traces discussed in Chapter 3. In so doing, we may also perhaps see the effects of the authorial-self subsisting within the social structure as a result of privately need-based innovation.

### 5.7.1 Olivier

Olivier the researcher of nearly 20 years, the professor, the dissertation advisor, the "self-made" field geologist, is also a writer, and an accomplished one at that. His list of publications includes 49 research articles, 27 of which he is the sole or primary author. This list includes 32 papers published in English, in various international structural and tectonic journals, as well as 17 papers in French. Nonnative speakers of English and ESP practitioners alike know that getting published in English is not a given, regardless of one's achievements as a researcher and the number of publications in one's native language (e.g., Mauranen 1994, Sionis 1995, Birch-Béccas 1997).

Now that we have gotten a sense of Olivier as a field geologist and savvy, experienced researcher, let us look at some samples of his writing to get a sense of how

he reports on his field research. I have looked at five of his articles, published between 1984 and 1993. The first excerpt is from an article published in 1984 in the *Journal of Structural Geology,* entitled "The curved translation path of the Parpaillon Nappe (French Alps)." This article is based on his dissertation research, which Olivier, as first author, co-wrote in English with J.P. Brun, his dissertation advisor. It is taken from his field results section (sentence numbers have been added).

> [1]The *D2* deformation is the most obvious in the landscape. [2]It is characterized by large and numerous large-scale recumbent folds with a SW vergence, and a mean axial trend N140°E. [3]All stratigraphic units and *S1* cleavage are deformed by the *F2* folds. [4]An *S2* cleavage, showing a fan disposition, is generally observed in the hinges but not always in the limbs where the *S1* cleavage is often completely preserved. [5]Because of the very heterogeneous deformation, where no *S1* cleavage has been developed, the *S2* cleavage is locally the only cleavage observed. [6]The associated stretching lineation has a mean trend N50°E (Fig. 2b). [7]The detailed description of the *D2* structures is best achieved by separating the nappe into two units (Merle 1982a, b).
>
> [8]The contact between the two units has not been observed in the field. [9]Nevertheless, because in the two units, (a) the sense of horizontal shortening variation is inverse, (b) the timing of the *D2* deformation is slightly different (Oligocene in unit 1 and Miocene in unit 2) and (c) horizontal klippes over unit 1 (Figs. 3a & b) are associated with an intense deformation of the top part of unit 1, as demonstrated by strongly developed cleavage and large-scale sheath folds, we deduced (Merle 1982a) that unit 2 has slid over unit 1 (Fig. 4).

Nothing appears out of the ordinary here, given what we have already seen as typical of field reporting in geology research articles from Chapter 3. Here, we can see a number of field details (measurements, descriptions) that serve to bolster Olivier's interpretations (s.5 'Because of the…' and s.9 'Nevertheless, because… we deduced that…'). And, once again, the geological formations are the prime movers and doers:

s.1: D2 deformation, landscape

s.2: large and numerous large-scale folds, vergence, mean axial trend

s.3: all stratigraphic units, S1 cleavage, F2 folds

s.4: S2 cleavage, fan disposition, hinges, limbs

s.6: associated stretching lineation, etc.

From a field report 1234 words in length, these geological formations and their descriptive qualifiers make up, as is typical, slightly more than a quarter of the entire textual space (26% of the overall text for this first article in contrast to 27% as an average for structural geology).

Olivier as an author does not overly draw attention to himself as the researcher in the field, and comparing the typical strategies for overtly marking field presence in the corpus, we can see that Olivier uses this strategy less frequently than is standard for the structural geology corpus. Accordingly, there are no traces of agential statements, and less than half the typical number of interpretive comments (16.82, compared to Olivier's 7), and as few evaluatives (22, in contrast with the average 46). Some of the evaluatives Olivier did use can be seen below:

**Category I traces: Strong authorial implicature in the Field Acccount**

Judgment-marking adjectives:

s.1: 'most obvious'

Comparative adjectives and adverbs of authorial evaluation:

s.4: 'generally', 'not always', 'often completely'

s.5: 'very', 'only'

s.9: 'slightly different'

The run-down of the other categories of traces of field presence, where Olivier as the author demonstrates the work he has done in the field, can be seen as follows:

**Category II traces: A 'disguised' account of researcher activit**

Measurements:

s.2: 'SW vergence', 'N140°E',

s.6: 'N50°E'

Nominal and verbal indications of research activity:

s.3, 5, 8: 'observed'

s.9: 'demonstrated

Locational adverbs and prepositions:

s.1, 4, 9: 'in'

s.4, 5: 'where'

s.5: 'locally'

s.8: 'between', 'in the field'

s.9: 'over'

Metadiscoursal references to visual data:

s.6: 'Fig. 2b'

s.9: 'Figs. 3a & b', 'Fig. 4'

Prior contributions/publications:

s.7: 'Merle 1982a, b'

s.9: 'Merle 1982a'

And so, we can see that this early publication is a relatively standard document, conforming nicely to the social field's required "researcher discretion". And this despite what we know about the particularly difficult time Olivier had in getting started as a field researcher, left alone to face the mountains at 3000 meters altitude. However, as we will see, an important shift appears to occur in Olivier's style as time goes on.

The second excerpt is from a 1989 article published in *Alpine Tectonics, Geological Society Special Publications,* that Olivier, once again as first author, co-wrote with a colleague from Rennes, P. Cobbold, as well as another from Switzerland, S. Schmid, on the Pennine zone of the central (Lepontine) Alps.

> [1]The main lineation observed in the Pennine zone is a preferred orientation of grain aggregates, individual grains, fold axes, rods and boudins. [2]This structure seems to track the direction of maximum total stretch. [3]Fold axes parallel to the stretch lineation occur at small to intermediate scales (Wenk 1955). [4]Occasional eye structures are probably transverse sections through sheath folds. [5]We have also observed definite sheath folds in oblique sections (Cobbold 1980).
>
> [6]We have measured the attitude of HTD stretch lineations at 882 localities throughout the Lepontine dome (Fig. 4), thus confirming the unusual radiating pattern first described by Wenk (1955). [7]If we compare the attitude of foliation and lineation (see Figs. 1, 2 and 4), we see that in the central areas of flat-lying foliation (Ticino and Simplon subdomes), the lineations fan out in directions approximately perpendicular to the peri-Adriatic fault system. [8]Even in the anomalous Maggia steep zone, this radial pattern of flat-lying lineations is not disturbed, the lineations following the strike of the steep zone.
>
> [9]As one approaches the northern steep belt near Airolo, both lineation and foliation acquire steep attitudes, suggesting that they have become folded together about a horizontal E - W axis during later events. [10]Similar geometrical relationships occur as one approaches the southern steep belt, either in the Bellinzona area and further E, where the lineation is nearly vertical, or in the Domodossola area, where it now pitches at about 45°E. [11]This suggests that foliation and lineation were folded together about horizontal axes during a later event in the southern steep belt as well."

At first glance, this passage may also appear to be typical of field reporting in structural geology, for we see familiar geological structures, domes, foliation, belts and faults going about their business of interacting with one another. But what is particularly striking here, and very atypical of field reporting discourse, is the high incidence of overt authorial implication. There is a higher than average number of the types of evaluatives that mark the

researchers' intellectual engagement in their fieldwork (nearly 10% of the whole text in contrast to the typical 3% of the corpus):

s. 4: 'occasional'

'probably'

s. 6: 'unusual'

s. 8: 'anomalous'

s. 10: 'similar'

'nearly'

'at about'

But we might notice especially the "We have observed", "We have measured...", "if we compare… we see that..." and especially interesting, "As one approaches...", not once but twice, all within this short passage. This is a trend that continues throughout the whole of Olivier's field account, occurring ten times, making up 3.8% of the whole Field Account, a frequency that is largely above the norm since typically this type of linguistic option occurs less than once per article in structural geology). In addition, there is a slightly higher than average number of discreet traces of researcher activity in Olivier's text. And so, in contrast to the other articles of the corpus, where we see relatively little textual researcher presence, we almost have the impression here that we are accompanying the field researcher as he makes his way over the terrain, tracking the course of structures. This may be as much "presence" as modern geological discourse will permit. This is a trend that continues in his later articles, as well, namely a 1993 article where he reports on the field research he undertook while on post-doctoral leave in Colorado.

Of course, one might rightly ask how it is possible to determine what is "Olivier", and what in turn has been influenced by the presence of other authors, especially P.

Cobbold, who is British, and as Olivier pointed out, had gone over this 1989 article with a fine-toothed comb. First, it must be noted here that regardless of "who said what", the very fact that this type of researcher-oriented discourse is so clearly present is highly unusual. However, as I soon noticed, excepting his first publication in English (1984) Olivier's other articles also have a tendency to be personalized in this same way. In this sense, Olivier's published writing style appears to have undergone a transformation over time, becoming more implicative of his researcher presence and authorial persona. When questioned about his writing practices, Olivier was a bit taken aback, not quite knowing how to respond.

DD: … Ce que j'ai remarqué, au moins dans tes articles de 1989 et 1993, c'est que… il y a une façon de parler du travail de terrain qui marque une présence agentive, on voit l'auteur sur le terrain. J'ai lu des choses du genre: "We have verified and where necessary supplemented the measures by..." On voit beaucoup, enfin relativement beaucoup de "we", "our" dans tes articles … j'avais beaucoup plus un sens que tu avais été sur le terrain, mais avec [Michel Ballèvre; NB. another 1993 article where a colleague from Rennes is first author], je n'ai pas eu cette impression…

OM: Oui, pourtant, c'est un géologue de terrain, hein. C'est un vrai géologue de terrain. Mais moi, j'aime bien m'impliquer en fait. Là je mets "we" mais dans les articles où j'écris tout seul je n'hésite pas à dire "I".

DD: Ah bon? Et à ton avis est-ce que c'est très commun qu'on écrit de cette façon? Ou est-ce que c'est un style particulier? Ce n'est pas forcément très commun…

OM: Non, je… c'est difficile à dire… Là, [NB. referring to the 1989 article, second excerpt seen above] de toutes façons, l'anglais a été particulièrement fignolé par Pete Cobbold, qui est anglais. Donc, le style déjà littéraire a été, bon, … c'est assez difficile de répondre à ça. Ça se voit partout?

DD: Euh, oui, mais pas beaucoup dans le premier article, justement, de 1984. Là c'est, c'est pareil, ça ressemble beaucoup plus au style de Michel. Et c'est pour ça que j'avais posé la question sur les coauteurs… quand est-ce que tu as soutenu ta thèse, en 1982?

OM: 1982, oui. Vraiment, vraiment je ne sais pas répondre à cette question. [We both laugh, a bit embarrassed] parce que … "We" quand même, "we argue" …

DD: Oui mais ça c'est différent. C'est pas juste l'usage du "we"… euh, je vais te montrer des… voilà tiens. "With the help of mapping", "With the benefit of mapping, we were able to map in even greater detail" blah blah blah, "we were able to explore" … and "track"... [NB. in the 1993 article on Colorado fieldwork].

OM: Ouais. Hmm.

DD: Ça c'est très intéressant comme implication … Et c'est justement ce type d'implication personnelle que je n'ai pas trouvé chez Michel, ni chez d'autres auteurs …

OM: … C'est, c'est, euh, moi j'aime bien m'impliquer de plus en plus à vrai dire. Je me sens plus, euh, en fait c'est dans ce premier papier que je m'impliquerais moins, je fais beaucoup plus jeune aussi, hein, parce que pour s'impliquer il faut, sentir qu'on peut le faire tu vois. Euh, donc euh, mais par contre au niveau de la véritable implication, je ne suis pas plus impliqué là [1993] que là [1984]. C'est clair. Donc, c'est juste une façon de présenter les choses, plus impersonnelle, parce que j'avais l'impression que plus c'était impersonnel, plus c'était scientifique, plus c'était quelque chose qui euh, je décrochais la vérité, les faits quoi, les modèles, … alors que maintenant en fait je, c'est effectivement peut-être le désir de montrer que, je m', c'est bien moi quoi, ce que je raconte c'est moi, un autre racontera autre chose. Je pense que c'est ca.

DD: Et donc...

OM: Mais je n'avais pas remarqué, tu sais…

And so, what we would appear to have here is evidence for the evolution of a writer's persona over time, although it seems to have been the result of some unconscious action. At the outset, as a young researcher rather low-down on the hierarchy of status, Olivier appears to have been driven especially by the need to conform, to make his text resemble the norm as much as possible so as to try and work his way into the community (Schryer 2001). However, as time went on, Olivier allowed himself to transgress the typical silential boundaries of the field account by clearly showing his own implication as a field researcher. His personal implication, however, does not make him "more" of a

field geologist than any other, for as he explained, his colleague Michel Ballèvre is, in his estimation, a "real" field geologist, too, although elements of his field geologist's persona do not appear in his writing. And so, it would seem that one must look also into other elements of Olivier's own *habitus,* extending beyond his professional experiences and into how the life-long interactional contact his social milieu has imposed upon him has shaped his world-view. As Olivier confided at the end of our interview, as we were walking back down the hall to the front door, he had grown up in a family of "Lettrés", where each member of his family holds a degree in the Humanities (his father is a philosopher; his siblings each hold degrees in literature). Olivier, in fact, is the "lone scientist" of the bunch, and he believes his family background may have something to do with how he chooses to express himself as a "scientific writer".

This account also provides further evidence for Bourdieu's (1984) description of social structure, where performative transgressions are conditioned by the structure of a social field, and for the crossing over into new fields where performative transgressions are permitted. The "seeds" for this performativity, its driving force, are perhaps to be found in Olivier's personal motivations constructed in a milieu where "presenting oneself" was considered important, and showing his field presence more overtly than is typical responded to some inherent private need on Olivier's part to do so (Leont'ev 1981). We might also suspect that in geology research articles reporting on fieldwork, the researcher is allowed by his community of peers to transgress typical practice when a corresponding increase in authority and status has been obtained. However, the concrete existence of such a status remains unclear, for it would appear to be self-imposed by the researcher himself, rather than actively imposed by the social structure. Thus, Olivier relates that if he were to come across an article written by a junior researcher in which the author implicated himself and his field presence more than usual, he, as a senior researcher, would not be offended or feel the need to cut him back down to size. And so the relationship between the individual and the collective is blurred: is conventional

social behavior but an imagined construct in an individual's mind? or are there instances when an individual from within the plures may actually reject such idiosyncratic innovations?

It is the sense of "self" in relation to the personal and private sphere which, like for Olivier, has reoriented Nick's research endeavors over time, as we shall see in following section.

### 5.7.2 Nicolas

Nicolas defended his dissertation in 1992, and since that time has published 33 articles, of which 9 are as first author and 27 are in English (6 of his first-author publications are in English). In this section, we will examine three of these English-language publications, the first of which was published in 1993 in a well-known and prestigious structural/tectonics journal, *Tectonics,* and is based on his doctoral research in the Kunlun mountain range in Tibet during 1989-1990. The second, based on a later fieldwork mission in the same area, was published in 1999 also in *Tectonics*, and the final article, the result of a more recent field mission to Tibet, was first submitted to *Tectonics,* and then to *the Journal of Geophysical Research,* which has accepted the article which will appear in 2002.

In the first sample of writing Nick has given me, he is the first author of an article called "High cooling and denudation rates at Kongur Shan, Eastern Pamir (Xinjiang, China) revealed by 40Ar/39Ar alkali feldspar thermochronology." As we might imagine from the title, the focus of his research is somewhat different from Olivier's, whose primary contact during the era of his field missions *was* the field. While Nicolas has also been trained as a structuralist, we can recall from an earlier part of this chapter that he also confines this field research with chronological parameters, making him more

dependent on the laboratory than Olivier for developing his interpretations and bringing them into fruition.

It was cowritten with J.-M. Cantagrel (from Clermont-Ferrand), M. Brunel (Montpellier) and P. Tapponnier (from the I.P.G. in Paris, or the Institut Physique du Globe, "the" very prestigious French geophysical institution founded by Claude Allègre in the 1960's). The latter two coauthors are leading French and world-renowned specialists on the Himalayas and Tibet, with multiple publications and citations. This article is also the first written by Nicolas in English as first author on the subject of his Ph.D. dissertation, which he defended in Clermont-Ferrand in 1992. His dissertation was a structural and thermochronological study of the Cenozoic[xxxvii] tectonics of the Asian plate in the northwestern Himalayas, for which he went to Tibet for two successive field missions (1989 and 1990).

Nearly his entire dissertation focused on the rocks he collected during these two original field missions in Tibet. For this article, he reports on sampling done in the Kongur massif, to the east of the Karakorum fault, along the western termination of the Kunlun-Altyn Tagh mountain ranges. What follows is an excerpt from his field account, in all appearances "embedded" within a Geological Setting section (see section 3.6), given that the subsection is entitled "Geological Setting and samples description". However, the field account here in fact replaces the scene-setting Geological Setting literature review and Nicolas in fact makes little reference to what is generally known about the region.

> [1]The northwestern part of the Kunlun, at the border between China, Tadzhikstan, and Afghanistan is a wedge where the Pamir, Kunlun, and Tien Shan ranges meet. [2]Covered by glaciers, the Kongur Shan and Muztaghata mountains (Figure 1) are the dominant structural features of this area. [3]The Kongur Shan mountain (Figure 1) and Muztaghata, its twin structure, are two topographic anomalies that necessitate a structural explanation at the crustal scale. [4]The Kongur massif is interpreted as a ramp antiform thrusted in a northerly direction (Brunel et al., 1992) over

> the Tarim crust covered with a thick (10 km) "décolée" sedimentary series of Miocene to Quaternary age… [5]The Kongur massif (Figure 2) therefore forms a large antiform, 10 km high and 25 km wide. [6]To the north, the contact between the antiform and its Paleozoic cover is marked by a thick (1 km) dextral ductile shear zone. [7]Extensive nappe tectonics was described in this area (Brunel et al., 1992)… [8]To the west and the southwest, the antiform is bounded by normal faults, which contribute to control the topography of the whole western face… [9]The core of the antiform exhibits many fewer deformed gneisses and leucogranites, while those outcropping on the outer parts are highly deformed, therefore suggesting a close relation between the present external deformation of the antiform and the activity of surrounding faults. [10]To the south, the Muztagh antiform is the twin structure of the Kongur … [11]All dated samples in this study were collected on the part of the antiform, where deformation in relation to active faulting is prominent. [12]The mylonite sample KLB1 is a phyllonite collected from the normal fault path at 3300-m altitude, where beds of muscovite, quartz, plagioclase, and K-feldspars alternate. [13]Small muscovites (50 μm) develop within shear bans associated with the normal fault movement. [14]Sample K90G08 was collected 2-km to the east of KLB1 at 2900 metres altitude, whereas K90G34 is 10-km away at 2500 metres altitude….

Moderately short with only 491 words in contrast to an average of about 1860 in general, this field account, as mentioned above, is a 'pseudo' GS section. The emphasis is really on the description of the site and this description uses the authors' own field data in order to establish the tectonic structure of the region. This peculiarity may be better appreciated within the context of doing fieldwork in Tibet and its surrounding regions, for as Nicolas explained during our interview, hardly anyone has done fieldwork in this area of the world. An absence of any prior literature on the region therefore necessitated the use of their own field results. This was, what is more, the first ever publication of such data on this one specific antiform.

It is nonetheless a more or less typical field account, both in terms of its internal structure and its linguistic traces of field presence. It shows an expected descriptive to interpretive back-and-forth movement, as seen in the following interpretive phrases embedded within a surrounding field description:

s. 4: ‘The Kongur massif is interpreted as…’ (NB. Here Nicolas was one of the publishing authors of Brunel et al., 1992)

s. 5: ‘The Kongur massif (Figure 2) therefore forms a large antiform…’

s. 9: ‘The core of the antiform exhibits many fewer deformed gneisses … therefore suggesting a close relation’

This field report essentially rides the line of conventionality, and Nicolas, although clearly present and active in the field, does not — at first view — draw overt and explicit attention to himself. Thus there are no statements of actual presence and only a reasonable number of evaluatives can be found (1.8% in contrast to the typical 3.29% of the corpus). This may reveal Nick’s novice status, and as he said himself in a later response to a draft of this chapter:

NA: I tried hard to mask my personal inclination to more “direct” writing. I was young and scared!

However, the relationship between the junior researcher to supposed community norms is not quite so clear here despite what Nicolas would have us believe, and we might wonder if his “personal inclination to direct writing” doesn’t come out a bit more than he intended. For example, he uses a few more “disguised” and “discreet” linguistic traces (i.e., Category II traces) to mark his field activity than is common, which while they formulate the different activities he engaged in the field, in fact also make him appear active in his text, giving it more “field sustenance”.

Of course, it is also a possibility that a door was opened for more explicit detail of the field given that there was so little literature to fall back on.

<u>Metric, angle and direction measurements:</u>

s. 1: ‘northwestern’

s. 4: ‘northerly’, ‘10 km’

s. 5: ‘10 km’, ‘25 km’

s. 6: ‘north’, ‘1 km’

s. 8: ‘west’, ‘southwest’, ‘western’

s. 10: ‘south’

s. 12: ‘3300-m altitude’

s. 14: ‘2-km’, ‘east’, ‘2900 metres altitude’, ‘2500 metres altitude’

Nominal and verbal indications of activity:

s. 11: ‘dated samples’, ‘in this study’, ‘were collected’

s. 12: ‘sample KLB1’, ‘collected’

s. 14: ‘Sample K90G08’, ‘was collected’, ‘KLB1’, ‘K90G34’

Locational adverbs and prepositions:

s. 1: ‘at the border’, ‘between’, ‘where’

s. 4: ‘in’, ‘over’

s. 6-8: ‘to’, ‘between’, ‘to’

s. 9-10: ‘on the outer parts’, ‘between’, ‘to’

s. 11-14: ‘on the part’, ‘where’, ‘in relation to’, ‘from’, ‘at’, ‘within’, ‘away’

Metadiscoursal references to visual data:

s. 2-3: ‘Figure 1’

s. 5: ‘Figure 2’

Geographical location markers:

s. 1: ‘Kunlun’, ‘China’, ‘Tadzhikstan’, ‘Afghanistan’, ‘Pamir’, ‘Tien Shan’

s. 2-3: ‘Kongur Shan’, ‘Muztaghata’

s. 4-5: ‘Kongur massif’, ‘Tarim crust’

Prior contributions/publications:

s. 4: ‘Brunel et al., 1992’

s. 7: ‘was described in this area (Brunel et al. 1992)’

In addition, in 8 of the 14 sentences from the excerpt, Nicolas continually draws the reader’s attention back to the research team through metadiscourse (s. 3), through reference to his or their own prior research (s. 4 and 7), in his choice of verbal phrase and argumentative markers, such as the interpretive comments found in s. 4, 5, and 9, and finally, in a number of explicit nominal and verbal indications of researcher activity (s. 11-14).

s. 3: ‘… two anomalies that necessitate a structural explanation at the crustal scale.’

s. 4: ‘The Kongur massif is interpreted as… (Brunel et al., 1992)’

s. 5: ‘The Kongur massif (Figure 2) therefore forms a large antiform…’

s. 7: ‘Extensive nappe tectonics was described in this area (Brunel et al., 1992)…

s. 9: ‘The core of the antiform exhibits many… while those outcropping on the outer parts … are highly deformed, therefore suggesting…’

s. 11: ‘All dated samples in this study were collected…’

s. 12: ‘The mylonite sample KLB1 is a phyllonite collected from…’

s. 14: ‘Sample K90G08 was collected 2-km to the east of KLB1 at 2900 metres altitude, whereas K90G34 is 10-km away at 2500 metres altitude….

