CHAPTER ONE: INTRODUCTION

1.1. ORGANIZATION OF THE VOLUME

This guide emerges from more than 28 years of field research in over two dozen languages of the Brazilian Amazon, as well as from teaching field methods courses in Brazil, the US, and the UK, and the enjoyable experience of taking many graduate students and seasoned linguists from Brazil, the US, Israel, and the Netherlands with me to train them in fieldwork methods. I have always enjoyed the isolation and intense challenge of field research and the presentation of the lessons learned thereby to the international linguistics community. Many of the suggestions found here are those I wish someone had made to me before I began (or that I wish had followed in) my field research career.

This guide has ___ chapters and ___ appendices, covering the topics I consider to be most useful for field research, including personal preparation for field work, designing 'lab sessions' (my suggested term for what are sometimes known as 'informant sessions'), the ethics of field work, selection of native speaker teachers, processing data, design of web-based data presentations, and suggestions on fieldwork (including suggestions on writing up results for publication) for specific linguistics subfields, e.g. phonology, morphology, and syntax, as well as the history and philosophy of fieldwork and suggestions on writing grant proposals to fund your research.

I will believe that this book is a success if it helps researchers collect and analyze linguistic data in a way that is helpful to other researchers.

1.2. WHAT THIS BOOK IS ABOUT

This book is intended for upper-division undergraduate students and above. It presumes basic knowledge of most areas of linguistics. Some parts are harder than others to understand at first blush. To lighten up the reading and make it more personal, I have added anecdotes from my and others' experience in highlighted discussion boxes. However, I believe that all chapters will be useful to the fieldworker. A special feature of this book is its detailed suggestions for phonological fieldwork, including chapters on segmental phonology and a very important guest-authored chapter by Robert Ladd and Nikolaus Himmelmann on prosodic fieldwork. It also includes a comprehensive phonological questionnaire, developed by the author, to aid in the development of professional phonological studies of a field language.

The major theme to be developed in this book is how to do field work, independent of any particular theoretical perspective. The book's major thesis is that *linguistic fieldwork can be successful with proper preparation and execution, bringing deep personal and professional satisfaction for the researcher and her native-speaker teachers.* The book's purpose is to help linguists do, enjoy, and succeed at field research. This guide is mainly step-by-step, detailed advice on how to go about the business of fieldwork, from prefield preparation to field and post-field phases. All the chapters will include many personal examples from my own and others' fieldwork experience.

The history of research in general and field research in particular, is the history of evolved creatures struggling to understand nearly agonizingly complex facts in an alien environment. No one person is fully up to the demands of fieldwork. So the outputs of our fieldwork will necessarily be incomplete records of each individual's progress in understanding parts of wholes that exceed any one person's abilities. Thus, our research reports, whether grammars or articles or talks or webpages are never more nor less than our efforts to communicate with interested interlocutors about the beliefs we have come

to form and hold, based on our experiences and how these beliefs affect our actions in science and in life. This is our canopy of epistemic humility. No one can do more, though there is still tremendous potential for qualitative variation in the effort.[1]

And we each need to approach the field well-armed with such humility. Arrogance is ignorance, especially in field research, where one's limitations appear in stark relief at all times. I am more and more convinced that the beliefs we have come to hold about a particular language or grammar are constrained and shaped by the totality of our experiences, not merely our linguistic training. If this is correct, an immediate consequence for fieldwork that emerges is that compartmentalization of knowledge and the isolation of knowledge from application should be avoided. Field research is holistic – it involves every bit of the researcher's personality in every bit of the language and culture under study, whether overtly or covertly, consciously or unconsciously. This thesis underlies this entire book.

1.3. Becoming an alien – Mind the gap

When you cross the boundary from your language and culture community to another, you need to realize that you are transmogrifying yourself from someone familiar into an alien. You could become a 'freak' instead of an attractive person; an incompetent, instead of a respected professional; ugly instead of lovely; fat instead of average; stinky instead of normal-smelling; and on and on. You may go from being articulate and witty in conversation to being perceived as an infantile dullard who can barely function in conversation. You will go from having many friends to having none. From enjoying good company, to stark loneliness. From having your personal space respected and being treated with dignity, to being seen as outside the normal politeness conventions and treated like a fruit tree (people get things from you whenever it is opportune for them). Familiar cues and clues are missing. You will be frequently disoriented, feeling despair on the worst days.