An analysis of the use of these traces of field presence and of the various verbal strategies thus paints a revealing picture of Nicolas as a young researcher-author as a confident author, who also happens to be working with such distinguished geologists as Brunel and Tapponnier. While his text is firmly conventional, following procedure to a ‘t’, it is secure in its presentation, and we might even espy a possible transgression of a silential boundary or two, something perhaps unexpected in one’s first article given what

has already been said about the role social structure and hierarchy may play in supposedly allowing or disallowing such transgressions.

For example, we might note with some interest that the mountains in which he did his fieldwork were "covered by glaciers" (s. 1). Such a qualifying statement is somewhat surprising and unusual, and we might rather expect for this to be the type of information geologists generally dismiss as having no scientific relevance. Yet does it in fact have no scientific importance, and is it merely a personal "tidbit"? Responding to an earlier draft, Nicolas had the following to say:

NA: "I think I meant two things here. First, coquetterie: see how high I went and also it was beautiful. And second, it was high … and I could not see below the glacier. So this is an unconscious mixture of personal and scientific reason."

This may very well be, then, a partial indication of a concealed, personal story, for we can very well imagine, without having been there, what it must have been like to do fieldwork on a glacier. This sense of importance accorded to the fieldwork endeavor is coupled by the physical significance the authors given to their results collection, as suggested by the following sentences taken from the Introduction:

> "The complexity of the present tectonic setting of that area may be better understood after **a more detailed study** of its present and recent dynamics. **In 1990, the Sino-French Karakorum mission sampled the whole region along the Karakorum highway, and we report here the first thermochronological data** and estimates of the exhumation rates of the Kongur Shan antiform…" (emphasis added).

As Nicolas confirmed, the study was indeed a major undertaking and they sampled the entire region along a highway extending some 500 kilometers, over the course of two years. In addition, we can recall that they are reporting on the very first thermochronological data ever published on this one specific antiform, making this article

an important element in the construction of new, community knowledge. And thus, we might surmise that this would place Nicolas in a rhetorically stronger position, allowing him a bit of leeway to talk about "snow-covered glaciers".

Pushing this logic a bit further, we can also come back to the curious collecting statements, seen in s. 12 and 14:

> 'The mylonite sample KLB1 is a phyllonite collected from the normal fault path **at 3300m altitude**…'
>
> 'Sample K90G08 was collected 2 km to the east of KLB1 **at 2900 metres altitude**, whereas K90G34 is 10 km away **at 2500 metres altitude**.'

And so we might be tempted to ask here why the authors include altitude information? What essential elements does this add to placing the sample in its "scientific" context? One might be tempted to believe, given the absence of such "qualificative trends" from the whole corpus (Chapter 3) that what we have here is the author once again drawing attention to the fact that doing fieldwork at such altitudes makes it difficult to work and breathe. However, Nicolas provided a different explanation for this apparent "transgression" of silential norms.

NA: "Altitude is essential to thermochronology, which uses it as data in calibration. Also, maps of the region are rare or lacking. The norms of locality are uncertain and GPS did not exist at that time. So altitude gives a frame of reference."

And so we learn more about the specific conditions of doing fieldwork in Tibet, where depending on where you get your maps from, the names of the same locales could be noted in Chinese or in Uygur, making simply "getting there" or comparing sites descriptions something of a chore. Given that map names change frequently in the area, not to mention that terminologies are extremely different between Chinese and Tibetan, deciding which names to use can be politically 'meaningful', and asking for directions,

even based on a published map, by no means assures one of getting to the right place. Nick thus describes the emergence of a local community of practice, where practioners working within the same region have developed a set of cues specifically related to their own needs and which differ from what geologists *generally* do, for "altitude information" is not a typical sort of detail one finds in fieldwork articles. This, then, perhaps constitutes evidence for the "local innovations" described by Cole and Engeström (1993), which may appear aberrant or irrelevant to outsiders of this tightly knit community where researchers commonly know each other and review each other's work. However, such detail for those "in the know" contains its own set of situated and standardized meaning, made necessary by the particular communicative needs of this small community. As such, it represents an instance of "a rhetoric of understatement", or the private transgressions by a community of a conventional silential norm through the use of a discreet set of indicators, or "linguistic expressions" in Ducrot's (1973) terms, allowing members to transmit a wealth of information to others through a mere "word to the wise".

Nick's apparently strong rhetorical position in the research community, as inferred from this article, is also granted perhaps more readily by the presence of heavyweights on his research team, who are further associated to his name by a number of English-language, and therefore "accessible", publications — indeed, there is at least one paper published each year between 1992-1996 in which Nicolas has co-authored a paper with P. Tapponnier, pointing to confirmed research ties. And so, while the field account overall is mostly conventional, admittedly working hard to conform to community standards with an accompanying reduction of agential voice so as to be allowed "a space", there are also a couple of not insignificant and highly unusual transgressions of conventional silence (e.g., "altitude markers" or "snow-covered glaciers"). As Nicolas explained, there are commonly a good deal of explicit field presence markers in the writings of researchers on the Himalayas and Tibet given the enduring importance of tacit conventions in Nick's smallish research community,

originating in practices from the last century, whereby field geologists 'signal' to their colleagues how to get someplace as proof of their good faith. Such transgression are thus motivated by the local innovations of his research community in their quest to adequately define their environment and communicate their findings to each other.

Others (e.g., "snow-covered glaciers") perhaps find their justification in the directness of Nicolas' style, which as he described, was both part of his personal 'make-up' and necessitated by his personal attachment to the work he does in the field. It is important for him, somewhere along the line, to mark that activity as something belonging to himself.

DD: ... Est-ce que tu, puisque c'est pas toujours important d'aller sur le terrain, c'est pas toujours nécessaire, mais que tu aimes bien y aller, est-ce que tu éprouves le besoin de dire,

NA: Oui.

DD: ... de dire oui que j'ai été,

NA: Oui.

DD: ... sur le terrain? Et, enfin, comment est-ce que tu le fais? …

NA: Moi je le fais discrètement. C'est une coquetterie. C'est une coquetterie que je m'autorise. Lorsqu'on est allé quelque part, bon, alors. ... Il y a une partie de coquetterie qui est de dire, euh, de glisser ça et là, des remarques qui ne peuvent venir que des gens qui ont vu, on peut dire, au-delà de ce tournant ou au-delà de la sortie de telle ville, vers l'est ou vers l'ouest, il faut y avoir été pour le voir. Donc euh, c'est aussi un signe de reconnaissance dans les communautés qui travaillent sur ces sujets-là. Celui-ci y va, celui-là n'y va pas. Ça peut dans un certain nombre de cas, habituer les gens à avoir une plus grande confiance dans ce que tu publies. Et puis il y a aussi le plaisir de, parce que ma vie est très attachée au terrain, parce que le fait de noter sur mon carnet les petits événements de la vie quotidienne, c'est un moyen technique pour me rappeler de la géologie, c'est aussi ... pour moi, euh, au moment où je l'écris, euh, ça me rappelle des souvenirs. Et donc je l'ai parce que je sais qu'à la relecture, parce que je vais relire très souvent, … euh, je sais que ça va me faire plaisir de retrouver ces petits éléments.

One might further surmise that this authorial affirmation and need to find himself in his text resulted in a stylistic difference, which was "authorized" and allowed visibility earlier than for most junior researchers because of Nicolas' collaboration with more established and well-known researchers. Some of his coauthors' status and reputation, one might say, rubbed-off on or was transferred to Nicolas, thus permitting his personally driven transgression of silent conventions, or his discreet "coquetterie".

And thus the boundaries of performative transgressions might in fact be shaped by the context of each specific writing situation. While in Olivier's case, this context appears to have been shaped over time, growing broader and less confined as he gained in relative status and reputation, Nicolas' publications tell a different story. Early on, Nicolas considered that he had the "option" and even obligation of discreetly transgressing conventional silence, for the reasons cited above. However, as we will see, he in fact shows less personal implication in his later publications. We will next look at the two remaining publications, 1999 and 2002, for the defining contexts of a situated writing event are also a factor in the manipulation of salient and silent conventions.

The second article is taken from a time when Nick had had the time to confirm himself as a researcher, and we would expect that this text to demonstrate a greater authorial persona or 'stylistic presence'. It was co-written with a colleague, his "good friend" Edward Sobel, an American from Chicago. Since his earlier dissertation research, Nick has continued his work on the Himalayas and Tibet, returning for 11 successive field missions since his dissertation. Sobel is also a specialist of the Himalayas, and in the references at the end their article, four other articles are cited where Edward has done work in the region. Although Edward is first author here, Nick gave me this article as a sample of his own writing given the intensive amount of passing back and forth they did. In this sense, they really did end up writing it together, giving Nick a sense of propriety over the published version of the field account.

What is unexpected in this field account, however, is that at no point throughout the entire section do we have any clear indication that Nick and Edward have ever been in the field, although I know for a fact that they have. The authors are peculiarly and almost even extravagantly 'silent' here, an impression corroborated by a lower than normal frequency of the types of discoursal options geological writers make use of to construct their field accounts. In fact, we must go outside of the Field Account to find proof of their field mission, to the sampling sites indicated in Figure 1 or to the Discussion section to have at last absolute textual 'proof' of their having set foot in the field:

> "This suture **is difficult to follow** as it trends obliquely to presently active faults. Moreover, the northwestern margin of the AT **is challenging to interpret**, as outcrops are covered by thick late Cenozoic deposits in the Tarim basin. **When followed to the west**, the northern side of the Altyn Tagh strike-slip fault zone (ATF) includes a series…"

The entire field account itself gives little or none of the authors' own field results. Instead, within the specific context of this recontextualization, the typical purpose of establishing field presence and competence appears to have been co-opted by the need to describe others' field data, and accordingly the discourse is constructed around these other researchers who are busy measuring, describing, mapping, considering, documenting, reporting, or presenting. An excerpt of this writing can be seen as follows:

> "[1]Several ultrabasic bodies are mapped and described within the range [Wang et al., 1993; Liu et al., 1998]. [2]The Hongliugou "ophiolite" is the best-documented ultramafic belt… [3]Geochemical results are reported for dunte, harzburgite, lherzolite, pyroxenite, diabase, and gabbro. … [4]The lithologic description and geochemistry are consistent with the designation of ophiolite [Coleman, 1977]. [5]The unit is mapped as intruding the country rock, shown as the Middle Proterozoic Jixian and Changcheng Groups, and cross cut by an undated Late Proterozoic grandiorite. [6]Likely for this reason, the ophiolite belt is classified as Middle Proterozoic. [7]It is unclear what radiometric age data were available when the 1:1,500,00 geologic map [Wang et al., 1993] was published.

> [8]Metapelites have recently been described from the Hongliugou ophiolite belt [Che et al., 1995a]. [9]These are described as grey-white mylonite and are mapped as the lower Middle Proterozoic Changcheng Group in contact with Middle Proterozoic Jixian strata to the south and bounded by a thick, schistose ductile shear zone to the north… [10]The description of the ophiolite body suggests that it has been strongly metamorphosed as well as deformed by a ductile shear zone [Che et al., 1995a]. …"

As we can see here, the task Nick and Edward have set out for themselves resembles much more the Geological Setting, a background scene-setting review of the current state of knowledge, than it does a description of their own fieldwork. The only personal implication we might find here are the following interpretive comments, still based on what other researchers have published as descriptions and interpretations of the region.

s. 4: '… are consistent with'

s. 6: 'Likely for this reason…'

s. 7: 'It is unclear what radiometric age data were available…'

s. 10: '[The description of the ophiolite body] suggests that…'

As Nicolas explained, the situated context of this particular field account was conditioned by an idiosyncratic set of circumstances that resulted in their making no reference whatsoever to their own field endeavors.

NA: "The explanation I offer is double. Ed and I were both in the field, but on <u>different</u> occasions. We never went there together! When we compared our fieldnotes, we had a hard time recognizing what was which place! We therefore reconstructed a map, and because it was already largely "depersonalized" and also because the geology there is very complicated, we added the description from the Chinese literature. We know that literature to be difficult to read (!) but usually very good. Also because we had few rocks from that place (less than we would have wanted), we used their literature to extend the significance of our own data to a broader area than just the sampled region.

> We regard that paper as, in a sense, being shared (in terms of co-authoring) with all the Chinese workers having published on the area.

> Read it as if written by 10 or 20 geologists. Because none dominate, it appears depersonalized."

Thus, their "withdrawal" from the text, while it has resulted in a somewhat unexpected and inhabitual "reverse" transgression of discoursal norms, causing them to be less loquacious that we expect they ought to be, can be explained by their need to "innovate", and to make up for the lacuna in their collaborative research experience. Instead, they grant priority and recognition to the accomplishments of other researchers rather than to their own.

The third paper, on the other hand, once again returns to a more affirmative and authorial implicating voice. While we would not necessarily call this text "authorially prominent", it is nonetheless exceedingly clear that the authors have been in the field here, especially in contrast to the preceding paper.

> "[1]Sampling areas are distributed (figure 1) roughly parallel to the structural limits of the northern topographical limit of the Tibet Plateau. [2]In the east, we sampled metamorphic rocks along the Kunlun fault south of Golmud over a distance of roughly 100 km. … [3]In the west, we sampled metamorphic rocks along the Karakax valley, over a distance of ca. 100 km following the south flank of the Kunlun mountains south of Yecheng. [4]A geological and geochronological description is given in Matte et al. [1996] and Mattern et al. [1996].
>
> [5]South of Naji Tal within eastern Kunlun (figure 2), a step-over between overlapping segments of the Kunlun fault created the impressive Xidatan-Dongdatan pull apart trough [Van der Woerd et al., 1998]. … [6]The granitic bodies just north of the fault appear to be largely of Triassic age [Harris et al., 1988, Mock et al., 1998]. [7]A belt of pegmatites, mylonites of granite and leucogranite, garnet schist phyllonites of various sedimentary origins, up to one kilometer wide, is intermittently exposed along the northern footwall of the Kunlun flank. … [8]A petrographic study of the ductile deformation of quartz and the recrystallization of muscovites suggest peak deformation temperatures of ca 350-400°C. [9]Such ductile fabrics are particularly well exposed to the west where the facets are well-developed and normal throw prominent.

> [10]The Karakax river follows the westernmost segment of the Altyn-Tagh fault about 80 km between Sanshili and Kanshiwar (figure 4). [11]The river is offset about this amount by the fault which continues westward to the Muztagh Ata Tagh and Kongur Shan [Brunel et al., 1994]. [12]The Karakax river then escapes towards the Tarim basin in a narrow gorge at approximately 78°E. [13]The active trace of the fault is particularly clear in this area with glacial and post-glacial terrace river offsets, seismic maul tracks and kilometer-long pull aparts and push-ups [Peltzer et al., 1996, Matte et al., 1996]. … [14]To the southwest of Kanshiwar, Triassic granites become progressively sheared toward the fault. [15]Such mylonitic granites, together with garnet-muscovite schists and leucogranitic lenses in the highly sheared zone along the corridor bear evidence of indicating syntectonic re-crystallization (micas) [Matte et al. 1996] …"

Most telling is the relative absence of citations to others' fieldwork as compared to the fair number of references to their own past field missions. Also, the authors refer to themselves overtly (s. 2-3), which, as we know, does not happen very often in the field account. Finally, and especially, the pointed and frequent use of evaluation-marking adverbs and adjectives (judgments and comparisons) points to a text that is field-situated, professional and affirmative. Here we have 4.7% of the text constituting of evaluatives, whereas the corpus norm is situated only around 1.2%.

s. 1-3: 'roughly', 'ca.'

s. 5: 'impressive'

s. 6: 'just [north]', 'largely'

s. 7: 'up to', 'intermittently'

s. 9: 'particularly', 'well', 'prominent'

s. 10-11: 'about'

s. 12: 'then',

s. 13: 'particularly clear'

s. 14-15: 'progressively', 'highly'

The reasons for the overt attention drawn to the authors' past field missions and expertise in the area are two-fold. First, in contrast to the earlier Kongur paper, it is not a "first paper" on the area and the authors had already published a good number of preliminary reports on the region which they wanted to draw attention to. But a second motivation, and a perhaps more important one, is, as Nick explained,

NA: 'USA authors tend to ignore French work and never cite us. So the tendency to somehow overcite ourselves has grown in the French "Tibetan" scientific community."[xxxviii]

And thus we have here an illustration of transgressional authorization as determined by the conditions of the social field. As we have seen, these conditions are the "context" which accompanies each text, and what may appear to be instances of silential transgressions on the part of the author are in fact regulated by the conditions of the social field. In this final article, the context of doing research and writing within the "French Tibetan geological community" pushed Nicolas and his co-authors to make themselves more overtly present in their text, to clearly stake their "national" territory, as researchers at odds with other non-francophone research groups. While without this context, an analyst might be tempted to take this positioning as solely motivated by personal interests, or as an instance of "free agency", we can thus observe that these transgressions are in fact generated and permitted by the institution in certain circumstances as a way of expressing and mediating contention — or even disregard — between members of a larger community, separated into smaller, interest-based communities. The instances of transgression discussed here would seem to illustrate yet another type of innovative and purposeful silence, namely, a set of *omissions of disregard*. By intentionally avoiding reference to the work of other research teams, one further bolsters one's own.

### 5.7.3 Gilles

Gilles has shared three articles with me, published over a period of four years. Of these three, it is revealingly the first, published on a chapter of his Ph.D. dissertation in 1993, where one gets the most sense of an authorial field presence. The remaining two articles, which we will examine first, are characterized by absence of any reporting of what was found in the field, where field results are instead minimally represented within 'Sampling Discourse' (see section 3.5), as shown in the following passages. The first excerpt comes from an article published in 1996.

> [1]Mantle xenoliths in Yemen have been collected in two different Quaternary volcanoes, Bir Ali on the southern coast, east of Aden, and Ataq in the Balhaf graben. [2]In the Bir Ali volcanic field, all the mantle xenoliths are anhydrous and only one (BA5) has been found to contain glass in small melt-pockets. [3]This sample is a spinel lherzolite with an equigranular, slightly foliated texture. [4]Some areas contain large (up to 0.5 cm) grains of olivine. [5]There are many small patches (< 1 mm) of clear glass disseminated in the rock. [6]They contain residual clinopyroxene and brown spinel as well as euhedral newly crystallized clinopyroxene and dark spinel. [7]Some of these small melt-pockets are connected by fine veins of glass along grain boundaries.
>
> [8]The Ataq diatreme contains many mantle xenoliths which have been previously studied by Varne (1970), Varne and Graham (1971) and Menzies and Murthy (1980).
>
> [9]Sample JK1 is an anhydrous spinel lherzolite similar to BA5, with a tabular equigranular texture and large variations in the grain size in some places. [10]Melt-pockets are disseminated in the rock and, as in BA5, contain clear glass surrounding residual clinopyroxene and spinel and containing newly formed clinopyroxene, spinel and in some cases olivine. [11]Here again, many of the melt-pockets are connected by small veins of glass along grain boundaries.
>
> [12]JK6 is a spinel lherzolite with a texture intermediate between porphyroclastic and mosaic equigranular. ...

As we can see here, Gilles' field mission has been reduced to what we might call a 'minimal minimum', with only the first two sentences making any distinctly overt

reference to the field. The remainder of the passage deals with the description of his samples, once back in the laboratory. As we can recall from section 3.5, the elements of 'Sampling Discourse' are typically (1) an indication of rock type, (2) a verbal indication of "sampling" activity, and (3) the general geographical location where the sample was found. The above account as brief reference to fieldwork thus falls squarely into a typical pattern of providing a basic 'site description'. In sentence 8, with the expertise-laden, professionally tagged term 'Ataq *diatreme*[xxxix]', there is also a minute description of the field, implied by the explicit choice of terminology which once again carries us back to the volcanic fields where we know Gilles has done his sampling. The accompanying published reference ('Menzies and Murthy (1980)') is to the fieldwork one of his coauthors did in the late 1970's, and with whom Gilles worked in London during his first post-doctoral position. The remainder of the text is a petrographical description of the samples' mineral characteristics. We can compare the scarcity of field details and authorial field presence here with the second 'field-bare' article, published in 1997, which follows.

> [1]Oceanic rifting in the Gulf of Aden began some 20 Ma ago and is associated with widespread volcanism represented by large strato-volcanoes (the Aden Volcanic Line, Cox et al., 1970) along the coast and by alkaline basaltic volcanic fields on the coast and inland. [2]The mantle xenoliths were sampled from two Plio-Quaternary volcanic fields on the south coast of Yemen: Bir Ali and Ataq. [3]At Bir Ali, the xenoliths are anhydrous spinel lherzolites and some samples show evidence of clinopyroxene, spinel, and glass. [4]In sample BA8, many spinels are surrounded by plagioclase, indicating upwelling of the upper mantle during rifting along the Gulf of Aden. [5]At Ataq, the xenoliths are amphibole-bearing spinel lherzolites or anhydrous spinel lherzolites and sample JK5 contains occasional melt-pockets, with residual spinel and clinopyroxene surrounded by glass and new olivine, clinopyroxene, and spinel. ...