Most disturbing, perhaps, if you are part of the majority group in your home country, is that you will become a minority. It may or may not bring you prestige to come from a wealthier background than people in the community who will be teaching you their language. In my case, it occasionally means going from being called 'Dan' to being called 'gringo', from being trusted, to being distrusted. Occasionally, traveling in some parts of Brazil, I feel like if I say 'It's a nice day, isn't it?', my interlocutor is likely to think 'Hmm. Why is he saying that? What is up his sleeve?'

People will almost certainly misclassify you. They may think you are an anthropologist, not understanding what a linguist is (actually, then I think they would be right, but that is another matter). They may think you're a missionary. They may claim you're are with the CIA. They may think you're a different nationality even when you're not. When they do find out your nationality, they are likely to have a different view of it than you do yourself, assuming they are of a different nationality from you. (So, for example, if you're German they may say 'Heil Hitler'. If you're an American they may say 'Hooray for Osama' (which I have actually heard), and so on.)

The prospective field researcher must give careful thought, therefore, to the many barriers they are crossing by entering another community to study their language. In the next section, I briefly consider some historical examples of barrier-crossing from the Americas.

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[1] Much of this section is taken directly from Everett (2004).

FIELDWORK IN HISTORICAL PERSPECTIVE

Linguistic fieldwork has as many histories as there are countries in which it has been carried out. And there is no global history of fieldwork (and virtually no local histories either). Nor is this the place to write one. Nevertheless, some historical issues are relevant to understanding the nature of field research and to preparing to do this work. Therefore, I want to offer just a very brief discussion of field research in Brazil and the USA, as examples of the kinds of issues, problems, and solutions faced by both individual field researchers and the general enterprise of field linguistics.

Arguably, field linguistics in the Americas, as field linguistics most places, began as an extension of colonial activity, specifically, missionary work. Let's first consider the case of Brazil, then move on to consider the USA.[2]

Fieldwork in Brazil
In the Colonial Era (1500-1822)[3]

On April 22, 1500, a flotilla of ships commanded by Pedro Álvares Cabral appeared off the coast of what is today the city of Porto Seguro, in the current-day state of Bahia. Almost immediately, the sea-weary sailors of Cabral's ships spotted men and women on the shore, looking out at the ships. A group of sailors rowed to shore and were greeted warmly by those people bold enough to remain and not flee into the jungle. Thus occurred one of the first contacts between Europeans and South American Indians, in this case the Tupinambá. Cabral eventually sailed off towards his intended destination of India, around the Cape of Good Hope, finally arriving back in Portugal, with news of the new land, to be called 'Brasil' (for the ***pau brasil***, a tropical redwood that came to be highly valued in Europe). As it had begun with Ignatius of Loyola (1491-1533), the founder of the Jesuits and the modern missionary movement, the Church recruited missionaries to take the gospel to the newly-discovered heathens of Brasil. One of the earliest missionaries to reach Brazil was the Jesuit Padre José de Anchieta (1533-1597). Anchieta turned out to be a brilliant linguist (and administrator – he was co-founder of both the cities of São Paulo and Rio de Janeiro). Anchieta began his work near what is today the city of São Vicente between Rio de Janeiro and São Paulo. The original people contacted by the Portuguese explorers were the Tupinambá, a language of the Tupi-Guarani family.

Along with the very closely related language, Guarani, spoken to the south, in what is today southern Brazil and Paraguay, Tupinambá was spoken along a sizeable portion of the Brazilian coast, from São Vicente to what is today the city of São Luis do Mararanhão. Wherever the Portuguese landed their ships north of São Vicente they encountered the Tupinambá, eventually coming to refer to their language as the 'Brazilian language'. It was to this language and people that Anchieta gave the majority of his attention during his missionary career in Brazil. Anchieta produced a grammar, a

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[2] Cite my SOAS paper, etc.

[3] Much of the introduction to this chapter closely follows my article, 'Coherent Fieldwork', Everett (2004).

dictionary, and translations of catechisms. His grammar and dictionary still rank among the best ever produced of a Brazilian language, nearly 500 years later. Although his missionary activity was partially responsible for the complete extinction of the Tupinambá people (largely because the Jesuits increased the size of Tupinambá villages, thus increasing mortality rates when European diseases infected local populations), Anchieta was a dedicated linguist whose work can be considered the beginning of Amazonian linguistics (indeed, it would not be stretching matters too far to call his work the beginning of linguistics in the Americas).

In addition to Anchieta, Tupinambá was also the object of some study by the French Calvinist Jean de Lery (1534-1613), who originally went to Brazil to establish a French Protestant colony. Lery's principal contribution was to record in written form some naturally-occurring Tupinambá conversations. These enhance the picture of the language presented in Anchieta's grammar and reinforce the importance of conversational data in the documentation of endangered languages, since Lery's data is now the only record we have of the living form of this language in use.

Several decades after Anchieta and Lery, another Jesuit, Padre Antonio Ruiz de Montoya (1585-1652) arrived in what is today the border region between Brazil and Paraguay to work among the Guarani people, speakers of a Tupi-Guarani language very closely related to Tupinambá. Like Anchieta, Montoya was a brilliantly talented and dedicated linguist, also producing a grammar and dictionary of the language (Montoya is a partial model for the composite character of the priest played by Jeremy Irons in the movie, *The Mission*).

After these few examples of precocious linguistic studies of endangered languages (though Guarani has managed to survive this early troubled history), the field of Amazonian studies was to lay fallow for the next several hundred years, aside from reports and word lists from a succession of European explorers, mainly from Germany, under the influence and example of Alexander von Humboldt (1769-1859).

So field research in Brazil began as a colonial activity. As such, its initial purposes were utilitarian, to serve the Church, to get catechisms and the gospel into indigenous languages. This story was repeated in country after country, around the world. Native speakers were not valued for their knowledge and language but rather for their role as objects in the colonial (and personal) goals of the missionary linguist. They certainly played no active role in shaping the goals of the studies of Anchieta, Montoya, and others, at least not that we have any record of or any reason to believe. In modern days, however, missionary efforts have been very important in the development of field research programs and traditions in different countries, though the attitudes have remained very similar, in the sense that the native speaker community plays no or very little role in shaping the missionary's objectives and activities among them. To see this, let us consider the modern history of field research in Brazil.[4]

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[4] Spratt (2004) is a fascinating and largely convincing study of the influence of Native Americans on new world philosophy, which has been both profound and uncredited. The book, 1491, by Mann (2005) also demonstrates, very convincingly, the intellectual richness of pre-Columbian Native American populations.

Fieldwork in the contemporary era

Brazilian linguistics in the modern sense arguably begins with Joaquim Mattoso Câmara Jr. (1904-1970), who dedicated a significant portion of his life to the introduction of modern linguistics into Brazilian university (and pre-university) training. Câmara did not spend much of his illustrious career on the study of Brazilian indigenous languages, but he did encourage their study as part of the development of Brazilian linguistics. In terms of the study of Amazonian languages *qua* endangered languages, the pioneer in Brazil surely is Darcy Ribeiro (1922-1997), perhaps the first government official of the Americas to invest government resources specifically earmarked for the documentation and description (and for him, the 'preservation') of endangered languages. During his tenure as Chefe da Casa Civil for Brazilian President Jânio Quadros in the early 60s, Ribeiro invited the Summer Institute of Linguistics to Brazil in the late 1950s. Ribeiro states his motive in inviting SIL to Brazil as (my translation, DLE):

> *"My objective was to save for linguists of the future, who possibly will know how to study them, the languages as crystallizations of the human spirit, in order that we might learn more about mankind."* (Ribeiro 1997, ---)

Ribeiro's adminstrative and anthropological concern for the indigenous peoples in Brazil's survival and welfare was admirable and extremely forward-looking. We return to the mixed results of his initiatives below.