Here, once again we can note the same tendencies. While the first sentence is a general statement of the Geological Setting, i.e., known geological processes of the region, we

can see the same Sampling Discourse elements present in sentence 2 as in the first text: rock or mineral type ('mantle xenoliths'), researcher activity ('were sampled' or 'samples'), and the geographical and geological location of the sampling sites ('two Plio-Quaternary volcanic fields on the south coast of Yemen: Bir Ali and Ataq'). However, this minimalist detail is in itself revealing, for rather than coming to this text as unknowledgeable outsiders, we can recall what Gilles revealingly had to say about sampling conditions in these very Quaternary volcanic fields during our interview ("ils sont coupants, chaotiques"). But as we can see, none of this intriguing 'story' even remotely appears in the published version.

One vital question to be asked at this point is whether Gilles' writing style might not in fact be typical of geochemistry in general. In many cases it is, given that the restricted field account or sites description characterized by 'Sampling Discourse' occurs most frequently in articles from the geochemistry corpus. However, the corpus also shows us that geochemists as field geologists might in fact show a good deal of textual field presence and perhaps even claims to territorial propriety, much more than one might expect given their close ties to the laboratory and typical distance from "the field", as we can see in the following brief examples of field reporting from geochemistry corpus.

**Sample 1.**

> "The Gardnos impact structure (at 60°40'N, 9°00'E) is located in the Hallingdal, a valley approximately 125 km northwest of Oslo, Norway, about 9 km north of the village of Nesbyen (Fig. 1). The rocks of the structure occupy a roughly circular area, about 5 km in diameter, on the west side of the valley....
>
> During and since the Pleistocene, the Gardnos area has been glaciated and deeply eroded. Present relief is high, with elevations ranging from about 200 m (above sea level) in the bottom of the Hallingdal to over 1000 m on the mountain plateau surrounding the structure. Block glacial moraine covers much of the area, but exposures are good enough in river beds and on steeper hillsides to permit fairly reliable mapping of the bedrock. Except for minor cultivated areas, the area is covered with dense

coniferous forest. The structure is located near the main road up the Hallingdal (Highway 7), and a network of forest roads and farm tracks provides easy access. ...

During field studies in the summer of 1990, two of the authors (J. A. Dons and J. Naterstad) identified a series of melt-bearing breccias overlying the Gardnos Breccia.....

Samples of Gardnos Breccia and melt-bearing breccias for this study were collected from excellent bedrock outcrops in the bed of the Dokkelvi River, a small tributary stream that drains the southwest side of the structure (Fig. 2). A few samples were collected from outcrops of fractured crystalline basement rocks in the central peak area. Further sampling was facilitated by a tunnel under the bed of the Dokkelvi River....” [from GCA-Fr]

**Sample 2.**

“Kornerupine-group minerals have been found in the southwestern Pamir Mountains near Kuhi-lal, Darai-Stazh and Mulroj, three localities for whiteschists 40–75 km south of Khoroy on the Tajik side of the Pyanj River (Grew et al., 1990b, 1994) and 105–130 km northeast of Sar-e-Sang, Afghanistan, Schreyer’s (1977) original whiteschist locality.

Sample KL604 (Table 2) is a silvery phlogopite schist collected from a lens of ultra-magnesian rocks 1–1.5 m thick and extending some 10 m along strike located near the village of Kuhi-lal...” [from CMP-Gr]

**Sample 3.**

“Systematic sampling of the basalt plains covered an area of over 11,500 km2. In some areas (e.g., adjacent to Melbourne and across the plains towards Geelong) sampling approximates a 5 x 5 km grid. Sampling in other areas is more dispersed in part for geological reasons; in the far west the oldest basalts are concealed beneath thick weathering profiles and in the south central region (Terang-Camperdown area; Fig. 1) the plains basalts are obscured by extensive tuff blankets in the vicinity of young cones and, particularly, the maars. Where possible, samples were taken from quarries or road cuttings, but the majority were broken from the freshest field boulders using sledge hammers or drill and feather wedges; a typical sample weighed 20 kg.” [from GCA-Pr]

As we can see in these three samples, there are quite a few more details about the field, the work done and its conditions than Gilles typically uses. We can note the

exactitude of the sampling location in terms of longitudinal–latitudinal readings (sample 1), precise geographical location by village naming, kilometric distances (samples 1, 2) or road directions (sample 1); detailed descriptions of the sampling site in terms of its features (sample 1) or its sheer size (sample 3). We can also find a number of comments about work conditions, be it ease of mapping (sample 1) or difficulty in sampling (sample 3). Some authors (sample 1) also inform us about how accessible the sampling site actually is, and others tell us about how they worked and how much they carried away (sample 3). We also have some extra-contextual personal comments ("During field studies in the summer of 1990" and "excellent" in sample 1). These accounts tend to be more textual as well, with an almost narration of research activity, as in 'Where possible, samples were taken from quarries or road cuttings, but the majority were broken from the freshest field boulders using sledge hammers or drill and feather wedges' (sample 3). And yet, among these possibilities, we find none of this in Gilles' writing.

However, it may very well be that it is the subject itself that constrains the amount of field description one may actually give. Gilles did explain that in his case, much of this field information is a mute point, given that he specializes in the study of xenolithic peridotites[xl], and it makes little or no difference where these particular minerals are picked up or by whom.

DD: Et du fait que tu n'as pas été échantillonner toi-même, ça change tes résultats?

GC: Non, pas, pas pour les péridotites. Lorsque les péridotites sont dans des enclaves, donc l'environnement dans lequel tu échantillonnes a finalement peu d'importance. Puisque ça peut être une coulée, ça peut être une cône, ça peut être... L'enclave vient d'en bas, donc qu'elle arrive au sommet d'une montagne dans un bassin, quelque soit l'environnement ça change rien à l'échantillon lui-même. ... Les enclaves de péridotites, même si on est dans le même endroit dans un volcan, donc on a une enclave ici et une enclave là, étaient peut-être séparées de deux ou trois kilomètres dans le manteau. Dans le manteau, c'est, c'est les laves qui remontent les péridotites dans le manteau, et les laves circulent dans des conduites, à

> travers le manteau, et elles peuvent arracher un morceau de manteau à un endroit, continuent à circuler et en arracher un autre trois kilomètres au-dessus, et monter à la surface. Et à la surface, les deux enclaves sont vraiment être côte à côte. Et tu n'a aucun moyen de savoir si dans le manteau elles étaient aussi à vingt centimètres l'une de l'autre ou à trois kilomètres. Donc euh, si c'est toi qui la ramasses ou c'est quelqu'un d'autre, ça n'a pas d'importance. C'est comme ramasser une météorite. Tu ramasses une météorite qu'elle soit tombée en France ou aux Etats-Unis, bon, là c'est encore pire parce que, que ce soit au Yémen ou en France ça a quand même des implications. Mais que tu la ramasses au sommet du Puy-de-Dôme ou à trois kilomètres, ça ne change rien. Donc, là c'est moins extrême mais c'est un petit peu ça. Donc l'importance du terrain n'est pas la même, pour les péridotites. ...
>
> Alors, ce qu'il faut savoir c'est d'où ça vient parce que, le manteau, ça vient du manteau lithosphérique, le manteau qui est sous la croûte, et le manteau peut être différent selon que tu es dans une zone d'extension, une zone de compression, suivant l'âge de la croûte qui est en surface, des choses comme ça, mais tu n'as pas besoin d'aller sur le terrain pour le savoir, ça, tu le sais en regardant des cartes géologiques, en regardant ce qui a été fait sur la géologie générale de la région, mais euh, aller sur le terrain pour échantillonner des péridotites c'est pas, c'est pas fondamental.

As Gilles further explained, a general geological context therefore largely suffices for situating the study of minerals such as these and he does not 'need' to give more detail about his field research. Indeed, in his mind, a detailed description of the field where a peridotite was picked up would be of no interest to journal editors, and would be rejected as 'unnecessary information' lacking any real scientific value.

And so, we can see the further imposition of institutionalized silential constraints on field reporting practices, here determined by the type of rock or mineral the geologist chooses to analyze. For some of these rocks and minerals, we simply need to know less about the field and quite typically a simple indication of where it was found will suffice. This is in fact what we find as the essence of 'Sampling Discourse'. The general tendency toward a minimalist field reporting, showing similar treatments of similar subjects, is confirmed by an analysis of the geochemistry corpus. In this corpus, there are seventeen articles (from a total of thirty-nine) that contain only Sampling Discourse as the textual

description of the field or of field presence. These articles include other minerals besides peridotites, for which a detailed field description is not as important for understanding the geochemical implications for Earth genesis (such as mylonites, gems, monazite, gases, barite, andesite, or glasses).

Five of these articles report, like Gilles, on the sampling of xenoliths. There are, in addition, two other articles on xenoliths from the corpus that give a shortish field account rather than simply Sampling Discourse — one is 394 words in length, the other is 349 words. This then makes seven articles out of thirty-nine which treat the same subject as Gilles, giving us some basis for comparison. Yet, of these seven, only three articles similarly give minimal information about the sampling site, as seen in the following excerpts that represent the only textual reference to the authors' fieldwork in the entire article:

**Sample 4.**

> "The samples investigated come from ultramafic xenoliths in a basanite from San Carlos, Arizona." [from CMP-Wi]

**Sample 5.**

> "More than 100 peridotite xenoliths were collected and 48 of them have been cut for thin sections." [from CMP-Xu]

**Sample 6.**

> "Seventy-nine eclogites were examined in hand sample and in polished thin sections from a collection that was obtained from the floors (open fields on which soft kimberlite was steamrolled, but xenoliths resisted crushing) of turn-of-the century mining and amassed between 1977 and 1991 (By SEH)." [from GCA-Py]

However, if it were purely the subject alone that constrained the amount of field description of xenoliths, then we would expect this to hold true for all other articles reporting on xenoliths and peridotites as well. And yet, we can see in the remaining four articles that the authors instead give quite a bit more detail about the field than does Gilles, although they are writing on the same research subject as he is.

**Sample 7.**

> "... Samples for this study are from Table Mountain, the northernmost of four massifs of the Bay of Islands Ophiolite. The geologic map of the Table Mountain massif is shown in Fig. 1 together with the studied sample locations. The lower-most mantle section (depth 5–6 km below the crust-mantle transition) is characterized by generally coarse grained peridotites which define a steep chemical gradient ranging from lherzolites (samples TM 1062, 922, 613) at the base of the ophiolite with a Cr# (100Cr/(Cr + Al) in spinel of <20 to harzburgites (sample TM 827; Suhr and Robinson, 1994). The lherzolites generally display a high strain overprint acquired during ophiolite detachment.
>
> The central mantle section (depth 2–5 km) is dominated by cpx-poor harzburgites (samples TM 1232, TM 1274). Stretching lineations indicate a strong ridge-parallel flow component. Sample TM 1141 comes from an area rich in dunitic pods and bands typically present at the boundary between the central and lowermost mantle section.
>
> The uppermost mantle section (depth 0–2 km), defined by a marked change in the high temperature stretching lineation from oblique to normal to the ridge, was interpreted on the basis of geological, microstructural, and mineral chemical data to contain a significant proportion of trapped melt of minerals formed from migrating melts... This is seen in the field as cpx-rich harzburgite (sample TM 1454), lherzolite (TM 599), or, in one observed case, several meters of plagioclase lherzolite (sample TM 1524)...." [from GCA-Ba]

**Sample 8.**

> "The Blow Me Down (BMD) massif is part of the 485 million year old Bay of Islands Ophiolite (BOIO), Newfoundland (Dunning and Krogh 1985, Fig. 1). It preserves a complete ophiolitic pseudo-stratigraphy ranging from mantle tectonite at the base to volcanic rock at the top. It is unique in that it contains a thick and — in the field — rather monotonous

> dunite sequence sandwiched between crustal gabbros and harzburgitic mantle rocks (Girardeau and Nicolas 1981). Measured perpendicular to the gabbro-dunite boundary, the thickness of the dunite sequence ranges from 3 km in the south to 1 km in the north (G. Suhr, unpublished, Fig. 2). … Flow lineations are more pronounced than foliations and trend towards the northwest. … In the BMD massif, a diapiric (vertical) flow component is not observed.
>
> The topmost harzburgite layer of this sequence is overlain with a sharp contact (cm-range) by completely opx-free dunite containing local spinel seams. No single relict of harzburgite or single opx grains (except within orthopyroxenite dykes) were observed in the dunite sequence...
>
> The only lithological features within the dunites visible in the field are: (1) bands enriched in spinel and/or Fe-Ni sulphides; (2) discontinuous orthopyroxenite dykes cross-cutting the foliation; (3) discontinuous wehrlite and plagioclase wehrlitic layers restricted to the upper third of the sequence. ...
>
> Above the dunites, a rapid transition into gabbro occurs, both being typically separated by 10–100 m of troctolitic rocks. The gabbros display predominantly magmatic fabrics; foliations in the gabbros are moderate, lineations are poor, layering is pronounced only in the lowermost sequence." [from CMP-Su]

**Sample 9.**

> "In addition to our existing collections, ... we have sampled four newly discovered mantle xenolith suites at Mt. Llangorse, Hirschfeld Creek and Dome Mt. in northern B.C. and West Dawson in Yukon (Fig. 1). ...
>
> The three bimodal xenolith suites are located near the B.C.–Yukon border, the Alligator Lake suite within the Coast Plutonic belt of the southern Yukon and the Mt. Llangorse and Hirschfeld Creek suites within the intermontaine belt of north British Columbia. Their location appears to be correlated with a P-wave slowness anomaly detected teleseismatically (Frederiksen A.W., Bostock, M.G., Van Decar, J.C., Cassidy, J., submitted to Tectonophysics) in the mantle beneath the southern Yukon (Fig. 3) ... Frederiksen argued that the mantle anomaly resulted from... and they proposed that..." [from CMP-Sh]

What we can overwhelmingly sense in these three passages is the sense of importance accorded by the authors to situating and describing their field data. It is not simply

supplemental information but constitutes part of the essence of their overall findings. This is shown not only in the sheer amount of traces of research activity and evaluative statements, but also in the number of citations to both published and unpublished work which implies some sort of urgency in getting the results 'out' (sample 9). We can further note, in passing, that although Xu-1998 (sample 5) was included as an instance of minimal field reporting, even he nonetheless indicated that he had collected "100 samples"... although in the end only 48 of them were actually used.

And so, we might surmise that Gilles could also perhaps give a bit more indication that he has been in the field, a few more details of this work, than he does. He could clearly talk about the field in a way allowed for by communally accepted, conventional terms, such as the weight of his samples — we do know that they weighed 25 kilograms! —, the number of samples he picked up, longitudinal and latitudinal measurements, nearby localities, ease of access, work conditions, or even more textual contextualization of "newly discovered" xenoliths. And yet, he very simply does not choose to give us any of this 'extra' information.

We might hypothesize that Gilles *might* also in fact be "permitted" (Bourdieu 1984) to develop more textual presence, along the lines adopted by the authors of the articles cited above. After all, the French team he went into Yemen with was the first and remains one of the few to do fieldwork in this region. Yet he does not take advantage of the possible opportunities to bolster his relative status and show that "he was there", sticking instead to the bare minimum required for situating his readers and preparing his analytical results. Gilles does, in essence, appear to have "over-silenced" his fieldwork mission, as it seems by personal choice rather than in response to perceived institutional standards. For as we have seen, and pains have been taken to demonstrate, the institutionalized discourses for reporting fieldwork results in the research article in fact *do* leave some space for the author in which to talk about the field, perhaps even allowing for some limited expression of propriety and pride. It is clearly a constrained and muted

discourse, but certainly not one in which the field endeavor has been silenced *entirely*. But, here it may simply be that Gilles does not like to 'show himself', and accordingly, that this silence upon silence may in fact reflect a personal, intimate choice to respect his own needs of discretion.

It is surprisingly in Gilles' first article, published in 1993, that we find the most complete account of a field mission. This, of course, is somewhat surprising given that we might by now expect to find the contrary to be true, namely, to have seen an increase in authorial marking strategies over time as the researcher gains in experience and status.

> "[1]We investigated several sectors previously described by *Moseley* [1969], in the southernmost part of the Yemen Trap Series (Figure 1). [2]The Oligo-Miocene magmatism in this region is characterized by thick olivine-basalt traps overlain by ignimbritic rhyolites, dipping gently 20° towards the SW. [3]In the Dhala area, the exposed thickness could approximate 3000 m, according to *Moseley* [1969]. [4]In the Alanad and Radfan Mountain area, numerous dykes were emplaced through the basement and sedimentary cover. [5]They have a prevailing N120-N140° E orientation but some of them are trending N-S or N70°E. [6]They consist of basalts, trachytes, rhyolites and peralkaline rocks (comendites and pantellerites). [7]In the Dhala area, some plutons are formed by gabbros and syenites and intrude both basement and traps."

The Field Account here more firmly situates the author in the field than Gilles' later two articles. Although it is relatively short (122 words) by typical geochemistry standards (the average length of the field report in geochemistry is 449 words), it is nevertheless marked by a number of clear and certain traces of field presence. We can see this in the more frequent indicators of professional expertise, with its use of a good number of geological nominals, descriptive qualifiers of the field, its features, and age, absent in later articles:

s. 2-3: The Oligo-Miocene magmatism.... characterized by… overlain by... dipping... thick olivine-basalt traps.... ignimbritic rhyolites... exposed'

s. 4-5: ‘numerous dykes... basement and sedimentary cover … emplaced… trending’

s. 6: ‘basalts, trachytes, rhyolites and peralkaline rocks (comendites and pantellerites)’

s. 7: ‘plutons... formed by… gabbros and syenites... basement and traps’

There are also a good number of traces of the researcher’s activity and presence in the field, as seen in the use of location naming, visual support, locational adverbs and prepositions, a series of measurements and directional indicators:

s. 1: ‘the Yemen Trap Series (Figure 1)… southernmost’

s. 2: ‘[magmatism] in this region ... towards… 20° … SW’

s. 3: ‘In the Dhala area...’

s. 4-5: ‘In the Alanad and Radfan Mountain areas... through… N120-N140° E... N-S or N70°E’

s. 7: ‘In the Dhala area, ...

Indeed, as the account of a field study that sampled and analyzed ‘volcanic’ rocks, rather than peridotites or xenoliths, more geological “context” of the fieldwork is necessary. However, tellingly perhaps of Gilles’ discreet personality, apart from one ‘overt’ reference to the researchers’ activity (‘We investigated...’ in s. 1), there are scant few other indications of the author’s *personal* implication in the field mission. To be sure, we can note the use of two evaluative adverbs (‘gently’ in s. 2 and ‘prevailing’ in s. 5), yet even these remain a conventional, thus commonly and frequently found, means for indicating interpretation. We therefore have no ‘personal’ implication on the part of the author, of the sort we may have found elsewhere along the lines of ‘anomalously’, ‘excellent’ or ‘superb’.

While the apparent effect of writing with the thesis advisor in other writings we have seen in this chapter and in Chapter 4, the supposed constraints of “community

voice" and the novice status of the young researcher has been to curtail expressions of authorial persona, the tendency here seems to be quite the opposite. We have come to expect that the junior researcher-author will stick more closely to perceived convention until his research experience has become more established, at which point the author may chose to begin to show more of himself. In this sense, Gilles' first text conforms quite nicely to the handed-down tradition and conventional framework for reporting only particular and relevant elements of the field mission, minimizing geologist participation except through specific traces of field presence. But in the end we might also wonder whether this early heed of convention may not have made "Gilles the author" appear more textually present than in his later texts, for any firm textual indications of field presence later on seem to simply disappear from his writing.

## 5.8 Chapter conclusion

The account of salient and silent features of geological field writing given thus far, as a site of interaction between the geologist's social field, his habitus, and his and his community's privately-motivated, need-driven innovations, has proven to be somewhat more complicated than originally expected. Accounting for these features entails looking not at the institution's conventions, nor the community's practices, nor even the individual's needs based on the situation, but rather, it necessitates an examination of all these various levels at once, thus empirically confirming both Askehave and Swales' (2000) and Bhatia's (2001) exhortation to complicate genre analysis, by fully examining what constitutes the "context" of a particular text's features — in sum, these features' "raison d'être". The extent to which, or indeed, even whether we ought to take heed of this multi-layered context in the search for general features of a genre will be discussed in the next and final chapter. However, we might provisionally take from the discussion thus far that a description of silent communicative behavior is

impossible without establishing the context for its occurrence, by looking not only at the transgressions of silence, or its concomitant salience's, but also at a description of agency, or the individual's privately or transiently motivated and contextually situated reasons for making the linguistic choices that he does.

## Notes to Chapter 5

[xxxiv] Indeed, Miller's own work frequently draws on Bakhtin's theoretical contributions.

[xxxv] The detailed references of these articles may be found in Appendix B, part II.

[xxxvi] As explained by Gilles, a mantle plume is technically "une remontée de matériel un peu plus chaud, à l'état solide, toujours, qui vient alors peut-etre de l'interface noyau manteau, à deux mille neuf cent kilomètres de profondeur, peut-être de la zone de transition à soixante-dix kilomètres, donc du matériel qui est un peu plus chaud donc un peu plus léger, qui remonte lentement à travers le manteau jusqu'à ce qu'il atteigne la base de la lithosphère terrestre. Sous la lithosphère il s'étale, et quand il a subi une décompression relativement importante, c'est qu'il est un peu chaud et il y a une fusion partielle, il crée de grandes quantités de magma. Ces grandes quantités de magma donnent ce type de volcanisme qu'on voit ici [au Yémen, au Djibouti, ou en Ethiopie]. ... C'est ce qu'on appelle des trappes."

[xxxvii] Very recent geological time, .01 – 37 million years ago.

[xxxviii] However, as one other influential and well-placed geologist has confided to me, such a statement seems more reflective of an imagined national paranoia than of reality, per se; Taponnier and Allègre, for example, as "French" researchers, are widely cited internationally; moreover, the process works both ways, and my informant has noted that French geologists will also omit to mention other research teams of a different national origin (Van der Voo, pers. comm., 2001).

[xxxix] Diatreme: A breccia-filled volcanic *pipe* that was formed by a gaseous explosion, perhaps linked to the brutal vaporization of phreatic waters at the point of contact with ascending lavas.

[xl] Xenolith: A foreign inclusion in an igneous rock.

Peridotite: A coarse-grained plutonic rock containing ferromagnesian minerals, composed chiefly of olivine, pyroxene or spinel, with or without other mafic minerals such as amphiboles or micas, and containing little or not feldspar. Their outcropping is varied: (1) in small basic intrusions, (2) at the base of thick, lense-shaped intrusions, (3) at the base of ophiolitic complexes, or (4) in tectonically deformed lherzolite massifs. It is

currently believed that the upper mantle, under the terrestrial crust, is essentially made up of peridotite or similar rocks which by partial fusion, segregation or migration, gives a variety of peridotites and basic magmas.