In terms of personally-conducted research, the modern pioneer of the documentation of Amazonian languages was Kurt Unkel (1883-1945) a German, later naturalized Brazilian. This famous explorer, linguist, 'indigenista' , and anthropologist, known to most Brazilians as Nimuendaju – the Guarani name he was given in 1906 and used until his death in 1945 (partially) documented and identified a very large number of Amazonian languages. Amazonian languages are still difficult to access physically, culturally, and linguistically today. They were far more so in Nimuendaju's day. Yet he managed to visit the majority of Brazilian Amazonian languages personally, taking competent word lists from the many groups he visited, which have been extremely valuable in the linguistic classification of these languages. Nimuendaju is today perhaps the most revered figure in the history of the study of indigenous languages in Brazil, making tremendous personal sacrifices to both study and support these languages and their peoples. Stories of his life are currently only available in Portuguese to my knowledge and even these are fairly superficial in their coverage. One hopes that one day Nimuendaju's life and contribution to the study of Amazonian languages will receive the attention it deserves. His concern for endangered languages and peoples motivated not only his professional career but his entire life, from about 1906 until his death. Nimuendaju was not motivated by the desire to change the people he studied, so in this sense his work was an ethical improvement over earlier missionary efforts. He wanted to provide a record of the peoples' languages and cultures. But his activities still represent an intermediate level of ethical relationships with the communities, because they still fall far short of engaging the native speakers as co-shapers of the records

about themselves. Indians did not sit with Nimuendaju, for example and guide his studies in any significant way, at least all records indicate otherwise, namely, that he approached his studies with pre-determined objectives that were not negotiated in the local context.

To most linguists, however, the true beginning of modern linguistic studies of Amazonian languages in Brazil, entailing historical and comparative research, emphasis on extensive grammars and dictionaries, begins with Aryon Rodrigues (1925 -) – who published his first articles on these languages before he was thirteen, as an eighth-grade student in his native city of Curitiba, Paraná. Later Rodrigues was a friend and colleague of Darcy Ribeiro at the University of Brasilia when Ribeiro served as the University's first Rector (Rodrigues currently is a Professor Emeritus at the University of Brasilia).

Rodrigues combines most of the positive characteristics of previous figures mentioned above. Administratively, he has founded linguistics programs, with strong emphases on Amazonian studies, at the University of Brasilia, the Federal University of Rio de Janeiro, the National Museum in Rio de Janeiro, and the State University of Campinas (UNICAMP). Although Rodrigues has done little fieldwork of his own, he has supervised countless graduate students' research (including my own MA thesis). Rodrigues is a beloved advisor, from my own experience, but he, as a result of his vast experience, has strong opinions (as most advisors will) about what the student should be doing and how she should be thinking about the data she has collected. This raises another issue, however, namely the role of the advisor in shaping the the field record of the student field researcher. Students working under dominating advisors, or insecure students working with a revered advisor, etc., can unconsciously or consciously allow the advisor to tell them what to look for, how to think about it, how to relate to the people, what conclusions make most sense, etc. This influence can be very helpful and the modern academic system is partially predicated on the assumption that it will be. However, it can also put the native speaker even farther away from the decision-making and goal-planning stages of research (see section ___below for more on the role of the native speaker). The student thus has an even more delicate and difficult task in field research. She must engage the native speakers as conscious, willing, and active shapers of the record, at the same time that she develops her own intellectual goals for the research, while simultaneously satisfying an advisor that may be impatient or at least skeptical of her decisions. These are natural tensions in life, of course, i.e. balancing multiple demands of various people, but they are pervasive in field research and students are particularly vulnerable. Therefore, the advice to the advisor is to give the student as much freedom as is possible to work out her own field program, while at the same time not relinquising the responsibility to ensure quality control.

Fieldwork in the USA

In the US, the Jesuits and other missionaries played a similar role to Anchieta and Montoya in beginning studies of indigenous languages.[5] However, professional linguistic and anthropological fieldwork began with Franz Boas (1858-1942), who trained a core of linguistically-aware anthropologists (Ruth Benedict (1887-1948),

[5] MARY DORIA RUSSELL's *THE SPARROW*

Edward Sapir (1884-1939), and in some classes and via Sapir, Mary Haas (1910-1996), among others) responsible for the birth and growth of North American linguistics. During the years of Boas's influence, roughly during his life and following his death until the 50s, North American linguistics was concerned with describing specific languages in detail, producing integrated studies of texts keyed to cultural studies, grammars, and dictionaries, providing exactly the kind of pragmatist study that has proven to be so important to knowledge of little-studied peoples and their languages throughout the intervening years. In fact, though this is not the place to attempt a more detailed intellectual history, a case can be made that this earlier descriptive linguists were heavily influenced by the pragmatist philosophy underlying much American intellectual endeavor until at the least the death of John Dewey (1859-1952), itself arguably influenced by Native American philosophy (Pratt (2004)). Thus in a roundabout way, Native American thought influenced the way that Native American languages were studied and documented, at least until the 1950s. Consider some remarks of Boas in his 1917 introduction to the first volume of the new **International Journal of American Linguistics** (IJAL). According to Boas one of the principal goals of the new journal was to provide what I would call a 'coherent' report of languages. For example, he (1917, 201) laments the fact that "… the available material gives a one-sided presentation of linguistic data, because we have hardly any records of daily occurrences, everyday conversation, descriptions of industries, customs, and the like. For these reasons the vocabularies yielded by texts are one-sided and incomplete." That is, Boas felt that a full 'picture' of a given language was only possible by looking at the language in the cultural context. Or consider Sapir's (1915, 186) assertion that more studies are needed of cultural 'modalities of attitude' and consonantal alternations (I discuss this further in Chapter Seven below), thus explicitly connecting grammar with culture.

Boas (1911, 63-67), in his introduction to the handbook of American Indians, provides perhaps the best statement of the relationship between language and culture ever given. His discussion of this relationship was directly related for him to the connection between fieldwork and theoretical research on the nature of language and the nature of culture.

> *"If ethnology is understood as the science dealing with the mental phenomena of the life of the peoples of the world, human language, one of the most important manifestations of mental life, would seem to belong naturally to the field of work of ethnology, unless special reasons can be adduced why it should not be so considered."*

In the same passage, Boas proceeds to consider and reject several proposed 'special reasons'. He goes on in this section to consider ways in which culture may affect a language's morphology, lexicon, and grammar, concluding this section by stating (p67)

> *"It does not seem likely, therefore, that there is any direct relation between the culture of a tribe and the language they speak, except in so far as the form of the language will be moulded by the state of culture, but not in so far as a certain state of culture is conditioned by morphological traits of the language."*[6]

[6] This passage is particularly interesting in that it seems to contradict the linguistic relativity hypothesis often associated with Boas. I think it is fair to say that Boas was

Sapir carried on the Boasian tradition describing languages as manifestations of culture, human psychology, and local conditions. And he influenced more than any other person the course of North American linguistics until the 1950s, with the possible exception of Bloomfield.[7]

Thus, for the first half of the Twentieth Century, the normal North American conception of the linguist's 'job' was to study little- or un-studied languages in the field and to produce coherent bodies of data on the interaction of culture, lexicon, texts, and grammar. But by the 60s this had changed radically, with field research given more or less the intellectual status of butterfly collecting. Postal (1968, ---) referred to previous linguistic theories as 'taxonomic' while even today some refer to fieldworkers as 'data fetishists' or 'hunters and gatherers'). What are the forces that changed the attitudes to field research in North America (and eventually the world) so drammatically? It is what we may call (with no perjorative intent), the 'Chomsky factor'. The twentieth-century withering of fieldwork began innocuously enough, in the restlessness of a graduate student at the University of Pennsylvania with his MA research:

> *"Harris suggested that I undertake a systematic structural grammar of some language. I chose Hebrew, which I knew fairly well. For a time, I worked with an informant and applied methods of structural linguistics as I was then coming to understand them. The results, however, seemed to me rather dull and unsatisfying. Having no very clear idea as to how to proceed further, I abandoned these efforts and did what seemed natural; namely, I tried to construct a system of rules for generating the phonetic forms of sentences, that is, what is now called a generative grammar." (Chomsky 1975,25).*

Chomsky's intellectual frustration with (an extremely easy version of) standard fieldwork led indirectly to some of the most important developments in the 2000 + year history of the study of language, so I am hardly complaining about the direction

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here concerned with something else, namely, the classification of languages by culture, which he rightly attacked as quite erroneous. Nevertheless, Boas does us a service here by showing how the language-culture connection is bidirectional. This has obvious and important consequences for field research, discussed in more detail in chapter six below.