# CHAPTER 6

# CONCLUDING ISSUES : IMPLICATIONS FOR GENRE THEORY AND TEACHING GEOLOGISTS TO WRITE

## 6.1 Questions of methodology

Any approach for uncovering textual silences is unavoidably a multi-layered task. Like in Physics where scientists study such 'invisible' phenomena as the black hole or the quark, a linguist can still study textual silence despite being unable to 'see' it by studying the manifestation of its *effects*. And these effects are perceivable only through the complex communicative *context* of field writing.

As a result, this study has revolved around two primary points: there is first and foremost what is essentially a theoretical question, namely, whether it is possible to identify and explain textual silence. As Huckin (2002) has written on this same point, namely how exactly to identify these silences, he notes that establishing the context is crucial:

> "The key to solving this problem is, first, to recognize that a textual silence refers to something that the context allows or even invites yet is 'not there'; and second, to then *use* this context to identify the silences. Textual silences must, in some sense, be relevant to the surrounding context; otherwise, virtually anything unsaid would count as a 'textual silence' in virtually any text. Analyzing this context in sufficient detail should enable the analyst to determine what *could* have been said yet *wasn't*."

He then goes on to pertinently raise the question, "But what exactly *is* 'the surrounding context'?"

Similarly, this dissertation has, perhaps most importantly and above all, raised a number of methodological issues, for identifying textual silence necessarily involves devising the appropriate methodology to grasp the context that will reveal the unsaid. Indeed, the principal research question raised here has been *how* exactly to make the unsaid appear in geological field writing.

The general approach has worked at making these silences more visible by establishing the discourse's context, by contrasting what *is* there with what we know *could* be there, by comparing what is textually attested within a corpus of texts with what is formally possible (based on what we know about geologists' field practices), and by pitting this against the backdrop of what we know to be contextually appropriate, or relevant, to the discourse as determined by what geologists currently reveal about the needs and practices of their research community. It is in the interweaving of these three levels that the context for saliences and silences may be defined.

It is also by narrowing in on the nature of discoursal expertise in geology that this context emerges further, where an important component of the equation lies in the linguistic markers available to the researcher as a means of rhetorically constructing his field account, and in so doing, his credibility and competence. We also see this discoursal expertise in the ways in which a researcher knows to silence specific details of the fieldwork expedition, what is made of this tacit removal of information by geological insiders, and how both readers and writers know to use saliences and silences in order to construct their understanding.

In order to make the context of "being a geologist" more tangible, the methodological approach I have designed has been a combination of various approaches: a socio-historical review of the discipline and the analysis of the recontextualizations of a field account, a series of text-based interviews with authors and a corpus of 103 published research articles. The analysis of these different pieces has been undeniably

embedded within the concerns and possibilities for understanding complex language phenomena currently found in genre theory, but also in Bakhtin, Giddens, Bourdieu's notions of *habitus*, and relevant aspects of Activity Theory. These various approaches have been used in order to establish the discourse's context and in so doing, to demonstrate how specific instances of textual silence, both institutionalized as well as intentionally meaningful, may be identified in modern field writing.

The reasoning behind the choice of title for this dissertation, 'A *situated* analysis of textual silence and salience', is thus revealed, for the entire account has been geared toward just that: defining the "situations", or situated contexts, for modern field reporting in academic geology, by investigating its community of users' needs, their *habitus*, practices, attitudes, ideology, and history. In a nutshell, it has been an attempt to get at the "story" behind geology's field writing.

### 6.1.1 The stories told by geology and its practitioners

Over the course of the project, however, the original task of identifying and explaining silences has become somewhat marginalized, as I have gotten caught up in other related issues. As such, the topic of textual silence has become, in a sense, more of a window through which I have studied, and focused the study, of other important details. Agency, of course, has been one. Another, however, has been the depiction and portrayal of a particular scientific community about whom relatively little has been written. In this regard, I have worked hard to develop an truthful description of the geological community as something that has face validity, not only for the pleasure of painting a picture in which a geologist may recognize himself, but also to set an objective basis for better understanding why geologists write like they do. By focusing on the things geologists might or might not do, think, see or write about, and why, I have also provided some explanation for why geologists' texts look the way they do, along the same lines as

Bazerman (1988) has succeeded in doing for physicists, Myers (1990) has done for biologists, and Swales (1998) has done for botanists.

Admittedly, I have not proposed a neat typology of actual silences, of all the things geologists do or do *not* talk about, such as Huckin (1999; 2002) has succeeded in doing for newspaper articles on American Homelessness. In the end, this clearly delimited task appears to me to be less important than the very *means* I have used for identifying and explaning silences, the possibilities this presents for identifying silences in other discourse domains, and the resulting description of the practices of a complex community.

This study has therefore also been, in essence, an attempt to get a 'feel for the game' (Bourdieu 1984), and to understand what an experienced geologist-writer writing in his discipline knows, and knows to say differently, to hush or to silence. In other words, in order to establish the context which would permit us to identify textual silences in academic geology field writing, I have tried to descriptively portray some of what it might be like to see, think and write like a geologist.

Therefore, this dissertation has sought to identify geology's multiple "stories". We have seen, in a geologists' historical retrospect, the attitudes of their own disciplinary circle toward its epistemological center, 'the field', and how their relationship and attitudes toward it and those who study it have evolved over time in response to shifting methodological inquiry. We have looked at what geologists do in the field and what their working field conditions might be like.

Although I have purposely not spent much time developing one other part of the story of fieldwork, namely, the 'personal' details, since I have assumed — and observed — that its traces will occur only very rarely in a modern published text, thereby making their infrequent appearances mere 'aberrations' or deviances from the norm, the weight of this part of the story's testimony has nonetheless seemed to me to speak for itself (e.g.,

being held hostage or having to do fieldwork at gunpoint, being attacked by fleas or by wild animals, having the research team mutiny, having to face wild climactic variations, going without eating, to name just a few). I have therefore argued that the 'contingencies' related to doing fieldwork are not without pertinence for field geologists themselves within the limits of their own research circle, and that their experiences do most likely shape their lives, identities, and attitudes toward themselves and others like them, in very particular ways, in turn laying the ground for some part of the context for modern field writing.

We have also seen some of the story behind the text itself, by examining how writers in the past might have talked about the field and why they may have felt more 'free' in relating what are now-irrelevant details, in contrast to today's rock-centered discourse that aims to present the geological account as something taking place entirely outside of man's intervention. Concomitantly, it has also modestly been proposed that today's textual features of the field account appear to be the manifestation of geology's contradictory attitudes toward its historical research locus, where we have a field account that is required to establish credibility and field competence while being downplayed, de-emphasized, skipped over in reading, with its many details passed over in silence in the actual writing.

This has also been a story about the discipline's various practices, seen for example in how geologists describe having to go out into the field in order to learn to see like the community, as well as how to see differently. Within the frame of community ways of knowing, we have seen that the field expedition gets narrowed down to "explainable fact". The drawings, notes, and measures the field geologist writes in his field notebook gradually come together to form a coherent whole, but the interpretative model is constantly being reworked and reshaped to take account of what he has seen. This shaping is further guided by the need to find the "right arguments" to make his interpretation convincing, and thus even before beginning to write, the experienced writer

has already begun to throw out various details of lesser importance. The model or scenario will thus turn out to be but an idealization of nature, which is "too complex" to truly manipulate, yet the way in which the writer will eventually describe this bit of nature will in effect constrain it even further. In the end, given the need for presenting a comprehensible interpretation to his research community, the description of the field gets shifted from construct to construct. And finally, given the restrictive environment of the written fieldwork account in the research article, as determined by modern genre conventions, the telling of field results is cut back, essentially "silenced" as the writer tries to slip more of its details into tables, figures, schemas, or photos, rather than into linguistic text. Such is the process of distillation of the field research account.

We have also seen that field geology is a visual science with a conventionalized visual language, allowing geologists to give a report of many of the field details they cannot linguistically develop as fully in the text. This has been illustrated in particular by the story of Philippe's field study where what emerges throughout the many recontextualizations is what has been silenced along the way: the raw field data, variously shaped and recounted into acceptably distanced and balanced prose. These transformations are expected, given the conventions and demands of modern scientific practices, but what has been less expected is that such seemingly 'irrelevant' details, like the mounds of measurements resulting from days and weeks spent in the field or the exceptionality of an exposure, in fact persist in the research article through their 'silential' expression. Here the raw field data is found to be revealed in the various figures in the text, and the somewhat personal story of the field account can be found hiding behind a turn of phrase (e.g., Searle, Nicolas) or in the choice to include particular visuals (e.g., Philippe's block diagram).

And finally, by examining a series of articles written by three experienced authors, and by comparing their field writing styles to the norm established by the corpus of texts, we have seen that the very act of constructing each text is itself highly situated,

subject to structural changes over time, to individual research story contingencies as well as to the author's personal inclinations and choices. All of this results in various levels of salience and silence in the text.

However, I have admittedly recounted this story as an 'outsider' to geology, mostly out of necessity, for a geologist I am not. But also, while the possibility of seeing the world through a geologist's eyes is necessary to understanding it, to merely reproduce what geologists do and say would not offer us much of anything new to be learned, for geologists can themselves say and explain what they do much better than I. Instead, these stories have to be understood within the cognitive frame I myself have brought to them as a linguistic study, shaped by my own "creative understanding" (Bakhtin 1986) of "geological culture". As Bakhtin explains,

> "In the realm of culture, outsidedness is a most powerful factor in understanding… A meaning only reveals its depths once it has encountered and come into contact with another, foreign meaning: they engage in a kind of dialogue, which surmounts the closedness and one-sidedness of these particular meanings, these cultures. We raise new questions for a foreign culture, ones that it did not raise itself; we seek answers to our own questions in it; and the foreign culture responds to us by revealing to us its new aspects and new semantic depths. Without *one's own* questions one cannot creatively understand anything other or foreign… Such a dialogic encounter of two cultures [results in them being] mutually enriched" (1986, p. 7; original emphasis).

My own approach undeniably finds its underpinnings in contemporary genre theory (Miller 1984, Bazerman 1988, Swales 1990, Bhatia 1993, Freedman and Medway 1995, Berkenkotter and Huckin 1995), where genres of text are assumed to be manipulated and manipulatable according to the writer's socially-embedded rhetorical situation and strategic purposes; where the "real" unit of speech (or writing) is taken to be the intertextual utterance; where a particular genericized text is thus not a mere collection of static features or text-types, but instead exists and is reproduced given, on the one

hand, the dynamic possibilities it provides for writers' rhetorical actions, and on the other, the stability — at least, "for now" (Schryer 1994) — it provides to a discourse community over time (see also Merton 1968). Genre thus defines the forms in which information enters into and is shared by a circle of speakers (or writers), without which participants would not know where to look for information or what that information might mean (Geisler et al. 2001). I would also add to this description of genre two of the basic themes developed in this dissertation: genres are also shaped and give shape to textual silences, which are generated and regulated within the conventions of the genre; also, individual intentionality is basic to any account of genre.

## 6.2 Genre theory and issues of silence and intentionality

To claim that textual silences, such as those that have been described here, are both a structural and an intentionally meaningful part of modern scientific writing is therefore to argue that a complete knowledge of genre *also* includes knowledge of those things that are left unsaid, and that writers may manipulate the genre's silential structure in order to communicate new, unexpected, and 'non-conventional' meaning. Silences in one way, shape, form or another, are clearly intrinsic to any genre. Accounting for the unsaid is, as has been noted by Beebee (1994), as equal a part of genre analysis as is accounting for a genre of texts' overt features, for while a proficient genre user knows how to manipulate apparent rhetorical and linguistic conventions, what the text might say and what he or she might do with it, the successful user of a genre must also know what the text may *not* say and ultimately, what he or she may *not* do with it. And therefore, we cannot dissociate the salient and the silent, for their cohesive relationship is constructed by their ongoing interplay as the experienced writer moves in and out of salience and silence in the text, depending on his or her needs, situational contingencies, or place in the social structure.

This study has therefore focused on two of the overarching types of silence identified in Chapter 1 (section 1.2), one structural and institutionalized, the other meaningful and purposeful. Some might argue, of course, that most of the silences that occur in geological field writing are unintentional for conventionalized, as writers may simply be reproducing the conventions of their discourse, subjected as they are to the ideological and epistemological pressures that accompany writing in the disciplines. This ideology is internalized to such a degree that it becomes part of the unconscious, resulting in the unproblematic and unchallenged acceptance of a world view engendered by the writer's *habitus*, further maintained by his or her social field's *illusio* (Bourdieu 1984).

However, the very notion of 'theory' in genre studies, such as it has been developed since Miller's (1984) seminal paper on genre as social action, points also and perhaps most importantly to the inherent *intentionality* of any use of genre, in both its salient and silent aspects. This intentionality is reflected in the highly-situated decision-making going on behind the writer's choice of words and phrases, the crafting of his sentences, or the construction of his text's overall coherence, and ultimately, the non-negligible linguistic variation that resultantly manifests itself between different exemplars of a same genre, between different writers as well as for the same writer over time. As a consequence, even the most conventional and expected silences are also governed by some degree of authorial choice and intentionality. However, the very idea that an autonomous actor in the discourse may, at various levels, control his or her use of textual silences also poses a basic epistemological problem for genre theory, if we take discourse to be the product of the interplay between social forces, and a viable and working theory of genre to be founded on the search for general tendencies in language use.

And so, we are led to ask whether instances of apparent intentionality, then, are truly important for developing a cohesive and coherent theory of genre. Does an account of intentionality, driven by private, need-based strategies for reaching a goal, add

anything that is so essential that we may not do without it? Does it have anything truly valuable to teach us? As Swales has recently commented,

> "[Aberrations] offer something... maybe even something a little special, but they don't offer a central methodology for genre analysis. They would if our primary focus was on individual texts and individual authors, but, or so I would argue, our primary focus is on the normal, on convention and on standard expectation" (pers. comm., 2001)

What then, *should* the place we reserve to accounts of agency be in a theory of genre? Certainly, it is clear from recent conceptualizations of genre, using Bakhtin (1986, 1990) or Giddens (1984) that genres subsist and are instantiated only because of active human 'engagement' (Swales 1993; Bazerman 1994; Berkenkotter and Huckin 1995) and that therefore, the acclaimed "death-to-the-author" stance is, in reality, but a figment of the post-structural imagination.

It is equally clear, however, that a pedagogically useful description of a genre is that which draws attention to a particular community's socially-determined and conventionalized means for communicating information. Only by emphasizing the "normal" and by learning to talk like the plures can one become an accepted participant in the plures. A deep understanding of a discourse's stabilized and conventionalized structure is a prerequisite to "playing the game" (Wittgenstein 1958, Bourdieu 1984); moreover, knowledge of this structure is the basic condition without which later "performative transgressions" (Bourdieu 1984) of that structure are not possible (Bakhtin 1986). All understanding is constrained by borders, and the possibility for change or transgressing borders lies in knowing as fully as possible what those borders are, so that they may be substituted by and translated into different borders. Therefore, for many reasons, an emphasis on a genre's conventionalized norms is simply essential to any pedagogy which uses genre. But in the process, is an account of agency then condemned to be paid nothing but lip-service? Do the social forces that shape genres and the

normalizing features that characterize them forever outweigh whatever individual intentionality may bring to the account?

### 6.3 Genre theory's quest for an increasingly complex 'context'

A possible answer to this question lies in the current direction genre theory has been taking over the course of the past decade, where genre studies have become increasingly concerned with establishing an increasingly complex account of 'the context', which, through a typified generic form, necessarily links both readers and writers, stabilizes practice, and signals function and meaning through its complex relationship with human activities and social structures. As Freedman (1999) has recently commented, this contextualizing and complicating trend has resulted in the belief that is no longer possible to teach a genre unless one also knows its cultural, historical and ideological underpinnings.

Central to both this discussion, as well as genre analysts' search for a text's context, is the continuing influence of rhetorical theory on genre theory, by describing the complex rhetorical situations in which writers and readers coexist: the occasions that draw them together, the motives they bring, the tasks they are engaged in, the rules of engagement they operate within, and the communities they belong to. Rhetoric is taken to be both a mode of conflict within and between communities, as well as a means of managing conflict and building community (Gross and Williams 1997). Here, as Miller (1997) has argued, intentionality and agency are unavoidably part of the equation, as human communication and activity take place within a conflictual and tension-wrought plures of *individuals* (Miller 1993), where each individual seeks to defend his own self-interests.

The influence of rhetorical theory has also, despite claims to the contrary (Gaonkar 1997), caused analysts to de-emphasize the social isolation of the actor, by

identifying ‘audience’ or reader reaction as key to determining the appropriateness and success of the communicative act (Paul and Charney 1995; Paul et al., 2000). We can better conceptualize the bond between the reader and writer in terms of Bakhtin’s description of the discursive relationship of the self to others:

> “I live in a world of others’ words. And my entire life is an orientation in this world, a reaction to others’ words, beginning with my assimilation of them… and ending with assimilation of the wealth of human culture…. This and the immense, boundless world of others’ words constitute a primary fact of human consciousness and human life… The complex interrelations with the other’s word in all spheres of culture and activity fill all of man’s life” (1986, p. 143).

Indeed, research in linguistics, cognitive psychology, reading and rhetoric has shown that little meaning is literally on the page and that much meaning must be contributed by the reader through a process of common inference and understanding of convention. A communicative act is considered to be effective and intelligible because it provides pertinent information to readers in a form they find appropriate, and binds itself and its readers as part of a relevant community (Suchan and Dulek 1990). Thus, it is the individual and his or her intentionality that substantiate the genre, but this individual is also endlessly set against the other and what it has already said, and what it might say or do. It is posed against the plures where defining the ‘context’ for a genre of texts is crucial, for it is this shared context that allows both readers and writers to meaningfully and strategically use and shape genres to their specific needs.

From literacy studies, we have further learned that the world of others’ words shapes the complex of abilities and knowledge that enable individuals to function and contribute in specific situations. Thus, a writer today needs to know not only how to write a specific genre exemplar (using standard conventions of professional language) but also when to write it and under what circumstances. Learning to write successfully is a complex and lifelong process, where writing and reading skills continue to develop into adulthood through the interiorization of language tools and systems in various contexts

(Vygotsky and Luria 1994). Several studies have shown that an essential part of later writing success as an adult or as a professional hinges on understanding the rhetorical nature of written discourse as a complex process, by making it 'one's own', and that many experienced writers and readers come to see texts not just as content or information but as rhetorically based actions within specific contexts, deeply implicated within social structure and practice, cultural inclinations, and the ongoing negotiation of meaning (Herrington 1981; Berkenkotter, Huckin and Ackerman 1988; Haas 1994; Geisler 1994).

And finally, an explanation for part of genre's global context has also been sought through recent applications of activity theory, based on the work of Vygotsky and Leont'ev (see also Wertsch 1999), which adds to the definition of a genre's context by providing analytical tools for studying how texts function within human activity. Under the view of genre afforded by Activity Theory, genres of text are seen to encode the organization of social groupings and their activities. It is the activities that further reveal the forms and patterns of communication and work, the tools they use, their enabling beliefs and knowledge, and other aspects of their culture (Berkenkotter and Ravotas 1997; Prior 1997). Geisler et al. (2001) have contended, for example, that legislation and court decisions have consequences for the activities of police enforcement and incarceration, and that the same may be said for accounting texts which organizationally participate in the producing, processing, and distribution of food crops. Bazerman (1994, 1997) has further argued that texts may serve to organize activities, not only through direct regulation, as in the official rules of a sport or of the patent system, but also through the "affordances" of the text, as in the way the spreadsheet organizes accounting and corporate planning.

Activity theory is therefore useful for examining the text-mediated interaction of multiple participants organized through the patterned social relations of activity systems that vary according to the practices and cultures of social collectivities (Engeström 1987; Hutchins 1995; Russell 1997a, 1997b; Berkenkotter 2001; Artemeva and Freedman

2001). It is suggested here that the attraction of activity theory to genre and text analysts of late stems from the possibility for the researcher to study genre instantiation and systems of genres within a group of users brought together by shared activities. These activities can be concretely defined by the analyst, thereby allowing one to bypass the difficult concept of 'discourse community', which has been seen to be less manageable and less specifiable than one would have hoped (Swales 1993).

The dynamic social context may accordingly be defined and described less as a rigidly boundaried social entity — difficult to define — which, as Rudwick (1985, p. 418) argues, unrealistically sets up boundaries between the initiated and the uninitiated, rather than focusing on the natural consequence of the active interaction among a group of actors bound together by their shared process of "dialogized unity" (Bakhtin 1990). As Rudwick suggests, a "circle" of like-minded individuals, such as the modern geological "community", or we might also cite biologists, physicists, historians, or linguists, are bound together by a very human activity, perhaps one of the basic activities characterizing modern scientific and academic communities, that is, the attribution of competence recognition by one's peers — through text. Rudwick takes this activity (although he does not himself refer to it as an 'activity') as an explanation for how modern scientific and academic communities function, where acts of competence recognition and distribution would be two of the fundamentally binding and concentric values holding 'members of a community' together, and of which the Ph.D. represents at least a minimal level of competence (op. cit., 418).

Text today, in its large sense, is thus clearly studied as something that carries out a multitude of specific functions within systems of genre, the active production of which is further linked together through interdiscursive chains and recontextualization (Linell 1998), where the actor shapes and formulates his account in the quest for competence recognition, or once attributed, to maintain it or add to it. This is done within the conflictual and agonistic "plures of individuals" (Miller 1993) where each participant or

group of participants can be seen to play a role in the instantiation and continuation of generic tendencies, through the intentional realization of need-driven, local innovations (Cole and Engeström 1993).

## 6.4 The place of author intentionality in genre theory's need for context

However complex the description of genre may be becoming, what is notable from this discussion, in terms of trying to respond to the question of whether or how a description of intentionality should find its way into a theory of genre, is that an answer to whether or not we ought to account for intentionality is already there. For what is similar across the intellectual trends currently feeding the development of genre theory is the place which is ultimately accorded to the individual. While they clearly do not focus on the individual, they do provide a place where we may not only "see" an actor physically at work, but also appreciate how his or her activity supports the linguistic system through the ongoing rhetorical interplay structured by dialogic unity. However, while I would argue that an account of intentionality is already "in the theory", the challenge here has been to bring it to the forefront in a meaningful and useful way, by comparing language conventions with deviations from the norm, and by searching for the reasons behind these deviations.