[7] One influential linguist deeply impacted by Sapir was Kenneth Pike, who was both a professor of linguistics at the University of Michigan and the President of the Summer Institute of Linguistics (SIL) for over a quarter of a century. The rise of SIL in the second half of the twentieth century and its Sapirian influence gave a huge impetus to the study of American (and other) indigenous languages, as well as to the general enterprise of field research. In the initial period of SIL's growth, there was little overt questioning of the missionary enterprise. To my mind at least, SIL's nearly worldwide acceptance and expansion in the late 40s and early 50s owes a great deal to the fact that developing countries were, for obvious political reasons, welcoming citizens of the country that defeated the Axis powers, with at least overt enthusiasm. The postwar period is a period of the expansion of US influence with parallels to the 16th century expansion of European colonies.

Chomsky decided to take. Nevertheless, the very intellectual vigor and power of Chomsky's subsequent work sufficed to pull most linguistics students and departments away from the traditional emphasis on fieldresearch to theoretical work on, for the most part, the linguist's native language. Though there is nothing inherently anti-fieldwork in Chomsky's research programme, his attitude, as expressed in the passage just cited, and his rejection of the intellectual priorities of Boasian linguistics led to an abandonment of fieldwork in the US and a nearly five-decade neglect of the study of indigenous languages and fieldwork throughout the linguistics world, as his influence soon became massive and international. Over the past decade as the spotlight has begun to shift to fieldwork once again, it has been primarily concerned with the study of endangered languages (see ___ below) and has not yet recovered the 'Boasian imperative' of coherent, integrated fieldwork. This is unfortunate and one hopes that we will continue to make our way 'back to Boas'. This guide is meant partially as an aid to that journey. In that sense, this guide is theory-situated.

As I say, the resurgence of interest in linguistic fieldwork (or at least in talk about it) from the late 20th century is largely linked to the concern for documenting and describing endangered languages. The interest in language endangerment itself had been an important motive for early field research (see the quote, for example, from Darcy Ribeiro above), especially among Boas and his students, but went out of vogue for decades, making a comeback in the early 1990s. It is perhaps best exemplified institutionally today by the Hans Rausing Endangered Languages Documentation Project at the School of Oriental and African Languages in London and the Centre for Linguistic Typology at Latrobe University in Australia, both established since the 'endangered languages' movement began, as well as through several funding (e.g. an NSF program and the DOBES (http://www.mpi.nl/DOBES/) project for the study of endangered languages) and technological initiatives (e.g. ELAN, part of the DOBES project, and EMELD (Electronic Metastructure for Endangered Languages Data, http://emeld.org), University linguistics departments and the general public have begun to appreciate the fact that languages are dying daily and that with them die millennia of accumulated knowledge and ways of talking about and experiencing the world and examples of different linguistic evolutionary paths (or so Everett (2005) argues). From a hard-nosed linguistic viewpoint, however, all languages need to be better described and documented and the most important criteria for determining which languages should be studied, to me at least, have to do with ensuring that the sampling of languages we document is sufficient to warrant linguistic claims about theoretical principles and typological universals of human grammars and languages. Although one can accept the claim that endangered languages are the most urgent priority, the long-term view of linguistics research must be to produce the best science it is able to do and this means that we need diverse and robust data to better understand whatever it is about Homo sapiens that ultimately underwrites their ability to have grammars and language and use them. This entails more fieldwork, since so many areas of the world are under-represented in linguistic research and because certain types of linguistic phenomena are under-represented in the documentation of languages (e.g. intonation, information-structuring, the phonetics underlying the phonology of a given language, etc.).

In spite of the general belief in the scientific equality of all languages, there is a sense in which workers on little-studied languages need a guide more than those who study better-known languages. To see what I mean, consider that if someone makes a claim about, say, English syntax or French phonology, there are hundreds of scholars

and millions of native speakers that are in a position to challenge analytical assertions they disagree with. But in work on little-studied languages it is often the case that very few people, if any, will be in a position to seriously test the actual data used by the linguist, unless the linguist has followed careful procedures that encourage, facilitate, and promote as much replicability and soundness of presentation and analysis and data-preservation as possible.