While some (Swales 2000b) justifiably raise the concern that such epistemological assumptions, which view genre analysis and pedagogy as inextricably tied to an ever-increasingly complex analysis of its socio-historical underpinnings, its culture and ideology places genre pedagogy out of the reach of its teachers,

> "If cognition is too "situated" we are paralysed as [EAP] practitioners" (Swales, pers. comm. 2001)

it can be argued that if what we intend to teach as EAP and ESP teachers is a full account of language, thus necessarily including both its silences and its saliencies, then we must also include as fundamental a description of the genre's *whole* context, without which the

presence of and motivations for silences, for example, cannot be fully revealed. However, to uncover this context in all its relevant forms, we must unavoidably also investigate instances of agency, intentionality, or in Bhatia's (1997) words, its "private intentions". Not in as much as they have something truly relevent to reveal about the personal stories themselves, but rather what the personal stories may themselves reveal about genre use and structure. It is undeniable that the author is shaped by his childhood and professional habitus, and that working within a structure that allows for performative transgressions ultimately conditions the extent to which need-based innovations may impact the system. But examining these types of intentional realizations, caused by various research contingencies and 'personal' structural changes over time, reveals much about the inner workings of the disciplinary community.

This is reflected for example in the samples of Nicolas' writing, who had every reason to be forthright and explicit in his writing, but was not always, for above all, it depended on the particular rhetorical situation. Or Gilles, who also over time has been granted the right to become more generous in the amount of field details he might give, but chooses not to. Olivier has given us further insight into the manipulation of silential boundaries, most notably by breaking out of them, and by disregarding them to some degree. This constrasts with the silence (self) imposed on the inexperienced, on those needing to make their way into a new community and who do so by imitating its discourse (Berkenkotter and Huckin 1995; Schryer 2001). And so, we have the means for clarifying not only what experienced researchers make of silence, but also how they show that they are professionals, and how they establish their authority as experts.

And thus, investigating the contexts of the situational contingencies that lie behind every writing story reveals more about the centralizing mechanisms of distinct communities than we might expect; where for example, it becomes important to know to 'finesse' the account and manipulate the genre's rhetorical options when the research

hasn't gone quite as expected, or to take advantage of opportunities to promote one's own national community.

While it is not important to teach proficiency-acquiring writers *how* to talk about research-related contingencies, it is important for them to realize that writers do talk about these things, but in extremely discreet ways and in very specific situations. While the collectivity sets the conditions for performative transgressions, these conditions might be better described by investigating the *situated reasons* for these transgressions, in addition to examining the community's practices, ideology or mind-set. In this way, we might get to a deeper explanation of why things are as they are, allowing us to give our students a fuller context of the underlying motivation for the community's doings, and in so doing, helping them to understand and deconstruct the unspoken, tacit rules of the community into which they are entering.

## 6.5 Implications for the teaching of geological and other applied discourses in a French university setting

Today, aside from the humanities and social sciences, where French scholars have a reputation among their European colleagues for being an internationally isolated and largely hermetic group, French scientific scholars are overwhelmingly obligated to publish and communicate the results of their research in English, as reflected in the great down-surge in the number of French-language publications that took place during the 1970's and 1980's. While a handful of French-language publications remain available, the reach of these journals is local; they are considered to be among the lower ranked in terms of importance, and do not weigh out in the distribution of promotions or funding. In this we can note that the use of English as a research language in France has, like in many parts of the non-Anglophone world, become ineluctable.

It seems very clear to me, however, from what my particular informants have said about their own writing practices as francophone writers writing in English, that they do

know how to successfully manipulate their [English's] discourse's silential and salient boundaries and conventions in rhetorically masterful ways, despite being non-native speakers of English. They know, like Nicholas or Philippe, how to "play the game" and to implicitly draw attention to their own national research community by exploiting citation conventions, 'overciting' themselves in order to defend their community's findings in the face of intense competition from Anglo-Saxon research teams, which, according to informants, tend not to read French research and more importantly, cite it even less. Through self-overcitation in prestigious scientific journals, French geologists report believing that they are thus maintaining the vitality of the French research community by drawing the attention of the international community to it.

While Philippe appears to still be in the process of explicitly sorting through at least some of the boundaries, all of them, including Philippe, have the requisite knowledge of what details in the field account are inappropriate and unnecessary. Indeed, they keep their field descriptions down to an appropriate minimum of pertinent and usable information. Likewise, they also know which "peripheral" details it is *acceptable* to retain, thereby demonstrating an intimate knowledge of the affordances allowed by the institutionalized social structure in which they operate. In using details that clearly move beyond a mere terse description of their terrain, they manipulate silences and saliences in ways which *implicitly* give us some idea of both the researcher's story and his relationship to the collectivity.

We see, in the case of Olivier, that he has allowed his "authorial voice" to emerge over time, thereby surely leaving himself more exposed to the obligation of having to take responsibility for what he says. But as we can recall from Chapter 5 (section 5.7.1), Olivier has explained having effectively felt the need to bring himself to the forefront and to more explicitly identify himself as the doer of his own deeds:

> "C'est, c'est, euh, moi j'aime bien m'impliquer de plus en plus à vrai dire. Je me sens plus, euh, en fait c'est dans ce premier papier que je

> m'impliquerais moins, je fais beaucoup plus jeune aussi, hein, parce que pour s'impliquer il faut, sentir qu'on peut le faire tu vois … alors que maintenant en fait je, c'est effectivement peut-être le désir de montrer que, je m', c'est bien moi quoi, ce que je raconte c'est moi, un autre racontera autre chose. Je pense que c'est ca."

There is therefore some evidence that the possibility for taking possession of one's discourse and to say certain things normally regulated by a system of silences, such as *not* drawing explicit attention to oneself as a researcher, may be considered by junior researchers to not be an option available to the novice contributors to a new community.

This *perceived* obligation to accept the silences imposed by one's disciplinary culture is also what one might retain from the comments made by a researcher in linguistic anthropology (see section 1.2.2), who felt less inclined to give explicit details of her fieldwork so as to avoid drawing attention to her novice status (R. Simpson, pers. comm., 1999). Further evidence for the claim being made here comes from the observation of a relative increase in one's authorial discursive space over time, to be deduced not only from Olivier's comments, whose writing style has effectively shifted over time, but also from the writing of such well-known scholars as Noam Chomsky, who, as a recognized "authority" in linguistics, has definitely attracted the interest, both positive as well as negative, of many scholars and writers over the years. Ard (1983), for example, has noted that Chomsky's later writings, published when he had already achieved a considerable degree of fame, display a much higher use of first person pronouns than his early publications.

Authorial discursive space thus effectively appears to be a shifting space, where the conditions of adhering to conventional silences and saliences are most likely binding in terms of the *effects* their perceived existence has on the writing strategies of junior authors, as well as in the variations of authorial strategy we might see over time. I am insisting on the notion of "perceived" conventions here, because nothing in this study has led me to believe that conventions exist as concrete units, rigidly handed down from one generation to the next. Rather, I would suggest that they appear to act as cognitive units,

existing in the collective imagination of a community who acts in a way concordant with how it *thinks* it ought to be acting, according to the conditions of its collective *habitus*. One might imagine that newcomers to a discourse construct the community's perceived conventions on the basis of what they hear and see other, more established members of the community say or do. These perceived conventions would be futher maintained through the well-documented, regulating influence of reviewers and editors (Myer 1990, Burrough-Boenisch 1999), leading to the impression that these conventions are effectively 'binding'.

Thus, the conventions of a community's system of saliences and silences do not appear as deterministically binding as some readings of Bourdieu might lead us to believe. We have seen, for example, Nicolas' choice to shift his authorial space in the Field Account, depending on the contingencies of both his authorial situation (e.g., having big-name co-authors may allow a junior researcher to give more voice to his personal experiences, allowing him to talk about "snow-covered glaciers"), and of the particular *situation* of his research activity (e.g., not having been in the field at the same time as Edward Sobel made it impossible for them to cooperatively construct their Field Account, leading them to present an account where their own participation was placed far into the background).

As another example, we can also observe the case of Philippe, who insisted on retaining a questionable block-diagram in the publication, which, to the contrary of his dissertation advisors, he took to be an important element of his Field Account. In later discussions on the subject (January 2002), Philippe explained to me that he did not feel bound by his advisors' opinions, and considered himself competent enough on this point to override their objections. Instead, he opted to wait for a a more 'valid' response from the wider community, in this case, the editor and reviewers of the journal to which he submitted his paper, for the definitive evaluation of his choice to include the visual. And, as we know from this part of the story in Chapter 4, its presence was ultimately granted.

However, we might also surmise as to whether the reviewers allowed for its presence precisely because they did *not* have access to the background details of its construction, and thus, its depiction of a concealed personal story. Philippe's advisors' certainly did, and it may be because they considered it to be, above all, a contingent learning tool that they felt it did not have its place in "proper" scientific discourse. The answer to these questions, however, lies in a future study.

And finally, there is Gilles, who has also manipulated the saliences and silences that mark his private, authorial space, although as we have seen, he has done this in particular by choosing to withdraw from his text. In so doing, he has chosen *not* to exercise his legitimate claim to the possibilities provided by the set of strategic optional traces identified in Chapter 3, for more overtly constructing the basis for his field competence, authority and credibility.

And thus, through the investigation of how saliences and silences appear to operate at three different levels, i.e., the institutional, the community, and the individual, we have a better idea of how the system of silences identified in Chapter 1 effectively gets played out on a daily basis by its users. As we have seen, the interaction between the levels of language consolidation, instantiation and localized uses appears to be an on-going interpretation and negotiation of perceived convention by both the community and its partipants, where through the process of instantiating or transgressing a norm, an individual fully retains a private, authorial space in which he responds to his own, particular situated needs.

## 6.6 Issues of English language instruction in France today: Broad and narrow proficiency vs. range of expertise

Given the expertise and the command of conventions that allow my informants to participate in international debates in significant ways, as reflected in their numerous international publications and conference presentations, the issues raised by this study's

underlying concerns about francophone scientific writers publishing in English seems to me to be less an issue of native versus non-native speaker status, than it is a question of range of expertise in the discourse.

In a recent appraisal of the view EAP practitioners hold toward speaker identity, Swales (2001) pinpoints the crucial distinction to be made today as having moved away from the non-native/ native speaker dichotomy. Instead, the most important distinction made by researchers from EAP, genre, literacy, composition and rhetoric today hinges around the notion of *expertise*. As Swales has suggested, there is a more relevant distinction to be made between experienced (or "senior") researcher–scholars and less experienced ("junior") ones, between those who "know the academic ropes [in contrast to] those who are learning them", rather than passing judgment purely on the basis of relative language skills. However, as Swales very rightly goes on to say, the language variable conveniently alluded to by the native/non-native distinction has hardly gone away.

Accordingly, he proposes one other two-way distinction that overlaps and complicates the facile and somewhat oversimplificatory junior-senior continuum. On the one hand, there are what he calls "broadly proficient" English-language researchers, who either posses English as their first language, are essentially "academically" bilingual, or have acquired a full range of linguistic and rhetorical strategies and skills during interaction with English in their disciplinary circles. Further, they have knowledge of the relevant sets of genres necessary to their full participation in their disciplinary culture. While such 'non-native' speakers of English may still be identifiable through their various "accents", as experienced researchers, they will likely be more procedurally competent than native English-speaking junior researchers. On the other hand, Swales (2001) also identifies "narrowly proficient" English-language researchers, who for various reasons, are weaker in oral English comprehension and speaking or academic

writing. For this reason, they are typically identified as needing further EAP support when they undertake English-language tasks.

There is some reason to believe that the state of English proficiency among geologists in France, and likely other French scientists as well, generally falls somewhere in the middle of this complex continuum of language and genre expertise. Indeed, French political and social aspirations to speaking foreign languages have always maintained a complicated and contradictory relationship with language pedagogy in its application. Foreign language teaching has notoriety among the "non-language teachers" (i.e., the students) for being poor, rigid and unsuccessful at producing proficient foreign language speakers; where by all accounts, foreign language teaching further suffers from a lack of prestige in the school system in comparison with other disciplinary subjects. This evaluation appears to reflect a long-standing problem, and is further held across generations, as indicated to me both by a young researcher in experimental petrology in his middle 30's, as well as by an older practicing respiratory kinestherapist and amateur mycologist in his 50's (T. Hammouda, pers. comm., 2001; C. Verny, pers. comm., 2001).

While the objective quality of foreign language pedagogy in France is not a subject for debate in this final chapter, it can be noted that in the past, however, many older (and often Senior) researchers and scholars from France and other francophone countries have had a reputation for having expert disciplinary knowledge in French and reading knowledge in English, but only moderate to poor oral comprehension, academic writing or speaking skills in English, a vision of older French scientists that has been corroborated in the literature (e.g., Sionis 1995). Moreover, this lower-level of proficiency in English language skills appears to accompany an absence of genuine recognition among older researchers about the usefulness of gaining more proficiency in English, as futher reflected in their lack of motivation in becoming autonomous in their writing process, beyond acquiring "a few recipes and tips" (Sionis 1995, p. 100).

This has had for an effect to essentially cut these researchers off from international venues for publishing their research findings, unless they are financially well-endowed enough to hire translators. Such narrowly-proficient Senior researchers, while they are clearly becoming fewer and far between, can still be met in the various science departments I have visited over the past ten years, and pose particular problems in the countries of the Maghreb, especially Tunisia, where second-language education has traditionally been in French, leaving its scientific researchers without the appropriate English-language knowledge to publish in international journals (H. Hemissi, pers. comm., 1997). This situation is the contrary to that met in neighboring Algeria, which in the late 1980's and early 1990's adopted English as its primary second language, in replacement of French with its persisting 'colonial' overtones.

For younger generations, such as the geologists I have interviewed for this study and the various other young, French geologists I have been in contact with over the years, the situation is quite different, of course, in the sense that they have fully accepted the need to be able to publish in English from the very beginning of their research careers. Nonetheless, the work of such prominent French researchers as Birch-Bécaas (1997), Cooke and Birch-Bécaas (1999) or Sionis (1995, 1997), on writing processes among francophone researchers, points to the fact the even those researchers who fully accept the necessity to publish in English may still have difficulty in finding English-language venues for their work. Indeed, their submissions may be refused not so much because of lexical or syntactic errors, which can easily be checked by local editors, but because of failure to pay sufficient attention to English-language rhetorical strategies (see Birch-Bécaas 1997, Sionis 1995). Evidence for differences between French and English argumentative strategies has further been recently discussed by Bachschmidt (1999).

Moreover, the task facing today's francophone scientist appears to be even more complicated when one considers that French scholars abroad at times still leave the impression of being uncomfortable in the forum of academic and scientific English-

*speaking* contexts, and have been known on occasion, across different disciplines, to read their paper *in French*, despite the context of the conference being "international", and therefore, *in English* (R. Van der Voo, pers. comm., 2001; B.-L. Gunnarrsson, pers. comm., 2001; C. Räisänen, pers. comm., 2001).

The implications of these academic scholars' observations can only be highlighted in the context of important, recent work by Rowley-Jolivet (1998, 1999, 2001), which has emphasized the *specific* nature of oral conference presentation skills in English. What Rowley-Jolivet pertinently identifies as "a valuable professional skill" cannot be appropriately accounted for within the models currently used to prepare students for public speaking (e.g., research articles, ordinary conversational practice). As a consequence, she proposes classifying conference presentations as a specific *genre* and examining their linguistic features in order to provide students with more useful support (Rowley-Jolivet 2001, p. 40).

The key word that seems to be emerging from this discussion, perhaps unsurprisingly given my own research and epistemological interests, is *genre*. The emphasis placed by genre pedagogy on gaining access to the knowledge of the rhetorical strategies and linguistic features of the genre used in particular situations, finds all its significance in the following statement made by Birch-Bécaas,

> "These modifications to the rhetoric and scientific content cannot be carried out by the angliscist corrector but only by the author himself who knows his readers, the risks he is taking and only he can gauge how certain claims will be received" (1997, p. 407).

She then goes on to finish her article by arguing that every attempt should be made to increase the autonomy of francophone researchers in the writing of their articles. Certainly, genre pedagogy, such as it has been described by Geisler (1994), Freedman and Medway (1995), Belcher and Braine (1995), Johns (1997), Swales and Feak (1994, 2000), Swales and Lindemann (2001), or Swales and Luebs (2002, forthcoming),

emphasizes just that: making the features of a particular genre of text *more visible* to its users, and therefore, more manipulatable.

Although *genre* is a complex and abstract concept, characterized in widely diverse ways (cf. Bazerman, Berkenkotter and Huckin, Fairclough, Freedman, Johns, Miller, Swales), it is considered highly valuable by those working in English for Academic Purposes, especially in the possibilities it offers for working with graduate students, where teaching such academic genres as the term paper, research article, conference abstract, literature review, "Geological Setting", "Field Account", oral conference presentation, or dissertation, to name just a few, is much more manageable and significantly 'real' to the student than broader categories such as "Scientific English". They are also more situated in actual language use, and therefore immediately useful, than are the more artificial "compare-and-contrast" paragraphs often taught in preparation of using academic discourse.

However, despite the contributions that genre studies could undeniably make to the teaching of English for Specific and Academic Purposes in a university setting in France, by providing university students with sufficient preparation for obtaining, early on, the level of proficiency necessary to make them full actors in their international — and local — research communities, genre analysis is largely absent as a focus of pedagogical policy in France. This is true in terms of the research currently being carried out, apart from one recently published genre-based monograph on the applied English of economy and business management (Thompson and Pindi 2001), a recent special issue drawing together work on journalistic and political genres in a little known, regional university review (Grosse 2001; Lits 2001; Dubied 2001; Moirand 2001; Lorda 2001; Herman and Jufer 2001; Revaz 2001), or a handful of other anglicist researchers who have examined particular genres in the contexts of their teaching assignments.

Furthermore, we might add the observations made by various instructors in the geology department in Clermon-Ferrand where this study was conducted, who have noted

that students attending their classes seem under-prepared for the complex uses of English demanded of them in their degree programs. This becomes especially apparent to them as students move into the upper levels of their degree programs, where the various tasks of professional discourse (e.g., conference abstracts, conference talks, literature reviews or other tasks) they are required to learn for participation in the professional community, seem compromised by their difficulties in English (J. Bouloton, pers. comm., 1999; B. van Wyk de Vries, pers. comm., 2002). While it is not a part of this dissertation, much interesting work has been undertaken in the study and application of specialized languages in university settings in France, which more clearly describe the specific contexts of English use by students and researchers in French academic settings than has been done here..[xli]

Given the difficulties that seem to accompany the professionalization of university students in France, such as they have been outlined above, the practical implications of this dissertation are therefore real, especially in terms of what it may add to a language pedagogy based on genre, by utilizing genre's multiple situated contexts, such as they have been examined and discussed in Chapters 2-5, in order to make the strategic use of the genre more accessible to students, thereby making them more autonomous writers. Autonomy seems today to be one of the basic conditions allowing researchers to actively partipate and compete in their research communities, where quick turn-around time and multiple, yearly publications are key to success.

Such an approach only grows in potential importance in light of the growing and generalized under-preparation of university students, a trend that has been observed to various degrees not only in France, but in other European universities as well, where open-enrollment policies and various social crises played out in the school system have resulted in students arriving at the university without knowledge of the appropriate skills and tools they must possess to participate in wider communities. There is arguably, then, a real need for applying the implications of this study and the other work done in EAP

and genre analysis, not only in terms of their potential for the English-language instruction of graduate students moving toward the professions, but also for preparing students to write and to speak as undergraduates. It can therefore be hoped, with some reason, that French universities will take this into account in the coming years.

## Notes to Chapter 6

[xli] e.g., Banks 1999, 1998, 1995; Beaufrère-Bertheux 1997, Birch-Bécaas 1997, 1996, 1994; Bourguignon 1999, 1997; Brouzeng 1995, 1992; Bylinski et al. 1999; Carnet 1997; Cooke 1992; Cooke & Birch-Bécaas 1999; Cotte 1999; Crosnier 1997, 1994, 1993; Dechet 1992; Fade 1994; Faure 1999; Galonnier 1997; Gledhill 1997; Greenstein 2001, 1998; Guyon & Guyon 1996; Hindley 1992; Joncheray 1997; Khaldi 1995; Labrosse 1997; Lautour-Briggs 1997; Leonarduzzi 1998; Lerat 1997, 1995; Magnet 2001, 1999, 1994; Magnet & Carnet 1999; Martin 1998, 1996; Mathis 1997; Mémet 1992; Mullen 1998; Percebois 1996; Petit 1997, 1993; Planes 1996; Rapatel 1996; Régent 1994; Resche 1999, 1998a/b; Rowley-Jolivet 2002, 2001, 1999a/b; Salager-Meyer 1998; Sionis 1997a/b/c, 1995, 1994; Tassard 1996; Thomas 1998; Thompson & Pindi 2001; Trouillon 1997; Vidalenc 1998; Villez 1996, 1994. Growing interest in this pedagogical dilemma is futher reflected in the increasing number and activites of research-generating and support ESP and LSP associations in France (GERAS, GLAT, APLIUT, SAES).

Most of the citations above have been taken from the list of publications appearing on GERAS' website, in its yearly publication, *ASp: La revue du GERAS*. The site can be found at the following address: http://www.langues-vivantes.u-bordeaux2.fr/GERAS/Acceuil GERAS

# APPENDICES

# APPENDIX A

## Geologic Time Scale

(from Dalrymple 1991, p. 60)

| Eon | Era/subera | | Period/subperiod | | Epoch | Estimated age in Ma (Millions of years) |
|---|---|---|---|---|---|---|
| Phanero–zoic | Ceno–zoic | Quaternary | | | Holocene | .01 |
| | | | | | Pleistocene | 1.6 |
| | | Ter-tiary | Neogene | | Pliocene | 5.1 |
| | | | | | Miocene | 24 |
| | | | Paleogene | | Oligocene | 38 |
| | | | | | Eocene | 55 |
| | | | | | Paleocene | 65 |
| | Mesozoic | | Cretaceous | | | 144 |
| | | | Jurassic | | | 213 |
| | | | Triassic | | | 248 |
| | Paleozoic | | Permian | | | 286 |
| | | | Carboniferous | Pennsylvanian | | 320 |
| | | | | Mississippian | | 360 |
| | | | Devonian | | | 408 |
| | | | Silurian | | | 438 |
| | | | Ordovician | | | 505 |
| | | | Cambrian | | | 570 |
| Protero–zoic | Late | | | | | 900 |
| | Middle | | | | | 1,600 |
| | Early | | | | | 2,500 |
| Archean | Late | | | | | 3,000 |
| | Middle | | | | | 3,500 |
| | Early | | | | | 4,000 |
| Priscoan | | | | | | 4,550 |

# APPENDIX B

# GEOLOGY ARTICLES

## I. Geology articles cited:

Dublanqc-Laborde, M. 1912. Sur l'existence de blocs calcaires métamorphisés dans les tufs ponceaux anciens de la Montagne Pelée. *Comptes Rendus de l'Académie des Sciences*, 154: 824-826.

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Jiang, S.-Y., Palmer, M., Slack, J., Shaw, D. 1999. Boron isotope systematics of tourmaline formation in the Sullivan Pb-Zn-Ag deposit, British Columbia, Canada. *Chemical Geology*, 158: 131-144.

Michel-Lévy, M., Lévy, C., Lefranc, J.-P., Wiik, H. 1970. La meteorite de Tiberrhamine (Sahara algérien). *Bulletin de la Société française de Minéralogie et de Cristallographie*, 93: 114-119.

Searle, M., Crawford, M., Rex, A. 1992. Field relations, geochemistry, origin and emplacement of the Baltoro granite, Central Karakoram. *Transactions of the Royal Society of Edinburgh: Earth Sciences*, 83: 519-538.

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**Arnaud**, N., Tapponnier, P., Roger, F., Brunel, M., Schärer, U., Chen, W., Xu, Z. In press. Evidence for cretaceous shear along the western Kunlun and Altyn-Tagh fault, northern Tibet (China). To appear in *Journal of Geophysical Research.*

**Chazot**, G., Bertrand, H. 1993. Mantle sources and magma-continental crust interactions during early Red Sea-Aden rifting in Southern Yemen: elemental and Sr, Nd, Pb isotope evidence. *Journal of Geophysical Research*, 98: 1819-1835.

**Chazot**, G., Lowry, D., Menzies, M., Mattey, D. 1997. Oxygen isotopic composition of hydrous and anhydrous mantle peridotites. *Geochimica Cosmochimica Acta*, 61: 161-169.

**Chazot**, G., Menzies, M., Harte, B. 1996. Determination of partition coefficients between apatite, clinopyroxene, amphibole, and melt in natural spinel lherzolites from Yemen:

Implications for wet melting of the lithospheric mantle. *Geochimica Cosmochimica Acta*, 60: 423-437.

**Goncalves**, P., Nicollet, C. & Lardeaux, J. M., 2000. La Tectonique Néo-protérozoïque du Centre-Nord de Madagascar : interaction entre forces aux limites et forces de volume. Paper presented at 18eme Réunion des Sciences de la Terre, Paris, March 2000.

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**Merle,** O., Brun, J.-P. 1984. The curved translation path of the Parpailon nappe (French Alps). *Journal of Structural Geology*, 6: 711-719.

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**Merle**, O., Nickelsen, R., Davis, G., Gourlay, P. 1993. Relation of thin-skinned thrusting of Colorado Plateau strata in southwestern Utah to Cenozoic magmatism. *Geological Society of America Bulletin*, 105: 387-398.

Sobel, E. and N. **Arnaud** (1999). A possible lower Paleozoic suture in Eastern Kunlun,Altyn Tagh range,China. *Tectonics*, 18: 64-74.

## III. CORPUS

(articles containing a Field Account are indicated by the bold-faced first author's name)

### Geochemistry Corpus

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## Structural Geology Corpus

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# APPENDIX C

## Example of Field Account embedded within the Geological Setting

Text taken from **Geldmacher**, J., Haase, K., Devey, C., Garbe-Schönberg, C. 1998. The petrogenesis of Tertiary cone-sheets in Ardnamurchan, NW Scotland: petrological and geochemical constraints on crustal contamination and partial melting. *Contrib Mineral Petrol*, 131: 196-209.

### Geological setting

[1]The peninsula of Ardnamurchan is the most westerly point of the British mainland and belongs to the British Tertiary Volcanic Province (Fig. 1). [2]Intensive magmatism occurred in the region about 60 Ma ago (Wells and Mac Rae 1969; Mitchell and Reen 1973; Mussett et al. 1988) in connection with the opening of the North Atlantic ocean. [3]The Ardnamurchan igneous complex is one of a number of intrusive centres in this province and lies at the westernmost point of the peninsula. [4]The igneous rocks intrude into the Proterozoic metasediments of the Northern Highlands, the so-called Moine schists, and thin overlying Mesozoic sediments (Fig. 1). [5]Although the thickness of the Moine schist is unknown (several kilometres has been suggested; Morrison et al. 1985) it is assumed that the boundary between the Moine schist and the Lewisian gneiss beneath the region lies at relatively shallow levels. [6]Seismic data from central Scotland, 150 km east of Ardnamurchan, indicate that the transition between amphibolite- and granulite-facies rocks may lie at depths between 6 and 14 km (Bamford et al. 1977).

[7]The ring-shaped igneous intrusions in Ardnamurchan and neighboring complexes have been named ring-dykes by Richey et al. (1930) and were divided into three different centres with decreasing intrusion age (Fig. 1). [8]The central complex of Ardnamurchan was intruded by numerous basaltic cone-sheets forming the latest stage of magmatism apart from several northwest-striking dykes.

[1]The thickness of individual cone-sheets varies between 10 cm and several metres. [2]Individual cone-sheets can occur side by side and occasionally cross or unite to a thick sheet. [3]The cone sheets of Centre II can be grouped into an inner and an outer suite relative to a large gabbroic ring intrusion which cross-cuts the latter (Fig. 1). [4]The outer cone-sheets are inclined at angles of about 30° in the direction of the focal point and intruded into Proterozoic Moine schists, Jurassic sediments and Tertiary plateau lavas. [5]In contrast, the inner suite dips with angles of the order of 70° and cuts the igneous ring-dykes of Centre II. [6]Thus, the inner suite post-dates the ring-dykes and therefore both cone-sheets suites represent different ages; [7]an older suite with shallower inclination and a younger inner suite with a steeper dip. [8]Because the inner cone-sheets dip more steeply towards the common centre than the outer cone-sheets, it was suggested that all cone-sheets originate from a centre at one defined depth (Anderson 1936; Phillips 1974), possibly 2 to 3 km below the present land surface in Ardnamurchan (Richey et al. 1930). [9]There is no evidence in the field for either spiral or lateral emplacement of cone-sheets as has been assumed by several authors (Jeffreys 1936; Durrance 1967; Hills 1972).

[10]In the outer suite two composite cone-sheets contain both basic and acidic magmas side by side, with the basic rocks forming the rims and acidic rocks the core regions of each intrusion. [11]A large composite sheet at the eastern coast of Kilchoan Bay (Fig. 1) near Port na Luinge was selected for more detailed investigation in this study. [12]The intrusion has a 40 cm wide dolerite rim on both sides which corresponds in form, mineralogy, dip and elongation to a normal cone-sheet of Centre II. [13]Within this follows a ca. 50 cm wide intermediate transition zone chemically classifiable as an andesite. [14]The felsic core of the intrusion is anomalously thick, reaching more than 60 m at its widest part.

### Sampling and analytical techniques...

# APPENDIX D

## Results of trace analysis from Chapter 3

1. TABLE KEY

| Letter identifier | TRACE TYPES | OTHER INDICATIONS |
|---|---|---|
| a | Agential statements in the field | SUM = Total number of occurrences |
| b | Evaluative adjectives and adverbs | |
| c | Interpretive comments based on field observations | |
| e-d | Nominal and verbal indications of activity | % = Amount of Field Account dedicated to the trace |
| f | Metric, angle and direction measurements | |
| g | Locational adverbs and prepositions of research movement | |
| h | Metadiscoursal references to visual data | |
| i | Geographical location markers | |
| j | References to own prior field publications | |
| k | Nominal and adjectival descriptive qualifiers of the field | AVG = Average number of occurrences |
| l | Geological age | |
| m | Structural/tectonic qualifiers<br>Laboratory / Petrology qualifiers | |
| n | Verbal adjectives and participles of technical relationships | |
| o | References to other researchers' field publications | |
| W | Overall number of words in the Field Account | STDEV = Standard deviation measuring typical range of divergence from the norm |
| T | Overall number of traces | |
| I | Number of Overt authorial traces (Category I) | |
| II | Number of traces of research activity (Category II) | |
| III | Number of traces professional expertise (Category III) | |

## 2. GEOCHEMISTRY COMPOSITE

| | Chemical Geology | | | | | | | Contributions to Mineral. Petrol. | | | | | | Geochim. Cosmochim.Acta | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 | 13 | 14 | 15 | 16 | 17 | 18 |
| | Ke | Be | Ji | Ja | Mu | Cr | Ha | Ge | Ro | Su | Sc | Bl | Ba | Fr | Er | Br | Ba | Pr |
| a | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 3 | 0 | 0 |
| b | 31 | 31 | 23 | 31 | 9 | 4 | 20 | 4 | 0 | 4 | 5 | 4 | 2 | 137 | 47 | 3 | 17 | 25 |
| c | 2 | 2 | 2 | 7 | 4 | 5 | 5 | 4 | 5 | 3 | 2 | 3 | 2 | 19 | 12 | 4 | 2 | 1 |
| e-d | 17 | 6 | 2 | 6 | 4 | 4 | 9 | 6 | 2 | 6 | 4 | 5 | 4 | 60 | 22 | 12 | 29 | 13 |
| f | 13 | 9 | 10 | 10 | 11 | 4 | 13 | 8 | 5 | 7 | 13 | 9 | 2 | 45 | 27 | 14 | 5 | 15 |
| g | 27 | 22 | 27 | 21 | 16 | 17 | 14 | 11 | 14 | 13 | 10 | 5 | 1 | 83 | 25 | 18 | 12 | 10 |
| h | 3 | 7 | 2 | 6 | 4 | 0 | 3 | 2 | 4 | 1 | 0 | 3 | 1 | 10 | 4 | 1 | 1 | 2 |
| i | 26 | 24 | 9 | 22 | 5 | 4 | 15 | 5 | 6 | 2 | 7 | 0 | 5 | 64 | 26 | 7 | 1 | 11 |
| j | 0 | 5 | 10 | 7 | 1 | 0 | 3 | 0 | 3 | 1 | 3 | 0 | 2 | 19 | 12 | 1 | 5 | 1 |
| k | 182 | 238 | 193 | 189 | 150 | 97 | 92 | 109 | 76 | 83 | 88 | 60 | 28 | 555 | 278 | 96 | 104 | 89 |
| l | 6 | 11 | 5 | 11 | 21 | 6 | 4 | 6 | 11 | 5 | 11 | 21 | 6 | 29 | 15 | 1 | 2 | 16 |
| m | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| | 16 | 1 | 10 | 4 | 0 | 10 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 2 | 10 | 2 | 4 | 0 |
| n | 17 | 42 | 32 | 21 | 15 | 7 | 4 | 4 | 9 | 9 | 5 | 9 | 8 | 79 | 38 | 11 | 19 | 10 |
| o | 8 | 6 | 10 | 24 | 6 | 5 | 1 | 9 | 1 | 2 | 6 | 0 | 0 | 12 | 16 | 0 | 2 | 7 |
| W | 669 | 559 | 548 | 545 | 372 | 316 | 309 | 428 | 365 | 349 | 285 | 208 | 148 | 1979 | 1000 | 411 | 394 | 392 |
| T | 348 | 404 | 335 | 359 | 246 | 163 | 184 | 168 | 136 | 136 | 154 | 119 | 61 | 1115 | 532 | 173 | 203 | 200 |
| I | 33 | 33 | 25 | 38 | 13 | 9 | 25 | 8 | 5 | 7 | 7 | 7 | 4 | 157 | 59 | 10 | 19 | 26 |
| II | 86 | 73 | 60 | 72 | 41 | 29 | 57 | 32 | 34 | 30 | 37 | 22 | 15 | 281 | 116 | 53 | 53 | 52 |
| III | 229 | 298 | 250 | 249 | 192 | 125 | 102 | 128 | 97 | 99 | 110 | 90 | 42 | 677 | 175 | 63 | 72 | 78 |

| | SUM | % | AVG | STDEV |
|---|---|---|---|---|
| a | 4 | 0.00 | 0.22 | 0.73 |
| b | 397 | 0.04 | 22.06 | 31.74 |
| c | 84 | 0.01 | 4.67 | 4.39 |
| e-d | 211 | 0.02 | 11.72 | 14.09 |
| f | 220 | 0.02 | 12.22 | 9.88 |
| g | 346 | 0.04 | 19.22 | 17.43 |
| h | 54 | 0.01 | 3.00 | 2.59 |
| i | 239 | 0.03 | 13.28 | 15.32 |
| j | 73 | 0.01 | 4.06 | 5.10 |
| k | 2707 | 0.30 | 150.39 | 120.28 |
| l | 187 | 0.02 | 10.39 | 7.52 |
| m | 0 | 0.00 | 0.00 | 0.00 |
| | 60 | 0.01 | 3.33 | 4.84 |
| n | 339 | 0.04 | 18.83 | 18.85 |
| o | 115 | 0.01 | 6.39 | 6.32 |
| W | 9277 | - | 515.39 | 412.22 |
| T | 5036 | 0.55 | 279.78 | 240.29 |
| I | 485 | 0.05 | 26.94 | 35.62 |
| II | 1143 | 0.12 | 63.50 | 59.70 |
| III | 3076 | 0.33 | 170.89 | 146.48 |

3. PETROLOGY COMPOSITE

| | Mineralogical Magazine | | | | | | | Journal of Petrology | | | | | | | | Lithos | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 | 13 | 14 | 15 | 16 | 17 | 18 | 19 |
| | Aa | Ae | Ms | Gr | Sa | Na | Ky | Vd | Ha | So | Pr | Mi | Co | Iv | Ve | Xu | Ke | Pr | Ch |
| a | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 1 |
| b | 43 | 31 | 7 | 7 | 5 | 2 | 4 | 37 | 54 | 19 | 16 | 8 | 16 | 4 | 3 | 32 | 13 | 28 | 3 |
| c | 3 | 4 | 2 | 2 | 0 | 0 | 1 | 8 | 8 | 9 | 10 | 0 | 2 | 2 | 1 | 8 | 3 | 23 | 1 |
| d-e | 6 | 12 | 2 | 8 | 2 | 1 | 0 | 98 | 12 | 30 | 6 | 6 | 2 | 2 | 2 | 12 | 19 | 20 | 8 |
| f | 16 | 7 | 7 | 11 | 8 | 0 | 2 | 64 | 17 | 0 | 10 | 17 | 6 | 5 | 3 | 13 | 6 | 30 | 1 |
| g | 26 | 16 | 7 | 12 | 8 | 3 | 4 | 76 | 17 | 24 | 25 | 22 | 14 | 5 | 6 | 39 | 14 | 40 | 4 |
| h | 7 | 2 | 0 | 4 | 1 | 1 | 0 | 20 | 2 | 9 | 4 | 3 | 9 | 1 | 1 | 12 | 0 | 21 | 2 |
| i | 7 | 9 | 1 | 2 | 3 | 10 | 1 | 42 | 18 | 13 | 22 | 11 | 3 | 11 | 0 | 36 | 27 | 40 | 1 |
| j | 9 | 1 | 5 | 0 | 1 | 0 | 2 | 0 | 4 | 3 | 3 | 2 | 7 | 3 | 0 | 2 | 4 | 16 | 0 |
| k | 335 | 143 | 78 | 65 | 32 | 34 | 23 | 199 | 196 | 159 | 139 | 117 | 159 | 55 | 68 | 261 | 214 | 411 | 62 |
| l | 9 | 0 | 4 | 0 | 0 | 4 | 0 | 0 | 0 | 0 | 10 | 0 | 8 | 10 | 0 | 10 | 15 | 32 | 1 |
| m | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| | 9 | 0 | 12 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 5 | 1 | 0 | 17 | 18 | 1 | 5 |
| n | 65 | 13 | 7 | 6 | 8 | 3 | 4 | 59 | 66 | 17 | 18 | 8 | 28 | 10 | 9 | 28 | 24 | 51 | 14 |
| o | 1 | 1 | 3 | 1 | 3 | 1 | 0 | 12 | 8 | 1 | 14 | 1 | 4 | 6 | 0 | 6 | 18 | 15 | 0 |
| W | 957 | 362 | 322 | 199 | 135 | 103 | 83 | 1743 | 1373 | 693 | 625 | 483 | 460 | 270 | 196 | 924 | 719 | 1004 | 172 |
| T | 536 | 239 | 135 | 118 | 71 | 59 | 41 | 615 | 402 | 285 | 278 | 195 | 263 | 115 | 93 | 476 | 375 | 729 | 103 |
| I | 46 | 35 | 9 | 9 | 5 | 2 | 5 | 45 | 62 | 29 | 26 | 8 | 18 | 6 | 4 | 40 | 16 | 52 | 5 |
| II | 71 | 47 | 22 | 37 | 23 | 15 | 9 | 300 | 70 | 79 | 70 | 61 | 41 | 27 | 12 | 114 | 70 | 167 | 16 |
| III | 419 | 157 | 104 | 72 | 43 | 42 | 27 | 270 | 270 | 177 | 182 | 126 | 204 | 82 | 77 | 322 | 289 | 510 | 82 |

| | SUM | % | AVG | STDEV |
|---|---|---|---|---|
| a | 3 | 0.00 | 0.16 | 0.37 |
| b | 332 | 0.03 | 17.47 | 15.61 |
| c | 87 | 0.01 | 4.58 | 5.56 |
| d-e | 248 | 0.02 | 13.05 | 22.01 |
| f | 223 | 0.02 | 11.74 | 14.67 |
| g | 362 | 0.03 | 19.05 | 17.73 |
| h | 99 | 0.01 | 5.21 | 6.40 |
| i | 257 | 0.02 | 13.53 | 13.72 |
| j | 62 | 0.01 | 3.44 | 3.99 |
| k | 2750 | 0.25 | 144.74 | 106.35 |
| l | 103 | 0.01 | 5.42 | 8.06 |
| m | 0 | 0.00 | 0.00 | 0.00 |
| | 69 | 0.01 | 3.63 | 5.96 |
| n | 438 | 0.04 | 23.05 | 21.22 |
| o | 95 | 0.01 | 5.00 | 5.73 |
| W | 10823 | - | 569.63 | 460.64 |
| T | 5128 | 0.47 | 269.89 | 202.68 |
| I | 422 | 0.04 | 22.21 | 19.14 |
| II | 1251 | 0.12 | 65.84 | 69.09 |
| III | 3455 | 0.32 | 181.84 | 134.72 |

## 4. STRUCTURAL GEOLOGY COMPOSITE

| Tectonics | | | | | | | | | | | | Geodinamica Acta | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Li | Wu | Be | La | Ae | Br | Ba | Go | Pa | Ke | Lu | Ma | Ko | Ga | Sc | Le |
| a | 8 | 10 | 3 | 1 | 0 | 0 | 2 | 3 | 2 | 2 | 0 | 6 | 0 | 4 | 0 | 0 |
| b | 70 | 126 | 125 | 105 | 39 | 39 | 39 | 48 | 44 | 21 | 3 | 31 | 22 | 40 | 9 | 5 |
| c | 43 | 20 | 24 | 31 | 20 | 4 | 22 | 24 | 14 | 11 | 2 | 10 | 6 | 12 | 7 | 0 |
| d-e | 82 | 74 | 79 | 38 | 32 | 4 | 26 | 32 | 10 | 47 | 4 | 27 | 22 | 19 | 2 | 0 |
| f | 142 | 158 | 50 | 127 | 41 | 47 | 26 | 26 | 29 | 17 | 5 | 28 | 42 | 10 | 3 | 3 |
| g | 157 | 130 | 105 | 105 | 77 | 45 | 59 | 48 | 54 | 34 | 5 | 44 | 48 | 55 | 11 | 1 |
| h | 70 | 11 | 42 | 51 | 35 | 13 | 13 | 30 | 21 | 4 | 0 | 30 | 20 | 4 | 2 | 3 |
| i | 127 | 135 | 43 | 84 | 19 | 52 | 58 | 35 | 31 | 16 | 2 | 2 | 23 | 67 | 18 | 6 |
| j | 13 | 40 | 9 | 6 | 1 | 8 | 0 | 0 | 18 | 4 | 5 | 0 | 9 | 11 | 0 | 1 |
| k | 760 | 494 | 650 | 506 | 466 | 317 | 392 | 349 | 270 | 237 | 42 | 414 | 346 | 319 | 108 | 23 |
| l | 85 | 18 | 40 | 54 | 50 | 25 | 27 | 8 | 31 | 13 | 1 | 2 | 16 | 35 | 26 | 7 |
| m | 1 | 9 | 0 | 10 | 71 | 3 | 2 | 29 | 0 | 7 | 0 | 3 | 4 | 0 | 0 | 0 |
| | 8 | 2 | 17 | 13 | 2 | 9 | 1 | 3 | 0 | 7 | 0 | 1 | 24 | 0 | 0 | 0 |
| n | 100 | 119 | 87 | 88 | 80 | 69 | 38 | 57 | 43 | 13 | 4 | 52 | 93 | 53 | 16 | 3 |
| o | 27 | 0 | 14 | 10 | 31 | 41 | 9 | 8 | 17 | 9 | 3 | 6 | 5 | 8 | 16 | 1 |
| W | 3394 | 2810 | 2462 | 2326 | 1600 | 1463 | 1346 | 1300 | 1172 | 866 | 135 | 1221 | 1073 | 1251 | 437 | 286 |
| R | 1693 | 1346 | 1288 | 1229 | 964 | 676 | 714 | 700 | 584 | 442 | 76 | 656 | 680 | 637 | 218 | 53 |
| I | 121 | 156 | 152 | 137 | 59 | 43 | 63 | 75 | 60 | 34 | 5 | 47 | 28 | 56 | 16 | 5 |
| II | 591 | 548 | 328 | 411 | 205 | 169 | 182 | 171 | 163 | 122 | 21 | 131 | 164 | 166 | 36 | 14 |
| III | 981 | 642 | 808 | 681 | 700 | 464 | 469 | 454 | 361 | 286 | 50 | 478 | 488 | 415 | 166 | 34 |