Unfortunately, there are few guides available to help the linguist go about the business of fieldwork in the twenty-first century, especially for the linguist with the goal of documenting and analyzing a large portion of grammar or language, while working within a community of speakers of the language, away from their base institution and confronted with the massive novelty of language in the real world. This is because nearly all of the extant linguistics field guides were written decades ago and fail therefore to respond to extensive developments in linguistic theory, methodology and technology over the years (e.g. personal computers, lap-top software for acoustic analysis, well-developed theories and notations for the study of intonation, advances in morphological theory, discourse theory, functional and formal theoretical developments, and on and on). To take one example, the field guide that I found most useful in my early fieldwork, beginning in the late 1970s, was William Samarin's 1967 ***Field Linguistics***, but this was already going out of date when I used it and many portions of it are simply no longer applicable. And the very small number of field guides that have appeared in recent years are for the most part orientated to special field areas or otherwise limited in their general applicability.

A Definition of fieldwork

Let's begin our discussion of field research by hazarding a definition, from Everett (2001, __):

(1.1) **Fieldwork** *describes the activity of a researcher systematically analyzing parts of a language other than one's native language (usually one the researcher did not speak prior to beginning fieldwork), within a community of speakers of that language, prototypically in their native land, living out their existence in the milieu and mental currency of their native culture.*

I think it is useful to consider this definition in more detail. Again, by dwelling on my own definition, I am not claiming that it is 'right' in some absolute sense. But it does raise issues worth considering, however one ultimately comes to understand the essence of field research.

'Systematically analyzing' should be clear. We go into field research with a system of ideas that guide our research. How does this system guide, then, what we are going to study? What are the subparts of the system? How do the different subparts of the system, projected onto the language of study, interact? For example, perhaps I am conducting research to test a specific claim in the literature, e.g. 'language **x** lacks embedding.' What system could there be to my investigation? Well, first, if a claim has been made to this effect, I want to check the data that was adduced on its behalf with native speakers. Do native speakers agree with all the grammaticality judgements offered to support the claims being tested? Are there discrepancies across speakers? And so on. To check data requires a plan. How many speakers should one check the data with? How should one subcategorize and test discrepancies in speaker judgments? How can one design and test alternative hypotheses? Second, if a language lacks

embedding, it should be reflected several places in the grammar, not merely, say, in the absence of complement clauses. Does the language have disjunction? Coordination? Verb phrases? Complex noun phrases? Adjectival phrases? And so forth.

Or let's assume a different kind of study, e.g. Topicalisation in narrative discourses of language **y**. How does one study this systematically? Another way of putting this is 'what is my specific hypothesis and what are its component parts? How do I order them for study?' Well, first it is crucial to separate out the various components claimed to be part of Topicalisation, based on theories of Topicalisation in the literature – things like the manner of use and frequency of proper names, constituent orders for different sections of discourse, types of anaphora or cataphora at different points in the discourse, and so forth. A study of each of these, independent of the global hypothesis, to determine their functions and structures within clause and sentences, should be undertaken, so as to minimise bias caused by looking at them only in light of the hypothesis being tested. These superficial examples give some idea of what I mean by 'systematic' in () above.

In other words, field research is like any other large, complex task. It requires planning, administration, progress checks, self-evaluation, and reports (at least to oneself). In Chapter Two we discuss the application of some basic management principles to field research.

Continuing on with a discussion of (1), why does it refer to 'parts of a language', rather than an 'whole language'? First of all, it is impossible to study a whole language. Just consider the thousands of studies of English language and the fact that there is no sign that research on English is coming to an end. A language is vast and beyond any single researcher's ability to study in a human lifetime. Language is everything: semantics, sociolinguistics, phonetics, phonology, syntax, morphology, ethnography of communication, and on and on. Second, Language as an object of study is unclear, unfocussed – there are no boundaries to identify either a coherent beginning or end of the study if its object is 'documentation of English' or some such. The goal of 'parts of language' requires a lowering of the of the sights from Language to *selected* components. Their selection requires hypothesis management, and a notion of the 'systematic' just discussed. It also requires a coherent vision of how the parts fit together, assuming that the study is to fit together at the end, that it is not strategically opportunistic.[8]

Next, why does my definition include the qualification 'other than one's native language'? First I am a bit of a purist. I think that being a '*field* linguist' should be different from being a 'linguist' (i.e. that hyponyms are not the same as hypernyms), and that its difference is exactly the crossing of linguistic and cultural boundaries. But also, I think that it is important to separate out different research foci, based on their peculiar requirements on the linguist of training for and execution of the research. Not everyone will accept this, I realize. But without some such qualification, field linguistics loses its distinctiveness as a term.

Now comes one of the most vital components of my definition and the component that sets it apart from many other conceptions of field research, namely, that field research should be conducted 'within a community of speakers of that language.'

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[8] It very legitimately might be if one finds oneself with access to a language one hadn't planned to work on, but which presents itself in a set of circumstances, with interesting properties or is endangered, etc.

This differs radically from others' conceptions of field research. So consider what Samarin (1967, 1ff) says in his classic **Field Linguistics**:

> 'Field linguistics can be carried on anywhere, not just *in the field*, as its name implies. [emphasis Samarin's, D.L.E.] A "field archaeologist" must go out to where he expects to collect his data, but a linguist can bring his data to himself. Thus, some fieldwork is done by bringing jungle dwellers to a city and is conducted in an office instead of a lean-to.'[9]

Before I say why I disagree with Samarin and all the linguists who agree with him (e.g. Hyman ()), let me hasten to say that circumstances have often forced me to work with native speakers in offices, hotel rooms, and missionary compounds. If there is a language I need or want to study, I will go where I have to go, even if the circumstances are not ideal in some way.[10] It will become clear in this book that we must always be prepared to improvise. But my position is based on the view that language and culture are inextricably intertwined. You cannot understand one without the other (Everett (2005)). This is discussed further, with several examples, in ___ below, and it is implicit in Boas's statements above. By taking speakers out of their communities or studying parts of languages outside their cultural contexts, I believe that vital pieces of understanding go missing and that the resultant grammar or study can be seriously flawed. The rest of the definition in (1) is there for the same reason, namely, because grammar and culture affect and, to some degree, effect one another.

POSTAL'S MAXIMS

One of the books I learned from early in my career was Longacre's *Grammar Discovery Procedures* (Longacre ()). The title seems like a tongue-in-cheek poke at Chomsky's assertions to the effect that there are no such things as 'discovery procedures', either in linguistics or science more generally (Chomsky 1957; 1975 [1955]). Interpreting the book charitably, Longacre lays out a general list of heuristic procedures for developing hypotheses on the grammar of the language under investigation. Although the book is still useful in many respects, however, it doesn't really get at the core components of building a theory of aspects of a grammar or language, the essence of field research.

Paul Postal noticed these shortcomings in his review of the book and made the following, extremely useful, observations:

> *"... I would strongly suspect that the two most important 'discovery procedures' [are to] learn the language of study as well as possible and attempt to*

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[9] In an interesting paper on fieldwork as a state of mind, Larry Hyman suggests that fieldwork can be done any place, so long as the linguist is properly prepared. To some degree I agree with this. On the other hand, if I am correct about the connection between language and culture, a language must be studied in a culturally robust community of speakers.

[10] George Carlin said in a routine at the Beacon Theatre in Boston that 'You know me. If it's got a zip code, I'll f___in' be there.' Well, I am like that, substituting, say, 'grammar I am interested in' for 'zip code'.

> *formulate an explicit account of the rules which generate the full syntactic structures of its sentences."* Postal (1966, 98)

I will refer to Postal's proposals here as *Postal's Maxims*. I restate them to better suit my objectives here in (1.2):

(1.2) a. Learn the language.
b. Generate and test hypotheses about the language.

To rephrase, since I believe that this is crucial: The most important thing to remember here and in all areas of linguistic analysis in the field that the most important two methods are: learn the language and formulate and test new hypotheses daily. All analysis is tentative. Nothing is ever certain. It is unreasonable for people to think that a grammar must 'stand the test of time', for example. Grammars are temporary documents. Ideally, they represent the best a given linguist can do in a particular slice of the space-time continuum. New evidence comes to light to force a change in analysis. New perspectives. New theories. There is no algorithm for discovering a grammar. Only hard work, ideas, alternative ideas, and testing.

That said, let us consider suggestions for succesful field research by turning our attention in Chapter Two to prefield preparation.