| Journal of Structural Geology | | | | | | | | | | | | | Totals for Structural Geology | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Be | Fl | Ch | Ke | Ca | Jo | Do | Az | Pr | Su | Au | Du | SUM | % | AVG | STDEV |
| a | 1 | 3 | 2 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 1 | 49 | 0.00 | 1.75 | 2.56 |
| b | 84 | 110 | 71 | 79 | 25 | 40 | 27 | 21 | 23 | 31 | 6 | 13 | 1296 | 0.03 | 46.29 | 36.19 |
| c | 25 | 28 | 22 | 18 | 19 | 44 | 10 | 9 | 24 | 9 | 5 | 8 | 471 | 0.01 | 16.82 | 11.27 |
| d-e | 60 | 63 | 66 | 37 | 66 | 37 | 26 | 27 | 28 | 2 | 7 | 10 | 927 | 0.02 | 33.11 | 25.24 |
| f | 113 | 158 | 117 | 63 | 23 | 13 | 18 | 38 | 13 | 23 | 14 | 25 | 1372 | 0.03 | 49.00 | 49.05 |
| g | 253 | 141 | 134 | 98 | 71 | 46 | 32 | 50 | 51 | 32 | 16 | 19 | 1921 | 0.04 | 68.61 | 55.58 |
| h | 85 | 46 | 24 | 36 | 17 | 8 | 33 | 46 | 16 | 5 | 9 | 2 | 676 | 0.01 | 24.14 | 21.47 |
| i | 171 | 158 | 39 | 61 | 0 | 31 | 7 | 39 | 55 | 24 | 19 | 11 | 1333 | 0.03 | 47.61 | 47.19 |
| j | 22 | 8 | 2 | 10 | 6 | 5 | 5 | 7 | 18 | 1 | 2 | 0 | 211 | 0.00 | 7.54 | 8.70 |
| k | 1393 | 812 | 527 | 592 | 510 | 432 | 282 | 378 | 247 | 300 | 110 | 103 | 11379 | 0.24 | 406.39 | 277.27 |
| l | 16 | 19 | 4 | 19 | 0 | 18 | 2 | 27 | 18 | 9 | 8 | 11 | 589 | 0.01 | 21.04 | 18.83 |
| m | 15 | 2 | 3 | 26 | 0 | 2 | 3 | 32 | 33 | 0 | 0 | 0 | 255 | 0.01 | 9.11 | 15.95 |
| | 10 | 18 | 15 | 5 | 0 | 0 | 0 | 0 | 15 | 3 | 0 | 9 | 162 | 0.00 | 5.79 | 6.94 |
| n | 190 | 128 | 86 | 92 | 55 | 62 | 71 | 58 | 29 | 53 | 23 | 15 | 1777 | 0.04 | 63.46 | 41.74 |
| o | 19 | 27 | 8 | 15 | 10 | 27 | 32 | 10 | 4 | 10 | 4 | 7 | 378 | 0.01 | 13.50 | 10.51 |
| W | 4905 | 3547 | 2503 | 2216 | 2152 | 1736 | 1495 | 1428 | 1268 | 1032 | 1062 | 726 | 47212 | - | 1686.14 | 1057.03 |
| R | 2457 | 1721 | 1120 | 1151 | 802 | 765 | 548 | 742 | 574 | 503 | 223 | 234 | 22796 | 0.48 | 814.14 | 541.24 |
| I | 110 | 141 | 95 | 97 | 44 | 84 | 37 | 30 | 47 | 41 | 11 | 22 | 1816 | 0.04 | 64.86 | 45.28 |
| II | 704 | 574 | 382 | 305 | 183 | 140 | 121 | 207 | 181 | 87 | 67 | 67 | 6440 | 0.14 | 230.00 | 184.27 |
| III | 1643 | 1006 | 643 | 749 | 575 | 541 | 390 | 505 | 346 | 375 | 145 | 145 | 14540 | 0.31 | 519.29 | 331.83 |

## 5. COMPARISON OF RESULTS ACROSS DISCIPLINES

| | GEOCHEMISTRY | | | | PETROLOGY | | | | STRUCTURAL GEOLOGY | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Traces | SUM | % | AVG | STDEV | SUM | % | AVG | STDEV | SUM | % | AVG | STDEV |
| a | 4 | 0.00 | 0.22 | 0.73 | 3 | 0.00 | 0.16 | 0.37 | 49 | 0.00 | 1.75 | 2.56 |
| b | 397 | 0.04 | 22.06 | 31.74 | 332 | 0.03 | 17.47 | 15.61 | 1296 | 0.03 | 46.29 | 36.19 |
| c | 84 | 0.01 | 4.67 | 4.39 | 87 | 0.01 | 4.58 | 5.56 | 471 | 0.01 | 16.82 | 11.27 |
| e-d | 211 | 0.02 | 11.72 | 14.09 | 248 | 0.02 | 13.05 | 22.01 | 927 | 0.02 | 33.11 | 25.24 |
| f | 220 | 0.02 | 12.22 | 9.88 | 223 | 0.02 | 11.74 | 14.67 | 1372 | 0.03 | 49.00 | 49.05 |
| g | 346 | 0.04 | 19.22 | 17.43 | 362 | 0.03 | 19.05 | 17.73 | 1921 | 0.04 | 68.61 | 55.58 |
| h | 54 | 0.01 | 3.00 | 2.59 | 99 | 0.01 | 5.21 | 6.40 | 676 | 0.01 | 24.14 | 21.47 |
| i | 239 | 0.03 | 13.28 | 15.32 | 257 | 0.02 | 13.53 | 13.72 | 1333 | 0.03 | 47.61 | 47.19 |
| j | 73 | 0.01 | 4.06 | 5.10 | 62 | 0.01 | 3.44 | 3.99 | 211 | 0.00 | 7.54 | 8.70 |
| k | 2707 | 0.30 | 150.39 | 120.28 | 2750 | 0.25 | 144.74 | 106.35 | 11379 | 0.24 | 406.39 | 277.27 |
| l | 187 | 0.02 | 10.39 | 7.52 | 103 | 0.01 | 5.42 | 8.06 | 589 | 0.01 | 21.04 | 18.83 |
| m | 0 | 0.00 | 0.00 | 0.00 | 0 | 0.00 | 0.00 | 0.00 | 255 | 0.01 | 9.11 | 15.95 |
| | 60 | 0.01 | 3.33 | 4.84 | 69 | 0.01 | 3.63 | 5.96 | 162 | 0.00 | 5.79 | 6.94 |
| n | 339 | 0.04 | 18.83 | 18.85 | 438 | 0.04 | 23.05 | 21.22 | 1777 | 0.04 | 63.46 | 41.74 |
| o | 115 | 0.01 | 6.39 | 6.32 | 95 | 0.01 | 5.00 | 5.73 | 378 | 0.01 | 13.50 | 10.51 |
| W | 9277 | - | 515.39 | 412.22 | 10823 | - | 569.63 | 460.64 | 47212 | - | 1686.14 | 1057.03 |
| T | 5036 | 0.55 | 279.78 | 240.29 | 5128 | 0.47 | 269.89 | 202.68 | 22796 | 0.48 | 814.14 | 541.24 |
| I | 485 | 0.05 | 26.94 | 35.62 | 422 | 0.04 | 22.21 | 19.14 | 1816 | 0.04 | 64.86 | 45.28 |
| II | 1143 | 0.12 | 63.50 | 59.70 | 1251 | 0.12 | 65.84 | 69.09 | 6440 | 0.14 | 230.00 | 184.27 |
| III | 3076 | 0.33 | 170.89 | 146.48 | 3455 | 0.32 | 181.84 | 134.72 | 14540 | 0.31 | 519.29 | 331.83 |

## APPENDIX E

# Text from TECT-Wu

Wu, C., Nelson, K., Wortman, G., Samson, S., Yue, Y., Li, J., Kidd, W., Edwards, M. 1998. Yadong cross structure and South Tibetan Detachment in the east central Himalaya (89°-90°E). Tectonics, 17(1): 28-45.



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## 1. Introduction

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In this paper, we describe the STDS in the vicinity of 89° east longitude in the Himalaya, together with a related feature in the area called the Yadong cross structure (YCS). The bedrock geology of this area has not previously been described but potentially provides several new insights into the nature and along-strike variability of the STDS, the nature of along-strike discontinuities in the deep structure of the Himalayan orogen, and possibly the temporal relationship between granitic magmatism and slip on the STDS. …

## 2. Description of the Yadong Cross Structure

At approximately 89° east longitude, the STDS and topographic crest of the Himalaya are offset in a left-lateral sense by about 70 km, along a north-northeast trending discontinuity in the range termed the "Yadong cross structure" by Burchfiel et al. (1992). The YCS is the largest along-strike discontinuity in the bedrock geology and topography of the High Himalayas in the ~2500 km length of the range lying between the Himalayan syntaxes. …

The actual bedrock offset defining the YCS occurs across the southern part of the Yadong-Gulu rift, which is one of the more prominent of the northerly trending Neocene/Quaternary graben systems that extend from the Himalayas into the interior of the Tibetan Plateau (Figure 1). These rifts are the geomorphologic expression of ongoing east-west extension of the plateau [Molnar and Tapponnier, 1978; Armijo et al., 1986]. Burchfiel et al. [1992] recognized the YCS from the map pattern depicted on existing small-scale geologic maps of the central Himalayas [i.e., Gansser, 1983; Liu et al., 1988] and suggested that the strike separation across the YCS might be as much as 150 km. They identified and described the STDS in the field a short distance east of the YCS at Wagye La. However, because of border-access

restrictions in existence at the time, they were unable to examine the YCS directly in the field, nor were they able to determine directly from field observations where the STDS projects into the TCS from the west. Thus, while noting the regional significance of the YCS, they were unable to determine whether it was a strike-slip fault cutting the STDS, a transfer fault on the STDS, or some other structure or combination of structures. Similarly, the actual strike separation across the YCS could not be determined.

In 1992, 1994, and 1995, the International Deep Profiling of Tibet and the Himalaya (INDEPTH) project undertook geophysical investigations along the Yadong-Gulu rift, aimed principally at characterizing the deep structure of the crust beneath the region [Zhao et al., 1993; Nelson et al., 1996]. As part of this effort, reconnaissance field geological investigation of the bedrock adjacent to the southern Yadong-Gulu rift was undertaken in hope of locating and characterizing the STDS in the area and determining the nature of the YCS.

## 3. Southern Yadong-Gulu Rift

The southern Yadong-Gulu rift is composed of the Pali and Duoqen valleys, which together extend approximately 90 km in a north-northeast direction across the southern Tethyan Himalaya (Plate 1). The width of the valleys varies from a few kilometers to a maximum of about 20 km. Seismic profiling shows that Duoqen valley is an asymmetric half graben that deepens to the east and contains a maximum of about 1.5 km of Plio(?)-Quaternary clastic sediments [Cogen et al., this issue]. The subsurface geometry of the smaller Pali valley to the south is unknown, but the occurrence of bedrock cropping out in the middle of the valley suggests that it is quite shallow. Both valleys are bordered on the east by a rugged, anomalously north-northeast trending segment of the High Himalaya dominated by Mount Chomolhari (7313 m), referred to subsequently as the Chomolhari range. The Chomolhari range is the geomorphologic expression of the YCS. The western foot of the range, bordering Duoqen and Pali valleys, is marked by an en echelon set of active high-angle normal faults which, in aggregate, we refer to as the Chomolhari fault system (CFS) (Figure 2). The CFS is evidenced by conspicuous scarp cutting moraines, hanging glacial valleys, and triangular range-front facets [Armijo et al., 1986]. These features are evident both in the field and on thematic mapped images. West of Duoqen valley, generally east-west striking Paleozoic and Mesozoic sedimentary strata of the Tethyan belt are exposed. These strata are succeeded southward by a diverse assemblage of granite, granite gneiss, schist, phyllite, and locally marble, the bulk of which we assign to the Greater Himalayan belt. These strata are readily observed along the west side of Pali valley and along the two principal north-south roads through the region, which converge near the southern edge of the map area at Yadong (SW corner of Plate 1). Immediately east of Pali, fossiliferous Tethyan belt strata [Lin et al., 1989] are exposed in an enclave on the west slope of the Chomolhari range. Our field observations together with the regional mapping by Gansser (1983) suggest that the crest of the Chomolhari range is underlain by high-grade metamorphic rocks and granites of the Greater Himalayan belt. To the east in Bhutan, the Himalaya are similarly underlain by greater Himalayan belt strata, with local outliers of Tethyan belt strata preserved above [Gansser, 1983].

## 4. Zherger La Detachment

Our new field observations show that the contact between Tethyan belt sedimentary strata and crystalline rocks of the Greater Himalayan belt trends west-northwest immediately west of Duoqen valley and intersects the southern end of Duoqen valley at Zherger La (Figure 2). Zherger La is a small pass in a northeast trending basement ridge that protrudes into southern Duoquen valley. Mylonitic granitic augen gneiss is exposed immediately south of Zherger La. The rock appears to be a typical “type I” mylonite [Lister and Snoke, 1984]. Feldspar augen average 2-5 mm across, and the rock exhibits a well-developed S-C fabric and obvious mineral elongation lineation (Figure 2b). The mylonitic foliation dips moderately north-northeast (representative S surface dips 39° toward 020° and representative C surface dips 48° toward 020°), and the lineation similarly plunges moderately north (representative plunge 34° toward 005°). The shear sense indicated by the S-C fabric is consistently top-to-the-north. Unmetamorphosed gray quartz-biotite sandstone is exposed in the ridge immediately north of Zherger La. These strata strike east-northeast and dip moderately north. They pass northward and stratigraphically upward into grapy limestone containing abundant brachiopod fossils. The sandstones immediately above the contact have been assigned a Devonian age by Liu et al. [1988] and Xia et al. [1993].

At Zherger La, the actual contact between the mylonitic gneiss and sedimentary strata to the north is covered by a few hundred meter wide talus zone. However, it is well exposed approximately 5 km along

strike to the west-northwest, in a west-northwest trending glacial valley at the headwaters of the Chobogabo River (Figure 2a). This location is easily reached on foot from the western road running southward from Gala through Keshe and Ding'ga to Yadong (the road follows the Chobogabo River south to Yadong). The contact is exposed in the north wall of the valley and dips to the north. Mylonitic augen gneiss, identical to that cropping out south of Zherger La, comprises the footwall. The mylonitic foliation dips moderately north-northeast (representative S surface dips 20° toward 020° and representative C surface dips 30° toward 015°). Lineation defined by elongated quartz grains and quartz ribbons similarly plunges moderately north-northeast (representative plunge 25° toward 020°). The shear sense indicated by the S-C fabric is top-to-the-north. The mylonitic gneisses are cut by a number of spaced brittle normal faults that dip somewhat more steeply north than the mylonitic foliation, which they offset (representative dip 40° toward 355°). The footwall mylonites are overlain by yellow-weathering limestone, assigned a Devonian age by Liu et al. [1988]. These, in turn, pass upward into reddish-weathering siltstone that forms the bulk of the valley wall above the contact. The yellow limestone is brecciated immediately adjacent to the contact with the underlying mylonitic gneiss, and the contact itself is occupied by an apparently undeformed quartz vein roughly 3 m in thickness. Taken together, the field observations indicate that the contact between the Tethyan belt sedimentary strata and Greater Himalayan belt crystalline rocks west of Duoqen valley is a north dipping detachment fault. We locally term this structure the "Zherger La detachment" and argue below that it is the local expression of the STDS.

Granitic gneiss, granite, and injection complex (undifferentiated migmatite, augen gneiss, granite, and schist) are exposed for approximately 15 km southward from Zherger La. Moderately north dipping mylonitic fabric is well developed in the northern 5 km of this belt; southward, the foliation flattens and becomes weaker. A possible erosional outlier of Tethyan strata occurs within this belt immediately southeast of Ding'ga. At this locality, yellow-weathering foliated limestones cap a high ridge. These limestones are generally similar in appearance to known lower Paleozoic Tethyan limestones cropping out a short distance farther east (immediately east of Pali, discussed subsequently). The belt of granitic gneiss, granite, and injection complex is succeeded southward by an approximately 10-km-wide belt of green-schist to low-amphibolite-grade polydeformed metasedimentary phyllites and schists. The age and tectonic affinity of this phyllite/schist belt is uncertain. Unfossiliferous low-grade metasedimentary strata have been found at number of localities along the crest of the Himalaya near the structural top of the Greater Himalayan belt, the North Col Formation in the Everest region being a relatively well-studied example [Yin and Kuo, 1978; Lombardo et al., 1993]. Chinese workers have tended to assign these strata a Sinian-Cambrian age on the assumption that they stratigraphically underlie fossiliferous Tethyan strata and overlie the Greater Himalayan belt "basement". For the purposes of this paper, we include the phyllite/schist unit south of Ding'ga in the Greater Himalayan belt, noting that (1) the rocks comprising this unit are markedly higher grade than known Tethyan strata to the north, (2) they exhibit a polyphase deformation fabric unlike that of the known Tethyan strata to the north (and east), and (3) the boundaries of the unit are not associated with a structural or metamorphic break comparable to that observed at Zherger La. The phyllite/schist belt is succeeded southward by a mixed assemblage of granites, quartzofeldspathic gneiss, and schist extending southward beyond Yadong. A large apparently undeformed leucogranite body which we term the Gaowu granite occurs within this assemblage (Plate 1). The Gaowu granite intrudes Tethyan sedimentary strata cropping out south of Pali.

## 5. Comparison of Zherger La Detachment with previously described STDS localities

In their study defining the STDS, Burchfiel et al. [1992] examined the Tethyan belt/Greater Himalayan belt contact at six localities spaced along an approximately 700-km segment of the north slope of the Himalaya. They concluded that where not intruded by granite, the contact was a north dipping normal fault and stated the following:

1. The fault places Paleozoic or Mesozoic rocks onto Cambrian to Precambrian(?) footwall lithologies;
2. The hanging-wall lithologies are unmetamorphosed or contain greenschist facies mineralogy, whereas the footwall mineral assemblages are indicative of middle to upper amphibolite facies;

3. North-vergent S-C mylonitic fabrics are well developed in the footwall, their intensity becoming greater near the contact between the Greater Himalayan and Tibetan sedimentary sequences; and

4. The footwall shows evidence for the progressive development of north-vergent ductile to brittle extensional structures [Burchfiel et al, 1992, p. 36].

The Zherger La detachment lies within the 700-km-long segment of the Greater Himalayan belt/Tethyan belt contact spot examined by Burchfiel et al. [1992] and exhibits each of these characteristics. We conclude that it is the local expression of the STDS immediately west of Duoqen valley. The strike separation of the STDS across Duoqen valley and therefore across the YCS is about 70 km.

In their descriptions of the STDS, Burchfiel et al. [1992] noted that there appears to be a west-to-east change in the slip direction recorded in the STDS footwall mylonites in the vicinity of the YCS. In the four localities they examined west of the YCS, the prominent STDS lineation trends northeasterly (Gyirong, Nyalam, Everest and Dinggye areas). In contrast, in two localities they examined to the east it trends northwesterly. As described above, the prominent lineation in the footwall mylonites at Zherger La trends north northeast, consistent with Burchfiel et al.'s western localities. We similarly examined the mylonitic STDS footwall at several localities at the south end of Nieru valley, just east of the YCS (including Wagye La), and confirm their observation that the prominent lineation there plunges to the northwest (representative plunge 14° toward 330°). We are presently unsure whether the northwesterly lineation trend evident in the vicinity of Wagye La reflects an actual along-strike change in the slip direction of the STDS or subsequent local rotation of the basement exposed in the Wagye La area along northeast trending splays of the Chomolhari fault system (suggested by Landsat and digital topography images). In either case, the combined observations at Zherger La and Wagye La constrain the along-strike change in lineation azimuth noted by Burchfiel et al. [1992] to occur across the YCS.

Finally, Burchfiel et al. [1992] also noted brittle north dipping normal faults cutting earlier STDS mylonites at several of the localities they examined (e.g., Everest and Lhozag-La Kang areas). More recently, Edwards et al. [1996] have described a multistage evolution for the STDS in the vicinity of Khula Kangri, which includes the development of an early top-to-the-north ductile shear zone ("Gonto La detachment"), which is cut by a steep north dipping brittle normal fault ("Dzong Chu fault"). INDEPTH reflection profiling across the projection of the Zherger La detachment beneath Duoqen valley similarly suggest that the detachment there is cut of reactivate by a younger (Plio-Pleistocene?) north dipping normal fault, which carries a small half graben in its hanging wall [Hauck et al., 1995; Hauck, 1997]. As noted by Searle [1986], the abrupt topographic break that occurs at a number of locations along the north flank of the Himalaya suggests that down-to-the-north normal slip has locally occurred along this boundary in Recent time. Taken together, these observations suggest that down-to-the-north normal slip has continued episodically along the north flank of the Himalaya since the inception of the STDS in middle Miocene time. We return to these observations subsequently.

## 6. Field observations along the YCS

As described above, the YCS is defined by the strike separation of the STDS, which occurs along the Chomolhari range. We examined the bedrock outcrop at several localities along the west slope of the Chomolhari range in the hope of characterizing this structure. The macroscale structure underlying the west slope of the range appears to be a north-northeast striking, west dipping monocline. In general, both Tethyan sedimentary strata and Greater Himalayan belt gneisses and schists cropping out along the west slope of the range are strongly deformed, with both lithologic layering and prominent foliation striking parallel to the range and dipping steeply west to northwest (Plate 1 and Figure 3a). This is in marked contrast to the generally east-west regional strike and weakly deformed (outcrop-scale) character of the Tethyan belt strata cropping out west and north of the range.

The west slope of the Chomolhari range east of Pali us underlain largely by Paleozoic limestones. These are locally fossiliferous and thus clearly of Tethyan affinity [Lin et al., 1989]. These strata were examined along several east-west side valleys leading into the range. In the southern part of the outcrop belt (SE of Pali), the limestones are little deformed, and bedding strikes east-northeast and dips gently to moderately north. Toward the north, the limestones become increasingly deformed. In the northern part of the outcrop belt, they exhibit a strongly developed foliation, and in most outcrops primary lithologic layering appears to be transposed parallel to the foliation. The foliation strikes north to northeast and dips

moderately steeply to the northwest (strike and dip varies from 355°, 45°W to 050°, 65°NW). Foliation surfaces exhibit a strongly developed downdip lineation, parallel to the hinge lines of intrafolial folds in the transposed lithologic layering. The deformation fabric in these strata is clearly suggestive of dip-slip shear (west-northwest azimuth). No evidence for strike-parallel slip (N to NE azimuth) was observed in these rocks.

Along the east-west side valley that leads to Qukalongla La (pass to Bhutan), the limestones can be seen in scattered outcrop to pass structurally downward (west) into pelitic schists and then granitic augen gneiss. The pelitic schists exhibit the same moderately steep northwest dipping foliation and downdip lineation as the overlying limestones. Notably, lineation in the underlying augen gneiss is subhorizontal and northwest-southeast trending (representative plunge 5° toward 125°).

North of the Pali area, the west slope of the Chomolhari range is underlain by Greater Himalayan belt gneisses, schists, and granites. Immediately east of Tang La feldspathic gneisses are exposed in a side valley leading up the southwest slope of Chomolhari. At the entrance of the side valley, gneissic layering strikes north-south and is vertical. The gneisses are riddled by little-deformed, centimeter-to-decimeter thick quartz veins. Eastward up the gully, the orientation of the gneissic layering changes to northeast striking and steeply northwest dipping (representative strike and dip 050°, 60°NW), and an approximately 50-m-wide, undeformed leucogranite body can be seen cutting the gneiss. Farter east, where moraine deposits dam the gully, the gneisses are cut by an approximately 200-m-wide shear zone. The gneisses within the shear zone are intensely fractured and overprinted by a strong northeast striking, steep northwest dipping foliation. Lineation associated with the foliation plunges essentially downdip (representative plunge 50° toward 305°), and abundant boudinaged quartz veins and S-C fabric observations indicate normal-sense (NW side down) shear within the zone (Figure 3b).

Immediately north of Chomolhari, the Chomolhari range bends to the east for a short distance and then continues its north-northeast trend. In the area of the bend, pelitic schists and phyllites exposed at the foot of the range locally strike east-west (attitude of foliation). To the north, glacial/alluvial cover extends high on the west flank of the range, and we were unable to examine bedrock directly. Reference to thematic mapper imagery, however, shows that the bedrock cropping out high on the western slope of the range along this segment strikes north-northeast parallel to the range. Examination of the float at the foot of the range indicates that these are Greater Himalayan belt strata.

We summarize the available geologic constraints on the nature of the YCS as follows: (1) Generally east-west striking Tethyan belt strata are exposed west of the Chomolhari range (west of Duoqen valley). In contrast, structurally underlying Greater Himalayan belt strata are exposed within and over o wide area east of the Chomolhari range at the same and higher elevation [Gansser, 1983]. (2) Tethyan belt and Greater Himalayan belt strata exposed along the western slope of the Chomolhari range are rotated into a regional north-northeast striking, west-northwest dipping monocline. (3) Tethyan strata within the monocline have been ductily deformed in dip-slip shear (west-northwest azimuth), and both Tethyan strata and underlying Greater Himalayan belt strata within the monocline have been cut by brittle west-northwest to north-west dipping normal faults (Chomolhari fault system, shear zone observed in gneisses beneath Chomolhari). (4) No evidence for transcurrent slip was observed along the Chomolhari range and (5) Our mapping shows that the generally east-west striking Tethyan strata exposed north of the Chomolhari range are not offset across the northern extrapolation of the YCS (Plate 1).

## APPENDIX F

# P. Goncalves. 'Late Neoproterozoic strain pattern in the Andriamena unit'

## 1. Introduction

In collision zones, the knowledge of the timing of the structural evolution is fundamental for understanding orogenic processes. Thus, the direct coupling of geochronological data with structural and petrologic information is essential to unravel the evolution of a huge orogenic belt like the Mozambique Belt. Before the opening of the Mozambique channel, during Mesozoic times, Madagascar was located adjacent to Kenya and Tanzania, in the eastern front of the Mozambique belt which results from the continental collision of East and West Gondwana (Shackleton, 1986; Stern, 1994). Nevertheless, in view of recent geochronological and geological works, the exact position of Madagascar before this amalgamation (~800 Ma) is still uncertain. Indeed, in central Madagascar, the SQC unit, composed by Proterozoic metasediments (Moine, 1974), have been interpreted as the eastern passive margin of the West Gondwana (Cox et al., 1998), while the NE Madagascar, characterized by the unreworked Archaean granitoids of the Antongil block (Caen-Vachette, 1979; Tucker et al., 1999) was linked with the East Gondwana and more particularly with the Dharwar craton (SW India) (Tucker et al., 1999). This controversy associated with the uncertainties about the precise duration and timing of the continental collision in Madagascar (Shackleton, 1996; Lardeaux et al., 1999; Martelat et al., 2000) clearly shows that the orogenic evolution of the Mozambique belt, and therefore Madagascar, is still poorly understood.

The aim of this paper is to constrain the structural evolution of a part of the north-central Madagascar where numerous geochronological studies have been performed in the last few years (Guérrot et al., 1993; Nicollet et al., 1997; Paquette et Nédélec, 1998; Tucker et al., 1999; Kröner et al., 2000; Goncalves et al., 2000). Nevertheless, almost no modern structural studies have been done, except in the area of the stratoid granites, west of Andriamena (Nédélec et al., 1994) and in the Antananarivo virgation area (Nédélec et al., 2000). Combining the structural data with the P-T metamorphic estimates and in-situ geochronological dating, we discuss the thermo-tectonic evolution of a portion of the north-central Malagasy basement.

## 2. Geological setting

The Malagasy basement is classically divided into two parts. The southern part, south of the Bongolova-Ranotsara shear zone (Fig. 1) is characterized by a generalized late Pan-African tectonothermal imprint with no record of Archaean ages (Andriamarofahatra et al., 1990; Paquette et al., 1994; Kröner et al., 1996; Montel et al., 1996; Nicollet et al., 1997; Martelat et al., 2000). The finite strain pattern results in the superposition of two Pan-African deformation events D1 and D2, which are respectively characterized by a flat lying foliation S1 bearing an east-west lineation L1 and by a network of kilometric vertical shear zones (S2) bounding folded domains (Fig. 1). The D2 structures are clearly related to a late Pan-African east-west horizontal shortening in a transpressive regime under granulite facies conditions (see discussions in Pili et al., 1997; Martelat et al., 1997, 2000).

Since the 1970's and the studies of Besairie (geological maps on scale 1/100000; Besairie, 1963) two main lithological units have been recognized in the north-central Madagascar: a basement mainly composed by late Archaean (~2,5 Ga) granitoids and migmatitic gneisses (Tucker et al., 1999 ; Kröner et al., 2000) (the Antananarivo block from Collins et al., 2000), which is structurally overlain by a late Archaean mafic sequence. This latter occur as three north-south elongated units, respectively from west to east : Maevatanana, Andriamena, and Aloatra-Beforona (Fig. 1). They are interpreted as a part of the same lithostratigraphic unit : the "Beforona group" of Besairie (1963) or the same tectonic unit : the "Tsaratanana thrust sheet" of Collins et al. (2000). Our study is focussed on the Andriamena mafic unit and the surrounding gneissic-granitic basement.

Geochronological results show that the late Archaean north-central Madagascar basement records a complex Neoproterozoic polymetamorphic and magmatic history (Guérrot et al., 1993; Nicollet et al., 1997; Paquette et Nédélec, 1998; Tucker et al., 1999; Kröner et al., 2000, Goncalves et al., 2000). After a large period of cratonic stabilisation during about 1.7 Ga, intrusive gabbroic and granitoid rocks were emplaced between ~820-720 Ma into the late Archaean basement (Guérrot et al., 1993; Tucker et al., 1999; Kröner et al., 2000). This widespread Neoproterozoic igneous activity, which also affect the central Madagascar and more particularly the SQC unit (see location in Fig. 1), is interpreted as the result of a continental arc magmatism related to the closure of the Mozambique ocean during the break-up of the supercontinent Rodinia (Tucker et al., 1997; Handke et al.,1999). North-west of Antananarivo, the late Archaean gneissic basement was intruded under LP-HT conditions by the "stratoid granites" at 630 Ma (Paquette et Nédélec, 1998). Finally, the finite strain pattern observed in the north-central Madagascar is related to a late Pan-African tectonic event (Kröner et al., 2000; Nédélec et al., 2000), contemporaneous with a period of high-grade metamorphism and intrusive igneous activity (580-500 Ma) (Tucker et al., 1999; Kröner et al., 2000).

## 2.1 The Andriamena unit

The Andriamena unit located north of Antananarivo (see location in Fig. 2), mainly consists of interlayered mafic and quartzofeldspathic biotite gneisses, metapelitic migmatites (grt-sil bearing rocks) and quartzites associated with plenty large deformed mafic to ultramafic bodies. They include dunites, peridotites, pyroxenites associated with chromite and gabbros requilibrated under PT conditions of about 4-5 kbar, 500-800°C and preserving locally igneous textures (Cocherie et al., 1991; Guérrot et al., 1993).

The few available geochronological data allowed to point out the occurrence of a high grade polymetamorphic evolution of the Andriamena unit since late Archaean to late Pan-African times (Guérrot et al., 1993; Nicollet et al., 1997; Goncalves et al., 2000). Relictual high Al-Mg granulites preserve Ultra-High Temperature assemblages (grt-spr-qz, opx-sill-qz), suggesting minimal PT conditions of about 1050°C for 11 kbar, which have been dated at about 2,5 Ga using electron microprobe dating of monazites (Nicollet, 1990; Nicollet et al., 1997; Goncalves et al., 2000). A second widespread granulitic event coeval with partial melting occurring at peak conditions of about 850°C, 7 kbar have been dated at about 790 Ma (Nicollet et al., 1997; Goncalves et al., 2000). This second granulitic event could be the consequence of a thermal perturbation caused by the emplacement of the mafic-ultramafic intrusions at 787 ± 16 Ma (Guérrot et al., 1993; Goncalves et al., 2000). This mafic magmatism associated with granulitic metamorphism are in good agreement with a continental magmatic arc setting as proposed by Handke et al. (1999) in the west-central Madagascar during middle Neoproterozoic times. Finally, the Andriamena unit and all Madagascar were reworked during late Pan-African times.

## 2.2 The gneissic-granitic basement

The basement in the north-central Madagascar, and more particularly west and south-west of the Andriamena unit, is mainly composed by an alternation, with variable thicknesses, of biotite-hornblende-rich gneisses locally associated with sillimanite-bearing metapelites and stratoid granites (sheet-like granites). These granites have been interpreted as syn-tectonic granites emplaced at 630 Ma in an extensional setting (Nédélec et al., 1994; Paquette et Nédélec, 1998). It is noteworthy that the tectonic setting associated with the emplacement of this particular granites is still unclear (Nédélec et al., 2000). Indeed, this 630 Ma event has been recognized only in the stratoid granites located west of the Andriamena unit and in one intrusive hornblende-granodiorite gneiss, located east of Antananarivo (637 ± 1 Ma) (Tucker et al., 1999). Near Antananarivo, about 20 kilometres south-east of a stratoid granite dated by Paquette et Nédélec (1998) at 627 Ma (Fig. 2), Kröner et al. (2000), obtained an age of about 720-800 Ma from "charnockites" and biotite gneisses, which are equivalent respectively to the biotite-hornblende-rich gneisses and the stratoid granites.

## 3. Strain pattern and related structures

### 3.1 Method

The finite strain pattern was derived from the study of satellite images (7 SPOT scenes-tableau 1), completed with the analysis of the geological maps (scale 1/100000) and field investigations. Satellite image analyses have been successfully used in the southern Madagascar to deduce the crustal-scale finite strain pattern (Martelat et al., 1995; Martelat et al., 1997; Martelat et al., 2000). These previous works proved that the structural trends mapped on satellite images correspond to the foliation planes measured in the field.

The map of foliation trajectories (Fig. 2) outlines a clear predominance of N160 to N180 directions, and more particularly in the Andriamena unit where these directions are accentuated by the north-south elongate shape of the Andriamena unit and the mafic-ultramafic bodies in the northern part of the Andriamena unit. These directions are consistent with the general orientation of the main late Pan-African structures observed at the scale of Madagascar (Fig. 1). In the gneissic-granitic basement, the trajectories of the regional foliation are more irregular and define complex folded and elliptical structures. South of the study area, near Antananarivo, the foliation trajectories form a complex pattern that includes the Antananarivo virgation which correspond to an east-west trending structure, the north-south Angavo shear zone and highly folded domains close to the Carion granite or to the north of Mahitsy (Fig. 2).

This study is mainly focussed in the northern part of the Andriamena unit where two different domains have been defined with respect to their lithological and structural characteristics: The Andriamena unit (Fig. 3a and 3b) and the gneissic-granitic basement, which is composed by the Andriba area in the western margin (Fig. 7) and the Ambakireny area in the eastern part (Fig. 8).

### 3.2The Andriamena unit

The foliation in the Andriamena unit is a transposed composite plane mainly composed by the parallelism of mafic, quartzofeldspathic gneisses and mafic-ultramafic bodies. At the regional scale, the foliation plane, denoted as S1, is dominantly oriented N160-N180 (Fig. 3a) and define a kilometre-scale synform, with a north-south axial trace (Fig. 4).

The S1 foliation is folded on various scales by post-schistosity folds F2 with a steeply dipping north-south axial plane and subhorizontal axe (Fig. 3b-stereo a, c, d and Fig. 4), coherent with an east-west horizontal shortening (D2). The D2 deformation is heterogeneous and shows a strain partitioning between large low strain zones (zones in light grey in Fig. 3a and Fig. 3b) limited by an anastomozed network of high strain zones globally oriented N160-N180 with a width until 10 km (zones in dark grey in Fig. 3a and Fig. 3b). In the low strain zones, the S1 foliation as the mafic-ultramafic intrusions are gently folded by F2 kilometric open folds, without any related axial plane foliation (Fig. 3a-b and Fig. 4). Locally, some leucosomes can underline the F2 axial planes. In the high strain zones, the foliation is sub-vertical (Fig. 3a-stereo d, e) and can be interpreted as the transposition of the previous S1 foliation into a new penetrative north-south vertical S2 foliation or as the verticalization of the S1 related to the upright F2 folding. Mafic-ultramafic intrusions located in these zones are characterized by high aspect ratios ($10<H/L<40$) consistent with a strong tectonic transposition in this zones (Fig. 3a).

In the low strain zones, where the D2 strain is moderate, the L1 stretching lineation, marked by biotite or amphibole, define a regular east-west trending, perpendicular to the Andriamena/basement contact, with a pitch around 90° and a variable plunging due to the F2 folding (Fig. 3b-stereo a, b, c). In the high strain zones, where the S1 foliation is verticalized, the L1 lineations are steeply plunging due to their passive rotation during the F2 folding (Fig. 3b-stereo d). Near Brieville, where a transposition of S1 into a new S2 occur, the L1 lineation seems to be replaced by a new L2 subhorizontal lineation broadly oriented N170 (Fig. 3b-stereo e).

Structures related to the D1 deformation can be observed more easily outside the high strain zones D2. At the outcrop scale, we observe numerous isoclinal intrafolial folds with a hinge parallel to the L1 lineation and a sub-horizontal axial plane (Fig. 3b-stereo a, b, c and Fig. 5). The initially horizontal S1 foliation is also affected by boudinage structures compatible with the E-W stretching lineation direction (Fig. 5). All these structures suggest that the D1 event implies significant amount of vertical shortening. The D2 high strain zones are characterized by numerous upright F2 folds, which can locally interfere with the previous F1 isoclinal folds. The lack of asymmetrical structures in these zones characterized by an intense transposition, as shown by the very high aspect ratio of the mafic-ultramafic bodies, is consistent

with a strong component of coaxial strain associated with a horizontal east-west shortening during the D2 event.

## 3.3 The gneissic-granitic basement

### 3.3.1 The Andriba area : kilometric fold interference pattern

In the Andriba area, west of the Andriamena area, the foliation in the basement is defined by the alternation, at various scales, of the gneisses with the parallel stratoid granites. Close to Andriba and Kiangara, the structural pattern is characterized by a constant and west dipping foliation bearing a subhorizontal WSW trending lineation (Fig. 3a-3b) (Nédélec et al., 1994). The foliation and its mineral lineation have been interpreted as magmatic structures developed during the emplacement of the magma under amphibolitic facies conditions (4-5 kbar; ~750°C) at 630 Ma (Paquette et Nédélec, 1998).

Bounded at the east by the Andriamena unit and at the west by the monoclinal stratoid granites, a highly complex folded domain oriented north-south of 15 km in width, is observed from satellite images (Fig. 3a and Fig. 6). The foliation trajectories define kilometric “boomerang” structures (Fig. 6) typical of type II fold-interference pattern (Ramsay, 1967). It results from the superposition of a first fold generation with an axial trace oriented N90 and a second oriented N180-N150 (Fig. 6). These later open folds, which affect the stratoid granites, are characterized by north-south axial planes and sub-horizontal axis, consistent with the F2 folding event defined in the Andriamena unit. The earlier folds, which have not been observed in the field, are probably kilometric isoclinal folds with a gently dipping axial planes and N90 axis. These folds can be compared with the isoclinal intrafolial folds F1 with a hinge parallel to the L1 lineation observed in the Andriamena unit.

### 3.3.2 The Ambakireny area : dome-and-basin structures

The Ambakireny area is located in the eastern boundary of the Andriamena unit (Fig. 2) and is bounded at the east by the north-south Angavo shear zone. The S1 regional foliation pattern in this area defines typical dome-and-basin structures (Fig. 7). The main features are:

1) The S1 foliation is parallel to the lithological contact between the mafic gneisses (Andriamena unit) and the underlying gneissic and granitic basement. The mafic gneisses of Andriamena are systematically located in the basins whereas the gneissic basement defines the domes. This clear lithological control of the structures suggests that the marked density contrast between the mafic gneisses of Andriamena and the less denser gneissic-granitic lithologies of the basement, i.e. the body forces, partly control the formation of the dome-and-basin structures.
2) In the foliation map (Fig. 7), we can observe that the structures are elliptical with their long axis oriented N160 to N180 (Fig. 7). In the central parts of the gneissic domes, the foliation is subhorizontal and becomes steeper at the boundaries. Into the basins, where the mafic gneisses outcrop, the foliation is subvertical and folded by the upright F2 folds with N-S steeply dipping axial planes and subhorizontal axis (Fig. 4). This folding, as well as the elliptical shape of the structures, is in agreement with the D2 regional east-west horizontal shortening (boundary forces).

The contacts, between the Andriamena unit and the underlying basement, are generally steeply dipping, but close to the synformal closures they become less dipping. Towards the contacts, there is a strain gradient marked by a high tectonic transposition, but we never observed kinematics indicators that should show a downward displacement of the mafic gneisses of the Andriamena unit relative to the granitic basement in relation with the density gradient. Locally, around the Andraikoro dome, just north of Ambakireny, the lineations characterized by a high pitch seem to display a radial pattern broadly centred on the core of the dome (Fig. 7) which is characteristic of a diapiric evolution (Bouhallier et al., 1995).

In conclusion, the regional horizontal E-W shortening (boundary forces) and the density gradient between the mafic gneisses of the Andriamena unit and the granitic rocks of the basement (body forces) control the deformation in this area.

### 3.4 The western Andriamena/basement contact : a major mylonitic zone

In the western boundary of the Andriamena unit, the basal contact between the mafic gneisses of the Andriamena unit and the gneissic-granitic basement, is characterized by the occurrence of a major mylonitic zone. This north-south trending structure extends over more than 200 km, suggesting that the Andriamena/basement contact acted as a major deformation zone during the tectonic evolution of the north-central Madagascar. It lies parallel to the S1 foliation, dipping to the east (Fig. 4), with a thickness ranging from one to several meters. The stretching lineation associated with the mylonitic foliation is defined by the elongation of quartz aggregates and the preferred orientation of syn-kinematic biotite and is oriented east-west, plunging to the east.

Numerous kinematic indicators occurring at various scales, like sheath folds developed in the YZ section of the finite strain ellipsoid or C/S structures, asymmetric microfolds and asymmetric boudins in the XZ section indicate a non-coaxial deformation regime (Fig. 8). The sense of shear in this mylonitic zone is consistent with a downward movement of the Andriamena unit, or in another words, a top-to-the-east sense of shear suggesting an apparent extensional shear sense. However, the late folding (D2 event) of this contact impede a direct kinematic interpretation of these shear sense indicators.

The mylonite is composed by a quartzite-phyllite alternation at cm-scale with a metamorphic assemblage (hornblende, feldspar, epidote, biotite and quartz) compatible with a deformation under amphibolite facies conditions. Quartz layers are composed by elongate monocrystalline quartz ribbons with an ondulatory extinction and/or polycrystalline quartz ribbons. The micaceous layers contain rounded fragments of feldspar, which can be locally, disrupted yielding an apparent opposite sense of shear (Fig. 8).

# APPENDIX G

## Interview questions: Geologists' fieldwork and reporting practices

1. Overview of the informant's specialty in geology
    a. What is your area of expertise?
    b. How would you describe your work?
    c. Where does your area of specialty "fit" into geology?
2. Biographical information
    a. How long have you been a geologist?
    b. When did you receive your doctorate?
    c. What training have you received? (e.g., D.E.A., Ph.D., Post-Doc, 1st position, 2nd position, etc.)
    d. How did you become a geologist, or what drew you to it?
    e. What does it mean to be a geologist? In other words, what kind of a person becomes a geologist?
3. The nature of doing fieldwork
    a. What is a general "picture" of a day in the field?
    b. How is fieldwork done?
    c. What kinds of information get written down?
    d. Does each field expedition have its own "story" that differs significantly from one study to the next?
4. The written fieldwork account
    a. What are the different areas (i.e., publications, presentations) where fieldwork is reported on?
    b. In its different stages, what sorts of information gets left out and why?
    c. How long is the process from "day in the field" to published account?
    d. How would you describe this process?
    e. How different is the field experience from the published account? (ex. Methodology sections in biochemistry articles)
    f. What is the importance of fieldwork (GS) for the whole of the published research?
    g. How does one talk about fieldwork in the publication?
    h. What is the role of visual representations in understanding/ interpreting/explaining your fieldwork?
5. Filtering out information
    a. What sort of information about fieldwork done for a particular project is important for other researchers?
    b. What sort of information is less, or not, important?
    c. How does one learn to sift through this information? (ex. jb's thesis on Moroccan geology published late 1970's, using explicit geographical features, such as "the outcrop by the doctor's hut")
    d. How much or how little of a personal field presence does one show in publications?
    e. How acceptable is personal style? What style do you consider to be "the norm", and how much can one reasonably deviate from that norm?

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